Alfred Bader fonds

Correspondence

P

1986-2005

QUEEN'S UNIVERSITY ARCHIVES

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BOX 5

FILE 43

Subject: Oldrich Paleta - new recipient of the Alfred Bader Award
From: "Oldrich Paleta" <oldrich.paleta@vscht.cz>
Date: Fri, 2 Sep 2005 15:24:34 +0200
To: "Bader Alfred Dr., Fine Art" <baderfa@execpc.com>

Dr. Alfred Bader
924 East Juneau Avenue, Astor Hotel - Suite 622
Milwaukee
WI 53202, U.S.A.

Fax: 001 414 277 0709

Sept. 02,

2005.

Dear Dr. Bader:

I would like to inform you and your wife about the new winner of the „Alfred Bader Award (I)“ for the contribution to organic chemistry in the year 2005. The prize was given today to Dr. Radek Cibulka (age 32) from the Department of Organic Chemistry, Institute of Chemical Technology, Prague 6 (i.e. our university). The winner is employed as a senior lecturer. He was selected in the last round from two candidates (the last secret voting of the members of the „Bader Committee“ was 8:0).

The Award will be given to Dr. Cibulka at the traditonal conference of the Czech Chemiscal Society „Advances in Organic, Bioorganic and Pharmaceutical Chemistry 2005“ which is held on Nov. 18-20, 2005 in Nymburk. The winner will present a plenary lecture at the Conference about the work that has been awarded and should publish a review article in Chemicke listy during the y. 2006.

I hope you are good both.

With all best wishes to you and your wife Isabel,

Yours truly,
Oldrich Paleta

PURDUE
UNIVERSITY DEPARTMENT OF CHEMISTRY

RECEIVED
APR 30 1979
ALDRICH CHEMICAL CO., INC.

April 27, 1979

Board of Directors
Sigma-Aldrich Corporation

Attn: Alfred R. Bader, President

My Dear Associates:

Now that I am closing my tenure as a member of the Board of Directors of the Sigma-Aldrich Corporation, I had hoped to say goodbye personally at the last meeting, May 2. However, the combination of a meeting of the National Academy of Sciences in Washington, April 22-26, and a meeting of the Board of Governors of Hebrew University in Jerusalem, May 4-12, made it impractical to come up for this last meeting. Consequently, I must say goodbye by means of this letter.

It has been an interesting experience for a University Professor to participate in your activities. On only one occasion, last Fall, was I temporarily disappointed by the discussion. Fortunately, the final decision came out as I thought desirable, but I decided to leave you with my thoughts on the topic discussed then.

Over many years my students and I had been exploring a new area of chemistry, that of organoboranes, with major promise in synthesis and chemical manufacture. I tried repeatedly to interest one of the major chemical companies in exploring the possibilities of this area of chemistry, but without success. Company after company expressed interest. They would authorize and pay for a market survey. This would establish that the compounds had not been manufactured or marketed in the past year and they would lose interest.

Only Alfred Bader showed imagination and courage. After he heard my story, he decided to go ahead to test the market for borane chemistry. As you know, Aldrich-Boranes is doing very well. Sales are far from being market-limiting and are growing as fast as the space, equipment, and personnel permit.

Chemistry Building
West Lafayette, Indiana 47907

There are still numerous new developments in chemistry and biochemistry coming out of our research laboratories. Regretably, our large chemical companies have apparently lost the ability to take such new developments out of the research laboratory into the market place. All too often, developments that had their origin in the research laboratories of the U.S. are today being introduced to the market by industrial organizations in Japan and West Germany, not in the U.S.

Sigma-Aldrich still has this ability. I believe that organizations which take advantage of these new developments will be far more profitable than those that stay with old, well developed areas. Apart from the greater opportunities for financial return, I believe that the bringing of these new research developments to the market place represents a major contribution to the research workers of the U.S. and a major contribution to the well-being of this country.

When we were discussing taking over the Israel development, there was some questions raised as to whether we might not be spreading our executive talent out too thinly. It may be that we are. If so, I hope that you will make a major effort to add to our executive capabilities and will continue to expand Sigma-Aldrich's capacity to bring new research developments to the market place and to emphasize that function.

This is an area where we can be idealistic while greatly improving the financial prospects for Sigma-Aldrich.

I hope you find these final comments helpful.

With all best wishes,

Sincerely yours,

Herbert C. Brown

Herbert C. Brown
Wetherill Research Professor Emeritus

HCB:aw
cc: Dan Broida

Subject: Caltech's Grubbs wins Nobel in Chemistry
From: "There's only One.Caltech" <One.Caltech@dar.caltech.edu>
Date: Wed, 5 Oct 2005 20:03:52 -0700
To: "'baderfa@execpc.com'" <baderfa@execpc.com>

Dear Dr. Bader,

By now, you may have heard that Caltech's Robert Grubbs, the Victor and Elizabeth Atkins Professor of Chemistry, has won the 2005 Nobel Prize in chemistry! If not, please allow me to share this wonderful news with you. The prize recognizes Professor Grubbs's work on "the development of the metathesis method in organic synthesis." Professor Grubbs shares the honor with Yves Chauvin and Richard R. Schrock of the Institut Français du Pétrole and Massachusetts Institute of Technology, respectively.

According to the Nobel citation, work on metathesis by Grubbs and his colleagues has led to industrial and pharmaceutical methods that are more efficient and less wasteful, simpler, and more environmentally friendly--representing a great step forward for "green chemistry," reducing potentially hazardous waste through smarter production. "Metathesis is an example of how important basic science has been applied for the benefit of man, society, and the environment," the citation continued.

Professor Grubbs came to Caltech in 1978 with full tenure as a professor, and has held the Atkins Professorship since 1990. He is currently spending a month at the University of Canterbury in Christchurch, New Zealand, as an Erskine Fellow. Grubbs's award is the 32nd prize won by Caltech faculty members and alumni through the years.

The successful endeavors of Institute faculty, research staff, and students give rise to the big ideas that change our world. There's only one Caltech. Please visit the following sources for more information on this exciting

announcement and the current research activities of Caltech's
Division of
Chemistry and Chemical Engineering.

Caltech press release:
http://pr.caltech.edu/media/Press Releases/PR12748.html
Nobel Prize website:
http://nobelprize.org/chemistry/laureates/2005/index.html
Caltech Division of Chemistry and Chemical Engineering:
http://www.cce.caltech.edu/
"There's only one. Caltech": http://one.caltech.edu

If you have any questions or would like more information about Dr.
Grubbs'
award, please don't hesitate to contact me.

Robyn Puntch
Director, Principal and Major Gifts
California Institute of Technology
rpuntch@dar.caltech.edu

I am hopeful that the report will be of interest to you.

Best regards,

[illegible] alleus [illegible] nt and [illegible] / NORINCO [illegible] ned with [illegible]

[illegible] mounted

[illegible]

[illegible]	100,000
[illegible]	1,200
[illegible]	5,000
[illegible]	400
[illegible]	700
[illegible]	4,500

[illegible] and the [illegible] production

[illegible] armour

[illegible] Iven [illegible] years

[illegible]

Panel

The future of Future Studies

Ruben Nelson, Chair, Canadian [illegible]

University of Pittsburgh

Office of Technology Management

200 Gardner Steel Conference Center
Thackeray & O'Hara Streets
Pittsburgh, Pennsylvania 15260
412-648-2206
Fax: 412-648-8525
E-mail: boni+@pitt.edu
http://tech-link.tt.pitt.edu

Arthur A. Boni, Ph.D.
Director of
Technology Management

November 15, 2000

Thomas E. D'Ambra, Ph. D.
Chairman and CEO
Albany Molecular Research, Inc.
21 Corporate Circle
Albany, NY 12203

Ref: Letter from D'Ambra to Boni dated September 25, 2000
Letter from Boni to D'Ambra dated October 11, 2000
E-mail from D'Ambra to Boni dated October 19, 2000

Dear Tom:

I want to bring you up to date on the status of the University's reconsideration of the request by you and Dr. Alfred Bader to have Professor Dennis Curran serve as a Director of Fluorous Technologies, Inc. Subsequent to the communications noted above, I brought this issue back to the Office of the Provost and Senior Vice Chancellor, and then to the Entrepreneurial Oversight Committee (EOC). According to University of Pittsburgh Policy 11-02-03, "Commercialization of Inventions Through Independent Companies", any exceptions to this policy must be approved by the EOC and ratified by the Senior Vice Chancellor. Board positions are generally prohibited under the policy and are permitted only in exceptional circumstances. I have shared your letter, your e-mail, and your concerns with all parties so that the issue could be thoroughly and fairly reconsidered.

The University wishes to be very supportive of Fluorous Technologies, Inc. and to act, in every way consistent with our policy, to ensure the success of this company. We value our relationship with Albany Molecular Research, Inc. We share your opinion that Professor Curran is key to the success of the technology. However, the University does not permit Board of Director seats for its faculty under circumstances where the Company is selling product. This policy, which has been approved by the University's Board of Trustees, represents a balance between conflict of interest, liability concerns, and an interest in promoting commercialization of University technology.

Within the bounds of our policy, it is still possible to have the input of Professor Curran and the benefit of his involvement in every aspect of the technology development process. This can be done without encumbering him or the University with respect to

potential conflicts of interest or liabilities. We are happy to agree with you if you wish to appoint Professor Curran as Chair the Scientific Advisory Board. Furthermore, you may also give Professor Curran board visitation rights and he may attend all Board of Director meetings and express his opinions to the voting members of the Board. I have specifically discussed this issue with the Chair of the Department of Chemistry, Professor Craig Wilcox, and with the Dean of the Faculty of Arts and Sciences, Professor John Cooper, and they are in full agreement with this approach.

You also make a point that by not approving a board seat for Professor Curran, the University may somehow be denying Dr. Bader his contractual right to appoint a director of his choosing. We would not want to constrain Dr. Bader in any way with respect to his contractual right, and indeed, he has made his nomination freely. We do not reject his nominee; we are merely working with Professor Curran to ensure that as a full-time faculty member of the University, Professor Curran meets with the requirements of existing University policies and practices.

We want to let you know that we understand your desire to actively involve Professor Curran in the advancement of Fluorous Technologies, Inc. There are many ways to accomplish this objective without Professor Curran assuming a company Board of Directors seat. I remain available to assist in any way with respect to your effort to recruit other board candidates. As you may know, I have been recovering from a severely herniated disc and therefore been limited as to my availability. However, I am now beginning to take on limited travel and will attempt to make myself available to work with you.

Sincerely,

Arthur A. Boni

cc: Drs. Don Kuhla, Alfred Bader, and Dennis Curran

ALFRED BADER FINE ARTS

DR. ALFRED BADER

ESTABLISHED 1961

Dear Professor Paleta,

Thank you for your e-mail of today.

Isabel and I look forward to being with you at 11 a.m. on June 16th.

Best wishes,
Alfred Bader

"Oldøich Paleta" wrote:

> Dear Dr. Bader,
>
> thank you much for your kindness.
> I would suggest to meet you on Friday, June 16 at 11:oo a.m.
> at the Secretariat of the Czech Chemical Society, which is placed at
> "Novotného lávka 5" street (parallel to Charles Bridge in direction to
> National Theater),
> phone number 2222 0184, secretary Dipl.Ing. Markéta BLAHOVA.
> The Society has invited you (both of course) for a lunch in the restaurant
> that is placed in the same building from which is view to Charles Bridge).
>
> With best wishes,
>
> Oldrich Paleta
>
> Alfred Bader wrote:
>
> > Dear Professor Paleta,
> > Indeed I will be staying in Prague from June 14th to the 18th and will
> > be happy to visit you. This time we will be staying in a different
> > apartment at Naprstkova street No. 10, where the telephone number is
>
> > 02-22-22 1144.
> > I do hope that your health has really improved.
> > With all good wishes,
> > Alfred Bader

By Appointment Only
ASTOR HOTEL SUITE 622
924 EAST JUNEAU AVENUE
MILWAUKEE WISCONSIN USA 53202
TEL 414 277-0730 FAX 414 277-0709
E-mail: baderfa@execpc.com

philip plvawer

From: [illegible]
To: [illegible]
Sent: [illegible]
Subject: [illegible]

Dear [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] Inc.
[illegible]
[illegible]

Subject: Anniversary motto
Date: Mon, 14 May 2001 11:00:20 -0800
From: apavlath@pw.usda.gov (Attila Pavlath)
To: baderfa@execpc.com

Dear Al,
I must apologize that I did not answer your letter earlier, but the Presidency is taking a lots of time. Last year while I was only the President-elect, I travelled 165,000 miles. This year I am already at 60,000 miles (not counting driving and train) and I spent 62 days on the road out of 115 days. I am not complaining, because I knew what I was getting into. Naturally, I could be a do-nothing President, but this is not the Hungarian way.
The motto which was selected is:
"ACS, all the elements for success.
This was not my first choice, but it was a democratic vote by the Anniversary Committee and I was outvoted. It is not a bad motto, but I wanted something which is more member-oriented.
I hope to see you in Chicago at the ACS meeting in August.
With best regards
Attila

==
Attila E. Pavlath
Western Regional Research Center
800 Buchanan
Albany, CA 94710
Tel.: 510-559-5620
FAX: 510-559-5818
Webpage: http://www.pavlath.org
==
BE FAVORABLE TO BOLD BEGINNINGS
Virgil, 30 B.C.
NOW IT IS REALLY TIME FOR A CHANGE
Attila E. Pavlath, ACS President 2001
==

Promega

September 12, 2005

Dear Promega Shareholder:

I am pleased to inform you that Promega and Hoffmann-La Roche have reached a final settlement with regard to all outstanding litigation between the two firms. As you recall, the dispute originated over the interpretation of a license that Promega had entered into in 1990. Roche commenced the lawsuit in November 1992, almost 13 years ago.

One of the important terms of the settlement agreement is that both parties must maintain

Dear Mr. Principe,

Dr. Bader left earlier today for the old master sales in New York. He will return to the office on Monday, January 27th. You may expect a call from him then. Thank you for giving us your new telephone number.

Regards,
(Mrs.) Ann Zuehlke, Gallery Manager

LAWRENCE PRINCIPE wrote:

> Dear Dr. Bader,
>
> Thank you very much for your phone message. Please excuse the delay in my responding to you, but I was out of town for a little while. I would be delighted to speak with you about the little guide which Lloyd and I wrote (I hope that you enjoyed it). I will be in my office most afternoons next week--although I do now have a new phone number: 410-516-4807. If you prefer, I would be pleased to call you as well, if you would let me know a time which is convenient for you.
>
> With all best wishes,
>
> Larry Principe

Subject: FW: Chemical Heritage publication : Principe address
From: "DeWitt, Lloyd" <ldewitt@philamuseum.org>
Date: Tue, 7 Jan 2003 12:16:28 -0500
To: baderfa@execpc.com

Dear Dr. Bader:
Its Lloyd DeWitt here, fromt the Philadelphia Museum. Thank-you again for the kind comments on the text of the CHF guidebook Transmutations. I had promised to get you contact information for Prof. Lawrence Principe, the co-author of the guidebook, who researches the history of Alchemy. He remembers you many years ago, touring a chemistry department where he was a grad student (U. Indiana?) saying "What can we do better?".

His phone number at Hopkins is: (410) 516-7280 and his email address is lmafp@jhu.edu

I talked to my brother Dave this (Tuesday) morning, to discuss dates for a possible visit. I look forward very much to meeting with you about Lievens and seeing some of your wonderful pictures!

Lloyd DeWitt
Curatorial Fellow
European Painting
Philadelphia Museum of Art
215-684-7222

VAUKEE J

Monday, October 15, 1979

3 from US win shares of Nobels

AP and UPI

Stockholm, Sweden — Three Americans, including a chemist who is a consultant to a Milwaukee firm, won 1979 Nobel Prizes Monday.

Two Harvard professors, physicists Sheldon L. Glashow and Steven Weinberg, both 46, shared the Nobel Prize for physics with a Pakistani for studies aimed at unlocking the mystery of forces that hold matter together. They believe their work may eventually find a single force underlying the universe.

Herbert C. Brown

The prize for chemistry was awarded to Herbert C. Brown of Purdue University. He shares the prize with a West German, Georg Wittig, for their development of the use of boron- and phosphorus-con-

Turn to Nobel, page 10

6

Edition

Nobel

3 Americans win Nobels

From Page 1

taining compounds as important reagents in organic syntheses.

Glashow, Weinberg and Brown are the second, third and fourth Americans to win Nobels this year. The physics and chemistry prizes carry a cash award of $193,000 each.

The medicine prize, the only other one announced thus far, was awarded last week to physicist Allan McLeod Cormack of Tufts University and British research engineer Godfrey Newbold Hounsfield for developing the X-ray technique known as computer-assisted tomography.

Brown, a naturalized American citizen born in London, is Weatherhill research professor in chemistry at Purdue in West Lafayette, Ind., Wittig is professor emeritus of organic chemistry at the University of Heidelberg.

Rearrangement Reaction

Wittig, 82, discovered the rearrangement reaction that bears his name. The Wittig reaction is a method of producing olefins — a kind of hydrocarbon. The theory has been used industrially to synthesize vitamin A.

Brown, 67, developed new reagents, things which convert one substance into another, using boron.

[He is a consultant for Milwaukee's Aldrich Chemical Co., 940 W. St. Paul Ave., which established a plant south of Sheboygan to produce his inventions. Aldrich has purchased all of Brown's patents having to do with the boron process.]

One of his reagents is sodium boro-hydride, which has become the reagent of choice for reduction of carbonyl compounds.

Boron is a nonmetallic element occurring only in combination, as with sodium and oxygen in Borax. Its compounds are used in the preparation of boric acid, soaps, water softeners, enamels and glass.

Sharing the physics award with Glashow and Weinberg is Abdus Salam, 53, professor of theoretical physics at the Imperial College of Sciences and Technology in London and director of the International Center for Theoretical Physics at Trieste, Italy. He is the first Pakistani to win a Nobel Prize.

In making the award, the Swedish Royal Academy of Sciences cited the three "for their contributions to the theory of unified weak and electromagnetic interaction between elementary particles, including ... the prediction of the weak neutral current."

"Fundamental simplicity"

Glashow, reached at his home in Brookline, Mass., told a reporter their work was "not something that is apt to give some practical applications anytime soon." But it has practical spinoffs in understanding radioactivity and how the sun produces energy.

"We suspect that there is at root a fundamental simplicity of nature," Glashow said. Their theory is that "the particles of nature are held together by four different forces, of which one is gravity. That's what Einstein spent his life on. The other three are weak, strong and electromagnetic."

The goal now is to show that the other three are "three different facets of the same thing" — a single underlying force of which they are simply manifestations.

The Nobel Prize in economics is to be announced Tuesday and the peace and literature prizes later this week. President Carter has been nominated

ILWAUKEE SENTINEL

Part 2

Tuesday, October 16, 1979

HERBERT C. BROWN — AP

Nobel prize pick linked to Aldrich

Herbert C. Brown, named Monday as a Nobel prize winner in chemistry, is a consultant to the Aldrich Chemical Co. and a former member of board of the Milwaukee company.

An Aldrich plant in Sheboygan County was set up two years ago to commercialize Brown's inventions in the field of hydroboration, synthesizing complex organic molecules.

"He is the father of hydroboration," said Alfred Bader, president of Aldrich.

"We bought a plant in Sheboygan County in the Town of Wilson and dedicated it entirely to commercializing Brown's inventions," Bader said.

Brown, 67, a Purdue University chemistry professor and a consultant to the Exxon Corp.'s research and engineering facility in Linden, N.J., shares his Nobel prize with Georg Wittig of West Germany. Both did work in the area of organic synthesis of compounds.

The Swedish Academy said Brown and Wittig's prize-winning work opened new possibilities for the linking of carbon atoms.

"Every organic chemist in the world knows him," Bader said. "It has been a miracle to us that he has not won a Nobel prize before this because his work has been outstanding."

Bader said Brown came to Aldrich in the early 1970s with inventions and patents he had in the area of hydroboration.

"I knew him by reputation. He had gone to some big eastern companies to see about commercializing his patents, but they did not want his work," Bader said.

Aldrich became immediately interested.

"He has a great many patents and we purchased all of them on hydroboration. He keeps inventing and we keep patenting," Bader said.

Brown gave up his position on the Aldrich board because he wanted to spend more time inventing and less time in the running of the company.

Bader said Brown frequently comes to Milwaukee and was here last week to meet with company officials.

Sigma-Aldrich, Inc., is the parent company of Aldrich. The company manufactures and sells 40,000 different chemicals.

Chemicals from the Sheboygan County plant are used exclusively in pharmaceutical reseach, Bader said.

Told of his Nobel prize, Brown said, "It's nice to have received recognition of one's life work. I'm certainly very happy."

"We have known all along that he was a great guy," said Bader. "We ae delighted to share his happiness."

ALDRICHEM MIL

017484C288 1400EST

ZCZC 001 MILWAUKEE WISCONSIN OCT 15
PROFESSOR HERBERT C. BROWN
DEPT. OF CHEM
PURDUE UNIVERSITY
WEST LAFAYETTE
INDIANA 47907

ALL OF US ARE OVERJOYED THAT YOUR SCIENTIFIC CONTRIBUTIONS TO
MANKIND HAVE BEEN ACKNOWLEDGED SO APTLY
THE ENTIRE SIGMA ALDRICH FAMILY
CLINT LANE
HARVEY HOPPS
BERNIE EDELSTEIN
IKE KLUNDT
MARVIN KLITSNER
ALFRED BADER
TLX 26843

WU INFOMASTER
+
ALDRICHEM MIL

014795C289 1252EST

ZCZC 002 MILWAUKEE WISCONSIN OCT 16
PMS PROFESSOR D. DAVENPORT
DEPARTMENT OF CHEMISTRY
PURDUE UNIVERSITY
WEST LAFAYETTE, INDIANA 47907
BT
PLEASE GIVE THIS TELEGRAM PERSONALLY TO PROFESSOR BROWN
MY DEAR HERBERT,
MAY I ADD MY PERSONAL GOOD WISHES TO THE MANY THAT ARE STREAMING TO
YOU TODAY. THE PEOPLE CLOSE TO YOU HAVE KNOWN FOR MANY YEARS THAT
YOU ARE ONE OF THE WORLD'S GREATEST CHEMISTS. NOW THE WHOLE WORLD
KNOWS IT.
MAY YOU AND SARAH BE ABLE TO ENJOY THIS FOR MANY YEARS TO COME.
AS I SAID AT THE TIME OF YOUR MYTHICAL RETIREMENT: MAY YOUR EYES
NOT BE DIM NOR YOUR NATURAL STRENGTH ABATED--TO 120.
FOND REGARDS
ALFRED BADER
TLX 26843

NNNN
+

ACCEPTED
00002

1-PC

University of Pittsburgh

Faculty of Arts and Sciences
Department of Chemistry

Chevron Science Center
Pittsburgh, Pennsylvania 15260
412-624-8240
Fax: 412-624-9861
E-mail: curran@pitt.edu

Dennis P. Curran, PhD
Distinguished Service Professor
of Chemistry and
Bayer Professor of Chemistry

August 15, 2005

Dr. Alfred Bader
924 East Juneau
Suite 622
Milwaukee, WI 53202

Dear Alfred:

As part of the process to increase the visibility of fluorous products in association with both FTI and Aldrich, I have recently put together an overview article for the *Acta* that focuses on commercially available products. I thought that you might enjoy seeing a copy, and I enclose a preprint for your review. You will see that the commercial aspects of fluorous chemistry have come a long way over the last couple of years.

I hope that you and Isabel are enjoying your summer, and I thank you for your continued support of FTI.

