DOCUMENTATION OF DR. TOM PASSELL'S LENR CONTRIBUTIONS AT THE ELECTRIC POWER RESEARCH INSTITUTE AND BEYOND

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

Third Draft Report

Tom Passell, Ph.D.
TOP Consulting
Mountain View, CA

Thomas W. Grimshaw, Ph.D.
LENRGY, LLC
Austin, TX

January 3, 2022

Contents

1 Introduction

Cold fusion (CF) was announced on March 23, 1989, by Dr. Martin Fleischman and Dr. Stanley Pons. The immense potential energy benefits of CF (also referred to as Low Energy Nuclear Reactions, LENR) were immediately recognized. Humankind's need for a source of cheap, clean, inexhaustible, and safe energy seemed to be permanently satisfied. However, LENR was rejected by mainstream science within a year or so, and it remains highly marginalized to this day. On the other hand, the phenomenon has continued to be rigorously pursued by many investigators in several countries. The mounting evidence for the reality of LENR shows that its potential benefits may yet be realized.

Because it is a "pariah" science, LENR has attracted relatively few new investigators to the field. Many of the researchers became active in the early months and years after the 1989 announcement. Now 30 years later many of these investigators are leaving the field for retirement or health reasons. The results of their many years of LENR investigation are at risk of being lost, which would be extremely unfortunate not only for the field, but also potentially for humanity.

An initiative is underway by Dr. Thomas Grimshaw's LENRGY LLC to mitigate the risk of loss of research records of LENR investigators. Its objectives are to collect, organize, document, and archive these records. The LENR Research Documentation Initiative (LRDI) assists researchers to ensure that their efforts are preserved and to keep the records available for additional analysis and interpretation. The LRDI is described in an article in Infinite Energy[1] and on a dedicated website[2].

Dr. Tom Passell (Figure 1-1) was one of the first researchers and supporters of LENR after the 1989 announcement. He held the position of Project Manager at the Electric Power Research Institute (EPRI) at that time and immediately recognized the importance of the discovery. He began working very quickly with Dr. Michael McKubre, Dr. Fran Tanzella, and others at SRI

[1] Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April 2020, p. 9-13.

[2] LENR Research Documentation Initiative: Collection, Organization, Description, Archiving of LENR Research Records. www.lenr-documentation.org.

international to build on ongoing research in palladium and deuterium systems to conduct experiments similar to those of Fleischmann and Pons. This work continued through 1995, with a total of more than $9 million in research funding.

Figure 1-1.

Dr. Tom Passell in His Office in Mountain View, CA
Photo Taken April 2021

Subsequently, Dr. Passell contributed to LENR research at the firm D2-Fusion and in his own company, TOP Consulting. Not only did he conduct his own investigations and direct (and fund) the work of others, but he also developed his own explanation of LENR based on the Oppenheimer-Phillips reaction.

A project is underway with Dr. Passell to document the records of his LENR work. The Passell LENR Contributions Documentation Project (PLCDP) is being performed under the umbrella of the LRDI. The purpose of this report is to present the results of the PLCDP. Future potential work is also proposed and information on how the project is being performed is presented. Thanks go to Dr. Passell for participating in and supporting the PLCDP.

2 Professional Background

Dr. Passell's extensive professional experience before the 1989 announcement provided a solid foundation for his contributions to the LENR field[3]. He had over 35 years of professional experience at the time of the 1989 announcement. He is a native of Oklahoma and received his bachelor's degree in chemistry at Oklahoma State University. He then obtained a Ph.D. in chemistry at the University of California, Berkeley. His seven areas of specialized professional competence are shown below from his resume:

- Nuclear and X-Ray physics; Plasma physics; Radiochemistry; Calorimetry
- Water chemistry of power plant corrosion; Surface analysis of metals
- Ion exchange chromatography; Impurity analysis of solids
- Thin film technology; Fiber optics-based sensors
- Neutron dosimetry; Neutron activation analysis; Gamma spectrometry
- Prompt Gamma Rays from Thermal Neutron Capture
- Beta ray and conversion electron spectroscopy in the transuranium elements

His resume also shows the following affiliations during the course of his career:

D2Fusion, Inc . Foster City, CA	1 year
TOP Consulting, Palo Alto, CA	10 years
Electric Power Research Institute, Palo Alto, CA	22 years
Lockheed Palo Alto Research Laboratory, Palo Alto, CA	4 years
Physics International Company, San Leandro, CA	2 years
SRI International, Menlo Park, CA	13 years
Phillips Petroleum Co., Atomic Energy Div., Idaho Falls, ID	2 years

Dr. Passell has authored over 30 publications in the fields of beta ray spectroscopy, surface analysis, radiative transfer, additives for corrosion control, water treatment, neutron dosimetry, ion chromatography and gamma ray spectroscopy.

[3] Passell Resumes and List of Publications. Memo to Tom Passell from Tom Grimshaw. May 1, 2021.

3 LENR Publications

After he became interested in LENR, Dr. Passell authored or contributed to a number of publications in the field. A search for "Passell" in the LENR-CANR.org library website resulted in 19 items, which are listed in Table 3-1[4].

[4] "Passell" Entries on LENR-CANR.org. Memo to Tom Passell from Tom Grimshaw. May 22, 2020.

Table 3-1.

Dr. Passell's LENR Publications as Identified on LENR-CANR.org

McKubre, M. C. H.	1990	McKubre, M.C.H., et al. Calorimetry and Electrochemistry in the D/Pd System. in The First Annual Conference on Cold Fusion. 1990. University of Utah Research Park, Salt Lake City, Utah: National Cold Fusion Institute.
McKubre, M. C. H.	1991	McKubre, M.C.H., et al. Isothermal Flow Calorimetric Investigations of the D/Pd System. in Second Annual Conference on Cold Fusion, "The Science of Cold Fusion". 1991. Como, Italy: Societa Italiana di Fisica, Bologna, Italy.
McKubre, M. C. H.	1994	McKubre, M.C.H., et al., Isothermal Flow Calorimetric Investigations of the D/Pd and H/Pd Systems. J. Electroanal. Chem., 1994. 368: p. 55.
Passell, T. O.	1994	Passell, T.O., Overview and Status of the EPRI Program on Deuterated Metals. in ASME Joint International Power Generation Conference. 1994. Phoenix, AZ.
Passell, T. O.	1995	Passell, T.O., Charting the Way Forward in the EPRI Research Program on Deuterated Metals. in 5th International Conference on Cold Fusion. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France.
Passell, T. O.	1995	Passell, T.O., Radiation data reported by Wolf at Texas A&M as transmitted by T. Passell. 1995, EPRI.
Passell, T. O.	1996	Passell, T.O., Overview of EPRI Program in Deuterated Metals. J. New Energy, 1996. 3(4): p. 1.
Passell, T. O.	1996	Passell, T.O., Search for nuclear reaction products in heat-producing palladium. in Sixth International Conference on Cold Fusion, Progress in New Hydrogen Energy. 1996. Lake Toya, Hokkaido, Japan: New Energy and Industrial Technology Development Organization, Tokyo Institute of Technology, Tokyo, Japan.
Passell, T. O.	1998	Passell, T.O., Search for Nuclear Reaction Products in Heat-Producing Pd. in The Seventh International Conference on Cold Fusion. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT.
Stringham, R.	1998	Stringham, R., et al. Predictable and Reproducible Heat. in The Seventh International Conference on Cold Fusion. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT.
Passell, T. O.	2000	Passell, T.O., and R. George. Trace Elements Added to Palladium by Exposure to Gaseous Deuterium. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
Passell, T. O.	2002	Passell, T.O., Evidence for Lithium-6 Depletion in Pd Exposed to Gaseous Deuterium and Hydrogen. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Tsinghua Univ., Beijing, China: Tsinghua Univ. Press.

Passell, T. O.	2003	Passell, T.O., Pd-110/Pd108 Ratios and Trace Element Changes in Particulate Palladium Exposed to Deuterium Gas. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
Passell, T. O.	2003	Passell, T.O., Pd-110/Pd108 Ratios and Trace Element Changes in Particulate Palladium Exposed to Deuterium Gas (PowerPoint slides). in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
Benson, T.	2004	Benson, T. and T.O. Passell. Calorimetry of Energy-Efficient Glow Discharge - Apparatus Design and Calibration. in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.
Kim, Y. E.	2004	Kim, Y.E. and T.O. Passell. Alternative Interpretation of Low-Energy Nuclear Reaction Processes with Deuterated Metals Based on The Bose-Einstein Condensation Mechanism. in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.
Passell, T. O.	2005	Passell, T.O., and T. Benson. Glow Discharge Calorimetry (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
Passell, T. O.	2008	Passell, T.O., ICCF-14 Summary. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
Passell, T. O.	2015	Passell, T.O., The Case for Deuteron Stripping with Metal Nuclei as the Source of the Fleischmann-Pons Excess Heat Effect. J. Condensed Matter Nucl. Sci., 2015. 15.

4 Office and Laboratory Facility

Dr. Passell has a combined office and laboratory in Mountain View, California[5]. The address is 2261 Old Middlefield Way. An exterior view[6] is shown in Figure 4-1.

Figure 4-1.

Dr. Passell in Front of His Office and Lab in Mountain View, CA
Photo Taken April 2021

The interior of the facility has three parts – a guest area and bookshelves in the front space, a laboratory in the back space and an office and kitchenette space in between. There is also an storage area reached by a stairway in the lab. Photos of the front space are in Figure 4-2. Three views of the laboratory space are shown in Figure 4-3. A photo of Dr. Passell with his experiment apparatus (glow discharge tube) is in Figure 4-5.

[5] Summary of Office and Lab Visit for the Passell LENR Research Documentation Project (PLCDP). Memo to Tom Passell from Tom Grimshaw. April 25, 2021.

[6] Office and Laboratory Photos. Memo to Tom Passell from Tom Grimshaw. June 23, 2021.

Figure 4-2.

Two Views of the Front Space of Dr. Passell's Office and Lab
(The office in the middle area can be seen on the left side of the upper phot.o)

Figure 4-3.

Two Views of Dr. Passell’s Lab (Front and Rear Portions)
Dr. Passell is at the foot of the stairs in the lower photo.

Figure 4-4.
Dr. Passell in His Lab with Glow Discharge Tube

An interesting feature of Dr. Passell's lab space is his "wall of pictures" located adjacent to the stairway leading to the storage area[7] (Figure 4-6). The photos are a representation of various phases of his professional and private life. Several of the prominent photos are provided in Figure 4-7 to 4-9.

[7] Wall of Pictures. Memo to Tom Passell from Tom Grimshaw. June 23, 2021.

Figure 4-6.

Wall of Photos along Stairway Leading to Upper Storage Area.

Figure 4-7.

Mahadeva Srinivasan, Eugene Mallove, Tom Passell and Douglas Morrison at ICCF-8, 2000, Lerici, Italy

Figure 4-8.

Group Photo of ICCF-10, 2003, Cambridge, MA

Figure 4-8.

Group Photo of ICCF-14, 2008, Washington, D.C.

5 Laboratory Notebooks

Dr. Passell has a collection of 19 lab notebooks for the period of his contributions to the LENR field[8]. They are in a bookshelf in his office (Figure 5-1). The approximate dates of the notebooks are shown in Table 5-1.

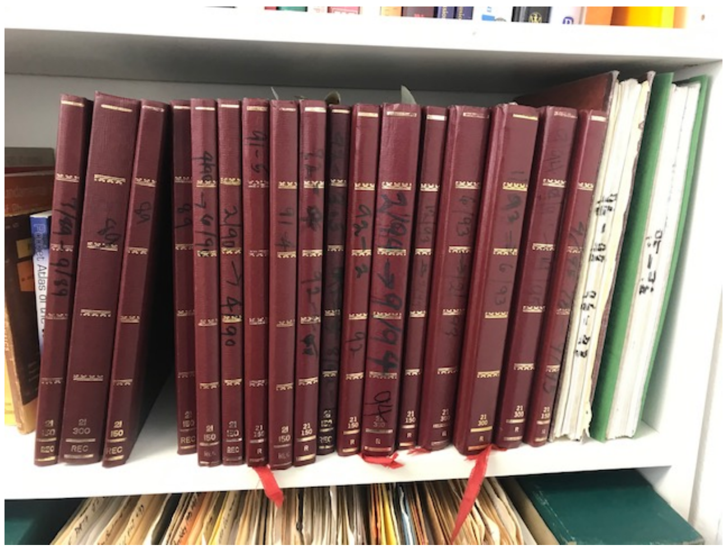

Figure 5-1.

Dr. Passell's Collection of Lab Notebooks

Table 5-1.

Approximate Date Ranges of Lab Notevnbooks

No	Dates	No	Dates
1	2/14/1989 to Unk/1989	11	Unknown 1992
2	Unknown 1989	12	11/1992 to 6/1993
3	Unknown 1989	13	1/7/1993 to 12/1993
4	Unk 1989 to 9/9/1989	14	12/1993 to 2/15/1994
5	2/14/1990 to 4/1990	15	2/1994 to 9/1994
6	4/1990 to 6/1/1990	16	Unk 1994 to 7/1995
7	7/11/91 to Unk 1991	17	4/2/1995 to 7/21/1995
8	Unk 1991 to 10/31/1991	18	9/26/1995 to 11/11/1996
9	9/16/1992 to 1/7/1993	19	Unk 1996 to 4/30/1998
10	Not Legible		

[8] Description of Notebooks with Laboratory and Other Entries. Memo to Tom Passell from Tom Grimshaw. June 26, 2021.

The notebooks include not only lab-related entries, but also notes of meetings, proposed experimental approaches, thoughts about LENR explanation, and other topics related to the field. Dr. Passell's first entry on cold fusion is dated March 28, 1989 and consists of the names of several individuals who apparently were in a meeting about the topic. It is on page 152 of Notebook #1 (Figure 5-2).

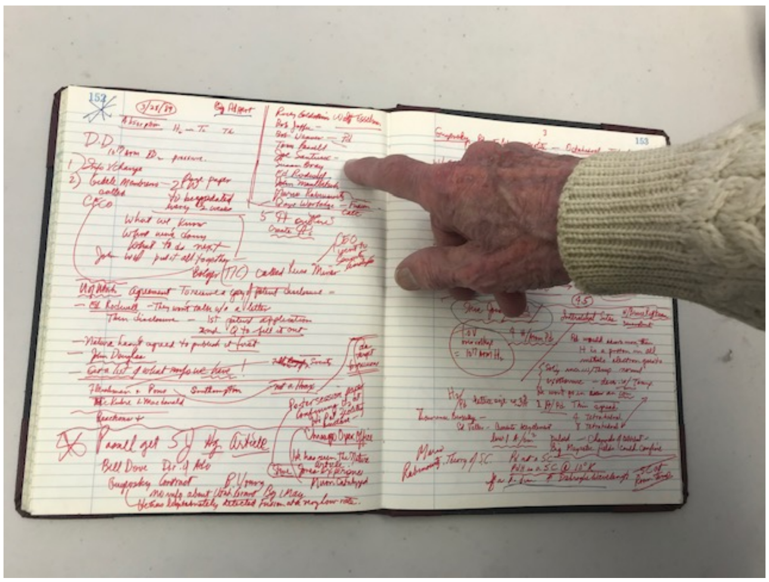

Figure 5-2.

Dr. Passell Pointing to First Cold Fusion Entry on March 28, 1989

6 LENR Library

Dr. Passell's office space has a large bookshelf (Figure 4-2) that contains mostly LENR-related items[9]. Example library bookshelves are shown in Figure 6-1. A primary component of the library is a set of proceedings and related materials of the International Conferences on Cold Fusion ICCFs). The collection (Table 6-1) includes ICCF-1 to ICCF-15, (with the exception of ICCF-13, which may be represented by the unbound unknown ICCF conference item). The collection covers a date range of almost 20 years, from 1990 to 2009.

Figure 6-1.
Example Shelves of Books in Dr. Passell's LENR Library

The LENR library includes 20 additional items, which are listed in Table 6-2. Two of the earliest publications in the LENR field, the Proceedings of ICCF-1 (1990) and the "BARC Studies in Cold Fusion" (1989) are in the library and are pictured in Figure 6-2.

[9] Documentation of Items in Passell LENR Library. Memo to Tom Passell from Tom Grimshaw. June 29, 2021

Table 6-1.

ICCF Materials in Dr. Passell's LENR Library

ICCF No	Conference	Dates
--	Unknown ICCF Conference, Unbound (Possibly ICCF-13)	--
1	First Annual Conference on Cold Fusion, ICCF-1	March 28-31, 1990
2	Second Annual Conference on Cold Fusion, ICCF-2, Proceedings (Unbound)	June 29 to July 4, 1991
3	Third International Conference on ColdFusion, ICCF-3 (Frontiers of Cold Fusion)	October 21-25, 1992
4	Fourth International Conference on Cold Fusion, ICCF-4, Volumes 1 to 4 (EPRI TR-104198)	December 6-9, 1993
5	Fifth International Conference on Cold Fusion, ICCF-5, Proceedings	April 9-13, 1995
6	Sixth International Conference on Cold Fusion, ICCF-6, Abstracts and Proceedings (Volume 1 & 2)	October 13–18, 1996
7	Seventh International Conference on Cold Fusion, ICCF-7, Proceedings	April 19-24, 1998
8	Eighth International Conference on ColdFusion, ICCF-8, Proceedings	May 21-26, 2009
9	Ninth International Conference on Cold Fusion, ICCF-9, Abstracts and Proceedings	May 19–24, 2002
10	Tenth International Conference on Cold Fusion, ICCF-10, Proceedings	August 24-29, 2003
11	Eleventh International Conference on Cold Fusion, ICCF-11, Proceedings	October 31 to November 5, 2004
12	Twelfth International Conference on Cold Fusion, ICCF-12, Proceedings	November 27 to December 2, 2005
14	Fourteenth International Conference on Condensed Matter Nuclear Science and 14th International Conference on Cold Fusion (ICCF-14), Proceedings, Volumes 1 and 2	August 10-15, 2008
15	Fifteenth International Conference on Cold Fusion, ICCF-15, Proceedings	October 5-9, 2009

Table 6-2.

Additional LENR Items in Dr. Passell's LENR Library

The Chemistry of Technetium in Medicine, Joseph Steigman and William Eckelman	1992
Unknown EPRI Transparency Presentation	--
Conference On Anomalous Nuclear Effects In Deuterium/Solid Systems (AIP Conference Proceedings 228)	22-24 Oct 1990;
Genie 2000, Geometry Composer Manual	2000
Cavitation and Induced Excess Heat in Deuterated Metals, EPRI	Undated
Understanding LENR Using QST, Russ Blake	Unknown
Review of SRI/EPRI Deuterated Metals Program: Heat Results	September 1992
National Cold Fusion Institute, Final Report	Unbound
Toward the Establishment of Solid Fusion As a Perpetual Energy for Humankind, Yoshiaki Arata	2007
Energy: Options & Issues, Henry O. Walker	1977
Cold Fusion Source Book, Belarusian State University	May 24-26, 1994
Coherent Fusion Studies: Final Report, Peter Hagelstein	January 1990 to October 1993
Cold Fusion: the History of Research in Italy 10	2008
Nuclear Transmutation: the Reality of Cold Fusion, Tadahiko Mizuno	1997
The Science of Low Energy Nuclear Reaction, Edmund Storms	2007
Cool Fusion, Edward Esko, Alex Jack	2011
BARC Studies in Cold Fusion, Iyengar and Srinivasan	December 1989
The Rebirth of Cold Fusion: Real Science, Real Hope, Real Energy, Krivit and Winocour	2004
Fire from Ice: Ice: Searching for the Truth behind a Cold Fusion Furor, Eugene Mallove	1991
Cold Fusion: the Scientific Fiasco of the Century, John Huizenga	1992
Excess Heat: Why Cold Fusion Research Prevailed, Charles Beaudette	2002

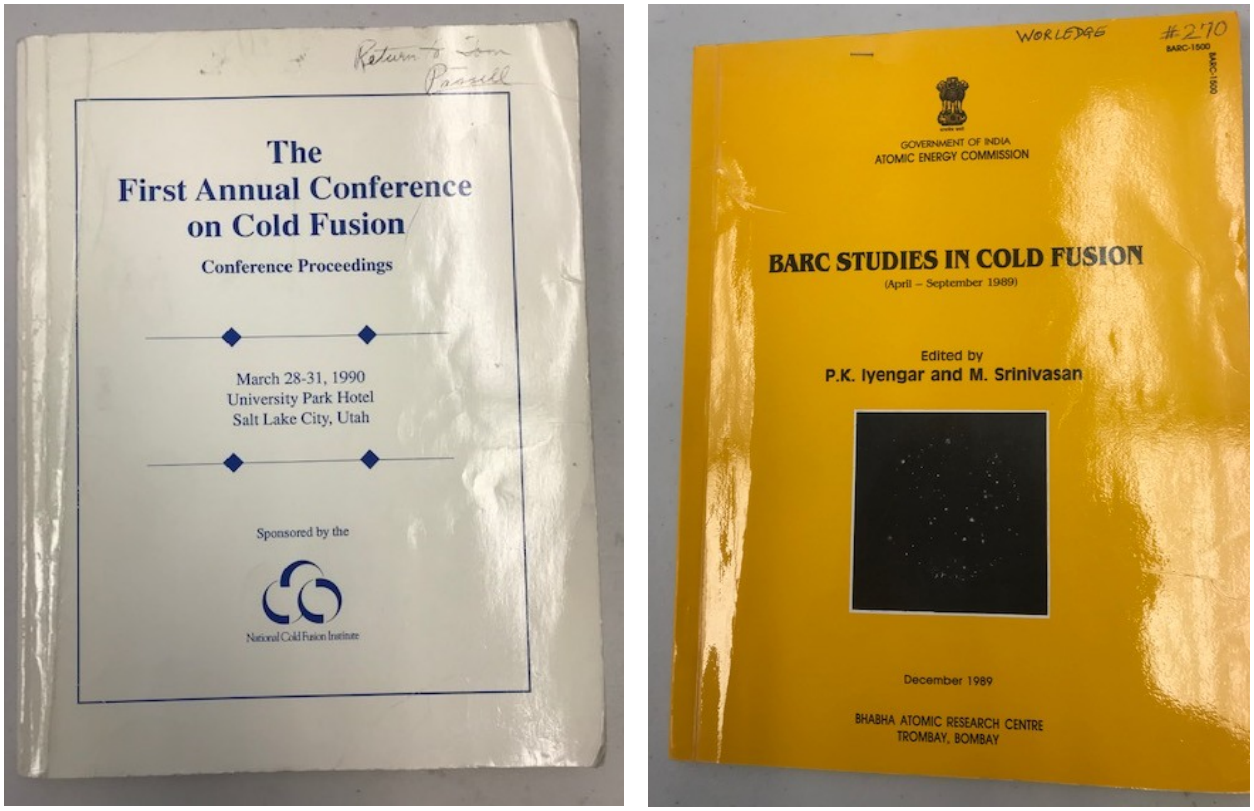

Figure 6-2.

ICCF-1 Proceedings and BARC 1500, Two of the Earliest Publications of the LENR Field

7 Electronic Files

Electronic files were obtained from two sources in Dr Passell's laboratory, a Sony laptop computer (Figure 7-1) and a number of 3.5-inch floppy disks[10]. The files on the laptop computer have been obtained for the Passell LENR Research Documentation Project. Many of these files are datasets from Dr. Passell's glow discharge experiments. An image of the contents is shown in Figure 7-2.

The floppy disks (Figure 7-3) consist of two sets, 17 disks of files created at the University of Missouri by Dr. Kutikad and 38 disks containing files created at The University of Texas at Austin by Dr. Ishkandar.

Figure 7-1.
Sony Computer Containing Hard Disk Electronic Files

[10] Electronic Files for the Passell LENR Research Documentation Project. Memo to Tom Passell from Tom Grimshaw. May 6, 2021.

Figure 7-2.

Folders Containing Files on Dr. Passell's Laptop Hard Disk

- Toms HD 210422
  - BkgandCalibOsprey
  - CathodePkgRuns011114
  - CoGraphene
  - CoTiAlVx17RunsJanFeb2014
  - FePowderrujs
  - genie2kCamfilescopy
  - getrs7072002
  - heliumOpticalStuff
  - LaWRuns011514
  - LiBO2runs1215
  - LiOH.H2O2.5KgRuns1215
  - memaloy1015
  - Nbpvcx2RunsThru050314
  - NiYiand TiVRuns
  - OspreyDataThru012513
  - ScandiumRuns1213and010214
  - SO3536Runs
  - SO4748NbHfMetl1215
  - SO5054
  - Ti99Sc0.5B100.50316
  - Ti99Sc1Runs0615
  - VB10 7.22016
  - VsqrdSO41LiveOsprey0416
  - Zr50Co50Runs1015
  - ZrOspreyo615

Figure 7-3.

Two Sets of 3.5-Inch Floppy Disks from Dr. Passell's Lab

8 Hardcopy Files

An inventory of Dr. Passell's hardcopy files has been completed[11]. They are in three file boxes in his office (Figure 8-1). A total of 218 items was found in the boxes. The files were tabulated in the order found in the boxes as shown in Appendix A1 with their descriptions and dates. Appendix A2 shows the files sorted in approximate date order by year.

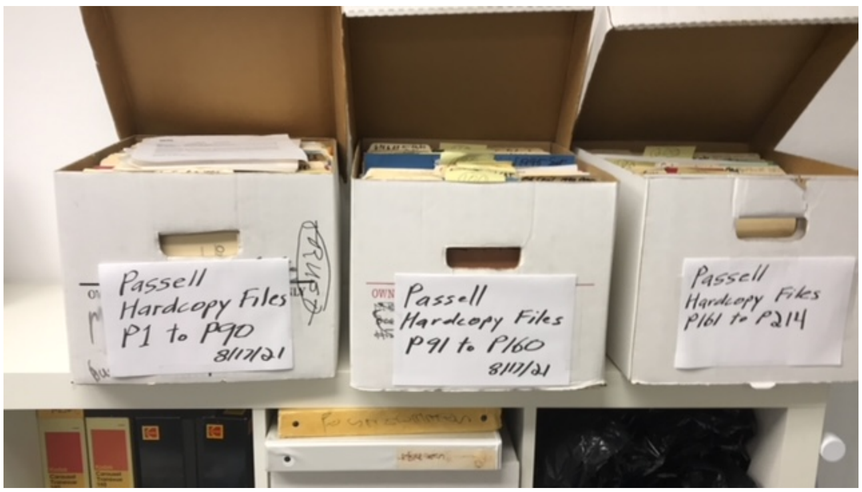

Figure 8-1.

File Boxes Containing Dr. Passell's Hardcopy Files. Four More Files
Photo by Dr. Passell, February 2022.

As the files were reviewed, 43 of them (20%) were selected for scanning. The scanned files have been included in the Passell Project. They are listed in Table 8-1.

11 Hardcopy Files for the Passell LENR Research Documentation Project. Memo to Tom Passell from Tom Grimshaw. December 29, 2021.

Table 8-1.

Items Selected for Scanning in Dr. Passell's Hardcopy Files (in Approximate Date Order)

No	Description	Year
185	Pons D B-10 & C-16. My Paper on B-10. PG AAA. B 10 Depletion. MURR	Undated
115	Tom Passell Dissertation. "Internal Conversion of Gamma Radiation in the L Subshells"	1951
127	Darwin, 1993–4. Correspondence	1983
26	Santa Fe Meeting, May 23-25, 1989	1989
66	First SRI CF Report, 5/89	1989
82	Fleischmann & Pons Original Paper	1989
133	ERAB Draft & Final Report. "Report of the Cold Fusion Panel". November 1989	1989
167	Schwinger Paper and Obituary	1989
172	Meeting 7/6/89 Minutes. Cold Fusion. EPRI-Sponsored Research	1989
28	Monterey Retreat Package	1990
168	Fleischmann Visit, 12/2/91. ICARUS TOP Note1s	1991
6	EPRI/SRI Retreat	1992
53	Russ George Correspondence	1992
114	September 1992 Review of SRI	1992
210	Schwinger, J. Cold Fusion – a Brief History of Mine	1992
3	EPRI Statement	1993
7	SRI Accident Summary	1993
79	Garwin Correspondence	1993
138	Russ George, 2/93	1993
161	Schneider Memos. Kevin Wolf	1993
42	Martin Fleischman Correspondence	1994
43	Fleischmann Reply to Kohn	1994
45	Minsk Meeting, 5/94	1994
67	Two SRI Papers, TECEC, 4/4/94	1994
194	First 1994 SRI Report on Electrochemistry. "Isothermal Flow… Systems"	1994
9	SRI Comments on ICCF-5, "Kevin Wolf Disclosure"	1995
10	Charles Beaudette File	1995
35	University of Missouri Symposium	1995
8	EPRI Advisory Committee, Review of SRI Work	1997
38	EPRI Advisory Committee	1997
78	Arata Samples. Russ George.	1997
124	NAA on C-2. Ben Bush, UT Austin. Cathode of Exploding Cell	1997
191	Beaudette Draft Letter, Fax with Suggested Letter to APS	2000
183	Project Cobalt. Initiation Papers. Ken Shoulders	2002
91	Popular Mechanics, CF & H-Bombs	2004
171	Melich Report. Six Manila Folders: 1) PowerPoint Village Meet, 10/11/06; 2) Nearly to Report (Uncorrected); 3) Fissionable Isotopes; 4) DCS Contract, Melich; 5) Village Report Re: Wolf, 2003	2006
173	Why D+D May Not Occur, 09-05-06. Reprints of Passell & Storms, 9/1/06. Argument for Non-D+D Source of Excess Heat. TOP Notes	2006
29	D2 Fusion Planning	2009
59	Trevithick Meeting, TOP Paper.	2009
48	Oppenheimer-Philips. ICCF-18 Presentation	2013
144	"The Case for Deuteron Stripping with Metal Nuclei As the Source of the Fleischmann-Pons Excess Heat Effect". JCMNS, Volume 15, Pages 1–7	2014

9 Media Collection

Dr. Passell's media collection consists of 38 VHS tapes in 20 sets[12] (Figure 10-1). An inventory has been prepared based on the labels on the labels on the tapes or their containers (Table 10-1). The items (or sets of items) in Figure 10-1 correspond to the numbers of the inventory in Table 10-2.

Figure 10-1.

