

# **DOCUMENTATION OF LENR INVESTIGATIONS BY MELVIN MILES AT THE U.S. NAVAL AIR WEAPONS CENTER – CHINA LAKE AND BEYOND**

*A PROJECT OF THE LENR RESEARCH  
DOCUMENTATION INITIATIVE*

**THIRD DRAFT REPORT, VOLUME 2, APPENDICES  
(REVISION 1)**

Melvin Miles, Ph.D., Retired  
St. George, Utah

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



August 11, 2020

## Contents

---

Introduction .....	3
Appendix A. Cold Fusion Papers by Dr. Miles .....	5
A1. List from Dr. Miles Professional Biography .....	5
A2. Set 1 from David Nagel (1990-2014).....	26
A3. Set 2 from David Nagel.....	31
A4. Publications on Jed Rothwell’s LENR-CANR.org.....	42
Appendix B. Photo Identifications .....	47
Appendix C. Transcriptions of Dr. Miles Interviews .....	52
C1. Round 1 Interviews, May 5-6, 2019 .....	52
Interview 1-1.....	52
Interview 1-2.....	53
Interview 1-3.....	60
Interview 1-4.....	68
Interview 5-1.....	74
Interview 1-6.....	75
Interview 1-7.....	83
C2. Round 2 Interviews, July 18-19, 2019.....	100
Interview 2-1.....	100
Interview 2-2.....	101
Interview 2-3.....	106
Interview 2-4.....	110
Interview 2-5.....	122
Interview 2-6.....	129
Interview 2-7.....	140
Interview 2-8.....	151
Interview 2-9.....	163

## ***Introduction***

---

An initiative is underway to mitigate the risk of loss of research records of long-standing LENR investigators. The objectives of the LENR Research Documentation Initiative” (LRDI) are to collect, organize, document, and archive these records. It is being performed by the firm LENRGY, LLC<sup>1</sup> in Austin, Texas. It is set up to assist researchers in making sure that their efforts are preserved and to keep the records available for additional analysis and interpretation.

An LRDI project is underway to document the LENR research of Dr. Melvin Miles, which took place at the U. S. Naval Air Weapons Center at China Lake, CA (NAWC – China Lake<sup>2</sup>) and subsequent locations. The Miles LENR Research Documentation Project (MLRDP) was established to collect and record as much of Dr. Miles’ research record as possible. The methods used in the Project are based on general LRDI procedures that are modified to meet the specific requirements of individual LENR investigators<sup>3</sup>. The overall LRDI procedure is set forth in a recent article in Infinite Energy<sup>4</sup>.

Four visits have been made to Dr. Miles at his home in St. George, Utah for interviews and information collection: 1) 5/5/19 to 5/6/19; 2) 5/27/19 to 5/29/19; 7/18/19 to 7/19/19; and 10/31/2019. During the visits, Dr. Miles’ professional resume was obtained, two sets of the interviews were conducted, electronic files were obtained, hardcopy records were inventoried, and the contents of his LENR library were recorded. The procedure included taking photos of Dr. Miles and his LENR records, recording the interviews and obtaining transcripts from an online service, conducting inventories of electronic, hardcopy and other files, and scanning selected materials.

---

<sup>1</sup> <http://www.lenrgyllc.com/>.

<sup>2</sup> The Naval Air Weapons Station (NAWS) has had several names over the course of its history – Naval Ordnance Test Station (NOTS, 1943), Naval Weapons Center (NWC, 1967) and Naval Air Weapons Station (NAWS, 1992). Dr. Miles worked in the Weapons Division of the Naval Air Warfare Center, so the acronym NAWC is used in this report.

<sup>3</sup> Grimshaw, T.W., 2019. Collection, Organization, and Documentation of LENR Research Results: Guideline. January.

<sup>4</sup> Grimshaw, T., 2020. Documenting Cold Fusion Research: Preserving a Vital Asset for Humankind. Infinite Energy, Issue 150, March/April 2020, p. 9-13.

Volume 1 of the MLRDP report describes the results of the information collection effort, including the important files of correspondence with Dr. Martin Fleischman. Other records collected are as follows:

- Publicly-available papers, presentations, and related documents
- Electronic files
- Hardcopy records, including laboratory and other notebooks
- LENR books and conference proceedings
- Video recordings of LENR events

Future potential opportunities for the MLRDP are also set forth. More information on the Project methods is provided in the final section of the report.

A preliminary draft report was provided to Dr. Miles on July 15, 2019, and a second draft was submitted on March 26, 2020. Additional records and memos have been added for the third draft report. This Volume 2 of the third draft of the MLRDP report presents the appendices – LENR papers, photo identifications and interview transcripts.

Dr. Miles' spouse, Linda, is acknowledged for her vital role in collecting information for the MLRDP.

## Appendix A. Cold Fusion Papers by Dr. Miles

---

### A1. List from Dr. Miles Professional Biography<sup>5</sup>

97. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE COLD FUSION EFFECT

M. H. Miles, K. H. Park, And D.E. Stilwell

In "Proceedings Of The First Annual Conference On Cold Fusion," Salt Lake City, Utah, 28-31 March 1991, Pp. 328-334

100. THEORETICAL NEUTRON FLUX LEVELS, DOSE RATES, AND METAL FOIL ACTIVATION IN ELECTROCHEMICAL COLD FUSION EXPERIMENTS

M. H. Miles and R. E. Miles

J. Electroanal. Chem., Vol. 295 (1990), pp. 409-414

102. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE ELECTROLYSIS OF WATER AND HEAVY WATER (D20 )

M. H. Miles, K. H. Park, and M. H. Miles

J. Fusion Energy, Vol. 9 (1990), pp. 333-336

103. ELECTROCHEMICAL CALORIMETRIC EVIDENCE FOR COLD FUSION IN THE PALLADIUM-DEUTERIUM SYSTEM

M. H. Miles, K. H. Park, and D. E. Stilwell

J. Electroanal. Chem., Vol. 296 (1990), pp. 409-414

104. HELIUM PRODUCTION DURING THE ELECTROLYSIS OF D20 IN COLD FUSION EXPERIMENTS

B. F. Bush, J. J. Lagowski, M. H. Miles, and G. S. Ostrom

J. Electroanal. Chem., Vol. 304 (1991), pp. 271-278

105. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, G. S. Ostrom, B. F. Bush, and J. J. Lagowski

In "II Annual Conference on Cold Fusion-Contributed Papers," T. Bressani and F. Iazzi, Editors, 29 June- 4 July 1991, p. 17

107. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS B. F. Bush, M. H. Miles, G. S. Ostrom, and J. J. Lagowski

in "Electrochemical Society Extended Abstracts," Vol. 91-2, 119, 1991

108. COLD FUSION EXPERIMENTER MILES RESPONDS TO CRITIC M.H. Miles

in "21st Century Science and Technology," Vol. 5, No. 1, pp. 75-80, 1992

110. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski

in Proceedings of the 2nd Annual Conference on Cold Fusion, Como, Italy, Italian Physical Society, Bologna, Italy, pp. 363-372, 1991

---

<sup>5</sup> Professional Biography as of 2005. Memo to Melvin Miles from Tom Grimshaw. July 14, 2019.

111. LITHIUM DEPOSITION/DISSOLUTION STUDIES FROM MOLTEN NITRATE SALTS

G. S. Ostrom, J. H. France, and M. H. Miles

Electrochemical Society Extended Abstracts, Vol. 91-2, pp. 132-133 (1991)

112. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS

B. F. Bush, M. H. Miles, G. S. Ostrom, and J. J. Lagowski Electrochemical Society Extended Abstracts, Vol. 91-2, p. 179 (1991)

113. EXPERIMENTAL EVIDENCE FOR CORRELATED HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS

M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski

in Hydrogen Storage Materials, Batteries, and Electrochemistry, D. A. Corrigan and S. Srinivasin, editor, The Electrochemical Society, pp. 287-297 (1991)

114. THERMODYNAMIC AND CALORIMETRIC PRINCIPLES IN COLD FUSION EXPERIMENTS

M. H. Miles and B. F. Bush

in 12th IUPAC Conference on Chemical Thermodynamics, Joint Meeting with the 47th Calorimetry Conference, 16-21 August 1992, Snowbird, Utah, Program, Abstracts, and Reports, p. 194

116. SEARCH FOR ANOMALOUS EFFECTS INVOLVING EXCESS POWER, HELIUM, AND TRITIUM DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES

M. H. Miles and B. F. Bush

in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 189-199 (1993)

117. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS M. H. Miles and B. F. Bush

in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 113-122 (1993)

118. CORRELATION OF EXCESS ENTHALPY AND HELIUM PRODUCTION DURING D<sub>2</sub>O AND H<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES

in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 189-199 (1993)

119. CORRELATION OF EXCESS ENTHALPY AND HELIUM PRODUCTION DURING D<sub>2</sub>O and H<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES

M. H. Miles, R. A. Hollins, B. F. Bush, J. J. Lagowski, and R. E. Miles

J. Electroanal. Chem., Vol. 346, pp. 99-117 (1993)

120. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS

M. H. Miles and B. F. Bush

Proceedings of the Third International Conference on Cold Fusion, Nagoya, Japan, pp. 113-122 (1993)

121. CALORIMETRIC PRINCIPLES AND PROBLEMS IN MEASUREMENTS OF EXCESS POWER DURING Pd-D<sub>2</sub>O ELECTROLYSIS

M. H. Miles, B. F. Bush, and D. E. Stilwell

Journal of Physical Chemistry, Vol. 98, pp. 1948-1952 (1994)

122. ANOMALOUS EFFECTS INVOLVING EXCESS POWER, RADIATION, AND HELIUM PRODUCTION DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES

M. H. Miles, B. F. Bush, and J. J. Lagowski

Fusion Technology, Vol. 25, pp. 478-486 (1994)

123. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM

M. H. Miles and B. F. Bush

In Proceedings of the Fourth International Conference on Cold Fusion (ICCF-4) Lahaine, Hawaii, U.S.A., December 6-9, 1993.

124. PRACTICAL ASPECTS OF HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM

B. F. Bush and M. H. Miles

in "Cold Fusion source Book," Hal Fox, Editor, Fusion Information Center, Salt Lake City, UT (1994)

125. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM SYSTEMS

M. H. Miles and B. F. Bush

in Proceedings of International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Belarus, 1994, pp. 349-356

126. NUCLEAR PRODUCTS COMMENSURATE WITH ENERGY GENERATED DURING D<sub>2</sub>O ELECTROLYSIS AT PALLADIUM CATHODES: QUANTITATIVE ANALYSIS

B. F. Bush, J. J. Lagowski, and M. H. Miles

in ACS Book of Abstracts, Division of Nuclear Chemistry and Technology, Abstract No. 45, April 2-6, 1995

127. CORRELATIONS OF EXCESS POWER AND HELIUM PRODUCTION M. H. Miles and K. B. Johnson

in ACS Book of Abstracts, Division of Nuclear Chemistry and Technology, Abstract No. 59, April 2-6, 1995

128. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM M.-H. Miles and B. F. Bush

Transactions of Fusion Technology, Vol. 26, pp. 156-159, December 1994

129. THE EXTRACTION OF INFORMATION FROM AN INTEGRATING OPEN CALORIMETER IN FLEISCHMANN-PONS EFFECT EXPERIMENTS

M. H. Miles

Proceedings of the Fifth International Conference on Cold Fusion, Monte Carlo, Monaco, April 9-13, 1995, pp. 97-104

130. ANOMALOUS EFFECTS IN DEUTERATED SYSTEMS Melvin H. Miles, Benjamin F. Bush, and Kendall B. Johnson NAWCWPNS TP 8302, Sept 1996

131. ELECTROCHEMICAL INSERTION OF HYDROGEN INTO METALS AND ALLOYS

Melvin H. Miles and Kendall B. Johnson

Journal of New Energy, Vol. 1, No. 2, pp. 32-36, 1996

132. HEAT AND HELIUM MEASUREMENTS USING PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER

M. H. Miles, K. B. Johnson, and M. A. Imam

Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 20-28

133. ELECTROCHEMICAL LOADING OF HYDROGEN AND DEUTERIUM INTO PALLADIUM AND PALLADIUM-BORON ALLOYS

M. H. Miles and K. B. Johnson

Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 208-212

134. IMPROVED, OPEN-CELL, HEAT CONDUCTION, ISOPERIBOLIC CALORIMETRY M. H. Miles and K. B. Johnson

Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 496-501

135. REPLY TO S. E. JONES AND L. D. HANSON CONCERNING CLAIMS OF MILES, ET AL. IN PONS-FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS

M. H. Miles

Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 524-527

136. NUCLEAR PRODUCTS ASSOCIATED WITH THE PONS AND FLEISCHMANN EFFECT: HELIUM COMMENSURATE TO HEAT GENERATION, CALORIMETRY, AND RADIATION

B. F. Bush, J. J. Lagowski, and M. H. Miles

Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 622- 626

137. CORROSION INHIBITION OF ALUMINUM ALLOYS COATED WITH POLY(2,5- bis(N-METHYL-N-ALKYLAMINO) PHENYLENE VINYLENES

P. Zarras, J. D. Stenger-smith, and M. H. Miles in PMSE-ACS, Vol. 76, 1997, pp. 589-590

138. REPLY TO "EXAMINATION OF CLAIMS OF MILES ET AL. IN PONS- FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS"

M. H. Miles

J. Phys. Chem. B, Vol. 102, 1998, pp. 3642-3646

139. OPTICAL AND ELECTRONIC MATERIALS FOR NAVAL AVIATION

J. D. Stenger-Smith, P. Zarras, M. H. Miles, R. A. Hollins, A. P. Chafin, And G. A. Lindsay

Society for the Advancement of Materials and Process Engineering, May 1997

140. ANOMALOUS HEAT AND HELIUM PRODUCTION USING PALLADIUM-BORON ALLOYS IN HEAVY WATER

M. H. Miles, K. B. Johnson, and M. A. Imam

Proceedings of 32nd Intersociety Energy Conversion Engineering Conference, 1997, pp. 1334-1337

141. THE EFFECT OF PASSIVATING FILMS INVOLVING THE LITHIUM ANODE IN THIONYL CHLORIDE, BROMINE TRIFLUORIDE, MOLTEN NITRATES, AND MOLTEN PERCHLORATES

M. H. Miles

Proceedings of 32nd Intersociety Energy Conversion Engineering Conference, 1997, pp. 1334-1337

142. ELECTROCHEMICAL CALORIMETRIC STUDIES OF PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER

M. H. Miles

NEDO Final Report, Tokyo, Japan, March 31, 1998

143. CORROSION INHIBITION OF ALUMINUM ALLOYS COATED WITH POLY(2,5- BIS(N-METHYL-N-ALKYLAMINO)PHENYLENE VINYLENE)S

P. Zarras, J. D. Stenger-Smith, And M. H. Miles ACS PMSE Preprints 76, 789, 1997

144. INTRINSICALLY ELECTRICALLY CONDUCTING POLYMERS AS CORROSION INHIBITING COATINGS

J. D. Stenger-Smith, P. Zarras, M. Miles, and J. Nelson NAWCWPNS TP 8364, April 1998

145. REVIEW OF POLY(BIS-ALKYLAMINO-P-PHENYLENE VINYLENE)S AS CORROSION INHIBITING MATERIALS

J. D. Stenger-Smith, P. Zarras, G. Ostrom, M. Miles

Submitted as an American Chemical Society Symposium Series Chapter, April 1998

146. EXCESS HEAT MEASUREMENTS AND NUCLEAR DETECTION EXPERIMENTS IN THE NHE PROGRAM

K. Matsui, N. Asami, M. Sumi, H. Kamimura, A. Kubota, R. Shimada, S. Miyashita,

A. Lipson, P. Tripodi, M. Miles and H. Watanabe

The Seventh International Conference on Cold Fusion, Program Manual and Abstracts, 1998,p.96

147. RADIATION MEASUREMENTS AT CHINA LAKE: REAL OR ARTIFACTS? M. H. Miles and B. F. Bush  
 Proceedings of the Seventh International Conference on Cold Fusion, Vancouver, British Columbia, Canada, April 19-24, 1998, pp. 236-240
148. NEW MAIN-CHAIN POLY(CROWN ETHER) MEMBRANES FOR LITHIUM-ION TRANSPORT  
 P. Zarras, S. Fallis, M. H. Miles, R. Y. Yee, L. H. Merwin and G. A. Lindsay  
 Polymeric Materials Science and Engineering, Vol. 80, Spring Meeting, Anaheim, Calif., March 21-25, 1999, pp. 612-613
149. CALORIMETRY OF THE Pd+D CO-DEPOSITION S. Szpak, P. A. Mosier-Boss and M. H. Miles  
 Fusion Technology, Vol. 36, 1999, pp. 234-241
150. LITHIUM BATTERIES USING MOLTEN NITRATE ELECTROLYTES M,H. Miles  
 Proceedings of the Fourteenth Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15, 1999, pp. 39-42
151. NEW MAIN-CHAIN (CROWN ETHER) MEMBRANE FOR LITHIUM-ION BATTERIES  
 G. A. Lindsay, M. H. Miles, P. Zarras, S. Fallis, R. Y. Yee, A. P. Chafin, and L.H.Merwin  
 Proceedings of the Fourteenth Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15, 1999
152. A REVIEW OF MAIN CHAIN POLY (CROWN ETHER) MEMBRANES FOR LITHIUM-ION TRANSPORT  
 G. A. Lindsay, P. Zarras, S. Fallis, M. H. Miles, L. H. Merwin, and R. Y. Yee  
 Polymer News, 1999
153. IMPROVED THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES  
 M.H. Miles  
 35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 259.
154. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD/Pt ELECTROLYSIS CELLS M.H.Miles  
 35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 255
155. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD ELECTROLYSIS CELLS M.H. Miles  
 J. Electroanal. Chem., Vol. 482, pp. 56-65, 2000
156. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN  
 M.H. Miles  
 Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, Conference Proceedings, Italian Physical Society, October 22-24, 1999
157. CALORIMETRIC STUDIES AT THE NEW HYDROGEN ENERGY LABORATORY IN JAPAN  
 M H. Miles  
 Bulletin of The American Physical Society, Series II, Vol. 45, No. 1, March 2000, Abstract C32
158. "MY IMPRESSIONS AS A NEDO GUEST RESEARCHER AT THE NHE LABORATORY IN JAPAN"  
 M.H. Miles  
 Infinite Energy, Vol. 5, Issue 30, pp. 18-21, 2000
159. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN  
 M.H. Miles  
 Infinite Energy, Vol. 5, Issue 30, pp. 22-25, 2000

160. CALORIMETRIC STUDIES OF PALLADIUM ALLOY CATHODES USING FLEISCHMANN-PONS DEWAR TYPE CELLS

M.H. Miles

Proceedings of the 8th International Conference on Cold Fusion, Lerici (La Spezia), Italy, 21-26 May 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings, Vol. 70, pp. 97-104

161. CASE STUDIES OF TWO EXPERIMENTS CARRIED OUT WITH THE ICARUS SYSTEMS

M.H. Miles, M.A. Imam, and M. Fleischmann

Proceedings of the 8th International Conference on Cold Fusion, Lerici (La Spezia), Italy, 21-26 May, 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings, Vol. 70, pp. 105-119.

162. LITHIUM THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES M.H. Miles

Proceedings of the 39th Power Sources Conference, June 12-15, 2000 pp. 560-563

163. ANOMALOUS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM- BORON SYSTEM

M.H. Miles, M.A. Imam, and M. Fleischmann

Proceedings of 8th Russian Conference on Cold Nuclear Transmutation (RCCNT-8), Sochi, Russia, October 4-11, 2000, pp. 142-162.

164. EXCESS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM

M.H. Miles, M.A. Imam, and M. Fleischmann

Transactions of the American Nuclear Society and the European Nuclear Society, Washington, D.C., 12-16 November, 2000, C.A. Yoelin, Editor, Vol. 83, pp. 371-372.

165. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE

M.H. Miles, M. Fleischmann, and M.A. Imam

Naval Research Laboratory Report NRL/MR/6320-01-8526, March 26, 2001, 155 pp.

166. CALORIMETRY OF Pd-D CODEPOSITION IN A FLEISCHMANN-PONS DEWAR CELL

M.H. Miles, S. Szpak, P. Mosier-Boss, and M. Fleischmann

Bulletin of the American Physical Society, Seattle, Washington, 12-16 March, 2001, Abstract 5403.

167. EXAMINATION OF LINEAR POTENTIAL SWEEP METHODS FOR DETERMINING THE CAPACITANCE OF HYDROUS RUTHENIUM OXIDE MATERIALS

M.H. Miles, T.J. Groshens, and C.E. Johnson

2001 Joint International Meeting of the Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 89.

168. ELECTROCHEMISTRY OF MOLTEN NITRATE ELECTROLYTES AND APPLICATIONS FOR HIGH VOLTAGE LITHIUM CELLS

M.H. Miles

2001 Joint International Meeting of The Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 125.

169. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE

M.H. Miles, M.A. Imam, and M. Fleischmann

2001 Joint International Meeting of the Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 811.

170. RECENT ADVANCES IN LITHIUM BATTERY TECHNOLOGY M.H. Miles

23rd Annual GaAs IC Symposium, Baltimore, MD, 21-24 October 2001, Technical Digest 2001, IEEE 01CH37191, pp. 219-222.

171. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE

M.H. Miles, M.A. Imam, and M. Fleischmann

In "Energy and Electrochemical Processes For a Cleaner Environment", C. Comninellis, M. Doyle and J. Winnick, Editors, The Electrochemical Society, Proceedings Vol. 2001- 23, 2001, pp. 194-205.

172. EXAMINATION OF LINEAR POTENTIAL SWEEP METHODS FOR DETERMINING THE CAPACITANCE OF HYDROUS RUTHERNIUM OXIDE MATERIALS

M.H. Miles, T.J. Groshens, and C.E. Johnson

In "Advanced Batteries and Supercapacitors", The Electrochemical Society, Proceedings Vol. 2001-21, 2001.

173. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM. VOL. I: A DECADE OF RESEARCH AT NAVY LABORATORIES

M.H. Miles, "Chapter 3: Excess Heat and Helium Production in Palladium and Palladium Alloys" in SPAWAR System Center San Diego, Technical Report 1862, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 19-30.

174. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM, VOL. II: SIMULATION OF THE ELECTROCHEMICAL CELL (ICARUS) CALORIMETRY M.H. Miles, "Chapter 4: Analysis of Experiment MC-21: A Case Study" in SPAWAR System Center San Diego, Technical Report 1862, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 31-89.

175. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES

M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga

American Physical Society 2002 Meeting, Indianapolis, Indiana, March 18-22, 2002.

176. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES

M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga

Ninth International Conference on Cold Fusion, Abstracts, Beijing, China, May 19-24, 2002, pp. 77-78.

177. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO- DEPOSITION

S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann

Ninth International Conference on Cold Fusion, Abstracts, Beijing, China, May 19-24, 2002, pp. 77-78.

178. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING Pd-B ALLOY CATHODE

M.H. Miles, M.A. Imam, and M. Fleischmann

In "Batteries and Supercapacitors", G. Abba-Nazi, E. Takeuchi, R. Koetz, and B. Scrosati, Editors, The Electrochemical Society, Proceedings Volume 2001-21, 2002, pp. 795-806.

179. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

Bulletin of the American Physical Society, Vol. 48, No. 1, Part II, Abstract Z334, p. 1382 (2003).

180. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES

M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga

In Condensed Matter Nuclear Science, Proceedings of the 9th International Conference on Cold Fusion. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 246-249.

181. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO- DEPOSITION

M.H. Miles, S. Szpak, P.A. Mosier-Boss, and M. Fleischmann

In Condensed Matter Nuclear Science, Proceedings of the 9th International Conference on Cold Fusion. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 250-254.

182. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No.M012.

183. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW

M.H. Miles

ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No. M015(2).

184. THE "INSTRUMENT FUNCTION" OF ISOPERIBOLIC CALOMETERS; EXCESS ENTHALPY GENERATION DUE TO THE PARASITE REDUCTION OF OXYGEN" M. Fleischmann and M.H. Miles

ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract TH 12

185. A CALORIMETRIC INVESTIGATION OF THE Pd/B SYSTEM M.H. Miles, M.A. Imam, and M. Fleischmann

ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No. M01S.

186. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CODEPOSITION

S.Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann Thermochemica Acta, 410, 101 (2004).

187. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW

M.H. Miles

Proceedings of the 10th International Conference on Cold Fusion (ICCF-10), Cambridge, MA USA, August 24-29, 2003, Abstract.

188. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

Proceedings of the 10th International Conference on Cold Fusion (ICCF-10), Cambridge, MA USA, August 24-29, 2003, Abstract.

189. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D2O AND Pd/D2O SYSTEMS. PART I: THE Pt/D2O BLANK SYSTEM

M. Fleischmann and M.H. Miles

Thermochemica Acta (in preparation).

190. THE Pt/D2O AND Pd/D2O SYSTEMS. PART II: THE Pd-BAND Pd-B-Ce SYSTEMS M. Fleischmann, M.H. Miles and M.A. Imam

SPARWAR System center San Diego, Technical Report (in preparation).

191. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D2O AND Pd/D2O SYSTEMS. PART III: THE Pd-D CODEPOSITION SYSTEM

M. Fleischmann, M.H. Miles, S. Szpak and P.A. Mosier-Boss SPARWAR System center San Diego, Technical Report (in preparation).

192. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART IV: AN EXPERIMENT WITH A Pd- CATHODE IN 0.1 M LiOD/D<sub>2</sub>O CARRIED OUT IN 1989.  
M. Fleischmann, M.H. Miles  
SPARWAR System center San Diego, Technical Report (in preparation).

193. EXPLORATION OF MOLTEN HYDROXIDE ELECTROCHEMISTRY FOR THERMAL BATTERY APPLICATIONS  
M.H. Miles,  
J. Applied Electrochem., Vol. 33, pp. 1011-1016, 2003.

194. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER  
M.H. Miles  
Bulletin of the American Physical Society, Austin, Texas, March 2003, Abstract.

195. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW"  
M.H.Miles  
Bulletin of American Physical Society, Montreal, Canada, March 2004, Abstract

196. SIMULTANEOUS EXCESS POWER AND ANOMALOUS RADIATION M.H.Miles  
Bulletin of the American Physical Society, Los Angeles, CA, March 2005, Abstract

197. ISOPERIBOLIC CALORIMETRY 'APPLIED TO THE Pt/ D<sub>2</sub>O BLANK SYSTEM M. Fleischmann and M.H. Miles  
Bulletin of the American Physical Society, Baltimore, MD, March, 2006, Abstract

198. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER  
M.H. Miles  
in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 23-28, 2006

199. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: AREVIEW  
M.H. Miles  
in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 123-131, 2006

200. THE INSTRUMENT FUNCTION OF ISOPERIBOLIC CALORIMETERS: EXCESS ENERGY GENERATION DUE TO THE PARASITIC REDUCTION OF OXYGEN in  
M. Fleischmann and M.H. Miles  
in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 247-268, 2006

#### PRESENTATIONS

1. FAST REACTIONS INVOLVING HYDROGEN BONDING IN 2,2-DISUBSTITUTED MALONIC ACIDS  
M. H.Miles  
Presented at American Chemical Society Meeting, Chicago, Illinois, September 1964

2. ELECTROCHEMICAL OXIDATION OF FUELS IN LIQUID AMMONIA  
M. H. Miles  
Presented at Electrochemical Society Meeting, Boston, Massachusetts, May 1968

3. ELECTRODE OXIDATION OF HYDRAZINE AND RELATED COMPOUNDS IN LIQUID AMMONIA ELECTROLYTES

M.H. Miles

Presented at Electrochemical Society Meeting, Detroit, Michigan, October 1969

4. THE SULFURE CATHODE IN LIQUID AMMONIA M. H.Miles

Presented at Electrochemical Society Meeting, Atlantic City, New Jersey, October 1970

5. PROPERTIES OF LiCl and (CH<sub>3</sub>)<sub>4</sub>NCl SOLUTIONS IN WATER-METHANOL MIXTURES

M. H. Miles

Presented at Electrochemical Society Meeting, Miami Beach, Florida, October 1972

6. EFFECT OF TEMPERATURE ON ELECTRODE KINETIC PARAMETERS FOR HYDROGEN AND OXYGEN EVOLUTION REACTIONS ON NICKEL ELECTRODE IN ALKALINE SOLUTIONS

M. H.Miles

Presented at Electrochemical Society Meeting, Toronto, Canada, May 1975

7. PERIODIC VARIATION OF OVERVOLTAGES FOR WATER ELECTROLYSIS IN ACID SOLUTIONS FROM CYCLIC VOLTAMMETRIC STUDIES

M. H. Miles

Presented at Electrochemical Society Meeting, Washington, DC, May 1976

8. THE OXYGEN EVOLUTION REACTION ON NOBLE METAL ALLOYS IN ACID SOLUTIONS AT 80°C

M.H. Miles

Presented at Electrochemical Society Meeting, Las Vegas, Nevada, October 1976

9. ELECTROLYTE EFFECTS ON THE OXYGEN REDUCTION REACTION M. H. Miles

Presented at Brookhaven National Laboratory, Upton, New York, 18 February 1977

10. THE CALCIUM ANODE IN MOLTEN NITRATE ELECTROLYTES M. H. Miles

Presented at Electrochemical Society Meeting, Seattle, Washington, May 1978

11. THE ELECTROCHEMICAL REDUCTION OF MOLTEN PERCHLORATE AND CHLORATE SALTS

M.H. Miles

Presented at Electrochemical Society Meeting, St. Louis, Missouri, May 1980

12. PROTECTIVE FILM EFFECTS ON THE CALCIUM ANODE IN OXIDIZING MOLTEN SALTS

M. H. Miles

Presented at Electrochemical Society Meeting St. Louis, Missouri, May 1980

13. ELECTROCHEMICAL KINETICS M. H. Miles

Chemistry Division Seminar, 1 February 1980

14. THE SILVER ION CATHODE IN MOLTEN NITRATES AND PERCHLORATES M. H. Miles, G. E. McManis, and A N. Fletcher

Presented at Electrochemical Society Meeting, Denver, Colorado, October 1981

15. CATHODIC REACTIONS IN MOLTEN NITRATES M. H.Miles

Chemistry Division Seminar, 4 August 1982

16. EFFECT OF ELECTROLYTE COMPOSITION ON CATHODE REACTIONS IN MOLTEN NITRATES

M. H. Miles, G. E. McManis, and A N. Fletcher

Presented at the 1982 Fall Meeting of the Electrochemical Society, Inc., Detroit, Michigan, 17-22 October 1982

17. ELECTRODE REDUCTION OF DINITROTOLUENE ISOMERS M. H. Miles and D. A. Fine  
Presented at Electrochemical Society Meeting, San Francisco, California, May 1983
18. ELECTRODE REACTIONS INVOLVING LEAD OR CADMIUM IONS IN MOLTEN NITRATES  
M. H. Miles, G. E. McManis, and A. N. Fletcher  
Presented at Electrochemical Society Meeting, San Francisco, California, May 1983
19. VOLTAMMETRIC DETERMINATION OF ORDNANCE MATERIALS IN WATER USING A SILVER WORKING ELECTRODE  
D. A. Fine and M. H. Miles  
Presented at International Symposium on LCEC and Voltammetry, Indianapolis, Indiana, 15-17 May 1983
20. DISCHARGE CHARACTERISTICS FOR LITHIUM COMPOSITES AND LITHIUM ALLOY ANODES IN MOLTEN NITRATE ELECTROLYTES  
G. E. McManis, M. H. Miles, and A. N. Fletcher  
Presented at Electrochem. Soc. Fall Meeting, Washington, DC, 9-14 October 1983
21. THE ELECTROCHEMICAL REDUCTION OF FORMATE G. E. McManis, G. E. McManis, and A. N. Fletcher  
Presented at the ONR Sponsored Meeting on Electrochemistry, Park City, UT, 19 to 23 March 1984
22. ELECTROCHEMICAL REDUCTION REACTIONS INVOLVING FORMIC ACID M. H. Miles and A. N. Fletcher  
Presented at 187th National Meeting of the American Chemical Society, St. Louis, Missouri, 8-13 April 1984
23. THE ELECTROCHEMICAL CONVERSION OF CARBON DIOXIDE TO METHANOL: THE FORMIC ACID REDUCTION STEP  
M. H. Miles  
Chemistry Division Seminar, 27 April 1984
24. THE ELECTROCHEMISTRY OF NITRATE-AMIDE MELTS G. E. McManis, A. N. Fletcher, M. H. Miles, and D. E. Bliss  
ONR Meeting on Electrochemistry, Naval Ocean Systems Center, San Diego, California, 9-10 October 1985
25. PROBLEMS ASSOCIATED WITH THE ELECTROCHEMICAL REDUCTION OF METAL IONS IN MOLTEN NITRATES  
M. H. Miles, G. E. McManis, and A. N. Fletcher  
Presented at 168th Meeting of the Electrochemical Society, Las Vegas, NV, 13-18 October 1985
26. MOLTEN NITRATE THERMAL BATTERIES: ANODIC AND CATHODIC REACTIONS  
M. H. Miles  
Chemistry Division Seminar, 9 December 1985
27. ELECTROCHEMISTRY IN MOLTEN NITRATES M.H. Miles  
Presented at University of Texas at Dallas, Richardson, TX, 20 March 1987
28. ELECTROCHEMICAL STUDIES OF CARBON DIOXIDE & SODIUM FORMATE IN AQUEOUS SOLUTIONS  
M. H. Miles  
Presented at American Chemical Society Meeting, New York City, New York, 13-18 April 1986
29. THE ELECTROCHEMICAL BEHAVIOR OF A SERIES OF 1,1'-DIALKYL VILOGEN COMPOUNDS IN AQUEOUS SOLUTIONS

M. H. Miles, R. A. Henry, and R. A. Hollins

Presented at 172nd Meeting of the Electrochemical Society, Honolulu, Hawaii, 18-23 October 1987

30. THERMODYNAMIC EFFECTS OF METAL IONS ON MOLTEN NITRATE ELECTROCHEMISTRY

M. H. Miles

Presented at 172nd Meeting of the Electrochemical Society, Honolulu, Hawaii, 18-23 October 1987

31. ELECTROCHEMISTRY OF MOLTEN NITRATES

M. H. Miles

Electronics Division Seminar, Los Alamos National Laboratory, Los Alamos, NM, 23 November 1987

32. ELECTROCHEMICAL GENERATION OF METHANOL AND OXYGEN FROM CARBON DIOXIDE AND WATER

M. H. Miles

Presented at ONR and ORI Workshop on Carbon Dioxide Reactivity and Reduction, Menlo Park, California, 25-27 April 1988

33. BROMINE TRIFLUORIDE AS AN OXIDIZING SOLVENT FOR BATTERY APPLICATIONS

K. H. Park, D. E. Bliss, D. E. Stilwell, R. F. Rhein, and M. H. Miles

Presented at 173rd Meeting of the Electrochemical Society, Atlanta, Georgia, 15-20 May 1988

34. THE ELECTROCHEMICAL REDUCTION OF CARBON DIOXIDE M.H.Miles

Presented at United Technologies, Hamilton Standard, East Granby, Connecticut, 24 May 1988

35. INTERHALOGEN SOLVENTS FOR BATTERY APPLICATIONS M. H. Miles

Presented at EIC Laboratories, Inc., Norwood, Massachusetts, 25 May 1988

36. INTERHALOGEN SOLVENTS FOR BATTERY APPLICATIONS M. H. Miles

Presented at Wilson-Greatbatch Ltd., Clarence, New York, 26 May 1988

37. LITHIUM INTERHALOGEN ELECTROCHEMISTRY M. H. Miles

Presented at Interagency Advanced Power Group Meeting, Silver Spring, Maryland, 19 July 1988

38. ELECTROCHEMICAL STUDIES OF NEW VIologen COMPOUNDS M. H. Miles, R. A. Henry, and R. A. Hollins

Presented at Electrochemical Society Meeting, Chicago, Illinois, 9-14 October 1988

39. ELECTROCHEMICAL STUDIES ON ELECTRODEPOSITED TUNGSTEN TRIOXIDE FILMS

D. E. Stilwell, K. H. Park, and M. H. Miles

Presented at Electrochemical Society Meeting, Chicago, Illinois, 9-14 October 1988

40. A COMPARISON OF LITHIUM, MAGNESIUM, AND CALCIUM ANODES IN BROMINE TRIFLUORIDE

K. H. Park, D. E. Stilwell, D. E. Bliss, R. A. Hollins, and M. H. Miles

Presented at Electrochemical Society Meeting, Chicago, Illinois, 9-14 October 1988

41. ELECTROCHEMICAL DETECTION OF NITRATE ESTERS M. H. Miles

Presented at Auburn University, Auburn, Alabama, 17 October 1988 INVITED

42. BROMINE TRIFLUORIDE AS AN OXIDIZING SOLVENT FOR BATTERY APPLICATIONS

M. H. Miles and K. H. Park

Presented at 39th Meeting of the Lithium Battery Technical/Safety Group Meeting, San Diego, California, 22-23 February 1989, INVITED

43. LITHIUM BATTERY SYSTEMS USING INTERHALOGEN SOLVENTS M. H. Miles and K. H. Park  
Presented at ONR, SDIO, and DARPA Sponsored Meeting on Battery and Fuel Cells, Menlo Park, California, 29-30  
March 1989, INVITED

44. COLD FUSION TECHNOLOGY STATUS  
M. H. Miles  
Presented at NWC Connnander's Meeting, 1 May 1989, INVITED

45. THE COLD FUSION PROCESS M. H. Miles  
Presented to Weapons Planning Group, NWC (Code 30), 22 May 1989, INVITED

46. ELECTROCHEMICAL CALORIMETRIC STUDIES ON WATER AND DEUTERIUM OXIDE  
ELECTROLYSIS  
M. H. Miles and D. E. Stilwell  
Presented at Workshop on Cold Fusion Phenomena, Santa Fe, New Mexico, 23-25 May 1989

47. COLD FUSION TECHNOLOGY STATUS UPDATE M. H. Miles and D. E. Stilwell  
Presented to Weapons Systems Analysis Division, NWC (Code 351), 2 June 1989 INVITED

48. ELECTROCHROMIC MATERIALS FOR MISSILE APPLICATIONS M. H. Miles, D. E. Stilwell, and R. A.  
Hollins  
Presnted at 2nd Navy IR/IED Symposium, Silver Spring, Maryland, 6-7 June 1989, INVITED

49. ELECTROCHEMICAL STUDIES OF VARIOUS METALS IN BROMINE TRIFLUORIDE  
K. H. Park, M. H. Miles, D. E. Stilwell, and R. A. Hollins Presented at Electrochemical Society Meeting, Hollywood,  
Florida, 15-20 October 1989

50. ELECTROCHEMICAL STUDIES OF VIOLOGEN, TUNGSTEN TRIOXIDE, POLYANILINE, AND  
PRUSSIAN BLUE MATERIALS  
M. H. Miles, D. E. Stilwell, R. A. Hollins, and R. A. Henry Presented at Electrochemical Society Meeting,  
Hollywood, Florida, 15-20 October 1989

51. AN ELECTROCHEMICAL CALORIMETRIC SEARCH FOR EVIDENCE OF COLD FUSION  
M. H. Miles, K. H. Park, and D. E. Stilwell  
Presented at Electrochemical Society Meeting, Hollywood, Florida, 15-20 October 1989

52. I. AN ELECTROCHEMICAL CALORIMETRIC SEARCH FOR EVIDENCE OF COLD FUSION. II.  
SUMMARY OF A COLD FUSION SYMPOSIUM, OCTOBER 1989  
M.H.Miles  
Presented at Vanderbilt University, Nashville, Tennessee, 23 October 1989, INVITED

53. AN ELECTROCHEMICAL CALORIMETRIC SEARCH FOR EVIDENCE OF COLD FUSION  
M. H. Miles  
Presented at Middle Tennessee State University, Murfreesboro, Tennessee 24 October 1989, INVITED

54. COLD FUSION  
M. H. Miles  
Presented at Austin Paey State University, Clarkesville, Tennessee 25 October 1989, INVITED

55. AN ELECTROCHEMICAL CALORIMETRIC SEARCH FOR EVIDENCE OF COLD FUSION  
M. H. Miles  
Presented at NWC Chemistry Division Seminar, China Lake, California, 8 November 1989 ·

56. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE COLD FUSION EFFECT  
M. H. Miles, K. H. Park, and D. E. Stilwell  
Presented at the First Annual Conference on Cold Fusion, Salt Lake City, Utah 28-31 March 1990, INVITED
57. ELECTROCHROMIC BEHAVIOR OF PRUSSIAN BLUE AND VIOLOGEN MATERIALS  
M. H. Miles  
Presented at The University of New Mexico, Albuquerque, New Mexico, 30 November 1990 (INVITED)
58. HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS  
B. F. Bush and M. H. Miles  
Presented at the Second Annual R and D Information Exchange Conference, 2-4 April 1991 (INVITED)
59. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles  
Presented at Chemistry Division Seminar, 9 May 1991
60. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M.H. Miles  
Presented at the IAPG Chemical Working Group Meeting, Sandia National Laboratory, Albuquerque, New Mexico, 21 May 1991 (INVITED)
61. EXPERIMENTS ON COLD FUSION M. H. Miles  
Presented at IEEE Luncheon Meeting, 19 June 1991 (INVITED)
62. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, G. S. Ostrom, B. F. Bush, and J. J. Lagowski  
Presented at Second Annual Conference on Cold Fusion, Como, Italy, 1 July 1991 (INVITED)
63. COLD FUSION, 1990-1991 M. H. Miles  
Presented at the University of Hamburg, Hamburg, Germany, 10 July 1991 (INVITED)
64. COLD FUSION RESEARCH AT THE NAVAL WEAPONS CENTER M. H. Miles  
Presented at Brigham Young University, Provo, UT, 23 July 1991 (INVITED)
65. REVIEW OF COLD FUSION RESULTS AT THE NAVAL WEAPONS CENTER M. H. Miles and B. F. Bush  
Workshop on Anomalous Effects in Condensed Systems, Office of Naval Research, Arlington, VA, 6 August 1991, INVITED
66. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles and B. F. Bush  
Condensed Matter and Radiation Sciences Division Seminar, Naval Research Laboratory, Washington, DC, 7 August 1991, INVITED
67. RECENT COLD FUSION RESULTS AT THE NAVAL WEAPONS CENTER M. H. Miles and B. F. Bush  
IR/JED Program On-Site Review, Naval Weapons Center, China Lake, CA 6 September 1991, INVITED
68. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS B. F. Bush, M. H. Miles, G. S. Ostrom and J. J. Lagowski  
The Electrochemical Society 180th Meeting, Phoenix, AZ, 16 October 1991, INVITED
69. LITHIUM DEPOSITION/DISSOLUTION STUDIES FROM MOLTEN NITRATE SALTS  
G. S. Ostrom, J. H. France, and M. H. Miles  
The Electrochemical Society 180th Meeting, Phoenix, AZ, 17 October 1991
70. COLD FUSION EXPERIMENTS AT THE NAVAL WEAPONS CENTER M. H. Miles and B. F. Bush  
Southern California Edison Workshop on Cold Fusion, Rosemead, CA, 29 October 1991, INVITED

71. COLD FUSION

M. H. Miles

Weapons Planning Group Seminar, Naval Weapons Center, China Lake, CA, 5 November 1991, INVITED

72. THERMODYNAMIC AND CALORIMETRIC PRINCIPLES IN COLD FUSION EXPERIMENTS

M. H. Miles and B. F. Bush

12th IUPAC Conference on Chemical Thermodynamics, 18 August 1992, Snowbird, UT

73. NAWCWPNs PROGRAM ON ANOMALOUS EFFECTS IN DEUTERATED SYSTEMS

M. H. Miles and B. F. Bush

Navy Program Review on Anomalous Effects in Electrochemically Loaded Deuterated Materials, 2 September 1992, Naval Research Laboratory, Washington, DC

74. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS M. H. Miles and B. RF. Bush

Third International conference on Cold fusion, 24 October 1992, Nagoya, Japan

75. MEASUREMENTS OF EXCESS POWER AND HELIUM PRODUCTION INVOLVING DEUTERATED PALLADIUM

M. H. Miles

Department of Chemistry, University of Utah, Salt Lake City, UT, 2 April 1993

76. ANOMALOUS EFFECTS IN THE Pd/D<sub>2</sub>O ELECTROLYSIS SYSTEM M. H. Miles

Seminar in Materials and Solid Mechanics, University of California, San Diego, CA, 20

May 1993

77. ANOMALOUS EFFECTS IN THE ELECTROCHEMISTRY OF HEAVY WATERS: COLD FUSION UPDATE  
M.H. Miles

Chemistry Division Seminar Series, NAWCWPNs, 24 June 1993

78. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM SYSTEMS

M. H. Miles and B. F. Bush

Presented at the Russian Conference on Cold Fusion, Abrau Durso, Russia, 30 September 1993, INVITED

79. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM M. H. Miles and B. F. Bush

Presented at the Fourth International Conference on Cold fusion, Lahaina, Maui, HI, 6 December 1993, INVITED

80. HEAT AND HELIUM M. H. Miles

Presented at ONR Review on Anomalous Effects in Deuterated systems, 17 March 1994, INVITED

81. PRACTICAL ASPECTS OF HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM

B. F. Bush and M. H. Miles

Presented at the International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Velarus, 24-26 May 1994 (INVITED), presented by

John Dash

82. ANOMALOUS EFFECTS IN DEUTERATED PALLADIUM SYSTEMS: CHINA LAKE RESULTS

M. H. Miles and K. B. Johnson

Presented at ONR Review on Anomalous Effects in Deuterated Systems, 6 Dec 1994 (INVITED)

83. ANOMALOUS EXCESS HEAT AND HELIUM PRODUCTION DURING HEAVY- WATER ELECTROLYSIS

M. H. Miles

Presented at the University of Utah, Department of Chemical and Fuels Engineering, 30 March 1995 (INVITED)

84. NUCLEAR PRODUCTS COMMENSURATE WITH ENERGY GENERATED DURING D2O ELECTROLYSIS AT PALLADIUM CATHODES: QUANTITATIVE ANALYSIS

B. F. Bush, J. J. Lagowski, and M. H. Miles

Presented at 209th ACS National Meeting, Anaheim, CA, 3 Apr 1995 (INVITED)

85. CORRELATION OF EXCESS POWER AND HELIUM PRODUCTION

M. H. Miles and K. B. Johnson

Presented at 209th ACS National Meeting, Anaheim, CA, 3 Apr 1995 (INVITED)

86. THE EXTRACTION OF INFORMATION FROM AN INTEGRATING OPEN CALORIMETER IN FLEISCHMANN-PONS EFFECT EXPERIMENTS

M. H. Miles

Presented at the Fifth International Conference on Cold Fusion, Monte Carlo, Monaco, 10 Apr 1995 (INVITED)

87. CHINA LAKE CALORIMETERS M. H. Miles and K. B. Johnson

Presented at ONR Review on Anomalous Effects in Deuterated Systems, China Lake, CA, 8 June 1995 (INVITED)

88. HEAT AND HELIUM MEASUREMENTS USING PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER

M. H. Miles, K. B. Johnson, and M. A. Imam

The Sixth International Conference on Cold Fusion, Hokkaido, Japan, 8-13 Oct 1996 (INVITED)

89. ELECTROCHEMICAL LOADING OF HYDROGEN AND DEUTERIUM INTO PALLADIUM AND PALLADIUM-BORON ALLOYS

M. H. Miles and K. B. Johnson

The Sixth International Conference on Cold Fusion, Hokkaido, Japan, 13-18 Oct 1996 (INVITED)

90. IMPROVED, OPEN CELL, HEAT CONDUCTION, ISOPERIBOLIC CALORIMETRY M. H. Miles and K. B. Johnson

The Sixth International Conference on Cold Fusion, Hokkaido, Japan, 13-18 Oct 1996 (INVITED)

91. REPLY TO S. E. JONES AND L. D. HANSEN CONCERNING CLAIMS OF MILES, ET AL. IN PONS-FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS

M. H. Miles

The Sixth International Conference on Cold Fusion, Hokkaido, Japan, 13-18 Oct 1996 (INVITED)

92. NUCLEAR PRODUCTS ASSOCIATED WITH THE PONS AND FLEISCHMANN EFFECT; HELIUM COMMENSURATE TO HEAT GENERATION, CALORIMETRY, AND RADIATION

B. F. Bush, J. J. Lagowski, and M. H. Miles

The Sixth International Conference on Cold fusion, Hokkaido, Japan, 13-18 Oct 1996 (INVITED)

93. SUMMARY OF NAVY RESEARCH ON ANOMALOUS EFFECTS IN DEUTERATED SYSTEMS

M.H. Miles

INVITED presentation at Pirelli Cavi S.P.A., Milan, Italy, 25 Feb 1997

94. ANOMALOUS HEAT AND HELIUM PRODUCTION USING PALLADIUM-BORON ALLOYS IN HEAVY WATER

M. H. Miles, K. B. Johnson, and M. A. Imam

Presented at 32nd Intersociety Energy conversion Engineering Conference (ICED-97), Honolulu, HI, 27 July-1 Aug 1997, presented by Dr. S. Szpak

95. PROGRESS REPORT ON PALLADIUM CATHODE STUDIES IN CELLS A AND B  
M. H. Miles  
New Hydrogen Energy Laboratory, Sapporo Japan, 9 Jan 1998
96. ELECTROCHEMICAL CALORIMETRIC STUDIES OF PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER  
M.H. Miles  
New Hydrogen Energy Laboratory, Sapporo, Japan, 6 Mar 1998
97. IMPROVED THERMAL BATTERY TECHNOLOGY  
M. H.Miles  
Active Sensor Technology Meeting, Huntsville, AL, 15 July 1998
98. LITHIUM BATTERIES USING MOLTEN NITRATE ELECTROLYTES  
M. H. Miles  
Fourteenth Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15, 1999 (Invited)
99. NEW MAIN-CHAIN POLY (CROWN ETHER) MEMBRANE FOR LITHIUM-ION BATTERIES  
G. A. Lindsay, M. H. Miles, P. Zarras, S. Fallis, R. Y. Yee, A. P. Chafin, and L. H. Merwin  
Fourteenth Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15, 1999 (Invited)
100. IMPROVED LITHIUM THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES  
M. H. Miles  
Sandia National Laboratory, 2 June 1999 (Invited)
101. PRODUCTION OF HELIUM IN THE COLD M. H. Miles  
Eighteenth Annual Meeting of the Society for Scientific Exploration, Albuquerque, NM, 3-5 June 1999 (Invited)
102. IMPROVED THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES  
M. H. Miles  
35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Ontario, CA
103. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LiOD/PT ELECTROLYSIS CELLS M.H.Miles  
35th ACS Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Ontario, California
104. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN  
M.H. Miles  
Asti Workshop on Anomalies in Hydrogen/ Deuterium Loaded Metals, Conference Proceedings, Italian Physical Society, October 22-24, 1999, Asti, Italy (Invited)
105. CALORIMETRIC STUDIES AT THE NEW HYDROGEN ENERGY LABORATORY IN JAPAN  
M.H. Miles  
The American Physical Society Meeting, March 20-24, 2000 Minneapolis, Minnesota (Invited)
106. CALORIMETRIC STUDIES OF PALLADIUM ALLOY CATHODES USING FLEISCHMANN-PONS DEWAR TYPE CELLS  
M.H. Miles  
The Eighth International Conference on Cold Fusion (ICCF-8) May 21-26, 2000, Lerici, Italy (Invited)

107. CASE STUDIES OF TWO EXPERIMENTS CARRIED OUT WITH THE ICARUS SYSTEMS  
M.H. Miles, M.A. Imam, and M. Fleischmann  
The Eighth International Conference on Cold Fusion (ICCF-8) May 21-26, 2000, Lerici, Italy (Invited)
108. LITHIUM THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES M.H.Miles  
The 39th Power Sources Conference, June 12-15, 2000  
Cherry Hill, New Jersey (Invited)
109. NEW THERMAL BATTERY SYSTEMS USING MOLTEN NITRATE ELECTROLYTES  
M.H. Miles  
B&P Review, China Lake, CA, June 29, 2000 (Invited)
110. HIGH CAPACITY METAL NITRIDE ANODES FOR LITHIUM BATTERIES M.H. Miles  
SBIR Review, China Lake, CA, July 13, 2000 (Invited)
111. PEAK POWER BATTERY  
M.H. Miles  
SBIR Review, China Lake, CA, July 13, 2000 (Invited)
112. ANOMALOUS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM- BORON SYSTEM  
M.H. Miles, M.A. Imam, and M. Fleischmann  
The 8th Russian Conference on Cold Nuclear Transmutation (RCCNT-8), Sochi, Russia, October 6-8, 2000  
Presented by Professor John Dash (Invited)
113. EXCESS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM  
M.H. Miles, M.A. Imam, and M. Fleischmann  
The American Nuclear Society and the European Nuclear Society Meeting, Washington, D.C., 12-16 November, 2000  
(Invited)
114. CALORIMETRIC STUDIES OF PALLADIUM ALLOY CATHODES USING FLEISCHMANN-PONS  
DEWAR TYPE CELLS  
M.H. Miles  
Integrity in Research Conference, Washington, D.C., 17 November 2000 (Invited)
115. CALORIMETRY OF Pd-D CODEPOSITION IN A FLEISCHMANN-PONS DEWAR CELL  
M.H. Miles, S. Szpak, P. Mosier-Boss, and M. Fleischmann  
The American Physical Society, Seattle, Washington, 12-16 March, 2001 Presented by T. Chubb (Invited)
116. ADVANCED THERMAL BATTERY TECHNOLOGY  
M.H. Miles and C.H. Hinners  
Weapons ATRB-VI Prep Meeting, Patuxent River, Maryland, 29 March 2001
117. EXAMINATION OF LINEAR POTENTIAL SWEEP METHODS FOR DETERMINING THE  
CAPACITANCE OF HYDROUS RUTHENIUM OXIDE MATERIALS  
M.H. Miles, T. J. Groshens, and C.E. Johnson  
2001 Joint International Meeting of The Electrochemical Society/International Society of Electrochemistry, San  
Francisco, CA, 2-7 September 2001.
118. ELECTROCHEMISTRY OF MOLTEN NITRATE ELECTROLYTES AND APPLICATIONS FOR HIGH  
VOLTAGE LITHIUM CELLS  
M.H. Miles  
2001 Joint International Meeting of The Electrochemical Society/International Society of Electrochemistry, San  
Francisco, CA, 2-7 September 2001.

119. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE

M.H. Miles, M.A. Imam, and M. Fleischmann

2001 Joint International Meeting of The Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001.

120. RECENT ADVANCES IN LITHIUM BATTERY TECHNOLOGY M.H. Miles

2001 IEEE GaAs IC Symposium, Baltimore, MD, 21-24 October 2001

121. TWELVE YEARS OF COLD FUSION RESEARCH: EXCESS ENTHALPY AND HELIUM-4 PRODUCTION

M.H. Miles

Middle Tennessee State University, February 14, 2002

122. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES M.H. Miles

American Physical Society 2002 Meeting, Indianapolis, Indiana, March 22, 2002

123. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES M.H. Miles

Ninth International Conference on Cold Fusion, Beijing, China, May 21, 2002

124. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO- DEPOSITION

M.H. Miles, S. Szpak, P.A. Mosier-Boss, and M. Fleischmann

Ninth International Conference on Cold Fusion, Beijing, China, May 21, 2002

125. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

The American Physical Society, Austin, Texas, 7 March 2003

126. COLD FUSION EXPERIMENTS: EXCESS ENTHALPY AND HELIUM-4 PRODUCTION

M.H. Miles

Bates College, Lewiston, Maine, March 12, 2003.

