LENR INVESTIGATIONS BY GEORGE MILEY AT THE UNIVERSITY OF ILLINOIS

A PROJECT OF THE LENR RESEARCH DOCUMENTATION INITIATIVE

DRAFT REPORT

George Miley, Ph.D.
Professor Emeritus
The University of Illinois
Urbana-Champaign, Illinois

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

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1 Introduction

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

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

An initiative is underway to mitigate the risk of loss of research records of LENR investigators. Its objectives are to collect, organize, document, and archive these records. It is being performed by LENRGY, LLC[1], whose President is Dr. Thomas Grimshaw. The LENR Research Documentation Initiative (LRDI) assists researchers in making sure that their efforts are preserved and to keep the records available for additional analysis and interpretation. The LRDI is described in a recent article in Infinite Energy[2] as well as on a dedicated website[3].

[1] LENRGY: LENR Energy – Pursuing the Benefits of Cold Fusion Realization. www.lenrgyllc.com.

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

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

Dr. Miley (Figure 1-1[4]), Professor Emeritus at the University of Illinois, has been one of the foremost researchers in LENR since the beginnings of the field. He is a native of Pennsylvania and received his undergraduate degree from Carnegie Mellon in the mid-1950s[5]. He went on to obtain MA and PhD degrees at the University of Michigan in nuclear and chemical engineering.

After a two-year position as senior nuclear engineer at General Electric's Knolls Atomic Power Laboratory, Dr. Miles joined the University of Illinois faculty in 1961. He remained on the faculty until he retired in 2011 and continues as Professor Emeritus to the present day.

Figure 1-1
Photos of Dr. George Miley at University of Illinois Research Campus.
Photos taken December 2019.

Dr. Miley begin investigating LENR soon after the 1989 announcement, starting with electrolytic cell investigations by undergraduate students under his supervision. However, his primary research in the early years was in LENR-related transmutation to form elements and isotopes in samples were they were not present before the experiments. More detail on Dr. Miley's LENR research career and timeline is in Section 8.

[4] Candidate Photo for Draft Report. Memo to George Miley from Tom Grimshaw, January 19, 2020.
[5] CV for Miley LENR Research Documentation Project. Memo to George Miley from Tom Grimshaw, August 31, 2019.

Dr. Miley's LENR investigations have been conducted as part of a much larger research career in nuclear fission and fusion. He has had a dedicated lab on the University of Illinois campus for many years. It is now designated the Fusion Studies Laboratory (FSL). His broad range of research interests and accomplishments is indicated by the contents of his professional biography, "Life at the Center of the Energy Crisis" [6,7] (Table 1-1). It is noteworthy that LENR constitutes just one of the 21 chapters in the book[8]. A copy of Chapter 12 of the book, which describes Dr. Miley's LENR research, is in Appendix A.

Table 1-1
Chapters of Dr. Miley's Life at the center of the Energy Crisis

1. Early Days and Searching for a Starting Path	12. Low Energy Nuclear Reactions (LENR)
2. Burnable Poison Control for Nuclear Submarine Reactors	13. Hydrogen Economy .incl Fuel Cells
3. Nuclear Pulse Propagation and Vission Reactor Kinetics	14. Fusion Propulsion and Space Colonization
4. Nuclear Pumped Laser (NPL) Research	15. Nuclear Rntteries
5. Direct Electron Beam Pumped Laser	16. Computation and Theory
6. Advanced Lasers	17. Nuclear Power Plant Safety and the Illinois Low-Level Waste Site
7. Alpha Particle Effects in Thermonuclear Fusion Devices	18. Teaching. Education. and University Administration
8. Alternate Fusion Concepts	19. Creation of a Small Company. NPL Associates, Inc.
9. Advanced Fuel Fusion and Direct Energy Conversion	20. Where Am I in the Search? What Have I Found?
10. Inertial Confinement Fusion (ICF)	21. Concluding Comments
11. Inertial Electrostatic Confinement (IEC) Fusion	

A project has been initiated under the umbrella of the LRDI to document the LENR research of Dr. Miley. The objective of the Miley LENR Research Documentation Project (MLRDP) is to collect and record as much of his research record as possible.

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[6] Miley, G., 2013. Life at the Center of the Energy Crisis: a Technologist's Search for a Black Swan. Singapore. World Scientific Publishing.

[7] 2013 Interview in Infinite Energy Magazine. Memo to George Miley from Tom Grimshaw, February 11, 2020.

[8] Life at the Center of the Energy Crisis: Chapter 12, Low Energy Nuclear Reactions (LENR). Memo to George Miley from Tom Grimshaw, November 8, 2019.

2 Publications and Reports

Dr. Miley has authored and co-authored numerous publications and reports both in LENR and in other areas of fusion and fission. Many of his LENR publications are listed in his professional resume. He also provided lists and copies of his publications in electronic files. In addition, many of his publications have been located on LENR-CANR.org.

2.1 Publications Listed in Professional Resume

The extensive list of publications and related work on Dr. Miley's CV[9] has been reviewed for LENR and related items. About 126 items were found in the categories shown below and are listed in Table 2-1.

Chapters in Books (1)
Articles in Books (2)
Articles in Journals (13)
Articles in Conference Proceedings (70)
Bulletins and Tech Notes (31)
Other (3)
Patents (6)

The numbers in column 1 of the table correspond to the listing of the items in the professional resume.

[9] LENR-Related Items on Your CV. Memo to George Miley from Tom Grimshaw, October 12, 2019.

Table 2-1
LENR-Related Items Identified on Miley's Professional Resume

Chapters in Books	
18.	M. Srinivasan, G.H. Miley, and E. Storms, "Low-Energy Nuclear Reactions: Transmutations," Nuclear Energy Encyclopedia: Science, Technology, and Applications, (First Edition, John Wiley & Sons, Inc.)
Articles in Books	
7.	G. H. Miley and P. J. Shrestha, "Overview of Light Water/Hydrogen-based Low Energy Nuclear Reactions" , LENR-CANR.org library, The 12th International Conference on Cold Fusion, Nov 27-Dec 3, 2005, Condensed Matter Nuclear Science, eds Akito Takahashi, Ken-ichiro Ota & Yasuhiro Iwamura , World Scientific Publishing Company, Imperial College Press, December (2006).
8.	G. H. Miley, H. Hora, K. Philberth, A. Lipson, P. J. Shrestha, "Radiochemical Comparisons on Low Energy Nuclear Reactions and Uranium," in Low-Energy Nuclear Reactions and New Energy, Volume 2, J. Marwan and S. Krivit (eds.), (2010).
Articles in Journals	
146.	M. Ragheb and G. H. Miley, "On the Possiblity of Deuteron Disintegration in Electrochemically Compressed D+ in a Palladium Cathode," Fusion Technology, 16, 243, (1989).
151.	M. Ragheb and G. H. Miley, "Deuteron Disintegration in Condensed Media," Workshop on Cold Fusion Phenomena, May 1989; Journal of Fusion Energy, 9, 429-436, (1990).
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195.	G. H. Miley, "Possible Evidence of Anomalous Energy Effects in H/D-Loaded Solids – Low Energy Nuclear Reactions, (LENRs)," Journal of New Energy, 2:3-4, 6-13, (1998).
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202.	G. H. Miley, "Comments about Tadahiko Mizuno's book, Nuclear Transmutation: The Reality of Cold Fusion." Infinite Energy, Issue 20, (1998).
204.	H. Hora and G. H. Miley. "Low Energy Nuclear Trasmutations of Protons and Deuterons in Host Metals and Magic Numbers," Czechoslovak J. Phys., (1999).
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230.	A. G. Lipson, A. S. Rusetskii, A. B. Karabut, and G. H. Miley, " DD Reaction Enhancement and X Ray Generation in a High-Current Pulsed Glow Discharge in Deuterium with Titanium Cathode at 0.8-2.45kV", Journal of Experimental and Theoretical Physics, Vol. 100, No.6, pp. 1334-1349, (2005).
Articles in Conference	

Proceedings	
196.	M. Ragheb, and G. H. Miley, "Deuteron Disintegration in Condensed Media," Workshop on Cold Fusion Phenomena, Santa Fe, NM, May (1989).
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228.	A. Lipson, G. Miley, and A Roussetski, "Energetic alpha and proton emissions on the electrolysis of thin-Pd films" Trans. of the ANS, San Diego, CA, Jun (2003).
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252.	H. Hora, G. H. Miley, "Maruhn-Greiner Fission Theory and Low Energy Nuclear Reactions with Magic Numbers", Bult. APS, Orlando, FL (2007).
253.	G. H. Miley, H. Hora, A. Lipson, N. Luo, P. J. Shrestha, "Future Power Generation by LENR with Thin-Film Electrodes" American Chemical Society Annual Meeting, Chicago, March 29, (2007).
267.	G. H. Miley, A. Lipson, H. Hora and P. J. Shrestha, "Cluster Reactions in LENRs", 8th International Workshop on Anomalies in H/D Loaded Metals, Catania, Sicily, October 12-18, (2007).
277.	G. H. Miley, K. J. Kim, E. Ziehm, P. Tapan, and B. Stunkard, "Progress in Development of an LENR Power Cell for Space," Abstract: Nuclear and Emerging Technologies for Space 2015 (NETS-2015), ICCF-19 International Conference on Condensed Matter Nuclear Science, Padua, Italy, April 13 – 17 (2015).
Reports	None listed.
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Patents	
8.	G.H. Miley and H. Hora, "Method and Arrangement for Nuclear Reactions at Low Temperatures," DE 3910806 A1, (1990).
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15.	G. H. Miley and J.A. Patterson, "Electrolytic Production of Excess Heat for Transmutation", U.S. 1997/005946, (1999).
16.	G.H. Miley and J. A. Patterson, "Low Temperature Electrolytic Nuclear Transmutation,", U.S. 1997/018552 (1999).
18.	G.H. Miley, "Electrical Cells, Components and Methods", U.S. 7244887 B2, (2000).
27.	Y. Gu, J. Javedani and G. H. Miley, "Electrostatic Accelerated-Recirculating-Ion Fusion Neutron/Proton Source", U.S. 20030223528 A1, (2003)

2.2 Items Included in Electronic Files

Dr. Miley has dentified in his electronic files two sets of publications with the designations "CTR" and "FSL". They apparently are lists of hardcopy papers and other items that are contained in file cabinets in the FSL open space (Figure 2-1). The CTR files are publications listed in two groups – "1 to 299" and "500 to 550"[10]. The dates of the items in the CTR list extend up to 1984, which is well before the 1989 LENR announcement, so the files and papers are not included in the Project.

The FSL list of publications is also in two groups – "1998-2003" and "2003-Present" [11]. The list in the 1998-2003 group includes items that are numbered from 580 to 872 (a list for 1 to 579 was not found). Approximately 72 of the items appear to be related to LENR (Table 2-2). Copies of these items have not yet been obtained for the Project. The 2003-Present group in the FSL publications includes 239 items, all of which have been obtained for the Project. Approximately 44 of the items are LENR-related (Table 2-3).

Figure 2-1
Dr. Miley Near File Cabinets in the Fusion Studies Laboratory

[10] Electronic Files with "CTR" Designation. Memo to George Miley from Tom Grimshaw, January 21, 2020.
[11] Electronic Files with "FSL" Designation. Memo to George Miley from Tom Grimshaw,January 24, 2020.

Table 2-2
FSL "1998-2003" Publication List

FSL-582	H. Hora, George H. Miley, Jak C. Kelly, "Low Energy Proton and Deuteron Long Range Nuclear Reactions in field screened Host Materials", submitted to Physics Letters
FSL-589	G. Miley, "Possible Evidence of Anomalous Energy Effects in H/D-Loaded Solids—Low Energy Nuclear Reactions (LENRs)", Proceedings, NASA Breakthrough Propulsion Physics Workshop, NASA Glenn Research Center, pp. 121-128, Jan (1999).
FSL-605	H. Hora and G.H. Miley "Low Energy Nuclear Transmutations of Protons and Deuterons in Host Metals and Magic Number ", (1998, not published).
FSL-606	G. Miley, "Possible Evidence of Anomalous Energy Effects in H/D-Loaded Solids—Low Energy Nuclear Reactions (LENRs)", Journal of New Energy, Vol. 2, No. 3-4, pp. 6-13, (1998).
FSL 635	G. Miley, "Characteristics of Potential Low-Energy Nuclear Reactions in Solids", Transactions, ANS 1998 Annual Meeting and Embedded Topical Meeting, Technology and Fusion Energy, Vol 78, No. 1-294, Nashville, TN, pp. 82-83, Jun 7-11 (1998).
FSL 657	G. H. Miley, "Product Characteristics and Energetics in Thin-Film Electrolysis Experiments", Proceedings, International Conference on Cold Fusion (ICCF-7), pp 241-246, Vancouver, Canada, Apr 19-24, (1998).
FSL 664	G. Miley and M. Williams, "Method for Sputtering Thin Metallic Films on Millimeter Diameter Polyester Spheres", Trans. ANS of (1998) Winter Meeting, Vol. 79, pp. 117-118, Washington DC, Nov 15-19 (1998).
FSL 665	H. Hora, J. Kelly, and G. Miley, "Nuclear Shell Magic Numbers Fitting Low-Energy Nuclear Reaction Experiments", Trans. of ANS (1998) Annual Meeting, p. 90-92, Nashville, TN, Vol. 34, No. 3, Pt 1, Jun 7-11 (1998).
FSL 666	G. Miley, "Compact Neutron Source Based on Accelerator-Plasma Target Technology", INEEL University Research Consortium Final Report, Contract No. CC-S-622904-003-C, Jul (1998).
FSL 668	M. Okuniewski, J. Patterson, G. Collins, G. Miley, "Amelioration of 235U, 234mPa, 212Pb, and 228Ac Within an Electrolytic System", Trans. of ANS (1998) Annual Meeting, Nashville, TN, p. 83-84, Jun 7-11 (1998).
FSL 671	G. Miley and H. Hora, "Transmutations by Low Energy Nuclear Reactions with New Magic Numbers Indicating Element 306X126 for Compound Reactions", Nuclear Physics A, May (1998).
FSL 673	Miley, G. H., "Emerging Physics for a Breakthrough Thin-Film Electrolytic Cell Power Unit," AIP Conf. Proc. 458, STAIF 99, American Institute of Physics, Melville, NY, pp. 1227-1231, (1999).
FSL 677	Hora, H., G.H. Miley, and J. C. Kelly, "From Cold Fusion to Low Energy Long Distant Nuclear Reactions of Hydrogen in Host Metals", APS Meeting, 43, pp. 1924, New Orleans, Nov 16-20 (1998).
FSL 679	H. Hora and G. H. Miley, "New Magic Numbers from Low Energy Nuclear Transmutations Predict Element306X126 for Compound Reactions", Czechoslovak Journal of Physics, 48, No. 9, pp 111-116 (1998).
FSL 680	G. Miley, "Comments on Mizuno's Book, "Nuclear Transmutation: The Reality of Cold Fusion", Infinite Energy, Issue 20, p. 35 (1998).
FSL 686	Hora, Heinrich, "Magic Numbers and Low Energy Nuclear Transmutation by Protons in Host Metals*", Czechoslovak Journal of Physics, Vol. 48, No. 3, pp. 321-328, (1998).
FSL 692	Miley, G., "Review of Transmutations During Electrochemical Processes", Contributions, Workshop on New Energy from Hydrogen, ENEA, Frascati, May 28-29, pp 220-282 (1998).
FSL 693	duplicated
FSL 699	Miley, George and Narne, Gokul, "Evidence for Compound Nucleus Formation by Proton-Metal Reactions in Thin-Films,", Bulletin, APS , 43, 8,, pp. 1860.
FSL 700	Hora, H., Miley, G.H., and Kelly, J.C., "From Cold Fusion to Low Energy Long Distant Nuclear Reactions of Hydrogen in Host Metals". Buletin, APS, 43, 8,, p. 1924.
FSL 708	Heinrich Hora, George Miley, and Jak Kelly, "Low Energy Nuclear Reactions of Hydrogen in Host Metals", Current Trend in International Fusion Research – Proceedings of the Third Symposium; edited by E. Panarella. NRC Research Press, National Research Council of Canada, Ottawa ON K1A OR6

	Canada, pp. 527-546, (1999).
FSL 710	G. Miley, M. Okuniewski, A. Tate, and J. Patterson, "Scientific Basis for Low-Energy Nuclear Reactions (LENRs) for Nuclear Waste Amelioration", Proceedings, 34th Intersociety Energy Conversion Engineering Conf., IECEC-99, CD-ROM, (1999)-01-2725 (1999).
FSL 726	G. H. Miley, "Characteristics of Reaction Product Patterns in Thin Metallic films Experiments", in the Proceedings of 4th Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, edited by W. J. M. F. Collins, Conference Proceedings Vol. 64, SIF, Bologna, Italy, pp. 77-87, (1999).
FSL 727	H. Hora, G.H. Miley, J. C. Kelly, G. Selvaggi, A. Tate, F. Osman, R. Castillo, "Proton-Metal Reactions in Thin Films with Boltzmann Distribution Similar to Nuclear Astrophysics", Fusion Technology, Vol. 36 No. 3, pp. 331-336, (1999).
FSL 738	G. H. Miley, M. Okuniewski, A. Tate, H. Hora, J. Patterson, "Status of Low Energy Nuclear Reaction (LENR) Research Related to Nuclear Waste Amelioration", INEEL Proceedings of the International Conference on Future Nuclear Systems (GLOBAL'99) - Nuclear Technology - Bridging the Millennia, Jackson, Wyoming, (1999).
FSL 739	G. Miley, T. Bauer, J. Nadler, H. Hora, "Converging Beam Neutron Source for Driving a Sub-critical
FSL 740	H. Hora, G. Miley, J. Kelly, "Low Energy Nuclear Reactions of Protons in Host Metals", Proceedings, 8th International Conference on Cold Fusion, ICCF8, Villa Marigola, Lerici (La Spezia), Italy, pp. 425-430, May 21-26 (2000).
FSL 742	G. H. Miley, G. Selvaggi, A. Tate, M Okuniewski, M. Williams, D. Chicea, H. Hora, J. Kelly, "Advances in Thin-Film Electrode Experiments", Proceedings, 8th International Conference on Cold Fusion, ICCF-8, Villa Marigola, Lerici (La Spezia), Italy, pp. 169-176, May 21-26 (2000).
FSL 743	G. H. Miley, "On the Reaction Product and Heat Correlation for LENRs", Proceedings, 8th International Conference on Cold Fusion, ICCF8, Villa Marigola, Lerici (La Spezia), Italy, pp.419-424, May 21-26 (2000).
FSL 746	G. H. Miley, D. Chicea, M. J. Williams, M. A. Okuniewski, G. Selvaggi, A. N. Tate, "Infinite Energy" Kit Project Final Report, Department of Nuclear, Plasma, and Radiological Engineering, University of Illinois at Urbana-Champaign, Urbana, IL, Nov 12 (1999).
FSL 751	G. Miley, "Low Energy Nuclear Reaction (LENRs) Studies Using Thin-Film Electrodes", Proceedings, 12th APS Topical Conference on Atomic Processes in Plasmas, Vol. 45, No. 1, Reno, Nevada, Mar 19-23 (2000).
FSL 752	G. Miley, D. Chicea, M.Williams, M Okuniewski, G. Selvaggi, A. Tate, "Status and Recent Studies Using Thin Film Electrodes", Proceedings, Science Program Committee Meeting, Frascati, Italy, Feb 2000.
FSL 755	G. Miley, G. Selvaggi, A. Tate, M. Okuniewski, M. Williams, D. Chicea, "Experimental Status and Potential Applications of a Thin-Film Low Energy Nuclear Reaction (LENR) Power Cell", Proceedings of ICONE 8, 8th International Conference on Nuclear Engineering, Nuclear Energy in the New Millennium, ICONE 8, Apr 2-6 Baltimore, MD (2000).
FSL 756	H. Hora, G. Miley, J. Kelly, "Low Energy nuclear Reactions of Protons in Host Metals at Picometer', 2000 ANS/ENS International Meeting, Vol. 83, p. 357, Washington DC, Nov (2000).
FSL 760	F. Celani, A. Spallone, P. Triposi, D. Di Gioacchino, G. Selvaggi, P. Marini, V. Di Stefano, S. Pace, A. Mancini, "A Preliminary D/Pd loading study: Anomalous Resistivity Transition Effect", in the Proceedings of 4th Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals, edited by W. J. M. F. Collins, Conference Proceedings Vol. 64, SIF, Bologna, Italy, (1999).
FSL 761	G. Miley, "Some Personal Reflections on Scientific Ethics and the Cold Fusion 'Episode'", Journal on Accountability in Research: Policies and Quality Assurance, Vol. 8, No. 1, pp 121-135 (2000).
FSL 762	G. Stoppini, "Nuclear Processed in Hydrogen-Loaded Metals", Fusion Technology, Vol. 34, No. 1, pp 81-85, Aug (1998).
FSL 772	G. Miley, M Okuniewski, A. Tate, G. Selvaggi, M. Williams, D. Chicea, H. Hora, "Advances in Thin-Film Proton-Reaction Cell Experiments", NASA Advanced Propulsion Workshop, Pasadena CA, May (2000).
FSL 774	A. Tate, "Anomalous Heat Production Using Multiple Layer Thin film Cathodes in an Electrolytic Cell", MS Thesis, Department of. Nuclear, Plasma, and Radiological Engineering, University of Illinois, Urbana, Illinois (2000).
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FSL 799	G. H. Miley, C. Castano, M. Okuniewski, G. Selvaggi, A. Lipson, "Anomalous Reaction Phenomena in Metals under High Proton Loading", Bult. APS, Vol. 46, No. 1, pp. ---, Seattle WA, Mar 12-16 (2001).
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FSL 801	G. H. Miley, "Studies of Energy Gain and Heat Production from unique proton reaction cells", 4th Symposium on Current Trends in International Fusion Research: A Review, March 12-16, Washington, D.C., pp. 144 (2001).
FSL 802	G.H. Miley, C. Castano, M. Okuniewski, G. Selvaggi and A. Lipson, "Design of a Low Energy Reaction Cell for Distributed Power Applications", Proceedings, 9th International Conference on Nuclear Engineering, ICONE 9, #ICONE-9, Nice, France, Apr 2-6 (2001).
FSL 803	V. Violante, A. Torre, G. Selvaggi and G.H. Miley, "Three-Dimensional Analysis of the Lattice Confinement Effect on Ion Dynamics in Condensed Matter and Lattice Effect on the D-D Nuclear Reaction Channel", Fusion Technology, Vol. 39, No. 2, pp. 266-281, Mar (2001).
FSL 808	G. H. Miley, "Some Personal Reflections on the Cold Fusion 'Episode", Bult. APS, Washington, D.C., Vol. 46, No. 2, pp 146, Apr-May (2001).
FSL 817	George H. Miley, "Plasma Screening for Endothermic Synthesis in Low Energy Nuclear Reactions," 43rd Annual Meeting of the Division of Plasma Physics, Bult. APS, Long Beach, CA, Oct 29 - Nov 2 (2001).
FSL 820	A. G. Lipson, G. H. Miley, V. A. Kuznetsov, and E. I. Saunin, "Anomalous Structural Changes in Non-equilibrium Condensed Matter Via Ultraweak Thermalized Neutron Field", 2001 IEEE Nuclear Science Symposium & Medical Imaging Conference, Symposium on Nuclear Power Systems - 12th International Workshop on Room-Temperature Semiconductor X- & Gamma-Ray Detectors, San Diego, CA, Nov 4-10, 2001, published IEEE (2002).
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FSL 826	C. Castano, A. Lipson, S. O. Kim, and G. H. Miley, "Thermal Measurement during Electrolysis of Pd-Ni thin-film-Cathodes in Li2O4/H2O", Bult. APS, APS Spring Mtg., Indianapolis, IN, Vol. 47, No. 1,Pt. II, pp. 1219-1220, Mar 17-22 (2002).
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FSL 834	G. H. Miley, C. Castano, A. Lipson, S.-O. Kim, and N. Luo, "Progress in Development of a Low Energy Reaction Cell for Distributed Power Applications", Proc. of ICONE 10: 10th International ASME Conference on Nuclear Engineering, Arlington, Virginia, Track 8, pp. 1-7, Apr (2002).
FSL 838	C.H. Castano, A.G. Lipson, S.O. Kim and G.H. Miley, "Calorimetric Measurements during Pd-Ni thin film–cathodes Electrolysis in Li2SO4/H2O Solution", Proc. of the 9th International Conf. on Cold Fusion, ICCF9, Beijing, China, pp. 24-28, May 19-24 (2002).
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FSL 840	N. Luo, C.H. Castano, S-O. Kim, A.G. Lipson, T. Woo, and G.H. Miley, "In-Situ Characterization of

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FSL 842	A.G. Lipson, V. A. Kuznetsov, E. I. Saunin, and G.H. Miley, "Anomalous Thermal Neutron capture and Substrate Pd-isotopes Separation in Cold-worked Palladium Foils as a Result of Deuterium Loading", Proceedings of the 9th International Conference on Cold Fusion, ICCF9, Beijing, China, pp. 208-212, May 19-24 (2002).
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FSL 856	G. H. Miley and A. G. Lipson, "Theory of X-ray Laser Emission from Highly Loaded Hydrides", ICENES 2002: 11th International Conference on Emerging Nuclear Energy Systems, Albuquerque, NM, pp. 382- 387, Sept 29-Oct 4 (2002).
FSL 857	G. H. Miley and H. Hora, "Nuclear Reactions in Solids", APS 2002 Fall Nuclear Physics Mtg, East Lansing, Michigan, Oct. 7-10 (2002).
FSL 863	G. H. Miley and P. J. Shrestha, "Review of Transmutation Experiments Based on Metals Highly Loaded with Proton/Deuterium", Speaker at ORNL, Oak Ridge, Tennessee, Oct. 29 (2002).
FSL 864	G. H. Miley and H. Hora, "Study of Compound Nuclei Produced in Low Energy Nuclear Reactions in Solids", 3rd Intl. Conf on Fission & Properties of Neutron-Rich Nuclei, Sanibel Island, Florida, Nov. 3-9 (2002).
FSL 866	T. Woo, N. Luo, G. H. Miley, L. Wu, H. Hora, and V. Violante, "Proton Collisions in Metallic Hydrides under Plasma Oscillation", Blt. APS, 44th Mtg Division of Plasma Physics, Orlando, Florida, Vol. 47, No. 9, pp. 292, November 11-15 (2002).

