CFP 研究の歴史から（4）

― S.E. Jones et al.（1989）による中性子エネルギー・スペクトルの最初の測定とその後の展開

常温核融合現象における中性子のエネルギー・スペクトルを最初に測定したのが Jones et al. [Jones 1989] であることに、異存のある人はいないでしょう。そして、Taubes[Taubes 1993] が経緯を克明に辿っているように、Fleischmann-Pons との間に優先権争いがあったことも周知の事実であろうと思います。この項では、Jones et al.の実験結果の意味を検討し、次にエネルギー・スペクトルの測定のその後の展開をたどり、最後に、優先権争いに簡単に触れたいと思います(cf. Sec.1.5)。

1. Jones et al. による中性子のエネルギー・スペクトルの測定 [Jones 1989]

Jones et al. による Ni-D 系から放射された中性子のエネルギー・スペクトルの測定結果は、1989 年の 4 月に発行された *Nature* に掲載されました。（読者の便宜のために、この論文をこのニュースの後に掲示します。）

Taubes によると[Taubes 1993, Chapter 2]、Jones のグループには radiation detection expert の Bart Czirr がいて、中性子の測定に関しては一流だったようです。*Nature* の論文の Fig. 2 には、膨大な量の背景中性子の中から、2.5 MeV に相当するチャネル 100 のあたりに、明らかなシグナルが浮き出して見えます。この測定結果から、Jones et al.は NiD_x における $d-d$ 融合反応(2) の存在を確認したと考えたのでした：

$$d + d \rightarrow {}^4_2\mathrm{He}^* \rightarrow t\,(1.01) + p\,(3.02), \qquad Q = 4.03 \qquad (1)$$

$$\rightarrow {}^3_2\mathrm{He}\,(0.82) + n\,(2.45), \qquad Q = 3.27 \qquad (2)$$

$$\rightarrow {}^4_2\mathrm{He}\,(0.07) + \gamma\,(23.66). \qquad Q = 23.73 \qquad (3)$$

この実験結果は、いろいろな意味で常温核融合現象の研究を刺激しました。まず、発生する中性子のエネルギー・スペクトルをより精密に測定しようとする試みがなされました(cf. Sec.1-2). 次に、$d-d$ 融合反応(1) – (3) が背景中性子によって引き起こされる可能性が検討されました(cf. Sec.1.3). さらに、意識的に熱中性子を照射することによって、常温核融合現象がどのような影響を受けるかが確かめられました (cf. Sec.1.4).

2. 中性子のエネルギー・スペクトルの精密測定

多くの核物理学者は、固体中の環境が核反応に影響を与える可能性について否定的でしたから、背景中性子の効果による $d-d$ 融合反応への影響を極力抑

えて実験をすることが要求されました。Jones 自身、できるだけ背景中性子の影響を排除した環境で実験をやり直そうと考え、実行しました。

　その一つの試みが、東京大学との共同研究の形で、岐阜県の神岡鉱山の地下2000mの坑道跡の実験室で行われた、背景中性子濃度が極度に低い環境で行われた実験でした[Ishida 1990].この実験を Jones の指導の下に行ったのは大学院生の石田卓氏でした。しかし、結果は常温核融合現象の存在を確認することに成功したとは言えないものだったようです。この結果にめげずに、Jones 達は極低背景中性子のもとで(in the deep-underground neutron detection facility in Provo Canyon）の精密実験（with state-of the-art detectors）を続けて、その結果を 1994 年に発表しました[Jones 1994]。しかし、その結果は論文の表題にもあるように “A null result” でした。

　この結果についての著者らの結論を検討してみると、彼らの結論が妥当なものとは言えないことが分かります。以下の引用文から分かるように、彼らは”hydrogen controls”との比較で差がないことから”null result”と結論しているのですが、これは S. Pons が control experiments について述べたというエピソードと同じ誤り、つまり軽水系（protium system）では CFP 常温核融合現象が起こらないという前提で実験結果を判断している誤りを犯していることになります：

