

new piece of apparently supportive evidence has come in, I have first of all acted as devil's advocate. I have contacted scientists from appropriate fields, to sample their views. And unless I have been certain that something is sound, I have always erred on the side of caution. My American and British publishers, too, have played an important role here, with the very careful scientific and legal readings which the book was given before publication. Not surprisingly, they wanted to be certain that I was able to justify everything I'd written.

Perhaps the best evidence that I have not bent the facts to the theory, or rushed to premature judgement, would be the thrice-given testimony given by Gaston Ninane in 1992, that the Koprowski vaccines had been made in "chimpanzee kidney cells". After I told Dr Ninane that the scientific literature at the time had mentioned only "monkey kidney cells" he suddenly retracted, saying that this was what he had intended to say – and I accepted that he had made a mistake. It would have been far easier, had I been intent on forcing the issue, simply to quote the words that he had said, and to ignore the subsequent denial. In the end, it took nine years of further research before I was finally convinced that what he had originally told me *was* the truth.

But what of "untruths" told by the other side? There are quite a few examples of these to be found in *The River*, and others that can be discerned in the rivulets that run through the endnotes, or between the lines. One of the more obvious examples involved Koprowski's repeated claims in the literature that he was making his OPVs in chick embryo, and his thrice denying the use of primate kidney tissue culture.<sup>412</sup>

Others examples are still coming to light. One relates to the "vaccinations in response to epidemics" which were staged in *Province Oriental* in January and February 1958. I have recently reviewed three different accounts of these outbreaks (one by Koprowski,<sup>413</sup> one by Courtois,<sup>414</sup> and the third by Wilfrid Bervoets, a Congo-based government inspector of hygiene),<sup>415</sup> and realised that they offer comprehensively conflicting versions of (a) the number of persons who tested positive for Type 1 polio antibodies before it was decided to initiate the "anti-epidemic" vaccinations, and (b) the number of polio cases that occurred among both vaccinees and non-vaccinees after the vaccinations with CHAT.

The evidence strongly suggests that these vaccinations were not primarily staged in response to epidemic outbreaks of polio, as claimed, but rather because a new batch or batches of CHAT pools 8 and 9 vaccine had been prepared, and needed to be field-tested. In reality, what may have been a serious polio outbreak (that at Bambesa) was ignored for a month, while the vaccine was instead transported to two large military camps, where the majority of the vaccinations were carried out. (Over the years, military camps have frequently been viewed as good testing-grounds for new biomedical materials.) When the vaccine finally did arrive in Bambesa, there was not enough to go around, and only a part of the village was vaccinated.

Koprowski, however, reported that in Bambesa as elsewhere, "every inhabitant received the vaccine". He also reported that "after vaccination, no more cases of paralysis were reported in the four localities involved in the outbreaks". In fact, the hygiene inspector's letter reveals that more than 17 cases were reported in these locales in the following months, including at least one in each of the four villages.

Bervoets reveals that 1,500 further people were vaccinated in the mining township of Kilo at the same time as the Watsa vaccination – and that Kilo experienced twenty further polio cases between April and July. (None of this is mentioned by Koprowski.) The likeliest explanation is that the vaccine was reverting to virulence, which seems to have been a regular problem with Koprowski vaccines over the years.<sup>416</sup>

It is, I believe, inconceivable that neither Koprowski nor Courtois were informed about any of the 17 post-vaccination polio cases before they wrote the reports which announced to the world that in the first large-scale field-trials of oral polio vaccine anywhere on the planet, CHAT vaccine had been an unqualified success.<sup>417</sup> [For further details, see Figure 7]

**Figure 7: Different accounts of the “vaccinations in response to epidemics” in Province Oriental of the Belgian Congo, January – February, 1958, and of the post-vaccination polio cases seen in the same towns and villages.**

Town or village where polio outbreak occurred	Numbers of suspected polio cases pre-vaccination (B)	Dates of epidemic outbreak	Dates when CHAT vaccine given	Numbers vaccinated with CHAT (Koprowski) (D)	Polio cases more than 4 days after vaccination (Bervoets)	Polio cases more than 4 days after vaccination (Koprowski) (G)
BANALIA	8	29/11/57 to 4/1/58	8/1/58 to 12/1/58	4,182	1	0
GOMBARI (A)	12	Late January 1958	27/1/58	3,482	1 (plus more from March onwards)	0
WATSA (A)	2	Not recorded	29/1/58 to 31/1/58	12,789	8 (F)	0
BAMBESA	7	Early January 1958	1/2/58 (C)	2,433 (E)	7	0

NOTES:

- (A) Both Watsa and Gombari boasted large military camps.
- (B) According to Koprowski, just 3 of the 29 persons suspected of having polio in the four towns and villages were confirmed serologically as having Type 1 poliovirus – all in Banalia. According to Courtois, only one case was confirmed as Type 1 in Banalia, and one other in Gombari. Bervoets, however, reports that eight cases were confirmed as Type 1 in Banalia, and six in Bambesa. Nobody reports any confirmed Type 1 cases in Watsa, the town where the bulk of the vaccinations occurred.
- (C) The four week gap between the report of the epidemic in Bambesa (which was the most significant in terms of cases per head of population) and the (partial) vaccination of that village with CHAT has not been explained.
- (D) These are the numbers vaccinated according to both Koprowski and Courtois. There are some small discrepancies with the vaccination figures provided by Bervoets for the last three towns. He records 2,925 vaccinees in Gombari; 13,069 in Watsa, and 2,350 in Bambesa.
- (E) According to Bervoets, 2,350 persons were vaccinated in Bambesa, and this was “only a part of the population”. The fact that there were subsequently seven cases of polio in Bambesa may well be related to the fact that part of the village was not vaccinated, and suggests that the vaccine may

have reverted to virulence. Koprowski, by contrast, says that “every inhabitant” of Bambesa was vaccinated, and reports no post-vaccination polio cases.

- (F) Bervoets mentions that in addition to the 13,069 vaccinees at Watsa, 1,500 people were vaccinated at the same time in the nearby mining town of Kilo, and that Kilo subsequently had 20 polio cases between April and July. The 1,500 vaccinees are believed to represent about a quarter of the population of Kilo, for subsequent arrangements were made for 5,000 more doses of CHAT to be given here. With regard to the original vaccination there appears, as in Bambesa, to have been reversion to virulence.
- (G) It is noticeable that details which conflict with Koprowski’s argument that CHAT vaccine is safe and effective do not appear in his *BMJ* paper. (Although Koprowski is officially recorded only as last author on this *BMJ* paper, I was told by at least two of the Belgian doctors from Stanleyville that Koprowski wrote it, and sent it to Courtois only for checking.) Koprowski claims that “after vaccination no more cases of paralysis were reported in the four localities involved in the outbreaks”, yet Bervoets makes it clear that polio cases occurred in each one of the four outbreak villages and towns, beginning five days after the vaccinations. (In his paper, published a few weeks after that of Koprowski, Courtois says that there were six post-vaccination polio cases, but does not say where.) Koprowski’s article was published on July 26, 1958, and Courtois’ paper appeared in August, so only in Kilo (which neither Koprowski nor Courtois mention in any case), could a persuasive case be made that the post-vaccination cases might have occurred after the papers had been written. Other potentially awkward details, such as the partial vaccinations at Kilo and Bambesa (both of which were followed by polio outbreaks) also go unreported by Koprowski and Courtois. Interestingly, in Koprowski’s paper there is no mention of the vaccinations at Stanleyville military camp, or along the eastern shore of Lake Tanganyika.

Sources: “Koprowski”: Article by G. Courtois, H. Koprowski et al.; *Brit. Med. J.*; 1958; (ii); 187-190.

“Courtois”: Article by G. Courtois; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 805-816.

“Bervoets”: Official government letter from W. Bervoets to P. De Brauwere, 17/9/58.

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What the CHAT vaccine researchers seem to have been concerned with are impressive statements which could be broadcast publicly, and enshrined in the medical literature, rather than with the reality of what was happening on the ground.<sup>418</sup>

And 44 years later, in an uncanny echo of these events, we have the publicly-broadcast statements that the OPV theory has been destroyed, when the reality is so very different.

This is not Science. It is phantom science.

I know that some who have followed the origins debate closely suspect that the main reason why there has been such a premature rush to “bury” the OPV theory is that certain scientists may be trying to buy time, so that they can continue with the search for some real (or perhaps not so real) evidence that would genuinely damage the theory – such as a sample of HIV-positive blood from before the start of the OPV trials.

If scientists are (even belatedly) staging an honest search for such materials, then that is commendable. However, there are indications that at least some of these searches for ancient HIV may not be entirely above board. Simon Wain-Hobson (when he still believed in the merits of the OPV theory) told me that late in 1999, a few months after *The River* was published, he spoke with a CDC scientist at a meeting in the U.S. This scientist claimed that some of his colleagues were working with a sample of HIV from 1952, from which they had already obtained a sequence. Simon asked him where the sequence sat in the phylogenetic tree. The scientist held out the fingers of

one hand, to represent the Group M star-burst, and pointed to near the end of one of the fingers. Simon swiftly pointed out that if the 1952 virus was positioned near the end of a branch, whichever branch, it must surely be a contamination with a modern virus. The other scientist quickly changed the subject.<sup>419</sup> Interestingly, nothing more has since been heard of this “ancient HIV sample”.

And it seems that something similar may be going on even today. Repeated attempts (some of them clandestine) have been made by scientists representing KUL (the Catholic University of Leuven),<sup>420</sup> among other institutions, to obtain a set of pathology slides which was recently located in the basement of the former medical laboratory in Stanleyville. These apparently include materials obtained between 1955 and 1958. Because this may represent the period both before and after the beginning of the OPV trials, I believe it is of paramount importance that these slides, and the accompanying data, should be investigated by “neutral” institutions, rather than by one that was itself directly involved in the original research programme.<sup>421</sup>

Because a *genuine* sample of HIV-1 Group M from before the time of the OPV trials would constitute a powerful piece of evidence against the OPV hypothesis, it is all the more important that the provenance of any such sample (if it exists) should be above suspicion.

#### k) The emperor’s new clothes.

Everyone knows the fairy story by Hans Christian Andersen. The emperor parades through town, showing off his new clothes. The courtiers and townspeople gasp and cheer, and applaud the finery. But then one day a young boy stands up at the back and says the unsayable....that the emperor’s clothes aren’t clothes at all. That he is naked. And the people stop bowing, and look up, and see that it is so.

I believe that, in the course of this debate, certain members of the scientific community have been acting with imperial and empirical disdain for the most basic tenets and ethics of science. And I think it’s time that someone stood up at the back, and pointed out just what the emperor is wearing today.

Until a moment ago, it seemed that perhaps I was going to have to be the little boy. But fortunately someone else has just stood up over there, behind the pages. I’m not sure who he is, but he’s rather small, with a beard and glasses, and he appears to be getting quite red in the face.

In fact, he really does seem quite upset....inflamed, even. By the looks of things, he could use a bit of dephlogistication.<sup>422</sup>

Now he’s shouting something. What’s that he’s saying? “*Desolé, mais il faut le dire.*” “I’m sorry, but this has got to be said.” Good grief, I think that must be Monsieur Émile Zola. I thought he’d died years ago. Anyway, by the look of things, he’s about to read out a series of accusations. Let’s listen.

- *J’accuse.*

- I accuse various scientists of having participated in research which resulted in a disastrous error of judgement perpetrated in Africa by an international scientific team nearly half a century ago.
- I accuse Dr Paul Osterrieth of having participated in a large-scale chimpanzee sacrifice programme in the Congo in the 1950s, in order to gather both organs and blood for purposes that are still largely unknown. I further accuse him of having prepared polio vaccine in his lab in Stanleyville in the 1950s, in a primitive Maitland-type tissue culture based on chimpanzee cells and chimp sera – and of having given incomplete and misleading answers about this episode over a period of several years.<sup>423</sup>
- I accuse Dr Hilary Koprowski of having instigated the programme under which Dr Osterrieth prepared the polio vaccine, and under which various scientists (including Koprowski himself, Ghislain Courtois, Gaston Ninane, Paul Osterrieth, Agnes Flack and George Jervis) conducted and oversaw the human field-trials of that vaccine in the Belgian Congo and Ruanda-Urundi. I also accuse him of having instigated the research programme at Lindi camp, and of consistently failing to provide meaningful and adequate answers about the work that was conducted there. I believe that Dr Koprowski was well aware of the true nature of the chimpanzee-related work that was going on in Stanleyville and at Lindi camp, from first to last.
- I accuse Dr Stanley Plotkin of having been involved with the Stanleyville/Lindi research programme, and of having coordinated an attempted cover-up in response to the hypothesis proposed in *The River*. I also accuse him of saying nasty and untrue things about my friend, Monsieur Hooper.
- I accuse doctors Beatrice Hahn, Paul Sharp, Bette Korber and Kevin De Cock, among others, of having demonstrated a bunker mentality in promoting a version of events which is increasingly far-fetched. It is apparent that neither their epidemiological scenario nor their phylogenetic dating argument stands up to close scrutiny, yet they continue to present both as if they were proven facts, not hypotheses. The evidence about the focal role that recombination has played in HIV-1 Group M makes it increasingly clear (at least to me and Madame Zola) that there is no reliable phylogenetic basis for making estimates about when the Group M epidemic began. We feel that the fact that such estimates are made at all is prompted (knowingly or unknowingly) by the perceived necessity of “disproving” the OPV hypothesis.
- I accuse Dr Simon Wain-Hobson of having failed to honour his agreement with Bill Hamilton, and having failed to properly investigate and report on the chimp samples which Bill and his colleagues brought back from the Congo. There is little enough primary data available on this issue, and many people consider that Professor Hamilton effectively sacrificed his life in order to procure these materials. The least he deserves from his former collaborator is a full and properly detailed scientific investigation and report. Furthermore, I accuse Dr Wain-Hobson of having done a *volte-face* on the origins issue, a flouncy flip-flop that has been prompted primarily by pragmatism.
- I accuse Dr Robin Weiss of having presided over a *Weisswash* in the origins of AIDS debate. I accuse him of having used his power (and he is indeed very powerful and influential within the world of science) to spin the arguments against the OPV theory, and to attempt to persuade both scientific and lay observers that the theory has been disproved. I further accuse him of having told a blatant untruth by stating that the OPV theory has been “destroyed”. He has failed to

provide any supporting evidence for that assertion, and yet he has also failed to withdraw it. In reality, Professor Weiss has based his arguments on nothing more or less than his own “plain, personal prejudice” – to quote his own (unpublished) words from the Royal Society meeting. I am informed that Professor Weiss has recently written to a journalist that “it strikes me that HIV researchers are to a man and a woman weary of [Hooper’s] mutating hypothesis”, an interesting comment, in that it suggests he thinks that hypotheses should not adapt to the arrival of new evidence. It also suggests that he and his colleagues are now too bored to think further about whether the world’s most disastrous human epidemic may have been started by Man himself. However, the word on the *vigne* is that, despite Dr Weiss’ protestations of *ennui*, he himself is now either editing, or organising, a new book of invited essays about “how AIDS began”. If the rumours are correct, then we hope that the book will be more balanced than Dr Weiss’ previous contributions on the subject. But we are not holding our breath.

- I also accuse my friend Mr Edward Hooper of having failed to investigate the provenance of the L70 sample (which produced the ZR59 sequence of HIV-1) with sufficient care in 1997, when he was submitting information about that sample to be published as part of an article in *Nature*. This only goes to show that we can all make errors of judgement, and this suggests also that Mr Hooper is sometimes *un petit peu* too willing to ascribe evil intent to others when they too make mistakes.
- I accuse the little boy of going home early without permission. He should be kept in for a week, and lose pocket money.
- And I accuse the emperor of being naked. Look up quickly, people of the town, if you want to see.

Gosh, that Monsieur Zola – once he starts accusing, he does get carried away, doesn’t he?

#### 6. Conclusion: the importance of the level playing-field.

The new evidence about the local production of CHAT in Stanleyville, and the strong probability that it was prepared in chimpanzee cells and sera, are vitally important.

*This new information does not, however, prove that CHAT vaccine started the AIDS pandemic.*

On the other hand, the scale of the attempted cover-up (both at the time, and in the last few years) suggests that others besides myself suspect that it may have done so.

Some things which have happened in the course of this controversy are deeply regrettable, and have reflected no credit on those responsible. Having said that, I do realise that many of those who have been so determined to prove the OPV theory wrong may have acted in good faith. When one sincerely believes that this simply has to be just another conspiracy theory, and that the African CHAT campaigns could not possibly have been the source of AIDS, then one may also come to believe that almost any action one might take is permissible provided it has the right outcome, and persuades others also that the theory is misguided.

It is hoped that this paper will persuade at least some of those who were previously convinced of the inherent wrongness of the OPV hypothesis that it might be moot to reconsider.

Because of the role he has played in this controversy, I am not alone in regretting that Professor Robin Weiss was invited to deliver the closing summary at this conference. In all other ways, the events here at Lincei have demonstrated an even-handedness that was sadly lacking from sections of the Royal Society meeting.

I would like to thank the Lincei academy for inviting me to speak (and write) in the place of the late, lamented Bill Hamilton (as if any could take his place!). I have not written the paper that Bill would have written, but I hope that what I have contributed, though sometimes blunt, has done honour to his legacy. The decision about how to write this paper has not been made lightly.

By honouring their promise to Bill, and by allowing this conference to go ahead on a level playing-field, the *Accademia Nazionale dei Lincei* has done a great service both to Science, and to ethical conduct in Science.

And by their conduct during and since the conference, the officers and members of the academy have acknowledged that truth does not automatically, or exclusively, come from the pens and the mouths of the “experts”. I thank them for that.

Let me close by adapting Benjamin Disraeli’s question to this dreadful and unprecedented epidemic.

Was AIDS caused by an ape, by an angel (if so, surely a dark one) – or by Man himself, in all his bungling and clumsy ambition and human frailty?

We still don’t know the answer. But my vote would go to the last of the three.

EH July 2, 2002

- *I would like to thank more than a dozen scientists from different fields, and a number of non-scientists who are interested in the debate, for their help with, and input to, this paper. Because this remains such a highly controversial subject, and because I do not wish to cause anyone any embarrassment, I shall refrain from citing them by name, at least for now.*
- *I would also like to thank my partner, Ms P. Griffin, for providing love, ongoing support facilities – and the hand-drawn maps.*
- *As I was completing this manuscript, I was informed of the sad death, on June 7, 2002, of Dr Kamil Kucera, the brilliant Czech parasitologist. Between 1991 and 1993, Professor Kucera prepared several hundred pages of detailed hand-written notes and tables about his researches into *Pneumocystis carinii*, which causes PCP, the pneumonia that is the classic opportunistic infection of “Euro-American AIDS”. He wrote these notes in meticulous copperplate on large sheets of ancient air mail paper, and he sent them to me in batches, inside home-made envelopes, stuck down with glue.*

- *Like Bill Hamilton, Kamil Kucera was a man whose scientific commitment and integrity shone through his work, and through his whole life, and I would like to dedicate this paper to both of these admirable men.*

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## 7. Afterword: the Culture of Secrecy.

Just as this paper was about to be submitted, I was informed that on November 20<sup>th</sup>, 2001, Dr Paul Osterrieth had posted on the Web an official response to the speech which I delivered at Lincei on September 28<sup>th</sup>, 2001 (which contained the bare bones of the evidence presented in the current paper).

Entitled “The truth is what happened and not what one wishes that had happened”, Osterrieth’s four page disclaimer initially comes across as an impressive riposte – one that is calm, measured, generally dignified, and seemingly authoritative.<sup>424</sup>

However, his belated decision to provide some additional details about his work in Stanleyville actually raises more questions than it answers. Firstly, much of the key information he provides is demonstrably inaccurate, or else contradicts material in his past statements. Secondly, Osterrieth’s latest declaration only highlights his unwillingness to make any clear statement about the most important issues – such as what he and his fellow scientists were doing with the Lindi chimps, and what happened to the CHAT polio vaccine once it had arrived at the Stanleyville medical lab.

Several of Dr Osterrieth’s claims have already been covered in the main text, but I shall examine his new claims point by point below:

- Osterrieth now states that he returned to Stanleyville on February 28<sup>th</sup>, 1958, and he sources this detail to the annual report of the LMS. What that report actually records is that he returned to work at the lab on February 23<sup>rd</sup>, but even that detail is inaccurate. We know this because in a tape-recorded interview in 1993, Dr Osterrieth told me that he arrived back in Stanleyville “a very short time” after Dr Deinhardt’s arrival (which is independently documented as having been on February 1<sup>st</sup>, 1958). His wife added that their arrival in Stan was “just a few days” after Deinhardt’s, and I therefore believe that his return must have been on or around February 4<sup>th</sup>. Support for this is provided by Osterrieth’s “first assistant”, who began work on February 12<sup>th</sup>, 1958 (I have seen his official stamped work records, which confirm this), and who recalls that Osterrieth was already making polio vaccine when he, the assistant, started work. Dr Osterrieth’s sudden insistence that he was not present in the lab until February 28<sup>th</sup>, 1958, may not be unrelated to the fact that the vaccination at Stanleyville military camp took place on February 27<sup>th</sup>.
- Osterrieth states that he “did not carry out any autopsy [on a chimpanzee], since that was the job of Dr Ninane the pathologist”. He adds that “when an autopsy was carried out, limited pieces of different organs were taken to carry out various analys[es]”. These statements are directly contradicted by the recollections of two witnesses from Lindi camp: Joseph the camp nurse (who carried out most of the

sacrifices and gross dissections, and who was present at most of the autopsies), and “Antoine”, who watched some of them “from the wings”. Both say that it was Dr Osterrieth who conducted most of the autopsies on the chimps, and who regularly used to remove entire organs and put them into metal canisters. On the basis of multiple pieces of evidence, it appears probable that Ninane conducted the autopsies on dead or diseased chimps, but that Osterrieth dissected, and removed organs from, those which were sacrificed for other reasons.