Best personal regards,

Dennis P. Curran
Distinguished Service Professor and
Bayer Professor of Chemistry

DPC:lcc
Enclosure

Aldrichimica Acta Draft, August 11, 2005

Organic synthesis with light fluorous reagents, reactants, catalysts and scavengers

Dennis P. Curran

Department of Chemistry, University of Pittsburgh, Pittsburgh, PA 15260

Email: curran@pitt.edu

Outline

9. References and Notes

1. Introduction

The need for rapid synthesis of small organic molecules in high purity has spawned a number of new approaches to reaction and separation in recent years.[1] Among these, the fluorous approach has emerged as an especially general and powerful alternative to traditional solution phase synthesis and to solid phase synthesis because it unites many of the most attractive features of both approaches.[2,3] This review covers recent academic and commercial developments in the use of light fluorous reagents, reactants catalysts and scavengers for the synthesis of small organic molecules and biomolecules.[4] The use of light fluorous components in small molecule organic reactions is typically coupled with a separation based on fluorous solid phase extraction.[5]

In 1994, Horvath and Rabai launched the fluorous field with the introduction of "Fluorous Biphasic Catalysis".[6,7] This liquid phase catalyst immobilization technique uses "heavy fluorous molecules" containing large numbers of fluorines (often 63 or more) to impart high partition coefficients of these molecules out of organic solvents and into fluorous solvents. The fluorine atoms are sported on multiple tags (often also called ponytails) that comprise perfluorohexyl (C_6F_{13}), perfluorooctyl (C_8F_{17}) or other perfluorinated or highly fluorinated groups. Spacers (for example, $(CH_2)_n$) are often present to insulate the reactive functionality from the perfluoroalkyl group. Research on methods of fluorous biphasic catalysis for large scale synthesis has flourished in recent years, and the techniques show excellent promise for industrial applications in chemical processes.

Introduced by our group in 1999,[8] "light fluorous chemistry" is more commonly applied in small scale, discovery-oriented synthesis in applications like drug discovery and natural products synthesis. Light fluorous molecules often bear a single perfluorohexyl or (more commonly) perfluorooctyl group. Such molecules have low solubility in fluorocarbon solvents[9] and high solubility in many organic solvents, so traditional liquid-liquid extractions are not efficient. But light fluorous molecules can readily and reliably be separated from organic molecules by the simple technique of fluorous solid phase extraction (see Section 3).

2. *Features of Light Fluorous Chemistry*

Organic synthesis involves reaction, separation, identification and analysis, and light fluorous chemistry provides attractive features at each of these key stages. In the reaction stage, light fluorous molecules are often soluble in a broad range of common organic reaction solvents, and this leads to clean solution phase reactions with standard kinetics and reliable scalability. Generally speaking, there is little or no "reaction development" with light fluorous reagents and catalysts—one simply hijacks conditions for the traditional non-fluorous reaction variant and uses these without modification. Light fluorous techniques are compatible with standard lab equipment and glassware, and they can be used synergistically with techniques such as supercritical carbon dioxide reactions and instruments such as microwave reactors. Fluoroalkyl groups are highly inert, so fluorous tags outshine all other classes of tags in terms of chemical stability to reactions of all types.

In the separation stage of a synthetic process, fluorous tags impress because they expand rather than limit separation options. In synthesis with light fluorous molecules, the preferred separation technique is fluorous solid phase extraction, but all traditional separation techniques

such as crystallization, distillation and standard chromatography remain in play. Contrast this to solid phase synthesis, where all the traditional techniques are replaced by the single technique of filtration. This is fine if filtration does the job, but there are no options if it does not. If desired, the fluorous components of a synthetic reaction can almost always be recovered and recycled.

In identification and analysis, fluorous techniques involve discrete molecules, not materials, so all traditional small molecule techniques are open to use. Reaction mixtures and products can be analyzed by standard tlc and hplc techniques, and again both fluorous and non-fluorous options are available. Even gc is a powerful option for analysis of light fluorous compounds because of their stability and volatility. For identification, solution phase variants of standard spectroscopic techniques like NMR and IR spectroscopy are directly applicable. Fluorous molecules are compatible with the full range of modern small molecule mass spectrometric techniques.

In short, if you are practitioner of small molecule organic synthesis, then you already know all of the experimental, analytical and instrumental techniques that you need to know for light fluorous synthesis, with the possible exception of…

3. Fluorous Solid Phase Extraction

Fluorous solid phase extraction (sometimes abbreviated fspe) is a simple experimental technique that resembles chromatography, but with key differences. Instead of using a standard stationary phase like silica gel, fspe uses silica gel with a fluorocarbon (or other fluorous) bonded phase. Fluorous silica gel has the property of selectively retaining poly-fluorinated molecules, and this allows for a simple bifurication of reaction mixtures containing fluorous and organic (non-fluorous) reaction components.

Figure 1 shows a photograph of the stages of a fluorous solid phase extraction with two dyes. A mixture containing organic (blue) and fluorous (orange) dyes is loaded onto the fluorous silica gel and the column is eluted in a first "organic pass" with a fluorophobic solvent like aqueous acetonitrile or methanol. Water is the fluorophobic solvent *par excellance*, so only small amounts (5-20%) are typically added to the organic solvent. During this organic pass, the fluorous components are extracted (adsorbed) onto the silica gel while the organic components are extracted off. Then a second "fluorous pass" with a fluorophilic solvent (ether and THF are commonly used, among many others) extracts the fluorous components off of the column.

- Figure 1 here -

These solid phase extractions are fast, powerful and reliable, but perhaps most importantly, they are generic. In other words, many different types of organic and light fluorous molecules can be separated by substantially the same method. These features recommend fspe for standard "one at a time" synthesis of organic molecules, as well as for manual or automated parallel synthesis.

From a practical standpoint, fluorous silica gel suitable for fspe's is sold by Fluorous Technologies, Inc. (FTI) under the trade name Fluoro*Flash* in loose format as well as in assorted tubes for individual solid phase extractions and plates for parallel ones. Fluorous columns suitable for flash chromatographic use in popular instruments are available. Also, fluorous tlc plates and hplc columns find use in analysis of products both before and after fspe. While a number of publications from our group describe experimental features of the technique, the best single source for detailed information on how to execute a successful fspe in the laboratory is contained in an application note that can be downloaded without charge from the FTI web site.[10]

Finally, we have very recently introduced the technique of reverse fluorous solid phase extraction.[11] Here the roles of the liquid and solid phases in a standard fspe are reversed—a polar stationary phase (standard silica gel) is used with a (partially) fluorous mobile phase. The technique is nascent, and it may not have the generality of standard fspe, but it is simple to test by tlc and simple to execute, so its use as a complement to fspe merits consideration.

4. Examples of Light Fluorous Reactions and Reaction Components

Despite its relative youth, the field of light fluorous chemistry has expanded rapidly and a comprehensive treatment is already beyond the scope of short review. In the following sections, I instead highlight topical areas where light fluorous chemistry has been used with illustrative reactions and reagents. This high level overview is intended to give the reader a grasp of the many possibilities and applications. Many, though certainly not all, of the fluorous reagents, reactants, catalyst and scavengers shown below are now commercially available, and both the original literature and commercial application notes[10] provide extensive experimental details.

4.1 Light Fluorous Reagents

Reactions that use fluorous reagents to promote the transformation of a small molecule substrate into a new product are probably the most common among all classes of light fluorous reactions. Two prototypical examples, a Mitsunobu reaction[12] and a Staudinger reaction,[13] are shown in Scheme 1. The Mitsunobu reaction is a rather challenging one between a nucleophile of relatively low acidity (*p*-methoxyphenol, **1**) and a 2°-alcohol (2-nonanol, **2**). The coupling is effected in solution by a combination of a fluorous Mitsunobu reagent (^{F}DIAD) and a fluorous phosphine (^{F}TPP-1) under the standard conditions of solvent, temperature and time for the

traditional Mitsunobu reaction. Simple fluorous solid phase extraction then provides the coupled aryl alkyl ether **3** from the organic pass along with the spent reagents from the fluorous pass. The Staudinger reduction of azide **4** to amine **5** is comparably facile. Both the Mitsunobu and the Staudinger reaction have good generality, and the generic nature of the separation is especially attractive in parallel synthesis applications.

- Scheme 1 here -

The relative reactivity of light fluorous reagents compared to traditional reagents has not often been studied in detail; however, reactions of fluorous phosphines are a significant exception. A series of light, medium and heavy fluorous triarylphosphines exhibited comparable reactivities to triphenylphosphine in an assortment of typical phosphine reactions.[14] This study supports the assumption that the reactivities of light fluorous reagents will be readily predictable from data on their non-fluorous relatives.

Figure 2 shows an assortment of known fluorous reagents including phosphines, organic tin reagents and catalysts, selenenic acids, ketones, hypervalent iodine reagents and sulfoxides. The phosphines have many and varied uses,[15] while the tin reagents[16] have been used for radical and ionic reductions and allylations as well as for azide displacements. The tin oxides promote selective functionalization reactions of diols[17] while the ladder distannoxanes are excellent esterification catalysts.[18] The selenenic acids are powerful oxygenating reagents,[19,20] while the ketones can be used for in situ dioxirane generation.[21] The hypervalent iodine reagents[22] promote many kinds of interesting oxidations, while the sulfoxide can be used in a odorless variant of the Swern oxidation.[23]

- Figure 2 here -

The activation and coupling of acids for reaction with nucleophiles to make amides, esters and related molecules is arguably the most common reaction class in drug discovery research, and is important in many other areas as well. An assortment of fluorous reagents, some of which are newly minted, are available to conduct these types of transformations. For example, coupling of acids with both amines and alcohols by using Mukaiyama's pyridinium reagent is a powerful transformation that is underused, perhaps because of the problems with removing the reagent -derived pyridone byproduct. However, the fluorous Mukaiyama reagent[24] promotes smooth coupling and the derived pyridone can readily be removed from the product, as illustrated by the example in Scheme 2. Figure 3 shows an assortment of fluorous variants of popular coupling reagents[25] that are also now available for amide, ester and other coupling reactions.

- Scheme 2 and Figure 3 here -

4.2 Organometallic Catalysts with Fluorous Ligands

The use of catalysts rather than stoichiometric reagents to promote organic transformations is increasingly important, and many heavy fluorous catalysts are already known. These are very useful for large scale chemistry, but some reaction development may be needed. However, essentially any heavy fluorous catalyst bearing multiple ponytails can be re-engineered into a light one simply by giving it a haircut to reduce the fluorine content.

Scheme 3 (top) shows several examples of fluorous ruthenium and palladium catalysts. First- and second-generation fluorous Grubbs-Hoveyda catalysts[26] are crystalline solids that promote metathesis reactions under standard conditions. Separation and recovery of both the product and the catalyst are readily achieved by fspe as illustrated by the ring-closing metathesis

of **9** to give **10**. The fluorous pincer complex (Scheme 3, bottom) efficiently promotes Heck reactions like the addition of **11** to **12** to give **13** under standard thermal conditions, or even more rapidly and conveniently by microwave heating.[27] Bifurification of the product mixtures as usual by fspe provides the Heck products along with the recovered complex. However, the results suggest that the complex does not catalyze the reactions, but instead leaches very small amounts of highly active palladium metal into the reaction medium. As such, the pincer complex can be considered as a reusable catalyst reservoir. Fluorous nickel catalysts can also be recovered and recycled by fpse.[28]

- Scheme 3 here -

4.3 Fluorous Scavengers

Scavenging is a popular technique in medicinal chemistry and allied areas for cleaning up crude reaction mixtures in which one of the key reaction components has been used in excess to promote a rapid, high-yielding reaction. The clean solution phase kinetics and the ease of separation by fspe recommend fluorous scavengers for general applications, and indeed a number of scavenging applications have been described.[29] Figure 4 shows representative examples of scavengers for nucleophiles,[30] electrophiles[31] and trace metals.[32]

- Figure 4 here -

Scheme 4 shows a typical use of a fluorous electrophilic scavenger in an isocyanate functionalization reaction. Reaction of excess amine **14** with a limiting amount of phenyl isocyanate **15** is followed by addition of a fluorous isocyanate to scavenge the excess amine. Fluorous solid phase extraction then provided the pure urea **16** from the organic pass, while the fluorous pass with the scavenged urea **17** is usually discarded. The tables can be turned by using

excess isocyanate to derivatize a limiting amount of amine; in this case a fluorous amine scavenger is used to scavenge the remaining isocyanate.

- Scheme 4 here -

4.4 Fluorous Protecting Groups

In multi-step synthesis, it is often attractive to use substrates bearing fluorous protecting groups (sometime called tags or labels) along with traditional non-tagged reagents. A single fluorous protecting group renders a subsequent series of individual compounds fluorous and this makes each succeeding reaction product susceptible to the same convenient fluorous solid phase extraction.[33] Of course, the convenience is further amplified in parallel synthesis.

Scheme 5 shows two typical examples of amide coupling reactions with fluorous-tagged amines (for amide coupling reactions with fluorous reagents, see Section 4.1). In the upper example, amino acid **18** bearing a fluorous *t*-butyloxycarbonyl (^{F}Boc) group is coupled with excess tetrahydroisoquinoline **19**, EDCI and HOBt in dichloromethane.[34] Fluorous solid phase extraction removes all the excess and spent reagents in the organic pass and provides the coupled product **20** in the fluorous pass.

- Scheme 5 here -

The variant with the fluorous carbobenzyloxy (^{F}CBz) groups in the lower part of Scheme 5 illustrates the extension to quasiracemic synthesis.[35] Here, (L)-phenylalanine (Phe) is tagged with an ^{F}CBz group bearing a C_8F_{17} group, where (D)-Phe gets a shorter C_6F_{13} tag.[36] The mixture **21** is not a true racemate—hence the name quasiracemate—because the compounds are not isomers. But it behaves like a racemate in most respects. Except, of course, if a fluorous separation is applied. Coupling of the quasiracemate **21** with tetrahydroisoquinoline **19** as above

is followed by charging the crude product onto an fspe column and subsequent fluorophobic pass to elute the non-fluorous reagent- and reactant-derived products. Now switching to a flash chromatographic mode on the same fspe column with standard fraction collection provides in earlier fractions the (D)-quasienantiomer **22** of the product and in later fractions, the (L)-quasienantiomer **23**.

This process of resolving products based on fluorous tag size is called demixing or sorting, and it is usually applied after a multi-step sequence to pull out individual compounds from a differentially tagged mixture in a process called fluorous mixture synthesis.[37] In quasiracemic synthesis—the simplest of fluorous mixture synthesis techniques—a pair of enantiomers are tagged with different fluorous tags.[35] But it is also possible to tag diastereomers and even analogs (non-isomers) for fluorous mixture synthesis. These techniques all leverage a synthesis by providing more compounds per unit effort.

Many other light fluorous protecting groups have been introduced recently, and a selection of reagents that are used to install these groups is shown in Figure 5. In addition to nitrogen protecting groups like ^{F}Boc, ^{F}CBz and ^{F}Fmoc,[38] there are also silane,[39] benzyl,[40] PMB[41] and THP[42] protecting groups for alcohols, a ketone protecting group[43] for diols, and a sulfonate for phenols. A number of these groups double as protecting groups for acids, and fluorous alcohols[44] also serve this function. The FluoMar reagent, a fluorous analog of the Marshall resin, doubles as both a protecting group during a synthesis stage and an activating group for subsequent displacement.[25c] These and other groups provide a broad spectrum of opportunities for rapid and efficient multi-step synthesis under solution phase conditions.

- Figure 5 here -

4.5 Fluorous Tagging in Multicomponent Reactions and Heterocycle Synthesis

A second use of fluorous substrate tagging methods is in multicomponent reactions, especially those directed towards pharmaceutically relevant heterocycles.[3c] The use of a key fluorous-tagged component as the limiting reagent in a multicomponent reaction with other reagents in excess allows one to quickly isolate the tagged product away from what can often be complex mixtures containing unreacted reagents and products derived from partial combination of several but not all of the reaction components. Following the multi-component reaction, the tag is generally displaced in a cyclization (cyclative cleavage) or is replaced by a proton (traceless tag) or even more valuably by another diversity element in a phase switch that provides a further purification gate for removing undesired products.[45]

Scheme 6 illustrates a simple two step synthesis of diverse quinoxalinones **26** that pairs the advantages of fluorous tagging with microwave reactions.[46] An initial Ugi reaction with mono ^{F}Boc protected phenylene diamine **24** as the limiting reagent provides ketoamides **25** after 20 min irradiation and fspe. In this first spe, the products are in the fluorous fraction. Now cleavage with TFA provides products **26** in an excellent state of purity, this time from the organic fraction. Both steps (reaction and separation) can easily be conducted in parallel in less than a half a day. In contrast, a solution phase approach with polymer scavenging required about 3-4 days to conduct the same sequence, in part because a microwave was not used and in part because the polymer-bound scavengers reacted slowly (the scavenging reactions took more time than the target reactions). The use of fluorous tags in place of scavengers, the switch to fully solution phase methods, and the use of microwave considerably expedite the creation of small, high quality libraries by parallel synthesis.

- Scheme 6 here -

5. *Examples of Light Fluorous Reactions and Reaction Components in Biomolecule Synthesis*

Biomolecules like peptides and oligonucleotides are typically made today by solid phase synthesis, so it might seem that fluorous techniques may have no role to play in this important area. But this is not the case. In the long term, solution-based techniques may supplant solid phase synthesis for some kinds of molecules. In the short term, fluorous techniques are already supplementing solid phase syntheses in important ways.

Wipf and coworkers described the first union of solid phase synthesis and fluorous synthesis techniques in a small molecule setting,[47] and applications in both peptide and oligonucleotide synthesis show great promise. For example, van Boom finishes off a standard solid phase peptide synthesis by capping the *N*-terminus with a fluorous CBz or Msc group.[48] Now removal of the product from the solid phase provides a mixture of the target sequence, which is fluorous-tagged, and truncated sequences and other impurities, which are not. This mixture is then purified by fluorous hplc rather than the usual reverse phase hplc, and the fluorous tag functions as a powerful chromatographic shift agent, thereby rendering very easy an otherwise difficult separation. Oligopeptides of superior purity should be generally available by this method.

Pearson and coworkers have simplified the process even further in oligonucleotide synthesis by using a solid phase extraction instead of an hplc.[49] In this approach (Figure 6) a standard solid phase synthesis of a DNA fragment by the phosphoramidite method is finished off by coupling the last oligonucleotide with a fluorous dimethoxytrityl (^{F}DMT) group rather than a standard one. The sample is then removed from the solid phase, and the fluorous-tagged (target) oligonucleotide is separated from the untagged impurities by solid phase extraction with a

Fluoro-Pak cartridge. After the fluorophobic pass to elute the non-tagged impurities, an ammonia solution is added to clip the ^{F}DMT group. At the same time, this elutes the target oligonucleotide off the cartridge, while leaving the residual fluorous protecting group behind.

- Figure 6 here -

The experimental procedure is very simple to conduct yet increases the purity of oligonucleotides from solid phase synthesis significantly. Despite the relatively small size of the fluorous group on the DMT tag, the method can be used for oligos containing as many as 50-100 nucleotides. This shows the unique power of the fluorous interactions. The materials and methods for this technique have recently been commercialized by Berry and Associates.

6. Making Your Own Fluorous Reaction Components

What reagent, catalyst, protecting group, scavenger, etc. do you need for your research tomorrow? Or next week? The field of fluorous chemistry is young, so there is a chance that it's not yet commercially available or even known. Why not consider making your own reagents? You don't need to start with incredibly reactive elemental fluorine. Indeed, in over a decade of fluorous research in our group, we have never had the occasion to do the defining reaction in organofluorine chemistry (the formation of a carbon-fluorine bond).

Figure 7 illustrates some of the most popular building blocks that you can buy in order to minimize your effort. These can be fashioned into a diverse array of new fluorous molecules by using established methods and reactions. A good starting point is the "Handbook of Fluorous Chemistry", which provides additional information on building blocks,[50] presents detailed experimental procedures for a number of important kinds of coupling reactions, and serves as a beacon to the original literature for many more such procedures.

- Figure 7 here -

7. Conclusions

If you are like many other bench chemists, you are probably aware of fluorous chemistry but have not yet tried it yourself. "Not ready for prime time", you might be thinking?

With the recent commercialization efforts by Fluorous Technologies, Inc., Sigma-Aldrich, Berry and Associates, and other companies, you can now buy everything that you need to try interesting transformations. You already know how to do the reactions, and the separation techniques are closely related to familiar chromatographic techniques. You can quickly teach yourself from readily downloadable application notes. Why not give fluorous chemistry a try, either with a commercial reagent or by making your own? I'd like to learn about your experience, so send me an email.

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9. References and Notes

1. Reviews on separation strategies: (a) *Chemtracts—Org. Chem.* **1996,** 9, 75-87. (b) Curran, D. P., *Angew. Chem., Int. Ed. Eng.* **1998,** 37, 1175-1196. (c) Tzschucke, C. C.;

Markert, C.; Bannwarth, W.; Roller, S.; Hebel, A.; Haag, R. *Angew. Chem. Int. Ed.* **2002**, *41*, 3964-4000. (d) Yoshida, J.; Itami, K. *Chem. Rev.* **2002**, *102*, 3693-3716.

2. The most comprehesive current guide to the field is the *Handbook of Fluorous Chemistry*; Gladysz, J. A.; Curran, D. P.; Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004.

3. Selected recent reviews: (a) Curran, D. P. In *Stimulating Concepts in Chemistry*; Vögtle, F., Stoddardt, J. F., Shibasaki, M., Eds.; Wiley-VCH: New York, 2000. (b) Zhang, W. *Tetrahedron* **2003**, *59*, 4475-4489. (c) Zhang, W. *Chem. Rev.* **2004**, *104*, 2531-2556. (d) Otera, J. *Acc. Chem. Res.* **2004**, *37*, 288-296. (e) Horvath, I. T. Aqueous-Phase Organometallic Catalysis (2nd Edition); Cornils, B.; Hermann, W. A., Eds.; Wiley: Weinheim; **2004**, 646-654.

4. Review of light fluorous techniques: Curran, D. P. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp128-155.

5. (a) Curran, D. P. *Synlett* **2001**, 1488-1496. (b) Curran, D. P. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 101-127.

6. Horvath, I. T.; Rabai, J. *Science* **1994**, *266*, 72-75.

7. For are review on fluorous strategies for reagent and catalyst recovery, see: Gladysz, J. A.; Correa de Costa, R. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran,

D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 24-40.

8. Curran, D. P.; Luo, Z. Y. *J. Am. Chem. Soc.* **1999**, *121*, 9069-9072.

9. However, recent developments with new fluorous solvents for extractions suggest that the scope and applicability of liquid-liquid extractions in discovery-oriented synthesis may be considerably underestimated. See, Yu, M. S.; Curran, D. P.; Nagashima, T. *Org. Lett.* **2005**, ASAP.

10. Product Application Note "Fluorous Solid Phase Extraction", at http://fluorous.com/download.html.

11. Matsugi, M.; Curran, D. P. *Org. Lett.* **2004**, *6*, 2717-2720.

12. (a) Dandapani, S.; Curran, D. P. *J. Org. Chem.* **2004**, *69*, 8751-8757. (b) Dandapani, S.; Curran, D. P. *Chem. Eur. J.* **2004**, *10*, 3130-3138. (c) Dembinski, R. *Eur. J. Org. Chem.* **2004**, 2763-2772.

13. Lindsley, C. W.; Zhao, Z.; Newton, R. C.; Leister, W. H.; Strauss, K. A. *Tetrahedron Lett.* **2002**, 4467-4470.

14. Curran, D. P.; Wang, X. A.; Zhang, Q. S. *J. Org. Chem.* **2005**, *70*, 3716-3719.

15. (a) Dandapani, S. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 175-181. (b) Hope, E. G.; Stuart, A. M. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 247-255.

16. (a) Curran, D. P.; Hadida, S. *J. Am. Chem. Soc.* **1996**, *118*, 2531-2532. (b) Ryu, I.;

Niguma, T.; Minakata, S.; Komatsu, M.; Hadida, S.; Curran, D. P. *Tetrahedron Lett.* **1997**, *38*, 7883-7886. (c) Curran, D. P.; Luo, Z.; Degenkolb, P. *Bioorg. Med. Chem. Lett.* **1998**, *8*, 2403-2408. (d) Curran, D. P.; Hadida, S.; Kim, S. Y. *Tetrahedron* **1999**, *55*, 8997-9006. (e) Curran, D. P.; Hadida, S.; Kim, S. Y.; Luo, Z. Y. *J. Am. Chem. Soc.* **1999**, *121*, 6607-6615. (f) Ryu, I. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 182-189.

17. Bucher, B.; Curran, D. P. *Tetrahedron Lett.* **2000**, *41*, 9617-9621.

18. Otera, J. *Acc. Chem. Res.* **2004**, *37*, 288-296.

19. Crich, D.; Zou, Y. K. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 202-221.

20. Crich, D.; Zou, Y. K. *J. Org. Chem.* **2005**, *70*, 3309-3311.

21. (a) Legros, J.; Crousse, B.; BonnetDelpon, D.; Begue, J. P. *Tetrahedron* **2002**, *58*, 3993-3998. (b) van Vliet, M. C. A.; Arends, I. W. C. E.; Sheldon, R. A. *Chem. Commun.* **1999**, 263-264.