“*When Pons was asked why he had not reported results of control experiments with light water substituted for heavy water, he replied ‘A baseline reaction run with light water is not necessarily a good baseline reaction.’ When asked to elaborate, Pons intimated he had performed the experiment with light water and had seen fusion, saying ‘We do not get the expected baseline experiment. . . We do not get the total blank experiment we expected’*” (CFRL News No. 89 http://www.geocities.jp/hjrfq930/News/news.html/ ）

　実際に Jones et al.の結論を見てみると、彼らの言う”neutron bursts” and “singles” が protium and deuterium systems で their state-of-the-art detector によって観測されていることがわかります[Jones 1994, p. 145] (underlined at citation)：

“*The Pd/LiOD cells described above were polarized for 708.8 hours. During this time, 24 neutron-like burst events were seen, all having multiplicity 2. (This represents approximately one burst candidate per 30 hours, a very low rate indeed.) Thus, the neutron-like rate for these events was 48/708.8h = (0.07 ~ 0.01) n/hr. These numbers are in complete agreement with those found with hydrogen controls discussed above. There was no significant* *change in rate for neutron-like burst events between background and runs with electrical currents in the Pd/LiOD cells. There is no indication of a neutron burst signal above a very low background.*"(Jones et al. [Jones 1994, p. 145])

"*Even though there is no neutron-burst signal, there may still be neutron counts above background which we consider 'singles." The background rate for such events has been established as (0.65 ± 0.1) counts/hour using Pd loaded with hydrogen. Figure 3 displays results from each run of the electrolytic cells, showing 1-sigma error bars (statistical only). <u>All of the observed rates are entirely consistent with background levels of 0.65 h^{-1}</u>. This exercise has as its conclusion that no neutrons were seen above very low background levels, in a high-efficiency detector. The most important observation may be that state-of-the-art neutron detectors are now available for studies requiring high-sensitivity instruments.*" (Jones et al. [Jones 1994, p. 145])

彼らの努力はさらに 2003 年まで続けられ、ついに TiD_x での中性子発生の検出に辿りつきます[Keeney 2003]。かくして、彼らの the state-of-the-art detector の優れた性能が、改めて日の目を見ることになったのでした。

3. 高エネルギー中性子の検出

常温核融合現象における中性子の検出は、Jones et al. 以外にも、中性子の測定に経験を持つ研究者によって試みられました。その典型的な例は Takahashi et al. [Takahashi 1990], Bressani et al. [Bressani1991, Botta 1992, 1999] および Nakada et al. [Nakada1993] の結果です。彼らは上記反応 (2) から予想される 2.45 MeV の中性子の確認を目的としながら、予想外の結果を得ました。Takahashi et al. は 7 MeV まで、Botta et al. は 10 MeV 以上 Nakada et al. は 10 MeV にまで達する高エネルギーの中性子を観測し、常温核融合現象が *d-d* 融合反応の枠から離れる切っ掛けの一つをつかんだのでした。[Kozima 2010] (cf. also CFRL News No. 89).

4. 背景中性子の効果

固体中の環境が、作用領域が 1 fm 程度の核力の作用に影響を与える可能性に否定的な多くの核物理学者は、Jones et al. の測定した中性子が、環境中性子の影響によるのではないかという疑いを抱いたようです。

その可能性を検証する最初の実験結果が、1989 年のうちに発表されました [Shani 1989]. この実験結果の解釈は、ともすると常温核融合現象に否定的なものと受け取られがちなのですが、拙著[Kozima 1998 (Sec. 8.2)] でも指摘したように、固体環境が核反応に強い影響を与えることを示す画期的な結果だったのでした：

"*The first experimental evidence of an effect of the thermal neutron on the nuclear reactions in solids was obtained by G. Shani et al. in Jerusalem, Israel. They measured neutron emission from targets irradiated with thermal neutrons from an artificial source where the targets were (1) palladium metal occluding deuterium (PdD_x) and (2) gaseous deuterium (D_2). The measured neutron in the case (2) was explained by the conventional nuclear physics very well but that in the case (1) was inconsistent with the conventional prediction.*

<u>The number of the obse4rved neutron in the case (1) was more than three orders of magnitude larger than the prediction</u>.