Table 2-3
FSL "2003 to Present" Publication List

FSL 899	Screening Effects in Low Energy Nuclear Reactions of Importance to Astrophysics.pdf
FSL 998	Overview of Light Water Hydrogen-Based Low Energy Nuclear Reactions.pdf
FSL 1054	Transmutation Reactions and Associated Low-Energy Nuclear Reactions Effects in Solids.pdf
FSL 1060	A Fascinating Review of the Emerging Science of LENRs.pdf
FSL 1072	Condensed Matter Cluster Reactions in LENRs.pdf
FSL 1073	Phenomenon of Energetic Charged Particle Emissions from the Pd and Ti Surfaces Loaded with Hydrogen_Deuterium.pdf
FSL1074	lncreased D-D Fusion Reaction Boosted by Electron Screening at the.pdf
FSL1080	Strong Enhancement of DD-Reaction Accompanied by X-Ray Generation in a Pulsed Low Voltage High-Current.pdf
FSL 1091	Connection of Preparata QED Theory and D-Pd-D Cluster Theory for Cold Fusion Reactions.pdf
FSL 1095	Evidence and Theory for Cluster Reactions in LENRs.pdf
FSL 1096	Compound Nucleus Reactions in LENR, Analogy to Uranium Fission.pdf
FSL 1109	Fusion Energy by Laser Driven Block Ignition of Solid Density Fuel.pdf
FSL 1111	Ultrahigh-Density Deuterium of Rydberg Matter Clusters for ICF Targets. pdf
FSL1118	Ultrahigh-Density Deuterium of Rydberg Matter Clusters_.pdf
FSL1123	Deuterium Cluster Target for Ultra High Density Fusion.pdf
FSL 1127	Small Power Cells Based on Low Energy Nuclear Reaction (LENR) – a New Type of –Green Nuclear Energy.pdf
FSL 1131	Condensed Matter Cluster Reactions in LENR Power Cells for a Radical New Typed of Space Power Source.pdf
FSL 1137	Low-Energy Nuclear Reaction Power Cells as a Novel New Space Power Source.pdf
FSL 1139	Fast Ignition ICF Fusion with Bose-Einstein Cluster Targets for p-B11 Powered Space Propulsion.pdf
FSL1142	Distributed and Large Scale Hydrogen Production Methods.pdf
FSL1144	Bose-Einstein Type D-Cluster Electrode Development. pp 342-350.pdf
FSL 1145	Clusters with Picometer Distance of Deuterons and LENR.pdf
FSL1150	Energetics of condensed matter cluster reactions in nanostructured palladium. docx
FSL 1153	Ultra-High Density Deuteron-cluster Electrode for Low-energy Nuclear Reactions.pdf
FSL 1155	Radiochemical Comparisons on Low Energy Nuclear Reactions and Uranium.pdf
FSL 1165	Distributed Power Sources for Mars.pdf
FSL 1169	Ultra-High Density Deuterium Cluster. . . Abstract. docx
FSL 1175	Fusion Power Sources for Mars Exploration. Abstract.pdf
FSL 1178	Ultra High Density Deuterium Clusters for Low Energy Nuclear Reactions. docx
FSL 1186	Fusion Power Sources for Mars Exploration. Abstract.pdf
FSL 1196	Low-Energy Nuclear Reactions. Transmutations.pdf
FSL 1211	A Game-Changing Power Source Based on Low Energy Nuclear Reactions (LENRs).pdf
FSL 1225	Use of D_H Clusters in LENR and Recent Results from Gas Loaded Nanoparticle Type Clusters.pdf
FSL 1226	Surface Effect for Gas Loading Micrograin Palladium for Low Energy Nuclear Reactions LENR.pdf
FSL 1226a	Poster_ Surface Effect for Gas Loading Micrograin Palladium for Low Energy Nuclear Reactions LENR.pdf
FSL 1227	A Space Power Source Using Low Enerav Nuclear Reactions (LENRs).pdf
FSL 1236	A Space Power Source Based on Low Energy Nuclear Reactions (LENRs).pdf
FSL 1241	Study of Composition Changes in Nanoparticles During Gas Loaded LENR Power Cell Operation. docx
FSL 1248	Distributed Power Source Using Low Energy Nuclear Reactions.pdf
FSL 1251	Progress in Development in Low Energy Nuclear Reaction (LENR) Power Cells for Space Applications.pdf
FSL 1258	Progress in Development of an LENR Power Cell for Space.pdf
FSL 1270	LENR Reactions Using Clusters.pdf
FSL 1271	Palladium Isotope Separation under High Mechanical Stresses Induced in Pd Foils upon Loading with Deuterium.pdf
FSL 1272	Quark-Gluon Model for Magic Numbers Related to Low Energy Nuclear Reactions.pdf

2.3 Publications Found on LENR-CANR.org

A search for "Miley" on LENR-CANR.org found about 81 documents[12]. They are listed in Table 2-4. Forty six of the items (about 60%) were available (indicated by "Y" in "DL?" – download – column) and have been downloaded for the MLRDP[13]. The 81 documents have been consolidated with the 126 items on Dr. Miley's CV for future comparison with the items in his electronic files[14].

[12] Miley Papers on LENR-CANR.org. Memo to George Miley from Tom Grimshaw, August 28, 2019.

[13] Downloads of Miley Papers on LENR-CANR.org. Memo to George Miley from Tom Grimshaw, September 2, 2019.

[14] Consolidated List of LENR Publications. Memo to George Miley from Tom Grimshaw, November 4, 2019.

Table 2-4
List of Papers with Miley as Author or Co-Author on LENR-CANR.org
("Y" in "Dl?" Column Indicates Available for Download)

No	Reference	Year	DL?	Citation
1	Ragheb, M.	1989		Ragheb, M. and G.H. Miley, On the possibility of deuteron disintegration in electrochemically compressed deuterium ion (D+) in a palladium cathode. Fusion Technol., 1989. 16: p. 243.
2	Hora, H.	1990		Hora, H., et al., Plasma and surface tension model for explaining the surface effect of tritium generation at cold fusion. Nuovo Cimento Soc. Ital. Fis. A, 1990. 12D(3): p. 393.
3	Hora, H.	1990		Hora, H., et al. Surface Models for Cold Fusion and the Possibilities of Multilayered Cells for Energy Production. in 8th World Hydrogen Energy Conf. 1990. Honolulu, HI: Hawaii Natural Energy Institute, 2540 Dole St., Holmes Hall 246, Honolulu, HI 96822.
4	Miley, G. H.	1990		Miley, G.H., Book Review: Cold Fusion,The Making of a Scientific Controversy by F. D. Peat. Fusion Technol., 1990. 17: p. 730.
5	Miley, G. H.	1990		Miley, G.H., O. Barnouin, and B. Temple. Detection of Reaction Products Induced in Plasma Focus Electrodes. in Anomalous Nuclear Effects in Deuterium/Solid Systems, "AIP Conference Proceedings 228". 1990. Brigham Young Univ., Provo, UT: American Institute of Physics, New York.
6	Miley, G. H.	1990	Y	Miley, G.H., M. Ragheb, and H. Hora. On Aspects of Nuclear Products. in The First Annual Conference on Cold Fusion. 1990. University of Utah Research Park, Salt Lake City, Utah: National Cold Fusion Institute.
7	Ragheb, M.	1990		Ragheb, M. and G.H. Miley, Deuteron disintegration in condensed media. J. Fusion Energy, 1990. 9: p. 429.
8	Shaheen, M.	1991	Y	Shaheen, M., et al. Anomalous Deuteron to Hydrogen Ratio in Oklo Samples and Possibility of Deuteron Disintegration. in Second Annual Conference on Cold Fusion, "The Science of Cold Fusion". 1991. Como, Italy: Societa Italiana di Fisica, Bologna, Italy.
9	Miley, G. H.	1992	Y	Miley, G.H., et al. Multilayer Thin Film Electrodes for Cold Fusion. in Third International Conference on Cold Fusion, "Frontiers of Cold Fusion". 1992. Nagoya Japan: Universal Academy Press, Inc., Tokyo, Japan.
10	Hora, H.	1993		Hora, H., et al., Screening in cold fusion derived from D-D reactions. Phys. Lett. A, 1993. 175: p. 138.
11	Miley, G. H.	1993		Miley, G.H. Comments About Nuclear Reaction Products. in Fourth International Conference on Cold Fusion. 1993. Lahaina, Maui: Electric Power Research Institute 3412 Hillview Ave., Palo Alto, CA 94304.
12	Miley, G. H.	1994		Miley, G.H., et al., Electrolytic Cell with Multilayer Thin-Film Electrodes. Trans. Fusion Technol., 1994. 26(4T): p. 313.
13	Miley, G. H.	1994		Miley, G.H., et al. Energy Amplifier with Multilayer Thin Film Electrodes. in International Symposium on Cold Fusion and Advanced Energy Sources. 1994. Belarusian State University, Minsk, Belarus: Fusion Information Center, Salt Lake City.
14	Hora, H.	1996	Y	Hora, H., G.H. Miley, and J. Kelly. Field Screened Long Range Nuclear Reactions by Thermal Protons. 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.
15	Miley, G. H.	1996		Miley, G.H. and J.A. Patterson, Nuclear transmutations in thin-film nickel coatings undergoing electrolysis. J. New Energy, 1996. 1(3): p. 5.
16	Miley, G. H.	1996	Y	Miley, G.H., et al. Quantitative observations of transmutation products occuring in

				thin-film coated microspheres during electrolysis. 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.
17	Hora, H.	1997		Hora, H., J.C. Kelly, and G.H. Miley, Energy gain and nuclear transmutation by low-energy p- or d-reaction in metal lattices. Infinite Energy, 1997. 2(12): p. 48.
18	Miley, G. H.	1997		Miley, G.H., Possible Evidence of Anomalous Energy Effects in H/D-Loaded Solids-Low Energy Nuclear Reactions (LENRS). J. New Energy, 1997. 2(3/4): p. 6.
19	Hora, H.	1998		Hora, H. and G.H. Miley, New magic numbers from low energy nuclear transmutations predict element (306)X(126) for compound reactions. Czech. J. Phys., 1998. 48: p. 1111.
20	Hora, H.	1998	Y	Hora, H., et al. Nuclear Shell Magic Numbers Agree With Measured Transmutation by Low-Energy Reactions. in The Seventh International Conference on Cold Fusion. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT.
21	Miley, G. H.	1998	Y	Miley, G.H. Product Characteristics and Energetics in Thin-Film Electrolysis Experiments. in The Seventh International Conference on Cold Fusion. 1998. Vancouver, Canada: ENECO, Inc., Salt Lake City, UT.
22	Hora, H.	1999		Hora, H., et al., Proton-metal reactions in thin films with Boltzmann distribution similar to nuclear astrophysics. Fusion Technol., 1999. 36: p. 331.
23	Miley, G. H.	1999		Miley, G.H. Emerging physics for a breakthrough thin-film electrolytic power unit. in Space Technol. Applic. Int. Forum. 1999.
24	Hora, H.	2000		Hora, H. and G.H. Miley, Heavy nuclide synthesis by neutrons in astrophysics and by screened protons in host metals. Czech. J. Phys., 2000. 50: p. 433.
25	Hora, H.	2000		Hora, H., G.H. Miley, and J. Kelly. Low Energy Nuclear Reactions of Protons in Host Metals. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
26	Miley, G. H.	2000		Miley, G.H., et al. Advances in Thin-Film Electrode Experiments. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
27	Miley, G. H.	2000	Y	Miley, G.H. On the Reaction Product and Heat Correlation for LENRs. in 8th International Conference on Cold Fusion. 2000. Lerici (La Spezia), Italy: Italian Physical Society, Bologna, Italy.
28	Miley, G. H.	2000	Y	Miley, G.H., Some personal reflections on scientific ethics and the cold fusion 'episode'. Accountability Res., 2000. 8: p. 121.
29	Violante, V.	2001		Violante, V., et al., 3 Dimensional Analysis of the Lattice Confinement Effect on ion Dynamics. Fusion Technol., 2001. 39(2 (March)).
30	Violante, V.	2001		Violante, V., et al., Three-dimensional analysis of the lattice confinement effect on ion dynamics in condensed matter and lattice effect on the d-d nuclear reaction channel. Fusion Technol., 2001. 39: p. 266.
31	Castano, C. H.	2002	Y	Castano, C.H., et al. Calorimetric Measurements During Pd-Ni Thin Film-cathodes Electrolysis in Li2SO4/H2O Solution. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press.
32	Castano, C. H.	2002		Castano, C.H., et al. In-Situ Charactorization of Sputtered Pd Thin-Films Undergoing Electrolysis. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press.
33	Hora, H.	2002	Y	Hora, H., et al. Shrinking of hydrogen atoms in host metals by dielectric effects and Inglis-Teller depression of ionization potentials. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Tsinghua Univ., Beijing, China: Tsinghua Univ. Press.
34	Kim, S-O.	2002	Y	Kim, S.-O., A.G. Lipson, and G.H. Miley. Characterization of Pd-Ni thin film by

				annealing method. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Tsinghua Univ., Beijing, China: Tsinghua Univ. Press.
35	Lipson, A. G.	2002	Y	Lipson, A.G., et al. Anomalous thermal neutron capture and sub-surface Pd-isotopes separation in cold-worked palladium foils as a result of deuterium loading. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Tsinghua Univ., Beijing, China: Tsinghua Univ. Press.
36	Lipson, A. G.	2002	Y	Lipson, A.G., et al. In-Situ Charged Particles And X-Ray Detection In Pd Thin Film-Cathodes During Electrolysis In Li2SO4/H2O. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press.
37	Luo, N.	2002	Y	Luo, N., et al. In-Situ Charactorization of Sputtered Pd Thin-Films Undergoing Electrolysis. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Beijing, China: Tsinghua University: Tsinghua Univ. Press.
38	Luo, N.	2002	Y	Luo, N., G.H. Miley, and A.G. Lipson. Modeling of Surface and Bulk Effects in Thin-Film Pd Cathodes and High Proton Loading. in AVS, ICMSC Conference. 2002. Cleveland, Ohio.
39	Miley, G. H.	2002		Miley, G.H., et al. Progress in Development of a Low Energy Reaction Cell for Distributed Power Applications. in 10th International Conference on Nuclear Engineering. 2002. Arlington, Virginia, USA: ASME.
40	Miley, G. H.	2002	Y	Miley, G.H., et al. Progress in thin-film LENR research at the University of Illinois. in The 9th International Conference on Cold Fusion, Condensed Matter Nuclear Science. 2002. Tsinghua Univ., Beijing, China: Tsinghua Univ. Press.
41	Lipson, A. G.	2003	Y	Lipson, A.G., et al. Phenomenon of an Energetic Charged Particle Emission From Hydrogen/Deuterium Loaded Metals. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
42	Lipson, A. G.	2003	Y	Lipson, A.G., et al. Strong Enhancement of DD-reaction Accompanied by X-ray Generation in a Pulsed Low Voltage High-Current Deuterium Glow Discharge with a Ti-Cathode. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
43	Luo, N.	2003	Y	Luo, N., et al. Enhancement Of Nuclear Reactions Due To Screening Effects Of Core Electrons. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
44	Luo, N.	2003	Y	Luo, N. and G.H. Miley. First-Principles Studies Of Ionic And Electronic Transport In Palladium Hydride. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
45	Miley, G. H.	2003	Y	Miley, G.H. and P. Shrestha. Review Of Transmutation Reactions In Solids. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
46	Violante, V.	2003	Y	Violante, V., et al. Study Of Lattice Potentials On Low Energy Nuclear Processes In Condensed Matter. in Tenth International Conference on Cold Fusion. 2003. Cambridge, MA: LENR-CANR.org.
47	Hora, H.	2004	Y	Hora, H., et al. Low Energy Nuclear Reactions resulting as picometer interactions with similarity to K-shell electron capture. in Eleventh International Conference on Condensed Matter Nuclear Science. 2004. Marseille, France.
48	Lipson, A. G.	2005		Lipson, A.G., G.H. Miley, and H. Momota. Enhancement of First Wall Damage in ITER Type TOKAMAK Due to LENR Effects. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
49	Lipson, A. G.	2005		Lipson, A.G., G.H. Miley, and H. Momota. Enhancement of First Wall Damage in ITER Type TOKAMAK Due to LENR Effects (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
50	Lipson, A. G.	2005	Y	Lipson, A.G., et al. Evidence of Supersoichiometric H/D LENR Active Sites and

				High Temperature Superconductivity in a Hydrogen-Cycled Pd/PdO. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
51	Lipson, A. G.	2005		Lipson, A.G., et al. Evidence of Supersoichiometric H/D LENR Active Sites and High Temperature Superconductivity in a Hydrogen-Cycled Pd/PdO (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
52	Lipson, A. G.	2005	Y	Lipson, A.G., et al. Generation of DD-Reactions in a Ferroelectric KD2PO4 Single Crystal During Transition Through Curie Point (Tc = 220 K). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
53	Lipson, A. G.	2005	Y	Lipson, A.G., et al. Generation of DD-Reactions in a Ferroelectric KD2PO4 Single Crystal During Transition Through Curie Point (Tc = 220 K) (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
54	Lipson, A. G.	2005		Lipson, A.G., et al. Reproducible Nuclear Emissions from Pd/PdO:Dx Heterostructure during Controlled Exothermic Deuterium Desorption. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
55	Lipson, A. G.	2005		Lipson, A.G., et al. Reproducible Nuclear Emissions from Pd/PdO:Dx Heterostructure during Controlled Exothermic Deuterium Desorption (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
56	Miley, G. H.	2005	Y	Miley, G.H., et al. Intense non-linear soft x-ray emission from a hydride target during pulsed D bombardment. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
57	Miley, G. H.	2005	Y	Miley, G.H., et al. Intense non-linear soft x-ray emission from a hydride target during pulsed D bombardment (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
58	Miley, G. H.	2005		Miley, G.H., et al. On Aspects of Complex Nuclei in LENR Relative to Transmutation Reactions and X-ray Emission from Localized Clusters. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
59	Miley, G. H.	2005	Y	Miley, G.H. and P. Shrestha. Overview of Light Water/Hydrogen-based Low Energy Nuclear Reaction (PowerPoint slides). in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
60	Miley, G. H.	2005	Y	Miley, G.H. and P. Shrestha. Overview of Light Water/Hydrogen-based Low Energy Nuclear Reactions. in The 12th International Conference on Condensed Matter Nuclear Science. 2005. Yokohama, Japan.
61	Miley, G. H.	2005		Miley, G.H., G. Narne, and T. Woo, Use of combined NAA and SIMS analyses for impurity level isotope detection. J. Radioanal. Nucl. Chem., 2005. 263(3): p. 691-696.
62	Osman, F.	2005	Y	Osman, F., et al., Supporting the Josephson Interpretation of Low Energy Nuclear Reactions and Stabilization of Nuclear Waste. Am. J. Applied Sci. 2, 2005. 6: p. 1049-1057.
63	Hora, H.	2007		Hora, H. and G.H. Miley, Maruhn-Greiner Maximum of Uranium Fission for Confirmation of Low Energy Nuclear Reactions LENR via a Compound Nucleus with Double Magic Numbers. J. Fusion Energy, 2007. 26: p. 349-355.
64	Miley, G. H.	2007	Y	Miley, G.H., et al. Cluster Reactions in Low Energy Nuclear Reactions (LENRs). in Proceedings of the 8th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals. 2007. Sicily, Italy.
65	Miley, G. H.	2007	Y	Miley, G.H., et al. Future Power Generation by LENR with Thin-Film Electrodes (PowerPoint slides). in 233rd ACS National Meeting. 2007. Chicago, IL.
66	Miley, G. H.	2007	Y	Miley, G.H. Preparata Medal Lecture - A Tribute to Giuliano Preparata, a TRUE

				Pioneer in Cold Fusion Theory. in Proceedings of the 8th International Workshop on Anomalies in Hydrogen / Deuterium Loaded Metals. 2007. Sicily, Italy.
67	Hora, H.	2008	Y	Hora, H., G.H. Miley, and K. Philberth. Radiochemical Observations for Comparison of Uranium Fission with Low Energy Nuclear Reactions LENR. in American Physical Society Meeting. 2008. New Orleans.
68	Miley, G. H.	2008	Y	Miley, G.H., A Fascinating Review of the Emerging Science of LENRs. 21st Century Sci. & Technol., 2008. 61.
69	Miley, G. H.	2008	Y	Miley, G.H., H. Hora, and X. Yang. Condensed Matter "Cluster" Reactions in LENRs. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
70	Miley, G. H.	2008		Miley, G.H. Summary of the Transmutation Workshop held in association with ICCF-14. in ICCF-14 International Conference on Condensed Matter Nuclear Science. 2008. Washington, DC.
71	Miley, G. H.	2008		Miley, G.H. and J. Shrestha, Transmutation Reactions and Associated Low-Energy Nuclear Reactions Effects in Solids, in Low-Energy Nuclear Reactions Sourcebook. 2008, American Chemical Society: Washington, DC. p. 173-218.
72	Hora, H.	2009		Hora, H., et al., Quark-Gluon Model for Magic Numbers Related to Low Energy Nuclear Reactions, in Low-Energy Nuclear Reactions and New Energy Technologies Sourcebook Volume 2. 2009, American Chemical Society: Washington DC. p. 219-234.
73	Miley, G. H.	2009	Y	Miley, G.H., X. Yang, and H. Hora. Bose-Einstein type D-cluster Electrode Development. in 15th International Conference on Condensed Matter Nuclear Science. 2009. Rome, Italy: ENEA.
74	Miley, G. H.	2009	Y	Miley, G.H., et al. Clusters with Picometer Distance of Deuterons and LENR (PowerPoint slides). in 15th International Conference on Condensed Matter Nuclear Science. 2009. Rome, Italy: ENEA.
75	Miley, G. H.	2009		Miley, G.H., et al., Radiochemical Comparisons on Low Energy Nuclear Reactions and Uranium, in Low-Energy Nuclear Reactions and New Energy Technologies Sourcebook Volume 2. 2009, American Chemical Society: Washington DC. p. 235-252.
76	Miley, G. H.	2011	Y	Miley, G.H., X. Yang, and H. Hora, Ultra-High Density Deuteron-cluster Electrode for Low-energy Nuclear Reactions. J. Condensed Matter Nucl. Sci., 2011. 4: p. 256-268.
77	Srinivasan, M.	2011	Y	Srinivasan, M., G.H. Miley, and E. Storms, Low Energy Nuclear Reactions: Transmutations, in Nuclear Energy Encyclopedia: Science, Technology and Applications. 2011, Wiley. p. 503-540.
78	Luo, N.	2012	Y	Luo, N. and G.H. Miley, First-principles Studies of Electronic and Ionic Transport in Palladium Hydrides/Deuterides. J. Condensed Matter Nucl. Sci., 2012. 6: p. 241-255.
79	Miley, G. H.	2012	Y	Miley, G.H. LENR Reactions Using Clusters (PowerPoint slides). in International Low Energy Nuclear Reactions Symposium, ILENRS-12. 2012. The College of William and Mary, Williamsburg, VA 23185.
80	Hora, H.	2014	Y	Hora, H., G.H. Miley, and X. Yang, Bose-Einstein Condensation and Inverted Rydberg States in Ultra-high Density Deuterium Clusters Related to Low Energy Nuclear Reactions. J. Condensed Matter Nucl. Sci., 2014. 13.
81	Miley, G. H.	2014	Y	Miley, G.H., et al., Use of D/H Clusters in LENR and Recent Results from Gas Loaded Nanoparticle-Type Clusters. J. Condensed Matter Nucl. Sci., 2014. 13: p. 411-421.