127. COLD FUSION EXPERIMENTS: THE EVIDENCE FOR EXCESS ENTHALPY AND HELIUM PRODUCTION

M.H. Miles

Amherst College, Amherst, MA, March 24, 2003.

128. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

Tenth International Conference on Cold Fusion, Cambridge, MA, August 23, 2003

129. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW

M.H. Miles

Tenth International Conference on Cold Fusion, Cambridge, MA, August 23, 2003

130. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

M.H. Miles

American Physical Society 2003 Meeting, Austin, Texas, March, 2003.

131. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW"

M.H. Miles

American Physical Society 2004 Meeting, Montreal, Canada, March 22, 2004

132. SIMULTANEOUS EXCESS POWER AND ANOMALOUS RADIATION M.H. Miles

American Physical Society 2005 Meeting, Los Angeles, CA, March 21-25, 2005

133. ISOPERIBOLIC CALORIMETRY APPLIED TO THE Pt/ D2O BLANK SYSTEM M. Fleischmann and M.H. Miles

American Physical Society 2006 Meeting, Baltimore, MD, March, 13-17, 2006

#### PATENTS

1. ELECTROCHEMICAL PREPARATION OF METALLIC TELLURIDES Melvin H. Miles and William S. McEwan

U.S. Patent No. 3,887,446, 3 June 1975

2. SILVER SULFIDE CATHODE FOR LIQUID AMMONIA BATTERIES AND FUEL CELLS CONTAINING SULFUR AND HS IN THE ELECTROLYTE

M. H. Miles

U.S. Patent No. 3,943,001, 9 March 1976

3. PROTECTED CALCIUM ANODE

Melvin H. Miles, Aaron N. Fletcher, and Dwight A. Fine

U.S. Patent No. 4,190,704, 26 February 1980

4. THERMAL BATTERY CELLS UTILIZING MOLTEN NITRATES AS THE ELECTROLYTE AND OXIDIZER Melvin H. Miles and Aaron N. Fletcher

U.S. Patent No. 4,260,667, 7 April 1981

5. THERMAL BATTERY CELLS UTILIZING AgNO<sub>3</sub> IN LiClO<sub>4</sub>-LiNO<sub>3</sub> MIXTURES Melvin H. Miles and Aaron N. Fletcher

U.S. Patent No. 4,416,958, 22 November 1983

6. DETECTION OF NITRATE ESTERS USING SILVER ELECTRODES Melvin H. Miles and Dwight A. Fine

Navy Case No. 66846, 27 July 1982

U.S. Patent 4,436,590, 13 March 1984

7. THERMAL CELLS NON-DEFLAGRATION DESIGN

G. E. McManis, M. H. Miles, and A. N. Fletcher Navy Case 66054, 12 August 1981

8. Li(B) ANODES IN NITRATE THERMAL BATTERY CELLS

G. E. McManis, M. H. Miles and A. N. Fletcher Navy Case No. 66975, 22 November 1982

U.S. Patent No. 4,535,037, 13 August 1985

9. HIGH PERFORMANCE MOLTEN NITRATE CELL George E. McManis, Melvin H. Miles, and A. N. Fletcher

Navy Case No. 67622, 6 December 1983

U.S. Patent 4,528,253, 16 July 1985

10. METAL ION ADDITIVES IN LOW-MELTING MOLTEN NITRATE CELLS

M. H. Miles

Navy Case No. 70845, 1 September 1987

11. INDIUM, LEAD, AND CADMIUM SALTS AS CATHODE MATERIALS IN OXYANIONIC MELTS

M. H. Miles

Navy Case No. 70848, 1 September 1987

12. ENHANCED CYCLE LIFETIME FOR ELECTROCHROMIC SYSTEMS

M. H. Miles, R. A. Henry, and D. A. Fine Navy Case No. 70847, 1 September 1987 U.S. Patent 5,516,462, May 14, 1996

13. EXCESS ENTHALPY ELECTROCHEMICAL REACTOR

M. H. Miles

Navy Case No. 73725

14. RECHARGEABLE LITHIUM THERMAL BATTERY

M. H. Miles, G. S. Ostrom, and J. H. France Navy Case No. 74172

15. CORROSION INHIBITION OF ALUMINUM ALLOYS COATED WITH POLY(2,5- bis(N-METHYL-N-ALKYLAMINO) PHENYLENE VINYLENES

P. Zarras, J. D. Stenger-Smith, and M. H. Miles Navy Case No. 78281

16. AMINO FUNCTIONED POLY(P-PHENYLENE VINYLENES) AS PROTECTIVE COATINGS

J. D. Stenger-Smith, P. Zarras, J. Fischer, M. Miles, and J. Nelson U.S. Patent No. 5,904,990, May 18, 1999

17. PALLADIUM-BORON ALLOYS FOR EXCESS ENTHALPY PRODUCTION

M. H. Miles and M. Ashraflmam

U.S. Patent No. 6,764,561, June 20, 2004

## A2. Set 1 from David Nagel (1990-2014)

1. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE COLD FUSION EFFECT. M. H. Miles, K. H. Park, and D. E. Stilwell in "*Proceedings of the First Annual Conference on Cold Fusion*," Salt Lake City, Utah, 28-31 March 1990, pp. 328-334.
2. THEORETICAL NEUTRON FLUX LEVELS, DOSE RATES, AND METAL FOIL ACTIVATION IN ELECTROCHEMICAL COLD FUSION EXPERIMENTS. M. H. Miles and R. E. Miles, *J. Electroanal. Chem.*, Vol. 295 (1990), pp. 409-414.
3. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE ELECTROLYSIS OF WATER AND HEAVY WATER (D<sub>2</sub>O). D.E. Stilwell, M. H. Miles, and K. H. Park, *J. Fusion Energy*, Vol. 9 (1990), pp. 333-336.
4. ELECTROCHEMICAL CALORIMETRIC EVIDENCE FOR COLD FUSION IN THE PALLADIUM-DEUTERIUM SYSTEM. M. H. Miles, K. H. Park, and D. E. Stilwell, *J. Electroanal. Chem.*, Vol. 296 (1990), pp. 409-414.
5. HELIUM PRODUCTION DURING THE ELECTROLYSIS OF D<sub>2</sub>O IN COLD FUSION EXPERIMENTS. B. F. Bush, J. J. Lagowski, M. H. Miles, and G. S. Ostrom, *J. Electroanal. Chem.*, Vol. 304 (1991), pp. 271-278.
6. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS. M. H. Miles, G. S. Ostrom, B. F. Bush, and J. J. Lagowski in "*II Annual Conference on Cold Fusion-Invited Papers*", T. Bressani and F. Iazzi, Editors, 29 June- 4 July 1991, p. 17, Abstract No. 6.
7. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS
8. M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski in *Proceedings of the 2nd Annual Conference on Cold Fusion*, Como, Italy, Italian Physical Society, Bologna, Italy, pp. 363-372, 1991.
9. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS
10. M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski in "*Electrochemical Society Extended Abstracts*", Vol. 91-2, p. 119, 1991.
11. COLD FUSION EXPERIMENTER MILES RESPONDS TO CRITIC
12. M. H. Miles in "*21st Century Science and Technology*," Vol. 5, No. 1, pp. 75-80, 1992.
13. EXPERIMENTAL EVIDENCE FOR CORRELATED HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS
14. M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski in *Hydrogen Storage Materials, Batteries, and Electrochemistry*, D. A. Corrigan and S. Srinivasan, editor, The Electrochemical Society, pp. 287-297 (1991).
15. THERMODYNAMIC AND CALORIMETRIC PRINCIPLES IN COLD FUSION EXPERIMENTS
16. M. H. Miles and B. F. Bush in *12th IUPAC Conference on Chemical Thermodynamics, Joint Meeting with the 47th Calorimetry Conference*, 16-21 August 1992, Snowbird, Utah, Program, Abstracts, and Reports, p. 194.
17. SEARCH FOR ANOMALOUS EFFECTS INVOLVING EXCESS POWER, HELIUM, AND TRITIUM DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES
18. M. H. Miles and B. F. Bush in *Frontiers of Cold Fusion*, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 189-199 (1993).
19. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS
20. M. H. Miles and B. F. Bush in *Frontiers of Cold Fusion*, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 113-122 (1993).
21. CORRELATION OF EXCESS ENTHALPY AND HELIUM PRODUCTION DURING D<sub>2</sub>O and H<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES
22. M. H. Miles, R. A. Hollins, B. F. Bush, J. J. Lagowski, and R. E. Miles, *J. Electroanal. Chem.*, Vol. 346, pp. 99-117 (1993).
23. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS

24. M. H. Miles and B. F. Bush in *Proceedings of the Third International Conference on Cold Fusion*, Nagoya, Japan, pp. 113-122 (1993).
25. CALORIMETRIC PRINCIPLES AND PROBLEMS IN MEASUREMENTS OF EXCESS POWER DURING Pd-D<sub>2</sub>O ELECTROLYSIS
26. M. H. Miles, B. F. Bush, and D. E. Stilwell, *Journal of Physical Chemistry*, Vol. 98, pp. 1948-1952 (1994).
27. ANOMALOUS EFFECTS INVOLVING EXCESS POWER, RADIATION, AND HELIUM PRODUCTION DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES
28. M. H. Miles, B. F. Bush, and J. J. Lagowski in *Fusion Technology*, Vol. 25, pp. 478-486 (1994).
29. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM
30. M. H. Miles and B. F. Bush in *Proceedings of the Fourth International Conference on Cold Fusion (ICCF-4)* Lahaine, Hawaii, U.S.A., December 6-9, 1993.
31. PRACTICAL ASPECTS OF HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM
32. M. H. Miles and B. F. Bush in "Cold Fusion Source Book," Hal Fox, Editor, Fusion Information Center, Salt Lake City, UT (1994).
33. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM SYSTEMS
34. M. H. Miles and B. F. Bush in *Proceedings of International Symposium on Cold Fusion and Advanced Energy Sources*, Minsk, Belarus, 1994, pp. 349-356.
35. NUCLEAR PRODUCTS COMMENSURATE WITH ENERGY GENERATED DURING D<sub>2</sub>O ELECTROLYSIS AT PALLADIUM CATHODES: QUANTITATIVE ANALYSIS
36. B. F. Bush, J. J. Lagowski, and M. H. Miles in *ACS Book of Abstracts, Division of Nuclear Chemistry and Technology*, Abstract No. 45, April 2-6, 1995.
37. CORRELATIONS OF EXCESS POWER AND HELIUM PRODUCTION
38. M. H. Miles and K. B. Johnson in *ACS Book of Abstracts, Division of Nuclear Chemistry and Technology*, Abstract No. 59, April 2-6, 1995.
39. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM
40. M. H. Miles and B. F. Bush, *Transactions of Fusion Technology*, Vol. 26, pp. 156-159, December 1994.
41. THE EXTRACTION OF INFORMATION FROM AN INTEGRATING OPEN CALORIMETER IN FLEISCHMANN-PONS EFFECT EXPERIMENTS
42. M. H. Miles in *Proceedings of the Fifth International Conference on Cold Fusion, Monte Carlo, Monaco*, April 9-13, 1995, pp. 97-104.
43. ANOMALOUS EFFECTS IN DEUTERATED SYSTEMS
44. Melvin H. Miles, Benjamin F. Bush, and Kendall B. Johnson, *NAWCWPNS TP 8302*, Sept 1996.
45. ELECTROCHEMICAL INSERTION OF HYDROGEN INTO METALS AND ALLOYS
46. Melvin H. Miles and Kendall B. Johnson, *Journal of New Energy*, Vol. 1, No. 2, pp. 32-36, 1996.
47. HEAT AND HELIUM MEASUREMENTS USING PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER
48. M. H. Miles, K. B. Johnson, and M. A. Imam, in *Progress in New Hydrogen Energy*, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 20-28.
49. ELECTROCHEMICAL LOADING OF HYDROGEN AND DEUTERIUM INTO PALLADIUM AND PALLADIUM-BORON ALLOYS
50. M. H. Miles and K. B. Johnson, in *Progress in New Hydrogen Energy*, M. Okamoto, Editor, Vol. 1, Japan, 1996, pp. 208-212.
51. IMPROVED, OPEN-CELL, HEAT CONDUCTION, ISOPERIBOLIC CALORIMETRY
52. M. H. Miles and K. B. Johnson, in *Progress in New Hydrogen Energy*, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 496-501.
53. REPLY TO S. E. JONES AND L. D. HANSON CONCERNING CLAIMS OF MILES, ET AL. IN PONS-FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS
54. M. H. Miles in *Progress in New Hydrogen Energy*, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 524-527.
55. NUCLEAR PRODUCTS ASSOCIATED WITH THE PONS AND FLEISCHMANN EFFECT: HELIUM COMMENSURATE TO HEAT GENERATION, CALORIMETRY, AND RADIATION
56. B. F. Bush, J. J. Lagowski, and M. H. Miles, in *Progress in New Hydrogen Energy*, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 622-626.

57. REPLY TO "EXAMINATION OF CLAIMS OF MILES ET AL. IN PONS-FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS"
58. M. H. Miles, *J. Phys. Chem. B*, Vol. 102, 1998, pp. 3642-3646.
59. ANOMALOUS HEAT AND HELIUM PRODUCTION USING PALLADIUM-BORON ALLOYS IN HEAVY WATER
60. M. H. Miles, K. B. Johnson, and M. A. Imam, in *Proceedings of 32<sup>nd</sup> Intersociety Energy Conversion Engineering Conference*, 1997, pp. 1334-1337.
61. ELECTROCHEMICAL CALORIMETRIC STUDIES OF PALLADIUM AND PALLADIUM ALLOYS IN HEAVY WATER
62. M. H. Miles, *NEDO Final Report*, Tokyo, Japan, March 31, 1998. EXCESS HEAT MEASUREMENTS AND NUCLEAR DETECTION EXPERIMENTS IN THE NHE PROGRAM. K. Matsui, N. Asami, M. Sumi, H. Kamimura, A. Kubota, R. Shimada, S. Miyashita, A. Lipson, P. Tripodi, M. Miles and H. Watanabe, in *ICCF-7, Program Manual and Abstracts*, April 19-24, 1998, p. 96.
63. RADIATION MEASUREMENTS AT CHINA LAKE: REAL OR ARTIFACTS?
64. M. H. Miles and B. F. Bush, in *Proceedings of the Seventh International Conference on Cold Fusion*, Vancouver, British Columbia, Canada, April 19-24, 1998, pp. 236-240
65. CALORIMETRY OF THE Pd+D CO-DEPOSITION
66. S. Szpak, P. A. Mosier-Boss and M. H. Miles, *Fusion Technology*, Vol. 36, 1999, pp. 234-241.
67. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD/Pt ELECTROLYSIS CELLS
68. M.H. Miles, in 35<sup>th</sup> ACS Western Regional Meeting, 37<sup>th</sup> SAS Pacific Conference, October 6-8, 1999, Abstract No. 255.
69. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD ELECTROLYSIS CELLS
70. M.H. Miles, *J. Electroanal. Chem.*, Vol. 482, pp. 56-65, 2000.
71. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN
72. M.H. Miles, in Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, Conference Proceedings, Italian Physical Society, October 22-24, 1999.
73. CALORIMETRIC STUDIES AT THE NEW HYDROGEN ENERGY LABORATORY IN JAPAN
74. M.H. Miles, in *Bulletin of The American Physical Society*, Series II, Vol. 45, No. 1, March 2000, Abstract C32.
75. "MY IMPRESSIONS AS A NEDO GUEST RESEARCHER AT THE NHE LABORATORY IN JAPAN"
76. M.H. Miles, in *Infinite Energy*, Vol. 5, Issue 30, pp. 18-21, 2000.
77. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN
78. M.H. Miles, in *Infinite Energy*, Vol. 5, Issue 30, pp. 22-25, 2000.
79. CALORIMETRIC STUDIES OF PALLADIUM ALLOY CATHODES USING FLEISCHMANN-PONS DEWAR TYPE CELLS
80. M.H. Miles, in *Proceedings of the 8<sup>th</sup> International Conference on Cold Fusion*, Lerici (La Spezia), Italy, 21-26 May 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings, Vol. 70, pp. 97-104.
81. CASE STUDIES OF TWO EXPERIMENTS CARRIED OUT WITH THE ICARUS SYSTEMS
82. M.H. Miles, M.A. Imam, and M. Fleischmann, in *Proceedings of the 8<sup>th</sup> International Conference on Cold Fusion*, Lerici (La Spezia), Italy, 21-26 May, 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings, Vol. 70, pp. 105-119.
83. ANOMALOUS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM
84. M.H. Miles, M.A. Imam, and M. Fleischmann, in *Proceedings of 8<sup>th</sup> Russian Conference on Cold Nuclear Transmutation (RCCNT-8)*, Sochi, Russia, October 4-11, 2000, pp. 142-162.
85. EXCESS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM
86. M.H. Miles, M.A. Imam, and M. Fleischmann, in *Transactions of the American Nuclear Society and the European Nuclear Society*, Washington, D.C., 12-16 November, 2000, C.A. Yoelin, Editor, Vol. 83, pp. 371-372.
87. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE
88. M.H. Miles, M. Fleischmann, and M.A. Imam, *Naval Research Laboratory Report NRL/MR/6320-01-8526*, March 26, 2001, pp. 155.
89. CALORIMETRY OF Pd-D CODEPOSITION IN A FLEISCHMANN-PONS DEWAR CELL

90. M.H. Miles, S. Szpak, P. Mosier-Boss, and M. Fleischmann, in *Bulletin of the American Physical Society*, Seattle, Washington, 12-16 March, 2001, Abstract 5403.
91. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE
92. M.H. Miles, M.A. Imam, and M. Fleischmann, in 2001 Joint International Meeting of the Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 811.
93. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE
94. M.H. Miles, M.A. Imam, and M. Fleischmann, in “*Energy and Electrochemical Processes For a Cleaner Environment*”, C. Comninellis, M. Doyle and J. Winnick, Editors, The Electrochemical Society, Proceedings Vol. 2001-23, 2001, pp. 194-205.
95. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM. VOL. I: A DECADE OF RESEARCH AT NAVY LABORATORIES
96. M.H. Miles, “Chapter 3: Excess Heat and Helium Production in Palladium and Palladium Alloys” in *SPAWAR System Center San Diego, Technical Report 1862*, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 19-30.
97. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM, VOL. II: SIMULATION OF THE ELECTROCHEMICAL CELL (ICARUS) CALORIMETRY
98. M. Fleischmann, M.H. Miles, M.A. Imam, P.A. Mosier-Boss and S. Szpak, “Chapter 4: Analysis of Experiment MC-21: A Case Study” in *SPAWAR System Center San Diego, Technical Report 1862*, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 31-89.
99. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES
100. M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga, in *American Physical Society 2002 Meeting*, Indianapolis, Indiana, March 18-22, 2002.
101. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES
102. M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga, in *Ninth International Conference on Cold Fusion, Abstracts*, Beijing, China, May 19-24, 2002, p. 77.
103. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO-DEPOSITION
104. S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann, in *Ninth International Conference on Cold Fusion, Abstracts*, Beijing, China, May 19-24, 2002, p. 78.
105. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING Pd-B ALLOY CATHODE
106. M.H. Miles, M.A. Imam, and M. Fleischmann in “*Batteries and Supercapacitors*”, G. Abba-Nazi, E. Takeuchi, R. Koetz, and B. Scrosati, Editors, The Electrochemical Society, Proceedings Volume 2001-21, 2002, pp. 795-806.
107. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER
108. M.H. Miles, in *Bulletin of the American Physical Society*, Vol. 48, No. 1, Part II, Abstract Z334, p. 1382 (2003).
109. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES
110. M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga, in *Condensed Matter Nuclear Science, Proceedings of the 9<sup>th</sup> International Conference on Cold Fusion*. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 246-249.
111. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO-DEPOSITION
112. M.H. Miles, S. Szpak, P.A. Mosier-Boss, and M. Fleischmann in *Condensed Matter Nuclear Science, Proceedings of the 9<sup>th</sup> International Conference on Cold Fusion*. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 250-254.
113. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER

114. M.H. Miles, in *ICCF-10, 10<sup>th</sup> International Conference on Cold Fusion*, August 24-29, 2003, Abstract No. M012.
115. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW
116. M.H. Miles in *ICCF-10, 10<sup>th</sup> International Conference on Cold Fusion*, August 24-29, 2003, Abstract No. M015.
117. THE “INSTRUMENT FUNCTION” OF ISOPERIBOLIC CALORIMETERS; EXCESS ENTHALPY GENERATION DUE TO THE PARASITE REDUCTION OF OXYGEN”
118. M. Fleischmann and M.H. Miles, in *ICCF-10, 10<sup>th</sup> International Conference on Cold Fusion*, August 24-29, 2003, Abstract TH 12
119. A CALORIMETRIC INVESTIGATION OF THE Pd/B SYSTEM
120. M.H. Miles, M.A. Imam, and M. Fleischmann in *ICCF-10, 10<sup>th</sup> International Conference on Cold Fusion*, August 24-29, 2003, Abstract No. M015.
121. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CODEPOSITION
122. S.Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann, *Thermochimica Acta*, **410**, 101 (2004).
123. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW
124. M.H. Miles, in *Proceedings of the 10<sup>th</sup> International Conference on Cold Fusion (ICCF-10)*, Cambridge, MA USA, August 24-29, 2003, pp. 123-131.
125. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER
126. M.H. Miles, in *Proceedings of the 10<sup>th</sup> International Conference on Cold Fusion (ICCF-10)*, Cambridge, MA USA, August 24-29, 2003, pp. 23-28.
127. THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART II: THE Pd-B AND Pd-B-Ce SYSTEMS
128. M. Fleischmann, M.H. Miles and M.A. Imam, in *SPARWAR System Center San Diego, Technical Report* (submitted but never published).
129. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART III: THE Pd-D CODEPOSITION SYSTEM
130. M. Fleischmann, M.H. Miles, S. Szpak and P.A. Mosier-Boss, in *SPARWAR System Center San Diego, Technical Report* (submitted but never published).
131. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART IV: AN EXPERIMENT WITH A Pd-CATHODE IN 0.1 M LiOD/D<sub>2</sub>O CARRIED OUT IN 1989.
132. M. Fleischmann, M.H. Miles, in *SPARWAR System Center San Diego, Technical Report* (submitted but never published).
133. THE “INSTRUMENT FUNCTION” OF ISOPERIBOLIC CALORIMETERS; EXCESS ENTHALPY GENERATION DUE TO THE PARASITE REDUCTION OF OXYGEN”
134. M. Fleischmann and M.H. Miles, *ICCF-10, 10<sup>th</sup> International Conference on Cold Fusion*, August 24-29, 2003, pp. 247-268.
135. ACCURACY OF ISOPERIBOLIC CALORIMETRY USED IN A COLD FUSION CONTROL EXPERIMENT
136. M.H. Miles and M. Fleischmann in *Low-Energy Nuclear Reaction Sourcebook*, ACS Symposium Series 998, J. Marwan and S.B. Krivit, Editors, American Chemical Society, Washington, DC., pp. 153-171, 2008.
137. TWENTY YEAR REVIEW OF ISOPERIBOLIC CALORIMETRIC MEASUREMENTS OF THE FLEISCHMANN-PONS EFFECT
138. M.H. Miles and M. Fleischmann in *ICCF-14 Proceedings*, Vol.1, D.J. Nagel and M.E. Melich, Editors, Washington DC, 10-15 August, 2008, pp. 6-10.
139. INVESTIGATIONS OF NANOPARTICLE PALLADIUM/DEUTERIUM SYSTEMS  
I. Parchamazad, M. Miles and J. Alston, in *ICCF-14 Conference*, Washington DC, 10-15 August, 2008, Abstract 13 and Presentation.
140. LENR RESEARCH USING CO-DEPOSITION
141. S.Szpak, P.A.Mosier-Boss, F.Gordon, J.Dea, M.Miles, J. Khim and L.Forsley, in *ICCF-14 Proceedings*, Vol. 2, D.J. Nagel and M.E. Melich, Editors, Washington DC, 10-15 August 2008, pp. 766-771.
142. NEW APPROACHES TO ISOPERIBOLIC CALORIMETRY

143. M.H. Miles and M. Fleischmann in *ICCF-15 Proceedings*, V. Violante and F. Sarto, Editors, Rome, Italy, October 5-9, 2009, pp. 22-26.
144. INVESTIGATIONS OF CO-DEPOSITION SYSTEMS
145. M.H. Miles in *ICCF-15 Proceedings*, V. Violante and F. Sarto, Editors, October 5-9, 2009, pp. 33-37.
146. A NEW LOOK AT LOW-ENERGY NUCLEAR REACTION (LENR) RESEARCH: A RESPONSE TO SHANAHAN
147. J. Marwan, M.C.H. McKubre, F.L. Tanzella, P.L. Hagelstein, M.H. Miles, M.R. Swartz, E. Storms, Y. Iwamura, P.A. Mosier-Boss and L.P.G. Forsley, *J. Environ. Monitoring*, Vol. 12.9 (2010), pp. 1756-1776.
148. INVESTGATIONS OF POSSIBLE SHUTTLE REACTIONS
149. M.H. Miles in *ICCF-16 Proceedings*, M. Srinivasan and J.P. Biberian, Editors, February 6-11, 2011, pp. 92-102.
150. NEW ANALYSIS OF MIT CALORIMETRIC ERRORS
151. M.H. Miles and P.H. Hagelstein in *ICCF-16 Proceedings*, M. Srinivasan and J.P. Biberian, Editors, February 6-11, 2011, pp. 103-109.
152. CO-DEPOSITION OF PALLADIUM AND OTHER TRANSITION METALS IN H<sub>2</sub>O AND D<sub>2</sub>O
153. M.H. Miles in *ICCF-17 Proceedings*, August 12-17, 2012 (in press as JCMNS publication).
154. EXAMPLES OF ISOPERIBOLIC CALORIMETRY IN THE COLD FUSION CONTROVERSY
155. M.H. Miles in *ICCF-17 Proceedings*, August 12-17, 2012 (in press as JCMNS publication).
156. COLD FUSION AFTER A QUARTER-CENTURY: THE PD/D SYSTEM
157. M.H. Miles and M.C. H. McKubre in *Development in Electrochemistry: Science Inspired by Martin Fleischmann*, D. Pletcher, Z.Q. Tian, and D. Williams, Editors, John Wiley, London, pp. 245-260, August 2014.
158. THERMODYNAMIC AND KINETIC OBSERVATIONS CONCERNING THE D+D FUSION REACTION FOR THE PD/D SYSTEM
159. M.H. Miles, *J. Condensed Matter Nuclear Science* (submitted).

Note: Still need to add papers for APS meetings organized by Scott Chubb and for ACS meetings (Chicago, San Francisco, Anaheim).

### **A3. Set 2 from David Nagel**

1. STRESS-RELAXATION AND RECOVERY TIME FOR GREASE AND POLYMER SYSTEMS. DETERMINATION OF THE RELAXATION-TIME PARAMETER M. H. Miles, D. W. Miles, A. F. Gabrysh, and H. Eyring *NLGI SPOKESMAN*, 28 (1964), pp. 172-179
2. INTRAMOLECULAR HYDROGEN BONDING IN CIS-CYCLOPROPANEPLOYCARBOXYLIC ACIDS J. L. Haslam, E. M. Eyring, W. W. Epstein, G. A. Christiansen, and M. H. Miles *J. AM. CHEM. SOC.*, 87 (1965), pp. 1-6
3. FAST REACTIONS INVOLVING HYDROGEN BONDING IN 2,2-DISUBSTITUTED MALONIC ACIDS M. H. Miles, E. M. Eyring, W. W. Epstein, and R. E. Ostlund *J. PHYS. CHEM.*, 69 (1965), pp. 467-476
4. DEUTERIUM OXIDE SOLVENT ISOTOPE EFFECTS ON FAST REACTIONS OF SUBSTITUTED MALONIC ACID M. H. Miles, E. M. Eyring, W. W. Epstein, and M. T. Anderson *J. PHYS. CHEM.*, 70 (1966), pp. 3490-3493
5. TEMPERATURE JUMP KINETIC STUDY OF SUBSTITUTED MALONIC ACIDS M. H. Miles Ph.D. Thesis, Department of Chemistry, University of Utah, June 1966
6. FUEL CELL REVIEW AND RESEARCH M. H. Miles NOLC Report 738, 1 October 1967, pp. 7-18

7. FUEL OXIDATION IN ACID LIQUID AMMONIA M. H. Miles and P. M. Kellett NOLC Report 754, 15 February 1968, pp. 7-11
8. ELECTROCHEMICAL OXIDATION OF FUELS IN LIQUID AMMONIA M. H. Miles and P. M. Kellett J. ELECTROCHEM. SOC., 115 (1968), pp. 1225-1227
9. ANODE STUDIES IN ACID LIQUID AMMONIA M. H. Miles and P. M. Kellett NWCCL TP 792, October 1968, pp. 1-29
10. TESTING OF ELECTROCATALYSTS FOR HYDRAZINE OXIDATION IN ACID LIQUID AMMONIA M. H. Miles and P. M. Kellett NWCCL TP 795, October 1968, pp. 1-11
11. LIQUID LITHIUM AMMONIATE ANODE W. S. Harris and M. H. Miles NWCCL TP 822, January 1969, pp. 1-3
12. ELECTRODE OXIDATION OF HYDRAZINE AND RELATED COMPOUNDS IN LIQUID AMMONIA ELECTROLYTES M. H. Miles and P. M. Kellett NWCCL Technical Memo 44-24, May 1969, pp. 1-24
13. FUEL CELL RESEARCH--THE SULFUR CATHODE IN LIQUID AMMONIA M. H. Miles and W. S. Harris NWCCL Technical Memo 44-27, October 1969, pp. 1-24
14. OXIDATION OF N<sub>2</sub>H<sub>4</sub> IN ACID LIQUID NH<sub>3</sub> M. H. Miles and P. M. Kellett in "Interstitial Compounds and Fuel Cell Catalysts: Their Preparative Techniques and Electrochemical Testing," U. S. Bureau of Mines Bulletin 652, 1970, pp. 50-52
15. ELECTRODE OXIDATION OF HYDRAZINE AND RELATED COMPOUNDS IN LIQUID AMMONIA ELECTROLYTES M. H. Miles and P. M. Kellett J. ELECTROCHEM. SOC., 117 (1970), pp. 60-65
16. THE SULFUR CATHODE IN LIQUID AMMONIA M. H. Miles and W. S. Harris J. ELECTROCHEM. SOC., 117 (1970), pp. 1225-1229
17. THE Zn<sup>2+</sup>/Zn(Hg) ELECTRODE REACTION IN BINARY MIXTURES OF WATER AND n-PROPANOL M. H. Miles and H. Gerischer J. ELECTROCHEM. SOC., 118 (1971), pp. 837-841
18. ELECTROCHEMICAL PREPARATION OF CADMIUM AND MERCURY TELLURIDES M. H. Miles and W. S. McEwan J. ELECTROCHEM. SOC., 119 (1972), pp. 1188-1190
19. PROPERTIES OF LiCl AND (CH<sub>3</sub>)<sub>4</sub>NC1 SOLUTIONS IN WATER-METHANOL MIXTURES M. H. Miles and V. Jiamsakul J. ELECTROCHEM. SOC., 120 (1973), pp. 709-712
20. ELECTROLYTIC SYNTHESIS OF SOME HEAVY METAL TELLURIDES M. H. Miles and W. S. McEwan J. ELECTROCHEM. SOC., 120 (1973), pp. 1069-1070
21. HYDROGEN PRODUCTION AND AUXILIARIES M. H. Miles In Brookhaven National Laboratory Report 19249, F. J. Salzano, Ed., July 1974, pp. 21-25, 65-69.
22. HYDROGEN PRODUCTION AND AUXILIARIES M. H. Miles In Brookhaven National Laboratory Report 19520, F. J. Salzano, Ed., October 1974, pp. 21-27, 52-54
23. HYDROGEN AND NITROGEN EVOLUTION REACTIONS IN ACID LIQUID AMMONIA M. H. Miles and C. A. Yates J. ELECTROCHEM. SOC., 121 (1974), pp. 230-233
24. DECOMPOSITION REACTION OF CONCENTRATED LITHIUM-AMMONIA SOLUTIONS M. H. Miles and W. S. Harris J. ELECTROCHEM. SOC., 131 (1974), pp. 459-463
25. DISCUSSION OF HYDROGEN AND NITROGEN EVOLUTION REACTIONS IN ACID LIQUID AMMONIA M. H. Miles and C. A. Yates J. ELECTROCHEM. SOC., 121 (1974), pp. 1609-1610
26. HYDROGEN PRODUCTION AND AUXILIARIES M. H. Miles In Brookhaven National Laboratory Report 20040, F. J. Salzano, Ed., January 1975, pp. 23-27
27. HYDROGEN PRODUCTION AND AUXILIARIES M. H. Miles In Brookhaven National Laboratory Report 20420, F. J. Salzano, Ed., April 1975, pp. 19-23
28. HYDROGEN PRODUCTION AND AUXILIARIES M. H. Miles, M. A. Thomason, J. R. Locker, and W. E. Serafin In Second Annual Progress Report, BNL 50472, F. J. Salzano, Ed., August 1975, pp. 29-33.
29. EVALUATION OF ELECTROCATALYSTS FOR WATER ELECTROLYSIS IN ALKALINE SOLUTIONS M. H. Miles J. ELECTROANAL. CHEM., 60 (1975), pp. 89-96
30. HYDROGEN PRODUCTION BY WATER ELECTROLYSIS. METHODS FOR APPROACHING IDEAL EFFICIENCIES G. Kissel, P. W. T. Lu, M. H. Miles, and S. Srinivasan IECEC '75 RECORD (1975), pp. 1194-1198
31. ELECTROCHEMICAL AND TRANSPORT PROPERTIES OF LITHIUM CHLORIDE SOLUTIONS IN DIMETHYL SULFOXIDE-WATER AND DIMETHYL SULFOXIDE-METHANOL MIXTURES M. H. Miles, L. W. McMahon, and S. M. Nelson J. PHYS. CHEM., 79 (1975), pp. 2312-2315

32. MIXED OXIDES AS OXYGEN ELECTRODES G. Singh, M. H. Miles, and S. Srinivasan In NBS Special Publication 455, Electrocatalysis on Non-Metallic Surfaces (1976), pp. 289-296
33. EFFECT OF TEMPERATURE ON ELECTRODE KINETIC PARAMETERS FOR HYDROGEN AND OXYGEN EVOLUTION REACTIONS ON NICKEL ELECTRODES IN ALKALINE SOLUTIONS M. H. Miles, G. Kissel, P. W. T. Lu, and S. Srinivasan J. ELECTROCHEM. SOC., 123 (1976), pp. 332-336
34. DISCUSSION OF EFFECT OF TEMPERATURE ON ELECTRODE KINETIC PARAMETERS FOR HYDROGEN AND OXYGEN EVOLUTION REACTIONS ON NICKEL ELECTRODES IN ALKALINE SOLUTIONS M. H. Miles, G. Kissel, P. W. T. Lu, and S. Srinivasan J. ELECTROCHEM SOC., 123 (1976), pp. 1858-1859
35. PERIODIC VARIATIONS OF OVERVOLTAGES FOR WATER ELECTROLYSIS IN ACID SOLUTIONS FROM CYCLIC VOLTAMMETRIC STUDIES M. H. Miles and M. A. Thomason J. ELECTROCHEM. SOC., 123 (1976), pp. 1459-1461
36. ELECTROCATALYSIS OF FUEL CELL REACTIONS M. H. Miles and E. A. Klaus In Brookhaven National Laboratory Report 50652, S. Srinivasan and H. S. Isaacs, Principal Investigators, September 1977, pp. 3-9
37. THE CALCIUM ANODE IN MOLTEN NITRATE ELECTROLYTES M. H. Miles, D. A. Fine, and A. N. Fletcher NWC Technical Memo 3334, November 1977, pp. 1-20
38. THE CALCIUM ANODE IN MOLTEN NITRATE ELECTROLYTES M. H. Miles, D. A. Fine, and A. N. Fletcher J. ELECTROCHEM. SOC., 125, No. 8 (August 1978), pp. 1209-1214
39. THE OXYGEN EVOLUTION REACTION ON PLATINUM, IRIUM, RUTHENIUM AND THEIR ALLOYS AT 80°C IN ACID SOLUTIONS M. H. Miles, E. A. Klaus, B. P. Gunn, J. R. Locker, and W. E. Serafin ELECTROCHIMICA ACTA, 23 (1978), pp. 521-526
40. THE OXYGEN ELECTRODE REACTION IN ALKALINE SOLUTIONS ON OXIDE ELECTRODES PREPARED BY THE THERMAL DECOMPOSITION METHOD M. H. Miles and Y. H. Huang J. ELECTROCHEM. SOC., 125 (December 1978), pp. 1931-1934
41. THERMAL BATTERY CELLS BASED UPON MOLTEN NITRATE SALTS M. H. Miles and A. N. Fletcher China Lake Technical Newsletter #3, 21 February 1979
42. DISCUSSION OF DISCHARGE REACTION MECHANISMS IN Li/SOC12 CELLS M. H. Miles J. ELECTROCHEM. SOC., 126 (1979), pp. 2168-2169
43. THE EFFECT OF TEMPERATURES ON THE CATHODIC BEHAVIOR OF SOME FUSED NITRATE SALTS A. N. Fletcher, M. H. Miles, and M. L. Chan J. ELECTROCHEM. SOC., 126 (1979), pp. 1496-1501
44. THERMAL BATTERY CELLS UTILIZING MOLTEN NITRATES AS THE ELECTROLYTE AND OXIDIZER M. H. Miles and A. N. Fletcher J. APPL. ELECTROCHEM., 10 (1980), pp. 251-260
45. CATION EFFECTS ON THE ELECTRODE REDUCTION OF MOLTEN NITRATES M. H. Miles and A. N. Fletcher J. ELECTROCHEM. SOC., 127 (1980), pp. 1761-1766
46. PROTECTIVE FILM EFFECTS ON THE CALCIUM ANODE IN OXIDIZING MOLTEN SALTS M. H. Miles EXTENDED ABSTRACTS, 80-1 (1980), pp. 103-105
47. DISCUSSION OF HIGH RATE DISCHARGE CHARACTERISTICS OF Li/SOC12 CELLS M. H. Miles J. ELECTROCHEM. SOC., 127 (1980), pp. 2651-2652
48. THE BREAKDOWN OF PASSIVE FILMS ON ACTIVE METAL ANODES IN OXIDIZING MOLTEN SALTS BY HALIDE IONS M. H. Miles J. APPL. ELECTROCHEM., 11 (1981), pp. 325-333
49. THE ELECTOCHEMICAL REDUCTION OF MOLTEN PERCHLORATE AND CHLORATE SALTS M. H. Miles and A. N. Fletcher J. ELECTROCHEM. SOC., 128 (1981), pp. 821-825
50. ELECTROCATALYSIS OF THE OXYGEN REDUCTION REACTION--ELECTROLYTE AND ANION EFFECTS S. Srinivasan, H. Olender, M. H. Miles, and E. A. Klaus EXTENDED ABSTRACTS, 81-1 (1981), pp. 1221-1222
51. DISCUSSION OF STRUCTURE-REACTIVITY RELATIONSHIPS OF METHYLATED TETRAHYDROFURANS WITH LITHIUM M. H. Miles J. ELECTROCHEM. SOC., 128 (1981), pp. 1292-1294
52. HIGH RATE DISCHARGE OF Ca/LiC104-LiNO3 THERMAL BATTERY CELLS M. H. Miles and A. N. Fletcher J. ELECTROCHEM. SOC., 128 (1981), pp. 1489-1491
53. THE SILVER ION CATHODE IN MOLTEN NITRATES AND PERCHLORATES M. H. Miles, G. E. McManis, and A. N. Fletcher EXTENDED ABSTRACTS, 81-2 (1981), pp. 1405-1407

54. THE REDUCTION OF PROPYLENE GLYCOL DINITRATE AND OTHER RELATED NITRATE ESTERS ON SILVER ELECTRODES M. H. Miles and D. A. Fine J. ELECTROANAL. CHEM., 127 (1981), pp. 143-155
55. EFFECT OF ELECTROLYTE COMPOSITION ON CATHODIC REACTIONS IN MOLTEN NITRATES M. H. Miles, G. E. McManis, and A. N. Fletcher ELECTROCHEM. SOC. EXTENDED ABSTRACTS, 82-2, (1982), pp. 548-549
56. HIGH POWER DENSITY THERMAL BATTERY TECHNOLOGY USING OXIDIZING MOLTEN SALTS G. E. McManis, M. H. Miles, and A. N. Fletcher Proceedings of the Thirtieth Power Sources Conference, THE ELECTROCHEMICAL SOCIETY (1982), pp. 39-42
57. ELECTROCHEMICAL REDUCTION OF DINITROTOLUENE ISOMERS M. H. Miles and D. A. Fine ELECTROCHEMICAL SOC. EXTENDED ABSTRACTS, 83-1 (1983), pp. 978-979
58. NEW CONCEPTS FOR THERMAL BATTERIES M. H. Miles, A. N. Fletcher, and G. E. McManis Naval Research Review, 35 (1983), pp. 19-23
59. THE LITHIUM-BORON ALLOY ANODE IN MOLTEN NITRATE ELECTROLYTES G. E. McManis, M. H. Miles, and A. N. Fletcher J. ELECTROCHEM. SOC., Vol. 131, No. 2 (February 1984) pp. 286-289
60. DISCHARGE CHARACTERISTICS OF LITHIUM COMPOSITES AND LITHIUM ALLOY ANODES IN MOLTEN NITRATE ELECTROLYTES G. E. McManis, M. H. Miles, and A. N. Fletcher In "Lithium Batteries," A. N. Dey, Editor, The Electrochemical Society, Pennington, NJ (1983)
61. ELECTRODE REACTIONS INVOLVING LEAD OR CADMIUM IONS IN MOLTEN NITRATES M. H. Miles, G. E. McManis, and A. N. Fletcher Proceedings of the Fourth International Symposium on Molten Salts, The Electrochemical Society (1984), pp. 662-675
62. PERFORMANCE AND DISCHARGE CHARACTERISTICS OF Ca/LiCl, LiNO<sub>3</sub>/LiNO<sub>3</sub>, AgNO<sub>3</sub>/Ni THERMAL BATTERY CELLS G. E. McManis, M. H. Miles and A. N. Fletcher J. ELECTROCHEM. SOC., Vol. 131, No. 2 (February 1984) pp. 283-286
63. THE REDUCTION OF PROPYLENEGLYCOL DINITRATE, NITROGLYCERIN, DINITRO-TOLUENE, AND TRINITROTOLUENE ON SILVER ELECTRODES D. A. Fine and M. H. Miles ANALYTICA CHIMICA ACTA, 153 (1983) pp. 141-147
64. VOLTAMMETRIC ANALYSIS OF ORDNANCE MATERIALS PART 1. DETECTION AND QUANTITATION OF NITRATE ESTERS AND VARIOUS NITRO COMPOUNDS IN WATER BY VOLTAMMETRY Dwight A. Fine and Melvin H. Miles NWC TP 6505, 24 pp (April 1984)
65. ELECTROCHEMICAL REDUCTION REACTIONS INVOLVING FORMIC ACID M. H. Miles, A. N. Fletcher, and G. E. McManis J. ELECTROANAL. CHEM. (1984) pp. 429-436
66. REACTIONS INVOLVING SILVER IONS AND SILVER METAL IN MOLTEN NITRATES M. H. Miles, G. E. McManis, and A. N. Fletcher J. ELECTROCHEM. SOC., Vol. 131, No. 9 (September 1984), pp. 2075-2081
67. EFFECT OF TEMPERATURE AND ELECTROLYTE COMPOSITION ON THE LITHIUM-BORON ALLOY ANODE IN NITRATE MELTS: PASSIVATING FILMS ON SOLID AND LIQUID LITHIUM M. H. Miles, G. E. McManis, and A. N. Fletcher Electrochimica Acta, Vol. 30, No. 7 (July 1985), pp. 889-897
68. ELECTROCHEMICAL CHARACTERISTICS OF SIMPLE NITRATE AMIDE MELTS AT AMBIENT TEMPERATURES G. E. McManis, A. N. Fletcher, D. E. Bliss, and M. H. Miles J. ELECTROANAL. CHEM., Vol. 190 (1985), pp. 171-183
69. THE ELECTROCHEMICAL CONVERSION OF CARBON DIOXIDE INTO METHANOL: THE FORMIC ACID REDUCTION STEP IN ACIDIC SOLUTIONS M. H. Miles, A. N. Fletcher, and G. E. McManis J. ELECTROANAL. CHEM., Vol. 190 (1985), pp. 157-170
70. DISCHARGE CHARACTERISTICS OF LITHIUM/MOLTEN NITRATE THERMAL BATTERY CELLS USING SILVER SALTS AS SOLID CATHODE MATERIALS G. E. McManis, M. H. Miles, and A. N. Fletcher J. Power Sources, Vol. 16 (1985) pp. 243-151
71. THE HIGH RATE DISCHARGE CHARACTERISTICS OF SILVER CHROMATE, SILVER MOLYBDATE AND SILVER TUNGSTATE CATHODES IN MOLTEN NITRATE ELECTROLYTES G. E. McManis, A. N. Fletcher, and M. H. Miles J. Power Sources, Vol. 15 (1985), pp. 141-156
72. DISCHARGE CHARACTERISTICS OF LITHIUM/MOLTEN NITRATE THERMAL BATTERY CELLS USING SILVER SALTS AS SOLID CATHODE MATERIALS G. E. McManis, M. H. Miles, and A. N. Fletcher J. Power Sources, Vol. 16 (1985), pp. 243-251

73. PROBLEMS ASSOCIATED WITH THE ELECTROCHEMICAL REDUCTION OF METAL IONS IN MOLTEN NITRATES M. H. Miles, G. E. McManis, and A. N. Fletcher Proceedings of the Fifth International Symposium on Molten Salts, The Electrochemical Society (1986), pp. 234-247
74. THE ELECTROCHEMISTRY OF NITRATE-AMIDE MELTS: REACTIONS OF THE CALCIUM ANODE IN ROOM TEMPERATURE NITRATE-AMIDE MELTS G. E. McManis, A. N. Fletcher, D. E. Bliss, and M. H. Miles J. APPL. ELECTROCHEM., 16 (1986), pp. 101-108
75. THE ELECTROCHEMISTRY OF NITRATE-AMIDE MELTS: REACTIONS OF NICKEL AND COPPER IN NITRATE-AMIDE MELTS G. E. McManis, A. N. Fletcher, D. E. Bliss, and M. H. Miles J. APPL. ELECTROCHEM., 16 (1986), pp. 229-238
76. THE ELECTROCHEMISTRY OF NITRATE-AMIDE MELTS: THE EFFECT OF MELT ACIDITY ON THE DISCHARGE CHARACTERISTICS OF LITHIUM ANODES IN ROOM TEMPERATURE NITRATE-AMIDE MELTS G. E. McManis, A. N. Fletcher, D. E. Bliss, and M. H. Miles ELECTROCHIM. ACTA, Vol. 31, No. 10 (1986), pp. 1271-1277
77. FACTORS AFFECTING THE DISCHARGE LIFETIME OF LITHIUM-MOLTEN NITRATE THERMAL BATTERY CELLS USING SOLUBLE CATHODE MATERIALS G. E. McManis, A. N. Fletcher, D. E. Bliss, and M. H. Miles J. APPL. ELECTROCHEM., Vol. 16, No. 5 (September 1986), pp. 636-642
78. ROOM TEMPERATURE DISCHARGE CHARACTERISTICS OF Li/NH<sub>4</sub>NO<sub>3</sub>-LiNO<sub>3</sub>-AMIDE CELLS USING SILVER SALTS AS ACTIVE CATHODE MATERIALS G. E. McManis, A. H. Fletcher, D. E. Bliss, and M. H. Miles J. Appl. Electrochem., Vol. 16 (1986), pp. 920-928
79. CATHODIC REACTIONS INVOLVING LEAD, CADMIUM, AND SILVER IONS IN NaNO<sub>3</sub>-KNO<sub>3</sub> MELTS AT 250°C M. H. Miles ELECTROCHIM. ACTA, Vol. 32, No. 2 (1987), pp. 247-254
80. PROBLEMS ASSOCIATED WITH THE ELECTROCHEMICAL REDUCTION OF METAL IONS IN LiNO<sub>3</sub>-KNO<sub>3</sub> AND LiClO<sub>4</sub>-KClO<sub>4</sub> MELTS M. H. Miles, G. E. McManis, and A. N. Fletcher J. ELECTROCHEM. SOC., Vol. 134, No. 3 (March 1987), pp. 614-620
81. CYCLIC VOLTAMMETRIC STUDIES OF NITRATO COMPLEXES OF VARIOUS METAL IONS IN MOLTEN LiNO<sub>3</sub>-KNO<sub>3</sub> AT 180°C M. H. Miles and A. N. Fletcher J. ELECTROANAL. CHEM., Vol. 221 (1987), pp. 115-128
82. THE ELECTROCHEMICAL BEHAVIOR OF A SERIES OF 1,1'-DIALKYL VIologen COMPOUNDS IN AQUEOUS SOLUTIONS M. H. Miles, R. A. Henry, and R. A. Hollins Electrochemical Society Extended Abstracts, 87-2 (1987), pp. 1968-1969
83. THERMODYNAMIC EFFECTS OF METAL IONS ON MOLTEN NITRATE ELECTRO-CHEMISTRY M. H. Miles Electrochemical Society Extended Abstracts, 87-2 (1987), pp. 2174-2175
84. ELECTROCHEMICAL STUDIES OF CARBON DIOXIDE AND SODIUM FORMATE IN AQUEOUS SOLUTIONS M. H. Miles and A. N. Fletcher In "Catalytic Activation of Carbon Dioxide," W. M. Ayers, Ed., ACS Symposium Series No. 363, American Chemical Society (1988), pp. 171-178
85. BROMINE AND CHLORINE FLUORIDES: A REVIEW R. A. Rhein and M. H. Miles NWC TP 6811, January 1988
86. BROMINE TRIFLUORIDE AS AN OXIDIZING SOLVENT FOR BATTERY APPLICATIONS K. H. Park, D. E. Bliss, D. E. Stilwell, R. F. Rhein, and M. H. Miles Electrochemical Society Extended Abstracts, 88-1 (1988), pp. 35-36
87. A COMPARISON OF LITHIUM, MAGNESIUM, AND CALCIUM ANODES IN BROMINE TRIFLUORIDE K. H. Park, D. E. Stilwell, D. E. Bliss, R. A. Hollins, and M. H. Miles Electrochemical Society Extended Abstracts, 88-2 (1988), pp. 42-43
88. ELECTROCHEMICAL STUDIES OF NEW VIologen COMPOUNDS M. H. Miles, R. A. Henry, and R. A. Hollins Electrochemical Society Extended Abstracts, 88-2 (1988), pp. 1014-1015
89. ELECTROCHEMICAL STUDIES ON ELECTRODEPOSITED TUNGSTEN TRIOXIDE FILMS D. E. Stilwell, K. H. Park, and M. H. Miles Electrochemical Society Extended Abstracts, 88-2 (1988), pp. 1016-1017
90. CHEMICAL CONVERSION OF CARBON DIOXIDE M. H. Miles and A. N. Fletcher Platinum Metals Review, Vol. 32 (1988), p. 187
91. THE DISCHARGE BEHAVIOR OF ACTIVE METAL ANODES IN BROMINE TRIFLUORIDE K. H. Park, M. H. Miles, D. E. Bliss, D. Stilwell, and R. A. Hollins J. Electrochem. Soc., Vol. 135, No. 11 (November 1988), pp. 2901-2902

92. A COMPARISON OF LITHIUM, MAGNESIUM, AND CALCIUM ANODES IN BROMINE TRIFLUORIDE K. H. Park, D. E. Stilwell, D. E. Bliss, and M. H. Miles In "Proceedings of the Symposium on Materials and Processes for Lithium Batteries," K. H. Abraham and B. B. Owens, Editors, Vol. 89-4, The Electrochemical Society, Pennington, NJ (1989), pp. 180-195
93. ELECTROCHROMIC MATERIALS FOR MISSILE APPLICATIONS M. H. Miles, D. E. Stilwell, and R. A. Hollins In "Second Annual Navy IR/IED Symposium," D. S. Eggleston, Editor, Vol. 1, Chemical Propulsion Information Agency, Laurel, MD (1989), pp. 149-156
94. ELECTROCHROMIC STUDIES OF VIOLOGEN, TUNGSTEN TRIOXIDE, POLYANILINE, AND PRUSSIAN BLUE MATERIALS M. H. Miles, D. E. Stilwell, R. A. Hollins, and R. A. Henry Electrochemical Society Extended Abstracts, Vol. 89-2 (1989), pp. 914-915
95. ELECTROCHEMICAL STUDIES OF VARIOUS METALS IN BROMINE TRIFLUORIDE K. H. Park, M. H. Miles, D. E. Stilwell, and R. A. Hollins Electrochemical Society Extended Abstracts, Vol. 89-2 (1989), pp. 128-129
96. ELECTROCHROMIC STUDIES OF VIOLOGEN, TUNGSTEN TRIOXIDE, POLYANILINE, AND PRUSSIAN BLUE MATERIALS M. H. Miles, D. E. Stilwell, R. A. Hollins, and R. A. Henry In "Proceedings of the Symposium on Electrochromic Materials," M. K. Carpenter and D. A. Corrigan, Editors, Vol. 90-2, The Electrochemical Society, Pennington, NJ (1990), pp. 137-156
97. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE COLD FUSION EFFECT M. H. Miles, K. H. Park, and D. E. Stilwell In "Proceedings of the First Annual Conference on Cold Fusion," Salt Lake City, Utah, 28-31 March 1990, pp. 328-334
98. BROMINE TRIFLUORIDE AS AN OXIDIZING SOLVENT FOR LITHIUM BATTERIES M. H. Miles and K. H. Park In "Proceedings of the 34th International Power Sources Symposium held at Cherry Hill, NJ, 25-28 June 1990, pp. 363-366
99. ELECTROCHROMIC FILMS INVOLVING PRUSSIAN BLUE AND VIOLOGEN MATERIALS M. H. Miles, D. A. Fine, and D. E. Stilwell In "Optical Thin Films III: New Developments," SPIE Proceedings, Vol. 1323 (1990), pp. 200-209.
100. THEORETICAL NEUTRON FLUX LEVELS, DOSE RATES, AND METAL FOIL ACTIVATION IN ELECTROCHEMICAL COLD FUSION EXPERIMENTS M. H. Miles and R. E. Miles J. Electroanal. Chem., Vol. 295 (1990), pp. 409-414
101. DISCHARGE AND AC IMPEDANCE STUDIES OF LITHIUM, CALCIUM, AND MAGNESIUM ANODES IN BROMINE TRIFLUORIDE M. H. Miles, D. E. Stilwell, and M. H. Miles J. Electrochem. Soc., Vol. 137 (1990), pp. 3393-3400
102. ELECTROCHEMICAL CALORIMETRIC STUDIES OF THE ELECTROLYSIS OF WATER AND HEAVY WATER (D<sub>2</sub>O) M. H. Miles, K. H. Park, and M. H. Miles J. Fusion Energy, Vol. 9 (1990), pp. 333-336
103. ELECTROCHEMICAL CALORIMETRIC EVIDENCE FOR COLD FUSION IN THE PALLADIUM-DEUTERIUM SYSTEM M. H. Miles, K. H. Park, and D. E. Stilwell J. Electroanal. Chem., Vol. 296 (1990), pp. 409-414
104. HELIUM PRODUCTION DURING THE ELECTROLYSIS OF D<sub>2</sub>O IN COLD FUSION EXPERIMENTS B. F. Bush, J. J. Lagowski, M. H. Miles, and G. S. Ostrom J. Electroanal. Chem., Vol. 304 (1991), pp. 271-278
105. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, G. S. Ostrom, B. F. Bush, and J. J. Lagowski In "II Annual Conference on Cold Fusion-Contributed Papers," T. Bressani and F. Iazzi, Editors, 29 June- 4 July 1991, p. 17
106. LITHIUM DEPOSITION/DISSOLUTION STUDIES FROM MOLTEN NITRATE SALTS G. S. Ostrom, J. H. France, and M. H. Miles in "Electrochemical Society Extended Abstracts," Vol. 91-2, 132-133, 1991
107. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS B. F. Bush, M. H. Miles, G. S. Ostrom, and J. J. Lagowski in "Electrochemical Society Extended Abstracts," Vol. 91-2, 119, 1991
108. COLD FUSION EXPERIMENTER MILES RESPONDS TO CRITIC M. H. Miles in "21st Century Science and Technology," Vol. 5, No. 1, pp. 75-80, 1992
109. ELECTROCHEMICAL STUDIES OF THE FACTORS INFLUENCING THE CYCLE STABILITY OF PRUSSIAN BLUE FILMS D. E. Stilwell, K. H. Park, and M. H. Miles J. Applied Electrochemistry, Vol. 22, No. 4, pp. 325-331, 1992

110. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski in Proceedings of the 2nd Annual Conference on Cold Fusion, Como, Italy, Italian Physical Society, Bologna, Italy, pp. 363-372, 1991
111. LITHIUM DEPOSITION/DISSOLUTION STUDIES FROM MOLTEN NITRATE SALTS G. S. Ostrom, J. H. France, and M. H. Miles Electrochemical Society Extended Abstracts, Vol. 91-2, pp. 132-133 (1991)
112. HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS B. F. Bush, M. H. Miles, G. S. Ostrom, and J. J. Lagowski Electrochemical Society Extended Abstracts, Vol. 91-2, p. 179 (1991)
113. EXPERIMENTAL EVIDENCE FOR CORRELATED HEAT AND HELIUM PRODUCTION IN COLD FUSION EXPERIMENTS M. H. Miles, B. F. Bush, G. S. Ostrom, and J. J. Lagowski in Hydrogen Storage Materials, Batteries, and Electrochemistry, D. A. Corrigan and S. Srinivasin, editor, The Electrochemical Society, pp. 287-297 (1991)
114. THERMODYNAMIC AND CALORIMETRIC PRINCIPLES IN COLD FUSION EXPERIMENTS M. H. Miles and B. F. Bush in 12th IUPAC Conference on Chemical Thermodynamics, Joint Meeting with the 47th Calorimetry Conference, 16-21 August 1992, Snowbird, Utah, Program, Abstracts, and Reports, p. 194
115. ELECTROCHEMICAL STUDIES OF THE FACTORS INFLUENCING THE CYCLE STABILITY OF PRUSSIAN BLUE FILMS D. E. Stilwell, K. H. Park, and M. H. Miles J. Applied Electrochemistry, Vol. 22, pp. 325-331 (1992)
116. SEARCH FOR ANOMALOUS EFFECTS INVOLVING EXCESS POWER, HELIUM, AND TRITIUM DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES M. H. Miles and B. F. Bush in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 189-199 (1993)
117. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS M. H. Miles and B. F. Bush in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 113-122 (1993)
118. CORRELATION OF EXCESS ENTHALPY AND HELIUM PRODUCTION DURING D<sub>2</sub>O AND H<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES in Frontiers of Cold Fusion, H. Ikegami, Editor, Universal Academy Press, Inc., Tokyo, Japan, pp. 189-199 (1993)
119. CORRELATION OF EXCESS ENTHALPY AND HELIUM PRODUCTION DURING D<sub>2</sub>O and H<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES M. H. Miles, R. A. Hollins, B. F. Bush, J. J. Lagowski, and R. E. Miles J. Electroanal. Chem., Vol. 346, pp. 99-117 (1993)
120. CALORIMETRIC PRINCIPLES AND PROBLEMS IN Pd-D<sub>2</sub>O ELECTROLYSIS M. H. Miles and B. F. Bush Proceedings of the Third International Conference on Cold Fusion, Nagoya, Japan, pp. 113-122 (1993)
121. CALORIMETRIC PRINCIPLES AND PROBLEMS IN MEASUREMENTS OF EXCESS POWER DURING Pd-D<sub>2</sub>O ELECTROLYSIS M. H. Miles, B. F. Bush, and D. E. Stilwell Journal of Physical Chemistry, Vol. 98, pp. 1948-1952 (1994)
122. ANOMALOUS EFFECTS INVOLVING EXCESS POWER, RADIATION, AND HELIUM PRODUCTION DURING D<sub>2</sub>O ELECTROLYSIS USING PALLADIUM CATHODES M. H. Miles, B. F. Bush, and J. J. Lagowski Fusion Technology, Vol. 25, pp. 478-486 (1994)
123. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM M. H. Miles and B. F. Bush In Proceedings of the Fourth International Conference on Cold Fusion (ICCF-4) Lahaina, Hawaii, U.S.A., December 6-9, 1993.
124. PRACTICAL ASPECTS OF HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM B. F. Bush and M. H. Miles in "Cold Fusion source Book," Hal Fox, Editor, Fusion Information Center, Salt Lake City, UT (1994)
125. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM SYSTEMS M. H. Miles and B. F. Bush in Proceedings of International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Belarus, 1994, pp. 349-356
126. NUCLEAR PRODUCTS COMMENSURATE WITH ENERGY GENERATED DURING D<sub>2</sub>O ELECTROLYSIS AT PALLADIUM CATHODES: QUANTITATIVE ANALYSIS B. F. Bush, J. J. Lagowski, and M. H. Miles in ACS Book of Abstracts, Division of Nuclear Chemistry and Technology, Abstract No. 45, April 2-6, 1995
127. CORRELATIONS OF EXCESS POWER AND HELIUM PRODUCTION M. H. Miles and K. B. Johnson in ACS Book of Abstracts, Division of Nuclear Chemistry and Technology, Abstract No. 59, April 2-6, 1995

128. HEAT AND HELIUM MEASUREMENTS IN DEUTERATED PALLADIUM M. H. Miles and B. F. Bush  
Transactions of Fusion Technology, Vol. 26, pp. 156-159, December 1994
129. THE EXTRACTION OF INFORMATION FROM AN INTEGRATING OPEN CALORIMETER IN  
FLEISCHMANN-PONS EFFECT EXPERIMENTS M. H. Miles Proceedings of the Fifth International  
Conference on Cold Fusion, Monte Carlo, Monaco, April 9-13., 1995, pp. 97-104
130. ANOMALOUS EFFECTS IN DEUTERATED SYSTEMS Melvin H. Miles, Benjamin F. Bush, and Kendall B.  
Johnson NAWCWPNS TP 8302, Sept 1996
131. ELECTROCHEMICAL INSERTION OF HYDROGEN INTO METALS AND ALLOYS Melvin H. Miles and  
Kendall B. Johnson Journal of New Energy, Vol. 1, No. 2, pp. 32-36, 1996
132. HEAT AND HELIUM MEASUREMENTS USING PALLADIUM AND PALLADIUM ALLOYS IN HEAVY  
WATER M. H. Miles, K. B. Johnson, and M. A. Imam Progress in New Hydrogen Energy, M. Okamoto,  
Editor, Vol. 1, Japan, 1996, pp. 20-28
133. ELECTROCHEMICAL LOADING OF HYDROGEN AND DEUTERIUM INTO PALLADIUM AND  
PALLADIUM-BORON ALLOYS M. H. Miles and K. B. Johnson Progress in New Hydrogen Energy, M.  
Okamoto, Editor, Vol. 1, Japan, 1996, pp. 208-212
134. IMPROVED, OPEN-CELL, HEAT CONDUCTION, ISOPERIBOLIC CALORIMETRY M. H. Miles and K. B.  
Johnson Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 496-501
135. REPLY TO S. E. JONES AND L. D. HANSON CONCERNING CLAIMS OF MILES, ET AL. IN PONS-  
FLEISCHMANN-TYPE COLD FUSION EXPERIMENTS M. H. Miles Progress in New Hydrogen Energy,  
M. Okamoto, Editor, Vol. 2, Japan, 1996, pp. 524-527
136. NUCLEAR PRODUCTS ASSOCIATED WITH THE PONS AND FLEISCHMANN EFFECT: HELIUM  
COMMENSURATE TO HEAT GENERATION, CALORIMETRY, AND RADIATION B. F. Bush, J. J.  
Lagowski, and M. H. Miles Progress in New Hydrogen Energy, M. Okamoto, Editor, Vol. 2, Japan, 1996, pp.  
622-626
137. CORROSION INHIBITION OF ALUMINUM ALLOYS COATED WITH POLY(2,5-bis(N-METHYL-N-  
ALKYLAMINO) PHENYLENE VINYLENES P. Zarras, J. D. Stenger-smith, and M. H. Miles in PMSE-  
ACS, Vol. 76, 1997, pp. 589-590
138. REPLY TO "EXAMINATION OF CLAIMS OF MILES ET AL. IN PONS-FLEISCHMANN-TYPE COLD  
FUSION EXPERIMENTS" M. H. Miles J. Phys. Chem. B, Vol. 102, 1998, pp. 3642-3646
139. OPTICAL AND ELECTRONIC MATERIALS FOR NAVAL AVIATION J. D. Stenger-Smith, P. Zarras, M. H.  
Miles, R. A. Hollins, A. P. Chafin, And G. A. Lindsay Society for the Advancement of Materials and Process  
Engineering, May 1997
140. ANOMALOUS HEAT AND HELIUM PRODUCTION USING PALLADIUM-BORON ALLOYS IN HEAVY  
WATER M. H. Miles, K. B. Johnson, and M. A. Imam Proceedings of 32nd Intersociety Energy Conversion  
Engineering Conference, 1997, pp. 1334-1337
141. THE EFFECT OF PASSIVATING FILMS INVOLVING THE LITHIUM ANODE IN THIONYL CHLORIDE,  
BROMINE TRIFLUORIDE, MOLTEN NITRATES, AND MOLTEN PERCHLORATES M. H. Miles  
Proceedings of 32nd Intersociety Energy Conversion Engineering Conference, 1997, pp. 1334-1337
142. ELECTROCHEMICAL CALORIMETRIC STUDIES OF PALLADIUM AND PALLADIUM ALLOYS IN  
HEAVY WATER M. H. Miles NEDO Final Report, Tokyo, Japan, March 31, 1998
143. CORROSION INHIBITION OF ALUMINUM ALLOYS COATED WITH POLY(2,5-BIS(N-METHYL-N-  
ALKYLAMINO)PHENYLENE VINYLENE)S P. Zarras, J. D. Stenger-Smith, And M. H. Miles ACS PMSE  
Preprints 76, 789, 1997
144. INTRINSICALLY ELECTRICALLY CONDUCTING POLYMERS AS CORROSION INHIBITING  
COATINGS J. D. Stenger-Smith, P. Zarras, M. Miles, and J. Nelson NAWCWPNS TP 8364, April 1998
145. REVIEW OF POLY(BIS-ALKYLAMINO-P-PHENYLENE VINYLENE)S AS CORROSION INHIBITING  
MATERIALS J. D. Stenger-Smith, P. Zarras, G. Ostrom, M. Miles Submitted as an American Chemical  
Society Symposium Series Chapter, April 1998
146. EXCESS HEAT MEASUREMENTS AND NUCLEAR DETECTION EXPERIMENTS IN THE NHE  
PROGRAM K. Matsui, N. Asami, M. Sumi, H. Kamimura, A. Kubota, R. Shimada, S. Miyashita, A. Lipson,  
P. Tripodi, M. Miles and H. Watanabe The Seventh International Conference on Cold Fusion, Program  
Manual and Abstracts, 1998, p. 96

147. RADIATION MEASUREMENTS AT CHINA LAKE: REAL OR ARTIFACTS? M. H. Miles and B. F. Bush  
 Proceedings of the Seventh International Conference on Cold Fusion, Vancouver, British Columbia, Canada,  
 April 19-24, 1998, pp. 236-240
148. NEW MAIN-CHAIN POLY(CROWN ETHER) MEMBRANES FOR LITHIUM-ION TRANSPORT P. Zarras,  
 S. Fallis, M. H. Miles, R. Y. Yee, L. H. Merwin and G. A. Lindsay Polymer Materials Science and  
 Engineering, Vol. 80, Spring Meeting, Anaheim, Calif., March 21-25, 1999, pp. 612-613
149. CALORIMETRY OF THE Pd+D CO-DEPOSITION S. Szpak, P. A. Mosier-Boss and M. H. Miles Fusion  
 Technology, Vol. 36, 1999, pp. 234-241
150. LITHIUM BATTERIES USING MOLTEN NITRATE ELECTROLYTES M. H. Miles Proceedings of the  
 Fourteenth Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15,  
 1999, pp. 39-42
151. NEW MAIN-CHAIN (CROWN ETHER) MEMBRANE FOR LITHIUM-ION BATTERIES G. A. Lindsay, M.  
 H. Miles, P. Zarras, S. Fallis, R. Y. Yee, A. P. Chafin, and L. H. Merwin Proceedings of the Fourteenth  
 Annual Battery Conference on Applications and Advances, Long Beach, Calif., January 12-15, 1999
152. A REVIEW OF MAIN CHAIN POLY (CROWN ETHER) MEMBRANES FOR LITHIUM-ION TRANSPORT  
 G. A. Lindsay, P. Zarras, S. Fallis, M. H. Miles, L. H. Merwin, and R. Y. Yee Polymer News, 1999
153. IMPROVED THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES M.H. Miles 35th ACS  
 Western Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 259.
154. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD/Pt ELECTROLYSIS CELLS M.H. Miles 35th ACS Western  
 Regional Meeting, 37th SAS Pacific Conference, October 6-8, 1999, Abstract No. 255
155. CALORIMETRIC STUDIES OF Pd/D<sub>2</sub>O+LIOD ELECTROLYSIS CELLS M.H. Miles J. Electroanal. Chem.,  
 Vol. 482, pp. 56-65, 2000
156. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN M.H. Miles  
 Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, Conference Proceedings, Italian  
 Physical Society, October 22-24, 1999
157. CALORIMETRIC STUDIES AT THE NEW HYDROGEN ENERGY LABORATORY IN JAPAN M.H. Miles  
 Bulletin of The American Physical Society, Series II, Vol. 45, No. 1, March 2000, Abstract C32
158. "MY IMPRESSIONS AS A NEDO GUEST RESEARCHER AT THE NHE LABORATORY IN JAPAN" M.H.  
 Miles Infinite Energy, Vol. 5, Issue 30, pp. 18-21, 2000
159. REPORT ON CALORIMETRIC STUDIES AT THE NHE LABORATORY IN SAPPORO, JAPAN M.H. Miles  
 Infinite Energy, Vol. 5, Issue 30, pp. 22-25, 2000
160. CALORIMETRIC STUDIES OF PALLADIUM ALLOY CATHODES USING FLEISCHMANN-PONS  
 DEWAR TYPE CELLS M.H. Miles Proceedings of the 8th International Conference on Cold Fusion, Lerici  
 (La Spezia), Italy, 21-26 May 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings,  
 Vol. 70, pp. 97-104
161. CASE STUDIES OF TWO EXPERIMENTS CARRIED OUT WITH THE ICARUS SYSTEMS M.H. Miles,  
 M.A. Imam, and M. Fleischmann Proceedings of the 8th International Conference on Cold Fusion, Lerici (La  
 Spezia), Italy, 21-26 May, 2000, F. Scaramuzzi, Editor, Italian Physical Society, Conference Proceedings,  
 Vol. 70, pp. 105-119.
162. LITHIUM THERMAL BATTERIES USING MOLTEN NITRATE ELECTROLYTES M.H. Miles Proceedings  
 of the 39th Power Sources Conference, June 12-15, 2000 pp. 560-563
163. ANOMALOUS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM M.H. Miles,  
 M.A. Imam, and M. Fleischmann Proceeding Proceedings of 8th Russian Conference on Cold Nuclear  
 Transmutation (RCCNT-8), Sochi, Russia, October 4-11, 2000, pp. 142-162.
164. EXCESS HEAT AND HELIUM PRODUCTION IN THE PALLADIUM-BORON SYSTEM M.H. Miles, M.A.  
 Imam, and M. Fleischmann Transactions of the American Nuclear Society and the European Nuclear  
 Society, Washington, D.C., 12-16 November, 2000, C.A. Yoelin, Editor, Vol. 83, pp. 371-372.
165. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B  
 ALLOY CATHODE M.H. Miles, M. Fleischmann, and M.A. Imam Naval Research Laboratory Report  
 NRL/MR/6320-01-8526, March 26, 2001, 155 pp.
166. CALORIMETRY OF Pd-D CODEPOSITION IN A FLEISCHMANN-PONS DEWAR CELL M.H. Miles, S.  
 Szpak, P. Mosier-Boss, and M. Fleischmann Bulletin of the American Physical Society, Seattle, Washington,  
 12-16 March, 2001, Abstract 5403.

167. EXAMINATION OF LINEAR POTENTIAL SWEEP METHODS FOR DETERMINING THE CAPACITANCE OF HYDROUS RUTHENIUM OXIDE MATERIALS M.H. Miles, T.J. Groshens, and C.E. Johnson 2001 Joint International Meeting of the Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 89.
168. ELECTROCHEMISTRY OF MOLTEN NITRATE ELECTROLYTES AND APPLICATIONS FOR HIGH VOLTAGE LITHIUM CELLS M.H. Miles 2001 Joint International Meeting of The Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 125.
169. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE M.H. Miles, M.A. Imam, and M. Fleischmann 2001 Joint International Meeting of the Electrochemical Society/International Society of Electrochemistry, San Francisco, CA, 2-7 September 2001, Abstract No. 811.
170. RECENT ADVANCES IN LITHIUM BATTERY TECHNOLOGY M.H. Miles 23rd Annual GaAs IC Symposium, Baltimore, MD, 21-24 October 2001, Technical Digest 2001, IEEE 01CH37191, pp. 219-222.
171. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING A Pd-B ALLOY CATHODE M.H. Miles, M.A. Imam, and M. Fleischmann In "Energy and Electrochemical Processes For a Cleaner Environment", C. Cominellis, M. Doyle and J. Winnick, Editors, The Electrochemical Society, Proceedings Vol. 2001-23, 2001, pp. 194-205.
172. EXAMINATION OF LINEAR POTENTIAL SWEEP METHODS FOR DETERMINING THE CAPACITANCE OF HYDROUS RUTHERNIUM OXIDE MATERIALS M.H. Miles, T.J. Groshens, and C.E. Johnson In "Advanced Batteries and Supercapacitors", The Electrochemical Society, Proceedings Vol. 2001-21, 2001.
173. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM. VOL. I: A DECADE OF RESEARCH AT NAVY LABORATORIES M.H. Miles, "Chapter 3: Excess Heat and Helium Production in Palladium and Palladium Alloys" in SPAWAR System Center San Diego, Technical Report 1862, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 19-30.
174. THERMAL AND NUCLEAR ASPECTS OF THE Pd/D<sub>2</sub>O SYSTEM, VOL. II: SIMULATION OF THE ELECTROCHEMICAL CELL (ICARUS) CALORIMETRY M.H. Miles, "Chapter 4: Analysis of Experiment MC-21: A Case Study" in SPAWAR System Center San Diego, Technical Report 1862, S. Szpak and P.A. Mosier-Boss, Editors, February 2002, pp. 31-89.
175. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga American Physical Society 2002 Meeting, Indianapolis, Indiana, March 18-22, 2002.
176. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga Ninth International Conference on Cold Fusion, Abstracts, Beijing, China, May 19-24, 2002, pp. 77-78.
177. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO-DEPOSITION S. Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann Ninth International Conference on Cold Fusion, Abstracts, Beijing, China, May 19-24, 2002, pp. 77-78.
178. CALORIMETRIC ANALYSIS OF A HEAVY WATER ELECTROLYSIS EXPERIMENT USING Pd-B ALLOY CATHODE M.H. Miles, M.A. Imam, and M. Fleischmann In "Batteries and Supercapacitors", G. Abba-Nazi, E. Takeuchi, R. Koetz, and B. Scrosati, Editors, The Electrochemical Society, Proceedings Volume 2001-21, 2002, pp. 795-806.
179. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER M.H. Miles Bulletin of the American Physical Society, Vol. 48, No. 1, Part II, Abstract Z334, p. 1382 (2003).
180. THE ELEVATION OF BOILING POINTS IN H<sub>2</sub>O AND D<sub>2</sub>O ELECTROLYTES M.H. Miles, H.D. Arman, J.D. Carrick, C.K. Gren, K.A. Haggerty, H.Y. Kim, A.G. Ky, J.E. Markham, C.F. Meeks, and D.E. Noga In Condensed Matter Nuclear Science, Proceedings of the 9th International Conference on Cold Fusion. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 246-249.
181. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CO-DEPOSITION M.H. Miles, S. Szpak, P.A. Mosier-Boss, and M. Fleischmann In Condensed Matter Nuclear Science, Proceedings

- of the 9th International Conference on Cold Fusion. Xing Z. Li, Editor, Tsinghua University Press, Beijing, China, 2003, pp. 250-254.
182. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER M.H. Miles ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No. M012.
  183. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW M.H. Miles ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No. M015(2).
  184. THE "INSTRUMENT FUNCTION" OF ISOPERIBOLIC CALOMETERS; EXCESS ENTHALPY GENERATION DUE TO THE PARASITE REDUCTION OF OXYGEN" M. Fleischmann and M.H. Miles ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract TH 12
  185. A CALORIMETRIC INVESTIGATION OF THE Pd/B SYSTEM M.H. Miles, M.A. Imam, and M. Fleischmann ICCF-10, 10th International Conference on Cold Fusion, August 24-29, 2003, Abstract No. M015.
  186. THERMAL BEHAVIOR OF POLARIZED Pd/D ELECTRODES PREPARED BY CODEPOSITION S.Szpak, P.A. Mosier-Boss, M.H. Miles and M. Fleischmann *Thermochimica Acta*, 410, 101 (2004).
  187. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW M.H. Miles Proceedings of the 10th International Conference on Cold Fusion (ICCF-10), Cambridge, MA USA, August 24-29, 2003, Abstract.
  188. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER M.H. Miles Proceedings of the 10th International Conference on Cold Fusion (ICCF-10), Cambridge, MA USA, August 24-29, 2003, Abstract.
  189. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART I: THE Pt/D<sub>2</sub>O BLANK SYSTEM M. Fleischmann and M.H. Miles *Thermochimica Acta* (in preparation).
  190. THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART II: THE Pd-B AND Pd-B-Ce SYSTEMS M. Fleischmann, M.H. Miles and M.A. Imam SPARWAR System center San Diego, Technical Report (in preparation).
  191. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART III: THE Pd-D CODEPOSITION SYSTEM M. Fleischmann, M.H. Miles, S. Szpak and P.A. Mosier-Boss SPARWAR System center San Diego, Technical Report (in preparation).
  192. OUR PENULTIMATE PAPERS ON THE ISOPERIBOLIC CALORIMETRY OF THE Pt/D<sub>2</sub>O AND Pd/D<sub>2</sub>O SYSTEMS. PART IV: AN EXPERIMENT WITH A Pd-CATHODE IN 0.1 M LiOD/D<sub>2</sub>O CARRIED OUT IN 1989. M. Fleischmann, M.H. Miles SPARWAR System center San Diego, Technical Report (in preparation).
  193. EXPLORATION OF MOLTEN HYDROXIDE ELECTROCHEMISTRY FOR THERMAL BATTERY APPLICATIONS M.H. Miles *J. Applied Electrochem.*, Vol. 33, pp. 1011-1016, 2003.
  194. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER M.H. Miles *Bulletin of the American Physical Society*, Austin, Texas, March 2003, Abstract.
  195. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW" M.H. Miles *Bulletin of American Physical Society*, Montreal, Canada, March 2004, Abstract
  196. SIMULTANEOUS EXCESS POWER AND ANOMALOUS RADIATION M.H. Miles *Bulletin of the American Physical Society*, Los Angeles, CA, March 2005, Abstract
  197. ISOPERIBOLIC CALORIMETRY APPLIED TO THE Pt / D<sub>2</sub>O BLANK SYSTEM M. Fleischmann and M.H. Miles *Bulletin of the American Physical Society*, Baltimore, MD, March, 2006, Abstract
  198. FLUIDIZED BED EXPERIMENTS USING PLATINUM AND PALLADIUM PARTICLES IN HEAVY WATER M.H. Miles in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 23-28, 2006
  199. CORRELATION OF EXCESS ENTHALPY AND HELIUM-4 PRODUCTION: A REVIEW M.H. Miles in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 123-131, 2006
  200. THE INSTRUMENT FUNCTION OF ISOPERIBOLIC CALORIMETERS: EXCESS ENERGY GENERATION DUE TO THE PARASITIC REDUCTION OF OXYGEN in M. Fleischmann and M.H. Miles in "Condensed Matter Nuclear Science", P.I. Hagelstein and S.R. Chubb, Editors, World Scientific, New Jersey, pp. 247-268, 2006

#### A4. Publications on Jed Rothwell's LENR-CANR.org

\*Original Order on LENR-CANR.org

No*	Author	Year	Title
100	Miles, M.	1990	Miles, M., K.H. Park, and D.E. Stilwell, Electrochemical calorimetric evidence for cold fusion in the palladium-deuterium system. <i>J. Electroanal. Chem.</i> , 1990. 296: p. 241.
110	Miles, M.	1990	Miles, M., K.H. Park, and D.E. Stilwell. Electrochemical Calorimetric Studies of the Cold Fusion Effect. in <i>The First Annual Conference on Cold Fusion</i> . 1990. University of Utah Research Park, Salt Lake City, Utah: National Cold Fusion Institute.
120	Miles, M.	1990	Miles, M. and R.E. Miles, Theoretical neutron flux levels, dose rates, and metal foil activation in electrochemical cold fusion experiments. <i>J. Electroanal. Chem.</i> , 1990. 295: p. 409.
700	Stilwell, D. E.	1990	Stilwell, D.E., K.H. Park, and M. Miles, Electrochemical Calorimetric Studies on the Electrolysis of Water and Heavy Water (D2O). <i>J. Fusion Energy</i> , 1990. 9(3): p. 333.
20	Bush, B. F.	1991	Bush, B.F., et al., Helium production during the electrolysis of D2O in cold fusion experiments. <i>J. Electroanal. Chem.</i> , 1991. 304: p. 271.
130	Miles, M.	1991	Miles, M., et al. Heat and Helium Production in Cold Fusion Experiments. in <i>Second Annual Conference on Cold Fusion, "The Science of Cold Fusion"</i> . 1991. Como, Italy: Societa Italiana di Fisica, Bologna, Italy.
140	Miles, M.	1992	Miles, M. and B.F. Bush. Calorimetric Principles and Problems in Pd-D2O Electrolysis. in <i>Third International Conference on Cold Fusion, "Frontiers of Cold Fusion"</i> . 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.
150	Miles, M.	1992	Miles, M. and C.P. Jones, Cold fusion experimenter Miles responds to critic. <i>21st Century Sci. &amp; Technol.</i> , 1992. Spring: p. 75.
160	Miles, M.	1992	Miles, M. and B.F. Bush. Search for Anomalous Effects Involving Excess Power and Helium During D2O Electrolysis Using Palladium Cathodes. in <i>Third International Conference on Cold Fusion, "Frontiers of Cold Fusion"</i> . 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.
170	Miles, M.	1993	Miles, M., et al., Correlation of excess power and helium production during D2O and H2O electrolysis using palladium cathodes. <i>J. Electroanal. Chem.</i> , 1993. 346: p. 99.
180	Miles, M.	1993	Miles, M. and B.F. Bush. Heat and Helium Measurements in Deuterated Palladium. in <i>Fourth International Conference on Cold Fusion</i> . 1993. Lahaina, Maui: Electric Power Research Institute 3412 Hillview Ave., Palo Alto, CA 94304.
190	Miles, M.	1993	Miles, M., Letter to Steven E. Jones. 1993.
30	Bush, B. F.	1994	Bush, B.F. and M. Miles. Practical Aspects of Heat and Helium Measurements in Deuterated Palladium. in <i>International Symposium on Cold Fusion and Advanced Energy Sources</i> . 1994. Belarusian State University, Minsk, Belarus: Fusion Information Center, Salt Lake City.
200	Miles, M.	1994	Miles, M., B.F. Bush, and J.J. Lagowski, Anomalous effects involving excess power, radiation, and helium production during D2O electrolysis using palladium cathodes. <i>Fusion Technol.</i> , 1994. 25: p. 478.

210	Miles, M.	1994	Miles, M., B.F. Bush, and D.E. Stilwell, Calorimetric principles and problems in measurements of excess power during Pd-D <sub>2</sub> O electrolysis. <i>J. Phys. Chem.</i> , 1994. 98: p. 1948.
220	Miles, M.	1994	Miles, M. and B.F. Bush, Heat and Helium Measurements in Deuterated Palladium. <i>Trans. Fusion Technol.</i> , 1994. 26(4T): p. 156.
230	Miles, M.	1995	Miles, M. The Extraction of Information From an Integrating Open Calorimeter in Fleischmann-Pons Effect Experiments. in 5th International Conference on Cold Fusion. 1995. Monte-Carlo, Monaco: IMRA Europe, Sophia Antipolis Cedex, France.
240	Miles, M.	1996	Miles, M. and K.B. Johnson, Anomalous Effects in Deuterated Systems, Final Report. 1996, Naval Air Warfare Center Weapons Division.
250	Miles, M.	1996	Miles, M. and K.B. Johnson, Electrochemical insertion of hydrogen into metals and alloys. <i>Infinite Energy</i> , 1996. 1(5 & 6): p. 68.
260	Miles, M.	1996	Miles, M., K.B. Johnson, and M.A. Imam. Electrochemical loading of hydrogen and deuterium into palladium and palladium-boron alloys. 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.
270	Miles, M.	1996	Miles, M., K.B. Johnson, and M.A. Imam. Heat and Helium Measurements Using Palladium and Palladium Alloys in Heavy Water. 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.
280	Miles, M.	1996	Miles, M. and K.B. Johnson. Improved, Open Cell, Heat Conduction, Isoperibolic Calorimetry. 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.
290	Miles, M.	1996	Miles, M. Reply to S. E. Jones and L. D. Hansen Concerning Claims of Miles, et al. in Pons-Fleischmann-Type Cold Fusion Experiments. 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.
300	Miles, M.	1998	Miles, M., Electrochemical calorimetric studies of palladium and palladium alloys in heavy water. 1998.
310	Miles, M.	1998	Miles, M. and B.F. Bush. Radiation Measurements at China Lake: Real or Artifacts? in The Seventh International Conference on Cold Fusion. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT.
320	Miles, M.	1998	Miles, M., Reply to 'An assessment of claims of excess heat in cold fusion calorimetry'. <i>J. Phys. Chem. B</i> , 1998. 102: p. 3648.
330	Miles, M.	1998	Miles, M., Reply to 'Examination of claims of Miles et al. in Pons-Fleischmann-type cold fusion experiments'. <i>J. Phys. Chem. B</i> , 1998. 102: p. 3642.
340	Miles, M.	1999	Miles, M. Production of helium in the cold. in 18th Annual Meeting of the Society for Scientific Exploration. 1999. Albuquerque, NM.
710	Szpak, S.	1999	Szpak, S., P.A. Mosier-Boss, and M. Miles, Calorimetry of the Pd+D codeposition. <i>Fusion Technol.</i> , 1999. 36: p. 234.
350	Miles, M.	2000	Miles, M., M.A. Imam, and M. Fleischmann. "Case Studies" of Two Experiments Carried Out With the ICARUS Systems. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical

			Society, Bologna, Italy.
360	Miles, M.	2000	Miles, M. Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
370	Miles, M.	2000	Miles, M., Calorimetric studies of Pd/D <sub>2</sub> O+LiOD electrolysis cells. J. Electroanal. Chem., 2000. 482: p. 56.
380	Miles, M.	2000	Miles, M., M.A. Imam, and M. Fleischmann, Excess heat and helium production in the palladium-boron system. Trans. Amer. Nucl. Soc., 2000. 83(371): p. 72.
390	Miles, M.	2000	Miles, M., Report on Calorimetric Studies at the NHE Laboratory in Sapporo, Japan. Infinite Energy, 2000. 5(30): p. 22.
400	Miles, M.	2001	Miles, M., M. Fleischmann, and M.A. Imam, Calorimetric Analysis of a Heavy Water Electrolysis Experiment Using a Pd-B Alloy Cathode. 2001, Naval Research Laboratory.
410	Miles, M.	2001	Miles, M., M.A. Imam, and M. Fleischmann, Calorimetric analysis of a heavy water electrolysis experiment using a Pd-B alloy cathode. Proc. Electrochem. Soc., 2001. 2001-23: p. 194.
420	Miles, M.	2002	Miles, M., et al. The Elevation of Boiling Points in H <sub>2</sub> O and D <sub>2</sub> O Electrolytes. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University.: Tsinghua Univ. Press.
430	Miles, M.	2002	Miles, M., et al. Thermal Behavior of Polarized Pd/D Electrodes Prepared by Co-deposition. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press.
670	Mosier-Boss, P. A.	2002	Mosier-Boss, P.A., et al., Thermal and Nuclear Aspects of the Pd/D <sub>2</sub> O System (1), ed. S. Szpak and P.A. Mosier-Boss. Vol. 1 A Decade of Research at Navy Laboratories. 2002: SPAWAR Systems Center, San Diego, U.S. Navy.
40	Fleischmann, M.	2003	Fleischmann, M. and M. Miles. The "Instrument Function" of Isoperibolic Calorimeters; Excess Enthalpy Generation due to the Parasitic Reduction of Oxygen. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
440	Miles, M.	2003	Miles, M. Correlation Of Excess Enthalpy And Helium-4 Production: A Review. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
450	Miles, M.	2003	Miles, M. Fluidized Bed Experiments Using Platinum And Palladium Particles In Heavy Water. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
460	Miles, M.	2004	Miles, M., NEDO Final Report - Electrochemical Calorimetric Studies Of Palladium And Palladium Alloys In Heavy Water. 2004, University of La Verne.
720	Szpak, S.	2004	Szpak, S., et al., Thermal behavior of polarized Pd/D electrodes prepared by co-deposition. Thermochem. Acta, 2004. 410: p. 101.
470	Miles, M.	2005	Miles, M. Simultaneous Excess Power and Anomalous Radiation (PowerPoint slides). in American Physical Society Meeting. 2005. Los Angeles.
480	Miles, M.	2007	Miles, M. and M. Fleischmann. Precision and Accuracy of Cold Fusion Calorimetry (paper and PowerPoint slides). in 233rd ACS National Meeting. 2007. Chicago, IL.
490	Miles, M.	2008	Miles, M. and M. Fleischmann, Accuracy of Isoperibolic Calorimetry Used in a Cold Fusion Control Experiment, in Low-Energy Nuclear Reactions

			Sourcebook. 2008, American Chemical Society: Washington, DC. p. 153-171.
500	Miles, M.	2008	Miles, M. and M. Fleischmann. Isoperibolic Calorimetric Measurements of the Fleischmann-Pons Effect. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
510	Miles, M.	2008	Miles, M. and M. Fleischmann. Twenty Year Review of Isoperibolic Calorimetric Measurements of the Fleischmann-Pons Effect. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
730	Szpak, S.	2008	Szpak, S., et al. LENR Research Using Co-Deposition. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
520	Miles, M.	2009	Miles, M. Investigations of co-deposition systems. in 15th International Conference on Condensed Matter Nuclear Science. 2009. Rome, Italy: ENEA.
530	Miles, M.	2009	Miles, M. and M. Fleischmann. New approaches to isoperibolic calorimetry. in 15th International Conference on Condensed Matter Nuclear Science. 2009. Rome, Italy: ENEA.
540	Miles, M.	2009	Miles, M. and M. Fleischmann. New approaches to isoperibolic calorimetry (PowerPoint slides). in 15th International Conference on Condensed Matter Nuclear Science. 2009. Rome, Italy: ENEA.
90	Marwan, J.	2010	Marwan, J., et al., A new look at low-energy nuclear reaction (LENR) research: a response to Shanahan. J. Environ. Monit., 2010. 12(9): p. 1765-1770.
550	Miles, M.	2011	Miles, M. and M. Fleischmann, Measurements of Excess Power Effects In Pd/D <sub>2</sub> O Systems Using a New Isoperibolic Calorimeter. J. Condensed Matter Nucl. Sci., 2011. 4: p. 45-55.
680	Mosier-Boss, P. A.	2011	Mosier-Boss, P.A., et al., Review of Twenty Years of LENR Research Using Pd/D Co-deposition. J. Condensed Matter Nucl. Sci., 2011. 4: p. 173-187.
50	Fleischmann, M.	2012	Fleischmann, M., et al., Experimental Evidence of Nuclear Reactions Generated in a Polarized Pd/D Lattice. 2012, LENR-CANR.org.
60	Fleischmann, M.	2012	Fleischmann, M. and M. Miles, Thermal Behavior of the Polarized Pd/D <sub>2</sub> O System. 2012, LENR-CANR.org.
560	Miles, M.	2012	Miles, M., Investigations of Possible Shuttle Reactions in Co-deposition Systems. J. Condensed Matter Nucl. Sci., 2012. 8.
570	Miles, M.	2012	Miles, M. and P.L. Hagelstein, New analysis of MIT Calorimetric Errors. J. Condensed Matter Nucl. Sci., 2012. 8.
10	Biberian, J. P.	2014	Biberian, J.P., I. Parchamazad, and M. Miles, Possible Role of Oxides in the Fleischmann-Pons Effect. J. Condensed Matter Nucl. Sci., 2014. 13.
580	Miles, M.	2014	Miles, M., Co-Deposition of Palladium and other Transition Metals in H <sub>2</sub> O and D <sub>2</sub> O Solutions. J. Condensed Matter Nucl. Sci., 2014. 13.
590	Miles, M.	2014	Miles, M., Examples of Isoperibolic Calorimetry in the Cold Fusion Controversy. J. Condensed Matter Nucl. Sci., 2014. 13.
600	Miles, M.	2015	Miles, M., Thermodynamic and Kinetic Observations Concerning the D + D Fusion Reaction for the Pd/D System. J. Condensed Matter Nucl. Sci., 2015. 16.
690	Mosier-Boss, P. A.	2015	Mosier-Boss, P.A., et al., Condensed matter nuclear reaction products observed in Pd/D co-deposition experiments. Curr. Sci., 2015. 108(4).
610	Miles, M.	2016	Miles, M. and R. Cantwell, Data from Melvin Miles' July 2016 experiment. 2016.
620	Miles, M.	2016	Miles, M., Excerpts from Martin Fleischmann Letters. J. Condensed Matter

			Nucl. Sci., 2016. 19.
630	Miles, M.	2016	Miles, M., Introduction To "A Summary of NRL Research on Anomalous Effects in Deuterated Palladium Electrochemical Systems" (published in 1996). 2016.
640	Miles, M.	2016	Miles, M. The Fleischmann-Pons Calorimetric Methods And Equations (PowerPoint slides). in Satellite Symposium of the 20th International Conference on Condensed Matter Nuclear Science. 2016. Xiamen, China.
650	Miles, M.	2017	Miles, M., The Fleischmann-Pons Calorimetric Methods, Equations and New Applications. J. Condensed Matter Nucl. Sci., 2017. 24: p. 1-14.
70	Fleischmann, M.	2018	Fleischmann, M. and M. Miles, Critique of N.H.E. Paper by Saito et al. 2018, LENR-CANR.org.
80	Fleischmann, M.	2018	Fleischmann, M. and M. Miles, Letters from Martin Fleischmann to Melvin Miles. 2018, LENR-CANR.org.
660	Miles, M.	2018	Miles, M. Excess power measurements for palladium-boron cathodes (PowerPoint slides). in The 21st International Conference for Condensed Matter Nuclear Science ICCF-21. 2018. Fort Collins, CO.

## Appendix B. Photo Identifications

---

- Photo Identifications Made by Melvin Miles
- (Note: ? Indicates Person Not Identified)

Set	Year	Description	Identifications
1	1959	Photo of Miles	1. Melvin Miles portrait photo, 1959
2	1992	ICCF-3, Nagoya, Japan	1a. Stage with banner 1b. Stage with banner 2. Dinner 3. Break 4. Registration 5. Melvin Miles at registration desk 6. ?, Ben Bush, Mahadeva Srinivasan,?, Robert Bush - Cal State, Pomona, CA) 7. Doug Morrison (center), Eugene Mallove? 8. Melvin Miles (center) 9. Melvin Miles (right) 10. ?, Dawn Dominguez, Pam Boss 11. ICCF-3 12. ICCF-3 13. ICCF-3. Talbot Chubb 14. ICCF-3 15. ICCF-3 16. ICCF-3
3	1993	Conference in Russia	1. Michael McKubre,?,?, Melvin Miles,?, Akito Takahashi,? 2. Melvin Miles on a beach 3. Michael McKubre on a beach 4. Melvin Miles. Volleyball 5. ?,?, George Miley, Michael McKubre, Akito Takahashi 6. Unknown Viger,?, Michael McKubre, Akito Takahashi?, Melvin Miles 7. Melvin Miles giving a paper 8. ?, Akito Takahashi,?,?,?,?, Michael McKubre 9. ?, Akito Takahashi, Melvin Miles, Michael McKubre 10. ?,?,?,?, Akito Takahashi,?, Michael McKubre 11. ?, Michael McKubre,?,?, Melvin Miles,?, Akito Takahashi,?? 12. Train stop 13. Melvin Miles,?, Vladimir Tsarev, Akito Takahashi 14. Melvin Miles, Vladimir Tsarev wife & mother, Vladimir Tsarev, Akito Takahashi 15. Vladimir Tsarev wife & family 16. Akito Takahashi, Vladimir Tsarev wife, Vladimir Tsarev 17. Akito Takahashi, Vladimir Tsarev 18. Unknown Karinsky and family (after meeting)

- |   |      |                                      |   |
|---|------|--------------------------------------|---|
|   |      |                                      | 19. As above  |
|   |      |                                      | 20. Bazhutov (eryzion theory), Melvin Miles, Akito Takahashi, University of Moscow. (Led the only ICCF conference in Russia)  |
|   |      |                                      | 21. Melvin Miles with Karinsky's wife and daughter  |
|   |      |                                      | 22. Melvin Miles, Akito Takahashi, Moscow University  |
|   |      |                                      | 23. Wall paintings in Vladimir Tsarev apartment   |
|   |      |                                      | 24. Akito Takahashi on beach with others  |
|   |      |                                      | 25. Unknown Karensky, Melvin Miles. Train stop.   |
| 4 | 1994 | U.S. Navy Cold Fusion Review Meeting | 1. ?,?,?,?, Dawn Dominguez,?,?,?, Melvin Miles, Ashraf imam, Michael Melich<br>2. ?,?,?, Dawn Dominguez,?,?,?, David Nagel, Michael Melich<br>3a. ?, Dawn Dominguez,?,?, David Nagle, Michael Melich behind<br>3b. As above   |
| 5 | 1995 | ICCF-5, Monte Carlo, Monaco          | 1. Melvin Miles, Talbot Chubb,?,?, Michael Melich and Wife<br>2. ?,?,?,?, Scott Chubb, Melvin Miles<br>3.?, Scott Chubb, Melvin Miles,?...<br>4. ?, Melvin Miles,?<br>5. ICCF-5<br>6. ICCF-5<br>7. ICCF-5<br>8. ICCF-5<br>9. Melvin Miles. ICCF- 5<br>10. ICCf-5, dinner<br>11. ICCF-5. dinner<br>12. ICCF-5, dinner (Michael Melich and wife?)<br>13. ICCF-5<br>14. ICCF-5,<br>15. ICCF-5, John Dash<br>16. Monaco scenery<br>17. Meeting hotel<br>18. ICCF-5.?,?, Michael Melich wife<br>19. David Nagel?, Michael Melich and wife,?, Melvin Miles,?,?<br>20. As above<br>21. Michael Melich wife, David Nagel,?, Melvin Miles<br>22. ?,?,?,?, Scott Chubb, Melvin Miles<br>23. ?, Scott Chubb, Melvin Miles,?,?,?, Michael Melich and wife,??<br>24. Melvin Miles, Michael Melich wife and Michael Melich,?<br>25. Michael Melich wife, Michael Melich,?, Melvin Miles,? ,?<br>26. Entrance to ICCF-5 hotel<br>27. Melvin Miles in front of hotel. 40 years<br>28. ?, Talbot Chubb, Michael Melich<br>29. Peter Hagelstein, others<br>30. Peter Hagelstein, others<br>31. ICCF-5<br>32. ICCF-5<br>33. Michael Melich and Martin Fleischman<br>34. ICCF-5 posters<br>35. ?, Michael McKubre,?<br>36. Scott Chubb<br>37. Martin Fleischman<br>38. Xing Li, Michael McKubre,?,?<br>39. Talbot Chubb<br>40. Talbot Chubb |

			41. Michael McKubre
			42. Julie and Roger Stringham, John Dash, Michael Melich, Jean-Paul Biberian, Melvin Miles
6	1996	ICCF-6, Lake Toya, Sapporo, Japan	<ul style="list-style-type: none"> <li>1. ?, Peter Hagelstein, Carol White (journalist?). At airport</li> <li>2a. Melvin Miles, Mahadeva Srinivasan</li> <li>2b. Melvin Miles and Mahadeva Srenivasan</li> <li>3. Melvin Miles, Xing Li, Talbot Chubb</li> <li>4. NHE Lab, Talbot Chubb</li> <li>5. NHE Lab</li> <li>6. Hotel lobby</li> </ul>
7	1997	Pirelli Research Program Attempt, Milan, Italy	<ul style="list-style-type: none"> <li>1. Del Gucci</li> <li>2. Del Gucci, Giugliano Preparata</li> <li>3. Unknown lab person</li> <li>4. Melvin Miles</li> </ul>
8	1998	New Hydrogen Energy Lab, Japan	<ul style="list-style-type: none"> <li>Part A</li> <li>1. ?,?, Linda Miles, Melvin Miles, Naoto Asami (NHE head)</li> <li>2. As above</li> <li>3. ?,?, Linda Miles, Naoto Asami</li> <li>4. Unknown</li> <li>Part B</li> <li>1. Naoto Asami, Melvin Miles, Linda Miles</li> <li>2. Group</li> <li>3. Group with Naoto Asami</li> <li>4. Group</li> <li>5. Andrei Lipson, Paolo Tripodi, Naoto Asami</li> <li>6. Dinner speech</li> <li>7. Andrei Lipson, Paolo Tripodi</li> <li>Part C</li> <li>1-11. Views of NHE Laboratory, Japan</li> <li>Part D</li> <li>1. Andre Lipson</li> <li>2. Andre Lipson</li> <li>Part E</li> <li>1-7. Fleischmann and Pons Electrolytic Cells</li> <li>Part F</li> <li>1-12. Views of NHE Lab Disassembly</li> </ul>
9	1998	ICCF-7, Vancouver, BC	<ul style="list-style-type: none"> <li>1. R. Huggins?, Peter Hagelstein</li> <li>2. NHE Person?, Martin Fleischman, Naoto Asami, Paolo Tripodi</li> <li>3. ?,?,?, Andrei Lipson,?,?, Naoto Asami</li> <li>4. ICCF-7 attendees</li> <li>5. As above</li> <li>6. As above</li> <li>7. As above (Fleischmann?)</li> <li>8. As above</li> <li>9. As above</li> <li>10. ICCF-7 poster</li> <li>11. ICCF-7</li> </ul>
10	1998	Takahashi Visit to Naval Weapons Center (Death Valley Pix)	<ul style="list-style-type: none"> <li>1. Akito Takahashi, Takahashi's wife, Linda Miles</li> <li>2. Akito Takahashi and Wife</li> </ul>
11	1998	Paulo Tripodi Visit to Naval Weapons Center (Death Valley Pix)	<ul style="list-style-type: none"> <li>1. Paolo Tripodi, Linda Miles, Friend</li> <li>2. As above</li> </ul>

			3. Paolo Tripodi, Melvin Miles, Friend
			4. Paolo Tripoli, Friend, Devils Golf Course
			5. Friend, Linda Miles, Paolo Tripodi
12	1999	Asti Meeting, Italy	1. Melvin Miles, Bill Collis, ?,?
			2. ?, Bill Collis,?,?, Francesco Celani, Melvin and Linda Miles
			3a. George Miley, Vittorio Violante, Michael McKubre
			3b. As above
			4. Tom Passell. Giuliano Preparata?
			6. George Miley, Bill Collis
			7. Truffle Award
			8. Melvin Miles with Truffle Award
			9. Michael McKubre, Jean-Paul Biberian, George Miley
			10. Pablo Tripoli, Bill Collis, Tom Passell
			11. ?, Bill Collis
			12. George Miley, Bill Collis, (count, countess)
			13. Francesco Celani,?, George Miley,?
			14. Unknown
			15. Unknown
			16. Unknown
13	1999	ACS Meeting, Ontario, CA	1. Ben Bush, Linda Miles, Talbot Chubb
14	2000	APS Meeting? ICCF-8?	1. Scott Chubb, Michael McKubre, Peter Hagelstein
			2a. Melvin Miles, George Miley,?, Talbot Chubb, Scott Chubb, Michael McKubre, Edmund Storms
			2b. As above
			3. Talbot Chubb, Edmund Storms, George Miley, Richard Oriani?
			4. Michael McKubre, Peter Hagelstein,?, Eugene Mallove
15	2000	ICCF-8, Lerici (La Spezia), Italy	1. Scott Chubb. Meeting room
			2. Meeting room
			3. Meeting room
			4. Leaning Tower of Pisa
16	2000	U.S. Navy Meeting, San Diego, CA	1. Melvin Miles, Linda Miles, Ashraf Imam
			2. Ashraf Imam, Martin Fleischmann, Melvin Miles
			3. Martin Fleischmann, Melvin Miles, Ashraf Imam
			4. Carol and Dave Nagel, Linda Miles
17	2000	Miles Office & Lab, Naval Weapons Center, China Lake, CA	1. Miles cold fusion lab in China Lake, "south hood"
			2a. China lake office
			2b. China lake office
			3. China lake, "north hood"
			4. China lake lab, overview of lab
			5. China lake library, carrell and journals
			6. China Lake office
			7. China Lake cold fusion experiment running
18	2000	"Before Weapons Meeting", San Diego, CA	Set 18
			1. Melvin Miles, Martin Fleischman
			2a. Linda Miles, Martin Fleischman
			2b. Linda Miles, Martin Fleischman
			3. China Lake lab hallway
			4. Martin Fleischmann, Ridgecrest, California
			5. Michael Melich, Martin Fleischman
			6. Michael Melich, Martin Fleischman, Ashraf Imam, Frank Gordon
			7. Melvin Miles, Martin Fleischman, Ashraf Imam, Frank Gordon
			8. Melvin Miles, Ashraf Imam, Michael Melich, Martin Fleischman

- |    |      |  |  |
|----|------|--|--|
| 19 | 2002 | ICCF-9, China                            | <ol style="list-style-type: none"> <li>1. ICCF-9, Dinner</li> <li>2. ?,?,?, Xing Li,?, George Miley,?</li> <li>3. ICCF-9</li> <li>4. Dave Nagel?, Michael Melich (on tour)</li> <li>5. Vittorio Violante, Martin Fleischman, Michael Melich, Talbot Chubb, Michael McKubre</li> <li>6. Talbot Chubb</li> <li>7. Talbot Chubb</li> <li>8. ICCF-9, banquet</li> <li>9. ICCF-9, initiation</li> <li>10. ICCF-9, initiation</li> <li>11. ICCF-9</li> <li>12. ICCF-9</li> <li>13. ICCF-9, factory tour</li> <li>14. Mahadeva Srinivasan, Martin Fleischman, Michael Melich</li> <li>15. ?, Talbot Chubb, Jean-Paul Biberian?,?</li> <li>16. ?,?, Martin Fleischmann</li> <li>17. David Nagle, Mahadeva Srinivasan, John Dash</li> <li>18. ?, Melvin Miles, Mahadeva Srinivasan, Fran Tanzella</li> <li>19. ?,?, Fran Tanzella</li> <li>20. Excursion trip</li> <li>21. Melvin Miles, Michael Meelich</li> <li>22. Excursion trip, Michael Melich, Fran Tanzella, Jed Rothwell, Peter Hagelstein</li> <li>23. Trip. Michael McKubre, Talbot Chubb</li> <li>24. Trip, Rothwell</li> </ol> |
| 20 | 2003 | ICCF-10, MIT, Cambridge, MA              | <ol style="list-style-type: none"> <li>1.?, Dennis Letts?, Graham Hubler, George Miley, Wilford Hanson</li> <li>2. Martin Fleischman, Xx Li</li> <li>3. Jean Paul Biberian, Smith?, John Dash</li> <li>4. Scott Chubb</li> <li>5. ICCF-10 Attendees</li> <li>6. Unknown</li> <li>7. Unknown</li> <li>8. Unknown</li> <li>9. Unknown</li> </ol>   |
| 21 | 2004 | EarthTech Test of Miles Cell, Austin, TX | <ol style="list-style-type: none"> <li>1. Melvin Miles in front of MOAC</li> <li>2. Dennis Letts in front of MOAC</li> <li>3. Cell and temperature control (wine) cooler</li> <li>4. Monitors in EarthTech lab</li> <li>5. View of MOAC?</li> <li>6. View of lab</li> <li>7. EarthTech office</li> </ol>   |

## Appendix C. Transcriptions of Dr. Miles Interviews

---

### C1. Round 1 Interviews, May 5-6, 2019

#### Interview 1-1

Tom Grimshaw: Okay. This is Tom Grimshaw. I'm here with Melvin Miles. We're at his home near St. George, Utah. It's May 6, 2019, and we're at his home, as I said, and we're going to talk about his experience with cold fusion, his personal experience, and Melvin, as I mentioned ... And again for the transcribers, I'm Tom Grimshaw and this is Melvin Miles.

Melvin, why don't you start by telling us a little bit about your background before the cold fusion announcement, and then we'll get right into what you were doing or the U.S. Navy at the time of the announcement in 1989 and why it was you were interested.

Before we get to that, can you tell us a little bit about your professional background and how you got to where you were in 1989, please?

Melvin Miles: Okay. Starting out, I got a PhD at the University of Utah working with Henry [Arring 00:01:16] on rate theory, and his son, Edward Arring, on experimental studies on fast reactions in solution. That was my PhD thesis. Then I went to Germany and got into electrochemistry with Dr. Hines [Gerisher 00:01:35] in Munich, Germany, working on a zinc electrode reaction. After that, I went to a Navy laboratory near Corona, California, Naval Ordinance Laboratory Corona and worked a couple years on batteries, and then that lab was going to close, so I got a job in Tennessee, Middle Tennessee State University, teaching and doing some electrochemistry research on the side. Because I knew people at China Lake, I would work summers at China Lake. And finally, they wanted to hire me for a thermal battery program. So I moved to China Lake in 1978, I think, 1978 and began research at China Lake as a research electrochemist.