Dr. Passell's Collection of VHS Tapes

Top Row: Sets 1 to 4 (left to right)
Middle Row: Items or Sets 5 to 12
Bottom Row: Items or Sets 13 to 20

[12] VHS Collection for the Passell LENR Research Documentation Project. Memo to Tom Passell from Tom Grimshaw. May 7, 2021.

Table 10-2.

Inventory of Dr. Passell's VHS Tapes

1. The New Environmentalists. EPRI. EA90–01. Approximately 30 Minutes. Three Copies.
2. Mills. October 6, Wednesday p.m. Tape #1 of 2 and #2 of 2.
3. Cold Fusion & New Energy Symposium. January 20, 1996. Cold Fusion Technology. P O Box 2816, Concord, NH, 03302–2816. Tapes # 1, #2, #3, #4.
4. ICCF-7. Akira Kawasaki.
   Volume 2, Part A.
   Volume 2, Part B.
   Volume 3, Part A.
   Volume 3, Part B.
   Volume 4, Part A.
   Volume 4, Part B.
   Volume 5., Part A.
   Volume 5, Part B.
   Volume 6, Part A.
   Volume 7, Part A.
   Volume 7, Part B.
   Volume 8, Part A
   Volume 8, Part B.
   Volume 9, Part A.
   Volume 9, Part B.
5. The Secret Life of Cold Fusion. CBC Prime Time News. Reporter: Jerry Thompson. Producer: Robin Christmas. Running Time: 27:55. Two Copies.
6. ConFusion in a Jar. NOVA. 1/15/91 Release.
7. Cold Fusion. Bob Kohn. Channel 7. February 7, 1996.
8. Fleischman Boiling Cell. Channel 4. UK.
9. Cold fusion. CBC.
10. Too Close to the Sun. CBC Witness. April 4, 1994.
11. Cold Fusion Composite Tape.
    CBC Witness. 4/4/1994. 49:42.
    Good Morning America. 5/31/1994. 49-58.
    Secret Life of Cold Fusion. CBC. 6/24/93.
12. Cold Fusion. Good Morning America. 5/31/94. 16:39 Minutes.
13. 2nd Annual Conference on Cold Fusion. 1991. Como, Italy. Tape #2. Copyright 1991 Nova Resources Group.
14. Cold Fusion & New Energy Briefing. 1995-1996. Copyright 1995. Cold Fusion Technology, PO Box 2816., Concord, NH, 03302–2816. Tapes #1 and #2.
15. Cold Fusion Day at MIT. 1/21/95. Copyright 1995, Cold Fusion Technology, PO Box 2816., Concord, NH, 03302–2816. Tape #1, Eugene F. Mallove, Peter Graneau. Tape 2, Buford Conley, Fred Jaeger, Peter L Hagelstein.
16. Cold Fusion. Mallove, Preparata. National Press Club. 3/27/92.
17. Untitled. From Gary Boos. Central Hudson Gas & Electricity Corporation. 10/12/94.
18. ACS. October 8, 1999. a.m. to noon. Tapes #2, #3, #4.
19. Cold Fusion: Fire from Water. A Documentary about One of the Greatest and Most Controversial Scientific Discoveries of All Time. 1999. Cold Fusion Technology, Inc. Infinite Energy. 2 Copies.
20. Cold Fusion Briefing Video. Undated. Prepared by Russ George. 3309 Alma St., Palo Alto, CA., 94306.

10 LENR Interviews

Four interviews were conducted with Dr. Passell for the PLCDP. The first two, which were conducted on June 8, 2020[13] and April 23, 2021[14], covered his involvement with and contributions to the LENR field generally. An interview on August 22, 2021 was about his LENR work with SRI International[15]. Then on August 23, an interview took place about his explanation of LENR, the Oppenheimer-Phillips Reaction[16]. The interviews have been transcribed, and the transcriptions have been reviewed and corrected by Dr. Passell[17,18]. The updated transcripts are provided in Appendices B1 to B4.

[13] Transcripts of Interviews on June 8, 2020. Memo to Tom Passell from Tom Grimshaw. June 8, 2020.

[14] Transcripts of Second Set of Interviews on April 23, 2021. Memo to Tom Passell from Tom Grimshaw. April 26, 2021.

[15] Passell Interview for Passell and SRI LENR Research Documentation Projects. Memo to Tom Passell from Tom Grimshaw. August 22, 2021.

[16] Passell Interview for the Oppenheimer-Phillips LENR Explanation. Memo to Tom Passell from Tom Grimshaw. August 23, 2021.

[17] Revised Transcripts of Interviews on June 8, 2020 and April 23, 2021. Memo to Tom Passell from Tom Grimshaw. August 1, 2021.

[18] A memo was not prepared for Dr. Passell's reviews of the August 22 and 23 interviews.

11 Summary and Future Opportunities

Dr. Passell has made major contributions to the LENR field. Many of them took place during his long career as Project Manager at EPRI, where he had major responsibilities in directing funds for LENR research. He already had a sterling 35- year career for several entities in addition to EPRI when cold fusion was announced in 1989. In addition, Dr. Passell contributed not only in his own firm, TOP Consulting, but also in his affiliation with D2 Fusion. He has performed many experiments and has developed his own ideas for theoretical explanation of the phenomenon, the Oppenheimer-Phillips Reaction.

A major future opportunity for the PLCDP will be to annotate the transcriptions of his interviews and develop a timeline of Dr. Passell's work in the field. His lab notebooks and hard copy files could also be analyzed for more detail for his LENR timeline. The electronic files have not yet been secured from his 3.5-inch floppy disks and may be recovered in the future. His collection of VHS tapes could also be digitized and added to the Project. Dr. Passell may also choose to make arrangements for archiving his LENR materials, such as at the Marriott Library at the University of Utah. That library has established itself as a premier destination for LENR archives, in part because the University is the birthplace of cold fusion.

Thanks go to Dr. Fran Tanzella for introductions to Dr. Passell and for facilitating the Project. Gratitude is expressed to the Anthropocene Institute for financial support of the LRDI.

12 Project Methods

The methods used in the Passell LENR Contributions Documentation Project are based on general LRDI procedures[19] that are modified to meet the specific requirements of individual projects. The PLCDP is being performed according to accepted project management practices[20]. The overall LRDI procedure is set forth in an Infinite Energy article[21].

An initial interview was conducted by phone in June 2020. Site visits were then made to Dr. Passell's office and lab in April and August 2021 to document the components of the Project and conduct additional interviews. As progress was made, the findings were documented in a series of memos for the components of the project (Table 12-1). These memos then served as the basis for preparation of this report.

[19] Grimshaw, T.W., 2019. Collection, Organization, and Documentation of LENR Research Results: Guideline. January.

[20] Project Management Institute, 2017. A Guide to the Project Management Body of Knowledge (PMBOK® Guide) — Seventh Edition and the Standard for Project Management (ENGLISH). Project Management Institute. Newtown Square, PA.

[21] Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April 2020, p. 9-13.

Table 12-1.

Memos Prepared for the Passell LENR Contributions Documentation Project

Date	Subject
May 22, 2020	“Passell” Entries on LENR-CANR.org
June 10, 2020	Transcripts of Interviews on June 8, 2020
April 25, 2021	Summary of Office and Lab Visit for the Passell LENR Research Documentation Project (PLCDP)
April 26, 2021	Transcripts of Second Set of Interviews on April 23, 2021
May 1, 2021	Passell Resumes and List of Publications
May 6, 2021	Electronic Files for the Passell LENR Research Documentation Project
May 7, 2021	VHS Collection for the Passell LENR Research Documentation Project
June 23, 2021	Office and Laboratory Photos
June 26, 2021	Description of Notebooks with Laboratory and Other Entries
June 29, 2021	Documentation of Items in Passell LENR Library
June 30, 2021	Wall of Pictures
August 1, 2021	Revised Transcripts of Interviews on June 8, 2020 and April 23, 2021
August 22, 2021	Passell Interview for Passell and SRI LENR Research Documentation Projects
August 23, 2021	Passell Interview for the Oppenheimer-Phillips LENR Explanation
December 29, 2021	Hardcopy Files for the Passell LENR Research Documentation Project
January 1, 2022	Request for Second Review of the "Passell Interview for the Oppenheimer-Phillips LENR Explanation”

Appendix A1. Inventory of Hardcopy Files (Numerical Order)

No	Description	Year	Scan?
1	ASTI Neutron Activation	1999	
2	Congressman Dick Swett	1994	
3	EPRI Statement	1993	Y
4	ICCF-4. Heat Measurements	1993	
5	Deuteron Stripping	Undated	
6	EPRI/SRI Retreat	1992	Y
7	SRI Accident Summary	1993	Y
8	EPRI Advisory Committee, Review of SRI Work	1997	Y
9	SRI Comments on ICCF-5, “Kevin Wolf Disclosure”	1995	Y
10	Charles Beaudette File	1995	Y
11	ICCF-14 Notes. IE Magazine. CD	2008	
12	Les Case “Shot Down”. Deuterium Spillover	2008	
13	Michael Village Conference. CD	2006	
14	“Cold Fusion History”. Noninski	2006	
15	“Evidence For... Titanium”	2012	
16	“Evidence For... Titanium”. Multiple Drafts.	2013	
17	ICCF-18 Paper, Local Lockheed Group	2014	
18	Talk at ACS Local Group, Broad CF	2009	
19	“The Case for Deuterium Stripping... Heat Effect”	2014	
20	Daniel Gozzi: “Not Enough Helium”	2013	
21	Gamma Ray Spectra	2013	
22	ICCF-14 Miscellaneous Material	2008	
23	Gianturco	1992	
24	Tritium Materials	2006	
25	8th International Workshop, Book of Abstracts	2007	
26	Santa Fe Meeting, May 23-25, 1989	1989	Y
27	ICCF-11, Schedule, Etc	2004	
28	Monterey Retreat Package	1990	Y
29	D2 Fusion Planning	2009	Y
30	Ed Storms & Dewhirst/Cram	2000	
31	Neutron Activation Analysis of Pd Samples: Calculations	2002	
32	CMNS Google Group – Storms	2006	
33	ICCF-8 Talk, Lerici	2007	
34	ICCF-12 Paper, D2Fusion	2005	
35	University of Missouri Symposium	1995	Y
36	EPRI Journal, Portions of Issues	1978	
37	ASME Article	1984	
38	EPRI Advisory Committee	1997	Y
39	Translated Abstracts (from Russia), Dubna Meeting	1991	

LENRGY® LLC

40	SIRS Talk, 04/03/13	2013	
41	ICCF-13 Abstracts. Results.	2007	
42	Martin Fleischman Correspondence	1994	Y
43	Fleischmann Reply to Kohn	1994	Y
44	Celani ASTI, 9/06	1996	
45	Minsk Meeting, 5/94	1994	Y
46	Oak Ridge National Lab, CF Work	1990	
47	MIT – Cal Tech Photocopies	1989	
48	Oppenheimer-Philips. ICCF–18 Presentation	2013	Y
49	Kucherov Papers	1990	
50	MURR Gamma Ray Work, MoU	1996	
51	Japan Impurity Analysis	Undated	
52	NIST on NAA (Neutron AA)	1996	
53	Russ George Correspondence	1992	Y
54	Das Kutikad, n-Gamma Data	1996	
55	ICCF-16, Chennai, India	2011	
56	ICCF-14, CD only	2008	
57	ICCF-15, Infinite Energy Magazine	2009	
58	ICCF-6, TOP Paper	1996	
59	Trevithick Meeting,. TOP Paper.	2009	Y
60	B10-B11 Separation Paper	1991	
61	Li-6/Li-7 Separation	2007	
62	Nanotechnology, C&EN Articles	2000	
63	Li-7/B-10 Search	2007	
64	Pd Metal Impurities	1994	
65	EPRI Meetings & Seminar	1992	
66	First SRI CF Report, 5/89	1989	Y
67	Two SRI Papers, TECEC, 4/4/94	1994	Y
68	EPRI Patent Information	1989	
69	Separation of Cl-35 and C-37 Isotopes	1995	
70	Gozzi Samples	1997	
71	CMNS Critical Experiments Workshop (CD Only)	2006	
72	Pd Data. Ewart Blackmore.	2002	
73	University of Texas, Iskandar, Set 5	2000	
74	"CF History", Including 3/24/89 Memo from Passell et al.ll to Santucci	1989	
75	He-3 Bubbles in Palladium Paper	2008	
76	NAA University of Texas, 1997–99. Iskandar	1997	
77	University of Texas in NAA Correspondence, Iskander	2000	
78	Arata Samples. Russ George.	1997	Y
79	Garwin Correspondence	1993	Y
80	Mengoli & Fabrizio	1983	
81	Spzak Paper	1996	
82	Fleischmann & Pons Original Paper	1989	Y
83	Oppenheimer Phillips Supporting References, 11/12	2012	
84	Old Notes. Physics 224. The Deuteron. Fall 53	1953	

85	79 Wolf Speech. Technology and the Welfare of Mankind. General Electric.	1979	
86	Viewgraphs. San Ramon.	Undated	
87	Breeder Talk. Notes. 11/2/81	1981	
88	San Francisco State Talk, 4/11/84	1984	
89	Engineering Public Affairs Talk	1984	
90	Talk to Sammy‘s Class, 4/10/08, with 2x2 Slides	2008	
91	Popular Mechanics, CF & H-Bombs	2004	Y
92	Seaborg & Hildebrand Speeches, C&EN Articles	1976	
93	Fiber Thermometry	2005	
94	Basic Programs, 1960s	1973	
95	ANS Talk, Mar 95. “Whatever Happened… Research“. American Nuclear Society	1995	
96	ICCF-18 Paper. “Evidence for… Hydrogen“	2013	
97	Santa Fe Workshop, May 1989. Partial Contents.	1989	
98	Missouri: MURR Handout	1995	
99	Ed Cecil Proposal, Including “Report on the EPRI Review on Anomalous nuclear Measurements in Deuterium/Metal Systems“	1992	
100	Cold Fusion Papers, 1989. 7 Papers	1989	
101	Trace Elements in Humans. 5 Articles	1965	
102	D2 Fusion Cold Fusion Strategy	2008	
103	Li Xing Zhong, Cold Fusion in China, 8/08	2008	
104	Nanotech Abstract, Passell & Benson	2009	
105	Microballoons	Undated	
106	Taylor & Brown, from Passell	2009	
107	GM Tubes. Invention of the GM	2012	
108	200K Gamma Runs	2013	
109	Szpak Codeposition	2007	
110	ICCF-14 Paper, D2 Fusion	2008	
111	Summary He-4. “Summary of Six-Pack…”	2006	
112	Ed Storms. “Critical Review of the “Cold Fusion Effect“	1995	
113	University of Texas NAA. Results (Arata). Pd A, B, C, D.7/10/01	2001	
114	September 1992 Review of SRI	1992	Y
115	Tom Passell Dissertation. “Internal Conversion of Gamma Radiation in the L Subshells”	1951	Y
116	Osprey Data Analysis & Germanium. Stringham Calculations. Bound Composition Book	2011	
117	Gozzi Papers. Two papers. Calculations	2013	
118	1989 Politics. Papers about Cold Fusion (3)	1989	
119	Data from NAA, 3/20/01. Mostly Data Sheets	2001	
120	Naval Research Lab Report. “A Summary of NRL… Systems”. NRL/MR/6170–96–7803	1996	
121	H Atoms Bound Together. Nature, Volume 356	1992	
122	D. Wu. University of Illinois. NAA. Mostly Data Sheets	1996	
123	Riola Thesis, 2005. “Enhanced… Environments“. From Tom Claytor	2007	

124	NAA on C-2. Ben Bush, UT Austin. Cathode of Exploding Cell	1997	Y
125	Storms ICCF-8. Pt. 2 Papers	2000	
126	India Work on Autoradiography	1991	
127	Garwin, 1993–4. Correspondence	1983	Y
128	Plots of Bkg in B-10 Region 5/97 & Hi Purity Pd. 7/10/97	1987	
129	Cold Fusion Politics. Many Articles.	1989	
130	Current Cold Fusion Interest. Many Articles.	2009	
131	Reference Material. Included in NAA Results	1994	
132	Russ George. September 2012. Story, Haida Salmon. Restoration Corporation. Report	2012	
133	ERAB Draft & Final Report. “Report of the Cold Fusion Panel”. November 1989	1989	Y
134	NAA. 11/99 to 9/01. “This is the basis for 8% delta in Pd208 among Pd A, B, C, & D”. Bound volume	1999	
135	Fritz Will Paper on T. Two Papers.	1993	
136	Sapporo. September 1995. Four Manila Folders: 94 MT Furth; Storms LASL Electrodepo, 2/90; 1995 NHE Papers; Cecil, University of Colorado, 89	1995	
137	Russ George He-4 Talk, 1/21/97	1997	
138	Russ George, 2/93	1993	Y
139	d,p Reactions, 07/01/2012. Envelope	2012	
140	NAA Iskandar.	2000	
141	Glow Discharge Summary	2012	
142	University of Missouri. PGAA. 1997	1997	
143	Seaborg Talk, 1991. Two Others.	1991	
144	“The Case for Deuteron Stripping with Metal Nuclei As the Source of the Fleischmann-Pons Excess Heat Effect“. JCMNS, Volume 15, Pages 1–7	2014	Y
145	The Golem Book. Partial Photocopy	1993	
146	D2 Stripping Paper Drafts, 2012-14. Two Folders. “30th Draft“. ICCF-18 Paper	2014	
147	Recent Cold Fusion Contacts. ICCF-18. List of Participants	2013	
148	ICCF Attendees. Start ICCF-4	1993	
149	Longchampt & Bonnatene. “Reproduction of Fleischmann and Pons Experiment”	1996	
150	Rollison/O’Grady. Surface Segregation	1991	
151	Will-Tritium. Several Papers by Fritz Will	1991	
152	Fellowship Forum Viewgraphs	1988	
153	T. Passell Personal. Various.	1975	
154	ICCF-10. Paper “Pd-110/Pd-108 Ratios… Deuterium Gas“	2003	
155	ICCF-11 Short. “Calorimetry of Energy and Efficient Glow Discharge“. Benson & Passell	2004	
156	ASME Paper. “Review… Research”	1994	
157	Cathode Pursuit, 2/96. Correspondence	1996	
158	PGAA, 11/14/93. Six Manila Folders	1993	
159	Pd PGAA Kutikad. Curves & Plots	1996	

160	Das Kutikad PGAA. 9/19/96	1996	
161	Schneider Memos. Kevin Wolf	1993	Y
162	Six-Pack. Four Manila Folders. Six-Pack a Delta P, 01/02-07. Data; Helium-Tight Glass – Polymers; LANL Lab Pictures, 01–24–07; Helium-4 Analysis of 08/22/05 Data	1998	
163	Passell/Brown at ICCF-11. "Calorimetry… Discharges". Plus CD	2004.	
164	NAA 1995. Data Sheets	1995	
165	Julian Brown. "Towards a Robust Theory of Nuclear Interactions … Deuteron Bands"	2004	
166	Charles Scott. "The Initiation of Excess Power… Heavy Water"	1990	
167	Schwinger Paper and Obituary	1989	Y
168	Fleischmann Visit, 12/2/91. ICARUS TOP Notes	1991	Y
169	Hillig Thesis, 1953, TOP Notes	2006	
170	Kuticad PGAA Data 96. Data Tables & Graphs	1996	
171	Melich Report. Six Manila Folders: 1) PowerPoint Village Meet, 10/11/06; 2) Nearly to Report (Uncorrected); 3) Fissionable Isotopes; 4) DCS Contract, Melich; 5) Village Report Re: Wolf, 2003	2006	Y
172	Meeting 7/6/89 Minutes. Cold Fusion. EPRI-Sponsored Research	1989	Y
173	Why D+D May Not Occur, 09-05-06. Reprints of Passell & Storms, 9/1/06. Argument for Non-D+D Source of Excess Heat. TOP Notes	2006	Y
174	Passell ICCF-9 & 10. Two Papers	2002	
175	ICCF-14 Passell Summary	2008	
176	EPRI CF Initiation Memo by Passell, 3/24/89	1989	
177	Li Paper, ICCF-5	1995	
178	Kim/Passell. Gamma Kim Theory. Paper Given in 1989, NIST/EPRI Meeting	1989	
179	Gozzi. Como 2. 9/95. Main Papers	1995	
180	ICCF-6 Talk & Viewgraphs. PGAA Paper. Toya, Japan. TOP Notes	1996	
181	He-4 Paper on Case. Basic Data on He-4 in Attempt to Confirm Les Case (We Proved It Due to Spillover D)	2006	
182	Liu/Li Permeation. Chinese Paper. ICCF-12	2005	
183	Project Cobalt. Initiation Papers. Ken Shoulders	2002	Y
184	NIST/EPRI Meeting, 8/4–5/93. Speech to NIST & Another One. August 1993. Transparencies	1993	
185	Pons D B-10 & C-16. My Paper on B-10. PG AAA. B 10 Depletion. MURR	Undated	Y
186	Pons Facts of Data. 4/2/98. PGAA on Stan Pons & A, B, C, D of Arata. Done on MURR	1998	
187	P9 & P12 PGAA, 1/14/97. PG AAA. Done at MURR. TOP Notes. Could Be Reanalyzed	1997	
188	N,p Reactions. Cross Sections. (Nothing to Do with Cold Fusion)	1960	
189	Gozzi Substitute Paper, ICCF-4, Italy, December 1993	1993	
190	LENRIA Calendar, 2019	2019	
191	Beaudette Draft Letter, Fax with Suggested Letter to APS	2000	Y
192	Cecil, Liu & Yan. Confirms No Enhancement of (d, D) Reactions. ICCF-2. 1991. Li-6, B-10 & D Branching Ratios	1991	

193	Rollison on Pd Surfaces. Analysis. Paper on Surface Analysis of Electrolyzed Pd. 2 Papers	1994	
194	First 1994 SRI Report on Electrochemistry. “Isothermal Flow… Systems”	1994	Y
195	Melich Report on Pu from U, 4-4–02. Melich Request to See If Wolf Results Could Be Applied to U & Th. “On the Possibility… Thorium“	2002	
196	Melich Report. Source. Data Re Replicating Wolf‘s data	1993	
197	HG Microsphere SRNL Bece 2/09. Microsphere Studies	2009	
198	Coolescence Data, 10/16/08. Trying to Replicate Others’ Claims. 2 Papers	2008	
199	Mengoli Letter. Refusing to Share Any Samples	1996	
200	Action 4/04. Miscellaneous	2004	
201	MIT NAA. Lin Wen Hu, Operator. Searching for Praesidium Melich re Iwamura Claims	2004	
202	David Thompson on D in Pd. Early paper on Johnson-Matthey	1991	
203	Cecil/George Chambers. Observing for Particulate Emission from Deuterium Charged Ti	1991	
204	Reference. Early Papers, Including Paneth Peters, 1926. Several Others.	1926	
205	Yamaguchi 12/1992 Paper	1992	
206	Cartoon Collection. Cold Fusion & Other	Undated	
207	Limericks by Tom Passell	Undated	
208	“Trace Elements Added to Palladium by Electrolysis in Heavy Water“. TR-108743. 2 Copies.	1999	
209	Stringham, RS, and DR George, 1998, Cavitation-Induced Excess Heat in Deuterated Metals. EPRI TR – 108474. March. Also PIs FL Tanzella and M Williams	1998	
210	Schwinger, J. Cold Fusion – a Brief History of Mine	1992	Y
211	BYU Conference, 1990, Anomalous Nuclear Effects on in Deuterium/Solid Systems – An International Progress Review. Abstracts. About 50.	1990	
212	Task Force Talk. “Innovative Nuclear Technology“. Robin Jones	1993	
213	Journal of New Energy, Volume1, Number 1. Hal Fox. Fusion Information Center	1996	
214	Jianyu, He, et al. “Experimental Study in Anomalous Neutron Production in Deuterium/Solid Systems”. AIP Conference Proceedings 228	1991	
215	APS March Meeting. “Helium Production… LENR”	2011	
216	TOP Notebook (Spiral), 5/04/09 to 03/08/12	2009	
217	TOP notebook (Spiral), 02/24/09 to 12/20/09	2009	
218	TOP Notebook (Spiral), 09/08 to 12/04/10	2009	

Appendix A2. Inventory of Hardcopy Files (Date Order)

No	Description	Year	Scan?
5	Deuteron Stripping	Undated	
51	Japan Impurity Analysis	Undated	
86	Viewgraphs. San Ramon.	Undated	
105	Microballoons	Undated	
185	Pons D B-10 & C-16. My Paper on B-10. PG AAA. B 10 Depletion. MURR	Undated	Y
206	Cartoon Collection. Cold Fusion & Other	Undated	
207	Limericks by Tom Passell	Undated	
204	Reference. Early Papers, Including Paneth Peters, 1926. Several Others.	1926	
115	Tom Passell Dissertation. "Internal Conversion of Gamma Radiation in the L Subshells"	1951	Y
84	Old Notes. Physics 224. The Deuteron. Fall 53	1953	
188	N,p Reactions. Cross Sections. (Nothing to Do with Cold Fusion)	1960	
101	Trace Elements in Humans. 5 Articles	1965	
94	Basic Programs, 1960s	1973	
153	T. Passell Personal. Various.	1975	
92	Seaborg & Hildebrand Speeches, C&EN Articles	1976	
36	EPRI Journal, Portions of Issues	1978	
85	79 Wolf Speech. Technology and the Welfare of Mankind. General Electric.	1979	
87	Breeder Talk. Notes. 11/2/81	1981	
80	Mengoli & Fabrizio	1983	
127	Garwin, 1993–4. Correspondence	1983	Y
37	ASME Article	1984	
88	San Francisco State Talk, 4/11/84	1984	
89	Engineering Public Affairs Talk	1984	
128	Plots of Bkg in B-10 Region 5/97 & Hi Purity Pd. 7/10/97	1987	
152	Fellowship Forum Viewgraphs	1988	
26	Santa Fe Meeting, May 23-25, 1989	1989	Y
47	MIT – Cal Tech Photocopies	1989	
66	First SRI CF Report, 5/89	1989	Y
68	EPRI Patent Information	1989	
74	"CF History", Including 3/24/89 Memo from Passell et al.ll to Santucci	1989	
82	Fleischmann & Pons Original Paper	1989	Y
97	Santa Fe Workshop, May 1989. Partial Contents.	1989	
100	Cold Fusion Papers, 1989. 7 Papers	1989	
118	1989 Politics. Papers about Cold Fusion (3)	1989	
129	Cold Fusion Politics. Many Articles.	1989	

133	ERAB Draft & Final Report. “Report of the Cold Fusion Panel”. November 1989	1989	Y
167	Schwinger Paper and Obituary	1989	Y
172	Meeting 7/6/89 Minutes. Cold Fusion. EPRI-Sponsored Research	1989	Y
176	EPRI CF Initiation Memo by Passell, 3/24/89	1989	
178	Kim/Passell. Gamma Kim Theory. Paper Given in 1989, NIST/EPRI Meeting	1989	
28	Monterey Retreat Package	1990	Y
46	Oak Ridge National Lab, CF Work	1990	
49	Kucherov Papers	1990	
166	Charles Scott. “The Initiation of Excess Power… Heavy Water“	1990	
211	BYU Conference, 1990, Anomalous Nuclear Effects on in Deuterium/Solid Systems – An International Progress Review. Abstracts. About 50.	1990	
39	Translated Abstracts (from Russia), Dubna Meeting	1991	
60	B10-B11 Separation Paper	1991	
126	India Work on Autoradiography	1991	
143	Seaborg Talk, 1991. Two Others.	1991	
150	Rollison/O’Grady. Surface Segregation	1991	
151	Will-Tritium. Several Papers by Fritz Will	1991	
168	Fleischmann Visit, 12/2/91. ICARUS TOP Nots	1991	Y
192	Cecil, Liu & Yan. Confirms No Enhancement of (d, D) Reactions. ICCF-2. 1991. Li-6, B-10 & D Branching Ratios	1991	
202	David Thompson on D in Pd. Early paper on Johnson-Matthey	1991	
203	Cecil/George Chambers. Observing for Particulate Emission from Deuterium Charged Ti	1991	
214	Jianyu, He, et al. “Experimental Study in Anomalous Neutron Production in Deuterium/Solid Systems”. AIP Conference Proceedings 228	1991	
6	EPRI/SRI Retreat	1992	Y
23	Gianturco	1992	
53	Russ George Correspondence	1992	Y
65	EPRI Meetings & Seminar	1992	
99	Ed Cecil Proposal, Including “Report on the EPRI Review on Anomalous nuclear Measurements in Deuterium/Metal Systems“	1992	
114	September 1992 Review of SRI	1992	Y
121	H Atoms Bound Together. Nature, Volume 356	1992	
205	Yamaguchi 12/1992 Paper	1992	
210	Schwinger, J. Cold Fusion – a Brief History of Mine	1992	Y
3	EPRI Statement	1993	Y
4	ICCF-4. Heat Measurements	1993	
7	SRI Accident Summary	1993	Y
79	Garwin Correspondence	1993	Y
135	Fritz Will Paper on T. Two Papers.	1993	
138	Russ George, 2/93	1993	Y
145	The Golem Book. Partial Photocopy	1993	
148	ICCF Attendees. Start ICCF-4	1993	