- Osterrieth states “I was not the one who took blood from the chimpanzees”. This is directly contradicted by the testimony of his first lab assistant, who recalls that he and Dr Osterrieth regularly visited Lindi camp on Saturdays in order to obtain bloods from the chimpanzees, which Osterrieth later spun down into serum in the sterile (tissue culture) lab in the virology department. The assistant says that this sometimes happened at weekly intervals, and sometimes about once a month. Furthermore, although the assistant does not know the details of how the polio vaccine was made, he associates the taking of chimp blood with the making of the vaccine.
- Osterrieth asserts: “clearly my primary task was neither to prepare cell culture nor to prepare vaccine” at the Stanleyville lab. Yet in my first interview with him in 1993, he told me the exact opposite with regard to making tissue culture. At the beginning of the interview, he explained that his work in the virus lab “was essentially to develop tissue cultures, to expand the virus work”. When I asked him what sort of cultures, he answered “certainly monkey kidney”, but was unable to remember the species, apart from assuring me that it was not the chimpanzee. (Only later did he decide it must have been the baboon.) Later, he stated that his four month visit to the US in October 1957 to January 1958 had been mainly to get training in cell culture and virology, and that he had spent two weeks [or a month, according to his latest statement] at the Wistar Institute, where he “was working only in the lab of tissue culture, and looking how to do things”. At another point in the 1993 interview, Osterrieth said that Koprowski had demanded that he come to the Wistar “because of the work on the polio” which, Osterrieth explained, related to the two vaccine strains, CHAT and Fox.
- Osterrieth states that “no Maitland type cultures of any animal were produced in the lab, and no vaccine was produced”. The first part of this statement is contradicted by the testimony of Courtois’ assistant, who says that chimpanzee tissue cultures were being produced in the microbiology lab where Osterrieth worked before he took over the virology lab. Furthermore, an AFEB report documents that the materials sent to the US in early 1958 were minced chimpanzee kidneys in isologous (chimpanzee) serum and Hanks’ solution, which effectively constitute Maitland-type cultures. Osterrieth’s second claim, that no vaccine was produced, is contradicted by both of the assistants in his virology lab. Indeed, the first assistant states that Osterrieth was already making polio vaccines before he (the assistant) started working in the lab in February 1958, and that he continued to make polio vaccines, on request from the provincial government, during the next two years.
- Osterrieth states that: “the allegation that chimpanzee serum was prepared to be used in tissue culture medium for chimpanzee cells in culture is sheer nonsense, since at that time one used calf serum or sometimes foetal calf serum to enrich the culture medium, since this type of serum was available in sufficient amounts.” Despite Dr Osterrieth’s protestations, he is incorrect. He ignores the fact that foetal calf serum was expensive to purchase, and that local production of calf

serum was unlikely, given that cows did not survive well in the rain forest environment of Stanleyville; (certainly in 1999, there were no cows living in or around Kisangani). By contrast, chimp serum was freely available, and it was a good idea to use precisely because it was isologous, and came from the same species as the kidneys. And crucially, we know from the AFEB report that in fact chimp serum *was used* to nourish the chimp kidney cells.

- Osterrieth states: “we could not even check reliably the titre of [polio] vaccine lots”. This is interesting on two counts. Firstly, that they proceeded with local vaccinations despite being unsure of the titre of the vaccine. And secondly, that Dr Osterrieth uses the collective “we” with respect to attempts to titrate the vaccine, which is the first time he has ever directly acknowledged that he himself was one of those who handled the vaccine in the lab. Despite his alleged desire to provide clarification, and despite having made seven oral or written statements about these matters over a nine year period, Dr Osterrieth has never revealed any significant details about the polio vaccine, or what was done with it.
- Osterrieth states that the chimps “were kept alone in individual cages and not together in a common one, with the possible exception of infants that could be handled easily.” This is incorrect. According to several contemporary accounts (including a review article prepared by his boss, Ghislain Courtois, in 1966), the chimps were often kept two to a cage, common chimp and bonobo together. Courtois also writes of a group-cage, where up to ten chimps could play at a time.
- Osterrieth writes: “Mr Hooper states that in the year 2000 I wrote to natives in Stanleyville to ask them to say nothing about what was carried out in my lab in the years of vaccination. This is simply not true. I never wrote such a letter, and if such a document exists Mr Hooper should produce it for examination.” Dr Osterrieth’s claim is interesting, because what I actually wrote in a footnote to my Lincei speech was that in late 2000, one of his ex-assistants “received a letter from Osterrieth, this being the first time he had heard from him for some forty years. What his old boss wanted to know was which of his former assistants at the virus lab and Lindi camp were still alive, and which were dead”. In any case, Osterrieth’s demand that I should produce the letter is inappropriate: since the letter was not written to me, I clearly do not have it to produce. What is available, however, is clear evidence to refute Dr Osterrieth’s denial, which will be produced in good time. Meanwhile, one is left to wonder why he should suddenly have been seeking such information.
- Osterreith states that Dr Ninane’s family denies that he had either Parkinson’s disease or Alzheimer’s disease, and he remonstrates with me for making claims about Dr Ninane’s health in order “to cast doubt on the value of his testimony”. This is both misleading and untrue. When interviewed in August 2000, two weeks after her brother’s death, Gaston Ninane’s sister (with whom he had lived for the last ten or more years of his life) told me that he had suffered from Parkinson’s disease, which led to his falls, and to his being hospitalised; she added that doctors Koprowski and Prinzie interviewed him after his first fall, while he was in hospital. I believe that Alzheimer’s was also mentioned during that conversation (though I can find no mention of it on the tape), but in any case Dr Ninane had often told me that he feared he was suffering the first symptoms of this condition. The key point, however, is that (whatever his state of health) the statement which Dr Ninane apparently signed in February 2000, stating that claims that he had tried to make tissue culture, and chimpanzee tissue culture, in Stanleyville were “lies”, is itself untrue. I have the tape recordings of our interviews, which

demonstrate that Dr Ninane was correctly quoted, and that he did indeed say these things.

- Finally, Osterrieth states: “It remains astonishing that people who recognise that they had no access to these facilities [the sterile room in his virology lab] would know precisely what kind of work I did carry [out] in this lab, what type of tissue culture was performed, what kind of virus was inoculated, what kind of serum was used”. This is a strange comment, for Osterrieth’s assistant did not make any claims to knowing these precise details. What he did know, however, was that Osterrieth was making a polio vaccine which he (the assistant) then fed to the people living at the local military camp. And fortunately, another African technician who had worked in another department of the Stanleyville lab since 1956 knew more precise details, including the key fact that chimpanzee tissue culture was being made in different departments at the Stanleyville laboratory.
- I believe that, taken as a whole, Dr Osterrieth’s comments are highly revealing of the culture of secrecy within which he worked – and with which he worked.

The title of Dr Osterrieth’s disclaimer, “The truth is what happened and not what one wishes that had happened”, would seem to be appropriate, though perhaps not in the way intended.<sup>425</sup>

In comments which he made to a journalist in October 2001, Robin Weiss said: “Either Osterrieth is lying through his teeth or [Hooper] has got it wrong”. I would put it a little differently. I have made clear statements about what can and cannot be proved, and I have the evidence to support those statements. By contrast, I believe that Dr Osterrieth’s often contradictory statements on this matter are not supported by evidence, and cannot be relied upon.

#### 8. POSTSCRIPT: January 1, 2003.

Since this paper went to press, there have been a number of significant developments with regard to the OPV hypothesis of the origin of AIDS.

In July 2002, a brief communication by Andreas Meyerhans and colleagues, published in *Nature*, revealed unprecedented evidence of rampant recombination occurring within the individual HIV-infected cells of AIDS patients.<sup>426</sup> It was left to Jon Cohen, writing in *Science* about what was referred to as a “beautiful study” by the Meyerhans group, to sum up its significance. Cohen commented that their work “raises serious questions about phylogeny trees that attempt to date the origin of HIV, all of which intentionally discard suspected recombinants to make the data interpretable”.<sup>427</sup>

So the cat is finally out of the bag. At long last, a consensus is emerging that phylogenetic dating is quite simply an inappropriate technique to apply to a virus as mutable and capable of recombination as HIV. The main scientific “disproof” of the OPV theory is revealed as an illusion.

Meanwhile, additional interviews conducted by this author in Europe have confirmed and substantially enlarged the central revelations of the present paper, concerning the events that took place in the 1950s at Lindi camp, and at what even Hilary Koprowski referred to, in 2001, as “the vaccine laboratory” in Stanleyville. It is neither practical

nor appropriate to try to incorporate this new information into the present paper, but details have been deposited with various parties, and the full story will be reported in due course, both in the scientific literature and elsewhere.

The questions that now have to be asked are these...

- For how long can mainstream Science continue to insist that CHAT was not made in chimp cells?
- How long will it be before independent laboratories conduct the tests that now clearly need to be conducted, to see if this vaccine was linked to the genesis of AIDS? (For the record, virological testing of chimpanzee and bonobo populations in the DRC is finally underway. An appropriate next step, I believe, would be an epidemiological and virological survey of CHAT vaccinees and non-vaccinees in the DRC and Burundi.)
- What does this story (including the attempted cover-up) have to tell us about the way that Science is conducted in the modern world?
- And to what extent will a proper understanding of how AIDS began advance the search for a vaccine or therapy?

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<sup>1</sup> Hooper, E., *The River: A Journey Back to the Source of HIV and AIDS*. (London: Allen Lane – Penguin Press; 2000). This edition was completed in March 2000. For a version that is updated to October 2000, with additional material about the Royal Society conference on pages 874-877, see: Hooper, E., *The River: A Journey to the Source of HIV and AIDS*. (Boston: Little, Brown; 2000).

<sup>2</sup> For those who are interested, an article about Dr John P. Moore's role in this debate (and some of the things that he has written about it) will be appearing in due course on Brian Martin's web-site.

<sup>3</sup> UNAIDS epidemic update – December 2001.

<sup>4</sup> Anon., "New UNAIDS report warns AIDS epidemic still in early phase and not levelling off in worst affected countries"; UNAIDS press release; July 2, 2002.

<sup>5</sup> Huet T., Wain-Hobson S., "Genetic Organization of a Chimpanzee Lentivirus Related to HIV-1"; *Nature*; 1990; 345; 356-359.

<sup>6</sup> Notably Beatrice Hahn, Paul Sharp and Bette Korber (but also enthusiastically propounded by others, such as Stanley Plotkin, Hilary Koprowski and Paul Osterrieth).

<sup>7</sup> For the cut hunter school, the recency of the epidemic is explained by new developments in the mid twentieth century, such as urbanisation and more liberal sexual mores, or else the emergence (and ill-advised reuse) of disposable needles, allowing the chimpanzee virus, once transferred, the new opportunity of being passed parenterally from human to human. At its most basic, the natural transfer theory of a casual chimp-to-human zoonosis seems quite reasonable, even if it is impossible to prove or disprove. On the other hand, the theory also has certain innate logistical problems, some of which are discussed later in this paper.

<sup>8</sup> In several instances in this paper, I have updated the epidemiological data published in *The River*. In particular, I have omitted one of the 28 CHAT trials that feature in the book (trial #22, of 64,000 persons at Lubudi, or Kabare-Lubudi), because I agree with Stanley Plotkin that there is no concrete evidence that it took place. Dr Plotkin, however, should not get carried away, for despite his efforts, I believe this is the only error (or potential error) of any significance that he has managed to identify in the book. In my own defence, I should point out that "Kabare-Lubudi" was apparently proposed as a forthcoming trial at a 1959 press conference at which Plotkin himself was one of the three major speakers. Furthermore, accepting that the trial is not proved to have occurred is not the same as accepting that it did not occur. (Kabare and Lubudi *territoires* are indeed hundreds of kilometres apart, as Plotkin observes, but they were also, in 1959, the sites of two of the Belgian Congo's three major cement factories. I still suspect that the trial may have taken place, perhaps at the behest of the chief medic of the cement company.) But for now, I agree that without proof, this trial should be omitted from the list.

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<sup>9</sup> *River*, 2000, pp 387-389.

<sup>10</sup> *River*, 2000, p. 385. Dr Koprowski is in many ways a ground-breaking scientist, one not afraid to step where others fear to tread, but there are also several examples in *The River* of occasions when he appears to have displayed recklessness. One such was his involvement in the staging of a field trial of an early genetically modified rabies vaccine in Argentina in the mid-eighties, without the permission or approval of the Argentine government. The London *Times*, in a leader, described this as a “reckless and illegal release”. [*River*, 2000, p. 449]

<sup>11</sup> Above this legend on the sign outside Lindi camp was the single word “Polio”, which presumably helped to deter the curious. To this day, the camp is referred to by local villagers as “Camp Polio”.

<sup>12</sup> Plotkin S.A. “Untruths and Consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus”. *Phil. Trans. R. Soc. Lond. B*, 2001; 356, 815-823. “The purpose of the camp was not at all mysterious”, writes Plotkin (p. 819), but his non-mysterious account substantially conflicts with the testimony of the Lindi workers, as reported in the present article, and it omits all details pertaining to sacrifice and removal of organs. This is despite the fact that both African and Western witnesses report that all the Lindi chimps (save for those which died natural deaths, and around 60 which were removed by lorry when the camp closed around the end of 1959) were sacrificed for experiments. [For instance, see *River*, 2000, page 725.]

<sup>13</sup> With the sole exception of a retrospective article published in 1967 by the former director of the *Laboratoire Médicale de Stanleyville*, Ghislain Courtois, there are only a few sentences published about the work carried out at Lindi camp, and these include next-to-no information about the polio work, and nothing at all about the sacrifice programme. [G. Courtois, “Sur la réalisation d’une sagerie de chimpanzés au Congo”; *Symposium international sur l’avenir des animaux de laboratoire*, Lyon, September 18-20, 1966 (Lyon, Institut Merieux, 1967, pp. 235-244).] The annual reports of the *Laboratoire Médical de Stanleyville* reveal that there was vaccination, vaccination and challenge, and intraspinal safety testing of the polio vaccines at Lindi, but provide no further details during the first three years (1956-1958), which were the period of the so-called “polio research”. The lack of information about the research conducted at Lindi is very striking, and even some of the Belgians who used to work in the Stanleyville lab commented upon it. A promised 1958 article about the polio research at Lindi, which was thrice referred to as “in preparation”, or “being edited”, was never released (at least to the public). During the period the camp was open, visitors to Lindi were discouraged, and apart from the Belgian and American doctors and their African assistants, nobody was allowed inside the second hangar, which housed the chimps that were scheduled for experimentation. Despite this secrecy, certain visitors managed to discover interesting details. For instance, early in 1960 a visiting Dutch primatologist, Adriaan Kortlandt, was told that 86 pygmy chimps had died in 3 weeks in the course of the polio research, and that he should not ask too many questions on the subject, because otherwise he “might cut [his] fingers”. It appears that all biomedical samples pertaining to this research have now been lost or discarded. Furthermore, all Koprowski’s records about Lindi camp, and the research on his vaccines conducted there, have apparently been “lost in a move”, while the relevant archives in the polio correspondence file in the Belgian Ministry of Foreign Affairs archives section for the key period (November 1956 to June 1958) are missing. See *River*, pp 708-723.

<sup>14</sup> The round figure of 400 chimps (in the first 20 months, the time of polio research) has been provided by many sources, and is now no longer in question. The figure of “over 600” for the entire three-and-a-half-year period that Lindi camp was open has been given by two of the Africans who worked there, and seems consistent with other available evidence.

<sup>15</sup> A. Kortlandt, “Statements on pygmy chimpanzees” [letter]; *Lab. Primate News.*; 1976; 15(1); 15.

<sup>16</sup> T.O. Jonassen et al., “Sequence analysis of HIV-1 Group O from Norwegian patients infected in the 1960s”; *Virology*; 1997; 231; 43-47. Also see *The River*, 2000, pp. 772-774, for the story of the Norwegian merchant seaman who was probably infected with HIV-1 Group O in Cameroon in 1961-2, who developed first symptoms of AIDS in 1966, and who died in 1976.

<sup>17</sup> Those very few European and American cases from before 1978 all appear to have been infected in Africa – and, more specifically, in the DRC, the former Belgian Congo. There are two possible exceptions, only one of which is confirmed by serology – this being a girl born in New Jersey in 1973 or 1974, who was clearly immunocompromised soon after birth, and who died of AIDS in 1979. The girl’s mother was a drug-injecting 16-year-old with multiple partners, who must have been born between late 1956 and 1958. The mother had thrombocytopenia at the time of the girl’s birth, suggesting that she might have passed HIV infection vertically to her daughter. In *The River*, I proposed that the mother might have been one of those infants who was born at Clinton prison, the major long-stay penitentiary for female prisoners in New Jersey, where almost every baby born

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between late 1956 and 1958 was vaccinated with one or more of Koprowski's OPVs, most of which were experimental. In his speech to the Royal Society, Dr Plotkin rejected this hypothesis, explaining that he had approached the pediatrician who had tended to the young girl, and discovered that the mother's name did not match that of any of the Clinton infants from the 50s. That appeared to have settled the matter. However, in June 2001 I was contacted by a man who knew that he had been born at Clinton prison, and suspected that he might have been one of the OPV vaccinees. From certain data he provided, I was able to confirm that he had indeed been one of the vaccinees. However, this man had been adopted at the age of two, and had been given the family name of his adopted parents. At the time he approached me, he did not know his original family name. Other vaccinated Clinton infants may similarly have had their names changed following adoption, and may not even know this fact. For this reason, Plotkin's "refutation" of a link with CHAT in this instance may not be a refutation at all.

<sup>18</sup> These comprise the 16 cases listed on page 746 of *The River*, less case 2 (which was caused by HIV-1 Group O), but including an additional 1976 case from Burundi, which would be entered into the table on page 746 as: "1976; Congolese; 40; M; Bujumbura; 'Slim', chronic diarrhoea, generalised KS, dry cough; 0 (same site)".

<sup>19</sup> Later in this article, two other potential AIDS cases from the 1960s are detailed, dating from 1964 and 1968-70. Both come from Leopoldville/Kinshasa, DRC, though the first apparently originated from "outside Leopoldville". Both cases were volunteered by doctors with considerable clinical experience of Africa. Other less specific potential AIDS cases from 1958 onwards are also detailed later in this paper. Because of various considerations, these two additional cases are not included in the maps and AIDS data in this paper.

<sup>20</sup> These comprise the 22 cases listed as numbers 17-38 on pages 746-7 of *The River*, plus one additional 1980 case from Burundi, which would be entered into the table on page 747 as: "1980; Burundian; 42; M; Muramvya; chronic diarrhoea, interstitial pneumonia, atypical KS; 0 (same site)".

<sup>21</sup> For the old list, see: *River*, 2000; pages 746-747. As for the new list, all 39 of these early African cases had been documented retrospectively as likely cases of AIDS by the doctors involved, either in medical journals, in books or dissertations, or in unpublished articles. For 9 of the 39, retrospective serology had confirmed HIV-1 infection. As explained above, the new list includes two additional Burundian cases of AIDS from 1976 and 1980, and omits the former case 2, since that relates to Group O infection. In addition, it seems that details of one AIDS case (#32) were somehow omitted from this table during the final production stages of the book. The relevant details should read: "1980; Congolese; 21; F; Congo; herpes, candidiasis".

<sup>22</sup> See *River*, 2000; pp 187-192 and 227-235, together with relevant endnotes.

<sup>23</sup> Twelve of the sixteen sites are from the DRC, Burundi and Rwanda. The remaining four sites, in Tanzania, Kenya, Congo Brazzaville and Senegal, begin to show evidence of Group M only in 1980 and 1981, when the trans-African spread of the virus first became detectable.

<sup>24</sup> Although two (later three) of the samples tested came from CHAT *pools* (10A-11 and 13) which had been used in Africa, it became clear that different *batches* of such pools of CHAT had been prepared in different laboratories, and in the kidney cells of different species. It was where and how the individual batches had been prepared which was significant.

<sup>25</sup> The word was that three major scientific journals had been competing to publish the results of the Wistar testing, and in the end *Nature* and *Science* shared the honours. See the three articles in *Nature*; 2001; 410; 1045-1048, plus one article in *Science*; 2001; 292; 743-744.

