# LOW ENERGY NUCLEAR REACTION (LENR): SCIENCE, ENGINEERING, AND BUSINESS

## *DOCUMENTATION OF THE LENR CAREER OF DAVID J. NAGEL*

### *PHASE 1. DESCRIPTION OF COMPONENTS*

***SECOND DRAFT***


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## 1 Introduction

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

One of the foremost LENR researchers is David J. Nagel, Ph.D. He has been involved in the field since March 24, 1989, when his noontime swim was interrupted with a notice from his boss (a hot fusion specialist) to call him right away. In the three decades since then, Dr. Nagel has been involved in diverse LENR activities. They range from calls, email and meetings, to over 100 presentations to more than 4000 people, to writing dozens of papers. He has attended all 21 of the International Conferences on Cold Fusion, as well as many other meetings and conferences on LENR. His central involvement in the science, programmatics and politics involving LENR make his records a significant part of the history of LENR.

A project has been initiated with Dr. Thomas Grimshaw to document Dr. Nagel's LENR research and other activities. It is planned that his archives will be ordered and eventually transferred to a university library. The project is planned to realize several benefits:

- A collection of all of Dr. Nagel's ICCF conference materials
- An ordered collection of everything he has written on LENR
- An ordered collection of all of his 100+ presentations
- Tabulations of notes from his phone calls and meetings

The project includes the following steps:

I. Discussions by phone to develop an initial plan.
II. A few day trip to Dr. Nagel's office by Dr. Grimshaw to review the materials.
III. Acquisition of a scanner for the laboratory.
IV. Scanning and labeling of files.
V. Production of ordered and searchable files of the scanned materials.
VI. Phase 2 plans for additional ordering and archiving of remaining materials.

### *1.1 Professional Background of Dr. Nagel*

Dr. Nagel has enjoyed a lengthy professional career that has included many stages. His career is well summarized on his webpage on The George Washington University website, which also includes a photo (Figure 1-1)[1].

> David J. Nagel graduated (magna cum laude) from the University of Notre Dame (B.S. in Engineering Science 1960), and performed graduate work at the University of Maryland (M.S. in Physics in 1969 and Ph.D. in Engineering Materials in 1977). During active duty with the Navy, he was Navigator aboard the USS ARNEB on OPERATION DEEPFREEZE (1960-2), and then served as a Technical Liaison Officer at the Naval Research Laboratory (NRL) (1962-4).
>
> After joining the civilian staff of the NRL in 1964, he held positions of increasing responsibility as a Research Physicist, Section Head, Branch Head and, finally, Superintendent of the Condensed Matter and Radiation Sciences Division. In the last position, he was a member of the Senior Executive Service, and managed the experimental and theoretical research and development efforts of 150 government and contractor personnel. At the NRL, Dr. Nagel's research interests centered on radiation physics, especially x-ray spectroscopy, and on materials sciences, with applications to materials analysis, plasma diagnostics, integrated circuit production, environmental studies, "cold fusion", and MicroElectroMechanical Systems (MEMS).
>
> Dr. Nagel has written or co-authored over 150 technical articles, reports, book chapters and encyclopedia articles. He is lead-author of a patent on x-ray lithography, which formed the basis of a 100-person startup company in Rochester NY. After serving as Commanding Officer of three Reserve units and the national Technology Mobilization Program, he retired as a Captain in the United States Naval Reserve in 1990.
>
> He left Government Service, and became a Research Professor in the School of Engineering and Applied Science of The George Washington University, in 1998. He is now working on the development and applications of MEMS and microsystems for the military and other sectors, with special attention to radio-frequency and acoustic systems.

Dr. Nagel's research and other activities in the LENR field up to early 2008 have been well described in an interview by Marianne Macy. The interview is included in Attachment I. His career to early 2000 is further described in another interview by David van Keuren, which is in Attachment II.

[1] GW School of Engineering and Applied Science. https://www.seas.gwu.edu/david-j-nagel.

*Figure 1-1. Dr. David J. Nagel*
*(As Shown on the GWU School of Engineering and Applied Science Website)*

Dr. Nagel's extensive contributions in the LENR field are demonstrated by his presentations and professional papers in several venues, which are included in appendices to this report:

- Talks on Cold Fusion and LENR, 1997 to End of 2017 (Appendix A)
- Journal Publications on LENR (Appendix B)
- Articles in Infinite Energy Magazine (Appendix C)

Dr. Nagel is currently preparing a comprehensive book on LENR. An outline of the book is shown in Appendix D.

## 1.2 Research Documentation Project

The project to document Dr. Nagel's LENR research is being conducted in phases. In Phase 1, the materials are being collected and described, and a plan is being developed for more detailed and thorough examination and documentation in Phase 2. At the beginning of the project, Dr. Nagel provided the following summary of the work to be accomplished.

> There are some paper LENR files at Dr. Nagel's home, but they are few and can be brought to the university. The computer LENR files at home are duplicates of what is at the university. Those electronic files are mainly on the desktop computer in his GWU office. The electronic files fall

into two categories, those no longer being used and those that are still being modified. It will be possible for him to put the archaic files in order for categorization and preservation. The files that are still evolving will have to be archived later, either when they become stable or before he actually retires.

The hard copies at the university in Tompkins Hall are of two types in two locations. In the office (Room 105K) are both books on shelves and files in cabinets. There are about 12 linear feet of books shelves. Many of the books are in routine use. Hopefully, they will be categorized and shipped to the Marriott Library, when he can no longer participate in LENR research. The paper files in cabinets are mainly in folders. There are 16 mostly-filled file drawers of LENR material. They are reasonably, but not perfectly ordered. One of the drawers has all copies of Infinite Energy magazine. Three drawers have materials that are the residue of papers that he has written. They need weeding to discard drafts and some other worthless papers. Six drawers are organized by people or organizations. One drawer has files in frequent current use. Five drawers contain organized files for a planned textbook on LENR. Before he gets to that book, he will complete a general book on LENR ("LENR from Science to Business"). That book is almost half done, and most of the files for it are already electronic.

It will be decided which of the files in those 16 drawers in the office to scan in the near future. It is probable that files not in active use will be scanned sooner rather than later. The five drawers with files for the textbook will not be scanned soon, since he plans to work from them when writing that book.

In the laboratory (Room 202), there are 15 file drawers and four boxes, each with the equivalent of a file drawer. They include some publications and many old transparencies, in additions to copies of papers, notes and correspondence. Some of the drawers contain materials from specific topics or people, though none of them is well ordered. Several of the drawers contain quite random material that was never properly ordered for filed.

It seems clear that the contents of the disordered drawers will have to be scanned using a file naming system that will permit electronic sorting, as well as searching. It would be much too laborious to attempt physical sorting, and the scanning would have to be done anyway after sorting.

Dr. Grimshaw visited Dr. Nagel's office and lab on September 17 and 18, 2018. The following components were identified for the Phase 1 report during the visit.

- Electronic Files on Work Computer
- Electronic Files on Home Computer
- Books on the Bookshelves in Office
- File Cabinets in Office
- File Cabinets in Laboratory
- Storage Boxes with Files in Laboratory

These components are described in the following sections of this report.

## 2 Electronic Files

Dr. Nagel Nagel's electronics files are located in his home and office computers (Table 2-1). There are about 8500 files in the home computer and over 20,000 files in the office computer. Although the total number of files is therefore indicated to be over 28,000, there is considerable overlap and redundancy in the files in the two computers.

It is clear that much of Dr. Nagel's LENR work, as indicated by the file folders in the two computers, has been related to or pursuant to the International Conferences on Cold Fusion (ICCFs).

*Table 2-1. Folders and Files and Dr. Nagel's Home and Office Computers*

| | Files | Size (MB) |
|---|---|---|
| Home Computer | | |
| 1. LENR | 8,662 | 17,060 |
| 2. LENR Talks | 331 | 862 |
| ICCF-14 Proceedings | 3 | 33149 |
| ICCF-18 | 770 | 2,920 |
| ICCF-18 Graphics | 510 | 1,070 |
| ICCF-18 NI Course | 144 | 232 |
| ICCF-19 | 524 | 1,270 |
| ICCF-20 | 149 | 862 |
| ICCF-21 | 317 | 908 |
| ICCF-21 DJ in Photos | 656 | 6,990 |
| ICCF 2 | 13 | 37 |
| LENR Textbook on Home Computer | 238 | 174 |
| LENR Textbook on Office Computer As of 25 June 2013 | 4,832 | 2,670 |
| Subtotal | 8,487 | 68,204 |
| | | |
| Office Computer | | |
| 1. LENR | 8,419 | 12,350 |
| 2. LENR Talks. | 270 | 661 |
| ICCF-10. | 253 | 206 |
| ICCF-11 | 162 | 82 |
| ICCF-12 | 59 | 60 |
| ICCF-13 | 1,038 | 2,560 |
| ICCF-14 | 900 | 658 |
| ICCF-15 | 105 | 81 |
| ICCF-16 | 2 | 13 |
| ICCF-17 | 111 | 253 |
| ICCF-18 | 1,731 | 3,390 |
| ICCF-19 | 4,323 | 46,000 |
| ICCF-20 | 363 | 1,360 |
| ICCF-20, China | 75 | 312 |
| ICCF-21 | 1,239 | 2,160 |
| ICCF-22 | 2 | 4 |
| Lab Room 202 in Tompkins | 679 | 13,960 |
| LENR Book 20171212 | 197 | 174 |
| LENR Textbook | 159 | 23 |
| LENRIA | 69 | 334 |
| Subtotal | 20,156 | 84,641 |
| | | |
| TOTALS | 28,643 | 152,845 |

# 3 Bookshelves and Hardcopy Files in Dr. Nagel's Office (Tompkins Hall Room 105K)

Dr. Nagel has four bookshelves and five file cabinets in his office for his LENR work. He provided a preliminary list of these items at the start of the project, which is shown in Table 3-1.

*Table 3-1. Contents in Dr. Nagel's Office (Tompkins Room 202)*

| Number | Contents [NA = Not LENR Related] |
|---|---|
| Bookshelf 1 | Books to Be Inventoried |
| Bookshelf 2 | Books to Be Inventoried |
| Bookshelf 3 | Books to Be Inventoried |
| Bookshelf 4 | Books to Be Inventoried |
| File Cabinet A1 | Text Book in Preparation |
| File Cabinet A2 | Text Book in Preparation |
| File Cabinet A3 | Text Book in Preparation |
| File Cabinet A4 | Text Book in Preparation |
| File Cabinet A5 | Text Book in Preparation |
| File Cabinet B1 | Organizations |
| File Cabinet B2 | People |
| File Cabinet B3 | Current |
| File Cabinet B4 | SRI + SPAWAR + Swartz + Miles |
| File Cabinet B5 | Miscellaneous + Melich |
| File Cabinet C1 | Empty |
| File Cabinet C2 | Empty |
| File Cabinet C3 | Hagel + Chubbs + Jounneau |
| File Cabinet C4 | Collective Theories + Compact Objects |
| File Cabinet C5 | Piantells, Rossi + Related |
| File Cabinet D1 | ICCF-16 + ICCF-18 + Miscellaneous |
| File Cabinet D2 | NA |
| File Cabinet D3 | Miscellaneous |
| File Cabinet D4 | DJN LENR |
| File Cabinet D5 | DJN LENR |
| File Cabinet E1 | NA |
| File Cabinet E2 | NA |
| File Cabinet E3 | NA |
| File Cabinet E4 | NA |
| File Cabinet E5 | Infinite Energy Copies |

### 3.1 Bookshelves

The bookshelves are shown with Dr. Nagel in Figure 3-1a. Closer views of the bookshelves are pictured in Figure 3-1b.


*Figure 3-1a. Dr. Nagel with His Four Bookshelves of LENR-Related Books.*

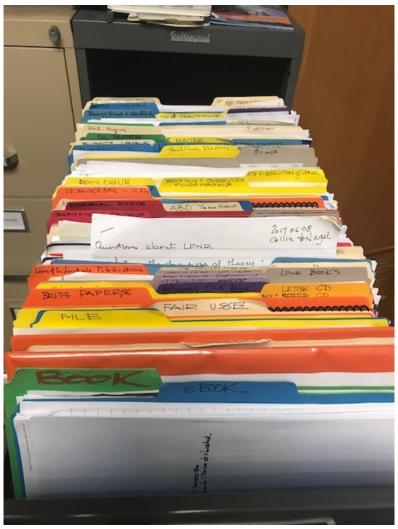
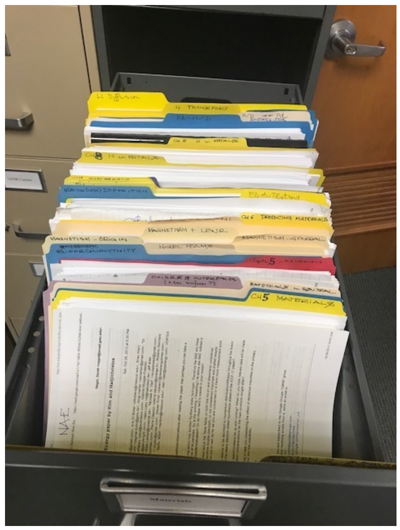
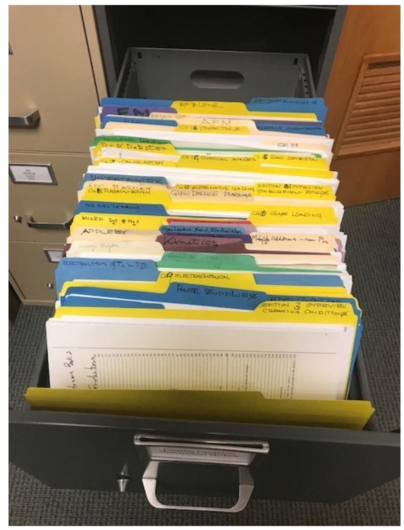

*Figure 3-1b. Closer Views of the Four Bookshelves.*

The five file cabinets in Dr. Nagel's office are shown in Figure 3-2. They are designated with the letters A to F from right to left. Each of them has five file drawers. The general contents of the drawers are described in the following sections along with photos of each drawer.


*Figure 3-2. Five Five-Drawer File Cabinets in Dr. Nagel's Office*

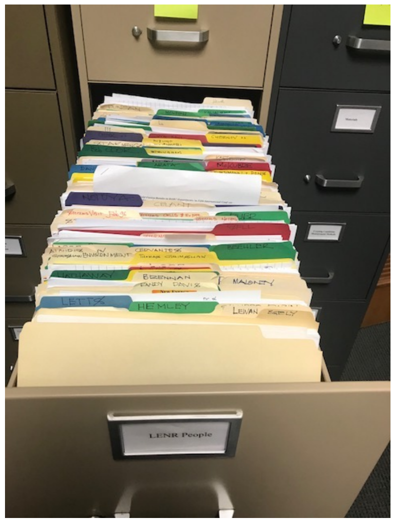
### 3.2 File Cabinet A

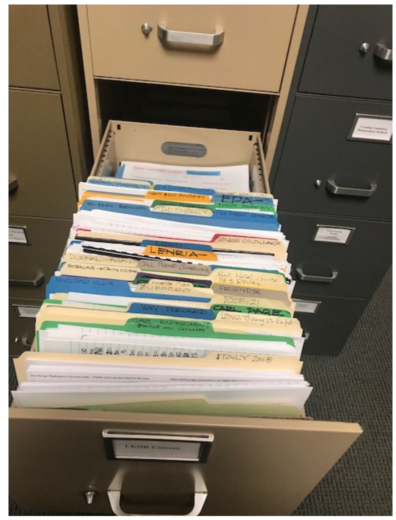
The five drawers of this file cabinet are dedicated to materials related to the LENR book being prepared by Dr. Nagel. Drawers A1 to A5, shown below, are organized as indicated in the outline in Appendix D.

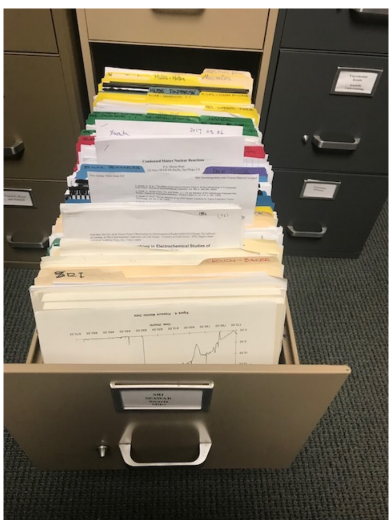




### 3.3 File Cabinet B

Drawers B1 to B5 of this file cabinet (shown below) contain current materials as well as files on organizations and people, including SRI, SPAWAR, Swartz, Miles, and Melich.



### 3.4 File Cabinet C

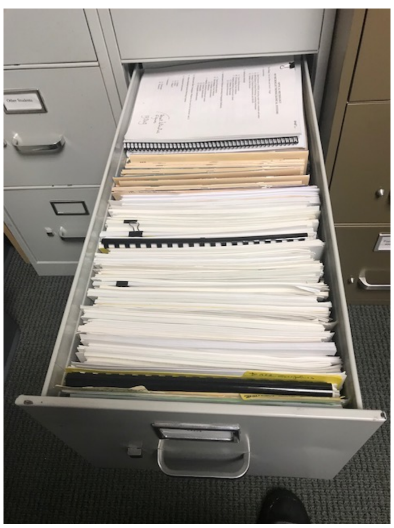
The upper two drawers of this file cabinet (shown below) are empty. Drawers C3 to C5 contain files related to Hagelstein, Chubb, Jonneau, Piantelli, and Rossi as well as material on theories and compact objects.





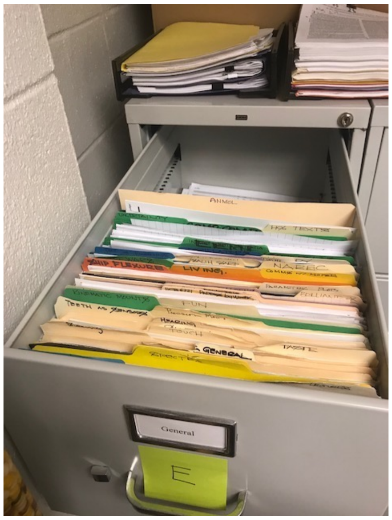
## 3.5 File Cabinet D

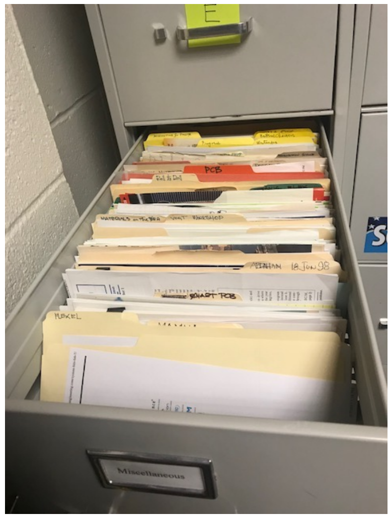
Drawers D1, D4, and D5 in this file cabinet (shown below) contain material that is relevant to Dr. Nagel's LENR work. They also include files on ICCF-16 and ICCF-18. Drawers D2 and D3 contain files not related to LENR.


### *3.6 File Cabinet E*

Only the lowest of the drawers in this file cabinet (shown below) contains LENR materials. They consist of copies of Infinite Energy magazine. Drawers E1 to E4 contain non-LENR files.






# 4 Hardcopy Files in Dr. Nagel's Laboratory (Tompkins Hall Room 202)

Dr. Nagel provided a preliminary list of the contents of the LENR materials in his laboratory at the start of the project. It is shown in Table 4-1. The lab has three file cabinets with five drawers each as well as four storage boxes. The file cabinets are designated F to H from right to left. The cabinets and boxes are shown in Figures 4-1 and 4-2.

*Table 4-1. Contents in Laboratory (Tompkins Room 202)*

| Number | Contents |
|---|---|
| File Cabinet F1 | Miscellaneous |
| File Cabinet F2 | Miscellaneous |
| File Cabinet F3 | Miscellaneous |
| File Cabinet F4 | Miscellaneous |
| File Cabinet F5 | Miscellaneous + VuGraphs + Hard Copies of Talks |
| File Cabinet G1 | Miscellaneous |
| File Cabinet G2 | People |
| File Cabinet G3 | Miscellaneous |
| File Cabinet G4 | Miscellaneous |
| File Cabinet G5 | Miscellaneous + Nuclear |
| File Cabinet H1 | Tapes |
| File Cabinet H2 | Miscellaneous |
| File Cabinet H3 | Foreign Experiments |
| File Cabinet H4 | Miscellaneous |
| File Cabinet H5 | Miscellaneous + Companies |
| Box 1 | ICCF 1 through 5 |
| Box 2 | ICCF 6 through 16 |
| Box 3 | ICCF14 |
| Box 4 | ICCF 17 through 20 |

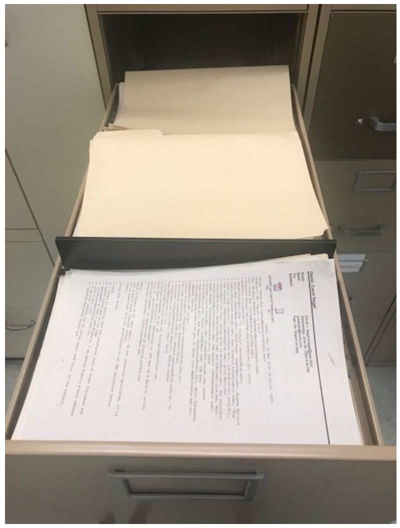
A large portion of the file drawer space in the lab is dedicated to "miscellaneous" LENR files that may be inventoried in the future..

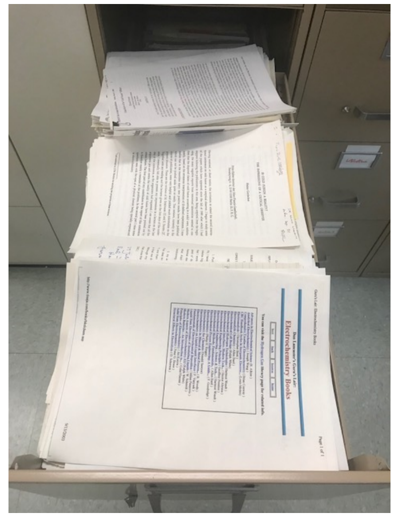

*Figure 4-1. Three Five-Drawer File Cabinets in Dr. Nagel's Laboratory*


*Figure 4-2. Four Storage Boxes in Dr. Nagel's Lab*

### 4.1 File Cabinet F

All five drawers of this file cabinet (shown below) contain miscellaneous LENR-related paper copy. Three of the drawers have a upright files and 2 contain stacks of paper. The lowest drawer includes VuGraphs and hard copies of talks given by Dr. Nagel.





### *4.2 File Cabinet G*

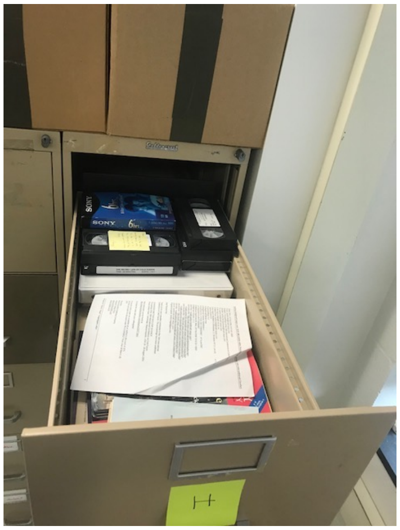
The drawers of this file cabinet (shown below) again contain mostly miscellaneous LENR-related materials. Drawer G2 has files related to people, with emphasis on those involved in the LENR field. Drawer G5 has in addition to miscellaneous LENR materials additional files described as "nuclear".





### *4.3 File Cabinet H*

Drawer H1 of this file cabinet (drawers shown below) has LENR-related VHS tapes and similar materials. Drawer H2 to H5 have miscellaneous LENR files, including some on foreign experiments and companies involved in LENR.



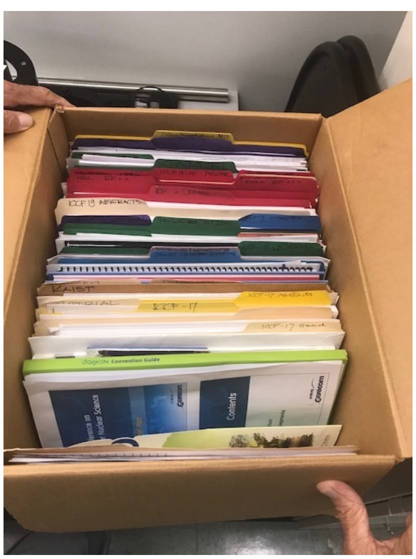
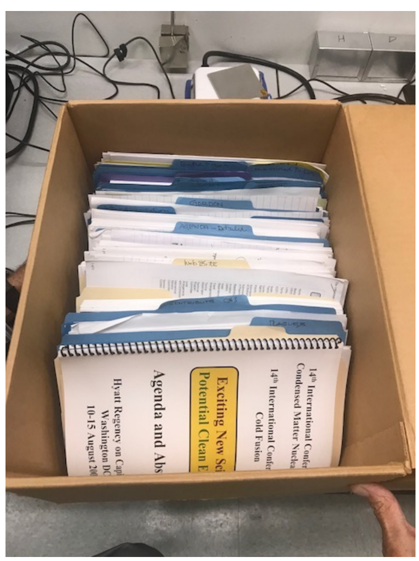


### 4.4 Storage Boxes

The four storage boxes, whose contents are pictured below, contain materials developed or collected by Dr. Nagel related to ICCF conferences:

- ICCF 1 through 5
- ICCF 6 through 16
- ICCF 14
- ICCF 17 through 20



# 5 Phase 2 Opportunities

The main emphasis of this Phase 1 report is on an preliminary inventory of Dr. Nagel's electronic and hardcopy files. Activities planned for Phase 2 are based on conversations Dr. Nagel during my site visit on September 17 and 18, 2018 as well as a phone call on about October 25.

- No additional work is anticipated at this time on the electronic files beyond the collection of files that you provided have provided on a flash drive.
- A complete inventory of the books on the bookshelves will be accomplished during Phase 2.
- An initial priority is to organize hard copy materials where they are not included in the scope of the project.
- Early emphasis will be placed on ICCF materials. They are found in file cabinet drawer D1 and in the four storage boxes in Tompkins 202.
- The hardcopy files in the other file cabinet drawers will be scanned on a prioritized basis (still to be determined). Student assistance is anticipated for this effort.
- A method of indexing the scanned files will be developed to ensure ready access to their content.

Because of competing priorities, a schedule for Phase 2 is not feasible. The possibility of preparing progress memos as Phase 2 is accomplished remains a good possibility. These memos would be prepared by me and would be based on information you provide by email, phone, and (possibly) narrative descriptions. The memos could then be the primary source for a Phase 2 reports in the future.

## *Appendix A. Talks on Cold Fusion and LENR, 1997 to end of 2017*

| Date | Location | Type | Listeners | Title | Slides | Other |
|---|---|---|---|---|---|---|
| 1997 | Ferrara, Italy | Overview | 80 | The Status of “Cold Fusion” | 20 | |
| 19980220 | DARPA DSRC | Briefing | 20 | Overview of Cold Fusion | 30 | |
| 20030827 | ICCF-10 Public | Overview | 150 | Cold Fusion: Problems, Progress and Prospects | 32 | |
| 20030827 | ICCF-10 | Technical | 150 | Energetics of Defects in LENR Materials | 20 | |
| 2003XXX | NRL | Overview | 30 | $10^{th}$ ICCF 24-29 Aug 2003 Cambridge MA | 55 | |
| 20031124 | GSFC | Overview | 100 | Cold Fusion: Problems, Progress and Prospects | 54 | |
| 20040107 | DoE BES | Overview | 20 | Cold Fusion: Problems, Progress and Prospects | 60 | |
| 20040129 | SAIC Workshop | Briefing | 60 | Cold Fusion: Problems, Progress and Prospects | 42 | |
| 20040428 | NRL | Overview | 30 | Cold Fusion: Problems, Progress and Prospects | 63 | |
| 20040323 | NRL ESTD | Overview | 50 | Cold Fusion: Problems, Progress and Prospects | 63 | |
| 20041103 | ICCF-11 | Technical | 150 | Reproducibility, Controllability & Optimization of LENR Experiments | 27 | |
| 20040716 | IDA | Overview | 20 | Cold Fusion: Problems, Progress and Prospects | 66 | |
| 20040910 | Notre Dame Physics | Overview | 30 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 65 | |
| 20041217 | JHU APL | Overview | 200 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 57 | |
| 20050114 | NIST | Overview | 100 | Cold Fusion: Problems, Progress and Prospects | 55 | |
| 20050320 | SAIC | Overview | 30 | Cold Fusion: Problems, Progress and Prospects | 57 | |
| 20050521 | MIT Colloquium | Technical | 40 | Evidence that Cold Fusion Involves Nuclear Reactions | 12 | |
| 20050916 | Notre Dame Physics | Overview | 30 | Evidence for Anomalous Low Energy Nuclear Reactions | 46 | |
| 20051117 | Amer. Nuclear Soc. | Technical | 15 | Evidence that “Cold Fusion” Involves Nuclear Reactions | 25 | |
| 20060802 | NDIA | Overview | 40 | Low Energy Nuclear Reactions: Status and Navy Contributions | 33 | |
| 20060811 | ONR Tech. Director | Briefing | 6 | Low Energy Nuclear Reactions | 35 | |
| 20060920 | APS Seniors | Overview | 30 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 57 | |
| 20060928 | Vice President Staff | Briefing | 6 | Low Energy Nuclear Reactions: Status and Needed US Actions | 23 | |
| 20061026 | CriticalExpt.Wrkshp | Briefing | 20 | Question: What Should Be Done? | 12 | |
| 20061212 | DTRA | Briefing | 15 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 38 | |
| 20061201 | MINT Malaysia | Overview | 50 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 59 | |
| 20070209 | Naval Reserve | Overview | 70 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 23 | |
| 20070211 | Congressional Staff | Overview | 2 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 4 | |
| 20070404 | GWU ECE Dept. | Seminar | 25 | LENR (aka Cold Fusion): Problems, Progress and Prospects | 42 | |
| 20070501 | OLLI Fairfax | Overview | 30 | Cold Fusion Confusion: Low Energy Nuclear Reactions | 39 | |
| 20070630 | ICCF-13 Sochi | Technical | 70 | Rates for Low Energy Nuclear Reactions at Surfaces | 35 | |
| 20070724 | NRL | Overview | 50 | Low Energy Nuclear Reactions: Status and Prospects | 24 | |
| 20070806 | DTRA | Briefing | 2 | Low Energy Nuclear Reactions: Status and Prospects | 44 | |
| 20070905 | SPAWAR | Technical | 15 | Diagnostic Overview | 9 | |
| 20070905 | SPAWAR | Technical | 15 | DTRA Expectations and Guidance | 13 | |
| 20071214 | GWU Chem Dept | Overview | 8 | Nuclear Science, Wireless Technology, ….and Collaborative Prospects | 32 | |

| | | | | | | |
|---|---|---|---|---|---|---|
| 20080315 | APS | Technical | 15 | Low Energy Nuclear Reaction Products at Surfaces | 15 | |
| 20080410 | Dominion HS | Overview | 50 | Cold Fusion Confusion | 46 | |
| 20081015 | Rimbach Houston | Overview | 300 | Instrumentation for Low Energy Nuclear Reactions | 36 | |
| 20081113 | DUSD | Briefing | 3 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 66 | |
| 20081114 | GWU ECE Dept. | Seminar | 30 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 47 | |
| 20081124 | Singapore Sci. Cen. | Seminar | 50 | Low Energy Nuclear Reactions: Problems, Progress and Prospects | 54 | |
| 20090529 | U of Missouri | Overview | 50+Internet | Scientific and Other Challenges of Lattice-Enabled Nuclear Reactions | 20 | |
| 20091007 | ICCF-15 Rome | Technical | 120 | Diurnal Variations During LENR Experiments | 20 | |
| 20091007 | ICCF-15 Rome | Technical | 120 | Can Water be the Origin of Excess Energy? | 16 | Poster |
| 20091215 | ICEE Malaysia | Overview | 50 | LENR: Exciting New Science & Potential clean Energy | 33 | |
| 20100629 | ARL | Briefing | 60 | Energy Gains in LENR Experiments | 8 | |
| 20100804 | OSTP | Briefing | 2 | Low Energy Nuclear Reactions: Status and Requirements | 12 | |
| 20100924 | GWU Emeriti | Overview | 30 | Scientific Challenge: Distributed Nuclear Energy | 37 | |
| 20101008 | GMU Physics | Seminar | 40 | Scientific and Other Challenges of Lattice-Enabled Nuclear Reactions | 46 | |
| 20110205 | ICCF-16 | School | 60 | An Overview of Low Energy Nuclear Reactions | 24 | |
| 20110207 | ICCF-16 | Technical | 80 | Characteristics and Energetics of Craters LENR Experimental Materials | 25 | |
| 20110208 | ICCF-16 | Technical | 80 | Robust Performance Validation of LENR Energy Generators (KG) | 22 | |
| 20110208 | ICCF-16 | Technical | 80 | Optimization of LENR Energy Generators | 16 | |
| 20110209 | ICCF-16 | Technical | 80 | Statistical Analysis of Diurnal Energy Variations in LENR Experiments | 22 | |
| 20110209 | ICCF-16 | Technical | 80 | Hot and “Cold” Fusion for Energy Generation | 18 | |
| 20110210 | ICCF-16 | Technical | 80 | Nano-Scale Materials for LENR Experiments and Generators | 21 | |
| 20110211 | ICCF-16 | Technical | 80 | Conference Summary for ICCF-16 | 0 | |
| 20110612 | KAIST | Seminar | 40 | LENR: Robust Results, Promising Potential and Critical Challenges | 56 | |
| 20110623 | MMU Malaysia | Seminar | 40 | LENR: Robust Results, Promising Potential and Critical Challenges | 42 | |
| 20110818 | LMCO Fairfax | Overview | 30 | LENR: Robust Results, Promising Potential and Critical Challenges | 44 | |
| 20111003 | Short Course | School | 30 | Perspectives on Low Energy Nuclear Reactions | 45 | |
| 20111004 | Short Course | School | 30 | LENR Energy Generators: Design and Manufacture | 35 | |
| 20111004 | Short Course | School | 30 | Low Energy Nuclear Reactions: Diverse Applications | 46 | |
| 20111004 | Short Course | School | 30 | Prospects and Discussion | 4 | |
| 20111121 | GW Garris Course | School | 30 | LENR: Robust Results, Promising Potential and Critical Challenges | 65 | |
| 20120229 | COFE | Overview | 20+ Internet | LENR: Science and Business | 20 | |
| 20120703 | ILENRS | Technical | 60 | Possibilities and Challenges for Commercial LENR Energy Generators | 24 | |
| 20120810 | ICCF-17 | School | 60 | Introduction to the Tutorial on Low Energy Nuclear Reactions | 69 | |
| 20120815 | ICCF-17 | Technical | 80 | Experimental Evidence for Bursts……..in LENR Experiments | 21 | Poster |
| 20120818 | ICCF-17 | Technical | 80 | Statistical Analysis of Transmutation Data from LENR Experiments….. | 24 | Poster |
| 20121111 | BEM Video | Overview | 40+Internet | LENR: Science, Engineering, Business and Education | 30 | |

| | | | | | | |
|---|---|---|---|---|---|---|
| 20121207 | Dupont Summit | Overview | 40 | LENR: Science, Engineering, Business and Education | 14 | |
| 20130721 | ICCF-18 | School | 25 | Low Energy Nuclear Reactions: Introductory Short Course | 36 | |
| 20130721 | ICCF-18 | School | 25 | Engineering, Testing and Applications: Possible New Industry !! | 36 | |
| 20130723 | ICCF-18 | Technical | 215 | Excess Heat Might Not Be Entirely from Nuclear Reactions | 16 | Poster |
| 20130723 | ICCF-18 | Technical | 215 | Surface Preparation of Materials for LENR: FemtoSec. Laser Processing | 20 | Poster |
| 20130723 | ICCF-18 | Technical | 215 | Simulation of the Formation of Craters in Cathode Materials | 20 | Poster |
| 20130723 | ICCF-18 | Technical | 215 | Abundance of Elements in Earth's Crust vs Widom-Larsen Model | 20 | Poster |
| 20130725 | ICCF-18 | Technical | 215 | Production and Destruction of Elements by LENR | 24 | |
| 20140911 | Naval Academy | Overview | 35 | LENR: Robust Results, Promising Potential and Critical Challenges | 14 | |
| 20150403 | Dr. Staffin at NRL | Overview | 15 | LENR: Robust Results, Promising Potential and Critical Challenges | 12 | |
| 20150415 | ICCF-19 | Technical | 200 | High Energy and Power Density Events in LENR Experiments | 18 | Poster |
| 20150415 | ICCF-19 | Technical | 200 | Electromagnetic and Electronic Frequencies Associated with Heat Production During Electrochemical Loading of Deuterium into Pd | 17 | Poster |
| 20150415 | ICCF-19 | Political | 200 | Industrial Association for LENR | 16 | Brochure |
| 20150513 | Tohoku University | Technical | 50 | LENR: Robust Results, Promising Potential and Critical Challenges | 33 | |
| 20150805 | GWU EEM Office | Overview | 6 | LENR: Robust Results, Promising Potential and Critical Challenges | 14 | |
| 20151029 | Industry Forum | Overview | 20 | Status and Challenges of Low Energy Nuclear Reactions | 18 | |
| 20160415 | Excelon | Overview | 30 | LENR: Robust Results, Promising Potential and Critical Challenges | 27 | |
| 20160930 | China Symposium | Technical | 50 | Another Approach to Reproducing Reported LENR Excess Heat | 30 | |
| 20161006 | ICCF-20 | Technical | 145 | Nickel and Light Water Electrolysis Experiments | 27 | |
| 20161006 | ICCF-20 | Technical | 145 | High Temperature Gas-Phase Ni-H Experiments in Ni and Stainless Steel Tubes in a Furnace | 32 | Poster |
| 20161006 | ICCF-20 | Technical | 145 | Production of Clean Water by Using Energy from LENR | 18 | Poster |
| 20170507 | 12th Asti Workshop | Technical | 70 | Simulations & Measurements of Thermal Behavior of a El. Chem. Cell | 24 | |
| 20170608 | 12th Asti Workshop | Technical | 70 | Expectations of LENR Theories | 17 | |
| 20170609 | 12th Asti Workshop | Political | 70 | LEAP: The **L**ENRIA **E**xperiment and **A**nalysis **P**rogram | 60 | |
| 20170906 | NRL | Overview | 70 | LENR: Status and Prospects for Science and Business | 32 | |
| 20170928 | U T Energy Institute | Overview | 200 | LENR: Status and Prospects for Science and Business | 32 | |
| 20170928 | Nat'l. Instruments | Briefing | 5 | LENR: Status and Prospects for Science and Business | 32 | |

## Summary

| Year | Overview | Technical | LENRIA | Seminar | Class | Briefing | Presentations | Listeners | Poster | Comments |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | |
| 1998 | | | | | | 1 | 1 | 20 | | |
| 2003 | 3 | 1 | | | | | 4 | 430 | | |
| 2004 | 6 | 1 | | | | 1 | 8 | 560 | | |
| 2005 | 3 | 2 | | | | | 5 | 215 | | |
| 2006 | 3 | | | | | 4 | 7 | 167 | | |
| 2007 | 5 | 3 | | 1 | | 1 | 10 | 287 | | |
| 2008 | 2 | 1 | | 2 | | 1 | 6 | 448 | | |
| 2009 | 2 | 2 | | | | | 4 | 220 | 1 | Internet |
| 2010 | 1 | | | 1 | | 2 | 4 | 132 | | |
| 2011 | 1 | 7 | | 2 | 6 | | 16 | 710 | | |
| 2012 | 3 | 3 | | | 1 | | 7 | 220 | | Internet |
| 2013 | | 5 | | | 2 | | 7 | 265 | 4 | |
| 2014 | 1 | | | | | | 1 | 35 | | |
| 2015 | 3 | 3 | 1 | | | | 7 | 91 | 2 | Brochure |
| 2016 | 1 | 4 | | | | | 5 | 225 | 2 | |
| 2017 | 3 | 1 | 1 | | | 1 | 6 | 345 | | |
| | | | | | | | | | | |
| Totals | 37 | 33 | 2 | 6 | 9 | 11 | 98 | 4370 | | |

**NOTES:**
**Management Briefings within the Naval Research Laboratory in the 1990s are not shown due to lack of records.**
**For posters at ICCF-X the number of people at the conference is listed, but not counted in the total numbers of listeners.**
**The audiences at ICCF-X schools were counted once.**
**If DJN was not the presenter, the numbers of attendees was not counted in the totals**

## *Appendix B. Journal Publications on LENR*

D. J. Nagel, “The Status of 'Cold Fusion'”, Radiation Physics and Chemistry, vol. **51**: pp. 653-658 (1998)

D. J. Nagel, “Fusion Physics and Philosophy”, Accountability in Research, vol. **8**: p. 137 (2000). Available from lenr.org by searching with Nagel.