158	PGAA, 11/14/93. Six Manila Folders	1993	
161	Schneider Memos. Kevin Wolf	1993	Y
184	NIST/EPRI Meeting, 8/4–5/93. Speech to NIST & Another One. August 1993. Transparencies	1993	
189	Gozzi Substitute Paper, ICCF-4, Italy, December 1993	1993	
196	Melich Report. Source. Data Re Replicating Wolf's data	1993	
212	Task Force Talk. "Innovative Nuclear Technology". Robin Jones	1993	
2	Congressman Dick Swett	1994	
42	Martin Fleischman Correspondence	1994	Y
43	Fleischmann Reply to Kohn	1994	Y
45	Minsk Meeting, 5/94	1994	Y
64	Pd Metal Impurities	1994	
67	Two SRI Papers, TECEC, 4/4/94	1994	Y
131	Reference Material. Included in NAA Results	1994	
156	ASME Paper. "Review… Research"	1994	
193	Rollison on Pd Surfaces. Analysis. Paper on Surface Analysis of Electrolyzed Pd. 2 Papers	1994	
194	First 1994 SRI Report on Electrochemistry. "Isothermal Flow… Systems"	1994	Y
9	SRI Comments on ICCF-5, "Kevin Wolf Disclosure"	1995	Y
10	Charles Beaudette File	1995	Y
35	University of Missouri Symposium	1995	Y
69	Separation of Cl-35 and C-37 Isotopes	1995	
95	ANS Talk, Mar 95. "Whatever Happened… Research". American Nuclear Society	1995	
98	Missouri: MURR Handout	1995	
112	Ed Storms. "Critical Review of the "Cold Fusion Effect"	1995	
136	Sapporo. September 1995. Four Manila Folders: 94 MT Furth; Storms LASL Electrodepo, 2/90; 1995 NHE Papers; Cecil, University of Colorado, 89	1995	
164	NAA 1995. Data Sheets	1995	
177	Li Paper, ICCF-5	1995	
179	Gozzi. Como 2. 9/95. Main Papers	1995	
44	Celani ASTI, 9/06	1996	
50	MURR Gamma Ray Work, MoU	1996	
52	NIST on NAA (Neutron AA)	1996	
54	Das Kutikad, n-Gamma Data	1996	
58	ICCF-6, TOP Paper	1996	
81	Spzak Paper	1996	
120	Naval Research Lab Report. "A Summary of NRL… Systems". NRL/MR/6170–96–7803	1996	
122	D. Wu. University of Illinois. NAA. Mostly Data Sheets	1996	
149	Longchampt & Bonnatene. "Reproduction of Fleischmann and Pons Experiment"	1996	
157	Cathode Pursuit, 2/96. Correspondence	1996	
159	Pd PGAA Kutikad. Curves & Plots	1996	

160	Das Kutikad PGAA. 9/19/96	1996	
170	Kuticand PGAA Data 96. Data Tables & Graphs	1996	
180	ICCF-6 Talk & Viewgraphs. PGAA Paper. Toya, Japan. TOP Notes	1996	
199	Mengoli Letter. Refusing to Share Any Samples	1996	
213	Journal of New Energy, Volume1, Number 1. Hal Fox. Fusion Information Center	1996	
8	EPRI Advisory Committee, Review of SRI Work	1997	Y
38	EPRI Advisory Committee	1997	Y
70	Gozzi Samples	1997	
76	NAA University of Texas, 1997–99. Iskandar	1997	
78	Arata Samples. Russ George.	1997	Y
124	NAA on C-2. Ben Bush, UT Austin. Cathode of Exploding Cell	1997	Y
137	Russ George He-4 Talk, 1/21/97	1997	
142	University of Missouri. PGAA. 1997	1997	
187	P9 & P12 PGAA, 1/14/97. PG AAA. Done at MURR. TOP Notes. Could Be Reanalyzed	1997	
162	Six-Pack. Four Manila Folders. Six-Pack a Delta P, 01/02-07. Data; Helium-Tight Glass – Polymers; LANL Lab Pictures, 01–24–07; Helium-4 Analysis of 08/22/05 Data	1998	
186	Pons Facts of Data. 4/2/98. PGAA on Stan Pons & A, B, C, D of Arata. Done on MURR	1998	
209	Stringham, RS, and DR George, 1998, Cavitation-Induced Excess Heat in Deuterated Metals. EPRI TR – 108474. March. Also PIs FL Tanzella and M Williams	1998	
1	ASTI Neutron Activation	1999	
134	NAA. 11/99 to 9/01. “This is the basis for 8% delta in Pd208 among Pd A, B, C, & D”. Bound volume	1999	
208	“Trace Elements Added to Palladium by Electrolysis in Heavy Water“. TR-108743. 2 Copies.	1999	
30	Ed Storms & Dewhirst/Cram	2000	
62	Nanotechnology, C&EN Articles	2000	
73	University of Texas, Iskandar, Set 5	2000	
77	University of Texas in NAA Correspondence, Iskander	2000	
125	Storms ICCF-8. Pt. 2 Papers	2000	
140	NAA Iskandar.	2000	
191	Beaudette Draft Letter, Fax with Suggested Letter to APS	2000	Y
113	University of Texas NAA. Results (Arata). Pd A, B, C, D.7/10/01	2001	
119	Data from NAA, 3/20/01. Mostly Data Sheets	2001	
31	Neutron Activation Analysis of Pd Samples: Calculations	2002	
72	Pd Data. Ewart Blackmore.	2002	
174	Passell ICCF-9 & 10. Two Papers	2002	
183	Project Cobalt. Initiation Papers. Ken Shoulders	2002	Y
195	Melich Report on Pu from U, 4-4–02. Melich Request to See If Wolf Results Could Be Applied to U & Th. “On the Possibility… Thorium“	2002	
154	ICCF-10. Paper “Pd-110/Pd-108 Ratios… Deuterium Gas“	2003	

27	ICCF-11, Schedule, Etc	2004	
91	Popular Mechanics, CF & H-Bombs	2004	Y
155	ICCF-11 Short. "Calorimetry of Energy and Efficient Glow Discharge". Benson & Passell	2004	
163	Passell/Brown at ICCF-11. "Calorimetry… Discharges". Plus CD	2004.	
165	Julian Brown. "Towards a Robust Theory of Nuclear Interactions … Deuteron Bands"	2004	
200	Action 4/04. Miscellaneous	2004	
201	MIT NAA. Lin Wen Hu, Operator. Searching for Praesidium Melich re Iwamura Claims	2004	
34	ICCF-12 Paper, D2Fusion	2005	
93	Fiber Thermometry	2005	
182	Liu/Li Permeation. Chinese Paper. ICCF-12	2005	
13	Michael Village Conference. CD	2006	
14	"Cold Fusion History". Noninski	2006	
24	Tritium Materials	2006	
32	CMNS Google Group – Storms	2006	
71	CMNS Critical Experiments Workshop (CD Only)	2006	
111	Summary He-4. "Summary of Six-Pack…"	2006	
169	Hillig Thesis, 1953, TOP Notes	2006	
171	Melich Report. Six Manila Folders: 1) PowerPoint Village Meet, 10/11/06; 2) Nearly to Report (Uncorrected); 3) Fissionable Isotopes; 4) DCS Contract, Melich; 5) Village Report Re: Wolf, 2003	2006	Y
173	Why D+D May Not Occur, 09-05-06. Reprints of Passell & Storms, 9/1/06. Argument for Non-D+D Source of Excess Heat. TOP Notes	2006	Y
181	He-4 Paper on Case. Basic Data on He-4 in Attempt to Confirm Les Case (We Proved It Due to Spillover D)	2006	
25	8th International Workshop, Book of Abstracts	2007	
33	ICCF-8 Talk, Lerici	2007	
41	ICCF-13 Abstracts. Results.	2007	
61	Li-6/Li-7 Separation	2007	
63	Li-7/B-10 Search	2007	
109	Szpak Codeposition	2007	
123	Riola Thesis, 2005. "Enhanced… Environments". From Tom Claytor	2007	
11	ICCF-14 Notes. IE Magazine. CD	2008	
12	Les Case "Shot Down". Deuterium Spillover	2008	
22	ICCF-14 Miscellaneous Material	2008	
56	ICCF-14, CD only	2008	
75	He-3 Bubbles in Palladium Paper	2008	
90	Talk to Sammy's Class, 4/10/08, with 2x2 Slides	2008	
102	D2 Fusion Cold Fusion Strategy	2008	
103	Li Xing Zhong, Cold Fusion in China, 8/08	2008	
110	ICCF-14 Paper, D2 Fusion	2008	
175	ICCF-14 Passell Summary	2008	
198	Coolescence Data, 10/16/08. Trying to Replicate Others' Claims. 2 Papers	2008	

18	Talk at ACS Local Group, Broad CF	2009	
29	D2 Fusion Planning	2009	Y
57	ICCF-15, Infinite Energy Magazine	2009	
59	Trevithick Meeting,. TOP Paper.	2009	Y
104	Nanotech Abstract, Passell & Benson	2009	
106	Taylor & Brown, from Passell	2009	
130	Current Cold Fusion Interest. Many Articles.	2009	
197	HG Microsphere SRNL Bece 2/09. Microsphere Studies	2009	
216	TOP Notebook (Spiral), 5/04/09 to 03/08/12	2009	
217	TOP notebook (Spiral), 02/24/09 to 12/20/09	2009	
218	TOP Notebook (Spiral), 09/08 to 12/04/10	2009	
55	ICCF-16, Chennai, India	2011	
116	Osprey Data Analysis & Germanium. Stringham Calculations. Bound Composition Book	2011	
215	APS March Meeting. "Helium Production… LENR"	2011	
15	"Evidence For... Titanium"	2012	
83	Oppenheimer Phillips Supporting References, 11/12	2012	
107	GM Tubes. Invention of the GM	2012	
132	Russ George. September 2012. Story, Haida Salmon. Restoration Corporation. Report	2012	
139	d,p Reactions, 07/01/2012. Envelope	2012	
141	Glow Discharge Summary	2012	
16	"Evidence For... Titanium". Multiple Drafts.	2013	
20	Daniel Gozzi: "Not Enough Helium"	2013	
21	Gamma Ray Spectra	2013	
40	SIRS Talk, 04/03/13	2013	
48	Oppenheimer-Philips. ICCF–18 Presentation	2013	Y
96	ICCF-18 Paper. "Evidence for… Hydrogen"	2013	
108	200K Gamma Runs	2013	
117	Gozzi Papers. Two papers. Calculations	2013	
147	Recent Cold Fusion Contacts. ICCF-18. List of Participants	2013	
17	ICCF-18 Paper, Local Lockheed Group	2014	
19	"The Case for Deuterium Stripping... Heat Effect"	2014	
144	"The Case for Deuteron Stripping with Metal Nuclei As the Source of the Fleischmann-Pons Excess Heat Effect". JCMNS, Volume 15, Pages 1–7	2014	Y
146	D2 Stripping Paper Drafts, 2012-14. Two Folders. "30th Draft". ICCF-18 Paper	2014	
190	LENRIA Calendar, 2019	2019	

Appendix B1. First LENR Interview, June 8, 2020

Part 1 of June 8, 2020 Interview

Tom Grimshaw: Can you hear me?

Tom Passell: Yeah, I can hear you.

Tom Grimshaw: I just pressed the merge calls and it says that we're both online, so I do believe that we are recording.

Tom Passell: Fire away.

Tom Grimshaw: Okay, good. I'll start with, this is Tom Grimshaw. I'm on the phone today with Tom Passell, P-A-S-S-E-L-L. I should note that today's date is June 8, 2020. The purpose of our call today is for Tom Passell to have the opportunity to talk about his adventures and kind of his history in the area of cold fusion often called now low energy nuclear reactions, or LENR. Tom, it was an important date in many of our lives, some more than others, March 23rd, 1989. When Martin Fleischmann and Stanley Pons held a press conference at university of Utah and made their announcement of their discovery of cold fusion using an electrolytic cell.

If you could start please by just telling us what you were doing at that time. Where you were working, what you were doing, how you heard the announcement and what was your initial reaction and response please?

Tom Passell: Well, this occurred at a time when I had a project with Mike Mckubre and his group at SRI International to measure the amount of hydrogen gas that was dissolved in water and the primary loop of a Pressurized Water Reactor (PWR) nuclear power plant PWR. We were using palladium and the resistivity of palladium as a measure of how much dissolved hydrogen gas there was in the water. That was the purpose of that hydrogen gas – to suppress corrosion in that water loop of the power plant – so we were sort of primed with all the ducks in a row, all the planets in a row to receive a note that there's something interesting happening with palladium and heavy hydrogen.

Tom Grimshaw: Very good. Now, you said you had a project, you were working at Electric Power Research Institute EPRI is that correct?

Tom Passell: Yes, I was a project manager in the nuclear power division of the Electric Power Research Institute.

Tom Grimshaw: Okay. As I understand the way EPRI was organized at that time, that was one of the main positions at that organization, to select and fund and oversee projects. Is that right?

Tom Passell: That's right. I was in a department that was in charge of maintenance of the power system or mostly having to do with correcting corrosion problems.

Tom Grimshaw: Okay. Very good. Let's divert just for a moment. Tell us a little bit about your technical background. Where did you receive your degrees? If you don't mind, in what timeframe and a little bit about your work up to a time you went to work for EPRI please?

Tom Passell: Well, I was a chemist. I got a BS in chemistry from Oklahoma State University (at that time it was Oklahoma A and M) in Stillwater, Oklahoma. I applied for a postgraduate position at four places, the universities of Wisconsin, Texas, Hawaii, and Berkeley. I chose Berkeley. In the fall of 1951, I came to Berkeley and so I was prepared to get my PhD and I had to choose, one of the professors in the University of California, Berkeley chemistry department. I chose to work with Glen Seaborg in the nuclear chemistry arena. I did eventually receive my PhD there in June 1954, having arrived in September 1951. I generally was working on the measurement of beta ray emissions, from all sorts of radioactive species, mostly in the heavy elements above uranium. In general that was the subject of my PhD thesis. That's a brief resume, but-

Tom Grimshaw: Can you tell me very quickly where you worked before you arrived at EPRI after you got your PhD?

Tom Passell: Right after the PhD at Berkeley, I worked at Stanford Research Institute (SRI), but first of all, at the Atomic Energy Division of Phillips Petroleum Company out in Idaho falls, Idaho, for a year and a half. Then I went to SRI International in Menlo Park, California for 13 and a half years from 1955 to 1969. That was entirely on the subject of something to do with the industrial uses or problems of radioactivity and chemistry.

Tom Grimshaw: Okay. Okay. Good. Then did you go from SRI on to EPRI at that point?

Tom Passell: No. I went to Physics International who were doing underground nuclear tests in Nevada for a couple of years, that was 1969 to 1971 later as an employee of Lockheed Missiles and Space. Finally I went back to the Nevada test site on 10 underground nuclear tests from 1971 to 1975, when I joined EPRI for corrosion research in nuclear power plants.

Tom Grimshaw: Okay. Very good. Thank you for that nice summary. I know that was kind of a surprise, but-

Tom Passell: Okay.

Tom Grimshaw: That's good. Okay. You knew Mckubre, you had another project that turned out you had just pretty much an ideal setup from an existing project to engage in maybe some cold fusion work. Tell me how that came about.

Tom Passell: Well, seeing that this was all connected, I went to my other managers at EPRI and asked them to join me in funding and finding money to fund the SRI group. Since I knew they had all the skills and background that would be useful in proving or disproving the claims of Fleischmann and Pons. I couldn't find nobody else in my department willing to co-sign this memo.

Tom Grimshaw: Oh, really? Okay. Interesting. Okay. All right. You we'\re working on the EPRI side, I guess on the SRI side of course, Michael Mckubre was the project manager on that side. Tell me a little bit about the work that they did under your supervision from EPRI.

Tom Passell: They had informed me that McKubre had studied for his PhD at South Hampton, under Fleischmann, Martin Fleischmann, and that was a better connection and that he could get a lot of information directly from Martin as a personal friend or student of his. So I said, well, why don't you switch from the project that I had already signed with SRI on the measurement of dissolved hydrogen gas in the high temperature water in a power plant – switch immediately to seeing if could you reproduce the claims of Fleischmann and Pons.

Tom Grimshaw: Okay, excellent. If you can just recall the course of events after they got started on that research and what you recall of the sequence of things that happened.

Tom Passell: Well, there were a number of committees, including the one by the national organizations that came to visit us, to visit SRI and ask them what they had learned and the very first few measurements they did on palladium against measurements of possible excess heat that they did appear to have produce a few episodes of excess heat, but not enough to say, well, we can turn it on and off, but all they did was they said, here's the data. People from John Huizenga of the national organization, were there to examine their work and visit their laboratory and make contact with us.

They were of course, skeptical of the whole thing. SRI was under continual pressure to show more data. I was around them and chairing some of those meetings around the 6th of July of 1989. That's all I remember off hand. It was a turbulent time and they were hopeful that we could very quickly lay this to rest either true or false in six months.

Tom Grimshaw: Right.

Tom Passell: We went back 31 years ago.

Tom Grimshaw: Yeah. It's been 31 years ago, but we're talking about the Department of Energy engaged what they call the Energy Research Advisory Board, or ERAB of which John Huizenga was the co-chair with Norman Ramsey. Although John Huizenga was really kind of the foremost speaker or manager in that arrangement, as I understand.

Tom Passell: That's right.

Tom Grimshaw: Then he subsequently wrote a book that was highly critical, as we both know. Okay. As you know, I'm also doing a project, similar to what we're doing and have interviews with Fran Tanzella on the SRI International side. What I learned from him was that there was a period of time when you funded SRI to do those initial studies and then there came a time, it was kind of a second phase when the large laboratory was constructed. Do I have that correct?

Tom Passell: Well, I'm trying to remember what Fran is talking about, but we did get a source of money from the exploratory research department at EPRI. I didn't have the restrictions of very much smaller project funding.

Tom Grimshaw: Okay. Okay. That was a different source of money then a second, you said a larger chunk of money?

Tom Passell: Right.

Tom Grimshaw: Okay.

Tom Passell: Almost everybody in various departments at EPRI was angling around and wondering if they should jump in with their budgets, you see. Everybody wanted to be involved in something that big that was so important. There's probably three or four separate groups at EPRI within the nuclear division, I guess, that were trying to find out all they could to see whether they could divert some of that money to answering this puzzle.

Tom Grimshaw: Interesting. Okay. You were kind of the one that was willing to take initially, and once you started to get some results back and so forth, and others got interested as well.

Tom Passell: That's right.

Tom Grimshaw: Okay. As is often the case, basic human nature, of course. The funding from EPRI to SRI then also came from other project managers in addition to yourself?

Tom Passell: I was trying to remember exactly where the funding was I can't quite pull that out of my memory, but ultimately ended up by 1994 in spending six and a half million dollars. Including of course, a recovery from an accident on January 2nd of 1992, where we had a researcher killed by an explosion of heavy hydrogen gas and oxygen. It killed Andrew Riley, former person from the Utah group that had joined McKubre’s group at SRI.

Tom Grimshaw: Okay. Yeah, there was a wait, a year, I guess, that it took almost to get through the investigations and determinations associated with that unfortunate accident before the research could begin again.

Tom Passell: That's right. It took a year to reconfigure the laboratory in order to be more safe in the handling of gaseous D2 and oxygen that was being separated, of course, in the electrochemical cells that were closed with the recombiner. The recombiner failed and that's what caused the accident.

Tom Grimshaw: Yeah. Yeah. Okay. Well, thanks for reminding me about that. That was an unfortunate incident on the long trail of cold fusion. You continued to be involved with SRI, even though others may have been the project managers and providing funding, right up to 1994 then.

Tom Passell: 1994 was about the end of that money from EPRI to SRI. They had not quite proven that the excess heat was due to the nuclear process, but they had made lots of progress and we ended up sponsoring the fourth International Conference on Cold Fusion in Maui in December of 1993.

Tom Grimshaw: Okay.

Tom Passell: Mckubre and I were co-chairmen I think, on that one and they had a record turnout.

Tom Grimshaw: There was a very nice set of volumes of the proceedings and so forth. Abstracts of proceedings and so forth, published by SRI-

Tom Passell: That's right. There were four volumes, I guess, altogether.

Tom Grimshaw: Yeah. Yeah. Anything else come to mind about that period of time from 1989 to 1994 that you've been talking about?

Tom Passell: Well, we were, I can't recall all the meetings that we had, but we generally were trying to first of all, prove that there was in fact excess heat in the first place. A lot of people had difficulty doing accurate calorimetry and so that was one of the biggest issues. Is there really, any excess heat? Okay. Let alone the nuclear processes, first of all, are we getting heat? I think pretty much the best examples of that again, by Fleischmann who really knew that whole process very well indeed. He kept working on it for several more years.

I think the effort that SRI, we tried to make sure that light hydrogen wouldn't do it and heavy hydrogen would, and at the same time making very careful measurements of calorimetry. We did get this one set of cathodes that showed yes, you got this with heavy hydrogen and no, you didn't get it with light, but that was notable by the rarity of getting any heat at all, such that it was difficult to make any firm proof of it.

Tom Grimshaw: In your estimation, just kind of a bottom line question. What do you think about this phenomenon called cold fusion or LENR, do you think it's real?

Tom Passell: Well, I was pretty sure that it wasn't the standard well-known D+D reaction to get tritium and protons or helium four. I was pretty convinced that it may very well be a nuclear reaction without being D-D reactions. In other words, that the impurities in the palladium might be a significant factor and the main impurity in palladium that we were using was calcium boride, which they added, as a getter for oxygen in pouring the metal. I thought, well, maybe it's a reaction of boron or boron isotopes that's producing the heat and the reason why we will have a batch effect between samples was the degree to which you had impurity boron present from that initial pouring of the metal to be melted and the source you used as a rod.

Tom Grimshaw: Yeah. Okay. Now we're getting into the theory or understanding or explanation of Tom Passell, so thank you for that.

Tom Passell: I was pretty much alone in thinking the impurities were significant. Almost everybody and many people are still convinced that it's a modified D+D reaction going directly into helium which is of course, a very rare event in fact, ten to the minus seventh of the reactions are going that way, and the others are going to tritium and protons. There was a lot of effort done to measure tritium, and something believable of tritium was observed, but not nearly enough tritium to explain the main source of excess heat.

Tom Grimshaw: Right. Well, you certainly weren't alone in that thinking of the influence of a boron, both Tom Claytor and Melvin Miles, I think went down that path.

Tom Passell: They are lot more recently on that, in those early days, they weren't particularly vocalizing that although there was a good person there at the Naval Research Lab (Imam) who made more alloys of palladium and boron and found that – with Miles work – that boron, with three quarters of a percent or something like that by weight, 10 times that by atoms, was producing excess heat of a modest amount, again, down in the middle range of milliwatts.

. Tom Passell: Imam is the person that I was talking about.

Tom Grimshaw: Yeah. Yeah. Any other words about your explanation before we move on? I think this is almost a unique kind of a viewpoint that you brought in those early months and years.

Tom Passell: Well, I thought it unfortunate that everybody decided to believe that somehow the nuclear reaction was completely distorted. You didn't see anything, but helium and even the helium measurements were controversial as you know there's four parts per million of helium in the atmosphere. They were always subject to “guilty until proven innocent” of contamination of helium when they did see helium. Gozzi in Italy later found helium, but not enough to explain the heat. Okay. All of these pointed me to an alternate explanation besides the DD reaction.

Tom Grimshaw: Okay. Can you tell us a little bit about how boron would play a role in a nuclear reaction in a palladium lattice with tritium in it?

Tom Passell: Right. First of all, I thought, well, that was just a D alpha reaction, which will make some helium, but the main energy was from the particles being born. Then only later did I think that the (D,p) reaction, the Oppenheimer Phillip's reaction might be the excess heat producer with the production of the proton from the Dp reaction can make the P alpha reaction to make beryllium 7 as a secondary thing, but at least does, or the boron 10 was definitely the focus of my interest because it has the largest energy involved in its reactions. I even went to the University of Missouri to measure their online beam or their neutron beam to find out if we were finding a depletion of boron 10, somehow related to the amount of heat that was claimed for a particular palladium electrode.

Tom Grimshaw: Okay. Do you mind if I ask who at the University of Missouri you were working with at that time?

Tom Passell: I was trying to recall the names and I couldn't quite come up with them. I'm sorry. 1995 that this is occurring.

Tom Grimshaw: Okay. Yeah.

Tom Passell: I think I know who it was and his name will come to my mind later. I think he himself who we're talking about was a student of George Miley at one time.

Tom Grimshaw: Okay.

Tom Passell: Well, I think CJ Lai was one of the students involved in it. I can't remember any more of those people, but they were very interested in the project. We couldn't measure boron 11, but they could measure boron 10 just by observing the gamma ray in lithium 7 from thermal neutron captured in boron 10.

Tom Grimshaw: How did you feel about those experiments? Did you feel they were successful in terms of your quest for the explanation?

Tom Passell: Yeah. There were alternative explanations for the boron 10. One was just plain old leeching. We didn't do enough of them to be definitive, but they were all very interested there and they were very cooperative and helpful, but that was a project that I would like to pursue if I live so long.

Tom Grimshaw: Yeah. Yeah. You had gone to Missouri for the purpose of inquiring or investigating the role of a boron 10 in excess heat. Did you feel that you were successful in that?

Tom Passell: Well, it was plausible let's put it that way, but not definitive.

Tom Grimshaw: Okay. Got it. All right. We're going to go beyond 1994 in a minute. Before we get past the point where the funding ran out or ran down at EPRI. Were there other people during that timeframe at EPRI, that you were providing funding for, besides SRI?

Tom Passell: Sometimes with Hagelstein and some with the people that were doing work with cavitation under heavy water interface with palladium

Tom Grimshaw: The name Roger Stringham comes to mind.

Tom Passell: Yeah, Roger Stringham was the guy who was most active in that arena.

Tom Grimshaw: He was formerly at SRI.

Tom Passell: That was small dollars of course, $50,000 or something less.

Tom Passell: He was in an independent little company.

Tom Grimshaw: Oh, okay.

Tom Passell: I've forgotten the name of it. Now it's changed it's name several times. Roger is very highly skilled at the whole business of cavitation and its uses and abuses and so he thought that he could load palladium metal, exercising cavitation with palladium submerged in the heavy water.

Tom Grimshaw: Okay.

Tom Passell: Yeah. He rode that horse for a long time, still is. That's why I think he has a recent paper, I haven't reviewed it but he and the Los Alamos people have produced a paper involving cavitation, [but that’s] an unlikely diversion.

Tom Grimshaw: Yeah. Yeah. Is there anyone else coming to mind? I think from a previous conversation I had with you I'm thinking of somebody at Texas A and M.

Tom Passell: Well, Hagelstein was in the theory and we were always trying to get him to the meetings to explain what we thought we saw in experiments, but experiments never have been so reliable as to be beyond suspicion.

Tom Grimshaw: Yeah.

Tom Passell: The fact that the most effective of all of the subsequent measurements was by a group headed by Daniel Gozzi, G-O-Z-Z-I of Italy. I thought he had the most convincing of all the experiments, but I didn't ever really get a chance to fully work with him. That was much later of course.

Tom Grimshaw: Yeah. Yeah. The funding that Hagelstein got for his work, as I recall came through SRI or were you directly funding-

Tom Passell: It was directly with Hagelstein.

Tom Grimshaw: It was okay. Excuse me. Okay. Now, I'm going to bring up this one and you can tell me what you want to expound about or not, but I think we did talk about Kevin Wolf at Texas A and M, want to talk about that or not?

Tom Passell: Well, that was not a good chapter of our history and actually, Mike Melich wanted to prove or disprove the claims of Kevin Wolf, who claimed the electrochemical experiment produced 100 gamma rays that can be identified very precisely for various isotopes in the palladium region. Could I use some money and prove that was the process? This of course was not related to excess heat measurements. It was just obviously a definite nuclear reaction. Nobody can deny it. It was a nuclear reaction.

Tom Grimshaw: Yeah.

Tom Passell: With all those gamma rays. What I proved was that you could get the same results by irradiating palladium of six millimeter diameter palladium rod and an 80 Mev proton beam which Kevin Wolf had access to in the floor above his lab in Texas A and M. Kevin Wolf made claims that he had three electrochemical cells and he had subjected them to liquid nitrogen so that it was freezing them for 24 hours and then upon coming back to normal temperatures that he was observing this set of the gamma rays in all three. Well, that was entirely unlikely from everybody's point of view that you could freeze those electrodes and expect anything good to happen other than widespread fracture of glass or plastic ware.

Tom Grimshaw: Yeah.

Tom Passell: In any case, it was a case of Kevin Wolf really giving us false information and that nobody, as far as I remember ever tried to really reproduce that what he said he had done. However I felt that in order to get someone else to try it in 1995 in Monaco in that fifth international conference.

Tom Grimshaw: Right. Okay.

Tom Passell: During that time there was Douglas Morrison was one of our chief critics. He gets up and says, well, did Kevin Wolf give you permission to reveal this information? He had sort of given it to the hierarchy at EPRI but did not publish it. He did give to us a bunch of view graphs. Well, I thought it was a wonderful case to find a nuclear process, but I was disappointed of course, and finding that it was a well known nuclear process, high energy protons and multiple neutrons coming out.

Tom Grimshaw: Okay.

Tom Passell: Anyway, it was a sad outcome, but I didn't want to put out more negative information that we were already under that gun about... We were always guilty until proven innocent.

Tom Grimshaw: Yup. Yup. That's the nature of the way it's gone. Thank you for that, that's solving off on that. Does anybody else come to mind that you were working with while still at EPRI?

Tom Passell:	Well, not out of hand, there were several other contracts, small, much smaller nature and I honestly don't remember anything significant coming from them. There were some people who thought that light hydrogen and the shrinking of it could be something that we ought to investigate. I was never convinced that would be possible. First of all, it's your word that you were converting hydrogen of normal ground state to a deep dirac, D-I-R-A-C states that would give you 75 kilovolts net reaction heat, which is of course not at all impressive, in the sense of solving our energy problem and furthermore, very unlikely from a theoretical point of view. I never did pursue that, but a number of people thought that would be the way you were shrinking the hydrogen atom. Remember that, "Honey, I Shrunk the Kids" or something?
Tom Grimshaw:	Old movie, yeah.
Tom Passell:	That was the whole movie. Yeah, an old movie. Anyway, there was a guy by the name of Robert T. Bush at Pomona who was pursuing that theory with his work, because he was seeing apparently some excess heat with light water instead of heavy water.
Tom Grimshaw:	Okay.
Tom Passell:	I guess it was.
Tom Grimshaw:	Yeah. Yup.
Tom Passell:	Well, those are the memories I have from that period, and I could never be convinced about Srinivasan's research – some of the light hydrogen research (that) was coming out of India through Srinivasan. He thought he had seen some excess heat with light hydrogen. He came to SRI to prove it or disprove it and he disproved it there at SRI from the particular set of small number of measurements that he was able to do. Down a lot of blind alleys that people were still, even to this day, interested in pursuing it because it's such a big pot of gold at the end of the rainbow.
Tom Grimshaw:	Absolutely. It would change everything. Let's do a reality check here. I would like to continue this process for the time after your EPRI involvement with cold fusion. I know you have your own laboratory now, for example, would it be okay to have a second round of this interview? Would right after this be a convenient time?
Tom Passell:	I suppose so, not saying right after this. Not immediately, right?
Tom Grimshaw:	Your choice, I'm available anytime you are. We've been at this for 40 minutes. I try to make them about 30 minutes to 45 minutes in length and then kind of take a break and then come back and pick it up. What would work good for you?
Tom Passell:	Well, we can do what we can do now and then decide later how much more to do.
Tom Grimshaw:	Okay, good. Well, let me wrap this one up and I'll call you back like I did before, and I'll wrap it up by going through the routine. This is Tom Grimshaw. I'm on the phone with Tom Passell talking about his cold fusion adventures, going all the way back to 1989 when cold fusion was announced by Martin Fleischmann and Stanley

Pons. I should note that this is June 8, 2020, and I'll call you right back and we'll continue this.