<sup>26</sup> J. Cohen, "Disputed AIDS Theory Dies Its Final Death"; *Science*; 2001; 292; 615.

<sup>27</sup> R.A. Weiss, "Polio vaccines exonerated"; *Nature*; 2001; 1035-1036.

<sup>28</sup> *River*, 2000, pp 843-846. "Antoine" was clearly indicated as a pseudonym, for the first time the name appeared, on page 843, it was in inverted commas.

<sup>29</sup> S.A. Plotkin et al, "Postscript relating to new allegations made by Edward Hooper at The Royal Society discussion meeting on 11 September 2000", p. 829. This is a good example of the way that, at least in this debate, Professor Plotkin has repeatedly set up straw men, or false issues, in order to shoot them down. By the time this updated version of the *River* postscript was being written, in January to March 2000, it was already apparent that opponents of the theory were resorting to a number of questionable tactics, which is why I decided to protect the witness by using a pseudonym.

<sup>30</sup> In this paper, to reduce the risk of further such mischievous claims, I shall place quote marks around "Antoine" every time he is mentioned.

<sup>31</sup> On my last day in Kisangani in 1999, I asked "Antoine" to see if he could locate Joseph. He managed to track down some people from Joseph's home town, who appeared to know the correct man: they stated that Joseph had died in 1964, having got "thinner and thinner" at the end of his life. It is now

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apparent that this was not the right Joseph, but I am satisfied that what “Antoine” reported to me (which I in turn included in the new postscript) was reported in good faith.

<sup>32</sup> Joseph had previous experience from working in the pathology lab at the Stanleyville hospital for Africans. The Lindi work was performed on a table in the second hangar – the one which nobody else apart from the Stanleyville doctors and the Lindi camp workers was allowed to enter. For many (though not all) of these operations, Joseph the nurse was the only African officially present, though it seems that the other camp workers sometimes watched from the wings.

<sup>33</sup> This ties in with my own figure of 416 chimps housed at Lindi during the first twenty, very hectic months after the camp opened (June 1956 – February 1958). It was, however, the first time that I had heard such a high overall total.

<sup>34</sup> W. Henle, G. Henle and F. Deinhardt, “Studies on Hepatitis”, Annual Report to the Commission on Viral Infections of the Armed Forces Epidemiology Board, March 1958 – February 1959, p. 5. [See extract later in this paper.]

<sup>35</sup> P. von Magnus, H. von Magnus et al., “Tissue cultures of trypsinised kidney cells from different monkey species”; *Dan. Med. Bull.*; 1955; 2(8); 236-240.

<sup>36</sup> In light of the dubious approaches made to some other witnesses, I am not prepared at the moment to identify this man, and certain others who feature in the text. They will be fully identified at an appropriate time in the future.

<sup>37</sup> Letter from W. Bervoets to P. De Brauwere, September 17<sup>th</sup>, 1958; file H4484/1058 at the Belgian Ministry of Foreign Affairs archives.

<sup>38</sup> This is intriguing, for the latter trips are not revealed by the annual lab reports for 1958 and 1959. (The 1960 report was never published.)

<sup>39</sup> R.A. Weiss and S. Wain-Hobson (editors), “Origins of HIV and the AIDS Epidemic”; *Phil. Trans. R. Soc. Lond. B*; 2001; 256; 771-977.

<sup>40</sup> P.M. Osterrieth, “Vaccine could not have been prepared in Stanleyville”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 839.

<sup>41</sup> S.A. Plotkin, “CHAT oral polio vaccine was not the source of human immunodeficiency virus type 1 Group M for humans”; *Clin. Inf. Dis.*, 2001, 32, 1068-84. S.A. Plotkin, “Untruths and consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus”. *Phil. Trans. R. Soc. Lond. B*, 2001; 356, 815-823. S.A. Plotkin, D.E. Teuwen, A. Prinzie, and J. Desmyter, “Postscript relating to new allegations made by Edward Hooper at the Royal Society discussion meeting on 11 September 2000”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 825-829. The last of these articles states at the end that “letters cited in this paper will be deposited” at one of two libraries, in Philadelphia and Leuven. However, I would also be interested to see the source documents that Plotkin quotes in “Untruths and consequences”, especially the passage chart, and the March 4, 1958 letter from Koprowski to Jarvis, which refers to Ninane’s telegram. Requests to Dr Koprowski to view the latter document have thus far been ignored.

<sup>42</sup> For what it is worth, I had neither heard Osterrieth’s presentation at the Royal Society, nor did I know of its content, before I left for Africa at the start of March 2001. It was only when I returned to England, that I was reminded of the statements by Osterrieth quoted by Stanley Plotkin in his Royal Society speech (an early version of which had been distributed at the time of the London meeting). Even then I intended to approach Dr Osterrieth once more, to give him an opportunity to respond to the testimonies from Kisangani. But then came the publication of the Royal Society article, and I realised that there was little point, for Osterrieth had already made his definitive statement.

<sup>43</sup> Dr Koprowski, at the Royal Society meeting, dismissed one of my other African witnesses as a “low technician”, a comment which prompted some booing from the audience. My own experience is that African witnesses frequently have more accurate memories of events than Westerners – which may be partly due to African traditions of oral history.

<sup>44</sup> Anon., “Application au Congo du nouveau (antipolyo) du Dr. Koprowski”; *L’Avenir* (Leopoldville); August 9<sup>th</sup>/10<sup>th</sup>, 1958. The article states that the new polio vaccine of Dr Koprowski “has been prepared at Elisabethville by the Wistar Institute, and is controlled from the point of view of efficacy and safety by the Stanleyville laboratory”.

<sup>45</sup> The records suggest that CHAT was fed widely – to well over 800,000 people in 27 campaigns I have been able to document, and probably in other places too. By contrast, it seems that Fox was fed only on a small scale, to a few thousand individuals in three places: Aketi (December 1957), Stanleyville (May 1958), and Leopoldville (starting in September 1959). In the latter place, it seems likely that only Europeans were vaccinated. By that stage, the experimental trials were over, and September 1959 was the month when both CHAT and Fox were first fed to the bulk of the European population. By contrast, it was apparently felt not to be necessary to vaccinate the African population

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against Type 3 polio. This again provides a clue about the real purpose of the Congo vaccinations, which appear to have been staged primarily in order to experiment with new varieties of vaccine, rather than in order to protect the local population (as has often been claimed since).

<sup>46</sup> Letter from W. Bervoets to P. De Brauwere, September 17<sup>th</sup>, 1958; file H4484/1058 at the Belgian Ministry of Foreign Affairs archives.

<sup>47</sup> The 1958 annual report of the medical service of *Province Oriental* reveals that “almost 50,000 individuals” had been vaccinated with the Koprowski strains in the province during 1958. Since the vaccinations in Aketi, Stanleyville and the epidemic areas totalled only about 30,000 people, it would seem that the hygiene department vaccinated a further 20,000 or so. I have records of 4,000 doses being given at Rungu in June, and 5,000 at Kilo (probably in July), so some 10,000 further vaccinations apparently took place at places unknown.

<sup>48</sup> S.A. Plotkin, “CHAT oral polio vaccine was not the source of human immunodeficiency virus type 1 group M for humans”; *Clin. Inf. Dis.*, 2001, 32, 1068-84.

<sup>49</sup> R. Sohier and O.G. Gaudin, “Monkey cell cultures in virology”; *Primates in Medicine*; 1969; 3; 80-92; see Courtois in “Discussion” on page 91.

<sup>50</sup> I also interviewed several other African witnesses in 1999 and 2001, but they were either less directly involved, or their recall of events was far less impressive or precise. The five referred to here are the two assistants from Osterrieth’s lab, the assistant from Courtois’ lab, together with Joseph and “Antoine” from Lindi.

<sup>51</sup> *River*, 2000, p. 569.

<sup>52</sup> Since 1993, Dr Osterrieth has claimed variously: (a) [between 1993 and February 2000] that chimp kidneys were *only* sent to the Children’s Hospital of Philadelphia, and not to the Wistar Institute; (b) [in 1994] that no chimp kidneys were ever sent abroad from Stanleyville; (c), in his latest [September 2000] account before the Royal Society, that chimp kidneys *were* after all sent to the Wistar Institute.

<sup>53</sup> W. Henle, G. Henle and F. Deinhardt, “Studies on Hepatitis”, *Annual Report to the Commission on Viral Infections of the Armed Forces Epidemiology Board*, March 1958 – February 1959, see p. 5.

<sup>54</sup> The accuracy of this date of arrival for Deinhardt is underlined by the fact that it featured in a report which was completed just nine days later. W. Henle, G. Henle and F. Deinhardt, “Studies in Viral Hepatitis”; *Annual report to the commission on viral infections of the armed forces epidemiological board*; March 1, 1957 to February 10, 1958.

<sup>55</sup> Anon, “Monkey Business”; *Thermometer* (published by the Children’s Hospital of Philadelphia); 1958; 9(2); 3 and 6.

<sup>56</sup> F. Deinhardt, G. Courtois, P. Dherte, P. Osterrieth, G. Ninane, G. Henle and W. Henle, “Studies of liver function tests in chimpanzees after inoculation with human infectious hepatitis virus”; *Am. J. Hyg.*; 1962; 75; 311-321.

<sup>57</sup> P.M. Osterrieth, “Vaccine could not have been prepared in Stanleyville”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 839.

<sup>58</sup> *River*, 2000; pages 352-355 and 595.

<sup>59</sup> S.A. Plotkin, “CHAT oral polio vaccine was not the source of human immunodeficiency virus type 1 Group M for humans”; *Clin. Inf. Dis.*, 2001, 32, 1068-84; see page 1071.

<sup>60</sup> *River*, 2000; page 688.

<sup>61</sup> This tissue culture of chimpanzee kidney cells and chimpanzee serum was of course different from the classic tissue culture of the fifties, which typically employed monkey kidneys (usually from rhesus or cynomolgus macaques), and foetal calf serum. The latter was expensive to buy, and would also have been impractical to make in Stanleyville, not least because of the lack of cows in this “island in the rain forest”. Besides, if serum from the same species which had provided the cells was available, it would have made sound scientific sense to use it. [*River*, 2000, pages 847-848.]

<sup>62</sup> Anon., “Expert committee on poliomyelitis. Third report.”; *W.H.O. Tech. Rep. Ser.*; 1960; 203; 1-53.

<sup>63</sup> Osterrieth quoted on page 818 of S.A. Plotkin, “Untruths and consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus”; *Phil. Trans. R. Soc. Lond. B*, 2001; 356, 815-823.

<sup>64</sup> I was originally working on the assumption that the tubes would have been of 10c.c., and the bottles of 500 c.c., (which would have meant a total output of up to 7,000 c.c., or seven litres, of tissue culture), but I recently learnt that the bottles used in most labs of the fifties were of 4 oz, or 100 c.c., capacity. If correct, this would reduce the total to just 3,000 c.c., which could almost certainly be produced from two kidneys, coming from a single baboon.

<sup>65</sup> By contrast, there were eleven sentences and a lot of detail in the 1958 annual report about the much smaller hepatitis research programme involving the chimps.

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<sup>66</sup> See: J. Kingdon, *The Kingdon Field Guide to African Mammals*, (San Diego: Academic Press, 1997), pages 32-35, which reveals that the olive baboon, *Papio anubis*, is found in savanna, light woodland and forest mosaics (ie transitional forest). Kingdon adds that: “here and there olive baboon populations range deep into the rain forest”, and Kisangani/Stanleyville is shown as being included in its range on the tiny map on page 33. None the less, baboon numbers near to Stanleyville would probably have been quite small, not least because of the long distance from its natural habitat.

<sup>67</sup> Ghislain Courtois’ notebook, marked “Mission USA. Trinidad. IOC Rio. 1955”, was kindly made available by his son, Dr André Courtois. “IOC” apparently stands for the *Institut Oswaldo Cruz* in Rio.

<sup>68</sup> *River*, 2000, pages 276-277.

<sup>69</sup> *River*, 2000, chapter 20, especially pages 277-279.

<sup>70</sup> Letter from Paulette Dherte to Fritz Deinhardt, June 20, 1959, made available by Jean Deinhardt.

<sup>71</sup> An hour later in the interview, when we returned to this subject of making tissue culture, Gaston Ninane told me that after a while he had stopped trying to make it, “because after three or four or five months, it was impossible to make tissue culture with the material we had.” However, this time he only mentioned using human cells. When I reminded him that earlier he had told me that he had used both human cells and chimpanzee cells, he replied: “Oh, it’s possible. I don’t remember.” By this time, I was familiar with this particular phrasing from Dr Ninane, one that indicated that he was unwilling to answer any further questions on a topic. In *The River*, I wrote that Ninane “tried, but failed” to make tissue culture from chimp and human cells in Stanleyville, and that he later reiterated that he had never managed to make any successful cultures [page 569]. In fact, on reviewing the tape and tape transcripts, I find that he *never specifically stated* that he had failed to make successful cultures, although he repeatedly implied this. I did make one final phone contact with Dr Ninane in September 1997, partly in order to check this point, and he told me: “Yes, I tried [to make tissue culture], but I surely don’t have success, because it was impossible in the lab, where nothing make success in tissue culture.” This was the only time that he claimed explicitly that he had never managed to make any tissue culture in Stanleyville. Despite this, I feel that his repeated avoidance of answering the question directly (and the tautological nature of his final answer) raise issues about the reliability of that answer.

<sup>72</sup> In her unpublished scientific memoir, Gertrude Henle relates how (presumably in around 1954) she and Dr Deinhardt sought the help of one of the great acknowledged experts on tissue cultures, Wilton Earle of the NIH, only to be told that “three years of intensive training in his laboratory would be required. When [Dr Henle] pointed out that we were virologists who wanted to use the cultures merely as a tool...he threw up his hands in disgust. Yet we learned enough during the visit to start out on our own, an ineptly as it was initially.” It seems clear that by 1958 Deinhardt possessed the background know-how to produce successful cultures from chimpanzee cells, especially if these were Maitland-type cultures, which were relatively straightforward to make, involving the cutting up of some kidneys with scissors, and then mixing them with some serum, growth medium and adding a few drops of antibiotics. Deinhardt did of course work with chimp kidney cultures, of the trypsinised variety, as soon as he returned to Philadelphia at the end of April 1958.

<sup>73</sup> Others who theoretically could have helped with the making of tissue culture in Stanleyville during this period include Dr Courtois’s deputy Dr Mangen (about whom little is known, other than the fact that he and Courtois did not get along), the nurse/pharmacist Paulette Dherte, and Professor Welsch, a bacteriologist from Liege, who appears to have visited both Stanleyville and the Ruzizi Valley in the course of a lengthy African tour in early 1958.

<sup>74</sup> P. Osterrieth, personal communication, 1993. Osterrieth told me: “He [Vandepitte] wanted us to take the samples, and the technicians to do the work. And I thought that was completely silly. To stay the whole morning in a place waiting for people to come, just to take blood, instead of being in the lab and doing the interesting work. So he told me: ‘tomorrow it’s your turn’. I said: ‘yes’. I didn’t go. And then he said: ‘You were not there’. I said: ‘No, I forget’. Looking him straight in the eyes, [I said] ‘I forgot’. It was finished.”

<sup>75</sup> G. Henle, personal communication, 1993. The Armed Forces Epidemiology Board reports for 1957-8 and 1958-9 make it clear that four shipments of chimpanzee kidneys took place during 1958, which are presumably the ones dispatched by Deinhardt between February and April of that year. Two further chimp kidney shipments were sent later, presumably by Osterrieth. [See: Deinhardt F., Courtois G., Dherte P., Osterrieth P., Ninane G., Henle G. and Henle W.; “Studies of liver function tests in chimpanzees after inoculation with human infectious hepatitis virus”; *Am. J. Hyg.*; 1962; 75; 311-321.] Dr Henle added that Dr Koprowski had helped set up Deinhardt’s visit. This further supports the possibility that Deinhardt would have been happy to deliver a bottle of the new CHAT vaccine pool (10A-11) to Stanleyville, as a *quid pro quo* to Koprowski.

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<sup>76</sup> My problem here was that on the basis of what I had been told by doctors Ninane and Osterrieth, I was still locked into believing that they had *not* been able to make polio vaccine in Stanleyville – and that the vaccine must have been made either in Philadelphia or Belgium.

<sup>77</sup> It is my belief that Alzheimer's disease was mentioned by Dr Ninane's sister as well as Parkinson's, though it is not referred to in my notes, or on the tape recording. Certainly in our last two interviews Dr Ninane had often told me that he feared that he was suffering the first symptoms of Alzheimer's, but this may have been in a bid to explain lapses in memory, rather than an accurate self-diagnosis.

<sup>78</sup> S.A. Plotkin, "CHAT Oral Polio Vaccine Was Not the Source of Human Immunodeficiency Virus Type 1 Group M for Humans"; *Clin. Inf. Dis.*, 2001, 32, 1068-84; see page 1074.

<sup>79</sup> In addition, there is further confirmation of other aspects of this question by other indirect witnesses. For various reasons these are being held in reserve for now, but they will be reported at a later date.

<sup>80</sup> According to his former assistant, Georges Lambelin, Alexandre Jezierski (the Polish vet who spent more than four years experimenting with locally-prepared OPVs and IPVs at his lab in Gabu, eastern Congo) was equally secretive about the process, which ceased upon his departure in late 1957. *River*; 2000, page 611.

<sup>81</sup> Source: the assistant's official work documents from the colonial era, viewed by the author.

<sup>82</sup> Personal communications: Paul and Odette Osterrieth, 1993.

<sup>83</sup> L. Quersin-Thiry, "Action of anti-cellular sera on virus infections. II. Influence on heterologous tissue cultures"; *J. Imm.*; 1959; 82; 542-552. See also the first part of this article, sub-titled: "Influence on homologous tissue cultures infected with various viruses"; *J. Imm.*; 1958; 81; 253-260.

<sup>84</sup> Letter from W. Bervoets to P. De Brauwere, September 17<sup>th</sup>, 1958; file H4484/1058 at the Belgian Ministry of Foreign Affairs archives.

<sup>85</sup> G. Courtois, H. Koprowski et al., "Preliminary report of mass vaccination of man with live poliomyelitis virus in the Belgian Congo and Ruanda-Urundi"; *Brit. Med. J.*; 1958; 2(i); 187-190.

<sup>86</sup> S.A. Plotkin, "Untruths and consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus"; see page 820.

<sup>87</sup> *River*, 2000, pages 572-3.

<sup>88</sup> G. Barski, A. Jezierski and P. Lépine, "Sensibilité au virus de la poliomyélite *in vitro* des tissus de différentes espèces des singes d'Afrique centrale. Non réceptivité des tissus des certains mammifères", *Ann. Inst. Past.*; 1954; 86; 243-247. A. Jezierski, "Action cytopathogène des trois prototypes de virus de la poliomyélite *in vitro* sur les tissus de différentes espèces des singes d'Afrique centrale. Non réceptivité des tissus des certains animaux"; *Ann. Inst. Past.*; 1955; 89; 78-82.

<sup>89</sup> The paper in question reveals that CHAT pool 10A-11 had by that stage been fed to 25 Clinton infants. By comparing with the dates on which these infants were fed [see table in *River*, 2000, pages 695-698], and cross-checking with the titres fed to each infant, it was possible to calculate between which dates the paper must have been written [see: *River*, 2000, page 701]. The Moorestown trial (in a New Jersey dormitory suburb of Philadelphia) also began on January 27<sup>th</sup>, and in the book, I hypothesise that it might have involved another CHAT pool, 4B-5. With the additional information now available from Dr Plotkin, it seems that I was probably wrong, and that the Moorestown feeding also probably involved pool 10A-11. However, it seems that it involved a different substrate in New Jersey to that used in Stanleyville and Ruzizi.

<sup>90</sup> Dr Deinhardt's widow, Jean, has reported that he booked two seats on the plane, one for himself and one for the "box of shit", this being a cool-box containing faecal specimens from the children of Willowbrook state school, who had been experimentally infected with hepatitis. (The stools were later used to infect some of the Lindi chimps.)

<sup>91</sup> Anon, "Monkey Business"; *Thermometer*; 1958; 9(2); 3 and 6.

<sup>92</sup> It cannot be stated with certainty whether Osterrieth's assistant helped with the CHAT feeding on February 27<sup>th</sup>, with the Fox feeding on May 27<sup>th</sup>, or with both, but it makes little difference to the overall argument. Fresh vaccine had to be produced locally for every new vaccination, the assistant knows that this locally-made vaccine was fed by mouth at the army camp, and chimp tissues were available in the lab, not only during the February to April 1958 period, but after that as well (for two further shipments were sent to Deinhardt in Philadelphia, almost certainly by Osterrieth, after Deinhardt had left in April 1958). Thus both the army camp feedings are likely to have involved vaccine prepared locally in chimp cells.