At the time of the cold fusion announcement in 1989, I first heard about it driving to work on the radio because I listened to the radio. I hadn't seen anything before then, but I heard the announcement talking about it on the radio. At that time, I was already working with palladium as a reference electrode by loading it with hydrogen, and it would form a reference potential. I was using that for studies of carbon dioxide electrochemical reduction, which is still of great interest today, fixing carbon dioxide. So I was already working with palladium as a reference electrode, already working with hydrogen. The only thing is I needed heavy water, and that was just across the lab in the [inamore 00:03:20] room. They had bottles of heavy water. So I had everything I needed.

I went in that very Saturday, just a couple days later, and started preliminary research. Didn't really know much about what I was doing, but I was trying to measure the energy going in and the energy coming out, basically. The trouble is that what I was doing at that time, I couldn't even come up with a one to one ratio. There was less energy coming out than was going in. So that's where I started on cold fusion research trying to figure out how

to measure heat, doing [calirimfy 00:03:57], that was a big challenge, to find a good way to doing that, accurate way of doing that.

Tom Grimshaw: Okay. Good. Well, we'll pick up the cold fusion story in a moment. I'm interested in knowing a little bit about the work that you were doing on the batteries and why you were using palladium electrodes as a reference. Tell us a little bit about the research you were doing, the objectives, the experiments, and the materials you were using leading up to 1989, please.

Melvin Miles: Well, my work at China Lake really started with thermal batteries. That's ... Every missile has a thermal battery. That's a battery that you can put in a missile, and it can have a shelf life of 20 years because it's a solid electrolyte. You can suddenly heat it up and melt the electrolyte like lithium chloride potassium chloride. It'll start operating as soon as you can melt it and becomes ionic conducting, but we wanted a lower temperature melting electrolyte. Lithium chloride potassium chloride melts at 352 degrees Celsius, and molten nitrate mixtures, they melt at 124 degrees Celsius. So it would be a low temperature, still go above the boiling point of water, 124 degrees Celsius, but it'd have a lot of advantages in that it would be low melting. It would melt to a liquid at 124 degrees Celsius.

This led to other project. That ended after about five or six years, and then I went on to other things, like electrochromics, things that change color by electrochemistry, like windows you can darken or lighten windows or rear view mirrors can be darkened or lightened by electrochemistry.

then another project was carbon dioxide reduction, and that's where I got into using palladium as a reference electrode because you load it with hydrogen and it would remain at the same potential for a day or so. You can do your studies with it. Working in things out of their water, you needed some other type of reference electrode. Palladium loaded with hydrogen was a good reference electrode for that kind of work.

Tom Grimshaw: Okay. Good. So we usually go for 30 minutes. This has been just about six and a half minutes, but I want to stop us here and go on to session number two just to make sure that everything's working okay so we don't wind up with a big blank tape here. So ...

## ***Interview 1-2***

Tom Grimshaw: Okay, Tom Grimshaw here again, this is session number two. I'm here with Melvin Miles. It's May 6th. We're at his home near Saint George, Utah, and we're talking about the beginnings of Melvin's involvement with the cold fusion field. So, why don't you pick up where you left off, Melvin, in the last session. You had just told us what you were doing up to the point where you first heard about cold fusion on the radio. If you don't mind, pick it up there again and just kind of repeat what you said before and go from there.

Melvin Miles: Well, after I heard the announcement, my first thought was, that was really a good idea, and why didn't I think of it? Because I was already working with palladium and hydrogen, and I knew palladium would absorb a lot of hydrogen. And I had almost everything there that I needed, the palladium, the electrochemical instrumentation, the heavy water across the hallway. And so, I thought I ought to take a look at it. But I did on a weekend and went in that weekend and other following weekends.

And, soon after that, my post doc working with me, David Stilwell, got very interested in it. And he wanted to switch what he was doing and focus on developing calorimetry. So,

we had a lot of discussion how to do calorimetry. We tried a lot of different things, and we finally got to where we could break even, the heat coming out would equal the heat going in.

Within, I think, our first set of experiments we didn't show any excess heat, but we got the ratio of heat out to heat in down to 1.00 plus or minus 0.04. And looking back, that palladium would never have given excess heat because it did not load very well, we found out later. It was just what we had in the laboratory at the time. It was from a sort of unknown company called Westco in California. And we didn't know until later that you only see this effect with certain types of palladium made in special ways. We didn't know that at the time.

And so, the first six months we did all sorts of studies, but we used the same palladium in every study and we always got heat out equals heat in. And it wasn't until that fall, about six months later, I guess, September, 1989 we placed the order to Johnson Matthey for a palladium rod six millimeter diameter, pretty thick rod, a lot thicker than what we were using. We were using about a one millimeter palladium wire at the time. And, when we got that in, we had developed the calorimetry pretty well up to that point, and we set up the experiment, and we didn't see anything at first. We saw heat out equals heat in. But, after a week or two, we started seeing more heat out than we were getting heat in. And that was our first sign of any excess heat effect. And that was six months later.

Caltech had already reported, there's nothing to it. MIT, Hardwell, public scientific opinion was kind of negative on cold fusion. That it was not the probably there, but that's about the time we started seeing something. And we did further studies, and first reported this at ICCF-1 in Salt Lake City. And I remember, after my talk, Fleischmann Pons came up to me. That's the first time I met him. And my paper was towards the end of the meeting on a Saturday. And he came up and said what I reported was exactly pretty close to what they had seen, and they were congratulating me on my talk. That was my first meeting was Stan Pons, following my talk in Salt Lake City.

And then, this led to further studies. I repeated that study for that same palladium I think three or four different times. Every time I got excess heat, and it seemed to be reproducible. So, I thought we could do other things with that. This led to my talking to Ben Bush on the telephone who was looking for a position at China Lake. And we got talking how we might measure helium-4 or helium-3. We really thought helium-3 was the main thing because at the same meeting in Salt Lake City, Dr. Sanger, Nobel Prize winner, had commented that the reaction might be deuterium plus hydrogen going to form helium-3. And so, we were really mostly interested in detecting helium-3. But, as long as we're doing that, we might as well measure helium-4.

Tom Grimshaw: Okay. So, I'm going to stop to backtrack here just a little bit. We'll pick up with the Ben Bush and the deuterium and helium-3 and helium-4 in a minute. And where I'd like to backtrack is, when you got the piece of palladium from Johnson Matthey, this six millimeter piece, were you using that entire piece of palladium as the cathode when you were doing the experiment?

Melvin Miles: No, it was a six centimeter in length. So, we cut two pieces off somewhere between one and two centimeters long. We had two calorimetry, so we ran two studies simultaneously. So, we had two different samples off the same rod. And we ran those over and over again in the three or four different experiments, and got excess heat every time in those experiments. We didn't use the whole rod. No. That would have been too long. But it was just probably somewhere between one and two centimeter length, I would think.

Tom Grimshaw: So, you basically cut the rod in half and used each half then for the cathode?

Melvin Miles: No, it was six centimeters long. So, we just cut two samples off the end.

Tom Grimshaw: Okay.

Melvin Miles: And we had a lot of the rod left.

Tom Grimshaw: Okay.

And tell us about the electrolytic cell. It was a Fleischmann and Pons type electrolytic cell that you were using based on what you had heard from their work? Or what was the experiment?

Melvin Miles: Yeah. At that time I think we'd heard that Fleischmann and Pons were using lithium dissolved in heavy water, gives you lithium deuterioxide, LIOD, similar to lithium hydroxide, but except deuterium. And in the detail we were using was some Cambridge isotopes across the hall. We later ordered our own from the same company. And so, that was the electrolyte, D20 plus the LIOD. And the concentration we used was about 0.1 molar or 0.2 molar lit LIOB as electrolyte at that time.

Tom Grimshaw: And you said that you were improving your calorimetry. What kind of calorimetry were you using? Was it iso parabolic, mass flow? What was the calorimetry?

Melvin Miles: It was always iso parabolic because that's what Fleischmann and Pons had recorded. And we thought we'd just follow what he used. And so, we kind of gained information on what you need to measure. For each cell you had to measure the cell temperature, you had to measure the VAT temperature, had to measure the cell voltage. And, to be sure, we used two thermistors in each cell. So, we measured temperature at two different points, and we relied on the gassing coming off for stirring, which is quite adequate as long as you use higher current densities. Like we use it- well, or at least current. We use it around never less than 50 milliamp current, but often up around 200, 300, maybe 500 milliamp current. And that gives you bigger stirring.

And the test tube itself was a small cell. We filled it with about 18 milliliters of electrolyte, which made it very small. But the smaller the cell, the more sensitive it is to heating the entire content of the cell, and it made it quite sensitive to any excess heat. So, you gained somewhat by doing that. You lose because the electrolyte level changes more rapidly and that changes the cell constant. In order to get around that problem, which was a serious problem, it's electrolyte level changes rapidly in a small cell, you need to find some way to get round that. So, we put the cell inside of another bigger cell filled with water and the thermistors were against the cell wall on the outside.

Tom Grimshaw: Okay. Sorry to interrupt there. Keep going.

Melvin Miles: So, unlike Fleischmann and Pons, we did not measure directly in the cell. We measured on the outside of the cell wall. And all the extra water in the outer tube came to prevent [inaudible 00:10:12] level stayed the same. And so, we got around the problem with electrolyte level changing. It was a constant. But it still was a problem. You had to be careful [inaudible 00:10:26]. You could do studies pretty well for a day or so before you refilled the [inaudible 00:10:31].

Tom Grimshaw: Okay.

And this initial work that you're describing, has that been written up in a report or in a paper that's been presented?

Melvin Miles: Yeah. It was written up. The first was in a referring journal, the same journal that Fleischmann and Pons used. It's called the Great Common Journal for Electrochemistry, a Journal of Electroanalytical Chemistry. It was the journal we used on our first writeup of reporting excess heat. Before that, we'd written up a paper reporting no excess heat, and that was in the journal that published the proceedings of the New Mexico meeting in May

or June of 1989. And that journal was called the Journal of Fusion Energy. But that was reporting our calorimetry but not seeing any excess heat in that first publication.

Tom Grimshaw: So, that was the so called Santa Fe Meeting put on by Los Alamos National Laboratory. And you were there and you gave a presentation, but it didn't involve the excess heat. You were just talking about procedure.

Melvin Miles: No, I didn't actually go. David Stilwell wanted to go, so I let him. We worked together making the view guy- making the presentation. I don't know whether we used [inaudible 00:11:55] graphs back then or slides. But we prepared the presentation and he went to the meeting, and he gave the presentation at the meeting. I missed that meeting in New Mexico.

Tom Grimshaw: Okay, but as you say, and this is something I didn't know, I have the materials that were prepared before and for the meeting. And there was a Los Alamos publication, which I can show you, kind of summarizing what had happened at the meeting. But I was unaware that many of the authors then submitted their work, their papers to this journal that you just mentioned. What was it again?

Melvin Miles: I think Journal of Fusion Energy.

Tom Grimshaw: Okay.

Melvin Miles: It's not to be confused with George Miley's journal, which is Fusion Technology, I think, was the name of that one. They're two different journals.

Tom Grimshaw: And I made that mistake when you first showed it to me this morning. I confused them with the George Miley journal. Okay.

So, that kind of, I think, I covered almost everything on that very first set of experiments you did. You reported it at the Santa Fe Meeting, and then you subsequently published it, as you just said, in another journal. So, why don't we pick up the story then after that very first experiment and what courses did you take at that point.

Melvin Miles: Well, I was working on another program for the Office of Naval Research. Bob Novak was the head of that program. And it was involved a new lithium type battery, lithium anode and a lithium bromide trifluoric electrolyte. And it was very dangerous material to work with. You could get five or six volts per cell in that battery.

But anyway, Bob Novak, who actually funded Fleischmann and Pons in some of the early work, before him it was another person at the Naval Research Laboratory that the naval research. But Bob Novak, towards the end- but it's under Fleischmann and Pons. So, he knew about their work. And he told me go ahead and work on cold fusion, but kind of on the back burner. He said there were a lot of enemies of cold fusion at the Office of Naval Research and made it difficult for him to fund it officially. But he said to go ahead and work on it where I could have the post doc work on it. And so, that's how I first got started in the cold fusion research. It was not part of any program. It was something we did on the back burner with a post doc that did the research, in addition to what we were supposed to be working on officially.

Tom Grimshaw: Okay. And so, again, to talk about the sequence of events, it was not until September that you started to see excess heat. And that's when you first met Fleischmann at ICCF-1 in Salt Lake City. So, and you were working with Ben Bush. You started down that path before I interrupted you. Can you pick up the thread of the story at that point, please?

Melvin Miles: Well, at the time of the meeting in Salt Lake, I had not yet met Ben Bush. He was not there. I didn't even meet Martin Fleischmann at that meeting. I met Stan Pons. He came over and thanked me for my talk and after my talk on the last day of the meeting. But I never really remember ever talking to Martin Fleischmann.

I got to know Martin Fleischmann at the second conference, the ICCF-2 in Como, Italy. And that's after we reported on helium-4. And that's after I met Ben Bush. Between those two meetings, ICCF-1 and ICCF-2, that's when Ben Bush called me inquiring about a position, and we got off onto discussing cold fusion and how he maybe could do helium-3. We really were talking mainly about helium-3, how we could do those measurements at the University of Texas.

Already checked with NMR people- or mass spec people, I mean, at China Lake. And they had a mass spec, but it was not designed for small molecules. It was more for larger molecules and more for work done by organic chemists there at China Lake. And they could not measure helium-4 accurately. And so, but Ben Bush said that the Mass Spec at the University of Texas could do that kind of measurement. And so, Ben Bush was an expert at keeping atmospheric air from getting into systems. And he had done a lot of work in that area, keeping air out. So, with his help, I found ways we could keep the air out and collect samples of gas in special flasks. Ben Bush made the flasks there, and then shipped them to me. They were basically a modified round bottom flask, 500 milliliter volume for the flask.

And to collect samples we would, when we had excess heat, we would insert the glass flask into the line of the gas that's coming out the cell, and they would run through this flask. And then, through an oil bubbler, which prevented any atmospheric gas coming back, flowing backwards into the cell. And so, we would use boiled off nitrogen that does not contain any helium-4, because if the nitrogen had boils, helium would have boiled off long before nitrogen. And we had that already piped into the lab in China Lake.

And so, we would flush the flask for maybe 30 minutes with this boiled off nitrogen, after it was connected. We would run it through the whole system. And then, we would run the experiment for maybe 30, 40 minutes and collect the gas sample after that. And then, we could close off the flask by stopcocks in and out. And then, disconnect it from the line, and then send it on to Ben Bush at the University of Texas. That's how we started that helium formation with.

So, we worked out very well how to keep atmospheric air out. And so, that was not a problem. So, that was the major complaint. The only complaint that anybody ever had about our study was, well, it might have come up from the atmosphere. But we were very good at keeping it out. And we showed that later when we ran H<sub>2</sub>O controls the same way, and it did not show any heat in four in those H<sub>2</sub>O controls.

Tom Grimshaw:

Okay.

So, I'll just ad lib a little bit here. The problem of contamination of gas samples from experiments has a long history, because helium is so prevalent in the atmosphere. And it goes back actually, I think, to the work in the 1920s with Paneth and Peters, in which they thought they had evolved helium from deuterium, and then withdrew the results saying that apparently it was contamination from the atmosphere. Although, I think there was some question whether that was really true or not, but they couldn't be certain enough. So, they withdrew the results of their paper. So, Mel, your ability to exclude contamination or prevent contamination from atmospheric helium was a major step forward. So, I just want to kind of add that piece in.

Explain, if you will a little bit, you started talking about helium-3, and then you went to helium-4. Which the difference then would be the products, if you were getting cold fusion reactions of deuterium and protium versus two deuteriums, I think. Can you tell a little bit about the difference in when you went from helium-3 to helium-4?

Melvin Miles: Well, the helium-3 is formed from one hydrogen fusion with one deuterium. And, according to Sanger, that gives you helium-3. That's the way it would work out. And there would be no gamma ray, no radiation. That's the way his theory worked out anyway.

And so, I thought that would be definite proof, because they're not very much helium-3 in air. I would actually prefer finding helium-3, and then it wouldn't have been so much controversy. But Ben Bush, we did look mainly for helium-3, but we never saw anything. Never saw helium-3 in any sample that we could measure, not enough to measure. But helium-4 showed up in samples and I did not tell Ben Bush which samples had which amount of excess heat or whether they had any excess heat. I coded these with- in the initial publication, the code numbers used were actual birth dates of my family member. Like my daughter is born 5/5/75, and that was one of the code numbers used on that. And so, Ben Bush did not know if there were any excess heat or not or how much.

And what we reported the large peaks usually turned out to be the large excess heat that I measured. And the medium peaks were usually the medium excess heat I measured. And the low peaks, small peaks were the lower values. And there were one or two samples I sent him where I had not measured any excess heat, and he also did not see any helium-4. But he didn't know any of this. And, from work, I just worked up this recent months for a chapter in a book, almost everything I calculated theoretically fits with his peaks that he reported, except for two samples. If they were interchanged somehow, that would be exactly right also.

So, we did not know how much helium-4 we were measuring, but you can calculate how much helium-4 should have been seen based on the excess power divided by the current. That would determine how much helium-4 you would expect. And that's an equation I just worked out recently. And it pretty well fit with what Ben Bush reported.

Tom Grimshaw: Okay.

And back to the experiment, the type, were you using the same kind of a experimental cell, same materials, same procedure as your original work, but with the addition of the helium measurement?

Melvin Miles: Yeah. It was this the same system. We had to gradually get the equipment. We found a water bath. We made the cell that would fit in the water bath quite well, and the calorimetry was a plastic bottle filled with insulating material with a cell in the center. In the inside, it was a little bit larger cell that contained the water on the outside. And so, this is all the same as we had used before. And I used the same palladium I had used before, because it had been given excess heat in every experiment. And, in this experiment, it actually turned out to be the largest excess heat I had ever measured with that palladium. And that was quite fortunate because that naval had been pushed to see the helium-4 more readily, because we did have rather large excess power effects.

And then, following those studies, we ran the H2O control studies and collected the samples the same way. And he did not find helium-4 in any of those controls.

Tom Grimshaw: Okay.

And remind me, where was this a second round of work published? Can you tell us where the papers were?

Melvin Miles: Well, the preliminary note came out first in March, 1991. I completed most of the work in late 1990. The control was done in January, 1991. The preliminary notes sent to Journal Electroanalytical Chemistry. And they published it right away, like they often do with preliminary notes. But, at that time, I was concerned that so much negative opinion about cold fusion, even to China Lake, to get a paper published, I had to go through a lot of

different layers of bureaucracy. And anyone could have prevented me from publishing that paper.

And I knew there were a lot of negative people on the side that did not believe that cold fusion was real. And so, I arranged for Ben Bush and Professor Lagowski to publish the paper there. I worked on writing the paper, but they sent it in from the University of Texas. And it wound up with Ben Bush and Lagowski being the first author. That's always bothered me somewhat, but they didn't do anything except the helium formation was there at the University of Texas. But anyway, that was the preliminary note.

And then, I went to Como Italy and I reported on the results there. That's when I first met Martin Fleischmann officially. I had seen him before at ICCF-1, but I didn't get a chance to talk to him. But I got to know Martin Fleischmann at that meeting, and reported on helium-4. And, in my opinion, that was one of the best cold fusion meetings we ever had, because we were left alone by the press, not unlike Salt Lake City where they were a nuisance, and we didn't have Chief Jones there and that helped in. And so, we just concentrated on the science. And another navy person, Stan Spock reported on co deposition and getting excess heat in. And Bob Novak was pleased with what was coming out of the navy, even though we hadn't funded anything yet. And he wanted to set up a navy program. So, this led to the Navy program from the Como meeting and the work that Stan Spock did at San Diego, and what I did at China Lake. And so, that was the second publication.

The first presentation later published in the book, ICCF-2 Proceedings. And then, about a year or two later, we worked up the full paper, and it was published in Journal Electroanalytical Chemistry with all the details and the followup work, even looking at the diffusion of helium forth through the glass. We answered that question as well in that publication.

Tom Grimshaw:

Okay.

So, I believe that then, the one you just mentioned, the third one, is the one that kind of established your reputation in the field, I think, where you were claiming and showing the correlation between helium production and the amount of excess heat. So, the heat helium correlation was very important to the field at that time.

Melvin Miles:

Yes. But by that time, cold fusion had been dismissed within six months, Cal Tech, MIT, Hardwell. And so, I think most scientists didn't know who to believe. And so, it should have been a major breakthrough, but by most scientists it was just passed off as a contamination problem. And I don't think anybody- most scientists didn't really look into it. It's unfortunate, but it did not really get accepted by most scientists, the helium-4 work.

Tom Grimshaw:

Right.

And, but I would say though that, within the cold fusion field though, it's still stands as a real seminal and very important work for the the field in establishing the reality of the phenomenon.

Melvin Miles:

Yeah. I think Mike McKubre's work really helped that a lot, because one of his programs at SRI was to verify our heat and helium work. And he spent several years working on it and came up with even measurements closer to what you would expect theoretically. I mean, like the energy per helium, for example. You should get 23.8 MEV per helium-4 form. And he got pretty close to that value. But that's the value that can vary widely because we don't know how much helium-4 stays in the palladium. If only 50% is released outside, then that value would be changed by a factor of two. And it would become, rather than 24, it would become 48. So, to get close to 23.8 you got to be sure you're getting all the helium produced reaching the outside of the palladium and getting into the sample.

And so, I don't take that 23.8 too seriously because it can vary over such a wide range, depending on how much helium-4 actually gets outside the palladium. According to Ed Storms, if it's near the surface where it might be, then half the helium would recoil into the palladium and half would recoil out, if it were on the surface where the action took place. So, he expected it to be about 50% of the helium-4 produced getting outside the palladium. And, on average, that's about what I found. About 50% of what you would expect gets outside the palladium.

Tom Grimshaw:

Okay.

And if I recall, McKubre did some work to try to force the helium out to get a better correlation by baking the palladium and measuring helium after it had been a forced out of the matrix?

Melvin Miles:

Yes, he got a value, after working to get all the helium out, he got 31 MEV per helium, versus the theoretical 24. So, that was closer than a lot of my work showed. My work was probably a little bit higher value than that. But McKubre pinned it down more closely in that test. I think that's what got it more widely accepted.

They were still a lot of skeptics. I've heard that a lot of people in cold fusion never did accept my helium-4, and it's is even discussed today whether it's correct or not. I mean, everybody has different opinions. Tom Passer was one. He was thinking it was maybe some other reaction. So, I don't know how 100% people accept helium-4. But I accept it 100%.

Tom Grimshaw:

Okay. Okay.

I think we'll call that the end of this session. We're at a little over 30 minutes, and these work best if we do them in 30 minutes segments.

Melvin Miles:

Right.

Tom Grimshaw:

So, why don't we pick up the story with what happened after you published the work and presented it at Como, ICCF-2. And we'll pick up the story at that point. Well, and after you published the paper as well. And we'll do that in the next session.

Melvin Miles:

Right. Okay.

Tom Grimshaw:

Okay.

So, this is Tom Grimshaw with Melvin miles. It's May sixth. We're at his home near Saint George, Utah. And we'll pick up again on our third session here shortly.

### ***Interview 1-3***

Melvin Miles:

Where were we going to go?

Tom Grimshaw:

Tom Grimshaw here again with Melvin Miles. This is session number three, May 6th. It's now pretty close to 4 O'clock in the afternoon. Melvin, the last two sessions you talked about the early work at the Navy, but not necessarily funded by the Navy, there when you were at China Lake, and you mentioned your heat and helium work with Ben [Lagowski 00:00:37] which was reported ICCF-2 in Como, Italy. That's where you met Martin Fleischmann. Maybe this would be a good point to pickup the work that you continue to do there at China Lake related to cold fusion.

Melvin Miles:

Okay, going ahead, the work I did on Cold Fusion from the announcement in 1989 until Como, Italy, in I think it was 1991. None of this was officially funded by the Navy. They knew about it. Bob Novak knew about it, but it wasn't an official program. The one thing he said, they had too many enemies of cold fusion at ONR and other places. It would be kind of hard to defend funding it, but he still thought it was okay to work on it.

He was quite pleased that Como, Italy, the work that I done showing the correlation between excess heat and helium-4, and the work that Stan Szpak also reported at that meeting about co-deposition, the new method to produce the effect. They didn't do calorimeter, but they saw other effects that seem to be reproducible every time.

Dave [Nago 00:02:05] of the Navy was also there at this meeting. There the idea developed that we should try to form a Navy program despite the opposition that would be existing. So this led to a Navy program being proposed to Fred Saafeld who was the director of ONR at the time, and he supported it, and he said he would fund it.

Dave Nago wrote up the first proposal. It was the three Navy laboratories to work on it, Naval Research Laboratory in Washington DC, China Lake, my work. Ben Bush was working with me there at the time too. Then they sent Nago to work with Stan Szpak and Pam Boss. So that was the beginning of the Navy program.

One of the major goals of the Navy program was to make our own palladium material. We knew by then that some palladium worked well, but most palladium did not work at all. So we didn't want to have that be an uncontrolled variable. We wanted to get control of that material, and find material that we could make ourselves, and that would work. That was one of the major goals of the Navy program. Also, we were supposed to pass on what we did at one lab to another and see if they could reproduce it, and ultimately see if NRL could reproduce it.

So the program started out, they wanted us to look at co-deposition that started with Stan Szpak and Pam Boss in San Diego. So Ben Bush and I started working on it. To hit into our calorimeter, we had to change things somewhat because of the size. We had to fit it within our cell. So we started on co-deposition, but we ran experiment after experiment, we never could find any excess heat. Sometimes we thought we saw excess heat, but the palladium particle deposit would sometimes float around in solution, get to the top, and dry out, and then 'cause recombination. In fact, one time it even caused an explosion, kind of like a loud ricochet because of the sudden explosion of D2 plus O2 mixture. So that was one problem that we had.

So we had to measure the amount of gas coming off so we could be sure any excess heat was not due to recombination. So we did that in every experiment, but we had only a few experiments, I think about 3 out of 30 where we measured excess heat. It was pretty poor reproducibility.

I later realized when I wrote the final report for the Navy at China Lake that this was probably mainly due to the fact that the amount of palladium was so small. Fleischmann explain that in normal conditions you'd get about one watt per cubic centimeter of palladium. We were depositing such a small amount we would have to get a lot larger than one watt. Maybe 100 watts per cubic centimeter before we'd see anything. So that's surprising after the fact that we didn't see excess heat because the amount of palladium was so small.

on the other hand, even though this is a secondary thing we're measuring, we had Geiger-Meuller counters placed near the experiment, and we started seeing that when the experiment was running we were getting high counts. When the experiment was off it was always normal counts. So we didn't know how significant this was, but we got under in

trying to measure radiation. There was a radiation effect, I'm pretty certain, but only when the cells were running. But not every time. So that was part of the early work.

We were getting NRL made palladium materials. They sent us palladium and palladium-silver alloys, but every time we ran an experiment with those we never saw any excess heat. Actually we went about a two year period almost where we did not ever see excess heat. We couldn't go back to the old palladium rods because Ben Bush had cut them up. I don't think I said he could do that, but he did it, and sent it off to Rockwell International to be examined for helium-4. Well, for one thing, we'd already run a D2 regular. I mean, an H2O study. So I think the H2O study had been run in-between and we'd loaded it and de-loaded it with hydrogen, so not likely much helium would have remained by then.

Anyway, we lost the two palladium rods that we used before that worked every time, and we could not recover from that position. We still had a little bit of that soft palladium we used, but other sections we cut off did not produce the effect. So we were kind of in limbo for two years. Nothing that NRL sent us gave excess heat. The Stan Szpak co-deposition did not work out well for excess heat. We were stuck in that situation from almost from 1992, the beginning of the program, until 1994 when finally Dr. Iman with NRL made palladium-boron. We started looking at palladium-boron and almost every sample he sent us gave us excess heat effect. So suddenly we had a material that seemed to work every time made by Naval Research Laboratory and Dr. Iman.

The problem was that we had to get NRL to verify this result, but their calorimetry had such a big error that they would not have seen it. So they worked on getting a Seebeck calorimeter from Hart set up that would be more accurate. Finally in January in 1995 I went back there to help them. I helped set up the two experiments and Roger Hart was there. He helped calibrate everything so we knew the calorimetry was working well. He was there while I was there making sure everything calibrated.

One time I went out to dinner with him and he gave me his opinion of the NRL work, which was not good. He said [inaudible 00:09:14] to me they sit and been collecting data for about several months. He said it was all useless. He says actually he couldn't tell where the zero point was. They'd never done the calibrations and you did not know where zero was. He said months of data... I saw the paper. It was stacks of paper, the data that he couldn't make heads or tail out of it, but he did make sure the system was working.

I set up experiments and I stayed there for the first week or so. I didn't see how they set the cell in the Seebeck calorimeter, however, otherwise I would have commented. You need the gas to escape directly upward and out, and they had the long line running parallel, which eventually would get clogged with D2 old vapor that comes out as well and it condenses. So the line would eventually get clogged up.

I never saw any of the data except what I had in my notebook until years later. Just two years ago, actually. Steve Krivit did a Freedom of Information Act and they got NRL and ONR to release these internal reports that show the data. Looking at the data I can see there was a small, but significant excess heat in these experiments. Unfortunately if we'd known that at the beginning it might have saved the Navy program, but because NRL could not reproduce palladium-boron excess heat the program ended in June of that same year.

In 1995 that was the end of the Navy program, but NRL wrote up a report, but they did not ever report either any excess heat anywhere, or any other measurement of cold fusion significance. So that's where the program ended in 1995. That's when my funding for cold fusion ended as well. I had a period at China Lake where I had no funding, and it was difficult to go through that period.

I finally wound up with a chance to go to Japan, NHE Laboratory. They were just winding down. They were in the last six months of the program, but they invited several foreign

scientists to come. Myself and Dr. [Lidstent 00:11:34] from Russia was there, and another person from Italy was there.

Tom Grimshaw: I'm going to ask to jump in for just a minute because there are a few questions maybe still from the Navy program. Kind of cover a little bit more of that, and then complete that. Then we'll go on to your work at NHE if that would be okay?

Melvin Miles: Right. Okay.

Tom Grimshaw: So in the Navy program, what I heard you say was that you weren't able to continue the work that you had done when you showed the excess heat and helium-4 correlation. There was money available, but you were kind of directed to go in a particular direction, and one of the reasons was because the samples that had been active didn't exist any longer. They'd been shipped off to Rockwell, I think you said, and I guess they were not ever returned so that you would have them available.

Melvin Miles: No. They were cut up pretty badly. They were made almost into a powder.

Tom Grimshaw: Oh. Okay.

Melvin Miles: Yeah. And so they were never returned and they were destroyed anyway in the way they were cut up into sections. They were cut into sections to look at different sections, and so they were pretty well destroyed. I never was able to use those two palladium again after that. With the Navy funding we did continue helium-4 work.

Tom Grimshaw: With co-deposition?

Melvin Miles: No. We never saw excess heat enough to really do helium-4. We finally got another Johnson-Matthey palladium wire. It was only one millimeter thick, but more than time it gave excess heat. We got an experiment running with that, and three different times we collected samples for helium gas and sent it to Rockwell International. Then they did a very accurate helium analysis and they could measure to .1 part per billion, 10 times better than any other laboratory had ever done.

It was a double-blind experiment. I did not tell them what sample gave excess heat, and how much, and they never told me how much helium-4 they measured. They reported separately to Professor Lagowski, University of Texas, their helium-4 results, and I reported separately my excess heat results. It was rather not a large excess heat. It was about .1 watt or 100 milliwatts was the highest. Because they could measure helium-4 so accurately, it turned out to be significant. There was the one that gave the highest excess power, the 100 milliwatts gave the most helium-4. The one that gave the second to the most excess power gave the second most helium-4, and the one that gave the lowest excess power gave the lowest helium-4.

Not only that, doing calculations, I got very close to the right amount of helium, especially in the higher excess heat. Theoretically you should get around 2.7 times  $10^{11}$  helium-4 atoms per joule. Now a joule is a watt-second, so you say per watt per second. So from that theoretical, I could calculate what was measured and what I should have got, and I got very close to that 2.7. I think I maybe have got about 2.5 times  $10^{11}$  helium-4 per second per watt. So that was one of the most accurate agreements I had with helium-4.

Not only that, these were the same glass flasks we used before, and they measured helium-4 over a period of about 90 days. Because of the fusion to the glass they could see that it would increase over time, but we could get the rate. How much helium-4 per day and it was too low to have really affected our previous results, and they extrapolated a straight line. It increased on a straight line fashion, a little bit higher, depending on the amount of time. They could extrapolate that back to zero. So their report was at zero time, the time the

sample was collected, the amount of helium-4 that was there. So there was no time to diffuse through the glass. That glass, in fact, was ruled out in those studies.

Tom Grimshaw:

Good.

Melvin Miles:

Right. So that was probably the most accurate helium-4 measurement, but not the most excess heat measurement. But everything, even though it was double-blind, and I don't know whether Brian Oliver would agree with me on this, he just thought each line had a slightly different slope so they came through at different points at zero time. At zero time those fit exactly with the results I measured, and so I think it was significant though even I talked to... I don't know whether I ever convinced him of that. Neither one of us knew what the other measured until later anyway.

So that was the follow up of helium-4, and probably the most accurate measurements that were ever done. Then we also, with the same funding from the Navy program, and Ben Bush was pretty big part in this, he liked to make things. He constructed metal flasks where we could collect samples and I had a metal to metal seal so there's no plastic or anything where helium-4 could diffuse through. Before we actually used these flasks he as a Post Doc was fired, and the Navy did not hire him further because they thought he was maybe, politically a danger the way he talked about things. So he got a job at SRI, and so I continued the helium-4 measurements using his flask, but he did make the flask and left before we did the measurements.

I did one measurement with palladium-boron. I only had time for one. That gave excess heat. I measured about five samples that did not have any excess heat. That gave me a baseline of when there's no excess heat, how much helium-4 was getting into your system. I used that to subtract that baseline from any other thing I measured and this brought everything in a better fit to what you would expect.

We did finally find some palladium samples that Fleischmann sent me a sample that he worked in his laboratory, and that worked at mine. So there's other samples we found besides the palladium-boron that gave excess heat and we reported this not in a journal publication, but at [inaudible 00:19:03] and conferences reported that. So we do have publications of that of the, I think about six or seven, experiments where we had excess heat, and also measured excess helium-4 above the background that we saw when there's no excess heat. Those fit fairly well too because I never ever got to the point of exceeding the large excess heat though that I saw with those original palladium-boron rods, six millimeter diameter. But we did get further studies with the Navy program showing helium-4 shows up when you have excess heat. When you don't have excess heat, you do get a baseline besides the same baseline.

Tom Grimshaw:

How did you deal with the problem of analysis between deuterium and helium when you were measuring the helium-4? I know that's a huge issue in the field. Malcolm Fowler has developed a new instrument in his lab in Espanola that's able to do that.

Melvin Miles:

I think I might be reviewing one of his papers, actually with Tom Claytor.

Tom Grimshaw:

Well Malcolm Fowler is currently working on an instrument that will distinguish between helium-4 and deuterium. He claims that this is one of the first instruments that's able to do that on a believable basis.

Melvin Miles:

Well the instrument that the University of Texas, Austin could do that as well. A good instrument can separate helium-4 and D2. It can. You see separate peaks, and I've got some of Ben Bush's actual photographs of the screen that show the separation. Also, the way that Ben Bush did it, and the way that Rockwell International's Brian Oliver did it is that you pass this gas sample through [gaiters 00:21:17] and these are things that will absorb hydrogen, but they don't absorb helium. Like activated charcoal at liquid nitrogen temperature. Helium will pass through that and keep going, but D2 will get absorbed on it,

and get delayed. So you can separate the helium-4 arriving first to the mass spec, then D2 arrives later. In order to do accurate work, you have to do that. You have to somehow separate them physically in a gaiter system where you cause the D2 to be absorbed and the helium goes on.

Tom Grimshaw: Got it. Okay. We may give him a call just to, I'd like to hear the two of you interact.

Melvin Miles: But also if anybody's going to measure gas samples in this study, they need to be able to measure to one part per billion. The paper I'm reviewing, and I don't know if this is anything. I know Tom Claytor's name was on it. I don't know who else is on it without going back and looking, but I think it should be published. They're claiming they can measure to one part per million. That's a thousand times different. That is not accurate enough to measure gas coming off a cold fusion electrolysis cell.

In fact, I've done a calculation to get 1,000 part per billion, you'd need about 10 watts of excess power. 10 watts of excess power would cause any cell to boil away. You'd see boiling before you would be able to measure the helium. So one part per million will not work. In fact, Nate Lewis' first paper against cold fusion, he has a section on helium-4 and his accuracy is one part per million, and he doesn't realize that you'd have 10 watts of excess power before he'd even reach the detection of that. I pointed it out to some people, and also in some of my papers. But one part per million won't do it.

Tom Grimshaw: I think you were saying he was getting down to... Well, let's not get stuck on that point.

So back to the Navy program for a moment. At the same time you were doing the co-deposition work, you were also being able to do some of this more normal electrolytic work.

Melvin Miles: When I had Navy funding for several years I had funding for other things, but I passed a lot of that off to... I had two Post Docs working for me. We had a battery program on lithium anode and a bromine trifluoride that serves electrolytes and as the cathode material, and it gives you about five [godes 00:24:18] per cell. That's better than anyone else can get, and this is part of President Reagan's Star War program to fund new ideas. This is from Bob Novak and some other people at ONR. We did show that we could get that. The only thing is, you could get a high current density with it. You could get lower current density, but somehow you probably form maybe a lithium-fluoride coating that doesn't pass current very well. So it would limit the current.

This even went on to another program at another laboratory back in Massachusetts. They looked at it, and they got similar results that we did. It actually became another program funded by the Navy, but it does work. It gives you high voltage, but it doesn't give you enough current. That program continued mainly with the help of some Post Docs.

Tom Grimshaw: Okay.

Melvin Miles: I liked cold fusion and I worked as much time as I could on the cold fusion. In fact, I'd work weekends. I put in a lot of extra time on it.

Tom Grimshaw: Yeah. Like so many. 24/7. So you were doing the co-deposition work. You were using calorimetry whereas Szpak down at [Staywar 00:25:42] was using the CR-39 tracks, I guess as the diagnostic-

Melvin Miles: CR-39 came later with Pam Boss mainly, I think, to measure neutrons. I think that came after Stan Szpak retired.

Tom Grimshaw: So what Stan Szpak-

Melvin Miles: He was looking for trillium.

Tom Grimshaw: Okay.

Melvin Miles: One of the big things he did, he found trillium production, and he looked also for radiation. He saw signs of x-rays. He got signs of x-rays. He also measured for acoustic effects, sound, and he found that he could get sound effects coming off the electrode. So those were the kind of things he was working on, but he never got into calorimetry. One of the main things he did, they got a thermo-imaging counter, a camera, and they could see these bright spots indicating higher temperatures coming and going all over the surface of the palladium rod.

Tom Grimshaw: Sparkling.

Melvin Miles: Right.

Tom Grimshaw: I remember that.

Melvin Miles: So these active sites would show up and then disappear, and the image was repaired, and it was always changing. Martin Fleischmann, when we had a meeting at San Diego, he thought these might be due to micro-fusion events going on. Fusion event on a small scale.

Tom Grimshaw: They would appear, produce energy, and then self-destruct, and reform elsewhere?

Melvin Miles: Right. Yeah. But they were always going on, and they never went away completely. They were just changing locations.

Tom Grimshaw: Interesting. Okay. All right. So the Navy worked... Your primary mandate was to do the co-deposition affirmation, I guess, is what I understood, but you were doing these other things at the same time.

Melvin Miles: I guess the main goal was for NRL to make materials, and I was to test these materials. I think that was the main thing. We wanted to make our own materials and have them produce excess heat. I got a lot of new materials and I did a lot of testing, but that was part of the two year period where nothing gave excess heat. That's why the program was in jeopardy by the third year. The third year was when palladium-boron was produced, that last and final year.

Tom Grimshaw: Yep. So the program went from 1992 to 1995, basically?

Melvin Miles: Right yeah. The first funding was January 1992, and it ended in June 1995. I remember how it ended. We had a meeting at China Lake. Bob Novak came, and Deborah Rawson, and Dr. Iman, and Don [Davinos 00:28:39] was there. Even Mike Makubre was invited because he was getting some funding from Bob Novak at that time. We met for the first day or two and suddenly the third day Bob Novak, it seemed like overnight, he changed his mind. He said, "Well, I'm just going to end the program. This is the end of the program." We never know what happened overnight. Maybe he and Deborah talked. I don't know who talked, but he came in with a different attitude on the last day.

Tom Grimshaw: What was it, three day meetings?

Melvin Miles: Yeah. Three day meeting.

Tom Grimshaw: Wow. Well, that's startling and disappointing, and devastating.

Melvin Miles: Oh yeah.

Tom Grimshaw: Wow. Okay.

Melvin Miles: We even had people from the University of Utah that were involved in the program. Also Utah State, a person there that... I'm trying to think of his name.

Tom Grimshaw: Hansen?

Melvin Miles: Yeah, Hansen. Wilford Hansen was there, two people from the University of Utah that were getting some funding. They were doing some work and I don't know how far they got,

but they were funded. One of them was my, when I first went to the University of Utah, I took Modern Physics. Bergeson was his name. He was my teacher, and he was interested in this field. He was working on it.

Tom Grimshaw: [inaudible 00:30:01].

Melvin Miles: Haven Bergeson was the name. These people, I think have passed away [inaudible 00:30:08] know.

Tom Grimshaw: It's been a long time.

Melvin Miles: I think Wilford Hansen has passed away, and Haven Bergeson has passed away, I think.

Tom Grimshaw: Okay. Well, that's probably enough on the Navy program. You were moving on when I interrupted you. We kind of covered a few more things on the Navy program. Why don't you pick that up again, please?

Melvin Miles: Now I'm not sure where I left off.

Tom Grimshaw: Oh okay.

Melvin Miles: You remind me.

Tom Grimshaw: All right. Well, let's see. Let me think here. I'm going to [crosstalk 00:30:36].

Melvin Miles: I can mention one thing. We knew the program was in jeopardy. In fact, it was hard to even plan what to do because I keep getting word back that the program will be... For the last year or two I keep hearing the program is going to end. When we had a review meeting in December of 1994, back at Washington DC, and Fred Saafeld always came to these review meetings. He was very interested in this program. He was us to join together and write a very defining publication in a major journal to come out of this work. That's when I first presented the palladium-boron at that meeting. We went out to dinner that night and Bob Novak told me, "The palladium-boron work saved the program, I think." He said we need to continue to look at that.

So that was my first reported palladium-boron was at the December 1994 meeting at Washington DC. Then I did more work to follow it up. Everything I did got excess heat from the results, but the failure was that NRL could not reproduce it, or not in time anyway. Even though they did reproduce it, but they didn't realize it. They never showed me the data. It was a small effect, but it was real.

Tom Grimshaw: Yeah. How ironic.

Melvin Miles: It starts off small.

Tom Grimshaw: How ironic. Okay. I'm going to put it on pause for just a moment.

Okay. Picking back up again, we're talking about the conclusion of the Navy program, the three year long Navy program. Did it have a name by the way?

Melvin Miles: Anomalous Effects in Deuterated Material. We wanted to stay clear of cold fusion.

Tom Grimshaw: Yeah, of course.

Melvin Miles: It's Anomalous Effects in Deuterated Materials.

Tom Grimshaw: Okay. And so that was shut down in surprise announcement in the meeting, you said in June. Then you what? You started to write a report, but there was no money?

Melvin Miles: I think I had a little bit of money to finish up some things, and I was supposed to write a report, but I was pretty soon unfunded. That's the first time I had ever been in that predicament in China Lake. Up to cold fusion I always had more funding than I could use

and I had other people working with me. After cold fusion nobody would fund me within the China Lake area, or ONR would not fund me.

Even Bob Novak's successor, Richard Carlin came in and he was upset that I worked on cold fusion. He'd tell people at China Lake that I ruined my reputation by working on cold fusion and reporting positive results. He did not want to fund me, though he was the one that should have funded me. He would fund people around me in electrochemistry in China Lake, and then these people would come to me, and have me help with the program, but I was not funded directly. But I had to help them to help do the electrochemistry. So that's the kind of situation I got into, and I would not have gone to Japan-

Tom Grimshaw: That's what we were about to talk about, your NHE work.

Melvin Miles: So this went on for a year or two. I didn't even have funding to write a report, and Dave Nago found out about it. So he came up with funding, to provide me funding to write the final report. If it weren't for Dave Nago my China Lake report would never have been written.

Tom Grimshaw: So it does exist. Is that the yellow one that you showed me?

Melvin Miles: Yeah. I have it in here.

Tom Grimshaw: Okay.

Melvin Miles: And thanks to Dave Nago that was ever written. That funded me for several months to write that. It wasn't completed until about 1996, however, because there was a gap in there where I didn't have funding to do it. But I was really happy that Dave Nago did that and I could go back and work on the cold fusion ideas into writing it all up. Then by the end new people had take over China Lake. Robin Nissan became chairman and he did not like my working on cold fusion from day one. Some people were very negative. They thought it was hurting the reputation of the China Lake Laboratory to even report anything positive about it.

Tom Grimshaw: Okay, so we're up at 30 minutes. So I'm going to start a new recording, and then we'll pick it up with the end of China Lake, and then your work at NHE in Japan. Are you still...

## ***Interview 1-4***

Tom Grimshaw: Tom Grimshaw here again. This is session number four with Melvin Miles here at this home. It's May sixth, and Mel, you were wrapping up, in the last session at the end you were telling me about the wrap up and writing the report for the work you had done at China Lake. Then some of the people that were coming in with negative attitudes and so forth. Why don't you pick up the story there?

Melvin Miles: Okay, because of not having funding, which puts you in a very bad position I never been in before at China Lake. I heard from Martin Fleishman, I was getting letters from Martin Fleishman, and he told me that Pepper Auto was trying to get funding to set up a laboratory in Italy, in Milan, Italy. Would I come and be the director of that laboratory on cold fusion research? If they could get the funding. I jumped at that chance. I said I would really like to do that, and I, they arraigned for me to fly to Milan, Italy, and I met with, then Fleishman came from England the next day, and the three people were Emilio Delgusi, Dr. Preperada, and Fleishman. That would be the three people.

We had to find a source of funding, and they thought they had the funding source from Pirelli Tire, a big company in Milan, Italy. They arraigned a meeting for us to give presentations to him, and we thought they would probably fund us. I flew there and we, and met Fleishman. Preperada had a technician working on cold fusion, he was running some experiments, but they wanted me to help with those. They wanted also to get enough funding to get set up and laboratory somewhere else, like on an island off Italy. I don't know. That'd be quite a lot of money. We went that day and gave our presentation to Pirelli, and went out to lunch.

I talked about my heat M4, helium correlation and Fleishman gave a talk, and Preperada gave a talk. Went out to lunch. I guess we thought it was pretty sure we were going to get a lot of funding from Pirelli, and so I kept waiting. Went back to China Lake and kept hoping to hear from him, and I kept calling Emilio Delgusi to see if he knew what was going on. He said they're still trying, thought they were going to get the funding, but finally Pirelli canceled out. They decided, what I heard from Emilio, who was, they thought it would hurt their reputation to get involved in cold fusion. Rather than funding us they would do work internally. Look out themselves.

They did wind up going to cold fusion meetings and reporting on cold fusion research after that. I remember one comment that Fleishman told me, he told me when I'm there that he thought this was a really a group that we would really do something. He said "He never quite had the right group before." I thought somehow he didn't think working with Pontz and the Japanese in France was ideal. I guess there's problems there. He thought with the three of us we'd have the right people together. He was really anxious that it would happen, but it never happened.

Tom Grimshaw: That's a nice, very nice complement and very nice thing for him to say though, about-

Melvin Miles: There you go.

Tom Grimshaw: About you and your reputation, so good.

Melvin Miles: I'd even bought books and tapes to learn the Italian language. I thought it was going to happen, but-

Tom Grimshaw: Yeah.

Melvin Miles: It didn't. Finally, I didn't find out that it wouldn't happen until several months later. The meeting was in February and I think I finally found out around March. I mean around April or May. In between, the head of the chemistry started sending me emails telling me what I could do what couldn't do any more. He said since I wasn't funded I was to report to the stock room person each morning at eight o'clock. I was supposed to work on inventory and stock room chemicals.

Tom Grimshaw: This is at China Lake of course.

Melvin Miles: This is at China Lake, and I, that was really told me he wanted to get rid of me, but didn't know any place to go, and I was really kind of frantic to get to Italy at that point. I had a few friends at China Lake. The head of research, he got wind of this, and he talked to me, and I think he also talked to the head of chemistry. I think he more or less told him this is no way to treat top scientists. They backed down on that, but they, I could tell they didn't want me to stay there. I did a little bit of the work.

I said "I'll start with my own lab," and I'll do inventory in my lab. I worked slow on it. They couldn't say I wasn't working on it. They wanted to waste money I thought "Okay with me," I could have been doing something useful. Instead, I just did a slow inventory of chemicals, and I never ever reported to the stock room clerk. That would have been an insult. She had a high school degree, to go to report to her for work every morning. Then I

got a chance to go to Japan, and I would never have gone to Japan if it weren't a better situation at China Lake.

Tom Grimshaw: How did that come about? The Japan offer-

Melvin Miles: I might have written them a letter, or they contacted me, I don't remember exactly. Anyway, they offered me a job there, and it was about maybe a little bit more than half the salary of China Lake, but it was a chance to get back into cold fusion. So I went there instead. Got away from China Lake. Took a leave of absence while I was there. That was one of the best periods of my cold fusion research, because we had, it was a good group and Pauli Tripota of Italy was there, and became pretty good friends with him. Lipson from Russia, and the Japanese people. I got along. The Japanese culture, you do everything together. You don't just come to work, you go bowling together, you go on a skiing trip together, and I'd never been skiing before, but I went. Fell down over and over again, but that's the way-

Tom Grimshaw: The ski areas up in Hokkaido.

Melvin Miles: That's where we were.

Tom Grimshaw: Yeah.

Melvin Miles: We were up in Sapporo, in Hokkaido.

Tom Grimshaw: Been there, went up there once. Well, after Sendai, my wife and I took the train up to Hokkaido, and then did an auto trip.

Melvin Miles: It's a pretty area of mountains.

Tom Grimshaw: Very different from the rest of Japan.

Melvin Miles: Oh yeah.

Tom Grimshaw: Very, very different.

Melvin Miles: The cold winter though, my wife didn't like it because starting in early December there was snow. We got there in October, and we left end of March, and it was snow most of the time on the ground.

Tom Grimshaw: Yeah. Yeah.

Melvin Miles: Except for October and November.

Tom Grimshaw: Yeah.

Melvin Miles: I enjoyed it. That was one of my high points in my scientific career.

Tom Grimshaw: Yeah.

Melvin Miles: The attitude of NHE at that time was, they already were going to close in end of March, I don't think they even wanted any XSE. I don't think they even wanted me to find anything. In fact, I wrote the report, and reported on the palladium and boron and everything. I got Jed Rothsfield, who translated the Japanese version, they more or less ignored my results. Or said I never saw anything significant. So the Japanese had a political game they were playing.

Tom Grimshaw: Yeah.

Melvin Miles: That justified closing it down. Dr. Sami was very nice. He was a chain smoker, he was always out in the lobby smoking, but they never did smoke where other people were.

Tom Grimshaw: Yeah.

Melvin Miles: Right.

Tom Grimshaw: Well, so tell us about the work. What did you do when you went there? What were you planning to do? What did you agree with them you were going to do? What did you wind up doing? How did-

Melvin Miles: [crosstalk 00:08:47] Pretty much run my own experiments. So I took some samples from China Lake, California. I took a plate of boron I'd never run before. I took another electro, doctor [inaudible 00:09:03] made. Plated cerium and boron. I ran it. I took electrodes that Martin Fleishman sent me, plated him cerium, and I ran those three and Fleishman found cells. That's the main part of my research.

Tom Grimshaw: Okay. Did you construct the cell there? Or did you bring stuff?

Melvin Miles: No, they already had them on hand.

Tom Grimshaw: Okay.

Melvin Miles: Pontz had made a trip in 1993 to Hokkaido NHE, and he helped him set it up. They even had a book of instructions about 80 pages. Lot of math written by Martin Fleishman.

Tom Grimshaw: Really?

Melvin Miles: How to do experiments exactly.

Tom Grimshaw: I'll be darned.

Melvin Miles: They ignored most of it.

Tom Grimshaw: Yeah. Written in English.

Melvin Miles: Yeah, it was in English. They didn't want to read the English. It wasn't in Japanese, English. I've got probably the only copy available in my room back there, and Martin, when I got back, Martin, he, Martin didn't even get a copy of his own report when he left France. He had me Xerox what I had and send it to him. It was a very detailed study of how to do the experiments.

Tom Grimshaw: So it was from the Tech Nova.

Melvin Miles: Yeah.

Tom Grimshaw: So NHE came as a kind of a follow on to Tech Nova? Would you say?

Melvin Miles: NHE? I think they were, well NHE was funded by the Japanese government. Tech Nova was at Toyota I think.

Tom Grimshaw: Right.

Melvin Miles: Right, so they were funded differently, but-

Tom Grimshaw: NHE came after Tech Nova?

Melvin Miles: Yeah. Yeah, Japanese was funded by the, they called it NEDO. New Energy Development Organization. We may even call it DOE here. NEDO in English, New Energy Development Organization. The lab was called NHE, New Hydrogen Energy. Was the name of the lab. We had one floor of the building. The [inaudible 00:11:05] people have the floor below us, and it was about the best equipped lab I'd ever seen for cold fusion. They had Cal [Rimick 00:11:14] who was in a separate room, where it was temperature control was very accurate. Except people coming in and out would kind of disturb that somewhat. The room temperature stayed pretty constant. They had the Fleishman pond three calorimetry cells. They run three at a time in a water bath. The water bath would be about this long and about this deep, and you could see each cell through the glass and through the water. What was going on in each cell.

Tom Grimshaw: Nice.

Melvin Miles: These were silvered at the top, and it was a very good calorimeter, best I've ever used probably. I came home doing very accurate calorimetry I'm sure, but NHE did not like to read all the instructions I guess, and they just developed their own method of analysis. Their big mistake was they would, the first heating post, they would use that to calibrate. They assumed there was no excess heat. Well almost every experiment I did have early excess heat. The plate of boron gives a very early excess heat effect. That threw their calibration way off. When I did co-deposition you get early excess heat, that threw it even off even worse because it's a bigger effect. Calibrating only by the third day effect threw everything off, what they did.

Tom Grimshaw: Yeah, would report much less success than actually occurred of course.

Melvin Miles: Yeah. Yeah, so their calorimetry was good, but their analysis was very bad. That was what I got out of it. I did my own analysis, and I didn't follow Martin's book because it's kind of complicated. I just developed my own way. I knew then that there was a, what Martin Fleishman called, a lower bound constant. You assume there's no excess heat, and that's a really good starting point. Then you just calculate the cell constant. The cell constant is your unknown, and if they have excess heat, the cell constant is lower than it should be. If you have no excess heat, then it's higher. You run the experiment and it reaches, the highest point it reaches is getting close to the correct constant.

When there's a small amount of excess heat, or none, that calculation will give you almost the correct constant. That's the method I used, and it worked quite well, the constant was still a bit too low, but it was close enough that I could get pretty much the correct results. To see the correct peaks, and Martin, when he did his analysis he came up with a little larger constant and little larger effects, but that was, it was still the same peaks are present. In co-deposition you see excess heat right away, the only thing I didn't know at that time, I changed the composition from this dense fog. He used lithium chloride and palladium chloride as the solves, and then he played out the palladium.