3 Electronic Files

The electronic files provided to the MLRDP by Dr. Miley are the “CTR” and “FSL” files, which are lists of his publications. They are described in Section 2.2 of this report.

4 Hardcopy Records

Dr. Miley has numerous file cabinets in his office and the FSL. His LENR files, not including his publications, are located in the upper drawer of a file cabinet in his office[15] (Figure 4-1). The drawer has "LENR" on the label identifying its contents.

Figure 4-1
File Drawer with LENR Materials: Upper Drawer of File Cabinet on Right

[15] Hardcopy Files in File Cabinets in Fusion Studies Lab. Memo to George Miley from Tom Grimshaw, April 10, 2020.

5 Laboratories

Dr. Miley has laboratories for LENR research at two locations on the University of Illinois campus. The FSL is on the main campus, and the dedicated LENR lab is in the EnvironmentWorks building at the University's Research Park.

5.1 Fusion Studies Lab

The FSL (Figure 5-1) is where Dr. Miley conducted his LENR investigations for many years along with his extensive work in other areas of fusion and fission research[16]. A photo of Dr. Miley in his office in the FSL is shown in Figure 5-2. Figure 5-3 shows him near the lab bench where most of the research with electrolytic cells took place.

[16] On-Campus Laboratory and Office . Memo to George Miley from Tom Grimshaw, April 8, 2020.

Figure 5-1
Two Views of Fusion Studies Laboratory

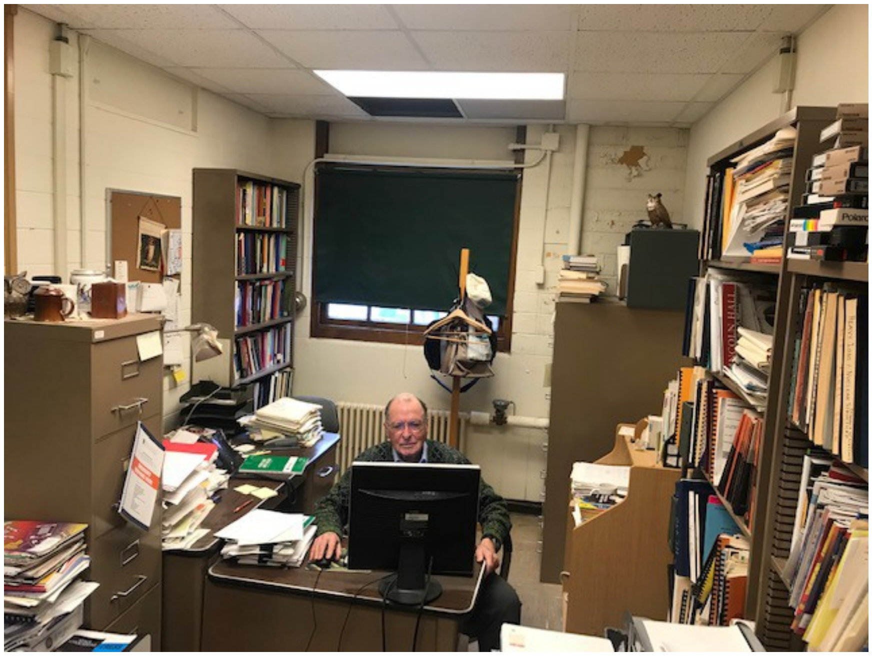

Figure 5-2
Dr. Miley in his Office at the University of Illinois

Figure 5-3
Dr. Miley in Front of the Bench Formerly Used for Electrolytic Cell Experiments

5.2 Erik Ziehm's CR-39 Research

An example of LENR research at the FSL is the work by student Erik Ziehm observed during the site visit in December 2019[17]. Mr. Ziehm was conducting experiments with CR-39, a solid-state nuclear track detector. The experiments detect the emission of charged particles from LENR experiments that utilize nanoparticles that have been pressure cycled with hydrogen. Mr. Ziehm developed an automated system for scanning and imaging CR-39 samples using Ilastic, described as "supervised machine-learning segmentation software".

Figure 5–4 is a photo of Mr. Ziehm in the FSL room where the CR-39 work was being done at the time of the visit. A closer view of Mr. Ziehm with his CR-39 scanning and imaging set up is in Figure 5-5.

[17] Erik Ziehm's CR-39 Lab Work and Posters. Memo to George Miley from Tom Grimshaw, April 12, 2020.

Figure 5-4
Lab Room in Fusion Studies Laboratory for Mr. Ziehm's Experimental Work

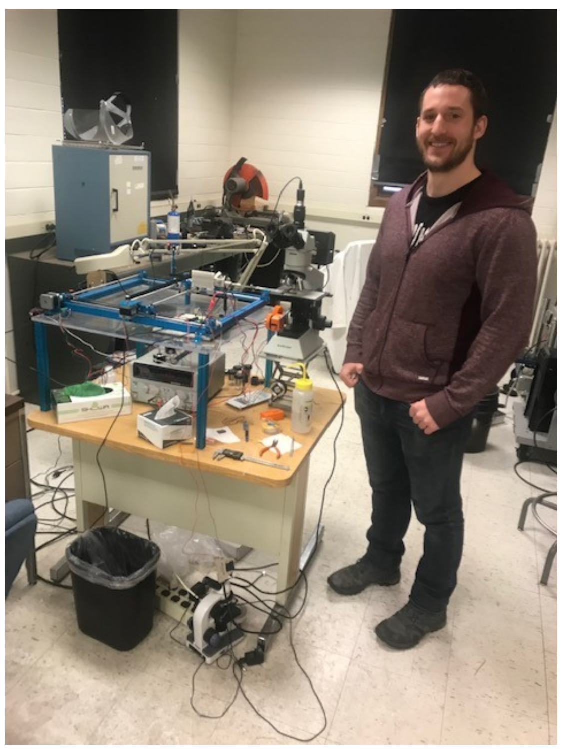

Figure 5-5
Eric Ziehm with His Automated Imaging System for CR-39

5.3 Research Park LENR Lab

Dr. Miley's Research Park lab is dedicated to LENR research for a confidential client[18]. Figures 5-6 and 5-7 show the Research Park sign and the exterior of the EnvironmentWorks building where the lab is located. A photo of the lab with Dr. Miley and lab assistant Ben Preecher is shown in Figure 5-8.

Figure 5-6
University of Illinois Research Park Photos

[18] Research Park LENR Laboratory. Memo to George Miley from Tom Grimshaw, April 9, 2020.

Figure 5-7
Exterior View of the EnvironmentWorks Building Photos

Figure 5-8
LENR Lab in EnvironmentWorks Building at the University of Illinois Research Park
Dr. Miley and Lab Assistant Ben Preecher Shown on Right Site of Photo

6 LENR Library

Dr. Miley's LENR Library is mostly located in a small bookshelf in one of the rooms of the FSL[19] (Figure 6-1). Approximately 40 items were found in the bookshelf (Table 6-1), most of which are related to the LENR field. They include proceedings and other materials from ICCFs as shown below.

ICCF	Year
4	1993
7	1998
9	2002
10	2003
12	2005
14	2008
15	2009

Also in the LENR Library is a collection of papers prepared by (or with) Andre Lipson during his visit to Dr. Miley, apparently in 2003[20]. The list of the papers, which are electronic files, are shown in Figure 6-2. Another LENR Library item is a poster[21] entitled "Lenuco – 24 hour sunshine". It is hanging on the wall in the FSL (Figure 6-3). The poster does not indicate the conference or event where (or when) it was displayed.

[19] LENR Library. Memo to George Miley from Tom Grimshaw, April 11, 2020.

[20] Andrei Lipson Documents in Miley Files. Memo to George Miley from Tom Grimshaw, January 20, 2020.

[21] LENUCO Poster. Memo to George Miley from Tom Grimshaw, April 13, 2020.

Figure 6-1
LENR Library Bookshelf in Fusion Studies Laboratory Room

Table 6-1
List of Items in LENR Library Bookshelf

15th International Conference on Condensed Matter Nuclear Science, October 2009	
Fusion Facts	Volume 4, Number 7, January 1993
Biological Transmutations and Their Applications	Louis Kervran
Cold Fusion: End of Act I	Research News, 2 June 1989
Cold Fusion Still in State of Confusion	Science, Volume 243, Page 296
Elemental Energy (Cold Fusion)	Issue #22, July 19 87
The Ninth International Conference on Cold Fusion, Abstracts, Beijing, May 2002	
Note Book for ICCF-9	
Handwritten Notes for ICCF-9	
14th International Conference on Condensed Matter Nuclear Science, Proceedings, 10-15 August 2008, Volume 1	
14th International Conference on Condensed Matter Nuclear Science, Proceedings, 10-15 August 2008, Volume 2	
Unlabeled Pink Manila Folder	
Excess Heat	Charles G Beaudette
The Rebirth of Cold Fusion	Stephen Krivit and Nadine Winocur
The Science of the Cold Fusion Phenomenon	Hideo Kozima
Feynman‘s Thesis	Laurie M Bowen
Theoretical Modeling of Cold Fusion	VA Kirkinski and Yu A Novikov, 2002
A Dialogue on Chemically Induced Nuclear Effects	Nate Hoffman
Nuclear Fusion and Transmutation of Isotopes in Biological Systems	Four Russian Authors
Low-Energy Nuclear Reactions and New Energy Technologies Sourcebook, Volume 2	Jan Marwan and Steven Krivit
Metal-Hydrogen Systems, Fundamentals and Applications, 1988, Volume I	Kirchheim et al.
Metal-Hydrogen Systems, Fundamentals and Applications, 1988, Volume II	Kirchheim et al.
Hydrogen Energy Progress VIII, 1990, Volume 1	TN Veziroglu and PK Takahashi
Hydrogen Energy Progress VIII, 1990, Volume 2	TN Veziroglu and PK Takahashi
Hydrogen Energy Progress VIII, 1990, Volume 3	TN Veziroglu and PK Takahashi
Metal Hydrides, 1968	WM Mueller , GP Blackledge, GG Libowitz
Hydrides for Energy Storage, 1977	AF Anderson and AJ Maeland
Cold Fusion – The History of Research in Italy,	S Martelucci, et al.
Cold Fusion – the Scientific Fiasco of the Century	JR Huizinga
Proceedings of the 12th International Conference on Cold Fusion	A Takahashi, et al.
Proceedings of the 10th International Conference on Cold Fusion	P Hagelstein and S Chubb
Hydrogen in Metals VI (Bound Photocopy)	H Wipf
Nuclear Energy Research Initiative, Research Abstracts,	Office of Nuclear Energy, Science and Technology, US Department of Energy
Seventh International Conference on Cold Fusion, 1998, Proceedings	
Confirmation Project for the Net-Energy Output of the Deuterium Crystalline Cell Reactor Technology, Center for Ask Astrophysics at Notre Dame University (CANDU), Proposal to LENR, LLC,	Grant Matthews
Lattice Energy LLC, Bound Document Containing 36 Pages of Confidential and Proprietary Information	GH Miley, Principal Investigator
Fourth International Conference on Cold Fusion, 1993, Lahaina	
Hydrogen Energy Progress V, 1984, Volume 2	TN Vezirogu and JB Taylor
Hydrogen Energy Progress V, 1984, Volume 3	TN Vezirogu and JB Taylor
Hydrogen Energy Progress V, 1984, Volume 4	TN Vezirogu and JB Taylo

Name	Date Modified
▼ 130 Andrei Lipson - Visit Dec 2019	Today at 11:22 AM
▼ Lipson 1	Dec 16, 2019 at 2:20 PM
Andrei LENR memo.doc	Jan 31, 2003 at 3:08 PM
Calorimeter New version, 5 02.doc	May 7, 2002 at 12:34 PM
Electrochemical article.doc	Mar 31, 2003 at 11:53 AM
Electrochemical last.doc	Feb 12, 2003 at 2:57 PM
Letter re article Anomalous Neutron activation.doc	Jan 30, 2003 at 9:07 AM
Letter re manuscript Thermal meas...al of Electroanalytical Chemistry.doc	Feb 17, 2003 at 11:03 AM
Newsletter bubble Article 4 02.doc	Apr 30, 2002 at 2:26 PM
Sample preparation.doc	Feb 27, 2003 at 3:09 PM
SQUID use.doc	Aug 9, 2002 at 3:30 PM
▼ Lipson 2	Today at 11:22 AM
1 23 03 TNS-new.text as of April 30 .doc	Apr 30, 2003 at 11:12 AM
1 23 03 TNS-new.text.doc	Jan 23, 2003 at 11:14 AM
5 7 03 IEEE.Trans.figures.doc	May 7, 2003 at 4:51 PM
5 7 03 RPC-reply.doc	May 7, 2003 at 2:16 PM
5 7 03 TNS-new.text.doc	May 7, 2003 at 4:30 PM
5 28 03 PRC complete manuscript.doc	May 28, 2003 at 5:57 PM
AL List of selected publications.doc	Dec 16, 2002 at 4:15 PM
Andrei -J1 Extension letter.doc	Jun 14, 2005 at 11:04 AM
BioX-ray.doc	Feb 13, 2003 at 11:07 AM
Cover Letter to Rad.Phys. Chem..doc	Jan 23, 2003 at 10:00 AM
IEEE.Trans.figures.doc	Jan 23, 2003 at 10:11 AM
INS letter.doc	Jan 22, 2003 at 11:08 AM
May 11 letter for Andrei.doc	May 14, 2003 at 3:24 PM
PRC revised complete 5 7 03 manuscript.doc	May 7, 2003 at 4:36 PM
RPC-DAB-SG-07(1)-110203 revised 5 7 03.doc	May 7, 2003 at 4:40 PM
RPC-reply.doc	Apr 30, 2003 at 11:12 AM
SP03 letter for Andrei.doc	Jan 22, 2003 at 1:52 PM

Figure 6-2
Files Related to Miley's Work with Andrei Lipson

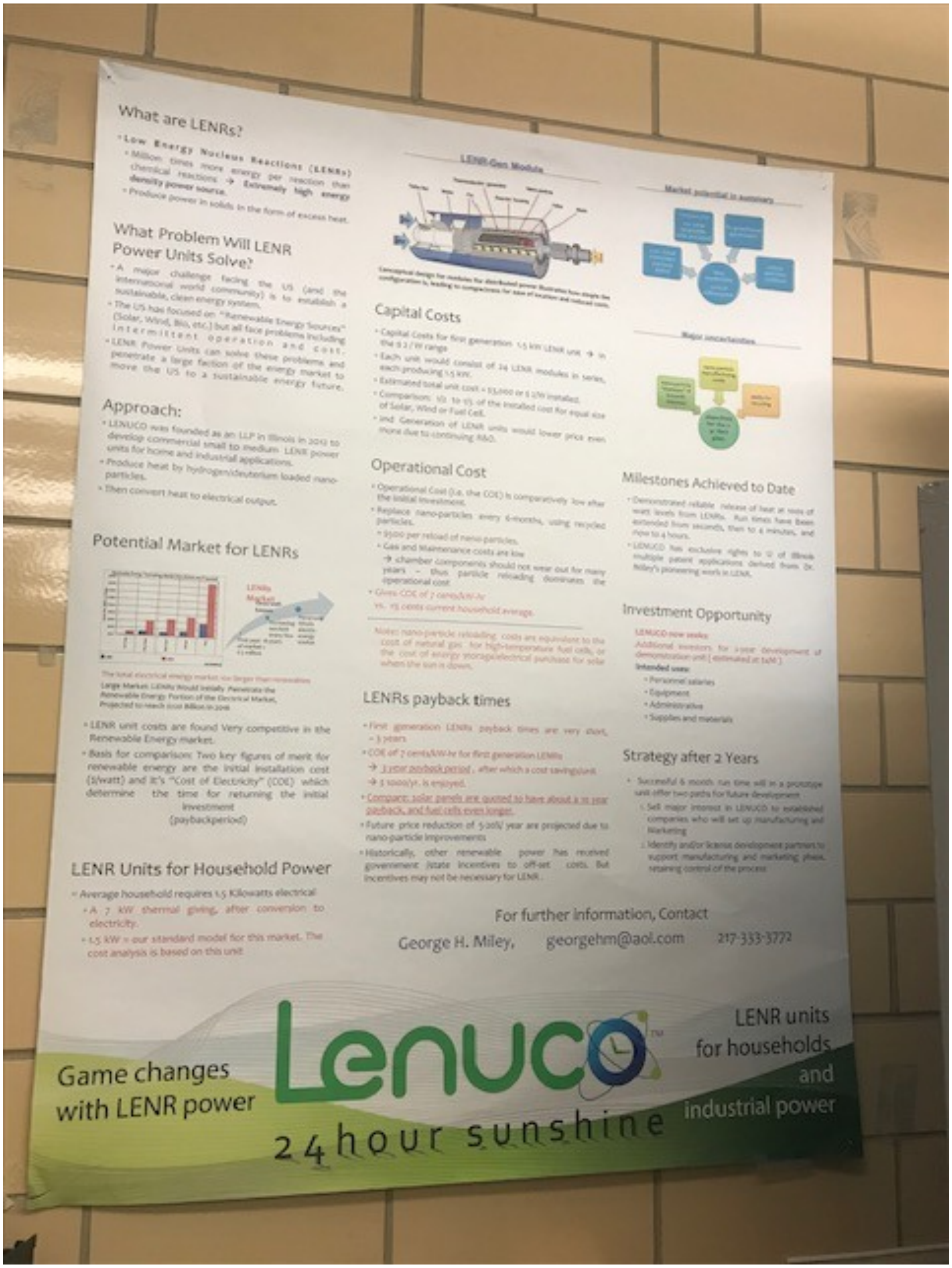

Figure 6-3
Lenuco Poster Located in the Fusion Studies Laboratory

7 Interviews

Interviews of Dr. Miley covering his LENR research were conducted on two occasions. An initial telephone interview took place in three parts on November 13, 2019[22]. A second interview was in four parts and took place on December 17, 2019 during the site visit[23]. It covered primarily his experience working with other researchers in the LENR field.

The interviews were recorded and transcribed by an online service (REV.com) for the MLRDP. They are included in Appendices B and C of this report. Future plans call for the interview transcripts to be reviewed and annotated. The annotations will then be used along with other Project components to develop the research timeline in Section 8.

[22] Transcripts of Interview for the Miley LENR Research Documentation Project. Memo to George Miley from Tom Grimshaw, November 15, 2019.

[23] Transcripts of Second Interview for the Miley LENR Research Documentation Project. Memo to George Miley from Tom Grimshaw, December 20, 2019.

8 LENR Research Phases

As noted in the previous section, Dr. Miley's LENR research phases are planned to be prepared in the future by integrating the interview annotations with Chapter 12 of his book and other components of the MLRDP.

9 Future Opportunities

There are a number of opportunities to extend the MLRDP, starting with annotations of the interview transcripts and preparation of Dr. Miley's research timeline as noted aboe. The lists of publications can be combined into a master list with removal of duplicate entries. Publication lists and copies from other sources such as ResearcGate could be added. The LENR-related hardcopy papers in file cabinets may be obtained and scanned into electronic format.

More information on the experiments and equipment in the FSL and Research Campus labs could also be added. Lab notebooks may also be located in the future. Additional electronic files beyond the CTR and FSL collection may be identified and added. Also, Dr. Miley may have photos or videos that could be included in the Project.

10 Project Methods

The methods used in the MLRDP are based on general LRDI procedures that are modified to meet the specific requirements of individual LENR investigators[24]. The project is being performed according to accepted project management practices[25]. As noted above, the overall LRDI procedure is set forth in a recent article in Infinite Energy[26].

The Project was initiated with a phone call from Dr. Grimshaw to Dr. Miley in January 2019, which included a description of the LRDI and its objectives. Dr. Miley then provided a copy of his professional resume[27], and Dr. Grimshaw obtained a copy of "Life at the Center of the Energy Crisis"[28]. A recorded telephone interview was conducted on November 13, 2019 and was subsequently transcribed. A site visit took place on December 16 and 17, 2019[29]. The accomplishments were summarized as follows[30]:

- Kickoff meeting at EnterpriseWorks. Tour and photographs of lab on second floor. Met Ben Preecher.
- After lunch at a nearby restaurant. Took photos of Miley for potential use in project report.
- Tour of Fusion Studies Lab on campus. Met Eric Ziehm.
- Obtained copies of electronic files from FSL server: Editors (includes Editors 1, 2, and 3) and Editors 4 folders. Copies of two indexes of publications: CTR and FSL. Copies of Andre Lipson files.
- Conversation, lab tour, and photos with Eric Ziehm and his CR-39 studies.
- Inventory of books and magazines on bookshelf in lab room for LENR Library. Also, two drawers of file cabinet.
- Recorded interviews with Miley on collaborations with Andre Lipson, Lew Larson, Heinrich Hora, and two students of Chinese origin.

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

[25] Project Management Institute, 2017. A Guide to the Project Management Body of Knowledge (PMBOK® Guide) — Sixth Edition and Agile Practice Guide (ENGLISH). Project Management Institute. Newtown Square, PA.

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

[27] CV for Miley LENR Research Documentation Project. Memo to George Miley from Tom Grimshaw, January 31, 2019.

[28] Life at the Center of the Energy Crisis Crisis: Chapter 12, Low Energy Nuclear Reactions (LENR). Memo to George Miley from Tom Grimshaw, November 8, 2019.

[29] Proposed Topics for Upcoming Visit. Memo to George Miley from Tom Grimshaw, December 5, 2019.

[30] Summary of Activities during Visit in December, 2019. Memo to George Miley from Tom Grimshaw, January 16, 2020.

- Preliminary inventory and photos of two (?) file cabinet drawers labeled "LENR" in Miley's office. Also, photos of contents of bookshelves in the office.
- Second tour of the FSL lab. Photos of FSL file cabinets and the bench where the electrolytic cells were located.

About 25 memos have been prepared[31] (Table 11-1). A Dropbox folder is being used to store the Project information and the memos and reports (Figure 11-1). The Dropbox subfolders were in general numbered sequentially as materials were located and progress was made. A high-capacity external hard drive is used for periodic backup of the MLRDP and other LRDI project files.