<sup>93</sup> Uvira is actually four miles south of the tarmac road through the Ruzizi Valley along which the teams vaccinated, and so whether or not they vaccinated in the town itself, it seems likely that the population of the Uvira (believed to be 3,000 to 4,000 in 1958) would, just like the other local populations, have answered the call of the drums. In one interview in 1993, Dr Ninane told me that he thought he had vaccinated in Uvira, but he did not confirm this the next time I spoke with him.

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<sup>94</sup> G. Courtois, G. Jervis, A. Flack, H. Koprowski and G. Ninane, "Preliminary report of mass vaccination of man with live poliomyelitis virus in the Belgian Congo and Ruanda-Urundi"; *Brit. Med. J.*; 1958; 2(i); 187-190. I can confirm the February 24<sup>th</sup> start date for Ruzizi from Dr Flack's diary, but not the end date. There is a line in the diary that reads "Will probably finish polio April 10???", but Dr Flack herself left Usumbura on March 30<sup>th</sup>, by which time the Ruzizi Valley part of the trial seems to have been completed. Dr Ninane told me that he had to go back to Stanleyville for more vaccine for the final (Lake Tanganyika) leg of the vaccination [*River*, 2000, page 358], so it seems reasonable to propose that he and Hector Meyus completed this leg in early April, ending on the 10<sup>th</sup>. Interestingly, the grand total of 215,504 vaccinees recorded in the *BMJ* article is correct for the entire vaccination campaign – Ruzizi Valley plus Lake Tanganyika – so Koprowski clearly *was* briefed on the lakeside feedings before he wrote that article. This once again raises the question of why he failed to mention them in the article.

<sup>95</sup> G.A. Jervis, "Comparative susceptibility of tissue culture cells in experimental animals and man"; *Ann. N.Y. Acad. Sci.*; 1955; 61; 848-851.

<sup>96</sup> Plotkin does not reveal whether Ninane telegraphed from Stanleyville, or from somewhere close to the Ruzizi Valley, like Bukavu.

<sup>97</sup> S.A. Plotkin, "Untruths and consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus". *Phil. Trans. R. Soc. Lond. B*, 2001; 356, 815-823, see p. 818.

<sup>98</sup> Source: Agnes Flack's diary. According to Dr Jervis's widow, Ruth Jervis, her husband had other demands on his time, and it was always intended that he would leave Africa early, and before Dr Flack.

<sup>99</sup> In Agnes Flack's diary, there is no mention of Dr Jervis after the first couple of days of vaccinating, on February 24<sup>th</sup> and 25<sup>th</sup>, which suggests that, after this period, he may have been based at the Bukavu laboratory, rather than with Flack and the vaccinating teams at the Kabunambo field centre. An article dated March 7<sup>th</sup> places him in Bukavu at the start of that week (March 3<sup>rd</sup>), and gives the clear impression that, though he is working with Dr Courtois, Jervis is in overall charge of the campaign. Anon., "Debut d'une grande campagne anti-polio dans la vallee de la Ruzizi"; *Courrier d'Afrique*; March 7, 1958, page 4.

<sup>100</sup> It seems likely that Koprowski and Norton would have called in at the Bukavu lab when they drove by from Usumbura, en route for Epulu and Stanleyville, between February 2<sup>nd</sup> and 5<sup>th</sup>, 1957. [*River*, 2000, p. 614.]

<sup>101</sup> Anon., "Compte-rendu de la première conférence des médecins-directeurs de laboratoire du Congo Belge et du Ruanda-Urundi (Bukavu, 25 février – 2 mars, 1957)"; unpublished document available at the library of the Institute of Tropical Medicine, Antwerp. This also reveals that the meeting of medical directors had originally been scheduled to coincide with the opening of the *Stanleyville* lab in February 1957, but that this opening had been postponed because of construction delays. This may explain why Koprowski came out first in February, 1957 and then again in September.

<sup>102</sup> Personal research in Bukavu, March 2001. It has always struck me as surprising that so much money was being poured into building new medical laboratories in the Congo in 1957 and 1958, when independence was clearly not far away. This is all the more surprising because Belgium was still a relatively poor country, and development money for the Congo traditionally had to be engendered from within the colony itself. Perhaps it was different on this occasion.

<sup>103</sup> The plane made long stops in the heat of Athens and Khartoum en route to Stanleyville, making meltdown all the more likely.

<sup>104</sup> S.A. Plotkin, "Untruths and consequences", page 816, confirms that "no seed system was used" for the early pools of CHAT.

<sup>105</sup> Passaging again both amplified the vaccine, and (even more importantly) boosted its titre. (Presumably the scientist would inoculate vaccine into several culture bottles, let the virus grow, and then choose the culture with the highest titre for further amplification.) Of course, the better the titre, the more people could be immunised with the vaccine, and the more impressive were the figures that one could publish. This was the period when Koprowski, Sabin, Cox and Lépine were battling to win the race to develop an OPV that would be adopted the world over.

<sup>106</sup> F. Przesmycki et al., "Vaccination against poliomyelitis in Poland with Koprowski's live attenuated strains", in "Second international conference on live poliomyelitis vaccines: papers presented and discussions held (Washington, D.C., June 6-10, 1960)", *Pan-American Sanitary Bureau*, 1960, Scientific Publications No. 50, pp 522-531. See original vaccine titre on page 522, and compare with dilution details on page 526. The ten-fold disparity between the original titre, and that at the Warsaw lab prior to dilution, indicates that local amplification must have occurred. Other clues suggest that local passage of the vaccine virus may have happened routinely in Poland, for the article mentions that

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in the previous small-scale trials (staged in Wyszkiw, Poland in 1958-9) the Polish doctors had employed “live attenuated vaccine, prepared from Koprowski’s type 1 CHAT strain”.

<sup>107</sup> In Africa, the problem was rather that of keeping the vaccine temperature below 4 degrees centigrade. Above this temperature, live poliovirus rapidly becomes inactivated. This was always the major headache involved in moving live vaccines, like CHAT, long distances around the world, and then out into the field. In reality, quite a lot of the live polio vaccine fed in places like South America, and the Congo, may have been useless, having fallen to a non-immunogenic titre because it got too warm.

<sup>108</sup> M. Bottiger et al., “Vaccination with Attenuated Type 1 Poliovirus, the CHAT strain. III. Antibody response and spread of virus in schoolchildren”. *Acta. Paed. Scand.*; 1966; 55; 422-431. Various doctors who used to work for the Stockholm labs during this period have told me that cynomolgus monkeys were exclusively used for vaccine production from about 1957 onwards.

<sup>109</sup> S.A. Plotkin, “Recent results of mass immunization against poliomyelitis with Koprowski strains of attenuated live poliovirus”, *Am. J. Pub. Health*; 1962; 52(6); 946-960.

<sup>110</sup> Anon., “Requirements for production of attenuated vaccine for poliomyelitis (Koprowski strain)” [English translation of title and article made by Croatian]; *Immunolski. Zavod. Radovi*; 1964; 2; 124-125. This brief article clearly described work from the 60s, not the 50s, for the article states that no more than two passages of the primary virus from the Wistar Institute can take place, in either African green monkey kidney cells, or in WI-38.

<sup>111</sup> TCID<sub>50</sub> (or 50% Tissue Culture Infectious Doses), this being the titre at the point when 50% of the inoculated tissue cultures are infected by the virus.

<sup>112</sup> TCID<sub>50</sub> was the standard measure of titre of the day, though PFU (plaque-forming units), a method in which individual plaques were supposedly counted, was also sometimes used. In practice, the two methods gave comparable titres, though work by Roderick Murray and others revealed minor discrepancies.

<sup>113</sup> F. Przesmycki et al., “Vaccination against Poliomyelitis in Poland with Types 1 and 3 Attenuated Viruses of Koprowski”; *Bull. W.H.O.*; 1962; 26; 733-743.

<sup>114</sup> A personal letter written in April 1962 by Drago Ikic to Koprowski’s chief lab technician and right-hand-man, Tom Norton, reveals that Croatia was indeed supplied with CHAT seed-lots; Ikic requested that a half litre of each new strain of seed virus, or at least “as much as possible”, should be sent to him “as usual”. But the evidence suggests that the other European countries which “made their own vaccines”, such as Poland, Switzerland and Sweden, may have done so by onward passage of the CHAT vaccine strains, not the seed-lots. At another point in the interview I asked Dr Koprowski about the adverse results reported by George Dick and David Dane, who had fed his previous polio vaccines, SM N-90 and TN, to 14 and 190 people (respectively) in Belfast and Oxford (U.K.) in 1956, and found that they became more virulent (in the case of TN, much more virulent) after passage through the human gut. Koprowski replied that the British doctors had passaged the vaccine virus again in their own lab, implying that any shortcomings in the vaccines were their fault, not his. When I put this to doctors Dick and Dane, they both vigorously denied it. The vaccines they fed were exactly what Koprowski had sent them, they insisted. But the significant thing was that Koprowski’s immediate instinct had been to claim that the vaccine had been amplified locally in Belfast.

<sup>115</sup> No use of Koprowski’s vaccines in South Africa is reported in the literature, and it is stated that Sabin’s OPVs were eventually preferred to Koprowski’s in that country. Koprowski, however, did mention that a vaccine trial had been planned in the town of East London, though he later acknowledged that it might not have occurred. [*River*, 2000, p. 471.] My suspicion is that he may have supplied vaccines for such a trial but, as in Kenya, they were never used.

<sup>116</sup> H. Koprowski, personal communication, December 1993. Even though Dr Koprowski was quite open about the theory of local amplification, he was less helpful about the practical aspects. I continually asked him about the quantities of vaccine which had to be sent abroad, which gave him an opportunity to explain the mechanics of the process, and the fact that only small quantities needed to be dispatched. But in response, he merely said that he couldn’t remember any details. And whenever I pressed him for specific details about the operation of the Congo trials, he either couldn’t remember, or gave what I found to be confusing information. An example came when I understood him to have confirmed that doctors Flack and Jervis had flown out from America with the vaccine for the Ruzizi trial in February 1958. Immediately he snapped back: “I said *possibly*. This is all invention, hypothesis and fantasy.” At another point, he told me that the first vaccine used in the Congo had been made by the Wistar, whereas later vaccines had been made, he thought, by the Wistar and by the Belgian firm, RIT. (*River*, 2000, pages 467-468.) At this stage of the interview, he avoided mentioning anything about local preparation of the vaccine in the Congo.

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<sup>117</sup> G. Courtois, G. Jervis, A. Flack, H. Koprowski and G. Ninane, "Preliminary report of mass vaccination of man with live poliomyelitis virus in the Belgian Congo and Ruanda-Urundi"; *Brit. Med. J.*; 1958; 2(i); 187-190. Gaston Ninane told me that despite Courtois' being lead author and he, Ninane, last author, this article had actually been prepared by Koprowski (and perhaps Norton) at the Wistar, and merely sent to the Belgians for checking.

<sup>118</sup> G. Courtois, "Vaccination antipoliomyelitique par virus vivant au Congo Belge"; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 805-816.

<sup>119</sup> David Ho was one of the members of the Wistar-appointed "AIDS/Poliovirus Advisory Committee", which sat three times in 1992 to respond to the controversy caused by the Tom Curtis article in *Rolling Stone*. Some years after he had given me a copy of the documents furnished by the Wistar Institute to the Advisory Committee, Dr Ho told me that he "got into trouble" from other unnamed scientists for doing this.

<sup>120</sup> Anon., "History of the use of CHAT strain 'Type 1' attenuated polio virus in humans", undated, but clearly written between January 23<sup>rd</sup> and January 27<sup>th</sup>, 1958; (single-page document provided by the Wistar Institute to the AIDS/Poliovirus Advisory Committee in 1992). My calculations on the date of origin for the paper tie in well with other historical evidence.

<sup>121</sup> This is not explicitly stated in the one-page paper. However, immunogenicity trials are traditionally conducted by feeding aliquots of the vaccine, and ten-fold serial dilutions of same – and the Clinton infants were fed 6.7, 5.7, 4.7 and 3.7 log doses of CHAT 10A-11. Moreover, the titre details on that single page (both for pool 10A-11 and for the first pool of CHAT, which may have been pool 8) match those given in the published paper on the Clinton trials: [S.A. Plotkin, H. Koprowski and J. Stokes Jr., "Clinical trials in infants of orally administered attenuated poliomyelitis virus"; *Pediatrics*; 1959; 23; 1041-1062.], confirming that the titre of pool 10A-11 at the Wistar Institute was log 6.7.

<sup>122</sup> The "units" in this particular instance are unspecified. The Wistar scientist (probably Tom Norton) may have meant TCID<sub>50</sub>, PFU (plaque forming units), or the "cytopathogenic units" referred to by Courtois. I am informed that they were taken by most virologists of the era to have equivalent values. In a study conducted in 1959 by Dr Roderick Murray and colleagues from the Division of Biologics Standards at the National Institutes of Health on different OPV strains (three each from Koprowski, Sabin and Cox), it was found that both TCID<sub>50</sub> values and PFU values for the Type 1 vaccines (as measured at the NIH) were usually less than the TCID<sub>50</sub> values as measured at the source laboratory. In the case of CHAT (pool 13), PFU titre as measured at the NIH was 0.1 of a log dose *higher* than TCID<sub>50</sub> as measured by the Wistar, but this minor difference falls well within the range of testing error. [R. Murray et al., "Comparative virulence for rhesus monkeys of poliovirus strains used for oral administration"; in *First international conference on live poliovirus vaccines: papers presented and discussions held* (Washington D.C., June 22-26, 1959), Pan American Sanitary Bureau, 1959, Scientific publication No. 44, pp. 39-64. See table 2 on page 42.]

<sup>123</sup> M.P. Chumakov et al., "On the course of mass immunization of the populations in the Soviet Union with the live poliovirus vaccine from Albert B. Sabin's strains", in "Second international conference on live poliomyelitis vaccines: papers presented and discussions held (Washington, D.C., June 6-10, 1960)", *Pan-American Sanitary Bureau*, 1960, Scientific Publications No. 50, pages 413-428; see figure 1 on page 418.

<sup>124</sup> Murray et al., "Comparative virulence for rhesus monkeys of poliovirus strains used for oral administration"; table 2 on page 42.

<sup>125</sup> S. Wain-Hobson, personal communications, 1999 and 2000. *River*, 2000, pages 852-858.

<sup>126</sup> S. Wain-Hobson, personal communication, October 2001.

<sup>127</sup> E.D. Cooper and W.J. Roberston, "Problems relating from the use of live attenuated poliomyelitis virus type 1 in a mass campaign in a large urban area", *S. Af. Med. J.*; 1961; 35; 232-235. Other papers on this subject cite 1958, rather than 1957, as the year when local vaccine preparation began in South Africa, but the fact that in this article the start date of 1957 is sourced to a "personal communication" (probably either from Gear or from Hubert Malherbe, who trained under Albert Sabin in the US in 1957, and who is known to have returned to South Africa with the three Sabin strains) suggests that this version is probably correct.

<sup>128</sup> James Gear, personal communication, 1993.

<sup>129</sup> *River*, 2000, pp 520-521.

<sup>130</sup> It is not known whether Koprowski went ahead with the visit, but the South African literature reveals that at around this time it was decided to use Sabin's strains in preference to Koprowski's.

<sup>131</sup> Anon., "City polio proposal. Talks held on trial of new vaccine in Kenya"; *East African Standard* (Nairobi); February 1, 1957.

<sup>132</sup> J. Gear, “The South African poliomyelitis vaccine”; *S. Af. Med. J.*; 1956; 30; 587-594. The British, also, were interested in this field. An article in the same journal in September 1955 reveals that a Medical Research Council expedition to Fajara, Gambia, earlier that year, to examine the viability of using African primate kidneys for polio vaccine production [*River*, 2000, p. 388] was related to OPV, rather than IPV, research. [R. Turner, “Active immunisation against poliomyelitis”; *S. Af. Med. J.*; 1955; 29; 833-844.] The minutes of the committee meeting following this expedition are (unusually) missing from MRC files, and may have been effectively “censored”, according to the MRC official with whom I spoke. In 1997, I visited the Gambia, and the current director of the MRC labs at Fajara, Dr Hilton Whittle, ended up failing to give a promised interview under rather extraordinary circumstances. The same Dr Whittle chaired the final session of the Royal Society conference in 2000, a session which featured Dr Weiss’s closing summary, and which was felt by some parties to be biased in terms of selection of speakers from the floor. Bill Hamilton’s partner, Luisa Bozzi, was passed over in favour of an apparently preplanned anti-OPV “closing line” from John Maynard Smith, and Walter Nelson-Rees was so disgusted at what he considered Dr Whittle’s deliberate ignoring of him that he eventually walked out.

<sup>133</sup> INEAC: *L’Institut National pour l’Etude Agronomique du Congo Belge*, based in Yangambi.

<sup>134</sup> *River*, 2000, page 611.

<sup>135</sup> G. Barski, A. Jezierski and P. Lépine, “Sensibilite au virus de la poliomyélite *in vitro* des tissus de différentes espèces des singes d’Afrique centrale. Non receptivite des tissus des certains mammiferes”, *Ann. Inst. Past.*; 1954; 86; 243-247. A. Jezierski, “Action cytopathogene des trois prototypes de virus de la poliomyélite *in vitro* sur les tissus de différentes espèces des singes d’Afrique centrale. Non receptivite des tissus des certains animaux”; *Ann. Inst. Past.*; 1955; 89; 78-82.

<sup>136</sup> A. Jezierski, “Atténuation des trois types des virus de la poliomyélite sur les tissus de singes de l’espèce colobus. Virus vivant modifiés et leur application par différentes voies sur les singes”; *Ann. Soc. Belge Med. Trop.*; 1959; 39; 69-96. A. Jezierski, “Destruction of poliovirus in the digestive tract of chimpanzees by means of specific high-titre gamma-globulin”; *Ann. Soc. Belge Med. Trop.*; 1960; 40; 169-171, plus related discussion on 172-181.

<sup>137</sup> *River*, 2000, p. 610.

<sup>138</sup> A. André, G. Courtois, G. Lennes, G. Ninane and P.M. Osterrieth, “Mise en évidence d’antigènes de groupes sanguins A, B, O et Rh chez les singes chimpanzés”; *Ann. Inst. Past.*; 1961; 101; 82-95; see Mambasa group on page 87, and note the interesting tautology in the title: “chimpanzee monkeys”.

<sup>139</sup> G. Courtois, “Vaccination antipoliomyélique par virus vivant au Congo Belge”; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 805-816.

<sup>140</sup> E. Hooper, “Experimental oral polio vaccines and acquired immune deficiency syndrome”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 803-814.

<sup>141</sup> The names of these witnesses will be published at an appropriate time.

<sup>142</sup> Although I asked several officials, none could furnish an explanation for when the other papers had been removed, and what had happened to them afterwards.

<sup>143</sup> A.B. Sabin, “Notes on international requirements for live, oral poliomyelitis vaccine”, [letter dated 29 October 1960 received from Dr Albert B. Sabin], WHO internal document WHO/BS/IR/87.

<sup>144</sup> A.B. Sabin and L.R. Boulger, “History of Sabin attenuated poliovirus oral live vaccine strains”; *J. Biol. Stand.*; 1976; 1; 115-118.

<sup>145</sup> Anon., “Le vaccin du professeur Lépine sera vendu par une firme pharmaceutique americaine”; *L’Echo du Stan* (Stanleyville); June 24<sup>th</sup>, 1957. This brief report from AFP makes it clear that this is referring to a live vaccine from Lépine, not his more well-known IPV.

<sup>146</sup> B.H. Sweet and M.R. Hilleman, “Detection of a ‘non-detectable’ simian virus (vacuolating agent) present in rhesus and cynomolgus monkey-kidney cell culture material. A preliminary report”, in “Second international conference on live poliomyelitis vaccines: papers presented and discussions held (Washington, D.C., June 6-10, 1960)”, *Pan-American Sanitary Bureau*, 1960, Scientific Publications No. 50, pages 79-85.

<sup>147</sup> For a good article about the risks of antigenic drift (reversion to virulence), written soon after a sabbatical at the Wistar during which he had worked with CHAT, see: S. Gard, “Immunological strain specificity within Type 1 poliovirus”; *Bull. W.H.O.*; 1960; 23; 235-242.

<sup>148</sup> H. Koprowski; “Immunization against poliomyelitis with living attenuated virus”; *Am. J. Trop. Med.*, 1956; 5; 440-452.

<sup>149</sup> Although Weller described his team of Uganda-based virologists as one that might be “preparing to investigate febrile entities of unknown etiology”, he made it clear in the same paragraph that tissue culture techniques applied as much to “the propagation of [viral] agents in quantity” as to methods of “viral isolation”.

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<sup>150</sup> T.H. Weller, "Problems revealed by the expanding use of tissue culture procedures in studies of infectious agents"; *Am. J. Trop. Med.*, 1956; 5; 422-429.

<sup>151</sup> *River*, 2000, page 549.