D. J. Nagel and M. Ashraf Imam, “Energetics of Defects And Strains In Palladium”, P. L. Hagelstein and S. R. Chubb (Editors), Proc. of the 10th International Conference on Cold Fusion, World Scientific, pp. 291-303 (2006). . Available from lenr.org by searching with Nagel.

D. J. Nagel, “Powers, Materials and Radiations from Low Energy Nuclear Reactions on Surfaces”, Y. Bazhutov (Editor), Proc. of the 13th International Conference on Condensed Matter Nuclear Science. Sochi, Russia (25 June-1 July 2007), Publisher Center MATI, Moscow, pp. 728-744 (2008). Available from lenr.org by searching with Nagel.

D. J. Nagel and M. E. Melich (Editors), “Proceedings of the 14th International Conference on Condensed Matter Nuclear Science and the 14th International Conference on Cold Fusion” (ICCF-14) Washington DC (10-15 August 2008). http://www.iscmns.org/iccf14/ProcICCF14a.pdf (Volume 1) http://www.iscmns.org/iccf14/ProcICCF14b.pdf (Volume 2)

D. J. Nagel, T. Mizuno, and D. Letts, “Diurnal Variations in LENR Experiments” V. Violante and F. Sarto (Editors), Proc. of 15th International Conference on Condensed Matter Nuclear Science, Rome (5-9 October 2009) pp.61-64. Available from lenr.org by searching with Nagel.

A. K. Al Katrib and D. J. Nagel, “Can Water be the Origin of Excess Heat”, V. Violante and F. Sarto (Editors), Proc. of 15th International Conference on Condensed Matter Nuclear Science, Rome (5-9 October 2009) pp.65-71. Available from lenr.org by searching with Nagel.

D. J. Nagel, “Hot and Cold Fusion for Energy Generation”, J. Condensed Matter Nuclear Science, vol. 4, pp. 1-16 (2011). Available from lenr.org by searching with Nagel.

D. J. Nagel, “Challenges, Attractions and Possible Impacts of Commercial Generators Based on Low Energy Nuclear Reactions”, International Low Energy Nuclear Reactions Symposium, ILENRS-12, Williamsburg VA (2012). Available from lenr.org by searching with Nagel.

F. Scholkmann, T. Mizuno, and D.J. Nagel, “Statistical Analysis of Unexpected Daily Variations in an Electrochemical Transmutation Experiment”, J. Condensed Matter Nuclear Science, vol. 8, pp. 37-48 (2012). Available from lenr.org by searching with Nagel.

F. Scholkmann and D.J. Nagel, “Statistical Analysis of Transmutation Data from Low-energy Nuclear Reaction Experiments and Comparison with a Model-based Prediction of Widom and Larsen”, J. Condensed Matter Nuclear Science, vol. 19, pp. 485-494 (2014). Available from lenr.org by searching with Nagel.

D. J. Nagel, “Characteristics and Energetics of Craters in LENR Experimental Materials”, J. Condensed Matter Nuclear Science, vol. 10, pp. 1-14 (2013). Available from lenr.org by searching with Nagel.

D. J. Nagel and M. Srinivasan, “Evidence from LENR Experiments for Bursts of Heat, Sound, EM Radiation and Particles and for Micro-explosions”, J. Condensed Matter Nuclear Science, vol. 13, pp. 443-454 (2014). Available from lenr.org by searching with Nagel.

S. A. Mathews, D. J. Nagel, B. Minor and A. Pique, ‘Surface Preparation of Materials for LENR: Femtosecond Laser Processing, vol. 15, pp.268-278 (2015). Available from lenr.org by searching with Nagel.

D. J. Nagel and R. Swanson, “LENR Excess Heat may not be Entirely from Nuclear Reactions”, J. Condensed Matter Nuclear Science, vol. 15, pp. 279-287 (2015). Available from lenr.org by searching with Nagel.

D. J. Nagel, "Energy Gains from Lattice-Enabled Nuclear Reactions. Current Science, vol. 108, pp. 641-645 (2015).

D. J. Nagel, "Lattice-Enabled Nuclear Reactions in the Nickel and Hydrogen Gas System", Current Science, vol. 108, pp. 646-652 (2015).

D. J. Nagel and A.E. Moser, "High Energy Density and Power Density Events in Lattice-Enabled Nuclear Reaction Experiments and Generators", J. Condensed Matter Nuclear Science, vol. 19, pp. 219-229 (2016). Available from lenr.org by searching with Nagel.

F. Scholkmann and D.J. Nagel, "Is the Abundance of Elements in Earth's Crust Correlated with LENR Transmutation Rates?", J. Condensed Matter Nuclear Science, vol. 19, pp. 281-286 (2016). Available from lenr.org by searching with Nagel.

F. Scholkmann, D.J. Nagel, and L. DeChiaro, "Electromagnetic Emission in the kHz to GHz Range Associated with Heat Production During Electrochemical Loading of Deuterium into Palladium: A Summary and Analysis of Results Obtained by Different Research Groups", J. Condensed Matter Nuclear Science, vol. 19, pp. 325-335 (2016). Available from lenr.org by searching with Nagel.

D. J. Nagel, "Expectations of LENR Theories", J. Condensed Matter Nuclear Science, vol. 26, pp. 15031 (2018).

## *Appendix C. Articles in Infinite Energy Magazine*

| Issue | Dates | Authors | Title and Link | Pages | |
|---|---|---|---|---|---|
| 69 | Sep/Oct 2006 | DJN | Program Strategy for Low Energy Nuclear Reactions<br>http://www.infinite-energy.com/iemagazine/issue69/index.html | 13-15 | 3 |
| 79 | May/Jun 2008 | DJN | Intersection of LENR with Nanometer-Scale Science, Technology & Engineering<br>http://www.infinite-energy.com/iemagazine/issue79/index.html | 12-20 | 9 |
| 80 | Jul/Aug 2008 | DJN & MMe | Past and Future International Conferences on Cold Fusion | 21-24 | 4 |
| 84 | Mar/Apr 2009 | DJN | Questions and Answers About Lattice-Enabled Nuclear Reactions<br>http://www.infinite-energy.com/iemagazine/issue84/index.html | 12-24 | 13 |
| 84 | Mar/Apr 2009 | MMe & DJN | Strategies and Agenda for ICCF14 | 45-46 | 2 |
| 88 | Nov/Dec 2009 | DJN | Scientific Overview of ICCF15<br>http://www.infinite-energy.com/iemagazine/issue88/index.html | 21-31 | 11 |
| 95 | Jan/Feb 2011 | DJN | Hot and Cold Fusion | 31-39 | 9 |
| 96 | Mar/Apr 2011 | DJN | Scientific Overview of ICCF16<br>http://www.infinite-energy.com/iemagazine/issue96/index.html | 9-19 | 11 |
| 100 | Nov/Dec 2011 | DJN & MMa | First Commercial Course on Low Energy Nuclear Reactions<br>http://www.infinite-energy.com/iemagazine/issue100/index.html | 26-27 | 2 |
| 103 | May/Jun 2012 | DJN | Potential Advantages and Impacts of LENR Generators of Thermal and Electrical Power and Energy<br>http://www.infinite-energy.com/iemagazine/issue103/index.html | 11-17 | 7 |
| 106 | Nov/Dec 2012 | DJN | Scientific and Commercial Overview of ICCF17<br>http://www.infinite-energy.com/iemagazine/issue106/index.html | 18-30 | 13 |
| 108 | Mar/Apr 2013 | DJN | Comments on Storms" Ideas About the Location and Mechanism of LENR | 19-23 | 5 |
| 112 | Nov/Dec 2013 | DJN | Scientific and Commercial Overview of ICCF18. Part 1<br>http://www.infinite-energy.com/iemagazine/issue112/index.html | 49-59 | 11 |
| 113 | Jan/Feb 2014 | DJN | Scientific and Commercial Overview of ICCF18. Part 2<br>http://www.infinite-energy.com/iemagazine/issue113/index.html | 9-21 | 13 |
| 118 | Nov/Dec 2014 | DJN | Question about Lattice Enabled Nuclear Reactions: Mechanisms and Materials<br>http://www.infinite-energy.com/iemagazine/issue118/index.html | 15-28 | 14 |
| 119 | Jan/Feb 2015 | DJN | Question about Lattice Enabled Nuclear Reactions: Experiments, Theories and Computations<br>http://www.infinite-energy.com/iemagazine/issue119/index.html | 17-36 | 20 |
| 120 | Mar/Apr 2015 | DJN | Question about Lattice Enabled Nuclear Reactions: Commercialization and Applications<br>http://www.infinite-energy.com/iemagazine/issue120/index.html | 18-38 | 21 |
| 122 | Jul/Aug 2015 | DJN | Scientific and Commercial Overview of ICCF19<br>http://www.infinite-energy.com/iemagazine/issue122/index.html | 10-28 | 19 |
| 123 | Sep/Oct 2015 | SBK & DJN | LENRIA, the New Industrial Association for Commercialization of LENR<br>http://www.infinite-energy.com/iemagazine/issue123/index.html | 17-19 | 3 |
| 126 | Mar/Apr 2016 | DJN | Indicators of Interest in Low Energy Nuclear Reactions<br>http://www.infinite-energy.com/iemagazine/issue126/index.html | 8-9 | 2 |

| | | | | | |
|---|---|---|---|---|---|
| 130 | Nov/Dec 2016 | DJN | The Satellite Symposium of the 20th International Conference on CMNS<br>http://www.infinite-energy.com/iemagazine/issue130/index.html | 26-34 | 9 |
| 131 | Jan/Feb 2017 | DJN | 20th Int. Conference on Condensed Matter Nuclear Science. Part 1. Introduction and Experiments<br>http://www.infinite-energy.com/iemagazine/issue131/index.html | 22-37 | 16 |
| 132 | Mar/Apr 2017 | DJN | 20th Int. Conference on Condensed Matter Nuclear Science. Part 2. Theory and Other Topics<br>http://www.infinite-energy.com/iemagazine/issue132/index.html | 7-22 | 16 |
| 134 | Jul/Aug 2017 | DJN&AK&VF | LENR, Energy and Water | 43-50 | 8 |
| 141 | Sep/Oct | DJB & SBK | Overview of the 21st International Conference on Condensed Matter Nuclear Science | 11-40 | 30 |

## *Appendix D. Work in Progress LENR Book: Current Skeleton Outline*

**Prologue**
**Part One. Fundamentals**
- Overview
- Chapter 1. Historical Background
- Chapter 2. Energy and Matter
- Chapter 3. Reactions: Chemical and Nuclear
- Chapter 4. Hydrogen in Materials

**Part Two. Creating Conditions**
- Overview
- Chapter 5. Electrochemical Loading
- Chapter 6. Gas Loading
- Chapter 7. Plasma Loading
- Chapter 8. Beam Loading

**Part Three. Measurement Methods**
- Overview
- Chapter 9. Calorimetry
- Chapter 10. Chemical Analysis
- Chapter 11. Energetic Radiations
- Chapter 12. Other Outputs

**Part Four. Experimental Results**
- Overview
- Chapter 13. Power and Energy Measurements
- Chapter 14. Transmutation Products
- Chapter 15. Energetic Particles
- Chapter 16. Other Observations

**Part Five. Scientific Understanding of LENR**
- Overview
- Chapter 17. Data Analysis
- Chapter 18. Mechanisms and Theories

**Part Six. Looking Ahead**
- Overview
- Chapter 19. Engineering
- Chapter 20. Prospects

**Epilogue**
**Appendices**
- Units and Dimensions
- Conversion of Units
- Dimensional Analysis
- Significant Figures
- Precision and Accuracy
- Thorough Documentation

## *Attachment I. Marianne Macy Interview of David Nagel, February 2008*

DAVID NAGEL
Washington D.C.

An Interview by
Marianne Macy
21 February 2008

EVERETT L. COOLEY COLLECTION
Tape Nos. U-1858

American West Center
and
Marriott Library
Special Collections Department

University of Utah

**MM:** Ok, we're with Dave Nagel in his Washington D.C. office on the twenty-first of February 2008. Dave's had an extensive oral history done with him already on his background and NRL so we don't want to repeat a lot of that material, but Dave, I always start by just asking for a little biographical sketch. So could you orient us in that way?

**DN:** Yah. Very briefly, I was born in 1938 in northern Illinois and went through parochial grade school and public high school. I did an undergraduate degree starting in chemical engineering, finishing in engineering science at Notre Dame. I had a Navy obligation because the Navy put me through university and I spent two years on a ship running cargo to the Antarctic. Went around the world and then for the second two years of Navy duty, I was sent to the naval Research Lab. 1964 I resigned my commission, took a reserve commission, stayed at the NRL as a physicist. Went from that to become a section head, then a branch head, then a division head. So for the thirty-six years I was there, a third of it and a little more was as a SES division head managing twenty million dollars worth of R and D a year, eighty PhDs, another seventy people and a wide variety of subjects generally under the heading of condensed matter and radiation sciences, ok? In 1998, retired from government service, took a position as a research professor at George Washington University, electrical and computer engineering and have worked with students, done research projects, remained active in LANR and here we are.

**MM:** Ok, let's start by saying how you got into cold fusion and what you were involved in eighty-nine when it all came along, in detail.

**DN:** I became a division head in 1985 and went through a couple of years of start-up, learning and problems, and by eighty-nine I felt I was in a cruise mode; that is, I knew how to do my job and management had gotten used to me and it was the sort of the calm before the storm in a sense, that of course the Soviet Union fell apart then. We lost a reliable enemy, funding went down and the nineties were very tumultuous, for cold fusion because of limited funding and having to fire friends and so forth. So eighty-nine was a good time for me in the position that I was in, for the laboratory where I worked, and so forth. And at that time, my immediate supervisor was hot fusion fellow who was an associate director for components and materials. His name is Bill Ellis, E-L-L-I-S. He worked at Princeton for a while and came to NRL as an associate division head—excuse me, associate director, one of the top management.

And I first learned of cold fusion in a swimming pool, ok? The Naval Research lab has a swimming pool in a facility left over from World War Two when there were a lot of sailors there learning how to operate radios, Arthur Godfrey included, as a matter of fact. And I was…

**MM:** Was the swimming pool there in case you fell overboard or what?

**DN:** No, it was for exercise. It was part of a recreation club. So I swam sometimes as often as five days a week at noontime and at time, a mile at a clip. So I was paddling back and forth oblivious of the world. When I came to the end of the pool, I felt a tap and it was the secretary who had just been sent down to get me out of the water by my boss, the hot fusion guy, to tell me to look into cold fusion right away. He had just heard about it. It was one of two times I was fished out of the pool; the other was on a day when my wife and I were due to go to Istanbul that evening and she thought that our car was stolen, so she had be fished out. But anyway, turns out it was just towed.

But the point of the second time, the cold fusion time, was that I immediately went back to my office, looked into it. Now that was pre-internet but faxes were the means for rapid communication, so in the…

**MM:** Was it the day of the press conference or how, what?

**DN:** Immediately after.

**MM:** Immediately.

**DN:** I don't remember the exact date, but I think it was the next day, ok, when things hit the newspapers and I say, the faxes started flying. So it was something I had to pay attention to, not only because my boss was interested in it, but because I headed a condensed matter and radiation sciences division, which was essentially the meld of the old solid state physics—read: cold—and the old nuclear physics division—read: fusion. You know, it was virtually my position that I had to look into it. Now…

**MM:** Dave, that made you kind of uniquely qualified for the science, didn't it?

**DN:** Yes.

**MM:** Could you elaborate on that a bit?

**DN:** Well, I was intimately familiar with solid state physics having done a PhD dissertation on the electronic structure of some alloys, so I knew the solid state side inside out and had, in fact, as one of the five branches of the division, a group of solid state theoreticians who were you know, playing on the international scale so I knew that part very well. I did not have the background in nuclear physics as strong, but nevertheless I had taken nuclear physics as a graduate student at the University of Maryland, College Park, and you know, worked with people who were doing applications of nuclear physics, radiation sciences and the like. So yah, I was very well qualified and I had to look into it, ok. Now there was a curious happenstance at that time. My specialty from the mid sixties when I took the civilian job at the Naval Research Lab, to and through that point, was x-ray spectroscopy. I started out by measuring the x-ray spectra of nuclear weapons at the Nevada test site for roughly five years in the sixties, then went from there to measuring the multi-million plasmas produced by a terawatt that it megavolt, mega-amp discharge machines for weapon simulation in fusion research. And reasons that was through the seventies, got in x-ray lithography; anyway, I had the technical background and as a expert on x-rays, x-ray lithography at that time, I had been invited by Larry McKnight to go to the Brigham…

**MM:** Who's Larry McKnight?

**DN:** Ok. Go to the Brigham Young University and give a lecture that was scheduled for two weeks after what turned out to be the announcement, ok? So by previous schedule, I went to visit McKnight who was a professor as I recall in the Brigham Young University, roughly two weeks after the famous press conference. Ok, so it was a very memorable trip. I say I went there, flew into Salt Lake City, drove down to Provo, was talking to Larry at some length and then gave my seminar and I went back to Salt Lake City and I roamed the campus of University of Utah on a Saturday. You know, I went to the offices of Pons and Fleischmann and others who were mentioned at that time. They weren't there, being a Saturday, but nevertheless I can fairly say that I was on scene very soon after the announcement. Now…

**MM:** Could you talk a little bit about the climate, what was going on both at NRL and your professional circles and the fact that you were now right where it was happening, but, what was going on at that point?

**DN:** Yah, absolutely, and I will come back to the NRL momentarily but let me…

**MM:** Sure [inaudible].

**DN:** Finish the story of this trip.

**MM:** Yah.

**DN:** Which was, which had a couple of interesting aspects to it. I was at Brigham Young where Steve Jones worked. So I went over to his place and in fact, did encounter him and talked to him briefly. I saw their setup with the so-called "Mother Earth" electrolyte, was interested that they were using rolls of pennies as shielding their experiment. Ok, for the reason that it provided a significant amount of mass and when they were done with it they could turn it back into the bank, ok?

And my relationship with Steve was and remained for a long time very pleasant, collegial. I later on provided him with some pictures of fumaroles from Iceland that he could use in his studies of isotopes coming out of the earth. And then as an aside, he recently went off into this conspiracy theory about the World Trade Centers, at which point I, since which I had no further contact with him, it's not vindictive on my part but I don't want anything to do with him, given his turn of events.

Ok, so I was able to visit Steve Jones in the same time frame, I don't know, it could've been the same day but close to when I gave the lecture on x-ray spectroscopy, or x-ray lithography, for Larry McKnight. McKnight, by the way, formed a company and supplied x-ray instrumentation. He's probably not active anymore, I really don't know. I lost track of him. But the other interesting thing at that is the conversation with McKnight. In the seventies, two people from the University of Utah announced that they had made an x-ray laser. And one was a graduate student and one was one of the Eyrings and the paper was published in the proceedings of the National Academy of Science, a very prestigious platform, of course, for a paper. And they claimed to have achieved actually x-ray lasing out an energy of eight kilobles that the copper wavelength, copper gave energy or wavelength, and that was a thousand times shorter wavelength than had been demonstrated at that time. And the theory indicated that the scaling with wavelength of the input energy, the energy required to pump an x-ray laser, was something like wavelength to the third or fourth power. So if you went a thousand times shorter in wavelength you'd need something in the order of a billion times higher power. And they did use a pulse laser with a coppersulfate solution on glass slides and in fact, it so got my attention that I went out and I bought jello to make the target setup and I was going to actually do the experiment.

So it turned out, and this is a whole story in itself, but it turned to be wrong that they did not make a short wavelength laser and that it took about three years to sort that out, there were spots on films, and it was sort of a sorry adventure, sub adventure if you will, in physics. So that had occurred well before the cold fusion thing came on the scene, before my visit to Provo and of course Larry McKnight and I were talking about the cold fusion news and he said to me that, "Oh yah, when they want they can power their fusion reactor with their x-ray laser." (Laughs) So he was very derogatory of it and was quite happy to bring up the previous failure of the completing university.

Now when I went out there, I'd been to Salt Lake City before as I recall, and I found it to be a very pleasant place, clean; nice, friendly people and all. I had heard of course that it's not a place for an outsider, that is, a non-Mormon, to live, there's a substantial sort of social barrier if you live there, you're an outcast so to speak—treated well, but you know, not on the inside. And I was unaware of that at the time, ok, I mean that there was this problem and in fact, I probably shouldn't admit this, but when I went there I was thinking not of the subject that Steve Jones studied, namely muon-catalyzed fusion but rather given the activities at the two universities there, Mormon catalyzed fusion! (Laughs) Now don't misunderstand this, it's not a pejorative comment, it was just the way my mind worked at the time. And I was interested to find that not only was there any solidarity between the two universities; there was an active antipathy that you know, they were very, very competitive with all their collective doses at each other and that was sort of the source of Larry McKnight's snide comment about the x-ray laser.

**MM:** Did you talk to Jones a great deal about his work on muon-catalyzed fusion and the Fleischmann and Pons announcement? How deeply did you get into it with him?

**DN:** Not very. I don't remember talking at all about the muon-catalyzed fusion, of course I wanted to see their setup; I'm a lab animal , I'm an experimentalist and he toured me with that and we talked about what he had done, what he was doing, what tectras he was using and so forth. But it wasn't a deep conversation, we didn't sit down in the conference room for instance and talk about you know, the options for what's being seen and so forth, you know, noise in the detectors and all kinds of things like that. So it wasn't deep but it was very interesting.

**MM:** Did you have a sense with what you just talked about with the competitiveness between the university that there would be a clash between Jones and Fleishmann and Pons?

**DN:** No. At that time I didn't know the history of the proposal that Jones sent to the DOE which Gaievsky sent back to Pons and Fleishmann for refereeing, if I remember correctly. And I was only starting to realize the tension between the universities but that of course does not translate to tension between individuals. Ok, so tension may not me the right thing; competitiveness is maybe a better word. But no, it was somewhere between friendly and naïve (Laughs); very simple. Ok?

**MM:** Yah.

**DN:** So that's essentially the story of that trip and having been to Utah, even though I didn't learn as much as I would like to have, I would've of course vastly preferred to also visit the Pons / Fleischmann lab and see some of that first hand.

**MM:** And you didn't get a chance to do that at all then?

**DN:** And I did not do that at all that trip.

**MM:** Can I, just one second, to go back to the x-ray laser and the, your previous trip. Was it Ted Eyring, or do you know who?

**DN:** No, it was I think Henry, it wasn't Ted. I'm pretty sure.

**MM:** Ok. And you said it was a thousand times more?

**DN:** Shorter wavelength.

**MM:** Yah?

**DN:** Ok, so the shortest wavelength at that time was in fact demonstrated by Ron Waynin for the Naval Research Lab and it was eleven hundred and sixty one angstroms, I remember. So this was a few angstroms, ok, so I say it was roughly a thousand times shorter wavelength and if you write an equation that says what power is required as a function of wavelength in order to achieve actually lasing, to get over the threshold for an inverted population, to achieve lasing, I'd say that scales with a very high power of wavelength. So the powers that were available Wayne in a traveling wave situation, which was absolutely solid science, it was understood and properly published and properly scrutinized; that was for real. It would've taken as they say a billion times more input power, ok. And that right away raised a red flag with us because it wasn't a billion times more input power, ok.

**MM:** Yah.

**DN:** So the expectation was that it was wrong and there was a fellow at NRL who was an expert on x-ray lasers, Ray Elton by name.

**MM:** Ray Helton?

**DN:** E—Elton. E-L-T-O-N.

**MM:** Ok.

**DN:** And he kept the head our group Birks, Laverne Birks—L-A-V-E-R-N-E B-I-R-K-S, the guy who hired me at NRL. At arms length, we wanted to go in and do certain measurements to prove or disprove it; that is, to use actodetectors instead of film and we were essentially stiffed by Elton, who's sort of a friend. I mean I bear him no grudge and cooperated and communicated a lot after that, but in any event if we had been allowed to do what we wanted to do, this thing could've been laid to bed in a scale of months instead of years. Ok.

**MM:** Did you have any further, do you know if at the University of Utah, did that it end the x-ray laser work, or did it continue?

**DN:** I think it ended the graduate student was named Kepros, K-E-P-R-O-S, I now remember, and he graduated and it ended.

**MM:** Ok, just one second I'm gonna… More to your trip? Ok. Alright, what happened next?

**DN:** Well, at NRL I think even before I went on that trip, we had started to meet to discuss these things. In fact, I don't remember who organized it but we had a meeting of all interested parties—from the division I headed, from the chemistry division, I think from the electronics division—and it was a get acquainted session because we didn't even know each other. In fact, that was the time I met Debra Rolison and immediately recognized her as a very capable but feisty person.

**MM:** What area did she work in?

**DN:** She was an electrochemist, ok. And so there might've been twenty people in the meeting, ok, and it was, "Oh who's gonna do what?" and I don't remember the outcome of the meeting except that other than that we knew who was interested at the NRL. Now the NRL is a six layer organization; the top two--the layers of the director and associate directors are the top management. Then you have two layers, divisions and branches that are the middle management, then you have the second-- heads and the working class, you know. And one of the hallmarks of that laboratory is that the individual investigators at the lower levels had a tremendous amount of autonomy to work on what the want to work on.

And when the high temperature superconductivity came on the scene in 1985, eighty-six, with first the announcement of the discovery by the two guys, Bedenaurs and Mueller in the IBM lab in Switzerland and then the verification in Japan, the physicists at MRL, many of whom were already working on superconductivity, you know, recognized that this was a historic event in their field. And when the management got around not too much later to asking if anybody's working on it, like forty hands went up. So you know, why do I say that, why do I mention this? A similar thing happened with cold fusion. You know, a lot of people thought that this is big game and by working in something that's hot, I have a better chance to become famous if not rich, and so I'll go work on that! Ok.

**MM:** Would there be—I can see in that meeting you had people from all different disciplines—but would there me communication between the different fields of people working on it at NRL?

**DN:** Well yah. And in fact I think it's fair to say that the subsequent activities which involved people from different disciplines, from different divisions (because the divisions were discipline oriented) working together on a given experiment was a result of our being co-located and knowing each other. Ok?

**MM:** Yah.

**DN:** So there are two stories to tell. One is what seemed to be happening in the field generally and then what was happening within the NRL. So let's talk about the field in general. And as is well known by now, the information especially the initial paper by Fleischmann and Pons was flying around the globe, literally, by fax and we would faxes that were faxes of faxes that would be nigh unto illegible and pour over perceived word and so forth in an attempt to try and understand what they did, because of course if they really did something and we could replicate it, then that would be the short rod. I mean there two options in science: one is to repeat something that's been done; the other is to do something new and the first impulse was in fact to repeat something that's been done. It was shortly followed by doing new things, things that had not been done before.

**MM:** Did you go into the lab and start replicating the experiments?

**DN:** I didn't personally because I was a division head at the time, I was a middle management, that I had the financial and the personnel and the building and facility responsibilities. You know, in my division was a vandegraph, three hyper-velocity guns, an electron [inaudible]—you know, a major facility that people came around.

**MM:** Yah, yah.

**DN:** So I was distracted.

**MM:** Assigned people to… did you assign people and oversee the work of people looking into it?

**DN:** I didn't assign people because the people just wanted to go and work it.

**MM:** Yah, ok.

**DN:** So I didn't have to. And in fact, the money in NRL comes from two sources: inside and outside. So it was raw numbers and remains about an eight hundred million dollar a year operation and roughly at that time, seven out of eight dollars came from the outside by individuals hustling other sponsors within the DOD and beyond. So the hundred million a year that came in through the top management was managed by the top management. I was middle management and I would route proposals from the individual scientists or small groups of scientists, but I didn't really have much control over the money.

So I could not dangle funding in front of somebody's nose and entice them to go work on it, but I didn't have to. People were interested and they just wanted to go off and do it. So work you know, started very, very quickly. Now again, I'll get back to that.

Staying with the theme of the activities outside, you know, it was almost a daily item in the newspaper that I remember one thing in particular: Georgia Tech reported that they had gotten positive results using some kind of a nuclear detector and then very soon after that, it could've been two days after that, they retracted it because it was noise in the detector, microphonics. Ok, and there were lurches and false starts and all kinds of things. On like May eighth, after the March twenty-third announcement, three major news magazines had cover stories on it. I've often wondered if the fact that all three appeared on the same day was a matter of you know, behind the scenes espionage so to speak, or you know, information stealing so they knew what each of 'em was gonna cover, but they had very different takes on it; but nevertheless, all on the same day.

And it reminded of the discovery of x-rays. I wasn't around at that time in 1895 but that had an immediate social impact as did cold fusion. I mean it went right from the science, you know, included but basically short-circuited the scientific publication, went right to reporters and the public. The, I knew about x-ray history because I had been an x-ray worker for you know, over two decades. And in fact I had the cover of a song sheet, a song entitled, "New Photography" and the cover for that song sheet showed a bathhouse on the on the beach and a fellow under a photogram outside presumably using x-rays to look through the bathhouse. And within a year, the announcement of the x-rays by Rankin, you could buy lead lined underwear, for instance. So you know, that had a dramatic, broad social impact and I say, I was reminded of that in this case.

Now the reporters got way ahead of the facts, there's no question about it. But the, I'm not faulting the reporters because they observed within the scientific community this incredible reaction. Now why was it so incredible? Well, it's because of the roughly million fold difference between chemical energies and nuclear energies. If you want to cause a chemical reaction to happen--consider burning, ok—you have only the supply, a small amount of energy like from a match, ok, and you get a chemical reaction. And in fact, it's a really good thing that chemical reactions do not in general go spontaneously because if they did, you know our clothes would burst into fire and so forth. And there's a reaction barrier in chemical reactions that is on the order of an electron volt that is, the energy achieved by an electron moving through a potential difference of one volt. And the corresponding barrier called the coulomb barrier in nuclear energy is on the order of a million electron volts, ok, more or less, ok. So what basically was being reported by Pons and Fleischmann is that they had triggered a nuclear—that is, million electron volt kinds of surmounting of the coulomb barrier and subsequent reaction energies with EVish kinds of ordinary energies, ok. And so the known nuclear physics at that time, which is still good, that is the reaction energy between say, two deuterons required energies on the order of forty thousand electron volts or equivalent temperatures I think of four hundred million degrees. And here's these guys saying, "Hey, at room temperature we can make this happen." So it was starling, ok, and the physics community basically didn't believe it. If it were possible to trigger these reactions, and they were the reactions that were seen at high energies, then there would've been so much neutron radiation that it would've been lethal—the so-called dead graduate student problem. And so for two reasons: one is it can't happen in the first place, and secondly, if it did happen, you're dead.

**MM:** What did think--because you had, you know, a more complex background—what was your opinion on it?

**DN:** That it was worth looking into. Ok, I didn't have an opinion on whether it was right or wrong or certainly the explanation of it. Here were experimental results that had been reported that could presumably be replicated quite easily; that is, it's gonna be a relatively cheap experiment on a tabletop!

So you go off and see if it's right. Ok, and a lot of work in that direction followed, and I say, I'll come back to that.

**MM:** Can you talk some more about when you—I don't want to interrupt where you're going now—but I would be curious about how many people at NRL you did oversee who worked on it and what they were doing.

Yah, I'll come back to that.

**MM:** You'll come back to that.

**DN:** So staying with the outside situation…

**MM:** Yah.

**DN:** For the time being, it was as exciting as can be. I mean it was tumultuous, I mean we were reading the Wall Street Journal like reading a physics journal trying to see what was happening. So the first couple of months right after were just tumultuous, there was all kinds of uncertainty and rumors and so forth. And then it settled down over the course of the summer, both inside and outside the laboratory.

The Secretary of Energy at the time appointed the ERAB—Energy Research Advisory Board—to look into it. They wandered around and they performed a study that was flawed from the start. It could not possibly have worked. First of all, people viewed these experiments as very, very simple. "They're off their… it's just chemistry on a tabletop." Well we know now that they're incredibly complicated, they require physics and chemistry and electrical engineering and good instrumentation; science, data analysis and material science and you know, very, very few people had all of these things in their own head in order to be able to do them. So you had to form groups and then you had interface problems, equipment problems, all kinds of things.

**MM:** Did you realize at the time that ERAB's methodology…?

**DN:** No.

**MM:** You didn't?

**DN:** No, this is twenty / twenty hindsight. Ok, so it was flawed both because it was as simple an experiment that could be done as easily as thought and produce results as quickly as hoped. So when they did their investigation, on a scale of a months and reported out, if I remember correctly, something like six or seven or eight months after the initial announcement, there just wasn't enough gestation time. That was problem one. Problem two was that a lot of people thought, "Well, if this is right and if we get in on the ground floor, this is gonna be a big deal and we will become rich and famous." So people were holding things close to the vest. So they, the ERAB folks toured around and visited appropriate places, but nevertheless, had no way to really fully learn the situation of what was known at that time. But not much was known.

Ok, I mean, twenty / twenty hindsight, we could see all that we know now that has been accumulated over the better part of twenty years and a very, very small fraction of what was known at the time they reported out has, is now in the short of valuable results, ok.

So you know, there were conferences, sessions at conferences. There was a conference in Sante Fe as I recall entirely devoted to the subject, I wasn't there but colleagues of mine were. There was a session at the American Chemical Society which Pons and Fleischmann gave a talk, among others.

**MM:** Are we still in all in eighty-nine?

**DN:** All in eighty-nine, as I recall.

**MM:** Yah.

**DN:** Ok. And that got a lot of attention, a lot of press and you know, it became increasingly evident that the neutron data that they reported was flawed, that you know, they're not nuclear physicists. This goes back to my fundamental point about people not having all the required disciplines and a guy named Rich Patrasso who was at MIT was particularly critical of that and properly so. Ok, he in my mind got a little carried away about it, but in any event, I think he was fundamentally right. So there was just a lot going on, and it was a very, very interesting time.

To go I think into ninety, the March meeting of the American Physical Society had a session on cold fusion in Baltimore and that was is widely viewed as the death of cold fusion. Nate Lewis and others stood up and reported inability to reproduce the results. They were very critical.

**MM:** Were you there too?

**DN:** Yes, I was there. And in fact, if you go into my office at home, there's a yellow sign about two feet by one feet with black letters on it that says, "Cold Fusion Session." At the end of the evening, I took the thing off and you know, it was going to be thrown away and brought it home. And there's a note attached to it by a colleague of mine from Naval Research Lab and it was probably put on in the eighty-nine / ninety time frame. He wrote "Dead" and pasted it, yah, so I still have that. It's faded, but I'll, it's a historic artifact right now.

**MM:** Yah. Can I ask you a couple questions just because…

**DN:** Of course, of course.

**MM:** I'm curious about you know, the fact that you were at, you know, that meeting and that you were so active at such a time. When you were going to those meetings, were you also, you know, armed with seeing the experimentation that your own people were doing and how did that contrast?

**DN:** Well, it contrasted in two ways. One is the experiments at NRL were not well along in the first year; that is, there'd been important things started and some of 'em had started to yield interesting data, but nothing was really conclusive. Ok, and so then by contrast you had the MIT and the Harwells and the CalTechs, you know, with a lot of people devoted to this.

**MM:** Yah.

**DN:** Just must man years, if you will. And they were already reaching conclusions that they were willing to state publicly. Ok, negative conclusions, ok.

**MM:** Yah, I just wanted to…

**DN:** So you know, we were just simply not ready to go public with anything yet based on any of the experiments that were going on.

**MM:** Did you feel that your experiments should be continued and were compelling?

**DN:** Yah, sure.

**MM:** Yah.

**DN:** Well and not compelling; they were work in progress.

**MM:** Right.

**DN:** Yah I mean, if the only way to describe it as compelling is that we were doing some new things that may or may not have worked out, but we hadn't 'em yet, ok? So that was a very, very memorable session at Baltimore. It was very crowded and people were cheering in a fashion that was very unusual for a physics meeting at the end of the talks. And I…

**MM:** Cheering pro-cold fusion or against?

**DN:** No, cheering the talks that were given, which were anti-cold fusion.

**MM:** Yah.

**DN:** Ok. And that spoke to the level of interest and tension in the field that people really wanted to know the answer, and when these people stood up and seemed to give the answer that it was enthusiastically received.

**MM:** And what about, I'm sorry, I'm just curious you were there and I would love to have been there, what about the contradictory, tell me who spoke and made the case and how that was received.

**DN:** As I recall there was one talk that basically was a progress report and I'd have to look back to see who gave it, but it wasn't booed but it was sort of ignored. I mean, Lewis is the guy that stood out and he stood out by virtue of calling Pons and Fleischmann names like, I forget exactly what but he was pejorative. So in any event, I was glad to be there. On one hand, I was in hindsight too accepting of the reports ok, because now we know that the some of these major reports, Harwell actually did get excess heat but so did MIT, but they fudged the results down. And then the situation at CalTech is less clear, it may be that they just failed to get excess heat, ok, but in any event…

**MM:** So that's interesting, at the time you were almost persuaded by what you were seeing in terms of [inaudible].

**DN:** Well yah, it took a situation that was largely neutral and tipped it towards the negative in my mind that ok, it'd mean that it was so clear cut that we should stop doing thing but it definitely did. Now if I knew then what I know now, that wouldn't happen because I'm aware now of course of you know, dozens of dozens of very good, positive results. Ok, but at that time, they weren't there. People were still working to get things working.

**MM:** Yah.

**DN:** Ok, and that's in the field as a whole and at NRL in particular. Could we pause now?

**MM:** Sure. Ok David, in that we were just talking about the, being at those meetings, I'm curious about what it was in you and the people that managed to keep an interdisciplinary perspective when you were getting such overwhelming, you know, false evidence that might make you wonder and you know, really forceful opinions from the physics community.

**DN:** Yah, I mean it was a combination of being at a laboratory in fact, being in a division, where we had these tremendous capabilities, lots of different disciplines. I mean if we wanted a neutron detector, we called up Gary Philips or Steve King and say,, "You know, can you come over?" And a neutron detector appeared. And it wasn't just a thing that we found in health physics that may or may not work; it was a state of the art neutron detector! You know, so we had these powerful tools available to us, A. And B, there was a certain combination of curiosity and arrogance that you know, basically said I'll believe what you're saying when I have a better base for believing it. And I get that base by doing experiments and they may or may not work out, but if I do experiments I'll know, you know, what might have gone wrong with others or might would've gone right with others and so forth. So I say it was an institutional setting A, and B, it was just human nature at work. Now…

**MM:** You were [inaudible].

**DN:** So there are a few other things I can say about the broader situation early on and then I'll get to what went on inside NRL over the years from the earliest times to recent times.

**MM:** Yah.

**DN:** So the big question experimentally was, you know, did Pons and Fleischmann really observe what they reported? And people realized that the deuterons are further apart inside palladium then they are in the d-two molecule. So it's less likely to fuse then it would just in the gas phase. Ok, both of which probabilities are infinitesimally small, but different, ok. So there was the experimental situation, but then of course theoreticians of all stripes looked at this from conventional nuclear physicists who calculated the miniscule probability ten to the minus forty, is in my mind a number you know, so small that it's meaningless.

The people who weren't nuclear theorists but who had some background in solid state theory and knew about screening of metals and the sort jumped at it, dismissed it, you know, said it couldn't be. But there were many other people, Chubbs being a primary example, that said, "Well you know, maybe there is something to this. How could it possibly happen?" So various theories arose and were discussed internally, presented, discussed externally and so forth. Normal machinations of science. Some of them were well known because they were presented at the international conferences and other venues, sessions at other conferences. And some were not widely known. There was a fellow, Fred Meyer, M-E-Y-E-R by name, who was a plasma experimentalist. He worked for KMS fusion in Ann Arbor for a long time. They were doing…

**MM:** Is that a private group?

**DN:** Yah, for a private company. It was a laser fusion company, ok. The KMS stands for Kip M. Siegel, I think it was S-I-E-G-E-L, a grossly overweight fellow who while testifying before Congress said, "Stroke" and we was right and dead. Ok?

**MM:** What do you mean?

**DN:** He died while giving testimony.

**MM:** Oh my goodness!

**DN:** Yah, had a stroke and died.

**MM:** Wow.

**DN:** So anyway, Meyer was and is a very good physicist who worked for them and then afterwards formed MARI, M-A-R-I, Meyer Applied Research Incorporated, his wife's name is Mary, M-A-R-Y, ok. And he informed me, he is a fellow I knew a long time from the laser fusion community, informed me that he had an idea that how this worked and ask some funding. So I went to the director, Jim Coffee, asked for a hundred thousand dollars to get Meyer. Coffee, a plasma physicist by background, knew Meyer and trusted him as I did, gave me the hundred to give to him.

**MM:** And how'd they deal with it?

**DN:** He was wrong (Laughs).

**MM:** Well, everybody's wrong! I'm curious, do groups like KMS Fusion, they sometimes contract with the government…

**DN:** Oh yah.

**MM:** Who basically would their, how would they exist financially?

**DN:** Well…

**MM:** Not just in terms of funding, but where do they sell their services?

**DN:** Well, they were a research company trying to make laser fusion go. Ok, so I think their funding if not D.C. angels, it was private funding, and the combination of the area not being near term commercial technology plus Siegel's death lead to the company going away. But I think Meyer left before it failed, in fact, to form his own one man company; MARI, M-A-R-I.