[31] Project Memos and Dropbox Folder. Memo to George Miley from Tom Grimshaw. April 16, 2020.

Table 1l-1.
List of Memos Prepared for the Miley LENR Research Documentation Project

Date	Memo	DBF*
1/31/2019	CV for Miley LENR Research Documentation Project	30
2/28/2019	Miley Papers on LENR-CANR.org	20
9/2/2019	Downloads of Miley Papers on LENR-CANR.org	20
10/12/2019	LENR-Related Items on Your CV	40
11/4/2019	Consolidated List of LENR Publications	50
11/8/2019	Life at the Center of the Energy Crisis: Chapter 12, Low Energy Nuclear Reactions (LENR)	60
11/15/2019	Transcripts of Interview for the Miley LENR Research Documentation Project	100
12/5/2019	Proposed Topics for Upcoming Visit	90
12/20/2019	Transcripts of Second Interview for the Miley LENR Research Documentation Project	80
1/16/2020	Summary of Activities during Visit in December, 2019	90
1/19/2020	Candidate Photo for Draft Report	130
1/20/2020	Andrei Lipson Documents in Miley Files	140
1/21/2020	Electronic Files with "CTR" Designation	150
1/24/2020	Electronic Files with "FSL" Designation	150
2/11/2020	2013 Interview in Infinite Energy Magazine	65
2/12/2020	"LENR Generator Is 'Crowdvoting' Favorite" on Cold Fusion Now!	67
4/8/2020	On-Campus Laboratory and Office	160
4/9/2020	Research Park LENR Laboratory	170
4/10/2020	Hardcopy Files in File Cabinets in Fusion Studies Lab	180
4/11/2020	LENR Library	190
4/12/2020	Erik Ziehm's CR-39 Lab Work and Posters	105
4/13/2020	LENUCO Poster April 13, 2020	200
4/14/2020	FSL Lab Room	165
4/15/2020	Intellectual Property Listings in Google Patent	120
4/16/2020	Project Memos and Dropbox Folder	210

*DBF = Dropbox Folder Designation

- George Miley 190827
  - 10 Reports 190828
  - 20 Miley on LENR-CANR 190827
  - 30 Miley CV 190827
  - 40 Pubs on CV 191027
  - 50 Pubs Needed 191027
  - 60 Miley Book, Etc. 191106
  - 65 Infinite Energy Interview 191106
  - 67 Miley on Cold Fusion Now 200211
  - 70 Miley Articles on Web 191106
  - 80 Interviews 191113
  - 85 Publications 191214
  - 90 Visit Dec 16-17 191205
  - 100 2nd Interview During Site Visit 191220
  - 105 Ziehm CR39 Work 191214
  - 110 Miley on Wikipedia Etc 200118
  - 120 Miley Patents on Google 200118
  - 130 Miley Photos 200119
  - 140 Andrei Lipson - Visit Dec 2019
  - 150 Electronic Files 200211
  - 160 FSL Lab & Office 200408
  - 165 FSL Lab Room 200414
  - 170 Research Park Lab 200408
  - 180 Hardcopy Files 200408
  - 190 LENR Library 200408
  - 200 Lenuco Poster 200412
  - 210 List of Memos & Dropbox
  - 1000 Misc 190827

Figure 11-1
Screenshot of "George Miley" Folder on Dropbox

Appendix A. Chapter 12, Low Energy Nuclear Reactions (LENR), of Dr. Miley's Book

(Note: Photos not included)

Left: I visited Jim "Doc" Patterson (second from right) periodically at his lab in Clean Energy Technology, Inc. (CETI), a company he formed to develop and market practical cold fusion power cells. The CETI lab was located in a one-story metal building (once used for car repair) located near Doc's home in Sarasota, FL. His grandson, Jim Redding, standing by him here, had just graduated from business school at the University of Texas, and was the business and devel- opment manager for CETI. At the time, Larry Forsley was visiting from JFK Company in Washington, DC, and is shown on the left. Right: A number of the LENR meetings have been in Italy. The photo shows Zing Zhang Li, Bill Collis, Vittoria Violante, and me at one. Zing Zhang, Professor of Physics at Tsinghua University, Beijing, China, has been very active in LENR research over the years. Bill, originally from the UK, organized many of these meetings and is the operating officerfor the Society on Condensed Matter Nuclear Science (CMNS) which hosts this series of meetings. Vittoria is a section leader in the Centro Ricerche Energia Frascati, Italy.

I started out doing cold fusion research shortly after the infamous public announce- ment of "fusion in a test tube" by Pons and Fleischmann. However, I changed the terminology describing my work to Low Energy Nuclear Reaction (LENR) research. I felt this was more descriptive of the physics involved in my type of experiment which led to transmutation type reactions rather than D-D fusion sought by Pons and Fleischmann. This was done during discussions with John Bockus, a chaired Professor in Chemistry at Texas A&M. John was an internationally recognized electrochemist who became involved in cold fusion early on. He too had observed effects such as transmutations and in fact held two early meetings on that and other effects at College Station, TX. Those days and work are described in the interesting book Nuclear Transmutation: The Reality of Cold Fusion (T. Mizuno, Infinite Energy Press, 1998). The terminology LENR is now more widely used in the cold fusion community.

(Note: Photos not included)

Left: While not as frequent as the interactions I had with NPL scientists in Russia, I have had a number of interactions with Russians working on LENRs. Yuri Bazhutov (center) is shown here with me and Yasser Shaban. Yuri was the organizer of the annual Soviet confer- ence on cold fusion and ball lightning held at thefamous resort in Sochi-Dagomys on the Black Sea. He spent ha{f a year in the U.S. working with me. During this time, he doggedly insisted that all my results could be explained in terms of "eriwns," unique high energy particles he had discovered in earlier space physics studies. Right: John Fisher, retired physicist from the General Electric Research Laboratory, is shown here at a blackboard explaining his theory for LENR reactions that involves "poly-neutrons." He and Richard Oriani, retired Head of the Corrosion Chemistry Laboratory at the University of Minnesota, visited me in the early 2000s to explain how "poly-neutrons" might explain our LENR transmutation data. Richard was the experimental contributor to this team and they continue to this day to expand on £he concept of poly-neutron reactions in LENR. This seems to tie into some features of transmutation reactions in LENR, but fails to explain other aspects.

Thus, the jury remains out on this, but I have continued to have interesting and productive interactions with both John and Richard.

The dramatic public announcement of "cold fusion" by Stanley Pons and Martin Fleischmann at the University of Utah occurred in March 1989. However, I first became aware of cold fusion a few days earlier when Steve Jones, Professor of Physics at BYU, phoned me. (I did not know it until later, but Jones was a competitor of Pons and Fleischmann. Their concerns about Jones' possible release of his work on the subject before they could disclose their work was an important factor in their decision to go ahead with the high profile premature public disclosure.) Jones and I had both worked on muon catalyzed fusion so we knew each other fairly well. Steve's phone call came just as I was preparing to leave my office for a trip to Japan. He asked me about the possibility of fast publication of an article on "cold fusion" in the journal *Fusion Technology*, for which I was editor. He told me that he was not sure if the topic fit into this American Nuclear Society (ANS) journal and wanted my decision on that. Plus, he was anxious to have rapid publication. I responded that I did not know what cold fusion was, and I had to leave for the flight to Japan immediately. I suggested that he send the article to me and I would start the review process when I returned in a week. I told Steve that if he felt it was an appropriate topic for the journal I was confident that it would be.

When I stepped out of the airplane in Tokyo, my host from the University of TQkyo met me waving a copy of the Wall Street Journal announcing the discovery of cold fusion. Thinking that I knew all about it coming from the U.S. where the discovery was announced my host anxiously asked me, "What is this cold fusion?" I couldn't respond, and thought to myself that it was too bad that I had not been able to take a few minutes to question Steve when he called me. As it turns out, the article Steve was speaking of was never sent to me, but was instead published through Nature. That publication played an interesting role in the controversy that grew between Steve and Pons/Fleischmann as cold fusion itself came under fire worldwide as replication of the experiment in many other labs failed. In a way, I am glad that I did not get to handle the paper – that would have drawn me even further into the controversy than happened otherwise.

The cold fusion announcement caused much excitement in nuclear laboratories around the world. Many raced to replicate the experiment. When I returned to my office from Japan, I again found a group of people waiting for me. This time they were students who wanted to do cold fusion experiments and hoped to get some heavy water (D2O) from a supply that I had for experiments at the University of Illinois' TRIGA reactor. With my help, the students enthusiastically began several electrolysis experiments based on what was known of the Pons and Fleischmann device. They designed the experiment using my copy of the famous fax of an unauthorized copy of the Pons and Fleischmann paper they had prepared for a journal publication. This fax went from laboratory to laboratory around the world by fax machine. (Fax machines were a common means of communication in those days before wide use of the Internet.) The paper was nearly illegible from being

'ransmitted so many times by the time I finally received it from a friend. Like so many others in numerous laboratories, our experiments did not succeed. Further, many rushing to do an experiment, including ourselves, failed to measure the amount of loading achieved in the experiment. It is now well known that success (achievement of "excess" heat) requires a loading ratio (atoms Diatoms Pd) over 0.85. Measurement of this ratio is difficult, typically done by observing the change in electrical conductivity of the electrode. Had the importance of the loading ratio been understood in these early days, many of the "failed" experiments would have been averted. At any rate, these experiences deeply influ- enced my interest in the field, something that I continue to work on today.

As already noted, many workers in the field have adopted the name LENR to replace cold fusion, because many reactions, strictly speaking, are not fusion per se, but instead are nuclear transmuted ions. Indeed, a more general terminology is Condensed Matter Nuclear Science (CMNS). But, turning back to the early days of cold fusion, shortly after the original announcement, I received an invitation to testify before a congressional committee investigating cold fusion in an attempt to determine whether or not a crash development program should be undertaken in the United States. By this time, however, a number of critics had come forth and the topic was emotionally debated in many circles. The congressional staffer who organized the congressional hearing wanted me to serve as an "unbiased" but open minded scientist recognized for innovative energy research. Testimony was placed between that of Martin Fleischmann and Harold Furth (then Director of the Princeton Plasma Physics Laboratory). Furth staunchly opposed cold fusion based on his hearing about failures to reproduce the phenomenon, plus his personal bias due to his long involvement in hot fusion. I served somewhat as a buffer between these two, but even so they waged an intense debate during the question session. Both were Brits and elegant speakers with quick minds, and armed with "cutting" remarks. My testimony was generally overlooked in the midst of all this turmoil. I mainly raised concerns about statements made by both Fleischmann and Furth. Luckily neither bothered to refute my comments, probably because they were more concerned about other politics involved in the situation. We were all exhausted and glad to reach the end of the congressional hearing and head for home. Martin Fleischmann passed away in August 2012 at his home in England. Some newspapers ran stories saying that this signaled the end of an era. But it might be said that the era of LENR that he was so instrumental in starting is just beginning. Before discussing that further, I should mention my involvement as an editor of several important professional journals.

In addition to doing research in the field, I was thrust into the controversy as edi- tor of Fusion Technology. I began to receive papers that were submitted to the journal for publication, but by the time these started arriving opposition to the Pons-Fleischmann experiment had grown in the field. I had to make a decision about how to handle these papers. After much thought, I took the position that if they could get through our normal peer review process (three independent reviews) the paper should be published. A fundamental problem remained – opponents claimed that cold fusion did not exist, so any experiment making claims about the phenomena must be in error, and hence should not be published. In fact, editors of other major journals such as Nature and the various American Physical Society and American Physical Society publications took that attitude. Thus they simply returned any papers on the subject without submitting them for review. I took the view that the purpose of Fusion Technology was to disseminate information, so if reviewers could not find an error in an experiment or theory then the paper should be published. This then would provide serious studies and information to the broad scientific community, especially fusion scientists. As independent scientists they would have to decide what to do with said information (e.g., to ignore it or attempt to incorporate it into their research).

I published an editorial in the journal stating my position. As it turned out, I then became the only editor of a major journal who would accept cold fusion papers, and I received a flood of papers. Researchers in the field looked to me as their "hero." But some members of my editorial board were not happy. I met with this board at the summer and winter annual meetings of ANS. The board was in effect my "boss," so I served with their approval. They could, under extreme conditions, vote me out as editor. Many questions and comments on my policy for cold fusion papers came up at these meetings. For example, some demanded that I have at least two reviewers from the hot fusion community review these papers. I usually included at least one, but I had built up a list of researchers in the cold fusion community and often called on them. My argument was that hot fusion papers were reviewed by people in the field who were experts in the specific area of the paper, and the same procedure should, in my opinion, be applied to cold fusion. That argument satisfied the board for a while, but the subject kept coming up. Thus I started a policy of confidentially disclosing the names of reviewers plus copies of the review to editorial board members requesting this information for a specific cold fusion paper. I received a half dozen such requests and after providing this information about the reviewers the board members ended their objections. However, this issue was very divisive and the board remained split on their view of my editorial policy.

Some years later when I stepped down as editor (due to time requirements, not the cold fusion controversy), the first major change in the journal that was made by successor was to drop the cold fusion papers. Cold fusion researchers were frustrated, and to this day they have not found a "standard" journal as "home" for their papers. A journal has been created by the Condensed Matter Nuclear Science (CMNS) Society, which carries out a peer review process. It has not yet secured an "impact index" like the various professional society journals published by ANS, APS, IEEE, etc., so publications in it are not widely read. Now, in retrospect, I believe my policy on this issue was the proper one, reflecting the responsibilities of a scientific journal. I discussed some of this issue in an invited paper in the Journal on Accountability in Research, which is included in the references for this chapter.

I should add that the cold fusion episode was an important one for the journal and for me as an editor. To put things into perspective, I believe my major accomplishment was creating this journal (fusion papers were previously included in the ANS "sister" journal Nuclear Technology, with Roy Post as the editor) and building up its reputation in the fusion community. In addition to this journal, I am also proud of my role as editor of two major Cambridge Press journals, Lasers and Particle Beams and Plasma Physics. An editor's job is not easy. One must make the journal attractive to the field being served in order to attract outstanding papers. One factor involved in that is to have a reasonably short time between submission and publication. That in turn requires finding reviewers who can provide timely responses, both for the original paper and for a revised version if required. The occasional case where reviewers strongly disagree, such as when one recommends publi- cation and the other recommends rejection, or when the author protests the review as being unfair, can be difficult for the editor. Diplomacy is needed to calm the situation but authority must be exacted to ensure high quality papers. All in all, the review process can be very time consuming and trying for the editor. In addition, journals are always under pressure to breakeven in terms of profit. Thus, the editor becomes involved in helping the publisher find subscribers. Despite the time demands, I found the job to be challenging but fun, and I am glad of my years devoted to being an editor. I feel good about doing this as an impor- tant service for the scientific community.

My approach to my own cold fusion research attempted to take a fresh look at the basic physics rather than simply duplicate the original Pons-Fleischmann experiment. While they used electrolysis to load deuteri um into the palladium, I initially decided to capitalize on my plasma background and use a plasma loading technique. In order to obtain a high deuterium flux impingent on the target, I elected to use a pulsed plasma focus. In addition, discussions with Professor Heinz Hora led us to believe that an improved reaction rate would be achieved by using multi-layer thin film targets. The layers would be selected from metals that could form hydrides and also provide a large Fermi level potential difference at the interface between them. This mismatch in Fermi levels would cause a large local electron density that we reasoned would provide enhanced screening of the deuterons, leading to larger reaction rates. We termed this concept, "The Swimming Electron Layer (SEL) Theory."

To implement the SEL theory, we sputtered alternate layers of sub-micron thickness, nickel, and palladium onto a steel substrate. This appeared to be a promising approach, but unfortunately, the thin films fell off of the substrate after one or two plasma pulses. The combination of plasma heating (and possible nuclear heating) caused the films to delaminate due to their dissimilar coefficients of expansion. I reported these preliminary results at an early International Conference on Cold Fusion (ICCF) meeting in Hawaii. After my talk, Martin Fleischmann came up to me and said he thought I had a good approach, but needed to redesign the thin film electrode. Despite his encouragement, I became discouraged with this approach since the combination of the intense plasma flux and internal heating were disastrous for electrodes. Thus, I began mulling over ways to use such electrodes in an electrolytic cell. My whole view on this changed, however, when I went to the next ICCF meeting in Monte Carlo and met Jim "Doc" Patterson. Many people knew about Jim because his so-called "Patterson Cell" appeared to have the best energy output of any exist- ing cold fusion experiment at that time. He formed a company, CETI, with his grandson, Jim Redding, as the business manager. They had an industrial booth at the meeting, which I happened to walk by. When I saw the electrode configuration of the Patterson Cell in their display, I realized that they were using small millimeter-sized plastic beads coated with metals as the electrode in a pack bed flowing electrolysis system. I caught Jim and said, "Those coated beads used for electrodes look like ICF fusion targets to me. I know a lot about such targets and think I could possibly make an improved version of yours." That "introduction" led to extended discussions, and we became good friends. Subsequently, Jim called me a few weeks later and suggested that he would bring one of his cells to Illinois. He proposed that I run his cell as an independent scientist to confirm his findings. I agreed, and later Jim brought the cell to Illinois and arranged for Dennis Cravens to visit us for a week to show us how to run it. Dennis was a consultant to Jim and was extremely accom- plished at experimental electrolysis. The runs we made during that week were spectacular. The cell was designed to produce kilowatt outputs with only hundreds of watt inputs. The calorimetry for it employed thermocouples on the inlet and outlet of the tubes carrying the electrolyte fluid to the packed bed region. Even without the thermocouples, it was obvious that the cell was performing well due to some visual boiling and distinct spots where some electrode beads began to burn their way into the plastic wall of the vessel. Jim wanted me to develop an independent calorimeter to study this cell. I told him, however, that calorim- etry was so controversial in this field that few would believe it no matter what. Thus I indicated that I would prefer to spend my time on something "more productive" and basic. I went on to propose a study designed to identify the reaction products using the extensive facilities at our Materials Research Laboratory (MRL) to do a quantitative analysis of the elements in the electrodes after operation. Jim agreed.

At that time, it was thought that the reaction products were from fusion reactions. However, Jim's cell used light water (i.e., Hp instead of Dp) for the electrolyte so any fusion reactions would have involved protons (i.e., hydrogen or p-p fusion versus D-D fusion as envisioned in Pons and Fleischmann type electrolytic cells). However the p-p fusion reaction has an extremely small fusion cross section. Thus I could not under- stand how that could occur. I reasoned that the protons must instead react with the host- metal atoms. So, I decided that the first thing to do was to look for nuclear reaction products from p-Pd type nuclear reactions involving transmutations to other elements. The first step was to run a Secondary Ion Mass Spectrometer (SIMS) analysis on the electrodes before and after a run in order to measure and quantify any change in isotopes in the metal layers. This produced amazing results where a whole host of isotopes were detected in the samples following runs that were not present beforehand. Jim, Dennis, and I became extremely excited about this and that led to research on this effect that extended over the next four years. The obvious issue was a challenging one, namely to ensure that the measurements were not being confused by impurities in the system, or by artifacts such as molecular ions, etc., in the analysis. These issues were tackled on several fronts. We started by constructing a cell which used mostly plastic and glass parts to minimize impurities. For analysis I employed a new technique, which I termed "combination" SIMS and Neutron Activation Analysis (NAA). Because NAA involves measurement of distinct gam- mas with energies characteristic of the nuclear energy levels in an atom, confusion due to artifacts like molecular ions is avoided. Also, NAA can be done over a large volume, mak- ing it possible to do analysis of the entire cell and electrolyte to establish impurity levels in the total system before a run. One problem with NAA is that it can only identify ele- ments that have a reasonable neutron reaction cross section. In this study, we were able to use NAA to identify seven key reaction products that had high neutron reaction cross sections and for which we had reference standards. I felt that their identification was essen- tially foolproof. Meanwhile, we used SIMS to roughly fill-in data for the other elements, normalizing the SIMS concentration data to the NAA measurements where that data over- lapped the key NAA elements.

I first publically presented these results at a CETI advisory/consulting board meet- ing in Dallas, TX. One of the attendees, Peter Hagelstein, a leading theorist in cold fusion from MIT, challenged me for the lack of an NAA measurement of isotope ratios (many feel that if transmutation products occur, they should have a non-natural isotope ratio). The SIMS data did provide such information, but still had the uncertainties already noted. Thus NAA would carry the most weight. After discussions, I proposed we do NAA with two elements, copper and silver. The reason for this selection was that these had adequate neutron cross sections for such a measurement. We found a statistically significant devia- tion from natural abundance in the isotopes for both. Later, Peter came to Illinois along with an experimental nuclear physicist from MIT to review the data. After a lengthy dis- cussion of the data, they agreed the transmutation data supporting the non-natural isotope ratios seemed convincing. That vote of confidence was very important to us because these two scientists had a reputation for being unbiased and extremely knowable and thorough. Despite this, Peter's theoretical development still focused on the "classical" D-D reaction experiments. He said the extension to transmutation type reactions would come later. Only now, after many years, has he started that extension.

Once we thought we had the non-natural isotope ratio effect pinned down, Jim Patterson, Gokul Narne (a student who did his MS thesis on this topic), and I published a full paper on these measurements. Gokul had spent many hours at the Materials Research Lab working on this analysis. His complete dedication to the work was a major reason we managed to do so much work on this so quickly. Also, Sheldon Landsburger, a Professor in NPRE, was at the time doing extensive NAA analysis on impurity metal transport in the atmosphere. Fortunately, the elements involved in his studies overlapped many we were concerned with, so his NAA setup was already calibrated against National Institute of Standards and Technology (NIST) standard samples for those elements. He then provided essential equipment and expertise for that part of our analysis. (When the TRIGA reactor at Illinois was shut down several years later, Sheldon moved to the University of Texas where he could continue NAA research plus other studies using their TRIGA. His loss, along with several others doing research at the Illinois TRIGA, was an unfortunate consequence for our department of the reactor shutdown.)

Gokul was an outstanding student and extremely enthusiastic about LENR as a result of his work for his master's thesis. I was confident that he would want to continue on to a PhD on this topic. However he came to my office one day to tell me that his father was quite ill and the family wanted him to return home to India to join his brother in taking over the family businesses. These businesses included a strange mixture of a shrimp factory and a coal-fired power plant. He felt obligated to return in order to help his family. Gokul did say that he would keep track of LENR research and try to do some research on the side. Despite his intent, that never happened. The business responsibilities consumed all of his time. Maybe this was for the best, as there are no jobs available in LENR, although that might be changing with the recent enthusiasm about LENR power units that I describe later.

I reported on these experiments at the ICCF meeting in Hokkaido, Japan. The magazine Infinite Energy traditionally covered ICCF meetings and highlighted the work as a "breakthrough." This publicity brought out all the critics of cold fusion, who were deter- mined to attack the work in any way that they could. That was not unexpected. But, in addition, to my surprise many members of the cold fusion community itself would not accept that such reactions take place.

I was not the first to find such transmutation reactions. Two pioneers in cold fusion, John Bockris and Tadahiko Mizuno, had previously reported evidence for nuclear transmutations. Another researcher, Professor John Dash at Portland State University, was well known in the field of metallurgy and reported finding local regions where transmutations took place. But this earlier work had not received the attention that ours now received. The "hardcore" cold fusion group had ignored those earlier results, convinced that all cold fusion reactions resulted from D-D reactions, leading to Helium-4 (alpha particle) produc- tion along with lattice heating. Now my new results reinforced the older transmutation reports and provided quantitative data about reaction rates. This produced a strange situa- tion. Once the original Pons-Fleischmann experiments came under attack, mainstream scientists in the physics and chemistry communities questioned all cold fusion work, saying that it defied fundamental laws of physics. Cold fusion researchers responded that these scientists did not have an open mind. However, now when something new came along in cold fusion, the cold fusion scientists themselves did not keep an open mind! As a result I could have been drawn into many arguments. My view was that this work stood on its own, and [tried to avoid loss of valuable time by getting caught up in these argu- ments, but at times that was not possible. But rather than debate endlessly, I wanted to continue new research to keep looking for the Black Swan.