<sup>152</sup> G. Courtois, H. Koprowski et al., "Preliminary report of mass vaccination of man with live poliomyelitis virus in the Belgian Congo and Ruanda-Urundi"; *Brit. Med. J.*; 1958; 2(i); 187-190. In fact, it seems that CHAT feeding in Africa may have begun in January 1957. When Hilary Koprowski and Tom Norton appeared in Nairobi (Kenya) at the end of that month, Koprowski announced to fellow-scientists and the local press that "we feel we have now reached the point where large-scale clinical trials [of OPV] should be attempted". "We are anxious", he went on, "to have it [his OPV, which apparently had "no name"] tried out in pilot schemes in any country in the world where it will be under the supervision of responsible medical people". He went on to invite participation by the medical communities in both Kenya and South Africa, which he said he would visit later in his trip, and his speech prompted a front page article and an editorial in one of Nairobi's two leading dailies. [Anon., "City polio proposal. Talks held on trial of new vaccine in Kenya"; *East African Standard* (Nairobi); February 1, 1957.] The article also stated that he and Norton, who were "carrying supplies of their vaccines" were leaving that very day for Stanleyville in the Belgian Congo, where they would "carry out experiments in chimpanzees". However, interviews have revealed that Koprowski also distributed some of his polio vaccines in Kenya. He gave Type 1 vaccine capsules (which may have contained either CHAT or his previous Type 1 strain, SM N-90) to both the man in charge of vaccine procurement for British East Africa, Dr Geoffrey Timms, and to Dr Jimmy Harries, who was at risk because he worked with polio patients in Mombasa. The former were apparently destroyed by the Kenyan health authorities, following the furore about the Northern Ireland trials that erupted a few days after this, when Timms' brother-in-law, George Dick, and his colleague, David Dane, reported that Koprowski's Type 2 polio vaccine strain, TN, reverted to virulence after human passage, and was therefore too risky to be tested further in human subjects; the two British doctors also raised questions about the suitability of Koprowski's Type 1 strain, SM N-90. [*River*, 2000, Chapter 15, especially pages 223-224.] However, by this stage Dr Harries had already given the capsules to his family to eat with their Sunday lunch. [*River*, 2000, page 618.] Since Koprowski's 1958 *BMJ* article reveals that CHAT vaccination began in Stanleyville in February 1957, this strongly suggests that it was the same CHAT vaccine that Koprowski handed out in Kenya, and this is further supported by the fact that CHAT capsules were fed in the Congolese town of Aketi in May 1957.

<sup>153</sup> Anon., "Live virus in the jungle"; *Time*; August 11, 1958, p. 30. The accompanying photo could only have been taken during February 1957, for Osterrieth was not present during Koprowski's second visit to Stanleyville in September and October of that year.

<sup>154</sup> *River*, 2000, page 383.

<sup>155</sup> From published reports in the formal medical literature we know just one detail of the polio work that was conducted on nearly 400 Lindi chimps between June 1956 and February 1958. This is that CHAT vaccine and its Type 3 cousin, Fox, were each tested intraspinally on five chimpanzees, without apparent ill effect. This hardly suggests that the safety testing of CHAT and Fox was the reason why Lindi camp had been created. Later, in 1959, Koprowski claimed at a polio conference that he and Courtois had tested an additional 29 chimps intraspinally with different vaccine strains. However, he failed to provide any significant details apart from the fact that the Type 1 predecessor to CHAT, SM, was among the vaccines tested. [Discussion by Koprowski in: in "First international conference on live poliomyelitis vaccines: papers presented and discussions held (Washington, D.C., June 22-26, 1959)", *Pan-American Sanitary Bureau*, 1959, Scientific Publications No. 44, p. 201.] This mass testing of vaccine safety in chimps is interesting, in that Albert Sabin had already reported in January 1957 (at a conference which Koprowski attended) that chimps were far less sensitive to polioviruses injected into the central nervous system than were lower monkeys, like macaques. [*River*, 2000, pp 528-529. A.B. Sabin, "Properties of attenuated polioviruses, and their behaviour in human beings"; in T.M. Rivers (ed.), "Cellular Biology, Nucleic Acids and Viruses", *Special Publications of the New York Academy of Sciences*; 1957; 5; 113-127.] Chimpanzees did not, in short, provide a good barometer of vaccine safety. At another discussion session at the 1959 polio vaccine conference, Dr Courtois revealed that 30 of the Lindi chimps had been fed with large amounts of virulent Type 1 and Type 2 polioviruses, Mexican and YSK, to check whether these viruses were capable of paralysing chimpanzees; he reported that nine animals became paralysed. As for the testing of immunogenicity, there is a record from Dr Fritz Deinhardt's hepatitis databook, which dates from 1959, of just one such experiment having been carried out. It was not, however, conventional vaccination and challenge, because it involved an assessment of Koprowski's previous Type 1 vaccine, SM N-90, which was fed to 14 chimps that were subsequently challenged with virulent Type 2 poliovirus. (It had long been one of

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Koprowski's pet theories that Type 1 vaccine might protect against both Types 1 and 2 poliomyelitis. The hypothesis, as we now know, is incorrect.) Because of the dearth of documentation about the Lindi polio work, there is no evidence that conventional vaccination and challenge work (such as vaccinating with Type 1 and then challenging with virulent Type 1) was ever carried out.

<sup>156</sup> Anon., "Guerre ⋄ la polio dans la brousse stanleyvilloise"; *Le Stanleyvillois*; February 11, 1957, pp 1 and 4.

<sup>157</sup> It was originally intended that the new lab at Stanleyville would have been opened in February 1957 (which may well explain the timing of Koprowski's first visit), but apparently there was a hold-up with the building work, which delayed the formal opening ceremony by seven months. However, Tom Norton's photos from February and March 1957 reveal that the building itself was completed, but that it apparently lacked windows. In any case, I am told that some of the individual rooms in the new lab were open and functioning well before September, 1957.

<sup>158</sup> Based on the annual reports of the *Laboratoire Médical de Stanleyville* for 1957 and 1958. Even though the official dates recorded in these reports for triannual leaves are not absolutely precise (for instance, Dr Osterrieth is recorded as having returned to Stanleyville on Feb 23<sup>rd</sup>, 1958, even though we know he returned some three weeks earlier), they do give a good general idea of when individual doctors were present at, or absent from, the lab.

<sup>159</sup> *River*, 2000, pp. 720-721.

<sup>160</sup> This figure of 60 is nicely supported by the Lindi camp records, which reveal that by far the greatest influx of chimpanzees was during the first seven months of the camp's operation, starting in June 1956. Over 200 had been admitted by mid-January 1957, of which only 60 were still alive at the start of February. If we assume that there was a death rate from natural causes of 25%, this means that over 100 chimps were sacrificed in seven months – before Koprowski and Norton had even arrived for their first visit. Even if the death rate was higher to begin with – say 50% - this still means that 70 chimps were sacrificed in seven months. Either figure is consistent with 60 chimps being "used" by the end of 1956.

<sup>161</sup> The Lederle lab notebooks [see later] reveal that the faecal virus from patient "Charlton" (which was the origin of the CHAT strain) was being plaqued out in July 1956, so the earliest known version of CHAT cannot have been given to the great Italian virologist, Renato Dulbecco, to test for neuropathogenicity until the final months of 1956. Dulbecco discussed Koprowski's new OPV strains at the polio conference in New York City in January 1957 [R. Dulbecco, "Discussion", in T.M. Rivers (ed.), "Cellular Biology, Nucleic Acids and Viruses", *Special Publications of the New York Academy of Sciences*; 1957; 5; 138-139], and later identified the versions he had been given as "Charlton, plaque 20" and "SM N-90, pool 21". [R. Dulbecco, "Mutants of poliomyelitis viruses with reduced efficiency of plating in acid medium and reduced neuropathogenicity"; *Virology*; 1957; 4; 141-155.] The timings strongly suggest that CHAT research in the Congo did not begin before February 1957, when the new vaccine arrived with Koprowski and Norton – and was tested in Lindi chimps, and local humans. It therefore seems probable that SM N-90, pool 14, was used in the Stanleyville and Lindi research staged before February 1957. But when did pool 14 arrive in Stanleyville? Ghislain Courtois was in central and south America on a yellow fever tour in 1956, and would probably have passed through the U.S., so perhaps he met Koprowski then, and brought pool 14 back to Stanleyville with him. However, in an article published in April 1958, Courtois claimed he first met Koprowski in New York in 1955. [Anon. "Ruzizi. Campagne de vaccination massive contre le poliomyélite", *Temps Nouveau d'Afrique*, April 13, 1958.] This version of events, published in a local African newspaper three months before Koprowski's "official" account [*Brit. Med. J.*; 1958; 2(i); 187-190] was published, is intriguing. Koprowski's version is that he was initially put in touch with Courtois by the Stanleyville vet, Tad Wiktor, whom he met at the Muguga rabies conference in Kenya in July 1955. However, Courtois was back in Stanleyville from his 1955 foreign tour by the time that Muguga started, and Koprowski only returned to the U.S. in late August. Unless Courtois flew back to America later in the year (which would have been extremely unusual, so soon after his previous visit), or unless he was wrong about the date, we are left to conclude that he must have met Koprowski in New York in spring 1955, before Koprowski went to Muguga. The latter scenario raises the possibility that whatever role Wiktor played in the Stanleyville OPV research, it was not the alleged one – that of introducing the two men.

<sup>162</sup> L. Hayflick, "The establishment of a line (WISH) of human amnion cells in continuous cultivation"; *Exp. Cell Res.*; 1961; 23; 14-19. L. Hayflick, S.A. Plotkin, T.W. Norton and H. Koprowski, "Preparation of poliovirus vaccines in a human fetal diploid cell strain"; *Am. J. Hyg.*; 1962; 75; 240-258.

<sup>163</sup> According to the definitions of Leonard Hayflick, cell strains consist of cells which have divided several times, up to a maximum of about fifty, *in vitro*, and which retain the characteristics of normal

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cells; by contrast cell lines have become immortalised, have changed karyology, and reproduce ad infinitum.

<sup>164</sup> B.F. Elsworth and R.B. Stricker, "Polio vaccines and the origin of AIDS"; *Med. Hypoth.*; 1994; 42; 347-354. B. Goldberg and R.B. Stricker, "Bridging the gap: human diploid cell strains and the origin of AIDS"; *J. Theoret. Biol.*; 2000; 32; 1068-1084.

<sup>165</sup> I believe that there are also some other, less important shortcomings in the Elsworth/Goldberg/Stricker hypothesis, about which I wrote to one of them in September 1999, but without receiving a proper reply. The only really serious flaw, however, appeared to be that relating to the timing. I believe that if one adapts their hypothesis so that it applies to cell lines (such as human amnion lines) which had been overtaken by HeLa, rather than to cell lines like WISH or human diploid cell strains, then the timing difficulties disappear. I would like to add that Billi Goldberg, in particular, is deserving of considerable praise for keeping open lines of communication about these topics (mainly through distributing relevant articles by e-mail to supporters of both major theories of origin).

<sup>166</sup> B. Forghani et al., "Advantages of a human immunodeficiency virus type 1 (HIV-1) persistently infected HeLa T4+ cell line for HIV-1 indirect immunofluorescence assay"; *J. Clin. Microbiol.*; 1991; 29(10); 2266-2272.

<sup>167</sup> L. Hayflick, "History of cell substrates used for human biologicals"; *Develop. Biol. Standard.*, 1989; 70; 11-26; see page 14.

<sup>168</sup> L. Quersin-Thiry, "Action of anti-cellular sera on virus infections. I. Influence on homologous tissue cultures infected with various viruses"; *J. Imm.*; 1958; 81; 253-260. L. Quersin-Thiry, "Action of anti-cellular sera on virus infections. II. Influence on heterologous tissue cultures"; *J. Imm.*; 1959; 82; 542-552.

<sup>169</sup> G. Henle and F. Deinhardt, "The establishment of strains of human cells in tissue culture"; *J. Imm.*; 1957; 79; 54-59.

<sup>170</sup> W. Henle, G. Henle and F. Deinhardt, "Studies in Viral Hepatitis"; *Annual report to the commission on viral infections of the armed forces epidemiological board*; March 1, 1957 to February 10, 1958; p. 2.

<sup>171</sup> J.F. Enders, T.H. Weller and F.C. Robbins, "Cultivation of the Lansing strain of poliomyelitis virus in cultures of various human embryonic tissues"; *Science*; 1949; 109; 85-87. This brief, but oft-cited article was published in 1949, and by the first half of the fifties, the use of tissue culture had revolutionised virology research.

<sup>172</sup> L. Hayflick, personal communication, 1993.

<sup>173</sup> W. McLimans, "Discussion", in *Papers and discussions presented at the Fourth International Poliomyelitis Conference* (Philadelphia: Lippincott, 1958), pages 284-287.

<sup>174</sup> F.W. Sheffield and G.M. Churcher, "The serial propagation of poliomyelitis viruses in cells derived from rabbit embryo kidney"; *Brit. J. Exp. Path.*; 1957; 38; 154-159.

<sup>175</sup> Anon., *Registry of animal cell lines certified by the cell culture collection committee* (Washington, D.C.: US Department of Health Education and Welfare, Public Health Service, 1964), page 1.

<sup>176</sup> M. Gold, *A Conspiracy of cells. One woman's immortal legacy and the medical scandal it caused*. (New York: SUNY Press, 1986), pp 27-31.

<sup>177</sup> W.A. Nelson-Rees and R.R. Flandemeyer, "HeLa cultures defined"; *Science*; 1976; 191; 96-98. W.A. Nelson-Rees, D.W. Daniels and R.R. Flandemeyer, "Cross-contamination of cells in culture"; *Science*; 1981; 212; 446-452.

<sup>178</sup> Nelson-Rees tells me he strongly suspects that Chang's conjunctival cells were also a HeLa contaminant.

<sup>179</sup> WISH began life as a human amnion cell culture obtained at the birth of Leonard Hayflick's daughter (hence the name: Wistar Institute Susan Hayflick) and it transformed into a cell line 35 days later, on December 31<sup>st</sup>, 1958. Years later it was revealed by Walter Nelson-Rees to have been immortalised because of HeLa contamination.

<sup>180</sup> W. A. Nelson-Rees, "Responsibility for truth in research"; *Phil. Trans. Roy. Soc. Lond. B*; 2001; 356; 849-851.

<sup>181</sup> J.R. Masters, "HeLa cells 50 years on: the good, the bad and the ugly"; *Nature Reviews Cancer*; 2002; 2; 315-319.

<sup>182</sup> J.C.N. Westbrook et al., "Transformation of normal cells in tissue culture: its significance relative to malignancy and virus vaccine production", *Brit. J. Exp. Path.*; 1957; 38; 138-154.

<sup>183</sup> W. Nelson-Rees, personal communication, June 2002.

<sup>184</sup> H. Koprowski, "Living attenuated poliomyelitis virus as an immunizing agent of man"; *S. Af. Med. J.*; 1955; 29; 1134-1142; see table 14 on page 1141.

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<sup>185</sup> H. Koprowski and S.A. Plotkin, *Notes on acceptance criteria and requirements for live poliovirus vaccines*; WHO internal document WHO/BS/IR/85; November 1, 1960; see especially pages 7-10.

<sup>186</sup> Just prior to this passage, Koprowski and Plotkin had written: "Since there is no evidence from the epidemiological surveys of a vaccinated population indicating widespread harmful effects of the non-polio viruses present in the vaccine, and since elimination of these viruses from each lot of vaccine prepared from monkey kidney may present insurmountable obstacles for successful launching of the manufacture of the vaccine, the following requirements are proposed, based on a realistic rather than an idealistic solution to the problem." The casual reference to a lack of *widespread* harmful effects in vaccinated populations is telling, as is the emphasis on commercial viability.

<sup>187</sup> L. Hayflick and P.S. Moorhead, "The serial cultivation of human diploid cell strains"; *Exp. Cell Res.*; 1961; 25; 585-621. In a communication in 1997, Hayflick stated that this same article (presumably bar certain updates) had been submitted to, and rejected by, the *Journal of Experimental Medicine* in 1960. See: L.A. Hayflick, "Mortality and immortality at the cellular level. A review", submitted August 1, 1997, and available on . My thanks to Billi Goldberg for alerting me to this unpublished article.

<sup>188</sup> L. Hayflick, "The establishment of a human cell line (WISH) of human amnion cells in continuous culture", *Exp. Cell Res.*; 1961; 23; 14-20.

<sup>189</sup> W. Nelson-Rees, personal communications, April and June 2002.

<sup>190</sup> There are countless examples, of which I shall cite just one. Several trials of DDT and the organochlorine pesticide, Lindane, were staged in the Ruzizi Valley in the fifties. In one such trial, so much Lindane was put into a tributary of the Ruzizi river, that the fish were killed as far as 50 kilometres downstream. It is reported that Africans flocked to the river to partake of the easy catch. It is even possible that the immunosuppressive effects of such chemicals may have rendered local populations more vulnerable to the effects of a primordial HIV.

<sup>191</sup> For comments on "volunteers" in the African CHAT trials, see: *River*; 2000; pages 733-734, 736.

<sup>192</sup> W.D. Hamilton, undated, part of hand-written note written in margins of draft of (unsent) letter from author to B. Elswood.

<sup>193</sup> Joan Phillips, personal communication, 1995.

<sup>194</sup> *River*, 2000, page 483.

<sup>195</sup> S.A. Plotkin, "Untruths and consequences: the false hypothesis linking CHAT type 1 polio vaccination to the origin of human immunodeficiency virus". *Phil. Trans. R. Soc. Lond. B*, 2001; 356, 815-823, see p. 818. Here, Dr Plotkin states that he has found a note in his files to the effect that CHAT lot 101 from RIT had been used in Ruanda-Urundi in late 1959 or early 1960. It now appears that on this detail, at least, he may well be correct, and this may indeed have been the first time that Belgian-made vaccine was used in Africa (It now seems that previously the vaccinators had used Wistar-made vaccine, albeit locally amplified in the Congo.)

<sup>196</sup> In 1997, Dr Thiry's last-minute refusal of an interview about these events was accompanied by dramatic accusations that I was misleading my research assistant (who, according to Dr Thiry, was apparently too young to know her own mind), and that I was "damaging vaccinations and reputations". *River*, 2000, pp 781-783. Dr Thiry, I have since learnt, has a mixed reputation among her contemporaries. According to one fellow-virologist, she is "Koprowski's humble servant", and has "skin as thick as cowhide".

<sup>197</sup> G. Courtois, letter to Jan Stijns in Leopoldville, February 17, 1961.

<sup>198</sup> J.P. Delville and S.R. Pattyn, "Epidémiologie de la poliomyélite au Congo Belge et au Ruanda-Urundi. Etat actuel de nos connaissances"; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 283-292.

<sup>199</sup> S. Pattyn, "A review of enteroviruses in the Belgian Congo"; *Trop. Geog. Med.*; 1962; 14; 71-79.

<sup>200</sup> A. Lebrun, J. Cerf, H.M. Gelfand, G. Courtois, S.A. Plotkin and H. Koprowski, "Vaccination with the CHAT strain of Type 1 attenuated poliomyelitis virus in Léopoldville, Belgian Congo. I. Description of the city, its history of poliomyelitis, and the plan of the vaccination campaign"; *Bull. W.H.O.*; 1960; 22; 202-213. S.A. Plotkin, A. Lebrun and H. Koprowski, "Vaccination with the CHAT strain of Type 1 attenuated poliomyelitis virus in Léopoldville, Belgian Congo. II. Studies of the safety and efficacy of vaccination"; *Bull. W.H.O.*; 1960; 22; 215-234. M. Vandeputte, "Endemicity of the enteroviruses in Leopoldville"; *Bull. W.H.O.*; 1960; 22; 313-318.

<sup>201</sup> G. Henle and F. Deinhardt, "The establishment of human cells in tissue culture"; *J. Imm.*; 1957; 79; 54-59.

<sup>202</sup> The same happens with HIV-infected macrophages, as illustrated, for instance, in: V. Maréchal et al., "Human immunodeficiency virus type 1 entry into macrophages mediated by macropinocytosis"; *J. Virol.*; 2001; 75(22); 11166-11177.

<sup>203</sup> *River*, 2000, p. 661.

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<sup>204</sup> I do not recall seeing any figures (or even estimates) for AIDS deaths from HIV-2, HIV-1 Group O, or HIV-1 Group N. The combined figure of 20,000 is my own very approximate estimate, as of June 2002.

<sup>205</sup> Anon., “Application au Congo du nouveau (antipolyo) du Dr. Koprowski”; *L’Avenir* (Leopoldville); August 9<sup>th</sup>/10<sup>th</sup>, 1958.

<sup>206</sup> S.A. Plotkin, D.E. Teuwen, A. Prinzie, and J. Desmyter, “Postscript relating to new allegations made by Edward Hooper at the Royal Society discussion meeting on 11 September 2000”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 825-829; see page 827.

<sup>207</sup> S.R. Pattyn, “Anti-poliomyelitic vaccination in tropical countries”; *Trop. Geog. Med.*; 1964; 16; 4-9. In this article, Pattyn also points out that for an oral polio vaccine, “shipping and conservation are difficult”, but that it can be “transported in concentrated form, to be diluted immediately before use”. Once again, the reference is only to dilution, and not to local amplification. And yet it is quite clear that these vaccines were routinely amplified in locally-available tissue culture, in the Congo as elsewhere.