**MM:** Do you remember what area they proposed to you, that what…?

**DN:** They, KMS Fusion, was not involved with me, Meyer was.

**MM:** No, but I'm sorry, Meyer, for what particular trajectory that he had that you funded.

**DN:** Yah, he thought there was a form of hydrogen which was responsible for causing the cold fusion reactions, ok. Now you may know that Randy Mills in Pennsylvania, if I remember correctly, has this hydreno theory and it's a form of hydrogen with a deeply bound level that he has been touting. Now Mills was and is an interesting case, reported to be one of the smartest guys in science and technology and his theory didn't attract me at all; in fact, quite the opposite because if there were a deeply bound level in hydrogen it would be visible by ultraviolet spectroscopy and had not been observed. Now I was a spectroscopist, x-ray and ultraviolet, I knew the technology and it wasn't there. So that was unappealing. And then the experiments that he and his colleagues did with nickel and light water, as I recall, had gains and I forget whether they were power or energy gains, buts gains that bounced around almost from week to week, sometimes as high as thirty something! Thirty-seven is what I remember. It's probably wrong, but anyway, and one of his colleagues put upon me very vigorously to get in the car and drive up to visit them. I didn't do that because the experiment sounded flakey and the theory as well. So I ignored it.

**MM:** How do you feel about what you've seen of Mills' work over the years? Do you feel you were right about your assessment of Mills?

**DN:** Yes. In a word, no, you know, he got involved in heat pipes for cooling chips in laptops at one time. In fact, some colleagues of his visited me and left me with a copper heat pipe which is a wonderful piece of technology. It had nothing to be with LE and R. it was a company, a coal company called Thermacore if I remember correctly. But anyway, I haven't seen anything solid come out of Mills in all these years, you know in terms of a device or data that was very interesting enough to be able to want to reproduce their experiment.

But anyway, Meyer, even though he had been doing experiments that came as in the laser fusion area, was a very, very good physicist. He had a semi-retired solid state physicist named John Reitz, R-E-I-T-Z, who had been a member of the faculty at the University of Michigan, came up with this theory and pursued it. And if you, so Meyer went to some of the earlier conferences and is you know, he existed on a basis of consulting contracts in areas that I am not aware of. But he has remained interested in his field and is you know, still actively considering possible theories even though he's invisible to the community outside. Maybe because he doesn't go to conferences and give talks or anything. So that a hundred thousand that I gave him, later on I bought him a computer in order to run experiments, you know, in his home, electrolytic experiments. It was you know, from my perspective normal science. The government gives out a lot of contracts for research that turns out not to work that's rushed reward.

**MM:** Yah.

**DN:** It was certainly not dishonest in any sense. I'm absolutely sure that Meyer's a straight arrow and you know, gave good reports on what he had done and how it had worked out. So the work through outside, through the early nineties was underfunded and a lot of it was done by people who were just using their own flexibility within the organizations where they worked, for instance, Tom Plater at Los Alamos.

**MM:** Were you in touch with him a great deal?

**DN:** Oh absolutely, yah, and I'll come to that.

**MM:** Yah, sure.

**DN:** Ok so, it was an interesting situation. Now I could go on talking about stuff outside but I can tie that…

**MM:** More interesting to hear what you had your hands on.

**DN:** Yah, I can tie off the outside situation through most of the nineties and into the current decade by saying that you know, the conferences and the experimental results piled up but there was nothing of such character that it would attract the scientific community at large to say that "Oh, it really is right after all." That is, cold fusion does exist even though it's not understood.

**MM:** Yah, you know, I see, when you're talking about not addressed or you know reiterating things we already know about some of the stuff that happened outside—I'm more interested in your direct experience with it and your direct experience of watching the science unfold.

**DN:** I understand, but I was involved directly in the outside community. For instance, by funding Meyer and by doing other people's experiments.

**MM:** Yah. Now, then that's what I want, definitely.

**DN:** Well, that's what I'm talking about.

**MM:** Thank you.

**DN:** So I say through the nineties the outside community continued to do this and we interacted with them. In order words, what was going on in NRL was not going on in a closed enclave; it was embedded in the larger community, which is why I think this is important. Now I can turn to what was in fact happening within NRL and it did fall into the two categories I already mentioned; namely the attempt to replicate the electrochemical experiments and then other experiments. Now I'll come to the electrochemical experiments momentarily but in the division where I was, there was a branch headed by Graham, by then Fred Schmidt, later Graham Hubler called the Service Modification Branch and they basically used ion beams to treat surfaces in order to produce desirable qualities, be they mechanical or chemical or optical or electrical or whatever, so that they were well equipped with ion beam sources.

So there was an experiment done by a post doc named George Chambers and Ken Ribowski and I think Graham Hubler was actually involved in it--I'd have to look back—where they loaded not palladium but titanium with deuterons by shooting a low energy beam of deuterons at a thin film of titanium. This was done in a vacuum; otherwise you couldn't have had a beam of deuterons. And they had the, so there were two characteristics of this that were unique. One is to have a beam rather than an electrochemical flux, and secondly, there was a surface barrier detector immediately behind the foil so that if any energetic particles were emitted, they had a high had a high probability of being intercepted by the detector, nearly half, in fact. And the detector had the characteristic of being energy resolving. The height of the electrical peak, the pulse that it would put out would be indicative or related to the energy of the particle; more energy, more electron hole pairs, bigger pulse.

Ok and they saw peaks that indicated five megavolt energies coming out of an experiment in which the highest input voltage was to the order of five hundred EV, ok? And that would indicate that something, conditions were made such that a nuclear reaction was made emitted this. Now by use of absorbers, they determined that it was probably tritons, that is the second or third isotope of hydrogen after basic hydrogen and deuterium tritium. So that was, that experiment had a couple of characteristics. One is it was not reproducible and another is that it looked real.

I mean we looked at the data from that experiment as thoroughly as we could. We went back to the inventor of the surface barrier detector, asked him if there was any known artifacts that produced that kind of behavior. We looked into the veracity of the post doc involved to make sure that he couldn't be faking the data! Ok and did a lot of things. Now that was published as I recall at one of the ICCFs. I'm not a hundred percent sure of that, but anyway, it was published in an NRL report, a yellow cover report. So the data's available and it got to be rather widely known because it was you know, one of the new kinds of input experiments with a new kind of measure.

**MM:** That's just what I was about to ask. Was the idea of doing that to have it be applicable to the cold fusion investigation or was it that they were doing it in the surface area and just realized that this is related. How did that…

**DN:** No, it was entirely motivated by cold fusion.

**MM:** It was. Ok.

**DN:** So they basically said, "If the loading of deuterium into a material—palladium—creates the conditions, can we load deuterium into another metal and get similar conditions? And by the way, we're going to diagnose in a way that cannot be done in the electrochemical cells where the ranges of the particles are so short because of the mass of water. We could do it where we may be able to see something. So it was directly and dramatically sold and motivated by the cold fusion reports but was in a different genre. Now I…

**MM:** Was it considered a breakthrough when it, you know, or how did you react and how did it react, what was the reaction when you took it to the community?

**DN:** Great interest. Ok, but the fact that it wasn't reproducible was you know, was a strong mark against it. Now the titanium targets were later analyzed and it was found that the ones that had some levels in aluminum in them produced results and the ones that didn't tended not to. Later in some electrochemical experiments it was found and reported that having aluminum present would promote cold fusion reactions in the power and heat. But that was never run to ground. Ok, it was just sort of post-experiment enticing possible correlation but that's still a hang fire that's still out there but not understood. In fact the whole role of impurities is a subject that we should return to at some point because I…

**MM:** Do you [inaudible] a little bit now while you're thinking about it?

**DN:** No, let's come back because that's a cross cut thing.

**MM:** Right, sure.

**DN:** What I would like to say is that there was even then, in the early nineties and certainly now, so much going on in the field that it's hard to organize it. And the way in which I organize it is the input / output. Ok, there are four kinds of experiments; that is, the means to achieve conditions, and then there are four kinds of measurements to say what happens when those conditions are achieved. The input are the basic electrochemical experiments; that is, the interaction of a solid with a liquid. The gas experiments where material is put in the gas chamber, deuterium gas commonly, and that's the interaction of a solid with a cooler thing of gas. Then there, I'm sorry, with a hotter than a liquid, a gas, ok, vapor if you will. And then the next is the plasma experiments will discharge another experiment where it's the interaction of a solid, no not with a liquid, not with a gas, but a higher temperature medium—namely a plasma.. And then finally the beam experiments where you have an even more energetic situation of directed particles. So that's four kinds of inputs. I say electrochemical, gas, plasma and beam experiments. And then the output side, the measurements fall into categories first and foremost, excuse me, heat measurements, excuse me.

**MM:** Yes. Sorry about that. Tend to wear people out.

**DN:** That's ok. So I say the first measurement is a heat measurement, it's power and heat measurements. The second higher are nuclear ash or transmutation measurements. The third are energetic particles such as Chambers observed and the fourth are lower energy manifestations such as infrared images and phonons and [inaudible] things. So this experiment of Chambers was in a new box; it was in a different input—ion beams, and it was a different output—energetic particles. You know, of those sixteen combinations of four inputs and four outputs it was literally in a class by itself which attracted a lot of attention. Ok, so the Chambers post doc expired, he left, we had an NRL report, he turned out as a personal comment, if you look at what happens to people in this field you know, several people died in the field over the course of it, some of natural causes like Juliana Preperata, some of uncertain and possibly suicide like this fellow down at Texas A and M, Wolfe, and then of course Gene Mallovs murder, well of the people who didn't did but underwent various misfortunes, Chambers misfortunes, Chambers was one of those.

**MM:** What happened?

**DN:** He was tracked as an internet pedophile and jailed.

**MM:** Wow.

**DN:** Yah, it's a very, very sad story. And I don't know whether he's out of jail or not but it was truly a sad case because he was a good guy, smart, I mean and diligent and so forth; he just had a dark side and who lamented that, but it happened.

Now there was a second experiment done at NRL where the input was not electrochemical and it was a beam and it wasn't an ordinary gas; it was a glow discharge experiment. Ok now, several of those started, Clainters is the most well known, ok. But there are several, there are a number of them and they continue in the work that's being done Luge and the company Coolescence in Colorado.

**MM:** Yah.

**DN:** By Rick Cantwell and his colleagues. Now at that time, you know, I was a card carrying plasma diagnostician and Kucherov and Sabid Demova and others and Luge were reporting remarkable combinations of transmutation products and energetic particles. And in fact, I visited Kucherov in the ninety-two / ninety-four time frame. At that time, I was not working x-ray lithography or x-ray spectroscopy on one hand or really working LENR. Personally I was managing a program on Russian nuclear pollution in the Arctic, ok.

**MM:** Oh, that was then.

**DN:** So the pre-story was in 1991a fellow who worked Minazm, the civilian atomic, and it was deep within what was the former Soviet Union, revealed that the Soviet Union, or the FSU, if you will, former Soviet Union, had a trashed to the order of ten thousand containers of radioactive waste and sixteen reactors into the Arctic Ocean, some of them being shipboard reactors; most of them being shipboard reactors. Some of them still fueled in water as shallow as a hundred meters and when this came to light, the Alaskans asked the question: "What's between all that radiation?" You know, radiation levels, amounts far in excess of Chernobyl. Ok, and our multi-billion a year fishing industry the answer is "Water" so Markowski, the Alaskan senator, had ten million put in the Navy budget for each of the three years, ninety-two, ninety-three, ninety-four, to study the problem. And the money went through NRL and I basically grabbed seven million of it with my division head add-on to study the problem. So we had a very, very broad based thing. And again, it was the breadth of NRL. NRL had a marine geosciences division. It had an oceanography division. It had our radiation detection capabilities. So we did a number of things; one was supercomputer circulation calculations of you know, where contaminated water that originated near the island of Nvoya Zemaria which was the, one of the two Russian test sites where they popped off bombs, some of 'em as big as five megatons, if I remember correctly. Ok, what would happen to contaminated water? Would it just wind up, you know, sort of going around towards the northern reaches of Canada or would it come across to Alaska? And so that was one set of things we did.

Another was to mount expeditions to go and take samples, both from the water and from the headlands of the major contaminated rivers. There was a nuclear reprocessing plant in Cheliobinsk that was so contaminating the Tetra River that you could go downstream to some town and you could go into the school and you could tell all the kiddies with no left arms to line up at that side of the room, no right arms on that side of the room—due to the multiple generations of birth defects because of the radioactive contamination. Gary Phillips who was an actor in the experiments with NRL took a team to the headwaters of the Enasay River and took samples and so forth. So anyway we did field work and so on.

**MM:** Yah, you and I, we've talked about this a little bit and I have the paper that I have, you know, I'll talk to you later about.

**DN:** Yah.

**MM:** I think it was attention I called this to, but I just wanted to ask: while you were in Russia working on this, did you meet with Cucharov there or…?

**DN:** Well, the answer is yes. And so we had this program and as the manager, I went to meeting in Norway and Russia, both in Moscow and in St. Petersburg. And one of the trips I arranged to meet with Kucherov so he picked me up at a hotel. He was driving and we drove downtown into the heart of Moscow just simply to pick up faxes that he couldn't get at Luge, about twenty-five miles south of Moscow before we drove down there. And then we got there and I think I was the first foreign visitor and there was a lot of attention for the place at that time was already in a evident state of decline due to lack of funding.

**MM:** Because that had…yah.

**DN:** Yah, the money had declined so much that the grass wasn't mowed and it was dirty and it was something. But anyway, when I checked through security there, I had the feeling that all eyes were on me and it was interesting. But we went into Kucherov's place. He had a new computer with Windows on it which he had bought from Taiwan, a pirated copy of probably Windows and went into the lab and looked at his glow discharge setup, saw the detectors and we looked at data. I basically spent the day with him, ok. And I got a pretty good idea of the equipment that was being used and I think still is being used by Everena Savildamola, ok.

**MM:** Did you talk to her then?

**DN:** I don't think so. I'm almost certain I didn't; I think it was because she just wasn't there. It was no grand plan or anything. On of the fascinating things of that, as an aside, is that you know, I was a materials guy and they had a materials division. So it became possible and worthwhile to pay a courtesy call to the head of the materials division, which I did, and he showed me some very, very interesting materials which I could talk about at length but won't. Things I'd never seen before—refractory underdense materials. So we chatted for a while through an interpreter and then I pointed to a closter over in the corner, sitting on a chair and I asked about that. And he says, "I can't tell you about that." And anyway it turned out later, I found from Kucherov that that was still classified and there was a KGB in the room and the guy caught holy hell, not turning it around before I came into the room! Ok, I could guess what it was but I don't really know. But in any event, I had a very pleasant visit with Kucherov and the, I much appreciated it. It was a day.

And so we come forward to some time not long after that. the people still in Graham Hubler's branch, Graham included, were interested in reproducing that experiment. So we set up a vacuum chamber in which we could do glow discharge andi think we were using titanium targets so we would pump it down, backfill it, turn off the gas input and then run the glow discharge and sure enough the titanium soaked up the deuterium because the pressure went down. We could tell that that was happening. So then…

**MM:** Did you do it, I'm just curious, just to back track a little bit…

**DN:** Yah, go, fine.

**MM:** It's pretty new into the former Soviet Union so was there, had there been any exchange of information and how did that work and so when you reproduced the experiment, could you know, how did, were you at that point, was that one of the early examples of working with the Russians on…?

**DN:** Yes.

**MM:** Yah.

**DN:** I mean absolutely. I mean it was, on a scale of three or four years after the Soviet Union fell apart, and it very, very collegial. We arranged for Kucherov to come spend time at NRL. The first day he was in there he had tools in his hands and was redoing our experiment, changing the power supply and so what. And he stayed us, I think, if I remember correctly, the order of a couple weeks. And it basically, we had a very active interaction and transfer of technology. In the end it did not produce quote "positive" results. We didn't see energetic particles. We did not measure any transmutation products. I'm not sure we even tried to do that. It was in no sense a success and it was never reported, ok. I don't remember any papers or any internal reports or anything on that.

It had certain interesting personal things. Graham and Kucherov hit it off very nicely and at one point Chamber, Hubler, Kucherov and Nagel were in the hot tub on Hubler's back porch being hand fed chocolate dipped strawberries by his African-American wife (Laughs). It was a rather unusual scene. And anyway, just to tie off that connectivity, Kucherov later moved to the United States to the company Anico in Salt Lake City and I wrote a letter at one point that he was evil nuclear weapons designer and oughta be left, permitted to emigrate. And he was essentially their chief scientist for a long time.

**MM:** At Anico?

**DN:** At Anico.

**MM:** Yah.

**DN:** And for the last maybe a year and half or two, I forget, he's been working at NRL in Hubler's division, ok. So on anything and everything but cold fusion, no, he didn't let me want to do that exclusively. So he's been working on something else, whatever Graham…

**MM:** So he still lives here?

**DN:** So he's in Washington.

**MM:** That's great, because we could do an interview..

**DN:** Yes indeed, yah, that would be highly recommended.

**MM:** Absolutely.

**DN:** Yah, and no, I have no idea, he may work at the NRL though he's no longer working, as long as Hubler's there. Hubler's gonna turn sixteen next week. He was born on February twenty-ninth, ok. So he's a young fellow and is I say, both a personal and professional friend of Kucherov.

**MM:** Ok. Just you know, I know this is a digression, but people hearing the first part are gonna want to know the second part. Just could you quickly, because people will be curious, what happened with your work, I mean, I know the answer but I'd like it on the record. Of the Soviet nuclear waste, and where was that project left and what were the political back and forth.

**DN:** Oh yah, I mean it was a successful project in that the ocean circulation calculations showed that significant amounts of radioactivity would not get transported. In any event the containers of radioactivity, well, a large number, to the order of ten thousand, were not, didn't have that much. It was low level stuff. The high level still fueled reactors were poured full of a plastic cement called furfural which would delay greatly their ability to move into the environment. In any event, they were inside the reactor vessels themselves so, and some of the reactor products, plutonium for instance, form a material that is like, that is as insoluble as gravel. I mean it just, that plutonium bond that went down in the crash in Tulee, Greenland or some place in Greenland near Tulee, you know, contaminated, spread radioactive material around but it just squats on the environment, it doesn't move, ok. So A, the material didn't, wouldn't get out soon; B, the stuff they could get out was relatively low level and in any event, it's not going to be transported into the area where the Alaskan fish are, breed.

**MM:** Could you…

**DN:** So we could say with high confidence, it's not a problem to Alaska.

**MM:** Did it end up being not a continued practice and would you recommend it not being a continued practice?

**DN:** Oh, it should not be. The radioactivity can [inaudible] stored on land and I'm not talking about Yukamom, I'm talking about just repositories. So it should not be dumped in the ocean, in my opinion.

**MM:** Yah.

**DN:** Even though that particular thing was not a problem. Now the science was wonderful. We found all kinds of things out about the movement of oceans due to studying these sore thumbs if you will, that stick out in the environment because they're not normally found in the environment. Ok, and I could go on and on about that, but.

**MM:** No thanks. I just wanted, I just felt since we had… Back to the now, the glow discharge.

**DN:** So we've talked about two experiments at NRL so far; the Chambers ion beam experiment and then the attempt to replicate the experiment at Luge, the Kucherov thing. There was an electronic warfare division at NRL, ok. It's a large division, it's one of the product oriented divisions at NRL and it's distinct from the research division.

**MM:** What is that? Sorry, let me ask you…

**DN:** So electronic warfare of course is listening to other people's signals or broadcasting your own signals in order to gain a military advantage. Electronic warfare, it's you know, the electromagnetic analog of information warfare where you receive and broadcast information over normal means. So the electronic warfare division was a product division because they made systems that would be prototypes for systems to be built for U.S. aircraft, ships and submarines. Ok, nd I say, that's distinct from research division. Our product was papers and pads, ok, they produced systems, ok, that were in another part of the laboratory. And in that division was a semi-retired guy by the name of Sidney Smith, ok. And he was just fascinated as could be by this, and he wanted to apply his expertise, namely vacuum tube technology to the study of cold fusion. And he was…

**MM:** That's wild.

**DN:** Contemplating impact experiments where he would basically take a deuterium loaded material and hit it with a hammer to see what happens! No, this is not entirely crazy because some of the early attempts to explain this were involved, what are called fractofusion; that is, the opening of cracks in materials that would lead to high fields and acceleration of particles maybe to the energies they could produce fusion. Ok, turned out that that was a non-starter. It got a lot of play at the conference in Santa Fe but it didn't go anywhere.

But so anyways, Smith was a prototypical lab animal. He wanted to do something. So it was like a hormone laden teenager full of energy but not knowing where to go. So I basically said, "Why don't you reproduce the Claytor experiment?" ok, it involves the kind of things you know, vacuum technologies and you know filaments and so forth. And "Ah, good idea Dave." So set off down that road, ok. And this was done entirely with the cooperation of Clatoer. I had visited Claytor at Los Alamos. I used [inaudible] is very familiar with it. They'd pay me to go out [there every week and I'd just go around and talk to friends about hot things in science. I'd give an occasional lecture. So Los Alamos was a well known [inaudible]. I stopped at the same Dunkin' Donuts on the way up there every year in Sante Fe. So I visited Claytor and it was interesting. I saw his setup for the first time.

**MM:** What year was that?

**DN:** That's a good question. I don't know exactly but it was probably in, it was the first half of the nineties, ok, and I would have to look back to be more precise on that. but so between seeing Tom at meetings and visiting him, when I called himup and said, "Hey, we've got somebody who would like to reproduce your experiment" he was very helpful, even sent us equipment and materials and so forth. So that setup, run and led nowhere. It was a failure in a sense that it didn't produce measurable tritium, ok. Now I'd have to look back to see if we had the same kind of tritium detector, the Femto-Tech inline tritium detector that Tom Claytor was using. But it was you know, on the list of experiments at NRL that were A. done, B. done reasonably well and C. didn't go anywhere, ok.

**MM:** Yah.

**DN:** Yah. And Smith is dead now. He retired, I mean fully retired after a while and then I heard later that he died. Now when you back off and look at things, there a number of weapon experiments, as electrochemical experiments. There was still other things that were done at NRL that this is a reasonable stopping point.

**MM:** Yah, probably there.

**DN:** When we get through all of them, you will see that almost everything that was attempted at NRL did not really work, ok, in terms of giving positive and publishable results.

**MM:** Do you feel that nonetheless it taught you a great deal?

**DN:** Oh absolutely. I mean we basically knew what the problems were and what could be done, what shouldn't be done and so forth. So you one could interpret that in a negative way, saying that it's additional evidence that there's nothing to this field, ok. That's a fair assessment but I will end now by telling a story.

John Bakras at Texas A and M had a conference on cold fusion at one point in the University and he wanted to have a sequel to it. And again, this was in the mid-nineties, I don't know exactly when. But he invited me to come down. So I flew down to some Texan Texas, some small plane went droning across the state to this outpost, to College Station where he worked. At that time, he was having enough troubles within the University that he was disinvited to hold it on campus, so he had it in some hotel off campus. And anyway, I gave a rundown of the experiments that had been done or were underway at NRL and the same thing I just said, anyway, that they in general did not lead to positive results so he referred to NRL as a black hole for cold fusion! And I took offense (Laughs) and called him on that and later on he sort of apologized to me at the end of the meeting. But that's just an amusing thing. I have been in correspondence with him.

**MM:** Was he serious or was he joking?

**DN:** He was serious. He says "You know, based on what you've said Dave, NRL is a black hole for cold fusion." (Laughs) So anyway, there's a lot to be said about the electrochemical experiments that have followed but that will remain for another time.

**MM:** Yes, I'll come back soon and we'll resume.

**END OF INTERVIEW**

## Attachment II. David van Keuren Interview of David Nagel, March 2000

**An Oral History with Dr. David Nagel**

By Dr. David K. van Keuren

van Keuren: It is 2 March 2000, and this is David van Keuren. I'm sitting in the office of Dr. David Nagel at George Washington University, and we are talking about his life and career.

David, I know this material is covered in the background documents that you gave me, but I want it to be covered in the interview, so could you tell me, please, when and where you were born?

Nagel: I was born in Aurora, Illinois, which is not far from Chicago, on the twentieth of February, 1938, in St. Joseph's Hospital, actually.

van Keuren: And what did your parents do?

Nagel: My father was a carpenter his entire life, from a very early age, probably before he was twenty, until he died at age ninety-one, when he still had some unfinished carpenter jobs. When he was very young he worked as a farm hand and had lots of stories to tell from that time, but his core expertise was carpentry. He worked on many jobs over the years, sometimes for the railroad, the Chicago Northwestern Railroad, which ran from his hometown of West Chicago, Illinois, into the Loop in Chicago and back, the commuter run. Later on, he worked for various contractors on major projects and in fact was a carpenter of the Great Lakes Naval Training Center which was built during World War II, as I recall. He tells the story that he would go to work with a set of tools every morning and come home with a set of tools every night, not always the same set of tools. In fact, if you went through his garage in the years 1950 and after, you would find many of the tools there painted orange so he could spot them on the job if somebody had swiped them from him. He set up a company, E. A. Nagel, Incorporated...

van Keuren: What was that again?

Nagel: His company was E. A. (Edgar Anthony) Nagel, Incorporated, which was a general contractor, so he contracted to build houses, garages, and other buildings and also did renovations and repairs.

van Keuren: Was this in Aurora?

Nagel: This was in West Chicago, Illinois. While I was born in Aurora, which is the city my mother's from, I lived my entire young life in West Chicago, Illinois.

van Keuren: Is that a suburb of Chicago?

Nagel: Yeah. It's thirty miles dead west of the Loop.

van Keuren: And what did your mother do?

Nagel: My mother never worked outside the home. She was a "homemaker", and she lamented that later in life, but that's the way it was, and it's gone full swing in three generations from her never working outside the home to our daughter who's entirely a professional, and my wife is in-between, working sometimes and not others when our kids were young. She was a skilled seamstress, knitted very well, did lots of projects like that, did something later in life that struck some of us as fairly curious. When I was on active duty in the Navy in the early sixties, I went to the Antarctic and took a lot of slides, and my mother would give talks on my Antarctic experiences using my slides to men's clubs and other organizations in town, which was a facet of her personality that surprised most of us who didn't know she had it in her, so to speak. And one other similar story is that when she passed away at age eighty-three in 1991, my father was left to cook for himself, and he turned out to be quite a good cook. And the reason that was is because his mother had died when he was eight years old, and he had a brother and sister. They divided up the chores, and he wound up with the cooking chore. So he actually was a functional cook--another surprise to all of us.

van Keuren: Did you have any siblings?

Nagel: I have a brother who's two years younger and a sister who's four years younger than I. My brother, Dennis Michael by name, is an electrical engineer. He's a one-man company in Boca Raton, Florida, and does contract work making microelectronics. And, as an aside, since I have become associated with an electrical engineering department, our interaction is even richer than it was. My sister married a fellow from college who manages a steel fabrication facility in the middle of Illinois (Bloomington, Illinois), and she and her husband have three kids, two now married and one still looking. And she has done some work outside the home as a consultant on interior decorating, but basically her lifetime work was a housemaker like her mother's.

van Keuren: Can you tell me about your early education? Where did you go to grade and then high school?

Nagel: I went to both grade school and high school near where I lived in West Chicago. The grade school was St. Mary's, a Catholic school about three blocks from where we lived, and we could walk to school and back. I also went to Scouting functions there as a member of Boy Scouts. And the high school was across town, but that was just a short bicycle ride--the town is only 7,000 people. It was West Chicago Community High School--the We-go Wildcats, it was called--We-go (W-E-hyphen-G-O), short for West Chicago, and the team was called Wildcats. And I took a fairly standard series of courses. It turned out that I never took Latin, for some reason. They had no other languages in high school. But I did take typing, and that was critical, and shop and the normal science courses. I was particularly fond of chemistry because of an inspiring teacher, George Barthels by name.

van Keuren: Why don't I get into that? Did you have any favorite teachers and subjects?

Nagel: Yeah, this guy Barthels.

van Keuren: Spelling?

Nagel: B-A-R-T-H-E-L-S, George Barthels, was extraordinary, and I was literally inspired by him. The math teacher....

van Keuren: He taught, again?

Nagel: He taught Chemistry.

van Keuren: He taught Chemistry.

Nagel: Right.

van Keuren: Okay.

Nagel: The math teacher was named Marie Balzhiser, B-A-L-Z-H-I-S-E-R (Balzhiser), as I recall. And she was very, very competent, and I enjoyed that math tremendously because of my bent towards it on one hand and her ability to handle it on the other.

van Keuren: These were high school teachers?

Nagel: These were high school teachers.

van Keuren: Uh-huh.

Nagel: In grade school I had nuns from the Order Sisters of Saint Joseph as teachers, and some of them were very memorable, especially the one that would carry my brother around the room, when he misbehaved, under her arms.

van Keuren: When did you first acquire a predilection for science?

Nagel: There's a person named Jack David Donahue, D-O-N-A-H-U-E, who was interested in chemistry, and we were both in the chemistry class, Barthels' chemistry class, and became friends, and I credit him more than any one other person with hooking me on science. We were encouraged and abetted by the fact that in this little town of West Chicago, by just a happy accident, there was a rare earth processing plant that was named Lindsay Chemicals, L-I-N-D-S-A-Y Chemicals, and it was one of the very few plants in the entire country that would take the raw materials called monazite (m-o-n-a-z-i-t-e)--that's a mineral name--sands from around the world and haul them all the way to the Midwest to process them to get the rare earth metals out of that ore and then to separate them from each other, a real challenge because of the similarity of their chemistry. So, Donahue and I would go to Lindsay Chemical Company and talk to the chief scientist after school, and he would give us chemicals, and we would buy other chemicals from supply houses, and we both had basement chemistry labs. I remember very clearly the day when my chemicals ate through the porcelain sink in the basement. There was a great deal of excitement in the family that day. We made gun powder. I remember drying it in his mother's oven one time, and she found that rather unacceptable when she got home. So anyway, it was a combination of a friend who liked chemistry plus this teacher Barthels.

van Keuren: Is there any history of careers of science in your family?

Nagel: Not at all. In fact, my brother, sister and I were first- generation college. My father did not start high school--no, my mother did not start high school; my father did not finish high school, and neither of them entered a college. So we were first- generation college with a first-generation scientist.

van Keuren: Did you have any other hobbies or pastimes in grade school and high school?

Nagel: Yeah. We lived a very outdoors kind of existence, and it was not untypical for my friends and myself to jump on a bicycle on a Saturday morning with a 22 rifle, a box of shells, and a lunch and go off saying, "We'll be back tonight, Ma." And we'd go off and shoot at things that we should have and things that we shouldn't have. So there was a lot of plinking, it was called, a lot of hunting and fishing.

van Keuren: Plinking?

Nagel: Plinking is just recreational shooting. We'd go out to the dump and shoot at bottles, shoot at rats, just shooting for the heck of it. And we did a lot of hunting. We hunted virtually everything that moved: frogs, squirrels, rabbits, pheasants, quail. We ate most of these things. My mother was a pretty good sport in cooking these things--also turtles, a lot of turtles. Ate a lot of turtle soup. The turtle's a very primitive animal. When you butcher it, the heart will beat for an hour on the table after it's cut out of the animal. I, also, because of an association with another friend, Neill Neumann, ran a trap line.

van Keuren: Neill?

Nagel: Neumann is spelled N-E-U-M-A-N-N. He and his father were very outdoor-oriented, and he interested me in trapping. So for a couple of years during the trapping season in the cold weather when the mink and muskrat did not have young and when their fur was well-developed, we'd set traps, get up at five in the morning and go out on my bicycle and check the trap line and then check it again. After work I would skin the animals and turn the pelts inside out and dry them on a board and sell them usually to Sears Roebuck. I'd get $20 for a mink and $2.25 for a muskrat. It was decent money for a kid in the fifties.

van Keuren: What type of student were you?

Nagel: I did well all through grade and high school. In grade school, I apparently did well enough to wind up tutoring slower students, and that's in fact how I got to notice the guy Neumann who did as well as I did, because we would wedge ourselves in the same desk, and I would help him with his math and everything. It wasn't that he was dumb, he was just not as fast. But despite general academic success in grade school, I got Fs in handwriting almost consistently, which was a source of real consternation for my mother. She, as a youngster, had learned the Palmer method of writing, which was spelled P-A-L-M-E-R, and there were laborious exercises of writing scrolls and other forms in order to train the hand and the brain to produce pleasing handwriting with conventionally shaped letters. And the result of her experience was that she had a remarkably wonderful handwriting to a very old age, but I never bought into the conventional letters, and I would have a mix of printing in my own inventions and what was taught and so forth, so I got lousy grades. As I said, the combination of having this F stick out on my report card, plus my mother's interest in handwriting, caused me trouble. In high school I did well also and would have enough spare time that I could be an audio-visual assistant. So I would set up PA systems and show movies in other classes because I could afford to skip some classes. I remember clearly showing a movie one time that was provided by a local company of a surgical operation on a patient when they amputated a leg, and another when they installed some plates and screws to reconstruct a damaged leg. And the reason I remember it is because I turned on the projector, turned on the lights, and this is a biology class, and I was riveted by the movie, and when I turned on the lights almost nobody else was left in the room. Most of the people had gotten sick and departed the premises. But, anyway, I wound up second out of ninety-eight in my high school class, as I remember. I still remember the name of the girl who beat me out: Evelyn Munson.

van Keuren: Spelling?

Nagel: M-U-N-S-O-N. I have no idea what happened to her. Subsequently I think she got married and had a family, but in any event I wasn't happy with second.

van Keuren: Where did you go to college?

Nagel: I went to college at the University of Notre Dame near South Bend, Indiana, and how I got there was kind of interesting. During the senior year in high school I became aware that the Navy offered ROTC scholarships, which was essentially a ticket through the university. They would pay tuition, books, fees, plus $50 a month towards college expenses, room and board, and the like. So four of us applied and took the test, and all four of us passed it. And then two of us made it by the physical. It turned out that this guy Donahue had happened to have a wart at the time he took the physical and was disqualified on the basis of that, so he went off into an academic career, research career, and didn't spend any time in the Navy. I forget why the other guy flunked out; I think it was a color blindness issue. And the two of us that made it through the physical as well as the mental tests both wound up getting ROTC scholarships. The other guy's name was Don Doherty, D-O-H-E-R-T-Y, and he went on to work for Rickover's outfit--in fact may still work for them in the Washington, DC area here.

So I had this Navy scholarship which could be tendered at any one of dozens of universities across the country, and I was interested in going to the Illinois Institute of Technology in Chicago, which was a first-rate school. By that time I had decided that I was going to go into engineering because even though I liked chemistry a lot, I didn't think I had it to be a research scientist, and besides engineers made good money at that time--still do, in fact. So I was in the basement one time where my dad had his office, and we were talking about this. And out of the blue my dad said, "Did you ever think of going to Notre Dame?" And I hadn't, and it was particularly true because as a kid I used to root for the football team from Northwestern near Chicago, where in fact my dad ushered on Saturdays as a hobby with his friend, and my brother rooted for Notre Dame. So I wouldn't go to the school my brother rooted for. So, in any event, my father sort of brought me up short, and we got talking about it. The more we talked about it, the better an idea it seemed, and he said that he had a subcontractor, a guy who was actually a tinsmith, who had gone to Notre Dame and would be willing to write a letter for me. So the shortened story: I applied to Notre Dame, and this guy wrote a letter, and I got into Notre Dame, and a few years later I was being interviewed by the Captain who was the head of the ROTC unit, and he opened his little book on me, and I heard him say, "Oh, yes, we had a letter on you." So I'm of the strongly held opinion that the letter written by this tinsmith was what got me into Notre Dame and set me on the path and led me to meeting my wife.

van Keuren: Hmh! I wonder why that would be. You'd think academics would be key.

Nagel: Well, my suspicion is that they had at that time--I know it's still the case--as many as five or more times as many applicants as they could accept, so that they had lots and lots of people with the academics, but then it was a matter of, you know, the other criteria, like diversity and the family members who already went, or somebody went. So, anyway, I didn't have a family member that went, but somebody knew me, could spell my name right, and wrote a letter.

van Keuren: I see. So when did you commence your college career?

Nagel: In 1956. I graduated from the West Chicago High School that year.

van Keuren: West Chicago?

Nagel: Yes. West Chicago Community High School, 1956. Then I went to South Bend and joined the freshman class in the fall of that year.

van Keuren: What was your course of study at Notre Dame?

Nagel: I started out in Chemical Engineering--chemical because I like chemistry and engineering, as I've already said, because I thought I could hack that. I might not be able to do research competitively. And it was entirely satisfactory. The first year was a general engineering background: the usual mathematics and physics and so forth. I didn't have any trouble in that. I remember very clearly when I started Notre Dame, I was fearful that I would be competing with a lot of people from the prep schools, especially in the East, and here I came out of this small town high school and wasn't at all confident that I would be able to even survive let alone do well. So I just hunkered down and worked like a nut, and that meant seven days a week. I would get so tired, I remember, that sometimes--all my life I've taken a nap after dinner--in the evening I would be so tired I'd just sleep right through the night and get up the next day and start again. So I didn't date. I didn't do anything recreationally except go to a few home football games during the year. And when the dust settled I found I did very well after the first year. It just continued on. Then in the chemical engineering curriculum it became clear that I was going to spend a lot of time in a course in a part of chemical engineering called unit operations, and that means the processes that are done sequentially, unit by unit, in a plant like an oil refinery or something. I started to look at that. I think it was, more than anything, poor teachers or advisors, but anyway it looked to me like a plumbing course.

That's very pejorative, but anyway I couldn't see the intellectual content of it and so forth on one hand, and on the other hand, just at the end of my sophomore year, the engineering school at Notre Dame instituted an entirely new curriculum called Engineering Science. You can think of it as Engineering Physics. I had loved physics, done well in chemistry and so forth, so anyway, at the end of my sophomore year and at the expense of losing some chemical engineering credits, I jumped into the new curriculum, and that's what I graduated in.

All this time, of course, I had ROTC every semester: a whole course on navigation, a whole course on leadership, a whole course of missiles, et cetera. And as a result of switching majors as well as the ROTC, I wound up averaging more than twenty-one credit hours a semester for the eight semesters. I remember on one occasion I took five final exams in one day. So it was very, very labor intensive. And in the end I wound up eighteenth out of a 1000+ people, and it was kind of interesting, that's the two percent level, essentially the same level I was at in high school. And I was surprised by getting a theology award at the end of my senior year, and that was entirely an accident. I had taken some religion courses; it was interesting to me, and I had done well enough that they instituted a so-called Cavanaugh, C-A-V-A-N-A-U-G-H Award that year, and I was the first recipient of it, which remains a surprise. I also wound up first in my Naval ROTC class and got some kind of award for that. And then there was a senior competition in the engineering school, and I did a study on the best way to use a lead pencil (you know, you have an option to use it a little and sharpen it right away or to use it all the way down to the wood and then sharpen it). Anyway, there's an analysis that can be done to show what's best there. And I did it, wrote it up, presented it, and I won an early electric watch. The watch was so big, and I had very skinny wrists, that I don't recall ever wearing the thing. But in any event I wound up with a Bachelor of Science in Engineering Science and some awards--an entirely satisfactory experience.

van Keuren: What was the science faculty like at Notre Dame?

Nagel: They were outstanding. The College of Science, I believe it was called, at Notre Dame had a long history that included (my recollection is) the initial vulcanization of rubber was done there, okay? And that was, you know, a very, very significant technological breakthrough. They also had a radiation biology lab that was important on a world class. So the quality of the education was very good. I had one utterly outstanding chemistry professor. Again, my first year, Rudolph Bottei, B-O-T-T-E-I by name, was an amazing guy, both in terms of his ability to teach as well as his requirement for discipline in the lab: cleanliness of the glassware and things like that. So in any event I got very, very good instruction in chemistry. The physics people were good. There was one particular mathematics prof I had seven semesters in a row. His name was Robert Weinstock, W-E-I-N-S-T-O-C-K. And he took me from the initial assumptions and analysis course all the way through calculus and variations. He was interesting because we would stand around after class and talk for a while, and of course there was only a ten-minute break between classes, so the next class would want to come in and he'd say, "Look, if you want to continue talking to me, you have to beat me back to my office." So we would race across campus. Now, he would do that because he was a former cross country runner. If we beat him back to his office, he'd talk to us some more.

van Keuren: Any other professors who made a particular influence?

Nagel: Yeah, there was a priest named Charles Harris, H-A-R-R-I-S, who taught quantum mechanics, and he was also the rector of the hall in which I lived. There was dominantly on-campus student housing at Notre Dame, so the first year I lived in a place called Cavanaugh Hall, the same name as that award I got for Theology, so that was some priest who had made his mark at Notre Dame, and then the second and third years I lived in a dormitory where the resident priest was this Father Harris, and I got to know him not only as a teacher but as a friend. That was Howard Hall, and I had a very good time. The other thing that should be said about my time at Notre Dame is that I hung around with three people, not only dominantly but almost exclusively, okay? They are a guy named Robert Fulton, same as the inventor of the steamboat. He and I roomed together for all four years, and then we palled around with two other roommates, a guy named Andrew Poltorak, P-O-L-T-O-R-A-K, and his roommate was Fred Picchioni, P-I-C-C-H-I-O-N-I. Picchioni was in the Air Force ROTC, and he had to walk from one end of the campus to the other, and that was interesting because he was a very short fellow with short legs. He'd go like crazy.

van Keuren: What about your ROTC courses? What were they like?