Well I read that you can get pure metallic plating deposited if use an ammonia solution. So I used deuterated ammonium hydroxide and D<sub>2</sub>O, and a little bit of the platinum, the palladium chloride. What I didn't know at that time though that, because when you plate palladium out, the positive palladium ions are replaced by hydrogen ions. Or D<sup>+</sup> ions. So the solution might start off at pH seven, and wind up around pH one or two. It becomes acid, and then you can start to get chlorine evolution, because of the chloride in the system. I found out later that, I didn't know this at the time in Japan, but the chlorine going through an ammonium hydroxide solution will react and produce nitrogen trichloride. Which is a little different smell than chlorine. I noticed when I was running the experiment in Japan people complained about the odor.

There's was the nitrogen trichloride odor, it wasn't quite the chlorine odor. It was a little bit different, and it gives you, as long as you have that reaction going on there is an extra, excess heat peak that appears very early. When Japan tried to calibrate it. It was a chemical effect, but excess heat chemically, but it was gone within a day because when you involve chloride ions it has to be replaced by negative ions, which is OH<sup>-</sup>, or OD<sup>-</sup>, and so the pH goes back up and then the chlorine evolution stops. The problem is only for one day. Then it clears up. Then you, but after that any excess heat is okay. This is a different story, but a few years ago when NRL tried to reproduce my co-deposition result, they did see this initial peak, and it's a chemical effect, and at that time I knew it was a chemical effect and I told them that. Then they wrote everything off as chemical after that. [crosstalk 00:16:32] The certain thing is this chemistry.

Tom Grimshaw: Yeah. Kind of the bottom line for your NHE work, you have the three electrolytic cells, did you do any other things? How do you feel that that's, you said that six months was some of

the best professional experience you had in cold fusion, do you think your results were successful? Did you feel that you were showing excess heat or the cold fusion effect?

Melvin Miles: I think almost everything I did, because I knew what to do, what glyose to use, almost everything produced excess heat. The only thing that was new for me was co-deposition producing excess heat, but instead of 18 milliliters of solution I had 90 milliliters. So I got a lot larger palladium deposit than before. That made a big difference. That'd be a five times deposit. The same concentration of solution I get five times the palladium deposit. For five times larger you see a bigger effect.

Tom Grimshaw: Did you write an NHE final report? You said that you input their Japanese report, but is there a report covering what you did there?

Melvin Miles: Before we left we all wrote our own reports. In fact, I got the report for myself that I wrote. I got the reports somewhere for Lipson's report. I got somewhere the report for Pauli Tripota. I got all three reports. I think [inaudible 00:18:06] has placed my report on his side. I have a copy in here.

Tom Grimshaw: Okay.

Melvin Miles: If you wanted to see it. I wrote up everything showed excess heat practically. Almost every experiment. I even brought my China Lake calorimetry and before I ran the Fleishman/Pontz I did my own calorimetry and my China Lake calorimetry with the two cells in the water bath. I brought the Johnson Matthew one millimeter diameter palladium material that I had a lot, quite a lot of it, and I still have some of that left. I ran those two in two cells, and I got excess heat in one, but not in the other. When I got back, you know I couldn't do this on Navy time, I wrote a paper on weekends, and I even went up to my cabin in Oregon-

Tom Grimshaw: Hide out.

Melvin Miles: Spent a couple weeks writing that paper. Well, mostly kind of analyze the data. You have to come up with a good analysis of the data. This was published in, actually my last reviewed journal publication, the Journal of Electro Analytical Chemistry in the year 2000. It was published.

Tom Grimshaw: Okay.

Melvin Miles: I later heard from Martin Fleishman that David Williams was one of the reviewers. He said all the reviewers were very impressed with that paper, and David Williams even said "This is about the best paper I've seen that shows the early excess heat effect." I was plotting the cell temperature over time, I had a graph that did that. It showed that just because the cell voltage is going down, the temperature would be going down. Then it would start going up, even though the voltage was still going down. Why is the temperature going up when the power going in is going down? You would see that right on the chart paper.

Tom Grimshaw: Yeah.

Melvin Miles: The voltage keeps going, but the temperature tracking the voltage and it goes up. I got that paper in there, and that's why I think they liked it because you can very clearly see the cell voltage and the temperature. The temperature's not behaving like it should.

Tom Grimshaw: Okay. Okay, so that kind of wraps up for the, for your New Hydrogen Energy adventure. Anything else you want to say about that six month period before we move onto the next phase?

Melvin Miles: Well I would, actually I was treated quite well there, but I think they had a political agenda. They had to follow what people had already decided the program was going to end, and they didn't want anything disrupting it. The last two weeks I remember was quite sad, all this very nice equipped laboratory being torn apart, put in boxes, and different university's

and other places coming and taking equipment. What was, over a two week period, it went from a well-equipped laboratory to empty rooms.

- Tom Grimshaw: Yeah.
- Melvin Miles: I was writing my final report during that time, because we had to cease all experiments by the first week in March.
- Tom Grimshaw: Yeah.
- Melvin Miles: The last three weeks of March it was being dismantled. We were working in the office area, and we didn't need to go in there. I got pictures of that, how that lab got dismantled.
- Tom Grimshaw: Let's look at what we have here.

### ***Interview 5-1***

- Tom Grimshaw: Tom Grimshaw here again. This is session number five with Melvin Miles. It's Melvin Miles and Tom Grimshaw for the transcriber. It's May 6th, and we're going to reel back and talk about the opportunity that didn't happen, Mel, and where it looked like Pirelli Tire might support cold fusion work in Milan. Please pick up from there.
- Melvin Miles: I just like to mention, [Peproda 00:00:39] had a nice laboratory, had a technician running. He did publish a paper or two on that work. One thing that went on there that always just kind of bothered me was he had a camera crew from Italy come, and they said they wanted to do a positive story on cold fusion. I guess they convinced [Fleishman 00:01:00] and Peproda to do it. So they interviewed me, and I said ... They wanted to pretend I was back in California. I said, "This doesn't look like California and this doesn't look like a California lab." They said it'd be okay, you know? So they talked to me like I was in California, and then they wanted to record, video my arriving in Italy at the hotel, driving around the corner, and then getting dropped off and going in the hotel. Did that several times and so on. Then they talked to Fleishman and Peproda and Emilio. They keep telling me it's going to be a positive program on cold fusion.
- I kept asking Emilio about it, and finally, he said ... Well, it came out. It was totally the opposite. It was a very negative. Everything was very negative about cold fusion. They tried to paint it as crooks and so on, I guess. I never did get to see it, but I didn't like even pretending I was in China Lake, but they insisted it'd be okay to do it. Maybe they kind of pointed that out. I don't know. I was faking that. Who knows.
- Tom Grimshaw: Yeah.
- Melvin Miles: But that was one story, but I was very disappointed that anything we thought was going to be favorable turned out so bad.
- Tom Grimshaw: Yeah.
- Melvin Miles: I've never seen it. I don't even know what happened to it. It was broadcast on television, national television in Italy.
- Tom Grimshaw: Well, that's probably what killed it right there.
- Melvin Miles: Well, yeah. That might have killed Pirelli.
- Tom Grimshaw: That's right.

Melvin Miles: That's funny, really though.

Tom Grimshaw: Yeah.

Melvin Miles: And the other thing, I really liked Peproda, but he was kind of fiery temper. The day we were supposed to go to ... The morning we were supposed to go to Pirelli, we were supposed to be there at a certain time. He got into shouting matches with his technician. He's constantly yelling.

Tom Grimshaw: That's so Italian.

Melvin Miles: Fleishman took me into a side room and said, "Well, this happens often." But he said, "Can't do anything. We just have to wait until he gets through." It made us late for the meeting, and I was kind of bothered that this happened. But Fleishman told me it's just something you have to put up with.

Anyway, when we went to Pirelli, it was almost like a city in a city. You had a gate. It was almost like going into a Navy base. You had a gate, had to check everything, before you could get in, and it just had a lot of buildings. You could get lost in there. In fact, Emilio got lost trying to find the right place we were supposed to go. We were supposed to meet at what used to be a very rich person's hunting lodge there on the outskirts of Milan. It was a very nice, old building that we had met in. I thought holy strange seeing such a large place where Pirelli was located. It's almost it's own city within Milan, where Pirelli's located. I thought that was interesting.

I probably spent ... We probably spent three or four days together. It was ... I spent a lot of time with Fleishman and Peproda and Emilio [Delgussi 00:04:16]. They would say they would pick me up at 9:00. I'd go down and wait at 9:00. They might show up at 10:00. They got started late everyday. We'd wind up going out to eat around maybe 9:00 or 10:00 at night and not getting done until midnight. I guess that's sort of the Italian way they do things in Italy. But that was kind of interesting.

Tom Grimshaw: Yeah.

Melvin Miles: That's the only thing I wanted to add on that.

Tom Grimshaw: Okay. All right. We'll make this be a short one because I want the other one to be a kind of a stand alone.

So Tome Grimshaw here again with Melvin Miles. Go on to the next ...

## ***Interview 1-6***

Tom Grimshaw: Tom Grimshaw here again with Melvin Miles. I believe that this is session number five, could be six. It's actually, yeah, it's actually session number six. Mel, we've been talking about the narrative of your experience with cold fusion going back to when it was first announced. In this session I'd like to take a little diversion. We're contemplating doing a project under what I've termed the LENR, the LENR Research Documentation Initiative.

Tom Grimshaw: I'm doing several projects under this initiative started with Ed Storms and there are about six or eight people that are cooperating with different projects under this initiative, including Dave Nagle, Chino Srinivasan, Dennis Pease and Arhill Gosford Skinner, Tom Claytor, and Malcolm Fowler. So under the assumption that we go forward with this project, I'd like to learn a little bit from you and I'll ask you a series of questions about what

you have that we might include in the project remembering, or the initiative, remembering that the objectives are to capture, organize, and document and perhaps even archive the results of your cold fusion research over the last 30-plus years, okay?

- Tom Grimshaw: So when I think about these projects, there are a number of categories. A very important category is the interviews that we're doing today, okay?
- Melvin Miles: Mm-hmm (affirmative).
- Tom Grimshaw: That's a big category for a project. Another big category is what cold fusion or LENR, L-E-N-R, publications that you have authored or co-authored that are in the public record. Things like journal articles, presentations at conferences and so forth. I think we have a pretty good start on that inventory of what you have, at least of what was done in the material that I got from Dave Nagle and now today Linda gave me another list of your publications, so our task there would be to go and get copies of everything and put them together in one place.
- Tom Grimshaw: I'd suggested Dropbox as one candidate but if we don't want to mess with Dropbox. We'll find out another way. Okay, so another category is unpublished reports, things that never did get into the public record. So can you tell me, do you have a lot of unpublished material that we might scan or otherwise incorporate in this project?
- Melvin Miles: There's a few. I didn't even try till the last year or so to publish in major journals. I'm trying to do that right now to see what their attitude is, but there's the paper by Marvin Fleischmann he wanted me to help him with and get it published. I went to Journal of Physical Chemistry and they sent it out for review and it got two out of three were good reviews, but then the editor squashed it. He didn't want to publish it so that remains unpublished. I've got documents on that including the editor's letter.
- Tom Grimshaw: What about manuscripts of your own that you've prepared that have never seen the light of day, so to speak in a publication?
- Melvin Miles: I tried to publish it somewhere. I think I do things with that goal in mind, right?
- Tom Grimshaw: Okay. So just about everything that you've written up has appeared in a publication then in other words.
- Melvin Miles: Yeah. I have a lot of notebooks of calculations on copies of cold fusion, but they're not really for publication. They're just how to calculate certain things. I have a lot of things like that. I keep going back and taking old data and doing new calculations, for example.
- Tom Grimshaw: Okay. Alright.
- Melvin Miles: And those are not published.
- Melvin Miles: For every experiment I've ever run, even before cold fusion, I have a notebook record.
- Tom Grimshaw: Yeah. Well that was going to be my next question. Are lab notebooks or other records that you have of the experiments, are there, I believe there are notebooks or things that we might at least inventory for the project.
- Melvin Miles: You can inventory, it'd be a lot of copying to scan all the pages.
- Tom Grimshaw: Well I tend to look at these kind of in layers. The first layer we get an inventory and then maybe the second layer we do the scanning.
- Melvin Miles: Yeah, right.
- Tom Grimshaw: Okay.
- Melvin Miles: There are some experiments more important than others. Like, the experiment I did in Japan on Palladium Boron is very important because it was documented by Fleischmann

and his analysis in the NRL Report. Then I have my own notebook data, and even the computer data is somewhere. I don't know where it is right now, but it was sent out to several people. Some of them are not living so I don't know what happened to it.

- Tom Grimshaw: Yeah, okay. Well we should try to capture as much of that as we can.
- Tom Grimshaw: Okay, moving on to the next category. You showed me in your office that you have publications by other authors. For example, you have the two copies of Storm's book.
- Tom Grimshaw: So we can get an inventory of what you have that's been done by others in the field. In other words, kind of, your library of your Cold Fusion stuff.
- Melvin Miles: Right. I've got the major books on cold fusion, like the critics and any people that are presenting the other side of it, my team now, I've got all their books, tracing that early history [crosstalk 00:06:30]
- Tom Grimshaw: Good, we will call that a category and we will include that. What about a library of papers by other writers over the years? Have you collected those? Kind of your own personal cold fusion papers library?
- Melvin Miles: Yeah, I have them collected by year, in folders. At least the ones I've made copies of, from other people. I have quite a lot of that material. Because when I write a paper I refer back to some of that.
- Tom Grimshaw: Sure. We may want to, at least, get an inventory of what you have in that category as well.
- Melvin Miles: I probably don't have things that Ed Storm doesn't have. I think he'd have all that.
- Tom Grimshaw: Yeah, yeah.
- Tom Grimshaw: Well we're talking about Melvin Miles now, we're not talking about anyone else.
- Tom Grimshaw: In other words, each one is a stand alone.
- Tom Grimshaw: What about hard copy files in general? Do you have boxes of material or file cabinets of stuff that might be considered to include in the project? At least by way of inventory?
- Melvin Miles: A lot of Navy reports that most people may not have. Including another report written by Martin Fleischmann that was published by the San Diego Navy Laboratory, but they re-wrote some of it which I don't think was good. And they left out one of the key pages of symbols that points out what he's even talking about. That was left out of that report. I was thinking of maybe sending it up to Jed Rothwell and have him scan it and put it on his website. I might do that sometime.
- Tom Grimshaw: Sure.
- Tom Grimshaw: Okay.
- Tom Grimshaw: Well of course another category would be the Martin Fleischmann correspondence that you showed me. There's stuff that you've already given to Jed Rothwell, and it's on his lenr-canr.org website, it's a big 600 page document, with a nice introduction.
- Melvin Miles: Right.
- Tom Grimshaw: I've seen that, but you have other stuff to that we might include in this project and then maybe send it on to Jed or something.
- Melvin Miles: Yeah, I have a stack of manuscripts by Martin, around the years 2003, 2004, and 2005, they're about his last major writing I think he ever did of things he wanted to publish and they never got published.
- Melvin Miles: I don't know what to do with it now.

Tom Grimshaw: Well I think one thing we can do with it is just describe it. In other words, for each manuscript, provide a little paragraph description, describing what it is and put it into the record, scan it and have it in the record. Then maybe there's another write up like Jed did for that 600 pages, maybe there's a separate write up for unpublished manuscripts by Martin Fleischmann. Something like that.

Melvin Miles: Right, yeah.

Tom Grimshaw: We can that.

Melvin Miles: Right.

Tom Grimshaw: So what about other stuff in file cabinets? Have you kept stuff over the years? Just in drawers of files?

Melvin Miles: Mainly lab notebooks and notebooks recording what took place on telephone calls and during the [inaudible 00:09:59]. Specially the early history of cold fusion. I have a pretty complete record of that.

Tom Grimshaw: That would be really, really valuable for this kind of a project.

Melvin Miles: There would be calls from a lot of different people in the field at that time.

Tom Grimshaw: Yeah.

Melvin Miles: The discussions we had.

Tom Grimshaw: Yep.

Tom Grimshaw: well your affiliation with Martin Fleischmann certainly gives you a lot of weight in the field.

Melvin Miles: Yeah.

Tom Grimshaw: Him being considered the [crosstalk 00:10:24]

Melvin Miles: I'm one of the few that took time to try to understand his mathematics.

Tom Grimshaw: Yeah.

Tom Grimshaw: He was kind of opaque to a lot of people in the field.

Melvin Miles: It was mostly physical chemistry. Of course, that's what I majored in.

Tom Grimshaw: Yeah.

Melvin Miles: And that's what I've taught, was physical chemistry. So that's what I'm really familiar with.

Melvin Miles: There's not a lot there that could write out a physical chemistry textbook that he does.

Tom Grimshaw: Yeah.

Tom Grimshaw: And the nuclear part of cold fusion, is kind of a completely separate category all in itself that's not really related very much to the chemistry. The chemistry brings it together just so that it can happen, but what actually happens is still a bit of a mystery.

Melvin Miles: Right, yeah.

Tom Grimshaw: That's what I say too, about Storms and his hydroton and cracks, you know it's all just chemistry. Until you get where the nuclei, the hydrogen atoms, the deuterium, the deuterons and the protons, the protium, what they interact, that's the big mystery. That's where the Nobel Prize is.

Tom Grimshaw: Anything else you can think of that we might incorporate?

Melvin Miles: Well we have all those tapes that Linda took when she went with me to meetings. She would often video tape things. That would take a long time to go through, but...

Tom Grimshaw: That's another very good category though.

Tom Grimshaw: Well like, I say, we would do this in layers. I would probably ask to come back and visit, maybe spend a couple, three days.

Melvin Miles: I think especially the tape on the ACS controversial meeting that we had in Ontario.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: A lot of people tried to keep us from having that meeting. And threatened to resign from the ACS. I've got a lot of records and notebooks about what different people said about this.

Tom Grimshaw: Yeah that would be really interesting.

Melvin Miles: How we finally go to hold the meeting but got sent off to a remote place in the same building. And not to advertise it anymore.

Tom Grimshaw: Well that's typical of this field as a pariah science. So that's part of the narrative.

Melvin Miles: I got [inaudible 00:12:53] saying that's one of the best meetings that took place. We had a lot of key people there. Martin Fleischmann came from England, Ben Bush from Texas, and McKubre came to that meeting and the other Bush was there. And there's other people too, but I don't remember who they all were. A one day meeting is what it was.

Tom Grimshaw: Anything else you can think of that we might consider, incorporate? At least in the inventory?

Tom Grimshaw: Well I guess I should ask this, sorry to interrupt. What about electronic files? Do you have old Cd's or zip discs or three and a half inch floppy's? Different kinds of media containing your experimental information?

Melvin Miles: If I do, Linda would know about it. Put it that way.

Tom Grimshaw: Okay.

Melvin Miles: I don't keep track of those things very well. I don't think I have too much in that form.

Tom Grimshaw: Okay.

Melvin Miles: And most of it is just a... there's a video recording that Linda made in Italy with Martin Fleischmann. He wanted me to report... He was very concerned that cold fusion could be weaponized. His words were that this should've never seen the light of day. That was his words. It should've been confined to a government laboratory, until we knew more about it. And he thought it could be weaponized and he wanted me to record what he said about it and give it to Navy Intelligence, which I did. But what they did with it, I have no idea. There's even a meeting at the Navy Intelligence in Washington, D.C. I was invited to give a presentation. They invited some people from the CIA to come there, and I went kind of over what Martin said about the possible use of it as a weapon. [inaudible 00:15:11] with deuterium and then you produce it... Subject to stress like a high temperature or explosive stress or something and... So at least I gave them what he wanted me to give them.

Melvin Miles: But afterwards a CIA person came up to me and said "This is off the record." I think that means I can tell somebody, I guess, I don't know. But he said when cold fusion came out the Russians were already experimenting deuterium in metals and subjecting it to an explosion. And they were getting a lot more energy back than they expected. At that time they [inaudible 00:15:52] cold fusion and I said hey this might be how it works. This might be why it works. I don't know any more about that, other than that.

Tom Grimshaw: In the Malcolm Fowler interview, he talks about, somebody at Los Alamos during the hey day, you know, just about everybody at Los Alamos was after this phenomenon in the weeks and months after the 1989 announcement. He said somebody was using C-4 on it. Doing the explosive compression on it.

Melvin Miles: Oh yeah, yeah.

Tom Grimshaw: So that's been looked at.

Melvin Miles: Somebody had to do it.

Tom Grimshaw: Somebody did look at that.

Melvin Miles: I had people at China Lake come in and say you need to get a big bunch of material and load it up deuterium and then give a big current pulse through it and see what happens. I never did that but that was his idea. He had a lot of explosive extras there at China Lake.

Tom Grimshaw: Yeah.

Tom Grimshaw: Lets go ahead then...

Melvin Miles: One other thing I didn't mention, Martin Fleischmann told me that we should try to get this looked at, at China Lake. So my last year or two at China Lake I wrote up a IR, means Internal Research, they later changed it to ILIR, Internal Laboratory Internal Research, because we joined another lab in California.

Melvin Miles: So I wrote up, with the help of Martin, the things to try. And Martin even wrote comments on the proposal I had. We set it, how things should be tried, how to stress these materials. We even got funding for it and we were about ready to do testing for it in remote areas at China Lake, suddenly the funding was pulled away. Which had never happened to me before.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: That's when, within a month or two, and suddenly I was out any funding again. Then I got offered to go to Tennessee and teach for a whole year so I did that. I never gave much else to China Lake after that.

Tom Grimshaw: Yep. So we will pick up that story in the next recording about what you did in your Leave of Absence to go to NHE.

Tom Grimshaw: Let's talk just for a moment about who you worked with, in the field. You mentioned a number of different names.

Tom Grimshaw: Of course, Martin Fleischmann, Stanley Pons, Legowsky, and Bush at the University of Texas. What other names come to mind?

Melvin Miles: Stan Spock and Pam Boss for Navy Laboratories. I knew the people that worked at NRL even though I don't think they did very good work. I think the people that could have really helped, they didn't want to touch it. Like Deborah Rolison, she was the well known electro-chemist there, who should've been involved. There were some other people there that was supposed to be helping her but they didn't want to get involved with it.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: I was involved a little bit with Dennis Letts, he...

Tom Grimshaw: What were the names, excuse me for interrupting. What were the names of the individuals at NHE that you worked with?

Melvin Miles: The main technician I worked with was Mr. Sumi. The head of the group was Dr. Asami, he's passed away. Dr. Asami and Mr. Sumi, he was an engineer I think, not a Ph.D. They weren't research scientists, they were kind of engineered into a useful device.

Tom Grimshaw: Yeah. Right.

Melvin Miles: Read my letter to nature. I bring this up, it should've never been discussed as a answer to energy until we new more about it.

Tom Grimshaw: Right.

Tom Grimshaw: That's still going on today by the way.

Melvin Miles: Right, yeah, on CMNS. Even today.

Melvin Miles: I mean unless we understand it, you can't know what it's going to be like or whether it's ever going to be useful.

Tom Grimshaw: Well some people think that you can Edison your way into this with brute force and the empirical rather than the true scientific way.

Melvin Miles: Yeah I think if it would have been announced... Again I blame Steve Jones for a lot of this, Martin Fleischmann did not want to go public for at least 18 months or longer. He said he didn't want to give a press conference, but Jones was going to present results that wasn't even accurate, claiming he did the work first. He would have wiped out their any claim to patents at the University of Utah. So they were put in a really bind by Steve Jones.

Tom Grimshaw: Yeah.

Melvin Miles: Nobody ever reproduced Steve Jones to this day. He was just jumping in there, trying to get credit. And he caused the whole mess, in my opinion.

Tom Grimshaw: Yeah because he had the proposal that...

Melvin Miles: I know, he's a member of my church, I don't care.

Tom Grimshaw: Yeah he had the proposal...

Melvin Miles: Yeah he reviewed the proposal.

Tom Grimshaw: Right.

Melvin Miles: And he did something somewhat related and then he wanted to jump right back in to what he was doing and claim he'd been working on it for three years continuously. But there was a big gap in between. The gap only started up again when he read the Fleischmann-Pons Proposal. He was nowhere close to what they were doing, but he wanted to claim that he did something that involved palladium and deuterium and he wanted to claim that he [inaudible 00:21:58] to show neutrons, and he wanted to go present that in a meeting that would've been prior work for any patent. And he caused the whole thing to get off to a bad start.

Tom Grimshaw: Yeah

Tom Grimshaw: So he ...

Melvin Miles: Do you even know where Steve Jones is? Do you know what he did later? He was in the news again a few years later, with a silver cooker. For how to use the suns energy to cook food in third world countries. Got back into the news. He has to be in the news somehow. Then he jumped into the twin towers, inside job thing.

Tom Grimshaw: Yeah, I remem...

Melvin Miles: Government people brought those buildings down, not airplanes. That's when BYU said we have to part ways. He had to be in the news somehow.

Tom Grimshaw: Yeah. I did not know about his being asked to part ways at BYU. I always figured there was some kind of competition going on between the University of Utah and BYU, that might have been at play in all that, but maybe not.

Melvin Miles: No. Steve Jones wanted to claim that he'd been working on this for years and that he had evidence for neutrons and he talked about a new way to do fusion and condense matter. All this came out after the Fleischmann-Pons Proposal. So naturally they were suspicious, they wanted to see what he had done and they saw little tiny baby food jars that he was doing experiments in. And Fleischmann-Pons said "That can't represent three years of work." Little stupid little jars, even other people around Jones commented on it, that their work, what they were doing looked so primitive compared to Fleischmann-Pons demonstration class they brought down.

Tom Grimshaw: Yeah.

Melvin Miles: Right.

Tom Grimshaw: Yeah.

Tom Grimshaw: Well that's probably enough on that. To be honest with you, a lot of times I kind of don't get into the personalities because they'll kill me in this...

Melvin Miles: I'm just saying that's why it got off to a bad start.

Melvin Miles: Fleischmann and Pons knew what Jones was doing and they knew he was trying to claim prior credit for patents and they had to protect what they did.

Tom Grimshaw: Yeah.

Melvin Miles: But they didn't want to do it.

Tom Grimshaw: Yeah.

Tom Grimshaw: Well part of the narrative is to the University officials putting pressure on...

Melvin Miles: I don't know what pressure...

Melvin Miles: The President, Chase Peterson, says in his book "He couldn't tell Fleischmann-Pons to hold a press conference." Maybe he didn't do it but I think somebody did. In order to protect patents you have to go public.

Tom Grimshaw: Yeah.

Melvin Miles: Probably wasn't the President of the University, but there could've been patent people at the University of Utah that said if you want any claim in this you've got to go public now, before Jones.

Tom Grimshaw: Yeah.

Tom Grimshaw: Okay, well anything else comes to mind, I guess any other collaborators that come to your mind that you worked with over the years?

Melvin Miles: No just right now with Dave Nagel and Steve Katinsky.

Tom Grimshaw: Right.

Melvin Miles: And Dr. Imam. And Peggy Boron.

Tom Grimshaw: On the LENREA Project, the LENREA EAP, I can't remember what that ...

Melvin Miles: Yeah.

Melvin Miles: I was supposed to go back and work for them, but I just told Dave Nagel that I just don't feel like I'm able to do it.

Melvin Miles: Well Linda would be upset if I left her here by herself, she doesn't have any relatives here like she did in Bakersfield.

Tom Grimshaw: Yeah. Right.

Melvin Miles: And she's followed me once or twice and something could happen and my being away and [crosstalk 00:25:45] I just think it's kind of practical point of life to be living by yourself.

Tom Grimshaw: Yep I get that.

Tom Grimshaw: Well lets end this recording and take a break.

Melvin Miles: Are you still recording?

Tom Grimshaw: Yeah.

Melvin Miles: Oh I didn't know that.

Melvin Miles: Well you have my thoughts about Steve Jones.

Tom Grimshaw: Yeah I kind of knew about it already.

Melvin Miles: I mean Steve Jones is dishonest even though he's Mormon. And Fleischmann-Pons are honest even though they're not members of my church. Hmmm that's the way it works out.

Tom Grimshaw: Yeah. Funny about that.

Tom Grimshaw: So this is Tom Grimshaw with Melvin Miles. This concludes session number six. And Mel if you're still up for it, we will make more progress, after we take a break this afternoon.

### ***Interview 1-7***

Tom Grimshaw: Tom Grimshaw here, again, with Melvin Miles. It is our seventh session here on May sixth at Melvin's home at St. George, Utah. So, Melvin, in previous sessions we've talked about your early career and then your early cold fusion career at the laboratory at China Lake. The Eagle Laboratory at China Lake. I think where we left off with this story was you had finished your time, your, six months at New Hydrogen Energy Laboratory in Japan and you were on leave of absence from the Navy at that time. Can you pick up the story about happened at China Lake after you got back? Excuse me.

Melvin Miles: I did not do much work on cold fusion, I know that. In fact, I wasn't allowed to work on cold fusion. I just did whatever I could to, you know, had funding. I got into [Croatian 00:01:22] studies and people at China Lake were developing polymer coatings to slow down corrosion and I ... the electro chemical wave testing that was what they call electro chemical impedance spectroscopy, it was done with a ... So, I had an instrument that could do that, so we got into that. I would still work on cold fusion things, paying my own way. I'd go to meetings. Scott Chubb at that time was sponsoring a lot of meetings associated with the American Fiscal Society that we'd have a session at those meetings. I remember going to them in various places like Minnesota. What's a big city in Minnesota, I don't remember?

Tom Grimshaw: Minneapolis? Could be ... Minneapolis

Melvin Miles: Minneapolis, right.

Tom Grimshaw: And there was one in New Orleans. I think I met ... That was the first time I met you was in New Orleans [inaudible 00:02:32].

Melvin Miles: Yeah, I was there at that meeting and actually, I went to that meeting. I was funded because of the electric chemical work I was doing actually for that meeting and about using this technique for[inaudible 00:02:45]corrosion. It was sort of just catching on. Using the electric chemical method to predict how things would hold up to corrosion just by doing electric chemical test. In short, it was called EIF, Electric Chemical [inaudible 00:03:01]. It got even more popular during the later. A lot of work done. But that was very early work in that area.

Tom Grimshaw: Sorry about this cough.

Melvin Miles: Then, Martin Leishmania urged me to try and get the Navy to look at cold fusion and weapon research. So, with his help, I wrote the proposal and, got some other people kind of like interested. They could do test ... help do the testing and ... and that was 1999, 2000. Maybe it was more 2001 and, we got funded and we were getting started on that. We were about ready to run tests. If you do calculations, you get one[inaudible 00:03:54][inaudible 00:03:54]centimeter flagellant load of the deuterium one to one that one cubic centimeter if you got all the energy out suddenly, it would be 31 tons of TNT.

Tom Grimshaw: Oh!

Melvin Miles: Lot of people don't recognize that fact. And so, when I told the people that we had to find a safe place to do the testing, because even a small amount, if we suddenly released all the energy, that would be ... that would wipe out the laboratory.

Tom Grimshaw: Hmm.

Melvin Miles: And so, we had to find a remote location that kind of like to even do that kind of testing. And, I had ... The people I was working with they had ways of stressing the latum by mechanical means and things like that. So,. ...

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: We were going to do those testing. And we got the funding and we were on the way to getting the test ordered when the funding was withdrawn. First time that ever happened to me. They gave out the internal funding and then they take it away. And, I never got an explanation for it. But, I sometimes wonder if ... if the military already new about a cold fusion weapon and, they tried to down play cold fusion because of that. They already knew about it. They didn't want anyone getting interested in doing it. Maybe, it may have been a black program. Nobody was supposed to know it except those working on it.

And, I sometimes wonder if Mike Millie knew about it because, he quite evasive exactly he was doing with the Navy. He said ... One time he told me, his job was to know as many as people as possible and, bring the right people together. He had a home in Florida. Often worked in Washington D.C. and, often ... and sometimes in Monterey, California. So, it was kind of an unusual situation. But, I never got a straight answer from him exactly what he did.

He said ... I knew he went to every cold fusion meeting and conference until he wasn't able to go to one. But he was always attending the meetings. And even, Edward Keller's protégé, Lowe Woods, sort of under the radar; was at a lot of cold fusion meetings.

Tom Grimshaw: I met him at a cold fusion meeting in Pindar [inaudible 00:06:20].

Melvin Miles: And, Martin Leishmania always thought he might be ... 'cause he knew something of what was going on in this Navy and weapon seal that was really ... and ... he was often ... He was even in the one Colorado last year, right?

Tom Grimshaw: I don't remember seeing him in Colorado. That doesn't mean he wasn't there.

Melvin Miles: Okay. I seen him at ... recently. It might have been Italy or somewhere perhaps. But he ... he's keeping. He's got pretty good track but, he might be retired now. But, he was working with Edward Keller for a long time.

Tom Grimshaw: I did not know that.

Melvin Miles: Yeah. They called him Edward Keller's protégé is what Martin Leishmania to discuss him.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: I just tried to get along in the Navy doing whatever I could. And ... before I went to China Lake, Richard Carr took over for Bob NATO ... I mean not ... Bob Nova. Bob Nova went to Darth. From Darth he funded Mike Vinculin for quite a few years, cold fusion research. So, I didn't get any more funding from Bob Nova after the cold fusion program ended. But, I talked to Richard Karin and he was going to fund me. He took over for Bob Nova.

Then, they found out I went to Japan to work on cold fusion, and he refused to fund me after that. When I came back from Japan, we supposed to ... I met him and was supposed to have a meeting with him after lunch and, he said, "Meet me a certain place at 1 o'clock." And, I went there at 1 o'clock and he never showed up. Finally he showed up about 20 minutes late and, he says really abruptly told me he didn't want to hear a word about cold fusion from me. And ...

So, I never really got any funding from him. But, he would fund people at China Lake on electro chemistry. I was really the only electric chemist there. But, he would fund other people in an electro chemical area then, I would have to work for them. Working on the electric chemistry part. And that's ... That's how my career went though. When I got a chance to go to back to Tennessee to teach for one year. I was quite happy to leave.

Tom Grimshaw: So, did you resign then from China Lake?

Melvin Miles: I took a leave of absence for one year. But ... I probably resigned. I came back. I left in August of 2001. I remember that because I was there only a few weeks in September when 9/11 happened. And, I... how that affected the students while I was teaching and everything. In fact, we couldn't hardly teach. We just had to discuss why, what happened in New York for a day or so.

But, I didn't do any ...I did do a little bit of research in cold fusion at the University. I was teaching a physical chemistry lab. And, Martin Fleischer once remarked to me; he would like to see amazement done on how boiling point of decal changes as a [inaudible 00:09:41] decentralization changes. Because as the decal boils away, the LYOD is more and more concentrated.. And ... and there's the boiling point evaluation depending on the concentration. And, So I brought the child eggs to mystery set with me from when I went to Tennessee for one year.

Towards the end of the semester, I had the students do the experiment of seeing how the boiling point is affected by the LYOD concentration. We got a paper prepared from that. I wrote the paper but, put all the student's names on it. The students are in the class. And, this was presented in China ICCF 9, I think in China. And, all the student's names were on the paper and they student's didn't have to go through teasings. But, it shows how the boiling point goes up with the concentration. There is a theoretical way it goes up but, the actual rate it goes up is actually a little less than that. So we compared with theoretical and experimental. And, that was one of the publications I have on cold fusion was ICCF 9, with the student's help.

And, then, I applied for the Falcon job there but, the person in charge, he had a friend he wanted to bring in and, I finished second. So, I got a good recommendations from the

department head. He sat in on one of my classes. He really liked it. But, I taught him physical chemistry. And, he said he would be glad to write a recommendation. He gave me a very good recommendation and so, there was a opening at Base College in Maine. And, with his recommendation, I got interviewed and got a one year position in Maine. I was really wanting to see what Maine was like and, I spent one year there. But it is a cold winter and, my wife didn't like it because of the snow and ice. It was quite a different climate. She had spent her whole life in California. But ...

Tom Grimshaw:

Remind me the names of the Universities in Tennessee and then Maine.

Melvin Miles:

In Tennessee, it's the second largest University in Tennessee. The University of Tennessee is the number one University. That is in Knoxville Tennessee. Then Middle Tennessee State University because the campus is fully the geographical middle of Tennessee from North to South and East to West, middle point of that is on the campus of MTSU.

Tom Grimshaw:

Ahh.

Melvin Miles:

Middle Tennessee State University and, also back in the 60's, they had to open up for blacks to attend. It couldn't be all white. They had a all black tenant, a University in Nashville that was call Tennessee State University. And, so it had to let blacks in and MTSU had to let blacks in, in the 1960's. Middle Tennessee State University, so we had 10% black students and they had about 10% white students. I don't know how it is today. But, that is how it was then.

Tom Grimshaw:

Yeah.

Melvin Miles:

And, for the first time they allowed blacks ... let blacks play on the basketball team when I was there. Of course, they[inaudible 00:13:06] the sport. But, they were best at it. I don't mind blacks at all. A lot of them ar quite friendly when they talk. But, when I think back, I never really had a really good black student. The best they ever did maybe, they got a "B".

Tom Grimshaw:

Yeah

Melvin Miles:

I don't think they grow up with a proper training of how to study and things like that. It holds them back somewhat. But, they were nice kids. I enjoyed teaching them.

Tom Grimshaw:

And, what about in Maine?

Melvin Miles:

This was in Tennessee.

Tom Grimshaw:

Right.Yeah, but what was the name of the University.

Melvin Miles:

Bates College

Tom Grimshaw:

Bates College.

Melvin Miles:

It was a private college. And, as I recall, it was almost all white students there.

Tom Grimshaw:

Yeah, that makes sense.

Melvin Miles:

I never thought about it. But, they are quite selective on who they let in. But they were great as students. They take the top students to go there.

Tom Grimshaw:

So, were you doing cold fusion while you were at Bates?

Melvin Miles:

Well, I taught a class on Electro Chemistry there. And, I went into Electro Chemistry Intersession class, a short class. We did laboratory things, and a Introduction to Electro Chemistry. And, we went into cold fusion. I think students were quite understanding and enjoyed hearing about it there. And, I had someone thought that they would, that I would become famous someday.

But, it was a nice one year there. The winter was quite cold. I remember walking to school with is being twenty below. And, you don't ... you have to keep your ears protected and things like that. But, quite different the Southwest where I grew up. Here you worry about getting stranded in the heat of the summer. There you get worried about getting stranded in the cold of the winter. That could be dangerous. Right.

Tom Grimshaw: Very much.

Melvin Miles: Right.

Tom Grimshaw: Yeah. When I was growing up in South Dakota. You never wanted to get caught in a blizzard.

Melvin Miles: Well, yeah.

Tom Grimshaw: I spent the night one blizzard night, I was working at ... in the Geology building and, I wound up spending the night there. I couldn't get out.

Melvin Miles: Yeah.

Tom Grimshaw: Had to stay there.

Melvin Miles: That reminds me one time I was late leaving Middle Tennessee State University, when I went, this is when I taught there. The first time I went there was 1969 to 1978. I taught full time until I went to China Lake there.

Tom Grimshaw: Oh, I did not know that.

Melvin Miles: But, you know, I started off working for the Navy. And, then I closed out the Navy and lapped down in Chron, California. And, we became part of China Lake. Some of the people were invited to China Lake. But, they did not invite the research people there. I was ... I was later told that if I waited, they would have hired me. But, they didn't want hire the whole research department.

Tom Grimshaw: I see

Melvin Miles: They started inviting me back for the summers. I would go work there in the summer. And, it added to my income. The only 2 summers I didn't go there was the two summers I got invited to work at Brookhaven National Laboratory. And, not on cold fusion, but on [inaudible 00:16:33] electrolysis which is somewhat related. They wanted to, this is back in the 1970's, they wanted to do ... for energy sources they want to black [inaudible 00:16:46] water and get Hydrogen, store hydrogen in a metal condition like titanium, iron titanium. And, then lease it out when they need energy into a feeder cell. They were kind of ahead of their times. But they did have a lot of that worked out. And, I worked on that at Brookhaven for two summers. And, that was before cold fusion existed.

Tom Grimshaw: Yeah. That was in the 1970's you say.

Melvin Miles: 1970's, Right.

I had two summers at Brookhaven National Laboratory, which was a nice experience. I thought it would be like New York City but it's out on Long Island, rather rural at that time. Maybe not today but, in the 70's you could pick strawberries and places like that on land.

Tom Grimshaw: Yep.

Melvin Miles: And, you drive all the way to the end and that's Montague Point[crosstalk 00:17:37] in Long Island. If you wanted to go to New York, you could. And, we did that a few times to see a baseball game or something or just walk around and the Statue of Liberty and did all that stuff.

Tom Grimshaw: Yeah. Right

Melvin Miles: But that was ... Those were the summers 1973 and again in 1974. I would have gone back and even, a few years later but, I don't remember the exact years later but, they wanted to hire me. They same time I got a chance to be hired by China Lake, I also had an invitation to be hire Brookhaven National Laboratories. And, I wound up because I wanted to go back west, I ended up going to China Lake.

Tom Grimshaw: Yeah.

Melvin Miles: And so, that ... and after Bates College, I went to University of Laverne. Because when you appointed, they kept renewing it and it and it wound up being three or four years.

Tom Grimshaw: Okay. So, at going back to base, I want to go ahead and ask this question again. Were you able to do any cold fusion stuff during that year you were at Bates?

Melvin Miles: Uh. Just teaching the class really ... a little bit of cold fusion. That's all I did there.

Tom Grimshaw: Okay. Then you went to the University of Laverne. Which is where?

Melvin Miles: The town, the city is called Laverne. But there is so many cities in that area. Pomona is to the south.

Tom Grimshaw: Oh, it is in the L.A. basin.

Melvin Miles: Yeah.

Tom Grimshaw: Okay. I didn't know that.

Melvin Miles: Lermontov ... Lermontov to the east. And mountains are to the north.

Tom Grimshaw: Yeah.

Melvin Miles: Right

Tom Grimshaw: I am very familiar with that 'cause I got interested in Montero college at one point ...

Melvin Miles: Yeah

Tom Grimshaw: and, still am.[crosstalk 00:19:19] They do process theology there, which I am very interested in. Anyway ...

Melvin Miles: That's only ...That's just about 10 miles east, 10 or 15 miles east of the University of Laverne. And, Pomona's to the South. You know some cities keep graffiti out but Pomona you have it all over and Laverne you didn't see any graffiti. The different police departments. They kept that kind of thing out of the city there. But, you see just across the border, Pomona you would run into it.

Tom Grimshaw: Yeah.

Melvin Miles: After 3 years, we got tired of living in an apartment and, we owned a house in Ridge crest so, we moved back to Ridge crest after that and, then somebody that was working for the Navy wanted to get a program going on thermal batteries. Now, I could of done this when I was doing ... put on stockroom duty. But, they got an army contract for me to do thermal battery research while I worked on that at China Lake.

Tom Grimshaw: So, back up to China Lake then.

Melvin Miles: Yeah, I wound up back in China Lake as a ... what do you call it .... uh. I wasn't part of the Latin Navy. But I was a ...

Tom Grimshaw: Consultant or uh...

Melvin Miles: No, there is another word for it. I can't think of it.

Tom Grimshaw: Yeah.

Melvin Miles: But uh ... Contractor.

Tom Grimshaw: Contractor, yeah. So, what year was this?

Melvin Miles: Um, about 2006, 2007.

Tom Grimshaw: Wow. Okay, I didn't know that you went back there.

Melvin Miles: Right.

Tom Grimshaw: As a contractor.

Melvin Miles: I spent 3 or 4 years back working at China Lake working on different things. At one point, I even got a grant, a funding from Defense Threat Reduction Agency.

Tom Grimshaw: DETRA

Melvin Miles: DETRA. And for one year to work cold fusion and they called [inaudible 00:21:37] is what they called it, I think.

Tom Grimshaw: Yeah. Yeah

Melvin Miles: So, even though I was unfunded but I was a Navy worker, they hired me back as a contractor to do things that I could have done back then.

Tom Grimshaw: Very ironic. So, I want to reel back to Laverne. During the years you were there. Did you do any cold fusion work?

Melvin Miles: Uh.. I taught the department Chairman, it was Dr. Parsvottanasana Irash. He fled Iran when the Mola's took over. He and his wife. They went ... they said they were going to a meeting in France and then they just stayed, never went back. Then, wound up in Laverne and, he was at the cold fusion conference in Italy. But, I got him interested in cold fusion. Well, he always thought there was probably something to it. But some people aren't just skeptics see, he thought it really sounded reasonable. And, I talked to him about it.

He wanted to try Zillite and get planum to put inside the faillite Not ... not the way normal ... most people do it but by making an organic metallic compound or buying a plated compound which is organic compound. It's not within organic solvent. Where it would flow into the Zillite. And then they would take the Zillite and heat it up and dry the solvent out and, heat it up and then the planum would become planum metal inside the faillite. And then it would emit But erium gas through this dried sample and, [inaudible 00:23:15] temperature and when they had a cladium and But erium gas, they would see the temperature rise gone.

They did the same thing with cladium in Hydrogen gas, it would stay level and so, the amount think he ever did.f cladium being in the faillite, I thought that was a very high ... in terms of maybe kilowatts per cubic centimeter was taken place to do that [crosstalk 00:23:46]. That was my estimate anyway.[crosstalk 00:23:50]

But Irash doesn't like to publish things unless he it has it protected by patten. You know ... he was quite reluctant to ever write anything out and, I did. He did give a presentation at ICC 14 in Washington D.C. He was there and he gave a presentation. His slides became public on that but, he never has written much.

I still correspond with him and talk with him on the telephone. He is about ready to retire probably but, he's done a lot of other studies; the faillite, and related to cold fusion. When he thinks he even has a theory to explain it, the affect of electric fields inside a cavity and, some where to see inflated metal.

And, he said that unpaired electrons are important because he is an expert on Sutra Ma spectrometry any more. And, he said, "If you have unpaired electrons, you can affects. Unpaired electrons affect the nucleus then affect the spectrum. So, electrons can in fact, the

nucleus of atoms, from what he's seen, within more. I have looked it up. It's called Dynamic Polarization Affect or, something like that. And, you can look it up on the internet. But unpaired electrons build the fact the nuclear magnetic resonance spectra. So, electrons do affect the nucleus. And, somehow that's related.

Tom Grimshaw: Mm-hmm (affirmative). Interesting Okay. Okay so then published.

Melvin Miles: No.

Tom Grimshaw: Do you have enough that you could write it up?

Melvin Miles: I am trying to get him to send me something so I can at least start writing it up. And, maybe he will, but he ... he has trouble seeing. Of course, his eyesight not that good and he had to more or less give up his position teaching because he couldn't grade papers anymore. Anyways, he is about time .... age to retire anyway.

Tom Grimshaw: Yeah. Okay. So that's Laverne. And then from Laverne you went up and you actually lived in China Lake during your contract years.

Melvin Miles: After Bates, I got a, like I said, a year by year contract for Laverne. And then, I ... could have stayed longer ... but I, we got tired of living in the L.A. area and so we....

Tom Grimshaw: That's very understandable.

Melvin Miles: I didn't get tired, but Linda did, put it that way. The traffic and trying to go anywhere. So, it was a great climate though. Winters were really nice there.

Tom Grimshaw: Yeah. That's where I met my wife was in Terence

Melvin Miles: Yeah, Yeah. Really?

Tom Grimshaw: Yet, I met ..

Melvin Miles: Winter weather is hard to bear.

Tom Grimshaw: Yeah especially when you get down there by the peninsula where you get those breezes coming in all the time. They are really nice.

Melvin Miles: Yeah.

Tom Grimshaw: No smog down around Terence, you know.

Melvin Miles: Oh, yeah.

Tom Grimshaw: Anyway.

Melvin Miles: Around Terence?

Tom Grimshaw: Yeah. But Hormonal is bad for smog.

Melvin Miles: Yeah. Right.

Tom Grimshaw: Because your right up against the mountains there. It blows all right up against the mountains.

Melvin Miles: Well, Laverne is direct north of Eomona.

Tom Grimshaw: Right

Melvin Miles: It can be bad at times. But the end of Santa Anna winds, they don't hit Lavern very hard. They come down more through the ... further south in the valley.

Tom Grimshaw: Yeah

Melvin Miles: I have been out running on the mountain areas and, you see the dust storm and it blinds dust all the way to the L.A. basin. But, up in the Laverne are is no dust. We missed the Santa Anna winds there. It protects them around.

Tom Grimshaw: I guess you had already left by the time the Northridge earthquake hit. Were you living in that area when Northridge?

Melvin Miles: I was actually in a Motel outside Disney... Disneyland when that happened.

Tom Grimshaw: Really?

Melvin Miles: Right. I remember being awakes by it and, really rattled things up. We thought they would already close Disneyland. But, it was open the next day.

Tom Grimshaw: Yeah

Melvin Miles: They closed one or two things. But, it was open.

Tom Grimshaw: Yeah. I was there when it happened.

Melvin Miles: Yeah.

Tom Grimshaw: I was living in Texas. I was spending half time in ....

Melvin Miles: 1993 or something like that. 90 ...

Tom Grimshaw: Yeah. Yeah. Somewhere in there.

Melvin Miles: I didn't know Linda then but she was in that ... down there too. They couldn't even drive up to Vegas because both ridges collapsed up in that area.

Tom Grimshaw: Yeah

Melvin Miles: Where the intersects come together. It was ... the interstate roads, where 14 goes off and I-5 keeps going over. Some people died there and it collapsed the interchange there. They had to drive clear over to west ... or over east to pick up the other pass ... and then back to Bakersfield.

Tom Grimshaw: San Bernardino or, what is that big pass there? Um, Anyway.... So, you went up and you were living and you had this contractor jobs. Was there any cold fusion during those years when you were up at uh ...

Melvin Miles: In China Lake?

Tom Grimshaw: In China Lake.

Melvin Miles: Yeah, the same department chairman was there that didn't like my work on cold fusion. Even to come back to thermal work, he didn't want me in chemistry. So, the person that was in Charge of this company, I was working for a contractor company called MEI, Illinium Engineering & Integration. And, they had quite a bit of political power in me. They didn't try to force that issue. But, they got me a lab in another area which, was actually better. Because they set up a new laboratory. I had the whole ... the small room as a lab and I had that room to myself for an office. Thermal Battery Research with an army contract. Then the DETRA work for cold fusion. And, then then[inaudible 00:30:04][inaudible 00:30:04]. They just kind of let me stay there. You know, your not allowed if you don't have funding. I worked another 2 years on cold fusion without funding.

Tom Grimshaw: Really.

Melvin Miles: Until a new department head came in and he said it was not legal, you know

Tom Grimshaw: Get out of here

Melvin Miles: To be here without funding. So when he came in, I left.

Tom Grimshaw: What was the nature of the work you were doing during that time frame in cold fusion ?

Melvin Miles: I was co-deposition except I was looking at a lot of other metals other than cladium, like the deposition of nickel and ruthenium and radium and different con station metals to see if you could see any affect. And, I could only get back to the cladium.

Tom Grimshaw: Yeah.

Melvin Miles: And then Peter Artemether came up with the idea that co-deposition, if you do it with a higher current, you create a lot more vacancies. He said [inaudible 00:31:01] had gotten away from that and gone to lower currents. And, he said try deposition of the higher current. It did give you more [inaudible 00:31:09] when you caused the playing at a higher current.

One thing you don't get is the deposit is not as nice. You get more indridae formations but, I even try to manipulate at the very end. Near the end of that research room and, I deposited nickel at a high current in deuterium D20 and I saw a extra seed affect the nickel. But, I was never able to follow up on it.

Tom Grimshaw: Mm-hmm (affirmative). With Deuterium.

Melvin Miles: Yeah.

Tom Grimshaw: OH well. You know. Parting line is you get nickel with Parapodium. Excess heat with podium and nickel; and deuterium and palladium.

Melvin Miles: There you go. I mean, nickel, if you can ... nickel would be a ... if you can get if delirium to soak in, that's the problem. By doing it at a high current, you create a lot of vacancies in the nickel.

Tom Grimshaw: Yeah

Melvin Miles: And that was the very end and I couldn't reproduce ... I didn't have time reproduce that experiment. The nickel deposited at a high current. It looks like it did give some excess heat [crosstalk 00:32:20] but, it's never been published.

Tom Grimshaw: Okay. I was wondering was there any kind of a report or anything? That you wrote it up anywhere?

Melvin Miles: I might of written a paper on that. I have to look for it. But, it's good with other metals I looked at.

Tom Grimshaw: Well, that's okay.[crosstalk 00:32:37] The whole effort would be worthwhile to document and, you know, that you did it. Okay, then what?

Melvin Miles: In 2017, there was interest in cladium form again. And, I didn't know what the cladium form electrode I used in Japan. I couldn't find it. Steve Kandinsky came up and, he started a day or two there. And, we went through my garage and we found that Cladium form electrode finally.

Tom Grimshaw: OH

Melvin Miles: It was in a little glass vial sitting on a shelf in my garage.

Tom Grimshaw: Hmm. I didn't know that story.

Melvin Miles: Same one I used in Japan was the same dimensions, the same size and I had it marked. But that's what it was.

Tom Grimshaw: Nice.

Melvin Miles: I set up an experiment in Ridge crest. Linda was in Bakersfield and I had the kitchen to run experiments in {laughter} and I only ran it for about 3 weeks but you see that paper I gave you, it's the Ridge crest Independent. And, that's part of what I presented in Colorado.

Tom Grimshaw: Yeah.

Melvin Miles: That was 2017. In 2018, we were getting ready to move. And, I'd probably write some papers but, I haven't set up anything here yet.

Tom Grimshaw: So, did you write up what you did at Ridge crest? Is there a manuscript or anything?

Melvin Miles: Uh. It's in that paper I gave you, the Palladium Born paper.

Tom Grimshaw: Oh yeah. Okay. All right.

Melvin Miles: I mean it is. It wasn't a tracking affect, not nearly as large as in Japan.

Tom Grimshaw: Yeah.

Melvin Miles: I had trouble with bubbles forming at the bottom of the electrode. We had a position on the Teflon. Stayed the same and somehow it caused bubbles to form. It's probably the DETRA. Different bottles DETRA will 'cause different bubble practices. Some will give lot more bubbles than others. But, if you have bubbles that attack on the electrode then, it blocks the surface so the voltage goes up and down a lot more. It's harder to make your voltage. 'cause you see it go up and then bubbles are disengaged and it'll go down. If you try to get an average voltage and, that makes it difficult.

Another problem with that experiment I just learned from that experiment, uh ...

Tom Grimshaw: Go ahead.

Melvin Miles: Is that okay?

Tom Grimshaw: Yeah

Melvin Miles: Usually, you can't see inside the cells so, you have to sort of estimate where the level is or else you have to get a string system and kind of pull up the string. Have them pull it up and find it that way. And, that experiment ... normally an experiment you can calculate by how much you loose by electrolysis. But, some of it evaporates and that is hard to figure. Typically, it's about... you add 5% more for evaporation, you come out pretty close. But the gas coming out still has Refuel in it because it is a higher temperature. And when it goes up the glass tube. Goes straight up the little glass tube. The room temperature is where it starts to condense, and someone is back. So not all of it that actually evaporated down below actually winds up going out.

In most other experiments where the top of the glass tube was at room temperature. But, somehow this experiment decided to insulate the top much more fully so he couldn't get out the top. And, that insulated the glass tube. And, I thought I had control of the [inaudible 00:36:46] level. But because of the insulation, that was not room temperature, that was a higher temperature.[crosstalk 00:36:51] So, more gas is getting out. And, when I ....