<sup>208</sup> There were apparently four quite impressive labs sited close together in Elisabethville – medical, veterinary and hygiene labs, together with the local branch of IRSAC (*Institut pour la Recherche Scientifique en Afrique Centrale*). There also appears to have been a lab at the local headquarters of FOREAMI [see main text], and there may have been others.

<sup>209</sup> J.P. Delville and S.R. Pattyn, “Epidémiologie de la poliomyélite au Congo Belge et au Ruanda-Urundi. Etat actuel de nos connaissances”; *Ann. Soc. Belge Med, Trop.*; 1958; 38; 283-292.

<sup>210</sup> S. R. Pattyn, “A review of enteroviruses in the Congo”; *Trop. Geog. Med.*; 1962; 14; 71-79.

<sup>211</sup> *River*, 2000, pages 567-568.

<sup>212</sup> Whether pool 13 of CHAT was initially manufactured in the U.S. and then forwarded to the lab in Brussels (as Gelfand now proposes), or initially manufactured in Belgium is uncertain. It is also no longer of importance, because it is now clear that the vaccine was later amplified in locally-available substrates in the Belgian Congo. Despite my frequently asking, Dr Gelfand was unable to give me any idea about the quantity of vaccine which he had carried to Leo. I believe that it was probably just a small bottle or two, and that this original vaccine was then amplified locally. Once again, it is the local amplification that is the step which, even now, those involved with the Congo CHAT research wish to keep a secret.

<sup>213</sup> In two phone interviews in 1996 and 2000, Dr Gelfand gave me six different answers on this question, ranging from: “I had the vaccine, I believe, for use throughout the Congo”, and “I feel with a moral certainty that I must have carried the vaccine around [to the other cities]”, to “My preference for recollection is that I did not carry the vaccine [to the other cities]. I’m not certain either way”. Since more of the views he expressed were positive than negative, and since the positive views were more forthrightly stated, I reported in my Royal Society speech that “Dr Gelfand is unsure, but believes that he also carried the vaccine onwards to Stanleyville, Bukavu and Elisabethville”. In retrospect, I accept that perhaps it would have been better had I written “believes he *may also have* carried the vaccine onwards...” However, Dr Gelfand has now reported to Dr Plotkin (as quoted in Plotkin’s “Postscript” article) that what he actually told me was: “I am unsure, but it is very *unlikely* that I carried vaccine from Leo to other cities in the Congo”. This is untrue. At no point in our two phone conversations, or in his several letters, did Dr Gelfand ever say, or intimate, that this was very unlikely.

<sup>214</sup> H. Gelfand, personal communication, December 1996.

<sup>215</sup> See for instance, Gelfand’s previous paper about poliomyelitis in Liberia (another African nation with close links to the US), in which he concluded that it was not then justifiable to vaccinate the indigenous population against polio, but that foreign visitors should consider vaccination. [H. Gelfand and M. J. Miller, “Poliomyelitis in Liberia”; *Am. J. Trop. Med. Hyg.*; 1956; 5; 791-796.] (Unfortunately, the journal details are not marked on my copy, but I believe that this is probably the correct reference. The page numbers are correct, and the article was almost certainly published in 1956.)

<sup>216</sup> A. Lebrun et al., “Vaccination with the CHAT strain of Type 1 attenuated poliomyelitis virus in Léopoldville, Belgian Congo. I.”; *Bull. W.H.O.*; 1960; 22; 202-213.

<sup>217</sup> S.A. Plotkin, A. Lebrun and H. Koprowski, “Vaccination with the CHAT strain of Type 1 attenuated poliomyelitis virus in Léopoldville, Belgian Congo. II. Studies of the safety and efficacy of vaccination”; *Bull. W.H.O.*; 1960; 22; 215-234.

<sup>218</sup> *River*; 2000; pages 737-739.

<sup>219</sup> *River*; 2000; p. 529.

<sup>220</sup> Anon., “Wistar Institute is both monument and prototype of modern research”; *Scope Weekly*; May 21, 1958, pp. 6-7.

<sup>221</sup> S. Plotkin, letter to F. Deinhardt, May 28, 1959 (made available by Jean Deinhardt).

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<sup>222</sup> *River*; 2000; pp 738.

<sup>223</sup> IRSAC: *L'Institut de Recherche Scientifique en Afrique Centrale*, based in Lwiro.

<sup>224</sup> I have found countless references to this nickname. It was mentioned in almost every review article that has been written about Meyer and his work (including obituaries), and in several other documents, such as personal letters from the era, and a memoir of the first Sonoma vaccination by Tom Norton.

<sup>225</sup> Probably part of FOREAMI: *Fond médical Reine Elisabeth pour l'Assistance Médicale aux Indigènes*, another royal-backed institute of scientific research. It is not immediately clear how this type of research would have benefited the indigenous population.

<sup>226</sup> Correspondence can be found on the excellent "Profiles in science: Joshua Lederberg" web-site at: <<http://profiles.nlm.nih.gov/BB/>>

<sup>227</sup> Anon., "Wistar Institute names chief", *New York Times*, March 27, 1957, page 6.

<sup>228</sup> N. Topping, with G. Cohn, *Recollections* (Los Angeles: USC Press, 1990), see pages 140-141. The book was apparently finished in 1989, and Topping writes that he recruited Koprowski to the Wistar first of all, and that this was "more than 35 years ago", ie 1954 or earlier. He also writes in his foreword, that "places, dates and sometimes names may escape me or become muddled". However, his recollection that Koprowski was involved with the Wistar from long before his formal arrival in 1957 is confirmed by multiple sources [see text], and I had previously discussed it in *The River*, 2000 (page 976, note 15; and page 707), long before I discovered Topping's book. Another relevant detail about the timing is that Geoffrey Rake is known to have been recruited in 1953, and Topping says he enlisted Koprowski before Rake.

<sup>229</sup> Available in the "letters" section in the "Profiles in science: Joshua Lederberg" web-site at: <<http://profiles.nlm.nih.gov/BB/>>.

<sup>230</sup> Anon., "Dr. Agnes Nelson Flack, 92, pioneer of polio research"; [U.S. local paper, title unknown]; December 19, 1989. See also: Anon., "Call to duty", *Wilkes-Barre Record* [Pennsylvania]; January 7, 1958.

<sup>231</sup> H.M. McClure et al., "Erythroleukemia in two infant chimpanzees fed milk from cows naturally infected with the bovine C-type virus"; *Cancer Research*; 1974; 34; 2745-2757. The C-type virus of the title was later named bovine leukemia virus. The abstract noted that this was the first time either leukemia or PCP had been reported in chimpanzees. The NBC researchers appear to have established a herd infected with bovine leukemia at least a decade before this report was published, for National Cancer Institute funding can be traced back to 1965, for research that included BLV transmission experiments to different animals and cell lines.

<sup>232</sup> P.S. Brachman and F.R. Fekety, "Industrial Anthrax"; *Ann. N.Y. Ac. Sci.*; 1958; 70; 574-584.

<sup>233</sup> The two main reports are: S.A. Plotkin, P.S Brachman et al., "An epidemic of inhalation anthrax; the first in the twentieth century. I. Clinical features"; *Am. J. Med.*; 1960; 29; 992-1001; and P.S Brachman, S.A. Plotkin, et al., "An epidemic of inhalation anthrax; the first in the twentieth century. II. Epidemiology"; *Am. J. Hyg.*; 1960; 72; 6-23. In the second article, there is a single, bland sentence which reads: "The potential civil defence problem posed by anthrax aerosols is also emphasized". This is the only overt clue that the study may in any way relate to biowarfare research.

<sup>234</sup> J.M. Barnes, "The development of anthrax following the administration of spores by inhalation"; *Brit. J. Exp. Path.*; 1947; 28; 385-394. Dr Barnes, who worked for Britain's BW research centre at Porton Down, devoted most of his paper to an analysis of how anthrax spores sprayed from solutions of a commercial detergent, *Tergitol*, increased death rates in guinea-pigs by a factor of ten. Brachman and Plotkin acknowledged that this was "an effect which also was demonstrated with [the] particular detergent" that had been used in the Manchester mill (which is not described by brand name, but only as a "non-ionic detergent"), before insisting "it seems impossible for patient no. 1 to have come into contact with detergent-scoured hair before the onset of his illness". Since the patient in question apparently died before he could be interviewed about his activities in the mill in the hours before he fell sick, I find this a surprising and unwarranted conclusion.

<sup>235</sup> P.S. Brachman, S.A. Plotkin et al., "Field evaluation of a human anthrax vaccine"; *Am. J. Pub. Health*; 1962; 52(4); 632-645.

<sup>236</sup> Meryl Nass, "Talk to anthrax vaccine committee, Institute of Medicine, October 3, 2000"; <<http://www.anthraxvaccine.org/IOMtalk.htm>>.

<sup>237</sup> Meeting of the medical committee, U.S. Army Chemical Corps Advisory Council; 11 June 1958, Biological Warfare Laboratories, Fort Detrick, Maryland.

<sup>238</sup> D. MacKenzie, "Anthrax attack bug 'identical' to army strain"; *New Scientist.com news service*; May 9, 2002.

<sup>239</sup> C.V. Harding, D. Harding. W.F. McLimans and G. Rake, "Cytological changes accompanying the growth of poliomyelitis virus in cells of human origin (strain HeLa)"; *Virology*; 1956; 2; 109-125.

<sup>240</sup> I can find no documentary evidence indicating that collaboration between the Wistar Institute and the US Army Chemical Corps extended beyond 1958. Geoffrey Rake died in March of that year, and soon after that the Fort Detrick team discussed bringing in William McLimans from the Wistar to head its virus research programme. It is not known whether a formal offer was made, or if he accepted it, but by the early sixties he was working at the National Institutes of Health.

<sup>241</sup> D.W. Ziegler et al., “The propagation of mammalian cells in a 20-liter stainless steel fermentor”; *App. Microbiol.*; 1958; 6; 306-310.

<sup>242</sup> E.H. Lennette and H. Koprowski, “Human infection with Venezuelan equine encephalomyelitis virus. A report of eight cases of infection acquired in the laboratory”; *J. Am. Med. Assoc.*; 1943; 123; 1088-1095.

<sup>243</sup> *River*, 2000, page 399; see also page 970, notes 3 and 4.

<sup>244</sup> See: J.C.N. Westwood et al., “Transformation of normal cells in tissue culture: its significance relative to malignancy and virus vaccine production”; *Brit. J. Exp. Path.*; 1957; 38; 138-153.

<sup>245</sup> It is now clear that Koprowski did not speak about CHAT and Fox at the *New York Academy of Sciences* meeting in January 1957, although a six-page paper about them did appear in the proceedings that were published that December. Koprowski was at that time President-elect of the Academy. [H. Koprowski, “Discussion”; *Spec. Pub. N.Y. Acad. Sci.*; 1957; 5; 128-133. See also: *River*; 2000; page 969, note 29.]

<sup>246</sup> *River*, 2000, pages 689-690.

<sup>247</sup> Ruth Jervis, personal communication, 1995.

<sup>248</sup> Anon., “‘Live’ polio vaccine aids in Congo epidemic area”; *Science News Letter*; August 9, 1958, page 94.

<sup>249</sup> *River*, 2000, pages 384-387.

<sup>250</sup> J.C.N. Westwood et al., “Transformation of normal cells in tissue culture: its significance relative to malignancy and virus vaccine production”; *Brit. J. Exp. Path.*; 1957; 38; 138-153.

<sup>251</sup> F.W. Sheffield and G.M. Churcher, “The serial propagation of poliomyelitis viruses in cells derived from rabbit embryo kidney”; *Brit. J. Exp. Path.*; 1957; 38; 154-159.

<sup>252</sup> They repeated this, for instance, at the Stanleyville virus symposium in September 1957. [J.P. Delville and S.R. Pattyn, “Epidémiologie de la poliomyélite au Congo Belge et au Ruanda-Urundi. Etat actuel de nos connaissances”; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 283-292.]

<sup>253</sup> L. Hayflick, “The establishment of a human cell line (WISH) of human amnion cells in continuous culture”, *Exp. Cell Res.*; 1961; 23; 14-20. L. Hayflick and P.S. Moorhead, “The serial cultivation of human diploid cell strains”; *Exp. Cell Res.*; 1961; 25; 585-621. Lise Thiry is thanked at the end of the first article for “valuable technical assistance”, and Stanley Plotkin at the end of the second as one of four Wistar scientists who “participated in several of the virus studies”.

<sup>254</sup> M.V. Fernandes, “The development of a human amnion strain of cells”, *Tex. Rep. Biol. Med.*; 1958; 16; 48-58. Intriguingly, both “Fernandes” and WISH transformed into what we can now identify as cell lines on the 35<sup>th</sup> day after being put into culture. Further research into “Fernandes” (now renamed “Amnion Ep. L”) was conducted at the Wistar in the years up to 1960, and revealed it to be almost identical to HeLa. [V. Defendi et al., “Immunological and karyological criteria for identification of cell lines”; *J. Nat. Canc. Inst.*; 1960; 25(2); 359-379.]

<sup>255</sup> P. Gage, personal communication, March 2000.

<sup>256</sup> P. Gage, personal communication, September 2000.

<sup>257</sup> These opportunities comprised two interviews between Dr Osterreith and myself in 1993 and 1994, a letter he wrote me in 1994, two statements he made to Stanley Plotkin in 2000, and his own brief paper delivered in September 2000, and published by the Royal Society in 2001.

<sup>258</sup> The following is an example, this one coming from Robin Weiss’s review of *The River* in 1999: “To me, the possible use of small batches of experimental OPV made locally seems a more plausible source of contamination than the Wistar preparations”. [R.A. Weiss, “Is AIDS man-made?”; *Science*; 1999; 286; 1305-1306.]

<sup>259</sup> There is one point I would like to add, however. In *The River*, I wrote about the death of Dr Osterreith’s son from kidney failure in the 1960s, and speculated that he (like the rest of the family) might have been fed CHAT. In retrospect, this was an error of judgement on my part, and I apologise to Dr Osterreith for it.

<sup>260</sup> John Crewdson, *Science Fictions* (New York: Little, Brown, 2002), p. 184. See also: O. Sattaur, “How Gallo got credit for AIDS discovery”; *New Scientist*, February 7, 1985, pages 3-4; and R. Weiss, “AIDS rivalry” [letter]; *New Scientist*, February 28, 1985, pages 45-46.

<sup>261</sup> There were several other problems highlighted by the testing process, not least the status of the supposedly independent “AIDS/Poliovirus Advisory Committee” which was set up (and presumably

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funded) by the Wistar Institute. It was chaired by a scientist (Dr Claudio Basilico) whose performance at the London meeting was felt by many observers to be anything but detached and independent – in that he acted throughout as if he was representing, and defending, the Wistar. Furthermore, Dr Basilico failed to provide certain basic information, such as the selection criteria used for those CHAT samples that were released.

<sup>262</sup> J. Cohen, “Forensic epidemiology. Vaccine theory of AIDS origins disputed at Royal Society”; *Science*; 2000; 289; 1850-1851.

<sup>263</sup> *River*, 2000, p. 600.

<sup>264</sup> At that point in time, the choice seemed to be between two labs in the US (those of George Jervis at Letchworth Village, and of Koprowski at the Wistar Institute), and three in Belgium (those of Lise Thiry in Brussels, RIT in Genval, and the Rega Institute in Leuven). Almost the last addition that I made to the original (1999) version of *The River* was to insert two pages [*River*, 2000, pp. 789-791] in which I discussed the theoretical possibility that the vaccine could have been made in Stanleyville, even though I did not, at that stage, know of any *persuasive* evidence to support such a scenario.

<sup>265</sup> R.A. Weiss, “Polio vaccines exonerated”; *Nature*; 2001; 1035-1036.

<sup>266</sup> Whether any samples of this vaccine still remain (perhaps in a freezer somewhere in the U.S. or Belgium), or (if they do) whether they would ever be released for independent analysis, are moot points.

<sup>267</sup> J.-P. Vartanian and S. Wain-Hobson, “Analysis of a library of macaque nuclear mitochondrial sequences confirms macaque origin of divergent sequences from old oral polio vaccine samples”; *Proc. Nat Acad. Sci.*; 2002; in press.

<sup>268</sup> S. Sternberg, “Polio vaccine AIDS issue ‘is resolved’”; *USA Today*; May 14, 2002, page 8D.

<sup>269</sup> S. Wain-Hobson, e-mail to author, October 4th, 2001. In this e-mail, Wain-Hobson accuses me of “changing the goal-posts – now the focus is on local amplification”. However, he continues, “this is frequent in science. There is no problem here per se.” Despite the latter comment, seven months later Wain-Hobson was declaring that the theory has been “scotched”, following his testing of CHAT vaccine samples that were prepared, not locally in Africa, but in the USA.

<sup>270</sup> Letter from Dr Stanley Plotkin to author, June 30, 1994.

<sup>271</sup> *River*, 2000, pages 572 and 718.

<sup>272</sup> Anon., “Belgian scientists have high hopes of new vaccine”, *Iraq Times*; March 15, 1959. Courtois’ estimate may have been based on a previous claim, made by Koprowski in 1957, that he could vaccinate a million people with a litre of vaccine. [Anon., “City polio proposal. Talks held on trial of new vaccine in Kenya”; *East African Standard* (Nairobi); February 1, 1957.] Since the typical vaccine dose was a millilitre, Koprowski was apparently building in an amplification/dilution factor of 1,000:1, and Courtois of 2,000:1 (based on a 1959 world population was 4 billion).

<sup>273</sup> Anon., “Expert committee on poliomyelitis. Second report.”; *W.H.O. Tech. Rep. Ser.*; 1958; 145; see especially pages 23-27. Anon., “Expert committee on poliomyelitis. Third report.”; *W.H.O. Tech. Rep. Ser.*; 1960; 203; 1-53; see especially pages 35-38. (Both of these expert committees stated that any suitable monkey species could be used to produce OPVs, employing either trypsinised or Maitland-type cultures.) The second committee sat in July 1957, and lit the blue touch paper for trials in places like Africa, when it declared: “the Committee strongly recommends that controlled field trials be carried out for the purpose of testing further the value of these agents [OPVs]”. It went on to make certain recommendations about conditions under which such field-trials could reasonably be staged. Later, it became clear that several of these recommendations may not have been adhered to by those, like Koprowski, who staged the early trials. (For brief analysis, see *River*, 2000, page 737.) Also see: Anon., “Requirements for Biological Substances. 7. Requirements for poliomyelitis vaccine (oral)”; *W.H.O. Tech. Rep. Ser.*; 1962; 237; 1-29. On pages 13-15 of the latter report it is stated, for the first time, that only monkeys which have not been used for experimental purposes of significance to the safety of the vaccine could be used for vaccine production, and that monkey kidney cultures should not be propagated in series. Monkeys of any suitable species could still be used, but this is the first time that detailed requirements for OPV production were laid down in writing. The WHO study group that produced the latter report sat in November, 1960, so all OPVs produced before that month were, to some extent, experimental.

<sup>274</sup> H. Koprowski, G.A. Jervis, and T.W. Norton, “Oral administration of poliomyelitis virus to man and ape – a comparative study”; *Arch. Ges. Virusforschung*; 1954; 5; 413-424.

<sup>275</sup> *River*, 2000, p. 718.

<sup>276</sup> There are a number of papers from the Stanleyville scientists which report on the occurrence of different arboviruses in both humans and chimps. In one such paper, Dr Osterrieth and colleagues report on the occurrence of Chikungunya in 141 humans and 79 chimpanzees, with roughly half of

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each group coming from the rain forest, and half from the savanna regions to the north. P. Osterrieth et al., “Recherche sur le virus Chikungunya au Congo belge. II. Enquête sérologique”; *Ann. Soc. Belge Med. Trop.*; 1960; 40; 205-213.

<sup>277</sup> L. Hayflick, S.A. Plotkin, T.W. Norton and H. Koprowski, “Preparation of poliovirus vaccines in a human fetal diploid cell strain”; *Am. J. Hyg.*; 1962; 26; 733-743.

<sup>278</sup> For instance: Anon., “7 David’s Island children first test new polio cure”; *Boothbay Register* (Bar Harbor, Maine); July 31, 1958. See also the *Iraq Times* article cited above.

<sup>279</sup> W.C. Osman Hill, “The use of primates in biomedical studies; a review of suitable species”; *Ann. N.Y. Ac. Sci.*; 1969; 162(1); 7-14.

<sup>280</sup> Indeed, this may be what is implied by the WHO technical report of 1962, which says that in future, only monkeys which have not been used for experimental purposes of significance to the safety of the vaccine could be used for vaccine production. [Anon., “Requirements for Biological Substances. 7. Requirements for poliomyelitis vaccine (oral)”; *W.H.O. Tech. Rep. Ser.*; 1962; 237; 1-29; see page 13.]

<sup>281</sup> L.A. Bruggeman, M.D. Ross et al., “Renal epithelium is a previously unrecognised site of HIV-1 infection”; *J. Am. Soc. Nephrol.*, 2000; 11, 2079-2087.