22. (a) Lindsley, C. W.; Zhao, Z. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 371-372. (b) Rocaboy, C.; Gladysz, J. A. *Chem. Eur. J.* **2003**, *9*, 88-95.

23. (a) Crich, D.; Neelamkavil, S. *J. Am. Chem. Soc.* **2001**, *123*, 7449-7450. (b) Crich, D.; Neelamkavil, S. *Tetrahedron* **2002**, *58*, 3865-3870.

24. Nagashima, T.; Petro, M. Lu, Y.; Zhang, W. *Tetrahedron Lett.* **2005**, in press.

25. (a) ^{F}CDMT: Markowicz, M. W.; Dembinski, R. *Synthesis* **2004**, 80-86; Zhang, W.; Lu, Y. Nagashima, T., Submitted for publication. (b) ^{F}HOBt: Nagashima, T., unpublished results. (c) FluoMar: Chen, C. H. T.; Zhang, W. *Org. Lett.* **2003**, *5*, 1015-1017. (d) ^{F}DCC: Palomo, C.; Aizpurua, J. M.; Loinaz, I.; Fernandez-Berridi, M. J.; Irusta, L. *Org. Lett.* **2001**, *3*, 2361-2364.

26. Matsugi, M.; Curran, D. P. *J. Org. Chem.* **2005**, *70*, 1636-1642.

27. Curran, D. P.; Fischer, K.; Moura-Letts, G. *Synlett* **2004**, 1379-1382.

28. Croxtall, B.; Hope, E. G.; Stuart, A. M. *Chem. Commun.* **2003**, 2430-2431.

29. Lindsley, C. W.; Leister, W. H. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 236-246.

30. (a) Lindsley, C. W.; Zhao, Z.; Leister, W. H. *Tetrahedron Lett.* **2002**, *43*, 4225-4228. (b) Zhang, W.; Chen, C. H. T.; Nagashima, T. *Tetrahedron Lett.* **2003**, *44*, 2065-68. (c) Werner, S.; Curran, D. P. *Org. Lett.* **2003**, *5*, 3293-3296. (d) Zhang, A. S.; Elmore, C. S.; Egan, M. A.; Melillo, D. G.; Dean, D. C. *J. Label. Comp. Radiopharm.* **2005**, *48*, 203-208.

31. Zhang, W.; Curran, D. P.; Chen, C. H. T. *Tetrahedron* **2002**, *58*, 3871-3875.

32. Fluorous Technologies, Inc., 2005 catalog.

33. (a) Zhang, W. In *The Handbook of Fluorous Chemistry*; Gladysz, J. A., Curran, D. P., Horvath, I. T., Eds.; Wiley-VCH: Weinheim, 2004, pp 222-235. (b) Zhang, W. *Curr.*

Opin. Drug Disc. Dev. **2004**, *7*, 784-797.

34. Luo, Z. Y.; Williams, J.; Read, R. W.; Curran, D. P. *J. Org. Chem.* **2001**, *66*, 4261-4266.

35. (a) Zhang, Q. S.; Rivkin, A.; Curran, D. P. *J. Am. Chem. Soc.* **2002**, *124*, 5774-5781. (b) Zhang, Q. S.; Curran, D. P. *Chem. Eur. J.*, **2005**, in press.

36. Curran, D. P.; Amatore, M.; Guthrie, D.; Campbell, M.; Go, E.; Luo, Z. Y. *J. Org. Chem.* **2003**, *68*, 4643-4647.

37. (a) Luo, Z. Y.; Zhang, Q. S.; Oderaotoshi, Y.; Curran, D. P. *Science* **2001**, *291*, 1766-1769. (b) Zhang, W. *Arkivoc* **2004**, 101-109.

38. Matsugi, M.; Curran, D. P., unpublished results.

39. (a) Zhang, W.; Luo, Z.; Chen, C. H. T.; Curran, D. P. *J. Am. Chem. Soc.* **2002**, *124*, 10443-10450. (b) Rover, S.; Wipf, P. *Tetrahedron Lett.* **1999**, *40*, 5667-5670.

40. Curran, D. P.; Furukawa, T. *Org. Lett.* **2002**, *4*, 2233-2235.

41. Curran, D. P.; Ferritto, R.; Hua, Y. *Tetrahedron Lett.* **1998**, *39*, 4937-4940.

42. (a) Wipf, P.; Reeves, J. T. *Tetrahedron Lett.* **1999**, *40*, 4649-4652. (b) Ethyl vinyl ether variant: Wipf, P.; Reeves, J. T. *Tetrahedron Lett.* **1999**, *40*, 5139-5142. (c) Wipf, P.; Reeves, J. T.; Balachandran, R.; Giuliano, K. A.; Hamel, E.; Day, B. W. *J. Am. Chem. Soc.* **2000**, *122*, 9391-9395.

43. Read, R. W.; Zhang, C. T. *Tetrahedron Lett.* **2003**, *44*, 7045-7047.

44. (a) Pardo, J.; Cobas, A.; Guitian, E.; Castedo, L. *Org. Lett.* **2001**, *3*, 3711-3714. (b)

Zhang, W.; Lu, Y. M. *Org. Lett.* **2003**, *5*, 2555-2558.

45. (a) Examples: Zhang, W. *Org. Lett.* **2003**, *5*, 1011-1013. (b) Zhang, W.; Lu, Y.; Chen, C. H.-T. *Molecular Diversity* **2003**, *7*, 199-202. (c) Nagashima, T.; Zhang, W. *J. Comb. Chem.* **2004**, *6*, 942-949. (d) Zhang, W.; Lu, Y. M.; Geib, S. *Org. Lett.* **2005**, *7*, 2269-2272.

46. Zhang, W.; Tempest, P. *Tetrahedron Lett.* **2004**, *45*, 6757-6760.

47. Wipf, P.; Reaves, J.; Roever, S., US 6,673,539 B1, 2004.

48. (a) Filippov, D. V.; van Zoelen, D. J.; Oldfield, S. P.; van der Marel, G. A.; Overkleeft, H. S.; Drijfhout, J. W.; van Boom, J. H. *Tetrahedron Lett.* **2002**, *43*, 7809-7812. (b) de Visser, P. C.; van Helden, M.; Filippov, D. V.; van der Marel, G. A.; Drijfhout, J. W.; van Boom, J. H.; Noort, D.; Overkleeft, H. S. *Tetrahedron Lett* **2003**, *44*, 9013-9016.

49. Pearson, W. H.; Berry, D. A.; Stoy, P.; Jung, K.-Y.; Sercel, A. D. *J. Org. Chem.* **2005**, ASAP.

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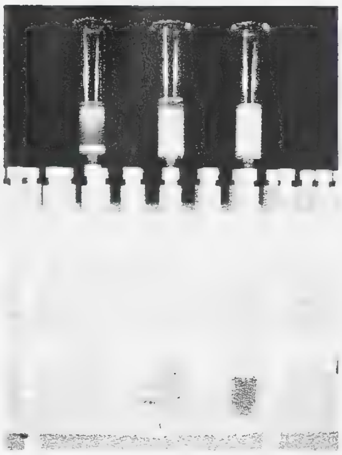
About the Author

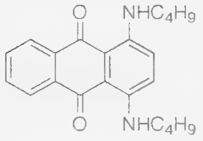
Dennis P. Curran received his B.S. in 1975 from Boston College. His Ph.D. was granted from the University of Rochester in 1979 where he worked under Professor Andrew S. Kende. After a two year postdoctoral stay with Professor Barry M. Trost at the University of Wisconsin, Dr.

Curran joined the faculty of the Chemistry Department at the University of Pittsburgh in 1981. He now holds the ranks of Distinguished Service Professor and Bayer Professor of Chemistry, and is the founder of Fluorous Technologies, Inc. (www.fluorous.com). Dr. Curran has received the Pittsburgh Magazine Innovators Award (2003), American Chemical Society Award for Creativity in Organic Synthesis (2000) and the Cope Scholar Award (1988), and the Janssen Prize for Creativity in Organic Synthesis (1998). He is currently an ISI "Highly Cited Researcher" (www.isihighlycited.com). Dr. Curran has authored over 300 papers, twenty patents and one book. Beyond fluorous chemistry, he is well known for his work on radical reactions in organic synthesis. Additional information is at http://radical.chem.pitt.edu.

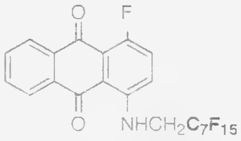


Left tube: loading and beginning of fluorophobic elution with 80/20 MeOH/water
Middle tube: Completion of fluorophobic elution
Right tube: Completion of fluorophilic elution (THF)

The dyes are:



Solvent Blue-35
Aldrich



F-Orange-1
FTI

Figure 1. A Fluorous Solid Phase Extraction of Organic (blue) and Fluorous (orange) Dyes over Fluoro*Flash* Silica Gel

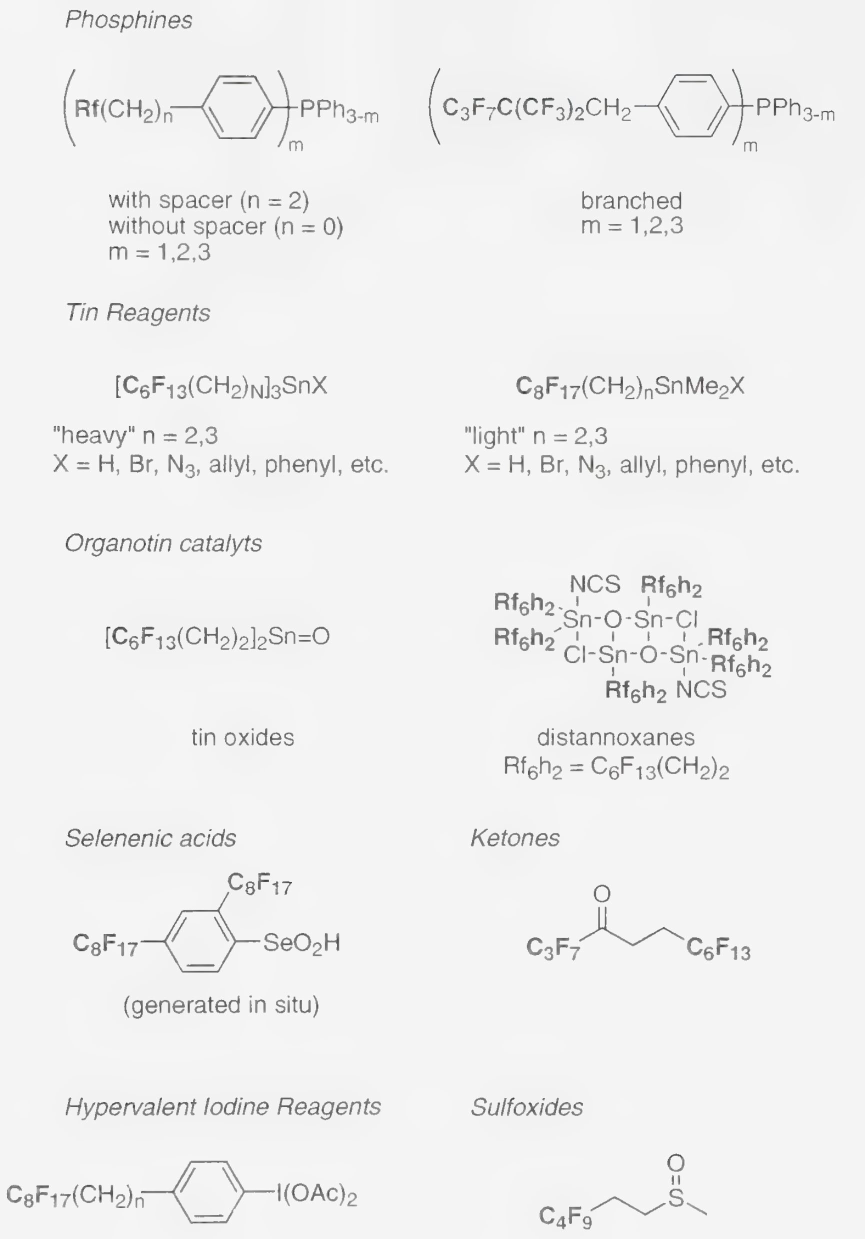
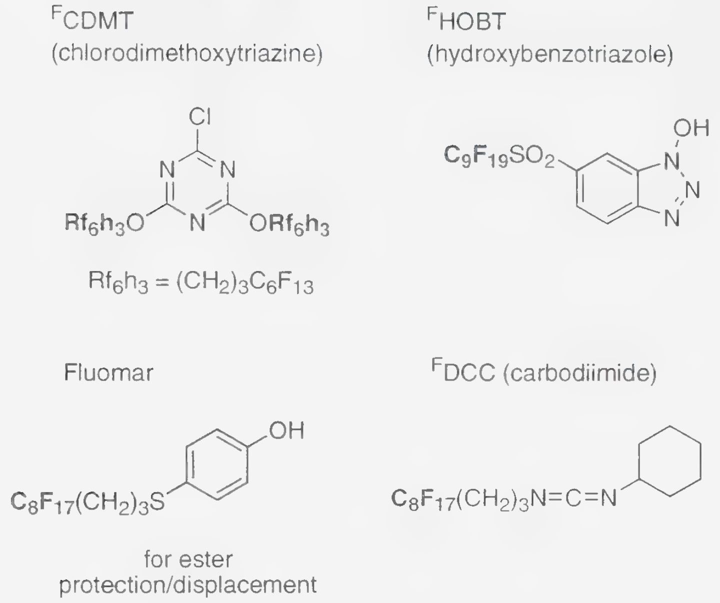


Figure 2. Assorted Fluorous Reagents and Organotin Catalysts



^{F}CDMT
(chlorodimethoxytriazine)

$Rf_6h_3 = (CH_2)_3C_6F_{13}$

^{F}HOBT
(hydroxybenzotriazole)

Fluomar

for ester
protection/displacement

^{F}DCC (carbodiimide)

Figure 3. Fluorous Reagents for Peptide, Amide, and Ester Formation

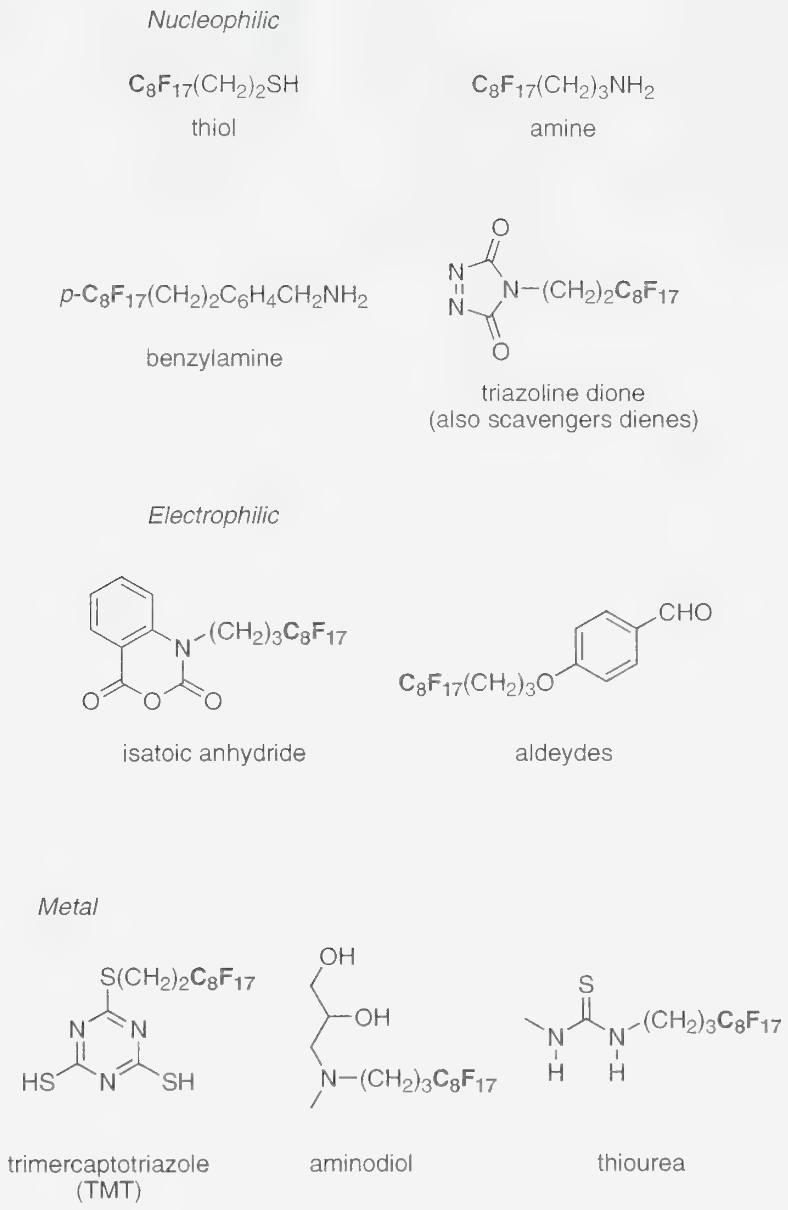


Figure 4. Representative Fluorous Scavengers

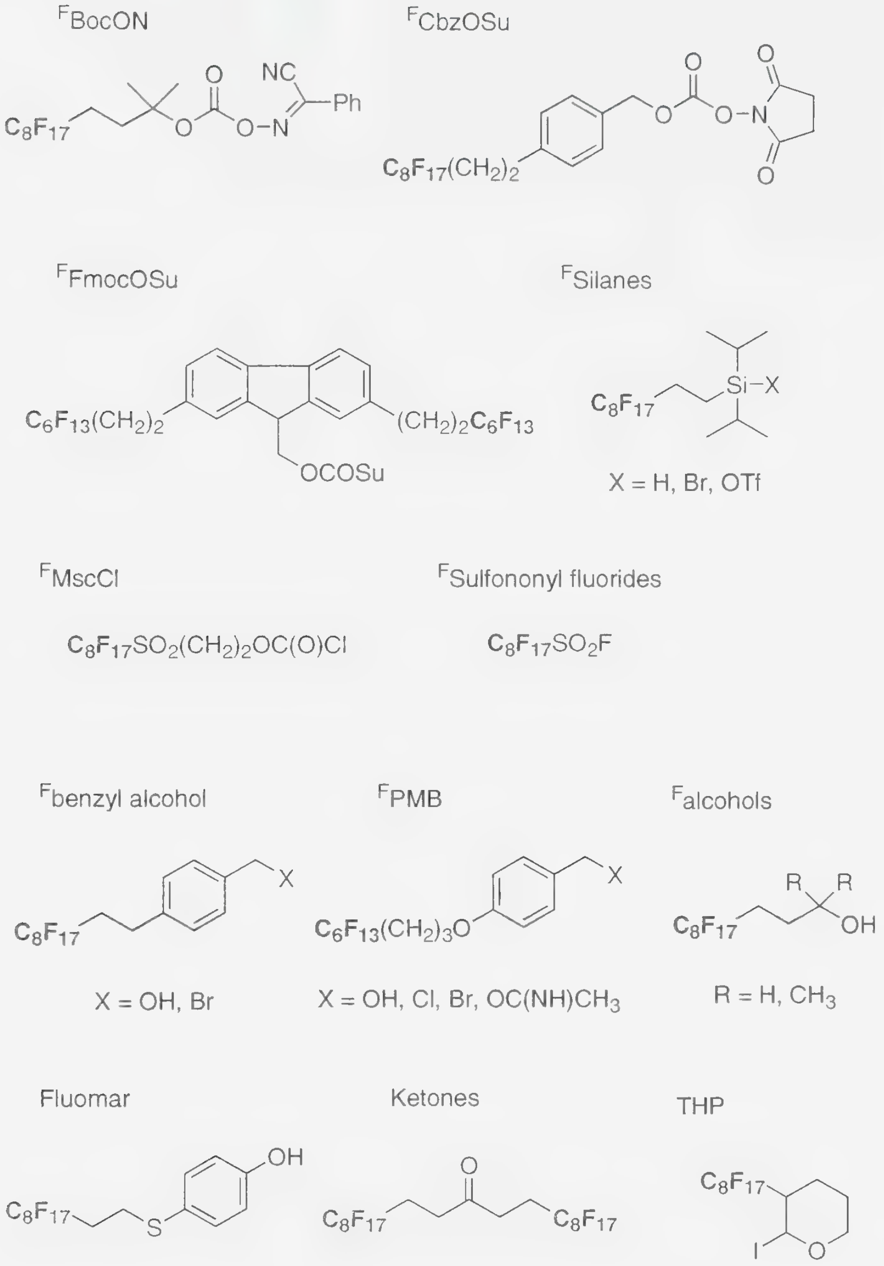


Figure 5. Representative Fluorous Protecting Reagents

5'-FDMT-TTTCTGGTTAAGGTGTGATATGCT
CAGCTACTAATTAACAGTTGTCTAAGCTGGTT
AACGTGAGTAATATGATCAGCTACTATTTAAC
AGTTGTCTT-**CPG**

CPG = controlled pore glass

5'-FDMT-T =

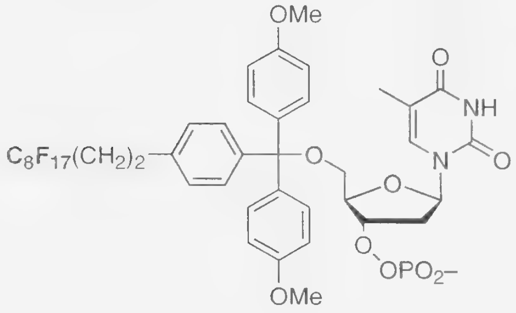


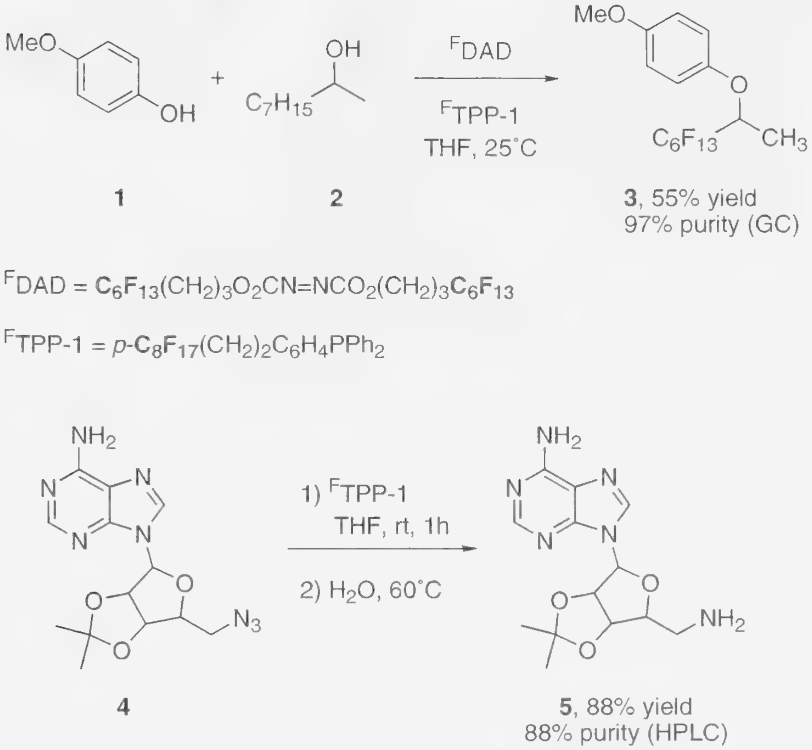
Figure 6. 100-mer Oligonucleolide Made by SPS with Fluorous Tagging and Spe Purification

RfI $Rf(CH_2)_nX$ $RfCO_2H$ $RfCH{=}CH_2$

$X = I, OH, CHO, CO_2H$

$R = Br, CH_2OH, CO_2Et$ $R = Br, CH_2OH, CO_2Et$

Figure 7. Representative Commercially Available Fluorous Building Blocks



1

2

3, 55% yield
97% purity (GC)