Tom Passell: Okay. Thanks very much. Bye.

Tom Grimshaw: Bye.

Part 2 of June 8, 2020 Interview

Tom Passell: Okay. We're back.

Tom Grimshaw: Yeah. We're back.

Tom Passell: Okay.

Tom Grimshaw: Yeah, and I did my merge call and I did check the previous recording and it's good.

Tom Passell: Oh good.

Tom Grimshaw: We're working well.

Tom Passell: Good.

Tom Grimshaw: I'll start with the routine again. This is Tom Grimshaw. I'm on the phone with Tom Passell, P-A-S-S-E-L-L. Right? Two Ls?

Tom Passell: Two Ls, yeah.

Tom Grimshaw: We're having this session to talk about Tom Passell's adventures in cold fusion, going back to when it was announced in March of 1989. This is our second interview. The previous one was 40 minutes and so we'll just take it from there. Let's see. I think I mentioned the date. I'd, better do it again. June 8th, 2020.

Tom, in our last interview, we covered a lot of ground from the time where we covered a little bit on your pre-cold fusion history, your career. Then we talked about your initial reaction to the 1989 announcement. Then your work at EPRI, as a project manager working primarily with SRI international, but with a couple of other people as well. I think the name Peter Hagelstein and Kevin Wolf came up in our last interview. That took us up to when the funding ran out in 1994 for cold fusion support. Tom, can you pick it up there? What was your involvement in this field after the funding ended at EPRI?

Tom Passell: I guess I was attending all the various meetings that I didn't have as much to contribute. I was very curious to see whether or not the main idea, number one, is there really excess heat. I'm pretty much convinced that there is real excess heat, but there is never, as far as I can see, been proof of it. Almost certainly from its amount, it almost probably has to be a nuclear process, but the exact nuclear process is not the D+D fusion that's been modified from this normal nuclear pathway.

To me, it's very unlikely that we have on our hands a modified nuclear process with D + D. It's more likely that there's some hidden reaction that just does not produce anything that you can see outside of the experiment. I have concluded that the only plausible alternative that would be showing these significant results is one in which we're getting an Oppenheimer-Phillips reaction on some of the elements, almost certainly the impurity elements in the palladium, and perhaps in the few cases where they used titanium and got excess heat, some of the lighter elements around the titanium, scandium and at that area. So that's been the focus of my subsequent very small efforts that I've done since I retired in 1997.

Tom Grimshaw:	Okay. So, you were three years, for three years at EPRI until you retired, during which there wasn't really too much going on in cold fusion with EPRI's support. But were you continuing your own work? You said you attended the conferences, primarily the ICCF conferences, probably.
Tom Passell:	That's right.
Tom Grimshaw:	When did you start your own laboratory work?
Tom Passell:	Well, I had a... I got the laboratories set up probably in 2001 with Bill McCarthy. Then I finally got my own lab separate from his in January 2008. That's when I had been working more independently and doing other kinds of measurements.
Tom Grimshaw:	Okay. So from 1997 to 2002, you were still active in the field. I suppose you were staying in touch with McKubre and Tanzella and those guys and what they were doing?
Tom Passell:	That's right. As best I could. Yeah.
Tom Grimshaw:	Yeah. So, tell us a little bit about your work with Bill McCarthy from 2002 until 2008.
Tom Passell:	Well, Bill McCarthy of course, had a lot of interesting ideas in the metallurgical arena. Bill passed away in 2021. He was a metallurgist. He was pursuing the possibilities of lithium that might be involved. So, lithium of course, was in the electrolyte of all the electro-chemical experiments. So, that would be where it would come from. Boron, having been added on purpose prior to the use of the metal. But he was also thinking that if it is lithium or something else in the way of light elements, it was something that would happen in other elements like molten iron. In other words, you could put lithium into molten iron and get the lithium nuclear reactions. Palladium was not involved in his estimation. Anyway, that's how he has pursued the business.
Tom Grimshaw:	Yeah. Yeah. So he was working with high temperatures, obviously.
Tom Passell:	In order to get this done... In order to get the reactions that he was working with lithium and generally... And he was also hopeful if he did detect excess heat at those higher temperatures, that it would be much more useful excess heat than the excess heat you got from an electrochemical cell.
Tom Grimshaw:	Yes. A thermodynamic reality.
Tom Passell:	Right.
Tom Grimshaw:	Okay. So what was your activity during that period in this cold fusion area?
Tom Passell:	Well, I thought, I would have just electrical discharges that would simultaneously load a metal with heavy hydrogen. Heavy hydrogen is the gas and that would possibly give some results. I must admit that all of my long time running... Running for months at low power, like a few Watts and electrical discharges and heavy hydrogen gas with the elements I'm testing as the electrodes. Nothing has come of that. I wasn't trying to measure excess heat. I was hoping just to measure gamma rays from the radioactive isotope produced by the Oppenheimer-Phillips reaction. That would be the indicator that we had a nuclear process.

Tom Grimshaw: Okay.

Tom Passell: All those experiments failed to show any nuclear gamma rays from the hypothesized D,p reaction of the Oppenheimer-Phillips type.

Tom Grimshaw: Have you had the opportunity to prepare reports? I know sometimes it's difficult to do when you're getting negative results, but I was just wondering if there is a record of your work there.

Tom Passell: No, I haven't spent the time to do that. Like most people, they don't like to spend a lot of time on recording negative results.

Tom Grimshaw: Right. Yeah. Yeah. Okay. So, that was the time from 2000. Let's see, 2002 to 2008. What about since 2008? Can you bring us up to date on what you've been doing?

Tom Passell: Most of that was the electrical discharges since then. Then for the last three years or so, I haven't been able to do much with that. I've been thinking of other ways to get a high surface area and a flux of heavy hydrogen in various metals. That's what I would like to pursue if I get a chance.

Tom Grimshaw: Okay. So the work both... All the way from 2002 then was primarily the discharge work.

Tom Passell: Well, the discharge work has been mostly since I was in my own lab from 2008.

Tom Grimshaw: Okay. And what... Okay, so back to the 2002 to 2008 timeframe, you were working with…

Tom Passell: Well, Tom Benson.

Tom Grimshaw: Of course, you have been, I'm sorry.

Tom Passell: Tom Benson was working with me during a lot of that time with D2 fusion, with Russ George and Tom Benson.

Tom Grimshaw: Oh, okay.

Tom Passell: That came and went from about 2005 to 2007, that period.

Tom Grimshaw: Oh, okay.

Tom Passell: We didn't get any successes from that work, but we did find that some of the reports of helium were not true. In other words, they weren't coming from the sample, but from some other source.

Tom Grimshaw: Okay. Interesting. Okay. So, that's what you can recall from that Bill McCarthy period, was the work you did with D2 fusion?

Tom Passell: Well, McCarthy and the D2 fusion were not connected.

Tom Grimshaw: Yeah. Okay. So, yeah. Thanks for keeping me straight on. I'm sorry to be...

Tom Passell: No, it's a little fuzzy in my mind too.

Tom Grimshaw: Okay. So, the actual... With Bill McCarthy then, he was doing his own thing as we talked about before. And you did a separate thing with D2 fusion. The gap I'm trying to fill in my mind is, were you doing anything of your own while you were affiliated with McCarthy's lab?

Tom Passell: Not really. No. I wasn't working very closely with Bill. He was pursuing things that required molten iron, and I wasn't comfortable with working with molten iron.

Tom Grimshaw: I can certainly understand that. Okay. All right. So, your primary thought then on this phenomenon, I've heard you say a couple of things. One of course, the Oppenheimer-Philips, and the other is the role of boron. Those are two things that I've heard about your understanding of this phenomenon.

Tom Passell: Right. Boron-10, by the way.

Tom Grimshaw: Boron-10. Thank you. Okay, good. All right. So if you had infinite time and money, what do you think needs to be done?

Tom Passell: Well, I would like to make thin-film boron 10. A large area thin-film of all the various possible elements that could produce a gamma ray from their Oppenheimer-Phillips reaction and prove that the nuclear process is possible at modest conditions such as these.

Various schemes that have come to mind have been using 3D printing to get a large area, small-size sample. A large area, exposed to the gas, and then the smallest sample size, rather than an acre of thin films. I thought that maybe 3D printing would be able to produce such a sample and I'm pursuing seeing if that's true or not.

Tom Grimshaw: Yeah. What are some examples of substrates and films that you might consider?

Tom Passell: Well, I guess I'd like to find a memory alloy that has boron in it, for example. A lot of memory alloys are titanium and nickel, and some of them have a little cobalt and copper, but... So I would... I'm thinking of elements like cobalt that show a very definite gamma ray if they do experience a D,p reaction, element 27. All the elements, all the alloys of cobalt and that have hopefully some... I've yet to find a memory alloy that has any boron in it. But the idea of the memory alloy was that at a particular temperature, you'd get a shuffling of the positions of the atoms between austenite and the other configuration metal (martensite). The memory alloys go through this transition every time you pass that transition temperature. So, I thought if you could get a large rate of temperature fluctuation and getting a large flux of the shuffling of those atoms between the two – martensite and austenite – that you would maybe have conditions that are such that bring about a D,p reaction in the metal.

That's what I'm pursuing. I don't know if there are such alloys. There are a tremendous number of memory alloys, and just because ones that are most common are nickel and titanium doesn't mean that there aren't a lot of others.

Tom Grimshaw: Sure. So what you just described, would that be taking place in the transition metals in the substrate or in the films?

Tom Passell: In the films.

Tom Grimshaw: In the films.

Tom Passell: This is trying to make the whole film an active agent.

Tom Grimshaw: Okay.

Tom Passell:	That's the dream anyway.
Tom Grimshaw:	Yeah. And so, not to be too persistent, what would you put the films onto?
Tom Passell:	That's why I'm trying to make that solid, rigid foam of the material. So that gas always has access for going in and out of the metal readily without having to penetrate deep into it.
Tom Grimshaw:	Okay. Interesting. So, I saw your lab very briefly. Would it be possible for you to do this if you found the right material and had money?
Tom Passell:	Yeah. Right. Well, I hope so. I'm hopeful that the same apparatus that I have... I have gas. I have the heavy hydrogen gas and it's no liquids, no electro-chemical stuff. It's just a gaseous diffusion in and out of, at a high flux, the active material to form any old (D,p) reaction whatsoever. They're all nuclear, they all produced a tremendous about of heat. Therefore they would not have anything in them that is what we would call related to D-D, it has to be a (D,p) reaction.
Tom Grimshaw:	Okay. And in the normal language of this field, would you call it gas loading?
Tom Passell:	Yeah. I would. Gas fluxing.
Tom Grimshaw:	Gas fluxing. Yeah.
Tom Passell:	That's what I would like to have, a high flux. I don't mind having gas be relatively stable. If you use memory alloys, let the metals move, you see? As they do in that transition between the two states of a memory alloy.
Tom Grimshaw:	Very interesting idea. Very interesting. Okay.
Tom Passell:	Well, I hope somebody picks it up if I can't.
Tom Grimshaw:	Yeah. So, of course I have to ask you this. Have you written any of this down as far as how you would go about doing it? Are there any-
Tom Passell:	No, I haven't. I haven't put down any reports yet. No. I was going to try it out first and then see if it works.
Tom Grimshaw:	Yeah. It's a really tantalizing idea. Gas flux. Makes a lot of sense. And with these memory metals going through transition and rearranging the atoms around, it sounds like a really neat idea.
Tom Passell:	It is based on a hypothesis that a flux of heavy hydrogen past the metal is the essential action that would allow the (D,p) reaction.
Tom Grimshaw:	Yeah. Yeah.
Tom Passell:	But that's a hypothesis that has by no means been proven, of course.
Tom Grimshaw:	Yeah. Are there others that you've talked to that advocate Phillips-Oppenheimer explanation, or are you kind of a lone guy?
Tom Passell:	Well, I'm looking for alloys memory alloys that have the boron in them at the moment.
Tom Grimshaw:	Yeah. I was wondering-
Tom Passell:	I have yet to find one.
Tom Grimshaw:	Yeah. Are there other people that adhere to the Phillips-Oppenheimer idea?

Tom Passell: Very few. I haven't run into any. I did get communication from Mel Miles quite awhile back, a few years ago. I just haven't gotten engaged with him because he's always been doing electrochemistry and I don't think electrochemistry is likely to be the winner, but I could be wrong.

Tom Grimshaw: Yeah. Yeah. So, you're not alone in that opinion. I mean, it's might be a good tool of investigation for some things. But anyway... So, any other thoughts about this fine field?

Tom Passell: Well, I think it's still worth pursuing because of the pot of gold at the end of the rainbow is sufficiently large.

Tom Grimshaw: Yeah. Yeah. Well, there are two lures. I think one of course is the financial. The other is just the new physics idea. If this phenomenon exists and most of us are convinced that it does, to explain it, eventually to come to an explanation like yours or another one would mean a whole new area of scientific investigation.

Tom Passell: Right. That's exactly right. But it could be done in a simple small lab too.

Tom Grimshaw: Yeah.

Tom Passell: If my hypotheses are correct.

Tom Grimshaw: Yeah. Well, we're not there yet. I'm not so much interested in the pot of gold myself. I have more than I can spend as it is, but I am having a lot of fun doing this.

Tom Passell: I bet you are. Well, that's the whole business of ideas. They've even tried to draw in, apparently, Stringham and Claytor are involved with trying to explain it with high frequency sound. They're bringing in the theory of mesons as being active agents in this process. I have yet to review that draft. I was asked to review it but I haven't had time to do it. Then someone else would have to redo it. I think that's a pretty long shot to get into mesons, but I could be wrong.

Tom Grimshaw: Yeah. Yeah. I totally know the answer just by hearing it. Some things are more likely than others though, of course.

Tom Passell: That's right.

Tom Grimshaw: Okay. Well, I'm quite aware of the recent change in your life with your wife, Tom. I want to really thank you for taking the time and putting the effort into these two rounds of interviews. What'll happen next is I will submit these for transcription. I'll get back Microsoft word files, which I will then share with you for you to review and make modifications or corrections. You know, it's a request, but nobody else will see these without your knowing who it was.

Tom Passell: No. It's a labor of love on your part. We appreciate it.

Tom Grimshaw: Okay. So, in the future, under normal circumstances, pre-COVID-19, I would ask to come and pay a visit to you. Usually, I do these interviews in person, but then we would survey records that you have, including any lab notebooks that you have, electronic files, hard copy files and so forth. Perhaps we can do that in the future, once the future gets a little more certain on this bug that is bugging everybody.

Tom Passell: Yeah. Well, that's another impediment. They've had a lot of scientific impediments too.

Tom Grimshaw:	Yes we have. A sociology of science impediments is what I call them. It's how we do science.
Tom Passell:	I am aware that if we should be so successful as to have a new heat source, that makes obsolete others, that we'll run into a little bit of opposition here and there.
Tom Grimshaw:	Yeah. No kidding. Yeah. Yeah, if you buy into conspiracy theories, there you are.
Tom Passell:	Well, they shouldn't really worry because it'll take a long time to develop, I'm sure.
Tom Grimshaw:	Yeah. Yeah. Okay. With that fine note, I'll thank you again, Tom. Look for a cover memo with those transcripts coming in the next maybe a week or 10 days. Okay.
Tom Passell:	Okay. Thanks for the work. I appreciate it.
Tom Grimshaw:	All right, take care. Bye bye.
Tom Passell:	Bye bye.

Appendix B2. Second LENR Interview, April 23, 2021

Part 1 of April 23, 2021 Interview

Tom Grimshaw: Okay. This is Tom Grimshaw. I'm here with Tom Passell. It's April 23rd, and we're here at his office and we're doing an interview about Tom's journey in cold fusion. But Tom, we'll start with a few words about your educational background. Well, actually your roots in Kansas and where you got your degrees and what you did before cold fusion. When we get to the end of that, we'll pause on this recording and make sure we got a good recording. So Tom, pick us up. Where you originally from, and where did you get your degrees?

Tom Passell: Well, I was born in Chicago on November 24th, 1929. Quite a few years ago and just before the Great Depression got started. My mother had married a farmer in Western Kansas who had an 11th grade education. She had a degree from Northwestern in biology. She was the 10th of 10 children in a Swedish family there selling coal and ice in Chicago. I grew up in Colby through seventh grade, and then the eighth grade in Page city, Kansas, a little town of 86 people south of Colby. That's in the northwest corner of Kansas up in the dust bowl.

In 1943 we moved down to Wichita, Kansas, where I was able to get a better education, and went to junior high and high school at Wichita East High School, and finally Wichita University for three out of my four years of college. I decided to be a chemistry teacher, but I basically wanted to be in the sciences, and so I had a good chance to have small classes. This is right after World War II of course, '47, with a lot of returning veterans, and so the competition was a little bit more than normal.

In any case, I had a very good experience there at Wichita U, and then along comes the Korean war in 1950, and I was wanting to avoid that war. So I went down to Oklahoma A&M, which is now called Oklahoma State, in my fourth year in college in 1950.

In general, my social life was tied up with a Baptist church there in Wichita, so I didn't have to join fraternities and things like that. I got a chance to learn how to speak before groups in that youth group dealing with religion.

Tom Grimshaw: Okay, good. So your background was basically in Western Kansas, which we somewhat share. I was born in South Dakota and went to college in Rapid City in western South Dakota. So we share that a little bit in common, although you preceded me by a few years. I think from that point you went to a graduate school, and I think this would be a good place for us to stop this recording.

Tom Passell: Okay.

Part 2 of April 23, 2021 Interview

Tom Grimshaw: Okay, this is Tom Grimshaw again. This is our second session. It's April 23rd, 2021. I'm here with Tom Passell. As I said, this is our second session to talk about Tom's journey in the cold fusion field. Tom, you just gave us a very brief introduction to your background, being from Western Kansas, where you went to school. Why don't you pick it up from there? You said you'd finished your undergraduate education at-

Tom Passell: Oklahoma A&M.

Tom Grimshaw: Oklahoma A&M. Pick it up from there, please.

Tom Passell: Okay. This is the spring of 1951, when I graduated with a degree of Bachelor of Science in Chemistry at Oklahoma State University, now it's called. I worked for the summer at the Boeing aircraft company, working on the bombing computer, an analog computer, being taught in classes by the Sperry Gyroscope people.

Tom Grimshaw: That was in Wichita.

Tom Passell: In Wichita.

Tom Grimshaw: It's a big Boeing facility there.

Tom Passell: That's right. I was tempted, of course, to stay with Boeing because it was paying well. I had a 10-hour day, seven-day week, however. Pretty well destroyed my social life, working the second shift, from 3:30 in the afternoon till 1:30 in the morning. Anyway, that made me decide to go on to school. I had applied to four places: Wisconsin, Texas, Hawaii, and California. The first one that come through was California, from Berkeley. so I jumped at that, even though the others eventually did come through too, but this was the most exciting place.

Tom Grimshaw: Which university in Texas? Just out of curiosity.

Tom Passell: The main University of Texas.

Tom Grimshaw: The University of Texas in Austin, okay.

Tom Passell: In Austin, yes.

Tom Grimshaw: Yep. Good university. That's where I live now.

Tom Passell: That's right, you know something about that. Anyway, I made the decision and when I got to Berkeley, the rule was, in the graduate department, you had to visit with every one of the professors in the department before you made a decision to actually take up one of them as your sponsor, as your professor. So I did that, and I decided to work with Seaborg because he was working on atomic energy, and I had found that that was the most exciting and interesting thing to get into.

Tom Grimshaw: That was Glenn Seaborg, of course.

Tom Passell: Glenn Seaborg, and a month after I arrived there in September of 1951, he gets the Nobel Prize for the discovery of plutonium jointly with Edwin McMillan of the physics department. Anyway, that's the beginning of it. Seaborg passed me off to professor Isadore Perlman, one of his sidekicks, and I was walked into Building 4 on the hill, above the campus, and shown a big round steel thing, around about a meter in diameter, which was a beta-ray spectrometer (it had just been finished being built) and said, "This is yours. We want you to run it and do the measurements of samples from everybody who comes in the door."

The beta-rays, of course, are ubiquitous radiations coming out of radioactive species. All of the students in the department were taking one or the other of the various possible radioactive isotopes in the region above uranium. It turned out to be the most important of which was americium-241, a 500-year half-life alpha-emitter and also emitter of conversion electrons from the low-energy gamma rays that convert in the L-shells of the atom. That was my thesis, basically, making all kinds of measurements, of electrons at very low energy.

I had my own custom-made Geiger-Muller tube with a very thin window. That was the biggest hurdle I had to jump over. First of all, make one that counted, and it's a gas counter with 20 centimeters of mercury, of pressure of argon and ethylene. Then in order to make this window thin enough to pass a low-energy electron, I had to use a combination of vinyl acetate and vinyl chloride, a drop of it on a surface of water, and lift it off with a wire loop – that's a very, very thin film – and lay it down on a support.

After about 100 tries, I got a leak-proof one. In other words, retain the gas, which I had to do. That's an opening point, and I had quite an adventure handling those samples. I actually contaminated the lab, walked it all over the area. I had to take up the top layer of the asphalt with xylene to decontaminate, but I had tracked americium all over the place. There was a lot of support. Seaborg, of course, had gotten quite a bit of government research money from having been so famous from the point of view of the whole nuclear energy business.

Tom Grimshaw: Tell me again, your supervising professor?

Tom Passell: Isadore Perlman.

Tom Grimshaw: Perlman, P-E-R-L-M-A-N?

Tom Passell: Right.

Tom Grimshaw: Okay. Thank you.

Tom Passell: He passed away at the age of 76 of Alzheimer's disease, but he was a very quiet guy, hands-off, and all of our conversations were jointly with various students who would tell what they were doing every Thursday morning, in an informal seminar. That's how they kept track of what we were doing. I just had one fun thing to do after another. I was living in I House, International House in Berkeley, so I was having some social connections. All the other people from my chemistry department were around, so I made a lot of friends from that group.

Tom Grimshaw: Okay. Now, I think you had told me in a previous conversation that you bypassed the master's degree and went directly for the PhD.

Tom Passell: That's what they encouraged me to do. The department chairman, Kenneth Pitzer, said they only give the master's degree to somebody that, for some reason, doesn't have the time or the inclination to stay there that long.

Tom Grimshaw: Yeah. We used to call it the "booby prize".

Tom Passell: That's what they called it there. Furthermore, from 1911 on, the chemistry department had an iconoclastic group of people at Berkeley who wanted to change the way things were done in graduate work, and to emphasize research and de-emphasize classwork. So they made a minimum of a thermodynamics course, and statistical mechanics, and a few others. But in general, their whole emphasis was on research and have publishable papers, and be learning from that process more than you did from anything in the way of classes. I thrived under that atmosphere. I got married 1st of March 1952, almost right after I got there.

Tom Grimshaw: You met your wife there in Berkeley?

Tom Passell: Yeah. In I House.

Tom Grimshaw: In I House? Okay.

Tom Passell: Yeah. She was seven years older than me and we decided to marry anyway. She had been a social worker and she wanted to have a family when she was 29 and I was 22. That was quite a event in my life.

Tom Grimshaw: No doubt.

Tom Passell: She was very supportive of everything I was trying to do at Berkeley, working late at night, all these radioactive species dying away, and having to get them measured before they were dead.

Tom Grimshaw: Remind me her name.

Tom Passell: Agatha Pearl Brown was her name when we married.

Tom Grimshaw: Okay. How do you spell the first name?

Tom Passell: A-G-A-T-H-A.

Tom Grimshaw: Okay. Agatha. Okay.

Tom Passell: Depends on how you say it.

Tom Grimshaw: Yeah, yeah.

Tom Passell: She went by the short form – Gaye. She had been a woman Marine, during World War in '44, and became a social worker after she got out. She spent VJ Day in Honolulu.

Tom Grimshaw: Really? Interesting. Okay. Okay. So you finished your PhD. What was your dissertation again?

Tom Passell: It was of conversion electrons of gamma rays in the L-shells of atoms, that show up superimposed on anything else of a direct nuclear reaction. In general, electrons at a very low energy, like 25 kilovolts or less.

Tom Grimshaw: Wow.

Tom Passell: Which is hard to get through a window that was thin enough to pass those into the Geiger counter. But somebody among the staff had already designed and built this double focusing magnetic spectrometer. That was all ready to go.

Tom Grimshaw: You just had to outfit it?

Tom Passell: I just had to run it, after supplying a gas supply for my custom-designed Geiger counter.

Tom Grimshaw: Yeah. Okay. Okay. Anything else about your time at Berkeley before we move on to the next step?

Tom Passell: I was in cultural shock in California after Kansas and Oklahoma. I was going through the discovery of San Francisco's Chinatown.

Tom Grimshaw: Yeah?

Tom Passell: Gaye and I got married at the local Congregational church.

Tom Grimshaw: Here again, we share something in common because when I left Western South Dakota, the South Dakota School of Mines, and went to the University of Texas, very similar situation.

Tom Passell: You had a total shock, huh?

Tom Grimshaw: Total culture shock. Absolutely.

Tom Passell: Right. It was going through quite a culture shock, having gone to Manhattan in New York City on a Baptist youth fellowship, special deal for young people, and getting acquainted with the politics of New York right out of Kansas.

Tom Grimshaw: That was prior to your going to California?

Tom Passell: That was 1950. The summer of '50.

Tom Grimshaw: Yeah, okay.

Tom Passell: A year before I went to Cal.

Tom Grimshaw: All right. Let me see how we're doing on time. Let's see. I think we can go ahead. We're at about 12 minutes, so pick up the story, please.

Tom Passell: Okay. I thought it was the Wild West, quite frankly. There was a bomb going off in the Pacific, the super bomb (a hydrogen bomb), and Seaborg was interested in measuring isotopes of higher and higher elements above uranium. So this bomb went off in Bikini and they scooped up a cubic meter of the coral and brought it outside my window on the concrete sidewalk. The chemists were separating out what they could find of all the heavier elements above uranium. They would bring me samples of a prime one, of course, americium-241.

It was a 500-year half-life, pretty impressive. High-intensity alpha emitter, with lots of low-energy gamma rays. They're mostly converted in the atomic electron shells. They are absorbed and out comes an electron, with the energy of the original gamma minus the binding energy of electrons of that particular shell of the atom. There were three L x-ray edges, and two or three low-energy gamma rays. That was the single thing that was impressive. I had to learn how to get the data of a series of low energy electrons energy peaks, and then do whatever was necessary to analyze the results.

Tom Grimshaw: I remember, the first hydrogen bomb was Eniwetok atoll in 1952. Was this the explosion or was it a subsequent?

Tom Passell: It might have been. I'm not absolutely sure of that.

Tom Grimshaw: Okay. There were a number of atmospheric tests at that time, before they were discontinued.

Tom Passell: Yeah, this was an atmospheric test.

Tom Grimshaw: Yeah? Okay. All right. Are you ready to move on to the next stage?

Tom Passell: All right.

Tom Grimshaw: Tell us how you got the next position, and where you wound up.

Tom Passell: Right. One of the hottest reactors in the world was in Idaho Falls, Idaho, at the National Reactor Testing Station. They encouraged me to be Berkeley's agent on the site. Which was okay with me, because the materials testing reactor had a neutron flux, in their radiation facilities, of 2 X 10 to the 14th, the highest flux of any other reactor in the world. What they wanted was to be able to use neutron capture to go to ever heavier elements above uranium, followed by beta decay, after neutron capture.

I went up there and I had a well equipped lab ready to use. We bought one of every element we could find in the catalog and began irradiating those, in addition to any other requests people had for our services to separate isotopes from each other in the heavy elements. So I had to learn the chemistry of separating neptunium from uranium, and plutonium from neptunium and so on. So you had an intensive educational experience on the line, working in the glove boxes. Sometimes just much simpler things, just irradiating pure metals. I learned a lot of things about very high neutron fluxes.

For example, you irradiate copper. First of all, you get a five-minute copper-66 from copper-65, but then with five minutes half-life, normally, a reactor, that would be it, but a very high-flux reactor, it goes on to copper 67. In other words, it was a small amount, but nonetheless very easily measured by analysis of the gamma ray emissions.

Then I worked with this guy who was an expert in gamma ray analysis, and he and I worked as a hand in a glove. Russell Heath by name, H-E-A-T-H. One of the first things that happened when I arrived at Idaho Falls was they had a fission break, as they called it. Radioactivity shows up in the coolant of the reactor. Is this a leak from the reactor fuel elements, or is it from something else? We immediately went to look at the question, can we tell fission products from anything else? You can't without doing a chemical separation.

Iodine, a whole string of iodine isotopes, and the beautiful thing about iodine is it has a negative charge in water. So all I had to do was take most of the background junk out with ion-exchange resins to take all the positive ions out, leaving only the negative ions to go through to be captured by an anion capturing resin. If there was iodine present, that screams at you in the second column. I said, "Yeah, it's a fission break. In other words, you have to shut down and see where the leak is on your fuel elements."

Tom Grimshaw: Somewhere on the aluminum cladding of the uranium-aluminum alloy fuel plates.