<sup>282</sup> J.P. Moore and R.W. Doms, “Much at stake with kidneys?”; *J. Am. Soc. Nephrol.*, 2000; 11, 2138-2140.

<sup>283</sup> *River*, 2000, p. 569.

<sup>284</sup> J. Beale and F. Horaud, “Polio vaccine and retroviruses”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 841-843.

<sup>285</sup> For details of the CHAT pool 23 protocol, see: *River*, 2000, 702-703.

<sup>286</sup> Cecil Fox, personal communication, June 2002.

<sup>287</sup> P. Ondoa, G. van der Groen, L. Kestens et al., “In vitro replication of SIVcpz is suppressed by beta-chemokines and CD8+ T cells but not by natural killer cells of chimpanzees”; *AIDS Res. Hum. Retroviruses*; 2002; 18(5); 373-382.

<sup>288</sup> J.L. Melnick, “Tissue culture methods for the cultivation of poliomyelitis and other viruses”; in *Diagnostic Procedures for Virus and Rickettsial Diseases*; (NYC: American Public Health Association; 1956); 97-151. The title of Melnick’s chapter is significant, and the content deals with the growth of polioviruses and polio vaccines, rather than relating to “diagnostic procedures”, as per the title of the book. In one place, Melnick discusses which tissues are best for cultivating polioviruses. Having listed a number of human and primate tissues derived from different organs, he comments: “It is difficult to recommend one of these tissues in preference to another, and final choice must depend upon their local availability”.

<sup>289</sup> M.L. Santiago et al., “SIVcpz in wild chimpanzees”, *Science*; 2002; 295; 465.

<sup>290</sup> This is despite the valiant efforts of Bill Hamilton, some of whose chimpanzee stool samples are now, I hear, being reexamined by Dr Hahn with, I believe, interesting results. See also: P.M. Sharp, B.H. Hahn et al., “The origins of acquired immune deficiency viruses: where and when?”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 867-876.

<sup>291</sup> P. Gagneux, M.K. Gonder, T.L. Goldberg and P.A. Morin, “Gene flow in wild chimpanzee populations: what genetic data tell us about chimpanzee movement over space and time”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 889-897.

<sup>292</sup> A. André et al., “Mise en évidence d’antigènes de groupes sanguins A, B, O et Rh chez les singes chimpanzés”; *Ann. Inst. Past.*; 1961; 101; 82-95. See also: *River*, 2000, pp 566-567 about the apparent fate of these chimp blood samples from Lindi.

<sup>293</sup> G. Courtois, “Sur la réalisation d’une singerie des chimpanzés au Congo”; *Symposium international sur l’avenir des animaux de laboratoire*; (Lyon: Institut Pasteur, 1967), pp 235-244. The fact that at least one ape (probably a *Pan troglodytes*, and possibly a *Pan troglodytes troglodytes*) came from Coquilhatville is, I believe, more significant than Ghislain Courtois’ retrospective claim that at Lindi they worked only with *Pan troglodytes schweinfurthii* and *Pan paniscus*, for it is very unlikely that Courtois and colleagues would have been able to distinguish between the two common chimp subspecies, *troglodytes* and *schweinfurthii*, on the basis of physical differences alone.

<sup>294</sup> According to several sources, it was decided at an early stage of the Lindi experiments that because the bonobo population was not hardy (with about half having died “naturally” from disease or stress), the remainder would be “used up” in experiments, and further bonobos would not be procured. When primatologist Adriaan Kortlandt visited Stanleyville in February 1960, Lindi camp had closed a month or two earlier, but had been superseded by a holding facility in a hangar sited behind the medical laboratory. Apparently there were no bonobos among the 60 apes housed there, but Kortlandt was told that a total of 86 bonobos had been procured for the former polio research programme at Lindi, and that all had “died within three weeks”. [*River*, 2000, page 717 and 1026, n. 51.] Although the details given

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to Kortlandt would appear to have been sanitised, his account does give a sense of the speed with which the surviving bonobos may have been utilised. Kortlandt also spoke of an atmosphere of secrecy surrounding both the Stanleyville lab and the chimp research (a detail which has been confirmed by several other visitors).

<sup>295</sup> Anon., “Belgian scientists have high hopes of new vaccine”, *Iraq Times*, March 15, 1959.

<sup>296</sup> There is also another reason. As Bill Hamilton suggested, it may still emerge that an SIV closely related to HIV-1 Group M exists in *Pan paniscus*. To date, very few *Pan paniscus* have been sampled for SIV – and sadly, Bill’s own efforts to obtain such samples appear to have been unsuccessful.

<sup>297</sup> S. Saragosti et al., “Molecular characterisation of primate lentiviruses from a *Cercopithecus wolffi* and a *Cercopithecus ascanius*”; 8<sup>th</sup> International discussion meeting on HIV dynamics and evolution, (Paris, April 2001, abstract p. 19).

<sup>298</sup> J. Kingdon, *The Kingdon field guide to African mammals* (San Diego: Academic Press, 1997), pp 70-75

<sup>299</sup> S. Van Dooren, A-M Vandamme et al., “Lack of evidence for infection with simian immunodeficiency virus in bonobos”; *AIDS Res. Hum. Retro.*, 2002, 18(3), 213-216.

<sup>300</sup> M. Worobey, personal communication, 2001.

<sup>301</sup> This answer does not feature in the published proceedings of the meeting, which do not include the discussion sessions.

<sup>302</sup> M. Peeters, B.H. Hahn, E. Delaporte et al., “Risk to human health from a plethora of simian immunodeficiency viruses in primate bushmeat”; *Emerging Infectious Diseases*; 2002; 8(5); page numbers unknown.

<sup>303</sup> E. Delaporte, M. Peeters et al., “Epidemiological and molecular characteristics of HIV infection in Gabon, 1986-1994”; *AIDS*; 1996; 10; 903-910. See also: J.N. Nkengasong et al., “Genotypic subtypes of HIV-1 in Cameroon”; *AIDS*; 1994; 8; 1405-1412. Although Delaporte referred to “central Africa” as the potential hearth, his supporting references all referred to the west central African countries of the former French Equatorial Africa (plus the related state of Cameroon). For further commentary, see: *River*, 2000, pp. 1064-1065, note 25.

<sup>304</sup> B.H. Hahn, G.M. Shaw, K.M. De Cock, and P.M. Sharp, “AIDS as a zoonosis: scientific and public health implications”; *Science*; 2000; 287; 607-614.

<sup>305</sup> I have been unable to locate Bwamada on the DRC map, though it apparently lies in Equatoria province. It is possible that it is a new name for Mbandaka.

<sup>306</sup> K. Yusim, M. Peeters, E. Delaporte, B. Korber et al., “Using human immunodeficiency virus type 1 sequences to infer historical features of the acquired immune deficiency syndrome epidemic and human immunodeficiency virus evolution”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 855-866.

<sup>307</sup> N. Vidal, M. Peeters, E. Delaporte et al., “Unprecedented degree of HIV-1 Group M genetic diversity in the Democratic Republic of Congo suggests that the HIV-1 pandemic originated in central Africa”; *J. Virol.*; 2000; 74; 10498-10507.

<sup>308</sup> J.L. K. Mokili et al.; “Genetic heterogeneity of HIV-1 type 1 subtypes in Kimpese, rural Democratic Republic of Congo”; *AIDS Res. Hum. Retro.*; 1999; 15(7); 655-664.

<sup>309</sup> Furthermore, it is not impossible that CHAT immunisation took place at Kimpese itself. It has a Baptist-run hospital which, in the fifties, was the major polio rehabilitation centre for the entire Belgian Congo. Elsewhere in the Congo, doctors from the same group of Baptist hospitals conducted their own CHAT vaccination campaigns (as, for instance, around Yakusu, near Stanleyville).

<sup>310</sup> M.L. Kalish, T.M. Folks et al., “Evidence for the presence of all known HIV-1 Group M subtypes and some unclassified strains in Kinshasa, Zaire, in the early to mid-1980s”; 8<sup>th</sup> Conference on Retroviruses and Opportunistic Infections (Chicago, February 2001), abstract 268.

<sup>311</sup> Stanley Plotkin and his team of CHAT vaccinators reported that the 1958 population was 350,000, while Dr Julian De Moor reported that this had risen to 550,000 by 1960, 850,000 by 1962, and would rise to an estimated 1.3 million in 1964. See *River*, 2000, pages 735 and 1034, note 73.

<sup>312</sup> T. Burr, J.M. Hyman, and G. Myers, “The origin of acquired immune deficiency syndrome: Darwinian or Lamarckian?”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 877-888.

<sup>313</sup> A. Rambaut, D.L. Robertson, O.G. Pybus, M. Peeters and E.C. Holmes, “Human immunodeficiency virus: phylogeny and the origin of HIV-1”; *Nature*; 2001; 410; 1047-1048.

<sup>314</sup> E. Holmes, personal communication, October 2000.

<sup>315</sup> The former Group M subtype E has now been reclassified a “circulating recombinant form”, CRF01-AE.

<sup>316</sup> Also based on Andrew Rambaut’s responses to a series of questions in July 2001.

<sup>317</sup> K.M. De Cock, “Epidemiology and the emergence of human immunodeficiency virus and acquired immune deficiency syndrome”, *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 795-798. Of course, as Kevin

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De Cock emphasised, I have only been able to provide a random selection of early cases and infections (generally those which can be traced through the literature, or which have been located through serendipity), and my data only provide a snapshot from the 1950-1980 era. None the less, the data do give some useful indications about where HIV-1(M) was present (and where absent) in these early years. And there has certainly been no conscious “observer bias”, though I suspect he would argue that such bias is usually unconscious. For HIV-1, the data pertain both to those whose blood was sampled in the years up to 1981, and who were only retrospectively checked for HIV-1 infection, and to those who tested HIV-1-positive later than 1981, but for whom it could be proved that infection must have begun by 1981 or before.

<sup>318</sup> B.H. Hahn, G.M. Shaw, K.M. De Cock and P.M. Sharp, “AIDS as a zoonosis: scientific and public health implications”; *Science*; 2000; 287; 607-614.

<sup>319</sup> D.M. Hillis, “AIDS. Origins of HIV”; *Science*; 2000; 288; 1757-1759.

<sup>320</sup> A. Chitnis, D. Rawls and J. Moore, “Origin of HIV type 1 in colonial French Equatorial Africa?”; *AIDS Res. Hum. Retro.*; 2000; 16(1); 5-8. See also the helpful debate about this issue on Jim Moore’s web-site.

<sup>321</sup> D. Low-Beer, “The distribution of early acquired immunodeficiency syndrome cases and conditions for the establishment of new epidemics”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 927-931.

<sup>322</sup> M. Smallman-Raynor, A. Cliff and P. Haggett, *Atlas of AIDS* (London: Blackwell, 1992), pages 332-334.

<sup>323</sup> A. Bobkov, R. Cheingsong-Popov et al., “Identification of an *env* G subtype and heterogeneity of HIV-1 strains in the Russian Federation and Belarus”; *AIDS*; 1994; 8(12); 1649-1655.

<sup>324</sup> Data obtained (for period up to and including 1980) by expressing (a) AIDS cases from Kinshasa, and (b) HIV-1(M) infections from Kinshasa, as a proportion of the total number of AIDS cases/HIV-1(M) infections which can be associated with a specific town, this being 13 of 31 and 21 of 47, respectively.

<sup>325</sup> Data obtained (for period up to and including 1980) by expressing (a) AIDS cases from CHAT vaccination sites, and (b) HIV-1(M) infections from CHAT vaccination sites, as a proportion of the total number of AIDS cases/HIV-1(M) infections which can be associated with a specific town, this being 21 of 31 and 40 of 47, respectively. It is worth adding that every instance of proven Group M infection in Africa up to the end of 1980 comes from a venue that is “within range” (within 225 kilometres, to be precise) of a CHAT vaccination site.

<sup>326</sup> *River*, 2000, pages 529 and 737-739.

<sup>327</sup> Their departures could have been as refugees following the ethnic unrest that began in 1959, or as economic migrants (some of whom had left only a few months before, and some of whom may have been settled outside Ruanda-Urundi for decades).

<sup>328</sup> For the cases in this paragraph, see: *River*; 2000; pages 764-765. In 2000, I too reviewed the Mulago pathology records, and found Bill Hamilton’s “flags” still marking specific cases. Like Bill, I was unable to find the case of *Pneumocystis carinii* pneumonia (PCP) from 1960, which Dr Jack Davies had recalled as Uganda’s “first case of AIDS”. I did, however, find the case in which death was ascribed to pneumonia caused by a “heavy pure growth of *Klebsiella*”, and suspect that this was probably, in reality, the AIDS-like pneumonia which Dr Davies had remembered as the “first case”. The occurrence of this case so soon after the fatal *Klebsiella* cases in Stanleyville is intriguing. I found two small discrepancies from the scribbled notes brought back by Bill in 1995: the 1960 *Klebsiella* case was *not* identified by tribe, and the third case was from 1965, not 1960-1961 as previously reported. (However, the case is still a potential instance of HIV infection via CHAT, for the boy in question may have been infected perinatally by a previously vaccinated parent.)

<sup>329</sup> J. Sonnet et al., “Early AIDS cases originating from Zaire and Burundi (1962-1976)”; *Scand. J. Inf. Dis.*; 1987; 19; 511-517. Also see: *River*; 2000, pp. 259-261.

<sup>330</sup> *River*; 2000, page 288.

<sup>331</sup> An article in a local Stanleyville paper from August 1957 refers to the hospital for blacks as being in “the sad state which everyone knows about – it is a collection of buildings which was suitable thirty years ago”, and goes on to claim that the hospital for Europeans is “falling into ruin”. Anon., “Toujours l’hôpital”; *Le Stanleyvillois*; August 19, 1957, page 1.

<sup>332</sup> S.A. Plotkin, “CHAT oral polio vaccine was not the source of human immunodeficiency virus type 1 group M for humans”; *Clin. Inf. Dis.*, 2001, 32, 1068-84; see p. 1076.

<sup>333</sup> Anon., “Belgian scientists have high hopes of new vaccine”, *Iraq Times*, March 15, 1959. An identical version of the article, entitled “Congo may lead world in fight against polio” was published in the *Uganda Argus* (Kampala), at around the same date, and a briefer version (“New polio vaccine in Congo”) appeared in the *East African Standard* (Nairobi).

- <sup>334</sup> P. Osterrieth, "Propriétés biochimiques des Klebsiella", *Ann. Soc. Belge Med. Trop.*; 1958; 38; 721-730.
- <sup>335</sup> P.N. Fultz et al., "Identification and biological characterization of an acutely lethal variant of simian immunodeficiency virus from sooty mangabeys (SIV/SMM)"; *AIDS Res. Hum. Retro.*; 1989; 5(4); 397-409. See also: U.S. Army Medical Research Institute of Infectious Diseases [USAMRIID], Fort Detrick, Frederick MD, Annual Report FY 1989, pp 189-193.
- <sup>336</sup> The fact that Osterrieth apparently sometimes used different syringes for different chimps indicates that keeping sera separate was seen to be important, at least on occasions, but it is also possible that those particular blood extractions were for diagnostic work rather than for tissue culture preparation.
- <sup>337</sup> Melnick, "Tissue culture methods for the cultivation of poliomyelitis and other viruses"; page 117.
- <sup>338</sup> Melnick's description of making poliovirus pools in HeLa cells specifies that for the growth medium, "serum pools obtained from 2 to 5 human donors are made, to avoid the use of serum deficient in antitryptic effect". See: Melnick, "Tissue culture methods for the cultivation of poliomyelitis and other viruses"; page 132.
- <sup>339</sup> One other example: in 1999, Florian Horaud told Simon Wain-Hobson of how in the fifties, in Romania, 150 rhesus monkeys destined for polio vaccine production in Romania were gang-caged together, and added how horrified he felt, looking back, at the risks that he had once taken. As for official recommendations, it was not until 1962 that the WHO proposed that monkeys used for the production of polio vaccine should be kept in cages, two to a cage, and that "cage-mates [should] not be interchanged". See: Anon., "Requirements for Biological Substances. 7. Requirements for Poliomyelitis Vaccine (Oral)"; *WHO Tech. Rep. Ser.*, 1962, 237, 1-29, see page 14.
- <sup>340</sup> S. Wain-Hobson, personal communication, December 1999.
- <sup>341</sup> D.P. Wooley, R.A. Smith, S. Czajak and R.C. Desrosiers, "Direct demonstration of retroviral recombination in a rhesus monkey"; *J. Virol.*; 1997; 71(12), 9650-9653. The article refers to retroviral recombination in tissue culture systems (generating doubly infected cells), and reported on recombination in a rhesus monkey that had been artificially infected with two different SIV strains.
- <sup>342</sup> Sharp et al., "The origins of acquired immune deficiency viruses: where and when?"; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 867-876. K. Yusim et al., "Using human immunodeficiency virus type 1 sequences to infer historical features of the acquired immune deficiency syndrome epidemic and human immunodeficiency virus evolution"; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 855-866. M. Salemi, K. Strimmer, W.W. Hall, M. Duffy, E. Delaporte, S. Mboup, M. Peeters and A.-M. Vandamme, "Dating the common ancestor of SIVcpz and HIV-1 Group M and the origin of HIV-1 subtypes by using a new method to uncover clock-like molecular evolution"; *FASEB J.*; 2000. Meanwhile, the phylogenetic dating argument continues to be lauded in the mainstream press. See, for instance, Gina Kolata, "The Genesis of an Epidemic: Humans, Chimps and a Virus", *New York Times*, September 4, 2001, which claims that these doctors have "unravel[led] the mystery of the origin of the epidemic".
- <sup>343</sup> In his Royal Society article, Paul Sharp wrote that my interpretation of the impact that recombination might have on dating was "strange", because "it is clear that recombination would make the date of the common ancestor seem more recent". [Sharp et al., "The origins of acquired immune deficiency viruses: where and when?"; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 867-876.] But what is Sharp's evidence for this claim? An increasing number of articles are being published which conclude that he is wrong in this assumption, and that ignoring recombination can lead to either under-estimation or over-estimation of the MRCA. See, for instance, recent articles by Mikkel Schierup and Michael Worobey, cited immediately below. One sceptical geneticist states that all the work done on dating the Group M epidemic up to now has been a "dog's breakfast".
- <sup>344</sup> M.H. Schierup and R. Forsberg, "Recombination and phylogenetic analysis of HIV-1"; in *Proceedings of the conference: origin of HIV and emerging persistent viruses*; (Rome: 2003; 231-245).
- <sup>345</sup> M.H. Schierup and J. Hein, "Recombination and the molecular clock" [letter]; *Mol. Biol. Evol.*; 2000; 17(10); 1578-1579. See also: M.H. Schierup and J. Hein, "Consequences of recombination on traditional phylogenetic analysis"; *Genetics*; 2000; 156; 879-891.
- <sup>346</sup> M. Worobey, "A novel approach to detecting and measuring recombination: new insights into evolution in viruses, bacteria and mitochondria"; *Mol. Biol. Evol.*; 2001; 18(8); 1425-1434.
- <sup>347</sup> K. Yusim et al., "Using human immunodeficiency virus type 1 sequences to infer historical features of the acquired immune deficiency syndrome epidemic and human immunodeficiency virus evolution"; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 855-866.
- <sup>348</sup> T. Zhu, B.T. Korber, A.J. Nahmias, E. Hooper, P.M. Sharp and D.D. Ho, "An African HIV-1 sequence from 1959 and implications for the origin of the epidemic"; *Nature*; 1998; 391; 594-597. [See also: *River*; 2000; 775-777.] The short ZR59 sequence is used by some phylogeneticists as a check on

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the accuracy of their dating calculations. However, I hear from “inside sources” that the scientist at the Aaron Diamond Center who analysed ZR59 “was having a most difficult time with the sequence”.

<sup>349</sup> A.G. Motulsky, J. Vandepitte and G.R. Fraser, “Population genetic studies in the Congo. I. Glucose-6-phosphate dehydrogenase deficiency, hemoglobin S, and malaria”; *Am. J. Hum. Gen.*; 1966; 18(6); 514-537. See also following papers in same volume, on pp. 538-545, 546-552, and 553-558.

<sup>350</sup> E.R. Giblett, A.G. Motulsky and G.R. Fraser, “Population genetic studies in the Congo. IV. Haptoglobin and transferrin serum groups in the Congo and in other African populations”; *Am. J. Hum. Gen.*; 1966; 18(6); 553-558.

<sup>351</sup> S.A. Plotkin, A. Lebrun, G. Courtois and H. Koprowski, “Vaccination with the CHAT strain of type 1 attenuated poliomyelitis virus in Leopoldville, Congo”; *Bull. W.H.O.*; 1962; 24; 785-792.

<sup>352</sup> *River*, 2000, pages 827-836.

<sup>353</sup> The abbreviations AEF and AOF refer to the more commonly-used French acronyms for *Afrique Équatoriale Française* (French Equatorial Africa) and *Afrique Occidentale Française* (French West Africa), respectively.

