IL NUOVO CIMENTO Vol. 37 C, N. 5 Settembre-Ottobre 2014
DOI 10.1393/ncc/i2014-11828-9

Colloquia: The Legacy of Bruno Pontecorvo

Bruno Pontecorvo: A life of two halves

F. Close
University of Oxford - OX1 3DP, Oxford, UK

Summary. — Bruno Pontecorvo's research was classified twice - in Chalk River in 1945 and in Dubna between 1950 and 1955. I review two particular physics contributions whose significance has been obscured or misconstrued due to being classified. They shed interesting light on two of Bruno's contributions to our field.

1. – Introduction

During the last three years I have been researching Bruno Pontecorvo's life for a scientific biography: *Half Life*. He quit Harwell for life in the USSR on 1 September 1950 at the midpoint of his life. In the course of trying to understand why he took this decision and the impact on both his science and his personal life, I have had access to many original documents that had been hidden for many years. Here I shall focus on two of his scientific achievements - one from Chalk River in 1945 and one in Dubna after 1950 - the true stories of which have not been widely known, not least due to the fact that original papers have been classified.

Now, thanks to his son Gil, I have seen his work on the associated production of strange particles, which he did independent of Pais and Gell-Mann around 1952. I will show in section **3** how this newly released material shows that he indeed had the idea of associated production, but did not develop the concept of strangeness as an additive quantum number. I shall begin, section **2** with his work on neutrinos in 1945, where his original paper of 1946, which inspired Ray Davis to seek and eventually find solar neutrinos, was actually preceded by his original paper of 1945. This 1945 paper has been hidden in The National Archives in England for many years, having remained classified secret for twenty years. This 1945 paper sheds new light on his role in the solar neutrino story. In section **4** I shall add some brief remarks, which are developed at more length in my upcoming biography of Bruno Pontecorvo.

2. – Solar neutrinos

For many years, Pontecorvo's reputation as a brilliant innovative physicist has rested to a considerable extent on the widely accepted thesis that he devised the means to pin down the existence of solar neutrinos, by the use of a target containing chlorine, and the neutrino-induced transition to a radioactive form of argon. This appears to derive from Ray Davis having used this technique after reading a review article, which attributed it to Bruno Pontecorvo. Pontecorvo's cited paper is his 1946 Chalk River report, – Chalk River internal report PD-205 – which indeed mentions this method. It is important to go back to original sources. The *Collected Works* of Bruno Pontecorvo [1] contains what purports to be his 1946 paper but with a drastic probable typo: according to this version Bruno cites the solar neutrino flux at earth of some $10^{16}/\mathrm{cm}^2/\mathrm{s}$. If this were correct it would appear to be within reach of a detector containing "a few cubic metres of fluid detector." [3] In fact, this number is a millionfold exaggerated relative to the correct value. The explanation is that the *Collected Works* appears to have a typo, which utterly changes the message of the paper. The correct value, which appears in a typed copy of his actual paper – PD-205 – is $10^{10}/\mathrm{cm}^2/\mathrm{s}$ ([1]) . The sole mention of solar neutrinos is dismissive: "The neutrinos emitted by the sun, however, are not very energetic". There is no comment about the flux being low (see fig. 2).

This 1946 paper, in any event, is not his original work. It follows an earlier paper – Chalk River internal report PD141 – from 1945. This was classified and remained unknown due to it being lodged within the British National Archives. Examination of that paper gives a rather different history of the solar neutrino problem and, indeed, of the provenance of ideas that have become associated with Pontecorvo.

The original 1945 document - Chalk River internal report PD141 - spells out the concept. Bruno states that detecting the neutrino "is *not* out of the question, and [I here] suggest an experimental method which might make an experimental observation feasible". He notes that when a neutrino hits, the nucleus that is formed will "in general be radioactive", so that "the radioactivity of the produced nucleus may be looked for". Further, the essential point is that the radioactive atom has "different chemical properties from the irradiated ones". He anticipates an irradiated volume "of the order of cubic metres" and so "it must be cheap". It is also important that the produced radioisotope have a lifetime of several days "because of the long time involved in the separation". Chlorine satisfies these criteria. Seaborg's table of isotopes shows that the transition to $^{35}S_{16}$ as the radioisotope with a suitable half life could be a method.