The main reactions involved in the transmutation experiments were not, strictly speaking, fusion reactions. Rather many involved standard nuclear reactions or transmuta- tions and some were a fission-type splitting of a heavy compound nucleus. The unusual aspect was that many of the reaction products could only be explained by assuming multi- body reactions, e.g., multiple protons and Pd or Ni reacting as opposed to normal single-body p + Pd type reactions. Thus, as already noted, I adopted the name Low Energy Nuclear Reactions (LENR) for these studies and this terminology eventually gained wide usage in the field. Unfortunately the terminology "low energy" is ambiguous. Actually, what I had in mind was that the low kinetic energy associated with the reacting particles. Thus, unlike cases such as neutron induced fission where a large excess energy is involved, here the compound nucleus formed in the collision has very little excitation energy. Consequently, when it splits up, the reaction products are formed near ground state, so little radioactivity is involved. Thus, remarkably, a LENR reaction only leads to minor radioactivity; e.g., some soft x-rays and beta emissions versus hard gammas and long lived radioactive isotopes. The result then is quite different from fission reactions where energetic neutrons and gamma rays are emitted. In summary, these LENR reactions can be viewed as exhibiting two of the "miracles" cited against cold fusion by early critics: overcoming the Coulombic barrier without using high energy reacting particles, and yielding almost stable reaction products.

LENR, like hot fusion, requires considerable development before practical power sources are possible. Some workers in the field take the view that if LENR is developed, hot fusion will not be needed. That view puts the two into direct competition for the same R&D money, and pits cold and hot fusion scientists against each other. I personally view LENRs as an alternate approach to nuclear energy. However, I have always felt that both hot and cold fusion, once developed, will assume extremely important niches in the future energy economy. Perhaps cold fusion or LENR cells will provide small distributed power units while hot fusion will provide large centralized power stations. Thus both deserve vigorous research support.

Indeed, it appeared that Jim Patterson found his Black Swan with his Patterson Power Cell. Others agreed, and he was featured on the Good Morning America television show. However, several events occurred later that set Jim's work back. One was that Jim soon ran out of the batch of his electro-plated beads that worked so well. He had kept the production method in his head, and when he tried to reproduce it he was not able to make another batch that worked as well. This seems to be the fate of many cold fusion experiments – the experimentalist does not fully understand the key parameters that must be controlled, causing non-reproducible results.

I had been slowly working on an improved sputtered coating on the bead-type elec- trode, as I told Jim that I would do when we first met. My students and I looked at some of Jim's original electrodes under a scanning electron microscope and were struck by the disordered structures with cracking and defects. Intuitively that seemed to be bad for high levels of hydrogen loading in the metal coating. Thus I reasoned a "smoother" coating would enhance the cold fusion reactions. To achieve that, I envisioned using a sputtering technique to create smooth layers with good structure. This could be done by clamping single particles in a target holder in the sputtering chamber. However, we needed to make a thousand bead-type electrodes to go into one electrolytic cell. Thus, I eventually devel- oped a "popcorn" shaker system that would keep the plastic beads suspended while sput- tering onto them. The main worry with this approach was that collisions between beads would harm the coatings. We found that the harm would be minimal if the shaking fre- quency was properly selected. This process took about a year and a half to develop, but we ended up producing beautiful coatings (Mike Williams, who did much of work on the shaker unit, and I obtained a patent on the process.) Unfortunately, when these beads were used in the Patterson Cell, instead of increasing the performance, they caused a significant deterioration in heat production and transmutations. I was very frustrated. More recently I rethought this and decided that the sputtered electrodes eliminated the local defects in the films, especially at interfaces, where hydrogen or deuterium atoms could "cluster" together to create reaction sites. This is one of the factors that lead us to the deuterium cluster type LENR we are currently studying as described later in this chapter.

During the time when Jim Patterson was working on duplicating his original beads, his grandson, Jim Redding, suffered a fatal heart attack while exercising. This was a dev- astating event for Jim. His grandson primarily worked on the business side of CETI. But, in addition, he was Jim's constant companion and was also involved in many technical discussions and decisions. Jim Patterson never quite overcame the grief of losing his grandson and fellow worker. While he continued work in his lab, it was never the same after that. A few years later, in 2008, Jim Patterson too passed away. This family team in CETI spanning two generations was an unusual and memorable part of the turbulent his- tory of cold fusion.

My transmutation results caught the eye of Lewis Larsen, who called me to intro- duce himself, saying that he heard of this work. Lewis lives in Chicago and proposed a visit to discuss the possible development of a company to create and commercialize these cells. Lewis has a degree in biology but was largely engaged in financial management and stock trading in Chicago. He was very quick in grasping a wide range of topics and had a real knack for making contacts with important people. After several meetings, Lewis set up a company, Lattice Energy, LLC, with me as the Chief Scientist and himself as CEO. Money was raised to support an aggressive program to develop a heat producing cell based on my thin film electrode design. A patent issued to me on the subject was the first major intellectual property owned by Lattice Energy. This association and support lasted several years until Lewis and I found we could not agree on plans and views. Basically, Lewis became convinced that a commercial unit was possible in a much shorter time frame than I did. Also, I favored more open publications than Lewis. We parted on agreeable terms and remain friends, though we do not meet often. Lewis continues to push LENR, support- ing several other projects through Lattice Energy, LLC. Most importantly, recently Lewis and Allan Widom, a physicist at Northeastern University, published a series of theoretical papers on LENR called the "Widom-Larsen Theory." This theory, involving formation of a heavy electron, which leads to low energy neutrons, has been quite controversial in the community. However, one interesting fact is that it does predict the array of transmutation products I observed in the Patterson Cell experiments reasonably well. In this theory, the low energy neutrons create the transmutation reactions so issues of how charged ions could overcome the Coulombic barrier are circumvented. Scientists at NASA Langley are attempting some basic experiments to verify this theory, but no results have been released yet. In any case, I proudly add Lewis to the list of people I have introduced to the field of LENR to the extent that they have become active in it.

I wrote this book in sections. One area in the LENR discussion that I put off in order to pay particular attention to it was my lengthy association with Russian scientist Andrei Lipson. But events overtook me just as I finally started that section, Andrei unexpectedly died in late 2010, suffering a heart attack while riding a subway train in Moscow. He was only 50, and I had just seen him several weeks earlier at the ICCF-15 meeting in Rome. He appeared vigorous at the meeting, giving talks and entering many side discussions. He seemed to be in good physical condition except for his continued smoking. His death was most unexpected and tragic. There were many e-mails on the Condensed Matter Nuclear Science (CMNS) server from colleagues around the world expressing their shock and sympathy. Andrei was a well-liked and highly respected member of the LENR research community. He left behind his wife and daughter. Indeed, his daughter, Maria, attended the University of Illinois studying art history. She ended up at Illinois as the result of the family accompanying Andrei to Illinois over a decade ago, when he first came to collaborate with me. Although Andrei and his wife returned to Moscow several years ago, Maria remained in Illinois to continue her studies. The loss of her father has been hard on Maria. He had wanted Maria to go on to graduate school, but lack of financial support after Andrei's death made that questionable. Somewhat by luck, I learned of a student job opening during clerical work in the Aero Department and recommended Maria for the position.

(Note: Photo not included)

Andrei Lipson (second from left) with col- leagues Carlos Sanchez (on the left) and Tadahiko Mizuno and Xing Zhong Li (to the right of Andrei) at the ICCF 15 conference in Rome shortly before his death. As dis- cussed in the text, Andrei Lipson was from the Institute (f Physical Chemistry and Electrochemistry at the Russian Academy of Sciences, and he spent four years working with me. (Photo by David Nagel)

She got the job, and it enabled her to complete her senior year. Then, after applying for graduate work, Maria fortunately obtained one of only two TA positions available in her department. Liz and I recently attended her graduation ceremony. She was one of four students in Art History to obtain a MS. Maria plans to go on to a PhD and hopes to become a curator in a major museum. Andrei would have been so proud of her!

Andrei was a great colleague who had a significant influence on my LENR research. It seems like only yesterday that I first met him at an ICCF meeting in Europe. He had just returned from a stay in Japan where he co11aborated with Professor Kosagi on low-energy nuclear cross sections using ion beam-target experiments. I approached Andrei after his talk and asked if he would consider coming to Illinois to join my work, assuming I could raise enough money. He consented. Later, in e-mails, I asked if he "believed" in cold fusion. He replied that he thought so but wanted to keep an open mind because the experiments would determine the truth. I told him that I shared that view. From that moment on we usually agreed on overall goals, though we differed in a professional way on some details. Over his four year stay with me, punctuated by periodic visits back to his lab in Moscow, Andrei and I formed a very productive team. Not only did we do continued experiments on thin-film electrode concepts, we successfully applied some analysis techniques that Andrei used in Russia, such as nuclear particle detection using CR-39 film. We also developed in collaboration with others at Illinois a pioneering method for creating near metallic density hydrogen (or deuterium) states in dislocation loops in palladium. The resulting *Physics Review* article about the superconducting properties of this state has received a number of citations.

This work with Andrei culminated in my most recent LENR power cell concepts based on deuterium "cluster" formation and reactions. We were working on this concept when lack of support forced Andrei to return to Moscow. He continued to collaborate with us on cluster research working from his lab in Moscow. Ironically, several weeks after his death, we received a package from him postmarked the day he died. It contained a new cluster type electrode he formulated for us to study. He had mailed it on the morning before his fatal heart attack! In addition to new experiments at Illinois, Andrei brought with him a wealth of data from experiments in Moscow that had only been written up as obscure lab reports in Russian. I helped Andrei provide added theoretical understanding for select experiments (ones that seemed to have the highest scientific importance) and worked to develop an English version of the Russian reports. Over six publications in technical journals resulted in this manner. Subjects ranged from pyrolytic fusion neutron production to LENR transmutation studies. One old publication by Andrei about "bubble fusion" was already in English, but was often overlooked. Based on that report, it appears that he and his group in Moscow were the first to achieve neutron production using the cavitation technique. Thus, when the article in Science by Rusi Taleyarkhan from ORNL came out about bubble fusion without mentioning Andrei's early work, Andrei was very upset. I pro- posed that he submit a letter to the editor, which he ultimately did. However, apparently due to the growing controversy over the original article, Andrei's letter was not printed.

There is not enough space to tell all I want to about Andrei's accomplishments and our close relationship. I grew to deeply respect him as a person and as a scientist. The interested reader can find out more about him from the article by his daughter, Maria, in *Infinite Energy* magazine following his death. Also, I spoke about some of his recent research at the March 2010 American Chemical Society meeting in San Francisco, CA. Andrei had prepared a presentation on the effect of electron beam bombardment on loaded hydrides for that meeting. I attended the meeting and made the oral presentation that had been scheduled for Andrei. He had sent a draft PowerPoint presentation to me for comment just before his death. Thus I was able to use many of his own slides for that talk. It was very sad that he was not with us to present it.

Another person who has had a very strong influence on my LENR work is Charles Entenmann. He retired from his family bakery business and now lives in Florida. Somewhat by accident he met Jim Patterson and became a collaborator in the CETI cold fusion work. Jim introduced me to Charles, who became a friend and strong supporter of my LENR research. He, along with his son-in-law, Alf Thompson, accompanied Jim for several visits to Illinois to discuss LENR with me. We also got together in Florida during meetings at CETI. Alf was particularly interested in transmutations and was collaborating on an experiment when, in early 2011, he died unexpectedly from a stroke. This was a blow to all of us. Charles and I recently wrote an article for the *New Energy Times* in Alf 's memory.

At this point, like some others, the reader may find it unusual that three relatively young people in LENR (Jim Redding, Andrei Lipson, and Alf Thompson) died over such a short timespan. Possible causes including strange radiation from LENR cells to a conspiracy of some type have been discussed by persons on the Internet. Personally I feel this is just an unrelated coincidence and that there is no relation between these unfortunate deaths. I hope to provide a counterexample.

Xiaoling Yang, a post-doc with me, has played a leading role in our present deute- rium cluster LENR research. She and a group of four enthusiastic undergrads have done most of our LENR experiments over the last two years. Xiaoling's post-doc position was to end in May 2013, but she obtained an excellent full-time position in a biochem company on the East Coast and left for that in December 2012. Xiaoling is hard to replace. Heinz Hora continues to provide valuable support in the theory of these unique clusters. Nie Luo also makes important contributions to the work, but is limited in time due to his other projects. Charles Entenmann continues to participate in email and phone discussions. An interesting and important aspect of the work is the connection to ICF fusion. We have proposed using the same cluster type foils used in LENR cells for a convertor foil to create energetic deuteron beams for fast ignition of ICF targets. Fast ignition involves hitting the compressed target with a burst of focused energy from a laser or ion beam to ignite a hot spot (small region of burning fusion fuel embedded in the larger volume of fuel) to achieve a target burn with minimum energy input. We have been collaborating with Kirk Flippo at LANL to study this possibility experimentally using the TRIDENT Petawatt laser facility. Kirk and others at LANL have been studying use of energetic proton beams for this pur- pose. Deuterium is similar, but has the advantage of providing added fusion by interacting with the D-T target to create added fusion reactions while it simultaneously creates heat- ing of the hot spot. Before our cluster-type foils, there had not been a good way to make the convertor foil that the laser hits to drive out and accelerate the deuterium ions. Xiaoling took several "cluster" foils to LANL for preliminary experiments which turned out to be encouraging. Thus, it looks like clusters could provide a scientific "tie" between hot and cold fusion!

Shortly after I finished the initial draft for this chapter, the LENR situation changed drastically. Andrea Rossi in Italy announced that he had developed a commercial scale 1-MW unit which could be purchased, and announced dates and times for upcoming dem- onstrations of its operation. This announcement resulted in much excitement; however the excitement was tempered by debates about whether or not Rossi's work could be taken seriously. These debates were intensified by Rossi's refusal to release full details about the unit, and the restricted viewing arranged for the demonstrations. Rossi defended his stance, saying that as a private company he needed to maintain proprietary information. However, he did disclose that the unit was based on a hydrogen/nickel reaction using nickel nano- particles along with a secret "catalyst." Other information slowly came out, including the observation that nuclear transmutations were occurring in the metallic nanoparticles, resulting in production of significant amounts of certain elements in the nickel such as copper after extended runs. There is not space here to provide more information about the strange events that occurred and continue to occur around Rossi's activities. Much of that story can be easily found from an online search.

When I first heard about Rossi's work, my initial thought was that this reminded me in many ways of the Patterson Cell. Patterson too claimed high power outputs, but maintained secrecy, including restricted demonstrations due to his company CETI's plan to commercial the cell. That also immediately led to controversy about his work. Jim used light water and nickel in the Patterson Cell, so one would think that the reaction involved would be quite similar to the Rossi hydrogen gas-loaded nickel cell. Further, when I stud- ied the reaction products from a Patterson Cell, as already noted, I found that nuclear transmutations in the nickel produced a variety of elements, including copper. Rossi too reported finding copper in his electrode after a run. Thus I began to think that I should get back into this area to take advantage of my considerable previous experience with such systems. In addition, I had always been impressed with the earlier work on gas loaded nanoparticles done in Japan by scientists such as Arata and Takahashi. These experiments also produced very interesting results, but at lower power levels. I did not worry about power levels, because looking at it from a scientific point of view, all the basics can be learned from low-power laboratory experiments. But outside the scientific community, low powers do not attract much attention, whereas discussion of a commercial MW unit does! Thus at the next meeting I went to where LENR was discussed, speakers and the audience focused so much on the Rossi work and speculations about it that other good scientific work received little attention.

At any rate, I set up a small gas loaded nanoparticle experiment in my lab, and assigned Xiaoling Yang and several undergraduates to work on it. Xiaoling had a great background for these experiments. She was already working on hydrogen/deuterium clus- ters in thin foils, and this could easily transition into nanoparticles. Xiaoling and I reviewed our previous thin film work with light water and nickel/palladium films as a basis for select- ing the composition of the nanoparticles. We were able to obtain the desired alloys, and milled the block of alloy into nanoparticles using facilities at the Materials Research Lab at the University of Illinois. The experiments went rapidly and within months we had some exciting positive results. I reported these results at several meetings with sessions on green energy, and also spoke at an invitation-only meeting set up at NASA Glenn Laboratory in Cleveland. At this time Rossi's work was still under debate. It created such excitement that several companies and also NASA scientists wanted to obtain a Rossi-type cell or obtain detailed information about operation of such a unit. Simply put, they were not sure that this was going to turn out to be practical, but did not dare ignore it, because a LENR reactor with large output energy and negligible radioactivity would be a game-changing power source. Rossi, however, turned out to be very difficult to work with, and refused to sell small demonstration cells for this purpose. Meanwhile, although I thought we were late getting into the game, the results from our experiments seemed to be the best to date in the United States (some others may be keeping their work secret, so the exact status is hard to know). Anyway, this resulted in a number of individuals and companies contacting me to either obtain access to our work or with offers to collaborate in formation of a company to commercialize it.

As I composed this update on LENR, I had just completed negotiations to set up a new company which would be a joint venture for NPL Associates, Inc., called LENUCO LLC. If angel or venture capitalist funding can be obtained we plan to move the LENR work into a laboratory in the University's research park. One major technical challenge remains that I feel we must solve before moving our university research to LENUCO. Currently run times are limited by "sintering" of the hot nanoparticles. To be of comercial interest, run times lasting many months before replacing the nanoparticles seems essen- tial. We are currently examining some techniques to accomplish that, and hopefully we will be successful soon. The objective is to move our technology ahead as rapidly as pos- sible, so that it could be commercialized before too many competitors catch up. Everyone involved realizes that this is a risky endeavor, because there are still many issues to be resolved before we can be sure that a competitive product is possible. Still, the possibility that this is likely to become a game-changing power technology is enough to excite and drive us all ahead.

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Appendix B. First Interview, November 13, 2019

B1. First Interview, Part 1

Tom Grimshaw:	Okay. George, we're on the air getting recorded. I'll start by saying that this is Tom Grimshaw having a conversation and interview with George Miley. George is at the University of Illinois. I think you are an Emeritus professor there, George.
George Miley:	Correct.
Tom Grimshaw:	What we're doing is we're having an interview or conversation about George's adventures in cold fusion. He was one of the principle investigators of that field, going back to when cold fusion was announced on March 23rd, 1989. The purpose, as I say, is to get an interview record of George's activities in cold fusion. This is a part of the Miley, L-E-N-R, LENR Research Documentation Project.
Tom Grimshaw:	It's a project as part of an initiative that I'm leading for various cold fusion researchers. LENR is Low Energy Nuclear Reactions, another term for cold fusion, which we'll talk about later George because I think you may have been the one that first came up with that idea or that term.
Tom Grimshaw:	I think without further ado George, why don't you start by telling us how you heard about the cold fusion announcement in 1989 and then maybe follow that up with what you were doing at the time and then when the announcement occurred and then what was your response to the announcement and what were the activities? Take it away George.
George Miley:	Okay, Tom. Well, it's very interesting actually. I learned of this before it actually was announced in the following sense. I had worked some with Steven Jones at BYU who was definitely involved in the initial episode. I think it was his work that prompted the premature announcement of cold fusion, I think by Martin Fleischmann. Anyway, I had worked some with him on muon-catalyzed cold fusion and not cold fusion. Muon-catalyzed fusion.
George Miley:	I was in the office one day and I was editor of Fusion Technology A&S Journal on the subject and he called me and said he had a paper he wanted to submit, which was concerned with cold fusion. I said to him, "What do you mean cold fusion?" He said well, he wasn't sure that this was the proper subject matter for the journal and he wanted to make sure it was and that it could be published quickly.
George Miley:	I told him, well unfortunately I had to leave in a couple minutes. I had to get out to the airport. I was going to fly to Japan, to Tokyo, to give a lecture at the university there. I said, "I don't have really time to discuss all that with you in detail." I said, "If you think it's appropriate, I'd certainly follow your judgement and I'll have it reviewed as quickly as possible."

George Miley: I got on a plane and left. When I landed in Tokyo, my hosts came out to meet me at the airport and they were waving this paper for the announcement of cold fusion and the fusion [inaudible 00:04:55] test tube, et cetera. They kept saying, "Well, what is this? Can you explain it to me?" I thought, "Oh my gosh, if I had only taken a little bit more time to talk to Steve, I could have found out what it was." Anyway. Sorry. There is another phone here.

Tom Grimshaw: Now George, I wanted to jump in and say that if you need to take that call we can put this on hold and I can call you back.

George Miley: No, I'll keep going. My wife's getting a call.

Tom Grimshaw: Okay.

George Miley: Yeah. Anyway, so that was my introduction to it. By the time I got back to the university, I discovered I was very popular. In fact, there were three or four students that weren't working for me, but working with other people that were at my office door waiting to see me to see if they could get heavy water because I happen to have a lot of heavy water in a drum that was used for other experiments and they wanted to borrow some of that and yhis, that, and the other. There was all this excitement going on.

George Miley: I didn't jump into this at the time because I really didn't understand it that well, even though I got a copy of the paper, which had been faxed around the world I think. In those days, fax was the way it was transmitted. But by the time I got it, I couldn't read the paper in any detail because it had been faxed so many different times, it was very blurry.

George Miley: Anyway, as time went on, people were working on this. I thought, well, I'd like to try it a different way. It depends on getting something loaded and I have a plasma focus device, which could load deuterium into metals just by plasma implantation into the metal. I decided to do some experiments on that. I hadn't done anything yet.

George Miley: Meanwhile, I had a call from one of the Congressmen from Illinois who we said that there was a hearing on this subject and I'd been nominated to participate in the hearing as an unbiased, but innovative type person.

George Miley: Anyway, he talked me into it. I went to the first congressional hearing and this was really a heated debate because by this time, a number of labs were claiming they couldn't reproduce the experiment and people in the hot fusion area were upset and saying that there were problems. Certainly people would be dead due to neutrons if this really worked and all sorts of things.

George Miley: I had my presentation or inquiry before Congress came late at night and I was really getting exhausted because this hearing had been going on. It was a very heated room. Steve Jones incidentally had already testified saying that he had observed tritium production. He was doing studies of tritium coming from volcanoes and so on. But he didn't believe that cold fusion could proceed anywhere near to reaction rates that Fleishmann and Pons were claiming.

George Miley: Anyway, I was sandwiched in between Fleishmann and the then director of the hot fusion lab at Princeton. I'm trying to think of his name. Anyway, those two guys were at each other's throats and so on. They are very clever speakers and very competent scientists. That was a little unpleasant to be between them, but I guess I was to try to throw all that down.

George Miley: I didn't quite know what all to say, but I did make a few statements like, "This is really interesting. You say that there's no problems, but it seems to me that if this reaction was occurring and neutrons are observed from deuterium, if we used deuterium and tritiium, one might get quite an interesting neutron source of DT neutrons at 14 MEV and this could be used to do various things."

George Miley: Such as I had been studying uses for neutron activation analysis to study impurities in various metals and so on using neutron activation with 14 MEV neutrons from DT reactions. But these were not produced by cold fusion. They were produced otherwise. I said all that and after my testimony, I had to go to the men's room, which I managed to squeeze out of the crowded hearing room to get to.

George Miley: When I got to the restroom, I was approached by a guy who said that he was from the CIA and he wanted to remind me that a new source of nuclear energy was classified when it was discovered and that I wasn't to say things like I said. He wanted me to stop talking about that type of thing.

George Miley: Since I had security clearance at the Lawrence Livermore Laboratory where I consulted frequently, they would yank that from me if I continued to talk along these lines. That was a side light, which certainly got my attention. Anyway, I went back home, I did do some of these plasma focused type of loading experiments.

George Miley: I reported on that and maybe it was the third international conference on cold fusion, but that was a very difficult way of trying to load and it's hard to tell if you were getting excess power. The instrumentation was hard because the plasma produced so much heat. Otherwise, so calorimetry was a real questionable thing. But I was still going to the international meetings that were occurring and I guess one of the next things that changed ...

