The Friend leukaemia virus and Etienne de Harven

In 1957 Charlotte Friend was able to transmit, in mice, “a disease having the characteristics of leukaemia” by using cell-free filtrates obtained from the spleen of leukaemic mice. (Friend, J. Exp. Med. 1957; 105:307). In 1958 de Harven and Friend reported the finding in the filtrates of “virus-like” particles. “Particles were found in about one-quarter of the examined specimens from leukaemic mice, and were never observed in non-leukaemic mice of the same strain.” Among the electron micrographs, one (Fig. 5) shows a cell with what appears to be a bud, which they called “pseudopod” (de Harven et al 1958, J. Biophys. Biochem. Cytol., Vol. 4). Since the injected filtrates transmitted “a disease having the character of a leukaemia” (because the filtrates were transmitting the disease especially a malignancy, it does not mean that the filtrates contained a virus, Rous pointed this out in 1911) and the particles were not seen in the non-leukaemic mice, in a paper published in 1960 de Harven and Friend arbitrarily decided to call the particle “virus” particles instead of “virus-like”. “The particles, however, will be referred to as “viruses” and no longer as “virus-like” since all specimens were checked for infectivity and proved capable of transmitting the disease” (“providing that they have been inoculated into highly susceptible inbred mice”). The only evidence that they had for “infectivity” was the transmission of the disease to “highly susceptible inbred mice” by the filtrate. “The virus observed in the leukaemic material under study is ***considered*** to be the etiological factor responsible for the induction of the leukaemia in the mice” (emphasis ours). Describing their electron microscopy finding they wrote: “In many cases the viruses are in intimate contact with cell membranes of leukemic cells, suggesting that the virus particle is formed at the level of the cell membranes by a budding process…The virus of Gross’s leukemia appears to be morphologically similar to the one described in the present paper. The budding phenomenon of viruses along cell membranes has also been described for several other viruses”. (de Harven et al, J. Biophys. Biochem. Cytol. 1960, Vol. 7; 747-).

In 1964 de Harven reported the finding of similar particles in the thymus of healthy “conventional and germ-free mice”. And although

he had no evidence that they were transmitting any disease, it was concluded that they were viruses. "The particles described here do not resemble any known cellular component….Furthermore, these particles are identical in their dimensions, fine structure, and localizations to several viruses, and especially those associated with the murine leukemias, the physiochemical properties of which are very similar to those of well known infectious viruses. Therefore, it seems highly probable that the particles observed in the thymus of both conventional and germ-free mice are indeed viruses. It follows that the germ-free mice subjected to our investigation were not virus-free." (de Harven, J. Exp. Med. 1964; 120). So in 1960 he claimed that the virus-like particles were actually virus particles because they transmitted the disease and were not found in non-leukaemic mice. In 1964 he claimed the virus-like particles found in non-leukaemic mice were viruses because they were identical to those found in leukaemic mice.

In 1965 de Harven published a paper with electron micrographs of purified "Friend leukemia virus". Discussing the method used for purification he wrote: "The first successful purification of a murine leukemia virus from the blood of leukemic mice was reported by Moloney and Dalton. The technique described by these authors has been recently combined with density gradient centrifugation and a very successful purification of the Rauscher viruses has been achieved. Our method of purification of the Friend virus was originally derived from that recommended by Moloney and Dalton…Some modifications of the technique proposed by Moloney and Dalton have made easy the purification of Friend virus from the plasma of leukemic DBA/2 mice. An isotonic medium was used to dilute the blood, and millepore filters were used to remove from the plasma all cellular debris of a size superior to that of the virus. It is also necessary to fix the viruses with osmium tetroxide before the negative staining technique is applied." In this paper he stated that "distorted viruses retained an almost unchanged biological activity".

Furthermore, "Murine leukemia viruses do not show any surface subunits and they have not been shown to contain internal components with cylindrical symmetry. It might be, therefore, that

murine leukemia viruses constitute a group of agents without a precise equivalent in classical virology." (de Harven, Winster Institute 1965; Path. Biol. 1965).

Note: In not one of Etienne de Harven's publications, or anybody else's, is there evidence which proves transmission of either the particles or the disease. Yet de Harven's particles are accepted by everybody to be the Friend leukaemia virus. So is his claim that he purified the Friend leukaemia virus.

Summary: Charlotte Friend obtained cell-free filtrates from the spleen of leukaemic mice. When the filtrates were injected into "highly susceptible inbred mice", the recipient mice developed a similar disease. In the same filtrates, de Harven and Friend found some particles and called them "virus-like" particles. With no further evidence the virus-like particles were renamed virus particles and claimed to be the cause of the disease. A claim which apparently is accepted by everybody.

At the Rethinking AIDS website it is stated that Etienne de Harven MD, "isolated and obtained the first electron microscopic studies of the Murine Friend leukemia virus, and retroviral budding". In the same site, one reads that "he produced the first electron microscopic studies of a retrovirus". As Peter repeatedly pointed out, retrovirologists are a small minority even among virologists. They all know each other's and everybody else's contribution to retrovirology. This means that at least the main experts in the "HIV" field such as Gallo and Montagnier are aware of de Harven's contribution to retrovirology. The questions arise: is the evidence for the existence of the "Friend leukemia virus" and its causative role in leukemia better than that for the existence of "HIV" and its causative role in AIDS? If not, what weight will de Harven's evidence have in a hearing which questions the existence of "HIV" or a hearing which questions its role in AIDS?

One argument against the "HIV" theory, particularly stressed by Peter, is that while all viruses are said to cause a given disease "HIV" is supposed to cause about 30 diseases. However, de Harven does not

exclude such possibility “whether the same agent is enclaved with a large spectrum of pathogenic potentialities, or whether different viruses display very similar morphology remains a central problem”. (J. Exp. Med. 1964). “Up to what extent these agents are responsible only for neoplastic diseases in mice or also for murine infectious conditions still awaits further investigation”. (Winster Institute).

Another argument against the “HIV” theory of AIDS, again particularly stressed by Peter, is the long period between “HIV” infection and the development of AIDS. However, in 1964 de Harven wrote: “It follows that when the electron microscope observation of a cell reveals no demonstrable pathological change, the cells may however, be heavily infected by a virus which is only going to slow up some time later”. “The particles present in the thymus of conventional and germ-free mice are indistinguishable from those associated with several types of mouse leukemia and similar to those of mammary tumors. The fact that they have been observed in apparently healthy animals is not surprising, since “leukemic” viruses presumably exist in many normal mice, without causing disease”. Moreover, we do not know how many of our mice would have developed “spontaneous” leukemia if they had been allowed to live longer. An extremely long latent period is well known for the mammary tumor agent.” (J. Exp. Med. 1964).

In 1998, when de Harven became a dissident the first remark he made was that “….according to E Papadopulos et al and S Lanka, isolation of HIV from fresh plasma of AIDS patients has never been achieved under any circumstances”. Subsequently, de Harven made the absence of electron microscopy evidence for the isolation of “HIV from fresh plasma” as his argument against the “HIV” theory of AIDS. However, in 1965 he wrote: “It is also fully realised that negative results in electron microscope virology do not mean that human leukemia is not associated with or induced by viruses”. (Winster Institute).