This would indeed be relevant, we now know, for a nuclear reactor produces antineutrinos, whose collisions with chlorine produce a positron and reduce the nuclear target by one, to leave sulphur. There is, however, a tantalising comment included. "According to Dr. Gueron, the best compound to irradiate from a chemical point of view would be CCl_4" and a neutrino can then produce $^{35}A_{18}$ which is radioactive. Thus the utility of cleaning fluid, in the form of carbon tetrachloride, originated with Gueron. In the 1946 paper we learn that the advantage of argon being chemically inert was realised to be "the most promising method according to Dr Frisch and the writer". Thus by 1946 focus has turned to the possible discovery of the neutrino by the chlorine to argon method. It is ironic that Davis first tried to discover the neutrino by using this technique at a reactor. That he found nothing actually helped confirm that neutrino and antineutrino differ. The place where the method bore fruit, of course, is in its application to the search for solar neutrinos.

In the body of the original 1945 paper [2] – Chalk River internal report PD-141 –

([1]) In 1988 W. Davidson retyped the NRC official version which was in bad shape. I am indebted to Art McDonald for providing me this document.

INVERSE β PROCESS

by

B. Pontecorvo

<u>Introduction</u>

The Fermi theory of the β disintegration is not yet in a final stage; not only detailed problems are to be solved, but also the fundamental assumption - the neutrino hypothesis - has not yet been definitely proven. I will recall briefly the main experimental facts which have led Pauli to propose the neutrino hypothesis.

1. In a β disintegration, the atomic nucleus Z changes by one unit, while the mass number does not change.
2. The β spectrum is continuous, while the parent and the daughter states correspond to well defined energy values of the nuclei Z and Z ± 1.
3. The difference in energy between the initial and final states involved in a β transition is equal to the <u>upper</u> limit of the continuous spectrum.

We see that the fundamental facts can be reconciled only with one of the following alternative assumptions:

i. The law of the conservation of the energy does not hold in a single β process.

ii. The law of the conservation of the energy is valid, but a new hypothetical particle, undetectable in any calorimetric measurement - the neutrino - is emitted together with a β particle in a β transition, in such a way that the energy available in such transition is shared between the electron and the neutrino. This suggestion was made by Pauli and on

PD-205

Fig. 1. – First page of B. Pontecorvo, Inverse β-process, Report PD-205, National Research Council of Canada, Division of Atomic Energy, Chalk River, November 13, 1946.

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Such a value of the neutrino flux, though extremely high, is not too far from what could be obtained with present day facilities.

<u>Sources</u>

The neutrino flux from the sun is of the order of 10^{10} neutrinos/cm^2/sec. The neutrinos emitted by the sun, however, are not very energetic. The use of high intensity piles permits two possible strong neutrino sources:

1. The neutrino source is the pile itself, <u>during operation</u>. In this case, neutrinos must be utilized beyond the usual pile shield. The advantage of such an arrangement is the possibility of using high energy neutrinos emitted by all the very short period fission fragments. Probably this is the most convenient neutrino source.

2. The neutrino source is the "hot" uranium metal extracted from a pile, or the fission fragment concentrate from "hot" uranium metal. In this case, neutrinos can be utilized near to the surface of the source, but the high energy neutrinos emitted by the short period fragments are not present.

In the case of the investigation of inverse β processes produced by electrons of γ-rays of high energy, the best source is a betatron or a synchrotron.

Chalk River Laboratories
Chalk River, Ontario

November 13, 1946
BP/YB

B. Pontecorvo

PD-205

Fig. 2. – Page 10 of Report PD-205, showing the correct value cited by Pontecorvo for the solar neutrino flux.

Pontecorvo had focussed exclusively on the opportunities offered by a nuclear reactor: there is no mention of solar neutrinos at all. He concluded that a reactor with just a little more power than that under design at Chalk River might produce enough neutrinos to give success. At the end of the paper however, after Bruno's signature closes the main report, there is a footnote. This appendage is an afterthought, due to Maurice Pryce, a British theoretician at Chalk River.