Tom Grimshaw: If I may jump in here, so I was a little bit distracted right when you were talking about the first experiments that you said. Unfortunately, somebody rang the doorbell here, but I want to back up to that. You said it was a plasma approach, not electrolytic and you reported on it probably at ICC03. Did you believe you actually saw the effect or were you not sure or where did that come up?

George Miley: It wasn't positive because as I say, it's very hard to do calorimetry on that and of course, we were really looking for heat production. I don't know. Other people, including ourselves, were still studying various ways of plasma loading. I'll come back to that maybe when we get to discuss some of the present work I'm doing.

Tom Grimshaw: Okay. All right. Well, I just wanted to clarify that because it'll be important as we go along and particularly when we get to the end, I'd like your bottom line assessment as to whether this effect is real and if so, what's the best way to achieve it and then we'll close with your ideas on where do we go from here? But I don't want to get ahead of the story. Taking it sequentially then, after you did the plasma work and reported it, what came next?

George Miley: Okay. I don't remember the exact year, but it was a couple of years after that, I was at a meeting. In fact, this meeting was in Monte-Carlo. We had a meeting there. I don't remember which number it was. I happened to run into a display that Jim Patterson had there. He and his grandson had founded his company, CETI, and they were claiming great results with a ... Well, it's more or less a pebble bed type electrolytic unit.

Tom Grimshaw: I'll jump in just for clarification. CETI is C-E-T-I. I think the grandson's name was Reding, wasn't it?

George Miley: Yeah, Jim Reding.

Tom Grimshaw: Okay, go ahead.

George Miley: Anyway, I was looking at that and the way he was doing this was he had plastic beads, which he had developed a process for when he was working for, I think it was Monsanto Chemical, and actually these little beads were so well produced and they were used in medical ... Instead of talcum powder, they were used as a way of putting medical gloves on people's hands.

George Miley: You'd use that instead of talcum powder to get the gloves on easily and out, but the use of the beads made it possible to sterilize and so this was a patent and a process that he had done. He was using that basis, the formation of these beads to make something where he would coat the bead with say, a copper and then with palladium or maybe a layer of nickel or something and that would be the electrode in this pack bed electrolytic unit that he was working on.

George Miley: I saw this bead and I said, "Well Jim, that looks like inertial confinement fusion." In other words, laser fusion target. I actually had been making that type of thing, so I know how to make them too. We got into this discussion. Anyway, that got his attention and later he talked to me when we got back about collaborating and he said he would send me some beads and a unit to work on and he would like for me to verify that he was getting excess heat from it, which he did do.

George Miley: I thought about all this. Calorimetry is something in this field that has been so hotly debated. It seems like people will do something and they'll claim to have gotten excess heat and then someone else will say, "All your calorimetry is wrong. Everything's wrong. The thermal couples are not in the center of the flow," this, that, and the other. I said, "Well look, I don't want to get in the middle of that type of thing." But the real question is, if a reaction's occurring to produce the excess heat, what is the reaction?

George Miley: At this time, he was using light water and nickel coatings in these experiments or at least in some of the ones he was doing and claiming that that worked. Now, that couldn't be a DD reaction such as Pons and Fleishmann were talking about. I said, "Well, what I'd like to do is find out if it happens to be a nuclear reaction, it must be nuclear products."

George Miley:	We ran this unit they sent us and took the beads, took the sample of the beads, [inaudible 00:19:43] over to our materials research lab where I did SIMS, Secondary Ion Mass Spectrometry, work on them to find out what had changed. I was quite amazed to discover that there were new products produced that had not been present in the original metal in the beads. For example, in nickel, you'd see a whole list of new isotope that were produced by nuclear reactions.
George Miley:	Now, when Jim and I were talking about this, we realized that the debate about, well, were these coming from the other ... From the wall, there's metal someplace else, impurities. Are you sure that this is a nuclear reaction? We worked hard and actually redesigned the experimental setup, so that it had all plastics and so on and except for the other electrode and that was pure ... We used a pure platinum electrode.
George Miley:	Anyway, got rid of all other possible impurities and after a lot of work on that, using a variety of different techniques to try to verify what was going on, namely in addition to SIMS, as I already mentioned, we did neutron activation analysis to study the write ups. One time we did a plasma mass spec type analysis.
George Miley:	Anyway, gathered a lot of data on that. I think the results, which I presented at the ICCF meeting, this was a meeting in Japan, I think in Hokkaido. I don't remember the number of it. But anyway, that's one of the things that I've done that has gained considerable attention and some criticism by people who were claiming that I was wrong or something. But anyway, I'm very proud of that result.
Tom Grimshaw:	Okay. I'm going to ask to jump in again here. Just to summarize what you said, you were initially working with Jim Patterson and his beads configuration, but you were using calorimetry looking for excess heat and then subsequently, you think you observed the occurrence of metals that hadn't been there before, which would imply transmutation.
Tom Grimshaw:	Then you did a lot of work to get rid of the other sources besides the reaction, potential sources for what you were observing with these metals that hadn't been there before. I have to again ask a bottom line question, did you feel that you saw the effect in the beads with the calorimetry or with the transmutation signature?
George Miley:	Yeah. The answer is yes. I feel that the measured transmutation, which I reported, fit well with the calorimetry that we were able to study. Now, the calorimetry is very difficult, but he was producing ... This pack bed unit he had was producing a lot of heat. Like up to a kilowatt in some cases. That was enough that you could have a pretty big error, but still be confident that you would have excess heat.
George Miley:	Anyway, you could account for all that roughly plus or minus 20% error, by the fact that the reactions that produced products all had exothermic energy. In other words, they produced energy in addition to the products, heat. Yes, is the answer.
Tom Grimshaw:	Okay. Very good. What happened after that? You reported the results, I guess on both bases, I'll call them, or stages of your work with Patterson and his beads. The electrolytic ... I mean, where you're using calorimetry and then using the transmutation. You reported that at the ICCF in Hokkaido and I don't remember the number. I don't have that memorized. It's easy to tell them just from where

they were. What happened after that? Did you continue to work with the beads or what happened next?

George Miley: Well ancillary, I also presented this work shortly thereafter at a meeting here in Texas that John Bockris had arranged. John Bockris had done pioneering work in a lot of these things and Mizuno from Japan had worked with him as a postdoc and they had done some ... He later said that he had done some transmutation experiments and I think was implying that he had done them ahead of what I had reported. Of course not using these pellets.

George Miley: But anyway, I became good friends with John and also met Mizuno at this meeting and we were all talking about the act that transmutations could also be a part of cold fusion. It didn't have to be a DD reaction as was originally claimed. That's [inaudible 00:26:25].

George Miley: Jumping back, I'm trying to think. Maybe the next thing that happened though was I had told Jim Patterson that I could make these beads also using techniques that I had developed for [inaudible 00:26:52] fusion. I took some of his beads over to our materials research lab, looked at them under a scanning electron microscope, and they looked like the surface of Mars or something. Terrible surfaces. It had all sorts of defects and this, that, and the other.

George Miley: I did spend a good deal of time and developed a way of elevating the beads and in a popcorn like device. You elevate them and then use a sputtering system to sputter, that's coating onto them while they are elevated in the air flowing up. It keeps them free from the surface, so you can get a smooth coating all the way around. That was quite difficult. In fact, we do have a patent on that, although it's probably expired now. I don't think anyone else has ever tried to do this.

George Miley: But when we tried to use those beads, they didn't work very well at all. If there was excess heat, it was very little and so it turns out in my mind that you need these defect type surfaces and so on to cause the reactions to occur. The question is, how does that all fit together? What I decided was that I would switch and went back to caliormetry. Not calorimetry. I went back to electrolysis for loading and so on.

George Miley: The bead and the Patterson type cell are difficult to do and incidentally, Jim Patterson then tried to make more beads. He ran out of the original batch and the ones he made didn't work. He had trouble reproducing his own experiment. I think many people in this field have had that trouble. Some things work and then you try to keep going forward and something happens and one step forward, two steps backwards. You aren't able to reproduce what you did to begin with and he was caught in this situation.

Tom Grimshaw: Yeah, it seems like it's almost a fixture of this field. You think you see the effects and try to reproduce it with almost identical conditions or as close as you can get and it doesn't reproduce. Like I say, that's very common in the field. I want to just review again. I'll do this periodically. Did I understand that you made your own beads using your process and then coated the beads yourself?

George Miley: Correct.

Tom Grimshaw: Okay. All right. I'm sure this is available in the papers and so forth, but can you say what was the material that Patterson was using for the beads that were coated and for your own beads?

George Miley: Yeah. Well, he had the plastic ... There's a certain type of plastic that had properties that are for the bead and then he had to put some type of copper flashing or something on it to get good contact between it and the metal. Normally he would put nickel, say on the outside. He has done it with palladium also. Now, I was just reproducing the nickel ones. Now with my process, I didn't have to put the copper flashing on. I directly coated and sputtered nickel onto it.

Tom Grimshaw: Okay.

George Miley: But all that was for naught. It didn't work well.

Tom Grimshaw: Right.

George Miley: We probably got some excess heat, but if he got a kilowatt, we got 10 watts or something. I mean, it was a drastic difference.

Tom Grimshaw: Yep. At that point, you threw up your hands. Maybe that's the wrong term to use, but you decided against further research with the beads. You went back to other ways of achieving the effect besides the beads.

George Miley: Correct. Well, the problem with the beads and all that is, well first of all, I couldn't make the plastic core for them. I'd have to get that from Jim. Meanwhile, Jim was having problems. His son died unexpectedly, or I mean, his grandson did, Jim Reding. It was a calamity. Meanwhile, that depressed Jim. Anyway, I didn't want to be in a situation where I had to keep getting stuff from him.

George Miley: In addition, since my coating method didn't work well and took forever to make enough of them, it's a tedious process, I wanted to do something more simple. I decided to make multi layer coatings on a surface and use this as an electrode in an electrolytic cell [inaudible 0:00:33:14]. What we were doing is, for example, if we had a silica base and we put a coating of nickel, we could also then put an extra coating of palladium.

Tom Grimshaw: Okay. George, I'm going to jump in and suggest before you get too far into this one, I'd like to suggest or request that we make that the beginning of our next 30 minute session. We're up to a little over 30 minutes on this one.

George Miley: Oh, okay.

Tom Grimshaw: Is it okay with you if we just continue this session and start another one? Do you have time now?

George Miley: Okay, let's do that.

Tom Grimshaw: All right, good. I'll close off here with some concluding comments. This is Tom Grimshaw interviewing George Miley at the University of Illinois in Urbana, Champaign, where he's retired or Emeritas professor, talking about his cold fusion experiments. We're focusing now on the very earliest work that he did. George, we'll pick up again then with the electrolytic work that you started to talk about in our next call, okay?

George Miley: Okay.

Tom Grimshaw: Thank you. I'll call you right back.

George Miley: Thanks.

Tom Grimshaw: Good.

B2. First Interview, Part 2

Tom Grimshaw: Okay. George, sorry about the delay. I just want to make sure everything was working okay. We're good to go. It's, it's creating MP3 files. So that's exactly what we want. So I'll make some introductory comments again here.

This is Tom Grimshaw. I'm talking with George Miley at the University of Illinois where he is Ameritas professor. We are talking about George's ColdFusion journey starting with the time of the announcement by Martin Fleischmann and Stanley Ponds in March, 1989. George and this is the second session.

George in the first session you talked about your initial reactions to the announcement and then sometime after the announcement you didn't initially do experiments, but then you did some sometime after you did some experiments of your own. You kind of started the first session with that. At the end of the first session you were starting to talk about another approach after working with beads with Jim Patterson, you decided to proceed in another direction. So if you don't mind kind of pick that up again from the beginning, please.

George Miley: Yes, I will. But I think I'll insert one comment before I pick that up. Since I had done these work with Jim Patterson and observed trans mutations, which are not ColdFusion they aren't DD reactions, I've wanted to rename what we were studying and that was where I came up with this terminology, Low Energy Nuclear Reaction, LENR, I think that was some of the mistake in my part because people didn't really understand quite what I meant, although now it's caught on a good bit.

The point is that the reactions that occur, occur by the collision of, or the combination of a reaction between two low energy ions and because it is such low energy, they end up not having a large excess energy when the reaction occurred. So you don't get copious neutrons nor copious scammers, high-energy radiation from the reaction. So it was an attempt to explain how you could have these heat producing reactions without having copious high energy neutrons or other radiations coming out.

Anyway, that was this terminology, which I then applied to what I was doing and others. I have two-

Tom Grimshaw: I would jump in at this point, George, and say that, that term certainly did gain wide adoption. There are a number of different names that people have used for this phenomenon, but I think ColdFusion and LENR have got to be the two most predominant terms that are used and have the greatest currency in the field.

George Miley: Right. Now the other thing that was happening about the time I was doing these experiments... I want to get back to the fact that Patterson's rough surface beads worked so much better than my smooth surface and great coating, didn't work, made me realize that these defects had something to do with it. So I wanted to produce more defects in a control manner in surfaces. So I came up with this process, which I was applying electrolytically with surfaces. Let's say we laid down a surface of nickel and then we could heat it on top or something and form a nickel oxide.

The interface between the two would be a nickel oxide nickel surface. Then if we loaded electrolytically, that being one of the electrodes, we'd load it and de-load it. When you load the metal expands regularly due to the loading as you ram the [deuterium 00:05:57] into the separation. When I say loading, I mean loading deuterium electrolytically. So, that tends to form defects at the surface. At the interface between the two because the two load differently and expand differently. So it causes defects to be rooted at the interface and creating more. So if you load, de-load, you get more defects and each time you load and de-load again you get more defects. The loading we found occurs by sort of a what Professor Kim at Purdue described as a Bose-Einstein type condensation into the defect region.

We, did experiments actually using a squid device, which measures the conductivity and so on, metallic and electromagnetic properties. We showed that the material that's loaded in these defects is carrying there is extremely high density and in fact is such that it becomes super conducting at around minus 70 degrees K or something. Now we aren't running it. There was no temperatures, but even at room temperature, the density is tremendously high.

So my concept is that with this very high dense region, if you can cause another ion to come in and hit the ones that are in there and say, have a deuterium, what I was calling a cluster, there's maybe a thousand ions crammed into this defect. You bring one more in, assess the rams has momentum enough to Ram into one of the ones that causes it to collide with the others. They are so close together that the coelomic barrier doesn't prevent them from reacting in the whole cluster where we're at. So I would view that as a reaction site. Now, other people were picking up on this. I don't know. People in the community were talking about nuclear reactor sites, nuclear reactive not nuclear reaction sites.

Various people had seen that once a electrode reacted that you had pitted regions that looked like miniature volcanoes at different places and that's where the reaction was occurring. They call that a nuclear reactive site. I was calling this a defect filled with a cluster. I complained about the others in that they were recognizing this was happening in these select regions, sites, but it didn't say how to make them and how to cause it to react there. Some people were saying, well what happens is that electrolysis, surface often gets contaminated with all sorts of metals and so on that come from other parts of the system and that forms these sites. But it doesn't give you a control method of forming.

Anyway, I felt that this process that we were using would control making them and would optimize the number that we could get in. In fact if we load, de-load these interface type electrodes. So if we would do that about eight, nine, 10 times we'd have maximum number of sites. If you continue to do it too long, these sites began to overlap each other, defects and that causes the whole thing to, well eventually just peel off. You don't want to try to continue too long, but you want to do enough to get an optimal number of sites created. Now we-

Tom Grimshaw: May i jump in just for a moment? Sorry to interrupt your train of thought, but I like to provide a little clarification at this point then we can pick it up again. So after you completed your work with the beads, you went to electrolytic cells using the standard cathode and anode and you were working on the cathode at this point. That's where you were doing your experiments at this time when you developed your ideas on the sites and working with Professor Kim at Purdue on the Bose, Einstein and the creation of these sites.

This was all taking place in your laboratory at the University of Illinois. So I have all that correct?

George Miley: Well, to some extent Professor Kim, a theoretician, we had talked about this some but he came out with a paper about the Bose-Einstein condensation, which we hadn't collaborated on explicitly. In fact, I've sent comments on, suggested some changes and so on. We did have discussions but it was after his seminal paper on this subject.

Tom Grimshaw: Okay. But you were running these electrolytic cells in your laboratory at University of Illinois.

George Miley: Right and again we were trying to show that what happens is that these reactions, cause often some of the cluster deuterium that was in these defects would react with itself. But often it would also span out and react with the surrounding materials.

So you might have multi-body deuterium reactions with nickel for example, and causing a transmutation and things of that sort. So we continued to try to correlate excess heat with the observed nuclear transmutation products that we produced. We've found it would be some correlation, but unfortunately with this type of gala type of electrode XL and so on, we could only produce small amounts like 10, 20 Watts of what we were calling excess heat. So that put more constraints on the accuracy and bigger error bars. So the experiments were not that as definitive.

However, what were we doing, I think we showed in a number of ways was very important. We relied a good bit on what we called a TPD, a temperature programmed type of heating of the material to try to show what was happening.

That is if you have volume metric loading, not loading in defects, where you load the material, many people like [Macuview 00:39:51] at SRI has numerous experiments showing how they get a volume metric loading of 0.85 or above, it would be a number of deuterium atoms per palladium atom in the volume of the material when it's loaded. It had to be that high to produce excess heat. In our case that definition of the loading doesn't apply because essentially we have zero loading in the volume and a loading of infinity in these defects because you don't have any palladium. So you'd have all deuterium.

So the parameter of importance to us would be in a number of these sites, not the loading per site. Because the loading per site is infinite. So anyway, that led us to a diversion. We got a contract from the Department of Energy to study this type of thing for a notional confinement fusion. What we did was we began working with people at Los Alamos, where to have a high powered pulse laser. We would load these multilayer thin film type of electrodes. But we would not put them in an electrolytic cell. We'd fly them down to Los Alamos and there what we do is we would hit them with the laser beam, which would drive out the deuterium that was stored in these defects and the deuterium would come out at high energy and bombard a subsequent target, which would be designed ultimately as a laser fusion target.

So we had sort of proof positive where we were able to cause these clusters in the sites. We were able to pretty well predict the number of deuterium that we had in the clusters, which could be measured in the laser target experiment. So that was, I thought, a very significant step in the understanding of what was going on in this type of thing. So to summarize, we...

Oh, I forgot one thing to tell you. I didn't complete the other type of experiment we did. If you have a volume metric loading of .85 or something like that and you heat the electrode up, all that deuterium will come out of it be de-sorbed, probably, by the time you get up to around 300 degrees.

However, if you have this stored in clusters, if you heat it up, it will not come out until it gets to maybe six, 700 degrees, even higher. We have some clusters that we've been able to make that would not come out until a thousand. The temperature to come out, they come out with is a measure of the binding energy that they are formed in the cluster. So this is another demonstration in measure of the number of clusters, in fact that we have the material forms in that fashion rather than in a volume metric type condition as used by all of the other people who work in these experiments.

Tom Grimshaw: Okay. Very good. I'm going to jump in if I may. I'd like to ask.

George Miley: Oh, sorry, something came in.

Tom Grimshaw: Okay.

George Miley: Not really my [inaudible 00:19:58]. Can you hear me?

Tom Grimshaw: Yeah, I can hear you. Can you hear me?

George Miley: Yes, we're still okay.

Tom Grimshaw: Okay, good. I wanted to jump in for a couple more opportunities for clarification please.

George Miley: Please.

Tom Grimshaw: Okay, so the electrolytic cell work you were doing in your own laboratory, I want to just ask this, you were using deuterium and you were focusing on defects that were on the surface of the substrate. Was the substrate generally a palladium or was it nickel or was it both at that time?

George Miley: Oh, we did experiments with both. What I think are sort of a standard would be palladium but where we took a blowtorch actually to the surface to form of palladium oxide so that we had an interface that would be between the base palladium and palladium oxide on the surface. So we're forming these defects, not quite on the surface, but somewhat beneath it.

Tom Grimshaw: Kind of at the interface between the metal and the oxide?

George Miley: Correct.

Tom Grimshaw: Okay, very good. Because the way you described it, the phenomenon of the deuterium loading into palladium, which was very essential in the Ponds-Fleischmann initial experiment, was not so critical in the experiments you were doing at that time.

George Miley: Right.

Tom Grimshaw: Okay. I think that's very interesting. I had not understood that.

George Miley: Well another important aspect too is their problem is that they had these very thick electrodes and to load them, they might have to run the cell for a week or something before they became loaded to the point where they could react and produce excess heat.

Also, it was hard because often the loading wouldn't work if we had micro cracking or something that allowed the deuterium to go in. But they would come back out through these cracks and you'd never get a high enough loading to react.

In fact, I think one of the big problems that people had in the early debates about all of this was that they didn't realize that this loading was what was so critical. So you might have somebody who had a faulty electrode, micro cracks and so on. He would claim that he had it loaded, quote-unquote, but he didn't measure loading. I think that's something that people at SRI demonstrated. If people had realized that they had to measure loading, that nothing would work unless you got a loading for .85. This is a volumetric loading that then there wouldn't have been so many controversial failures versus successes.

Tom Grimshaw: I understand. Yeah. Okay. So you were focusing on the defects rather and that the loading was of less importance in the way you were pursuing it. You were working off this idea of the Bose Einstein clusters in the defects and that was going on in your own laboratory. Then you did this work with Los Alamos, which further verified the idea of clusters and defects using the high power laser. Who at Los Alamos were you working with? Do you recall?

George Miley: I'll have to look that up.

Tom Grimshaw: I was wondering, was it Tom Plater by chance or was it-

George Miley: No, no, these were people in their laser fusion group.

Tom Grimshaw: Okay. Different people at Los Alamos.

George Miley: Different people. Yeah.

Tom Grimshaw: Okay. All right, good. Okay, so I'll ask you to look up-

George Miley: I had a post doc gal from... A post doc, she was from China that was instrumental in all of this at that time. She went down to Los Alamos and did most of the experiments herself with the help of their people.

Tom Grimshaw: I see. What was her name? I don't want to put you on the spot.

George Miley: Charlene. Charlene Shen.

Tom Grimshaw: Okay, very good. Okay, well this might be a good time to pick up the thread. What was next?

George Miley: Right. What was next was we thought about this and decided that instead of doing this with, electrolytic cells, it would be good to use nano particles instead of electrolytically loading into the defects, use a high pressure deuterium. Nano particles give you much more surface area. Since this phenomenon is occurring, essentially at the interface near the surface, you'd have many more defects per unit mass and material. So that's sort of a natural evolution. Doing it with a high pressure gas is much preferential too because the problem with electrolytic cells is it's very hard to get a high enough temperature to have good conversion from heat to electricity. If electricity is the main product you'd like to do. If you just worry about the conversion efficiency, it depends upon the difference in temperature between the hot region and the coal. So you can't get much above boiling water temperature of electrolytic cell whereas with a high temperature gas nano particles go up much higher.

In fact this is the type work we're doing now. I could continue on that but I'd like to divert for a few minutes and then maybe we can pick this up at some later time when we talk about what I'm doing now. But I wanted-

Tom Grimshaw: Yeah, let's go ahead and do the diversion because we want to kind of keep it as much time sequential as we can. So by all means.

George Miley: I was going to jump back because I mentioned to begin with that Steve Jones and so on had talked about publishing this article on Fusion Technology. Now it turns out that he didn't do that. He submitted it to Nature. He double crossed me when he told me he was going to send it to me and he didn't. But anyway, I was editor of this Fusion Technology Journal and I've jumped in and done these experiments that we've been talking about now.

I've done them at the University of Illinois, but I had tenure there. I was full professor and people couldn't hold up my promotion or something else because I was working on quote ColdFusion. It's amazing what a bad name ColdFusion got after Ponds and Fleischmann episode. So but I said, "Look, this is a fundamental, nuclear physics problem and that's what I'm interested in and I'd like to study it. I'm not trying to claim that we have ColdFusion in a test tube or something else. We're doing basic science and we should be free to do that without having people criticize us." But some people always want to criticize you for doing something like that it seems like in ColdFusion.