Nagel: Well, they were interesting. The Introduction to Naval Science, I think, the first one was sort of an orientation course and then the Navigation course was a very mathematical course because at that time the sextant was still used for navigation and one had to spend a lot of time looking things up in tables and the like. And I found that fascinating. And it turned out to be also very useful for me, as we'll get to later. The Leadership course that I mentioned already was the single most valuable course that I took in all of the courses in all of the years in college, undergraduate and graduate. I didn't realize it at the time, but it was certainly the case with so-called twenty/twenty hindsight. It was an Applied Psychology course, and what we were taught during that course was every bit as germane to dealing with coworkers and with family as it was to dealing with sailors. And I really value that course and count

myself as very fortunate in having taken it.

van Keuren: You also said you took a course on missiles.

Nagel: Yes. There was an entire... missiles were relatively new at that time and very important. It was clear that they would play a significant role for the Navy, in addition to guns, forever. And there was a course on missiles. You divide a missile--I still remember--into four components: the airframe (the structure, if you will), the propulsion, the guidance, and the payload (the warhead). And we would go through these things systematically and study them. We got involved in control groups, and it was quite technical and very interesting. In fact, another part of my good fortune there is that the technical aspects of naval science were sort of a piece of cake for those of us in engineering, where the people who were ROTC from arts and letters and from other curricula who didn't have an engineering background really had a hard time with it.

van Keuren: You graduated in... ?

Nagel: 1960.

van Keuren: 1960. Did you have to write an undergraduate thesis of any sort?

Nagel: No. The closest thing to it was that voluntary paper on how to use a pencil, but no, there was no requirement for an undergraduate thesis. There was an engineering magazine at Notre Dame published by the students, and I wrote a couple articles in that over the years. I remember writing one on the Mt. Palomar Observatory (P-A-L-O-M-A-R), the sixty-inch telescope that's still in use in southern California. I know I wrote a couple others, I forget about what now, but anyway, I did that, but no thesis requirement as such.

van Keuren: You did a number of summer cruises for ROTC.

Nagel: Right.

van Keuren: Do you want to tell me about that?

Nagel: The summer cruises were a requirement, and they were some of the most memorable experiences in my entire life. The first one was set against a background of never having seen the ocean and never having seen a ship. Okay? So I packed a sea bag with all the proper uniforms and things in it and got in a train and went from Chicago to Norfolk, Virginia. And I saw the ocean and saw ships. I reported aboard the Battleship Iowa, USS Iowa BB61, along with about 2,000 other midshipmen and set to sea. Went down to Rio de Janeiro. My job on the ship was essentially two different things. The routine job was scrubbing decks, wooden decks, so I'd get up in the morning in my working whites and after breakfast join the line of sailors, both midshipmen and ship's company. The ship's company, so-called deck apes, were generally sailors who were presented with an alternative at some point either to join the military or go to jail, and I learned how to swear that summer, incidentally--an interesting skill, actually. What we would do is form a line, and each of us would have something called the holy stone which was a brick-like object, looked like concrete about six inches square and two inches thick, and it had a dimple in the center. Then we'd have a broom handle. And the foreman, the leader, of this gang would hose down the deck with salt water and throw sand around, and we'd spend the entire morning bent over with this broom handle stuck up under our armpit grinding it back and forth on the teak deck to make it nice and white. Then I'd go to my second job, namely battle stations, where we would fire guns, and sometimes I was inside a five-inch gun mount, and it would, when we were having this general quarters it's called, this battle station practice, it would track objects. And I remember that we'd spend hours and hours inside of this gun mount. Sometimes it was very easy to sleep. I would wedge myself down between the bulkhead where the shells came up, and I could sleep very well. I got so skilled that I could sleep when the gun mount was rotating jerkily, but of course if we were going to have to fire then we woke up to handle ammunition shot. The more interesting job was in the number two tour of the sixteen-inch gun where we would actually fire these giant ton and a half projectiles.

So, the way the gun worked is the three guns would come to a certain elevation, and we'd turn a wheel on the back of the breech partially to disengage it and swing open the breech door in back, then the hydraulic trough would unfold to bridge to the breech, and the sixteen-inch diameter six-foot tall ton and a half projectiles would come up on an elevator from down below in the turret and be tipped forward and rammed into the gun by this hydraulic ram. Then the ram would withdraw, and the doors near this trough-like bridge would open, and six fifty-pound bags of gun powder would roll out. The grains of the gun powder were about as big as my thumb, and the bags were fifteen inches long and six inches in diameter, roughly. Anyway, they would be pulled into place on the trough manually, and then the ram would insert them behind the projectile, and then the ram got out of the way, the bridge would unfold, the breech would be closed, the gun would go back to the firing position, and that entire sequence could be done in a couple of minutes, okay? Then the gun would fire. All we would hear inside the turret was a rumble and then see the gun recoil towards us, and then we'd go back in position and restart the whole thing over again. Now, if you go forward into the eighties, I think it was, there was an explosion in that very turret on that very ship that killed forty-six people. And the investigation of that, which involved a tremendous amount of effort within the Navy Lab system and Sandia and other places was very contentious technically but also a problem because it was thought that one of the sailors might have initiated an explosion voluntarily because of some interpersonal relationship problem. My idea has been--and I've never taken the time to check this out--but my suspicion is that the projectile was not rammed fully forward and that when the gun powder in the bags was rammed into place, it was in fact pushed against this ton and a half projectile with enough force to break some brains and to initiate an explosion which then killed those sailors.

van Keuren: I think I remember hearing about that. I think the Navy was taken to task for its rather facile explanation.

Nagel: Yes. It was mishandled on two levels, a political level, if you will, in terms of this potential problem with the sailor, and then the technical aspects of it weren't handled in a very clean fashion either. But, anyway, to pull these two jobs together, the deck-cleaning job and the gunnery job, when those guns went off all of that silt that made up the bags, held the gun powder, would be charred, turned into something that was then discharged onto the deck, making our nice clean deck full of black charcoal-like spots, and we could solve that very easily. We would just go out and scrub it down again.

Van Keuren: Hm. Any other cruises that had a particular impact?

Nagel: Well, the second summer was a split summer. We spent three weeks in Little Creek learning how to be Marines and three weeks at Corpus Christi for aviation training. And the reason for this is because upon graduation we had the option of going into the line Navy--that is, being a boat driver, or going into the air Navy, or in fact going into the Marines. Now, the time in Little Creek was during a crisis in Lebanon, and the ships that we were going to have to embark on and work with were in fact involved in real-life military operations at the time, so we didn't spend much time at sea but we spent a lot of time crawling around on beaches with guns. It was, I remember, a very uncomfortable time. It was hot and full of insects and not a whole lot of fun. By contrast, when we went down to Corpus Christi we got to fly in sea planes out over the Gulf of Mexico, and that was very interesting because we would drop flares and then shoot rockets at them and the like. I remember two things from that experience. One was the occasion when we dropped the flare and the midshipman who was at the controls flew over that region, and it would disappear behind the bulbous nose of the airplane, and he'd count to some number, ten maybe, and then nose the airplane down, pickle off the rocket, and he mistook the wake of a fishing boat for the smoke from the rocket, so he actually shot at a fishing boat instead of the smoke flare. And the other thing that was memorable about that is we were given no ear protection, but onboard this P-5 sea plane was a 400 hertz generator, standard airplane generator, that winds so loudly that although we didn't know it at the time our ears were numb to the point where we couldn't hear for a couple of hours after the flight. We'd get off and talk with each other and nothing was coming through. I look back at the things that I've been exposed to either willfully or not, that is one of the worst insults I've ever had to my body. Apparently there's no lasting problem.

Now, the third summer was on-board the carrier USS RAINGER. The cruise was from Norfolk to Quebec, so it wasn't as interesting as going to Rio, from a tourist viewpoint, although we enjoyed Quebec. I have a couple of strong memories from that. One is that my bunk was right underneath the flight deck and very close to one of the catapults, so if they were having nighttime ops when we were trying to sleep, the catapult would keep us awake most of the night. It would launch forward and then rattle backwards as it was withdrawn. But the other thing, I had two flights, one in an S-2, which is an ASW aircraft, the propeller variant of the one that's now used, the S-3. So I was in a back seat and got catapulted off the ship, in a flight and recovered. The cat launch is like being in a sling shot: you're sitting there one moment, the next moment you're out in the air. And the recovery is dramatic. You'd be tooling along and look down and see this postage-stamp size ship, and the next thing you know you're on it, plop. It's just an amazing experience. The other flight was a short haul in a helicopter from the deck of a carrier onto the deck of a moving submarine. So, we would sit in the open door of the helicopter hovering over the submarine, put on the horse collar, and then be winched down onto the moving sub. Spent the day in the sub. A couple of dives. I remember just how noisy the submarine is as it goes down into the water, not from a hydrodynamic noise but the creaking, clicking of the hull as it takes the pressure. When we were being recovered from the submarine at the end of the day, there was a line of midshipmen with life jackets there on the deck, and the guy ahead of me put on the horse collar for the winch, and he was just being pulled up when the helicopter got a wave off, so he was dangling as they reeled him in. I was next up and went back in. So, in any event, that was the final summer cruise.

van Keuren: And this led into your early career in the Navy. Do you want to tell me about that? What were your early assignments?

Nagel: Well, as a senior we had the option of expressing where we wanted to go on active duty. And those of us who did well in the ROTC program had the likelihood of getting our first choice. I had become very interested in hydrodynamics, for a bunch of reasons, and was aware of the David Taylor Model Basin, as it was called, near Washington, D.C. It was a center of research on hydrodynamics, so I asked for orders to the David Taylor Model Basin, and instead I got orders to the USS ARNEB (A-R-N-E-B), hull number AKA 56, which was an old ship that had its hull strengthened for running cargo into the Antarctic, okay? So basically instead of going into science and technology I went to sea, and I had a first job as the Administrative Officer, rewrote the ship's admin manual, then did well enough that I got promoted to being a division head, namely the navigator, which goes back to my course in navigation. And I actually practiced the art of sextant navigation. The mission of the ship was to resupply the U.S. Antarctic bases in something called Operation Deepfreeze that grew out of the so-called International Geophysical Year, which I think was 1958 or 1959.

van Keuren: Fifty-seven to fifty-nine.

Nagel: Fifty-seven to fifty-nine, thank you. And these bases that had been set up in the Antarctic were going to be maintained, so our job was to haul cargo down to these bases. And the sequence was to leave the home port of Norfolk to go up to Quontset Pt., Rhode Island, to load thousands of fifty-five gallon drums of oil and crates full of provisions and magazines and all kinds of things, and then to go south past Cuba through the Panama Canal and across the Pacific. Usually we'd stop in New Zealand then run into the ice. The transit from the U.S. to the ice was interesting because on one occasion we picked up a large number of Panamanian school teachers who had never been through their own canal on a ship, and they were with us for the day it took us to transit the Isthmus of Panama. They played music [?]. That was very nice. And then, on another occasion, one going from Panama down to New Zealand, we changed course by a couple of degrees to be able to go by Easter Island.

**SIDE TWO**

We did not have permission from the country of Chile to stop in Easter Island; we hadn't put in the required diplomatic applications. We came up on the island intending to just stop for a while and look. We could see these large statues clearly on the island, and when we were looking through the binoculars, the people working the fields were dropping their tools and getting on their horses and riding over the hill. We learned later that only twice a year did ships visit that island, so they were very fascinated when they saw a ship. So when we stopped not far off shore, dozens of boats came out, and they wanted us to take their mail. But there was a fellow from southern Texas who spoke Spanish, and he got across to them that we were going to the Antarctic. It really wasn't a good idea for us to take their mail. Anyway, we traded various things. They wanted cigarettes and shirts and pants and the like, so I went below and got a pair of pants and a shirt and traded it for a very interesting hand-carved wooden statue which I still have by string over the side of the ship.

The first year after the cruise in the northern winter of 1960-1961, the southern summer, we went over to Australia and visited Sydney and then came back to New Zealand, Auckland, and came back through the Panama Canal. The second year on the ship, the December to February roughly period when we went down to the Antarctic, we returned around the world. So we went from New Zealand to Melbourne to Perth to Cape Town to Brazil and Trinidad and home. And it was while I was en route from Perth to Cape Town I got a teletyped message on this cheap yellow paper. I remember it said, "Lt. Nagel: Next duty station NRL." I had asked to be sent to the Taylor Model Basin again. I told the Navy I was willing to extend my three-year obligation to a fourth year, so I would avoid being sent to Little Creek to fly a desk for a year. And they figured that if I wanted to go to the Washington area and do research I could go to the Naval Research Laboratory. I'd never heard of it before, so I went to Captain Bobczynski (B-O-B-C-

Z-Y-N-S-K-I), Sigmund Bobczynski, and I asked, "What's NRL?" and he said, "I think it's a lab." He was right.

van Keuren: Let me take you back a little bit. What were your impressions of the Antarctic?

Nagel: The Antarctic was dramatic. Of course, it was like nothing I'd seen before. We were there in the southern summer, but it rarely got above freezing. Having said that, there was a period of a couple of weeks I remember very clearly when it was hovering around freezing that we would work on unloading cargo in sweaters. It was vigorous work, and the wind wasn't blowing. It wasn't that chilly, but during that period in the middle of winter in the Chicago area it was below freezing the whole time, so it was actually colder at home than it was in the Antarctic. The scenery was stark. The island on which one of the bases was located, McMurdo (M-C-M-U-R-D-O) Station, is on Ross Island. That island is the location of the only active volcano in the Antarctic, Mt. Erebus (E-R-E-B-U-S), and it sort of smoked, vented steam constantly, so we were down here with the water, the ice, the mountains, the volcanic ash, the steaming volcano, and the wildlife, including various kinds of seals, school of gulls, and orcas, so-called killer whales, and penguins of two varieties, the little Adelies (A-D-E-L-I, no, that's not right, there are more letters, but that's phonetic) and also the Emperor penguins, the larger ones. I attempted to get a flight to the South Pole but I was bumped by a senior officer, so I never got to the South Pole even though I got within a few hundred miles of it. We were able to visit the huts of the Antarctic explorers such as Scott, who perished with his colleagues after reaching the Pole, one month after the Norwegian beat him. Scott left behind a lot of provisions, including boxes of crackers that had been sitting out in the open for fifty years, so I had a very stale fifty-year-old cracker. Now they've put some kind of fence around the place so people like me can't eat the history. We had a lot of interactions with the wildlife, close up, and there are many, many stories I could tell, but it was just a remarkable experience. And coming as it did, when I was young, it left a vivid impression. I took 2,400 color slides during the time I was on the Antarctic.

van Keuren: You were on part of an operation that was in support of the International Geophysical Year. Did you meet any of the scientists involved?

Nagel: Yes. We met the scientists two ways. Some of them rode the ship from New Zealand into the Antarctic. In fact, I remember one entomologist. I don't remember his name, but he would put up conical nets in the rigging of the ship, and it would pick up insects hundreds of miles out to sea, wind-borne insects, which impressed me at the time. And then when we got down to the Antarctic and went ashore just to look around, we would meet these scientists who were stationed there and making auroral measurements and other kinds of measurements. As a precursor for my career at NRL, unbeknownst to me at the time, I had two experiences with nuclear things. On one occasion, we'd brought as part of the cargo a remote weather station that was powered by a radio-thermal generator that is a device in which the decay of radioactive materials produces heat that then produces electricity that ran this weather station. So it was a cylinder of maybe a foot and a half in diameter and six feet tall, and it would have this generator on the bottom, then the instruments and then like a cap, so a hole would be dug in the ice, and it would be put into it and just the instruments would stick out and a radio of course for radioing the information. The other thing that was very interesting is that the second year I went down there we took a nuclear reactor built by Martin Marietta in Baltimore, and I was signed for the core of the thing that had an entire hold on the ship by itself. I have a photograph showing this barrel-shaped object all by itself in the hold of the ship, so that nuclear reactor powered the McMurdo Station for ten or fifteen years after we took it down there. Some people recoil at the thought of putting radiation, a reactor, into that kind of environment, but it turns out that it was far more environmentally friendly than hauling down tens of thousands of fifty-five gallon drums and having them litter the landscape, which was done up to that point. The reactor, as I say, worked for many years, and I heard that it was later shut down and pulled out, and it was a basically successful way to power the station.

van Keuren: You were assigned to the Naval Research Lab in 1962?

Nagel: Yes.

van Keuren: Tell me about this.

Nagel: Well, as I said, it came out of the blue, and it was a requirement. I had a set of orders, so I reported for duty at NRL. I had a job as a "technical liaison officer". The basic idea, I think, that brought a few of us into the Laboratory as junior officers was that it would be useful to have some people in uniform know something about technical things. But the reality of it was that they didn't know what to do with us. And it was compounded by the fact that the Commander who was my immediate boss was clueless as to what to do with me. So basically he told me to go out and find something to do. And I was happy to become associated with a group including a fellow named A. J. Martin who still works at NRL as a contractor, and we programmed the NAREC computer (N-A-R-E-C) which had like 1,400 vacuum tubes. So we were using this computer that was designed in 1950 and later turned off in 1972 to do missile defense calculations where the Naval Space Surveillance facility, which was a thing the lab had made earlier that put a radio frequency fence across the southern United States to detect the passage of satellites for satellite monitoring. The question was, could it be used for missile defense? So we went down and programmed that. I would sleep at the computer center at night because long runs would be lost if a tube burned out. If a tube did burn out, they'd stop at some register, they'd wake me up and tell me what register had stopped, and then I would look at my code sheets and recite a number, ones and zeros, and go back to sleep. We did this in machine language. We didn't get the assembler until like 1963, and that made all the difference in the world. We didn't have to keep track of registers any more. I didn't have to do so many erasures on my code sheet. So that was one very, very worthwhile, instructive experience.

I also worked with a guy named Bill Pellini (P-E-L-L-I-N-I) who was head of the Metallurgy Division and essentially got information on ocean depths for him. He was a specialist in developing high strength steels for submarine hulls, and he wanted to know what fraction the oceans were at a certain depth. I worked on a classified project. Two of us designed a system to choke the Naval Space Surveillance system. That consisted of orbiting a package of resonant dipoles that would have so many targets returning strong signals that it would overload the data handling characteristics of the system.

van Keuren: Why did you do this project?

Nagel: It occurred to us that this system that was becoming more and more important nationally in keeping track of satellites could be rendered useless by a very, very simple payload. Basically one could scope up what would be necessary to do that.

van Keuren: What other uses was the NAREC being used for, do you recall? The NAREC computer. What was it being used for generally?

Nagel: It was the super computer for the laboratory, so it was used by a large number of leading edge research groups just as later the advanced scientific computer and the Crays were used. So, a nonspecific answer is a lot of leading edge work.

van Keuren: You were at the Laboratory in 1963 when the Thresher went down?

Nagel: Yes. If I may go back to what I did, I also started to work on the gravity gradient stabilization of satellites.

van Keuren: Who did you do that work with?

Nagel: I don't remember, actually. It was somebody in what I now think of as Wilhelm's area, but whether or not he was involved....

van Keuren: Mayo's group? Reid Mayo?

Nagel: I knew Reid Mayo. In fact, I think he was in the Reserves with me, but I just simply don't remember why I got involved in that. It came late in my two years of active duty at NRL, and I never really got into it.

van Keuren: How did you get involved in that particular project? It seems like you're all over the map at the Lab.

Nagel: Well, I don't remember whether that was something that I got interested in. You know, I basically had latitude to do what I wanted. Or, on the other hand, somebody asked me to do it. But, you know, the various projects, I did the NAREC thing because I was interested in it. I did the job for Pellini because I was asked to. There were actually two jobs that I was asked to do. One was to work with a group headed by Harry Clark. That was on an airborne infrared surveillance system, an attempt to find submerged submarines using infrared.

van Keuren: That was a classified project?

Nagel: That was a classified project. They had been flying the system on blimps, and they transitioned to fixed wing aircraft, but they also had a boxcar-sized laboratory that hung underneath the Chesapeake Bay Bridge. So they would look down from that laboratory using not only the infrared scanners but also radars at the submerged passage of a two-man submarine that would come out of Annapolis. So we would drive to the west end of the Chesapeake Bay Bridge in the early afternoon and get on board a truck with lots of lights and would go out onto the bridge and actually stop in traffic. We'd scramble out, climb over the side, down the ladder into this lab that had the instruments, and then when it came time after dusk for the experiments, I would climb out of that box into a cage that hung underneath, and I had an underwater phone that I could communicate with the submarine, and they'd have an upward looking light so that I could make sure that it passed directly under the instruments. There were fixed instruments aboard the submarine. And I also was equipped with a megaphone to warn away pleasure boaters so they wouldn't have a collision with it. And that project ran for a long time. I spent many, many a day in that thing and driving home in the wee hours of the morning through northwest Washington. I remember that was interesting.

van Keuren: That project, did anything come out of it?

Nagel: No. Like a lot of projects at the Lab, it was a very reasonable thing to do, but it turned out, at least with the instrumentation and signal processing and so forth that was available at that time, not to be feasible. Now, I'm implying, mean to imply, that it may now be feasible, but I have no longer kept pace with it, so I don't know what the situation is.

van Keuren: What was the Laboratory like when you came to it in 1962?

Nagel: Before I answer that, let me tell you about one other job I had given me, and that was a logistics task. The Commander, Williams by name, who had replaced this incompetent supervisor I had, called me in one day and said, "Dave, I've got a job for you." And the job was to take a World War II landing craft, a so-called Mike boat, from the Brooklyn Navy Yard around the tip of the Battery of New York up through the Hudson River through the Erie Canal and deliver it to Lake Seneca, which is one of the Finger Lakes in upstate New York--very deep, very long. And the Laboratory had a transducer calibration facility up there and needed a work boat. So there were two civilians from the Sound Division who were trained in the operation of such a boat by riding it for a few hours one time, and they needed an officer. I asked them, "Why do you need an officer? I don't know any more about this than anybody else does." The answer I got was, "They need somebody to court-martial if they screw up." So in the fall for a week we putted up the Hudson River and through the Erie Canal. And there was a fellow in the Sound Division, a retired Navy officer named Harry Eney (E-N-E-Y) who got himself a set of orders to support our transit. And he would do that by showing up around ten in the morning and yelling at us, "What do you guys want for lunch?" And we'd tell him, you know, "Ham with rye" and so forth, and then he would catch up with us two hours later, hand us a lunch and then disappear for the rest of the day. So he just drove around upstate New York for a weekend on the government payroll. That boat broke loose from the moorings where the Navy had it in a storm later on and had the misfortune of plowing into the Mayor's yacht and damaging it, so I got to go back up there later on to do an investigation of what was going on.

Okay, returning to your question about what the Lab was like at the time. You have to recognize that I came in by a nonstandard route. I was one of a small number of people--I was in uniform, I was not part of the mainstream of the Laboratory--but I worked with the people at the Lab and obviously as a result came to like it very much, enough to spend my career there. But, anyway, I found it to be just a remarkable place. They had people doing leading edge work with a tremendous amount of latitude. They could basically do what they wanted. It was an exciting place. I saw, I know now, only a very, very small piece of it, was totally unaware of the many advances that the Laboratory had already made in radio and electronics radar and other areas, sonar in particular. But the piece I saw, I liked. You noted earlier about the loss of the Thrasher, and I remember very well the excitement that was associated with that. I knew Buck Buchanan because we were both in the Reserves together, and he was leading the efforts at sea looking for the Thresher.

van Keuren: You left active duty in 1964 and took a civilian position as a GS-09 physicist in the X-ray Optics Branch of the Laboratory. Can you tell me why you chose this position? I believe you had choices.

Nagel: Yes. In fact, the first question in my mind is why I decided to leave active duty. I was enjoying a very good Naval career, I was getting good fitness reports, but there was a push and a pull. The pull was NRL was such an attractive place to stay that I really wanted to spend my career there, the way it looked at the time, although I had no way of imagining it would be as long as it was. And the push side of it was that my first commanding officer on the ship that I went on was a mad man. He would scream at people, myself included, at the top of his voice, right in front of everybody on the bridge watch, and I was under the impression that, you know, a bad fitness report would essentially torpedo a career. And I wasn't willing to spend my life rolling the dice that I'd get another nut like him, who would essentially kill my Naval career. By that point, I was married. I was married in 1963. We were expecting a child. And I didn't want to face the numerous moves and the sea duty and so forth. But there was a particular driver, and that was Admiral Rickover. At that time, there was a shortage of Naval officers for nuclear power school to learn how to run reactors, run reactor-powered ships, and so forth. And I was first invited and then later ordered to an interview with Rickover. And I went over there, and his office was still at main Navy down on Constitution Avenue. I waited with a group of people, and my turn came up. I went in, and he was seated at a desk piled high with reports, the famous tilting chair in front. I sat down in that and without looking at me he said, "Are you married, Nagel?" And I said, "Yes, sir." And he screamed at me, "Get out, Nagel!" And I thought, "Good, I flunked the interview; I'm not going to have to worry about that." So when I came around and rejoined the group a little while later, the secretary walked in and said, "Congratulations, you've all been accepted into nuclear power school. When you report, you'll be tested on math and physics. Read the following things." And he began to dictate a series of books to us that we should run right out and read. At that point I wrote my resignation letter, because I was going to do it anyway. But it then became an immediate action item. So, incidentally, I decided not to terminate my relationship with the Navy but rather to transfer to the Active Reserves, and that's another story which maybe we'll come to, but the situation then in early 1964, in terms of potential jobs, came down to three things. There was a job in one of the two nuclear divisions that looked quite attractive, but it was not necessarily permanent because the so-called ceiling point, the billet at that time, was a position occupied by a pregnant secretary who was off having a baby, and when she came back she'd reclaim her position, and I might or might not have a job. It seemed a little tenuous. The second position was in a hypervelocity impact program, a dynamic program of long duration and great productivity at the Lab, headed by a guy named Atkins (A-T-K-I-N-S), Walt Atkins. But it was classified work, and I didn't want to do classified work at that time. I wanted to publish in the open literature and establish a reputation. So the third opportunity was in the X-ray Optics Branch headed by Verne Birks (V-E-R-N-E, B-I-R-K-S). So the third opportunity was in the X-ray Optics Branch with Verne Birks. And it was a physicist job starting at a GS-09, and it was interesting because some of my other uniformed colleagues who were leaving active duty and staying at the Lab were starting at 11s, so it gave me some pause for thought. But I hadn't had any graduate school at that time, and it was a bird in a hand. So I took the job and expected to settle down and do hands-on research for a long time.

van Keuren:  What was the Branch like?

Nagel:  The Branch was entirely dominated by Verne Birks. He had instituted the Branch many years earlier. I think at that time it was called the Electron Optics Branch. But he was the leader both in terms of longevity, in terms of intellectual stimulus, and in terms of personality--a very singular individual. It was a relatively small group of people, maybe a half a dozen or so at the time I joined it, and it about doubled not too long after that, because in the mid-sixties we began doing research on nuclear weapon output at the Nevada Test Site.

Now, the group was quite social, actually. They would stop for a bridge game. People would come from around the Lab to play, and they'd tried to get me into that at one point, but it really wasn't my cup of tea. They would talk sometimes for an hour or two afterwards about particular hands, and I wanted to get back to work. I just didn't enjoy it. So that was a non-starter. Then late in the afternoon there would always be a Coke break. So, anyway, it was a fairly civilized place. And Birks was very well known in the x-ray analysis arena. He'd built the first electron micro-probe in the United States around 1950. That's wrong. I don't know what the date was. Fifty is too early. It's probably in the mid-fifties. And it was a device pioneered by a Frenchman, and then Birks, as I said, built this one in the U.S. He was very, very well known, and with good reason. I mean, he had a real bent for doing innovative work, leading edge work. He was the first one to adapt the multichannel analyzers that the nuclear physicists used to the x-ray analysis business. The character of the Branch changed dramatically when we started doing the work at the Nevada Test Site.

van Keuren:  Before we get to that, who were some of your other colleagues?

Nagel:  There was a fellow named Robert Seebold (S-E-E-B-O-L-D), who was, as far as I remember, sort of the senior guy. And there were two other folks, Roger Labrie (L-A-B-R-I-E) and John Grosso (G-R-O-S-S-O), who were very well in place at the time I came there. There was a technician named Dick Saunders (S-A-U-N-D-E-R-S), and there were a few other people associated in some part-time fashion, but that was the central group along with Birks, and then a secretary, of course. Now, the group essentially doubled in size over the next few years, okay? I can tell you now or later the names of the people who joined the group.

van Keuren:  Just the main people I want.

Nagel:  Okay, well, each one of the people who joined the group became very significant, stayed a long time. They include John Gilfrich (G-I-L-F-R-I-C-H). We hired him away from the Naval Ordnance Lab, and he was an expert in x-ray analysis as well. I met with him at a Howard Johnson's up in Beltsville one time to try and get him to come to the Lab. He wanted to ask questions about the character overall, and he's retired but still associated with the Lab. Another fellow was Charles Dozier (D-O-Z-I-E-R), a physicist, and he is still at the Laboratory. Yet another fellow is Robert Whitlock who joined the group and is still at the Laboratory. There's a fellow named John Criss (C-R-I-S-S) who was a computer expert and did the software for the x-ray analysis function, who joined the group in that timeframe and then later went to form his own company, Criss Software, which he still runs. There's a woman named Judy Sandelin (S-A-N-D-E-L-I-N) who worked in the group for a while and then left when her Air Force husband was reassigned. Another woman named Jackie Vierling (V-I-E-R-L-I-N-G) was in the group for a while, and that was interesting to me because her husband Tony was one class behind me at Notre Dame. He was an instructor at the Naval Academy, and the two of them wrote what I think was the first book on computer-aided instruction in the time-frame of the seventies. I'm not sure of the date; I'd have to look back. But it was a leading edge book. And there were other people who came along later, but that was the group that, as I remember, joined the Branch in the sixties and, as I've indicated by the people who stayed there a long time, really carried the load for many years.

van Keuren: And what about your research?

Nagel: The opportunities, very similar to when I was on active duty, were extremely diverse. And the thing that I wound up doing was studying diffusion in metals, using the electron micro-probe. So the way in which the work was done was to take samples of metals like titanium and niobium, polishing the faces and welding them together, then putting them in an oven, certain temperature, certain duration. They would inter-diffuse, and then the sample would be taken out, mounted in plastic, cut for final to the interface, and then the electron micro-probe would be used to measure the distribution of the two elements across the interface. And from that could be calculated the diffusion coefficient. I worked on that from essentially the time I joined the group until around late sixty-five, certainly sixty-six, when this Nevada Test Site program came up.

van Keuren: You spent almost five years at the Nevada Test Site, right?

Nagel: Yes. The way that went was, and of course it was the mid-sixties and both sides of the Cold War had nuclear weapons, both H-bombs and A-bombs; that is, both fusion and fission devices. And the concern was ICBMs and the defense against ICBMs. And there was no hope of an anti-missile defense such as now is being contemplated by proximity or hit-to-kill. The idea was the reentry vehicle containing the nuclear warheads was in space en route, say, from Russia to the United States or from the Soviet Union to the United States. It could be exposed to a high flux of x-rays from a nuclear weapon, those x-rays would be absorbed in the surface of the reentry vehicle, which would be blown off, and the reaction would be like kicking the reentry vehicle, hitting it with a physical object and denting it and in some way upsetting its aerodynamic characteristics so when it reentered the atmosphere it would be unstable and burn up. So the nuclear weapons labs, Los Alamos and Livermore, designed devices in that time-frame in the early sixties that would emit over half their killer tonnage in the x-ray spectrum, okay? And if one of these went off in space, a shell of x-rays would expand out, and if it intercepted an incoming reentry vehicle would damage it.

So Birks was on a flight back one time with an associate director, Wayne Hall (H-A-L-L), and learned that this x-ray source now existed. And we counted ourselves as the premier x-ray group in the DoD. So it became, you know, a natural thing to look into. And I remember Birks coming back and almost whispering, (we didn't have any clearances at the time) "Hey, nuclear weapons emit x-rays. We're going to look into it." So Birks and Criss and I went on a swing through the West visiting facilities that were involved in this kind of research, not only the weapons labs, specifically we went to Los Alamos, but also some of the companies: research institutes involved in it, such as SRI (Stanford Research Institute). And it became evident that the thing to do was to measure the spectrum of the nuclear weapons with high resolution. It wasn't being done, and it would be valuable two ways, one as the input to the effects people. Experiments in Nevada were designed not only to test the weapons to see if they worked as indicated but also to use the radiation from the weapons to impact military objects like reentry vehicles and like the study of effects. So the spectrum not only said how the weapon worked but what hit the test pieces, the materials, and the systems. So Birks had developed and in fact patented the curved crystal spectrograph. It turns out that he reinvented it. It was actually invented in the teens by a guy named De Broglie, and Birks was unaware of that work and later got a U.S. patent on it. But we decided to take that to the field. And there's a choice to record the x-rays either using film or active detectors, and we did both. There's a tremendous amount of work done on the quantitative measurement of x-rays using films, the so-called h and d curves and background...

van Keuren:  H and D?

Nagel:  H and D. The calibration curves for x-ray film and also the background fogging. Anyway, we became very, very expert at the details of photographic film, how to handle films. Early in that adventure I took a semi-trailer and turned it into our headquarters at the Nevada Test Site. It was divided into thirds. The front third was an x-ray lab in which an x-ray machine could actually make exposures. The center was a dark room, and then the part nearest the door in the back was a combination office and shop. So that served as our headquarters at the Nevada Test Site for the duration, the five years. And one of the things that I remain curious about is whether or not that thing is still sitting out there with flat tires or if somebody hauled it away. But we just walked away from it at the end. We never hauled it away. The routine was to make a proposal to DASA (D-A-S-A), the Defense Atomic Support Agency, who ran these weapons tests for the DoD. And if funded we would develop an experiment package, calibrate it, put it in place, and after an event, as they were called (after a shot), we would go back in and get the films, pull them out, develop them and so forth. We'd do about two of these a year. We had failure after failure, initially, for various reasons. And finally we got it to work. Then we went on from there to using active detectors. We would have cables run up out of the ground to semi-trailers with dozens of oscilloscopes in them run by a contractor, EG&G. And we could record the signals electronically and get away from the film. In the end, the net assessment of the project was a great success. We pioneered the high resolution measurement of the spectrum of x-rays from nuclear weapons, saw details that were heretofore unseen. We were able to go to the people, for instance, at Livermore, and tell them a spectrochemical analysis of the weapon. And they would just take it on board because we weren't cleared to learn what was actually inside of the weapons. But we would find it out by the details of the x-ray spectrum. The work was very demanding, both the tempo and the time away. I think there was one summer when I was gone virtually all the summer. We had a young daughter at the time and, anyway, it was a difficult period personally but a very rewarding period professionally.

van Keuren:  How did your work fit into the whole nuclear program?

Nagel:  The entire nuclear program--this is not the nuclear power program, of course, but the nuclear weapons program--had two major facets that I've already alluded to. One is the development of nuclear devices that performed in a certain way, would put out a certain spectrum of x-rays with a certain time history. And the work that we did essentially validated the performance or not of the weapon. So it was extremely useful feedback to the two weapons labs, Livermore and Los Alamos.

van Keuren:  How did it validate the performance?

Nagel:  The designers would make a weapon that if it worked the way they designed it would put out a certain spectrum of x-rays that would peak at a certain region and have a certain shape. And we would tell them whether or not that happened, quantitatively. On the other side of it, the effects side was a matter of putting material samples and subsystems and full-up systems in these giant vacuum tubes underground in Nevada, with the weapon at one end and then the pipe flared out to a region that might be twenty feet in diameter and would have expensive hardware mounted in every square inch. My recollection is that they cost $5,000 per square inch for these exposures, so they used all the area that they could. So the spectrum that we measured was what the effects people took as the input to their calculations.

van Keuren: And you provided a good chart of this?

Nagel: And we would provide a single graph. The output of a half year's work and at that time $50,000 to $70,000 per project was one sheet of paper.

van Keuren: In the form of a chart?

Nagel: A chart, yes. And, of course, it would be part of a report to the sponsor, DASA, that would include the design and the calibrations, everything that went into it as well as the data analysis and the final answer.

van Keuren: How many nuclear tests did you actually work with?

Nagel: It was probably ten.

van Keuren: About ten?

Nagel: Of that order.

van Keuren: This was over a five-year period?

Nagel: Yes. And they were both tests in horizontal tunnels and Ranier Mesa (R-A-N-I-E-R) up in Area Twelve to the Test Site as well as vertical tests down in Frenchman Flats. There was one particularly interesting experiment that was a horizontal type-- buried not drilled into a mesa but buried underground-- that was a Los Alamos shot called Snubber (S-n-u-b-b-e-r), and it was going to give us a remarkable opportunity to do a physics experiment because of the way in which the earth had been mined out. We were going to have wonderful shielding, just a small pipe bringing the x-rays to our experiment, and we made curved single crystals of aluminum and copper and silver and, anyway, it looked like it was going to be a gold mine in terms of information, but the stemming, it was called, the sealant around the weapon failed, filling the entire tunnel with radiation, and nobody ever got back into it. So it was an entire loss.

van Keuren: At any point during this period, did you feel that your health was at risk? Were you ever subject to radiation yourself?

Nagel: We were more subject to radiation in the Laboratory, working with x-ray machines, than we were subject to any radiation from a nuclear weapon. Now, if there had been a catastrophic leak in one of these tests, then it's conceivable that airborne particles could have come over us. But the tests were carefully conducted so that the wind would carry the radiation away from population centers and ourselves. So I never felt in any danger. There was a concern associated with beryllium, not only in regard to these weapons tests but x-rays in general because it's a very transmissive material for x-rays. So we had to take inhalation tests in order to make sure that we weren't developing beryllium oxide-induced lung problems.

van Keuren: So this whole project was initiated by NRL?

Nagel: Yes.

van Keuren: Particularly by Verne Birks?

Nagel: By Birks. And the most memorable moment was after several failures. Birks and I were in the trailer developing the film from a shot called Hudson Seal (H-U-D-S-O-N, S-E-A-L). And instead of the film not having anything on it or suddenly turning black due to background radiation neutrons and gamma rays, we got a very, very nice x-ray spectrum with details in it. That was a dramatic moment. There were some other things that we saw and did during the time out there that were very memorable, like visiting the Sedan (S-E-D-A-N) crater where there was a demonstration of how you can use a nuclear weapon to move earth, say, for making another Panama-style canal. I was a witness to several shots. They would have weapons tests almost weekly at that time, so seven o'clock Saturday morning, if you got up and went out, you'd see, you know, the desert would jump up and an expanding ring of dust would be visible and then, where you're standing would shake like a small earthquake. And then you'd get up the next Saturday, and it was the same thing again. Then the third Saturday you wouldn't even get up, so when the bed started shaking you'd know it was seven o'clock. We also saw mustangs, wild horses, running wild in box canyons, a lot of rattlesnakes, and ate a lot of bad Mexican food. It was wonderful.

van Keuren: You were spending a lot of time at the Nevada Test Site, I take it.

Nagel: Yes. I'd have to sort of do an audit on travel orders, if I still have them, but my guess is I spent two to four months each one of those five years out there.

van Keuren: That was a total of five years.

## Interview 2

van Keuren: Today is 24 March 2000. This is Dr. David van Keuren, History Office, Naval Research Laboratory. I am talking with Dr. David Nagel about his career and work at the Naval Research Laboratory.

David, simultaneous to your work at NRL, you were pursuing an advanced degree in science at the University of Maryland. Can you discuss this?

Nagel: I would be happy to, because it was kind of an odd graduate experience. It was clear from beginning as a civilian at NRL in 1964 that I should continue in graduate school. I had first enrolled when I was still on active duty in 1963, seeking a Ph.D. in physics from the University of Maryland at College Park. And despite having a new job, and being married, and having a child, the Ph.D. seemed like a very desirable thing to get. So, I took the course work, some of the courses on campus. And at that time some of the courses were actually taught at the lab because there were enough people to justify sessions by NRL scientists as teachers for other NRL scientists. So, during that period I had a couple of memorable courses from Jerry Karle who was teaching the electromagnetics out of Jackson's book. Very mathematical, very difficult. J-a-c-k-s-o-n. Jackson's book is THE book on electromagnetics.

So, I could tell stories about that particular course and about the series of courses, but the central situation was that after work I would go over to Building 72 and go to a classroom there and then go home and do homework until all hours of the night. So, I accumulated the thirty credit hours that were necessary for the Ph.D., and started to talk to the people at College Park about doing a thesis. And at that time there were 300 full-time physics graduate students in the department, and it was a seller's market. The professors really wanted on-campus thesis work which would have required me to interrupt my work at the NRL and lose the salary in the process while I did research on campus at College Park, which incidentally was all the way across town from where I lived in Arlington. So, that was out of the question. So, I was faced with the choice of staying in physics and starting over somewhere else, or staying at Maryland and changing to another curriculum. So, I shopped around Maryland for what made sense, and got a warm reception in the engineering materials curriculum, which spanned two departments at that time: chemical engineering and mechanical engineering. And it was clear that I should stay at Maryland and continue along in that direction.