Pryce pointed out that the sun could irradiate the earth with a neutrino flux of 10^{10} neutrinos /cm^2/s, Bruno credits Pryce unambiguously with this suggestion: "Dr Pryce pointed out to the author that the flux of neutrinos from the sun is quite considerable (see fig. 4).

So the father of the solar neutrino idea is Maurice Pryce. However, he and Bruno then dismiss it, because the intensity of solar neutrinos at the earth would be: "too low for an experiment of the type suggested". They estimated that a flux a million times brighter than this would be required for success.

This 1945 paper was classified secret and remained so until 1964. Few seem to have been aware of its existence, let alone its content. When I found it in the National Archives in London, I was surprised to discover the differences with the public 1946 paper. The latter has no mention of Pryce's role, for example.

More bizarre, however, is that if you believe what appears in the *Collected Works* [1] Bruno builds his 1946 paper on the supposition that the solar neutrino flux is not 10^{10}/cm^2/s, as in 1945, but a staggering 10^{16}/cm^2/s. However, as I later discovered and pointed out above, is a major error in the *Collected Works*; the original article clearly states 10^{10}/cm^2/s and dismisses solar neutrinos as a practical route to discovery.

The eventual discovery of solar neutrinos, by Davis, was inspired by Bahcall's calculations of fluxes for the higher energy neutrinos from the boron and beryllium sequence of solar fusion. Davis used the chlorine target, which is in Bruno's 1946 paper, but whose provenance owes some credit to Gueron. The first mention of solar neutrinos in theory is due to Maurice Pryce, not Bruno, and the solar source is dismissed as practical. Bruno later developed important insights about oscillating neutrinos, which inspired Davis to his eventual success. However, his original paper from 1945 shows a different story about Bruno and solar neutrinos than has been widely believed.

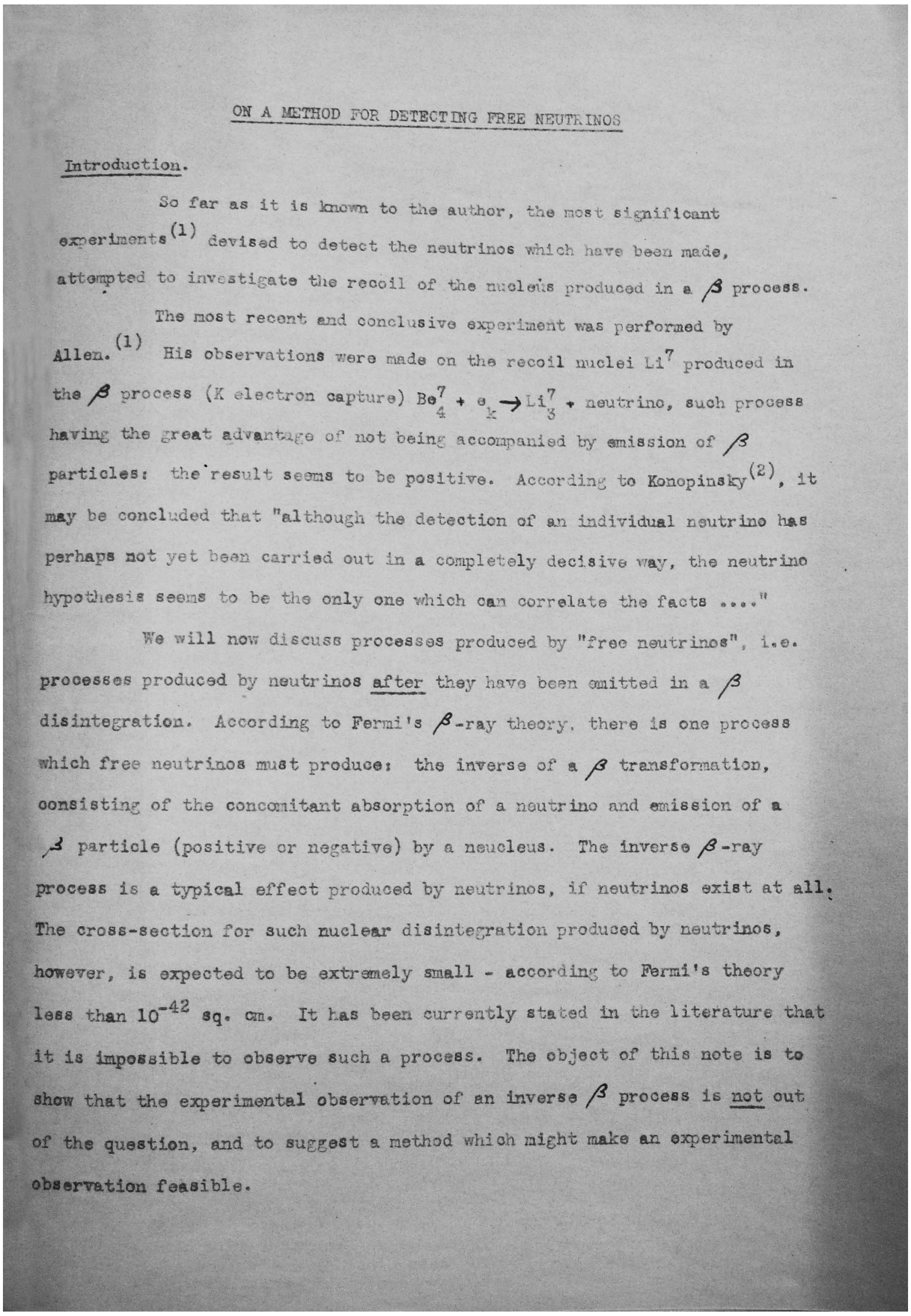
If this is a case where Bruno has been credited with rather more than his due, my next example is the converse: Bruno independently discovered the concept of associated production, but has not received wide recognition.