Likewise, I said, "Well we will accept papers in Fusion Technology if they can be reviewed and pass the review procedures. The same as papers in Hot Fusion." So I began accepting papers. I've been given a lot of credit for that. I don't know that I disclaim that. I should be given credit for that because it just makes common sense to me. This is what the journal is for. This is a fusion process. It doesn't have to be hot or cold. It's a fusion process. At least the type of, not the cluster type thing I was doing, but it had transmutations mainly involved, but the Ponds-Fleischmann type of fusion and that which a lot in the community continue to do, you know, [Recuvery 00:39:51] , others.

It just made sense that they should be gotten through. But then my editorial board got very upset about all this. Saying that I shouldn't be doing this. This was giving the journal a bad name. American Nuclear Society, a bad name. I stuck to my guns about it. They were saying, "Well, look, who reviews these?" They wanted to see that and I would pull out, I would show them reviews. They said, "Well, the people that are reviewing it are people in the field. That's not fair."

I said, "Well, in Hot Fusion or in any of these journals, if someone wants to publish a paper, the peer review people are also people who are working in the same area because they are the ones that are best qualified. They know what it's about. They're the ones who can properly peer review the paper."

So they were saying, "Well, we want one person from Hot Fusion to review all the ColdFusion papers." I said, "Well, I can't do that because Hot Fusion people, first of all, I have sent papers to them and they won't even look at them saying, well, we know that this is a bunch of nonsense. Again, they don't understand the field. So it isn't really fair to ask them to be your reviewers."

So we had this sort of standoff and I had, fortunately, I don't think that the board ever got together enough people to vote me out, but they were certainly very upset. I've been continuing in this state for a good while. Most of the papers that were published at that time and grew attention were in Fusion Technology. Other journals just wouldn't take them.

I finally decided that, well, I didn't to argue with my advisory board and the American Nuclear Society any longer and retired from being the editor. If you'll back and look what happened, you'll notice that as soon as I retired, no more ColdFusion papers. But anyway, that was an episode and a contribution I made to the field at the beginning.

Tom Grimshaw: Yeah. Thank you for stepping in and filling in that. That's a big part of that narrative.

George Miley: Well, it was a big part of my life.

Tom Grimshaw: Right. Okay, good. Yeah, it's well known and well appreciated in the field. I know from my discussions with others, so thank you for doing it.

George Miley: Well thank you for thanking me.

Tom Grimshaw: You want to pick up the thread now where you went after the... Let's see, I think you were talking about what you were doing after your work with Los Alamos. Maybe that'll bring you up to what you're currently doing.

George Miley: Right. Well, no, we completed that work and we showed that this would be an approach to having laser... One of the concepts for laser fusion is to drive deuterium out of a target into another target.

It hits the second target and that's where the reaction and DD reaction occurs. So you drive it out of a one, it would be held in this is multilayer target, which has the defect loaded clusters. You drive it out of that at high energy and it hits another target, which has more deuterium in it and high energy reaction with the cold deuterium produces fusion. That's a competitive concept that Los Alamos was fostering for laser fusion.

Unfortunately, we might've continued more experiments in that, although I think we demonstrated the basic principle and are considered as one of the ways of approaching the target. But the DOE decided to shut down the use of this pulse laser to outside persons. I mean it was a user facility at the time we went there, by definition.

So what that meant was that we could get all this help from the people there. Essentially it's free because we had submitted a proposal. That was accepted as being something that they were interested in and they wanted to participate in. We'd provide part of the manpower and part of the equipment. Then they would provide the other. So that was a great system.

University of Rochester and Lawrence Livermore Lab, they all have a part of their time of operations devoted to outside users that submit proposals in that fashion. But the laser at Los Alamos was shut down and I guess this is one of these things about money. They said they didn't have the money to keep supporting that aspect. Everything that was done there had to be done on classified work for the government. So, we'd stopped that.

Meanwhile, we were proceeding on this nano particle type pressurization approach, here at Illinois. At the same time I was very interested in other work. I mentioned earlier Mizuno in Japan has always done pioneering work. I met him and knew him and he was doing some plasma type creation of clusters, but he stammered. My version of it. I don't know what he would agree with exactly, what was the reaction mechanisms and so on.

But anyway, he was doing some work we thought was very good. So I decided to go back to a dual approach and work on a plasma loading process as well as this high pressure gas nano particle loading. That sort of brings me up to almost where I am today in doing those two things. I can talk more about that and so on. I propose we delay that until a future session when-

Tom Grimshaw: Yeah, I agree with that. We're up to 38 minutes on this one anyway. So let's go ahead and wrap this one up. You want to do another session this afternoon or should we schedule another time later in the week or what works best for you?

George Miley: I think it'd be better, I'd like to collect my thoughts on it. Maybe you and I can also talk a little bit off wire about that.

Tom Grimshaw: Okay.

George Miley: There's some things I'm not sure quite what I can talk about. I'll have to explain that to you.

Tom Grimshaw: Okay. Well let's go ahead and wrap this one up then. I'll call you right back after we finish here and we'll have a brief offline discussion.

George Miley: All right.

Tom Grimshaw: This is Tom Grimshaw. I'm here with George Miley at the University of Illinois talking about his ColdFusion research journey.

So George, we'll wrap this session up. Yeah, I should have mentioned on the earlier session, and we'll do so now, it's about 3:15 PM central. I forgot to mention that today's date is November 13. That needs to be a part of each one of these interviews as well. So we'll get that into the earlier interview session as well.

So George, thanks very much, and we'll pick it up in our third session later on.

George Miley: Well, thank you as you said, this has been fun.

Tom Grimshaw: All right, good deal. Take care. Bye.

B3. First Interview, Part 3

Tom Grimshaw: Okay. Tom Grimshaw here with George Miley. This is a session number three in our series of interviews regarding George's cold fusion research. George is emeritus professor at the University of Illinois. And this is, as I say, the third session. The date is November 13. George, please pick up where we left off in session number two, and we're going to talk about the work that you've been doing in the two main areas and related to clusters, so please proceed.

George Miley: Right. Well let me start by saying that funding is always a problem in these areas. And particularly in cold fusion where it's virtually impossible to get money from the government because they still have this memory of what they think is the debacle of the beginning of cold fusion with Pons and Fleischmann. It's unfortunate they don't have an open mind.

But anyway. I've been supporting my work with finding some private sources of money, and also putting some of my own money. And in fact, I also created a scholarship for undergraduate students in our department, Nuclear, Plasma and Radiological Engineering department, University of Illinois. And two of these awards are given out every year at our honors banquet. And this is a way of keeping other people apprised that this is a new upcoming area in our field, and something that I hope young people will get excited about. I don't know I've gotten too many involved other than the ones that received the award, but it's something.

Meanwhile, to support the research, over the years I've been somewhat entrepreneurial, and I had started a company, NPL Associates about 20 years ago and received money through the Small Business Innovative Research Awards, SBIRs. I think at one time, for example, I had the largest number of that type of award in Illinois from NASA. But it was years back. None of them were for cold fusion or LENR.

So I decided to get money to do the cluster type experiments that we needed to do, I'd have to do something else. So I spun off a company called LENUCO, and found four people who would invest in a company. And we set up as an LLC. And so these people put money in, I put some money and patents into it. And so that allowed me to start these experiments.

Meanwhile, I went to a meeting at MIT that Mitchell Swartz has held a meeting there in usually January, February timeframe periodically over the last few years. And it's been quite a good meeting, often attended by a number of interested people. And I made a presentation about this work that we were starting. At which time Dewey Weaver, who works for Industrial Heat came up to me, and said that it was very interesting and he'd like to talk to me about possibly working for Industrial Heat on it.

And I said, "Well certainly, I'd like to discuss it anyway." And so I went away and forgot about that. But then half a year later he called back, and he got me in touch with some of the other officials in Industrial Heat. Long and short of it, Industrial Heat decided to to buy LENUCO which means they essentially bought the company, gave us all the people who had invested, plus myself, stock in Industrial Heat. And also agreed to set up a laboratory and support it here in the University of Illinois, which they did. We have a lab in the incubator here which is called EnterpriseWorks.

And this is a very good arrangement because we have a well equipped lab and supporting services at a modest cost. It's much better because a lot of the facilities are shared facilities, et cetera, that we can use. And this is very typical. It is out in the University of Illinois Research Park. So anyway, it's working out very well. And we're presently doing our experiments there that are the pressurized nanoparticle experiments.

Meanwhile, we are also doing plasma implantation experiments to create the clusters and defects. And that's being done back at the University of Illinois. And I've talked about both of these. I mean the objective of both is to maximize the number of clusters you can produce using the techniques associated with either the nanoparticles or the plasma implantation.

The nanoparticles, we mainly... well as I already mentioned the fact you have the small particles means that you can have a larger surface area. And since the clusters are formed in defects near to surface, that's a big advantage. That's part of the surface are for unit mass. Anyway, we do that using a cryomill. We mill the articles in a cryomill. Which doing that in a fashion, where the milling is brittle at the low temperatures. Liquid nitrogen temperatures allows you to produce more defects. And compositions of particles are based on types of things that we used back on the electrolytic flat surface type multi-film electrodes.

So we will have a nanoparticle that's perhaps composed of palladium, palladium oxide, maybe some silicate. All aimed getting interfaces where the defects are producing the nanoparticles can have more interface regions to anchor the formation of the clusters. And we're measuring all of this again by using this temperature controlled pressure type of experiment where we will take the nanoparticles, load them, and then measure the gas pressure, say deuterium, or hydrogen say it's loaded into them. We measure the amount that comes out as a function of temperature.

And we do manage to find that we get [inaudible 00:10:10] to come out at the higher temperatures as opposed to the volumetric load which comes out of the low temperature. The shape of the release, so it's quite different from a flat surface type of thing that we were doing before. We find that that we have several humps, one at maybe 400 degrees. This is shortly after you get rid of anything that was in the volume. And you may get something else at 600, and maybe something at a higher... Anyways, it's sort of spread out more. So we're learning a lot about that and doing experiments to determine excess heat. We have a calorimetry [inaudible 00:11:07] we're doing.

Meanwhile, we have a second lab back at the university where we are doing plasma surface bombardment to create defects in the surface to the plasma. And we're also analyzing that in a similar way. Now we have put a fair amount of effort into some advanced techniques. One thing that we've been very successful in doing, one of my students who's doing a PhD on this is the main person that's pushing this ahead. His name's Eric Ziehm and he's focusing on CR-39 tracking foils.

CR-39's a plastic that when a high energy particle ion or something goes through it, it'll break the bonds in the plastic. And then if you etch it with an acid or something, it will form a opening where the bonds have been broken, that'll be etched away, and you can see the track. And the size and depth or track are characteristics of the type of particle and the energy of the particle that went in to makes them.

So what you have to do is have some source of standardized particles, say alpha particles, of different energy you can study. We've calibrated them with sources of alpha particles, MeV alpha particles and also MeV protons. And we observe those particles coming out from loaded surfaces, both the nanoparticles and of the plasma from implantation particles using the CR-39 process.

Now it's somewhat difficult to do these experiments for several reasons. One is it's difficult to get the CR-39 into the experimental device without causing it to melt or do something. So we have to have special insertion techniques to do that. Meanwhile, Eric has designed a automated reading system with an AI system such that it can recognize the type of track compared to these standard measurements. And hence, we can take a CR-39 foil, it might be several centimeters long, a centimeter wide, and we can keep scanning the whole surface automatically with a microscope which is set up on a frame such as it goes back and forth along the surface, recording the whole way, and counting the types of each particle that has been identified as corresponding to a track of a type that is produced when we did the standardization experiment with the known source was particles.

And this has allowed us to do a lot of experiments. I think Eric has more data about this type of thing than anyone else that we've seen in in our field who has studied that. Incidentally, we first became familiar with this approach way back when Andrei Lipson was working with me number of years ago, where he did a lot of work with CR-39. But we've come a long way since then in terms of being able to automate it, and be able to read this as opposed to old techniques of very tediously having observations made by individual eyes to a microscope to identify what's going on. So that's one thing.

We also have various neutron gamma ray detectors around the experiments. We're working on some other things like a diamond detector that can be used at high temperatures. And so in addition to excess heat production that we're studying, which of course is the real meat of this, we're very hard at work trying to identify the other products are produces like alphas, protons, any neutrons or gammas. Incidentally, we don't measure many of those, fortunately. We don't want them.

So we, I think, are really characterizing what's going on very well. And this is a very important aspect of trying to... we have to understand the reaction mechanisms, reaction products, I think, in order to ultimately optimize the experiments to get the maximum excess heat out of them to end up with a commercial product. That's the ultimate goal.

But at the moment, all these measurements lead you to only one conclusion. We are observing nuclear reactions in these clusters. And with the nuclear reactions, there has to be some excess heat. The question is, how much? We want to get a large amount compared to the input heat, input power. And so I think I can confidently say this is a low energy nuclear reaction mechanism that is occurring. We have copious proof of that. The question of how successful we're being in making an economic heat source has yet to be determined. We're still in the scientific phase of study. So Tom, that brings us up to date.

Tom Grimshaw: Okay. Very good. Well that's a very nice summary of what you're doing. I'm going to take the opportunity to flesh in a little bit here and there. Speaking of your work with the nanoparticles and the cryo grinding, I got a little confused. And maybe this isn't important, but I'll ask... Oh, and I also want to make the comment that what we're talking about here are things that have been presented at ICCF conferences, and perhaps elsewhere, and is in the public record.

George Miley: Oh, absolutely. We've talked about everything. I've told you, both at the conference that was in Japan, and the one that was at the University of Colorado. Or, Colorado State, I should say.

Tom Grimshaw: Right? Yep. ICCF-20 and ICCF-21 to be specific.

George Miley: Correct.

Tom Grimshaw: And so the work that was done on the nanoparticles, was that done in Illinois or was that done in North Carolina?

George Miley: No, it's all been done here.

Tom Grimshaw: All been down there. For some reason, in an earlier conversation I had the idea that some of your lab work was was at your sponsor's location, but that's not true. It's all been there at Illinois.

George Miley: Well, Industrial Heat is building up the lab down there. In fact, one of the people who's one of my key workers here has gone down there to work with them there. So they are doing work, but the particular two types of experiments that I described to you are all being done here. They're doing different things there.

Tom Grimshaw: Okay. Understood. And so then focusing on what's relevant to this project. The nanoparticle work you're using, well, you're using calorimetry and excess heat for both the nanoparticle work and the plasma work. Is that true?

George Miley: Well, for the nanoparticle work, what we're doing is calorimetry. And the type of thing that we're doing is, I don't know, it's parabolic calorimeter in the sense that we use an external heater and heat it up to a temperature. And then we measure the temperature per watt input to heat it up. And so if we do that with a standard sample, which is, let's say, you could use nanoparticles of helium that shouldn't react. You'll get a certain number if you then convert to deuterium, and same nanoparticles, and heat it up to that temperature. And let's say you get a higher temperature per unit watt in the heater. We're defining that as a measure of the excess heat.

Tom Grimshaw: Understood. Okay. Good. And you said that you're also looking at other signatures in addition to calorimetry and excess heat. Remind me what those are again, for the nanoparticles.

George Miley: We're also doing, we have a separate... it's not this calorimeter I just described. We have a separate vessel that we can insert CR-39 into, and then put the particles in it, and pressurize them. And then take all that out. That runs at low temperature. But we can still measure reaction products with CR-39 at the lower temperatures. I mentioned that we have purchased a diamond detector, which we're going to use to be able to do the same thing at a higher temperature. But at the moment we were limited to the... we don't want to melt the CR-39.

Tom Grimshaw: Right. Okay. And now turning our attention to the plasma work, you're again using calorimetry and excess heat as the signature. And there you're using [crosstalk 00:23:58].

George Miley: I was just going to say, Eric Ziehm, who's doing this work, has focused a lot on the reaction products to date. And so he hasn't been doing much calorimetry. We have to revise the setup. I don't know. It's complicated with the plasma. It doesn't look like you can do both at the same time the way we do it. So he has to either revise his set up and not measure the other particles, or he has to make a new setup so he has dual chambers and do excess heat in one, and his other measurements in the other. This is a problem trying to... he can't get all the equipment together in one spot the way we do it.

Tom Grimshaw: Yep. Yeah. It gets back to which signature do you really want? You want the signature that's more the gold standard of nuclear product, nuclear rations, or do you want a signature that indicates potential for a useful device, i.e. excess heat?

George Miley: Right. Well I think in Eric's case his argument is, okay, if we can maximize the production of the reaction products, we're probably maximizing the excess heat. We haven't proven that yet, but there is a correlation. And so ultimately he plans to be able to get that correlation.

Tom Grimshaw: Good. Let's, just out of curiosity on your clusters idea, and how the Coulomb barrier is reduced and allowing the reactions to take place. And when the reactions do take place and energy is released, what is the transfer from the clusters, and from that environment, over to the lattice where it can be measured as excess heat.

George Miley: Yeah. Well that is complicated, and that's something we need to study more. We are trying to do some modeling of that type of thing using COMSOL Multiphysics. But at the moment, there are several confusing aspects of this, right? Number one is, as I've described it, if what I'm saying is correct, the whole thing is going to react at once, and that's going to tear the whole layout of something in the nearby region. And you'll no longer have a cluster. And it doesn't look like you'd have a site left to make another cluster.

And yet, it seems like not everything reacts at once. I don't know. I mean the question I'm imposing is in one extreme if you have, let's say, 100 clusters distributed around 100 defects, and they all react, and you have nothing else produced, then the thing won't work anymore. It defines the lifetime of the thing. But it appears that we get a longer lifetime in that.

And so I don't know the dynamics of how we create additional defects and clusters as we operating. That's mystery number one. And the other one is, well, we use, essentially through vibrational modes and so on, you end up transferring the kinetic energy of the reaction products to ones that stay in the cluster region into the lattice, and ends up showing up as heat.

But that's again, with the amount of energy released in that little region, there's some question about how you... I think you raise a really fundamental question about how you actually transfer to kinetic energy to vibrational energy to heat. And this whole area is just spectacular and crying out for more work because we have all these fundamental questions that no one has the time, people, and the money to study.

When fusion reactors were first studied, people didn't know how to calculate a critical mass or anything else. You had to do with the famous Fermi experiments, stag field, and what have you. And we've learned so much. And this field is almost like that.

Tom Grimshaw: Yeah. Except more in slow motion owing to the amount of time that we consumed. Yeah.

George Miley: Yeah, you're right. And that's because of the lack of money in the field.

Tom Grimshaw:	Absolutely. Yep.
George Miley:	Yep. But all these things, I think we're beginning to make progress. I mean, not we, I'm think the community's making progress. I didn't get to the last ICCF meeting, but the reports I got from it continue to impress me that great progress is being made. [inaudible 00:30:38] one person makes it, and that information gets to others and we all benefit.
Tom Grimshaw:	Yeah. Yeah. And there's a lot to be discussed in the whole realm of openness versus intellectual property protection. A lot of issues in that regard as well.
George Miley:	Oh yes. It's complicated. So I don't know. We probably should do another one of these discussions after your visit to us, or maybe we can do it during your visit, if it's possible.
Tom Grimshaw:	Well, that would be my hope. I think we've learned a lot in these three sessions, and it certainly got the wheels turning. And it would be my hope and intent that we can do another parallel series while I'm on site in the middle of December. I found, as I said before, that we cover the same ground. Different things come up at different times, but it's always, in my experience, been very consistent. It's just different things get emphasized or remembered in each time. For example, I've been through three of these with Ed Storms, and also with Mel Miles, and some others as well. But they're the ones that come to mind because I've done three of them.
George Miley:	Yeah. Real good. I don't know, I should have mentioned Ed Storms. I think Ed Storms is the one who started talking about these nuclear reactive sites, and brought out that. And I think he and I had this little debate privately about discussing. I kept saying, "Well great, you have that terminology, but how do you make them?"
Tom Grimshaw:	Exactly.
George Miley:	Yeah. But meanwhile, I was going to say, going back, very interesting, I was describing these CR-39 plates. And you see there all these hotspots where reactions seem to be occurring in this little region, that little region, and so on and so forth.
Tom Grimshaw:	Yeah. In this field, when one thinks of CR-39, of course, one thinks of SPAWAR and Pam Boss, and Larry Forsley, and those folks. And I remember they had made this film of these nuclear reactive environments firing off, dying out, and then reforming, others reforming elsewhere. It was just like a motion picture of these sparkles coming on. I'm sure you remember that.
George Miley:	Yeah. But I don't think that was CR-39. They were looking at an infrared camera that was observing hotspots.
Tom Grimshaw:	Yeah. Okay. Thank you for that clarification. They're known for doing a lot of CR-39 work, and then they had also done this camera work.
George Miley:	Well, I don't know if they will recall this or not, but actually I'm the one that got them started on CR-39. Larry Forsley and I were talking about all this, and I said

they should do those experiments. And this was back when Andrei Lipson was working with me and doing CR-39. And, I think-

Tom Grimshaw: Oh, I did not know that connection. Their interest-

George Miley: Yeah. So we talked a lot to them about that. But then, they added much to the field. They saw these triple tracks and other things which had not been observed before. So they've added a lot.

Tom Grimshaw: And wasn't it true that wasn't it Oriani in Minnesota that had done some of this kind of work early on as well?

George Miley: Yeah, I'm trying to think. Gee, I talked to him a lot about this. He did some pioneering work. But he did some other things that were spectacular. I'll have to stop and think about all that.

Tom Grimshaw: Yeah. Well we can talk a little bit about that, other early interactions like that in the field. Because I know there were a lot of conversations taking place amongst you and the others at that time in those early days.

George Miley: Yeah. Well particularly Oriani because my daughter lives in Minneapolis. So several times when I'd go up to see her, I would also see him.

Tom Grimshaw: Very interesting. Yes. We must cover that in more detail. [crosstalk 00:35:46].

George Miley: Okay. Glad you brought that out. I'd forgotten to mention it.

Tom Grimshaw: Yeah. Well one of the advantages I have is I can't do experiments and I can't do theory, but I do get to talk to people and learn some of the research history. And I should mention, I guess, that I really like history. For example, I have a 370 plus page website on Grimshaws. Not just my family, but all of the Grimshaws from England, and how they spread throughout the New World. So I really enjoy this kind of work.

George Miley: That's spectacular. Good. Incidentally, we're not being recorded now, are we?

Tom Grimshaw: Yes. We're still on. So what we'll do is I'll call you right back if that's okay.

George Miley: Okay, sounds good.

Tom Grimshaw: So I'll wrap this up by saying that, again, that this is Tom Grimshaw with George Miley, and we're talking about his cold fusion research going back to the early days, not long after 1989, or even before. And I should also mention this is November 13. And this is session number three that we're completing. And this completes this round. George, I'll call you right back.

George Miley: Great.

Appendix C. Second Interview, December 17, 2019

C1. Second Interview, Part 1

Tom Grimshaw: This is Tom Grimshaw. I'm here with George Miley, and we are pulling together information about George's work in the cold fusion or LENR field. This is a site visit for the Miley LENR Research Documentation Project, and as a part of this collection of information, George has information about phases of his career and who he worked with on cold fusion. And so the purpose of this recording, is to talk about specifically, Andre [Lipson 00:00:49] , and the interactions that George had with Andre. I should mention this is December 17th. And George, why don't you go ahead and tell us the about the work you did with Andre Lipson?

George Miley: Right. So I met Andre at several of these IC staff meetings, I can't remember exactly which one. But anyway, we talked some about work he was doing, he gave a paper there. In fact, he gave several papers, and I was fascinated with that, and I said it would be great if he had any interest in coming to the U.S., maybe I could arrange that. In particular, he had already gone to several different places, in addition to being there in the Institute of Material Science, or something like, that in Moscow.

And so later, I got some money through a fellow by the name of Louis Larson, and I can talk about that separately later, but the money that he supplied me support for, I used partly to support Andre. So Andre came over, and he initially brought his wife and daughter with him. And he had done a number of things back home ranging from electrolysis type of loading studies or LENR to just a variety of different projects. And he also had done a lot of pioneering work on use of CR-39 for tracking articles.