I decided to do that and started taking courses in engineering materials, which were essentially applied solid state physics courses and were very closely related to what I was doing at the lab, and also very useful. I didn't give much thought to a Master's degree until one day a diploma showed up in the mail. A Master's of Science in Physics. I didn't apply for it. I didn't pay for it, but apparently somebody in the Physics Department said I had the credits, and they spit out a diploma. So, this went on through the '60s. I got the Master's degree in 1969. I think the courses in materials continued into the '70s, and then I should have started really buckling down and working on a thesis project at NRL for the degree in Materials, but the work at the lab at that time was very exciting, and anyway, I didn't pay much attention to school. So somewhere in the '74, '75 time frame I was given an ultimatum to get a thesis in, or lose my credits. And I settled on a topic, the electronic structure of some transition metal compounds with aluminum, and proceeded to do a combined theoretical and experimental thesis on those materials. I did far more than I should have done, frankly, but in the end I put together a pretty good thesis. Submitted it. I found out later it got very good reviews by the Committee. I remember the thesis defense vividly because I came in, and I made my presentation to the Committee, which included Robert Park who is the spokesman for the American Institute of Physics now, with whom I still deal. After my presentation and the discussion, I was dismissed. And I went down the hall, and I heard the Committee laughing, and I was very bothered by that, and I came back in and I was congratulated, and I asked them why they were laughing. They said well, we had to sort of wait a respectable time, and so we were telling stories during that time. So, in any event, I got a Ph.D. in Engineering Materials in 1977. The work that was the subject of my thesis wound up in only one small paper, which was basically a mistake. I should have turned it into a series of published papers, but I never got around to that because I was charging on to something else at the time.

van Keuren: What was your thesis title? Dissertation title?

Nagel: The title?

van Keuren: Yeah.

Nagel: Was the "Electronic Structure of Transition Metal Aluminides." They were compounds of aluminum with iron and with cobalt and with nickel.

van Keuren: And so this was done, you said, in?

Nagel: I finished it in '77.

van Keuren: '77.

Nagel: And the net assessment is that I, you know, missed the usual interaction with other graduate students. I was not a teaching assistant or research assistant. So, it was really an anomalous graduate school experience, but it worked because I learned a lot of useful information and I got the full degree.

van Keuren: Did the thesis work, the research, relate to your work at the Laboratory at all?

Nagel: It was entirely done at the Laboratory. That's right. And it was just part of the research that I was doing at the time. Later when we talk about what it was like to work at NRL over the decades, I can describe it in more detail. But basically as long as it was viable research, I could basically do what I wanted during that period.

van Keuren: Now, you told me a little bit about the department you worked in at Maryland. What was it like to...I know you were living on campus. But what were your impressions of the faculty?

Nagel: In the Physics Department, which as I said was very large--three hundred full-time physics graduate students, and they must have had way over fifty faculty--it was a high level of excitation. They somewhere along the line got a new cyclotron up there. The chairman of the department was a man named Toll, T-o-l-l, as I recall. And he later went on to become President of the State University of New York at Stoney Brook, and then came back to be the President of Maryland. So, it was an outstanding department with a lot of activities and high quality research. But I was just beginning graduate student taking part of my courses up there. Now, by contrast the Engineering Materials curriculum which spanned the two departments I mentioned, chemical and mechanical, had a very different feel about it. It was nowhere near as excited and advanced and some of the professors there were very good, but some of the others were far from good. But the bottom line on that engineering materials program was that it worked. It provided worthwhile instruction, and I was very happy that I got it. It was a far more practical degree for me than I would have gotten in physics.

van Keuren: The professors, their backgrounds were in physics, or engineering, or both?

Nagel: In the engineering materials curriculum, they were in general in engineering, although there were a couple of physicists.

van Keuren: How did acquiring the Ph.D. advance your career in the Laboratory?

Nagel: It was a ticket. At that point,I would have to look to see what my grade was, but it didn't have any impact on my immediate situation in terms of either activities or grade level. But it is absolutely certain that I would not later have advanced to be a division head if I had not had the Ph.D. And in fact, at one point in talking about an individual who was in the division I did later head, with the Director, Tim Coffey, he made very clear that this person wasn't ever going to be a division head at the lab because he lacked a Ph.D.

van Keuren: So, the academic pedigree was very important?

Nagel: Yeah. It was fundamental to my career at NRL, in the end.

van Keuren: Stepping back a little bit, in 1970 you concluded your work at the Nevada Test Site and began laboratory research on x-ray spectra. Can you tell me about the genesis of this research and how the research progressed?

Nagel: Yeah. The pre-history goes back to the hiring of Herbert Friedman in the late 1930s. And then in the '40s he hired several key people, among them were Jerry and Isabella Karle who, of course, did crystallography. But also LaVerne Birks, B-i-r-k-s, whose interest was also in X-rays. Friedman did X-ray astronomy; Birks did spectro-chemical analysis with X-rays. So, he measured the X-rays that were excited from material samples, and the wave lengths of the lines told him what elements were present, and the intensities of the lines told him how much of those elements were present. So, when I joined the X-ray optics branch headed by Birks in 1964, I joined a group that was doing as its bread and butter activity, X-ray spectroscopy for chemical analysis. Now, when we got involved in the Nevada Test Site project to measure the emissions spectra from nuclear weapons, it was a new spin on an old topic. In X-ray spectro-chemical analysis you take a sample of material and excite it to emit X-rays. By contrast in the case of nuclear weapons, they are so naturally hot that they emit X-rays. But the technology of making those spectroscopic measurements was very much the same. So, by the end of the Nevada project, we were capable of measuring spectra from X-ray sources that occurred in very short pulses. And this was directly germane to the measurement of X-rays from laser-heated plasmas that occurred generally in nanosecond long pulses. It was also applicable to the measurement of X-rays from these hot plasmas, the multi million degree plasmas, that came from pulsed power machines, which was a strength of the NRL in the Plasma Physics Division. So, in the early '70s we applied the same capabilities that were in place at the Nevada Test Site to the measurement of laboratory plasmas heated by two different kinds of sources: lasers and pulsed power machines.

van Keuren: So, there was an easy segue from your work at the Nevada Test Site to your laboratory work?

Nagel: It was indeed an easy segue, and it was dramatically more pleasant because instead of engineering a large scale experiment, and putting it together against a tight deadline, and calibrating it, and then shipping it out to Nevada, and installing it into a mountainside or in a tunnel underneath the flats out in the desert, and then waiting for the shot to happen, and getting the data or not getting the data in failed shots, and doing the analysis against a deadline, and all of the pressures of the experience in Nevada, we would go over to the next building at NRL or at another building at NRL and work for a day, take several spectra, and have a quick turnaround time, and be able to publish the results in open literature. So, it was a dramatic improvement from the test site experience.

van Keuren: Your test site work was being funded by the AEC?

Nagel: No. The AEC funded the laboratories in Livermore and Los Alamos that developed the nuclear weapons. The testing of hardware exposed to radiation from those weapons was the responsibility that DOD, and in particular DASA, D-A-S-A, the Defense Atomic Support Agency, which was located on Telegraph Road and has since morphed first into the Defense Nuclear Agency and now into the Defense Threat Reduction Agency, DTRA. So we worked for the organization --

van Keuren: What was that last part?

Nagel: The Defense Threat Reduction Agency, DTRA. Straight faced, David. Straight face. So, we worked for the DOD, that is for DASA, and they would give us contracts that were sometimes as small as \$50k in the money then, and sometimes as large as around \$100k, in order to quantitatively measure the X-ray spectra from these nuclear weapons.

van Keuren: And they continued to support your work in Washington on the X-ray spectra work?

Nagel: The experiments in Nevada were extremely expensive, and as I said, they had a slow turnaround. They could only get a couple of major experiments done a year. So DASA was very interested in having Laboratory simulators for X-ray spectra and these pulse power machines such as the Gamble machine, G-a-m-b-l-e, at NRL which would provide terawatts of power. They were multi...no, that is wrong. They were megavolt, mega-ampere sources. So, they were terawatt sources. And if that power was discharged through a small piece of material, like a thin wire, it would cook a plasma up to the temperatures that were similar to what was in the nuclear weapons, with, of course, overall dramatically lower energy, but at least the X-ray spectra that would come out would look similar. So, indeed, DASA did support the production of multi million degree plasmas by both lasers and by pulsed power machines at NRL, in the early '70s.

Now, it was evident early on that the lasers weren't ever going to provide the kinds of total energy that was needed in terms of joules of energy. So that the focus then went to the pulsed power machines, first for the exploded wires at NRL, and then with multiple exploded wires at two companies in California, namely Physics International and Maxwell Laboratories. So, we went from measuring both laser plasmas and discharge-heated plasmas at NRL, to measuring laser plasmas at NRL and elsewhere, and discharge-heated plasmas at NRL and elsewhere. I did a lot of suitcase physics through the middle '70s, taking spectrographs to both university and company laboratories.

van Keuren: And how did your research progress?

Nagel: That period was so successful that in the early '70s, when I should have been working on my thesis, I remind you, my wife and I were considering a sabbatical at the Naval Postgraduate School in Monterey. In fact, we went so far as to fly out there and look at homes. But, in that same time-frame the work on X-ray spectroscopy of the high temperature plasmas from both the laser and the pulsed power site was picking up in tempo, and it was clear to me that it was going to be very, very productive of high quality research and further programs that we decided not to go on the sabbatical, in order to take advantage of it. And I have a plot of the numbers of papers I published, per year, down through the decades, and there was a big spike in the early '70s, reflective of that high level of activity. We gave many papers at American Physical Society meetings. Lots of invited talks. It was a very, very productive time. I published in the best journals, like Physical Review.

van Keuren: So, it was a very productive period in your professional career?

Nagel: Yes. In fact, my global view is that the '70s, in particular the first half of that decade, were my physics period.

van Keuren: This was also the time that you built a scanning X-ray spectrometer, wasn't it?

Nagel: Yes.

van Keuren: Do you want to tell me about that?

Nagel: The X-ray spectrometers that were employed for the pulse measurements, had to handle all wavelengths of X-rays simultaneously. If you had a steady state source, such as a material that was excited in order to do chemical analysis on it, or some other steady state source, you could afford the luxury of scanning through the spectrum, and in the process measuring the X-rays with improved sensitivity. In 1970, I saw a Physical Review Letters article written by a group in Texas, and they were shooting high energy ion beams at targets, exciting X-rays, and measuring the spectra. And they measured the spectrum with a so-called SILI detector, that is capital S-I for Silicon and capital L-I for Lithium. And it is a detector that has very good sensitivity, but poor resolution. And the spectrum that they published showed hints of structure, but no actual structure. So, I went to the Space Sciences Division where they were flying X-ray spectrometers and borrowed the plans for a scanning X-ray spectrometer that was only about eight inches in diameter, and maybe four inches high. I built that; I calibrated it. In fact, that was an interesting operation because I spent at least two weeks over in the Engineering Services Division looking through an auto collimator to calibrate the details of the gear(?)in this spectrometer in order to get high precision. I had formed a team with another fellow in our group, namely Phil Burkhalter, B-u-r-k-h-a-l-t-e-r, and a fellow who worked in another branch, namely the Van De Graff Branch, V-a-n-D-e-G-r-a-a-f-f, and that person was Al Knudson, K-n-u-d-s-o-n. So, he had the accelerator that could produce the high energy ions, I had the X-ray hardware, and Burkhalter was the specialist in interpreting the X-ray spectra. So, we installed this spectrometer in a vacuum chamber over in Building 74 and directed a 5 MeV beam of nitrogen ions at an aluminum target and scanned through the spectrum. And where the people in Texas had measured only a big hump, we got detailed spectra with lots of individual lines in it. And we understood that the origin of those lines as being due to multiple inner- shell vacancies in the aluminum atoms that had been struck by the high energy nitrogens. And we prepared a paper and published it in Physical Review Letters, and our paper appeared just a few weeks before the sequel to the Texas group paper was published, also using high resolution. And that began a roughly five year period of friendly competition with that group and other groups that had jumped into this area, because it was just good fundamental physics. And there were a lot of stories to tell from that period, among them the attitude of a professor in the Texas group, not the lead fellow but another fellow, who wanted to have some number of papers before he hit forty years old. Maybe 100 papers. So he would measure a spectrum and send it off, and measure a spectrum and send it off, sort of dribbling his results into the literature, and we would get all these things to referee. Anyway, we had wonderful refereeing wars during that time.

van Keuren: A refereeing war. Do you want to go into that a bit? How was it a war?

Nagel: Well, to get papers into top journals, be it Nature or Phys Rev. Letters or whatever, one usually has to argue. The general procedure is you submit an article for publication, the editor sends it out to two, or three, or four reviewers and unless it is really, really, you know, the top of the top work, the negative reviews will happen. So you get those back and then you have to respond to them. And the only way to win the war, that contest between the authors and the referees, mediated by the editor of course, is to just argue them down point, by point, by point. So, we were on both sides of that. Sometimes we'd submit papers, and we'd have to argue to get them published. Sometimes we would get papers from others that we didn't like, and we would have to argue to get them rejected.

van Keuren: I see. Your work at the Nevada Test Site was pretty applied in nature. The work that you sound like you're doing in the Laboratory on X-ray spectra sounds much more basic. Am I correct in this?

Nagel: Absolutely. The ion excited X-ray spectra work was very basic. Toward the end we were getting some guidance, shall I say, from the division level at the Laboratory to figure out applications for that work, and in fact we did some research to see whether or not ion excited X-rays would be good for chemical analysis as distinct from the usual X-ray excited X-rays or electron excited X-rays. And there is a place for that, and in fact companies have been formed on that, but we really never followed-up on that. The X-ray spectroscopy of the high temperature plasmas, be they laser excited or discharge excited, was more applied because the bases for that work were two-fold. One was, as I already mentioned, the simulation of nuclear weapons radiation, and the other, which I haven't talked about, was energy generation. Fusion energy, which is also applied. So, we were really doing the gamut from basic research, the ion impact work on our 6.1 money, through relatively applied, still publishable, but relatively applied work on outside funding.

van Keuren: And this was all within the X-ray Optics...what was the division name again, I'm sorry.

Nagel: Well, it was the X-ray Optics Branch.

van Keuren: X-ray Optics Branch. And the division was?

Nagel: The division was varied, and in fact Birks used to call it the X-ray orphan branch. We would bounce from one division to another, change our code numbers, change our bosses, but do the same thing as if we were still in the same division. And in fact, one of my interests is in the evolving division history at the Laboratory, and we can talk about that separately.

van Keuren: Okay. In the mid 1970s you were involved in the study of extremely hot plasmas as produced by powerful lasers. I am interested in this work. Now, this was related to the Laboratory's work in laser fusion, I understand. Do you want to talk to me about this work and how it related to the laser fusion program, and secondarily I would like to hear talk about the whole laser fusion program under John Emmett and then Steve Bodner. But let's start about your own work.

Nagel: Yeah. A fellow named Mallozzi from Battel, M-a-l-l-o-z-z-i, from Battel in Columbus had a high powered laser in the 1970 time frame, and he directed pulses at targets and found that they emitted X-rays. And he gave a paper in Ishfahan, I-s-h-f-a-h-a-n, Iran, sometime around 1970, '71. And the fact that you could use a laser to produce X-rays caught our attention immediately, and also the attention of the people who had the high powered lasers at NRL, namely John Emmett and John McMahon. And we agreed to measure X-ray spectra from their lasers, produced by their lasers, when they were ready. And I remember there was a period of some months, maybe half a year, when we were concerned that Mallozzi would be right on the following point, namely that a pre-pulse, ahead of the main laser pulse, was necessary in order to produce X-rays. Our feeling was that that wasn't the case. The issue was whether or not the laser could be configured to produce such a pre-pulse. So, there was that uncertainty, I remember clearly, and it happened when once we got into the lab and the laser was working without a pre-pulse [that] we saw X-rays abundantly. So that pre-pulse really wasn't necessary. We made some very, very simple spectrographs. Some I had the technician make, I could have made them myself. They had no moving points. They were not scanning spectrometers, because as I said already, the short pulses precluded that. And it was easy to measure X-ray spectra, and to interpret them actually, and to publish them, again, in Physical Review Letters. I mean, it was straightforward. So, once we got going in this, it was so much fun and such interesting work, that I remember coming in on a Saturday one time, and one fellow John Holzrichter, H-o-l-z-r-i-c-h-t-e-r, ran the laser, and I ran the lab, taking X-ray spectra. We just accumulated spectra there. We shot all kinds of different elements. We shot alloys. We shot glass targets. I remember one time we shot Lincoln in the temple on a penny in order to get a copper spectrum. It was almost like playing, except that we had the combination of a first rate laser capability, and a first rate X-ray capability, and everything we did was new at that time. We accumulated much more data than we could analyze. We analyzed much more data than we could publish, actually, because it was a matter of like going to the grocery store and picking out only the best fruit. You know, we had a tremendous latitude there.

The departure of Emmett to Livermore was memorable because he tried to get a number of us to go there, especially John McMahon, who was the laser expert, and John decided to stay at the NRL and later went on to a very successful career in the Optical Sciences Division. He was Associate Superintendent and Chief Scientist, as I remember, until his retirement. Holzrichter did follow Emmett to Livermore and did very well there, and I believe is still there and runs what amounts to their 6.1 program. I was offered the job as head of the target group at Livermore, and told that I would have my own little laser on the side to play with, which I found out later wasn't the case. I turned down the job, one of several major offers I had along the way during the time at NRL. The first really serious one. And was glad I did for two reasons. One is I had a very enjoyable and successful career at NRL. Another reason is that the target chamber, a vacuum system inside a massive structure at Livermore where these large lasers were, was mis-aligned during an earthquake one time, and I was happy I didn't have the responsibility for getting that system back in operation. It was a few stories tall and maybe 30 feet in diameter, and it was a large engineering structure. Anyway, I made the right decision by staying at NRL.

The experimental side of the work was headed by John Stampfer, S-t-a-m-p-f-e-r, Stampfer, S-t-a-m-p-f-e-r, who worked in the Plasma Physics Division at NRL for a long time, and may still be there. I've seen him recently. I am not sure whether he is retired or not. And he was the one who discovered that these laser-produced plasmas made mega-gauss level magnetic fields. And he was well-known for that. He was not an organized leader of a group. Barry Ripin, R-i-p-i-n, and Steven Bodner joined the group. Bodner was the head of the group, the overall group, laser side and experiment side, and Ripin headed the experiment side. After many years, Ripin left to join the American Physical Society, working in College Park. And then many years after that, after a very successful career, of course, Steven Bodner retired from the Laboratory roughly a year ago now, I think.

Now, I can comment on our relationship between the X-ray spectroscopy group and the laser plasma group. It involved, on our side, not only myself but also Phil Burkhalter, and Bob Whitlock, and Charles Dozier, a number of people, technicians. It was an important component of their overall program and went on from year to year, largely under DOE funding. It started out AEC and then it was ERDA, E-R-D-A, the Energy Research & Development Administration, and then finally the DOE. The relationship was productive technically, but not entirely pleasant for two reasons. One is when funding cuts came, we were always the first to get cut because we weren't part of the family. And secondly, Bodner had an interesting management habit of pitting researchers against each other in order to get the best out of them. And it was, in some sense, productive, but was very, very difficult from a personal viewpoint. And he chose to compete Jacob Grun against Robert Whitlock.

van Keuren: Jacob?

Nagel: Grun, G-r-u-n. And it was a very difficult time for those people because of the way in which it was structured and managed. And it was something I basically despised for all of the success at NRL, and in representing the NRL to the outside world, and in defending our turf against the Laboratory's turf, that is against the DOE and everything. Bodner had a highly successful career, but he had what I viewed as a very glaring weakness in his handling of individual researchers. Let me pause for a minute. During that same period in the early '70s, we were also working with another part of the Plasma Physics Division, namely the pulsed power people, and the relationships there were much more amicable. That group was headed by Jerry Cooperstein, C-o-o-p-e-r-s-t-e-i-n, and had in it some extremely capable experimental theoreticians. One of the people spanning the two groups was a fellow named Shyke Goldstein, S-h-y-k-e, Goldstein, G-o-l-d-s-t-e-i-n. And he now works for a company only a few blocks from the University here. Another key fellow was David Mosher, M-o-s-h-e-r, who I got along with particularly well. When I became a division head at NRL in 1985, I suggested to Coffey that I needed an associate division head, and Mosher might be a good candidate. And Coffey was unenthusiastic on both counts. He didn't think that Mosher was the right kind of person. And in fact, wanted me to go without an associate division head for a while, as a trial. And that was a failed experiment. I later got an associate.

van Keuren: Can you give me your overall views of the laser fusion program at the Laboratory? I also would like you to discuss the circumstances under which Emmett left, because that was relatively controversial.

Nagel: Well, let me start with that. Emmett's departure was controversial, I think, for two reasons. One is just the fact that he was leaving. I mean, he was the main person. The acquisition of the high powered neo-dynium glass laser was a very complicated affair. It was made in France, and we couldn't buy from France, so it was, you know, handled through Canada or some other back channel. But the bottom line was, you know, he and John McMahon succeeded in getting that laser to NRL, ahead of all other defense labs, most other major laboratories, with a notable exception of Batell, Columbus, as I already said. So, he was a leading light in terms of front-line work, and the simple fact that he wanted to leave to go to Livermore was noteworthy. Now, it was compounded by the fact that he was trying to suck a number of the good people out of NRL, and that raised concerns. In fact, Edward Teller who was key in getting the laser fusion program started at Livermore, and who essentially convinced Emmett to go there, would come into NRL and move from building to building to recruit people. Which, of course, raised some hackles as well. And as I have already intimated, there were promises made at that time that ultimately just weren't delivered by Livermore, although the people who did go out there did well, in the larger sense.

Emmett's adventures, if you will, in leaving NRL paled in comparison to what he did when he got to Livermore in building up the National Laser Fusion Program–these lasers that essentially were a large fraction of the size of a football field. And he has been gone for some time now, but the current laser, the National Ignition Facility, which is, you know, part of the stream of big facilities that Emmett started out there, is a very expensive facility. A fair fraction of $1 billion. Associated with it [is] a great deal of controversy, both in terms of its justification and its expense. Emmett when he left Livermore did under some kind of a cloud of problems, having to do with personal use of Laboratory equipment. So, Emmett was throughout his career, from the time he was leaving NRL through his entire career at Livermore, a very controversial fellow. Now, I can go on to talking about the Laser Fusion Program, if you like now, as a separate entity?

van Keuren: Yes. I have a question I would like to pose first. In discussions with both Bodner and Coffey, and I think with John McMahon, I have been told that a major reason why Emmett left is a difference in philosophy. Emmett wanted to go on to create a laser, a practicing laser program, which was more developmental as opposed to experimental, whereas Berman's and others view was that the Laboratory should stay as an experimental facility, rather than a large developmental center. Do you have any sense of this?

Nagel: Yeah. I think that there is a lot of truth in it. That Emmett wanted to build bigger, and bigger, and bigger lasers, which of course he did at Livermore. And that is essentially incompatible with the NRL approach to things. Now, having said that, you know, the NRL has not chided away from building some pretty large facilities like the Madre Radar and other things over the years, too. But Emmett had this agenda that he wanted to just go on from one system to another, to another. Even at that time it was clear, I think. And I don't know if there were any personal elements involved. It could have been entirely, you know, a business viewpoint. But I believe what you said is certainly consistent with my perception of the time.

van Keuren: Okay. Do you want to continue talking about the Laser Fusion Program?

Nagel: Yeah. The Laser Fusion Program was interesting, on all levels from the highest to the lowest nitty-gritty levels. At the highest level, it was really a program funded by the military side of what later became the Department of Energy, as distinct from the energy side of the Department of Energy. So, the production on a small scale of what goes on inside of a hydrogen bomb really was the fundamental basis for the Laser Fusion Program all along, and these scenarios of dropping pellets at high rates in the chambers, and having big lasers fire at the associate high rates in order to produce energy, you know, were stated both to Congress, and the public, and to other scientists, but they gagged most of us. So there was always an element of falsehood hardwired into the Laser Fusion Program from the highest levels. Okay. It was, of course, a competitive program with the Soviet Union, as was most of plasma physics from the beginning.

van Keuren: Excuse me. Let me push you on that.

Nagel: Yes.

van Keuren: You're saying that the whole Laser Fusion Program, and, in fact, indeed the field of plasma physics was an outcome, an outgrowth of the Cold War competition between the U.S. and Russia?

Nagel: It was if not an outgrowth, at least fed by it. Plasma physics became a subject unto itself somewhere in the '50s. It was classified at that time, to a large degree. It was clearly not sustainable as a classified program, so it was declassified around 1960 or so. And there are many, many stories that the plasma physicists will tell you about going to Russia and trying to get information out of the Russians, while the Russians are trying to get information out of them and all of this. But the whole field of plasma physics writ large, and the field of high temperature plasmas created both by lasers and by pulsed power discharges, was very, very competitive for both nuclear weapons and energy reasons all the way through. So, what I'm saying about the Laser Fusion Program is that it was justified on an energy basis, but the roots of it really were in weapons related physics and the production of X-rays to simulate what comes out of weapons.

van Keuren: And this was true at the Laboratory?

Nagel: This was true at the Laboratory because of the fact that the bulk of the money came from what is now, as I say, the DOE, but some of it came from the Defense Nuclear Agency or first DASA, then DNA, then DTRA, whose responsibility --

van Keuren: DTRA?

Nagel: The Defense Threat Reduction Agency, DTRA.

**Side Two**

The responsibility of DTRA was to produce,to fund devices that would produce X-rays to simulate nuclear weapon effects. So, they provided some of our funding too, and that was, you know, hard core military. It had nothing, really, to do with energy production. So, to summarize, the high level justification of the Laser Fusion Program was interesting in itself. Then when you get down to the level of actually doing the work, you had institutionalized in the DOE the competition between Livermore and Los Alamos. Livermore was doing glass lasers, Los Alamos was doing CO2 lasers. Short wave lengths and long wave lengths lasers, and there was a constant argument about what would be better for laser fusion. Then there was another argument that cut across both laboratories on whether or not directed radiation of pellets would be better or the production of X-rays, and then the use of those X-rays to implode a pellet. Essentially, a micro version of what actually goes on inside of an H bomb, or an A bomb, provides the energy that causes the laser fusion reaction.

van Keuren: And the genesis for all this work was the nuclear test bomb treaty? The testing treaty. Or because it was cheaper to do it and easier to do it this way?

Nagel: Both. The fact that there might be a total prohibition of underground as well as above ground tests, as well as the fact that,as I said earlier, you can get a lot more data, and experiments are cheaper, and so forth, by doing laboratory rather than field work.

van Keuren: Okay.

Nagel: So, then there was a competition between the experimental side of the program, wherever it was pursued, and the theoretical side. And the people at all the labs that played--Livermore, Los Alamos, and NRL--you know, had vigorous arguments over the theoretical and computational side as well as the experimental side, whatever way [?] were used. Now, in this whole mix, I say most of the money went to Livermore and Los Alamos, but a few percent went to NRL. And the function of NRL, and a function which Bodner did spectacularly, was to keep the big labs honest. To ask the hard questions. To do the really telling experiments, and the NRL has a spectacular record over the '80s. Well, starting with the '70s, through the '80s, and into the '90s in performing that function. Now, Los Alamos lost the wave length war--that the CO2 lasers were shown to produce too much of the high energy electrons during the interactions. And they retain some function in the overall laser fusion community, but basically Livermore ran away with the action. And meanwhile, NRL had developed a technique for smoothing the laser pulse when it hits the target in order to provide a uniform acceleration and implosion. And that was the basis of the Laboratory getting the NIKE program, which has been a many year, many millions of dollars laser development. First at low rep rates and in the current time frame, high rep rates. The NIKE laser, of course, is an excimer laser, e-x-c-i-m-e-r, which is a wavelength that is roughly a quarter of a micron. That is shorter even than the endemnian [?] wavelengths that were pushed at Livermore. Curiously, the excimer laser was invented at NRL. Stu Searles, S-e-a-r-l-e-s, who is still at the Laboratory, is the person who first found that excimers could be made to lase. So, the irony of NRL having a major laser development program in this Laser Fusion Program overall, is fascinating.

Now, the Laser Fusion Program became more important with the cessation of underground tests. And in fact, the two parts of a hydrogen bomb, namely the A bomb part and the H bomb part, remain of interest for at least two reasons. One is the potential development of new weapons (A). And (B), the so-called stockpiled stewardship, that is the maintenance of the nuclear weapons that we now have in the inventory. So, while Livermore continued with the building of larger and larger laser fusion facilities that could simulate the implosion on the fusion side, the H part of the H bomb, the hydrogen side if you will, Los Alamos succeeded in getting funding to develop giant flash X-ray facilities that would study the primary or the atomic fusion side of the weapons. And in fact, over the years I have sat on many evaluation committees, as has Tim Coffey, and Steve Bodner, and other people. And I remember being part of a study of the flash X-ray facility that Los Alamos was proposing to study the implosion of the uranium or the plutonium and their primaries. The facility I remember was called DAHRT, D-A-H-R-T. Dual Access High Radiation Test Facility or something like that. And anyway, I suspect that it has gotten built, but I don't actually know it.

van Keuren: Stepping back a moment again, when we were talking about the justification of the programs at the Laboratory, the Laser Fusion Program, you talked about being gagged at a certain level that...

Nagel: No, I wasn't gagged. I gagged on the justification of the Laser Fusion Program as an energy program. And the reason --

van Keuren: And that is what they sold it to Congress as?

Nagel: Well, that was one of the things. And that always struck me as dishonest. And if you are going to turn electrical energy into some other form of energy in order to ignite fusion, which is what the whole fusion program was about, be it on lasers or in tokamaks, t-o-k-a-m-a-k-s, every time you do an energy conversion scheme, it's lossing. So, if you go from electrical to light energy, laser energy, and then to fusion energy, you have gone through an extra conversion. If you can go from electrical directly to fusion energy, as you do with pulse power, which NRL was involved in, as I said, and Sandia, the Sandia National Laboratories in Albuquerque, has been the main leader of, then you bypass the light stage. So, the other thing is that if you are going to go through a laser stage, material is damaged. Glass, thin films, and other things, limits the amount of energy it can handle per square centimeter, which means you are going to have to have very large systems. And the other thing that overlaid the entire fusion program that doesn't get much attention, but whether you're using laser fusion or these confinement schemes such as tokamaks, you know, some people were given to believe, or led to believe, that the nuclear waste problem, nuclear waste problem would go away. That the leftover from nuclear fuel rods, which is the current reactor waste problem, would just have no analog if you went to fusion, but that's not true. Because the high energy particles that are produced in these fusion reactors would activate the materials around them, so there would still be a waste problem. Okay. So, that was another kind of flimflam on my view, on the part of the energy and physics community in selling fusion of both kinds. Now, even though I spent the bulk of my efforts during the '70s and into the '80s looking at laser produced and discharged produced plasmas, I did also get involved in some tokamaks spectroscopy. The reason for it was that the X-ray measurement techniques that we had developed were among the best, not only nationally, but worldwide, and they were in need. I'm sorry. The people who made these dilute but very hot plasmas and tokamaks were in the need of developing first rate X-ray instrumentation to analyze what was going on inside those plasmas.

van Keuren: Now, the work on the tokamak facilities at the Laboratory. There was one at Princeton. Was that the one you were involved in?

Nagel: We were only peripherally involved in the one at Princeton, because of a person named Ken Hill, H-i-l-l. Ken had postdoced in the time when we were shooting ions against material and measuring X-rays, and he went on to be one of the main X-ray diagnosticians at Princeton, and was a professional friend, so we would talk to him. But we got funding to put X-rays instrumentation, and also spectrometers for measuring ultraviolet radiation, on a tokamak in Austin, Texas. The name of the machine was TEXT, T-E-X-T. The Texas Experimental Tokamak. So, the two of us, Richard Bleach and I, would work on that.

van Keuren: Richard Bleach?

Nagel: Bleach.

van Keuren: B-l-e-a-c-h?

Nagel: That work was in the early '80s, and it was in March 23, 1983 that Reagan gave his famous Star Wars speech. One of the questions that Congress had at that time was whether or not the American public wanted a Star Wars program, a shield against missiles in space. So, they did the survey and they asked people, are you satisfied with the defense that your country is now providing you against nuclear missiles from the Soviet Union or elsewhere. And half the respondents said yes, we're happy. And then the second question was you don't have one, do you want one? I learned this in a Congressional briefing somewhere around 1985. So, it was clear that the American public wanted a defensive shield against missiles. The next question was, Reagan was proposing a five year, $26 billion a year project, and the question was could the U.S. prosecute that kind of a program. There was going to be a Congressional study on that. Bleach was dating a woman who knew a Congressional staffer. At the time, it came out, that he was a scientist. He was asked to participate in that three month study. I agreed to it as his supervisor because it would be good for him professionally, and it would get the Laboratory connected at the outset to the Star Wars program. Bleach did a good job. They asked him to stay another six months. And to put it on fast forward he transferred full-time to the Strategic Defense Initiative Office, SDIO, sometime in the mid '80s, became an SESer there, and still works for them.

van Keuren: So, if you were to summarize both your own personal experience in the Laser Fusion Program at the Laboratory and the history of the Laser Fusion Program at NRL itself, what would you say?

Nagel: Well, from a personal viewpoint it was one of the most successful parts of my overall career, because we did absolutely leading work. Produced a large volume of high quality work. We were really playing at the forefront. I was invited out to Los Alamos as a visiting scientist for a week every year, without any agenda. They just paid me, and I would go out there and talk to people. And it was very productive and very pleasant. From a larger viewpoint, from a Laboratory viewpoint, I was critical of, you know, one aspect of Steven Bodner's handling of people. But globally he did a spectacular job for the Laboratory and for the country in maintaining a slice of the funding coming to NRL, in the face of great hostility on the part of Livermore and Los Alamos on one hand, and then doing utterly wonderful work with that funding, both in terms of laser R&D and in terms of the use of the lasers to other things, of both military and non-military character. Tim Coffey also played a very, very central role in this because he was a plasma physicist and former head of the Plasma Physics Division and very well wired into this community. And he worked the higher level, serving on national review committees and the like, while Steve, you know, kept the detailed program running. Sid Ossakow played a key intermediate role between Bodner and Coffey, both on a programmatic and personal basis, and I think was essentially indispensable to the progress that the Laboratory made. The future of the Laser Fusion Program has been of great fascination to me, because this repetitively pulsed excimer laser will come on-line at NRL before too long. The National Emission Facility will come on-line at Livermore. Meanwhile, other countries, even Russia, but especially France and Japan, remain active in this overall area. So, it will be fun to see if the Laboratory maintains this strong position over the coming years.

van Keuren: In 1972, you became a section head. What section was it? Was it the X-ray Optics Branch?

Nagel: It was within the X-ray Optics Branch, and the section, I think, was entitled the X-UV, X-ray and Ultraviolet Spectroscopy Section, because we were employing both x-ray tools and grazing incidence grating tools.

van Keuren: And how were you selected for this position?

Nagel: The branches at NRL were monolithic and, in many cases, funded entirely by internal 6.1 money in the '60s. As we grew because of the nuclear weapons work in the '60s, and as the funding became more complicated and diverse. First only 6.1 money, then 6.1 in DASA, then some DOE money, and then money transfers within NRL. Anyways, the funding situation became more complex. A branch head really wasn't able to stay on top of the overall situation, so that sections were set up within branches. And when the sections were set up, I was asked by Birks to be the head of one of them. And it was a sort of a natural thing, because I was in a very literal sense leading the work.

van Keuren: How did this affect your work?

Nagel: It meant that I had responsibility for people. And in fact, one of the most dramatic transitions that NRLers make is going from the sixth level to the fifth level. The Laboratory, of course, is a six level organization with workers, section heads, branch heads, division heads, associate directors, and a director. And the structure is very, very simple. The top management, the director and the associates, half a dozen people roughly, run the lab, and then there is a middle management layer of roughly twenty division heads and one hundred branch heads. And then underneath them the first line supervisors, the section heads, and then beneath them the workers. So, when you take the transition from the work only level to the section head level, you accept responsibility for people. That is a very, very major transition. Operationally it wouldn't change things a lot, because I was already, like I said, serving as sort of the father of the group. But practically speaking, the responsibilities for people and for the funding of those people, you know, were a serious addition to my overall scientific responsibility.

van Keuren: But your work in the Navy and then the reserves had helped prepare you for this, hadn't it?

Nagel: Absolutely. By the time '72 came around, I had already served as first a division head and then the department head on a ship. I had a leadership courses as part of my ROTC training. I had a continuing responsibility in the reserves, and I think by that time I had already been commanding officer of a unit, or at least as the executive officer. So anyway, I was used to being in a leadership position.

van Keuren: And did you find any major differences between commanding men within a Navy or reserve division and leading scientists within a laboratory section?

Nagel: It was a stark difference. I mean, the Navy if you told somebody to do something, more often than not they would do it. At NRL you told somebody to do something, more often than not they wouldn't do it. The expression is herding cats. I mean, one of the biggest...one of the reasons I took a section head job was to multiple my effectiveness. I had lots of ideas. Lots of things I wanted to do. I figured if I had people working for me, in quotes "for me", than I could get more done, and it turned out I was working for them. And the strength of NRL then, and to a large degree now, remains the autonomy that the individual researcher has. As I was quick to learn that I had the additional responsibilities, and I had authority, but it wasn't that valuable.

van Keuren: Did you end up spending a lot more of your time making other people's work easier?

Nagel: Oh, absolutely. And that's everything from insuring that the heating, and the ventilation, air conditioning, and so forth worked. That the roof didn't leak. I spent an inordinate amount of time on leaky roofs during my career at NRL. Anyway, just the housekeeping kinds of things to the more serious problems of how we should spend our time and to what proposals we should make, writing those proposals, giving the programming reviews, and so forth.

van Keuren: You became heavily involved in development of X-ray lithography. You want to date this for me, and tell me about it?

Nagel: It could have been 1975; it was certainly close to that. The technique called lithography is the method by which patterns are put down on integrated circuits and other chips. It is one of three steps that you need to make chips. The other two being the deposition of material, and removal of material etching. So, it is really central to integrated circuits, and therefore, to the whole computer revolution, information revolution, internet revolution, satellites, everything. It was clear, even in the '60s, but through the early '70s, that the line widths in chips would continue to shrink in order to put more on a chip and drive costs per transistor down. And the diffraction limit, which is the minimum size to which you can focus a beam of light, is wave length dependent. The shorter the wave length, the shorter the diffraction limit, the smaller feature you can pattern. So, even in the early '70s, it was evident that the line widths would go from a few microns, to a micron, to sub-micron widths, and the diffraction limit of ordinary visible light, which is in the .4 - .7 micron range, would run up against a wall. You could no longer make finer and finer wave lengths using visible light. Well, the field had already gone to ultraviolet light, shorter wave lengths, and I could see it going to shorter and shorter wave lengths, but there is a barrier at two-tenths of a micron.

Yeah, there's a barrier at two-tenths of a micron set by atmospheric absorption. So, it was thought that some time, possibly in the '80s, the requirements for relentlessly decreasing line widths, would require going to wave lengths that were shorter than 200 nanometers, or two-tenths of a micron, and it would be necessary to go to use X-rays.

X-ray lithography had been invented in 1971 by Professor Henry Smith at MIT, Hank Smith. And in the few years after that, the problem with it was exposure times. Ordinary X-ray sources, like X-ray tubes, are very inefficient. Only a small fraction of the electrical energy gets converted into X-ray energy. A percent or less. Now, we were, as I said earlier, measuring the X-rays from laser heated plasmas, where ten and more percent of the energy that was in the laser beam could be converted to X-rays. So, somewhere in the mid '70s Bob Whitlock, W-h-i-t-l-o-c-k, and I went to the Advanced Technology Laboratory of Westinghouse near Baltimore to visit a fellow who used to be in the X-ray Optics Branch, namely Martin or Marty Peckerar, P-e-c-k-e-r-a-r, and we were talking about X-ray lithography. And it dawned on us that the laser plasma sources might be a good way to do X-ray lithography. There was enough energy that even after you suffered the inefficiency of converting laser light to X-ray light, you would have enough total energy to expose photo resists. So, two things happened after that. Peckerar returned to the Laboratory, to the Electronics Science & Technology Division, and I wrote with him an invention disclosure that later became a U.S. patent in around 1982, as I recall. And then the two of us, and Whitlock, and two people from the Plasma Physics Division who had a laser, namely Bob Pechacek, P-e-c-h-a-c-e-k, I think. And Bob Greig, G-r-e-i-g. The group of us did an experimental demonstration that this worked. We sent a paper to Applied Physics Letters and lost the refereeing war there, they ultimately rejected it. And we published it in Electronics Letters.