3. – Associated production of strange particles

Most textbooks attribute the concept of associated production to Pais and Nishijima in 1952. However, Bruno Pontecorvo had this idea, independent of them, and possibly earlier. Soon after the discovery of the Lambda, in 1951, Bruno wrote a classified paper in which he drew attention to the anomalous properties of the strange particles [4]. This articulated the possibility that "the process of formation of these particles is not the reverse of their decay". What he meant was that strange particles are produced by the strong force in pairs but decay individually due to the weak force.

He correctly identified the following (where I use modern notation, in place of Bruno's V and τ for Λ and K). I quote from Bruno: "The production of the K cannot happen via $N \rightarrow N + K$".

He then makes the assumption that "particles of the V class" (*i.e.* strange particles) "appear together": $N \rightarrow \Lambda + K$. This is associated production. He points out that



ON A METHOD FOR DETECTING FREE NEUTRINOS

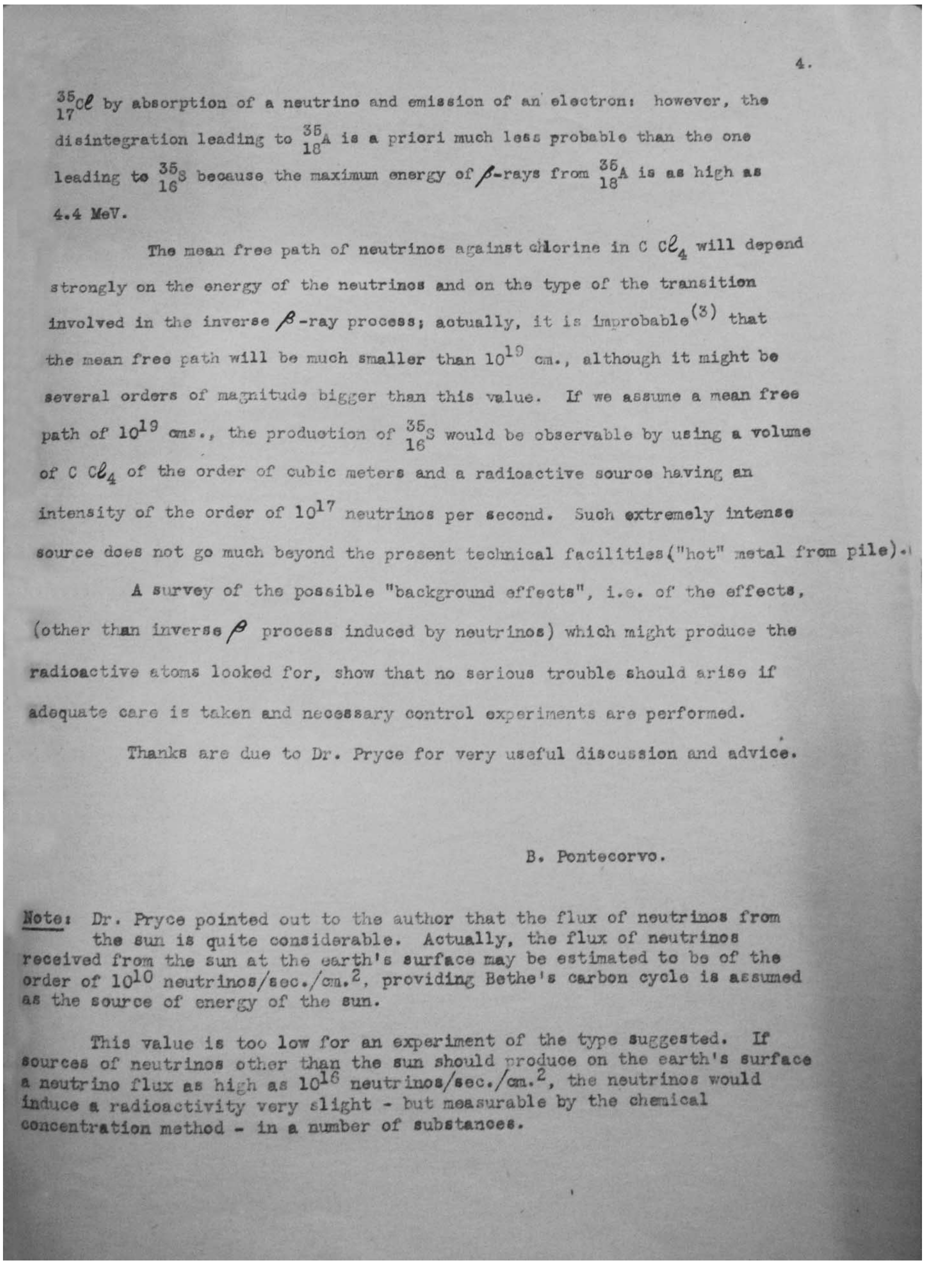
Introduction.

So far as it is known to the author, the most significant experiments(1) devised to detect the neutrinos which have been made, attempted to investigate the recoil of the nucleus produced in a β process.