Tom Grimshaw: Let me just jump in before you launch into the details. George, do you recall about what year it was that you started working with him, just off the top of your head? If you don't remember, that's okay.

George Miley: I don't. I think what we'd have to do is we can check with some of these papers that I published with him, help establish my memory.

Tom Grimshaw: Okay, very good. Go ahead.

George Miley: Yeah. Well he worked with me for about five years or so. And during that time we did new work based on things that I was already working on, he pitched in. And in particular was helping in some of the thin film electrolysis experiments that I was doing. He was doing CR-39 experiments where he would often send the CR-39 back to Russia to have it analyzed and the tracks identified. We were observing several different energies of alpha particles coming out, protons and other things. And so all of that work was going quite well. But after about the fifth year, I was

	running out of money. The support from Lou Larson had ended, and I was having trouble getting more money.
	But he had talked to people particularly at, for example, NRL and so on. And this project, the university of Missouri. Let's see, what's the name of that?
Tom Grimshaw:	SKINR, S-K-I-N-R.
George Miley:	Yeah, SKINR project. That had started, and they were very anxious to get him to work there. In fact, they gave him a full-time job offer, and he left though before that to go back home, to Moscow, and do some things there. His wife had returned there, his daughter had stayed here. In fact, by this time she had started actually in fine arts at a local place here, Parkland, local community college. And she completed two years of studies there and came to University of Illinois. And she was studying here at that time. Nothing scientific, but she was very smart person. And so-
Tom Grimshaw:	If I may, George, we'll pick up with the story after the fifth year. I want to stop this recording and [inaudible 00:06:28] setting. I'm sure it is, but let's do a check.
	Tom Grimshaw. I'm here with George Miley, and this is an interview about his work with Andre Lipson. It's December 17th. So we'll pick this up in just a few moments.

C2. Second Interview, Part 2

Tom Grimshaw:	Tom Grimshaw here, again, with George Miley. This is our second session here in this series about his work with Andrei Lipson. It's December 17th. So George, if you would please pick up the story after Andrei's work with you came to an end after five years?
George Miley:	Right. And he was back in Moscow where he was tidying up some things before he was to come back again to join the Skinner project at Missouri. At this time suddenly I was a ... I belonged to CMNS group that ...
Tom Grimshaw:	Yeah, you're talking about the blog, the CMNS Google group, where communication among cold fusion and LENR researchers takes place, yeah?
George Miley:	Right. And suddenly people are talking about the fact that he died of a heart attack on the Russian subway. There were a lot of different theories about that at the time, some people suggested the KGB had killed him for some reason.
Tom Grimshaw:	I had not heard that story.
George Miley:	Yeah, well it was a widely circulated story, but had no proof and I don't know. But anyway, hearing all of this I felt really badly about it, obviously. So I called his daughter, Maria Lipson. He had one child, Maria. I was telling her it was so horrible that I heard this. I assumed she knew about it, but it turned out when I called she didn't.
Tom Grimshaw:	Oh my.

George Miley:	So I had the unpleasant opportunity to be the first to tell her. She became somewhat hysterical when I did. So Liz and I jumped into the car and raced over to her apartment, which is right near here. Fortunately she had a neighbor who she was very friendly with, another young lady that came into the room to help comfort her. She called her mother who was back at Moscow about this.
	It turns out what had happened was her mother was coming to visit her. she'd decided to come and tell her in person, not call her about it. And she was to arrive here in about five days. So I had spoiled that plan. In a way it may have worked out better in the sense I think it'd been a horrible plight for her mother to have all this held as a secret and waiting to tell her and so on.
Tom Grimshaw:	Sometimes things work for the better, even if it was an inadvertent informing her.
George Miley:	Right. So I'd like to continue that very personal part of the story, because Maria was working library science and she wants to be a ... Not a librarian, but maybe work for the Smithsonian Institute or do something like that. She was at this time a junior, so she was also upset about not having money now, because it was very hard to from her mother here to cover her expenses, tuitions, and so on. Very fortunately I was in the Aero office and I knew the person there who worked on the aero business books and so on. She happened to tell me when I was in there that they needed someone to help. See, it was routine work, but she wondered if I knew anybody and I said, "Well, Maria Lipson is just the person."
	So I got them in contact and she hired Maria as a half-time RA, which here what that amounts to is it covers the tuition fee and also provides a salary. So that enabled her to be able to continue and graduate. When she graduated she applied to a number of places and she got offers. Well, she was admitted to four or five different schools, including Illinois. But Illinois, she got an offer to be a TA in the department, which is quite something because this department only has two TA positions in the whole department. It is not a very big thing.
	Anyway, Maria continued, got a master's degree and now she's virtually completed her PhD as we're speaking. I think she would have finished it last summer, but she sort of slowed down on purpose because of a problem. She wants to stay in the US and she has to get a job here so she can get a green card, a visa, to stay. If she graduates before she gets a job she'll have to go back to Russia. Anyway, I'm hoping something's going to happen. I haven't talked to her recently, but.
Tom Grimshaw:	That's a really wonderful story. What a service you did for her. Remind me, what was her field and what did she get her advanced degrees in?
George Miley:	Well, I was thinking library science, I'm trying to think. It's the study of history of science or something like that.
Tom Grimshaw:	Okay. Good.
George Miley:	So maybe we could stop and start a new thing?
Tom Grimshaw:	Okay, very good. Was there anything more you wanted to say about the work you actually did with Andrei? We have a nice list of publications and collection of his

papers as a part of our project. You mentioned thin-films and CR-39, anything else you'd like to say about the work you did with him for those five years?

George Miley: Well we did two things, I think. One was he brought with him a lot of data, he brought with him papers he was writing, and we worked together trying to clarify what he was doing. He had trouble writing in English. He spoke fluent English. He often, I think, elaborated so much he lost people and they couldn't understand what he was trying to say. So I helped a lot in the writing of things to clarify work that he had already done back in Russia.

In the process I learned some of the work they'd done. At that time I think Russia was one place that did more cold fusion work than almost any other country. They had a lot of people working and he was one of the leaders. So he was doing that while he was also helping do what I was doing.

Tom Grimshaw: Okay, good. Let's just say a few words, what was the LENR technique that he was using and was he using other signatures besides CR-39?

George Miley: Yeah, well that's a good question. They had done an amazing variety of studies. I'm going to have to refresh my memory to actually talk about that sensibly. But he had done some things, for example, where the loading and then presumably heating during the loading and this, that, and the other that caused metals to bend. Some of these studies they had done were remarkably different from anything that I had been aware of in the field. But I'll have to refresh my memories to talk intelligently about ...

Tom Grimshaw: Sure, no problem. Well, we have the papers. You've got a nice collection of those that's been added to our project, so that'll be good. Okay, then with that we'll wrap this one up. Thank you very much. [inaudible 00:09:59] a little narrative about Andrei Lipson and his daughter and the service you did for her after her father passed. So I'll end this with, again, saying this is Tom Grimshaw interviewing George Miley. The subject of this particular one is Andrei Lipson and his daughter, Maria. It's December 19th, no it's December 17th, and we'll wrap this one up for now.

George Miley: Good.

C3. Second Interview, Part 3

Tom Grimshaw: This is Tom Grimshaw. This is session number three with George Miley. We're doing a project to document his research in the cold fusion area and in this session in particular, George is going to talk a little bit more about Andre Lipson. We had a couple of recordings already and also about Lou Larsen and I think it's Lattice Energy was the name of Louis' company. So go ahead George.

George Miley : Right. Well, Lou Larsen lives in Chicago and I think he made a lot of his money in the stock market, but he had a degree and he's I remember biology or something like that. Anyway, he contacted me one day and started talking about cold fusion

	and wanted to come down and discuss all that. He didn't know what cold fusion was and...
Tom Grimshaw:	He was in Chicago as I recall? Was he not? Or?
George Miley :	Yeah (affirmative).
Tom Grimshaw:	Yeah, okay.
George Miley :	Yeah, he had a beautiful sort of penthouse and one of the high buildings that overlooks Lake Michigan.
Tom Grimshaw:	Okay.
George Miley :	So anyway, he came down and we talked and eventually out of this discussion he decided to support the work that I was doing. And I guess the money he was... He was making money in the stock market. He was very analytic and I think he had written some codes and so on. So, he's one of these, I don't know, day traders or something. I don't know that much about it that part. But anyway, Lou came down a number of times and learned what we were doing. This time we were working these thin film electrolysis experiments and he he's an extremely bright person and he picked up on all this. So I think after working with me for maybe a year, he decided he knew more than I did about all of this. But that's okay. But seriously, he began trying to tell me what he thought I should be doing as much as listening to what I said I was doing. But also, he was very, very concerned about the intellectual project, what we were doing. He wanted what we were doing to be kept secret, which is, we've discussed this before. Unfortunately, without the government being involved, we have all these small companies, he's Lattice Energy or whatever you called it. So, they started doing things and they don't want to have other people know what they're doing. They want to get ahead. I mean that's what business is all about I guess. So, I'm not criticizing, I'm just saying as the state of affairs.
Tom Grimshaw:	Yes sir.
George Miley :	Now, Lou though at one point coming back to Andre Lipson. Unfortunately, Lou was not happy about the fact that Andre Lipson would go back to Russia several times a year.
Tom Grimshaw:	Part of the IP paranoia.
George Miley :	Yeah (affirmative). And so, he accused me of letting Andre disclose things from our project here that the Russian shouldn't know or something. And so, he wanted... So we got into a disagreement there. I said that Andre was okay. He wasn't disclosing things. So, because of that though, Lou and I parted ways and it was a nice parting though. What he did was, well, he wanted to patent that I had filed based on this work and I called us at that time, and I can't remember the exact title, but I called a snowflake device.

Tom Grimshaw: A code name.

George Miley : Yeah (affirmative). That was because of some of the patterns that we observed. It reminded me of a snowflake at the time. In addition to CR 39 I was doing some infrared film or...

Tom Grimshaw: Okay.

George Miley : So, we gave the patent to Lattice Energy. He supplied some money to close out the work with Andre, so I didn't have to send him home immediately. And also, I ,at that time, established an undergraduate fellowship LENR. I say fellowship for a scholarship, I should say for undergraduates. And he put some money into that to help that. So, we did good.

So, we parted good friends, but didn't agree on some things. So, as time went on, he developed his theory with, he and... I couldn't tell you the name...

Tom Grimshaw: Widom.

George Miley : Widom, yeah. The original work that they did was use that theory to predict the trans mutations that were occurring.

And now, you men mentioned the Patterson cell. I had measured trans mutations in the beads and the Patterson cell and had published that. In the initial papers that he then produced for the Widom Larson theory, he tried to match those measured trans mutations and claimed to do a good job of it. So this was one of his claims that was to verify a theory. Although, I think after a bit, they were trying to get other people to do some experiments and he quit talking about the trans mutations and changed the subject matching other aspects of the phenomenon with the theory.

Tom Grimshaw: Okay.

George Miley : So, Lou never goes to ICCF meetings and things like that. I know I called him. For example, before the one that was being held at Colorado and...

Tom Grimshaw: ICCF 21 at Fort Collins, yep.

George Miley : I said, "Are you going?" And he said he had been... He's received death threats and so on about things and that he doesn't want to go and he feels that he'd be exposing himself if he did.

Tom Grimshaw: Wow.

George Miley : And he sort of rambled on about that and warned me that I should be careful. I didn't take that seriously, but...

Tom Grimshaw: Interesting.

George Miley : But it is. So, that sort of concludes my remarks about all of that.

Tom Grimshaw: Okay, good. Well, one more thing to mention. I don't know if you follow the CMNS Google group yourself, but it came out on there that Lou Larsen had passed away and I didn't know if you, you didn't mention that, so I thought.

George Miley : Oh my gosh, I haven't been following it.

Tom Grimshaw: Yeah.

George Miley : When was that?

Tom Grimshaw: Yeah, I think it was in around the September timeframe. We could look it up, find out it would be readily available information. But he did pass away in the early fall, I believe. Well, it might've been, it must've been after September because it was not well known. I mean, it wasn't announced during the conference in a CC or the ICC at 22 conference. So let's look that up after this conversation. We can readily determine that, I think.

George Miley : Yeah. Well, that's too bad. He, appeared to be in good health. I saw him a couple times in Chicago when I went up for other reasons and I would have lunch with him and he's... I don't remember. I don't know exactly what age he was, but he's a lot younger than I am.

Tom Grimshaw: Yeah, okay.

George Miley : That's too bad, I learned something.

Tom Grimshaw: [inaudible 00:10:19]. Wrap this up George, thank you.

C4. Second Interview, Part 4

Tom Grimshaw: Tom Grimshaw here. This is session number 4 with George Miley, and we're talking about various aspects of his work, his research, and the cold fusion as part of the Miley LENR research documentation project. And George, we've had three sessions, very good ones, and you indicated you have some topics to talk about on this, our fourth session. Did I mention it's December 17th? If I didn't I should. So George, why don't you go ahead?

George: Yes, well I thought I would just mention some other people that have worked with me and have influenced what I'm doing. So ones I might mention, in addition to Andre [Lebson 00:00:50] would be Nie Luo, also [Heinz Houra 00:00:54] and Charlie [Nyang 00:00:57] and [Kunjim Kim 00:00:59]. So I could start with maybe Ni Lu. And Ni Lu is... He-

Tom Grimshaw: As you get into it, let's help the transcriber here and spell his name, please.

George: N-I-E L-U-O.

Tom Grimshaw: Thank you. Okay, proceed.

George: Yeah. And Nie had a degree in theoretical physics from Northwestern, and I happened to go to an APS meeting, American Physical Society meeting, where he gave a paper, and I was quite fascinated with what he was talking about. I thought it had some vague connection with work we were doing, but was really on superconductivity.

And turned out that he not only was a very good theoretician, but he was also I would say a good experimentalist, although the way he did experiments was a little different. I think he would sort of force an experiment, and some way he would make it work when other people felt that he was being reckless. But it maybe doesn't make that much difference.

Anyway, I had some other gift funds that allowed me to bring him here as a post-doc. So he overlapped with Andre [Lepson 00:03:02] some, and the two of them, well, they could work together, but they also were somewhat competitive. So they-

Tom Grimshaw: Healthy competition can be good.

George: Yeah. And so... Anyway, he pitched in and was doing work on these cold fusion experiments. Meanwhile, I worked for a long time in hot fusion with [Heinz Houra 00:03:41] who's chair of theoretical physics at the University of New South Wales in Australia, and he and I became friends at meetings and had like minds about hot fusion, so we also began working some together on cold fusion. And Heinz came up with this theory of swimming electron... Well, we called it a swimming electron layer.

What it was is if you put together two different materials, let's say palladium and nickel, the Fermi layer [inaudible 00:04:23] discontinuous, and you get a high density of electrons in that region to cause a continuity between the two. And so if you pick the metals such that their Fermi levels are quite a bit different, you can get higher densities of electrons in the interface between them.

So the thought was that in order to have reactions occurring between the ions if you had this high density of electrons in the swimming layer, that would tend to suppress the negative charge or repulsive charge that keeps them from coming together and allow them to react, causing a nuclear reaction. So this was a theory for a way for cold fusion to work. So we were pursuing that in some of thin film work.

However, I got off of... Well, I was also at that time looking at the formation of clusters and defects, and that extended the swimming electron layer work, because you could also form defects and have the swimming layer aid the formation of the reaction in the defects, the way it all came together. So we were working on that simultaneously, and Nie Lou was helping some with that also.

Tom Grimshaw: I should jump in and ask, you mentioned Heinz Hora. H-E-I-N-Z H-O-R-A?

George: Yes.

Tom Grimshaw: Okay. Thank you.

George: It's actually Heinrich, but I call him Heinz.

Tom Grimshaw: Okay. Heinz, okay.

George: And, yeah. Heinrich was from Germany, and he had to leave when the Nazi regime got involved and so on. And he got this chair at the University of New South Wales. And I've visited him three of four times there, and in fact I've spent a semester there teaching and so on.

Tom Grimshaw: Very good. Good.

George:	So Heinz is very interesting. I don't know... I guess the one time Liz and I were there and he wanted to show us a bush. And we went out into the back into the blue mountains and were trampling around there on a trail and what have you. And it got about 5:00 and we said, "Well, we better get back to the car." And Heinz said, "You know, I really don't know where the car is. I don't know how we're going to get back." But he was very happy. Anyway-
Tom Grimshaw:	I take it he was kidding you.
George:	Well, only partly. He really didn't know where we were. We'd gotten lost. We did get back. So anyway, about this time, I also got another post-doc from China who was Xialong Yang
Tom Grimshaw:	Okay, go ahead and spell it, please.
George:	Yeah. X-I-A-L-O-N-G Y-A-N-G.
Tom Grimshaw:	Okay, thank you.
George:	But she said to call here Charlene.
Tom Grimshaw:	Okay.
George:	Yeah. And this was a new experience for me in several different ways. She and her husband came over. Her husband, he was a pharmacist. She was a material science, nuclear science scientist. She and Nie Lou got along well and were focusing on things, but I think, again, it was a little bit of a competition. She decided that she wanted to do some things that were different from what Nie thought she should be doing. But she was instrumental, I think, in the work that we did to demonstrate this formation of clusters in the defects. And I was very impressed with... She was very, what should I say? Aggressive, hard working. We wrote a proposal. I began thinking, we can't get money for cold fusion, but if we have these clusters of deuterium in a metal, one thing that we could do is hit that with a laser and drive the deuterium out and use it as a target to create laser fusion. And we drew a proposal and got a contract from DOE to do experiments at Los Alamos using their pulse laser there. And the reason to do it there was that they had marvelous diagnostics already set up to measure the beams and energies and everything else coming out of this one laser. And she went down there for maybe three months at a time over two years four or five times to do these experiments. And they were extremely successful. So [inaudible 00:11:23] the thing I was going to say, one thing that happened was that sometime along this period, she had a baby.
Tom Grimshaw:	In her spare time.
George:	So I haven't had many post-docs who have had babies. That was a new experience for me.
Tom Grimshaw:	Very good.

George:	But again, money's hard to get in this field, and I encouraged her to... She wanted to stay in the US. I encouraged her to try to find a different job. Well, she got a job offer very quickly from a company in Boston or in that area, and it was related to material science type things we were doing, but it was quite a bit different. But they were impressed with the papers that she had published on this laser fusion business.
Tom Grimshaw:	Do you recall who she was working with down at Los Alamos?
George:	Oh, yeah. I said I do. It'll come back to me.
Tom Grimshaw:	Okay. I remember in a phone conversation, a phone interview we had, it was somebody different from [Clader 00:12:48] or Fowler or-
George:	Oh, no, none of these guys.
Tom Grimshaw:	None of the old cold fusion crowd from-
George:	No, these were hot fusion people. Los Alamos, they did not compete with Livermore so they didn't have a huge laser there. But they had this pulse laser, which actually traces back to KMS in Michigan. When it went belly up, that company, they transported the laser to Los Alamos, DOE did. And so they were always using... They were going to use, instead of direct laser fusion, they were going to use laser driven beam fusion. In other words, you had a target drive a deuterium beam, which then would go into a second target. And that's where fusion would occur. And the logic in this, it gets a little complicated, but a big problem in the direct laser driven fusion which still is causing problems is that you have instabilities that occur when you try to compress the thing. Doing it this way with the external beams, you avoid all that. It's a direct beam driven fusion, rather than trying to compress the target.
Tom Grimshaw:	Okay, got it. Yeah.
George:	So they were specializing that. And these were the guys that we were working with.
Tom Grimshaw:	Okay, good.
George:	But at that time, this was a user's group. DOE has various facilities around the country where people can go in and if you have the money to do it, you pay them to sue their facility. But they're supposed to help you in supplying diagnostics and so on, and some of their expertise. And so all that... That is a good thing. Unfortunately, that laser project, it was a user's group up until we left. Some way, they classified it again, so only classified experiments can be done on [inaudible 00:15:18] which is a shame. But I was going to say, it's interesting, Charlene got this job I'm telling you about, and I don't remember the name of the company, but they had a... They were writing a proposal to the DOE to get money for his project. It was some type of energy project. And unfortunately, they didn't get the project, which is why they were hiring her. But some other company that was about a mile away got the project instead of them. That company turned around and hired her

to come and work on the project. And as far as I know, she's well on her way to becoming a citizen today, and the last time I communicated with her, I think last Christmas, she has another child and everybody's very happy there.

Tom Grimshaw: Wonderful. I think it's really great to relate the success stories of various people who've worked under you and been students and moved on to very significant and satisfying careers.

George: Well, I'm proud of these people, because they've done well. And let's see. Kyujung Kim, K-Y-U-J-U-N-G K-I-M, came here to work on fuel cells with me. And actually, I describe, if you look in that book, Black Swan, there's a chapter about fuel cells. And this was something when Nie lou was here. He and I were talking about the problem of getting money for cold fusion and so on, and were talking about various things, and, to his credit, he came up with the thought that maybe we should work on fuel cells. And, well, it's a long story, but the salt was using sodium borohydride as the source of the hydrogen in the fuel cell. And we started work on that. We got a DARPA contract to work on the fuel cells.

So the fuel cell work was sort of a spin off of cold fusion, and we were desperately trying to figure out how to get money.

Tom Grimshaw: I understand.

George: And being in a university, you have to write papers, and you have to get money to support people. So it's a little bit different. I feel that I've been very fortunate. Some way I kept coming up with money so that I could stay in the field. But a lot of the people or others, we talked about Ed Storms, well he retired, and I know he's gotten money from people, but it's mainly supporting himself. I've been trying to support other people.

Tom Grimshaw: Yeah. Well, and I think you've been in a good position also to do other kinds of research that was perhaps much more conventional and much more able to attract funding and students, so you kind of carried the cold fusion alongside, as I understand it from previous conversations, you were able to conduct cold fusion research or LENR research kind of in parallel with other things where it was easier to get support and get students.

George: So Kyujung Kim had been head of the... I don't know, it's a fuel cell project there in Japan before he came here. So he came sort of as a post-doc, but he was older than a post-doc. His PhD had been 10 years earlier fuel cell project, but he was an expert in metal hydrides. So he also contributed to the LENR work. And originally, when we were working with IH, he helped us a lot there in doing some initial work, and they were paying him to work with us.

Tom Grimshaw: IH meaning Industrial Heat, of course.

George: Industrial Heat. I don't know... Kyujung Kim is very... I don't know. He's a little feisty I think, and he got into an argument with some people at Industrial Heat and actually just quit, because he didn't appreciate what they were doing, and he wanted to do it differently.

Tom Grimshaw: Yeah. Sometimes you see that, the passion of your own line of thinking leads you away from current opportunities sometimes.

George: Yeah. So anyway, he's still here and he teaches a couple classes and does other things, and we're good friends still.

Tom Grimshaw: But he's not actively involved in the field, in cold fusion at this time?

George: No. Again, he got some... He's been writing proposals and got some money back in energy storage. Things like geothermal energy and things like that.

Tom Grimshaw: Okay, good. Okay.

George: So I guess a lot of us have to do other things to be able to work in cold fusion.

Tom Grimshaw: Right. It reminds me of a graphic artist when I was a manager in the environmental field. She would come and do graphics and artwork to support herself to go and do her real passion, which was painting and artwork. Similar kind of situation, where she wasn't able to make it with her artwork and selling her artworks, so she had to sustain her habit.

George: Well, I think we've covered everything we wanted to add quickly. And honestly, I've forgotten a few names and things that I need to go back and look up for you, but this gets it recorded and started so I don't forget to do this.

Tom Grimshaw: Good. I'm really glad you took time to do this and give credit to others that you've worked with, in which giving acknowledgement. So I really appreciate your taking that step. So that's a really nice addition to our project here. So this is Tom Grimshaw. It's December 17th. I'm here with George Miley. This is the fourth in a series of interviews. Most of the subject here in this sequence has been other people that George has been working with. This is part of our Miley LENR research documentation project. So thank you again for [crosstalk 00:23:15] nice little side trip here.

Footnotes