Tom Grimshaw: Counter production

Melvin Miles: ... finish the experiment, I found I was off quite a ways by the electrolyte level. And, that made me say, how do I correct that? It is hard to do. so, I knew it ... I left out some terms that would increase ecstasy but then the [inaudible 00:37:12] level would have decreased eustacy because the consult been smaller. No, the consulate would have gone larger, I guess because more heat would go up to the cell wall.

You know, probably I did a lot of work and tried calculate it but it will probably come out the same. So, I just left it the same.

Tom Grimshaw: Yeah

Melvin Miles: That's an important point, I finally found one of the found more flashing letters. You said, " the amount of retooled, actually the say escapes depends on the temperature off that end of the glass tube for the exhaust..

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: His refluxing goes on. All the way to that glass tip you got refluxing going on . If that glass tube is warmer, you get more insulation getting out the top.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: And, I just found that out in the last experiment. It you have that to the room temperature, it would have been okay probably.

Tom Grimshaw: But the insulation through it all and ....

Melvin Miles: Right.

Tom Grimshaw: Okay, I guess that pretty much brings us up to the present. As far as your cold fusion journey is concerned. I thing we will do one more recording session, if you are up for it on the future. But before we go there, we are a little over 30 minutes also. Any other thoughts about your cold fusion journey that you would like to share with us?

Melvin Miles: I ... my main goal is I wanna see this gets accepted while I am still alive. That would be my goal right now.

Tom Grimshaw: Yeah.

Melvin Miles: I was trying to think of ways to get it accepted. One way is what I'm working on with Dave Nigel and Steve Kinesthesias getting more [inaudible 00:39:00] made and have the other labs that you can get excess heat with it. That would help. Another way we could get funding would be to run the cladium cubic experiment. And, I talked to Martin Leishmania about that cubic experiment.And, he creates my ideas. YOU have to put a counter electrode symmetrically around all four phases, all six phases of the cube.

There is one publication where they repeat the cubic, extreme tint and found nothing, but they just used a cylinder a counter electrode with a cube inside of it. So, you only have the sides, the top and bottom are open for Trimurti to escape and that won't work. The counter electrode has to be a bigger cube, a screen or something that surrounds the smaller cube. When I discussed it with Martin, he said I was correct, you have to have good symmetry to, what you are trying to do is force diarium in equally in all six spaces of the cube.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: If it's going in all six spaces for a them to meet. It's just a point for the meet, that's the center, the cube. So your pulsing everything into the center point of the cube. That's when that experiment is dangerous. If you use a rod to counter electrode forces into a line.

Tom Grimshaw: Not a point.

Melvin Miles: If you use a plane, two planes, it forces everything in on equal sides, you have the plane of symmetry in the middle. And you get smaller affect with the plane.

Tom Grimshaw: Bigger affect

Melvin Miles: ... with a rod and the big, big affect with a cube or you can do it with a sphere. That experiment should be done. You have to have digital equipment. You have to be careful that it doesn't get to big of an explosion, I mean too[inaudible 00:41:06] all the water and make a whole in the bench and a whole in the cement. That's a lot of energy dissipated.

Tom Grimshaw: I remember trying to back calculate what the energy was and what the power was.

Melvin Miles: That would be pretty high.

Tom Grimshaw: Very high.

Melvin Miles: Right.

Tom Grimshaw: So, I think what we will do is to stop here and take another and then we will talk a little more about the[inaudible 00:41:33] - [inaudible 00:41:46]

Melvin Miles: One of my main goals right now is to get the publication of the caladium born resolved. The Navy lab in China Lake and the two labs in Japan. And my Ridge crest experience. All in my paper, plus the NRL experiment to show that I have a graph for that. What I got from Steve Rivet, it shows that there is excess power there and that, if you think about it, let's see ... one ... two ... three ...four different counterfeiters right in that one paper. We measure similar facts using four different counterfeiters. You can't argue with this, the counterirritant.

Tom Grimshaw: Yeah. Wonder what about Iridium on a caladium boride or caladium boride alloys?

Melvin Miles: What did you say?

Tom Grimshaw: What about using Iridium as the signature instead of heat?

Melvin Miles: Um, I have looked at it but I have never seen with that convincing?

Tom Grimshaw: The reason I am bringing it up is because that is what Pleader gets.

Melvin Miles: Right. But it is hard to do with electrolysis. Because ...

Tom Grimshaw: Yeah he wasn't using electrolysis. All right.

Melvin Miles: Well see when you do electrolysis, you are going to go up here anyway, because Hydrogen electrolyzed more readily then radium. So as you electrolyte it away, your going to be, your platiniridium is going to be more and more complicated as time goes on. So, it makes it hard to use be creative and what is being concentrated.

Tom Grimshaw: Yeah.

Melvin Miles: Right

Tom Grimshaw: Right. Because there is a certain amount of Iridium in every sample of d20. Yeah

Melvin Miles: Yeah.

Tom Grimshaw: Yeah. Well I have then actually talked about the future. If you had unlimited money, and unliMited time, what would you do?

Melvin Miles: Well, I would ... I guess I would go back and look at Clayton moron in more detail and, get in the lab and try to reproduce it. But if you said unlimited money, and I would try to work with David Nigel to do the cubic experiment.

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: And that would probably answer a lot of critics if they could see video evidence of that. Of course, they could say, well you just take the video. I mean they could always come up with an answer. You know, to counter whatever you do.

Tom Grimshaw: Yeah.

Melvin Miles: But, if I ... was going to do anything, right now if I had ... going to run one experiment. I am going to look at rapacuronium and that initial exceed affect. See how much is due to an

seizures is due to this, your used to maybe an incorrect thermal neutral [inaudible 00:44:46].

Tom Grimshaw: Mm-hmm (affirmative)

Melvin Miles: Try to isolate that and, to do that, you would need to run different current densities. Because, the difference in excess power you'll get is proportionate to the current, not the current density but the current. Its the difference in the thermal neutral potentials times the current. So, you can try and do a range of initial current to see if it follows that equation. That initial affect you see. That would be trying to prove that the initial affect is due to the thermal neutral potential. Then try to isolate that from a little bit later affects. I think ecstasy appears but, it doesn't appear the first 10 minutes, and that's early.

Tom Grimshaw: Okay

Melvin Miles: And then again, try why Palladium does almost always works. That is the other question, that's another question that needs answers.

Tom Grimshaw: Yeah, and why do some work and other's not. What's the materials question. I was glad to see the Immodium is back in the game because, he is Mr. Materials.

Melvin Miles: Well I have never had planum born electrode that didn't work to some degree. Some work more than others but, there is one failure that we had that Padus Born is so hard that they have been [inaudible 00:46:09] trying to make it into raw. They have created a crack. They cut a whole in the mid over. So, it's almost the full length of the crack. That would be like a real crack and you don't have electro chemistry going in there. So, you your go ahead and do the electro chemistry and come back out through the crack.

Tom Grimshaw: Yeah

Melvin Miles: Right.

Tom Grimshaw: I think you sent that sample to Storms.

Melvin Miles: Well yeah.

Tom Grimshaw: He told me that sample one time.

Melvin Miles: Yeah, Storms, what sample he ... what with little papers he to and as little papers saying he had extra seed and another sample he has given me said he didn't get extra seed. And then, now he doesn't remember, only the one who didn't give extra seed.

Tom Grimshaw: Oh really

Melvin Miles: Till I showed him the paper he wrote. It was never published. It is a written paper. I have it that he wrote and never published it. Shows his extra seed [inaudible 00:47:06].

Tom Grimshaw: Very good, well I guess, Thank you. It is Tom Grim shaw with Melvin Miles. We are wrapping up. I think this is going to be session number 7. Very good series of sessions of your cold fusion adventure, going back to 1989 and, the different faces and stages along the way since then. I think we have kind of covered the water front. Anything else you can think of that we should bring up?

Melvin Miles: Uh not necessarily. I have a lot in my notebooks. In my notebooks, I have a lot of conversations of interest but, those things can be read.

Tom Grimshaw: Yeah. Yeah. Well, inventory them first, as is always the case and then once we know what we have, then we will figure out about how to do the ... how to dig into the next level

Melvin Miles: The DOE Crab report, I never ever even talk to any members of that panel directly. They hired somebody. I had his name written down but, he worked in Colorado Silver Energy research institute. And, he called me the end of July wanting to know if I had seen

[inaudible 00:48:34]. At that point my answer was no, and I told him and then that's when I ended up in their quarters being on the negative side. Um, in September, after getting this Jos Matthew plug in. I called him back. I reported, I think I see that I don't [inaudible 00:48:53]. I thought we might be seeing extract. So, I guess he didn't think that was a real Palsy thing yet. And, I didn't either.

It's just a possibility. So that's how I found out it never got changed. It did drop all those written things. I telephoned him and reported extract. They ignored it.

Tom Grimshaw: YEAH.

Melvin Miles: I just mentioned it and didn't emphasize that I knew it was right or anything. I didn't know for sure. The first time I've seen accedence. I'll have to check it out a lot more. I only gradually became convinced.

Tom Grimshaw: Yeah

Melvin Miles: I tried to look at lot of different things and gradually I became convinced but that was ... that's how I got on the Crab report with lectures at Cal Tech and MIT and Howell. Yeah and other labs.

Tom Grimshaw: Okay, I will watch more of that. What about the 2004 deal we recorded. Were you involved in that at all.

Melvin Miles: The What

Tom Grimshaw: The Doe re-did the Crab Report in 2004 with Holstein and Nigel and others.

Melvin Miles: No, they didn't hardly even consult with me on that.

Tom Grimshaw: Okay. You weren't involved.

Melvin Miles: I though Hee gan 4 would have been a big plus to try and present but, they ... I don't think they were convinced enough of it and I don't think it was really presented as a major argument. And, I don't agree with Deal or Ab Abbot...

Tom Grimshaw: Lomax

Melvin Miles: On a lot of things. But, at least he did recognize the importance of the Hee gan 4 and, he has written a paper on it.

Tom Grimshaw: Yeah.

Melvin Miles: But I don't like reading all his words. They are too long.

Tom Grimshaw: Takes a long time to read his stuff.

Melvin Miles: I know he. I see his arguments that se turns back and fourth. Et doesn't want him to comment then he ought to honor that. He doesn't need to comment on everything. ....on other peoples work. Leave that charms alone

Tom Grimshaw: It's a sociological phenomenon that is interesting to observe from a distance. Okay, I think we will wrap this up.

Melvin Miles: You meant Abbott. That is the way you pronounce it, Abbott?

Tom Grimshaw: I think its ABB.

Melvin Miles: [inaudible 00:51:23]

Tom Grimshaw: I did too and then I saw him again at MIT.

Melvin Miles: Right.

Tom Grimshaw: In March. He's .. He's

- Melvin Miles: He doesn't have much money because people always help him out to go to meetings and things like that.
- Tom Grimshaw: Yeah, he gets a little bit from Carl P. every once in a while. Same place I get my money.
- Melvin Miles: I mean he can state his opinions. Another [inaudible 00:51:46] that I had with him and Todd [inaudible 00:51:49]. You can't write  $D + D = C4$ . Have you seen that argument they made?
- Tom Grimshaw: No. I didn't. I don't read all those things. I don't have time.
- Melvin Miles: Because you know I have taken modern physics and provided nuclear reaction, if I have a reaction. Your not stating a mechanism, your saying you start with this and you wind up with this. Then you go through a lot of things to get there. There thermostatics. All you need is the initial stage and the final stage. You don't care how it gets there. And so, when you write a chemical reaction, we are not saying we know the mechanism. We are just saying these are the what you start with and these are the products. That is all you are saying. You are not saying how it gets there.
- Tom Grimshaw: Yeah.
- Melvin Miles: But whenever you bring out  $D + D = C4 + 23.4$  MPV. It really gets talk to Hash and Abbott really argues against that. How are you supposed to write it then?
- Tom Grimshaw: Yeah.
- Melvin Miles: I mean we are talking Hash would be  $4D$ . Trillium 8 going into Helium 4. But I don't care. It's still, the ratio is still 2 Deuterium and 1 Helium or 4 Deuterium and 2 Helium. I don't care.
- Tom Grimshaw: Yeah, so what's the Trillium doing in there? You know?
- Melvin Miles: He thinks that the intermediate.
- Tom Grimshaw: I see. So, what? I see your point. Yeah.
- Melvin Miles: But, you know in chemistry, we say you never prove a mechanism. There interesting to talk about. You can't absolutely prove a mechanism because it goes through a state that is a transition state. It's not around very long unless you get better instruments to measure stuff in, it happens in a very short time period. You don't really know what the mechanism is. You can theorize about it.
- Tom Grimshaw: You know, it's interesting, I have heard this same sort of discussion from Tom Later regarding the Shaman Diagrams. You know the beginning and you know the end but, you have no idea what the intermediates are on a nuclear basis. And...
- Melvin Miles: The Lineman?
- Tom Grimshaw: Lineman, Yeah.
- Melvin Miles: Yeah Lineman Diagrams.
- Tom Grimshaw: Yeah.
- Melvin Miles: It goes to a virtual state and then it goes to [inaudible 00:54:15] state.
- Tom Grimshaw: So, what is that virtual state?
- Melvin Miles: Well, Yeah.
- Tom Grimshaw: What is it?
- Melvin Miles: You can't call it chemistry. What is that transition state? This is the Irving theory. And he fished, you have a dolly here like Salt Lake City. He was in Salt Lake and he was

wanting to go to Provo. You would go over the mountain and you would drop down to the lower valley. And, then the intermediate state would be the lowest energy state will take you to that valley.

You don't up the highest way and back down. You go the lowest elevation pass. You don't take the highest elevation pass. And, chemical reactions are the same way. They follow the lower end, there is a pathway.

Tom Grimshaw: Yeah. Unless it is held by a catalyst.

Melvin Miles: Right. Even a catalyst can be a new pathway. That's what they do. They give you a new pathway, this [inaudible 00:55:12]. They might find a secret tunnel or something through the grass. So, catalyst is just a quicker way to get there.

Tom Grimshaw: Yep. Yeah well right.

Melvin Miles: It doesn't change their dynamics any or how you get there.

Tom Grimshaw: Yeah, right. I mean the beginning and the end are still the same.

Melvin Miles: Oh Yeah.

Tom Grimshaw: So Mel, I think we will wrap this up. The sun is coming around for one thing. So, thank you again for spending this time and sharing this journey. I will close this down and we will have a discussion. This is Tom Grimshaw with Melvin Miles, Session number 7 on May 6 and I believe this will be the last session for this round of interviews. Thank you.

## **C2. Round 2 Interviews, July 18-19, 2019**

### **Interview 2-1**

Tom Grimshaw:

Okay, Tom Grimshaw here with Mel Miles, and this is our second round of interviews, the first session of the second round and we're talking about Mel's journey and cold fusion, his research, starting shortly after the 1989 announcement by Martin Fleischmann and Stanley Pons and Mel, oh I should mention that today is July 18 where it's about 2:30 in the afternoon and the temperature out here on the courts overlooking the river is about 100 degrees.

So that's kind of the setting and the dogs need to settle down a little bit here too. And Mel, I think just before we started the recorder I suggested we talked about your roots here in St. George. You were born and raised here. You were born in 1937, you told me earlier. So why don't we pick it up there and talk a little bit about your roots here in St. George and then your move and so forth leading up to when you started working for the [inaudible 00:01:17].

Mel Miles:

Okay, my roots. My roots here chase back to 1861. My great grandfather and grandmother came from London, England and they were in the group that was the original settlers of St. George, the first pioneer settlement of St. George in 1861. That was George and Aunt Jarvis and my grandfather was also born in London and he came here later on and then he married Josephine Jarvis and he was my grandfather, and that was my grandfather and grandmother moved here. My grandfather at one point owned the tar city block in St. George and then he sold part of that off to my dad and also my dad's brother and we had about two to three acres growing up.

We had grapevines, gardens, cows, and chickens and so it was kind of within a city, it was kind of rural growing up. And I attended Dixie High School and Dixie College here and I played both baseball and basketball on the high school and college teams. And then I spent two and a half years in Germany on a LDS mission, which was common for most people here. And then came back and went to BYU for two years, got a bachelor's degree in chemistry and a minor in mathematics. And then on to the University of Utah where I got a PHD in physical chemistry and a minor in physics.

And then I got a NATO, N-A-T-O, NATO post-operative fellowship. Spent a year in Germany, Munich, Germany, learning electrochemistry with one of the great electro chemists of the time, Heinz Gerischer. And that's where it got started on electrochemistry. Then from there without even interviewing, the visitor from the Navy Ordnance Laboratory Corona was visiting and I got hired there when I got through in Germany. I started work at the Naval Ordnance Laboratory in Corona, California. It was called NOLC. Within a year after I started, it became part of China Lake. They combined the two labs and then they were going to close the Corona laboratory and we were going to China Lake except they weren't going to move everybody, just ones they wanted and they never even needed another

research group there and so I got a job in Middle Tennessee State University teaching and spent the next nine years teaching.

But the person who's head of chemistry at China Lake he'd come down and met me and he told me he would've hired me but he didn't want to hire the whole group, but he hired me to come work every summer. If I wanted to, I would come and work at China Lake in the summer and then go back to Tennessee for the school year except for two summers when I went to Brookhaven National Laboratory instead, up to New York on Long Island and so that's my background up to the time of the cold fusion announcement.

I was hired by China Lake in 1978. I have to think again, 1978. I've been working summers on a new thermal battery program and got enough money for it, they wanted to work full time on that in electrochemistry. A new electrolyte for a thermal battery. This thermal battery is used in practically every missile and China Lake had a lot of interesting missiles. It was a Naval laboratory for air-to-air missiles. The sidewinder missile was developed there. A key component is the battery, the thermal battery, they can sit for 10 to 20 years and then suddenly be activated because its a frozen solid that suddenly melted, is the way it works. And so that brings me up to about the time of the cold fusion announcement in China Lake.

Tom Grimshaw: Right. Okay, that's a good milestone for us to stop here for a moment. I want to reel back on questions about the ground you just covered. We've been at this for about five minutes. I always like to make sure and check the recording is going well, so we'll call that the end of session one. Tom Grimshaw with Mel Miles here at his home in St. George.

## ***Interview 2-2***

Tom: Miles. This is session number two covering Mel's research record in the cold fusion area, which began shortly after the Fleischmann and Pons announcement on March 23rd, 1989. And in the first session Mel, you covered a lot of ground on your, from your roots here, born and raised here in St George and then you attended Dixie high school and then Dixie college and then went on to Brigham Young University and the University of Utah. I want to fill in some of the, some information on those various phases at this time before we, before we go on. So I guess the first question is, you went to Brigham Young and you got your bachelor's in chemistry and then you went to the University of Utah and got a PhD in physical chemistry. And remind me, who is your major professor? And what you recall, what was the title of your dissertation?

Mel: Okay, at the University of Utah, I worked for two people. One is very famous. A lot of people thought he should have received a Nobel prize for his reaction rate theory. That was Henry Eyring and the other person was his son who was hired just a few years before I came there, and his name is Edward M Eyring and he'd done a postdoc in Germany with was Dr. Eigen on studying fast reactions in solution. Some reactions are very fast in solution like the reaction of hydrogen ion with a hydroxide ions, but you could measure

the rate and then you could study other acids. I studied organic acids and the rate of the reaction of their hydrogen ion with hydroxide ion, and we even did some studies in D<sub>2</sub>O. Kinetics are different in D<sub>2</sub>O because of the much heavier deuterium than hydrogen, especially if the reaction involves the hydrogen species and so even I got into detail a little bit, even in the called the temperature jump method of studying faster reactions was invented by Dr. Eigen in Germany. He later won the Nobel prize in chemistry for that work, and that was my PhD dissertation.

Tom: Do you recall the exact title or close to the title of your dissertation? Doesn't have to be exact.

Mel: Temperature Jump Kinetic studies of substituted Melonic acids.

Tom: Okay. Not Bad. Okay. And then you finished your PhD and went on to do postgraduate work in Germany. You mentioned the person's name, Gerischer, I think. Tell us about the work that you did at that time and what was the report?

Mel: The project he gave me, electrochemistry project, was to study a water methanol mixture and the reaction kinetics, electrochemical reaction kinetics, in mixtures of water and methanol, and how the methanol affects the rate of the reaction. And, so this is my early studies in electrochemistry was, we were studying, trying to remember the reaction we were studying, it was a zinc, it was reduction of zinc ion to zinc metal. They were looking at apparently it goes through an intermediate zinc plus two to zinc plus one, to zinc metal. We were measuring what they called the exchange current density, which is the rate of the reaction at equilibrium of going back and forth. In electrochemistry that's the exchange current density, that's small 'i' with the subzero on it and and how that changes with the mole fraction of alcohol in the solution.

Mel: And this was later published in, I think Journal Electrochemical Society I think. And with a little bit of alcohol, the rate goes down sharply, then it kind of levels out and rises gradually, and then when you get into pure, pure alcohol then it goes up again. This has to do the fact that ions have to exchange back and forth and normally the zinc ion is solvated with water molecules in order to react, and the methanol solution has to make exchanges with water, solvent molecules with methanol water molecules, and that slows it, that tends to hinder the rate as you add alcohol to the solution. And so it was a kinetic study of electrochemical exchange current density.

Tom: Okay. So this then was the time where you really got your feet wet, so to speak, in electrochemistry. You came equipped with your PhD background, and then studying under Gerischer, specifically on electrochemistry which then served you not only for your subsequent work at the Navy but also for electrochemical work in collaboration with Martin Fleischmann. So is that true that this is really where you cut your teeth in electrochemistry at this, during the postdoc?

Mel: Yeah. Yeah, that was the beginning of my electrochemical studies, but actually Henry Eyring theory did ... was applied also to electrochemistry. And so I understood some electrochemistry just, in fact I took his class on absolute reaction rates, and some of that was related to electrochemistry and how the overpotential effects the reaction rate. And so at least I'd been exposed to it by, through his theory at least. And then I was hired by a battery group in Corona, California.

Tom: Yeah. So tell us about that. You were finishing up your postgraduate work in Germany. How, tell us about the connection, how did they find you or did you find them to get that job?

Mel: Well, and now that I think about it, I think ... there was a professor who visited our group in Germany, Professor Tobias, and he told me about this group in Corona, California, and said I ought to apply and go there. And he gave, he said he would talk to people there and advise that they hire me and, and so I was hired directly while I was still in Germany by this group in California. I didn't even have to interview. I just was hired to go to there starting on, my postdoc ended in November, 1967 and I was hired to start work at Corona, California in January, 1968.

Tom: Okay. And a little side note I wanted to ask you about. When you went to Germany for your postdoctoral work, you had familiarity with Germany and had quite a bit of experience from your LDS assignment there. How did you wind up going to Germany for a postdoc? Was it because you knew Germany or?

Mel: Well, Henry Eyring was well known and he had been invited to go to places all over the world and, so he advised me on where to, what group to go to. He had already been and gave a talk at the technical university in Munich and he had met Dr. Gerischer, who was the head of that group, and he thought I would like working for that group. And so it was at his recommendation, Dr. Henry Eyring, that I applied for the noble, I mean the NATO postdoctoral fellowship program and applied at Dr. Gerischer's institution. And that's why I wound up there.

Mel: But, but also I knew German quite well and that was another reason I wanted, I kind of liked going to Germany. And in fact, when I first met Dr Gerischer, he was struggling to speak English to me, with me, and so I just told him in German, I said [foreign language 00:09:08], and his jaw dropped open and he never spoke another word of English to me the rest of the time. And even all the coworkers, we only communicated in German and very seldom in English unless they wanted to learn a little English, and so everything was in German. And in fact I wound up translating papers that Dr. Gerischer wrote in German and helping him translate them into English for submission to the English journals. I did that on several of his papers while I was there.

Tom: Very good. Okay. So your fellowship was a two year fellowship I suppose.

Mel: One year.

Tom: One year only. Okay. Alright. Okay, so tell us then, you moved from Germany to southern California, I guess to work there. Tell us about your initial work there, and then, from then on up to China Lake.

Mel: Yeah, Corona, California is in southern California, about 20 miles from Riverside, and maybe 30, 40 miles to LA. It was a Navy lab there, and like I said, NOLC, Navy Ordnance Laboratory Corona, and they never completely closed, but they moved a lot of people up to China Lake. Other people were not invited to go to China Lake, and that's why I went to Middle Tennessee State University, but my work, I was there about almost two and a half years I guess at Corona, the Corona laboratory and our work there was on some battery

systems involving liquid ammonia, so you can do electrochemistry on liquid ammonia and that's where I got started with that.

Mel: And lithium forms a very interesting solution, lithium metal dissolved in liquid ammonia will form a blue solution first and then it becomes more and more concentrated, it'll become copper, it becomes metal conducting actually because the lithium ion and what it forms is a very conducting metal like solution in liquid ammonia. And so we studied, our focus was on a lithium anode in a liquid ammonia battery and then some cathode. That was the work we're doing. Some of this I did publish later after went to Middle Tennessee State University in the Journal Electrochemical Society.

Tom: Okay, so your work at Corona came to an end. You did not go to China Lake, you went to Middle Tennessee State University on the faculty there, I guess, as a professor. Oh. And then did you continue to do research while at the university there? Or was it primarily teaching? Or how did that work?

Mel: Well, they, you had kind of a heavy teaching load, but they hired three new faculty members in chemistry that year, and we were all hired because they wanted to get research to going there, and I was mainly hired because of my research experience. And so even though it was hard to do with the teaching load, I bought equipment in electrochemistry, I got liquid ammonia, and then I continued some of the liquid ammonia studies, electrochemistry and liquid ammonia at Tennessee state university, and some of that got published. And later when I, summers, I'd go to Brookhaven National Laboratory, and two years, 1973, 1974, worked there and it was to do water electrolysis to produce hydrogen, store the hydrogen in a metal hydride and then run the hydrogen when you needed it back into a fuel-cell and get the energy back out.

Mel: And so they were thinking of the hydrogen energy economy clear back in 1973. Much before most other people got interested in it, and that was the work of Brookhaven. And so when I went back to MTSU, I started to have a project in water electrolysis, and how the electrochemical kinetics were on different electrode materials. We investigated almost every metal in the transition metal period, on the area of the periodic chart, and we would, we'd report on a periodic fashion how the overvoltage would vary with the metal, and the best catalysts are always in the family. Nickel, palladium, and platinum. Those are the best catalysts for water electrolysis, and of course often, evolution, a lot of metals didn't hold up, they would corrode, under anodic conditions. So, only a few metals who are stable enough, again, those are nickel, palladium, and platinum. That's where I got started in palladium metal, was in that study.

Tom: That's very interesting that that early back in the 1970s you were already involved with hydriding palladium.

Mel: Right.

Tom: Interesting.

Mel: Well, actually the hydriding, the catalyst there, they used to store hydrogen was, iron titanium alloy. It's actually intermetallic compound. One iron per one titanium. And it would absorb lot of hydrogen at room temperature, and it would release it again back

out, near room temperature. So, and I never got around to it, but it would have been another metal to study for cold fusion, would be iron titanium.

Tom: Interesting.

Mel: Right.

Tom: Okay. All right. So you continued at Middle Tennessee State University, but your work was apparently well thought of there at China Lake where you were working other summers, not the two when you were at Brookhaven. So tell us about your transition from your academic posts to China Lake.

Mel: Well, [inaudible 00:15:35], most of my family lived in the West, in Utah and other places, and I liked the western country and scenery. I was obviously interested in getting back to the western part of the United States, and so when the thermal battery program in China Lake looked like it was going to get funding, they wanted to hire me, and so that summer I had a choice of being hired at China Lake, Brookhaven National Laboratory wanted to hire me there permanently and also a naval research laboratory. So I had three choices to go, but I picked China Lake because I wanted to live out west again mainly. And so that's the reason I wound up at China Lake, and then I continued with thermal battery work for about four or five years, and then later worked on other programs as well, like electrochromic materials that changed the color of glass, on the surface of glass, they'll change color based on electrochemistry.

Mel: It could be used in darkening windows for example, by electrochemistry you could darken the light in glass like windows or car windows, I don't know. I think it's now available on some cars, especially like your rear view mirror, you can push, press a button and it'll darken the mirror, and so you don't have the reflection of headlights. So I don't think it's common, but it's used in some cars and there was interest in electrochromic materials later on in the 1980s, at China Lake and that's when I got into having two postdocs working for me, [Quang 00:22:27] Park from Korea, with a PhD from Caltech I think or maybe I think was UCLA, I'm not sure. And then David Stilwell, to work on electrochromic materials and he was from the University of Mexico, and they both got involved with the cold fusion. They were both interested in it. In 1989, announcement came out and you'll see their names on some of the early papers like, David Stilwell and [Quang 00:22:27] Park, and they were the two that worked with me mainly on the cold fusion in the 1989, 1990 timeframe.

Tom: Okay. Well I think we'll, since it is such a milestone, we may go ahead and start a third session here and really focus on the initial announcement, and your response to it, and the work that you did. But before we go there, are there any other things that come to mind about the context or the foundation? Context you had with the foundation that you laid prior to the March 23rd, 1989 announcement that contributed the most, I suppose first year interest and second to your ability or expertise to address the cold fusion problem? Anything you want to see about that before we go there?

Mel: Well, my work at China Lake. I also had funding for a project on the electro-chemical reduction of carbon dioxide. You could theoretically reduce it to methanol or maybe even methane, though it's hard to get past formic acid. And to do that, I did studies in non-aqueous solvents because I'd worked on those in Tennessee, Middle Tennessee State

University, and so I had to devise have reference electrode for those non-aqueous solvents, and that was a palladium reference electrode loaded with hydrogen. And it would last for about a day or two before the hydrogen had all escaped, but for one day it was set up a pretty stable potential. So the background, I was already working with hydrogen in palladium and that, so I had most of the things that I needed at China Lake. I had the palladium, I had the heavy water, which was in an NMR room.

Mel: NMR is nuclear magnetic resonance, we called it at the time. And so I could get bottles of D2O, you know, I had the palladium and so I had everything I needed. We had lithium metal, you just add that to the heavy water, and it would react and give off oxygen and form LiOD. And so it was not hard to get the things I needed to start the study.

Tom: Okay.

Mel: So that's how come I studied, I heard about it on the radio driving into work like probably the following day, March 28th, and I thought, really? You know why I'll look into that, and so I did on the first Saturday. I went in and started doing experiments, but it was very hard to know what you're doing, with such limited information.

Tom: Good. Okay. So we're going to start another session to pick up the thread at that point when you really started doing the cold fusion, but what I was trying to do in these first couple of sessions, it just kind of set the framework or the context which made you so well qualified, not only to pursue cold fusion but with the electrochemistry, in-depth experience, you know, and working with the electrochemical cells and working directly with Martin Fleischmann in subsequent years. So that was really what I was hoping to accomplish in these first two sessions. So any concluding remarks? Before we come back and pick up the cold fusion story?

Mel: Well I should also mention that at Middle Tennessee State University, I taught physical chemistry and there's always some discussion in there about calorimetry and one of the laboratory experiments involved calorimetry that we use in the physical chemistry laboratory. So calorimetry was somewhat familiar as well, besides electrochemistry.

Tom: Okay, good point. Okay. Tom [inaudible 00:22:09] with Mel Miles. We're at his home in St George, Utah. I think the temperature is still around a hundred degrees. It's January 18th and we're, there is a nice breeze coming off the river. So it's really actually quite pleasant. So now we'll wrap this session up and start with session number three in a moment.

### ***Interview 2-3***

Tom Grimshaw: Tom Grimshaw again, here with Mel Miles. This is session number three. It's July 18 in the afternoon and we're sitting on the back porch of Mel's home overlooking the beautiful Virgin River. The temperature, as I said in an earlier recording, is about 100 degrees, but there is a nice breeze.

Mel, in the first two sessions, this is session number three, we were talking about your roots, and your graduate studies, and how you came to work for the Navy, and so forth. And we're just about to embark on your cold fusion journey, but if

you'd like to backtrack a little bit and talk about your family story and your grandfather, I think that would be terrific.

Mel Miles:

Okay. My grandfather, Georgie Miles, was born in London, England and he grew up rather poor. He would tell stories of, he'd have to go collect coal along the railroad track in order to keep warm where he was living in London. His mother was not married, but she was maybe a mistress to a rich person in London, that, last name Miles. Her last name was Wyatt, by the way, but Miles, he owned the Red Lion Pub on Parliament Street in London, just a block or so away from the Parliament. And so he was quite wealthy. My cousin has gone there and it's still there.

The pub's still there, the Red Lion Pub on Parliament Street, and so my grandfather's biological father was actually quite a rich man in London. And when he died, one of his sons found out about this other, second family, Mary Wyatt and three kids, I think. And he joined the Mormon church, and he was going to migrate to Utah, and he found out about them, and he paid their way to come to Utah, take the ship across to New York. And then by then, they had the railroad, and then the railroad to Salt Lake City, and then they wound up in St. George, Utah. And so that's part of my background, is that great-great-grandfather was the owner of the Red Lion Pub in Parliament Street, in London.

And I've always wondered how my grandfather wound up, he had enough money at one point early on, when he was in his young age, to buy a city block of St. George with one house on it, and that was a big pioneer home, quite large. It had upstairs, and downstairs, and even a dirt floor basement. I've always wondered how he got that money, but when he was a teenager and young kid, or young man, the Silver Reef Mining, 20 miles up I-15 in a town called Leeds, they found silver there and there was a lot of money came out of that mining. He was not a miner, but he worked hard, I guess, at hauling supplies back and forth between St. George and the mining camp, mining town up there. It actually became a small little town up there. But I guess he was frugal and saved his money, so at some point he was able to buy a city block and a large home. It was kind of like the Southern style mansion, set back in, quite a ways back in the street, maybe 200 yards in from the street. It was quite a nice home.

That home, right now, part of the home, anyway, when it was later sold and they were going to tear it down, some people came up with the idea of move it, and save the home, and move it to Ancestor Square. So it's one of the pioneer homes you'll, at least part of the home. They weren't able to move the whole entire home, but part of the home, at least half of it, the main entryway and everything is still there in St. George in the main part of town, a place called Ancestor Square. It was a Judd, Judd was the person who built it. It was a Judd Miles home, I think his house is labeled, Judd Miles.

And so I grew up in plenty of land. My Dad bought three acres of that block, and my uncle bought a couple acres on the south. We were on the north and they

were on the south, and then sold off pieces to other people over the years, like real estate.

Tom Grimshaw: So I have to ask you this question. When your forebears came to this part of the world, were they a part of the initiative I think started with Brigham Young, the so-called Dixie movement, attempt to grow cotton here in Southern Utah, which had a much milder climate than Salt Lake City?

Mel Miles: Yes. That was another side of my family, the Jarvis family. George Jarvis had been in the British Navy. He had been all over the world with the British Navy, even been in China. And then he lost an eye somehow, working on ships, and he left the Navy, and he and his wife, they enjoyed the Mormon church later. And then they migrated to Boston, lived in Boston a couple years. And then the church authorities told them to try to get to Salt Lake City, so they got to the Mississippi River and then they got a hand cart, and they traveled across from the Mississippi River to Salt Lake City with just a hand cart. And my great-grandmother, Ann Jarvis, always like to tell people that she'd walked every step. She never rode once the whole way.

Tom Grimshaw: Interesting. Do you recall which party? What was the name of the party they came? Each of those migrations had a leader and the party was named after the leader, I think, if I recall.

Mel Miles: Yeah. I don't remember the name of the party. My Dad's sister, Mary [Clyman 00:06:44] wrote a book about all this. I have this book, and most of the Jarvis' have a copy of that book, and it tells a lot about the Jarvis part of the family, how they joined the church, how they wound up in Boston and later moved on to Salt Lake City. And then Brigham Young, in a meeting, asked for volunteers to settle St. George area, and my grandfather stood up, and my grandmother was trying to pull him back down. He wanted to volunteer, but she didn't. They finally got somewhat settled in Salt Lake City, but he did volunteer, and she went along with it, and they wound up in St. George late in the year, in 1861 when it was first settled.

There were some other settlements, like Washington, 10 miles to the east, but the St. George group was to grow cotton, like you said. And they did, they successful in doing it. And a lot of Southern people were in that company, because they already knew how to grow cotton. The Jarvis' had never grown cotton before, but they wanted to help be part of the settlement, and they helped in other ways, and so on. But of course, the cotton mission ended when the railroad came, because it was less expensive to just bring cotton in grown in the Southern states, and so that ended it. But they were able to grow cotton here, and did for several years, because they were isolated until the railroad came, for things like cotton and other things like that.

But they were part of the first encampment in St. George there. There's a plaque on the Dixie's State University Campus that lists the names of all the people in that first camp, and George and Ann Prior Jarvis are two of the names. They also had some kids with them, but they just list the parents' names.

It was late in the year, and the story they tell is that there was a period about 40 days where it rained every day. That's unusual here, but they were just living in wagons at that time, and they had to go through that winter with just rain practically every day for about 40 days. And then when spring came, they were able to start growing food, and a lot of crops grow quite good here. And they found out they had irrigation water from the Virgin River, but it took a lot of years before they could really manage the Virgin River. They would build diversions for agriculture, but you always get big floods when you get rain up in Zion and up in the mountains. And you get, the river becomes a torrent, really, a flood, and it would just wipe out everything they'd built. And so it was hard to control the Virgin River, and that was part of the story of the settlement here, just the fight against the Virgin River to control it and to do agriculture with water from the Virgin River.

- Tom Grimshaw: I think it's probably worth mentioning, too, very interesting story, it really shows how far back your roots go in this part of the world and in your heritage, and I think it's worth mentioning that George, from St. George was one of the church apostles, one of the original members of the church, if I recall. I did not understand that story. Do you recall who George was?
- Mel Miles: George ... It wasn't George Jarvis, I know. It wasn't my great-grandfather. It was ... One of the early leaders was Erastus Snow, and there's one other-
- Tom Grimshaw: Yeah, and there's a bust of him in-
- Mel Miles: There's a lot of Snows in this area. When I grew up, at least, Snow was a common name, and there's also the Snow Canyon State Park of Utah.
- Tom Grimshaw: Yeah. I was just-
- Mel Miles: But St. George is named after somebody named George. They thought he was a leader here. I can't remember his last name, but St. George is named after a leader here that they thought was like a saint, you know, called it St. George.
- Tom Grimshaw: Yeah, he was very well respected in the church, obviously, and so designated as a saint.
- Mel Miles: Right, but I can't remember his last name now.
- Tom Grimshaw: It doesn't matter. I'm sure it's easily found.
- Mel Miles: I just know it wasn't my great-grandfather.
- Tom Grimshaw: Okay. Well, good. This has been a good side trip. Thank you for this.
- Mel Miles: I get the Deseret News [inaudible 00:11:27], one on Saturday has a section on church news, and it said that from the years 1847 to about 1867 about 97% of the people migrating were from Europe. England, Germany, Denmark, Sweden and so on, but 75% were from England. But 97% were from Europe that migrated here, so that's the ancestry of most people traces back to Europe. And there's a small town west of St. George called Santa Clara that was settled by Swiss settlements, Swiss settlers from Switzerland. And common names are Frei, which is German

for free, Hafen, which you pronounce [Hoffen 00:12:20] in German, which means harbor. And there's a lot of German type names from the Santa Clara area. It was a very, very clean farming community, and well known for the produce they came up with at farms over in Santa Clara. And if you ever go there, there's a Frei Market open in the summers for the ... They've sold farming products from that, along the old highway.

Tom Grimshaw: Interesting. Okay. Well, I think we'll go ahead, start another session for the cold fusion story. Any last thoughts about family history and ancestry, or along those lines?

Mel Miles: Well, my Dad taught chemistry at Dixie College for many years, then he went on to work at Titanium Metals in Henderson, Nevada. And we didn't move there because, that's a different story, because my father and mother basically split up, and didn't get divorce, but he lived down there. And he became a chief chemist of Titanium Metals later on in his career, after teaching many years at Dixie College, as well as running the St. George Airport for many years, and training pilots that later flew bombing missions over Germany, that got their initial training from my father in St. George, Utah.

Tom Grimshaw: Wow. What a history.

Mel Miles: Right.

Tom Grimshaw: Okay, I'm going to pause here, and we'll pick it up again with session number four. Tom Grimshaw here, with Mel Miles on July 18th. We'll pick up session four, as I've said, in a few minutes.

### ***Interview 2-4***

Tom Grimshaw: Tom Grimshaw here with Mel Miles, July 17th, no July 18th. I believe this is session number four Mel. We had a nice interlude on your family history and the history of this area in session three. And before that, we had just gotten through your professional career and educational background and childhood here in southern Utah leading up to the fateful day on March 23, 1989 when Stanley Pons and Martin Fleischmann announced Cold Fusion.

Tom Grimshaw: So why don't you pick up the story there? Mel, you had brought us up, you were employed at the Naval Air Weapons Station at China Lake and you were doing work that was very closely allied to, or made you well equipped to do cold fusion experiments. So, why don't you pick up the story when you heard about it and how you responded.

Mel Miles: When I was first hired here it was, in China Lake, it was called the Naval Weapons Center, NWC, pretty simple. But a lot of Navy labs and different places started combining, joined with the Corn Laboratory and then Corn Laboratory got sort of closed down but it never really completely closed down. So they changed the name to Naval Air Warfare Center and then Weapons Division is the group I was working with, NAWCWD. But starting off it was simpler, NWC. The Navy changes

names quite often. Anyway, like I said, I heard the cold fusion announcement driving to work and thought I had everything I needed, so why not go on and try it?

Mel Miles: My first try was on Saturday, that following Saturday, and I just used the heavy water and the plating that I had, which I found later doesn't absorb deuterium very well and hard to get very high loading, so I probably never would have found excess heat with it. And, if I'd just stuck with that. I just used a test tube and a thermometer and I, it was learning experience. I found ways to measure the heat in and the heat coming out, but they didn't match up. Mainly you wouldn't get enough heat coming out as you put in. Heat in was easier to calculate. It was more difficult to get an accurate measurement of heat coming out.

Mel Miles: And then, David Stilwell diverted from what he was doing and started working on it full time. And he came up and he needed to get a thermistor system, so we ordered that. The thermistor could measure temperature to a hundredth of a degree. It was a lot more accurate than the thermometer. And we tried various calorimeters and we had problems because the small test tubes, the electrolyte level changes too rapidly, and so your cell constant is changing rather rapidly. It made it hard to get accurate measurements. I remember we tried a thermos that you buy for drinks, we tried a thermos and different ways of doing it. I think I eventually came up with the idea to use a plastic container and then an outer test tube filled with water that wouldn't change levels, and then put a smaller test tube with the D2O in the center that would change levels. And it kind of helped wipe out the effect of the electrolyte. It wasn't nearly as great. And we could calibrate it quite accurately and that was the calorimetry we finally wound up with. It was this, the China Lake calorimetry we called it. And it was accurate to maybe 20, plus or minus 20 milli watts, I would say.

Mel Miles: So, we weren't sure of anything. Eventually we got more than 50 milli watts out of it. And we never saw that starting off because we were still using the old palladium and we came up with the value of heat out to heat in at 1.00 plus or minus 0.04, which is pretty accurate. Because I'm sure we're not any excess power, but to get 1.00 was pretty good, I thought at that time.

Mel Miles: So, then we finally put in an order and received that in September from Johnson-Matthey. This was a palladium rod, thick rod, about the thickness of a pencil. About six millimeters. And so we had a lot bigger volume. I didn't know that at the time, but the excess heat at the normal current density of a hundred milli amps per square centimeter. Fleischmann reported excess heat of about one watt per cubic centimeter. And because of this bigger volume, and I'm not saying it's a volume effect, though I think it could be, most people like to say it's a surface effect, but the excess heat seems to be roughly approximate to the volume of the palladium. Bigger volume you get larger excess heat. And I found that to be approximately true in all my studies I've done.

Mel Miles: And so when we started using Johnson Matthey, right off, well not right off, but within a week, we started seeing excess powers that, of maybe 10, 20, 30%. Ratio

of heat out to heat in would be 1.2, 1.3. And this was in September of 1989. And I didn't believe I was correct right off. I knew we had a lot of questions. We had to try it again. We had to test a lot of different things. So, I just reported to the person from DOE that called me that it was maybe a possibility. I didn't say it was or wasn't. But I thought we could possibly be measuring excess heat. But it never made it in to the DOE report. I was still left on the side with CalTech and MIT finding no excess heat.

Tom Grimshaw: So I'm going to jump in here just to clarify for our audience about what you're referring to is the Energy Research Advisory Board. The part of DOE that had the special commission to look into and evaluate cold fusion. And I think the draft report was in July and the final report was in November of 1989. And the report was prepared based upon interviews and visits made by selected members of the committee. And I think the person who talked to you, I think you mentioned his name once, was representing that committee and that report.

Mel Miles: Yeah, I can't think of his name right now but he worked at a government laboratory in Colorado. And I have it, his name is in a notebook and the conversation I had with him in September is written up in a notebook. But I didn't outright say that I found excess heat. I just said, I reported that it looked like it might be. A still open question.

Tom Grimshaw: Yeah to clarify the point you made that in the report then, because of your uncertainty, you were kind of put in the category of those who did not see it or who did not support the

Mel Miles: The two conversations we had before I told him we hadn't seen any excess heat, and so that's the way it was left. He called me several times prior to that call. But I never actually ever talked to anybody actually on the ERAB Board. None of them ever called. Fleischmann told me, nobody, they didn't ever contact him. Nobody even came to his laboratory at University of Utah to even look at this system. So, they didn't really do much work in actually finding out what people did. I don't know if they even visited Mike McKubre for example. They just had a person call people and they reported groups that reported excess heat on one side, on one table, and then a group reporting no excess heat and they were about even in numbers as I recall. But I don't think they ever did much actual work. Except have somebody call laboratories for them. They never made trips as far as I know to look at any laboratory.

Tom Grimshaw: I know trips were made, but I don't know exactly who, how they were commissioned by the actual board. And that background, of course as you know I'm doing work with retirees from Los Alamos National Lab. And there were visits made there, and people were interviewed.

Mel Miles: Yeah, I think their minds were made up that it was not a correct discovery. And they didn't want to change their mind. And the only person who was maybe open minded was Rampus, or what was his name?

Tom Grimshaw: Ramsey.

Mel Miles: Ramsey.

Tom Grimshaw: He wrote an introduction to the report.

Mel Miles: Yeah, he had them revise the introduction that even one experiment showing excess heat would be significant.

Tom Grimshaw: Yeah, revolutionary I think.

Mel Miles: Revolutionary, right.

Tom Grimshaw: Yep.

Mel Miles: And he's since passed away. But after I reported the, a year or two later, the heat helium correlation, somebody from that report called me and said he agreed with me. And he liked the fact that I did report this. It could have been after I published the calorimetry paper in the Journal of Physical Chemistry, reporting on errors made by CalTech and MIT. So that was on our work, that might have been when he called me.

Mel Miles: And he more or less thought it probably related something to cold fusion. I can't remember his name. Maybe Landis, might of been Landis if that's a name.

Tom Grimshaw: So we're going to take a short pause here. Because we have a visitor.

Mel Miles: Yes, I'm going to.

Tom Grimshaw: Okay Tom Grimshaw here with Mel Miles. We had a brief break and so, Mel I think you were in the middle of talking about having just received a new palladium rod from Johnson-Matthey.

Mel Miles: Yeah, with that rod, I used it over and over again. You know Fleischmann once answered people should report on whether, once the rod gives excess heat, does it give excess heat in the next experiment or another experiment. My experience is that it even gets better often. If you run an experiment, repeat an experiment with the same rod. So I had a streak there, I didn't realize how unusual this was, that every experiment gave excess heat effect. As long as I used those palladium rod samples. And this is what led to my consulting with Ben Bush, because I was pretty sure I could get excess heat. And then we wanted to measure helium three and helium four that would come off.

Tom Grimshaw: Yeah so tell us a little more about how you came to know Ben Bush. How you compared notes and where you wanted to measure helium and correlate with excess heat. He was at the University of Texas.

Mel Miles: Yeah. Yeah. Ben Bush, he grew up around Sacramento. I don't remember exactly where he went to school, but he wound up at the University of Texas running laboratories for a professor there. And I can't-

Tom Grimshaw: Lagowski.

Mel Miles: Lagowski. Professor Lagowski. But he was, he wanted to get a post doc

Linda: I got to feed my chicken, chipmunkies.

Mel Miles: All right. Sorry. Anyway, do you want to stop for a minute, or?

Linda: No, you don't have to.

Mel Miles: I don't remember where I was.

Tom Grimshaw: You were talking about

Linda: Do you need me to come back?

Tom Grimshaw: Connecting up with Ben Bush at the University of Texas and he wanted to do a [inaudible 00:13:05] post doc.

Mel Miles: What were you saying?

Tom Grimshaw: Okay, we had a pause for chipmunk feeding and now we're back talking about your introduction to Ben Bush and his laboratory at University of Texas in Austin.

Mel Miles: Yeah. Ben Bush was looking for another job, post doc, and I guess he seen an advertisement China Lake for post doc. He'd called somebody in China Lake, not me, and talked to him for a while. And they had me call him. And so that was the way I contacted him, just by telephone. And anyway, he wanted to come to China Lake as a post doc, and I was the one mainly hiring post docs at that time. Somehow we got on to discussing cold fusion. I don't know whether he knew I was already doing it or I guess maybe found out or else I told him. I don't remember the entire conversation, but Ben Bush, once you talk to him, he can go on for a long time. And so we covered a lot of topics.

Mel Miles: I had already been thinking about looking for helium four ever since ICCF One in 1990, where a professor there, I don't remember his name again, he reported that he thought the reaction was H.

Mel Miles: I don't want it right now, we're doing an interview.

Linda: Now? Oh yeah, look at you. I want one now.

Mel Miles: We're actually recording an interview right now.

Linda: Oh, hello. My name is Linda.

Tom Grimshaw: And the dog just licked my spoon.

Mel Miles: Do you got another spoon?

Tom Grimshaw: No it's all right. I don't mind dog spit.

Linda: Oh, actually there's more antibiotics in a dog's mouth spit than our spit.

Mel Miles: Sorry about the interruption. Is it still recording?

Tom Grimshaw: Yeah, keep going.

Mel Miles: Professor Schwinger, that's his name.

Tom Grimshaw: Julian?

Mel Miles: Yeah, he'd won a Nobel Prize. He's a theoretician. And he had given a talk at ICCF One in Salt Lake City, that the reaction might be an H + D going to helium three.

That doesn't give off any gamma radiation. That would answer why there may not be any energy, high energy radiation given off.

Mel Miles: And I, the only equation I'd wondered about was there really enough H in D2O? We tried to keep H out. But it could be that there's an impurity and it might be enough. And so, I thought, to be a good way to test helium three would do, collect a sample and do a mass spec analysis of it. I'd already asked people at China Lake about it and they said they could not measure mass that low. Their mass spec was more for organic chemists who had molecules with rather large masses. So it couldn't be done at China Lake, and so I asked Ben Bush about it and he thought they could do it there at University of Texas.

Mel Miles: And so we started discussing ways to do it. Ben was an expert in working in ways to keep air out of systems, which is important. And he had a lot of good ideas about that. And so we thought we'd have to first find out what gases we could use to purge the system that didn't have helium. And so we started off, I sent him samples from our helium tank and also from boiled off nitrogen. We had a nitrogen line that actually came right in the laboratory. We had a huge tank outside next to the lab. The boiled off nitrogen would tend to be pretty pure, because anything like helium four would go off much earlier and be gone. But the argon tank, the test showed it did contain too much helium four to be useful. So we found that the way to do the experiment was to use boiled off nitrogen. And this was before we even sent samples relating to cold fusion. Had to find a good way to purge the system.

Mel Miles: Ben Bush would get the flask ready at the University of Texas and he would make the opening so the gas could come in and go out of the flask. He used filament tape so they wouldn't break and chipping is easy. And it turned out that would probably also fill them. Helium four from the air getting in because it kind of had this tape on the outside.

Mel Miles: Anyway, we worked out a way to do it. Before we would collect a sample, we would flush it with boiled off nitrogen, right in place, in the system. And then we would turn off the nitrogen. The gas always ran out. The exit was through oil bubblers, so air could never come back in. The pressure would build up, and then a bubble would form and it would come up through the oil. So that way air could never flow back into the system. The only way helium could get into the system could have to diffuse through some part of the system. Not by back flow.

Mel Miles: And so that started the helium four studies. And I was already planning on running an experiment and I wanted to do several other things, which I did. I wanted to test neutron activation of gold and another metal. My brother was a nuclear engineer and he could, I could send a sample to him and he could test for nuclear activation by neutrons of the samples. So that was the second part of the experiment. And the third part was to use film I got from the dentist and put those in and see if they got exposed.

Mel Miles: So we had three ways, three things we were testing for. Film exposure, metal activation, especially gold foil, and helium four. And those were the three things

we measured in that experiment. And we found helium four but no helium three. The neutron activation was, the sample was sent off through the Christmas season and it got lost, delayed in the mail. And so it didn't get there right away. I should have sent FedEx. But my brother did give me an answer that would have to be, the neutron production would have to be less than ten to the fifth neutrons per second, which is certainly true. Actually I found out later that neutrons more like a few neutrons per second, at most.

Mel Miles: So that was the first helium four, I think I mentioned that we thought we should do a quick publication. But if I published through China Lake, it had to go through review and be approved by several layers, starting with my division head. Then head of research and then people higher up that I didn't even know. And I thought cold fusion by then was not a very popular topic. A lot of people, a lot of enemies. And I thought it could easily not get through review at China Lake. So I had Ben Bush and Lagowski send it for publication through the University of Texas.

Mel Miles: I think Ben Bush did the final writing of it. I did a lot of the writing, but, and he put his name and Lagowski's name as first and second, which I thought was not right because everything was done at China Lake except for helium four measurement. But that's the way it wound up, on a short note. And it was accepted right away. Published within a month after we sent it in by the journal Electroanalytical Chemistry. And that's, I think when my work really got, first got noticed. Because I started getting, I got a call from John Bockris. I got a call from several people in the field. A call from Preparata, who happened to be in the United States at the time, and he wanted to come visit. And Bockris wanted to come visit. They both came to China Lake and looked at my set up and spent a couple of hours at China Lake after the report of helium four. Preparata and John Bockris were the two that I remember that came.

Mel Miles: You know, it got publicized in the newspapers. I don't know how that happened. I didn't do it. I think Ben Bush talked to some newspaper and that's how it got published. China Lake was not too happy that, it was not supposed to be released directly to newspaper. But that was not me that did that. But it got reported in newspapers right away as well.

Tom Grimshaw: Let me ask a quick question if I may. When you first started describing this, you said that Ben Bush could detect helium three. And yet you said that it was helium four that was used for the correlation. Can you explain the, how that [inaudible 00:23:18] about?

Mel Miles: If you're going to measure for helium, it's pretty easy to measure both. One is mass three, one is mass four. So they just come off a little bit separately on the mass range of the spectrometer. And the way they do the helium measurements. You have to separate out the D<sub>2</sub>, because it's a lot of, mostly D<sub>2</sub> in the sample, when you collect it. The bottle that you collect is mostly D<sub>2</sub> and oxygen that come off in electrolysis. But everybody that does helium four accurately, they have to separate the helium four from the deuterium. And activated charcoal is the way

they did it at the University of Texas. You put the activated charcoal at liquid nitrogen temperature and make the gas flow through there, and there's a lot of charcoal there. A lot of surface area. The oxygen, well you're not too worried about oxygen really because it's much higher mass. But the D2 gets adsorbed in the charcoal and gets slowed down, so it doesn't come out at the same time as the helium four. The helium four just goes right through. And so that's the way you separate them so they don't show up at the same time in the mass spec. And you can just see helium four as a separate peak from D2.

Tom Grimshaw: Okay, and what you were saying is that you saw helium four but not helium three.