The most recent and conclusive experiment was performed by Allen.(1) His observations were made on the recoil nuclei Li^7 produced in the β process (K electron capture) $Be^7_4 + e_k \rightarrow Li^7_3$ + neutrino, such process having the great advantage of not being accompanied by emission of β particles: the result seems to be positive. According to Konopinsky(2), it may be concluded that "although the detection of an individual neutrino has perhaps not yet been carried out in a completely decisive way, the neutrino hypothesis seems to be the only one which can correlate the facts ...."

We will now discuss processes produced by "free neutrinos", i.e. processes produced by neutrinos after they have been emitted in a β disintegration. According to Fermi's β-ray theory, there is one process which free neutrinos must produce: the inverse of a β transformation, consisting of the concomitant absorption of a neutrino and emission of a β particle (positive or negative) by a neucleus. The inverse β-ray process is a typical effect produced by neutrinos, if neutrinos exist at all. The cross-section for such nuclear disintegration produced by neutrinos, however, is expected to be extremely small - according to Fermi's theory less than 10^{-42} sq. cm. It has been currently stated in the literature that it is impossible to observe such a process. The object of this note is to show that the experimental observation of an inverse β process is not out of the question, and to suggest a method which might make an experimental observation feasible.

Fig. 3. – B. Pontecorvo, On a method for detecting free neutrinos, Report PD-141, National Research Council of Canada, Division of Atomic Energy, Chalk River, May 21, 1945 (source: The National Archives, London).