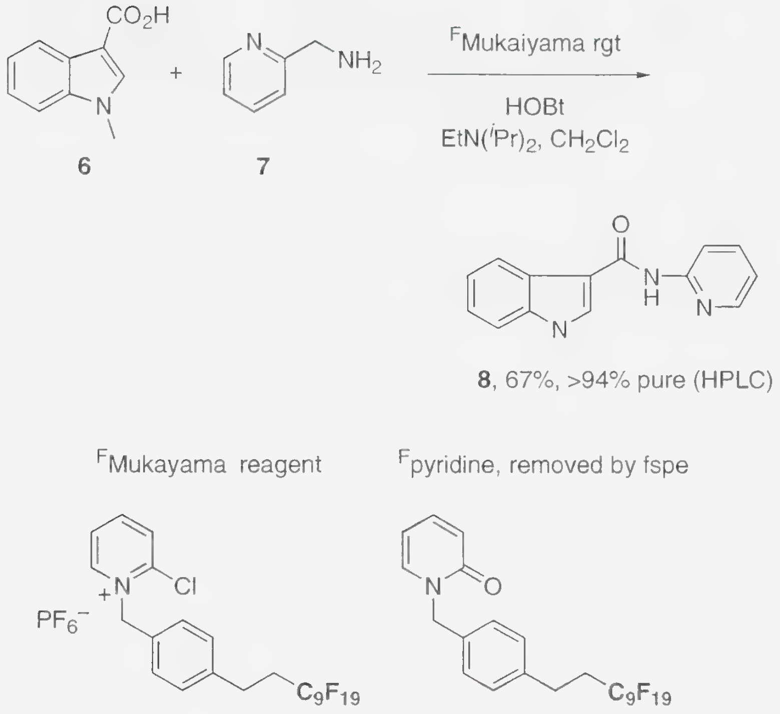
^{F}DAD = $\mathbf{C_6F_{13}}(CH_2)_3O_2CN{=}NCO_2(CH_2)_3\mathbf{C_6F_{13}}$

^{F}TPP-1 = p-$\mathbf{C_8F_{17}}(CH_2)_2C_6H_4PPh_2$

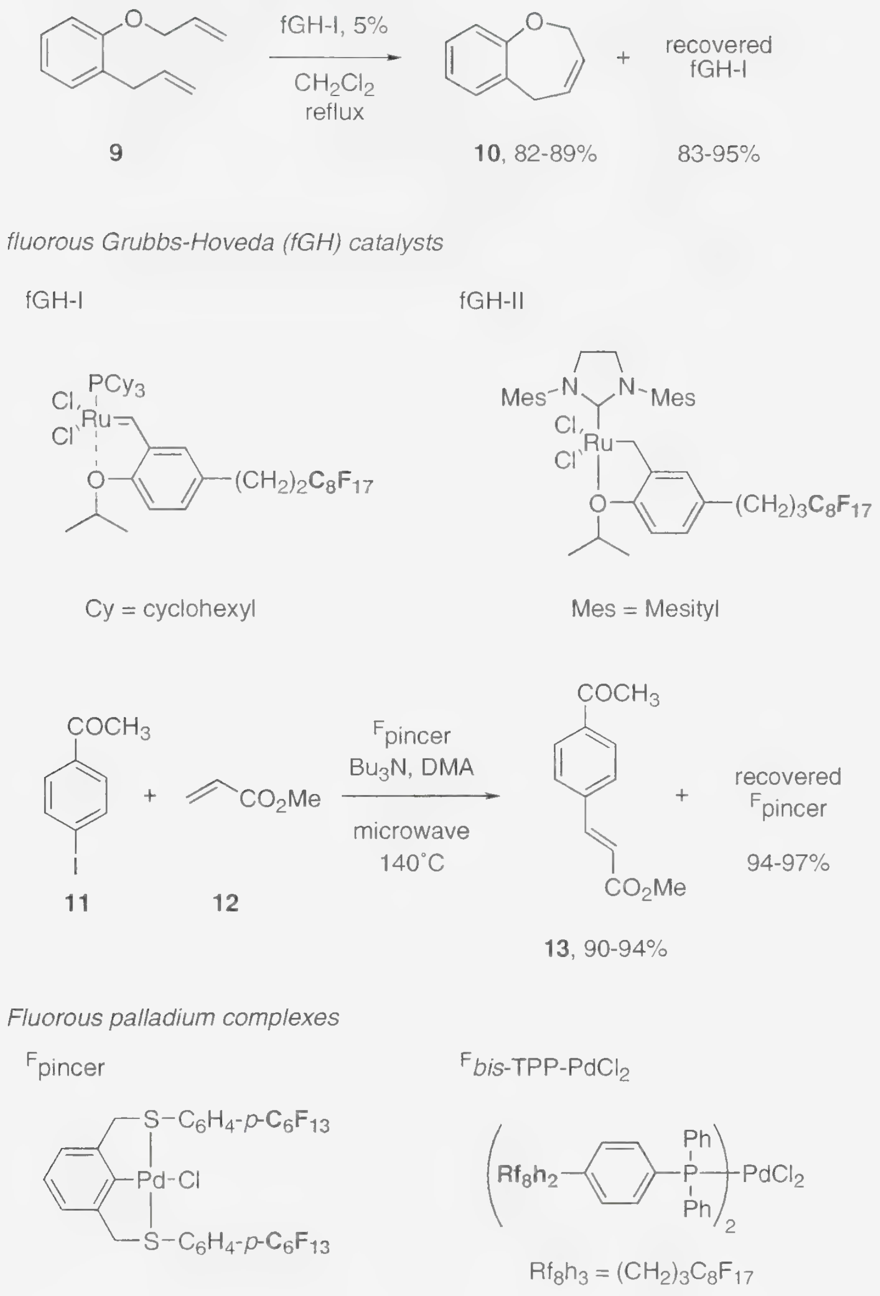
4

5, 88% yield
88% purity (HPLC)

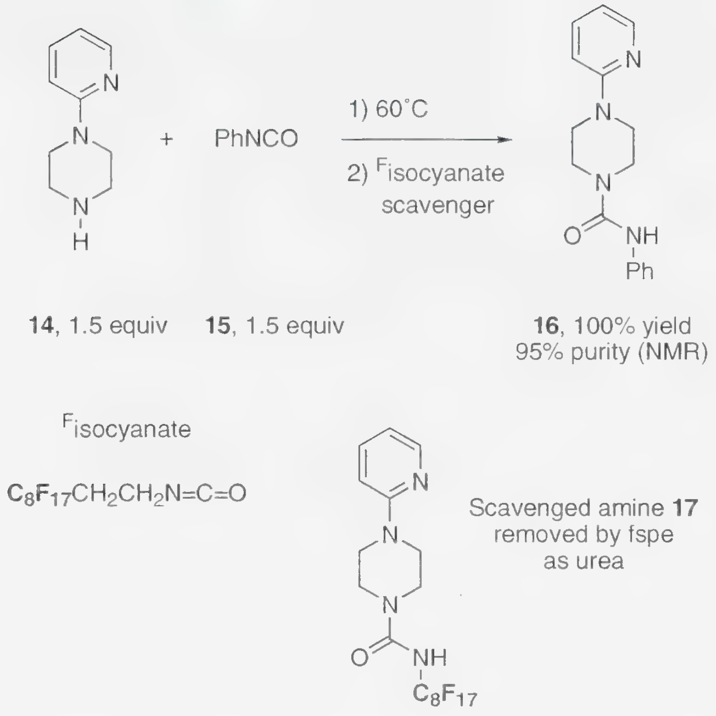
Scheme 1. Light fluorous Mitsunobu (top) and Staudinger (bottom) reactions



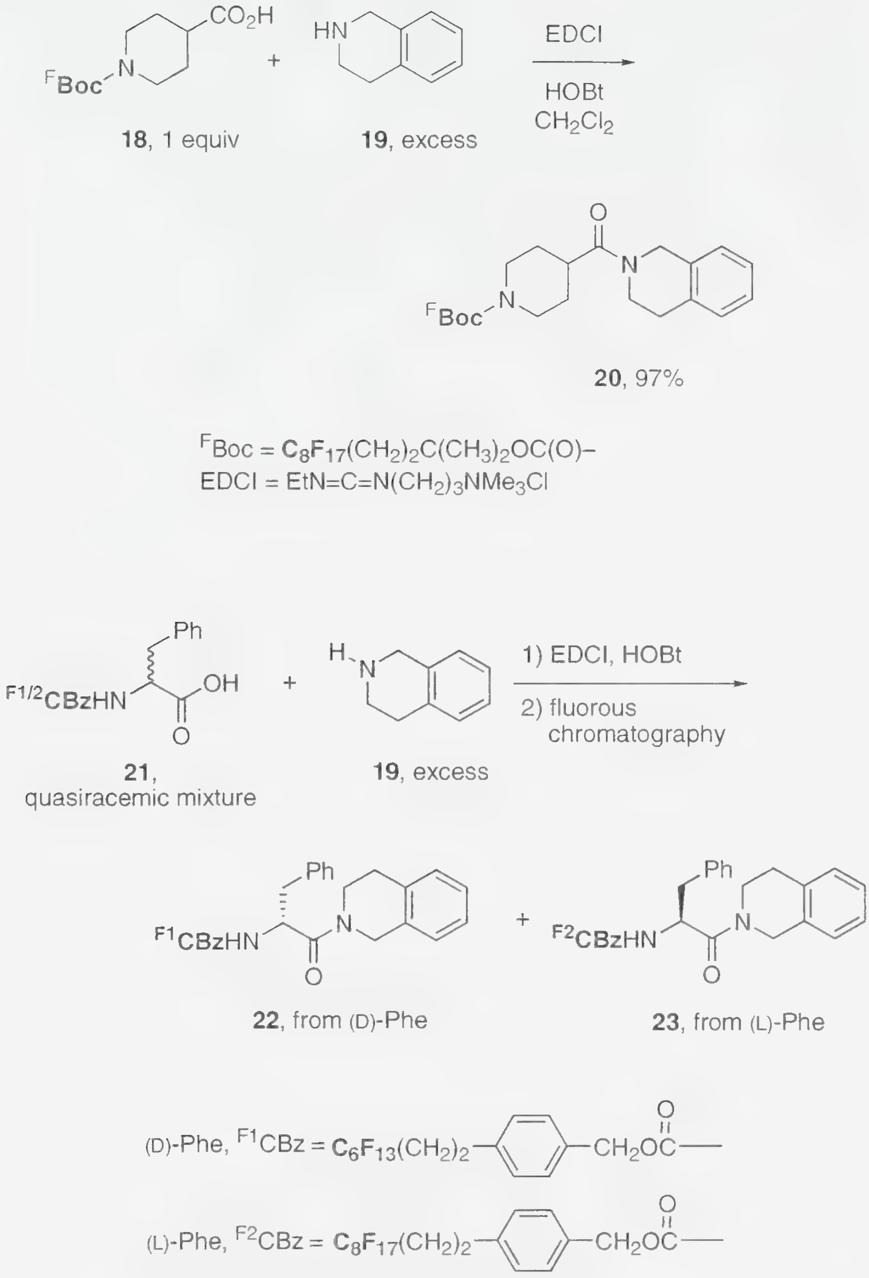
Scheme 2. Amide formation with a fluorous Mukaiyama reagent



Scheme 3. Examples of fluorous organometallic reactions and catalysts



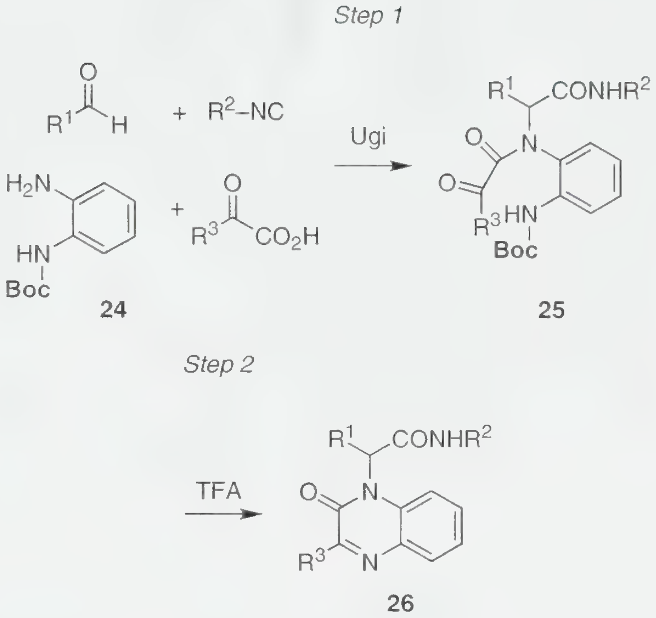
Scheme 4. A typical application of fluorous scavenging



^{F}Boc = $C_8F_{17}(CH_2)_2C(CH_3)_2OC(O)$–
EDCI = $EtN{=}C{=}N(CH_2)_3NMe_3Cl$

(D)-Phe, ^{F1}CBz = $C_6F_{13}(CH_2)_2$–C_6H_4–$CH_2OC(O)$–
(L)-Phe, ^{F2}CBz = $C_8F_{17}(CH_2)_2$–C_6H_4–$CH_2OC(O)$–

Scheme 5. Examples of fluorous protecting groups in single compound (top) and quasiracemic (bottom) synthesis



Scheme 6. A rapid two-step heterocycle synthesis with fluorous tagging

Subject: Re: Your Confirmation Requested - O.Paleta
From: "Prof. Oldrich Paleta" <oldrich.paleta@vscht.cz>
Date: Tue, 16 Sep 2003 12:27:22 +0200
To: "Yechiel Bar-Chaim" <yechiel@jdcparis.org>
CC: "Alfred Bader Fine Arts" <baderfa@execpc.com>, "Ing. Markéta Bláhová" <mblahova@csvts.cz>

Dear Yechiel,

excuse please a delay with reply. The annual meeting of the Czech and Slovak Chemical Societes was held last week in Slovakia and the Secretariat of the CCS participated in the meeting organization.

I confirm your conclusion in sponsoring the Alfred Bader Awards that after receiving the sum of $23,333 the Czech Chemical Society will have the funding for both prizes until the end of 2007.

Many thanks for your kind assistance.

Kind regards.
Sincerely yours,
Oldrich

----- Original Message -----
From: Yechiel Bar-Chaim
To: 'Prof. Oldrich Paleta'
Sent: Wednesday, September 10, 2003 12:29 PM
Subject: Your Confirmation Requested

Dear Professor Paleta,

I have been in touch with Dr. Bader about getting the prize funding on a common calendar.

Am I right in thinking that if we send you now the sum of $23,333, the the funding for both prizes will be assured until the end of 2007?

2004	$ 3,283 in hand	$ 3,333 to be sent
2005-2007		$ 20,000 to be sent (3 years * 2 prizes per year * $3,333 per prize)
Total	$3,283 in hand	$23,333 to be sent

These figures do not take into consideration bank charges, of course.

Sincerely yours,

Yechiel

-----Original Message-----
From: Prof. Oldrich Paleta [mailto:oldrich.paleta@vscht.cz]
Sent: 02 September 2003 16:38
To: Alfred Bader Fine Arts
Cc: Ing. Markéta Bláhová; Yechiel Bar-Chaim
Subject: Re: Suspected as SPAM: Re: O.Paleta - dollar exchange rate

Dear Dr. Bader,

I have been informed that the Czech Chemical Society has received the funding for the A.Bader Awards in the period 2000-2003 as follows:

Netto sum received (the bank subtracts the transfer fee from the sum transferred):
year 2000: $9,959 for 3 prizes
2001: $9,985 for 3 prizes
2003: $3,283 for 1 prize

Total $23,227 for 7 prizes

Awards given:
year 2000: 1
2001: 1
2002: 2
2003: 2

Total 6 Awards

Thus, the Czech Chemical Society has available $3,283 (netto) for the year 2004.

My best personal regards to You both.

Yours truly,
Oldrich Paleta

----- Original Message -----

> **From:** Alfred Bader Fine Arts
> **To:** Prof. Oldrich Paleta
> **Cc:** Yechiel Bar Chaim
> **Sent:** Friday, August 15, 2003 9:59 PM
> **Subject:** Suspected as SPAM: Re: O.Paleta - dollar exchange rate
>
> Dear Professor Paleta,
>
> Thank you so much for your e-mail of today.
>
> I would like to leave each of the two awards at $3,333 annually and if the Czech Chemical Society would then like to add the small difference to make that 100,000 Czech Crowns, that is fine.
>
> I would very much like to have the Czech Chemical Society have the same amount of money for both of the awards.
>
> Now I know that you have sufficient funds for each for the year 2003.
>
> Please let me know how much you now have for the biorganic/inorganic award II and then I can send you exactly the same amount for award I.

> > Thus, if you have $6,666 for award II for 2004 and 2005, then I would send you that amount for award I and then in 2006 you would receive $20,000 to cover both awards for the following three years.
> >
> > Thank you for your help.
> >
> > With best personal regards to you and to Ing. Marketa Blahova I remain
> >
> > Yours sincerely,
> > Alfred Bader
> >
> > Prof. Oldrich Paleta wrote:
> >
> > > Dear Dr. Bader,
> > >
> > > thank you very much for all kind arrangements you are doing to support our young chemists. Concerning the funding of the Alfred Bader Prizes in the Czech Republic, I would like inform you about the exhange rate of the US dollar to -Czech crown. The exchange rate has falln down dramatically this year and was the lowest in June. The rate has been slightly increasing since that time, but still is low: 1USD ca. 28.21 Czcr (purchase).
> > >
> > > This means that the fund of 3333 USD is equal ca. 93400.- Czcr. Will you agree that the lacking part of the Prize (6600.- Czcr) is given by the Czech Chemical Society ?
> > >
> > > Best regards.
> > >
> > > Yours truly,
> > > Oldrich Paleta
> > >
> > > ---
> > >
> > > This message scanned for viruses by Corecomm

This message scanned for viruses by Corecomm

University of Pittsburgh

Faculty of Arts and Sciences
Department of Chemistry

Chevron Science Center
Pittsburgh, Pennsylvania 15260
412-624-8240
Fax: 412-624-9861
E-mail: curran@pitt.edu

Dennis P. Curran, PhD
Distinguished Service Professor
of Chemistry and
Bayer Professor of Chemistry

August 26, 2003

Dr. Alfred Bader
924 East Juneau Avenue
Astor Hotel – Suite 622
Milwaukee, WI 53202

Dear Alfred:

Enclosed please find two packets of handouts from the most recent FTI Board Meeting; one is for you and the other is for Daniel.

Over the past several months, our sales rose but then seemed to hit a plateau. Also, a couple of contracts that we thought might materialize in 2003 have been pushed off until at least 2004. Although some of this may be a reflection of the tough climate at the moment, we have taken steps to conserve our precious cash and we are also actively looking to build sales and pursue new leads for contracts.

As always, feel free to give me a call if you have any questions or suggestions.

Best personal regards,

Dennis P. Curran
Distinguished Service Professor and
Bayer Professor of Chemistry

DPC:lcc

Cc: Dr. Philip Yeske, w/o enclosures

[illegible]

[illegible] [illegible] [illegible]

[illegible]

[illegible]

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[illegible]

[illegible]

[illegible]

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[illegible]

[illegible]

PENNSTATE

[illegible]

[illegible]

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

January 15, 1997

Dr. W. Gerhard Pohl
Langfeldstraße 85
A-4040 Linz
Austria

Dear Dr. Pohl:

Many thanks for your various communications showing how you are making Loschmidt the chemist better known in Austria.

Enclosed, please find a copy of the paper that appeared in the September issue of *Chemistry in Britain*.

With all good wishes, I remain,

Yours sincerely,

AB/nik

Absender

Dr. W. Gerhard Pohl
Langfeldstraße 85
A - 4040 Linz
Tel. 0732 / 24 76 79

Kurzmitteilung

Datum 10. 11. 1996

Mit der Bitte um

☐ Kenntnisnahme ☐ Weiterleitung
☐ Erledigung ☐ Stellungnahme
☐ Rücksprache ☒ zum Verbleib
☐ unter Hinweis auf unser Gespräch

Anlage

Ihre Zeichen/Nachricht

Unsere Zeichen

Herrn
Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee
Wisconsin 53202
USA

Sehr geehrter Herr Bader,

beiliegend sende ich Ihnen die Kopie eines soeben erschienenen Artikels über Loschmidt.

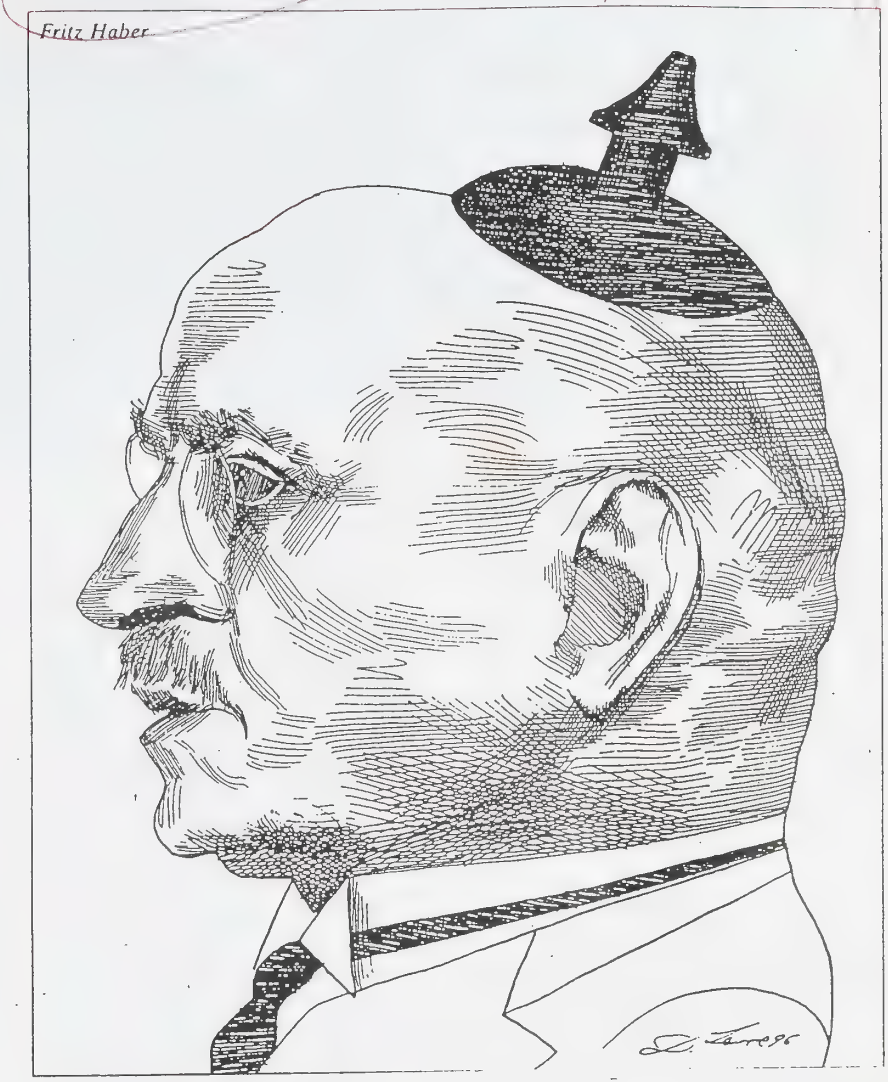
Ich hoffe, es geht Ihnen gut und grüße Sie herzlich

Ihr W. G. Pohl

The Cabinet of Dr. Haber

M. F. Perutz

File Perutz



Fritz Haber

Fritz Haber, Chemiker, Nobelpreisträger, Deutscher, Jude: Eine Biografie
by Dietrich Stoltzenberg.
Weinheim and New York: VCH,
669 pp., $60.00

Der Fall Clara Immerwahr: Leben für eine humane Wissenschaft
by Gerit von Leitner.
Munich: C.H. Beck, 232 pp., DM 39.80

> *"As far as science is concerned, there is no doubt whatsoever in my mind that to look upon it as a means of increasing one's power is a sin against the Holy Ghost."*
>
> —Karl Popper, "The Moral Responsibility of the Scientist"

*

> FRITZ HABER:
> *"It was never, ever my intention, to engineer more deaths by my invention."*
>
> CLARA HABER:
> *"Your process led to death and devastation."*
>
> FRITZ HABER:
> *"It saved the world that hurtled to starvation."*

These lines from Tony Harrison's play *Square Rounds*, which was recently produced in London, epitomize the ambiguous personality and career of Fritz Haber. He was a German chemist, born in 1868, famous for being the first scientist to have synthesized ammonia from the nitrogen in the air; this opened the way to the synthesis of the nitrogen fertilizers that have dramatically increased agricultural production throughout the world. He is also infamous for having introduced poison gas in the First World War.