<sup>354</sup> *River*; 2000; pages 853-861.

<sup>355</sup> However, in a second interview with Wain-Hobson (given after he had spoken with some of the other former Pasteur workers, including the late Florian Horaud, who was apparently a friend of Stanley Plotkin), this man apparently retracted on the latter piece of information – saying that he had used only IPV in Brazzaville. What is not disputed, however, is that he administered both OPV and IPV in French Equatorial Africa between 1957 and 1959.

<sup>356</sup> S. Wain-Hobson, personal communications, October to December 1999. Dr Wain-Hobson was kind enough to give me copies of his notes from these (and other) interviews with former Pasteur Institute employees, and I would like once again to thank him for sharing this information. What is not clear, however, from these notes is whether he specifically asked this doctor whether he had grown *polio vaccine* in locally-prepared cultures. When I review my own notes of previous conversations with Wain-Hobson in September and October 1999, it is clear that he suspected that the French vaccine had been locally prepared, so it seems strange that when he came to interview one of the key doctors involved, he merely recorded that he “grew polio on local monkey kidney cultures”, without specifying whether or not this meant vaccine amplification. (I was also remiss for not pursuing this more rigorously with him.) I now wonder whether this rather ambiguous phrase in the written notes which he gave me in December 1999 indicates that he was already becoming uneasy about some of the implications of his findings, and was not wishing to put anything too damaging on the record.

<sup>357</sup> *River*, 2000, pp. 852-858.

<sup>358</sup> S. Wain-Hobson, personal communication, October 2001.

<sup>359</sup> L-J André and E. André-Gadras, “16 cas de poliomyélite observés dans un district de brousse de Gabon” [sic]; *Med. Trop.*; 1958; 18; 638-641. The vaccination is mentioned only briefly at the end of the article.

<sup>360</sup> P. Lépine, “Discussion”; *Rev. Lyonnaise de Med.*; 1959; 8; 46. (See also the preceding speech by Hilary Koprowski about the Congo vaccinations, on pages 39-40.)

<sup>361</sup> L.J. André, “Le S.I.D.A., a-t-il déjà existé?”; *Med. Trop.*; 1987; 47(3); 229-230.

<sup>362</sup> P. Lépine et al., “Présence apparemment insolite et conservation de microfiliaries du singe dans les cultures de tissus”, *Bull. Soc. Path. Exot.*; 1955; 48; 838-843.

<sup>363</sup> *River*, 2000, pages 827-835.

<sup>364</sup> B.A. Castro et al., “Persistent infection of baboons and rhesus monkeys with different strains of HIV-2”; *Virology*; 1991; 184; 219-226.

<sup>365</sup> *River*; 2000; pages 338-346.

<sup>366</sup> See figure in *River*; 2000; page 625.

<sup>367</sup> As commentators like Tom Schulz have acknowledged, the fact that the major pandemic may have had an iatrogenic origin would not *require* that the minor outbreaks also had an iatrogenic origin. My personal position at present is that either theory of origin (natural or iatrogenic) is tenable for HIV-2, and for HIV-1 Groups O and N, not least because it is possible that the minor outbreaks were only recognised by the medical community because of the research that had already taken place in response to the Group M-related pandemic.

<sup>368</sup> Transcript of relevant passage from Chuck Cyberski’s interview with Leonard Hayflick, recorded May 7<sup>th</sup>, 1992 at UCSF in San Francisco, and made available by Blaine Elswood.

<sup>369</sup> *River*, 2000, page 487. See also page 488, where Hayflick admits that Koprowski had done “things behind my back which I didn’t appreciate”, which included “something which I thought was underhand”. One wonders what these “things” might have been, and whether they included the early testing in Africa of vaccines made in experimental substrates, such as human cells.

<sup>370</sup> It was Elswood who, back in 1991, had initially alerted Tom Curtis to the potential importance of the CHAT vaccine trials in Africa.

<sup>371</sup> Letter from L. Montagnier to H. Koprowski, February 12<sup>th</sup>, 1992, made available by the Albert B. Sabin archives at the University of Cincinnati.

<sup>372</sup> *River*, 2000, pages 802-804.

<sup>373</sup> For the local preparation of Sabin's strains in the Soviet Union, see: A.A. Smorodintsev et al., "Experimental and epidemiological data on the effectiveness of live poliomyelitis vaccine. Part 1. Experience in the production, biological control, and use of live poliomyelitis vaccine made from the Sabin strains", in *First international conference on live poliovirus vaccines: papers presented and discussions held* (Washington D.C., June 22-26, 1959), Pan American Sanitary Bureau, 1959, Scientific publication No. 44; pages 305-312. This reveals that the first batches of Sabin's vaccines were further attenuated and then produced in rhesus macaque tissues in the USSR in 1957, and that further batches were prepared in the tissues of other Asian monkeys in 1958-9. The local preparation of polio vaccines in South Africa was being reported in the literature by 1956 (for Salk's IPV) and 1959 (for Sabin's OPV). [J. Gear, "The South African poliomyelitis vaccine"; *S. Af. Med. J.*; 1956; 30; 587-594. Anon., The South African Institute for Medical Research, annual report, 1959, pages 134-135.] For a summary of the South African trials, see: Anon., "Immunization against poliomyelitis"; *S. Af. Med. J.*; 1963; 37; 497-498, and the equally important articles on the following pages: 499-518.

<sup>374</sup> "Meretricious" is defined in the Cassell Popular English Dictionary as: "pertaining to or befitting a prostitute, alluring by false or empty show; unreal, tawdry."

<sup>375</sup> S.A. Plotkin, "CHAT oral polio vaccine was not the source of human immunodeficiency virus type 1 Group M for humans"; *Clin. Inf. Dis.*, 2001, 32, 1068-84.

<sup>376</sup> W.D. Hamilton, "Foreword"; *The River*, 2000, pages xxvii-xxxiii.

<sup>377</sup> Dr Weiss added that this was an inversion of the original phrase, which involved "ugly facts" destroying a "beautiful theory". I have since been told that the original phrase may have first been coined by Thomas Henry Huxley, but have been unable to confirm that.

<sup>378</sup> B. Lederer, "Chimp and see"; *POZ*; December 2001, p. 17.

<sup>379</sup> Anon., "Protest is an ally of science" [editorial]; *Independent on Sunday* (London); May 26, 2002.

<sup>380</sup> J. Crewdson, *Science fictions. A scientific mystery, a massive cover-up, and the dark legacy of Robert Gallo* (Boston: Little, Brown, 2002), p. 627.

<sup>381</sup> J. Creamer (ed.) *Biohazard: the silent threat from biomedical research and the creation of AIDS* (London: National Anti-Vivisection Society, 1987).

<sup>382</sup> The man who sent the letter was a Dr Herbert Ratner, from Oak Park, Illinois, and he included various articles; [such as: H. Ratner, "Monkey viruses, AIDS and the Salk vaccine, Parts I and II"; *Child & Family*; 1988; 20; 134-138; see also *River*; 2000; pp. 236-237, and notes 1 and 2 on page 951.] This was the same Herbert Ratner who, in 1955, had reservations about the safety of the Salk vaccine, and refused to inject it into his patients. His caution seemed well justified a few days later, when the Cutter incident showed that some batches of that vaccine had been incompletely inactivated. Ratner's samples of the Salk IPV stayed in his freezer for more than 40 years, but just before his death he gave them to Michele Carbone. Dr Carbone (who had been unable to procure such samples from the FDA, which had apparently destroyed all its ancient stocks in the early 1990s, officially as a cost-cutting measure; see: *River*, 2000, page 866) tested the Ratner samples, and found them positive for SV-40. So despite being wrong about the origins of AIDS, Dr Ratner has none the less played a heroic role in protecting the public health.

<sup>383</sup> T. Curtis, "The origin of AIDS: a startling new theory attempts to answer the question: 'Was it an act of God, or an act of man?'" *Rolling Stone*; March 19, 1992; issue 626; pp. 54-60, 106 and 108.

<sup>384</sup> J. Cribb, *The White Death* (Sydney: Angus and Robertson, 1996); see pp 254-257 for the Hamilton letter to *Science*, and 182-184 for Cribb's analysis.

<sup>385</sup> Extract from referee's comments on Hamilton's letter, forwarded by editorial staff at *Science* to W.D. Hamilton on April 28<sup>th</sup>, 1994. The typed text at this point reads "answered", but an unknown hand has written "un?" beside the word, and it seems clear from the context that the writer [i.e. R. Weiss] meant "unanswered".

<sup>386</sup> *River*, 2000, page 877.

<sup>387</sup> For background to this episode, see *River*, 2000, pp. 508-511.

<sup>388</sup> E-mail from R.A. Weiss to B. Martin, June 8, 2000.

<sup>389</sup> S. Wain-Hobson and R.A. Weiss, "If free speech costs lives that's a high price to pay" [letter], *Nature*, 2000, 407, 834.

<sup>390</sup> Dephlogistication: a "depriving of phlogiston", here in the figurative sense.

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<sup>391</sup> S. Wain-Hobson, *The River – a journey back to the source of HIV and AIDS* (review); *Nature Medicine*; 1999; 5(10); 1117-1118.

<sup>392</sup> *River*, 2000, pp. 852-858.

<sup>393</sup> A brief search in *Nature* has not revealed the letter in question. Possibly it ended up as the “If free speech costs lives...” letter cited above, and was finally signed not by fifty scientists, but by just the two of them.

<sup>394</sup> Letter from S. Wain-Hobson to W. Nelson-Rees, October 19, 2001. This rather crude attempt to separate Nelson-Rees and myself is especially surprising, given that Nelson-Rees had ended his Royal Society talk by frankly stating that there was “no logical reason” why chimp cells would not have been used (either at the Wistar or elsewhere) to grow the CHAT vaccine used in Africa, and that the prevailing custom in the fifties was to use cells “about which little or nothing was known, except that they could optimally support the growth of a given virus”. The fact that two letters have been written by the Royal Society organisers seeking to persuade others to modify their positions on (or public statements about) the OPV debate (one to Brian Martin from Robin Weiss, and one to Walter Nelson-Rees from Simon Wain-Hobson) suggests that other, more successful approaches may have been made to other scientists.

<sup>395</sup> B. Martin, “The burden of proof and the origin of the acquired immune deficiency syndrome”; *Phil. Trans. R. Soc. Lond B*; 2001; 356; 939-946.

<sup>396</sup> B. Martin, “The politics of a scientific meeting: the origin-of-AIDS debate at the Royal Society”; *Politics and Life Sciences*; 2003; in press. See also: B. Martin, “Investigating the origins of AIDS: some ethical dimensions”; *J. Med. Ethics*; forthcoming.

<sup>397</sup> Bill Hamilton apparently wrote this in a letter to Professor Patrick Bateson, secretary of the Biology section of the Royal Society, in around June 1999.

<sup>398</sup> The geographer, Daniel Low-Beer, did make a significant and more open-minded contribution. However, since he was only a “discussant”, not a full speaker, he was allotted only seven or eight minutes on the second day. None the less, he had time to make some interesting points – notably the very short average distances from CHAT vaccination sites to sites of AIDS cases, especially at the start of the epidemic. He also pointed out that if Kinshasa really was a hub, it was hard to explain why HIV-1 spread had only taken place eastwards from there, and not to the north or south. D. Low-Beer, “The distribution of early acquired immunodeficiency syndrome cases and conditions for the establishment of new epidemics”; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 927-931.

<sup>399</sup> The association between Plotkin and Moore had been rumoured for several months before Plotkin thanked Moore, at the end of his article in *Clinical Infectious Diseases*, for “lending me some of his courage to face defamatory accusations”. Apart from the instances already detailed in the first section of this paper, Moore’s “courage” has apparently involved approaching several of those who had seen merit in the OPV theory, sometimes sending them abusive letters, and sometimes inviting them to friendly meetings, because he happened to be about to visit their part of the country. According to several sources, Moore was also actively engaged trying to persuade people not to attend the Royal Society meeting. He publicly declared that he himself would not be attending, having “more important things to do” (but neglecting to add that he had only been invited as discussant, not as a speaker). Later, however, when he realised that most key figures in the debate were going, he after all found the time to attend. I myself received two unsolicited e-mails from Moore after he read my description of his *Nature* review of *The River* as “scurrilous” [*River*, 2000, pp. 851-852]. They opened by accusing me of being a “tenth-rate journalist”, and went on from there. One of these e-mails was copied to doctors Weiss and Wain-Hobson, and Robin Weiss subsequently offered to tell Moore (a former pupil of his) to “cool it”. I thanked Weiss, but told him that I would prefer him not to intervene. Since then, however, I have not heard further from Moore, (although I am told that he did ask me a question from the floor during the Royal Society meeting). I believe that it is important that people are able to see how Dr Moore operates, and for this reason his two letters to me will be posted on Brian Martin’s web-site. All in all, Moore seems to have acted from the outset as a spin doctor for Plotkin and Koprowski and the natural transfer school, and one wonders what his motivation, or incentive, may have been.

<sup>400</sup> Brian Martin’s web-site, at <<http://www.uow.edu.au/arts/sds/bmartin/dissent/documents/AIDS/>>.

<sup>401</sup> At one point, Plotkin even claimed that I had wrongly included two cases of AIDS from Kikwit in my analysis of early cases, and produced a map to support this claim; unfortunately, the two cases did not appear in the map in my book, but only in his version of it!

<sup>402</sup> Of particular interest is the letter from Dr Koprowski to George Jervis in the Congo, dated March 4, 1958, of which only the postscript is quoted. Plotkin states that there is “no reference to local production in the Congo”, but for obvious reasons, it would be valuable to be able to see the rest of the letter.

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<sup>403</sup> These remarks were spoken by Dr Weiss in his closing address at the Royal Society meeting, but do not feature in the published version of his speech.

<sup>404</sup> J. Cohen, "Disputed AIDS Theory Dies its Final Death", *Science*, 2001; 292; 615. I had had dealings with Jon Cohen in the summer of 2000, when I spent (I estimate) a total of five or six hours on the phone answering his questions while he was preparing two big feature articles for *Science* and *The Atlantic Monthly*. Rather to his chagrin, I sensed, he was unable to catch me out on any awful blunders, or devious motives. (At one point, he even told me that some scientists feared that I might be able to out-argue them at the London meeting, because I had spent so much time devoted to the subject – as if this, somehow, gave me an unfair advantage.) The article that Cohen wrote for *Science* was largely fair. [J. Cohen, "Forensic epidemiology. Vaccine theory of AIDS origins disputed at Royal Society"; *Science*; 2000; 289; 1850-1851.] Later, however, came the *Atlantic* piece, which rather cravenly reported the words of OPV sceptics like Beatrice Hahn, and which performed a gentle but none the less determined assassination job on me. [J. Cohen, "The hunt for the origin of AIDS"; *Atlantic Monthly*; October 2000.] After that, I decided to cross Cohen off my greetings card list.

<sup>405</sup> Another person who was less than happy with the published proceedings of the Royal Society was Walter Nelson-Rees. Certain changes were made to his copy, allegedly for legal reasons. The fact that one scientist had repeatedly lied about the origin of his cells was omitted, even though this information had already appeared in a 1981 article in *Nature*. [D. Dickson, "Contaminated cell lines"; *Nature*; 1981; 289; 227-228.] And an extra sentence was inserted before one of Nelson-Rees's paragraphs which quoted John Maddox, the former editor of *Nature*, who ended a 1981 editorial on the topic as follows: "There is no reason to suppose that the few cases of dishonesty that have come to light are in any sense the tip of an iceberg....It would be tragic if civilised habits [of honesty in research] were to be corrupted by the activities of self-appointed vigilantes." [J. Maddox, "Responsibility for trust in research", (editorial); *Nature*; 1981; 289; 212-213.] This appeared to be a fairly frank closing condemnation of whistle-blowers like Nelson-Rees. The sentence that was inserted at the start of Nelson-Rees' paragraph in the Royal Society proceedings read as follows: "Editors of scientific journals do have to strike a balance between undue criticism of people's ethics, and uncritical acceptance of their results." Nelson-Rees wrote to the editorial office of the Royal Society, protesting the changes which had been made, and observing that "the points I am making are precisely the sorts of items I had intended to talk (write) about under the title of 'Responsibility for Truth in Research'." He eventually accepted the changes, but only "reluctantly". [W. Nelson-Rees, "Responsibility for Truth in Research"; *Phil. Trans. R. Soc. Lond. B*; 2001; 356; 849-851.] Nelson-Rees points out that Sir John Maddox is on the editorial board of the Royal Society journal which published the proceedings, "Philosophical Transactions B". [W. Nelson-Rees, personal communications, May 2002.]

<sup>406</sup> Found at <<http://www.uow.edu.au/arts/sds/bmartin/dissent/documents/AIDS/>>.

<sup>407</sup> W.D. Hamilton, "Foreword"; *The River*, 2000, pages xxvii-xxxiii.

<sup>408</sup> T. Radford, "Britain losing its scientific grip, says PM", *Guardian* (U.K.); May 24, 2002, page 13.

<sup>409</sup> This sentence was updated to July 2002.

<sup>410</sup> This is my recollection of the figure that Dr Maddox suggested, though I see that in an account of this episode penned only a couple of weeks after it took place, I wrote "100 million". It hardly matters, for whichever it was, it was a significant number of millions.

<sup>411</sup> R. Weiss, "Reflecting on the origin of human immunodeficiency viruses"; *AIDS and Hepatitis Digest*, January 2002, page numbers unknown, but available on Brian Martin's web-site..

<sup>412</sup> *River*, 2000, pp. 384-387. The rest of Chapter 28 contains other similar examples.

<sup>413</sup> G. Courtois, H. Koprowski et al., "Preliminary report of mass vaccination of man with live poliomyelitis virus in the Belgian Congo and Ruanda-Urundi"; *Brit. Med. J.*; 1958; 2(i); 187-190. This is the paper which was, in reality, written by Koprowski, and merely sent to Stanleyville for checking.

<sup>414</sup> G. Courtois, "Vaccination antipoliomyelitique par virus vivant au Congo Belge"; *Ann. Soc. Belge Med. Trop.*; 1958; 38; 805-816.

<sup>415</sup> Letter from W. Bervoets to P. De Brauwere, September 17<sup>th</sup>, 1958; file H4484/1058 at the Belgian Ministry of Foreign Affairs archives.

<sup>416</sup> *River*, 2000, pages 221-222, 416 and 734-736.

<sup>417</sup> An additional clue that Koprowski was aware of at least some of the data that Bervoets reported to De Brauwere in his letter of September 17<sup>th</sup> is that although Koprowski records 2,433 vaccinations at "Bambesa and Kule-Ponge", he mentions "2,350 inhabitants" for Bambesa in the text. 2,350, according to Bervoets, is the number who were actually vaccinated at Bambesa, although it was not the full population of the village.

<sup>418</sup> This lack of candour may once again be in evidence in Koprowski's vague claim in his *BMJ* article that, in Stanleyville, "a large group of schoolchildren, mostly European in origin, were vaccinated with

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living virus every week during...the 12 months preceding the mass trial at Ruzizi Valley”. None of the Belgians I spoke to could recall such an early and ongoing vaccination of Europeans in Stanleyville, and the Bervoets letter states simply that overall, the vaccinations had included “a certain number of European volunteers”. I believe that Koprowski’s version of events represents, at the least, a substantial exaggeration.

<sup>419</sup> *River*, 2000, pp 864-865, and see note 81 on page 1066.

<sup>420</sup> The university at Leuven, and the affiliated Rega Institute, had been directly involved with the original CHAT research through the virologist Dr Pieter de Somer.

<sup>421</sup> I have a couple of suggestions to make of “neutral” institutions, where scientists are known to be interested in participating in testing the Kisangani samples.

<sup>422</sup> Dephlogistication: the relieving of inflammation.

<sup>423</sup> Despite the comments made in this paper, I find it difficult not to feel some real sympathy for the position of Dr Paul Osterrieth, not least because it is clear that whatever was done in Stanleyville was very likely done under instructions from “superiors”. However, I do believe that it is now time for him to give a full and candid account of what happened at Lindi and in Stanleyville. This might well stir up a hornet’s nest, but I believe that the courage and integrity that such a baring of the soul would require would also engender a good deal of respect, understanding and support from his peers.

<sup>424</sup> P. Osterrieth, “The truth is what happened and not what one wishes that had happened”, November 20, 2001. Available at: <http://www.aegis.com/news/misc/2001/PO011101.html>

<sup>425</sup> The epigram to Dr Osterrieth’s disclaimer seems rather a strange choice. It reads: “The point is that creationists and social critics who decry science as dogmatic obedience to authority and old-boys networks of closed-minded fogies are simply mistaken”, and it comes from an article by Michael Shermer in the October 2001 edition of *Scientific American*, which was entitled “I was wrong”.

<sup>426</sup> A. Jung, A. Meyerhans, et al., “Multiply infected spleen cells in HIV patients”; *Nature*; 2002; 418; 144.

<sup>427</sup> J. Cohen, “Tough challenges ahead on political and scientific fronts”; *Science*; 2002; 297; 312-313.