The evident availability of an alternate source for X-ray lithography got a lot of attention. I offered a manuscript to a conference in California, and on the basis of the abstract that I sent in, the fellow organizing the conference offered me a job and wanted to form a company. I turned him down, of course, gave the talk and many others. And there was a burst of activity in the late '70s on advancing the state of the art in X-ray lithography. We got, as a result of that work, a significant amount of money from the DOD VHSIC program, V-H-S-I-C, which stands for Very High Speed Integrated Circuits. That was a program that began with a group of people touring the country, quite literally, to visit companies and university laboratories that had capabilities relevant to X-ray lithography. Sources, and photo resists, and masks, and the liners and so forth. I was part of that group and got to know a set of people that many of whom are still active, and it was a good bonding experience, traveling around with those people.

The DARPA eventually formed an X-ray lithography program because of a Congressman from Long Island who was representing the Brookhaven National Laboratory, who wanted to work in that area. And that program still exists. It has been run for maybe fifteen years by a fellow named David Patterson, P-a-t-t-e-r-s-o-n. Patterson once ran the Fabrication Facility at NRL that was later run by Marty Peckerar, and then Christy Marian. Christy Marian is now at DARPA, as is Patterson.

So, the X-ray lithography was going to supplant optical, or actually UV lithography. Sometime around 1980 a fellow named Moshe Lubin, M-o-s-h-e, Lubin, L-u-b-i-n, bought the rights to our patent from the Navy, and this was before the Technology Transfer Act of 1986, so it wasn't the kind of thing that you would have now. And he also bought the rights to a patent from Batell, where Mallozzi had done the work in about ten years earlier, roughly. And he formed a company called Hampshire Instruments, Incorporated. Now, Lubin was an interesting case. A plasma physicist by training, a brilliant, pleasant, well-spoken fellow. When he was at the University of Rochester, he formed the Laboratory for Laser Energetics, which is a major laser Laboratory that has been funded maybe the last fifteen years or so by the DOE to perform much the same function of the overall Laser Fusion Program that NRL does: as an alternative intellectual center doing good experimental work. Lubin, after setting up that Laboratory, became a Vice President of Standard Oil of Ohio and left there with some buzz in the community, which I have never understood, some question about behavior or something or other. Anyway, he returned to Rochester to set up Hampshire Instruments. The company existed for roughly ten years, it burned $100 million of venture capital and government money, and had an aligner, a full up machine for taking wafers in and processing them, exposing them with X-rays, for sale for $5 million a unit.

But the acceptability in X-ray lithography was still a question at large in the community, because the optical lithography people had, despite being stuck with certain limited wave lengths, they had gone to techniques such as planarization and thin resist, and light that had allowed them to, if not beat, at least ameliorate the diffraction limit. So, at some point it became evident that X-ray lithography wasn't going to make it commercially. This was probably around 1990, it might have been somewhat before or after. And Hampshire Instruments folded, and it was a very sad time for me, because here was a technology, X-ray lithography, based on our idea, laser heated plasmas, that I thought was going to wind up producing chips around the world for the entire industry. And, of course, by that time the computer revolution was in full swing, and the internet hadn't appeared yet. But anyway there was plenty to be happy about. So, the company folded, which was bad news, but the worst was Lubin blew his brains out then, which was sickening, because he really was a wonderful human being. I played golf with him one time in a free afternoon during a conference in New Hampshire, and he was a genuinely likeable guy. It was a very sad story.

Now, the X-ray lithography has not gone away. As I say, the DARPA program continues. The program has actually been renamed lithography or advanced lithography, more generally. There are other things in it besides X-rays, but it's potentially still the technology of the future when it comes to making chips.

In the 1990 era, I continued to participate in that program as a reviewer. I would go to their annual meetings, not at all because they were in Florida or in Arizona during January, of course, and yet I could see that I had essentially shot my wad in that community. I worked in it for roughly fifteen years, and it was oozing forward, and I was quite happy when the environmental nuclear pollution came on the scene in 1992 to jump into that and step off the X-ray lithography bandwagon.

So, right now chips are being made with line widths as fine as .25 microns, soon .18 microns. The limit of photo-resists is probably .005 microns. So there may still be a little window between 180 nanometers and five nanometers where X-ray lithography could step in and be the way in which chips are made. But it may not be that way, because of the fact that when you change from optical or UV lithography to X-ray lithography, you have different sources, different masks, different resists, different aligners. It's a radical,

radical change. And there is so much production pressure and so much installed capital in the lithography industry, that the change may never come about. So, it could turn out to be that the efforts spent on X-ray lithography by us, by NRL, by the community at large, which will in the end total way over $1 billion may be for nought.

However, I would say by way of closing here, that I stepped from nuclear weapons, to laboratory X-ray plasmas, to X-ray lithography, so it was a key part of my overall professional development, and formed the basis for the next step to micro electro-mechanical systems where I am working now.

van Keuren: Just one last clarifying question. So this was your primary involvement from about the mid '70s until the early '80s?

Intellectual involvement.

Nagel: Yes, that's right. I continued to do some plasma diagnostics that would come in and out of the X-ray spectroscopy, but that was the main thing. But, of course, during that time I went from being a section head, to being a branch head. I had five years as a branch head, then became a division head in the mid '80s, so I was increasingly distracted. In fact, this plot of numbers of papers versus years shows a down slope when I became a branch head, and then a steeper down slope when I became a division head.

van Keuren: And the development work in X-ray lithography, was it funded by 6.1 money?

Nagel: Yes, there was some 6.1 money, but initially it was mostly the VHSIC program money, and then DARPA money. So that the bulk of it by far and away was outside funding.

van Keuren: Outside funding?

Nagel: Yeah.

van Keuren: As you have mentioned, you succeeded LaVerne Birks as section head in 1980.

Nagel: As branch head.

van Keuren: Excuse me, as branch head. Do you want to tell me about this promotion, and how does this affect your responsibilities?

Nagel: Yeah. Now, Verne, as he was called, Birks, LaVerne Stanley Birks, full name, was as I said, hired by Friedman, along with others in the early '40s. He I think had a masters degree from the University of Illinois, if I remember correctly. And he knew from shortly after being born that he wanted to do X-rays, to talk to him. I mean, he was very singular in that regard. And he tells a story of having a project during an open house in the engineering school at Illinois where he made an X-ray tube and people could come by and look at the bones in their hands and so forth. And the only thing that kept him from being fried while he was running it, was the fact that the control console was underneath the X-ray tubes, so the X-rays went over him. But, it was like these old X-ray shoe machines that people that came in were exposed to significant doses of radiation, but for short enough times that they probably weren't harmed by it. So, when Birks came to the lab he worked on X-ray spectrum chemical analysis. He built the first electron microprobe in the United States. It was pioneered by a Frenchman, and Birks built that instrument sometime probably in the '50s. I worked on it when I joined his group in 1964 for a year before I went off to the Nevada Test Site. And he was a very tough fellow. He wasn't abrasive or loud or anything, but he really was a strong guy inside. And I remember one time I went over to get an o-ring from somebody, and they asked me whether it was for me or for Birks. I said it was for me, and they gave it to me. So, he had gotten cross threaded with some people within the lab, I don't know over what. I never did and never will. But he treated me very well. He treated some other people far less well. He basically would bin people into one or two boxes early on, the good guys and the bad guys. And then if you were in the good guy box you could do no harm, and if you're in the bad box you could do no good. He was very opinionated that way. So, the big change in his career was the Nevada adventure that we described, and then while I went off into plasma diagnostics, Birks essentially sat that one out. And then I got involved with synchrotron radiation in 1978, which is another story, and Birks absolutely refused to get involved in that. He knew already at that time that he was late in his career, and he wasn't going to spend any time getting spun up into a new field.

He had one serious medical problem. He had a tumor in his back that pinched his spine, and I think that was taken care of. And I think that essentially put him in the frame of mind that he was going to enjoy some part of his life, and he wanted to retire. So, much to my surprise when he retired in 1980, he just disappeared. I mean, he absolutely didn't have any professional tail at all. He made a significant amount of money in real estate, and he and his wife Mary Jane moved to Columbia, South Carolina, built a three story house on some lake, and he golfed and fished until he developed leukemia and died probably before 1990.

Now, regarding my responsibilities, once I moved into the branch head job, then I was responsible for a much larger group. But, I had more funding problems, more space problems, more equipment problems, more everything problems, and my time to do science decreased. Time to do research decreased accordingly, but it was something that I expected to do. I, in fact, felt that I was parked behind Birks for a large fraction of the '70s. You know, I could have run the branch probably from 1975 on after a short stint as the section head. I wasn't unhappy not doing it, but basically I was bringing in money more successfully than others, and in order to balance the funding within the group, Birks would take it away from me and give it to other people who weren't as successful at bringing money in. There was a background of tension associated with that as is commonly the case within groups at NRL. And, anyway, when Birks decided to retire, I was ready to do some other responsibility.

van Keuren: So, at that point in your career the intellectual activities were directed towards the work with the X-ray lithography and the other half was directed towards administration?

Nagel: Yeah. And the intellectual activity side of it still had some [?].

van Keuren: Now, you took a sabbatical in 1978?

Nagel: That's right. I mentioned already, I was considering it in 1972, thinking to go to Monterey, but because things were going so well at the Lab, I decided not to do that. And then in the middle '70s, as I mentioned, I was forced to pay attention to my thesis and took a couple years to mock that up, write it up, so forth. That was still a day and age when things were typed out the old fashion way. And changes were difficult, so anyway I finished the Ph.D. in 1977, and then was interested taking a year, but I didn't want to leave the area because we had kids in school, and we had moved into a house in 1975, we didn't want to step out of there. So, it became a question, what to do in the Washington area, and the answer was to go to the NBS, National Bureau of Standards at that time, and work on their small storage ring, which was a source of ultraviolet and soft X-rays. The emission mechanism would be called synchrotron radiation. So two of us, Dick Williams from the Optical Sciences Division, and I designed the beam line for the facility at NBS, which was called SURF, S-U-R-F, standing for Synchrotron Ultraviolet Radiation Facility. And the beam line brought radiation from the vacuum of the storage ring, a doughnut shaped vacuum vessel, out to an experiment that we designed and ran to do photo-emission spectroscopy of materials. It was a very interesting sabbatical for two reasons. One is I changed from basically a management function to a hands-on plumbing function. I was putting together vacuum pipes and cleaning them, and we would wash them with a trio of organic fluids. And at that time we would dump them out in back in the open environment. I remember the soles of my hush puppies would become so gummy from this from walking around in acid [?]. And, anyway, I put the beam line together and it flunked the residual gas analysis test, and I had to take it apart and clean it again, put it back together. Anyway, it was a very laborious thing.

The other thing that made it interesting was since I was in the Washington area, I really never got away from my responsibilities at NRL. So I sort of had a job and a half during that time, and it wasn't at all a restful period. I did almost no intellectual, had almost no intellectual growth during that time, but I was pretty well saturated from just having finished a thesis. So, there were three sabbaticals in that time period. Milt Kabler, K-a-b-l-e-r, went to England, Earl Skelton ...

**Tape 2**

**Side 1**

With the three sabbaticals as a backdrop, and the field of synchrotron radiation clearly becoming a hot area in X-ray physics and chemistry. We all wanted to go into it, and we basically had a choice. Either build our own storage ring, or go off and work at other storage rings. So, we developed a proposal to put a storage ring in the reactor building. I miss-spoke, I'm sorry, in the cyclotron building at Bowling Air Force Base, which the lab owned. The building had four megawatts of power in and cooling out. It was available because the cyclotron for which the building was made in the '60s had run its course, and it was essentially in its latter stages.

So, we went to Berman and asked him for $10 million, and fortunately he said no. Because if he would have said yes, we would have been building a significant engineering facility and having to manage that. So we said, okay, if you won't give us $10 million, how about giving us $1 million? We'll go out to Brookhaven and use the storage ring there to build beam lines of light. So, we made a team with NIST and between the two institutions built four beam lines at the National Synchrotron Light Source at Brookhaven. And those beam lines have been very productive and remain active to this day. In fact, NRL still has a contract researcher working on-site up at Brookhaven.

Now, as I said, Birks had no interest in this, but Kabler, and Williams, and Skelton and I did, and we essentially led it. Kabler and I, in particular. Williams left the Laboratory to become a professor at Wake Forest. Skelton was in the same group. He, Skelton, I should say, developed another beam line at Brookhaven for a high energy measurements at high pressures and light. And the next question was whether or not we would follow the action from the leading edge facility at Brookhaven to a brand new half billion dollar facility being built at the Argonne National Laboratory, the so called Advanced Photon Source. By the time that came, and I was on the committee that provided the justification for it, incidentally, I was a division head and could no longer lead the charge in the development of the new facility kind of project, and I wanted Mike Bell, who was a branch head in the division, to do it. And he worked at it, but never really seized it the way it was necessary. So the NRL never got and still doesn't have a presence at this leading edge X-ray facility in the U.S. right now. We remain active at Brookhaven, and that's good, but it's a shortfall, as far as I'm concerned, that we weren't able to go to the next level.

van Keuren: Why synchrotron radiation?

Nagel: The X-ray tubes are inefficient and they broadcast unpolarized radiation out in all directions. By contrast, synchrotron radiation is very efficient, it forms sharp beams in space that are polarized, and they are extremely intense. So that you can do experiments that are possible with other X-ray sources, like tubes, in some cases as much as 1 million times faster. But more importantly, you can do experiments that you just couldn't do otherwise. So, experiments that involve X-ray coherence and very sophisticated multiple scattering experiments and the like in order to study materials, are now doable on synchrotron radiation sources that were heretofore not possible. Because of the high intensity you can get diffraction data from very small crystals. You no longer have to grow larger crystals, you can work with small crystals, which is very important, for instance, for the proton crystallography. You can take the data before the radiation fries the crystal, damages the crystal. So, there are many advantages to synchrotron radiation. It can be thought of as the X-ray equivalent of a laser, although it is not.

van Keuren: So, the book end dates for your involvement in synchrotron radiation were 1978 to --

Nagel: To the time I left NRL, because we were still --

van Keuren: You were still involved?

Nagel: ...getting about $1 million a year of 6.1 money in the division for that work.

van Keuren: So that was 1999?

Nagel: '98, yeah.

van Keuren: '98.

Nagel: It was a twenty-year run.

van Keuren: Twenty-year run, but at the moment there's really nothing happening at the Laboratory, in that field, you say basically?

Nagel: No. There is still money, and there's still activity at Brookhaven. We just did not go to the next level.

van Keuren: The next level. So, you're working at the old facility.

Nagel: Yes. Yes. You can view it as, they hung on to the old Mercedes, they didn't go for the new model.

An interesting aspect of the synchrotron radiation business was that it is very demanding. You get a slot of time, even if you have your own beam lines, and you have to get sophisticated experiments. Some of them including time-correlated lasers with the circulation of the electrons and the beam. Get sophisticated experiments ready, samples prepared, everything, all the analyzers working, and the people who developed the beam lines and then went to use them for several years, essentially got worn out. And they found ways to go less and less, so the involvement of Brookhaven tapered off. And I think the reason that nobody stepped up to lead the charge to go to the advanced photon source had two characters. One is the people involved, were ground down. And secondly, the amount of money involved was roughly an order of magnitude larger. Where the involvement of Brookhaven was $1 million a year kind of thing, with a $1 million up front, the involvement at the advanced photon source would have been maybe $10 million up front, and a couple of million a year. And we would have had to go to ONR to get that kind of money, but to do that we would have had to convince not only the top management, especially and Fred Saalfeld in particular, but several of the key associates there, and they probably would have viewed it as unacceptable for two reasons. One is NRL is already getting a lot of our money. And B, if we give it, you know, to these people, even if they're going to make it available to ONR contractors, it's going to be less money we have to play with. So, that combination of circumstances led to the work winding down, even though it was and still is absolutely leading edge work.

This synchrotron radiation example of groups tiring has many other examples at NRL. I mean, there are many other examples like that. One of the ones that is most notable was the people in the Space Sciences Division who designed the Apollo Telescope Mount Experiment (ATM) worked for a significant fraction of their professional lifetime justifying it, designing it, building it, testing it, and flying it. And they amassed a refrigerated room full of spectroscopic data and were essentially unable to take advantage of it. Now, the good fortune of NRL is that there are people coming along at different stages in their career, and two people, George Doshek, D-o-s-h-e-k, and Uri Feldman, F-e-l-d-m-a-n, came along at a time when this data was available, recognizing it for the valuable resource that it was, and exploited it magnificently, to their benefit and to the benefit of the team that built the experiment, flew it, and the Lab as a whole. In fact, I worked with Doshek and Feldman early in the days of the laser target interaction work, and found them to be extraordinarily capable and productive spectroscopists. And they got that basis by analyzing the spectrum from the sun, plasmas from the sun. But I think if you go around the Laboratory you will find that there are not just a few instances of where people would justify a program and do everything up to, but not including, the data analysis.

**Interview No. 3**

van Keuren: This is April 14, 2000, and I am David van Keuren. I am sitting and talking with Dr. David Nagel in his office at George Washington University. This is interview number three. David, we may have touched upon this in part already, but could you tell me how this Strategic Defense Initiative affected your work and that of your branch?

Nagel: Yes. The most dramatic change was that in the past, before the SDIO, the time constant for something to go from the President down to me, nine levels lower in the government was about the length of an administration. So, presidents would come and presidents would go, and I would be more or less unaffected by it. But that changed immediately and dramatically with the March 23rd, 1983 speech by President Reagan announcing what came to be called the Strategic Defense Initiative, popularly known, of course, as “Star Wars.” The reason for that was two fold. One is the amount of money involved was going to be very significant for research organizations like the NRL, so it represented a brand new sponsor community. The second is that a colleague of mine, Richard Bleach, who worked in the same branch as I did became involved with the SDIO almost by accident very early on. He was dating a woman who knew a congressional staffer and at some party Bleach met this staffer, and the staffer was looking for scientists and technologists to do a study, addressing the question of whether or not the government had the wherewithal to prosecute a several year, twenty-six billion dollar project, which is what it was about in the early stages. Bleach was asked if he would participate in that study, he came back to ask me, we talked about it, the answer was yes, then he went on a three month assignment to Congress and did a good job, and it was extended for another three months and during that time he got to know General Abrahamson. A-B-R-A-H-A-M-S-O-N. The initial leader of the SDIO. Of course, Abrahamson and his colleagues were looking for good people to staff the organization, so they asked Bleach to leave NRL and join the SDIO, which he did, and he did very well there and became an SES’er and remains there even to this day. He’s currently the Chief Technology Officer. So, on both a programmatic level and on a personal level I was involved with the SDIO from the earliest stages.

van Keuren: The date of which would be, approximately?

Nagel: It was in1983. The Reagan speech was in 23 March 1983. So, what happened is as the program got started the people in the SDIO needed help in formulating the details, and they went to organizations like NRL and asked for program plans, and we, many of us at NRL, created program plans which we knew very well that we could make a contribution to later. It could be viewed as incestuous, but it was a proper government function. It went to the people who knew what the technology was in these various weapons and satellite information areas, and then as the funding came in tho finally prosecute the programs, indeed NRL got a significant amount of funding. I was somewhat frustrated with the slow response of the Laboratory from a top management viewpoint, and I remember one time talking to the director, Tim Coffey about it, and he shrugged, and he said "so we miss the first billion." That's a quote. By contrast, Los Alamos and Livermore were setting up whole new organizations to support the SDIO. So, the amount of money coming into NRL from the SDIO grew over the years and peaked close to 80 million dollars sometime in the late '80's and then started to roll over and went down very significantly. So, it has to be said that the Director's approach for NRL was in fact the right approach. That if a separate organization had been set up, then it would have essentially atrophied after less than a decade because of the declining funding. It came and it went is one way to look at it.

Another general comment about the SDIO, it was the beginning of what some of us view as the "XXI syndrome." By that we mean "this initiative" or "that initiative". The SDIO; there was a C-D-I-O, Conventional Defense Initiative. I guess no "O." CDI. Conventional Defense Initiative. Then there was an ADI, Air Defense Initiative. What was basically happening is that the traditional organizations that funded R&D at the NRL: the DARPA, ARPA, DARPA-- the names go back and forth--on one hand and the ONR and the acquisition organizations like NAVSEA were essentially being undercut by the formation of these initiatives. Some of the money flowed into these initiatives. So, the number of sponsoring organizations for research at NRL increased significantly during the '80's, and with it, much greater overhead in hustling money. I mean we had to give far, far more proposals to far, far more people in order to get the funding that we needed. So, it was more complex and not entirely satisfactory.

Now, another general comment on the SDIO was that it did a ghastly job of documenting the results of the programs that it funded. NRL, of course, writes roughly a 1,000 scientific articles a year. It's very good documentation of the work that goes on at the NRL. You go to some organizations like what used to be DASA, D-A-S-A, and then DNA, and now DTRA, D-T-R-A. Those organizations have their contractors write reports, and they have a good documentation of the work that's done. By contrast, the SDIO never institutionalized a reporting system, and a great deal of information that was generated essentially didn't go beyond the viewgraph stage and got lost in the cracks over the years, and the amount of money that has been spent to date is probably of the order of, it's probably approaching fifty billion dollars in the SDIO. A few billion per year for roughly fifteen years. It's amazing; if one went back to find the results of all of that work it would be very, very difficult.

van Keuren: How did it affect your own particular branch?

Nagel: We were successful, both in the division, and in the branch, in capturing a significant amount of the SDIO money. I remember it as a period that was somewhat like the '60's in terms of the basic research money, the 6.1 money, in the Lab where there was plenty of it. In fact there was even a problem spending it, at times. There was a program that I succeeded in getting on Directed Energy Warfare which brought in sufficient money, and even though I shared it with two other divisions, the Plasma Physics Division and Space Sciences Division, there would be a few hundred thousand dollars of money that I could essentially use arbitrarily within a fiscal year. It was a good feeling that didn't last very long. After a few years they tightened things up and that latitude went away. But the SDIO was a significant sponsor in the Branch from the time I took over in '85 until I left the Branch level in 1990.

van Keuren: Were you doing a lot of new hiring based on it? Did you take on contractors? How did you manage all the research and funding?

Nagel: We basically grew a contractor work force. When I joined NRL, it had only one work force, namely government employees, and then in the '80's, especially the late '80's, we grew a second work force, the contract work force. And then [from the] late '80's on, the post-doc work force started to grow to be a significant component of the productivity of NRL, and then in the '90's, the student work force came up, and all of these were driven by a combination of two things, one is the rather incredible stability of the government work force, the low turnover rate in the face of a need to keep new people, new ideas coming in, on one hand and just simple economics on the other hand. The contractors were cheaper than government employees, the post-docs were cheaper than contractors, and the students were the cheapest of all.

van Keuren: Did you have a billet head that you had to work under such that you couldn't hire more government people, you really had to hire in the contractor realm, or was it a strategy that you wanted, besides the economics, that you wanted to hire people who could be laid-off if need be?

Nagel: It was both. We were limited in how many government positions we could hire. But the last point you mentioned, namely a flexible work force that could be laid-off if things went down, as well as the cost factor, were the two drivers.

van Keuren: I've talked to Giallorenzi in the past, and he's indicated that "that" was his strategy within the Optics Division: to hire a lot of contractors, so that if the money he considered reasonably soft disappeared, that it would be easy to lay them off.

Nagel: That's right. It was the same in our branch and our division.

van Keuren: How many contractors...How did you grow in total numbers of contractors per government employees during this period, would you guesstimate?

Nagel: Yeah, we probably reached a point where a quarter of the work force was contractors. Which is about reflective of the fraction of the funding that came from the SDIO.

van Keuren: And how many workers were there in the division at this point?

Nagel: Well, I don't remember the division. The division as a whole was probably150, but in our branch it was in the twenties.

van Keuren: And what type of research were you working on for SDIO?

Nagel: The work fell into two main categories. One was directed energy and that had a lot of facets. Early on, the SDIO considered using nuclear weapons as pumps for a wide variety of directed energy weapons. It's probable but some of that's still classified, so I won't go into any detail. But the general scenario was so called "first generation weapons" [which] were atomic bombs that went back to the '40's; the second generation nuclear weapons were H-bombs in which the primary was an atom bomb and the secondary, the hydrogen bomb, provided most of the energy; and the SDIO dealt with the so called "third generation nuclear weapons," where the H-bomb was a pump for a directed energy weapon, such as a laser.

van Keuren: How did that work?

Nagel: Physically?

van Keuren: Yeah, physically. When you say a pump what do you mean?

Nagel: I mean the H-bomb went off and provided energy that was used to excite a medium that would then emit radiation or material in a beam rather than out in all directions at once.

van Keuren: And the hard part of the technology was getting it to work in a beam? The directed energy part?

Nagel: Well, it was all new. I mean, lasers were well known. Beams of RF were well known. But they were pumped by various mechanisms at powers that were minuscule compared to what an H-bomb provides. So, it was a part or parameter space that has never been explored before. And of course the experiments could only be done underground in the Nevada test site, so the turnaround rate was relatively low. The cost of each underground experiment was in the range of fifty million dollars. So that they were expensive experiments, besides being infrequent and complex. So, the division as a whole worked on various aspects of the nuclear-pumped directed energy weapons. The group that I was part of, that I headed, the X-ray group, worked on nuclear pumped X-ray lasers. The second thing that we did was support another branch in the division, headed by Jim Ritter, that worried about the effects of nuclear radiations, both natural and from weapons, on micro-electronics in space. So there was a section within the branch headed by Dennis Brown and containing Charles Dozier among others who did radiation effects on electronics during that period. Dennis Brown later transferred in fact to Ritter's branch, and he's now on assignment to the National Reconnaissance Office.

van Keuren: So, those were the two main areas?

Nagel: Those were the two main areas.

van Keuren: What can you tell me, in an unclassified sense, of the results of your research?

Nagel: Our participation in the nuclear pumped X-ray laser program was advisory. The program was executed dominantly by the Lawrence Livermore National Laboratory, and the situation was quite analogous to the situation that was created in laser fusion, where Livermore and Los Alamos had the big programs and NRL and Rochester had a small piece of the action in order to keep these larger fellows honest. It was very much the same in this nuclear pumped X-ray laser program. Livermore did the design and the fielding of it, but we were closely enough involved in reviewing the program that we could provide guidance on diagnostics, as well as comments in critiquing the experiments.

van Keuren: As I remember my interviews with Steve Bodner, Los Alamos and Lawrence Livermore weren't particularly happy about receiving this sort of technical guidance? What about in your case?

Nagel: I didn't have a problem for two reasons; one is, in the 1960's, as we discussed earlier, our group had pioneered the high resolution measurements of X-rays for nuclear weapons, and that was still remembered and respected. And the other thing is that in the SDIO in the '80's there was plenty of money for everybody, so there wasn't as competitive situation as existed in the much older laser fusion field. And the other might be personal where some people are less combative than others.

van Keuren: Were you asked by DOE to take on this role or did you volunteer? What were the mechanics of your becoming involved? Or did it come through Bleach again?

Nagel: No. This was independent of the DOE. It was SDIO.

van Keuren: Ok.

Nagel: This went back to what I said a few minutes ago about they asked us what ought to be done programmatically, and then they said, "ah yes, we will fund some of this and let's see, who's the best people to do it? Oh, it's that group at NRL." So, we basically recommended this work and then raised our hand when the money actually became available and were hired to perform this function.

van Keuren: Okay. Was it an exciting period to do research? How would you classify it?

Nagel: It was exciting indeed because from the highest levels. First of all, the idea of trying to make a missile shield for the entire country that was highly effective was revolutionary and exciting in it's own right. Some few years after the SDIO got started, I spent a day listening to congressional staffers talk about the SDIO as one of these broadening exercises for government employees, and I remember very, very clearly one of the staffers saying that when the President gave his speech, Congress, which was as surprised as the rest of us about the possibility of a highly effective shield against intercontinental ballistic missiles, was trying to decide whether or not they wanted to do this and then, as I said later, whether or not the country could do it. So that what was done was the Congress polled the public and the first question was, “Are you satisfied with the defense against ballistic missiles that your country is providing you?”. And half the citizens said “yes.” A second question is, “you don’t have one; do you want one?” So, I’d say from a national level, it was exciting. From a nuclear weapons viewpoint it was exciting because it was another generation of nuclear weapons that had a whole new set of design challenges. Many people in the nuclear weapons design community were essentially bored. I remember Lowell Wood from Livermore complaining that he had to redesign hydrogen bombs to change the center of gravity by an inch so that some re-entry vehicle would fly better. This is not very exciting physics. It’s crank turning work that was, truth be told, dull to the weapons designers at Livermore and Los Alamos. So, they had a new sandbox to play in. The underground tests were far more complex than the tests involved in the devices, first and second generation devices, that put their energy out into all angles, because you had to measure the angular distribution as well as the time and spectrum, and so forth, depending. So, that was more complex. The diagnostics had evolved to a point where more components were available: crystals and detectors and things that could be brought together to do very sophisticated measurements. Streak cameras are devices that can provide sub-nanosecond time resolution of light and...

van Keuren: sub-nanosecond?

Nagel: Nanosecond, yeah. Pulses of light and X-rays and remember, before the SDIO, we essentially saved up to buy a fifty K streak camera for use in the laser fusion experiments. Well, in these nuclear weapons tests, they would put ten of them down a hole, and they would be vaporized in three milliseconds just as part of the cost of doing an experiment. So, it was, I’d say, exciting on a national level, exciting on a nuclear weapons design level, exciting on a complex experimental level, exciting on a component level.

van Keuren: How much of your time did the work for SDIO take up? You said about 40% of the work force was involved in SDIO ...

Nagel: About a quarter.

van Keuren: About a quarter.

Nagel: It was somewhat more for me because of the need to go to a lot of reviews in order to stay up on what was going in the program more generally and in the part that we were involved in. I had to give reports to the sponsors on a semi-annual basis and would go to the Nevada test site and look at what’s going on in the experiments and so forth. So it was probably in the range of a third to a half of my time.

van Keuren: And, in summary, what else were you doing at this point in time? What was your other research? I think we’ve covered some of this.

Nagel: Yes, the other thing was basically X-ray lithography. The sequence, as we’ve said before, went from learning how to measure X-rays from nuclear weapons to learning how to measure X-rays from lasers and tokamaks to recognizing that X-rays were in fact a uh... I mis-spoke that. Laser heated plasmas were a good source for X-rays. We got this patent, and we’re involved in X-ray lithography. So, in the ‘80's, I was an advisor to the DARPA X-ray lithography program and would participate in their reviews, go to their annual meetings, which fortunately were in the south in the winter. So, my dominant thing was the SDIO and secondary thing was the X-ray lithography work.

van Keuren: Okay. Shifting back to the SDIO work. What was your personal opinion of the science and the technical possibilities? Did you think the country could develop an effective anti-missile defense?

Nagel: Well, the short answer to your last question is, no. Okay. And the reason is very, very simple. When I first heard Reagan give this speech, I don't remember if it was 'live' or whether it was a recording, but I listened to the whole thing. I was flabbergasted because you can imagine a matrix that says "who's going to nuke the United States and how they're going to do it," and the "who's" of course first on the list was the Soviet Union, but you can imagine others and this changes over time. Terrorists and the other members of the nuclear club. You could worry about China, for instance. Eventually if you go down the road you can imagine that fifty years from now we'd be at odds with France. You know, "who" might nuke us is a fairly short but not invariant list. And "how they might do it" includes sending missiles through space, but you could also have a Cessna come in from Cuba, or a van from Canada or, my gosh, it's already in the hold of a ship tied up in Manhattan, okay. And the point is that the SDIO covered only one box in that matrix, namely the Soviet Union and an intercontinental ballistic missile. Now that matrix was only one of three matrices of the same ilk: the nuclear weapons one, the second being chemical weapons, and the third being biological weapons. I felt all during the Cold War that the possibility of having a Soviet agent live upstream of the Washington D.C. water supply with a bottle of anthrax was a far more credible threat than an intercontinental ballistic missile because it was essentially bulletproof. You'd have a light goes on in that residence, and the guy goes out and pours this bottle into the reservoir under MacArthur Boulevard and enough people get sick in Washington to cripple the Capital. So, anyway, I didn't feel that the SDIO, as bold as it was,

was anywhere near comprehensive even in the nuclear area, let alone in the other areas (A ), and (B), that if you focus just on that one box, namely, the delivery of nuclear weapons through space by the Soviet Union, it wasn't going to be effective anyway because they had the capability for such massive launches that even if you had leakers at the 1% level, there's a "Whoops, there goes Baltimore!" syndrome. So, on the highest level I thought it was overstated.

Now when you go down from that and look at some of the details, I'll go back to the directed energy warfare case, there were things being discussed that were enough to gag a technical maggot, such as nuclear pump shot gun, okay. And, you know, that disappeared as a programmatic concept very early on, but what I mean by that is nuclear pump hypervelocity accelerator. So, anyway, when you go down from the overall systems level to some of the details of the system, it was very bothersome, and that was true in the area that I knew something about, as well as in the areas that I wasn't a technical expert on, such as the communications problem and the surveillance problem, and the discrimination between decoys and real re-entry vehicles.

van Keuren: How much progress was made? Did it ever come anywhere near to achieving any degree of successful development?

Nagel: Well on the, you might say component level, that is small functions level or sub-system level, if you will, there was a spectacular amount of progress in adaptive mirrors and in all kinds of things: lasers, and the ability to pass information around, and simulation. For instance, the national test bed out at the Falcon Air Force Base, F-A-L-C-O-N, Air Force Base. So, if you take the view that to do a high level function, as such to defend the country against re-entry vehicles, you need various major functions, such as survey lens and early warning and missile response and weapons that can engage re-entry vehicles and decoys. You need those high level functions, and then you go down a layer from that. You need the pieces to do those, for instance in the case of radars, you need transmitters, receivers, information processing and communications, and so forth. Anyway, when you got down to the third and forth level there's dramatic progress, no question about it. But that does not mean that the second and third level functions that were necessary for a inviolable shield were enabled by the SDIO because these things are like a chain. If you were going to do a surveillance of essentially the entire world all the time, you need a system that has all the pieces and is reliable. In most cases, all of the pieces weren't put in place. van Keuren: There has in the last couple of years been renewed interest establishing an anti-missile defense mechanism. What do you think of that?

Nagel: Well, I have a split personality you might say, a split view of this. On one hand, it seems eminently reasonable for a country such as this to develop some kind of a missile defense capability for itself and for it's allies. So, the fact that the program continues and it may reach a decision point soon in itself doesn't bother me, but, on the other hand, I see continuing problems. The spectacular failures in some of the missile engagement experiments where a target is fired out at Kwajalein–how do you spell Kwajalein? K-W-A-J-E-L-A-I-N, I don't know--and engaged by a interceptor from Vandenberg Air Force Base. You know, those experiments have been very, very problematic. The same is true of the employment of the Navy standard missile in a missile defense mode. They've had a lot of problems in that program as well. So, you know, I think that the work should continue. It's very daunting technologically, but that we're really not adequately prepared for a decision in the next few years of whether or not to field the system, to actually build the system.

van Keuren: At the time and recently, the whole program had been criticized as being destabilizing, particularly in relations between the U.S. and Russia, and what used to be the Soviet Union. Did you have any personal opinion on this point? Do you have a personal opinion on this point?

Nagel: Yeah, almost. I mean, as a technologically oriented and reasonably informed citizen, I was aware of and somewhat concerned about two aspects of the SDIO that were often discussed. One is the destabilization business, that it was a mistake. It would introduce an asymmetry into the situation between the U.S. and now Russia that would be different than the symmetry that we had during the days of mutual assured destruction and that could be viewed as destabilizing. And the other, of course, has to do with the treaties and whether or not experimenting, let alone fielding, part or all of a missile defense capability would violate any of the treaties that were in place. So, you know, I was aware of these concerns but that was above my pay grade.

van Keuren: In 1985, you became division head. Could you tell me about your promotion and the new responsibilities it brought?

Nagel: Yes, the situation that existed that created the opportunity for me was that Tom Schriempf, it's spelled S-C-H-R-I-E-M-P-F, had been head of the division for a few years and he decided to leave NRL to go into industry. At that time, Al Schindler was the Associate Director of Research for Material Science. So he had to find a new division head. And it was advertised, as is always the case. I had been a branch head at that time for about five years, so the possibility of moving up appealed to me. I further had the good fortune of having worked on more or less an intimate basis with most of the other divisions in the branch, so it would be quite natural for me to move in a leadership position. So, I put my application in and saw Schindler at some point over in the lobby of the ONR building, at which point he told me I had been selected for that position. Now that had an overtone that was unlike my previous promotions in the past when I went from being a physicist to being a section head to being a branch head. I was part of the ordinary GS government service scale, but now I would become a member of the Senior Executive Service and have what the Director described as a corporate responsibility for the Laboratory, in addition to the responsibility for my piece of it. There were thirty, maybe at most forty members in the Senior Executive Service at the NRL, and they essentially were the people who ran the Laboratory, predominantly, of course, the half a dozen people in the top management, the director and the associate directors, but also the division heads, and they had some of the support services.

The division at that time had around a 140 people and the budget was around twenty million a year. Now in the late '80's, the SDIO fraction of that grew to be roughly one third, and it ramped up rapidly, especially because the largest branch in the division, the Radiation Effects Branch, headed by Jim Ritter, was very successful at attracting SDIO money. And then, as the program matured and the funding at NRL went down, the amount of money in our division from the SDIO went down about a million per year for about five to seven years to where we were left in the early '90's with roughly one or two million dollars worth of work. Now, despite that, we were able to attract other programs and keep the overall division funding at a level necessary to take care of the government employees and the contractors. The five branches in the division were the one that I previously headed, the Surface Modification Branch, under Fred Schmidt, S-C-H-M-I-D-T, the Radiation Effects Branch under Jim Ritter, already mentioned, the Directed Energy Effects Branch under Joe Aviles, A-V-I-L-E-S , and the Condensed Matter Theory Branch under Barry Klein K-L-E-I-N.

The division historically derived from parts of the old Solid State Physics Division and the old Radiation Technology Division. So, the underpinning areas of physics were solid state physics and nuclear physics. It was probably the most diverse division at the NRL and was quite suited to both my experience with the branches in the division and my personal bent to being interested in the broad range of science and technology. Soon after I took over, the director asked me to zero sum the basic research of the 6.1 program. To give them back to the RAC with essentially a clean sheet on how we would manage what was then roughly seven million dollars or about a third of the division's funding.

van Keuren: So, approximately about a third of your funding was basic research monies?

Nagel: Yes. Let me pause for a second.

So, I reviewed the entire program and went back and made a presentation to the RAC that was deemed to be unsatisfactory, and I was told to go do it again.

van Keuren: This was when, approximately?

Nagel: Oh, 1986. Soon after I took over. And I never got what I considered satisfactory feedback on what was wrong with it. But, anyway, I went back and did it again, changed it. It seemed to me not a great deal but enough to be satisfactory, and the director and the RAC continued to give us the 6.1 funding that we had been getting under revised programs. But the interesting thing is that if you back off, I inherited five branches when I took over, and all five of those branches were in existence when I left, okay. The basic research program was not a revolutionary change but, in my mind, an evolutionary change. And as the '80's wore on and especially in the '90's, the need to roll over the basic research program on a ten year time scale sort of moved on to an eight year time scale, so the dynamics sort of picked up and even more importantly in the '90's, the viewpoint was that the 6.2 program had to roll over on a three or four year time scale, rather than a much longer time scale than they had previously.

The 6.2 program was under the most duress in terms of the amount of money coming into NRL, certainly in the '90's. So, my trump card always was the scientific productivity of the division. We got roughly five or six percent of the 6.1 money in the Laboratory, but we produced twelve to fifteen percent of the publications from the entire Laboratory, as a division. Now there were a couple of reasons for that. One is there was a very good culture of research in four of the five branches, okay,(A). And (B), the theoretical...

van Keuren: What is the exception? What was the other branch that was the exception?

Nagel: The Directed Energy Branch was heavily involved in a lot of testing. For instance, large laser experiments at White Sands and other places. And ship-board testing, so they were not as prone to writing scientific papers. It wasn't that there was anything wrong with them. They were just doing a different kind of work, more focused work. Another reason for the productivity was the Condensed Matter Physics Branch, first under Barry Klein later under Dimitri Papaconstantopoulos, was extraordinarily productive. I've worked with him for thirty years. In fact, as an aside, it's Dimitri Alexis Papaconstantopoulos and his wife Gayle. So that branch was very productive. Now if you back off and look at my entire tenure as a division head, all five branch heads needed replacement during the thirteen years that I was Division Head. I don't remember the order in which they left but, Joe Aviles retired, and I replaced him with Terry Wieting, W-I-E-T-I-N-G. Fred Schmidt retired, and Graham Hubler, H-U-B-L-E-R, took his place. I already mentioned that Dimitri Papaconstantopoulos replaced Barry Klein. When I moved up, then there was a vacant branch head job. I hired Mike Bell, B-E-L-L, from NIST. And finally, Jim Ritter retired last and was replaced with Art Campbell, C-A-M-P-B-E-L-L. Are you near the end?