Tom Passell:	Of the cladding, yeah. Turns out they had made a change in the design of the hangar to the fuel element, and that had changed the flow just enough to allow it to get hotter than normal, and to have a corrosion on aluminum cladding. In other words, I immediately became famous for solving a problem and doing it with ion exchange.
Tom Grimshaw:	Yeah?
Tom Passell:	Ion-exchange resins and their use, are the one thing they used so much to separate all the various elements.
Tom Grimshaw:	Yeah. Okay.
Tom Passell:	So I found that it was nice and clean, and it was fairly new at the time. Again, I could hardly turn around without doing something brand new that I had the opportunity to do. So I had a really nice time, but my wife was really unhappy having our first child still in Berkeley. She was about two years old. So she says, "You have to get a new wife, or let's go back to California," which we did in November of '55.
Tom Grimshaw:	Okay. So you were in what is now INEL, I think that's-
Tom Passell:	Right.
Tom Grimshaw:	... Idaho National... INEL, was it Energy Laboratory? INEL.
Tom Passell:	It might be Idaho nuclear Engineering Laboratory.
Tom Grimshaw:	INEL, yeah. It got that name later, but it's what it's known by now.
Tom Passell:	That's right.
Tom Grimshaw:	How long were you at that facility? A couple of years?
Tom Passell:	17 months.
Tom Grimshaw:	17 months. Oh, okay. All right.
Tom Passell:	It was very intense, though. They drove us in a bus 65 miles out into the desert to the Materials Testing Reactor (MTR) and then back each night. That was pretty severe, because they had a pretty severe duty time-wise.
Tom Grimshaw:	Yeah. Okay. Anything else you want to say about this phase? Then we'll probably go on to the next step in the recording.
Tom Passell:	It was an exciting time for me. I was unhappy to leave the atmosphere, having the hottest reactor on the face of the earth, but I knew that I had to move back to California. So I applied and got a job with Stanford Research Institute in Menlo Park.
Tom Grimshaw:	Okay. What we'll do is we'll pick that up in the next recording.
Tom Passell:	In the physics department.
Tom Grimshaw:	In the physics department. Okay.
Tom Passell:	Running a Van de Graaff. A 2 million volt Van de Graaff.
Tom Grimshaw:	Very good. All right. Now, we're at about 21 minutes, which is fine, but it's a good stopping point. So we'll stop here. This is Tom Grimshaw. I'm here with Tom

Passell. It's April 23rd. We're here at his laboratory in Mountain View. Is that right? Yeah. In Mountain View, California. We'd taken a tour of his lab and collected some other things for the LENR Research Documentation Initiative. With that, Tom, we'll sign off and pick up again in a few moments. Okay.

Part 3 of April 23, 2021 Interview

Tom Grimshaw: Tom Grimshaw here. Again, this is our third session with Tom Passell. We're doing a recording of his adventures with cold fusion. It's April 23rd, 2021. And Tom, we were just in our last session. This is session number three. In our last session, you were kind of wrapping up your brief 17 month time in the Idaho MTR reactor. And you were just getting into your next phase at SRI. Why don't you pick up the story from there?

Tom Passell: Okay. At SRI, they put me in charge of a 2-MeV Van De Graaff machine. And so since I had success with my basic earlier experiences, always I fell in love with neutron activation analysis. And then in this case, using gamma rays from a radioactive isotope that you're producing by neutron capture or any other reaction. So, that was kind of the organizing principle. I had those learned almost every day in my scientific career. Another thing having to do with running a Van De Graaff and they can use it a bit in other projects at SRI.

Tom Grimshaw: Right. A lot of federal contracts I think at that time. Yeah.

Tom Passell: And one of the first ones was to see if we could devise a system for measuring which of a whole bunch of fuel rods were leaking at a power reactor. And of course, Edwin Zebroski from General Electric had just joined the group and he was the higher department boss over my immediate regular boss. And so he gets a contract with GE San Jose. And for them, we did all kinds of design work. And again, I had to get acquainted with the steam tables and what you can and can't do with hot water at 300 degrees centigrade in addition to running, doing research using this Van de Graaff generator.

So I had two bosses, one of whom just wanted to do nuclear physics. He came out of Caltech, Sylvan Rubin by name, and we were doing a proton Coulomb scattering off of boron-11, point by point using a magnetic spectrometer for the off coming protons. So, again, fast learning experiences are rolling in. And I'm having a pretty good enough success to feel good about the whole thing, but they're pretty demanding for learning new things all the time. And so I made that the centerpiece of my interests with doing neutron activation analysis and using the sodium iodide measurement method for gamma rays.

Tom Grimshaw: Yeah. The detector for the gamma rays. Yeah.

Tom Passell: So that was the... and the most of our multi-channel analyzers you'd buy, they were rather crude by current standards. The one that I have out here right now, a two-inch by two-inch diameter crystal. All of the high voltage and multichannel analyzer is in the tube base instead of two big racks of electronics.

Tom Grimshaw: Yeah. Yeah.

Tom Passell: Incredible. And so the gamma energy spectrum shows up on the computer screen. The electronic revolution, every time you turn around is into a new device and then along comes germanium, but not in time for me to be making the best use of it.

Tom Grimshaw: Okay. So how long were you at SRI? It was then called Stanford Research Institute, I think before it became SRI International.

Tom Passell: SRI international during the Vietnam war because students who were objecting to its doing all this research for the military. And so they had to throw a fence around the place in 1969 and become a little bit of a barrier to demonstrations.

Tom Grimshaw: Oh, okay. And it was divorced from Stanford at that time.

Tom Passell: That's right. It became cut off from Stanford and they let us keep the name of SRI international, but no longer Stanford Research Institute.

Tom Grimshaw: Right, okay. This was an interesting time in history with the Vietnam protests going on.

Tom Passell: That's right.

Tom Grimshaw: I was not aware of that though. That reason for the split from Stanford by SRI-

Tom Passell: Shortly after that, that I left SRI, I did go to Physics International.

Tom Grimshaw: Okay. How long were you at SRI?

Tom Passell: 13 and a half years.

Tom Grimshaw: Oh, good. Long time then. Was your work pretty much of the same type during that time or were you on a variety of different projects?

Tom Passell: Yeah. Anything having to do that kind of physics technology is what we did.

Tom Grimshaw: Yeah, of course.

Tom Passell: And so we didn't control that. And one of the jobs I had was finding the cause of fires in potato chip factories who were using microwaves to do the final de-watering of the potato chips. Every now and then, they would burn to the ground very quickly.

Tom Grimshaw: Do you need to check that? If you need to take that, we can pause. We'll pause here.

Okay. Tom Grimshaw here with Tom Passell. We took a short pause while Tom took a phone call and Tom you were I think kind of wrapping up what you had been doing during your 13 years at Stanford Research Institute, later SRI International.

Tom Passell: One of the other things that came in the door of course, were the civil defense things right after the Russians blew off the 65 megaton bomb in Novaya Zemlya, north of the Arctic circle there. And so they wanted to know what a big bomb would do and they were building all kinds of fallout shelters. And so, we were doing calculations of how much, how big area would you be setting fire to with a bomb at high altitude. And so it's calculations like that. I published that work that got in the open literature actually, because it was for civil defense. And we discovered that a cloud deck load reflected 80 to 90% of the bomb radiation back into outer space if it doesn't wipe out the whole cloud deck and that's pretty hard to do. So that was one saving grace. And on the other hand, it was pretty clear that it was far better to avoid a nuclear war than to try to figure out how to live through it. I was pretty well convinced on that one.

Tom Grimshaw:	Yeah. The balance of terror set in at that time, yeah.
Tom Passell:	No matter how many underground shelters you built and they had all kinds of different ones, the cheapest one was to bury a car, an old car, and dig down to it. That was the cheapest one.
Tom Grimshaw:	Okay. So you had an interesting time for 13 years at SRI. What came next and what attracted you to your next step?
Tom Passell:	The Physics International job for a few months on that 17 months or so. They were a group that was making measurements of ground motion around underground bomb test in Nevada.
Tom Grimshaw:	Okay. In Nevada testing site, yeah.
Tom Passell:	And they were hoping that I would bring some of that work on measuring the x-ray flash. That was the end of the SRI thing. We had a group measuring the x-ray flash from a bomb and a vacuum in an underground test in Nevada. And that was learning all about x-rays and very intense x-rays I did a pinhole camera of an x-ray emission showing the bomb in its own emissions. And then showing the cables to the detonators and that became notorious for having gotten a regular piece of film to do it, the slowest film that's ever been made. That's a special order of film S0303 Kodak ASA of 0.001. Fine grain stuff and the high flash of course is cut down by the pin hole. But still that overwhelmed most of other films. Los Alamos invited me to come there in 1970 and then enter the secret worlds, which I resisted because all my six kids were going to live with somebody else, in fact, if I went to Los Alamos. For many reasons the dress code in Los Alamos. It was the Vietnam era '69. At that time, I remember running for city council on the local politics and failed to get the win, but saved my marriage. I was also the moderator of my church.
Tom Grimshaw:	Yeah. Busy man.
Tom Passell:	Yeah. Nobody wanted to be president of anything and I was always saying yes, so I was seven or eight presidents in small organizations.
Tom Grimshaw:	Okay. So Physics International, what was your gig there?
Tom Passell:	There, I was just working with the ground motion people. And so I'm trying to get some projects on more pinhole cameras or whatever else. I didn't succeed. I was laid off from that job. That was the only time it was ever laid off. They lost the hard rock silo project. And therefore they couldn't afford to have as many people. They had to lay off some people. So I had two weeks of unemployment and 500 phone calls later I got a job with the same group doing underground tests in Lockheed Martin in Palo Alto, Lockheed Palo Alto Research Lab. I worked there for four years, then from '71 through mid '75. And EPRI was just beginning then. And so my ex boss from SRI, Zebrowski, had ended up at EPRI and he called me up and said, that I have a job here if you want. So all of a sudden I jumped into that. This was the nuclear power business on corrosion.
Tom Grimshaw:	Okay. Well, let's stay with Lockheed Martin. You were there for how long?

Tom Passell: Four years.

Tom Grimshaw: Yeah. And what was the nature of your work there?

Tom Passell: Supporting the apparatus to measure the pulse of a flash of x-rays to measure the black body temperature of the bomb, by measuring six or seven different bands of x-ray energy. And you get that and find the best fit to a black body. And that was what we were doing. And there were a lot of tests, ten different underground test.

Tom Grimshaw: Really? Not the Nevada test site? So you were onsite then at the Nevada test site?

Tom Passell: Well yeah, for about six weeks or so on each test.

Tom Grimshaw: On each test, okay. And that was also the case when you were previously at SRI?

Tom Passell: That's right.

Tom Grimshaw: Yeah. So you'd always go out in the field for a few weeks for each test.

Tom Passell: That's right, it was then back out in the field. I had a bigger responsibility in the case of Lockheed because they hired me because they wanted to get the secrets of SRI. They're competitive for the next shot. I know, in retrospect, I know why I was hired. It was an industrial espionage.

Tom Grimshaw: Well, apparently it worked.

Tom Passell: No, it didn't work. And then of course I got... I was almost fired from Lockheed, so they got what they wanted from me at Lockheed. They tried to fire me once and I stayed for another two years and finally got a chance to jump into a brand new job at EPRI where I stayed for 22 years.

Tom Grimshaw: Okay. All right. Well that's good. Let me check our time here. Yeah, we're doing fine. Let's move on then to the year you got hired by Zebroski, okay.

Tom Passell: It was the materials technology group having to do with corrosion mostly.

Tom Grimshaw: All right. Now we're getting pretty close to the time when you started getting involved with cold fusion, but let's talk about that period of time before that at every what you were doing. And then we'll move on to the 1989 announcement.

Tom Passell: As an expert in corrosion. I took a one-week course at Ohio State, all about the electrochemistry of corrosion. And it was then I hired as my main contractor, some people big on electrochemistry at SRI. So they were working on all kinds of those kinds of problems.

Tom Grimshaw: Now, excuse me, for interrupting. This was the time you started working with Michael McKubre, right?

Tom Passell: That's right.

Tom Grimshaw: And Fran Tanzella, I guess also, and had you known those gentlemen or were either of them in your previous time at SRI?

Tom Passell: No. This is all since I left SRI.

Tom Grimshaw: They had come in, since you had left SRI?

Tom Passell: Back in 1979, I think. I was at SRI through 1971.

Tom Grimshaw: Okay. Okay. So you were doing corrosion work in nuclear reactors in a new job at EPRI.

Tom Passell: The secondary side (the steam generator) of a PWR would clog up with impurities and cause a corrosion, even in fairly corrosion-resistant alloys, such as Inconel 600. And other things that they thought were impervious to corrosion exhibited stress corrosion cracking as well as denting.

Tom Grimshaw: And was this corrosion taking place in the actual fuel rods themselves or…?

Tom Passell: Well, it was not the fuel rods themselves but tubes that were bringing the hot water and had just been in the core into where they've raised steam in the steam generator.

Tom Grimshaw: Yeah, all right. There's the heat exchanger.

Tom Passell: Steam generator corrosion was the big thing.

Tom Grimshaw: Yeah. Okay, got it.

Tom Passell: At least that was my responsibility. Other people had responsibility for the cladding on individual fuel rods, but I didn't get involved with zirconium cladding. And then in case it was working with that group that led me to work in the secondary side, they are in the primary side actually of the water, of a pressurized water reactor. They have a pressure in the pressurizer, a gaseous tank that's gas pressure and there's also a buffer for pressure changes using hydrogen gas. And they saturate the hot water with hydrogen and it reduces corrosion, also suppresses radiolytic decomposition of water in the core. Well, we would like to have a measurement of just how much dissolved hydrogen gas there was in the water to have suppressed corrosion. And that was the project that used palladium's electrical resistivity to measure the hydrogen gas content of the water. It comes into equilibrium with the hydrogen dissolved in water and its resistance changes. This is the hump curve. We were hot on that trail of the measure dissolved gaseous hydrogen using palladium with hydrogen to measure resistance, to use it as a probe.

Tom Grimshaw: Okay. So the palladium was absorbing the hydrogen.

Tom Passell: It comes into equilibrium and when it is in our palladium wire probe its resistivity changes. So that was the group though that was developing the probe. And furthermore McKubre had studied under Fleischmann at Southampton for his degree. You see, all the planets were aligned. And so he thought that the Fleischmann wouldn't make a fool of himself and indeed that was the case.

Tom Grimshaw: So this might be... we're about to start to get into the cold fusion story.

Tom Passell: Yeah, this is the beginning of the cold fusion story.

Tom Grimshaw: So let me see here. We're at about 19 minutes, I think we'll start that on the next recording.

Tom Passell: Okay.

Tom Grimshaw: All right. So this is Tom Grimshaw here with Tom Passell on April 23rd, 2021 here at his lab in Mountain View, California. And we're going to stop here and we'll go on to the next recording, Tom.

Tom Passell: Sure.

Part 4 of April 23, 2021 Interview

Tom Grimshaw: Tom Grimshaw here again, here with Tom Passell. This is session number four, covering his journey in cold fusion. We've been talking about your pre-cold fusion era, Tom. And I think now, here on this fourth session, you're just getting into your involvement with cold fusion. You were working with Mike McKubre, and I think also Fran Tanzella on a corrosion problem at nuclear power plants. You had joined EPRI Electric Power Research Institute or EPRI. And so this is when you first heard about cold fusion. And why don't you tell us, how did you first hear about cold fusion, and what was your reaction when you heard it?

Tom Passell: All right. The announcement was in 1989 I believe, around March 23rd. Okay, and so that's quite a few years into my time at EPRI, and I saw that the skill that Fleischmann had claimed he was making his excess heat with, were aligned with what I was doing with measuring hydrogen content of the high-pressure water in a pressurized water reactor, hydrogen gas dissolved in water.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: So I called McKubre up and I said, "Throw away your current statement of work and see if you can repeat what Fleischmann has claimed." And that was the start of it.

Tom Grimshaw: And you said that he knew Fleischmann.

Tom Passell: He had been a Ph.D. student at Southampton with Fleischmann, in that group.

Tom Grimshaw: Okay.

Tom Passell: So we had a lot of faith in him to lead the effort to test the claims of Fleischmann. Martin had already made his name for himself by becoming a discoverer of a special effect of on surfaces. I don't remember, I can't repeat yet, but in any case, it was an enhancement by a factor of 10 to the something like 5th power, in the Raman effect on the surface. Fleischmann had explained that, and therefore had a reputation based on that. But anyway, in this new claim with Stanley Pons.

Tom Grimshaw: Okay. I guess, I didn't know that he had established a reputation based on the Raman effect.

Tom Passell: The Raman Enhancement, yeah.

Tom Grimshaw: Enhancement. So he a Royal fellow, a fellow at the Royal society-

Tom Passell: That's right.

Tom Grimshaw: ... perhaps as a result of this advanced work.

Tom Passell: Oh, he was pretty high up, and of course McKubre had almost a reverence for his professor, and so he thought Fleischmann could do no wrong.

Tom Grimshaw: Yeah, Okay.

Tom Grimshaw: So pick up the story, please. You called McKubre and said, "Forget about what you're doing. Go this way."

Tom Passell: "Scratch what you were doing with the hydrogen probe, and move to doing what Fleischmann had claimed.

Tom Grimshaw: Okay. Now a minor point, but had you heard it, and told McKubre, or had McKubre already heard about it, or how did that play out, I guess?

Tom Passell: Good question, I am sure I can retrieve that, but it was very quick. I have that memo filed somewhere. And then I wrote, and I did it on the 24th of March-

Tom Grimshaw: Okay, so the very next day-

Tom Passell: ... to my colleagues saying, "This looks like an opportunity, would you like to sign this request for diverting money into it?" And they all refused to do it.

Tom Grimshaw: Yeah, I remember that. Okay, pick up the story.

Tom Passell: Anyway, eventually I convinced the guy who was in charge of the exploratory research budget, another budget, not ours. And Joseph Santucci was a man who had finally sprung loose enough money for us to really rather aggressively pursue the replication of what Fleischmann had claimed. And of course, in the meantime, there was a tremendous upheaval of phone calls and faxes. And I had decided to go take a trip to Grand Canyon on April eight, nine and 10 of that year. And I was phoning from the bottom of the Grand Canyon. It was an old fashioned non-digital phone.

Tom Grimshaw: Radio telephone?

Tom Passell: It wasn't a radio. I don't know what it was. It was a old clicker.

Tom Grimshaw: Yeah? Okay.

Tom Passell: So anyway, it was a very intense time with claims and counterclaims and so on. One day they'll claim that they had repeated it and then they had to retract it. That whole period was turbulent. And then of course they decided the ERAB panel decided to have a meeting on July the sixth of '89, and they came to EPRI, and we had a session in which McKubre showed his attempt to measure excess heating, and he claimed to have done so already. And I never really checked whether that data would stand up or not, but it appeared to stand up at the time.

Tom Grimshaw: Do you recall who was at that meeting from the DOE?

Tom Passell: Yeah, Chairman Huizenga was there.

Tom Grimshaw: John Huizenga? Okay.

Tom Passell: He was there in person.

Tom Grimshaw: Okay.

Tom Passell: And there were several others that I don't recall. I have the list of attendees.

Tom Grimshaw: Oh, it doesn't matter. I just was curious.

Tom Passell: They visited his lab request at that time.

Tom Grimshaw: Okay. At SRI? Okay.

Tom Passell: At SRI, and they had seen that he was a credible person, as he was. So overall we had a good start there. But SRI had a hell of a time reproducing the excess heat.

	And it was a mystery, and of course they fell on the idea that some batches of palladium would work and others would not.
Tom Grimshaw:	Yeah.
Tom Passell:	And that the less pure ones would work. The more pure ones didn't. That was the first thing. And that's all Pd from Engelhard. And of course, Johnson Matthey had the palladium that had been running for Pons and Fleischmann. But they were also very close to the chest about that particular sample. And so it was thought to be something magic about the samples. And so I observed and then discovered eventually that and had the impurities measured by ICPMS, inductively coupled plasma mass spectrometry.
Tom Grimshaw:	Right.
Tom Passell:	And ICPMS found a hundred parts per million, by weight, of boron, and larger amounts of calcium. So it was the calcium-boron as the oxygen getter being added to the metal as you pour it into the mold, from making high-purity metallic palladium, because they've learned to not get it contaminated with too much oxygen from the air. That reduces the catalytic effect. So in general, it was very high, and that's 10 times that on an atomic basis. The boron has only two isotopes, 10 and 11. And so in general, I had a suspicion this boron, or some kind of impurity must be involved if you had a batch effect. Okay, and so we kept trying to track that down.
Tom Grimshaw:	In other words, whether you had success dependent upon which batch of palladium you were using.
Tom Passell:	That's right, they noticed that, the batches that they had access to. But that it was not so airtight. It was a working hypothesis, now that caused McKubre to add boric acid to the electrolyte. And that seemed to be a help, but it also tended to screw up his method of measuring the loading. He fell onto the idea that loading was the lead variable of hydrogen per palladium atom added. And since the boric acid tended to mess up his loading curve versus resistance, he was tending not to do it as much.
Tom Grimshaw:	The loading being the perceived most important variable. Not to be too elementary, but the amount of deuterium that entered the palladium lattice up to almost one to one of loading.
Tom Passell:	That's right. He tried to get it as high as possible. And he thought that that was the whole story. And he may still believe that, I don't know.
Tom Grimshaw:	I believe he does. Yeah, there are others who disagree with him of course, in this field. But yeah, loading has always been a big deal at SRI.
Tom Passell:	He had a whole farm of loading individual cells, each of which did a different thing for improving the loading, having to deal with the electrolytes that were used.
Tom Grimshaw:	Okay. Okay.

Tom Passell:	And he can measure... and again, that followed along with measuring the resistivity of palladium as a function of hydrogen loading that he had done for the work on the power plant corrosion.
Tom Grimshaw:	Of course.
Tom Passell:	You see?
Tom Grimshaw:	Yeah.
Tom Passell:	He already had a leg up on that. And that was a pretty small resistance too. So he had to be pretty accurate in that range of resistance.
Tom Grimshaw:	Mm-hmm (affirmative). So during this time, this was what, the first couple of years, I guess, that you were supporting EPRI funding for SRI. You were pretty hands-on. I mean, you were making frequent trips over to SRI.
Tom Passell:	Every day, yeah.
Tom Grimshaw:	Every day you were over there? Okay.
Tom Passell:	Meeting with them.
Tom Grimshaw:	Yeah? Interesting. Okay. So bring us up then, do you recall exactly how he started the work and then how it evolved. Any words about that?
Tom Passell:	Probably a good group of about five or six people. And most of the people who were from New Zealand or Australia.
Tom Grimshaw:	Yeah.
Tom Passell:	I was trying to remember, oh yeah. Fast forward a little bit to '92, they were looking at a cell that was a closed cell, but they had a pressure gauge on it, but the pressure gauge tube had a leak in it. So they decided to close it off, so they lost their measurement of pressure. And that was the thing that exploded, went through the face of one their people, killed him, and the others got damaged a little bit. But this was January 2nd of 1992. Now that's quite a way off, but not too much later. But that was the event that caused a year of hiatus when they had to remake the lab safer and with more rules.
Tom Grimshaw:	Okay. What was the gentleman's name that was killed
Tom Passell:	Well, Andrew Riley was the man who was killed. He had been hired from the group at Utah.
Tom Grimshaw:	Okay. Oh, he had come down from the Cold Fusion Institute of Utah?
Tom Passell:	I think so.
Tom Grimshaw:	Okay, interesting. And so the explosion, actually-
Tom Passell:	He was looking over it, and then it goes up through his face.
Tom Grimshaw:	I see, my goodness, okay. I hadn't heard that.
Tom Passell:	It was a deuterium-oxygen explosion in which the pressure kept rising because the recombination catalyst had gotten water logged. And they didn't know the pressure, and looking back on it, it had got up to about 10 times the pressure in the room at which it was seriously explosive.

Tom Grimshaw: Yeah.

Tom Passell: It was well-researched as to what caused the problem, and it was high pressure. D2O2 reaction.

Tom Grimshaw: Okay. I remember it was well-researched internally at SRI, but there was also an external panel, as I recall.

Tom Passell: Yeah. There were all kinds of panels, but in general we had a report. Somewhere in my literature of that accident, and it was a report by a group at SRI.

Tom Grimshaw: Well, I mean the important finding was that it was not nuclear.

Tom Passell: Well that's right. The local police department thought it might've been nuclear and therefore they had all kinds of restrictions as to what to do to discover the cause. But anyway, it was the wild west. SRI was trying to push everything along and they were willing to sacrifice the knowledge of the pressure, which they were ill-advised to do, but they wanted to see if this produced excess heat.

Tom Grimshaw: Yep. Look, we'll call that, it's kind of like a milestone in the progression of things. Anything more to say about the period from the time you redirected McKubre and others from the corrosion study to cold fusion, and the work that was done there? You say you were there almost every day?

Tom Passell: Yeah. Practically everybody, every department at EPRI wanted to get in on this. Even the fossil fuel people. And so there were all kinds of committees, meetings and Sy Alpert was one of them. It was one of the people from fossil-fired power plants. And there were lots of discussions. Meeting after meeting. I had a big review meeting, but SRI had learned it all by September, 1992. After that year McKubre made a presentation to everybody to see if it was okay to go ahead and work with the new lab. And indeed extra money was provided for them.

Tom Grimshaw: Okay.

Tom Passell: I was in interested in having a gamma ray detector with germanium available so that if anything nuclear happened, I would be able to capture it. And so SRI invested about a hundred thousand dollars into that. So those are the things that stand out in my memory. They had the meeting in Como, Italy in 1990, I believe, well before the January 1992 accident.

Tom Grimshaw: ICCF-2?

Tom Passell: Yeah.

Tom Grimshaw: Yep, I remember that.

Tom Passell: I didn't go to that, but Santucci who was putting the money in from his EPRI exploratory research budget.

Tom Grimshaw: Okay.

Tom Passell: And it was apparently a pretty good meeting in general.

Tom Grimshaw: Yeah, so I heard from others that went.

Tom Passell: The Italians were really jumping in with both feet.

Tom Grimshaw: Yeah. And the Navy was very active at that time too. Mel Miles was very impressed at that conference, I remember he told me.

Tom Passell: Did he?

Tom Grimshaw: Yeah.

Tom Passell: I didn't get to it myself, but I could see that it was difficult to pin down... And everybody who would have an experiment that didn't work, you would always have some reason why it didn't work and hope for the best on the next one, maybe changing the sample.

Tom Grimshaw: Mm-hmm (affirmative). So after the accident, in the years after, you continued as program manager at EPRI, to support SRI research.

Tom Passell: Santucci was fired and I was hired to run it, because it was under Santucci's watch that the explosion had occurred.

Tom Grimshaw: I see. I did not know that. So there was kind of a consequence.

Tom Passell: Well, he was pushing, pushing, pushing, hoping for a successful experiment.

Tom Grimshaw: I see. Santucci was after the IP, intellectual property, yeah.

Tom Passell: Yeah, intellectual property. And he was pushing every angle and trying to make... They sensed that it was a big thing and Nobel prize sort of thing.

Tom Grimshaw: Of course.

Tom Passell: ... or a commercial thing. And so I guess that was irrational exuberance.

Tom Grimshaw: Yeah. So he left SRI?

Tom Passell: He stayed at EPRI, but he worked on other things.

Tom Grimshaw: Okay.

Tom Passell: He got involved with other things. And so I took over the management of it at EPRI.

Tom Grimshaw: Okay. So can you kind of relate what the... What was the trajectory of cold fusion research after that?

Tom Passell: After that? Well, we had a new lab and a new set of procedures, and it started in 1993. And I do remember access to making it a big showing, because we sponsored, EPRI sponsored, along with McKubre, the fourth international conference (ICCF-4) in December of '93 in Maui. And at the last minute McKubre found that they made a mistake in wiring one of the cells, and they had mis-measured the temperature. And so he had to withdraw his claims on one of his samples. And that was a dispiriting thing, but we still went ahead with the meeting, had 300 people from all over the world. I brought the Russians in, paid their bills. I had to pay for a person to drive them around, because they didn't have a driver's license, all over Maui. And I had plenty of money left over from previous conferences of a corrosion nature. And they didn't stand in my way from using them, for hiring Russians to come, and Kucherov was there.

Tom Grimshaw: Jan Kucherov who later got involved with ENECO.

Tom Passell: That's right.

Tom Grimshaw: Yeah, I remember that.

Tom Passell: Kucherov seemed to have some data that looked like it was definitive.

Tom Grimshaw: Yeah.

Tom Passell: But he's now gone, I guess.

Tom Grimshaw: Yeah. I think he's passed. Okay.

Tom Passell: It's hard to put my finger on any other thing that would be of note. Except that it was just one meeting after another and intense search for the thing that worked.

Tom Grimshaw: Yeah. And so when did that era come to an end?

Tom Passell: Well EPRI support for the project at SRI ended at the end of '94.

Tom Grimshaw: '94, okay.

Tom Passell: And that's when it cut off, and they had to... We let them have all the equipment if they wanted for other contract work.

Tom Grimshaw: Yeah, and they had to find funding elsewhere to continue the work?

Tom Passell: Right. And so they did find some other support from the government. And I don't know who all, but in general it wasn't at the level that we had been doing.

Tom Grimshaw: Of course, yeah. Yeah. Okay, so let me see here, we're at about 22 minutes. I'm thinking we'll take a break at this point and maybe also take a break, a coffee break and we'll come back and talk about the rest of your career after 1994.

Tom Passell: Sorry, I can't dredge up all these details.

Tom Grimshaw: No, that's fine. It's whatever you can remember.

Tom Passell: All I remember though, is the intensity of it all.

Tom Grimshaw: Yeah.

Tom Passell: Every day there was something new. And so it was quite an exciting time.

Tom Grimshaw: Yeah.

Tom Passell: And you always had a hope that it would be a breakthrough somewhere, and that didn't come. However, the people... McKubre tended to want to stay with electrochemistry and other people were going in to glow discharge and all sorts of other experiments.

Tom Grimshaw: Gas loading.

Tom Passell: What was that?

Tom Grimshaw: Gas loading.

Tom Passell: Yeah, gas, directly. And the ultrasonic people were using cavitation.

Tom Grimshaw: Roger Stringham.

Tom Passell: Roger Stringham and Russ George

Tom Grimshaw: Who was at SRI at the time also.

Tom Passell: But they had spun off into that group across the street, 2156 Old Middlefield Way.

Tom Grimshaw: Okay. I did not know that, yeah. Well, we'll pick that up also. Because I know you did a lot more in the field afterwards, but we'll take a break. And my stomach's growling a little bit. So this is Tom Grimshaw. I'm here with Tom Passell it's April 23rd, 2021. And we're talking about Tom's adventures in the cold fusion field. And I believe this is the end of session number four. And so Tom, we'll pick it up after a break and talk about the rest of your journey in cold fusion.

Part 5 of April 23, 2021 Interview

Tom Grimshaw: Okay. Tom Grimshaw here again with Tom Passell. It's April 23rd, and we're talking about Tom's adventures in cold fusion in his varied career. And Tom, I think when we ended the last session you had just wrapped up your two phases, as I would say. You were at EPRI, Electric Power Research Institute, and there was a two-year period where you were supporting SRI International for their cold fusion work, Michael McKubre and Fran Tanzella. Then there was that accident in 1992, and then you continued to be a program manager to support them until about 1994 after that.