Mel Miles: Yeah, helium three would have been very easy to detect. Because there's not much that interferes with it. The only thing that would interfere with it would be tritium. Tritium would be mass three. Would be not the same as helium three but close. Let's see tritium has two neutrons one proton, that'd be about roughly mass three. Deuterium, a DH molecule would also be mass three, but that would get adsorbed by the activated charcoal and get separated out anyway. So the only thing that would really interfere, even tritium, if tritium were really a product, it would get adsorbed by the activated charcoal. So nothing would get through except for helium three and helium four. And they'd both be separated by their masses. And nothing showed up around mass three.

Tom Grimshaw: So, going back to the talk you heard at ICCF One by Schwinger, he was predicting helium three, I think you were saying.

Mel Miles: Yeah Schwinger. His theory to account for no gamma, no gamma radiation and

Tom Grimshaw: No dead graduate students.

Mel Miles: No neutrons. See the question, why weren't there any neutrons as well? Well the D H reaction goes directly to helium three and no neutrons. So there's no neutrons and there's no gamma.

Tom Grimshaw: So you see where I'm going. You thought maybe Schwinger had, was on to something with the helium three. But yet you didn't see helium three. You saw helium four. And you saw the correlation. So in other words, Schwinger's idea didn't really pan out.

Mel Miles: No it didn't. Too bad. Because it would not have been controversial. Helium three, so little of it in the atmosphere, it would not be a factor. The reason most people just threw out my report, they said, "Well helium four is in the atmosphere and helium four can diffuse through glass. And that explains it." And that was why it was not accepted. It wasn't until later that I did studies of helium diffusion through glass and found it, yes it does diffuse through glass, but at a rate so slow that it would not have made a difference in my results.

Tom Grimshaw: Well and this actually somewhat goes back to Paneth and Peters, who had reported that helium, through a nuclear reaction. And then they withdrew it because they said that it had those helium contamination because of diffusion through the glass, if I recall.

Mel Miles: Right, that's what I remember. And that's, nobody knew the rate of how fast does helium diffuse through glass. There was equations were you could calculate it. It depends on the glass thickness and it depends on the surface area of the glass. If you know the thickness and the surface area, and it varies with the kind of glass. So different glasses have different rates of diffusion. And I, we didn't know the thickness exactly of the glass, but we later found that out by. Let's see, how did we find the thickness? We would submerge the flask in water and see what the volume level was of the glass. And then you put water into the glass flask, at a certain temperature, and find the weight of that. And you can calculate the volume of the water and the volume displaced by the outer surface of the glass, and from that difference you can calculate a thickness out. It would be, if it was a perfect sphere it would be  $\pi r^3$  versus  $\pi r^3$  plus, it was  $r$  plus the delta amount.

Tom Grimshaw: Right.

Mel Miles: Right. [inaudible 00:29:29] You could calculate. We did that and we found the thickness of glass is actually, you could also break it and measure it, I guess. That's another way to do it. Well, that might even be more accurate to do it. We didn't want to break them yet, so we just did it that way. And it came out at about one millimeter, which sounds about right. I think 1.0 millimeters is what it came out. And you could calculate it either theoretically, or measure it experimentally. And both came out pretty close. The theoretical measurement. Well, no. I would say, if the flask didn't have deuterium in it, it came out quite close. But I'd already postulated in my talk in ICCF Two, I knew this would be a criticism and my argument was, I read up and diffusion also diffuses through glass. And it is diffusion from the inside toward the outside. And it might be going through the same sites as helium coming in, from the outside and to the inside. And the outward flow and the inward flow, it would slow down the helium coming in. And that was my argument I used in Italy. ICCF Two. Como, Italy.

Mel Miles: And when we did measurement, accurate measurement later, we found that yes, either deuterium or hydrogen in the flask does give you a lower diffusion rate of helium coming into the flask. So that part was true. It wasn't a magnitude ten like I hoped, but it was a magnitude of like two or three difference. And so that also helped dispel that argument from contamination by helium from the air.

Mel Miles: Later when I used metal flasks, I could actually measure how much, when I had no excess heat, how much was getting in. And I later, the amount I measured was too large to be coming from the glass. I found that I did have sections of glass tubing were connected by rubber vacuum tubing. And the rubber vacuum tubing diffuses much faster. Helium goes through that much faster. I didn't have much of it, but enough and my calculation showed that was the major pathway of helium getting in. When I did studies in metal flasks. No helium comes through the metal. But we still found a background amount of helium, and that's due to the amount that got in when there's no excess heat, there's still a little bit that got in. And that was due to diffusion through the heavy rubber vacuum tubing. And it came out about the right amount. From what I would calculate.

- Tom Grimshaw: Well, I don't want to over push this point too much. But, you started with Schwinger and the D plus H. But you didn't see helium three. You saw helium four and were able to establish a correlation. So taking the argument back to the source, what do you think is the source of the helium four?
- Mel Miles: Well, I think that the most likely source is, without getting in an argument with Avid
- Tom Grimshaw: Abd Lomax
- Mel Miles: And Hashish. Do they say you can't write D plus T going to helium four. Well, I say you can because you're just saying what you start with and what you end up with. That's all I'm saying. I'm saying inside the palladium you have D, you don't have D2. You have mostly D, maybe D+. And that's what you have inside. And it somehow combines, maybe screening by electrons, to minimize electrons, the coulomb barrier. I applied the Eyring rate theory to my cold fusion results, and it predicted, it shows that the rate is likely controlled just by diffusion of deuterons in palladium. Storms has measured this experimentally and come to the same conclusion. The rate is controlled by the diffusion of deuterons through palladium into an active area. A neutron active environment. And the Eyring theory, which has never been published, I tried to publish it in a journal but I haven't succeeded yet. I sent it to Journal of Condensed Matter Nuclear Science, but the referee was so critical I said, "Okay, I'm going to take it somewhere else." And so I'm meaning to go back to that, but I'm trying to publish it somewhere else right now.
- Mel Miles: Anyway, the Eyring Rate Theory indicates, as well as Storms results, the coulombic barrier doesn't really exist inside the palladium, or wherever it takes place. Wherever the reaction takes place, the coulombic barrier has been neutralized. It's not really a factor. It's just the diffusion of deuterons into that active region. That controls the rate. And I don't remember what the original question was, but.
- Tom Grimshaw: Well, if it's not D plus H equals three, if you're H four, helium four instead of helium three, what is the reaction? That was the original question.
- Mel Miles: Yeah, it might go through a number of deuteriums combining to form beryllium eight, like Taki likes to argue. Then the beryllium eight breaks up into helium four. I don't know what the mechanism is and I've always taught, and I've had it stated in textbooks, you can never prove a mechanism is correct. You can just argue that it could be a mechanism.
- Tom Grimshaw: You look at the reactants, and you look at the products.
- Mel Miles: You know what? Thermodynamics is only based on the reactants and the, the beginning reactants and the final product. The beginning reactant is D, or if you want to write it as D2, you could. Going back further it starts off as D2. So it's D plus D going to helium four. That's the net reaction from reactants to products. How it gets there, it maybe Takahashi's mechanism and it maybe any other mechanism. But I find that the ratio of helium four, there's one helium four for

every two deuterons. That's the ratio of products to reactants. Two to one.  
[inaudible 00:36:17] two deuterons.

Tom Grimshaw: I'm intrigued by the statement you made that if you used Eyring's reaction rates, which have not been published, which surprises me, but that's another issue. But if you apply the reaction rates, then you come up with a model or a hypothesis that says there is no, that the coulomb barrier is neutralized, is not effective within the palladium lattice. Tell me more about that, because that sounds kind of revolutionary to me.

Mel Miles: Yeah. The Eyring theory, from the, if you know the reaction rate, and on the average we know that usual current densities, you get about one watt per cubic centimeter palladium. And that gives your reaction rate. And one watt means there's so many helium atoms per second produce one watt. So you know the reaction rate in atoms per second. Based on the one watt.

Tom Grimshaw: Ten to the 22nd, if I recall.

Mel Miles: Well, ten to the, it's something times ten to the 11th. Like 2.1 time ten to the 11th.

Tom Grimshaw: Thank you.

Mel Miles: Helium atoms per second per watt. But a watt second is a joule. You could say, you could also say that many helium atoms per joule. Is another way to say it.

Tom Grimshaw: Energy instead of power.

Mel Miles: Right. And so from the typical rate constant, you can calculate the free energy of activation in Eyring's theory. And from that you can calculate the free energy of activation, the enthalpy of activation, and the entropy of activation. And I do all this. And the enthalpy of activation is pretty close to what they call the activation energy by Arrhenius theory. And if you compare the result I get with what has been reported for deuterium diffusion through palladium, they come out very close.

Mel Miles: And so, just running off of the basic cold fusion reaction rate, you wind up with an activation energy that is about equal to diffusion of deuterons in palladium. And therefore that must be the rate determining step, is that diffusion, not the coulombic barrier. It's the diffusion that's determining the rate.

Tom Grimshaw: Very interesting.

Mel Miles: You by-pass the coulombic barrier somehow.

Tom Grimshaw: So since you're only dealing with reactants and products, you don't worry too much about how you do that.

Mel Miles: Right.

Tom Grimshaw: Overcome the.

Mel Miles: Yeah, I.

Tom Grimshaw: Tom Grimshaw here, we had an interruption due to a phone call. But serendipitously, we're at 39 minutes, we usually try to stop at about 30. So Mel, Tom Grimshaw here with Mel Miles. We're going to stop this session, session number four I believe, and start a new session. It's July 18, and we're here at Mel's home in St. George, Utah.

**Interview 2-5**

Tom Grimshaw: Grimshaw here with Melvin Miles. It's July 18th, this is session number five as we do some interview and transcription of Mel's journey through cold fusion research. So Mel, I'm sorry to interrupt you, in the last session you were talking about Eyring's rate theory and how that applies to the cold fusion case.

Melvin Miles: Yeah, actually I was gonna mention it. It's this was published an abstract at this, the soundbite meeting at ICCF 20 in China. Xiamen, China, as well an abstract and poster at ICCF 20 in Japan. And so, in that way it's been published and, since Dr. Lee was a co-chairman though he wasn't able to attend the meeting in China. He had to write back and said that Eyring theory application was very important because that correlated with his work he's doing in China. That there is no real activation cool on the energy, activation barriers. So it just depends on diffusion, because some of their work had showed something similar, I think that's what he wound up saying. And so I'm trying to posit how this may happen. [Ed Storms 00:01:33] argues for cracks. I think it might be reaching near the double layer. On the table there you have, if you're doing a cathodic reaction on the palladium then you would have a build up of electrons on the capsule surface and a positive lithium ions outside the double layer.

Melvin Miles: That'd be the double layer, negative charged on electrode and positive ions in the solution phase. So that'd be an area of very high electron concentration. And so there's ways, even like Hagelstein argues for defects in palladium. And even there you could have a layer of a lot of electrons and lot of deuterons opposing the electrons. So you can find situations where you could get a lot of deuterons and a lot of electrons. And this might be a nuclear active environment and the reaction rate, or depending on how fast the deuterons can come into that active area of the palladium. So whichever way it turns out, I don't know whether Ed Storms is right, or Hagelstein is right, or Mckubre's right. Argued more of Mckubre's side and there, I do think there is an active region that. That it doesn't take place everywhere in the palladium. But active regions probably come and go, and they're probably very rich in electrons.

Melvin Miles: And electrons tend to address or pause your deuterons enough that the activation barrier is not, there's no longer, the Coulombic barrier's not a factor. And after, I think I did put the abstract on the discussion group one time and the person in Italy who's in charge of the next cold fusion meeting, what's his name? Collus? Or-

Tom Grimshaw: [Bill Kollus 00:03:50].

Melvin Miles: Bill Kollus, he can go right back and answer that that's what he thinks is happening. There's no Coulombic barrier inside the palladium wherever it takes place. It's not limited by the Coulombic barriers, by diffusion. So whatever his theory is, he right away agreed with what our theory said. So I'd still like to

publish the full paper sometime, but I need to, I've got another paper I'm trying to publish first and that's palladium in boron, and so the place will publish one off, and then submit the other. So that's about where that stands. It is on the computers. You have the computer files and that paper is on there.

Tom Grimshaw: Okay. Well good. I'm not going to do anything with it unless you tell me to or asked me to, but right now we're just kind of collecting the information of course. So this goes back to your graduate school days perhaps when you first were working with Eyring and became aware of his work. The general version of his work, not as it's applied to the cold fusion case, is pretty well published, I take it? The one you said he probably should have gotten the Nobel prize for.

Melvin Miles: Right.

Tom Grimshaw: So as well published in the more general form.

Melvin Miles: Yeah. You know in almost every physical chemistry textbook, you'll find a page or so on the Eyring Rate Theory. It is still controversial, not everybody agrees with it, but they'll present it as a one of the prominent theories on reaction rates, and Henry Eyring passed away some years ago, but he son too, I got my PHD thesis with at least most of it was with Ted Eyring, and look part of it was with Henry Eyring. I sent him a copy of this Eyring Rate Theory and he's retired now and he wrote back right away and said his dad would have been thrilled to see that paper. He said it was a very good application of his dad's theory to the cold fusion area.

Tom Grimshaw: And it's in the lattice of the palladium, his words being applied.

Melvin Miles: Right. And I took a full year course on the Eyring Rate Theory as a [inaudible 00:06:24], in the textbook they had written Eyring and couple, two other people, Gladstone and another person. But we covered all aspects of the Eyring Rate Theory, chemical reactions as well as electric chemical reactions as well as diffusion and viscosity. Anything that involves time, any rate process. It can be treated by the Eyring Rate Theory and I wanted, when I sent it to the Journal of Condensed Matter and Nuclear Science, whoever the referee was, he tried to argue with me about the Eyring theory. He has never taken a class, you know like I did, but he was arguing that I was not right on the Eyring Rate Theory. And all Eyring Rate Theory was just a simple equation. It's not an equation, it's the whole book. There is a most useful equation that I used, but there are hundreds of other equations involved in the Eyring Rate Theory.

Tom Grimshaw: Okay. Okay. Let's take it back to the days when you were working. You'd gotten the palladium rod, you were working with the two postdocs, then Ben Bush wanted to come for another postdoc and you were working with him on a correlation between heat generation and what turned out to be helium-four not helium-three, using the D+D reaction. Can you kind of pick up the thread at that point? Bush hung around for a while and then he moved on. And so can you kind of tell us the story from that point?

- Melvin Miles: Well, the postdoc appointments are usually for when you're maybe renewed for another year, and before Ben Bush came, both David Stillwell moved on to another position. I think back east for working for, in the State Laboratory.
- Tom Grimshaw: He was the one who was supporting your work?
- Melvin Miles: Yeah, he did a lot of the early work and he was on most of my early papers. [Quan Park 00:00:08:47] did some of the early work but not as much, and he moved on to another position and finally I think he went into dentistry eventually.
- Tom Grimshaw: Right.
- Melvin Miles: And so then that's when Ben Bush came in. He first, I first met Ben Bush, it was after I, after the paper was already pretty well completed that he came to to like, it leaked through the interview. And I'd never met him before. And you know, talk to somebody on a telephone, you have a picture of what they're like and when you meet them sometimes it's not quite. And then it was not quite the same as what I thought. One thing he was older, he had been around for quite a while, I think he's already in his thirties. So he was older than I thought he was and we got along fine, except he was kind of hard to work with because he always had his own ideas and if what you wanted to do didn't agree, he would do it his own way and he wouldn't do it your way.
- Melvin Miles: And that got me in trouble with people sponsoring in a Navy program later because he would not follow the program and he'd just go his own way. And he was hard to supervise because he would come into work at three in the afternoon. He'd work until maybe three at night, after midnight. And so he is, most times he's there on his own and he would not, I didn't know exactly what he's doing and most of the time, one time he had a cell running and I looked at it and I said, what's that gunk in there? And he said, well, I put a polymer coating on the glass, it's on the inside of glass, and the polymer coating's come loose. Well, I never knew he did that. We've since learned that, if you use a polymer coating, you don't get an effect. Somehow the silicates in the glass are important in creating the effect and [inaudible 00:10:44] explains that you need to build up a somewhat inert silicon layer on the cathode so you get high current densities through just little small regions of the cathode.
- Melvin Miles: That gives you very high over voltages and that gives you a lot higher pressure thermodynamically of deuterium in the palladium. And so anyways, it's been argued that as far as I know anyway, nobody has found cold fusion effects without using a glass cell.
- Tom Grimshaw: Yeah.
- Melvin Miles: Right. And so anyway, Ben just got me in trouble several times. He wrote a letter to the head officer of Naval research told them how the program should be run. And [Bob Novak 00:00:11:34], my direct supervisor, was quite upset. He said he had to intercept that letter. He said we would have been real trouble that got to Fred Southfield, you know, somebody down below telling them how to run, how the program should be done. And Mike Mckubre will tell you about the same

thing. Ben Bush was skilled in a lot of ways, but he was, you couldn't get him to do what you wanted. He did what he wanted.

Tom Grimshaw: Right.

Melvin Miles: I don't know how much is right to go into, but...

Tom Grimshaw: Oh that's, you know.

Tom Grimshaw: Right. He could have maybe been hard to terminate. Except his personality was, so those people, some people thought he could be dangerous in the Navy and the way he would he would talk without permission.

Tom Grimshaw: It's called, in part of, a loose cannon.

Melvin Miles: Yeah. Loose cannon. Right. And the head of research talked to Ben Bush one day and Ben Bush, I don't know what Ben Bush was telling him that he later told me, you said that. He said, you know, that guy can be dangerous. Yeah. So, he wouldn't have been hard, even if it were possible to terminate, because it wouldn't have been approved higher up. Okay.

Tom Grimshaw: Yeah.

Melvin Miles: He wound going back University of Texas until [Lagowski 00:12:51] retired. That's the last I've heard from him.

Tom Grimshaw: Yeah. I tried to look him up for you at your request. It was ICCF 14, 2008, you asked me to see if I could find him and I went over there and they didn't have anything at the chemistry office and Lagowski was not around. He had retired by then and so I could not find him, you know, there was no record of him.

Melvin Miles: Last time I saw him was in this March meeting of 2003, was held in Austin, Texas of the APS, American Physical Society. I told Ben Bush about the meeting and so he was already in Austin, so he was at that meeting. I don't think he presented to anything, but he was at the meeting and later I spent an evening, Linda and I go into his lab and talking to him. He showed us the lab in Austin, Texas. He pointed out a tower, there were some gunman that shot a lot of students at one point. And-

Tom Grimshaw: Charles Whitman.

Melvin Miles: Yeah. Right. And that's the last time I saw Ben Bush and he had a second, he had a second job working, you know, electronic manufacturing company. And he was in the good quality control, I think.

Tom Grimshaw: Safe place for a loose cannon. Okay.

Melvin Miles: And what happens since I don't have no idea. Yeah. Well you could probably pay to do a search. I looked up his name and I got the point where I'd had to make a payment to go further.

Tom Grimshaw: Yeah. Yeah, you can hire a detective and run him down. Sure.

Melvin Miles: Or you can do it on Google. They have searches for people, but when you get down to really trying to get information, they want you to make a payment.

- Tom Grimshaw: Yep. Okay. So you have this long thing with Ben Bush, what then? What happened after that?
- Melvin Miles: Well Ben Bush went to a SRI after about two years, that'd be in 1993 and they were trying, like you kept talking about the fact that we haven't, NRL has been sending us material for almost two years. Palladium, silver, other palladium materials, and I would run test on them and one after another was no excess heat, two years almost noise excess heat. The only thing is in between, Fleischman sent me a palladium sample that worked for him and that also worked for me. And that's the only thing I saw. I saw I positive excess heat on in about a two year period was this palladium, I mean Fleischman said it was palladium silver. And I send it, I send a sample onto Bannon. He also measured excess heat with it. He did analysis of it and he said it was palladium cerium, not palladium silver, I don't know.
- Melvin Miles: Either Fleishman was wrong or Bannon was wrong, but after that I called it palladium cerium.
- Tom Grimshaw: Okay.
- Melvin Miles: And I studied it in a, I took the same electrode with me to Japan and it gave excess heat there. It was experiment run in cell A-3, A-2 was palladium boron, that's the one in the middle. And the one on the end was the palladium cerium from Fleischman and again gave excess heat in Japan, and the position A-1 was a NRL material made by Dr. Ingham, was palladium boron cerium. And thing now, the cerium in that case didn't make, didn't help out, it didn't give any real excess heat. Or very, very little if any, and so that was the one experiment they did that it did not give excess heat in Japan. But because it didn't give excess heat, the very early cell constant I calculated was pretty close to the true cell constant. I calculated about 0.83, I think Fleishman later out the true cell constant was 0.85 something.
- Melvin Miles: The one that looked almost identical to me, the three cells, they looked very identical. I would assume they would have the same cell constant. Well the one giving no excess heat, it looked like it was rigged on and measured 0.83 with it. The early measurement was just palladium boron, there's just excess heat present, gave your lower value. It comes, since you don't know the excess heat you calculate that assuming it's zero, and according to them Fleischman that's the Fleishman method of getting a lower bound constant, and so you just run it. You just get lower bound constant and take their highest value it ever hits, and use that for the true constant, and that's what I did in Japan.
- Melvin Miles: I base everything eventually on that lower bound value, that and the highest one I measured. Not exact, but it was pretty good approximation there. A lot better than NHE. They did a calibration on the third day. It was already getting excess heat on the third day, and they came out with a cost of like 0.78. That's big difference than 0.85. And so they, a lot of their period they, this is in the in the NRL report. Half the time they're way below zero and half the time they're way

above zero. It just moves to the zero line. You just move those terrible, there's the lineup, the two between all the peaks.

Tom Grimshaw: Yeah. Hm. Well, okay, so we're now at 18 minutes. I think what I'd like to ask is that we pick this up again tomorrow morning. Right. We'll pick it up at the point-

Melvin Miles: You know where to, wherever we left off.

Tom Grimshaw: Yeah, it'll be-

Melvin Miles: We've jumped around a little bit from China Lake to [inaudible 00:19:04], I mean the China Lake program ended in 1985 in June. I'll maybe want to talk about, that ending was very strange way it ended.

Tom Grimshaw: Yeah. Well I think where we'll pick it up tomorrow is again what you were doing, you know, with your two postdocs. Then with Ben Bush and I think, we'll pick it up at that point. We did a little bit of that and we'll take it forward in time. To the end of your time knit with China Lake and then we'll see where we go with that.

Melvin Miles: Yeah. In 1994 and enough to June, 1995 was mostly palladium boron, which gave excess heat in every experiment, I think at least except with one and even then, and I think the one gave us some, but it was seven out of eight.

Tom Grimshaw: That was the Johnson Mathy?

Melvin Miles: No, that was the NRA. Palladium boron.

Tom Grimshaw: Okay, so that's what you got from [crosstalk 00:20:00]

Melvin Miles: One of the big goals as the Navy program was to make your own material and get excess heat. Well we actually did that right here at the end, but because NRL could not repeat it, it was not believed. So the program ended. I even went back to NRL in January, 1995, and set up a palladium boron experiment there in their new CBET calorimetry. In one of my papers I'm trying to get published right now, it did give early excess heat, but they didn't recognize it. It was because of, they had the gas outlet lines going horizontal. I didn't know that. I helped get everything ready, but I didn't see how they assembled the gas line. If you have a horizontal gas line, the gas comes out hot, the [detail 00:20:55] was hot and so is the gas, when it hits a cool area, that all will go back to the liquid.

Melvin Miles: So these lines get clogged with liquid unless it goes straight up. It goes straight up, it will cool, but then it will flow back down into the cell. So I had the exit line, the glass exit line like this, and so did Flieschman and [Palms 00:00:21:14]. So I saw their gas exit line, about the same as what I usually try and make. You'll see detail condensing, but it condenses the liquid and it goes back down.

Tom Grimshaw: Yeah.

Melvin Miles: If you have them, you connect to a Teflon line to that and have it horizontal, it'll get clogged. That's what happened later in their experiment, they saw big oscillations in the excess power. Well, you're seeing big oscillations in the cell pressure. The cell pressure builds up until it gets big enough to have a little burp, and then the pressure goes back down. It goes up and down, up and down, and

pressure's one of the variables. I mean the work is cell does, is equal to the negative of the external pressure times the change in volume. And you're having pressure builds up and goes back down and you're seeing that work term fluctuate all over.

Tom Grimshaw: Yeah, yeah, I can see that. Well let's, let's go ahead and wrap this up and pick it up again tomorrow with what I believe will be session six. I think we're on five now. So it's Tom Grimshaw with Mel Miles here at his home in St. George, on July 18th. We'll pick it up again on July 19th. Thank you Mel.

Melvin Miles: Oh, I didn't know you recorded that last part, but-

**Interview 2-6**

Tom Grimshaw: Tom Grimshaw here again with Melvin Myles, Mel Myles. We're talking about his cold fusion career and today is July 19th. We had five sessions yesterday Mel, this will be session number six. We're here at Mel's home in the back yard overlooking the beautiful Virgin River Valley, flood plain. And Mel, when we discontinued yesterday I think you were talking about the heat and helium correlation work that you were doing with Ben Bush. And what I would like to ask you to do is start there again for our sessions this morning and talk about the work that you were doing with Ben, Ben Bush. And the fact that you were doing the electric chemical work and the measurements the calorimetric measurements for excess heat and the helium measurements were being done by Ben Bush at the University of Texas. Now, I think that's an important collaboration for us to focus on as part of these interviews, so perhaps if you could make some more comments about that, Mel.

Mel Miles: Okay this, this is part of the work where I found the Johnson Matthey plate in the six millimeter diameter rod was the work every time and so I thought reproducing was easy and it was as long as we had that rod. I ran two different cells in the same water bath. I ran the cells in series, that is the same current went through both, and Ben Bush was an expert in keeping air out of systems. He was already working in that area. He had a lot of expertise in it and we had found that the gas he used to flush the system would be boiled off nitrogen. Which is, by the time nitrogen boils anything like helium would be long gone, it would boil off a lot earlier. And so it would be helium free so that was not a problem.

Mel Miles: And so he, he prepared the flask at the University of Texas and he modified the flasks, they were round bottomed flasks about 500ML volume, and he changed the inlet and outlet so you'd just have a single inlet and a single outlet and he would tape them over with forma tape so they wouldn't break on shipment and that would, turned out it would also impede the heat [inaudible 00:02:57] more than just the glass alone would. And this happened to be one of the best excess heat effects I've ever seen. It was the largest excess power was in this experiment, so we were getting a large effect, which was good because that made the heat of margin it could be measured well above any contamination by head and diffusion through glass. And I didn't realize at the time but that turned out to be the biggest effect I'd ever measured was that experiment.

Mel Miles: It got up to about almost 40% excess heat, the ratio of heat out to heat in was about 1.4. Now, that doesn't sound like a lot but that's more than I've ever seen since as well. And I didn't know Ben Bush, and I'm kind of glad I did this. I didn't tell him which flasks showed large excess heat and which showed the low excess heat and which showed the medium or none. So he didn't know that before hand. And the reason I'm kind of glad I did that is I found Ben Bush was really so anxious to prove cold fusion that he might have change things, I'm glad he didn't have

that chance, because he just thought cold fusion was absolutely true and had, didn't seem to second guess the work enough, I thought.

Mel Miles: So what I did, I, the numbers I put on was the birth dates of some of my kids and family members and so they were just, they had actual 1/18/37 that would be my birthday and then one of my kids was 5/5/75 and things like that. And I didn't I didn't really expect a correlation, I was hoping for one but I didn't really know. And I was kind of surprised when it started to really correlate everything quite well. And I'm always a critique of my own work and I try to figure out why and I never could come up with a good reason other than that it was being produced in the experiment by the excess heat.

Mel Miles: And I think we mentioned yesterday that I was concerned because so much anti-cold fusion people everywhere, including the Navy. I think some people in China Lake, I've heard this from several sources, thought I was making a mistake, it wasn't true no matter what I reported. And these were people in the Navy and in fact ought probably should have gone through different chains and I run into one of those type of people and they might not want to allow the Navy to publish it, so that's what my concern was. So to get published quickly, we had it submitted through the University of Texas. And the head of research later told me that I should never have done that. He said the work was done in China Lake, it should have been submitted through China lake. And so I got into a little bit of trouble but he was one of the supporters of cold fusion so I think he probably would have helped me get it through if I had done it the right way.

Mel Miles: But anyway, it got published quite quickly, it went to the Journal of Electroanalytical Chemistry where Fleischmann and Pons published their first paper. And the editors there knew Fleischmann quite well because they'd worked with him and happened to have seen a lot of his work so they really believed, knew that Fleischmann was a good scientist and that's why they published cold fusion quite readily. And so it passed review right away and got published. I think we submitted in January and it got published in March. And so that was the first study and then after that Ben Bush got a post up at China Lake and he, early in the year was 1992 or I think 1991. We actually did this work in, towards the end of 1990 and it got published in 1991, I think. So that was fairly quick as far as science goes that it got published.

Mel Miles: We found helium 4 within, in less than two years after the announcement. I don't know why other people weren't looking for it but it is a difficult experiment because you have to be good at keeping air out and Ben was the top person for that. And you also have to have large excess heat. If you don't have large excess heat it doesn't, wouldn't do any good to look for helium 4 anyway. Both CalTech and MIT did helium 4 but, they didn't find any excess heat so of course they didn't, they wouldn't find any helium 4. If they didn't see excess heat, so why bother. And so, Ben Bush worked with me for two years [inaudible 00:08:39] but like I said before, he was kind of hard to manage, he did things his own way. He worked mostly late at night, so I didn't know what he was doing.

Mel Miles: And when the idea, and I don't know if I gave him permission or not, I don't remember. But Brian Oliver and another lad that did helium 4 in LA area, they wanted us to send a sample of the electrode so they could analyze it for helium 4, but by then we'd already written a control in H<sub>2</sub>O, so I think if helium 4 was there it would have been probably placed out by lowering the hydrogen and deloading again, but anyway Ben Bush cut up those two electrodes and that was the end of those two electrodes. And I don't know whether I gave him permission, if I had I shouldn't have because if you find electrodes that give excess heat you really should not let them out of your sight, because that a very rare thing to have, is an electrode that will give you excess heat in every experiment.

Tom Grimshaw: You mentioned these were from Johnson Matthey in yesterday's conversation.

Mel Miles: Yeah these were both Johnson Matthey and if you took, I worked on cold fusion I guess unfunded from 1989 to 1991, I think the funding, I think we didn't get official funding for it until 1992 and so, but I worked on it for the Navy from 1989 in the background and then we had Navy funding, that was for three years. I think those three years were 1992 to 1995.

Tom Grimshaw: So I'd like to reel back to the work you were doing with Ben Bush from what I heard you saying that they, the work you were doing with Ben when he was at the University of Texas, that was, the heat and helium correlation, that would have taken place before he came for his post dock because he would have been gone from the University of Texas then. So, he was still at UT at the University of Texas, you were at and you were at the Naval Air Weapons station, so that was during that period of time, that you established the correlation, is that true?

Mel Miles: Yeah I'm trying to get the dates straight in my mind. 1989 was the announcement, in 1990 we had the cold fusion meeting in Salt Lake where I reported for the first time we had excess heat. That was the first time I was probably convinced that we had real excess heat and I reported it at the cold fusion conference in Salt Lake City. And I knew of Martin Fleischmann and Stan Pons, but I'd never really met them and talked to them and nobody knew anything much about me or my work at that time and my presentation in Salt Lake City was on the last day of the meeting. It was on a Saturday and I remember when I gave my talk I guess it looked, it agreed so closely with what Fleischmann and Pons were doing that after I finished my talk and sat down, here comes Stan Pons across the room and really congratulates me on that talk. That's how I first met Stan Pons, he came over after my talk and really liked it. You know, because it showed many things that they had found as well, I think.

Mel Miles: And then following that, that was in march of 1990 it was a very strange meeting because we had such a mix of hot fusion people from MIT that wanted to shoot down the cold fusion idea and people from different parts of the world like India, Dr. Srinivasan that reported positive results. And anytime anybody had a positive talk, one of these cold fusion people would jump up. They knew the press was there, a lot of press was there, and they would just make statements of just ridicule of cold fusion and they were just playing to the press. I've never seen a

scientific meeting where a person gives a talk, positive, and then somebody jumps up and tries to just use a ridicule. Ridicule would be the right work. They would use all types of ridicule like, one I remember exactly, was they, this guy got up and said this is something like Alice in Wonderland, you know. Things like that.

Mel Miles: That doesn't normally happen at a scientific meeting but they were making these statements for the press. This was not even believable and they made an attack of ridicule way that I've never seen ever happen in a scientific meeting. And so that was March 1990 and then in end of June, first of July, we had the meeting in Como, Italy and now the press left us alone, the critics weren't there and we one of the best meetings that I can remember in cold fusion and a lot of people also comment on that. It was the most, the most advancements happened at that meeting.

Mel Miles: I reported on the heat and helium results and Stan Szpak of the Navy, reported on codeposition, a different way to do cold fusion and there was quite a few other. McKubre report on his calorimetry, which was flow calorimetry with excess heat, and of course Fleischmann and Pons reported on their work, and then Utah State University person, whose last name is Hanson he had done an independent study of the Fleischmann and Pons report and he came up with it. It was real excess heat and that was the independent study authorized by the state of Utah for someone to independently analyze the results.

Tom Grimshaw: That was the paper published in the Journal of Analytical Chemistry by Fleischmann, Pons, and the graduate student?

Mel Miles: Right, right, yeah.

Tom Grimshaw: Okay.

Mel Miles: Yeah, the Hanson paper was, probably shouldn't have cold proceedings as well as it was submitted to the state of Utah for review. The state of Utah had given 5 million to cold fusion for the cold fusion institute. It didn't last more than a year or so at the university of Utah.

Tom Grimshaw: [inaudible 00:15:58]

Mel Miles: Yeah and but they wanted somebody independently to analyze the data and that was Hanson that did that.

Tom Grimshaw: Okay, so, what, so you reported the heat helium results at Salt Lake City ICCF1 in early 1990.

Mel Miles: Well no, we the first time I reported that was in cold fusion, ICCF2

Tom Grimshaw: In Como?

Mel Miles: In Como, right.

Tom Grimshaw: Okay, okay.

Mel Miles: The thing I reported in Salt Lake was the, my, the use of Johnson Matthey material and getting excess heat. I even showed where we had a power failure

during the experiment and so it had to deload, the power was down, it deloaded and when I started it back up it was back to zero. That's a good test really if you do that. It was accidental, but if you turn it off and deload it and restart up, you should be back at zero and that's exactly what happened. Then it kicked in again a little bit later. Now I think that's one thing that Pons liked about that result, it showed that when the power went off and it deloaded back to zero excess power and then it comes back up again.

Tom Grimshaw: So when in that time frame did you, do you recall, did you first send a sample to Ben Bush? In other words, when did your collaboration with Ben Bush start during that time frame?

Mel Miles: It was, after the Salt Lake City meeting it was about in the fall about, I guess about October/November that I, we started to collaborate. And I set up a new experiment for the helium study and it correlated with some other things I wanted to do and so that experiment was done, all the details and parts of that experiment were completed by the end of 1990. And following the ICCF1 meeting and before the ICCF2 meeting. And it was probably in March of 1991 and the Como meeting came in end of June first of July 1991. That's how that all fit together, I was trying to get the data correct, that was 1991 and the Como meeting a bunch of us Navy people got together, there, several of the Scott and Talbot Chubb, NRL, David Nagel from NRL, myself from China Lake, Stan Szpak from the Small World Laboratory in San Diego, and Frank Gordon, all these Navy people were there at this meeting and that's when we decided we should try to get a funded Navy program.

Mel Miles: And that was the end of, that was the Como meeting in December 1991 and this program got set up by the end of that year and that was the end of 1991 and then Ben Bush started in 1992. January 1992, that's how that fits together. But all that, the first study was all done with Ben Bush still in Texas and me at China Lake. That was all completed in 1991, about the last three months of 1991. In January of 1992 we did a control study vestibule and the helium samples, I mean the gas samples did not show helium 4, in those it did not show excess heat so we knew we had to have a control so people knew that we could, we didn't have helium 4 contaminating from the air. But I guess a lot of the critics ignored the controls. And that was good proof that we were, we knew how to keep helium from the air out of the satin system.

Tom Grimshaw: Okay so your first samples went, you set up the experiments, you conversed with Ben Bush, you set up the experiments and you started doing the calorimetry and the excess heat studies and then sending the samples to Ben Bush, that was in late 1990 then, in October of 1990.

Mel Miles: Yeah, 1990.

Tom Grimshaw: And then you completed the work with Ben Bush and he came to his post dock by the following year.

Mel Miles: January, January 1991. [inaudible 00:20:49]

- Tom Grimshaw: Yeah I understand, that's what I'm kind of working on a little bit here.
- Mel Miles: And so when I went to Como we didn't have enough, we didn't have enough funding for Ben Bush to go because we didn't have any Navy funding for cold fusion anyway. And so I was the one that, I was at Como and Ben was still at China Lake I remember, that would be in 1991. So Ben Bush started, he started January 1991. And he worked two years and he left, went to SRI in 1993.
- Tom Grimshaw: So about how many samples did you send to Ben Bush for helium analysis, do you recall?
- Mel Miles: It was like 8 or 10, somewhere in that area.
- Tom Grimshaw: And they were in batches, how many batches did you send to him? In other words, how often did you send samples to him? How many batches?
- Mel Miles: Probably about three, three different times. Some samples earlier on and kind of I would collect at different times and I think we had the dates listed when the experiments were actually done and the way we covered, the first was my family member birthdays, but when we published it, I think by then we did [inaudible 00:22:21] when the sample was collected. And so we had, we had the graphs of the excess heat by date and we, on the excess heat chart we would show the collection points, when it was published. At least the later publication maybe not on the initial preliminary paper. But on the final paper, it was about a year or two later before we completed everything. We had to study the diffusion of heat through glass and things like that.
- Tom Grimshaw: And do the control?
- Mel Miles: We did the control actually for the first paper as well. But it took longer to write the full paper it was a much more thorough paper and that was probably published in '92 or '93 I think. But the first helium study was all, all the detail on heavy water studies was completed at the end of 1990. And published early in 1991 and reported at Como, Italy in 1991.
- Tom Grimshaw: Right, okay thanks for your patience. I wanted to kind of drill in on this because it was such an important part of, not only your career in the field, but for the field as a whole. This correlation between heat and helium. Okay. Any further comments about that work, that early work with Ben and we'll move on I guess to what you did with Ben after he was a post doc at Naval Air Weapons Station and then your other work from that point.
- Mel Miles: Well, because those good electrodes got destroyed, we really had a hard time ever seeing excess heat again. I don't think, I don't know of any experiment that Ben Bush set up that ever showed excess heat for example. And I, we went almost that entire year, 1991 to not see any excess heat. Toward the end of the year we saw a small excess heat effect it was another Johnson Matthey electrode that was just a small wire, 1mm diameter. And so based on the volume you would expect only a small effect, and that's what we saw. I didn't know about the volume effect then, but looking back at it, the size of that electrode being only 1mm thick you would not get the big effect we had seen earlier. That's what we

saw, it was, it was large enough I could be pretty certain about it. I mean it was 100MW, well above the error of my calorimetry. And another sample was 50MW and a third sample was 20MW, and that would have been right near the borderline of what I could measure.

Mel Miles: And the reason we collected this was because Rockwell international and Brian Adler wanted us to send samples to them so they could measure it and we did that. And it was double blind, we didn't tell him which samples had any excess heat, if any. And he didn't tell us what he measured for helium 4. And this was all reported back to Glowdowski, University of Texas, independent. And so it was double blind. But when it came back, well another thing they did they made the rate of heat diffusion through the glass, they did it over about 100 days and you could see the helium, kind of in a linear fashion, increasing with time. There is some diffusion through the glass. And they could extrapolate the straight line back to zero when the sample was collected, and that's why they reported how much helium there was initially in the flask by this extrapolation.

Mel Miles: And this turned out to be some of the best results in correlating with the predicted amount of helium 4. It came very close to what is predicted and of course the 100MW sample showed the largest amount of helium and the 50MW showed a medium value and the 20MW showed the lowest value. It was right in the right order. And I don't think Brian Alder was convinced it could have just been from the extrapolation but he probably says in the book by Hoffman, I have that book. They reported in like it doesn't mean anything, but to me it did mean something. They didn't know the amount of helium I was measuring and it came out, it was just by accidental it came accidentally just the right amount, so that seems strange that would happen that way to me. So I thought it was quite significant, and some of the, you know they say you should get, based on helium it should be 23.8MEV per helium, well it came out quite close to that. The 100MW came out the best, it was the most accurate as far as the calorimetry was concerned.

Tom Grimshaw: I'm familiar with that book, that Hoffman book. We did record it in your library for our report.

Mel Miles: He was an honest researcher but he was not convinced at all in cold fusion.

Tom Grimshaw: Coincidentally, he was well known by Arik Albor cause of some work he had done in Israel. That I've learned from Arik.

Mel Miles: Who was that?

Tom Grimshaw: Arik Albor who was the laboratory leader for energetic technologies and then Skinner. He knew Nate Hoffman, when I mentioned this book, Arik was, he said, could that be the same guy? It was quite an interesting phenomenon.

Mel Miles: I think Nate Hoffman was Jewish, I think. And I think his background is Israel.

Tom Grimshaw: Yeah and Arik even told me the story, we're not going to digress on this very much for this recording, but there was a, oh it was quite a intereting story where they had, in Israel, done an experiment. And they were very cautious. And it

might not even have been cold fusion. I better just clench this up right away. But it was an interesting side story from Arik Albor I have it in the recording of that interview, so. Okay.

Tom Grimshaw: So, you mentioned that you worked with Ben Bush after he came on as post doc, but of course the helium work did not continue because you didn't have the University of Texas spectrometer to do the helium analysis anymore. And maybe just a few words, if you would, to kind of review that two year period with Ben Bush and what you did. And what you reported, and then we'll move on to the next phase.

Mel Miles: Well you know, I divide my helium results into three groups. The first was with Ben Bush and the University of Texas. The second was Ben and I going [inaudible 00:30:22] to Brian Oliver at Rockwell International. And that becomes the best results as far as correlating that I've ever seen despite the fact that Brian Oliver would probably not accept that. And then the third group was some, we were trying to convince people it was not using diffusion through glass so we, we made metal flasks instead. And so we cut through the sample in metal flasks. And Ben Bush is good at building things. He actually built these metal flasks, and I still have them out in the garage actually, at least a couple of them, I may have given one of them away but. It just had inlet and outlet and it has a special valve that it makes when you close it, it's a metal to metal seal. There's not any plastic involved, to keep helium out. Make sure even the way you close the flask has metal to metal, it doesn't allow any helium diffusion. Ben Bush left before we actually got to use these.

Mel Miles: He went on to SRI, so I did, I finally got some samples of excess heating in and a lot of samples that didn't. I had the helium measures done for both and while he was there Ben Bush had located a person at, in Amarillo, Texas that had a government laboratory there, department of interior laboratory. And Amarillo is called the helium capitol of the world. And they would routinely, they did helium analysis and so they could measure down to +/- one part per billion. Which is almost as good as Brian Oliver, he claimed +/- .1 part per billion. Brian Oliver was the best I guess. But this Amarillo laboratory was very good.

Mel Miles: And so I sent samples to there. Some that did not have excess heat some that did. And the five or six that did not have excess heat, they reported pretty close to the same background. And so I finally knew what the background was. I kept my equipment, on purpose, exactly the same except whether we collected in glass or whether in metal. I knew if I made changes, I wanted to make changes I thought we could make the system better. The lines, you know the gas lines and so on but I knew if I made changes I would change everything. So I kept everything the same. So there's a period that everything stayed the same. And so when we went to metal flasks I finally had the background. I knew what the background was. It was not very large but, and then when I had excess heat it was quite a bit larger than that and I could extract the background.

- Mel Miles: We're talking about parts per billion though. Like the background was 4.5 parts per billion and I was getting measurements up to 10 or so parts per billion. So we had to do extraction. You could do it without the subtraction but I found it agreed better with theory with the subtraction one that could have excess heat, subtract the background off, you got even closer to theory, theoretical amount.
- Tom Grimshaw: That's always convenient.
- Mel Miles: Yeah. And I, anyhow, you gotta be pretty sure the background is correct. But I think I did five studies and got a mean value which was 4.5 +/- .5 it was that accurate, +/- .5 and so I. And I was getting results back to a tenth of a part per billion or well, I guess it, I guess Amarillo reported +/- 1 part per billion that was kind of typical. Sometimes it was a little larger. I don't know why it varied. But it was never less than +/- 1 sometimes +/- 1.5 but somewhere in there.
- Tom Grimshaw: And we're talking about parts per billion helium
- Mel Miles: Parts per billion, and so. You know one thing people don't understand and don't think Ben Bush even understood this when we first talked about measuring helium I'd already done some calculations. I knew the amount of gas, you know the gas you generated by electrolysis you can calculate that and that comes out to fill a 500ML flask, that's .5L and if you calculate the number of atoms in there it comes to like  $10^{22}$  atoms of D<sub>2</sub> and O<sub>2</sub>. I should say molecules actually, D<sub>2</sub> and O<sub>2</sub> molecules.  $10^{22}$ . And then you calculate how much helium you get per certain excess power and I could see it was going to be in parts per billion and I knew that pretty much at the beginning. That's why I questioned Ben whether how accurate he could do everything.
- Mel Miles: You know Ben, much later, would tell people he talked me into doing helium 4. I was trying to just make sure he could do it accurate enough. Not that he was trying to convince me, I was just bringing up how accurate can you do this, because I knew how accurate it had to be. And even in the book that Charles Beaudette did, Ben Bush talked to Charles Beaudette and I saw what was written and it was something like it was mostly Ben Bush's idea and he convinced me to do it. And I had to tell Charles, that's not quite the true story, so he could revise it down somewhat. So that's a little bit of conflict to have with Ben Bush that he tried to claim a lot of the credit for what we did, at least early on.
- Mel Miles: But I say, the way I believe it, he could not have done it without me and I probably could not have done it without him.
- Tom Grimshaw: That's called a collaboration. Okay so three phases. Phase one: University of Texas helium measurements, Phase two: Rockwell international Mr. Oliver, and then Phase three: a department of interior laboratory in Amarillo.
- Mel Miles: Right yeah, those are the, with metal flasks right.
- Tom Grimshaw: With metal flasks the last time.
- Mel Miles: And they all come, they all agree, all three different studies, different times, different labs, but they all got the same conclusion.

Tom Grimshaw: You were getting the same ratio of heat and, helium to heat.

Mel Miles: And it still corresponded about the same way, and this is a third paper, actually I'm preparing for a meeting ICCF-22. Iruqby already gave it at MIT but I'm, again I'm going to submit a similar abstract to ICCF-22.

Tom Grimshaw: I hope you're able to come and give it. So, okay. Good. Not to get too picky, you mentioned about 8 maybe 9 samples, in that range for that first phase, about how many samples were involved in the second and third phases, do you recall?

Mel Miles: Well I know exactly three for the second phase because they were all samples collected from the same experiment, just different times, when I had different amounts of heat. So those were three samples in that second phase, and that was the most accurate helium 4 measurements. It was not the most accurate excess heat because we're pressing too close to the limit. Especially the 20MW. And in fact in this new paper I'm preparing for ICCF-22 and actually I wrote it first as a book chapter because Biberian has gotten a company that is going to publish a book on cold fusion and he had different people submit chapters, I submitted a chapter on the helium 4 and that's the basis of the MIT talk and the other ICCF-22 as well.

Tom Grimshaw: Coming up, yes. And third phase, how many samples?

Mel Miles: There's about five controls and about five or six with excess heat.

Tom Grimshaw: Okay.

Mel Miles: Right, and I think the total if you combine everything, I think, I say I, there's a total of about 33 experiments and there's a correlation between excess heat and helium 4 in 30 out of 33. And the three exceptions all had something wrong with them. One there's a calorimetric error that was in set one. We found that the D2 level had gone too low, and if the level gets too low then you see a false excess heat. So there's a false excess heat and there was no helium for that sample. The other two exceptions were platinumserium that electrotoplasm gave me. Some how it gave excess heat but when we measured helium we didn't find it and so you could argue that's a different experiment its not really plagum but maybe somehow serium keeps the heat from coming out of the electrode or something and if so, it's probably still in there.

Tom Grimshaw: Yeah, okay. Alright so.

Mel Miles: I had no money to test that.

Tom Grimshaw: Right, so this is a good pausing point. We're going to go to lunch, you're going to have breakfast, I'm going to have lunch. And we'll pick this up when we come back. And I think we'll pick it up at the point after the helium work that you did in those three phases and talk about what you did at Naval Air Weapons Station and thereafter, after lunch.

Mel Miles: And with the help of a person at the University of Utah on statistics in my final reply I do a statistic analysis. The chances of getting those experiments, 30 out of 33 if you repeat exactly by statistics the chances is 1 out of about 500,000.

Tom Grimshaw: Wow, I did not know.

Mel Miles: If that was just a random error and they were just random, that's just a random error. The chance would be, and that's even including the three exceptions it still comes out one in 500,000.

Tom Grimshaw: Wow, I did not know about that. Is that also included in the report? The final report?

Mel Miles: In my China Lake report, I might add it when I write a paper up for ICCF-22 I might put it in there.

Tom Grimshaw: Yeah, that would be great.

Mel Miles: I think I did reference it in the book chapter. Biberian is the editor of that book I don't know how that's progressing. They wanted the papers in by April. But I wrote two papers, one on calorimetry one on helium 4. And I haven't heard anything back. I finally wrote to them and asked about it and they said Biberian is still reviewing the papers, I guess he's doing it all himself. It might take him a while.

Tom Grimshaw: Yep, okay. Tom Grimshaw here with Mel Miles. It's January, no July 19th. It's only 90 degrees out here this morning instead of 101 like it was yesterday. Well, it might be 101 when we pick up again after lunch. Okay, alright. Thank you, Mel.

**Interview 2-7**

- Tom: Tom Grimshaw here with Melvin Miles, Mel Miles. We're doing a series of interviews on his work career in cold fusion research. I believe we're now on session number seven. Mel, in the last session, I think you were, oh and I should mention that this is July 16th at about 2:45 in the afternoon. We're on the back porch of Mel's house overlooking the Virgin River and the temperature back here, the thermometer says 102. So, it'll be nice and toasty while we go through this.
- Tom: So this is session number seven I believe, I'll check here real quick, yes session number seven. And Mel you were, at the end of last session I think we covered the work that you did with Ben Bush during the two years he had a Post-doc at Naval Air Weapons Station working with you. We talked about some of the challenges in that, for example is cutting up the two palladium rods that you were using and sending them off for analysis. So, let's consider that description pretty much complete.
- Tom: Now, I understand Ben Bush went on to SRI in Palo Alto, working with McKubre. So if you did do some work with him while he was there, that would be great. But let's focus on what you did after Ben Bush here at, during his Post-doc here at the Naval Air Weapons Station. So take it away Mel.
- Mel: We were part of the Navy program. The Navy wanted to make our own materials. We knew that at the start of the program that the material was key problem. We didn't have the right material, whatever right meant, like Johnson-Matthey, generally it would not work.
- Mel: And so Dr. Imam started making palladium materials. I remember he made pure palladium. We tested it. We made palladium silver and some other materials. And the problem is, we tested these for a two year period and never found excess heat. And so, I guess, most of '92 and '93 were fairly negative as far as finding anything. The only thing that worked were, Fleischmann loaned me a sample. I was invited to give a seminar at the University of Utah sometime in that period, and Fleischmann was there. I remember Mike Nelic was there. Anyway, unlike my seminar I had given at BYU, they were quite respectful and treated me good. It was just a normal seminar and I got compliments on it. The seminar was on my heat helium results. And then, I met with Fleischmann and Mike Nelic later, and I told them I wasn't seeing any excess heat. And Fleischmann offered to give me two electrodes that he had gotten excess heat with. So these, I took these and tested these at China Lake, and sure enough they gave excess heat.
- Mel: If you have the right electrodes, you see the effect. If you have other electrodes, for some reason they don't work.
- Tom: Were those two electrodes that you got from Fleischmann from Johnson-Matthey as the ones you had had worked, that you had gotten previously that did work?
- Mel: Yeah. Yeah and I don't remember who made them. I assume Johnson-Matthey made them. They gave a pretty good excess heat effect. I took one of them with me to Japan. It

gave excess heat again. I know in the notebooks they wanted me to return them and I think I did. But I don't have them anymore. But they did produce excess heat.

Mel: It was kind of a negative period we spent. Even with Ben Bush there, we started on co-deposition. That was from the SPAWAR, and that was part of the Navy program. We ran co-deposition over and over and over again. Because it's a quick experiment. It only runs a few days. And we never did see any, in most cases, no excess heat. I later realized afterwards that we deposited such a small amount of palladium, if it gives one watt per cubic centimeter, then it would not have been measurable. And maybe that's the reason the deposition just does not deposit a large amount of palladium onto copper substrate.

Mel: And there were a couple of experiments that did show some excess heat, but I think only about one or about two in 30 gave excess heat. That was a lot of experiments we ran. And Ben Bush is part of that program. One thing we did see almost every time we ran co-deposition, we got excess heat, I mean, we had a Geiger Muller tube nearby and it normally was just normal. But whenever we started co-deposition experiment, it would shoot up, the amount of radiation. Even though we weren't seeing excess heat, apparently there was some radiation it was picking up. But that was hard to trace down. But it was some very large effects. In terms of standard deviation, I think like 27 standard deviations above background.

Tom: Wow. Well a Geiger Muller is not sensitive to the type of radiation, whether its alpha, beta, or gamma, I guess. So you didn't really know what it was that was producing, or what was being produced.

Mel: It doesn't tell you what kind of radiation. It'll pick up all three, alpha, beta, gamma. And so it doesn't give you information. Because of the radiation effect, Dave Nagel arranged for somebody from NRL to come out and measure it with instruments that could detect the type of energy. But every time he, he came out twice, and we set up experiments and run them, one after another, real quick, and every time he didn't pick up anything. And my Geiger meter didn't pick up anything either. So neither one picked up anything. We called it the NRL effect. Whenever they were there, it didn't show up.

Mel: I later realized that what we were doing, we were running experiments very quickly without letting the lines clear out. And co-deposition in the procedure, as the solution becomes acidic, it generates chlorine. And so we'd have chlorine left over in the lines. If we didn't let it sit, and let that dissipate, we wouldn't get the effect. And so it seems to correlate to the fact we didn't allow the chlorine to escape from the lines we were using. And apparently if you have chlorine present anywhere, even traces of it, it kills the effect. I mean that's the way I explain it now.

Tom: Who was the individual? Do you recall his name?

Mel: I have it written down.

Tom: Okay.

Mel: But he-

Tom: It's not a big deal.