4.

$^{35}_{17}Cl$ by absorption of a neutrino and emission of an electron: however, the disintegration leading to $^{35}_{18}A$ is a priori much less probable than the one leading to $^{35}_{16}S$ because the maximum energy of β-rays from $^{35}_{18}A$ is as high as 4.4 MeV.

The mean free path of neutrinos against chlorine in $C\,Cl_4$ will depend strongly on the energy of the neutrinos and on the type of the transition involved in the inverse β-ray process; actually, it is improbable(3) that the mean free path will be much smaller than 10^{19} cm., although it might be several orders of magnitude bigger than this value. If we assume a mean free path of 10^{19} cms., the production of $^{35}_{16}S$ would be observable by using a volume of $C\,Cl_4$ of the order of cubic meters and a radioactive source having an intensity of the order of 10^{17} neutrinos per second. Such extremely intense source does not go much beyond the present technical facilities ("hot" metal from pile).

A survey of the possible "background effects", i.e. of the effects, (other than inverse β process induced by neutrinos) which might produce the radioactive atoms looked for, show that no serious trouble should arise if adequate care is taken and necessary control experiments are performed.

Thanks are due to Dr. Pryce for very useful discussion and advice.

B. Pontecorvo.

Note: Dr. Pryce pointed out to the author that the flux of neutrinos from the sun is quite considerable. Actually, the flux of neutrinos received from the sun at the earth's surface may be estimated to be of the order of 10^{10} neutrinos/sec./cm.^{2}, providing Bethe's carbon cycle is assumed as the source of energy of the sun.

This value is too low for an experiment of the type suggested. If sources of neutrinos other than the sun should produce on the earth's surface a neutrino flux as high as 10^{16} neutrinos/sec./cm.^{2}, the neutrinos would induce a radioactivity very slight - but measurable by the chemical concentration method - in a number of substances.

Fig. 4. – Last page of Report PD-141, showing the footnote acknowledging Pryce's suggestion about the existence of a considerable flux of neutrinos from the sun (source: The National Archives, London).

"quasi-stable systems of nucleons and V particles can be expected to form in favourable conditions". In modern language this amounts to the prediction of hypernuclei.

This far he has anticipated Pais and Nishijima, and with hypernuclei probalby gone further. However, he does not have the concept of strangeness, as it is clear he does not recognise the concept as an additive quantum number. This is evident from the following.

He correctly predicts that the following cannot happen: $N + N \to N + \Lambda$. At Dubna he makes a search for this, and establishes that it is not seen. However, his picture of associated production leads him to believe that the following reaction **is** possible: $N + N \to \Lambda + \Lambda$.

The concept of strangeness, as later articulated by Gell-Mann and Pais, forbids this reaction, which has never been seen. So in a nutshell: Bruno Pontecorvo can claim to be the father of associated production, and possibly also of hypernuclei. However, he did not come up with the idea of strangeness.

4. – Conclusion

We have seen two examples of where classification of Bruno's work, either side of his move from west to east, have distorted the history of his contributions. It is now clear that the early ideas attributed to him for solar neutrinos were stimulated by others and were not regarded by himself with much enthusiasm. Conversely, his work on associated production has not received adequate recognition, although, as we have seen, the concept of strangeness appears to have eluded him. As to why he took the decision to move to the USSR so abruptly, leaving both his family and Marianne's bereft, only he ever knew for sure. I have spent three years investigating his life for my forthcoming book on his *Life of Two Halves*, or - "Half Life." In 2015 I will be able to answer some questions - such as why he took the decision so suddenly, and unplanned. Above all, I am pleased that this interest in Bruno, and this celebration of his centenary, has enabled me at last to meet Gil, as well as Antonio and his nephews. I hope that my book will help dismiss some of the nonsense that has been written about Bruno, and to clarify some issues.

I am indebted to the organisers of the conference, to many participants for discussions, to Ludo Pontecorvo for loaning me his copy of the *Collected Works*, to Luisa Bonolis and Giuseppe Mussardo for help with history, research and translation, and to Art McDonald for obtaining a version of the 1946 paper from Chalk River.

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