Haber was larger than life in every sense. Photographs show him taller than everyone else in the picture, stiffly erect and formally dressed with a pince-nez and a starched collar turned down at the corners, lording it over his assembled laboratory staff, a *Geheimrat* par excellence. After April 1933, when the Nazis had forced him, a Jew by birth, from all official positions, Haber told a friend: "I have been German to a degree which I feel fully only now." To Chaim Weizmann he described himself as one of the most powerful men in Germany:

> I was more than a great leader of armies, more than a captain of industry. I was the founder of great industries. My work opened the way to the great industrial and military expansion of Germany. All doors stood open to me.

As Dietrich Stoltzenberg makes clear in his detailed biography, Haber had been devoted to the glory of Bismarck's German Reich and the German Emperor with an intensity hard for present generations to comprehend. He continued to visit the Emperor during his exile in Holland after Germany became a republic. He was a man of intellectual brilliance, with wide knowledge, overriding ambition, and a certain lack of humanity. The father, a respected businessman trading in dyes and pharmaceuticals, was more observant of the Prussian virtues of hard work, sense of duty, order, and discipline than of the Jewish rites. He compelled Fritz to enter his flourishing, carefully managed business, but when one of Fritz's impulsive transactions resulted in a severe loss, he allowed him to launch himself on what was then thought to be a badly paid academic career in chemistry instead. He did not foresee that one day guests invited to Fritz's Berlin residence would dine off gold plates.

Chemistry had fascinated Haber as a schoolboy. As was customary in Germany, he studied at a succession of universities, and finally landed at the Technical University in Karlsruhe. Knowing that academic careers were closed to non-Christians, he had himself baptized in the Lutheran faith.

At the turn of the century, it was hard for anyone without an independent income to follow a university career, because assistant professor (*Privatdozent*) and associate professor (*Ausserordentlicher Professor*) were honorary posts rewarded only with the fees paid by the students they were able to attract to their lectures, and only full professors received an adequate salary. His poverty drove Haber to earn money from patents, and books, and to accept assignments from private industry. He worked furiously, determined to get to the top. When he failed to be appointed to a coveted chair in physical chemistry, chemistry's elder statesman Wilhelm Ostwald counseled him: "Achievements generated at a greater than the customary rate raise instinctive opposition amongst one's colleagues."

In 1901 Haber married Clara Immerwahr, a thirty-year-old woman, daughter of another respected Jewish family in Breslau, whom he had known as a teen-ager. As Gerit von Leitner's biography of Clara shows, she matched him in ambition and determination, having fought against prejudice and opposition to become the first woman Ph.D. in science at Breslau University. She was not pleased when shortly after the birth of their first son, Hermann, Haber left for a three-month tour of America.

In 1908, when he was only forty, Haber was appointed full professor of physical chemistry at Karlsruhe, where a contemporary described him as impulsive, temperamental, and quick-thinking, an excellent lecturer and engaging talker on virtually any subject. But in a letter to a friend quoted by von Leitner, Clara complained about his treatment of her:

> What Fritz has gained in these eight years, I have lost, and what is left of me fills me with profound dissatisfaction. I have always felt that it is only worth having lived if one has developed all one's faculties to the full, and has experienced everything that life can offer. That is what made me decide to get married, since otherwise one chord of my soul would lie fallow.
>
> If my elation was short-lived… that is due mainly to Fritz's overpowering way in his home and marriage, besides which anyone perishes who doesn't assert herself more ruthlessly than he…. I ask myself whether superior intelligence makes one person more precious than another and if much of me that has gone to the devil, because it has gone to the wrong man, is not more valuable than the most important electronic theory.

If you want to make your name in science, try to accomplish something that has defeated everyone else. In 1784 the French chemist C. L. Berthelot discovered that ammonia consists of one atom of nitrogen and three atoms of hydrogen. For the next 125 years many chemists tried to make ammonia from these two gases and failed, largely because the laws governing chemical reactions were not fully understood. Haber, an excellent theoretician and talented experimenter, determined to solve the problem, at first without any thought of practical applications.[1] He and his young English collaborator Robert Le Rossignol made a careful study of the temperatures and pressures required to combine free nitrogen and hydrogen gas so as to produce more than tiny quantities of ammonia.

They found that the formation of ammonia required a pressure on the two gases of more than two hundred times that of the atmosphere at sea level and a temperature of 200°C (390°F), extreme conditions never reproduced in a laboratory before. Even then the ammonia was made only very slowly. To hasten the reaction, a catalyst was needed, in this case a metal on whose surface hydrogen and nitrogen would combine faster. Haber and Le Rossignol tried one possible metal after another until a powder of the rare metal osmium accelerated the reaction spectacularly. On July 2, 1909, they triumphantly demonstrated an experiment producing about seventy drops of ammonia a minute to the directors of the Badische Anilin und Soda Fabriken, then Germany's largest chemical firm.

At the time, saltpeter mines in Chile were the main sources of natural nitrogen fertilizer, but their output was limited and expected to be exhausted by about 1940. Some nitrate was also recovered from coal gas, but not nearly enough to satisfy demand. On the other hand, the nitrogen in the air was unlimited, hydrogen was abundant in coal gas, and their compound, ammonia, could be used as a fertilizer either by combining it with sulfuric acid or by oxidizing it so as to produce nitrates.

Convinced by the promise of Haber's demonstration, the directors of the Badische firm provided two of their ablest chemists, Carl Bosch and Alwin Mittasch, with unlimited time and resources to develop the process for industrial production. Badische Anilin took an option on the entire world stock of osmium (220 pounds), but Mittasch also performed over 10,000 tests of ammonia synthesis on 4,000 other catalysts. He finally selected a mixture of iron, which is abundant and cheap, with small amounts of the oxides of aluminum, calcium, and potassium. On September 9, 1913, the first industrial unit set up by Bosch and Mittasch started to produce between three and five tons of ammonia daily, a thousand times the output of Haber and Le Rossignol's original laboratory apparatus. Current world production of ammonia for fertilizer is about a hundred thousand times greater; all of it is still made with Mittasch's original iron catalyst, whose efficiency and durability has never been surpassed.

Haber was rewarded with generous royalties and the Nobel Prize for Chemistry for 1918; Carl Bosch received the prize in 1931 for his development of an entirely new technology for the production of ammonia under high pressure, despite an explosion of the ammonia works at Oppau on the Rhine on September 21, 1921, which killed 561 people and made 7,000 homeless (Stoltzenberg fails to mention that appalling disaster).[2] Unjustly Mittasch was left out. Having accomplished so much, Haber might have taken life easily, but that was not in his restless nature, and in any case he became embroiled in controversy over his discovery. His patents were immediately challenged by an Austrian chemical firm which had suggested the possibility of synthesizing ammonia from its elements and had financed his first experiments. Other firms, which recognized the patents as a gold mine, also made claims against him. Caught up in these disputes, he no longer did original scientific work, but other opportunities now beckoned.

---

[1] See his speech later accepting the Nobel Prize, in *Les Prix Nobel, 1918 and 1919* (Stockholm: Nobel Foundation, 1920).

[2] See Wilhelm Roggersdorf, in cooperation with BASF, *In the Realm of Chemistry* (Düsseldorf and Vienna: Econ Verlag, 1965).

Courtesy of the Imperial War Museum, London

Gassed, *painting by John Singer Sargent, 1918*

In 1910, the German Emperor founded the Kaiser Wilhelm Gesellschaft zur Förderung der Wissenschaft, a semi-independent body for the support of research which was to prove of immense benefit to German science and learning. It was supported by Leopold Koppel, a respected Jewish banker, who also offered to pay for an Institute of Physical Chemistry in Berlin under Fritz Haber's direction. Haber said he would accept if he was also appointed to a chair at Berlin University, made a member of the Prussian Academy of Sciences, and given a salary of 15,000 marks a year (equivalent to about $85,000 today). These exacting demands were duly met and Haber accepted. Along with Max Planck and Walter Nernst, Berlin's leading physicists, he persuaded Einstein to leave Zürich and move to Berlin, and he attracted many excellent young scientists to his new institute. The Kaiser Wilhelm Gesellschaft also built a second large institute for Germany's greatest chemist, Emil Fischer, who had received the Nobel Prize in 1902, partly for his work on the structure and synthesis of sugars.

In October 1912, Wilhelm II in person was to open both institutes, Stoltzenberg writes. Two of the members, Otto Hahn and his associate Lise Meitner, later famous for their discovery of atomic fission, suggested to the Emperor's adjutant that they take him to their darkroom and show him the scintillations which alpha rays from radium produced on a fluorescent screen. The adjutant objected, because Wilhelm would be frightened in the dark.[3] (Meanwhile the scientists' wives had hired a gym teacher to teach them how to curtsy to His Majesty.)

Nitrates form an essential part of explosives. When war broke out in August 1914, the British blockade cut Germany off from Chilean supplies of saltpeter, the traditional source of nitrates. The Germans captured 20,000 tons of saltpeter in Antwerp harbor after their invasion of Belgium, but had it not been for Haber's synthesis of ammonia, German nitrate supplies would have been exhausted and the Germans would have had to sue for peace. Haber volunteered for the army, in which he had served in his teens, but he was rejected now on account of his age. Instead, he became chief of the chemistry section in the War Department for Raw Materials. In December 1914 he attended a test of artillery shells filled with tear gas, but he found the gas was too widely dispersed to have any effect.

According to his assistant, Fritz Epstein, Haber then suggested that the discharge of chlorine gas from cylinders would be more effective.[4] Chlorine is a greenish-yellow, blinding gas, heavier than air, which immediately produces violent coughing; it corrodes the eyes, nose, mouth, throat, and lungs, and finally asphyxiates the person who inhales it. If blown by the wind toward the enemy lines, Haber proposed, it would sink into the trenches and drive the soldiers out into the open, where they could easily be killed. The idea appealed to the Chief of the General Staff, Erich von Falkenhayn. However, there was the awkward matter of the Hague Conventions of 1899 and 1907, which Germany had signed and ratified. According to the first convention: "The Contracting Powers agree to abstain from all projectiles whose sole object is the diffusion of asphyxiating or deleterious gases." The second convention also prohibited all use of poisons and poisoned weapons in war.

Falkenhayn saw a fine distinction between projectiles filled with noxious gases and gases being blown by the wind from cylinders on the ground, which the conventions had not foreseen. He put Haber in charge of a project to make such cylinders and promoted him from a noncommissioned officer in the reserve to the rank of captain. In *The Poisoned Cloud*, his scholarly book on the history of poison gas, Haber's son Ludwig writes: "In Haber the [High Command] found a brilliant mind and an extremely energetic organizer, determined, and possibly also unscrupulous."[5] Stoltzenberg confirms that Haber was without any doubt the initiator of chemical warfare.

Haber threw himself into the task. He worked himself to exhaustion organizing the manufacture of hundreds of tons of chlorine gas and thousands of gas cylinders; he trained special troops to test them and oversaw their installation in the trenches at the front, regardless of danger to his own person.

He also recruited his own young collaborators and many other chemists for the task. When Otto Hahn objected that what he was doing was contrary to international law, Haber argued that in the autumn of 1914 the French had broken it first by firing rifle shells filled with tear gas; according to Stoltzenberg and Ludwig Haber this was untrue. Stoltzenberg writes: "When one reads the reports of Haber's activity and behaviour at that time, one gains the impression that he was obsessed by his self-imposed tasks." His boundless ambition seems to have made him determined to win the war single-handed. He planned to

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[3] A story Lise Meitner told the author of this review many years ago.

[4] L.F. Haber, *The Poisonous Cloud* (Clarendon Press/Oxford University Press, 1986).

have chlorine gas blown toward the Allied lines on a front of fifteen miles, which would either have killed the enemy soldiers or put them to flight. Massed German infantry were to follow and break through the Allied lines. He advised the High Command to use gas only if it could ensure victory, and he also urged that the German troops should be protected with a primitive kind of gas mask.

When the High Command asked the divisional commanders in charge of different sections of the front to cooperate with such an attack, all but Duke Albrecht of Württemberg refused. His troops were engaged in some of the fiercest fighting on the front at Ypres, twenty miles from the Belgian coast. There, Haber's special troops dug into the German trenches 5,730 cylinders capable of releasing 150 tons of chlorine gas along a four-mile-long front. It was to be blown toward the enemy when the wind came from the east; but this was known to happen, on average, on only one day out of three, and besides, the wind so near the sea was capricious. Ludwig Haber writes:

> Here was Haber himself, an academic in uniform, paunchy, rarely without a cigar, pockets bulging, surrounded by young acolytes who managed to look respectful, busy, and unconventional in dress and bearing.

All the wartime documents refer to him deferentially as "Geheimrat Haber." He prevailed upon Otto Hahn, who commanded a machine-gun section, to become a participating "observer," and the future Nobel Laureates in Physics James Franck and Gustav Hertz also joined him. But Max Born, another young physicist at Haber's Institute and a future Nobel Laureate, refused to take part. The chemist Hugo Stoltzenberg, the father of Haber's biographer, ran chlorine filling stations near the front. By April 11, 1915, the unwieldy cylinders, each weighing nearly 200 pounds, had been installed at night; but the masks for the German troops never arrived.

In his book dealing with his father's work on poison gas, Ludwig Haber writes:

> The first gas alert order was given on 14 April at 22.30 and cancelled at 01.45 on 15 April. The second was on 19 April at 15.00, but was countermanded. By then the [High Command] had become cautious and owing to the Russian threat on the Austro-Hungarian front became reluctant to commit reserves earmarked for the east to something as uncertain as the follow-up to a gas attack. The third alert was given on 21 April at 17.00, postponed first to 04.00 on 22 April, then to 09.00 and then to the afternoon.
>
> The troops, the Pionierkommando, and the specialists had had very little rest and were on edge. They were sure the Allies had been alerted. They had indeed. Three weeks earlier the French, then still in the south of the Salient, were told by prisoners of the installation of cylinders, and there was visual evidence of gas cylinder explosions in March. But the French ignored these warnings....
>
> The simultaneous opening of almost 6,000 cylinders which released 150 tons of chlorine along 7,000 meters within about ten minutes was spectacular.... Within minutes the Franco-Algerian soldiers in the front and support lines were engulfed and choking. Those who were not suffocating from spasms broke and ran, but the gas followed. The front collapsed.
>
> The Germans advanced cautiously. They too were taken by surprise and followed the cloud, delayed not by resistance, but by patches of gas in low ground and ruins.... German hesitations and darkness saved the French by giving them time to regroup.... The Germans' elation at their initial success soon turned to disappointment when on 23 April their Divisions, upon being ordered to advance, met with increasingly stiff resistance.

The gas attack caused 15,000 Allied casualties, 5,000 of them fatal; but Haber's great victory still failed to materialize. He arrived home in Berlin disappointed and exhausted a few days later. On the evening of May 1 the Habers had guests for dinner, and that same night, while Haber was asleep, Clara shot herself with his service pistol, waking up their fourteen-year-old son, who found her in a pool of blood in their garden. Next morning Haber dutifully left for the Eastern front.

Gerit von Leitner, in her biography of Clara, and Tony Harrison, in his play about Haber's career, attribute her suicide to her disgust with Haber's activities, which included tests of chlorine and other poison gases on animals at Haber's Institute, next door to their official residence. Gerit von Leitner writes that Clara was heartbroken when the young chemist Otto Sackur, a friend of hers from student days at Breslau University, was killed in an explosion at the institute. She also describes a row in which Haber blamed Clara's gossip for the army's failure to break through.[6] Stoltzenberg could find no evidence that Clara's suicide was a protest against Haber's war work. But according to Kurt Mendelsohn's book *The World of Walter Nernst*, Clara had pleaded with Haber repeatedly not to work on chemical warfare;[7] von Leitner has found a recorded statement by James Franck saying that Haber's part in the gas war certainly influenced Clara's suicide, and that Haber blamed himself bitterly for it throughout his life.

Stoltzenberg writes that their marriage was happy at first, but that this changed after their son's birth, when

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[5] Haber, *The Poisonous Cloud*, p. 27.

[6] I asked Dr. von Leitner for the source of this statement. She writes that no written record of that fatal row survives. She heard about it from the late A.H. Frucht, a grandson of the first president of the Kaiser Wilhelm Gesellschaft, Adolf von Harnack. Harnack in turn heard it from F. Schmitt-Ott, a friend of Haber's who was the "Kulturminister" in charge of science and learning at the time, to whom Haber had confessed his feelings of guilt. Dr. von Leitner also kindly informed me of James Franck's recorded statements.

[7] Kurt Mendelsohn, *The World of Walter Nernst: The Rise and Fall of German Science* (Macmillan, 1973).

Clara became increasingly concerned with domestic trivia, which irritated Haber. He also writes that Clara was hospitalized more than once for depression, a crucial point which von Leitner fails to mention. He cites Clara as one of those people "whose search for self-fulfillment makes them build a wall around themselves which becomes their self-imposed prison." She wrote some farewell letters which have not survived, and von Leitner suspects that Haber destroyed them.

From the front Haber wrote to a friend: "For a month I doubted that I would hold out, but now the war with its gruesome pictures and its continuous demands on all my strength has calmed me." He continued to devote himself to chemical warfare, and according to Stoltzenberg this satisfied him completely. Once Haber had unleashed chlorine gas, the Allies soon matched the German effort, and the prevailing west wind blew in their favor.

Haber's actions continued to contradict Montesquieu's belief that knowledge makes men gentle. Despite his complaints of his overwhelming responsibilities at the front he found time to conceive strategies for research on armaments when the war was over. When some of the German military were making plans to annex all of Belgium and parts of northern France, and for good measure to invade England to teach the English a lesson, Haber got his benefactor Koppel to propose to the war minister that he finance a Kaiser Wilhelm Foundation for War Technology. Haber also induced Carl Duisberg, the head of Bayer, to propose that a Kaiser Wilhelm Institute for Chemical Warfare be established with himself as director. Strangely, Haber made this proposal even though he apparently was convinced as early as 1916 that Germany would lose the war. The Emperor approved the Foundation on December 17, 1916, with Fritz Haber, Emil Fischer, Walter Nernst, and three less famous chemists on the governing board. The Kaiser Wilhelm Gesellschaft hesitated to collaborate with the Foundation at first, and one of its members objected that killing people was not the Gesellschaft's job. But in September 1918 its directors accepted, and the War Ministry assigned six million marks for the project.

After Germany's collapse two months later, Haber and Nernst were branded as war criminals by the Allies, who demanded their extradition. Haber fled to Switzerland, where he was given Swiss citizenship, a privilege reserved for the very rich. After a few months the Allies dropped their demand that he be extradited, and he returned to Germany to help with reconstruction and to continue the secret manufacture of poison gas in violation of the Treaty of Versailles. The Spanish government sought German help in manufacturing and using chemical weapons for suppressing Abd el Krim's revolt in Morocco. The Soviet government entered into a clandestine agreement with the Germans to manufacture weapons, including poison gas, and the German War Ministry set up a secret chemical warfare factory near Wittenberg. Haber directed these enterprises through his wartime collaborator Dr. Hugo Stoltzenberg, whom Ludwig Haber describes as "a plausible rogue who in other circumstances might have been believed if he had claimed to grow mushrooms in the desert." In Spain Stoltzenberg set up a poison-gas factory near Madrid and personally advised King Alfonso XIII and his dictatorial prime minister, Primo de Rivera, about the best gas tactics to be employed against the Moroccan rebels.

Hugo Stoltzenberg apparently negotiated contracts allowing him to set up some of the factories as his own private enterprises. In 1925 the German foreign minister, Gustav Stresemann, and his French counterpart, Aristide Briand, met at Locarno and agreed on a rapprochement which induced the German government to stop the secret manufacture of poison gas and to close down Stoltzenberg's factory. He went bankrupt, but to his fury Haber refused to support his claims for compensation.

Chemical warfare had failed to break the stalemate on the western front, but it had succeeded on the southern front, where the Austrian and Italian armies faced each other near the present frontier between Italy and Slovenia. In my youth in Austria I learned of "our" great victory at Caporetto (now Cobarid in Slovenia) in October 1917, where the Austrian and German armies broke through the Italian lines and advanced seventy miles westward to the River Piave. (The Italian retreat forms the background to Ernest Hemingway's moving novel *A Farewell to Arms.*) The books by Stoltzenberg and Ludwig Haber show that the Austrians owed their breakthrough to an attack on the unprotected Italians with a mixture of chlorine and phosgene gas that had been prepared by Otto Hahn and other co-workers of Haber. Otto Hahn's autobiography suggests that he regretted this later.[8] In September 1939, after attending a meeting at the German Army Ordnance Department where the possibility of exploiting his discovery of nuclear fission for an atomic bomb was discussed, Hahn declared: "If my work leads to a nuclear weapon, I will kill myself." He sounded desperate when he heard of Hiroshima during his internment at Farm Hall in England. Fortunately, he would have found it difficult there to carry out his threat, had he still wanted to do so.[9]

Emil Fischer killed himself in 1919 in despair over the loss of his son in the war and over Germany's defeat and its postwar chaos. It seems that Haber had no regrets. He justified his invention of chemical weapons by claiming that the French had used them first, which was untrue, and that these weapons were more humane than high explosives because most soldiers survived the chemical attacks. He did not mention that many of the survivors were broken in both body and spirit for the rest of their lives. Haber continued until 1933 to advise Germany's government on its secret production of chemical weapons; but his main energies were devoted to the rebuilding of his institute as a leading center of fundamental research, to the revival of German science, and to the restoration of contacts with scientists abroad. While Haber was overbearing and dictatorial at home, he was wise enough to give his young collaborators scientific freedom. After their seminars he would say apologetically that he may not have been been able to follow all their arguments, and then summarize them more lucidly than they had themselves. The discussions were animated by the search for the scientific truth, regardless of one's rank or fame. Haber's institute became again an outstanding center of chemical research, and it still bears his name.

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[8] Otto Hahn, *My Life* (London: Macdonald, 1970).

[9] *Operation Epsilon: The Farm Hall Transcripts* (University of California Press, 1993).

The Treaty of Versailles made Germany's huge reparations payable in pre-war gold marks, which crippled Germany's recovery. Haber had read that a ton of sea water contains between five and ten thousandths of a gram of gold, which meant that the oceans might contain as much as eight million tons of it. Once again Haber set out to save Germany singlehanded. He decided to devise chemical methods to extract the gold, and to use it to pay Germany's reparations. In strictest secrecy, he raised money and recruited fourteen young collaborators. Disguised as members of the crew, he and his assistants took a German passenger liner to New York, and then another to Rio. Some of the initial analyses they made on board confirmed the earlier high estimates, but their variability made Haber decide to ship all samples of sea water back for analysis at his Berlin laboratory. After careful analysis of about five thousand samples from sources throughout the world, Haber's assistant Johannes Jaenicke reported a mean gold content of no more than a thousandth of the original estimates. It was a shattering blow.

In 1917 Haber had married Charlotte Nathan, an attractive, independent, enterprising woman twenty-one years his junior, who lacked his Prussian sense of duty and had a passion for travel. Marriage to a man who was absent from home on important business most of the time and returned exhausted satisfied her as little as it had the very different Clara, and the marriage broke up after ten years. Ludwig Haber, the author of *The Poisonous Cloud*, is a son of that marriage.

Early in 1933, a few weeks before the Nazis seized power, Haber wrote to a friend: "I fight with ebbing strength against my four enemies, insomnia, the economic claims of my divorced wife, lack of confidence in the future and awareness of the grave mistakes I have committed in my life." He did not specify the mistakes, but feelings of guilt toward Clara may have been among them.

In April the Nazis ordered that all Jewish civil servants be dismissed, including employees of the Kaiser Wilhelm Gesellschaft. Max Planck, its president, used his official courtesy call on the newly appointed chancellor to plead that Haber and other Jewish scientists be allowed to continue their work. Hitler retorted that he had nothing against the Jews, but that they were all Communists. When Planck remonstrated and pointed out that Germany would harm itself if it expelled all its excellent Jewish scientists, Hitler slapped his knee, talked faster and faster, and whipped himself into such a rage that Planck had to leave the room.