**Side Two**

Nagel: To finish talking about branch head replacements, I view that as the single most important thing that I did during the time that I was the division head. The selection of leaders, especially at the branch level, is utterly pivotal to the Laboratory, and I was quite careful and, from my perspective, quite successful in each case. I mean, two of them came from within the branches that they... No, three of them, I'm sorry, came from within the branches they headed. Hubler, Papa.., Campbell? Okay. Four of the branch head selections came from within branches they headed and one was hired not only from outside the branch, but outside the Laboratory. Some were entirely competitive because multiple people internally and externally were interested in the jobs. Others were almost done by default because there were obvious candidates in place, for instance in the case of Dimitri Papaconstantopoulos.

Going back to the early times as the division head, I should have mentioned, when I took over I said there were five branches; there were six. Don Gubser's branch was in the division at that time but, after about a year, Gubser became head of the Materials Science & Technology Division, and he wanted to take his branch with him and did in fact. So, the division lost a very, very good branch, with which we continued to work closely, but in fact we did go from six branches to five branches somewhere in the 1986 time frame. Now, I was given latitude at that time to grow back up to make up for the loss of those couple of dozen-plus people, but that was the time frame when the SDIO funding was going down, and it was hard enough to replace it without being able to grow at the same time. So, we were never successful in growing back to the size that we were initially. In fact, if you take the very long range view over the entire time that I was division head, the division shrunk from around a 150 people to somewhat below a hundred people, and there were two causes for that: the loss of the branch, initial decrement, and then the gradual decline that we, along with others in the Laboratory, suffered during the '90's as the budgets shrunk.

Now the reason Gubser became head of the Material Science Division was because that position was vacated when Bhatka Rath became the Associate Director for Material and Component Technology. So soon after I became Division Head in 1985, that associate director job opened up, and I had applied for that and was given an interview. I remember one of the things that the Director had told me at that time was that were I to take that job, I would have to work the problem of bringing Tom Giallorenzi's Optical Sciences Division back more into a research mode rather than a product-oriented mode. The director viewed the Laboratory as having a few product-oriented branches, a few divisions, I mis-spoke, a few product-oriented divisions. Among them, the Space Craft Technology Center, the Tactical Electronic Warfare Center, and also, less to his pleasure, the Optical Sciences Division. It still amuses me to contemplate the prospect of me influencing Tom Giallorenzi to become less systems and engineering-oriented and more research-oriented. He's been very successful, of course, as a member of the National Academy of Engineering, and that was a job that I was glad never to have.

I note also in passing, that when Tim Coffey became director of the Laboratory, it was said that Giallorenzi was the runner-up, and for that reason, as well as his great success with fiber-optic sensors and other major military programs, his trajectory within the Lab, while it has not treated basic research as a priority, has been very, very successful.

The position that Schindler vacated was filled by Rath, as I said, and I found out about the director's decision in the lobby of the National Academy of Sciences when Coffey said to me that he wanted to quote "strengthen the Material Science Program at NRL." I did not take the opportunity to remind him that my Ph.D. was in material science.

So, my early days as division head involved this dual loop [?] at times on the revision, renovation of the 6.1 program, a failed attempt to become an associate director, the declining SDIO money, and still some personal involvement in the x-ray lithography program.

van Keuren: And the basic research money was going towards which research topics again, basically?

Nagel: Well, let me go through each one of the branches. The branches could be arrayed according to whether or not they were skewed toward the Condensed Matter or the Radiation Sciences part of the Division. Now, the Condensed Matter Physics Branch was almost entirely, guess what, condensed matter. And that branch was funded almost exclusively on 6.1 money. So their job was to do what used to be called "solid state physics," or solid state theory and computational physics," which they did with international impact. Now there were then two experimental branches that were heavily involved in condensed matter physics, but more involved in radiation physics, and they were the Surface Modification Branch and the old X-Ray Optics Branch. I'm going to have to look up the right name for that branch. I'm drawing a blank right now. But, anyway, the Surface Modification Branch, headed first by Fred Schmidt and later by Graham Hubler, made and measured thin films by a variety of techniques that grew from the ion implantation that had been practiced during the '70's and '80's to ion beam activated deposition and then pulse laser deposition. They kept adding to their arsenal of techniques for making thin films and for measuring them by a variety of spectroscopic, among other means. The old X-Ray Optics Branch under Mike Bell did experimental solid state physics primarily using synchrotron radiation as a tool. Let me stop. Did we talk about synchrotron radiation yet?; I think we did?

van Keuren: Yeah, we did.

Nagel: So, it had started in the '80's through the efforts of Earl Skelton and Milt Kabler and myself. We had beam lines at Brookhaven, and the branch under Mike Bell used those beam lines heavily and highly successfully. Then the final branch, the Radiation Effects Branch under Jim Ritter, did dominantly radiation effects work and relatively little ordinary condensed matter physics work. That branch, when I took over, was viewed as a job shop, a 6.2 job shop, that is an applied research organization. I was basically told to move it back into more of a basic research realm, and that was accomplished in cooperation with Jim Ritter by a combination of things: first hiring some very, very good people, first among them Jeff Summers, S-U-M-M-E-R-S, who came from Oklahoma State to head a section and was a very good physicist and researcher, and hiring of other good young people into that section and others. The second reason that the branch began to publish very good basic research papers was because Ritter had enough money in the branch that he could use some of it for publishable research, as well as programmatically relevant things. His branch was the dominant branch in the division, the one that was most successful at attracting SDIO money, money from the National Reconnaissance Office, and other places. The last branch, headed first by Joe Aviles, later by Terry Wieting, the Directed Energy Effects Branch, was of a split character, and it did roughly half radiation effects work, laser beam and microwave effects research on one hand, but it also had a very strong component in experimental solid state physics using optical probes. It had the least of the 6.1 money in the division, and as I already noted, was the least productive of research papers, but they did very high quality work. I'm going top make a parenthetical note here to change the order of those branches. I want to move the comments on Wieting's branch ahead of the comments on Ritter's branch. Okay, end of comment.

The other macroscopic, I mean, large scale thing to happen during my time as a division head was the hiring of a relatively large number of very good junior people, in the late '80's and early'90's, dominantly in Ritter's branch and in Schmidt's branch. A couple of dozen really outstanding young people joined the division and started programs in femto-second lasers and...

van Keuren: Femto-second?

Nagel: Femto-second, F-E-M-T-O, femto-second lasers, in pulse laser deposition, and other areas that have turned out to be wonderfully successful. So, when it comes to people, I succeeded in what I had to do, namely hiring good branch heads, and we together succeeded in attracting to the NRL and adequately supporting a cadre of young people some of whom will be branch heads and maybe division heads in the future. So, that was a very gratifying overall experience.

Programmatically, as we've said already, we were succeeding at great effort in replacing the declining SDIO funding and in fiscal '94, very late in the year, probably in the last third of the year, we were projecting solvency and two major programs were suddenly cancelled, one by DNA. For some political reason they needed, the Director needed money. They pulled the money back, and we went from a situation of expected solvency to being a million dollars short in a matter of weeks, and we were unable to go out and find another million dollars to make up for that shortfall. That was the low point of my time as a division head, because I expected that the insolvent division would be disbanded by the director, and I was planning with the branches where they might go, and I was ready as such to be "canned." It was a time of tremendous personal stress for me. I'd wake up at night, and my mind would seize on the problems, though I couldn't solve them at night. It caused me a lot of mental and physical strain. I was very put off by the response of some of the people to the situation in two ways. One is--to jump ahead--what happened was the director essentially filled in the gap with money from the general overhead at NRL, and we continued on to the next year as if nothing happened. It would have certainly been helpful to me if I had known in advance that that was an available degree of freedom. I found later that a few divisions are short from year to year, and that's how the Director uses some of the overhead in order to smooth things out, just as I would transfer money and people across branches to take care of difficulties from year to year. But, I didn't know that, and I could have saved, I could have avoided a tremendous amount of anxiety if I knew that was a possibility, okay.

The other thing was the Director of Business Operations, Bob Doak, had revised the overhead when he came to the NRL in the early '90's, I believe. Late '80's, early '90's, and it penalized divisions that had large facilities such as ours: the accelerators, and the laser laboratories, and so forth. He and I never agreed on how additional hit we took in increased overhead due to the change in the overhead system. But, at any rate, I remember being in Coolfont, at one of the every three-year retreats that the director had for SES'ers at NRL, in Virginia. Anyway, at the time, I was having difficulties with the funding situation, and I was talking casually to Bob Doak and Dave Whittington about that one time and said I would wake up at night worrying about it and, in a very crude fashion, they expressed to me that I ought to be doing other things at night. It was entirely insensitive to me, as well as crude.

So, anyway, when the dust settled, we weathered that storm in fiscal '94 and were able to keep ahead of it. Now the situation that existed through most of the '90's was that we would enter a fiscal year with a workforce that would require a certain amount of money, and then we would work vigorously, month by month through the fiscal year to bring that amount of money in and at some point in the fiscal year, we would arrive at a situation where the amount of money that we had in hand would equal the amount of money that we needed. And in some fiscal years, that wasn't until the twelfth month of the fiscal year. A good year, would be, we would have enough money in hand in the eighth month or at least the promise of it. And, I instituted a very demanding monthly reporting system that would require the branch heads to report in detail what their funding situation was and what the protections were, and then for me to integrate it over the division level and report to the associate director, my boss, about where we stood. It was not a lot of fun. There was a tremendous amount of tension associated with the fact that over half the time we were in a situation where our promised funding, our likely funding, was insufficient to meet what we needed to make it through the year. So, basically what we were doing at the middle management level, the division level, and the branch level was risk-based management as a way of life and it was very stressful for all of us.

Another global comment that I should make is that when I became division head, the division office was headed by a retired Navy Commander named Don France, who basically kept books by having pieces of paper around, no uniformed file system, let alone computerized system. He and I got cross threaded early on because of two things. One is the poor job he did in the management of the division office and also his dealings with a woman who worked in that office, upon whom he showered undo attention. And I remember clearly having a visitor from Canada one day, Friday, I could probably think of the date if I worked at it, but anyway, France bursts into my office, and I had to ask the visitor to leave, and I closed the door, and he "blew up" because of my talking directly to the people and the women worked in his office, rather than going through him. So, I basically listened to him, and then he went off, and the visitor came in and I dealt with him with apologies. First a sit down meeting and then some tours. Well, later that morning I was summoned by the EEO Officer and by ... No, I'm sorry. Later that morning I discovered that one of the women had developed an emergency medical problem in response to the stress that was generated by France, and she'd collapsed, and was being cared for, and that became known management of the Laboratory. So, early that afternoon I was summoned by the Equal Opportunity Employment Officer and the Women's Council Head and the Head of Human Resources, and they essentially grilled me for an hour to find out if I was part of the cause of the problem, in other words, if I was responsible for France's behavior. My reputation at the Lab was known to them and so was France's, and they satisfied themselves after about an hour that I was actually part of the solution and not part of the problem. And it was taken to the Captain, and it was decided that Mr. France would be put on "administrative leave" effective immediately and that his badge would be withdrawn so that he wouldn't come into the office and trash the office or the computer system during the weekend. All of this is going on while I had this visitor from Canada. So, anyway, in the end, France resigned over the weekend and disappeared from the scene, and I was able to hire from Building 43, a woman named Barbara Murphy, to head the office, who managed the women of the office with a deft hand for the entire time that I was there and who set up a computerized accounting system and just made things right. She's one of the most competent administrative people I've dealt with at the NRL. So, my reputation within the division due to the misadventure with France changed significantly. I was perceived, viewed from being a, you know, relatively normal NRL scientific type who blundered into a management job into somebody who could effectively "axe" a person. But it wasn't that, he was self-destructive and essentially took care of himself.

There was another thread that ran through the entire time that I was division head, and that started with the case with Don France, and that was personnel difficulties. It seemed as though that there were chronically one or two people involved in EEO complaints or bringing some kind of a suit against the Laboratory. And I was very significantly burdened by a long sequence of problems involving maybe six or eight different people over the years, most of whom either voluntarily left or self destructed. A particular technician who was one of the worst apples at NRL, was known to be a problem for a better part of two decades. Finally made a mistake of pushing a fellow worker and causing her to fall over. I was at a meeting in Hawaii at the time. I remember very clearly having to deal with that by remote control, and it was a very simple situation. We offered the person the opportunity to resign in exchange for not bringing assault charges. I could go on to more detail but, for all of the good people at NRL, and there are many and they are wonderful, both professionally and personally, and I enjoyed working with many of them for times ranging up to three decades, exceeding three decades, there are inevitably a few bad apples, and being a division head at NRL requires that one deals with that.

van Keuren: Any more general comments about the division at this period?

Nagel: Let me do an inventory; I've talked about people, the most important thing, at the branch head level, and at the good young people level, and at the bad apples level. Talked about programs, okay. Facilities is the other thing that you need to do research. You need people, you need money, and you need toys to play with. The facilities business, because of the character of this division, having come out of the nuclear physics world with these large machines, was a major challenge for me. The very first thing that happened is that the old cyclotron in the building on the south of the Bolling Air Force Base, which belonged to Jim Ritter's Radiation Physics Branch, was shut down soon after I came in. So, I was involved in that although Ritter carried the ball there. The next big facility was the Van de Graff in Building 74, in Fred Schmidt's branch, and the old machine, the single-ended 5 MeV machine, was dying. So, in the late '80's we replaced it with a commercial tandem Van de Graff. That was an effort led by Al Knudson, K-N-U-D-S-O-N, with whom I collaborated in the early '70's. So, that system was put in place. To continue with that, several years later some of the people in the division recognized that the new tandem Van de Graff might be the heart of a accelerator mass spectrometer facility, so we went to the director and proposed that upgrade, and he gave us 1.2 million dollars of his discretionary equipment funding. A team of about six people has been working for about six years now to build a one of a kind in the world facility called "TEAMS', T-E-A-M-S. It stands for TRACE ELEMENT ACCELERATOR MASS SPECTROMETER, and it's going to be a very, very important facility for the NRL, nationally and internationally, for many years to come. Then moving from that branch to Bell's branch, the continued maintenance and use of the synchrotron radiation facilities was a major effort. We failed, as I noted already, to go from the facility at Brookhaven to the new facility at Argonne National Laboratory for a variety of reasons, but we still maintained a strong hand in synchrotron radiation. In Weiting's branch, a major, high-powered microwave test facility was built in Building 2 and remains very important. So, there was a major facility thrust in that branch some time in the late 80's.

Okay, then finally, in the Condensed Matter Theory Branch, a group without experimental facilities, there was a chronic need for access to computational facilities, so the acquisition of computers and the getting of access to the super-computers at the national sites was a thing that I was involved with constantly. Now, the actual facilities, of course, go in buildings, and the NRL, being what it is, namely a collection of buildings from three eras, a few from pre-World War II, many from the World War II era, and then a few from after. We were housed dominantly in the World War II vintage buildings that were aging and due for renovation. Of course, the Laboratory, the director has a large skill renovation plan that included redoing, for instance, one of the three main building that we were in, Building 75. So, at some point, we had to move everybody out of the four floors of Building 75 and go through a renovation that took the better part of two years, which was capped by a Sunday morning leak on the fourth floor that flooded the building, the carpets, the walls, and everything, and grew mold, and moss, all week. It was just a terrible situation. So, anyway, the renovation of buildings was a chronic chore and problem. It was just part of the job through most of the time that I was there. So, anyway, peoples, programs, facilities, and buildings essentially were part of my job, continually.

Addendum to go after my comments on Barbara Murphy. The division office had a staff of four women who were among the most pleasant professional individuals that I've worked with at NRL. The head of the office was Barbara Murphy, and she had working directly for her two women, Ellen Cox and Edna Gloaden. Ellen took care of equipment and procurement, charge cards and the like, and Edna took care of the financial and personnel side. And then, in addition to those people in the division office by itself, there was a division secretary named Colleen Carlson, C-A-R-L-S-O-N, who was both the secretary to the division head, as well as the mother hen for the rest of the division. The branch secretaries were by and large highly capable people. In almost all of these cases, there was a high level of intelligence, and had they happened to go to school longer and worked in a time of more equal opportunity, they could just as well been researchers at NRL. Remarkable people on whom I was totally dependent and very appreciative of all their skills and services.

van Keuren: Between 1992 and 1994, you were involved in the issue of arctic pollution, especially in nuclear pollution. Can you tell me how you became involved and what your involvement here was?

Nagel: It's very clear and well defined. I would get a stack of mail every week that was somewhere between a foot and two feet thick. It fluctuated, but lots of journals and magazines and the like. Well, buried in that flow of stuff one time, was a single page from the congressional record, noting that there would be a ten million dollar program to address the question of pollution by the former Soviet Union, of the Antarctic waters. It was sent to me by the director, no note or anything, just torn out, and my code number written on it. The existence of that program, a nuclear program in a division that had its roots in nuclear science and a pollution program in a division that was already doing a significant amount of pollution-oriented work, got my immediate attention. If you recall, this was at the time when the SDIO funding had gone down and were looking for new sources of money. So, I sprung into action immediately and learned that there was conference scheduled for only a month later in the Russian city Arkhangelsk, that's A-R-K-H-A-N-G--E-L-S-K. So, from a standing stop, and this only a few years after the end of the Cold War, we went through numerous bureaucratic hoops in order to get permission to attend this conference, and three of us were able to do that. They were, beside myself, Peter Vogt and Kathy Crane. So, I flew to Moscow for the first time and took an Aeroflot flight an hour north to Arkhangelsk, a very memorable flight because it rained indoors the entire flight. Condensation from the overhead of the airplane dripped from the lights down into the aisle during the flight. There were other aspects of the flight that were also memorable, like the boarding process. We went out on the tarmac and all crowded around the end of the latter that led up on to the airplane. It was very pushy and disordered. So, Peter and Kathy and I went to this remarkable and somewhat remote city, still in winter, it was October, snow was on the ground, and attended an extremely interesting conference.

We got to know a wide spectrum of international scientists and activists, including a wild-haired Norwegian from their equivalent of Greenpeace. But, anyway, we met the fellow, and in fact had breakfast with the Atomflot, A-T-O-M-F-L-O-T engineer, who revealed this dumping of nuclear waste to the world in 1991, which was directly responsible for the conference and the impending program. Basically, the former Soviet Union in the years from roughly 1965 to '85 had trashed over 10,000 containers of low-level nuclear waste into the Arctic Seas and sixteen nuclear reactors, six of them with fuel, were dumped in the Kara Sea, K-A-R-A Sea, in water that was as shallow as a 100 meters. Now, this particular body of water is to the east of the island of Novaya, N-O-V-A-Y-A , Zemlya, ZEMLYA, which is an extension of the Ural mountain range to the north, that is the mountain range that divides European Russia from Asiatic Russia. Novaya Zemlya was also the northern test site, nuclear test site, of the Soviet Union, and while at the conference at Arkhangelsk, we saw a movie of a Soviet weapons test where the nuclear device was so large, it would not fit inside of the airplane, it was strapped underneath. It was, as I recall, a sixty megaton device that was airdropped over that island during that time. We also were made well aware of the displacement of the natives from that land. There was a very small, not even five foot tall, very old, maybe eighty-year old woman, who was brought in front of the conference, and she spoke in her native tongue to a Russian person, who then translated into English. So, it went through three languages to get to be intelligible to us. And she was making the case for the return of the native peoples to Novaya Zemlya.

Anyway, the simple problem, simple in being stated clearly, is that the Alaskan fishing industry, which at that time was like three billion dollars a year, is separated from all of this nuclear pollution at levels that exceeded what Chernobyl put into the atmosphere, by water. So, the question was "would that radiation migrate and either actually threaten the viability of the Russian fish, or least cause people to be so worried that it was a perception problem?" So, the Alaskan senators had money earmarked in the Navy budget for addressing that question that first time in Fiscal '92 and then another ten million in each of Fiscal years '93 and '94. So, with the experience of the conference under our belts, Vogt and I, especially, gave talk after talk after talk around Washington to a wide variety of organizations, outlining the problem and what we felt ought to be done about it. Well, a program was put in place through the Office of Naval Research to use this thirty million to address the questions, and in the end, NRL got seven million out of that in a multi-division program that I managed.

Our program had four major parts, two of them were in computers. One was super-computer calculations of ocean circulation to see if the radiation were ex-soluble and in contact with the water column would be carried without enough dilution to a place where it might do damage. The second computer-oriented thing was a database and atlas study that Kathy Crane led. Then there were two experimental components to the program: one was to go to sea on a variety of ships and take samples and analyze the samples with the backgrounds; then finally to collect samples in Russia to make measurements about the terrestrial sources of radiation in the Kara Sea. There was other instrumentation development work. For instance, one of the people in the division made a radiation-sensitive monitor that can be sunk to the sea floor and sit and record data over a year and then would bob to the surface and radio its data out through a satellite annually when the ice was open, when the ice went away. But as far as I know, that's never been put to sea.

So, we had a very dynamic, very interesting program. For me, it was a totally new set of everything, science and technology, people, and mores, and I found it very refreshing to be involved in that. I learned a lot because it was so new to me. I took considerable pleasure in being able to jump into something sort of from the cold and be able to function at a reasonable level. During the course of the program, I went to Russia three times and to Norway twice to conferences and to do various things. For instance, one trip to Russia was to visit an oceanographic institute and arrange for the renting of a Russian ship to prosecute the seagoing program. We had some major problems along the way, ranging from stupid things, like tension between ONR and NRL, not for the first time, to very serious difficulty when we mounted equipment to put to sea, loaded it on a ship and actually got ready to go a full expedition, only to have it cancelled because a Russian admiral wouldn't give permission to go to the site where we needed to make the measurements. They were concerned that it was an intelligence mission rather than a scientific mission. So, that was an extremely expensive and demoralizing misadventure during the course of this program. Involved in it were some fairly senior people in the State Department, as well as in the DOD. It really was a mess. As an aside, you know that story, right?

van Keuren: I've heard about that.

Nagel: Yeah. I could go into more detail, but we'll let it go at that.

van Keuren: Well, if you think it's important for the historical record.

Nagel: Well, the seagoing work north and east of Norway was set against the background of mutual Cold War intelligence gathering by the Soviet Union on one side and by the U.S. on the other. So, that this concern on the part of the senior Soviet people only less than five years after the ... senior Russian people, less than five years after the dissolution of the Soviet Union was a matter of habituation. That they were conditioned to be suspicious of what was going to be done. I'll let it go at that.

Another new experience in this for me was writing a report for Congress. I had given congressional testimony once on the synchrotron radiation, but I had never actually prepared a formal report to Congress. So, in the first year of the program, when much of the money went to NRL because we were ready to start immediately, unlike a rationally chosen group of contractors, it fell to me to prepare this report, which I did. It turned out that even at that point, only about a third of the way into the program, we basically knew the answer already. In the end, the bottom line was that the dumped radiation is not a threat to the Alaskan fishing industry. It doesn't mean it's entirely benign, because the radiation levels in certain localities are very high, but most of the radiation is in containers. The reactors with very high levels of radioactivity were filled with a plastic substance, like a plastic cement, in order to retain the materials, and then some of the materials, even if they were just thrown into the ocean, are insoluble, like the oxides of thorium, are akin to gravel. They're very insoluble and probably are not going to be carried by these slow currents. So, even if they're in the environment, they'll squat in the environment. For instance, there was a B-52 crash near Thule, Greenland. The plane had some nuclear weapons on it, and the radiation that was strewn around that environment was of significant levels, but it is not going to move around much. By the way, I said thorium; it's plutonium, plutonium, not thorium. So, the first line of defense is that the materials would be contained. The second line is, even if they aren't contained, they may not move because they're insoluble, and the third line of defense is, even those things that are soluble would get diluted to a level that was sufficient to bring the levels due to dumped radioactivity down into the noise that the already-present radioactivity that is set by a number of sources, including natural radioactivity, such as potassium 40, fall-out from the 1950's atmospheric testing, and discharges of the giant Russian rivers, the Ob,  the Yenisey, Y-E-N-I-S-E-Y, and ...

van Keuren:  What was the last one?

Nagel:  Yenisey, Y-E-N-I-S-E-Y, or something like that. I'd have to look it up. And the Lena rivers. Each of these rivers is on the scale of the Mississippi, but unlike the Mississippi, which was essentially...

**Tape 2**

Side 1

Nagel:  So, unlike the Mississippi River that flows more or less uniformly the year around, these giant Russian rivers that have a net annual flow similar to the Mississippi freeze up during the winter, and then during the spring thaw they discharge north into the Arctic Seas a tremendous amount of fresh water which is, of course, less dense than salt water so that the fresh water flows over the top of the salt water in the Arctic Seas and can be found, the fresh water, as much as a hundred miles to sea. Now located in the tributaries of some of these big rivers are nuclear reprocessing plants: the plants that take spent fuel rods from nuclear reactors and reprocess them into something that can be used again, as fuel or nuclear weapons material. These plants would discharge a tremendous amount of radioactivity into the rivers that would find it's way ultimately into the Arctic Seas. Now, the Russians weren't the only ones that did this. The French have a fuel reprocessing plant in La Hague, and the English have one in Sheffield that dumped a lot of radiation into the sea that migrates up along the west coast of Norway and eventually reaches the Arctic Oceans, as well. But, those plants are located essentially on the sea shore rather than upstream on a long river. So, the Russian reprocessing plant would put nuclear radiation into relatively small rivers that would pass through villages. And, there is an infamous river called the Techa, T-E-C-H-A, River, and along its shores, the people are in their fourth generation of birth defects due to the high levels of radiation. One can go into an elementary school in that region and ask all the children with no left arms to line up in one side of the room and all the children with no right arms to line up on the other side because the birth defects were so prevalent. So, what I'm saying is that in order to understand the radiation levels due to the dumped nuclear waste, we had to sort out radiation from natural sources, from atmospheric fallout, from both Western and Soviet Union and then Russian reprocessing plants, and we could do that by isotopic identification, after measurement of the radiation from these materials. So, the expeditions that we sent into the Arctic Seas on ships and also on land within Russia to the head waters of some of these rivers would take samples and make radioactive spectral measurements and be able to say what the material was and therefore identify the source.

So, you know, one would not want to throw all of this radioactivity into the environment for scientific purposes, but given that it's out there already, it turns out to be extremely useful in terms of tracing the motion of materials in the environment. A famous case came to light during this program, which still sticks out in my mind. The U.S., of course has many polluted sites due to the Cold War and the arms race, as well as nuclear power generation, and one of the worst is at Hanford in the state of Washington. H-A-N-FORD. There are certain elements of isotopes that are just simply not in the environment under ordinary circumstances, and there's an isotope of Europium that's one of them. So, it was generated in Hanford, went through the ground water into the Columbia River, down the Columbia river into the Pacific Ocean, and down into a subterranean canyon. Then people went out and took samples of the sediments down there and found this isotope, europium. So, a remarkable ability to track radiation. In another example, some of this radiation from the European reprocessing plants that goes up along the coast of Norway then basically enters the current that flows west and then comes south along the coast of Greenland. So, the motion of some of these radioactive elements can be traced over time scales that exceed a decade in these slow-moving cases. So, there was a lot science as well as politics involved in this program. In the end, we arrived at the conclusion that Alaska would not be threatened and had high confidence in that conclusion. It was for me, professionally, a hick-up, a diversion, a bump in the road like a "busman's holiday," but it was extremely refreshing scientifically and otherwise, and the travel was truly remarkable.

The changes in Russia over those three years were appalling. The first time I went there in '92 it was sort of stable. Things weren't good, but they were good compared to what they would be. When I went back the next year, the third year, it was evidently much, much worse. It was very sad when one, for instance, approached the Moscow subway station to find a woman standing on every other step down into the station, in most cases sixty, seventy years old, selling shoe laces or something. A person could come to this station with no clothes and be fully dressed by the time they got down to the bottom by buying things off these so called "babushkas," these old women trying to eke out a living.

There were many extremely memorable things during these travels. While in our Arkangelsk we went to a park that had examples of wooden architecture from the Northern regions of Russia, stave churches and other things, that were just wonderful. We visited the home of the parents of a young scientist that we met when we were in St. Petersburg, and it was at that time in the late winter. The fellow brought out an acorn squash that he had grown this summer before this last one. It was all orange by that time, wrapped in newspaper carefully to be enjoyed at some future meal. Later on I sent a wide selection of seeds back to that fellow. I'm a gardener also. He might have been the only guy in St. Petersburg growing silver-queen corn that year. In a memorable meeting in a ski resort in Norway near Oslo, called Hollmenkolen, H-O-L-L-M-E-N-K-O-L-E-N, we ate like kings during the conference. The chef of the hotel had recently won a major international award. When the conference ended on Friday noon, I was due to fly out on Saturday morning, so I went to the ordinary restaurant and found it to be fully booked with a party. So, they were not open to the public. So, I went to the good restaurant there, and the individual parts of the menu were like fifty dollars for the entre, thirty dollars for a salad, and so forth. So, I went back to my room and had two bags of peanuts for dinner that night. Just bizarre. I could tell a lot more stories. Suffice it to say that was a very pleasant and very unusual, but very successful program.

van Keuren: That was between 1992 and 1994?

Nagel: Yes.

van Keuren: It went on for three years?

Nagel: Yes. It was funded for three years, and we essentially laid it to bed. Some of the work didn't get actually finished. For instance, the atlas that Kathy Crane and her colleagues put together wasn't totally finished until five years later.

van Keuren: And you said approximately seven million dollars out of thirty came to the Lab during that.

Nagel: That's right. No. For the three years.

van Keuren: For the three years.

Nagel: So, ten million a year for three years was thirty, and we got seven million.

van Keuren: Okay. And the science?

Nagel: The science was entirely satisfactory. The atlas by itself was a tour de force of enlightened work. I have a tremendous respect for Kathy Crane because she not only had a very clear idea of what would be useful but had a tremendous number of contacts around the world and was able to pull information out of Russian institutes that wouldn't otherwise have been available and integrated it into a geographical information system that put it in presentable fashion. And then to integrate that with good text and that with a wide variety of related pictures made a very, very nice document. So, it was an unusual scientific publication, but it was of incredible quality.

Then there was lots of ordinary papers that came out of it. Journal papers. I find myself still attracted by the science there. For instance, the modeling of ocean circulation at the crudest level can be done on what are called box models, where you essentially make a box for a sea like the North Sea, then a box for the related seas, and you connect them, and material flows in and out, and energy flows in and out, and you can have a very simplistic program at that course level all the way up to the very fine scale that is tightly gridded super-computer circulation codes that are run by the professionals, some of who were involved in the program.

van Keuren: You've already mentioned the suspicions of the Russian military towards your expeditions. What was it like on the American side? Here you were, American scientists at a naval laboratory kind of doing cooperative work with the Russians. What was the response within the U.S. military, within the U.S. Pentagon?

Nagel: Well it was officious, but effective I would say. Officious because we had a heck of a time getting the approvals necessary to go, for instance, to that initial conference. Multiple reviews, lots of sign off. I was hand-carrying papers around town and cajoling people but, it worked. The Laboratory already had a tremendous history of working with the Norwegians at all kinds of unclassified and classified levels. People like Vogt himself, Hank Fleming, excuse me, Hurdle had over the years routinely interacted with the Norwegians. So, one of the ships we rented was in fact a Norwegian vessel–a remarkable oceanographic vessel that could be run with only a crew of seven. So one summer...

van Keuren: Which ship was that?

Nagel: The Sverdrup. Sverdrup, I'm pretty sure. And one summer a number of people from the division and elsewhere in the Laboratory used it as their base. And they went up, and they took water samples, took sediment samples, took marine life samples from bottom trawls. I remember them reporting one time that they saw a polar bear far out at sea, dozens of miles at sea, away from anything close to land. So,

there was an asymmetry in suspicion, I would say, because we were going into their backyard. We were investigating their nuclear dumping. They, the Russians, were willing to cooperate for a couple of reasons. One is most of the people involved were scientists and liked the chance to finally interact with western scientists and do some things together, so we were warmly welcomed on an individual low level. And the other thing is they smelled money. They thought they could get funding. Of course, funding at that time was a tremendous problem. But, the Russian institutes would be willing to rent ships for prices that were glorious to them and a pittance to us, compared to what it cost for United States ships.
There were other things as I think about it more that came out of it. One of the trips to Moscow the lead fellow was a Navy Captain named Ed Pope, P-O-P-E. I met him earlier when he was the secretary for the CNO Executive Panel, and a very nice guy, and he was very prone to taking pictures of the equipment that we were shown. I remember him asking the people to lay it out on the table so he could take good photographs. I learned last week that he's in jail in Leningrad right now. When he retired from the Navy, he started a business to import Russian technology to the United States, and he was grabbed by the new regime in Russia just a few days ago and jailed so, I have an acquaintance in a Leningrad jail right now.

van Keuren: You mean St. Petersburg...

Nagel: I mean St. Petersburg.

van Keuren: Any other comments about that project?

Nagel: I don't think of anything now.

van Keuren: A very interesting project.

Nagel: It's one of the times in my career similar to the time I was at the Nevada test site where everything was sort of colored brighter and faster moving and more vivid. Very pleasant.

van Keuren: I think at heart you're really a field scientist.

Nagel: I won't argue with that. As a youngster I lived a very outdoors-y life, just roaming the outdoors and hunting and trapping and the likes. I have, for a long time, before this program and especially since, felt that I could have been perfectly happy as a marine biologist. If it turns out my friends in India are right, the Hindus and the Buddhists, and I get to come around again, then maybe I'll be a marine biologist.

Well, there is one other thing that just came to mind. A tale, residual from this program. Peter Vogt is one of the most remarkable people that I've met at NRL. He's a character, but he's also an incredible scientist. Very, very creative, lots of ideas. And I've enjoyed my association with him over the years so much that I'm dabbling in an effort that he's leading to map the entire sea floor of this planet with a resolution of a few meters similar to the resolution with which we've already mapped the surfaces of the Jovian and other moons by using satellites. And the reason I'm involved in it is because the long term idea in that is to design and build at least dozens and maybe hundreds of unmanned underwater vehicles that could be sent to sea and told to do a job and then would simply do it. It would be powered by batteries. As batteries run down, they would bob to the surface, recharge the batteries with solar cells, broadcast their data through a satellite, and then sink themselves again and go about "mowing the lawn." So, anyway that's a very interesting possibility. It involves instrumentation of which I'm aware and am interested in, and I hope to attend a workshop that Peter's hosting in Mississippi in June of this year.

van Keuren: You became very interested in the issue of cold fusion. This would have been back in the 1980's. Can you tell me about why and how you became interested in the subject?

Nagel: As I've already said, I headed a division whose roots were in solid state physics, in cold fusion, and in nuclear and radiation physics. The fusion in cold fusion. So, from a positional view point, it was square in my sandbox. Even if it weren't, I would have found it extremely interesting, because as an episode in science during my lifetime, it was extremely unusual. It burst on the scene as some things burst on the scene. It's not unusual for things to suddenly come to light, but the way in which this came about--a press conference-- was unprecedented. The claims were just incredible on their surface, and it got some people so excited that they essentially stopped what they were doing. Now, for most of my time at NRL, I took advantage of the recreation club at the laboratory, and I would swim commonly, sometimes five times a week, usually two times a week, and only twice during that time was I fished out of the pool. One time was when we were going to fly, my wife and I were going to fly, to Istanbul later in the day for a vacation, and she thought her was stolen. So, she had a secretary come over and get me out of the pool, and it turned out it was just towed away. Well, the other time was when my boss, who was involved in hot fusion for years, learned about cold fusion, and he sent somebody to get me out of the pool to see what I thought of it. An overreaction to be sure. But about forty people at NRL stopped what they were doing and immediately started looking at that. It was very similar to the announcement a few years earlier about high temperature superconductors, and the individual scientists at NRL have enough latitude that if they think something's more worthwhile than what they're working on, they usually can get permission to just change it, or they just do it. They go off in a new direction. So, all of a sudden, I was in conversation with chemists and materials people I had never before, about what we ought to do about it. I include a number of people with whom I worked more or less closely off and on in the now eleven years since that announcement on the 23rd of March 1989. That, incidently, was six years to the day after the Star Wars announcement. And I've got a lot of professional colleagues and friends as a result of that program, as well.

So, I was scheduled before the announcement to go to Brigham Young University in Provo, Utah, in early April of 1989 to give a talk on x-rays. There's a fellow, Larry Night, N-I-G-H-T, who was a professor of physics there and editor of the journal, and he wanted a seminar from me in some aspect of x-rays. So, it was just an accident that I was in Utah roughly two weeks after the initial announcement, and when I first heard the announcement, it was amusing, and then it got more amusing. There is a way to induce nuclear fusion by the use of muons, M-U-O-N-S, or muon-catalyzed fusion, and I could explain the reasons for it, but suffice it to say, they're like heavy electrons, and they promote the rate at which fusion reactions occur in gases like hydrogen and deuterium. Now, one of the people involved in the early stages of the cold fusion that was held in Utah, in the state, was a fellow named Steve Jones, a professor at Brigham Young, and his research had been on muon-catalyzed fusion, and then suddenly these two chemists from Salt Lake City, the University of Utah, made this announcement, and there was all this excitement. So, anyway, I mistakenly thought that the state of Utah was more monolithic because of the dominant Mormon population out there, and I was amused that here you have two research groups an hour's drive apart that were working in this radically new area, and I thought it must be Mormon-catalyzed fusion. I get out there, and I found out I couldn't have been more wrong, because there is a tremendous tension between the University of Utah people at Salt Lake, and the Brigham Young people in Provo. In fact, when I talked to Larry Knight, he reminded me that many years earlier that people in the University of Utah had reported the operation of an x-ray laser at eight kilovolts energy, which was a thousand times higher energy that had been demonstrated in a laser, in any laser up to that point. It turned out to be wrong, simply, and it was a mistake that involved taking some spots on film that might have produced by static or other imperfections, as signatures of x-ray lasing. So, anyway, my host, Larry Knight, when we started talking about this new energy source, cold fusion says "yeah, they can use it to power their x-ray laser."

Anyway, the situation was very tumultuous through the entire initial year. The D.O.E. set up a review committee called the ERAB, E-R-A-B, Energy Research Advisory Board, headed by a professor from Rochester, John Huizenga, H-U-I-Z-E-N-G-A, and they went around checking things, and eventually came up with a report in the fall of that year, saying that there was nothing to it. On May 8th, I believe it was, of that year, all three magazines, Time, Newsweek, and U.S. News and World Report, had cover stories on it, all on the same day. There were announcements and retractions, it was just all kinds of activity, and the level of interest and activity peaked somewhere around 1990, the amount of papers per year wound down, but international conferences on cold fusion have been held every year to a year-and-a-half starting in the first conference in Salt Lake City in 1990 and extending to the eighth international conference, which I'll attend in Italy next month.

Now, this subject is extremely broad and deep. It would take a long time to treat it in full detail, and possibly we should do that as a separate subject. But for the time being, let me say that I've that been very close to it, even to the point of some hands-on work, but primarily from the management a review posture. I've supervised or followed closely roughly half a dozen different experiments within the NRL, aimed at producing anomalous effects in various kinds of experiments, some of them electro-chemical cells, like the original Pons and Fleischman, P-O-N-S, Fleischman, F-L-E-I-S-C-H-M-A-N, cells, and others that are radically different. Some of these experiments at NRL gave inexplicable results, most of them didn't. But my net assessment is that there is definitely something there. It is not as most people in science and most people at large believe, all a mistake. If you discount all of the negative papers, if you throw out the positive papers that have poor signal to noise, or have other problems like people you don't trust, and you keep only those papers done by scientists with good publication records, good reputations, who had adequate equipment, good calibrations, and good signal to noise, and you take those papers only, you're left with a few dozen reports of the effects that are entirely outside of our present theoretical knowledge and far, far beyond the levels of power and energy generation that can be explained by ordinary chemistry, often a hundred times what could potentially be explained by ordinary chemistry. So, in my mind, there is no question but there's something there, and it remains to find reproduceable ways to make the effects happen and then to understand how they happen.

van Keuren: What research was done within your division, on the topic?

Nagel: The first thing was an experiment in which deuterium was put into a metal, it had to be titanium, not palladium, by ion bombardment, rather than by electro-chemical means. That experiment produced energetic particles that were measured with an appropriate radiation detector, and after a great deal of testing, they appeared to be 5 MeV, make that megavolt, tritons, T-R-I-T-O-N-S. The experiment was remarkable in that it took sub-kilovolt particles as input and had megavolt, over a thousand times higher, energy particles coming out as output. We examined that experiment most rigorously, to the point of questioning the individuals involved, going back to the inventor of the kind of detector that we were using, all kinds of data analysis, additional chemical analyses, many, many things that we did, and we could never either understand how it came to pass, that the signals came to pass, nor get back to the same conditions and parameters faced to make them happen again. Now, ONR funded a program involving NRL, China Lake, and the Naval...bear with me...the Navy laboratory in San Diego, which is now called SPAWAR.

van Keuren: The old electronics laboratory?