Tom Passell: They finished their project, yeah. They couldn't get any more money from EPRI.

Tom Grimshaw: Yeah. So, why don't we pick it up there, Tom? I know you had a lot more involvement with cold fusion after that 1994. Pick it up from there, please.

Tom Passell: Yes, okay. Well, let's think about where we were when SRI finally had to find other sources. I continued to go to all the ICCF meetings. The big one that they did was the ICCF-4 in 1993 in December in Maui in Hawaii. So, after that we had a big one in Monaco, ICCF-5. It was there that I did the exposure of Kevin Wolf. I essentially put out in the public arena as an appendix through my talk what Kevin had claimed because at that time he hadn't been able, or maybe he hadn't really done anything in attempt to replicate what he claimed.

He claimed that he had put his three cells down to liquid nitrogen temperatures but having them still work. Looking back on it, that had to be a good reason why people wouldn't try that because they cracked everything in sight. And he claimed that he was seeing this bunch of gamma rays on the way as he warmed them up. I could see that his lab was in the basement underneath the cyclotron, the 80 MeV cyclotron for protons, so I said, "Here's a situation. It sure looks like somebody irradiated these cathodes." So I said, "I'll find a similar cyclotron," and I did it in Canada at their place in Vancouver. They would irradiate a sample like I had for three different energies to see if I could replicate their gamma ray observation 100 gamma rays of every isotope of Pd, Ag and Rh that you could imagine from 80 MeV and that's lots of (p,n) reactions, and an occasional (P,α). And then indeed, that was exactly what I precisely measured, what Wolf had claimed.

Tom Grimshaw: Now, Kevin Wolf was at Texas A&M at that time?

Tom Passell: Right, he was. We did this expose of what he had claimed, that is, in November of '92. It wasn't until several years later that Mike Melich `wanted to see if it was true. So, I took it on, and he gave me a few thousand bucks. And that's what I found out More than likely Wolf had irradiated these samples with about 80 MeV protons. That was how I almost exactly replicated what he had claimed. Of course, I did the proton irradiation well after this time, in 2000 or 2001.

Tom Grimshaw: Okay.

Tom Passell: And of course when I was revealing his claims in the Monaco meeting, ICCF-5 in '95 who gets up, Douglas Morrison. You know who Douglas Morrison is?

Tom Grimshaw:	Yes, the skeptic.
Tom Passell:	Did Kevin give you permission to reveal this? I said, "No, but we had funded it, and I had given him two years to replicate it again, so I decided to put it out there and see if somebody else could replicate it.
Tom Grimshaw:	So you were still at EPRI?
Tom Passell:	I was still at EPRI at the time, yeah. I could see my SRI people were uncomfortable with what I was doing.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	Because they were worshiping at the feet of Kevin Wolf as an icon of virtue.
Tom Grimshaw:	Okay.
Tom Passell:	So, in general ...
Tom Grimshaw:	But he didn't have anything to do with John Bockris?
Tom Passell:	No, they were pretty much at swords point all the time that they were at Texas A&M. But, in any case that transpired, and then I discovered later that Kevin had died on September 11, 1997. This was in 1995. And that whole saga then ended with his death and that they had sequestered his death certificate for 25 years.
Tom Grimshaw:	Okay.
Tom Passell:	So, that's where I left it. And the 25 years is up next year.
Tom Grimshaw:	Okay.
Tom Passell:	In 1997. That was a saga that I was truly acting like a naïve person that believed people when they claimed something.
Tom Grimshaw:	Yeah.
Tom Passell:	And clearly if I was giving a brief thought I could say that you can plunge your experiment down to liquid nitrogen temperature and bring it back up without destroying it. And it turned out McKubre said that Kevin was very much interested in dark matter and that in fact he had gone so far as to put samples of palladium in the pockets of pilots that he knew who were flying high in their piloting of airplanes hoping to see that some particle or other from outer space might be part of the dark matter and would give the kind of signals that we were seeing in the whole cold fusion business. At least he was most interested in that and not interested in my objectives. He was willing to say goodbye to the cold fusion business, but he was just using it as a means of getting money, and he got a quarter of a million bucks.
Tom Grimshaw:	Yeah, okay.
Tom Passell:	Anyway, he died in a mysterious way, so I asked Mike Melich, "Are we going to publish this?" He didn't want to publish it. He was embarrassed that he had asked for money on something that didn't work out.
Tom Grimshaw:	Yeah, okay.
Tom Passell:	So, it's still buried in my files. I could ...
Tom Grimshaw:	Yeah, I saw the report.

Tom Passell: The report of ICCF-5, I presume.

Tom Grimshaw: I photographed it actually.

Tom Passell: Did you? In any case I ... So, that was an example of things I wouldn't change anything that we did. I had to act as if I believed people, and indeed I think that's a better way to go than to actively disbelieve everybody.

Tom Grimshaw: Yeah. I agree with you. Take people at face value until you learn otherwise.

Tom Passell: And I wouldn't be surprised if he committed suicide having been exposed that way in a public arena, but I don't know that. It could be he just had a heart attack or something.

Tom Grimshaw: Yeah. Well, there's some reason that his death certificate was not to be disclosed for 25 years. It makes you wonder.

Tom Passell: So anyway, that was an experience. I can see that Douglas Morrison who was in the high energy physics business, who knew Kevin from other things who had asked that question at Monaco, "Did Kevin give you permission to reveal this?" You could begin to see that there was something going on here that in effect they were trying to filter off money from cold fusion to things that they were interested in.

Tom Grimshaw: Yeah.

Tom Passell: That was the bottom line. And Douglas Morrison in all of his negative stuff he was fond of doing by visiting all of our meetings and giving his report each year. He's dead too, by the way.

Tom Grimshaw: Yeah, I heard that.

Tom Passell: So, that was an experience that I found a little disturbing.

Tom Grimshaw: Yeah. More than a little distasteful.

Tom Passell: It was distasteful. And how could I have not seen through that right away. Again, I think I played it okay. I begin to wonder just how pervasive is this business of exaggerating things to get money from people who are not knowledgeable about science. I could see that that was an underlying principle that was going on more widely than in just my little neck of the woods. In fact, the whole hot fusion game looked like that kind of thing. They're getting money for things that were very, very long shots to be able to make a plasma stable.

Tom Grimshaw: Yup.

Tom Passell: I think that was a bit of an education as I saw it. I was trying to think of what happened then. Well, about 1998 my wife dies, so I am forced to retire June 4th of 1997 at the age of 67.

Tom Grimshaw: I see. At EPRI?

Tom Passell: At EPRI.

Tom Grimshaw: That was a mandatory retirement age?

Tom Passell: Yeah, I couldn't go on. I had already gone two years beyond 65, and I had to work half time to do it.

Tom Grimshaw:	I see.
Tom Passell:	But they gave me my office access for a subsequent nine years.
Tom Grimshaw:	Oh, okay.
Tom Passell:	So I could play table tennis every day. And I played table tennis for 40 some years every noon hour from everywhere I worked at SRI and Lockheed. I credit that mild exercise as being a helpful thing to me.
Tom Grimshaw:	No doubt, yeah. Okay, so from '94 to '97 you were at EPRI, but it was in the era after the SRI support, but you were still supporting other investigators.
Tom Passell:	Not many others. They were the last big one.
Tom Grimshaw:	They were? Okay.
Tom Passell:	There was a small one here and there, but I can't remember them. But they were unsuccessful. One guy thought he could do something with light hydrogen, and actually having the idea that there was a deep Dirac level in an atom, shrunken atoms like Randy Mills believes.
Tom Grimshaw:	Okay, and the Hydrino.
Tom Passell:	The Hydrino. They thought that that was maybe the reason why you could get some excess heat with nickel and light hydrogen. That's why everybody's interested in lately and Ed Storms is claiming.
Tom Grimshaw:	Okay.
Tom Passell:	That you mentioned. That's why I was interested in Storms' latest results.
Tom Grimshaw:	Do you remember his name, that individual?
Tom Passell:	It'll come to me. I'll have to look it up. It's been a while since I've talked to him.
Tom Grimshaw:	No problem.
Tom Grimshaw:	So you retired from EPRI, and that was the end of that phase of your cold fusion involvement. What did you do next in cold fusion?
Tom Passell:	Well, I didn't so much except for I did attend I think some of the meetings all the way to 2002. I went to Beijing in 2002. In 2003 and 2004 where was I, whatever meetings I attempted. I went to one in Yokohama and one in Marseilles. I think the 11th one was in Marseilles in 2004. I had a lab across the street with Tom Benson.
Tom Grimshaw:	Okay.
Tom Passell:	We were trying to do glow discharge or whatever else we could in this lab across the street.
Tom Grimshaw:	Okay. Was that the D2 fusion?
Tom Passell:	The D2 fusion was after that.
Tom Grimshaw:	Okay, so let's talk about when did you start with Benson?
Tom Passell:	Benson was sort of around with Roger Stringham.
Tom Grimshaw:	Okay.

Tom Passell: And then he came over to work with me for a while. Benson is somewhat disabled now from sciatica I guess. In any case, he survived. He was only in his middle 50s.

Tom Grimshaw: Okay. So, tell me about the work that you were doing with Benson.

Tom Passell: Well, we were trying to do glow discharges to see what we can see in the way of some nuclear emissions or excess heat.

Tom Grimshaw: Okay, so you were looking for gamma?

Tom Passell: I was looking for gammas or heat. Sometimes I was even trying heat, but I didn't have much ... I even had a bunch of solid state dosimeters. You could monitor people. They would give you dosimeters, and then they'll read them back to you and I would put them around and see if I could pick up any radiation of a general nature.

Tom Grimshaw: That's pretty clever, yeah.

Tom Passell: But nothing came back.

Tom Grimshaw: Okay.

Tom Passell: They had the place down in Gilroy where they did analyses of that kind of thing. Basically we were experimenting with glow discharge. I think that was about it.

Tom Grimshaw: What was his first name again?

Tom Passell: Tom.

Tom Passell: Tom Benson. He's still around. He's pretty much immobile in a wheelchair, but he's doing other things now.

Tom Grimshaw: What came after Tom Benson? What was your next escapade?

Tom Passell: I had this lab by myself, so I was able to try a lot of different, a tremendous number of long time runs with all kinds of metal electrodes in deuterium gas.

Tom Grimshaw: Now, I think you had told me you had opened this lab in 2013, right?

Tom Passell: No, I've been here 13 years.

Tom Grimshaw: 13 years, okay.

Tom Passell: Since 2008.

Tom Grimshaw: Oh, okay, thank you.

Tom Passell: January 1st. I moved in here in January, and Joan liked the idea, so she took the first two rooms here for her interests, and I had the back place for the laboratory.

Tom Grimshaw: Okay. So, she was doing her photography work?

Tom Passell: Photography mostly, and she talked about doing quilting but didn't do any.

Tom Grimshaw: Okay. Well, there's still some of her things here.

Tom Passell: That's right.

Tom Grimshaw: Your carousels full of 2X2 slides.

Tom Passell: I know some of those quilting things have been given away to people who were interested. And I had all the pictures that we took on all our trips digitized, so I have those in a form that I can see on a computer.

Tom Grimshaw: Sure. Okay. This was her space up until the time of her passing or until she began to not be able to function anymore?

Tom Passell: That's right.

Tom Grimshaw: Yeah, okay.

Tom Passell: She liked to watch all of the lectures on the life and music of 19 major composers.

Tom Grimshaw: Okay.

Tom Passell: We saw them four times. And all the art history since Charlemagne. There with a lecturer talking about each picture that was famous for those periods. I could get her interested and keep her interest by using those lecture series from the teaching company.

Tom Grimshaw: Mm-hmm (affirmative). Yeah, the great painters. There's just a lot on my mind. The great story ... It's right here on the shelf if you look up.

Tom Passell: Yeah, right.

Tom Grimshaw: Great stories of great history or something, anyway, sorry.

Tom Passell: Yeah, a lot of history. And then I always threw in a bunch of science things. Sometimes I could get her interested in the origin of the Earth and plate tectonics. She really enjoyed understanding how plate tectonics works. I even used that when she was over at a nursing home.

Tom Grimshaw: Yeah.

Tom Passell: For at least a while.

Tom Grimshaw: Yeah.

Tom Passell: So, that was pretty much it. I still kept track of things. I was trying to remember the last one of the ICCFs that I was at. What would that be?

Tom Grimshaw: Colorado?

Tom Passell: I was able, but I didn't actually go.

Tom Grimshaw: Well the one before that was in Japan in Sendai.

Tom Passell: I didn't see that or the one in India either.

Tom Grimshaw: Yeah. ICCF-14 was in Washington.

Tom Passell: 14 in Washington may have been the last one I attended, in Columbia, Missouri. That was 18, wasn't it?

Tom Grimshaw: Yeah, that was ICCF-18. 16 and 17 were in Rome and Sendai.

Tom Passell: I went to Rome in '09.

Tom Grimshaw: Okay. And then it was India.

Tom Passell: Yeah, I missed the India one.

Tom Grimshaw: Okay. And then it was in Washington.

Tom Passell: Okay. Washington I went, and that may have been the last one that I was at.

Tom Grimshaw: No, I'm sorry. It was in Korea 17.

Tom Passell: Korea? I missed Korea too.

Tom Grimshaw: Okay. So 16 was India, 17 was Korea, 18 was in Columbia, Missouri. You went to that one?

Tom Passell: I think that may have been the last one.

Tom Grimshaw: 19 was in Padua, Italy.

Tom Passell: I thought about that and didn't go.

Tom Grimshaw: Okay. And then 21 was the one in ... Well, 21 was in Colorado.

Tom Passell: Yeah, in Fort Collins.

Tom Grimshaw: In Fort Collins, yeah okay.

Tom Passell: I was interested, and I've still got some of the literature and stuff, and I followed it but I didn't go to it.

Tom Grimshaw: Okay. Now what about D2 Fusion?

Tom Passell: Yeah, that started in '05.

Tom Grimshaw: Okay.

Tom Passell: That was under Russ George, a venture capitalist who was willing to fund us for a few million bucks. His main interest, of course, was in Planktos to spread iron oxide in the ocean for energizing the plankton to feed the fish using the existing nutrients, the phosphate in the ocean that without the iron as a catalyst plankton didn't bloom.

Tom Grimshaw: Okay.

Tom Passell: He was trying to sell that idea more widely as a means of controlling global warming.

Tom Grimshaw: Right, Planktos, okay. I hadn't heard that term.

Tom Passell: Planktos in the name of the company.

Tom Grimshaw: Okay.

Tom Passell: Along with that was D2 Fusion under the same roof.

Tom Grimshaw: Okay.

Tom Passell: From '05 through the end of '07. When they folded at the end of '07 I inherited all of the equipment, and I brought it over here.

Tom Grimshaw: I see. You said it was very nearby, wasn't it?

Tom Passell: Yeah, it was in Foster City.

Tom Grimshaw: Oh, okay.

Tom Passell: It's not far away.

Tom Grimshaw: Yeah, okay.

Tom Passell: So, that was Russ George, and he was interested in finding helium and replicating Les Case. It looked like we were seeing what Les Case claimed. What we were doing was reducing the pressure due to spillover hydrogen off of the catalyst, palladium, and causing a reaction between D2 gas and the carbon substrate to make methane, other longer-chain carbon molecules. Overall pressures going down, and the residual trapped helium that was in the original D2 was increasing in concentration. It looks like you were making it, but it was just concentrating it. So again, he didn't want to publish that. The surprise I know we have it prepared to publish, but never did. It was a painful thing for him to admit that that was the basis on which he had sold the whole project, finding helium. He did have a helium detector or you know, the quadrapole. So, that was what his main interest was in running during D2 Fusion.

Tom Grimshaw: Now, were you doing lab work?

Tom Passell: I was doing lab work. Benson was also side by side with me over there.

Tom Grimshaw: Okay. And you were doing Les Case type cells then, I guess.

Tom Passell: I'm trying to do Les Case replications.

Tom Grimshaw: Yeah, okay.

Tom Passell: There was a whole bunch of stainless steel capsules about 100 cc in volume, and trying to keep track of the helium. Sampling them for increase in helium.

Tom Grimshaw: Right. How many cells were you ...

Tom Passell: I don't know, probably in the order of 20.

Tom Grimshaw: Yeah, and you were using the charcoal from coconuts.

Tom Passell: That's right.

Tom Grimshaw: Now, Les Case, I recall from other sources, had worked for United Catalyst Company.

Tom Passell: Had he?

Tom Grimshaw: And that's where he got his barrel of coconut catalyst.

Tom Passell: Oh is it?

Tom Grimshaw: And he used that. And I remember there was a treatment of high pressure, depressurization, and the palladium chloride would deposit palladium, and the chlorine would flush on out, and that's where the palladium got onto the charcoal, is what I understood about Les Case. You may know more.

Tom Passell: He was the first to claim to see helium in a robust way, and it looked like the concentration was going up.

Tom Grimshaw: So you were using the charcoal from Les Case?

Tom Passell: I don't recall what charcoal we were using, but we were using the same idea.

Tom Grimshaw: Okay.

Tom Passell: I didn't think that the charcoal was ... It did look like we were making helium because take out all the helium concentration going up because it had some impurity helium in the original D2 that we were using.

Tom Grimshaw: Yeah.

Tom Passell: So, that was ...

Tom Grimshaw: That came to an end?

Tom Passell: It was hard to explain that to the guy who had sold the project based on his beliefs that helium was it.

Tom Grimshaw:	But they made it straight with him, and ...
Tom Passell:	Well, it was too painful to publish.
Tom Grimshaw:	Yeah, okay.
Tom Passell:	The ironic thing, and again was that Russ George and Mike McKubre were trying to compete with each other as to who was going to prove the helium production process.
Tom Grimshaw:	Yeah. And I think McKubre felt that he did replicate Les Case.
Tom Passell:	That's right. He still, I think, believes in the production of helium by some experiment. And if he believes that Les Case is correct, I'm afraid that I don't.
Tom Grimshaw:	Yeah.
Tom Passell:	Because I can see the explanation is just plain wrong.
Tom Grimshaw:	Well, Ed Storms tried to replicate it, and so did Tom Claytor.
Tom Passell:	Did they now?
Tom Grimshaw:	Yeah. Both of them had ... When I first heard about it, it was called the Case study, and I said case study of what?
Tom Passell:	It was Les Case.
Tom Grimshaw:	Until you learned that it was Les Case. So, anyway, kind of a side note.
Tom Passell:	Well, helium seemed to be ... If it had been true it would've been hard to beat.
Tom Grimshaw:	Yeah.
Tom Passell:	Because it was a pretty strenuous job to rely on the measured helium without regard to being contaminated by helium in air.
Tom Grimshaw:	Of course.
Tom Passell:	And helium impurity in the original deuterium was what we found to be the cause of it. And I did the same thing with light hydrogen, and we also had the increase in the helium.
Tom Grimshaw:	Well, that's a bad sign.
Tom Passell:	Yeah. So, overall I was pretty sure that it was ... And I learned all about spillover hydrogen on a catalyst and that whole thing. And you can see that was an industrial process that any company could use to make hydrocarbons.
Tom Grimshaw:	Hydrogen spillover was the explanation then because it was reducing the concentration of D2.
Tom Passell:	The catalyst was being attacked by the hydrogen spilling off of the little particles of palladium apparently. That was how it was described.
Tom Grimshaw:	So, that was the end of D2 Fusion, which went on for a couple of years with Benson, you said. And then that was '07, so then you opened up here in '08 then in this lab. So, let's talk about what you did here.
Tom Passell:	Yeah, that's where I continued on to make glow discharge tubes, and I can't remember doing any other major thing.

Tom Grimshaw: You were looking for gamma-ray emitting isotopes using glow discharge, and what were the electrodes again that you were using?

Tom Passell: Well, I was using anything, any metal that would absorb hydrogen as an electrode in an atmosphere of the deuterium. Well, I had small voltages, so I had to keep the pressure down while still having discharge on. I set up about 12 times six, about 70 different discharge tubes.

Tom Grimshaw: Okay.

Tom Passell: Six at a time, and was all the metals that I could find that would be compatible with the system and let it absorb hydrogen. And in no case was I able to find any gamma rays coming out.

Tom Grimshaw: Okay. Have you ever described this set of experiments that you did? Have you ever written that down?

Tom Passell: I don't think I have. Actually, I never did it officially since I didn't see anything. You think a failure I shouldn't have to document. It takes a lot of work to document things.

Tom Grimshaw: Well, you documented it here.

Tom Passell: That's right. So far as that goes, it's out in the open. I didn't succeed no matter what I put in there. I tried to get lithium and boron into the act insofar as I could, so I got B. I either got separated isotope boron 10 you can buy and make an alloy and with an alloy making people locally and they make you Vanadium B2. That was one of my samples. I get a lot of boron in there. It still works as an electrode, so it has enough electrical conductivity. And I thought if boron would do it, then maybe if it's a DP reaction the proton would energize something in the substrate other element, Vanadium. And indeed, I looked for and didn't find anything there.

Tom Grimshaw: Okay.

Tom Passell: Anyway, that was the general approach after, and then I did not write all that up, but all that data is on those tapes that you had, the absence of any gamma rays.

Tom Grimshaw: Yup. Well, it would be nice to know what were all the compounds that you had tried.

Tom Passell: Yeah, right. I suppose I do have that list someplace.

Tom Grimshaw: Yeah, okay, interesting. One of the sets of data that we looked at yesterday when were collecting things was the diskettes from your work with the University of Missouri and the University of Texas. That was while you were still at EPRI or ...

Tom Passell: That's right.

Tom Grimshaw: Yeah. Talk to us a little bit about that work.

Tom Passell: Okay. That was when I was thinking that in the case of the University of Missouri I would look in their neutron beam they could look for boron 10 in the thermal beam coming out of the reactor. And you could put a sample in there and measure the signal from boron 10 absorbing a thermal neutron and an N-alpha reaction. But the fact that it's an N-alpha reaction means that the alpha coming out Doppler spreads the nice peak you would expect for a gamma ray and it becomes a long rectangle. I did that though, and I did see some depletion in boron 10. I reported that in my one of my reports in the various ICCF meetings.

Tom Grimshaw: And I think what you told me was that it was suggestive.

Tom Passell: But not definitive.

Tom Grimshaw: But not definitive of the cold fusion effect.

Tom Passell: In other words, it did seem that we were having some depletion of boron-10. The samples that I would collect from various people including Stanley Pons and Arata sent me samples or actually Russ George received a bunch of samples from Arata. It was pretty close to the chest. Arata wanted to know all about ultrasound, ultrasonics. Russ went over there, and he comes back with four samples of which three were samples that had produced a lot of excess heat according to Arata. So the fourth one was a virgin sample. And now those are four samples to check for boron depletion, boron-10 depletion. I think I'll also look for lithium-6 depletion using time of flight secondary ion mass spectrometry, time of flight SIMS. I did see again an occasional rather dramatic drop in lithium-6 in one sample or a couple of samples and just slightly beyond the limit of error, boron-10 depletion in other samples.

Tom Grimshaw: In the Arata samples, did you see any difference between the excess heat samples and the virgin sample?

Tom Passell: Yeah, that's the thing. I always had the virgin sample and then a sample of the same material that had produced heat and helium. Pons sent me some of their samples.

Tom Grimshaw: Did you see any difference between ...

Tom Passell: No, again, I reported that and it's in I forgot which meeting, but it was one of those that shows the boron rectangle, the boron-10.

Tom Grimshaw: Okay.

Tom Passell: That approach, if it had been able to be funded more thoroughly would eventually prove or disprove whether or not boron was the active agent. And the reason I was pretty sure is number one, if the heat was real and was too big to be atomic, it had to be nuclear by the size of it per unit mass, and then no nuclear things were coming out that anybody could see, it had to be something that was buried in there. And in the case of boron-10 DP, the P would go 100 microns. That doesn't get out of most samples. And the same thing with lithium-6 where you're dealing with electrolysis. I couldn't do lithium as sensitively as I did boron because it just isn't as easy to do. And I always worry about sampling something that's representative as opposed to a tiny spot.

Tom Grimshaw: Right.

Tom Passell: But again, again, suggested there's just enough there that says push this a little harder and see if you can prove it one way or another, and that's a really far extended effort.

Tom Grimshaw: Yup. And lots more money. Tell me the individual's name at the University of Missouri you were working with? It started with an I, I think.

Tom Passell: That's Das Kutikkad.

Tom Grimshaw: Kudikad, okay.

Tom Passell: And it was Iskander at the University of Texas.

Tom Grimshaw: Talk to me about the University of Texas work. Is it similar to what ...

Tom Passell: Yeah. I was just mentioning NAA to see if I could see if there was some change between a heat producing sample and a virgin.

Tom Grimshaw: Okay.

Tom Passell: And again, the things that I saw there were depletion like palladium-108 relative to the virgin.

Tom Grimshaw: Okay.

Tom Passell: At least in one sample. I had to be forced to think it could possibly be a palladium isotope interacting and making excess heat. If I believe my data, and I was pretty careful, and outside the limits of error as I did it, it does depend on an accurate integration of a gamma ray peak. And there were some pretty sharp ones, so that may be part of the problem measuring palladium-108.

Tom Grimshaw: So, your work at the University of Texas was kind of like what I would say, I don't want to use this term gross, but kind of an overview.

Tom Passell: Yeah, overall. All the elements that might show up in a radiation in a sample without worrying about which element you're dealing with.

Tom Grimshaw: Right. Okay. And then work at the University of Missouri was looking specifically for depletion of ...

Tom Passell: Boron-10.

Tom Grimshaw: Boron-10 primarily and lithium-6 somewhat.

Tom Passell: That's right.

Tom Grimshaw: Okay.

Tom Passell: That seemed to be a pattern looking back on it now.

Tom Grimshaw: Yup. All right. Talk to me about Oppenheimer-Phillips. When did you come to the conclusion that the most likely explanation for the cold fusion effect would be through Oppenheimer-Phillips.

Tom Passell: Oppenheimer-Phillips is a deuteron stripping reaction in which you don't have a compound nucleus, but the dumbbell nature of a deuteron has a proton on one end and a neutron on the other. And sometimes it vibrates and spends a lot of its time apart from each other, the proton and the neutron. So, in passing through a material the neutron can slip in and strip away the proton.

Tom Grimshaw: Okay.

Tom Passell: And that process had been observed by Oppenheimer and Phillips in 1935 at Berkeley with a 60-inch cyclotron. And they observed how much enhancement

	there was to reactions in which that reaction was happening versus other deuteron reactions in which the whole deuteron had been a part of the compound nucleus, not just the deuterium being stripped of its neutron. A traget enhanced the probability of your penetrating because the neutron didn't have anything standing in its way.
Tom Grimshaw:	Right, if you go right in there because it has no charge, of course.
Tom Passell:	Right. And that would also fit all the other things about it. The proton, upon being produced, takes all the energy of the DP reaction with it, but it still doesn't go very far. It maybe goes 100 microns in palladium.
Tom Grimshaw:	Okay.
Tom Passell:	So that explains why you don't get any ...
Tom Grimshaw:	Radiation.
Tom Passell:	Any radiation. It's a totally captured product. But you could get heat.
Tom Grimshaw:	Okay.
Tom Passell:	So, it's most probable compared to say the D-alpha reaction on boron-10 where you have to have a charged particle coming oout. The D has to entirely get into the boron-10 nucleus before the alpha can come out. And this is a much lower cross section, less probable. It is more probable that we would have a stripping action. And then of course if you get excess heat with heavy hydrogen and not with light, that's another good indicator.
Tom Grimshaw:	Yeah, okay. And when did you decide that this Oppenheimer-Phillips reaction was the most likely explanation?
Tom Passell:	About that time. I couldn't think of any other, using logic and assuming that some people were measuring excess heat.
Tom Grimshaw:	Yeah. So you could get the heat without the radiation.
Tom Passell:	So that I put the paper in the CMNS journal, volume 15.
Tom Grimshaw:	Oh you did? Okay.
Tom Passell:	Online it's 2015.
Tom Grimshaw:	2015, okay.
Tom Passell:	That's that final item in our list of my publications.
Tom Grimshaw:	Yup.
Tom Passell:	And I announced it, of course, in Columbia, Missouri.
Tom Grimshaw:	ICCF-18.
Tom Passell:	I mixed up my slides, so I couldn't get the right set of slides that I had time to do. In any case, I exposed the whole idea there, and a lot of people were having trouble with that. They didn't want to believe it.
Tom Grimshaw:	Do you still believe it?
Tom Passell:	I think that's its most likely. If we're getting anything at all, I think that fits all the requirements. There's no external radiation.