Haber now devoted all his remaining energy to securing work abroad for his Jewish staff. Einstein happened to be visiting the United States, where he stated publicly that he would not return to Germany because it no longer recognized "civil liberty, tolerance and equality of citizens before the law." The Nazi press responded with a flood of abuse and the commissioner in charge of the Prussian Academy of Sciences demanded that disciplinary action be taken. Planck believed that as a German, Einstein should have stood up for Germany abroad whatever the faults of the new regime and decided that Einstein had made his continued membership of the Academy impossible. When Planck put this view to the assembled members, Haber concurred and only the physicist and Nobel Laureate Max von Laue had the courage to object to the shameful decision. Einstein deeply resented it. When a friend asked him later if he could take greetings from him back to Germany, Einstein replied: "Only to Laue." "Really no one else?" "No, only to Laue."

Haber himself eventually fled to Cambridge in England, where the Professor of Chemistry, William Pope, his adversary in chemical warfare, received him with honors, but the laboratory technicians who had fought in the trenches shunned him. After a short stay, Haber traveled to Switzerland, where he died of a heart attack after his arrival in Basel in January 1934, aged only sixty-five.

Had he lived, he would have had to face the most gruesome of his mistakes, to which Tony Harrison alludes in *Square Rounds*, when Clara and a veiled chorus sing:

He'll never live to see his fellow
Germans use
his form of killing on his fellow
Jews.

In 1919, when Allied inspectors of his institute prevented further research on chemical warfare against human beings, Haber turned to chemical warfare against agricultural pests. He became National Commissioner for Pest Control and founded a new firm, the German Society for Pest Control. The firm developed a preparation combining hydrocyanic acid, which is highly toxic, with a sweet-smelling, volatile, nontoxic irritant; both were absorbed in a porous powder. Another firm, Tesch and Stabonov, undertook to spread the powder in insect-contaminated fields and buildings. When it was spread on an open field, the acid evaporated, killing the insect pests, and the irritant warned people to keep away. The preparation was called Zyklon B. In 1943 Dr. Peters, the director of the pest-control firm, received a secret order from an SS officer to deliver Zyklon B *without the irritant* to Auschwitz and Oranienburg. He was told that it would be used to kill criminals, incurables, and mentally deficient persons, and he was threatened with the death penalty if he broke the secret.[10] So the pesticide which began in Haber's institute ended up as an instrument of the Holocaust, in which some of Haber's own relations perished.

In 1946 Dr. Tesch, the sole owner of the firm Tesch and Stabonov, was convicted by a British Military Court of delivering Zyklon B to Auschwitz and hanged; ironically, Hugo Stoltzenberg, the secret poison gas manufacturer of the interwar years, was appointed by the British as trustee of the firm. In 1949 a Frankfurt court sentenced Peters to five years in prison for complicity in manslaughter; later, a Wiesbaden court sentenced him to six years for complicity in murder. In 1955 he was acquitted for lack of proof that he had known what was going on at Auschwitz.

Stoltzenberg's excellent biography is written with scholarly detachment. He confesses that he found it hard to imagine himself in the mind and role of Haber and to understand his unquestioning nationalism and sense of patriotic duty. Stoltzenberg's documents show that Haber continued to be held in the highest esteem in Germany as a great patriot, scientist, and statesman, despite the widespread public disillusionment with World War I; he also captivated people by his liveliness, charm, Old-World courtesy, and quick repartee. By a terrible irony of fate, it was his apparently most beneficent invention, the synthesis of ammonia, which has also harmed the world immeasurably. Without it, Ger-

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[10] Zyklon B was not used for killing mental defectives, who were not sent to Auschwitz. They were killed in Germany with coal gas.

many would have run out of explosives once its long-planned blitzkrieg against France failed.[11] The war would have come to an early end and millions of young men would not have been slaughtered. In these circumstances Lenin might never have got to Russia, Hitler might not have come to power, the Holocaust might not have happened, and European civilization from Gibraltar to the Urals might have been spared.

Haber's synthesis of ammonia for fertilizer was an extremely important discovery, but, unlike relativity, it did not take a scientist of unique genius to conceive it; any number of talented chemists could, and no doubt would, have done the same work before very long. □

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[11] For documentary evidence of these plans, see Fritz Fischer, *War of Illusions: German Policies from 1911 to 1914*, translated by Marian Jackson (Norton, 1975).

The New York Review

June 20, 1996

BERNARD PROTTER
25 George Street/505
Toronto M4A 4L8
Toronto: 416-367-1922 Tel
416-367-9205 Fax

London: 0171-794-3053 Tel
0171-794-7772 Fax

02.07.96

MEMO TO FILE

RE: prototype fuel cell powered car

This memo to file supersedes the M.T.F. dated 11-19 June 1996

I attended the 11th World Hydrogen Energy Conference in Stuttgart, Germany, 23-28 June 1996.

The organizers of the conference provided each participant with 3 large bound volumes of all the scientific papers read and prepared by the several hundreds of participants, enough reading material for many a night.

It would appear that there were almost as many different conclusions as there were PHD participants.

B.M.W. seems to have concluded that the purchasing public needs to be lured into thinking that a transition vehicle powered by <u>Liquid National Gas</u> is the way to capture the market for hydrogen.

Therefore BMW demonstrated a well appointed LNG powered car at this World <u>Hydrogen</u> conference.

Daimler Mercedes appears to believe that an hydrogen burning internal combustion engine powered car is the way to excite the public.

The demonstration car was temporarily equipped with compressed hydrogen bottles - as used for weldings, intended to be stored in the trunk area?

Since an I.C. engine uses up to double the amount of hydrogen compared to a fuel cell driven car, Daimler Mercedes seems to have opted for a methanol on board storage system complete with a miniaturized steam reformer - a veritable mini refinery. Good luck with government approvals!

The principal problem with the mass production of an hydrogen driven car at present is the absence of a safe hydrogen storage system on board the passenger car (neither busses nor trucks have much problems with storage).

Many of the scientists attending the Stuttgart conference dealt primarily with hydrites presently limited by weight and temperature, hence limiting storage capacity, hence limited range.

Some of the hydride systems offered (in theory), at the conference used magnesium at 250° - 400°, dangerously scalding temperatures, promising 3-4% hydrogen storage per weight of tank.

Therefore I believe that in the long run passenger cars will have to be propelled by a fuel cell, which has the enormous advantage over I.C. hydrogen drive cars, by saving between 40%-50% of fuel, hence weight and range regarding on board storage.

I have chosen the normal hydride system capable of storing hydrogen at relatively low temperatures, hence safe for the passengers. Anything above 1.3% of the hydrates weight is acceptable.

Others suggested, as mentioned, liquid hydrogen for storage which however presents the additional problem of evaporating at 1-3% daily, as well as maintaining the ultra low dangerous temperature of - 235°C of liquid hydrogen. High temperature hydrides are also likely to face government approval problems. The Hindenburg as well as the spacecraft complexes are alive and churning.

How much hydrogen do we really need?

Leaning on the "Mobile Wasserstroff Technik" by Dr. Helmut Buchner 23-05-1996, I primarily used pages 5 & 8 for a basis.

Midsize car Mercedes 190 or similar American styled car or European car, about 100KW.

Curb Weight 1750Kg - INC. A/C
Max Speed 180 - 185 KMH
Range 500 KM =

H2 I.C. Motor:	1 KM = 17.4 GR (High 26 GR - Low 13 GR) Total required 500 KM x 17.4 = 8700 GR Efficiency: High - 22%, Low = 15% = 18.5% average Storage = 8700GR Low Temp. Hydride Tank - 670 Kg
Fuel Cell:	1 KM = 11.5 GR. Total required:: 500 KM x 11.5 = 5750 GR Efficiency High 36%, Low = 20% = 28% average Storage 5.750GR 1.3% Low temp Hydride Tank = 443 Kg

Dr Ron's patented A/c will further reduce weight of hydrates.

The proposed bivalent operation of hydrogen/petrol by Mercedes does not reduce the West's dependence on oil, nor will it slow down the CO_2 Built up in our atmosphere.

General Motors has been quoted as claiming that for mass production they could produce fuel cell motors for $50-70.00 per kilowat.

In the meantime, the world is marching on apparently helplessly - as if hypnotized - while the weather and climate changes march on relentlessly.

When, not if, will governments act decisively to stop the build up CO_2 in our atmosphere??

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Jun [illegible]

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[illegible]

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[illegible]

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- [illegible]

It is [illegible] the [illegible] goes over an [illegible] the place of the [illegible] $100,000 while the [illegible] come in [illegible] $12,000 : Fuel Cell [illegible]

To put it down in numbers: "Capital Costs [illegible] PRACTICAL life time of the Equipment"

[illegible]

[illegible]

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

May 7, 1996

Dr. W. Gerhard Pohl
Langfeldstraße 85
A-4040 Linz
Austria

Dear Dr. Pohl:

Please accept my sincere thanks for the various articles about Loschmidt, which you so kindly sent me.

I very much hope that we will be able to meet before or after my talk in Vienna on Tuesday afternoon, June 11th.

In various articles about Loschmidt's life, there are some minor mistakes and one lacuna. The mistakes are related to what Loschmidt did before he settled in Vienna as a high school teacher, and in that connection, his curriculum vita, published in de Martin's Ph.D. thesis, is instructive, and I enclose a copy.

I know from parts of his diary, which I own, that he was in Brno, not in Vienna, in 1852 and 1853. From Brno, he went to Neuhaus in Bohemia and only after that unhappy experience there did he come to Vienna, probably in 1854, and obtained a Hofmeister Posten. Unfortunately, we don't know with which family he was Hofmeister. Between his position as Hofmeister and teaching in the Leopoldstadt, beginning in September 1856, he had at least two trial positions as a teacher at other schools.

With all good wishes, I remain,

Yours sincerely,

AB/cw
Enclosures

bc: Kosner

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

May 17, 1996

Professor Dr. Vladimir Prelog
Postdoctoral Researcher
Laboratorium für org. Chemie
ETH-Zenstrum
Universitätstrasse 16
CH-8092 Zürich
Switzerland

Dear Postdoctoral Researcher:

I don't know how quickly the Bulletin for the History of Chemistry reaches Zurich, and you may not have seen that delightful review of your book.

Isabel and I plan to be in Switzerland the week of June 17th and of course it would give us great pleasure if we could stop by just to say hello.

With all good wishes, I remain,

Yours sincerely,

AB/cw

yielding 85% of the pure base, m.p.
103/104°C.
4.32 g. of base converted to
the HCl salt in [illegible]. The
product was [illegible] in the mono-
hydrate, m.p. 201.5°C, after drying
→ 75.3 g. hydrochlor. m.p. 210-4°C
([illegible], [illegible], p [illegible])

H (1)

9-Amino-1,2,3,4-tetrahydroacridine

Ref: A. Albert & W. Gledhill, J. Soc. Chem. Ind., 64, 169 (1945). C.A. 39, 4872 (45)

250 g. of the 9-Cl-compound heated to 150 in abs. phenol & NH₃ [illegible] [illegible] hrs. The [illegible] [illegible] yields of 85-90%. Purification [illegible] dissolved in dil. HCl [illegible] the [illegible] [illegible] [illegible] [illegible] several times [illegible]. It's [illegible] [illegible]

[illegible] [illegible], the papers were collected
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③

9-Amino-1,2,3,4-tetrahydroacridine

Ref.: A. Gilbert & W. Gledhill, J. Ind. Chem. Soc., 64, 169 (1945). CA. 39, 4873 ('45) 100 g. of the 9-Cl-compd. added to 250 cc. of cresol & NH_3 bubbled rapidly thru the mixt. for 4 hrs. while heating at 180-190°C. Reaction mixt. cooled, diss'd in dil. HCl & Et_2O, the layers sep'd & acid sol'n ext'd several times c̄ Et_2O. Sol'n made basic

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yielding 35 g of the pure base, m.p. 183-185°C.

A 32.7 g sample of base converted to the HCl salt in MeOH-Et_2O. The product was isol'd as the monohydrate, m.p. 281-3°C, after recryst. → 43.2 g. Reported m.p. 283-4°C (W. A. Petrow, JCS, p. 634 ('47)

c̄ solid NaOH, the ppt'd solid collected by filtr'n & dried i.v. Suspension of product in Et_2O removed all impurities (oily) leaving 93g. of solid, m.p. 100-105°C. This solid diss'd in a small amt of MeOH & dil'd c̄ Et_2O afforded 20g. of solid which proved to be the HCl salt occluded during ppt'n. This material, on treatment c̄ 10% NaOH, afforded the free base. The solvent fr. the filtrate was removed under red. pressure, the residual solid diss'd in boil. CH_3CN & allowed to cryst. in the cold

③

9-Amino-1,2,3,4-tetrahydr$\overset{H}{o}$acridine

Ref.: A. Gilbert & W. Gledhill, J. Ind. Chem. Soc., 64, 169 (1945). CA. 39, 4873 ('45)
100 g. of the 9-Cl-compd. added to 250 cc. of cresol & NH_3 bubbled rapidly thru the mixt. for 4 hrs. while heating at 180-190°C. Reaction mixt. cooled, diss'd in dil. HCl & Et_2O, the layers sep'd & acid sol'n extd. several times c̄ Et_2O. Sol'n made basi

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yielding 35 g of the pure base, m.p. 183-185°C.
A 32.7 g sample of base converted to the HCl salt in MeOH-Et_2O. The product was isol'd as the monohydrate, m.p. 281-3°C, after recryst. → 43.2 g. Reported m.p. 283-4°C (W. A. Petrow, JCS, p. 634 ('47)

c̄ solid NaOH, the ppt'd solid collected by filtr'n + dried i.v. Suspension of product in Et_2O removed all impurities (oily) leaving 93 g. of solid, m.p. 100-105°C. This solid diss'd in a small amt of MeOH + dil'd c̄ Et_2O afforded 20 g. of solid which proved to be the HCl salt occluded during ppt'n. This material, on treatment c̄ 10% NaOH, afforded the free base. The solvent fr. the filtrate was removed under red. pressure, the residual solid diss'd in boil. CH_3CN + allowed to cryst. in the cold

FAX FROM

DR. ALFRED BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone: 414/277-0730
Fax: 414/277-0709

April 30, 1996

Page 1 of 1

To: Dr. W. Gerhard Pohl
Fax: 0732-78 35 16

Dear Dr. Pohl:

Thank you for your fax of April 24th.

Both you and my old friend, Dr. Robert Rosner, have now told me how successful the meeting in Innsbruck was.

I will be in Vienna from Tuesday, June 11th until early Saturday morning, June 15th, and it would give me great pleasure to meet you.

I have been invited to talk on Loschmidt and Couper at 17:30 on Tuesday, but I don't know the exact place. The arrangement was made by Dr. Reinhard Schlögl, whose telephone number in Vienna is 587-5819.

With best regards, I remain,

Yours sincerely,

Alfred Bader

AB/cw

Danke für die gerade angekommenen Drucksachen – hoch interessant.

Schade dass Sie keine Loschmidt Marken verwenden!

Beste Grüsse
Alfred Bader

TELEFAX

[illegible]
2961 North Shepard Avenue
Milwaukee Wisconsin 53211

Dr. W. Gerhard Pohl
Langfeldstraße 85
A-4040 Linz
Tel 0732 / 24 76 79

[illegible] 001-414-2770709

[illegible] 24. 4. 96

Ihren Brief vom [illegible] 96

Sehr geehrter Herr Bauer,

spät aber doch [illegible] ich Ihnen [illegible] freundlichen [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] sie nicht [illegible] kommen [illegible] sind Montage u. Dienstage an [illegible] (abgesehen von Wochenende) an denen wir nicht ins Museum [illegible] können.

Mit getrennter Post sende ich als Drucksache: [illegible]

[illegible]

4. [illegible] Ausstellung [illegible] Ausstellung von der [illegible] [illegible]

5) Notiz zum [illegible] / Umfrage bei [illegible] war bisher erfolglos

6) Zusammenfassungen der Innsbrucker Vorträge.

Die Tagung in Innsbruck war sehr schön. Ich hatte noch nie so viel Lob für meine Arbeit erhalten wie in Innsbruck. Das ist natürlich erfreulich. Vielleicht mache ich 1998 eine Fortsetzungstagung

Herzliche Grüße Ihr G Pohl

618 Van Liew Ct.
Neshanic, NJ 08853
April 15, 1996

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202

Dear Dr. Bader:

Many thanks for your prompt response to my request. I would have answered you sooner, but these last 4-6 weeks have been very busy for me as I prepared for my retirement. I visited my colleagues in Lognes, France; in Regensburg, Germany; in Candiac, Quebec; and in Syracuse, New York. It was very pleasant to reminisce with friends and co-workers about the "good old days".

To answer your question about my plans after April 1 - initially, I intend to take about a six month rest and visit our extended families. Thereafter, I would like to consider teaching and/or consulting on a part time basis. I also look forward to pursuing the many outside interests I have that were subsumed necessarily to my job responsibilities. I have been told by various retired colleagues from the company, that there is a life after Bristol-Myers Squibb.

Thank you for your inscription. The book, with its fascinating story, is a wonderful addition to my library.

My best wishes to you and your wife.

Sincerely,

Richard

Richard A. Partyka

P.S. An addressed and stamped envelope is provided for the return of the book.

Returned
4/19

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

March 5, 1996

Richard A. Partyka, Ph.D.
Executive Director, Chemical Process Research
Bristol-Myers Squibb Pharmaceutical Research Institute
One Squibb Drive
P.O. Box 191
New Brunswick, NJ 08903-0191

Dear Richard:

Thank you for your gracious letter.

I don't think of you as the executive director of chemical process research, but as the ablest medicinal chemist I ever met at Bristol-Myers.

Of course, I will be happy to inscribe your copy of my autobiography and return it to you.

What are your plans after April 1st?

If you ever come to the Midwest, please do plan to visit Isabel and me in Milwaukee.

With all good wishes, I remain,

Yours sincerely,

AB/cw

COPY

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

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Yours sincerely,

AB/cw

Bristol-Myers Squibb
Pharmaceutical Research Institute

One Squibb Drive P.O. Box 191 New Brunswick, NJ 08903 0191
[illegible]

Richard A. Partyka, Ph.D
Executive Director
Chemical Process Research
Process Research and Development

February 26, 1996

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

Dear Dr. Bader:

My purpose in writing this note to you is twofold. Firstly, I have recently purchased your "Adventures of a Chemist Collector" and find it to be an absolutely fascinating addition to my library. I would like to inquire if you would be kind enough to sign the book for me upon mailing it to you. I would be happy to provide funds for a return of the book to me.

Secondly, reading certain passages in your book evoked fond memories of my formative years in the chemical profession. I wish to inform you that as of April 1, 1996, after 34+ years with the Bristol-Myers Squibb Company, I have decided (voluntarily) to retire to pursue other areas of interest. It has been a rewarding career during an incredible period of growth for the pharmaceutical industry. Amongst the memorabilia collected, I have a preview issue (fall, 1967) of Aldrichimica Acta. Unfortunately, the arrow of time only points in one direction.

My best wishes to you for good health and continued success.

Sincerely,

Richard

Richard A. Partyka, Ph.D.

File Max Perutz

THAT WAS THE WAR

The New Yorker
12 August 1985

ENEMY ALIEN

IT was a cloudless Sunday morning in May of 1940. The policeman who came to arrest me said that I would be gone for only a few days, but I packed for a long journey. I said goodbye to my parents.

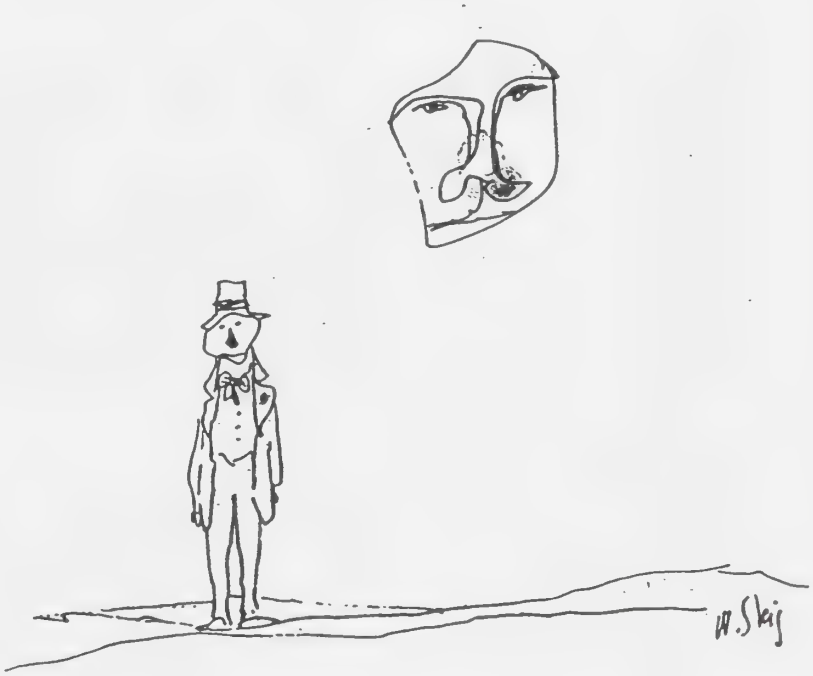
From Cambridge, they took me and more than a hundred other people to Bury St. Edmunds, a small garrison town twenty-five miles to the east, and there they locked us up in a school. We were herded into a huge empty shed cast into gloom by blacked-out skylights thirty feet above us. A fellow-prisoner kept staring at a blank piece of white paper, and I wondered why until he showed me that a tiny pinhole in the blackout paint projected a sharp image of the sun's disc, on which one could observe the outlines of sunspots. He also taught me how to work out the distances of planets and stars from their parallaxes and the distances of nebulas from the red shifts of their spectra. He was a warmhearted and gentle German Roman Catholic who had found refuge from the Nazis at the Observatory of Cambridge University. Years later, he became Astronomer Royal for Scotland. In the spring of 1940, he was one of hundreds of German and Austrian refugee scholars, mostly Jewish and all anti-Nazi, who had been rounded up in the official panic created by the German attack on the Low Countries and the imminent threat of an invasion of Britain.

After a week or so at Bury, we were taken to Liverpool and then to an as yet unoccupied housing estate at nearby Huyton, where we camped for some weeks in bleak, empty semi-detached two-story houses, several of us crowded into each bare room, with nothing to do except lament successive Allied defeats and worry whether England could hold out. Our camp commander was a white-mustached veteran of the last war; then a German had been a German, but now the subtle new distinctions between friend and foe bewildered him. Watching a group of internees with skullcaps and curly side-whiskers arrive at his camp, he mused, "I had no idea there were so many Jews among the Nazis." He pronounced it "Nasis."

Lest we escape to help our mortal enemies, the Army next took us to Douglas, a seaside resort on the Isle of Man, where we were quartered in Victorian boarding houses. I shared my room with two bright German medical researchers, who opened my eyes to the hidden world of living cells—a welcome diversion, lifting my thoughts from my empty stomach. On some days, the soldiers took us out for country walks, and we ambled along hedge-flanked lanes two abreast, like girls from a boarding school. One day near the end of June, one of our guards said casually, "The bastards have signed." His terse message signified France's surrender, which left Britain to fight the Germans alone.

A few days later, tight-lipped Army doctors came to vaccinate all men under thirty—an ominous event, whose sinister purpose we soon learned. On July 3rd, we were taken back to Liverpool, and from there we embarked on the large troopship Ettrick for an unknown destination. About twelve hundred of us were herded together, tier upon tier, in one of its airless holds. Locked up in another hold were German prisoners of war, whom we envied for their Army rations. On our second day out, we learned that a German U-boat had sunk another troopship, the Arandora Star, which had been crammed with interned Austrian and German refugees and with Italians who were being deported overseas. More than six hundred of the fifteen hundred people aboard were drowned. After that, we were issued life belts.

Suspended like bats from the mess decks' ceilings, row upon row of men swayed to and fro in their hammocks. In heavy seas, their eruptions turned the floors into quagmires emitting a sickening stench. Cockroaches asserted their prior tenancy of the ship. To this revolting scene, Prince Frederick of Prussia, then living in England, restored hygiene and order by recruiting a gang of fellow-students with mops and buckets—a public-spirited action that earned him everyone's respect, so that he, grandson of the Kaiser and cousin of King George VI, became king of the Jews. Looking every inch a prince, he used his royal standing to persuade the officers in charge that we were not the Fifth Columnists their War Office instructions made us out to be. The commanding colonel called us scum of the earth all the same, and



Entr'acte

"You've got the right number but the wrong mood."