From their result, Shani et al. deduced a conclusion that the cold fusion phenomenon observed in solids is a result induced by the background neutron with a negative nuance against its revolutionary character."
([Kozima 1998, Sec. 8.2a] Underline is at citation.)

5. 熱中性子照射の効果

意識的に熱中性子を照射したとき常温核融合現象はどうなるのか、という疑問が当然のように起こります。この疑問に答える実験が多くの研究者によってなされました。結果は、熱中性子の照射によって常温核融合現象が誘起され、あるいは強化されることが明らかになりました。典型的な例は Celani et al. [Celani 1992], Stella et al. [Stella 1993] および Lipson et al. [Lipson 1996] のものでしょう。その他の実験については、拙著[Kozima 1998 (Sec. 8.2)] に譲ります。

このようにして、背景中性子の非常に少ない環境では常温核融合現象の発生は極めて稀にしか起こらず、熱中性子を照射すると常温核融合現象は誘起され、常温核融合現象に伴って発生する中性子のエネルギーは 10 MeV 以上に達し、2.45 MeV 以上のエネルギーを持つ中性子の数は、2.45 MeV の中性子の数より多いことが実験的に示されました。

6. 実験結果の説明

以上に説明したように、常温核融合現象に伴う中性子のエネルギー・スペク

トルの実験結果とそれにたいする熱中性子の効果は、$d-d$ 融合反応の発生が固体環境の影響で何桁も大きくなるという考えで説明するには無理があることが明らかになりました。これは、核物理学と固体物理学の常識からは極めて正常な結論と言ってよいでしょう。

我々は TNCF モデルを使って、電界溶液の中に含まれている $^6_3$Li と固体中の捕獲中性子の反応、およびが重陽子と捕獲中性子の反応によって生ずるトリトンが引き金となって起こる連鎖反応により、Jones et al. の実験結果を説明し [Kozima 1997, 1998 (Section 11.4)]、また 14.1 MeV までの中性子が生ずる可能性をシミュレーションによって示すことができました[Kozima 1997, 1998a (Fig. 11.1), 19998b, 1999].

7. The Competition for Financial Funds – an Episode

1989 年に発表された Fleischmann-Pons-Howkins の論文［Fleischmann 1989］と Jones et al. の論文［Jones 1989］との関連については、G. Taubes の本［Taubes1993］で克明に追跡されており、ほとんど周知の事柄と思われます。ここで Jones et al. の論文の常温核融合現象研究における意味を明らかにしたついでに、Taubes がその本の Chapter 2. The Competition で述べている事情から判断した著者の見解を付け加えておきます。

下に引用した Taubes の文章から、Jones が muon-catalyzed fusion and piezo-nuclear fusion を手掛けていた事実にもかかわらず、the Pons-Fleischmann proposal を読んだことが 1989 年の Jones et al. の論文での電解法による実験を実施する切っ掛けを与えたことは事実であり、Taubes の言う通り Jones は率直にそのことを認めるべきだったと思われます。

Taubes [Taubes 1993] からの引用 (Underlines are at citation)。

"Chapter 2 *The Competition*

[pp.. 26 – 29]

A few weeks after Palmer broached his theory to Jones, they came upon a paper by Boris Mamyrin, a Soviet researcher, who found excessive amounts of helium 3 in nickel foils. Fusion? Why not? In a memo dated April 1, 1986, Jones wrote, "Could it be that metal hydrides provide an environment conducive to confinement and fusion of hydrogen isotopes?"