- Mel: He was an expert for radiation at NRL that Dave Nagel knew. In fact, I think he worked for Dave Nagel actually. At NRL.
- Mel: So, we also examined the way we were using the Geiger Muller tube, and he said what we were doing was correct. Where you set the voltage on a certain plateau. What we were doing with the Geiger Muller was correct according to him. But even the Geiger meter didn't respond when we did these tests with NRL there. I reported this at ICCF Seven. I said cold fusion radiation with a question mark. As the title. At the meeting in Vancouver, Canada.
- Mel: And so it was kind of, not a lot of progress. If you don't get excess heat, you can't really test any variables. And so you just sort of, you're just stalled. You can't really make progress. And then the sponsor at O & R, because of this he started talking about ending the program. And from month to month, we never knew how long the program would continue from about 1993 on.
- Mel: The next success came about mid year in 1994. Dr. Imam sent me palladium boron he just made. The palladium boron he sent me was just as good as the Johnson-Matthey. It gave excess heat almost every time. So, when I first reported this, it got the sponsor at O & R quite interested. And when I gave a presentation, we had periodic presentations of our results at NRL, or else back in Washington, D.C. And the head of O & R was always there. That was Fred Saalfeld. He was the one that was giving money to the program. And he was quite interested in having the Navy solve this cold fusion puzzle. So he funded us for about three, two and a half years I think it was.
- Mel: When we reported results on palladium boron, there was quite a lot of interest in that. I went out to dinner that night. My sponsor said that probably saved the program. At least for a while. So it continued another year into 1995. And I continued to get success with palladium boron. But they wanted O & R, they wanted NRL to reproduce it. And they never seemed to be able to do that. Of course, their calorimetry was pretty flawed. So I think if NRL had reproduced it, it would have really been, the program would have continued. But that's, the main problem was NRL could never reproduce anything. And I think that's because of the people they had working on this at the time.
- Tom: So, may I ask a couple questions? So the Navy program got it's start, tell me again, when it was started and who was the sponsor and how many participants?
- Mel: Well the idea started with Bob Novak and Dave Nagel and other Navy people at Como, Italy, ICCF Two. That's where it started. So Dave Nagel pushed it and Bob Novak pushed it. And it finally got Fred Saalfeld to fund it. I think Dave Nagel did most of the writing of the proposal on that actually. The Navy proposal. Navy program. And of course the key feature we wanted to make, have Navy in house materials and produce the effect. That was, and then we wanted reproduction by other laboratories like NRL. Those were the main goals of the program. And so, it really got funded in January 1992 and Ben Bush started back practically. He was actually funded out of a different program though. A post-doc program.
- Tom: Okay. And who were the participants? It was NRL, SPAWAR and China Lake?

Mel: Right. It was mainly Stan Spock and Pam Boss at SPAWAR. It had a different name at that time when we started. It changed names several times, but today it's know as SPAWAR. Navy laboratory in San Diego. And it was myself and Ben Bush started with the Navy program because the other post-docs had left. And then towards the end it was myself and Kendall Johnson, who was, he was very good and he built a new, much more accurate calorimeter that could measure within a couple milli watts. He spent a year doing it. Almost his whole time at China Lake was spent building the calorimeter. And he only got to run a few experiments before the program ended, actually. And so that was, it was interesting. But it didn't lead to very much future work, using that calorimetry. He got another position at China Lake and eventually, he didn't like the heat of the desert. He was from up in Northern Utah, and I think he wound up somewhere back further north where it's cooler.

Tom: Okay. So, the palladium boron samples that you were using. That was part of the Navy program. You had success with those samples for about the last year or so, I think you said, of the Navy program before it was shut down. And when the Navy program shut down, did you discontinue cold fusion research at that time?

Mel: Yeah, I didn't really have any choice. Without funding, you really can't do anything. And I was told to find other funding. I spent a lot of time just trying to find funding on something else.

Mel: So I didn't really do cold fusion research. I was really looking for, during those years, I think I was looking for another place to go to continue cold fusion research. I was talking to John Dash, there's a possibility of going to work with John Dash up at Portland State University. I was just discussing options with George Miley. Maybe a chance to go work in his laboratory. I even got a offer from Preparata in Italy. They paid my airfare to go to Italy. This was in February 1997. They were trying to get funding from Pirelli in Milan, Italy. I saw Preparata's lab. He was working with Emilio del Giudice. The three of use. Fleischmann flew in from England and we were, all three going to meet, and give a presentation at Pirelli at a following date I think. But I saw the lab that Preparata set up in Italy. And this technician that was doing his work. And Emilio del Giudice, who was more in theoretical work as was Preparata. Fleischmann thought that would be the ideal group. He said that would be the best group that he could ever hope to work for. Better than when he was working in France with Japanese funding.

Mel: Because he was not too happy, as I found out in letters and so on that. I guess he wanted to do more basic research. They wanted development into product. And that's often the case. They want to skip the research and go to development.

Mel: Anyway we did meet and give a presentation at Pirelli. And we thought that they were going to fund us. And I was thinking of, you know, Linda and I were going to move to Italy. But then the funding never came. And I heard later that Pirelli decided it might hurt their reputation to get into cold fusion and setting up a laboratory where they funded it. But they were, it seemed like a nice group. You know, I ate dinner with them and I thought it would likely happen. But they changed their mind at the last minute I think. And just thought it would, there was too much bad publicity about cold fusion going on.

Tom: Too much risk to their, it might besmirch their reputation.

- Mel: Right. They did set up some work in house. And I saw Pirelli people. At the next few conferences there were always some Pirelli people there that were doing work in house.
- Mel: And another incident happened. While I was there, a TV film crew came in and Fleischmann and Preparata told me they were going to do a good story on cold fusion and I should talk to them. And so they interviewed me and I said. They wanted to, they did things I didn't like. They said, "Let's pretend, make it look like you're in California." And they took things off the wall. And that's not really truthful, I thought. You know, trying to interview me and claim I was in California. And then they had, they took some films. You know I had already arrived and checked into the hotel the day or two before. They [inaudible 00:18:04] me coming in a taxi and being dropped off and going into the hotel.
- Mel: And I thought Fleischmann, Pons, Fleischmann and Preparata were correct. Later I heard from Emilio del Giudice that they did a terrible story attacking cold fusion. And using, showing us in bad light. Everything they took. They might have even said here I was lying that I was in California. You know, who knows. I told Emilio to send it to me, but I never got to see it. So I don't know how it wound up, but. But Emilio said it was terrible.
- Tom: So these were the years when you're still at China Lake, but you were not doing research. You weren't doing cold fusion research because the Navy program had come to an end. But you were still getting paid at China Lake for some period of time.
- Mel: Well, if you don't have funding, you're in a bad position. I didn't like it. You're on down time. And you charge to overhead, and nobody likes you doing that. Or else you charge to somebody else's job order, JO, and they don't like you doing that. So. It's not enjoyable at all. I did get some, indirectly some funding to do corrosion work. Electrochemistry involving corrosion and some other work. But I was, and this was in 1997, April, a couple months after I returned from Italy. That's when I'd given the memo to report to the stockroom, and do stock, inventory of chemicals. And I was really desperate, trying to go to Italy. But the funding never came. And so next, I approached NHE and got a job offer to go there.
- Tom: New Hydrogen Energy, okay.
- Mel: So that came through. And it was quite a large cut in salary, but it was, at least I'd be working on cold fusion. So from October 1997 until early April 1998, I was in Japan working on their program. The NHE laboratory. And it was really a good group. They were mostly engineers. And again they were trying to develop a product more than basic research.
- Tom: Right.
- Mel: Right.
- Tom: So I want to come back to NHE, but let's cover some ground. When you went to NHE, did you end your affiliation with China Lake? Or did you come back to China Lake afterwards? I'm trying to just wrap up on China Lake.
- Mel: Okay. Yeah, I got a leave of absence from China Lake to go to NHE. So you know, you're on a leave basis, but you're still an employee of China Lake. You're just not getting paid by China Lake.

- Tom: Right. And so when you came back, your leave of absence ended. Did you continue then at China Lake?
- Mel: Then I continued at China Lake. I got back early April. I didn't have any cold fusion funding, of course. So, I went to the ICCF Seven meeting in Vancouver. But I just took a leave of absence. I mean I, you just take annual leave. And I just used annual leave and my own money to go to any conference after that. So I just paid my own way. And used annual leave. And, no, I continued work on different things. Mainly corrosion. They had a polymer group, they were developing polymers to protect against corrosion. Corrosion is a big issue with the Navy. The corrosion of ships and so on. And we were investigating polymer coatings that would retard corrosion. That and other areas of navy.
- Mel: And so I used a special electrochemical technique that was really gaining favor at that time. And I already had it in my laboratory. It was called EIS, or electrochemical impedance spectroscopy. In a quick experiment, you can get results that are quite predictive of how well something will stand up to corrosion over years of time. And it was a new way of doing corrosion research, but I already had been using this for other electrochemistry, so I knew how to use it. And so I spent most of my, the rest of my time at China Lake was spent on corrosion research and also on super capacity research. Super capacitor was another area that they were interested in in the Navy. That's where you store a lot of charge. And it's still an active area now. It's a way to store energy, electric energy and then release it. Like in automobiles, you can store the energy in a super capacitor and then when you need to go uphill and so on, you don't need to just use the battery. Super capacitors can contribute to the energy, extra energy needed.
- Mel: And so I would say mainly corrosion research and research on materials for super capacitors. For example, like ruthenium oxide, which I'd worked with before. Ruthenium if you run as an anode in water, it will form an RuO<sub>2</sub> coating. Ruthenium oxide, which stores charge very well apparently. And so it's a super capacitor material.
- Mel: So just by doing cyclic voltammetry, which one of my main electrochemical techniques, you can judge how effective different things are for storing charge, by just seeing the capacitances present.
- Tom: Good. So, were you able to do any cold fusion work at all during this time after you got back from NHE?
- Mel: No. I never touched anything related to cold fusion.
- Tom: Okay.
- Mel: I mean, I would have got in trouble if I had.
- Tom: Yeah, okay.
- Mel: Even to write a paper on my Japan work. I did write a paper and it was published in the Journal of Electroanalytical Chemistry in the year 2000. I just did that on my own time. You know, wrote it up. In fact, a lot of the work, I have a cabin in Oregon. And Linda was busy working. I just took off after Christmas and spent a week or two at the cabin in Oregon and wrote most of that paper up there. And I had to go through all the data, like

we had in that envelope. And analyze it. A lot of time spent analyzing the data and then you have to do that, and then you write it up.

Tom: Okay. So how long were you in this position doing these two different kinds of research at the Naval Air Weapons Station? And what came, when did that end and what happened next?

Mel: Yeah my, when cold fusion ended, my main source of funding had always been internal funding. It's called IR, internal research and the office of Naval Research. I heard that Bob Novak thought I, I didn't find an answer to the cold fusion, and therefore I didn't do my job and he didn't want to fund me anymore. Of course, nobody today has even found 100% reproducible experiment. What he wanted me to do was still not even accomplished yet today by anybody.

Tom: So you were...

Mel: So he wouldn't fund me. And new people took over China Lake. Younger people. They liked to push the older people out. That's typical of what happens when you become older and the supervisors become younger. And so, I wasn't given any of the internal funding that I had for years and years there. So those two sources were gone. So I spent a lot of time trying to get new funding. But it was very difficult to get. In thermo battery research, I tried to bring back some of that funding. And electro chromic materials, corrosion research, and super capacitor research. Those were the main areas I worked in, in that period.

Tom: So, but it was like pushing a boulder up a hill, I understand.

Mel: Right.

Tom: And so, you made a decision at some point to do something else. Tell us about that.

Mel: Well, the background to that is that Fleischmann had always, several times talked to me about a cold fusion weapon. He wanted me to try to get funding to look into that. He even gave me some ideas and I wrote a proposal with a friend of mine at China Lake. And we actually, in 1999, or around 2000, we put in an internal research proposal for study of cold fusion weapon. And I wrote it up and Martin helped write it up. I mean he gave me some ideas. We actually got funding for it. We spent time leading up to it. So I did work on that aspect of cold fusion right towards the end.

Mel: But then, when we're almost ready to go to a remote place at China Lake and do actual testing, the funding was pulled back. And I don't know. I thought it was funny. I mean strange. That I wonder if somebody, sometimes you're doing research and interfering with some black program. They'll pull the funding from you. And Frank Ornitz told me that's happened to him. If it's a black program and they're already doing work in that area, they'll tell you to stop or they'll stop the funding. And so that could have been a reason. I don't know. But I was quite disappointed when they did that. That was in the summer of 2001. Within a month after that, I got a call from the head of chemistry at Middle Tennessee State University that their teacher of physical chemistry had suddenly taken another job and left. And wanted to know if I'd come on a one year appointment. And so I of course said yes.

- Mel: And so I got another leave of absence from China Lake. And if you don't have funding, they're glad to give you a leave of absence. And Linda and I moved to Tennessee. We didn't really move anything. We put what we could in the car and rented an apartment and then bought everything we actually needed when we got there.
- Tom: Because this is where you had been for nine and a half years previously before you went to China Lake.
- Mel: Right. Before I worked at China Lake, I worked there from 1969 to 1978 and they had my information. And when this one teacher suddenly left, then they called me and I went back there and taught for one year.
- Tom: Okay.
- Mel: And then I came back. I came back and worked at China Lake a little bit during the Christmas break. And then I retired at the end of January in, that'd make it 2002.
- Tom: Okay.
- Mel: Because I was at, just then started teaching at MTSU in September when 9/11 2001 happened. And for a day or two all the students wanted to do was talk about the attack on New York. So I remember that quite vividly. That was the day I was, in fact I watched it on TV as I was getting ready to go to work. I heard that one plane had crashed into the tower, and I turned on the TV. And before long the second plane on TV crashed in.
- Tom: That was the fall that you were teaching?
- Mel: Yeah, that was when I just started teaching in Tennessee. So I taught one year there and I went back to China Lake and actually retired after working a couple of weeks during Christmas break. Retired at the end of January in 2002. Then they hired me to work during the summer break in 2002, so I went there for about eight weeks in the summer and did some research that actually wound up in an interesting paper. A paper that was on the use of hydroxide as a molten salt. LiOH and KOH and molten salt electrochemistry. I thought it was very interesting. I actually went back, by then I had gotten an offer to teach at Bates College in Maine for the next year. So soon as I got back to Bates in Maine, I wrote that up as a paper. It got published.
- Tom: When you were at Middle Tennessee State for that one academic year, were you able to do any cold fusion work during that time?
- Mel: No. Teaching loads are pretty heavy. But Fleischmann had, sometime before that had asked me if I could measure how the boiling point of D2O increases as the concentration of LiOD increases. Because when you boil off the D2O, the LiOD concentration is increasing. So the boiling point changes. And he did that in his analysis. The boiling point of the solution itself as it gets more and more concentrated. So I thought that was an interesting thing. I even thought about that before I left to go to MTSU, so I took the equipment with me to do that experiment. You know the thermistors and so on. The D2O to do it with. And I had that done as a physical chemistry laboratory experiment. We took about two weeks working on it. We determined how the concentration of LiOD affects the boiling point of the electrolyte solution. And this was presented at, I think ICCF nine in China. This is in that proceedings. And I put all the students' names on the paper.

- Tom: Good. So you were able to give Fleischmann what he needed then. What he was asking for.
- Mel: Right, yeah.
- Tom: Good.
- Mel: Right.
- Tom: So that was something you got accomplished then. Then you came back and spent eight weeks in the summer. And then that following fall, I forget what year we're in now, you went to Bates College in Maine.
- Mel: Yeah, it was 2002-2003 school year.
- Tom: Okay.
- Mel: I was in, at Bates College in Maine as a, I'm trying to think of the city's name, but it's inland from Portland, Maine. Lewiston, Maine. That's the name of it. Lewiston. There's a river that goes through and one side is called Lewiston and the other side is called, I think Auburn. Auburn, Maine and Lewiston, Maine. They are really just one city but they are separate by a river and have different governments.
- Tom: Okay, so you probably had a heavy teaching load at that time. Did you have, well, question: did you have a heavy teaching load? And were you able to do any cold fusion work?
- Mel: Yeah, unless you get a major university, it's always heavy. You know like, you teach three classes and that takes up most of your time.
- Tom: A lot of prep and then a lot of grading?
- Mel: Yeah.
- Tom: Yeah. A lot of counseling with students, student time.
- Mel: But I did give a seminar there. Both at MTSU and at, in Maine, I gave a seminar on cold fusion. And they were pretty well received, except there was one person at Middle Tennessee State University, I can't remember his name, but he had been at Texas Tech University when Bockris was having all that trouble. And he was very much against cold fusion. Even though he didn't bring it up during the presentation, I heard a lot of talk that he was attacking my, going around telling other people it was all wrong. You know, or whatever.
- Mel: But Lewiston, I never had any feedback like that. In fact, I was asked to teach an intersection class related to cold fusion, which I did. And we also brought in electrochemistry and did some laboratory work on electrochemistry. I actually developed experiments for the college there for electrochemical experiments. And those were pretty good experiments. I later heard that one student got a job in some battery company there because he knew about electrochemistry.
- Tom: Good. Okay, so you were there for just one academic year.
- Mel: Just one year, right. They were both one year appointments.

Tom: Okay. Yep. And what then?

Mel: Then that summer I looked for other jobs and talked to Iraj Parchamazad, who I'd met previously when he came to China Lake, trying to get people to collaborate with him on a Fuelsa program. So I called him and they had an opening, and so then I wound up that next year going to University of La Verne in the L.A. area. And I was there for three years. Starting 2003, 2004, 2005. And I left in 2006.

Tom: Okay, so you had summer before you went to La Verne and then you had summers during that period. Were you able to participate in any cold fusion work during your time at La Verne.

Mel: I got Iraj interested in it. Parchamazad, and he was an expert in zeolites. So he set up a zeolite experiment where you would deposit, get palladium deposits inside zeolite cavities. And then you would feed D2 gas into it and measure the temperature. And it got a spike upward in temperature with the D2. When they did it with H2, it just stayed normal temperature. And so it got him into the zeolite as a possible way of doing cold fusion experiments.

Tom: So there were small particles. It was obviously a palladium powder that somehow you got into the zeolite cage of the mineral phase of the zeolite. Is that correct?

Mel: Yeah, I was, it was done in way that he put the zeolite in organic solvent and he used an organometallic palladium compound, and it was dissolved in the solvent. So anywhere there're cavities, the solvent would flow in and so would the palladium complex. And then he would, so all the pores got filled with electrolyte and the palladium complex. And then he would evaporate off the solvent and then heat it up. And that would decompose everything except the, in the palladium metal, inside the cavity.

Tom: Interesting.

Mel: Right.

Tom: Very small diameter particles in those cages. Zeolite cages.

Mel: Right.

Tom: What a few nanometers?

Mel: I think it might be ten angstroms wide. You can get zeolite pores of different diameters, different cages. And you can tailor, make them to only fit certain molecules if you want. Like organic chemists, I already knew about zeolites because organic chemists at China Lake used them to remove water from organic solvents. You would put zeolite in an organic solvent and any water would get into the pores and get stuck there. And then you'd have the rest of the solvent free of H2O. And so that was already known to me as a way to get rid of water in an organic solvent area, substance.

Tom: So you were collaborating with your friend, the professor at La Verne. And he was doing this work. So, what came of that collaboration?

Mel: He actually went to ICCF 14 in Washington, D.C. and gave a presentation on it. He never wanted to publish it because he thought it should be patented first. So he didn't want to

publish it. But you can find his view graphs on the internet. I think Steve Krivit has them, or used to have them at least on his sites.

Tom: Well was it in the proceedings of the conference?

Mel: No, just the abstract. The only thing he reported was the abstract. But I remember that meeting, somebody started talking about transmutation, and he got really excited. He sat next to me and he said, "You know, I wonder how I got these other metals mixed into this material." You know, because you shouldn't get any other metals into the palladium, when he examined the, the zeolite material. But he was finding some things like nickel, and like zinc, and other things showing up. And he said, "How did they get there?" And that was the only explanation that he had. It must have been transmutation. Because he had instruments that do careful analysis of what was actually in the zeolite, and he had these other metals, transition metals, showing up in the analysis that were never in any starting material. I wish he'd published more. But he's about ready to retire now, and he did a lot of work that's probably never going to be published. I think he even has work on nickel in zeolite, and he has some interesting results he's told me about, but he hasn't published them.

Tom: So he's still on the faculty ready for retirement at La Verne University then?

Mel: Right.

Tom: Interesting. Okay. Okay. So you were at La Verne. You worked with him on the zeolite. What happened then?

Mel: Then somebody from China Lake came down and he had an interest in thermo batteries. He wanted to know if I would come and work at China Lake as a contractor on thermo battery research. And I said, "Are you sure?" Linda was tired of moving around and living in apartments. We had a house in Ridgecrest that we'd rented out and we'd just moved back to it, an actual house rather than an apartment. We had, in La Verne we had an upstairs apartment. You had to go up stairs to get to it. Not a big apartment. I think, two bedroom. And I guess, Linda was tired of moving around. She didn't like the traffic in that area. But it was really nice weather in the winter. Almost perfect weather. Except for smog. We lived in the part that didn't get much smog. La Verne is right against the mountains and most of the smog misses that area. So it was, a lot of ways it was ideal except too many people, like St. George was getting.

Tom: Okay. Well, I'm going to, we're up at 42 minutes now, so I think it's time to take a break and then we'll pick it up on what you did after La Verne, okay?

Mel: Okay. Okay.

Tom: So this is Tom Grimshaw with.

Tom: So Tom Grimshaw with Mel Miles signing off till the next.

## Interview 2-8

Tom Gimshaw: Here with Mel Miles on July 17th. We're here at Mel's home. I think the temperature has gone to 103 now. We're in his backyard overlooking the beautiful Virgin River. Mel, I think in our last session, number six, we ended that with-

Mel Miles : It was number seven.

Tom Gimshaw: Oh, it was? You're right. It was number seven. So now we're in session number eight. Thank you. So we ended that session with the completion of your three or so years at La Verne University in the Los Angeles area. And I think you were about to launch into the next phase of your career.

Mel Miles : Okay. They're trying to make contact. I knew before. He wasn't in my chemistry area, but he was into thermal batteries. He wanted to know if I would come up to China Lake and to work as a contractor. And I said I'd be interested in it. He tried to find a place where I could work. And when you went to chemistry my main nemus there, [Robyn Nissan 00:01:14] would not allow me to come back and work in chemistry. I don't know why, but he wouldn't allow it. And so he added another laboratory, kind of away from everything. And he said, "There's a room there." He said he could set up as a laboratory. So he did that and he made it into a laboratory and I was the only one using it for the next couple of years.

Tom Gimshaw: Remind me, which year did that start, after you finished La Verne?

Mel Miles : Around 2006 to 2007.

Tom Gimshaw: Okay.

Mel Miles : And he got me involved with a group called MEI, which Millennium Engineering and Integration and a member of that group was a person that I'd known from China Lake. And he'd been able yo get another job in Washington DC and he'd heard me give a seminar there, at one time, on thermal batteries, and he thought it was very interesting. So he got MEI to get into the thermal battery area and they finally wound up being the source of funding for the contractor. It was this MEI group. And so I was actually an employee of MEI but worked through a contractor at China Lake into this laboratory, and they bought the equipment I needed. They bought me an electrochemical instrument like I had at China Lake, even a newer version that could do any type of electrochemistry I wanted and vacuum ovens, everything you would need in the laboratory.

Mel Miles : It was a brand new laboratory. It was very nice. And I thought there'd be other people working there, but I wound up being the only one that used it after the first couple of years. But when it got funding from MEI, I had one of the students from La Verne came up and worked for the summer on the program and so I had a little bit of help with that program.

- Mel Miles : And so that lasted about three years and we then did what they call SBI, this was a small business company, Small Business Innovative Research, SBIR, and so they went to the Army and got an SBIR contract that funded us for about another two year period. It was Army funding. They came through the Navy.
- Mel Miles : I went to this company first and then came to the Navy and paid my contractors so that program ended after about two, three years because another person I had, that was hired by MEI, he had taken another job and he was the one that really knew ... He was a battery engineer and knew how to build batteries and so he didn't have any expertise. The job was going to take my research and start building batteries. But they had trouble doing so the program ended. That was work that was done more back in Washington DC area. But the two people had never worked on batteries before and they ran into a lot of the trouble trying to get everything to work.
- Tom Gimshaw: So you were at China Lake during the first years of this program, and then when it got extended under the Army funding, then changes took place and they lost their battery expert then and so they had to discontinue then, I guess.
- Mel Miles : We finished the SBR, we got that and I even went back and gave a presentation to the Army. I think they were quite happy with it. It was hard to get a battery company to pick it up. You needed an actual battery company to go forward with it, which they didn't get that quite accomplished. I think it would still work. You just need people, expertise in building batteries to follow-up on the research.
- Tom Gimshaw: Okay.
- Mel Miles : But by then, [Mike Munich 00:05:44] had came by and he had a contact at a Navy program, probably the one where he took funding from Frank Gordon. I don't know.
- Tom Gimshaw: Okay. Well, let me just close real quick. During this time, you were working on the battery work at China Lake and then under the Army SBIR. Were you able to work in any cold fusion work during that period?
- Mel Miles : I think on the side, yes, I did some because I had everything there and so I tried to doing more codeposition studies. I think as during that period is when I did codeposition of other transition metals like ruthenium and other metals. I found nickel and so on. And basically they did produce excess heat, only the codeposition only worked with palladium and I think this is when Mike Munich came in. He got funding from the Navy person and he wanted to reproduce the codeposition work I did in Japan. In Japan, codeposition did work in a Fleischmann Pons cell, the bigger cell where you get a larger deposit of palladium. All three experiments gave excess heat and I revised the electrolytes somewhat, so it wasn't just lifting chloride, it was ammonia solution. Ammonium chloride, ammonium hydroxide, deuterated.
- Mel Miles : I got some large effects which has been published several places. When [Stan Tok 00:07:35] found out about it, right away he wanted to publish it and even published Fleischmann and Pons' analysis. I think I told you before, Fleischmann

and Pons had a chance to publish and in a way he wanted to write it. So there were two papers that came out in journal publications, Journal of Fusion Technology and Electro Analytica Octa. They both report on this codeposition I did in a NAG.

- Mel Miles : But anyway, Mike Munich wanted to reproduce that experiment and also have it reproduced at NRL and this is the office was the Defense Threat Reduction Agency, DTRA, and I think somebody by the last name of Wilson was in charge of that, if you've heard of him. So I was asked to write a proposal. It was kind of nice. Your sponsor asked you to write the proposal and of course, you get funded that way. That doesn't happen very often, but it happened then.
- Tom Gimshaw: So you worked at, at China Lake in this dedicated laboratory that was set aside for you. Was this work continuing at that location?
- Mel Miles : Yeah, at one point, I supposed to be doing both, by funded for both battery research and cold fusion research. And that was kind of hard to do both. And I know there's a period that I was actually trying to both, cover both, battery and cold fusion research.
- Tom Gimshaw: And the cold fusion research was the attempt to reproduce what you had done at NHE years earlier.
- Mel Miles : Right, yeah.
- Tom Gimshaw: Okay.
- Mel Miles : And also to study other depositions of other metals. I think that was part of it.
- Tom Gimshaw: Okay.
- Mel Miles : And it led to several publications, at least in Journal of Condensed Matter, Nuclear Science and some of my publications during that time period resist. Tom, I had my own little lab set up the China Lake, but a couple of years later, I probably worked from 2006 to about 2011, maybe five or six years there. But then the DTRA funding ended and in the MEI funding ended. And I didn't have any funding, but the one in charge there, he was a supporter of cold fusion. So he allowed me to keep working even though I wasn't funded, which is kind of breaking the rules. Without funding, you're not supposed to be working. But I probably continued to work on through about 2015 or so.
- Tom Gimshaw: Another four years.
- Mel Miles : Right. Yeah.
- Tom Gimshaw: Amazing. And what was the nature of your cold fusion research during that period?
- Mel Miles : I didn't do it full time, but as I recall it is mostly codeposition I was doing. Oh, I developed the new [Couder 00:10:44] Imager that I'd use. I adopted the new ... that I have right now, and I tested the new Couder imagery and I think a lot was like that for that time. But I don't know whether I got any large effect but I saw a

lot of small effects and I found a lot of deposition of other metals does not work very well.

Tom Gimshaw: Yeah.

Mel Miles : One thing I did right at the end that I'd never had a chance to go back and repeat, Peter Hagelstein had a theory that Stan Spock and Pam Foss, early on, good codeposition at a larger current, you deposit a large current and the deposit is not as good. It may fall off but you produce a lot more defects that way and he thought doing that it might get bigger effects. I just started on that and I think that was true. I even did one study with nickel doing a fast deposit and I turned the CXE XSD with nickel with a deposit where you get a lot of vacancies.

Tom Gimshaw: Electrochemically?

Mel Miles : Yeah. Only got to do one experiment on that but looked like maybe Peter was right. If you deposit a higher current, you get more defects, you get a bigger result, bigger excess heat effect but never got to follow-up on that.

Tom Gimshaw: Okay.

Mel Miles : Yeah.

Tom Gimshaw: All right-

Mel Miles : I guess nothing much happened after that until I decided to set up experiments in my Ridgecrest home. Because we had a home in Bakersfield and Linda was there most of the time so I had the kitchen to myself when I was over there and I was still getting funding coming in from the private sponsor and so I wanted to do something. So I ran another Johnson Matthew wire that I had. I still have some of that wire that gave the second set of [inaudible 00:12:52] for resolves.

Tom Gimshaw: This was, excuse me, this was after the lab?

Mel Miles : Yeah.

Tom Gimshaw: [crosstalk 00:12:59].

Mel Miles : I had to move everything out.

Tom Gimshaw: Okay.

Mel Miles : But besides the equipment they bought, which I had to leave there, I had my own equipment, which actually was better in a lot of ways. It was analog and so you turn it on and you'll run day after day. The computer controlled, newer instrument, you have to program it and they don't allow you to go long time periods and then it shuts itself down. So the analog equipment, I still have in there, it turns out better for these type of work. You can set it and it continues and continues day after day. You don't have to keep adjusting computers.

Tom Gimshaw: So the time almost, well a five or more years that you were working in the laboratory at China Lake, your home was in Riverside, which I guess is what within commuting distance or-

Mel Miles : Ridgecrest.

Tom Gimshaw: I'm sorry Ridgecrest.

Mel Miles : I applied when I was-

Tom Gimshaw: Oh, okay. Okay. And so when the lab closed, you opened up your own lab in-

Mel Miles : The Navy has a fence around the area. That's called China Lake. And next door to it is Ridgecrest.

Tom Gimshaw: Oh, okay.

Mel Miles : In fact they're side by side.

Tom Gimshaw: Oh, okay.

Mel Miles : Yeah.

Tom Gimshaw: So that laboratory closed. You had done a little bit of cold fusion work at that laboratory, mostly codeposition using different metals and different alloys, I guess. And then when the laboratory closed, you start doing work in your home and in the kitchen in Ridgecrest.

Mel Miles : Yeah.

Tom Gimshaw: Tell us about. What was the nature of that work?

Mel Miles : The first study was the Johnson Matthew palladium wire that produced excess heat again and that was reported. I forget what I accomplished before that. But I was still going composition, I'd like to have something new to report.

Mel Miles : And then at ICCF20 in China, Dave Nagel and Steve Kaczynski talked with me. They wanted to know what I would recommend for research in the cold fusion area. They were thinking of codeposition but I told them, "Well, codeposition had a lot of problems." One thing it generates chlorine. You can't go to the good in a home. You need to have a laboratory to do it.

Tom Gimshaw: With a hood.

Mel Miles : Yeah and a hood. And I said really to me the biggest advantage of cold fusion not followed-up on his palladium boron and I said, "This is a Navy material that gave excess heat nearly every time." And I said, "That's where we should focus." And they agreed with that and that that program is part of that program. That's what Dave Nagel and Steve Kaczynski are doing right now.

Tom Gimshaw: Currently underway.

Mel Miles : They're trying to have a material made in by another company was Dr. [Umam's 00:34:07] help, did they do it right and then they'll be able to developing new calorimetry. He'll test it at George Washington University and I'll help them as much as I can and especially by email, which I spend a lot of time even now with emails back and forth.

Tom Gimshaw: Good. So the cold fusion in the kitchen work, how long did that go on in your home lab?

Mel Miles : Well, I just did one experiment in, I guess it was probably about 2017 I think. And I reported that in I think 2018. Oh, I think I reported at ICCF20 in Japan and China, both.

Tom Gimshaw: Okay.

Mel Miles : And then last year before I moved, I ran a palladium boron in my kitchen. Again, it gave us early excess heat effect, which I'm not quite sure how much of that is correct, but it still gave excess heat but not as big as it did in Japan, the same palladium rod. We had a problem. One problem with it is there sometimes the detail you start having more bubbles than normal. And this had a lot of bubbles forming on the cathode. They blocked the surface so when then start blocking the surface the voltage goes up and when they disengage the voltage goes down so it's hard to get a good voltage reading. That causes a kind of big error source. So that was probably what I'll try to do next is repeat that.

Tom Gimshaw: So you did that in your kitchen. Was that work that, I guess that was the last work you did-

Mel Miles : Right.

Tom Gimshaw: In your home lab. Was that kind of spurred on or should I say as a result of your conversations with Nagel and Kaczynski or was that before?

Mel Miles : Yeah. I told them I didn't even know where that palladium boron was, if I even had any, and Steve Kaczynski lived in LA area, so he came up and we spent a day going through my garage looking for the materials that I had and they're sitting in the garage in a little vial, all by itself. It was labeled palladium TD 0.5 boron. And then NHE was on it, I think, too and it was the palladium rod I ran in Japan.

Tom Gimshaw: Okay.

Mel Miles : That was around 2016. He found it. I think I ran it in 2017. The Johnson Matthew was probably the 2016 experiment. I think I did one in 2016 and another in 2017. I haven't done any since I got here.

Tom Gimshaw: Okay. All right. So the main thing you're doing now is just collaboration with Nagel and Kaczynski on the work at George Washington University, the palladium boron work.

Mel Miles : Yeah.

Tom Gimshaw: Okay.

Mel Miles : And I hope I get a chance to work with Google, with some of their people. At Least giving them some feedback, give them the palladium rod to test, another one I've never tested before and how to set up experiments, what current density to use, and if they're following a little bit of that as a guide, they, I think, it they would probably see the excess heat.

Tom Gimshaw: Okay. Okay. Good. All right, so right now you're writing up a lot of things. I know you're writing three abstracts for ICCF22. You've been trying to give a papers published in Nature and other mainstream journals. What other things have you

got going on in the current situation here? Well, first of all, when did you move here?

Mel Miles : End of November, 2017.

Tom Gimshaw: Okay.

Mel Miles : So I've been here about a year and a half.

Tom Gimshaw: Okay. So you haven't done any lab work here? You haven't-

Mel Miles : I haven't even found all the things I need to do an experiment. somewhere, probably in boxes in the garage.

Tom Gimshaw: Yeah. Okay. All right. I kind of listed a few things.

Mel Miles : The main thing missing is a palladium rod, the rod I used before. I found from some other ones right off. I can't find that one I use at NAG. I remember I said this is special. I need to do something different with it. I didn't put it in with the other materials. I just should've. Now, I don't remember what I did with it.

Tom Gimshaw: Yeah. I've run into that myself, a few times. Put it in a special place. So what else are you doing in the current situation here in lovely St. George, Utah?

Mel Miles : Well, I'm pretty busy last year because I was asked to write two chapters for this book coming out, Elsevier book. That took an awful lot of time to write that up and get it written the way the I wanted it. And so those are what I worked on most of the last eight months or so, mainly that.

Tom Gimshaw: And I think, reminds us, what are the two topics? One of them is calorimetry.

Mel Miles : Yeah, the details of calorimetry that Dave Nagel says he wanted me to give him details. I said, "Oh, just write up your chapter on it."

Tom Gimshaw: Okay. And what's the other chapter?

Mel Miles : A new look at the heat healing results and where it worked out an equation because you theoretically, or at least quite you should expect to see and what you actually measure.

Tom Gimshaw: Good. Okay. All right. Plans for the future?

Mel Miles : Well, some people have told me I should write a book. I don't know if anybody's interested in reading a book unless cold fusion took off somehow.

Tom Gimshaw: Yeah.

Mel Miles : I mean it'd be a lot of time to write a book, but I could do it and-

Tom Gimshaw: Well, I'm hoping that what we gather together for this might serve as a resource for such a project.

Mel Miles : Right. But there's, I think, a lot of books written on cold fusion by other people and-

Tom Gimshaw: Yeah, but none by you and you would have your own special angle on it and perspective on it.

Mel Miles : I'd have the inside look at the Navy program, that's for sure. And an NRL would not like what I would say.

Tom Gimshaw: Yeah. Or look at certain things too, like a heat helium correlation. You can focus on that or the palladium boron-

Mel Miles : [crosstalk 00:23:22] Fleischmann and-

Tom Gimshaw: That.

Mel Miles : Ans so on.

Tom Gimshaw: There are a lot of topics that are very positive. The Navy program didn't work out, but you got a lot out of it, I think, at the time. So those are at least three areas. Martin Fleischmann, the palladium boron, and the heat helium could all be mainstays of a book.

Mel Miles : An inside look at Navy research laboratories too.

Tom Gimshaw: If you want to go there.

Mel Miles : And there's a lot of politics that go on.

Tom Gimshaw: Yeah. I think the real value of a book is if cold fusion takes off or when it does, it's a documentation of the science that was going on during this 30 year period.

Mel Miles : Yeah, it's coming up near the top of what I wanted to do next.

Tom Gimshaw: Okay. Good.

Mel Miles : What I would actually like to do next is find the palladium boron that I lost. I hope it's here somewhere and then go over the period, over and over. Load it and de-loaded it, load and de-load. Look at what happens in that early period and try to get a better handle on how to analyze that kind of data. Does it show up? Does excess heat really show up soon in palladium boron, which I think it does. But it's hard to major accurately because when you're loading the [inaudible 00:24:55] into the palladium, you don't really know exactly what the thermal neutral potential is. You know what it is if all goes in. you know what it is if none goes in. You don't know the in between, hardly at all.

Tom Gimshaw: Right.

Mel Miles : Unless you had some accurate way to measure the amount of gas coming off.

Tom Gimshaw: Yep.

Mel Miles : Then you could get it because the amount going in is the amount that should be coming off minus ... I mean what should be coming off minus what you actually coming off. That would be the amount going in. You could determine that way. You know theoretically, what should come out by Faraday's Law and how much do you measure. Of course, oxygen is always coming off. It's an anode, so you're getting-

Tom Gimshaw: The other side of it.

Mel Miles : That's one third of the amount. Well, the amount of hydrogen is twice the amount of oxygen, the way the palladium works out. You get 2H<sub>2</sub> and two waters to one oxygen. it's a two to one ratio. So if all went in, you'd have about one third of the gas you'd expect normally.

Tom Gimshaw: I want to be sure I'm understand the cells. As you were getting ready for your leave of absence to go to NHE from NAWS, you told me you had three cells there and that you had borrowed the material for these cells from Martin Fleischmann. Is that correct? There were three cells. And you've got the palladium from Fleischmann?

Mel Miles : No, I just took with me electrodes I had that previously had worked, but I took a palladium boron that I'd never tested. I said, "This would be a chance to test it." I know from the dimensions and the composition, it was one I never tested.

Tom Gimshaw: So what was the source of the three-

Mel Miles : Oh they were already there at NHE.

Tom Gimshaw: Okay.

Mel Miles : In fact, Pons actually visited the laboratory, brought the cells to them, and he helped set up their exact equipment, their computer controlled equipment was just duplicated at NHE, the Fleischmann Pons equipment.

Tom Gimshaw: He came there. Is this leftover from the work in France that Toyota-

Mel Miles : No, it was during the same time period. It was about 1993 when they got this set up.

Tom Gimshaw: Okay. But it was independent of the other work in France then?

Mel Miles : Except Fleischmann and Pons helped set it up in NHE.

Tom Gimshaw: Okay.

Mel Miles : I think they had the calorimeters made and gave the to NHE. So that it was a Fleischmann Pons actual cells that they had made and gave the NHE was their actual computer program that they gave to NHE.

Tom Gimshaw: So where did they get those things that they gave to NHE? Was that leftover from their France work or did they do it-

Mel Miles : Oh, the France work was still going on.

Tom Gimshaw: Okay.

Mel Miles : This is both going on. The two programs are going on simultaneously and they both ended at the same time. They both ended March of 1998. Japan ended and the work in France ended exactly that date.

Tom Gimshaw: I'll be darn right. Okay.

Mel Miles : Because it was the same sponsor, I think basically.

Tom Gimshaw: Yeah. Yeah. The auto-

Mel Miles : Toyota was one of the big sponsors.

Tom Gimshaw: Yeah. Okay. That makes sense. All right, so NHE was outfitted and equipped by Fleischmann and Pons by whatever means, we don't know.

Mel Miles : Yeah. They even gave him a very detailed manual, over a hundred pages, on how to do the experiments that apparently they never used.

Tom Gimshaw: Yeah. You have a copy of it. You showed it to me.

Mel Miles : Yeah. When I left, I asked if I could keep that, and they told me yes. I asked if I could take the Fleischmann and Pons cells with me, and they said no.

Tom Gimshaw: So there were those three cells?

Mel Miles : Yeah. They thought about it. Dr.[Sawmi's 00:29:09] the one I asked and he just kind of poses about my taking the cells. He said, "No, I better keep him here." But about the manual, he said, "Yes, go ahead." They didn't use it anyway. Why not?

Tom Gimshaw: Yeah, why not? All right, so subsequent work you made, what was the source of the of the cells that you used subsequently? You said that they were from Martin Fleischmann? I got a little confused on the cells.

Mel Miles : The cells?

Tom Gimshaw: That you were using-

Mel Miles : At NHE?

Tom Gimshaw: Well, after NHE. The cell stayed at NHE and you had other cells that you'd mentioned getting, I think, from Martin Fleischmann but [crosstalk 00:29:49].

Mel Miles : Well, I came back in 1998 and I was still working for the Navy so I couldn't do any cold fusion research there so I didn't have any ... I just had my old cells but I didn't do anything with them though. They were just sitting on a shelf somewhere.

Tom Gimshaw: Okay. Old cells from your work at-

Mel Miles : Yeah, at China Lake.

Tom Gimshaw: At China Lake. Okay.

Mel Miles : I was at University of La Verne when I started thinking about making a new cell and I kind of diagrammed how to do it and I did a lot of calculations to get a cell constant that you'd like to get. How much insulation do you use? A lot of thought goes into how to make a cell, a good cell and then I thought how to do it with the using copper tubes.

Mel Miles : It was sort of a scale up of what the [postdoc 00:30:42] had done earlier on ... He used a small cell, about like this. I just came up with a bigger, basically. I'd worked with him on his cell and it's the same time the inner copper and outer copper and they don't contact each other. They're separated by installation all around and at the bottom. So they're totally out of contact. And so now heat gets transferred, but copper's a very good conductor.

Mel Miles : And then all you need is a heat transfer fluid between your glass cell and the inner copper wall. And then that gets transferred to the installation to the bath.

Tom Gimshaw: Okay. Was the graduate student, was that Stillwell or was that Kendall?

Mel Miles : Kendall. That was Kendall. That was very at the very end. He worked China Lake in 1994 and 1995 and he spent almost the whole year building the cell.

Tom Gimshaw: Right. So that was the cell idea, basically, that you built upon.

Mel Miles : And that cell stayed there. I should've taken a with me when I left, I guess, but I never did. But it was only used in a couple of experiments. He excess heat in one. He saw exactly where it turned on and then it turned off. It's in my final report at China Lake.

Mel Miles : It shows he moved the current up and the excess heat kind of kicked in and then he changed something else and it fell right back down again. You could have done a lot of interesting experiments with that sensitive cell, about storage and why it happens and a lot of things related to it.

Mel Miles : But I just made it a bigger version of it and it works quite well.

Tom Gimshaw: You have that here now?

Mel Miles : Yeah.

Tom Gimshaw: It's ready to go again when you-

Mel Miles : Yeah. The only problem is you can't see into the cell. That's true of almost every cell, except Fleischmann Pons Duracell, where you could actually see in and so the electrolyte level will change the electrolysis if you could just track where it should be. You can compensate theoretically, but if that level gets off, that's the main reason the calorimetry gets off. That level gets out of control and you're incorrect from what you think it is.

Tom Gimshaw: Yeah. It changes the cell constant.

Mel Miles : It changes the the cell constant.

Tom Gimshaw: Yeah. Okay.

Mel Miles : That's the major problem with that cell.

Tom Gimshaw: Yeah.

Mel Miles : Then Nagel was trying to get around by making a cell out of clear plastic that you can see through, but plastic is a lot better conductor than installation. And the way he built it, he comes out with a cell constant about twice as large as mine. Instead of going 0.14 he's more like 0.28. I knew that when he started. I said, "You're going to have to go build that thickness pretty large to get down to where my constant is." But he didn't want to make it that big. You need quite a thick piece of plastic to block the heat that much.

Tom Gimshaw: Yep.

Mel Miles : Insulation blocks it pretty readily.

Tom Gimshaw:

Yeah. Okay. So we're going to do one more small session. So we'll wrap this one up because we're at 30 minutes. And so this is Tom Grimshaw with Mel Miles. It's a July 19th, I believe. This is session number eight and we're going to have a quick final session here.

**Interview 2-9**

- Tom Grimshaw: Tom Grimshaw here with Mel Miles on July 19 at his home in St. George. Mel we've had I think eight sessions to this interview. This will be number nine and the final one and the question I'd like to ask you to address in this session explanation... tell us what do you think is the current thinking what is the most educational [inaudible 00:00:23]?
- Mel Miles: Everybody has their own ideas on this.
- Tom Grimshaw: I want your idea.
- Mel Miles: I think you have little reasons, special reasons for a lot of electrons to accumulate. And that's what is necessary for this to take place. And with enough electrons I think you defeat the Coulombic barrier. And so two neutrons can get together and fuse and form Helium core. And I'm not sure where this is, I would rule out Ed Storm's cracks. I would maybe be more in favor of Peter Hagelstein's vacancies. I think the double A region is also a possibility. In fact, Heinz Gerischer when he went to the meeting in Italy, ICCF 2, he wrote up something about it and he thought that the electrochemical double areas were very strong electric fields that might be part of where it takes place.
- Mel Miles: So, I just think it's electron rich area so that this electron... very electron rich area that somehow is created in a vacancy or in a crack or near the double error. And I think you get electron concentrations so high that of course positive deuterons are attracted there and they get... the charge probably gets neutralized by the electrons. So many electrons there that the Coulombic barrier doesn't exist. And this is shown by Ed Storm's experiments that the activation energy seems to be related to the fusion of deuterons in palladium and my application of our eyring rate theory leads to that same conclusion that the activation energy is very close to what you'd expect for the major value of deuteron diffusion in palladium. So it's diffusion in... deuterons are diffusing into a special area.
- Mel Miles: Probably a lot of these little areas and the rate is determined by how fast they can diffuse in. Maybe how fast... how the Helium can get out. It can end because Helium gets built up and too high. And it might cause it to end but of course the co deposition seems to show that these little hot spots tend to move around a lot. So I don't think it just stays in one spot. I think it varies, changes, is undergoing changing all the time in the experiment.
- Tom Grimshaw: Self destruction of the NAE, in other words the deuterons get together in the electron-rich area where the Coulomb barrier is negated or is neutralized and then the fusion takes place, then a lot of energy's given off so maybe the NAE is destroyed as soon as it's formed.
- Mel Miles: Yeah. Yeah, the energy given off... you know, that's a big question, how they dissipate all this energy. I don't really know. I think if I remember right, Hagelstein

thinks it's dissipated by photons... phonons, not photons, but phonons, the elementary particle of sound, vibration. And that could be correct, but somehow it gets dissipated to neighboring Palladium and gets spread out into the Palladium, I think.

Tom Grimshaw: Yeah, into the lattice as heat so the Palladium atoms vibrate and so... more... and so the heat is dissipated back into the Palladium lattice. And as far as the rate being controlled by diffusion, I think Ed Storms would certainly agree with you on that. Nearly everyone has found that the rate depends upon the temperature. The higher the temperature the greater the rate and Ed would say it's because of the greater mobility of the deuterons that the higher temperature allows them to diffuse in quicker.

Mel Miles: Right. Yes, that's correct. Diffusion is known to be temperature dependent. It gets faster at higher temperatures and that's how you made your activation energy. You need at least measurements of two different temperatures and if you even got... have even more, you can do a plot of the plot versus one over temperature and get the activation energy that way. The rate of diffusion plotted versus one over T, and the slope will be related to the activation energy divided by gas constant R. So you get it that way as well. That would be the slope of the line. EA... activation energy divided by gas constant R.

Tom Grimshaw: Well if you could achieve, if you could neutralize this Coulomb barrier and achieve fusion through electron clouds, very highly concentrated electron clouds with a huge negative energy localized, then that would eliminate one of the major issues that Ed Storms has with his nanocrack and hydroton hypothesis and that is that he has to have the deuterons approaching each other and a little bit in a vibrating situation where the deuterons are suspended by the negative walls of the crack, suspended in these linear structures he calls hydrotons, and then the hydroton vibrates and then as they approach each other in this negative environment then they don't fuse but they approach each other and some of the matter of the deuteron is given off. In his case as photons that then go out into the lattice and eat the lattice.

Mel Miles: Okay.

Tom Grimshaw: But it's an incomplete fusion, and then it goes to the next cycle and two other ones approach each other and a part of the mass is converted to energy into photons that go, and so through enough cycles of the vibration of this hydroton, the mass is finally consumed and the conversion by  $E=MC^2$  is complete, but not instantaneously but over a period of time under his theory. And I think he's running into some major resistance to this idea of partial conversion from atom to energy in that manner.

Mel Miles: Yeah, I don't know who's right, I'll put it that way.

Tom Grimshaw: Okay, all right.

Mel Miles: I... Peter actually is very good as a theoretician and he has... something seemed to verify his theories.

Tom Grimshaw: Superabundant vacancies.

Mel Miles: Yeah, right. So.

Tom Grimshaw: Well if you're right and it takes these pockets that constitute the NAE of extremely high negative charge to overcome the Coulomb barrier, you don't have the problem that Storms does, cause the fusion takes place and maybe it self-destructs the NEAE by the energy given off, but it certainly seems a lot simpler.

Mel Miles: Yeah, I think that there is something happening inside the palladium, I've always thought that. Otherwise, why does it seem to be related to volume? The more volume, the more heat you get generally. And it can't be surface because co deposition produces a very large surface. In fact, my experiment shows it may be a million times larger.  $10^6$  Larger than the surface of the substrate, but you don't see that multiplication of excess heat, still seems dependent on the volume or how much palladium you have there. So I know [inaudible 00:09:12]. Initially they claimed it was a volume effect they thought, and I don't think he ever changed his mind on that. He didn't know for sure I think he... it could be a surface effect but he didn't... electro-tendency seems to favor maybe it might be surface is the double layer I think, but.

Tom Grimshaw: Explain the double layer again real quick.

Mel Miles: When you put high charge on an electrolyte a cathode you're pushing extra electrons there. And so they tend to gather the surface electrode and the negative charge to attract with, positive ions from the solution and so they might not be having....on the electrode surface you have negative electrons and opposing that you have positive ions. And then it gradually less, and less positive once you go further away, that's called the double layer. This layer of opposite charges and it happens almost on any surface not just electric chemistry. In very dry conditions in the winter, you walk across the carpet and touch a door knob, you'd build up a surface charge. And it discharges, so it's quite common in everything that you tend to get a surface charge on things.

Tom Grimshaw: If you believe the NAE is the highly concentrated region of electrons, where would that region occur in the double layer set up.

Mel Miles: It could be right within the double layer even. You know the negative electrons on the surface and the deuterons are attracted opposite so there you have a region of very high concentration of electrons and charges opposed by deuterons. And so that alone might be in place, but then inside the metal you have these electrons on the surface and  $D^+$  is mobile in the palladium it may come from inside of the palladium near this negative charge on the opposite side. So it could be inside the palladium and that sense. And if you form a vacancy, as the cathode charge would build up on the surface of the vacancy and deuterons would be attracted to that negative charge. And that might be where the action takes place. Maybe it's not right in the vacancy, it might be the electrons are on the surface of the vacancy and deuterons attracted to them and they're still in the metal but not in the vacancy then it might take right on the surface of the vacancy. Even.

Tom Grimshaw: Yup. I get it. Yup. Okay. Well I have to ask you one last question. Let's say that you were twenty or thirty years younger, knowing what you know now. You know the old cliché if I could just go back and know what I know now. If you were twenty or thirty years younger and if abundant funding were to become available. What would you do? What kind of experiments would you run? How would you connect a hypothesis to the experimental results and so forth.

Mel Miles: Well I think part of the answer would be, how does a scientific community judge this area. You know it's very, very negative of you to say "Do I want to ruin my career with this?" You'd have to answer that first. Because I think a lot of people...I think it destroyed my Navy career. I think it really hurt Peter Hagedstein's career at MIT, and I think any younger people would have to think about that before getting in [inaudible 00:13:17] but if it was an accepted topic than of course there would be very interesting to.

Tom Grimshaw: Well what would you do? What would you're actions...

Mel Miles: If I were not worried about hurting my career and if you're older you don't care, that's why a lot of older people got into this. Where would I start today? I'd probably start right where I am right now. With Palladium [inaudible 00:13:44] and co deposition and that would be the two key areas I'd focus on.

Tom Grimshaw: Is there anything you can say what experiments you would run in terms of verifying an explanation. You know there are these competing hypotheses, is there any experimental work that could, you could suggest that would help to clarify the explanation?

Mel Miles: Not off hand I'd have to think about that.

Tom Grimshaw: Okay, I don't mean to put you on the spot, of course.

Mel Miles: It's very hard to design experiments, that would tell you what you want to find out unless it becomes quite reproducible. If it's not reproducible with it, its very hard to study variables. You know I think that's the problem we have right now. If you get a very good reproducible effect, then look how the variables effect it, and then maybe you can maybe answer that question.

Mel Miles: But right now I don't know how you do it.

Tom Grimshaw: Yea, I like to tell people when they ask about how is cold fusion. I say "well Coal fusion is doing great, we're only lacking two things. Reproducibility, and a decent or satisfactory explanation. And of course the two are related.

Mel Miles: Right yeah,

Tom Grimshaw: Okay

Mel Miles: It needs a theory, and a reproducible experiment.

Tom Grimshaw: Yea, it really in play, and I being some of a purist on some occasions. The term fury is of course in wide currency and can not be offset in this field or replaced. But really they should all be called hypotheses because theories don't happen until hypotheses are tested and verified under many different conditions, and by

different investigators. So we really don't have any theories, all we have are competing hypotheses in my Lexicon.

Mel Miles: It's very...I mean [inaudible 00:15:54] never functioned like they did with confusion in saying because of our theories it's not possible. That's not the way to conclude something. If the experiment is correct your three has to change....and explain that. You can't just throw out experiments because it doesn't fit a theory.

Tom Grimshaw: The absence of evidence is not evident of absence.

Mel Miles: Right, yeah.

Tom Grimshaw: It's one of my favorite cliché's. I think we'll wrap this up. This has been really good, any final comments. I think we're at sixteen minutes so we have a few more minutes. Any final comments you want to make about your cold fusion in general, the investigations that have been done, and your own career there in.

Mel Miles: Yeah I'm trying to think of anything we left out that I'd like to bring up. Do you have any questions or anything to focus on?

Tom Grimshaw: No, I think we've done a very thorough job. This combined with the earlier set of interviews will make a very nice narrative of your own career and as well as the cold fusion field in general. So thank you very much for the time that you've given. We do have the transcripts that which I'll show you again of the earlier set of interviews we did back in May.

Mel Miles: I tried to think of a saying I think if I noticed the little people including Dan Spark, the same is correct. The experiment, well theories guide, but experiments decide. And I think that's the way it should've been. Theories guide experiments decide. And they turned that around in cold fusion. Theories decide and experiments that don't fit are wrong.

Tom Grimshaw: Right, whelp on that concluding note I'll just say that Tom Grimshaw is here with Mel Miles. This will be the last of our sessions. It's July 19th, we're at his home here in beautiful Saint George, overlooking the Virgin river. The temperature is probably now at about 103.

Mel Miles: Yeah. Exactly.

Tom Grimshaw: Exactly 103. It's been a great session Mel. Thank you very much, and we'll continue on other areas.