Nagel: Yeah, it was NCCOS, something or other, Naval Communications Command. Command and Control, anyway, something or other. But anyway, it's a Navy lab in San Diego. And the experiments were done in the Chemistry Division, with the support from the Materials Division and our division, and they never produced anomalous effects and were shut down. There was a fellow in electronic warfare, a retired guy who came back and had a lab in which he could work as he felt, when wasn't playing golf, and he came to me one time and was describing a series of rather amateurish experiments. He clearly had the time, enthusiasm and talent, so I basically arranged it for him to reproduce an experiment done at Los Alamos by some people out there that got very interesting results. And this fellow in electronic warfare worked at it for a couple of years and was never able to get the same results that we got at Los Alamos, and then he eventually abandoned it. That came to mind. There were other kinds of experiments that were of lesser character, in that they didn't get as much attention. A group of people in Space Sciences and Materials made an attempt to reproduce an experiment in which a molten salt was used as an electrolyte rather than an aqueous electrolyte. And that didn't go anywhere, and there were other lesser experiments. Throughout all of this a particular fellow named Imam, I-M-A-M, in the Materials Division was producing palladium and other related materials, not only for experiments within NRL, but also experiments at a variety of other places. Some of his material was used by a fellow from China Lake and a laboratory in Japan and produced excess heat. In fact, Japan is being sought on the production of those materials right now. So, I could probably think of some other things. There was a considerable amount of theoretical work both in our division and elsewhere within the laboratory in an attempt to understand this that was extremely controversial. We had a lot of discontent. At one time, I got a memo saying that cold fusion had ruined my reputation.

van Keuren: This was from?

Nagel: This was from George Miller of the division, okay. This is the same fellow who held a wake for cold fusion during 1989. A little get-together within the Laboratory where the punch bowl was configured like an electro-chemical cell. George is a skeptic who I like and respect a lot. We pull on each other, and that's fine. There were other instances, for instance. I hosted the fellow from Italy one time to give a seminar at the Lab, and he spent the first third of the seminar in a rather philosophical basis, and one of the theoreticians of the division exploded all of a sudden right in the middle of this talk and was berating him for his philosophical ruminations, and I basically had to ask the fellow, the NRL guy, to be polite and give our guest the chance to express himself, and would you please sit down and listen, and he did with steam coming out of his ears. He's now a dean at Georgetown University, and every time I see him we still smile over that episode. Many, many stories I can tell.

These various cold fusion conferences oscillated from North America to Japan to Europe to Japan, North America to Europe, Japan. So, in the course of these I've been in Salt Lake City, in Como, Italy and Nagoya, Japan, N-A-G-O-Y-A. And then there was one in Maui. I count it as a U.S. one. There was one in Monaco; there was one in Hokkaido, H-O-K-K-A-I-D-O. There was one in Vancouver, and the next one was back in Europe in the Italian Riviera. So, I've certainly enjoyed some nice travel. The first few of these trips were funded by NRL normal travel money. At some point, they were unwilling to do that, so I've been going to them on my own funds in the recent past.

My view at one time is that this thing would have been sorted out, plus or minus. I mean it was either real or nonsense, on a scale of a few years, and that was wrong. It's come to be that I've since, having that view in the early '90's, come to feel that it might not be sorted out for a generation or more. This current generation with few exceptions has lost interest in it. Part of the reason is due to the fact that the ordinary scientific communications within the community of researchers have been disrupted by the unwillingness of editors, by and large, to publish information in this arena. It's been disrupted between the people interested in cold fusion and the rest of the world and because of lack of newspaper and magazine interest. So, what will probably be necessary is this current generation to die off, and then somebody else will come along and do something that will get the ball rolling, again. I have learned of the history of plate tectonics where of all things a weatherman postulated the idea that South America and Africa once fit together because of the interesting shapes of those two continents, the west coast of Africa, east coast of South America. He put this idea forward. He was reviled, but had a group of followers and it took roughly fifty years for his ideas to become accepted. And what put him over the top and made him generally accepted in scientific circles was data from two sources, one the sediments in the sea floor were shown to have a magnetization that altered north and south over the eons as the poles of the Earth flipped, and also to go down in depth. So both the lateral distribution and the depth distribution of magnetization was oscillatory. And then the other thing was that during the Cold War, the United States put in place a network of seismic stations that would listen for Soviet or any other nuclear detonations and of course what they mostly saw was earthquakes. So, when they started plotting on the map where these earthquakes happened, it turned out that they'd ring what we now know as the plates that cover the surface of the Earth and move with respect of each other a few centimeters per year.

van Keuren: When did the plotting become available?

Nagel: It was in the late'50's or early '60's that that kind of data came to play, as I recall. So it was roughly fifty years from 1912 to maybe the early '60's. Now when I first became aware of plate tectonics it was significantly later than that. So the fusion of this idea out of the community of geologists to the general population itself took a while.

So, anyway the point is that while initially I felt the cold fusion thing would be sorted out in a matter of years it may now take a matter of decades. If that's the case, it's certainly not without precedence in the history of science.

van Keuren: At the Laboratory, in which divisions was the research mainly done? It was your division, Chemistry.

Nagel: Chemistry, Material Science, and then there were outliers, like an individual in Electronic Warfare and another individual in Space Sciences.

van Keuren: Did you do any collaborative work between the divisions or was this kind of individual efforts?

Nagel: No, it was mostly collaborative.

van Keuren: Mostly collaborative?

Nagel: Yeah, for instance when a measurement, when an experiment was done in chemistry then we would bring radiation detectors to it. The Materials people would provide materials and so forth. So, it was a wonderful inter-divisional collaboration, almost without any inter-personal problems.

van Keuren: And it was funded from 6.1 money?

Nagel: Could be there was some 6.1. Some from a special program that Fred Saalfeld had. It might have been 6.1 money. I'm not sure of that, but in any event that was money that came in specifically for that program. And then I think there was some money from the Spacecraft Technology Center that found its way into various experiments. I've heard that Wilhelm, Peter Wilhelm, provided some modest funding for something. The people involved. Tim Coffey was very supportive. In fact, at one point I went to him and asked for $100,000 to give a fellow in Ann Arbor, Michigan in order to do a certain calculation, and he just gave me the money and I funded this guy. Okay, he's been supportive. At some point, he ran out of patience and said, "No, let this thing sort itself out on the outside." But he hung in there for a long time, as did Fred Saalfeld. Bhatka Rath has always been very, very supportive. Jim Murday and Gubser, two key division heads, were guardedly supportive, but nevertheless they did not impede it. The branch heads within my division took it in a variety of ways ranging from active support to just letting it run hands-off and not either supporting or interfering with it. The individuals were very interesting. Scott Chubb, who...C-H-U-B-B, who postdoc'd in our division, was actually in the Remote Sensing Division. He did theoretical work. He did it with his uncle, Talbot Chubb, T-A-L-B-O-T, Chubb, who was once the head of a very large branch in the Space Sciences Division. The experimental work in chemistry was done by

Dawn Dominguez, D-O-M-I-N-G-U-E-Z, and Pat Hagans, H-A-G-A-N-S. I've already mentioned the materials guy, Imam. The experiments in our division were done by George Chambers, Ken Grabowski, G-R-A-B-O-W-S-K-I, and Graham Hubler.

van Keuren: Grabowski's first name is...?

Nagel: Ken. And Graham Hubler. Okay, the radiation measurements were done by Steve King and Gary Phillips. The fellow in Electronic Warfare was Sid "Blank." I don't remember his last name right now, I'll find out. I'll look it up. And the fellow in the Space Sciences Division was Martin Daehler, D-A-E-H-L-E-R. Another person in chemistry, who was close to the experiments was Debra, D-E-B-R-A, Rolison, R-O-L-I-S-O-N. A key fellow in all of this is an electro-chemist named Bob Nowak, N-O-W-A-K, who used to be at NRL and was then at ONR and now works at DARPA. I could probably think of a few names that were peripherally involved, but then those are the main people.

The current status of cold fusion internationally is that only a few dozen people are still active in it. There will probably be two hundred people at the conference next month with a variety of papers. There is a special issue of the Journal of Accountability in Research that will come out soon, edited by Scott Chubb, and I have an article in that. There is a book written by a freelance writer from Maine, one Charles Beaudette, B-E-A-U-D-E-T-T-E, Charles Beaudette, entitled "Excess Heat" that I read in the manuscript form and wrote an introduction for. Curiously the forward to that book was written by Arthur C. Clarke, C-L-A-R-K-E, who is well known for his story "2001." So the field has not gone away. I think the data is incontrovertible but it's impossible to get funding to do anything.

van Keuren: Any work going on at the laboratory at the moment?

Nagel: There's nothing still going on at the Lab except for the attempt to get the patent on this thoron-containing palladium that Imam made.

## Interview Four

### 2 June 2000

van Keuren: This is David van Keuren. I am sitting in the office of David Nagel, George Washington University. We are talking to him about his life and career. This is interview number four.

David, starting in 1995 you led the Naval Research Laboratory's involvement in micro-machine technology. How did you become involved here?

Nagel: It was a willful choice to get involved in this. If you go back a few years, I was heavily involved in X-ray lithography and serving as an advisor to DARPA's program on X-ray lithography, and that was going along, and that was my main technical focus. Then, as we already discussed, from '92 through '94 I had an interlude in environmental matters that came on the scene, ran its course, and disappeared. So, after that I had a choice: would I go back to the X-ray lithography world, or would I get into something new. I was not attracted to going back to the X-ray lithography world because that was a program that was evolving, it wasn't exploding, and I wanted to get into something that was really hot. So, I looked around, and it was an easy decision to choose to get into micro-machines or MEMS, as they're called. It stands for Micro Electro Mechanical Systems. The technology has its roots as far back as the mid '60's, but there was just isolated pieces during the '60's and '70's. The first commercial part, a micro pressure sensor, developed in '79, and then thru the 80's a few additional pieces became commercialized, but there was a tremendous amount of research sponsored first by NSF and then by DARPA, which continued into the '90's. It provided a really strong base, a lot of demonstration, a lot of prototypes. So, it looked to me like we had in MEMS a technology that was at a take-off point. It was going to be very, very important, so I decided to get into that. For a long time I felt that the best thing to do from a career point in science and technology is to "catch a wave," that is to jump into something when it's really new and really rolling and then you can ride the wave for a few years and get off of it. So, it appeared to me that MEMS was a big wave, and it was a perfect time to hop on board, in order to do two things. One is to establish some recognition of NRL as a place for MEMS work and also to pull together the spotty MEMS work that was going on in the Lab. Spotty in terms of distribution not quality. I organized post as a two-day workshop on MEMS at NRL in 1995 and brought in experts around the U.S. I could recite the names, but that's a matter of record from the workshop. It succeeded in attracting a lot of NRLers to the workshop. They learned a lot, and the people who visited us learned a lot about NRL. It was a quite successful workshop, in retrospect.

van Keuren: So where did the work lead to?

Nagel: The first question that might be asked, and it was asked, we discussed this with the management of NRL, particularly Dr. Rath, was whether or not the Lab ought to have a major program in MEMS. A basic research program, a exploratory development program, whatever. The answer to that question was no. The reason is MEMS is ultimately a conforming technology. Everything is stacked up. Materials make components, components go into subsystems, sub-systems go into systems. The MEMS are like integrated circuits, chips. There are pieces that go into other things. Or viewed another way they are tools to accomplish functional ends. So, just as integrated circuits popped up all over the laboratory in the technology and engineering communities in the 60's, so also MEMS was popping up broadly within the lab and beyond in the 90's. The people in the Electronics Division, the Chemistry Division, and other divisions were interested in them, not to develop them, but to use them. So, anyway, we didn't put in place any kind of over-arching MEMS program. Having said that, the immediate impact was to heighten the interest in the Lab. Within the next couple of years all five branches of the division that I had headed at that time were doing something on MEMS, whether it be a technology assessment, or experiments and calculations, and the like. So, I would say that you could attribute to the fall out of that workshop an increase in the number of MEMS research projects within the Lab, largely on outside money.

van Keuren: And this was spread through several different divisions.

Nagel: Yes, I would say that there were probably a third of the divisions at the Lab that had an interest in either making MEMS or applying MEMS.

van Keuren: You acted as a facilitator for bringing this into the laboratory.

Nagel: I did as far as my division went and as far as to a limited extent others were involved. I went to the head of the MEMS program in DARPA, a fellow name Ken Gabriel, and offered my services to him as a senior point of contact to the Navy, just as I had served as an advisor to the lithography program at DARPA, and he agreed to that and gave me $125,000 a year to spend within the division to start things. It also got me involved in the organizational meetings, semi-annual meetings, for DARPA, both DOD-wide and the so-called principal investigators meetings--the people who were actually running the projects within the DARPA/MEMS program. That was extremely useful because, even though a relative latecomer to the overall community, some people had been working in it for ten or more years, I was instantly credible as a Navy point of contact for DARPA, which had most of the funding at that time. So, I got to be reasonably well known within the MEMS community as far as knowing most of the senior people myself, and that was very helpful.

van Keuren: Why micro-machines?

Nagel: As you know, integrated circuits, chips, are useful for the storage and manipulation of information, but to gather information from the world automatically, that is to sense it, and then to use information that control the world, that is to actuate things within the world, you need something that moves, and micro-machines are indeed that something. So, MEMS provide the opportunity to both listen to the world and push back on the world. So, they enable functions that simply can't be performed by integrated circuits alone. Micro-sensors are the most important of those general functions.

van Keuren: Where do you see these machines heading in the next ten, twenty years?

Nagel: Well, I think before answering that, I thought I would say where they went during the '90's. The technology definitely turned the corner in terms of beginning an exponential growth during the '90's. The number of patents versus year show that it took off in the early '90's and is now roughly at a point of one patent worldwide per work day, okay. In terms of commercialization, MEMS, depending on what you count, is somewhere between a three and thirty billion dollar a year industry now, and it is doubling, depending on who you listen to, every two to four years. So, it is definitely taken off. It won't be as big as integrated circuits, but it is going to be very, very important. So, my expectation during the current and the next decade is that this growth will continue, that the addition of new classes of products will occur. By that I mean pressure sensors were the first things, as I've already said, to be commercialized; micro-acceleromators; a big thing that is going on now is micro-optics for all optical transmission over fiber optic networks. In fact two months ago three MEMS companies were sold for a combined total of 5 billion dollars. So, optical MEMS has really taken off. The next thing that is going to take off is radio frequency MEMS, and then the use of MEMS for data storage is being very heavily researched right now and has the possibility of offering storage densities of terabits per square centimeter, on a scale of about ten years. If that happens, we will essentially be able to carry around a library in our wallet.

van Keuren: And what role did NRL play in this?

Nagel: The answer to that is varied. If you look at the division that I headed, one of the branches did some radiation effects testing on MEMS. Radiation in space, natural radiation in space, I should say, and also radiation from nuclear weapons is always been a big concern to DOD, satellite designers, and manufacturers. The electronics that are associated with micro-mechanics in MEMS would be subject to the same radiation effects. I got some samples from analog devices of air bag triggers, and I gave them to people in one of the branches, and they irradiated them with protons and studied the response. The change in the behavior of these air-bag triggers as a function of radiation dose. The second branch did an extensive review on optical MEMS that resulted in a NRL report that was put out as the first NRL report on a CD. In fact that's kind of interesting because the hard copies of the report, which cost around $50 dollars because they were something like 70 figures and a thousand references and a lot of color, anyway the hard copies were expensive. So, the CD's were $2.25. We got a couple of thousand of those, and put it on the web. It tends to be a very popular website. That's what the second branch did. The third branch was already working on a MEMS-scale acceleromotor out of the Trident missile program, and they expanded that work into other devices, involving micrometer scale, moving parts.

The fourth branch, they looked at the use of MEMS as chemical sensors. They developed a lot of good ideas, but it really didn't go anywhere. The fifth branch, the theoretical branch, started doing super computer scale modeling of MEMS scale structures. The work went very well. In fact, both the people who did it became very well known for that. One of them has subsequently gone off and got an MBA, and the other now works at Livermore, and the second fellow continues in that area and is very widely recognized for it. Now beyond that the MEMS program was started, I think, under 6.2 money in the Electronics Science and Technology Division. There was work on MEMS devices in Chemistry, led my

Rich Colton, aimed at chemical and vital sensors, and there are some other things that went on, as well. The work, while not globally coordinated, was extremely diverse and of very high quality.

Van Keuren: This work is continuing at the Laboratory?

Nagel: Yes, in fact the interest in MEMS at the Lab is growing, the people in the division that I once headed, who are making electronic noses for sniffing a wide variety of things, from chemicals on the battlefield to spoiled food to various kinds of fuels are interested in starting a MEMS program, and they call me in, and I try and help them because it's an area in which I am working, and they are my professional friends.

van Keuren: Are you working in this area here at GW?

Nagel: Yes, the situation in GW was willfully fabricated, just as my foray into MEMS was. What was happening was that in 1998 I was due to turn 60; my father died the year before, which really wasn't that much of a motivation, bit it was obviously a turning point in my life. I had lost my second to last parent, and I was in a situation in the Lab where I had been in line management for 25 years; I had been a division head for, at that time, around 12 years; I was working six days a week steady state, long hours, would be extremely tired on a routine basis, and not unhappy, but I was clearly in a situation that I had no glide path from. As long, as I was going to stay in that position I had to do what was required and that was quite demanding. So, it became clear that I was going to have to find another kid of a job that would allow me to back off in my level effort as my energy and abilities declined with age. It's inevitable. So, when you look at the major sectors--government, industry, academia, and then the think tanks, and the institutions in between and so forth--of those, hey, I was already in government. Had done that. Industry didn't appeal to me because I would be hustling money, much as I was in NRL, but without the benefit of the reputation of NRL, and so by default and by attraction, the academic arena was where I would like to be. So I did a systemic survey of the universities in the area. My wife and I wanted to stay in the Washington area, and I wanted to stay in MEMS. So, I did such things as talk to the provost out at George Mason University, and he offered me a half-time job, ten minutes into the discussion, which was kind of strange behavior. Anyway, it wasn't a satisfactory situation. I went out to the University of Maryland where I did my Ph.D. and talked to the head of the Materials program there, a fellow who used to work at NRL. He took me over to the dean, and I had a nice chat there, but the trouble with that university is it's diagonally across town from where I live, a long commute, and they never really came forth with an offer. So, that left the George Washington University, which had in the Electrical and Computer Science Department an institute for MEMS and VLSI (Very Large Scale Integrated) circuits. The institute was formed in 1996, headed by the woman who was then chairman of the department, Mona Zaghloul. I went to the head of the DARPA MEMS program at that time, Al Pisano, and asked if he got a proposal from the George Washington University for me to work on MEMS whether or not he would fund it. He said he would. So, I wrote such a proposal and brought it to Ona Zaghloul, and she signed it out and sent it over to DARPA. It was to have gone through the Army but ran afoul of details of contractual procedures. At one point I was told that it was dead. That was profoundly disappointing since I had been working on that for about 9 months at that time. We found that the Navy in San Diego had a BAA (Broad Agency Announcement) out that would cover this, so the proposal was resubmitted through that. Another nine months went by and finally the Navy provided a funding document to the University, at which point they had the basis for offering me a position.

The appointment is as a research professor, as distinct from a teaching professor. It's a soft money position. I had to get about $150K/year to generate overhead to pay for the office right here on K street. It's a very satisfactory situation because it's flexible in two ways: one is my time is covered only about 60% by the DARPA contract, so I had the 40% of my time available for other things. Within that 60% I have a lot of latitude on what I actually do for DARPA. The current contract has three requirements. The first is to produce a technology assessment on radio frequency MEMS, and that's a basically a sequel to the optical MEMS study that was done at NRL that I was co-author on. The one that appeared on the CD. The second task is to develop a concept for integrated systems with multiple MEMS in order to perform functions such as being a micro weather stations or a machinery monitor. The third was to actually develop applications in these things for Navy ships. So, the contract currently is in the second of three years and is still running satisfactorily. I have to go to the DARPA meetings on a semi-annual basis. The next one is in Boston in August to give a poster presentation and make reports.

The other thing that's worth saying is that their research program in this institute that I joined, has been underway for a better part of ten years. The students are actually located at NIST (National Institute for Standards and Technology) because they have very good laboratories up there. We do not have the facilities to do the semi-conductor processing here on campus. So, I probably in the next year or so might get into some hands-on researching, but that is neither a requirement or a certainty. The other thing that came up is before the academic year that just ended MEMS had never been taught at this university, and Ona Zaghloul and I were interested in the possibility of offering a pair of courses, starting this year, and, in fact, did that, and it worked out very well. She taught a course in the fall semester entitled, "Introduction to MEMS," and I helped out with that, and in the spring semester I taught my first course, "Applications of MEMS", and she helped me with that. We had about a dozen students in each one of these during the first semester. They designed MEMS and the designs went off and were, in fact, made at a foundry in North Carolina. During the second semester, students in my course designed four different projects, a micro weather station, a prototype electronic field [?], an electronic nose, and a machinery monitor. It was a lot of fun. Those projects, incidentally, had two nice benefits. One is they form a basis for proposals, and (2) they might turn into Ph.D. research projects. As an aside, because I am not a teaching professor I am not permitted to supervisor thesis students in this department, but that's okay, I can work closely with the so-called "real professors."

van Keuren: Sounds like you are doing interesting and exciting work.

Nagel: I'm very pleased with the university, with the department, with the people here, the work. It doesn't mean that there aren't problems. I have ordered two computers since I coming to the Lab, to the University. The first one, a desktop computer, came with the wrong case. I had to take it as is because I needed it so badly. For the last ten months I have trying to get a laptop computer and have not yet succeeded in doing that. Like any large organization with its bureaucracy, I have some problems, but globally I am very pleased to be here.

van Keuren: After the three-year grant, do you think you will reapply? What are your long-range plans?

Nagel: I would like to work as long as I can. It doesn't mean I want to work full-time, but I am attracted by the experience of my father who was a carpenter. He worked for about 50 years full-time and then tapered off from there. Did jobs as he felt like it, and when he died at age 91 he still had a few unfinished carpenter jobs. My hope is I would like to continue to do academic intellectual scholarship type of work, either in the university setting or for companies as a consultant, as long as I can do it mentally and physically. So, the situation I am in right now will cause me to get additional contracts, and that is an action item right now, as it generally takes a year to get something in place. Over the course of the summer, I am going to participate in writing myself two or three major proposals. One of them will be on something called pervasive sensing work, where large numbers of MEMS sensors are put out over a broad spatial extent in order to essentially extend both in terms of capability and in terms of geography our own senses and pull information into it, and that is going to be part of a virtual reality proposal that will be co-authored with some people and sent to the National Science Foundation. Another proposal I intend to write is on a scheme to manufacture very large numbers, thousands or more, of small-scale robots, in order to study the interaction of machines which will talk to each other by RF (radio frequency) and optical and even acoustic means. So, you can imagine a gym floor covered with these micro-robots that could be re-programmed on fly [?] to accomplish either cooperative or conflicting jobs. I have some concept to produce these in a cost-effective fashion so that each one of these coupling size robots would be no more than expensive than about $50 dollars. It would allow us to develop a system, a test-bed in order to study such things as are now called ”swarm smarts.” “Swarm smarts,” that’s a term that applies to the emerging field of computational and experimental science technology that seeks to understand how individual ants (none of which are very as smart) can do something as sophisticated as build and manage an anthill or termite mountain, or beehive, and things like this. So, I want to get what might be called, flippantly, robot sociology. There are some other proposals that I will tend to in order to maintain my position in the university. So, basically, I expect to teach the course in application of MEMS every year that we can attract enough students. In fact, during the summer I have to work with the recruiting of students. We need a half a dozen students in order to run a course, but in order to make it viable, we should have a dozen or more. That means that if I have certain half-time or so funding from some contract that I can teach a course this semester. Then I also have many consultant opportunities, I think, as well.

van Keuren: The DARPA grant covers 60% of your time; the other 40% is the consulting, teaching?

Nagel: Teaching and consulting, and golf, gardening, and other good things.

van Keuren: And travel.

Nagel: and travel, yeah. So, basically I am in the situation I have a lot of latitude, a very strong base in a good university in a hot technology area, and I count myself as very, very fortunate.

van Keuren: You continue to consult with the Laboratory?

Nagel: I do that, indeed, and I do it free. I don't have any financial arrangment with the Laboratory right now. I have a badge; I can get into the Lab. Every time I go into the laboratory, someone wants to talk. I was in there a couple of days ago, and I didn't make it off the ground floor of the building before I had a meeting scheduled for later that day to help some of the people with a proposal they wanted to put together. I called the Associate Director, Rath, this morning to ask if he wanted to listen to a presentation that two of us would make on the cold fusion meeting that was in Italy last week. He said yes, and by the way would I send him ten view graphs for a talk he has to give in England later this year. So, these are people with whom I worked with great pleasure for a long time, and I am glad to help them, of course.

van Keuren: I want to step you back just a little bit, to the point of your retirement from the Naval Research Lab. Shortly after your retirement, which would have been 1998. In the beginning of 1999 your old division, Condensed Matter and Radiation Physics, was disbanded, with some elements being closed out and others being transferred to other divisions. Why did this happen?

Nagel: Well the demise of the Soviet Union left the United States without a reliable enemy. The Defense budget globally peaked somewhere around 293 billion or so somewhere around 1990, and then it declined by roughly 10 percent over the ‘90's. That decline resulted in several parts of the Lab shrinking, especially in the materials and the component technology part where we belonged. Of course, some parts of the Lab were booming: the Electronic Warfare division, the Information Science and Technology Division, Optical Sciences, were doing very well. But the disciplinary divisions like Chemistry and Materials and our Condensed Matter and Radiation Sciences Division were, in general, all shrinking. There is a fixed base cost to run a division because you need a division head, a secretary, and a division office to take care of financial and personnel, equipment, and other matters. So, basically the fixed cost of the division office was being supported by a shrinking base of direct charges with its associated overhead. So, the decision was made by the director when I told him I was going to leave that he would take parts of the division, the five branches and the individuals in the division office, and distribute them. But none of them were shut down, they were just redistributed. So, one of the branches went to the Electronics Division, three others went to the Materials Division, one went to Chemistry. The one that went to Chemistry was later disbanded as a branch and absorbed into another. All the people were retained. The people who were in the division office, the four women who basically ran the division-–the secretary, the head of the office, and three people who worked in the office--were all relocated within the Lab. So, from my perspective the labs did the sensible thing in bolstering three other division with the pieces of our division and cared for all the people involved, in a sensitive and sensible fashion. In fact, I have a great deal of appreciation to Dr. Rath, as well as the Director, Tim Coffey, for in the way in which they handled that.

Some people view the closing of the division as a negative thing in the sense that if we were stronger, or if things were better, then it wouldn't have been a candidate for closing. But, the reality is, that the two sciences that were underpinning that division, namely solid state physics and nuclear physics, had each had their heyday around 50 years ago. While the technology based on them was still important, it was not like bio-technology, or information technology, or something that is really having its heyday in the present times. So, I did not suffer any decrement to my ego because of the closing of the division. Quite the opposite, I tried to facilitate and appreciated the manner in which it was handled. On the process of course, it did mean that certain areas, that were once very, very strong at NRL, in particular, a very large area of nuclear science & technology and also another area like X-ray science and technology, sort of disappeared as organizational entities. There are still pockets of people who are knowledgeable in those areas, but you can't find on the organization chart. You have into  the Lab and starting asking around in order to find them.

One of the things that I think it would be very worthwhile doing, and it is still possible, because the people who did this work are still around and most are in the Washington area, would be to go back and trace the thread of nuclear research and technology at the NRL over the roughly five decades from the time before the Manhattan Project and the isotope separation work from NRL that was subsumed into the Manhattan Project all the way down to the work on the radiation effects on satellites, which is still continuing.

van Keuren:  Was this then the end of a research era? The Nucleonics Division and nucleonics area of the laboratory were established in approximately 1948 in the immediate heyday of post-World War II. So, with the closure of Condensed Matter and Radiation Physics was this the end of an research era?

Nagel:  In a very real sense, yes. It was no longer big enough to be identified organizationally, and the topics, as such, had become part of the fabric of science and technology and engineering. If you to plot the number of people involved or the amount of money involved or organizations or something as a function of decade, you would have something in the forties and then it grew through the fifties and the sixties. The cyclotron was built in the building that is now the Artificial Intelligence Center located at the Bolling Air Force Base. The work continued strong through the seventies. In 1970 I remember very clearly that the Navy decided not to fund basic research in nuclear physics anymore because of two reasons: One is things nuclear were already reduced to practice, we had nuclear submarines. There was a lot that was well beyond the research stage. Secondly, the leading edge from a research viewpoint was then in high energy physics that required very, very large accelerators and for which the Navy did not have much of a justification. So, in 1970 that were people at NRL in what used used to be called the Vandegraff Branch, who had a choice. They could stay at NRL and do something else or alternatively stay in nuclear physics and move from NRL. Almost to the person they stayed at NRL and used their computer oriented tools in the areas of solid state and thin film science and technology. So, through the 80's there was still significant work and some into the 90's, but by that time you'll recall that the Nuclear Physics Division was gone, the Nucleonics Division was gone. Their successor, the Radiation Science and Technology Division, was gone, and what was left had been joined with the Solid State Physics Division to form the Condensed Matter Radiation and Science Division. So, basically, what was once two divisions became one division, became half of a division, and then went away. Perfectly normal evolution from my perspective.

van Keuren: Looking back over a long career here at the Laboratory, what was it like to work at NRL over the years?

Nagel: Well, it was relentlessly interesting. It was also invariably flexible. I always had the feeling that I could pretty much do what interested me as long as it was within broad boundaries in terms of mission, and reasonableness, and also could be funded. The thing that I have never been able to satisfy myself on is sorting out the changes in NRL and the changes in me, over the years. There are undoubtedly major changes in the Lab over the 36 years that I was there, and I can address those and will in a moment, but during that same period I was changing too from somebody who came in and worked in a middle of a project that someone else had designed to a point where I could conceive of my own research studies and pursue them from beginning to end, from a concept stage to a paper that was presented and published. To a point where I then would have responsibilities not just for individual projects, but for whole programs and then for people and for a branch and a division. In kept growing. In terms of scope, as far as my own personal moment was concerned, the simple fact was in the six levels of NRL I worked on four of them over the time I was there. The part of the Lab that I was familiar with changed significantly, independent to me, from a situation in the '60's where we didn't have any outside funding: the head of the branch would go down and tell the management in a hour-long presentation once a year all the things that we were doing, and then he would be funded for another year. It was really big news in the mid-60's when we had the opportunity to get outside funding, and it was viewed as an opportunity. The basic situation that we've already touched on was that nuclear weapons were designed to emit a lot of x-rays. We were, in fact, an x-ray group, so we could get money from the Defense Atomic Support Agency, at that time, in order to field experiments at the Nevada test site, and that was the beginning of our work for sponsors, beyond the in-house ONR money. It went from becoming an opportunity to becoming a necessity over the years. If we wanted to maintain a viable group, then we had to bring in money from the outside. So, as time went on it became more challenging to do that. The rate of change in science and technology increased dramatically during the '80's and during the '90's, and it was a real scramble to the point where, as we have already said, there was a time in fiscal '94 when as division head I literally lost a lot of sleep over where the money would come from. So, the laboratory evolved in that fashion. But, I am well aware both from knowing what else went on at Lab, as well as reading the book that Ivan Amato wrote on the 75th anniversary of NRL in 1998, that there were parts of the lab that always worked on outside funding. They worked for the Navy, they worked for other parts of the Defense Department, or other agencies within the government. For them, getting outside money and doing project-oriented work, building satellites and the like, was a way of life. So, the answer to the question depends, of course, on where you worked in the Lab. For me, because I was in the relatively basic research part of the Laboratory, it changed dramatically, from being a very sufficiently funded and entirely flexible situation to being one in which not only the middle management but even the section heads and some of the individual workers had to scramble for money during the '90's.

van Keuren: Did the Laboratory change in any other ways? Culturally, for example. I get the impression from talking to some long time Laboratory residents that the culture of the '50's was very different than that of the '60's & '70's. Do you have any sense of that?

Nagel: I was not around in the 50's, but when I came in the '60's I was really like a mole. I mean I saw my own little furrow and not a whole lot else. I would have some contacts around the Lab, either because I was trying to mooch equipment from someone or because I was trying to respond to a request for assistance. They gave me the feel that the Lab was balkanized, yes, but the parts would cooperate with each other quite nicely. But, basically, it was a bunch of fiefdoms. In my view, the shrinking funding during the ‘90's forced the pieces of the Laboratory to cooperate more with each other and, in fact, not only internally but to team with people from the outside in order to be able to accomplish something that sponsors are willing to provide sufficient funding [for]. Basically, the people who made Materials were being asked to make devices, and people who make devices are being asked to provide functions, systems, and so forth. You could not always do that within your own group, so that you had to team with other groups and so forth. I think that the ‘60's and ‘70's, in my experience, were pretty much alike. There was plenty of money; there were times when we had to work to spend the available money before it would be pulled back. And we would stockpile the equipment, crystals [?], and supplies, and things.

The ‘80's were a period of big change because of the emergence of the SDI and other initiatives that would pull money away from the core funding and so forth. And then the ‘90's were dramatically different because with the global political situation now I am aware that throughout all of this, the funding of the Laboratory sailed along at a sort of high level. My view of the Lab has been kind of like a bush. Parts are shrinking, parts are growing, but the whole bush remains large and healthy. It has been my experience; I can't say that the changes were between the ‘50's and ‘60's and ‘70's. I would say that changes were between the ‘60's and ‘70's and what followed, the ‘80's, were the beginning of the big changes.

van Keuren: Do you have any sense of where you see the Laboratory going in the next twenty years?

Nagel: Well, there are a couple of responses to that. One is that it appears that we are alternating laboratory directors in the following sense. Robert Page, was a director who remained very interested in the technical details of the work and sort of left the rest of the lab to work alone. He was succeeded by, of course, Alan Berman, who found a laboratory that had parts that were quite calcified and that needed some shaking up, and he was well equipped both intellectually and psychologically to shake things up. I don't know if I mentioned this, but I remember having breakfast with him one Monday. He would come in at 7:00 in the morning and then at 7:30 join the branch heads over at the cafeteria for breakfast. He was all excited this particular Monday cause he had spent the whole weekend re-organizing the Space Sciences Division after Herbert Friedman’s retirement. Berman was, in my mind, a liberal; he shook things up. Now in Coffey we’ve had for the last roughly sixteen years a conservative who has been very, very loathe to shake things up. He started two new divisions, as far as I know: the bio-materials division under Joel Stern, and the Remote Sensing Division under Phil Schwarz. He’s closed a couple of divisions because of the departure of the division heads. One in the Navy spacecraft center, and then the one that I used to head. So, I expect that the next director will have to tend to more of the large-scale reorganization of the laboratory, than is presently being done. It is not to say that Coffey isn't doing his job and isn't doing it well. It's just that he’s sort of a steady hand on a ship that was going in the right direction, at a good speed.

Now, I thought for some time now, and have expressed to anybody who would listen, the view that the lab should form a high level organization aimed at biotechnology much more broadly than is going on in Schnur's division. Let’s form at least another division with the expectation that they might be a directorate in enviro-related things, not medically not necessarily, although that is an issue. I could comfortably see the NRL taking on a medical mission, but there are people, Coffey included, who say there is so much money being spent at NIH and elsewhere in the DOD on medicine that they don't want to fool with that. But there are many aspects of modern biology that could fall within the purview of the NRL. One example would be to take the genomes that are now being deciphered for a wide variety of plants and animals and use them as a templates in order to design biology base systems to accomplish military goals. It’s been demonstrated already that drosophila fruit flies can have eye parts on their legs due to random genetic modifications, and if that can happen, under stimulus of some mutating factor, possibly in the scale of 10-20 years we will be able to do that willfully in order to be able to incorporate additional sensors on existing animals or take sensors from one animal and put them on another. You take the best of hearing from the bat, the best of seeing from the fly, the best of smelling from the dog, and all incorporate them in one military useful animal. I believe that if the NRL is going be as successful in the current century as it has been in the last century, it's going to have to go far beyond its physical sciences space in order to stay up with, in fact, even lead some of the military applications of bio-systems. The incorporation of artificial systems such as MEMS with ordinary biology, humans included, is an absolute given. Thought control computers will be available broadly. They are in their earliest stages now. The insertion of sophisticated micro-systems into people is already happening , and will happen at an increasing tempo. The soldier, the marine, the land warrior of the future will almost certainly have an implanted micro-system, including medical records, analyzers, all kind of things that will enhance both their performances, as well as the abilities of their commanders to know what the current location condition, sleep, and emotions, and all kinds of things are. I really think that whether it's under the next director or whoever follows him, that the Laboratory is not going to continue to be the force that it is on the national scale unless it gets into aspects of bio-sciences and technology broadly considered.

van Keuren: Looking back what do you consider your major achievements at the Laboratory.

Nagel: That's an interesting question from a lot of viewpoints. I've already mentioned that I have been on four different levels, and I've basically passed muster in each one of those levels. In so far as it is useful to be a section head or a branch head or a division head, check the box, I did that. More significant, of course, is what was accomplished in the actual work. I have a mental picture of my career that shows levels vertically and years across the horizontal access, and then there are boxes. In each one of those boxes we could look to see what was accomplished. The first significant one was the five years at the Nevada test site, and, as a group, I had a very central role in that. In fact, did the first successful experiment with my own hands. We showed that it is possible to measure the X-ray spectrum with high resolution, obtain a lot more information, and it was immediately evident that was worthwhile. It was picked up by Livermore, picked up by Los Alamos, picked up by Lockheed for the DOD. So, that was a genuine accomplishment, which followed me through my whole career to my benefit. Then when you step beyond that to the laboratory plasma diagnostics we measured the X-ray spectra from laser heated plasmas and discharge heated plasmas in great detail, obtained a lot of quantitative information, published absolutely first-rate papers in The Physical Review Letters and in other good journals. It was work that was programmatically important, as well as scientifically important. So, during that period, I did some good research.

Now, overlapping that was the five years I spent during ion excited X-ray spectra in the Van de Graff Branch, and that was, as I have already said, my physics period. It was productive of some absolutely first rate papers, again, in the best journals, with lots of invited papers and work that got picked up in reviews. It was pretty first rate stuff. The foray into X-ray lithography that came from the laboratory plasma diagnostics resulted in an important patent and lots of papers, again. Again, lots of recognition. I might have said already that I sent an abstract to a meeting and got a job offer on the basis of that from a guy. Basically, what we put in place was the technology for a source for X-ray lithography. You can go from that point in the mid-'70's to the current work and what is now called extreme ultra-violet spectroscopy that uses laser-heated plasmas. So, there is a direct thread with a emerging current technology that may be used for producing chips worldwide, in the work we did. So, that was quite successful.

The next big thing was the environmental nuclear pollution, and the question was posted to us whether or not they've dumped radiation in the waters north of the Soviet Union which would harm Alaska. We put in place with a very high confidence an answer that it wouldn't, but we got a lot of good environmental data out of it, in finding certain things. We've talked about MEMS. So, I feel two things. One is I've jumped from one hot subject to another as time went on, which was very pleasing to me. I was able to get into an area, assimilate the necessary background, establish a position, make the contribution and get funding, time and time again. When the dust settled we had contributions in terms of published publications and programs that were recognized within the Lab.

van Keuren: Any major regrets?

Nagel: Yeah, I made some statements to this effect when I gave my last talk at NRL before retiring. I came close to making all possible mistakes. Now, that's not really true, but what I mean by that is if you think about the time-line for a given project, from the basic idea, "Oh, I think it's worthwhile studying this," to the initial considerations, maybe some trial calculations, trial experiments, to a full blown calculation experiment with the data analysis, to the presentation, publication, and so forth, I left a trial of debris that stopped at each one of those levels. And it was as bad as one time we wrote a paper, sent it off to a journal, we got back referees comments, and I never responded to them, and it never got published because I was off doing something else. So, if you look at the roughly 150 or so publications that I have my name on, reports included, there are two things to say. One is a lot of them really went no where. They provided fodder if you will, kept our reputation and things like that, but in terms of real impact, they didn't go anywhere, on one hand. On the other hand, we did a significant amount of good work that never got accurately documented. So, if I were to push the reset button I would be even more judicious about what I chose to work on and what I saw through complete to publication.

There is a question that has been asked of me. "Let's see, Dave, you are not a fellow of any societies, you haven't gotten many awards, (I got some awards), anyway, "you haven't had the kind of recognition that a lot of people who have been at the NRL for a long time have had." That's true. Do I have any regrets about that? No. Do I understand it? I think I do, and the reason for it that I was going from one area to another. I wouldn't settle down in a given intellectual community and do all the things -- conference organization, refereeing, and journal editing, and so forth -- that generally results in a person getting well enough known to be nominated for awards and the like. I don't feel this is an excuse. I think it is what actually happened. If I'd spent the time in a collision physics community, I would get a stature within that community, but while the people were continuing on I would be off in X-ray lithography or something. The fact that I haven't gotten a lot of awards doesn't bother me in the least and is not a regret, but it is a thing that has come up when people have asked me about my whole career. My bottom line is that I have hard time imagining being more fortunate than to wind up at a place like the NRL, with the size, diversity, and opportunity, and flexibility, and productivity, and reputation, and everything that I enjoy. If you put in my own personal context, coming from parents who never darkened the door of a university, to a point where I was a fully functional part of a really great institution for a long time, that is very gratifying.

van Keuren: Thank you very much.