Tom Grimshaw:	Right. The proton can only go a short distance, and then it gives up its energy to the lattice as vibrational energy or heat. So, the Oppenheimer-Phillips occurs in a solid material?
Tom Passell:	Yeah, right.
Tom Grimshaw:	Yeah.
Tom Passell:	It was first discovered, of course, in the cyclotron at Berkeley.
Tom Grimshaw:	Yeah, okay.
Tom Passell:	That's why it's called Oppenheimer-Phillips.
Tom Grimshaw:	Yeah.
Tom Passell:	They observed the ratio of increase in a cross section, a rather dramatic difference for a DP reaction compared to a DN.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	The DN requires the whole deuteron to get into the compound nucleus. The DP reaction, the proton can slip on by.
Tom Grimshaw:	And the neutron goes in.
Tom Passell:	And the neutron goes in.
Tom Grimshaw:	There's no Coulomb barrier to a neutron.
Tom Passell:	Kind of got that from logic thinking. If you believe the excess heat has to come from somewhere, heat is too big to be anything but a nuclear process, and if so it has to be a nuclear process and one that doesn't put out anything external. That's how you come out with DP.
Tom Grimshaw:	Okay.
Tom Passell:	That's the most likely thing in which a deuteron can participate that is unique to it.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	That's why I'm always suspicious, guilty until proven innocent nickel plus light hydrogen.
Tom Grimshaw:	Yeah. Here's no deuteron there.
Tom Passell:	There's no deuteron there.
Tom Grimshaw:	Yeah, I hear you. All right, so it's kind of getting toward the end here, but I always like to ask this question. Do you think cold fusion is a real phenomenon?
Tom Passell:	You can rephrase it. Do I think there is a single experiment somewhere that you can believe? Okay. That's how I would rephrase this question. And I guess that the answer is yes, but a very small fraction of those experiments are convincing, very small fraction. And the most careful people in calorimetry I think have been maybe the Japanese and SRI, and a lot of the others I'm not so sure about. The heat business of Arata looks pretty robust also with his double cathode, but in general I think unless the people are outright lying to me, which isn't impossible to imagine just because of the stakes that are so high, if that's happening and there's

	no excess heat to explain, then that's the only case where I think they would make me feel that it was all a bunch of hyperbole as they call it these days.
Tom Grimshaw:	Yeah, hyperbole.
Tom Passell:	Yeah, it probably is an exaggeration to make money.
Tom Grimshaw:	Yeah, okay.
Tom Passell:	Or to become elected.
Tom Grimshaw:	Right. Well, you've seen a great deal, a lot of experience with the human experience, and certainly your perspective is very valuable in all this. We've talked about the religious wars at the root of some of these phenomena.
Tom Passell:	The whole saga of cold fusion is similar to a religious war!
Tom Grimshaw:	And I very much appreciate your taking the time and giving me the energy and so forth to do these interviews and the other things that we've done.
Tom Passell:	Thanks for doing what you're doing because not everybody wants to document reality.
Tom Grimshaw:	Well, that's the hope that as I've told you before, my hope is to have some little bit of legacy on bringing this thing to fruition for the benefit of humankind because without it I don't see how we're going to make it.
Tom Passell:	Fleischmann had to stay with it because it was just too big to let go of.
Tom Grimshaw:	Yeah.
Tom Passell:	It was truly too big a benefit to say no to if you can keep it going.
Tom Grimshaw:	Yeah.
Tom Passell:	And I guess when I see Fleschman I don't see him faking it.
Tom Grimshaw:	No.
Tom Passell:	So that to me was reassuring.
Tom Grimshaw:	Yup.
Tom Passell:	However, a disturbing number of things have happened that show people exaggerating their results.
Tom Grimshaw:	Yeah, chief among them being the Kevin Wolf situation that you ...
Tom Passell:	Yeah, that was the extreme case.
Tom Grimshaw:	Yup. Okay, so we'll wrap this up. Thank you again. This is Tom Grimshaw with Tom Passell. We've been focusing primarily on his work in the cold fusion arena. This is April 23rd, and I believe that this is session number five. It will be our last session for this round anyway. So, thank you again, Tom, and we'll sign off here.
Tom Passell:	You're welcome Tom Grimshaw.

Appendix B3. Interview for EPRI-Supported LENR Research at SRI International, August 22, 2021

Tom Grimshaw:	Okay. This is Tom Grimshaw. I'm here with Tom Passell. It's August 16th, 2021. We're here to talk about his work with SRI International, namely Michael McKubre and Fran Tanzella and others. Let's see. I think I should say that we're here at Tom's laboratory in Mountain View and that this interview is part of a project that Fran Tanzella and I are doing for documenting the entire research record at SRI over the years. This interview will cover the period from the time of the announcement in March of 1989 up to the end of 1994, which is when Tom Passell, when he was at EPRI, when EPRI discontinued support of SRI. That's kind of the framework, Tom.
Tom Passell:	Okay.
Tom Grimshaw:	If you can go back to the very beginning and tell us what happened there around that March 23rd timeframe and how EPRI and you became involved with SRI International.
Tom Passell:	All right. Okay. Well, on hearing the announcement, I decided to write a memo, and I have it in the files, to see if my colleagues in my little department, five or six of them, would like to sign this memo asking that EPRI support investigating whether this announcement had any reliability. I got nobody to sign on with me. However, later, there was sign-on by Joe Santucci in the area of the exploratory research department. In any case, I had built up, from my work with McKubre on this hydrogen dissolved in water measure that we had, and it had developed that palladium... A dipstick of palladium in the hot water was going to come to equilibrium with hydrogen entering the palladium lattice, changing the palladium lattice's resistivity... and use it as a measure of how much hydrogen is in equilibrium in the water. That would be at the high temperatures like 300 degrees centigrade. There was no uncertainty about that being an impediment to the buildup of hydrogen dissolved in the palladium metal of the measuring probe.
Tom Grimshaw:	I'll jump in and summarize from our previous discussion that it's a good thing for there to be a high level of dissolved hydrogen gas in the fluid in the high-pressure and high-temperature side of a pressurized water reactor-
Tom Passell:	Right.
Tom Grimshaw:	... and that the idea here was to have more hydrogen in the water to, I guess, to suppress corrosion.
Tom Passell:	Right.
Tom Grimshaw:	What was the other reason?
Tom Passell:	Well, to reduce the amount of decomposition of water by the intense radiation in the core.

Tom Grimshaw:	Thank you.
Tom Passell:	That was a nuisance. It was decomposing the water into hydrogen and oxygen, but the oxygen wasn't good from the point of view of corrosion.
Tom Grimshaw:	Okay. Right.
Tom Passell:	Anyway, and then it turned out, later, I guess I knew it already, that Fleischmann was well known to McKubre and therefore trusted him and his judgment based on his history of discovering the enhanced Raman effect on the surface of metals like silver of a million times more likely if you have a substance attached to a surface in the Raman effect that occurs than if it were by itself away from that precious-metal surface.
Tom Grimshaw:	It was for this work that Fleischmann might've been considered for a Nobel Prize-
Tom Passell:	That's right.
Tom Grimshaw:	... but, in any case, he did become a member of the Royal Society.
Tom Passell:	He was given credit for having discovered it, the enhanced Raman effect. All of that gave everybody, McKubre included, a lot of respect for Fleischmann. So he starts out... he's innocent until proven guilty. In other words, he's to be believed over and above the average person that might make such a claim.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	Since I had that project already going, I called him up and said, "Forget the statement of work. Let's see if we can repeat to confirm or deny what Fleischmann has claimed," and he agreed and did that and almost immediately set up a cell to measure excess heat. That started everything off, and very quickly, of course, the ERAB panel show up on July, 6th, after and gave McKubre a chance to show his very quick results early, and they showed some excess heat, but not yet definitive.
Tom Grimshaw:	Do you recall who was the representative of the panel that attended that meeting?
Tom Passell:	Huizenga was there.
Tom Grimshaw:	John Huizenga.
Tom Passell:	That was the main person, but I don't remember all the others.
Tom Grimshaw:	What were there, two or three more?
Tom Passell:	Yeah, there were several more.
Tom Grimshaw:	Several. Okay.
Tom Passell:	In any case, I was representing the fact that I had turned McKubre's project on hydrogen as a measure of the dissolved hydrogen gas for corrosion into a project to try to reproduce Fleischmann's claims. Fleischmann had a great deal of confidence, in McKubre's mind, of being a very reliable person and therefore not likely to make such a terrible mistake as to go out public with something that wasn't true.
Tom Grimshaw:	Mm-hmm (affirmative).

Tom Passell:	That was the beginning, and I ran out of money fairly soon in that project, there wasn't that much in there, so we had to get a wider group of people at EPRI. Practically every subgroup in EPRI, in the nuclear division as well as the fossil, wanted a piece of the action, and they were very much interested and had meetings as to whether they believed it or not and whether they would use some of their budgets to be diverted into this.
Tom Grimshaw:	So, this was quite a turnaround from the initial outreach that you made within your own division-
Tom Passell:	Mm-hmm (affirmative).
Tom Grimshaw:	... to now where much of EPRI-
Tom Passell:	The whole institute.
Tom Grimshaw:	Yep, wants a piece of the action. Okay.
Tom Passell:	Yeah. Right, and there were subgroups within nuclear who wanted in. The guy working on the breeder for nuclear reactors and others, who wanted a piece of the action and went to all their speculative meetings at SRI. SRI, being close, was easy to access, and so I was going over to SRI almost every day, or certainly once a week.
Tom Grimshaw:	Yeah. When you say close, how close? Was it a few blocks?
Tom Passell:	I rode my bike over there, so it was several miles.
Tom Grimshaw:	Yeah. Okay.
Tom Passell:	But not too far. Downhill going, and uphill coming back.
Tom Grimshaw:	Okay.
Tom Passell:	So, that was the beginning of it all, but I couldn't get any of my immediate little small group on corrosion to sign on to my memo. But many of them were not so nuclear oriented as I was with my background. My background out of Berkeley and doing radioactivity and gamma rays and so on was not necessarily on par with the people who were in this little subgroup.
Tom Grimshaw:	Right. I understand.
Tom Passell:	Anyway, I didn't blame them or anything, but I got a lot of heat from my immediate supervisor, who was worried about the standard stuff that I had been doing and being diverted by my frequent absence over at SRI, because I could go over there almost any day and get an update. That was a pretty good connection, and, again, Joe Santucci was an up-and-coming younger fellow in the nuclear division, but eventually he got budget and moved from the nuclear division to the exploratory research division. So he was able to get a much bigger budget and was very much interested in commercializing this thing, so he was very much into the patents, "Can we get something patented?" and getting a little more secretive about things. He actually went to the second International Conference in Como, Italy. I was at the first one in Salt Lake City.
Tom Grimshaw:	But not at the second one. You did not go to the second one.

Tom Passell: No, I didn't go to the second one. He outranked me on that because he was putting more money in it, and he was also much more aggressive in getting it commercialized; patented.

Then, of course, the accident occurred on the 2nd of January, 1992, 3 years into it, almost. That took Santucci out of the picture because it was under his watch that this occurred, being so eager to make everything work and taking risks. They had disconnected the pressure gauge from the closed cell because there was a leak in the line to the pressure gauge. So they pinched it off and continued on, but they lost track of what the pressure was. It got up to a point where, when it exploded, it took the top of the can into the people's face that was looking over it, and one guy was killed.

Tom Grimshaw: Mm-hmm (affirmative) Riley was his name, if I recall.

Tom Passell: Let's see, what was his name? Oh, gosh. It was Andy Riley.

Tom Passell: He had come to EPRI, actually, by way of SRI, I think, or maybe he was hired at EPRI. Who knows. Anyway, he was attached and he had experience at a previous location in Utah. He was the guy who was killed as an eager young fellow. That caused a big upset and we had to redo the laboratory according to OSHA requirements.

Tom Grimshaw: Right. Before we elaborate on that timeframe, which would've been January of '92-

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: ... January 2nd, tell us about that timeframe between March of '89 until '92. What kind of experiments were being done. Was the money coming from your budget, or at that time it was coming from Santucci's budget?

Tom Passell: Yeah. Santucci's budget was big in those days.

Tom Grimshaw: Yeah.

Tom Passell: So I can't honestly remember all of the financial issues.

Tom Grimshaw: Okay, but you were still going to SRI in that timeframe.

Tom Passell: And I got, eventually, in charge. EPRI put me in charge when the accident occurred.

Tom Grimshaw: Okay.

Tom Passell: They essentially said, "Well, we're going to have new management." Something like this. It was the first time that SRI had had a casualty in their laboratories and it shook up things.

Tom Grimshaw: Mm-hmm (affirmative). Not a good first.

Tom Passell: So they had to change something, and they put me in charge in place of Santucci. He still hasn't talked to me much about it lately, since then, so it was a bit of a falling out.

Tom Grimshaw:	So, you were in charge, starting at the time right after the accident, and then of course you were in charge during the lost year when the laboratory was being made in conformance with OSHA requirements.
Tom Passell:	That's right, and I got in charge of the big budget required to redo the lab.
Tom Grimshaw:	Where was that money from? Was that from also-
Tom Passell:	That was coming from a project within my department and nuclear.
Tom Grimshaw:	Okay. Okay.
Tom Passell:	I was in charge from then on, and Santucci was excluded. Not on purpose, I always offered that... but he never... He was shell shocked from it all.
Tom Grimshaw:	Yeah. Not a good thing to happen on your watch, of course.
Tom Passell:	No, it isn't, and it was avoidable if they'd been a little more careful.
Tom Grimshaw:	Yeah.
Tom Passell:	The C-2, C-dash-2, cathode was in that system when it exploded, and I got a bunch of measurements on the cathode, on C-2, with neutron activation.
Tom Grimshaw:	Okay. So, that was a analysis of the cathode to see if there was anything in the cathode or about the cathode that might have contributed.
Tom Passell:	Heat also have been there but not the cause of the explosion.
Tom Grimshaw:	Okay. Okay.
Tom Passell:	In other words, it was a normal hydrogen–oxygen explosion from the fact that the recombiner had quit, unbeknownst to anybody because they had cut off the line to the pressure gauge in the closed cell.
Tom Grimshaw:	Right. So, the recombiner had become overwhelmed-
Tom Passell:	Probably overlogged by liquid water.
Tom Grimshaw:	Overlogged. Okay.
Tom Passell:	That's my guess, yeah.
Tom Grimshaw:	Okay.
Tom Passell:	You can get too much recombination going on and the water drops down, but if it stops the whole initial reaction by covering the surface, then that's apparently what happened.
Tom Grimshaw:	Okay. That makes sense.
Tom Passell:	Anyway, that was a closed cell, but I made some use of the cathode material.
Tom Grimshaw:	Did you find anything that you can recall of interest?
Tom Passell:	Well, anything to do with C-2 was in there among all of those measurements.
Tom Grimshaw:	Okay. It's in the records that we'd been working on.
Tom Passell:	That's right.
Tom Grimshaw:	Yeah.
Tom Passell:	I did find some alterations, but nothing strikes me at the moment.
Tom Grimshaw:	Yeah. Okay.

Tom Passell: It wasn't definitive, but perhaps suggestive.

Tom Grimshaw: So, I'm going to put us on hold just for a minute or two.

Tom Passell: OK.

Tom Grimshaw: Okay. Tom Grimshaw here again with Tom Passell. We had a very short break there. We were just concluding, I think, our discussions about the explosion that occurred.

Tom Passell: Right.

Tom Grimshaw: Now you wanted to say something about Garwin at this point.

Tom Passell: Yes. The JASON group invited McKubre to present his results at one of their meetings. The JASONs were advisors to the Department of Defense on anything technical. They had a meeting rather regularly down in San Diego, and there were claims of all kinds showing up that they had to either put up or put down to keep the money from the Department of Defense being used usefully.

Anyway, Garwin was one of the chief guys, and he came and had been convinced by McKubre's talk at the JASONs as well as his visit to our laboratories. So, there's a whole exchange in one of those folders about the back-and-forth negotiations and letter writing.

Tom Grimshaw: Was this during 1992.

Tom Passell: This was '93.

Tom Grimshaw: Oh, okay. All right. So, we'll talk about that in just a moment. Going back before the explosion, during 1991-

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: ... were there any reports that were prepared that you recall, pre-explosion, from SRI about results?

Tom Passell: Oh, yeah. They had regular progress reports.

Tom Grimshaw: Okay.

Tom Passell: I don't remember seeing them in all of this pile of stuff that I'm sharing.

Tom Grimshaw: I haven't seen them.

Tom Passell: I haven't seen them, so it could be that [inaudible 00:18:40] misplaced them, or else that... Whatever happened, it's hard to say, but, in general, I had a file of them and-

Tom Grimshaw: Well, maybe we can look for it again after.

Tom Passell: Right.

Tom Grimshaw: Yeah. After this conversation. So, they were sending you regular progress reports. Even, I guess, during the Santucci era, they were-

Tom Passell: That's right. They applied for patents too.

Tom Grimshaw: Okay. So, that's a part of the record as well.

Tom Passell: That's part of the record-

Tom Grimshaw: Okay.

Tom Passell: Nothing of significance happened before that.

Tom Grimshaw: Then, during the last year, 1992, there wasn't much going on experimentally, so probably no reports.

Tom Passell: Yeah, I was mostly overseeing the revamping of the lab.

Tom Grimshaw: Yeah, revamping the lab and going through all the investigations.

Tom Passell: In fact, after the accident in '92, September 1st, all the people involved, that were interested at EPRI, gathered, and McKubre was presenting the status of things. Even SRI had, much later, a session from all of the alumni of SRI to hear it, one of their dinner meetings, but that was much, much later.

Tom Grimshaw: Okay. So, that was 1992, and not much record, but you had the Garwin thing going on with the JASONs.

Tom Passell: We had to prove that we didn't have a nuclear reaction, for one thing. Although, I was pretty sure it wasn't nuclear. Very quickly, the people from the Poulter Laboratories, who did explosive research using high-energy shock waves, they provided, from how much the can has bulged, what the peak pressure must've been. That led them to believe that the pressure in there had got to a factor of 10 atmospheres above atmospheric, and it explained why the can came apart.

Tom Grimshaw: Yeah. Popped the lid.

Tom Passell: Yep, popped its lid, and the top went past the head of the guy who was leaning over it.

Tom Grimshaw: Yeah. Unfortunate.

Tom Passell: Yep.

Tom Grimshaw: Okay. So, you revamped the lab.

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: ... and SRI had these internal meetings during 1992.

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: Pick the story up from there, I guess, going on into 1993.

Tom Passell: Okay. 1993, we had some more basic things, measurements, going on, and I was in charge of ICCF4 in December of 1993. We were preparing for that and discovered just before the meeting that some of his excess-heat measurements weren't true after all. But because somebody had miswired the system such that called into question that we were getting excess heat, and so he had to quickly change his tune and say, "We found that there was an error here and we didn't have excess heat after all in a new can." About 300 people had come from all over the world, and I had charge of it, so I could use my excess money from all my other corrosion meetings to fund the Russians to come.

Tom Grimshaw: Now this meeting was in Hawaii.

Tom Passell: Yeah, it was in Maui.

Tom Grimshaw: Maui.

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: Okay.

Tom Passell: First week in December of 1993.

Tom Grimshaw: Okay.

Tom Passell: Anyway, that was a disappointment. We thought we had it by the tail, but it wasn't so. But it was a tough time. McKubre wasn't going to go public with something if he found an error that made it not true.

So, that was a big point, and from '93 it was getting back to getting data.

Tom Grimshaw: Yeah. Any highlights of 1993 that come to mind besides the conference, of course, at the end?

Tom Passell: And preparing for that, yeah. Well, all the JASONs interactions occurred in that year.

Tom Grimshaw: Oh, it was. I thought that was in '92. So, JASONs wasn't until 1993.

Tom Passell: A lot of the dates are in '93.

Tom Grimshaw: Yeah, okay. All right. Thank you. All right. Any particular reports that came out? I guess they were still doing progress reports during that time.

Tom Passell: I have them on the shelf there, but there was a big one that... Exactly what the date was, I can look it up here. A big tome that went... sort of all the data they had collected that they believed in, and all the evidence. They had 16 or 17 different what I would call experiments, and showing what they had learned from each one.

Tom Grimshaw: Now, was this before he discovered the wiring problem? Do you know? In other words, was that reported affected?

Tom Passell: Well, they were different experiments-

Tom Grimshaw: Okay.

Tom Passell: ... so the wiring problem didn't figure in all the other experiments.

Tom Grimshaw: Okay.

Tom Passell: If they had a person working on the wiring, it was almost like a gnome sitting in the back room.

Tom Grimshaw: Yeah.

Tom Passell: So it was difficult to detect until you looked at the data that there was something wrong here.

Tom Grimshaw: Mm-hmm (affirmative). Yeah. Yeah. The results of that ICCF, I think, came out as EPRI reports.

Tom Passell: That's right.

Tom Grimshaw: Three volumes or-

Tom Passell: Four volumes.

Tom Grimshaw: Four volumes of the EPRI publication from ICCF-4.

Tom Passell: That's right. We published it in the same way I had been publishing the work on corrosion. I should say water treatment and corrosion.

LENRGY LLC

Tom Grimshaw: Mm-hmm (affirmative). Okay.

Tom Passell: I have it on the shelf here. I forget the date on it.

Tom Grimshaw: All right. Well, you're going to see if you can dig out the report. After we have this conversation, let's get out all your EPRI reports on this subject-

Tom Passell: Okay.

Tom Grimshaw: ... and we can document them.

Tom Passell: ... report summary. [inaudible 00:26:20] energy production. Okay. What's date on it? June 1998, final report.

Tom Grimshaw: Well, that was later.

Tom Passell: That's later. Yeah.

Tom Grimshaw: Yeah.

Tom Passell: Many months, so that's a lot later. Many.

Tom Grimshaw: Yeah. Well, let's look those up after our recorded interview here.

Tom Passell: Okay.

Tom Grimshaw: That'd be great because what I to do is kind of catalog all that you can remember that we can find during that timeframe.

Tom Passell: Sure. Yeah. I'll try to remember any other notable things to put on the record, but the JASON thing occupied... We followed through on Santucci's idea that we shall ask three wise men to look over the whole thing and give us a report. That was one of his good contributions. They were famous people in the whole chemistry and electrochemistry arena.

Tom Grimshaw: Now, is that a report that also appeared-

Tom Passell: They have three reports, one from each person.

Tom Grimshaw: Okay, and this was in 1993 also?

Tom Passell: About that time.

Tom Grimshaw: Yeah?

Tom Passell: Maybe a little earlier.

Tom Grimshaw: Oh, we'll find those. Get them locked in on the timeframe.

Tom Passell: Yeah. In some of those files there, I'm pretty sure there are reports of the three wise men.

Tom Grimshaw: Okay. So, I think that covers us through 1993-

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: ... and we have one more year remaining. Tell us about 1994.

Tom Passell: Right. Well, let's see. The controversies were beginning to get to our management and we were beginning to lose confidence since we were having trouble repeating the excess heat. So they finally decided to pull the string on the thing and ask for a final report at the end of '94.

Tom Grimshaw: Okay.

Tom Passell: So I had to switch onto other things. I retired eventually on 1997, so I was still involved with them in some form or another, but not as intensive. Of course, McKubre was looking for other sources of money, and he found some small ones with the government. I didn't try to keep track of that, but, in general, they wanted to take advantage of the fact that he had a big laboratory.

Tom Grimshaw: Sure. So, there was a final report at the end of 1994, I guess.

Tom Passell: Yeah, I think so, and I can-

Tom Grimshaw: We'll look.

Tom Passell: I'm sure it's in the files. Where it is is another question. A lot of the stuff came out in the ICCF Proceedings subsequently. The next one after that was the fifth one in Monaco, and even till the end McKubre supported Kevin Wolf.

Tom Grimshaw: Yeah. Okay.

Tom Passell: Again, he believed in the guy regardless of what evidence I had about this accident.

Tom Grimshaw: Well, now, you had known the Kevin Wolf work after you left EPRI. Is that right, or was it while you were still at-

Tom Passell: No, I was still at EPRI, and-

Tom Grimshaw: That's when you found the-

Tom Passell: When I tried to repeat his work, that was done on my own, outside of EPRI.

Tom Grimshaw: Right. Okay. All right. So, you continued from '95, '6 and '7 at EPRI, and then you retired in 1997.

Tom Passell: Right.

Tom Grimshaw: So, anything that comes to mind vis-a-vis SRI during those remaining years at EPRI?

Tom Passell: Well, I was still interested in what he was learning, if he was learning anything; with some other sources of money. At the moment, I can't come up with anything or individual, but I'd have to look and see if I could look at my diary and see.

Tom Grimshaw: Well, we're just kind of going by what you recall. That's fine.

Tom Passell: What I can recall that stands out. I did to some extent all of this work at Columbia, Missouri, with MURR and with the University of Texas at Austin doing NAA on samples. Then most of that was asking for samples from all sorts of people, including Pons.

Tom Grimshaw: This was after 1994 that you were doing that?

Tom Passell: I think so.

Tom Grimshaw: Well, again, we're going by memory.

Tom Passell: In any case, I spoke to the people at MURR in December of 1995, I believe. It was in that era that I was asking for samples, and some of them of course came from SRI. Others, we got from Arata in Japan and wherever I could with Russ George bringing them home. I asked for a lot of them from Stanley Pons, and he was very generous in sending the samples, so I give him credit for that. I made the measurements and the NAA and PGNAA, the prompt gamma from a thermal neutron out at Columbia. In the case of the Triga reactor down in the University of Texas, NAA was being done down there. They were interested, and so I got them to do it.

Tom Grimshaw: Perhaps we should back up at this point. We've been focusing on your SRI relationships and support from 1989, March '89, through the end of '94. What other entities come to mind that you were working with besides SRI during that timeframe? Who else were you working with?

Tom Passell: Well, I think I had a contract with a couple of people. Jerry Maly was thinking that he had shrunk the hydrogen atom and most of the energy was not a nuclear process but a shrinking of the ground state of hydrogen.

Tom Grimshaw: I'm sorry. Who was that with again?

Tom Passell: This was Jerry Maly. M-A-L-Y.

Tom Grimshaw: Okay.

Tom Passell: He got some money, and Peter Hagelstein got $100,000 to put out one of his reports on what he thought he was going on. Let's see. I'm trying to come up with other people. In any case, I was spending it. I also gave some money to Russ George-

Tom Grimshaw: While you were still at EPRI?

Tom Passell: While I was still at EPRI. I had money from EPRI. That played itself out. I don't remember the timing, what the timing was, but in general I had other projects that added up to $4 million, actually, total.

Tom Grimshaw: All cold fusion or other things?

Tom Passell: All cold fusion.

Tom Grimshaw: Really? Okay.

Tom Passell: I ended up spending about $10 million of EPRI money, as I remember, adding it all up.

Tom Grimshaw: I think you told me in a previous conversation that the total that went to SRI was about six and a half million.

Tom Passell: Six and a half million.

Tom Grimshaw: Yeah. Six and a half million. Okay. That's a pretty good chunk. That's another three and a half million that went elsewhere-

Tom Passell: Oh, right.

Tom Grimshaw: ... before you retired.

Tom Passell: That's right. I don't know if I could put my finger on it at all, but-

Tom Grimshaw: Well, whatever comes to mind.

Tom Passell: ... about 100 K went to Hagelstein. A small amount went to Jerry Maly and the people who thought they saw an effect with light hydrogen and nickel. That had to be a shrinking of the hydrogen atom. Honey, I Shrunk the Kids. You remember that?

Tom Grimshaw: Oh, yes. I do.

Tom Passell: Anyway, Honey, I shrunk the hydrogen atom and I got energy.

Tom Grimshaw: So, this is Maly. Is that related in any way Randell Mills' idea of the hydrino?

Tom Passell: Yeah, it's related to it. Yeah.

Tom Grimshaw: Okay.

Tom Passell: Hydrino is essentially the same thing.

Tom Grimshaw: I've seen some recent correspondence between Ed Storms and Muhlenberg has a different term for it, but apparently it's the s-electron or electrons getting down closer to the nucleus.

Tom Passell: Right.

Tom Grimshaw: Suborbital. Yeah.

Tom Passell: Oh, yeah. Suborbital. Right.

Tom Grimshaw: Yeah.

Tom Passell: A lower energy state.

Tom Grimshaw: Yeah. Yeah, we can look that up after this conversation if you're interested.

Tom Passell: Yeah. Well, I was highly prejudiced against that idea because it just seemed a bigger stretch than what I was doing.

Tom Grimshaw: Yeah. Well, you had a superior explanation that required deuterium.

Tom Passell: That's right. I don't know what the magic is that determines the bottom ground state of an atom, but there's something that won't let them go below that.

Tom Grimshaw: The S level, yeah. Two electrons in the S level, if I recall. Yeah.

Tom Passell: That's what I would say is less likely than the nuclear issue in terms of the nuclear process.

Tom Grimshaw: Yeah. Well, anything else come to mind in a broad sense with what you were doing with SRI in those early months and years?

Tom Passell: I spent a lot of my excess money from previous meetings on that big ICCF4 at Maui. I had to put money to cover the expenses of all the Russians who came. They didn't have a way of running a car, so I had to hire a consultant to drive them around.

Tom Grimshaw: Yeah, of course. You look at the timeframe here. The collapse of the Soviet Union was in 1990, and the-

Tom Passell: '91. Yeah.

Tom Grimshaw: '91, and the Berlin Wall came down during that timeframe.

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: So yeah, the Soviet scientists were really in a bad way at that time.

Tom Passell: That's right, they were, and they couldn't come at all unless I paid their whole way. I even had to have a driver for them around Maui. Al Miller was one of my colleagues at EPRI, and he agreed to... I guess he had just retired, but in any case he was available as a consultant. I paid him a consultant fee for him to drive the Russians all over Maui.

Tom Grimshaw: I'll be darned. Okay. Yeah, I've been to Maui, but not for the conference. But I've been there a few times.

Tom Passell: Well, it was in a giant hall, and the wind blowing the biggest gale I've ever seen. Steady wind, and I had my papers out with copies all over the area around the hotel.

Tom Grimshaw: Was this in Lahaina?

Tom Passell: It's around the... and it looks over to Lanai.

Tom Grimshaw: Okay. Yeah. Well, that would be Lahaina. That's the main city, of course.

Tom Passell: The big hotel there.

Tom Grimshaw: Yeah. Okay.

Tom Passell: Yeah. It's well known at EPRI how that was the first time that anybody had been bold enough to have a major meeting at a resort.

Tom Grimshaw: Yeah. Yeah.

Tom Passell: I broke all kinds of rules. That's still being talked about, I guess.

Tom Grimshaw: Well, you plowed new ground.

Tom Passell: I plowed new ground. That's right. Well, I thought, "Well, I could avoid that by having it in Anchorage, Alaska, in December, but that was voted down by almost everybody. I was trying to build a wall against the criticisms.

Tom Grimshaw: Countermeasures.

Tom Passell: That was my alternative. It was access accessible to the Japanese, and everybody could get to Anchorage.

Tom Grimshaw: It was also accessible to get to Maui.

Tom Passell: Yeah. Every time I saw one of the guys has since died

Tom Grimshaw: Jaeger?

Tom Passell: No, one of the Japanese... No, no, it was not the Japanese. One of the Russians.

Tom Grimshaw: Karabut?

Tom Passell: No, it wasn't Karabut. He's since died at the age of 50-something. In any case, every time I'd see him, he says, "Tom, thanks for Maui."

Tom Grimshaw: I know. His name starts with a B, I think.

Tom Passell: It wasn't him, but it was a Russian.

Tom Grimshaw: Oh? Okay.

Tom Passell: Anyway, one of those guys that had been to most of the meetings.

Tom Grimshaw: Yeah? Well, I think we probably covered the ground for the purposes of this project with Fran.

Tom Passell: Okay.