On April 7, Jones met at BYU with Palmer, Bart Czirr, the resident radiation detection expert, and Johann Rafelski., a theorist who was now collaborating with Jones on the muon-catalyzed fusion work. The four scientists discussed various strategies for catalyzing fusion at room temperature. Later Jones

liked to call this meeting "the brainstorming session." <u>The scientists discussed using diamond anvil presses to condense deuterium, or even electric charges or lasers to shock deuterium atoms into fusing</u>.
Jones's notes for the day, as was his style, were cryptic. His handwriting bordered on the illegible. And, if he was then planning to use electrolysis to condense deuterium in a metal and induce fusion, as he would claim later, <u>he never actually wrote down the word electrolysis.</u> What is indisputable is that he scribbled a list of elements: "Al, Cu, Ni, Pt, Pd, Li. . ." And next to Pd, palladium, and Pt, platinum, were the portentous words "dissolves much hydrogen." And Jones did, at Rafelski's suggestion, take the lab book to the BYU patent attorney, Lee Phillips, and ask that the page be notarized.
Three years later, and several weeks after the March 23 announcement of the discovery of cold fusion, the BYU press office released an official history of "piezonuclear" fusion, which was now simply Jones's term for cold fusion. This documented the progress of the BYU cold fusion research program, with the aim of dispelling Pons and Fleischmann's accusations that Jones had somehow pirated the idea from them. The account described this April 7 meeting as the beginning of "Brigham Young University's experimental program." This made the BYU effort sound like a concerted three-year program, which is how Jones described it later to Pons and Fleischmann, and later still to reporters. Such was not the case."

"Chapter 3 *Autumn 1988*
pp. 36 – 37
Shortly after March 23, 1989, the BYU public relations office distributed an official history of piezonuclear fusion research at BYU. Its purpose was to protect Steve Jones from any possible allegations of conflict of interest or worse—scientific piracy.
This account, which was compiled predominantly by Jones, cited a fusion group meeting on August 24, 1988, during which Jones and his colleagues discussed their piezonuclear fusion program. (This was approximately one month before Jones received the Pons-Fleischmann proposal (on September 20)). The account asserts that from August 24 onward the fusion group's program was "vigorously" pursued. Jones told reporters, "From that day [August 24] we were essentially 100 percent working on this other piezonuclear fusion."

However, when presented with the facts that nothing was done on the subject for twenty-nine days after the meeting and that he had reviewed the Pons-Fleischmann proposal by then, Jones insisted that this level of activity still legitimately meets the definition of "vigorous pursuit." He did not deny that he may have had "impetus" from the Pons-Fleischmann proposal but argued that Pons and Fleischmann had not accused him of "impetus"—they had accused him of stealing ideas wholesale. Jones conceded that perhaps in drafting BYU's official account he should have noted that he had assigned a student to do electrolysis experiments (of the kind Paul Palmer had pursued two years earlier and Pons and Fleischmann were now proposing) only after reading the Utah proposal.

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To this Gajewski added his own quasi-rhetorical question: would he be surprised to discover that Jones, consciously or subconsciously, intensified the pace of his cold fusion research because of what he saw in the Pons-Fleischmann proposal? He said he would be unable to answer definitively. "Maybe he did or maybe he didn't, but I would not be surprised if he did. I have no evidence to that effect. It's just human nature."*

Whether he did or not was important merely because Pons and Fleischmann believed that Jones only "vigorously" began his research *after* reading their proposal, and that the fate of billions of dollars, among other things, hinged on whether he did or not. And what Pons and Fleischmann believed, rightly or wrongly, was what led them publicly and emphatically to disclose their invention on March 23, which is to say well before they had gathered sufficient data to support their claim.

*Ryszard Gajewski was an administrator of Office Advanced Energy Projects (OAEP) at DOE, to whom Pons submit his proposal in September 1988."

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