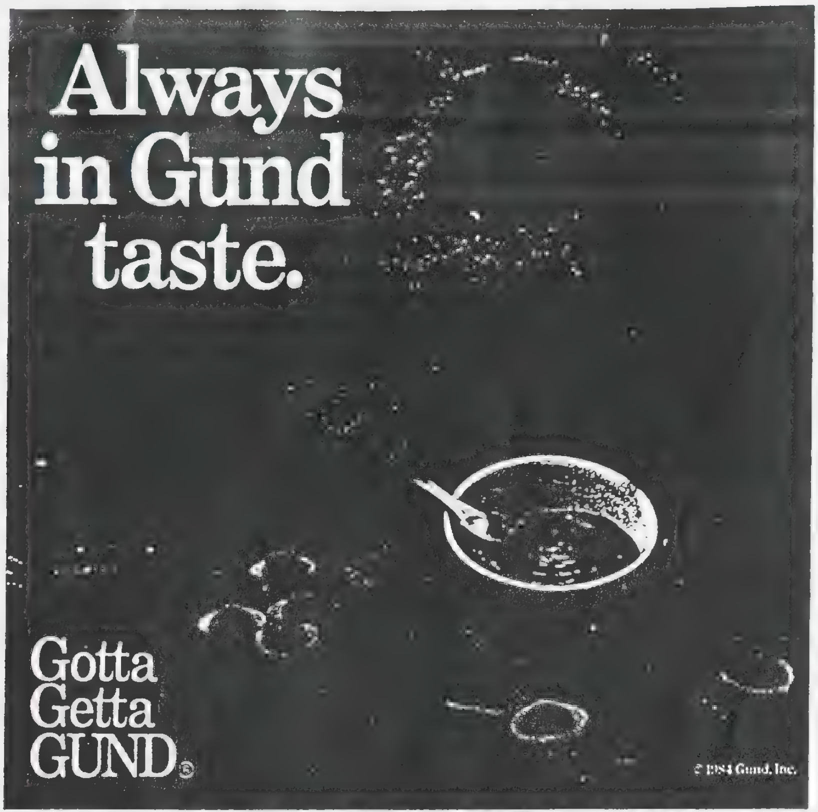
• •

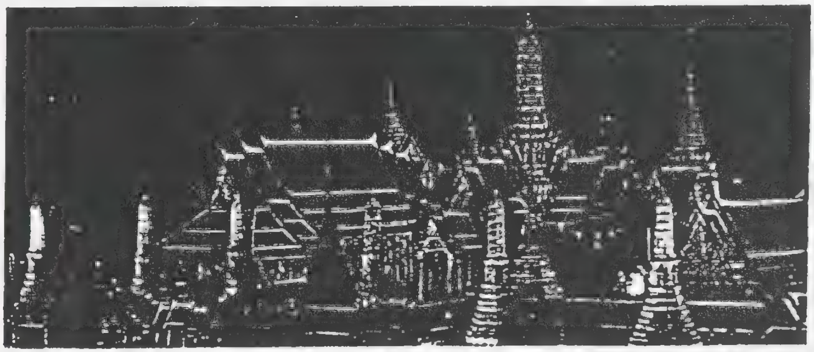
once, in a temper, ordered his soldiers to set their bayonets upon us. They judged differently and ignored him. One day, I passed out with a fever. When I came to, in a clean sick bay that had been established by young German doctors, we were steaming up the broad estuary of the St. Lawrence River, and on July 13th we finally anchored off gleaming-white Quebec city. The Canadian Army took us to a camp of wooden huts on the citadel high above the town, close to the battlefield where the English General James Wolfe had beaten the French in 1759. The soldiers made us strip naked so they could search us for lice, and they also confiscated all our money and other useful possessions, but I forestalled them by dropping the contents of my wallet out the window of the hut while we were waiting to be searched, and went around to pick them up the next day, when the soldiers had gone. Sometimes jewels are safest on a scrap heap.

In Canada, our status changed from that of internees to that of civilian prisoners of war, entitling us to clothing—navy jackets with a red patch on the back—and Army rations, which were welcome after our first two days, when we were without food. Even so, the fleshpots of Canada were no consolation for our new status, which made us fear that we would remain interned for the duration of the war and, worse still, that in the event of England's defeat we would be sent back to Germany to be liquidated by Hitler. To have been arrested, interned, and deported as an enemy alien by the English, whom I had regarded as my friends, made me more bitter than to have lost freedom itself. Having first been rejected as a Jew by my native Austria, which I loved, I now found myself rejected as a German by my adopted country. Since we were kept incommunicado at first, I could not know that most of my English friends and scientific colleagues were campaigning to get the anti-Nazi refugees, and especially the many scholars among them, released. I had come to Cambridge from Vienna as a graduate student in 1936 and had begun my life's research work on the structure of proteins. In March of 1940, a few weeks before my arrest, I had proudly won my Ph.D. with a thesis on the crystal structure of hemoglobin—the protein of the red blood cells. My parents had joined me in Cambridge shortly before the outbreak of war; I wondered when I would see them again. But, most of all, I and the more enterprising among my comrades felt frustrated at having to idle away our time instead of helping in the war against Hitler. I never imagined that before long I would be returning to Canada as a free man, engaged in one of the most imaginative and absurd projects of the Second World War.

Our camp offered a majestic panorama of the St. Lawrence and of the lush green country stretching away to the south of it. As one stifling-hot, languid day followed another, freedom beckoned from the mountains on the horizon, beyond the United States border. I remembered the Bishop's advice to King Richard II: "My lord, wise men ne'er sit and wail their woes, but presently prevent the ways to wail." How could I escape through the barbed-wire fence? Suppose I surmounted that hurdle without being spotted by the guards, who stood on watchtowers with their machine guns trained on us? Who would hide me after my absence had been discovered at the daily roll call? How could I persuade the Americans to let me join my brother and sister there, and not lock me up on Ellis Island? These questions turned over and over in my mind as I lay on my back in the grass at night, listening to the faint hooting of distant trains and watching the delicately colored flashes of the northern lights dance across the sky. Soon I began to dream of jumping on goods wagons in the dark or of fighting my way across the frontier through dense mountain forests—or just of girls.

As a Cambridge Ph.D. of four months' standing, I found myself the doyen of the camp's scholars, and organized a camp university. Several of my Quebec teaching staff have since risen to fame, though in different ways. The Viennese mathematics student Hermann Bondi, now Sir Hermann, taught a brilliant course in vector analysis. His towering forehead topped by battlements of curly black hair, he arrived at his lectures without any notes and yet solved all his complex examples on the blackboard. Bondi owes his knighthood to his office as chief scientist at Britain's Ministry of Defense, and his fame to the steady-state theory of the universe. This theory postulates that, as the universe expands, matter is continuously being created, so that its density in the universe remains constant with time. A universe like that need not have started with a big bang, because it would never have known a beginning and it would have no end. Bondi developed that ingenious theory with another Viennese interned with us—Thomas Gold, who, like him, was still an undergraduate at Cambridge, and who is now professor of astronomy at





Cornell University. The theory's third author was Fred Hoyle, the Cambridge cosmologist and science-fiction writer.

Theoretical physics was taught to us lucidly by Klaus Fuchs, the tall, austere, aloof son of a German Protestant pastor who had been persecuted by Hitler for being a Social Democrat. Klaus Fuchs himself had joined the German Communist Party shortly before Hitler came to power, and fled to England soon afterward to study physics at Bristol University. After his release from internment in Canada, he was recruited to work for the atomic-bomb project, first in Birmingham and then at Los Alamos, and when the war was over he was appointed head of the theoretical-physics section of the newly established British Atomic Energy Research Establishment, at Harwell. Everywhere, Fuchs was highly regarded for his excellent scientific work, and at Harwell he was also noted for his deep concern with security. Then, in the summer of 1949, just before the explosion of the first Russian atomic bomb, the Federal Bureau of Investigation found reason to suspect that a British scientist had passed atomic information to the Russians, and the Bureau's description in some ways fitted Fuchs. After several interrogations, Fuchs broke down and confessed—in January of 1950—that from the very start of his work he had passed on to the Russians most of what he knew of the Anglo-American project, including the design of the first plutonium bomb. A few days after Fuchs' conviction for espionage, the Prime Minister, Clement Attlee, assured Parliament that the security services had repeatedly made "the proper inquiries" about Fuchs and had found nothing to make them suspect him of being a fanatical Communist. Neither had I gathered this during my contacts with him in Canada, but when I recently said so to an old colleague he told me that Fuchs and he had belonged to the same Communist cell while they were students at Bristol. "The proper inquiries" cannot have been all that searching.

Having no inkling of the tortuous mind that later made Fuchs betray the countries and the friends that had given him shelter, I simply benefitted from his excellent teaching. In my own lectures, I showed my students how to unravel the arrangement of atoms in crystals, and I spent the rest of my time trying to learn some of the advanced mathematics that I had missed at school and at the university.

The curfew was at nine-thirty. The windows of our hut were crossed with barbed wire. Its doors were locked, and buckets were put out. Stacked into double bunks, about a hundred of us tried to sleep in one room where the air could be cut with a knife. In the bunk above me was my closest friend from student days in Vienna. We had roughed it together in the mosquito-ridden swamps of northern Lapland and had almost suffered shipwreck on a small sealer in the stormy Arctic Ocean. These adventures had inured us to the physical hardships of internment, but the exhilarating sense of freedom that they had instilled in us made our captivity even harder to bear. Lacking other forms of exercise, we made a sport of reading our jailers' regulation-ridden minds. One day, the prisoners were told that each could send a postcard to his next of kin in England, but two weeks later all the postcards were returned—without explanation, at first. The camp seethed with frustration and angry rumors, but my friend and I guessed that after leaving the postcards lying around for a couple of weeks the Army censor returned them all because not every card carried its sender's full name. It took a month more before my card reached Cambridge, with the laconic message that Prisoner of War Max Perutz was safe and well.

In time, we learned through rumor that our scenic and efficient camp was to be dismantled and we were to be divided between two other camps. Would friend be separated from friend? By age or by the alphabet? It occurred to me that the pious Quebecois might divide us into believers and heretics—that is to say, into Roman Catholics and the rest—and my hunch was soon confirmed. Since my Viennese friend was a Protestant and I was a Roman Catholic, we were destined for different camps. Adversity tightens friendships. Our familiar Viennese idiom, my friend's keen sense of the ridiculous, and shared memories of carefree student days with girls, skiing and mountain climbing, had helped us to escape from the crowd of strangers around us into our own private world. I decided to stay with the Protestants and the Jews, who also included many scientists, and soon found a Protestant who preferred to join the Catholics. Like Ferrando and Guglielmo, the handsome young swains in "Cosi Fan Tutte," we swapped identities. The false Max Perutz was sent with the faithful to the heaven of a well-appointed Army camp, while I, the real one, was dispatched with the heretics and Jews to the purgatory of a locomotive shed near Sherbrooke, Quebec. To start with, it had five cold-water taps and six latrines for seven hundred and twenty men.

Some weeks later, our comedy of errors was unmasked. The stern camp commander, though he was impressed by the purity of my motives, sentenced me to three days in the local police prison. Here was privacy at last—yet not quite. They locked me up in a cage resembling a monkey's in an old-fashioned zoo. It had no chair, no bed—only some wooden planks to rest on. Unlike the prisoner in Oscar Wilde's "Ballad of Reading Gaol," I did not look

> With such a wistful eye
> Upon that little tent of blue
> Which prisoners call the sky,
> And at every drifting cloud that went
> With sails of silver by,

because I never even saw the sky. But I had smuggled in several books inside my baggy plus fours, so I was not as bored as the poor soldier who had to march up and down on the other side of the iron grille to guard me. My reading was undisturbed and my sleep interrupted only by the occasional drunk; the little mites burrowed into my skin without waking me. Only when they had made themselves at home there during the weeks that followed did the scabies rash keep me awake at night.

Back in the Sherbrooke camp, where my spirits sagged at the prospect of wasted years, the camp commander summoned me again—this time to tell me that my release had been ordered by the British Home Office and that I had also been offered a professorship by the New School for Social Research, in New York City. He then asked me if I wanted to return to England or remain in the camp until my release to the United States could be arranged. I replied that I wanted to return to England, and this drew the admiring comment that I would make a fine soldier. I have never heard that said by anyone else, before or since, but what led me to my decision was that my parents and my research were in England, and from the safe distance of Sherbrooke the U-boats and the blitz did not frighten me. My American professorship had been arranged by the Rockefeller Foundation as part of a rescue campaign for the scholars whom the foundation had supported

"No, I don't want to see a note from your mother."

before the war broke out, and in principle it would have qualified me for an American immigration visa, but I was sure that as a prisoner of war without a passport I would never get it. The camp commander raised my hopes that I would be sent home soon.

From our perch on the citadel of Quebec, we had been able to watch the ships go by on the St. Lawrence, but in the locomotive shed we could only watch the men line up for the latrines. In Quebec, we had had a room in a hut set aside for quiet study, but here among a milling, chatting crowd of men my assaults on differential equations petered out in confusion. Camp committees, locked in futile arguments over trivial issues, were chaired by budding lawyers fond of hearing themselves talk. In excruciating boredom, I waited impotently from day to day for permission to leave, but weeks passed and my captivity dragged on. There was little news from home except for hints that my father, who was then sixty-three and had been an Anglophile from youth, had been interned on the Isle of Man. He shared that fate, I learned afterward, with a frail, meticulous old Viennese with sensitively cut features who was distraught at having his life's work interrupted for a second time. This was Otto Deutsch, the author of the then incomplete catalogue of Franz Schubert's collected works. He finished it in later years at Cambridge.

Early in December, I was among some prisoners destined for release from my camp and from several others who were at last put on a train going east. From its windows, the snow-clad forest looked the same each day, so that we seemed to move merely to stay in the same place, like Alice running with the Red Queen. I had been sad at leaving my Viennese friend behind but was overjoyed to find his father—whom he had feared drowned on the Arandora Star—among the prisoners on the train. Some weeks earlier, the father, on discovering that his son was interned in another Canadian camp, had asked to be transferred there, and he was disconsolate that instead the Army had now put him on a train carrying him even farther away. The train finally dumped all of us in yet another camp—this one in a forest near Fredericton, New Brunswick. No one told us why or for how long. In the Arctic weather, I contracted a bronchial cold that made the dark winter hours seem endless. My father had taught me to regard Jews as champions of tolerant liberalism, but here I was shocked to run into Jews with an outlook as warped and brutal as that of Nazi Storm Troopers. They were members of the Stern Gang, which later became notorious in Israel for many senseless murders, including the murder of the Swedish Count Folke Bernadotte, whom the United Nations had appointed as mediator in the Arab-Israeli conflict.

At Christmas, we were finally taken to Halifax, where we were met by one of Britain's prison commissioners—the shrewd and humane Alexander Paterson, sent out by the Home Office to interview any of the internees who wanted to return to Britain. His mission was stimulated by public criticism—"Why Not Lock Up General de Gaulle?" was one of the sarcastic headlines in a London paper that helped to make the War Cabinet change its policy. Paterson explained that it had been impossible to ship any of us home earlier, because the Canadians had insisted that prisoners of war must not be moved without a military escort, yet had refused either to release us in Canada or to escort us to England, on the ground that our internment was Britain's affair. The British War Office had now fulfilled the letter of the regulation by detailing a single Army captain to take us home.

Chaperoned by one urbane captain, two hundred and eighty of us embarked on the small Belgian liner Thysville, which had been requisitioned by the British Army complete with its crew, including a superb Chinese cook. From this moment, we were treated as passengers, not prisoners, but I became fretful once again when days passed and the Thysville had not cast off her moorings; no one had told us that we had to wait for the assembling of a big convoy. As we finally steamed out to sea, I counted more than thirty ships, of all kinds and sizes, spread over a huge area. At first, Canadian destroyers escorted us, but we soon passed out of their range, and our remaining escort consisted of only one merchant cruiser—a passenger liner with a few guns on deck—and a single submarine, neither of them a match for the powerful German battleships Scharnhorst and Gneisenau, which, so our radio told us, prowled the Atlantic not far from our route. We steamed at only nine knots—the speed of the slowest cargo boat—and took a far-northerly course, trusting to the Arctic night to hide us. Both my Viennese friend and his father were on board.

Early in the voyage, I stood at the rail imagining a torpedo in every breaker. Like the Ancient Mariner,

> Alas! (thought I, and my heart
> beat loud)
> How fast she nears and nears!

But time soon blunted my fears, and I began to enjoy the play of wind and waves. I slept in a warm cabin between clean sheets, took a hot bath, brimful, each morning, ate my meals from white table linen in my friends' company, walked in the bracing air on deck or retired to read in a quiet saloon. Toward the end of the third week, we were cheered by the sight of large black flying boats of the Coastal Command circling over us, like sheepdogs running round their flock, to keep the U-boats at bay. One gray winter morning, the entire convoy anchored safely in Liverpool Harbor. On landing, I was formally released from internment and handed a railway ticket to Cambridge, and I was told to register with the police there as an enemy alien. When I presented myself at a friend's house near London that night, she found me looking so fit that she thought I must have returned from a holiday cruise, but then she admired the elaborate needlework by which I had kept my tweed jacket in one piece for all those months, so as not to have to wear the prisoners' blue jacket with the large red circle on the back. Next morning, at the Cambridge station, our faithful lab mechanic greeted me not as an enemy alien but as a long-lost friend; he brought me the good news that my father had been released from the Isle of Man a few weeks earlier and that both he and my mother were safe in Cambridge. That was in January of 1941.

LESS than three years later, I returned to Canada as a representative of the British Admiralty and was accommodated in a suite in Ottawa's luxurious Hotel Château Laurier

Healthy Harry Harcourt Takes a Sick Day

• •

without being searched for lice. I owed that change of fortune to the remarkable Geoffrey Pyke, former journalist and amateur strategist, who enlisted me for a project that bore the mysterious code name Habakkuk. In 1938, I had taken part in an expedition to the Swiss Alps, where we found out how the tiny snowflakes that fall on a glacier grow into large grains of ice. It had never occurred to me that the expertise I gained there would be of any use to the war effort. When I returned from internment, my professor, W. L. Bragg, encouraged me to resume my peacetime research on the structure of proteins, with the continued support of the Rockefeller Foundation, and for a long time nobody wanted my help for anything related to the war except fire-watching on the roof of the laboratory at night.

At last, one day in the spring of 1942, an urgent telephone call summoned me to London. I was directed to an apartment in Albany, a building —owned by the eccentric Sir William Stone, who was also known as the Squire of Piccadilly—where wealthy Members of Parliament and writers like Graham Greene rented pieds-à-terre. There I was met by Pyke, a gaunt figure with a long, sallow face, sunken cheeks, fiery eyes, and a gray-ing goatee, who was camped out amid piles of books, journals, and papers, and cigarette butts lying scattered on oddments of furniture. He looked like a secret agent in a spy film and welcomed me with an air of mystery and importance, telling me in a gentle, persuasive voice that he was acting on behalf of Lord Louis Mountbatten, then Chief of Combined Operations, to ask my advice about tunnelling in glaciers.

Six months went by before Pyke called me again. This time, he sized me up with a volley of provocative remarks, and then told me, with the air of one great man confiding in another, that he needed my help for the most important project of the war—a project that only he, Mountbatten, and our common friend John Desmond Bernal knew about. When I asked him what it was, he assured me that he would willingly disclose it to me, a friend who had understood and appreciated his ideas from the first, but that he had promised to keep it to himself, lest the enemy or, worse, that collection of fools on whom Churchill had to rely for the conduct of the war should get to hear about it.

I left excited and not much the wiser about what I was supposed to do, but Bernal, who had been my first research supervisor at Cambridge, told me a few days later that I should find ways of making ice stronger and freezing it faster, never mind what for. The project had the highest priority, and I could requisition any help and facilities I needed. Despite my glacier research, I was not sure exactly what the strength of ice is, and could find little about it in the literature. Tests soon showed that ice is at the same time brittle and soft, and I found no way of making it stronger. Then, one day, Pyke handed me a report that he said he found hard to understand. It was by Herman Mark, my former professor of physical chemistry in Vienna, who had lost his post there when the Nazis overran Austria, and had found a haven at the Polytechnic Institute of Brooklyn. As an expert on plastics, he knew that many of them were brittle when pure but could be toughened by embedding fibres such as cellulose in them, just as concrete can be reinforced with steel wires. Mark and his assistant, Walter P. Hohenstein, stirred a little cotton wool or wood pulp—the raw material of newsprint —into water before they froze it, and found that these additions strengthened the ice dramatically. When I had read their report, I advised my superiors to scrap our experiments with pure ice and set up a laboratory for the manufacture and testing of reinforced ice. Combined Operations requisitioned a large meat-storage facility five floors underground beneath Smithfield Market, which lies within sight of St. Paul's Cathedral, and ordered some electrically heated suits, of the type issued to airmen, to keep us warm at 0° F. They detailed some young commandos to work as my technicians, and I invited Kenneth Pascoe, who was then a physics student and is now a lecturer in engineering at Cambridge, to come and help me. We built a big wind tunnel to freeze the mush of wet wood pulp, and sawed the reinforced ice into blocks. Our tests soon confirmed Mark and Hohenstein's results. Blocks of ice containing as little as four per cent wood pulp were weight for weight as strong as concrete; in honor of the originator of the project, we called this reinforced ice "pykrete." When we fired a rifle bullet into an upright block of pure ice two feet square and one foot thick, the block shattered; in pykrete the bullet made a little crater and was embedded without doing any damage. My stock rose, but no one would tell me what pykrete was needed for, except that it was for Habakkuk. The Book of Habakkuk says, "Behold ye among the heathen, and regard, and wonder marvellously: for I will work a work in your days, which ye will not believe, though it be told you," but this failed to solve my riddle.

At one stage, Mountbatten sent Pyke to Canada in aid of Habakkuk with a personal introduction from Winston Churchill to Mackenzie King, the Canadian Prime Minister. King received Pyke with outstretched arms, saying, "Mr. Chamberlain has sent me such a nice letter about you." He was just one Prime Minister behind. While Pyke enlisted the Canadians' help, Mountbatten decided to demonstrate the wonders of pykrete to the British Joint Chiefs of Staff. For this show, Pascoe and I prepared small rods of ice and of pykrete which were exactly the same size. We could break the ice rods easily in our hands, but the rods of pykrete stayed in one piece, however hard we tried to break them. We also prepared large blocks of each material as targets to be shot at.

So secret was Habakkuk that no one was supposed to know even who I was, lest my nationality (Austria=mountains=glaciers=ice) or my research record betray it. Pascoe and I worked down in the meat-storage facility while on the upper floors burly Smithfield porters in greasy overalls carried huge carcasses of meat to and from the elevator. They never gave us any of it to supplement our meagre ration.

Who was to demonstrate pykrete to the Chiefs of Staff? Surely not a civilian and an Austrian—an enemy alien at that! It was decided to assign this task to Lieutenant Commander Douglas Grant, who had been an architect in peacetime, and who administered Habakkuk. He had not handled pykrete before, but he did have a uniform. I gave him our rods of ice and of pykrete, packed with dry ice into thermos flasks, and also large blocks of ice and of pykrete, and I wished him luck. I waited for news the next day, but none came.

Rationing had hit the small restaurants and tea shops in the City. Pascoe and I therefore used to take the bus down bomb-scarred Fleet Street to the palatial Combined Operations Headquarters, in Richmond Terrace, just off Whitehall, where we could get a square meal at an affordable price and could listen to the latest gossip. But that day the entertaining Pyke was still away in Canada, and everybody else seemed to avoid us. After lunch, I searched for the normally unruffled Grant and found him in a black mood. When he had handed our little rods of ice and of pykrete around, the old gentlemen had been unable to break either. Next, he had fired a revolver bullet into the block of ice, which duly shattered, but when he fired at the block of pykrete the bullet rebounded and hit the Chief of the Imperial General Staff in the shoulder. The Chief was unhurt, but Habakkuk was under a cloud. Worse was to come.