Tom Grimshaw: Thank you very much-

Tom Passell:	You're welcome.
Tom Grimshaw:	For your insight and recollections of what all took place during that early timeframe.
Tom Passell:	Well, I notice that there are blanks in my memory that don't seem to be easily retrieved, but-
Tom Grimshaw:	Well, we got 99%, so that's all-
Tom Passell:	We got pieces.
Tom Grimshaw:	Yeah. So we'll close by my saying again that this is Tom Grimshaw here with Tom Passell. It is August the 16th. We're in his laboratory here in Mountain View. This is actually part of a different project with Fran Tanzella where we're trying to document everything that happened in SRI. We're starting with the earliest months and years here. Thank you again, Tom-
Tom Passell:	You're welcome.
Tom Grimshaw:	... and we'll sign off.
Tom Passell:	I apologize in advance for any memory lapses.
Tom Grimshaw:	No problem.

Appendix B4. Interview for Oppenheimer-Phillips Explanation of LENR, August 23, 2021

Part 1 of Tom Passell Interview on the Oppenheimer-Phillips Explanation of LENR

Tom Grimshaw:	Yeah. So this is Tom Grimshaw. I'm here with Tom Passell and it's August the 16th, 2021. We're here in his laboratory in Mountain View, California. And we've been conducting a project of LENR Research, an L E N R research and documentation project, for Tom's work in cold fusion. And we're taking a little side trip, well not a side trip, but kind of a special trip and talk about the thinking that has guided Tom in his investigations into cold fusion over the years. So we had a very interesting start, I think, during lunch today Tom. So if you want to pick it up there, I would appreciate that, and let us know what's been guiding your thinking in your cold fusion endeavors over the years.
Tom Passell:	Thanks Tom. I'm glad to try to share with people, including yourself, how I got into it, and how I continue to hope for something of a good outcome. I was in charge of the work at SRI with McKubre, on electrochemistry of corrosion, and he had studied under...
Tom Grimshaw:	Martin Fleischmann?
Tom Passell:	Martin Fleischmann in South Hampton and got his degree there. So he thought that Fleischmann was a reliable person. He had already gotten famous from his work on the enhanced surface Raman effect. And I was in charge of money for corrosion. And he had been working on a method of measuring the dissolved hydrogen in water that is used in power plants.
Tom Grimshaw:	This is when you were at the Electric Power Research Institute EPRI?
Tom Passell:	That's right. I was at EPRI and I was the project manager, for doing anything that would enhance the fight against corrosion in the power plant.
Tom Grimshaw:	Right. Now let's be sure that I get properly calibrated here. In this conversation let's talk about your broad thinking, in a subsequent conversation we'll talk about the SRI.
Tom Passell:	All right, sure.
Tom Grimshaw:	Okay.
Tom Passell:	Well I was just introducing it that way but, I had a good deal of faith in McKubre. So McKubre being an electrochemist, when he measured some excess heat very early in the game after the announcement of March 23rd, 19, whatever it was-
Tom Grimshaw:	'89.
Tom Passell:	'89, right. Then I tended to believe his excess heat, the complete absence of neutrons or the usual things having to do with fusion, even without thinking much about it or doing many measurements, there wasn't a commensurate

amount of those things. So I said well, if it's excess heat, it's too big to be chemical. So it must be a nuclear process. And if it's a nuclear process it has to be contained, and none of the particles will have the energy to get out of the experimental cathode. So what are the options? I completely threw out the idea of the standard DD reaction.

Tom Grimshaw: Okay.

Tom Passell: That was so unusual.

Tom Grimshaw: Right. And we're going to pick it up there because I don't trust this device.

Tom Passell: Okay. You want to try and see if it's working.

Tom Grimshaw: I want to make sure it's working. So this is Tom Grimshaw. I'm here with Tom Passell, and we're doing an overview of his conclusions or his thoughts about how cold fusion works. And so with that we'll stop here and check to make sure things are working.

Part 2 of Tom Passell Interview on the Oppenheimer-Phillips Explanation of LENR

Tom Grimshaw: Okay. This is Tom Grimshaw again with Tom Passell. This is our second recording. It's August 16, 2021, and we're here in his laboratory in Mountain View, California. And this is the second session. And, Tom, in the first session, before we check to make sure the recorder was working, you were just talking about how you did not believe that cold fusion could be the result of D+D reaction. Can you pick it up from there, please?

Tom Passell: The one thing that was convincing, to me, and its reliable nature was the amount of excess heat that, even though not very easily reproduced, it has had a series of enough times. You had to believe it, and it was far too large to be any chemical reaction in that system. So it had to be nuclear by difference, but there were no obvious neutrons or tritium of any commensurate value compared to what you'd expect from the amount of heat. So, I think, as a premise, I believe in McKubre. I believe his excess heat, even though he thinks it's a very tricky set of conditions that are required. And he doesn't get it every time, and that is the downside.

But assuming, just for the sake of thinking, if you believe in the heat, even though it's rare and not easily reproduced, then its size has to be nuclear, that there's too much excess heat, too. And there's no obvious chemical reaction going on, so it had to be some nuclear reaction. So what are you left with? Well, you're left with, "What is the nuclear reaction?" And he did, at the beginning, find that he got nothing with light hydrogen, water, electrolyte, but he did get it with heavy water electrolyte. Again, only a couple of cases but enough to make us think that the heavy water was essential or let's just say that deuterium was involved.

So I was pretty sure, with the nuclear process, deuterium was involved. What else had to be there? Well, you have to look at the impurities, maybe, because the impurities were always the significant factor in corrosion. He's been working on corrosion. So what I'm curious is, how do you analyze it. The big standout is the number of cathodes with boron. Boron was purposefully added as calcium boride, as a getter, when they poured the metal into the mold, a graphite mold, when they were making metallic palladium.

Tom Grimshaw: Just to jump in, earlier you were talking about deuterium has to be involved in the process, with the palladium metal, because hydrogen will go into the lattice of palladium.

Tom Passell: Right.

Tom Grimshaw: And so, I just wanted to add that in for clarification.

Tom Passell: They're used to working with palladium, with me, for measuring the amount of hydrogen that is dissolved in the water of the nuclear power plant's primary system.

Tom Grimshaw: Before cold fusion ever was announced.

Tom Passell: That's right. And he had a meter that was measuring the resistivity of palladium that was sensitive to hydrogen that got into the lattice.

Tom Grimshaw: Okay.

Tom Passell: And indeed at the hot, high temperatures of a power plant, that was no problem of getting the hydrogen in. So he was just making his hydrogen meter based on resistivity. He could say how much hydrogen was in the water based on what the resistivity of the palladium was and equilibrium with it.

Tom Grimshaw: Got it.

Tom Passell: And it's also good enough for very high temperatures because it was a precious metal and, in general, a reliable thing. It did have two valued system. The resistivity goes up and then back down again as hydrogen loading is increased.

Tom Grimshaw: Okay.

Tom Passell: There was a slight difference in resistivity between light and heavy hydrogen.

Tom Grimshaw: Okay.

Tom Passell: Anyway, so what are we left with? Well, it couldn't be the palladium itself. Well, indeed. Later on, we discovered some variation, the isotopic ratios of the five palladium isotopes, palladium-102, 104, 105, 106, 107, 108, and 110. So there were lots of isotopes. And we saw some strange changes in those. But atomic number 46, which is palladium, has a pretty high barrier to any nuclear reaction.

Tom Grimshaw: To any kind of fusion.

Tom Passell: Any kind of reaction, any kind of nuclear nature.

Tom Grimshaw: Right.

Tom Passell: And so, therefore, what's left? Do we have any impurities in the system? And, of course, we mainly focus on boron because, in spite of the fact that we knew boron was there, all the manufacturers refused to tell us any detail as if that was a trade secret.

Tom Grimshaw: I see.

Tom Passell: And so, we had to work around that. But I just eventually found that calcium boride is on purpose added to cut down the amount of oxygen present in the metal, where that was deleterious to it use as a catalyst in automobile exhaust.

Tom Grimshaw: Okay. All right. So just to recapitulate for my satisfaction, that's called a getter. In other words, the boron, calcium boride, was used to get the oxygen out of the palladium because it reduced its effectiveness as a catalyst for auto emission control.

Tom Passell: Right. That's the idea.

Tom Grimshaw: Thank you.

Tom Passell: So boron was the only light element that was significant, but they also, according to the electrochemistry world, they preferred lithium as electrolyte, lithium hydroxide. So that we had the lithium, as electrolyte, and boron were the two elements that we will be suspicious of because of the lowest Z, the lowest atomic number yet, obviously, couldn't have done the D-D reaction, because otherwise we'd be swamped by the neutrons. We' be dead by the neutrons. Since we're not dead, it must be reactions having to do with those impurities, or the diffusion of the electrolyte into the body of the metal.

And we had a 300-hour delay between the time we started the experiment at SRI. That implies that there must be some diffusion going on. That takes time to get there, and that would be an explanation of the 300-hour delay. So all of this thinking led me to avoiding the whole business of expecting the D-D reaction to suddenly make helium-4 more likely than neutrons or tritium.

Tom Grimshaw: Okay.

Tom Passell: So that being the case is, what have we left with there? Well, boron is big. It doesn't seem like much of 100 ppm by weight, but that's 1,000 ppm by atoms because there's only a mass of 10 compared to the 100 roughly in the palladium. So, all in all, that brings you to what can you do with heavy hydrogen and boron into isotopes. Well, you can get the D-alpha reaction on boron-10 to make beryllium-8 and that would be 3 atoms of helium, but there wouldn't be helium from the D-D. The helium is from boron-10 plus D, to beryllium-8 which almost immediately turns into two helium-4's.

Tom Grimshaw: Okay. I got you.

Tom Passell: So you've got other sources of helium. When you do see helium, it could be a real aspect but not the helium that would be D plus D equals a helium.

Tom Grimshaw: Different source.

Tom Passell: As a normal nuclear physics vacuum conditions, D plus D makes helium-4, but only one in every 10 to the seventh reactions. So it's a very rare branch.

Tom Grimshaw: Branch of the branching ratio.

Tom Passell: The branching ratio is so low that it, depending on that, suddenly becoming the dominant thing to explain were just nothing about helium, but a neutron and a tritium. So, overall, it's just of, "So, what reaction among boron and lithium isotopes do we have available to us?" And then the stripping reaction with deuteron got the least impairment of jumping over the coulomb barrier. It still has the coulomb barrier. So the least coulomb barrier you can get, of course, it's passing deuterium past lithium atom, and stripping off its neutron of the deuterium, and the neutron captures in the substrate lithium or boron, and makes then a neutron capture.

And the subsequent reagent isotope gives you a certain amount of energy that is almost always being carried off the reaction as a fast proton, the P in the DP reaction. And you can check the path length of, say, an 8.0 MeV proton. It's only 100 microns. So that would also explain why nothing ever got out of the cathode.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: And if you tried to open or dissolve the palladium, you'd never find hydrogen atoms from the DP reaction on all of that. Boron-10 makes that, but the boron-11 and d,p reaction has a very low amount of energy. That would be obvious if boron-11 was the active isotope to produce boron-12, and that decays with a big energy drop into carbon-12.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: So it wasn't boron-11, and boron-11 is four-fifths of all boron. I should say three-fourths or four-fifths of boron, but boron-10 is 20%. And lithium-6 is only a small fraction of all lithium.

Tom Grimshaw: Okay.

Tom Passell: I think 5% or 6% or something of that order. So, those are all. And if it took time for the fusion to occur 300 hours, lithium-6 getting in, it might very well be a reason for that delay. So, this is the thinking that led to, eventually, saying, "Well, the most likely thing happening here is boron-10 DP reaction, and then nothing comes out except P or lithium-6 DP, which is not as energetic and not present in the metal initially. Only if it's in the electrolysis process added as the Li-6 isotope diffusing in.

Tom Grimshaw: Okay. So, let me jump in if I made this point. What you're talking about is the Oppenheimer-Phillips reaction. And what I hear you saying is that it's either lithium-6 or boron-10 that can get involved in the stripping reaction. So, either one of those contaminants can lead to the fast proton that then contributes to the heat and creates excess heat.

Tom Passell: That's right.

Tom Grimshaw: Okay.

Tom Passell: And that will explain why we never see anything outside the cathode for a result of that fast proton. In the case of boron-10, that fast proton can then get a P-alpha reaction on its original boron-10. It can make beryllium-7, a radioactive isotope of the 50-some day half life, but that's the secondary reaction. Only one in a million of those protons actually does that. So it's understandable why we practically never saw any radioactivity remnant in the palladium. So that explains everything, but I had difficulty getting anybody to making a boron alloy the way Imam did at the NRL out of pure separated isotope boron-10. And you can get it in any quantity without any trouble because it's so easy being a low element to be separated from B-11.

Tom Grimshaw: Yeah, the percent difference is high enough that you can get a separation.

Tom Passell: Besides that, that's available commercially.

Tom Grimshaw: Yeah.

Tom Passell: And same way with lithium, you could find lithium-6, of course, which was hard to deal with because of its use in the hydrogen bomb as lithium-6 deuteride.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: The big hydrogen bombs, so that meant that you can rather use lithium. The electrolyte ought to use lithium-7, and then I'm trying to talk people into separating lithium-7 for our electrolyte. What's the argument against that? Well, the argument against that was there's always ~~anybody~~ who made a separated isotope has a remnant of mercury. Mercury wasn't good to the whole business in the electrochemistry arena, and I refused to do that. That would prove if you've never gotten anything with lithium-7. But, occasionally, with normal lithium, that would put your finger on lithium-6. You can use lithium-6 hydroxide, of course, if you go to the trouble of getting it.

And in the case of boron, getting the manufacturer to be using boron-10 instead of natural boron and use calcium boride wasn't likely. So the best way to do that would be people like Imam doing alloys and palladium with boron and having used separated boron-10.

Tom Grimshaw: Mm-hmm (affirmative). Okay.

Tom Passell: This is 20% of normal boron.

Tom Grimshaw: Right.

Tom Passell: And so, that's something yet to be done. But again, that's the procedure. And by somehow or other extended experiments of even the electrochemical arena, we might be able to decide that's the source of the excess heat.

Tom Grimshaw: Mm-hmm (affirmative). Okay.

Tom Passell: That would be my prognostication of what we should do, given enough time and resources to pursue it.

Tom Grimshaw: Right. So experiments have not been done yet to definitively investigate your hypothesis.

Tom Passell: That's right. I could go Mel Miles if I hopped on with better results than if the palladium-boron alloy that Imam had made for him out of normal had used boron-10 enriched boron.

Tom Grimshaw: Okay.

Tom Passell: He got seven out of eight and going to make a small amount of excess heat. And that is another piece of evidence that boron was involved, and we might find it to be enhanced another factor-5 if it is pure boron-10.

Tom Grimshaw: Right. So the reaction was taking place with whatever boron-10 was in the sample.

Tom Passell: Right. So anyway, that's the way forward based on believing everybody's excess heat like McKubre's and explaining why we don't see neutrons.

Tom Grimshaw: Right.

Tom Passell: And that would again say the DP reaction is showing its effects by being much more probable than the case which requires whole nucleus, which is a bigger barrier than just stripping a neutron off the edge of that loose-as-a-goose deuterium.

Tom Grimshaw: Yeah.

Tom Passell: The neutron's just hanging on there the smallest absorbed Q value of binding of only 2.2 MeV compared to 6.0, 7.0, or 8.0 MeV for all the other elements in the entire system or atoms.

Tom Grimshaw: Mm-hmm (affirmative). Mm-hmm (affirmative). So, just to recapitulate, this is the stripping of the neutron and the high energy of the proton you offer as a way to get excess heat, not involving the D-D reaction, which is a hell of a lot, excuse me, a whole lot more difficult to achieve.

Tom Passell: That's right.

Tom Grimshaw: Yeah. Okay, very clever. Okay.

Tom Passell: So all of that made sense based if you believe the heat.

Tom Grimshaw: Yeah.

Tom Passell: And indeed, that's the foundation. And McKubre went to extreme measures to do the heat properly.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: But he didn't ever get to the conditions, enough of conditions to always get the heat each time.

Tom Grimshaw: Right.

Tom Passell: Even when he did get it, there seemed to be a 300-hour delay before you even see it on any given experiment.

Tom Grimshaw: How does the time required for diffusion relate to what you're telling us about the Oppenheimer-Phillips?

Tom Passell: I don't really know how long it takes, but the temperatures are modest and so pretty slow to get in from the outside electrolyte.

Tom Grimshaw: Okay.

Tom Passell: The other thing that McKubre did do though was to put boric acid of 100 ppm or something like that into the electrolyte.

Tom Grimshaw: With natural boron.

Tom Passell: In the electrolyte.

Tom Grimshaw: Okay.

Tom Passell: Again, natural boron. And that seems to enhance probability of excess heat.

Tom Grimshaw: Just with a 20% of boron-10.

Tom Passell: But you abandon that approach because heat was completely dependent on the palladium resistivity to tell how much loading there was.

Tom Grimshaw: Okay.

Tom Passell: And the loading hypothesis, that's the reason he was getting the reaction. And the difficulty getting high loading was such that that was his explanation for why it's so rare to find excess heat.

Tom Grimshaw: And if he used the boric acid, it would throw off that measurement of loading.

Tom Passell: That's right. It would throw off the measurement, the reliability on measuring a good loading. He worshipped – at the feet of the god named "loading".

Tom Grimshaw: Right.

Tom Passell: And I couldn't take that, but it takes what you get with these guys that they have their own eccentricities and beliefs. And, of course, I had that the loading experience, doing his work for me to measure hydrogen content in the water the power plants primary site in a pressurized water reactor.

Tom Grimshaw: Right.

Tom Passell: So, he was continuing on them, and probably he was going with something he could measure.

Tom Grimshaw: He was locked into that approach.

Tom Passell: Right, he was locked in and it was a very low resistance. And so, he had been measuring very low, milliohm resistances that were key to that. And if anything disturbed that, he was losing for that test.

Tom Grimshaw: Of course, and that's hence the rejection of further use of boric acid.

Tom Passell: Right.

Tom Grimshaw: Okay, got it.

Tom Passell: In the electrolyte.

Tom Grimshaw: So, if you had infinite time or say substantial time and money, what would your experiments look like?

Tom Passell: Okay, experiments. Again, going on what we do, and see if we can get reliable excess heat based on separated isotopes of the impurities, both in the electrolyte and in the original metal.

Tom Grimshaw: So, in other words, lithium-6 and boron-10 and also show that boron-11 and helium-7 DO NOT work!

Tom Passell:	Boron-10. That's what I would do assuming that we could repeat that very carefully. So, with that, I would say, "Well, that would begin to prove that we had a process.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	It does not make a feasible power plant." So, that I would like to admit that we haven't done that. In the process of doing all that, obviously, the DP reaction on boron-10 makes boron-11. So you should see a build-up of boron-11 in the boron fraction.
Tom Grimshaw:	Sure.
Tom Passell:	We didn't measure the boron-10 or 11 ratio by Secondary Ion Mass Spectrometry, SIMS.
Tom Grimshaw:	Okay.
Tom Passell:	Okay. And we didn't measure the ratio of lithium-6 and lithium-7 also in case you wanted to pursue the lithium-6 hypothesis.
Tom Grimshaw:	Solid isotopic shift.
Tom Passell:	Using separated isotopes.
Tom Grimshaw:	Mm-hmm (affirmative).
Tom Passell:	And with both the electrolyte and deuterium.
Tom Grimshaw:	Okay.
Tom Passell:	That means I have to build on that whole thing, just to prove that we don't have a power plant yet based on cold fusion, but prove that there is a reaction there of a nuclear nature, which by itself implies a remarkable achievement. If we could amplify it, it didn't have essentially burning just deuterium, but also some of these light isotopes of lithium-6 or boron-10.
Tom Grimshaw:	Mm-hmm (affirmative). Got it. If you were to do these experiments, and if you were to get consistent reproducibility, that in itself would be revolutionary.
Tom Passell:	That's right. I'm going to be happy to go to the crematorium having achieved that.
Tom Grimshaw:	Oh, of course, then you have your lab rat.
Tom Passell:	Got it.
Tom Grimshaw:	And then you can use that approach resulting in a device that others can use to confirm what you have found and to perhaps embellish in their own way.
Tom Passell:	That's right.
Tom Grimshaw:	And as far as getting to a nuclear power plant, it's all just engineering once they understand.
Tom Passell:	Right, you get to the engineering because you've proved that you can get a nuclear process.
Tom Grimshaw:	Yeah.

Tom Passell:	At the modest temperatures and conditions. I think, I haven't seen anything else that's as reliable as the electrochemical thing. But I thought that the future would be the best for a gaseous discharge system.
Tom Grimshaw:	That was my next question. So you're talking about a gas discharge as opposed to an electrolytic setup.
Tom Passell:	Right, and I tried to do a lot of different electrodes for gas discharges with varying pressures and heavy hydrogen only. And I was able to do that, and I didn't get any nuclear reactions. And I had set them up so that if I did get a nuclear reaction, we're going to be observing a radioactive isotope, well known gamma ray, that would prove that I had a nuclear reaction. But again, that'd be another power plant yet.
Tom Grimshaw:	Right.
Tom Passell:	But I would have proved that you can do that reaction, more likely at higher atomic numbers.
Tom Grimshaw:	But you weren't able to. Now did you do... When you're doing your gas discharge experiments with different materials, was boron-10 among the others?
Tom Passell:	Yeah, I had vanadium alloy with two boron-10s. I thought that would be interesting, and I have supplies of that. We have few tens of grams of vanadium alloy with two boron-10s.
Tom Grimshaw:	Okay.
Tom Passell:	I have that, and I have also boron-10 loaded palladium, I believe, among our samples. So these were, again, ways of getting boron involved, but not necessarily the palladium.
Tom Grimshaw:	Right.
Tom Passell:	And certainly, palladium wasn't necessary.
Tom Grimshaw:	Right. Palladium is just the host for this reaction.
Tom Passell:	That's right.
Tom Grimshaw:	Okay, not involved.
Tom Passell:	But I got into palladium because of the history of what we can do with some small amount of money and project likely result.
Tom Grimshaw:	But you were looking for a gamma ray from another product.
Tom Passell:	Right.
Tom Grimshaw:	Which would have been what?
Tom Passell:	Well, as if you had a neutron capture or something impressive.
Tom Grimshaw:	Okay.
Tom Passell:	In the case of vanadium, you get vanadium-51 from vanadium-50 (d,p) vanadium-51.

Tom Grimshaw: Mm-hmm (affirmative). Okay. One last question that's still a little unclear in my mind. If you have lithium-6, or if you have boron-10, so what you're doing is, according to Oppenheimer-Phillips, you're bombarding it or there is a neutron coming via the deuteron or joining with the deuteron. That's right. The neutron comes into the target via a passing deuteron.

Tom Passell: It starts there.

Tom Grimshaw: It starts there, okay.

Tom Passell: And then it's a very loosely held neutron. And so, if you can get a flux of the deuterium through some motion, you have a hope of there being a chance for the stripping reaction to occur more likely than if it's a static system. And that's another thing that has been observed. It's usually a dynamic experiment. It's always more successful than ones with no relative motion between the deuteron and target. So that I assume the message from that to me was you have to have motion of the heavy hydrogen through whatever system you are dealing with.

Tom Grimshaw: Okay. Okay. Now, the neutron is coming in via the deuteron. What's the source? Are you doing neutron bombardment?

Tom Passell: No. The purpose of the deuteron is that it had a neutron hanging on very loosely.

Tom Grimshaw: Okay.

Tom Passell: And so, if you move it through any metal and hydrogen readily moves through quite a few metals, not just palladium. Palladium's big on that, but it goes through nickel and all kinds of other species of metals, so that you can get a flux of heavy hydrogen. And essentially, it's the source of a neutron. It's barely hanging on to this original deuteron.

Tom Grimshaw: Okay, that's the stripping end.

Tom Passell: That's the stripping and it has that neutron.

Tom Grimshaw: Okay, thank you.

Tom Passell: That it gets rid of.

Tom Grimshaw: Okay. So the neutron strips off and the proton absorbs most of the energy of the total reaction, the largest part of which is due to neutron capture in the target nucleus. And then, it goes just a short distance and then imparts its energy to the crystal lattice.

Tom Passell: Right.

Tom Grimshaw: Okay, very interesting. Okay.

Tom Passell: A deuteron beam or a flux through a metal that's hanging on to a neutron. And if the neutron escapes it is captured, because it's neutral, no coulomb barrier. So if it does get in and is captured, then that amount of energy, and neutron capturing almost everything has a positive Q reaction. But the energy and the immediate reaction is for almost all the energy of the neutron capture into the target. The proton comes off and has a very short path to come to a stop – that is about 100 microns in most metals.

Tom Grimshaw: Okay, on into the lattice.

Tom Passell: Into the lattice.

Tom Grimshaw: And increased vibration and heat.

Tom Passell: That's the idea.

Tom Grimshaw: Mm-hmm (affirmative). Okay, thank you for taking me off of that.

Tom Passell: I'm always suspicious that the result of anybody claiming anything with light hydrogen because the barrier is much bigger.

Tom Grimshaw: Mm-hmm (affirmative).

Tom Passell: And indeed, I still look with a prejudiced eye against anybody's results with light hydrogen and that's what bothered me a little bit in considering the use of light water in the electrolysis of nickel.

Tom Grimshaw: Rossi?

Tom Passell: He was down in Santa Fe, New Mexico.

Tom Grimshaw: Oh, Claytor or Storms.

Tom Passell: Storms, yeah.

Tom Grimshaw: Oh, he was saying.

Tom Passell: Storms, I think, told me that he was getting some reaction that light hydrogen and nickel.

Tom Grimshaw: Yeah. Yeah. Well, of course, that's the whole Rossi fiasco. Yeah, nickel-hydrogen system that is proven so far not to be true.

Tom Passell: That's just one of those things. And the trouble with Rossi was doing good calorimetry and high temperature that, to me, was almost impossible to do accurately.

Tom Grimshaw: Okay.

Tom Passell: That's pretty well it. That's what I would do if I had a lot of money and a lot of time.

Tom Grimshaw: Yeah. Well, I mean, if somebody came along with a lot of money and two or three bright assistants, that you can impart your knowledge to, and do the experimental plan, it would be a fun thing.

Tom Passell: Right. Well, I would like to go to my end of having shown that we have reason to be believing that we have low energy nuclear reactions with heavy hydrogen, and there was enough heavy hydrogen. One of every 6,000 hydrogen atoms on the surface of the earth is heavy hydrogen.

Tom Grimshaw: Mm-hmm (affirmative), has that extra brother neutron.

Tom Passell: But there's another real side effect going on. If you look at the sun, you find that instead of 6,000:1 in favor of light hydrogen, it's 25,000:1. Again, in other words, the heavy hydrogen has a hard time in any situation (very high temperatures) of being preferentially used up compared to what otherwise happens to light hydrogen.

Tom Grimshaw: Let me be sure I understood what you just said. Because of the light hydrogen ratio is higher, that means that the heavy hydrogen is being consumed preferentially to light hydrogen.

Tom Passell: That's right, being extra consumed by just being more fragile.

Tom Grimshaw: Yeah, more subject to the reaction since the neutron is so connected to the proton.

Tom Passell: More reactive.

Tom Grimshaw: Because of the presence of neutron.

Tom Passell: Because of the DP reactions.

Tom Grimshaw: Yeah.

Tom Passell: Which is that happened at a lower temperature than any of the other reactions.

Tom Grimshaw: Interesting, but that's just the hydrogen reaction. It doesn't involve lithium or boron.

Tom Passell: That's right, but it does say that... Then you look at the abundance of all the elements on the face of the earth. And what you see is hydrogen, of course, is most likely evolved but light hydrogen. Then you go to lithium, beryllium, boron, very low abundance relative all the other elements.

Tom Grimshaw: Right, it is like a cliff.

Tom Passell: Those are fragile and subject to lower temperatures. They are chewed up, and then it comes back at the carbon and going up in atomic number.

Tom Grimshaw: Okay.

Tom Passell: Take going up to iron. So this fits the fragility of the heavy hydrogen, and its tendency, even in the hot fusion business to being used up preferentially out of all the other nuclei, iron being most stable of the other nuclei in the system.

Tom Grimshaw: Which would make sense for that extra neutron there makes it more fragile.

Tom Passell: That's right. That extra neutron is really, so it's being carried around but it is as loose as a goose.

Tom Grimshaw: Yeah, 2.2 MeV, you said.

Tom Passell: If somebody said, "Well, the neutron and the proton of a deuteron spend half of their lifetime separated by a greater distance than expected from the extent of the strong force around each…"

Tom Grimshaw: The binding on energy.

Tom Passell: The amount of energy involved in it.

Tom Grimshaw: Yeah, interesting.

Tom Passell: In other words, outside the limit of the strong force.

Tom Grimshaw: Yeah.

Tom Passell: Otherwise, it's always vibrating. Essentially, I'd say spending half of its time outside the limits of its being bound.

Tom Grimshaw: Mm-hmm (affirmative). That's very interesting.

Tom Passell: So, it's like a neutron hanging out that piece of baggage. Anytime you move the deuteron around, you might be able to strip it.

Tom Grimshaw: Strip it.

Tom Passell: Strip it with that neutron because the neutron gets across all barriers, all the coulomb barriers.

Tom Grimshaw: Sure. As a neutral particle, it goes right into the nucleus of another element.

Tom Passell: We know exactly that almost all isotopes in the entire system. When you add a neutron capture gamma reaction, in other words, neutron capture is a positive Q reaction.

Tom Grimshaw: Yeah, yeah. Very good. Well, that's been very helpful. Thank you.

Tom Passell: Okay. Well, I hope that explains my thinking. Up to now, I've been suspicious of anything happening with light hydrogen.

Tom Grimshaw: Yeah. Well, and even with any D-D reaction because of the coulomb barrier.

Tom Passell: Mm-hmm (affirmative).

Tom Grimshaw: Well, this is a way around the coulomb barrier.

Tom Passell: That's right. That's right. I'm judging it by having that neutron around.

Tom Grimshaw: Yeah. Cool. Okay. Well, we'll wrap this up.

Tom Passell: Okay.

Tom Grimshaw: Thank you, Tom.

Tom Passell: You're welcome.

Tom Grimshaw: This is Tom Grimshaw with Tom Passell talking about his ideas of what the causes of cold fusion. And so we'll call that a wrap. And I'll say that this is August 16. And we're at his lab here in Mountain View. And thank you again, Tom.

Tom Passell: You're welcome. My pleasure.