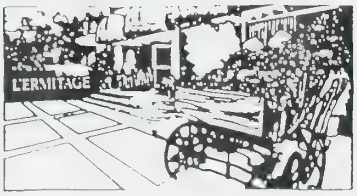
In Pyke's absence, an Admiralty committee headed by the Chief of Naval Construction had examined Habakkuk and sent an unenthusiastic memorandum about it to Mountbatten. When this news reached Pyke in Canada, it merely confirmed his disdain for the conservatism of the British establishment, which he epitomized in a derisive motto: "Nothing must ever be done for the first time." He sent back a cable headed "HUSH MOST SECRET. CIRCULATION RESTRICTED TO CHIEF OF COMBINED OPERATIONS ONLY." The message read, "CHIEF OF NAVAL CONSTRUCTION IS AN OLD WOMAN. SIGNED PYKE." The classification "Hush Most Secret" was normally reserved for operational matters, and was therefore treated with respect, but the contents of Pyke's cable quickly reached the ears of its victim. He was an admiral; outraged at having his courage questioned by a mad civilian, he marched into Mountbatten's office and demanded Pyke's instant dismissal. Habakkuk seemed doomed. Then Pyke returned from Canada, elated by the success of his mission, and especially by the splendid performance of a prototype that the Canadians had succeeded in launching on Patricia Lake, in Alberta. Prototype of what?

Geoffrey Pyke was born in 1893, the son of a Jewish lawyer, who died when the boy was five years old, and left his family with no money. Geoffrey's mother seems to have quarrelled with all her relatives and had made life hell for her children. She sent Geoffrey to Wellington, a snobbish private school attended mainly by sons of Army officers, and yet she insisted on his wearing the dress and observing the habits of an Orthodox Jew. This made him the victim of persecution, and bred in him a contemptuous hatred of the establishment. Though he never finished his schooling, it was possible for him in those days to start studying law at Cambridge. When the First World War broke out, Geoffrey decided to stop his studies and become a war correspondent. Characteristically, he began his career by persuading the editor of the *Daily Chronicle* to send him to the enemy's capital, Berlin. He bought an American passport and made his way to Berlin via Denmark, but he was soon caught, and was told that he would be shot as a spy. After some time in jail, he was put into an internment camp at Ruhleben instead. Less than a year later, the *Daily Chronicle* appeared with the banner headline "CORRESPONDENT ESCAPES FROM RUHLEBEN." By ingenious and meticulous planning, Pyke and another Englishman, Edward Falk, had made their way to Holland and then back to England. Confident now that he could solve any problem by hard thinking, Pyke devised an infallible system for making money on the commodities market. At first, he succeeded, and in 1924 he used the money to finance a startling new experiment in education. He founded the Malting House School, at Cambridge, where children between the ages of two and five were to receive no formal teaching but instead were to be guided to discover knowledge for themselves in purposeful play—"discovery of the idea of discovery." For a time, the school flourished, and it became a laboratory where the great child psychologist Susan Isaacs studied the intellectual growth and social development of young children. Pyke's lawyer urged him to endow the school with the fortune he had made on the Metal Exchange, but he had more grandiose educational plans. To finance them, he bought metals on credit through several different brokers, keeping each of them in the dark about the full extent of his operations. At one point, he cornered as much as a third of the world's supply of tin. Then the day came when Pyke's infallible graphs misled him: prices fell when they should have risen, and Pyke went bankrupt. His school had to close, his marriage broke up, and his health collapsed. He tried journalism again, but no one would print his long articles, and he lived on the charity of friends. In the mid-thirties, recovering, he organized a campaign for sending supplies to the Loyalists in the Spanish Civil War. Later, he raised a band of young English volunteers to conduct a clandestine public-opinion poll in Nazi Germany. Its results were to prove to Hitler that the Germans did not want to go to war, but Hitler forestalled an evaluation of Pyke's poll by the invasion of Poland.

Despite his failures, Pyke remained unshaken in his faith that he knew how to perform any job better than those whose profession that job happened to be, and as soon as the war broke out he became intent on telling the soldiers how to win it. Initially, no one would listen to him, but persistent campaigning and connections in high places brought him an introduction to Mountbatten. In March of 1942, Pyke proposed to the Chief of Combined Operations that Allied commando troops be parachuted into the Norwegian mountains to establish a base on the Jostedalsbreen, the great glacier plateau, for guerrilla warfare against the German Army of Occupation—a base from which the commandos would be able to attack nearby towns, factories, hydroelectric stations, and railways. These troops should be equipped with a snow vehicle of Pyke's design, which would allow them to move at lightning speed across glaciers, up and down mountainsides, and through forests. Pyke persuaded Mountbatten that such a force would be invulnerable in its glacier strongholds and would tie down a large German Army trying vainly to dislodge it. Despite Churchill's enthusiastic comment "Never in the history of human conflict will so few immobilize so many," the plan was dropped, perhaps because someone had found out that there are no towns, factories, hydroelectric stations, or railways near the Jostedalsbreen. The snow vehicle that Pyke had demanded for it was meanwhile built by Studebaker and named the Weasel. It proved its worth during the war in France and Russia, and afterward conveyed research expeditions safely to the South Pole.

While Pyke was in the United States organizing the manufacture of Weasels, he composed his great thesis on Habakkuk. From New York, he sent it in the diplomatic bag to Combined Operations Headquarters in London with a label forbidding anyone other than Mountbatten in person to open the parcel. Inserted opposite the first page was a green sheet of paper with a quotation from G. K. Chesterton: "Father Brown laid down his cigar and said carefully: 'It isn't that they can't see the solution. It is that they can't see the problem.' " In Pyke's accompanying letter, he wrote, "The cover name for this . . . project, because of its very nature, and partly because of you, is Habakkuk, '*parce qu'il était capable de tout.*' "

I cannot remember anyone's ever revealing to me officially what Habakkuk stood for, but gradually the secret leaked out, like acid from a rusty can. Pyke foresaw that for several purposes air cover was needed beyond the range of land-based planes. Conventional carriers, he argued, were too small to launch the heavy bombers and fast fighters that would be needed for the invasion of any distant shores. Already, to extend air cover for Allied shipping over the entire Atlantic, floating islands were needed; such islands would allow planes to be flown from the United States to Britain instead of being shipped. They would also facilitate the invasion of Japan. But what material could such islands be made of, since every ton of steel was needed for ships and tanks and guns, and every ton of aluminum for planes? What ma-



terial existed that was still abundant? To Pyke, the answer was obvious: ice. Any amount of it could be had in the Arctic; an island of ice melts very slowly, and could never be sunk. Ice could be manufactured for only one per cent of the energy needed to make an equivalent weight of steel. Pyke proposed that an iceberg, either natural or artificial, be levelled to provide a runway, and hollowed out to shelter aircraft.

Mountbatten told Churchill of Pyke's proposal. Churchill wrote to his Chief of Staff, General Hastings Ismay:

> I attach the greatest importance to the prompt examination of these ideas. . . .The advantages of a floating island or islands, even if only used as refuelling depots for aircraft, are so dazzling that they do not at the moment need to be discussed. There would be no difficulty in finding a place to put such a "stepping-stone" in any of the plans of war now under consideration.
>
> The scheme is only possible if we make Nature do nearly all the work for us and use as our materials sea water and low temperature. The scheme will be destroyed if it involves the movement of very large numbers of men and a heavy tonnage of steel or concrete to the remote recesses of the Arctic night.
>
> Something like the following procedure suggests itself to me. Go to an ice field in the far north which is six or seven feet thick but capable of being approached by icebreakers; cut out the pattern of the ice-ship on the surface; bring the right number of pumping appliances to the different sides of the ice-deck; spray salt water on continuously so as to increase the thickness and smooth the surface. As this process goes on the berg will sink lower in the water. There is no reason why at the intermediate stages a trellis-work of steel cable should not be laid to increase the rate of sinking and giving stability. The increasing weight and depth of the berg will help to detach the structure from the surrounding ice-deck. It would seem that at least 100 feet in depth should be secured. The necessary passages for oil fuel storage and motive power can be left at the proper stages. At the same time, somewhere on land the outfits of huts, workshops and so forth will be made. When the berg begins to move southward so that it is clear of the ice floes, vessels can come alongside and put all the equipment, including ample flak, on board.

Could an ice floe thick enough to stand up to the Atlantic waves be built up fast enough? It was to find the answer to this question that Pyke and Bernal first called me in, but without being allowed to tell me what the question was. As anyone knows who has tried to make a skating rink in his back yard, a long time is needed even in very cold weather to freeze a thick layer of water, because the thin film of ice that forms at the top delays the transfer of heat from the underlying water to the cold air above. By Churchill's method, it would have taken about a year to build up an ice ship a hundred feet thick—and then only if the action of natural forces could somehow be prevented from causing it to disintegrate. But this was deemed unfeasible. Then, what about a natural floe? In the nineteen-thirties, a Russian expedition had discovered that even at the North Pole the pack ice was no more than ten feet thick. Atlantic waves can be as high as ninety feet, with a distance of more than fifteen hundred feet from crest to crest. Our tests showed that a slab of ice ten feet thick and suspended on two knife edges eight hundred feet apart would snap in the middle. Besides, bombs and torpedoes would crack it, even if they could not sink it. And natural icebergs have too small a surface above water for an airfield, and are liable to turn over suddenly.

The project would have been abandoned in 1942 if it had not been for the discovery of pykrete: it is much stronger than ice and no heavier; it can be machined like wood and cast into shapes like copper; immersed in warm water, it forms an insulating shell of soggy wood pulp on its surface, which protects the inside from further melting. However, Pascoe and I found one grave snag: though ice is hard to the blow of an axe, it is soft to the continuous pull of gravity, which makes glaciers flow like rivers—faster in the center than at their sides, and faster at the top than near their beds. If a large ship of ordinary ice were kept at the freezing point of water, it would gradually sag under its own weight, like putty; our tests showed that a ship of pykrete would sag more slowly, but not slowly enough, unless it were to be cooled to a temperature as low as 4°F. To keep the hull that cold, the ship's surface would have to be protected by an insulating skin, and its hold would have to carry a refrigeration plant feeding cold air into an elaborate system of ducts.

All the same, plans went ahead. Experts drew up requirements, naval designers settled at their drawing boards, and committees held long meetings. The Admiralty wanted the ship to be strong enough to stand up to the biggest known waves—a hundred feet high and two thousand feet from crest to crest—even though such gigantic waves had been reported only once, in the North Pacific, after prolonged storms. It said that the ship must be self-propelled, with enough power to prevent its drifting in the strongest gales, and that its hull must be torpedo-proof, which meant that it had to be at least forty feet thick. The Fleet Air Arm demanded a deck fifty feet above water, two hundred feet wide, and two thousand feet long, to allow heavy bombers to take off. The strategists required a cruising range of seven thousand miles. The final design gave the bergship (as it came to be referred to) a displacement of two million two hundred thousand tons—twenty-six times that of the Queen Elizabeth, the biggest ship then afloat. Turboelectric steam generators were to supply thirty-three thousand horsepower to drive twenty-six electric motors—each fitted with a ship's screw and housed in its own separate nacelle—on the two sides of the hull. These motors were to propel the ship at seven knots—the minimum speed needed to prevent its drifting in the wind. Steering presented the most difficult problem. At first, we thought that the ship could be steered simply by varying the relative speed of the motors on either side, like a plane taxiing along the ground, but the Navy decided that a rudder was essential to keep it on course. The problem of suspending and controlling a rudder the height of a fifteen-story building was never solved. Indeed, even today rudders cause problems in supertankers of only a tenth the bergship's tonnage: in 1978, failure of the rudder control caused the supertanker Amoco Cadiz to be blown onto the rocks off the coast of Brittany, spilling its oil onto the white beaches.

While plans for the bergship became more elaborate with each committee meeting, Pyke's mind raced ahead to work out how such ships should be used to win the war. He argued that the bergships would solve the difficult problems of invading hostile coasts, because they would be able to force their way straight into the enemy's harbors. The defending troops would be petrified, literally, by being frozen solid. How? The bergships were to carry enormous tanks full of supercooled water—liquid water cooled below its normal freezing point—which could be sprayed at the enemy to solidify on contact. Afterward, more supercooled water would be pumped ashore to build bulwarks of ice, behind which Allied troops could safely assemble and make ready to capture the town. It was Pyke's best piece of science fiction. In reality, the cool-

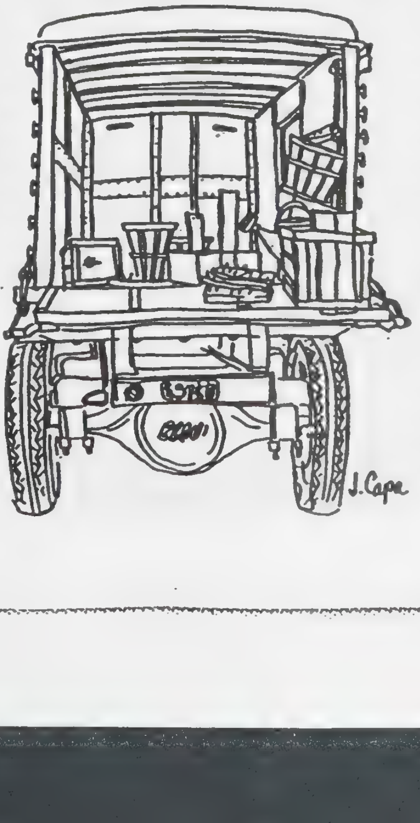
ing of liquid water below its freezing point is observed only in the tiny droplets that clouds are made of. Pyke could not have found reports in the scientific literature of anyone's making more than a thimbleful of supercooled water, but this did not diminish his enthusiasm for its use by the ton.

My own next problem was to find a site for the building of a bergship. How could we follow Churchill's sensible directive to let nature do the job? Surveying the world's weather maps, I was unable to find a spot on earth cold enough to freeze two million tons of pykrete in one winter. Nature would have to be aided by refrigeration. Eventually, we chose Corner Brook, in Newfoundland, where wood pulp provided by the local mills was to be mixed with water and frozen into blocks in a two-hundred-acre refrigeration plant. The problem of launching our Leviathan was to be circumvented by laying down the first pykrete blocks on wooden barges cramped together to form a large floating platform. This would gradually sink as the mass of pykrete was built up. The prototype was to be built in the winter of 1943-1944, to be followed by a fleet of bergships constructed on the North Pacific Coast the following winter, in time for the invasion of Japan.

One day, Mountbatten called me into his office to ask who should represent Habakkuk at a high-level meeting. I suggested Bernal as the only man who possessed the technical knowledge, the intellectual stature, and the persuasiveness to stand up to the war leaders. Bernal was the most brilliant talker I have ever encountered. The son of a wealthy Irish Catholic farmer, he soaked up knowledge from an early age like blotting paper, and became mesmerized by science. Once, he tried to generate X-rays by focussing the light from a paraffin lamp so as to see through his hand, and nearly set the farm on fire; he was beaten by his father. He was converted to Communism in 1922, when he was a student at Cambridge, and remained a faithful Party member all his life. (He died in 1971.) Bernal is mentioned in Andrew Boyle's recent book "The Climate of Treason" as one of the founders of the Cambridge Communist cell in the nineteen-thirties, but he made no secret of his allegiance and was never suspected of disloyalty to Britain. As a Cambridge undergraduate in the early twenties, he studied natural sciences and then took up X-ray crystallography, a physical method used for determining the arrangements of atoms in solids. When I joined him as a graduate student, in 1936, he was at the height of his powers, with a wild mane of fair hair (no beard), sparkling eyes, and lively, expressive features. We called him Sage, because he knew everything from physics to the history of art. He was a bohemian, a flamboyant Don Juan, and a restless genius always searching for something more important to do than the work of the moment.

When war broke out, the authorities asked Bernal to assess the likely damage from aerial bombardment. He requested that his former research assistant be taken on to help him, but, to his astonishment, the request was refused on security grounds. Bernal ridiculed the decision and demanded to see the reason. When he was reluctantly shown the file, the papers stated that the man could not be trusted because he was associated with the notorious Communist Bernal.

Mountbatten, who liked to have unconventional people around him as counterweights to naval orthodoxy, appreciated Bernal's prodigious knowledge and his original approach to any kind of problem. Mountbatten himself impressed me greatly by his quick and decisive mind. The high-level meeting he was preparing for took place in Quebec in August of 1943, and was headed by Roosevelt and Churchill. Bernal staged a demonstration of pykrete which so impressed the war leaders that they decided to give Habakkuk the highest priority. Detailed plans for the immediate construction of a prototype were to be drawn up in Washington. The British team was ordered there forthwith—except for Pyke, whose mordant wit had upset the American military to the point where he was forbidden to come.



When the people at the United States Consulate in London saw my invalid Austrian passport, they said that they were not allowed to issue visas to enemy aliens, however vital for the war effort. Mountbatten's Chief of Staff tackled this trivial obstacle by phoning the Home Office and telling the people there to make me a British subject within the hour. But, like a parson asked to perform a shotgun wedding without calling the banns, the Home Office insisted on at least the semblance of its customary naturalization ritual. That night, a detective called on me at my lodgings, in Holland Park. Would I give the names of four British-born householders who could vouch for my loyalty? Normally, the detective said, he would make careful inquiries from each of them, but in my case he wouldn't bother. What near relatives did I have in enemy territory? Normally, he would cross-check my answers, but in my case he wouldn't bother. Had I been convicted of any crime? Yes, of riding a bicycle in Cambridge without lights. Normally, he would check the police records, but in my case he wouldn't bother. After an hour of such banter, I signed his form. Supposing I had gone and betrayed the secrets of pykrete to the Eskimos. Would the Prime Minister have assured Parliament that "the proper inquiries" had been made at the time of my naturalization to ascertain that I had not been an Eskimo sympathizer from an early age? The next morning, I swore allegiance to the King before a justice of the peace; my wife merely had to sign a piece of paper at home in Cambridge. The following day, I was issued a shiny blue passport that described me as a British Subject by Certificate of Naturalization Issued 3 September 1943. You cannot become an Englishman, as you can become an American, but at least we were no longer enemy aliens liable to be interned, and my new passport solved the United States visa problem.

The other members of the Habakkuk team had already sailed to New York. To catch up with them, I was now sent there by air. First, a Sunderland flying boat took me from Bournemouth to Shannon, where the British officers on board donned civilian clothes in deference to Eire's neutrality. From Shannon, Pan Am's Yankee

Clipper flying boat ferried us to Newfoundland in fourteen hours, and thence to New York Harbor, where we landed thirty-four hours after leaving London—a record time. When the immigration officer read in my passport that my British nationality was of exactly four days' standing, he decided to unmask this foreign agent whom the wily British were trying to foist on their unsuspecting ally, and subjected me to a sharp interrogation. When I had told him most of my life history, except for my involuntary sojourn in Canada, he began questioning me about relatives in the United States. A brother. What is his name? When was he born? What does he do? Where does he live? My heart thumped as I remembered that my brother's house had been searched by the F.B.I. when they found out that he had been in correspondence with a prisoner of war in Canada. Would this be in the immigration officer's file? If it was, he gave no sign, but continued. What other relatives? A sister. Where does she live? Prytania Street, New Orleans. Suddenly, his tense face relaxed into a broad grin. "But that's the street where I was born." And I was admitted. No one whose sister lived on Prytania Street, New Orleans, could be a spy.

On arriving in Washington, where I imagined the British team to be busy sixteen hours a day with the planning of the bergship's construction, I was surprised to find them all welcoming me at Union Station in the middle of a weekday afternoon. They wondered what the weather had been like in London when I left—a question that I diagnosed as an expression of homesickness—and seemed in no hurry to get back to their desks. The next morning, when I reported for duty in a hut outside the Department of the Navy Building, on Constitution Avenue, I heard that Habakkuk was under scrutiny by the department's naval engineers, and that pending their report there was nothing we could do. Lord Zuckerman, another of Mountbatten's wartime scientific advisers, recently explained to me why no one paid much attention to us in Washington. Shortly after our arrival there, Mountbatten left Combined Operations to become Commander-in-Chief of the Allied Forces in Southeast Asia. Since he had been Habakkuk's principal advocate, its priority took a deep plunge. So as not to idle away my time, I asked for permission to visit the Canadian physicists and engineers who had carried

out tests on ice and pykrete parallel to ours and had built a model ice ship, complete with insulation and refrigeration, on Patricia Lake. It was on this trip that I reëntered Canada as a free man, but I evaded my hosts' conventional question about whether this was my first visit. Back in Washington, I took a room in the suburbs, where I listened to a Republican fellow-lodger's denunciations of Roosevelt as a greater menace than Hitler. I read in the Library of Congress or went rock climbing on the banks of the Potomac until the United States Navy finally decided that Habakkuk was a false prophet. One reason was the enormous amount of steel needed for the refrigeration plant that was to freeze the pykrete, but the crucial argument was that the rapidly increasing range of land-based aircraft was making floating islands unnecessary. This was the end of Pyke's ingenious project.

It was hard for a civilian to find a place on a ship back to England, but finally I was allocated a berth in a first-class single cabin on the Queen Elizabeth, England's newest and fastest liner. When I stepped into my cabin, I found that I shared it with five others. One, a tall, dignified old gentleman, introduced himself as Mr. Coffin, Moderator of the Presbyterian Church in the U.S.A., and proudly announced that he was going to London to have tea with the Queen. To belie his lugubrious name, he entertained the rest of us by day with a great fund of stories; he also kept us awake at night with his loud snoring. The ship carried fourteen thousand American soldiers, sent to join the great armies that were to liberate France the following summer. Under big notices of "No Gambling," piles of dollar bills slid across mess tables in the lounge every few minutes as the great ship heeled over, steering its zigzag course to evade the U-boats. After six days, we steamed up the Firth of Clyde, where a large Allied battle fleet lay assembled in the gloomy winter morning, the sinister gray shapes anchored between the dark, cloud-covered mountainsides lending drama to a scene that looked like a Turner painting of a Scottish loch.

When I reported the demise of Habakkuk to my superior at the Admiralty the next morning, he was not surprised. Pyke was disappointed, but he was already busy on new schemes. One of them was the construction of a gigantic tube from Burma into China much easier than building a road over the mountains, he argued. Through this tube, Allied men, tanks, and guns were to be propelled to China by compressed air, like the pneumatic post in department stores, to help Chiang Kai-shek defeat the Japanese Army there. Another of Pyke's plans plotted the destruction of the Rumanian oil fields from which Germany derived most of its fuel. In the dark of night, one squadron of planes was to attack the fields with high-explosive and incendiary bombs, while another squadron was to drop a force of commandos nearby, charged with destroying the fields on the ground. How could they penetrate the defenses? Disguised as Rumanian firemen, they should capture a fire station and drive into the oil field with its engines, pretending that they were on their way to extinguish the fires started by the air raid but fanning them instead.

I had come to realize some months earlier that construction and navigation of the bergships might prove as difficult as a journey to the moon then seemed to me, yet Habakkuk was one of several apparently impossible projects conceived during the war; in

each case the question was not so much one of absolute feasibility as of whether the strategic advantages to be gained by carrying out the project were in proportion to the manpower and materials required. In retrospect, it seems surprising that Mountbatten should have taken any of Pyke's projects seriously, but then Mountbatten was the youngest member of the Chiefs of Staff and headed an organization set up for unconventional warfare. Faced with that task, he liked to attract to his headquarters men who had not been to Staff College and whose ideas were therefore less likely to be anticipated by the enemy—never mind if they wore no socks. In peacetime, most of Pyke's ideas would have been discarded as the science fiction they were, but Mountbatten relied for scientific advice on Bernal, without realizing that Bernal's one great failing was a lack of critical judgment. Pyke had the Cartesian's arrogant conviction that an intelligent human being could reason his way through any problem, rather than Francis Bacon's humble maxim that "argumentation cannot suffice for the discovery of new work, since the subtlety of Nature is greater many times than the subtlety of argument." I returned to Cambridge, sad at first that my eagerness to help in the war against Hitler had not found a more effective outlet, but later relieved to have worked on a project that at least never killed anyone—not even the Chief of the Imperial General Staff.

UNTIL recently, I did not know how and why, four decades ago, the British government had decided to intern and deport many thousands of innocent German and Austrian refugees and Italians living in Britain, and to start releasing them again a few weeks later, long before the danger of a German invasion had receded. I have now read "Collar the Lot!," Peter and Leni Gillman's history of the internment of aliens in Britain, which is based on a scholarly study of official documents that were released thirty years after the events and on interviews with many of the survivors. The book reveals a disheartening story of official callousness, interdepartmental intrigue, newspaper hysteria, public lies, lies told to Parliament and to the governments of the Dominions, and, as John Maynard Keynes said of David Lloyd George, decisions taken on grounds other than the real merits of the case. The book tells also of human suffering, and of a few upright individuals whose compassion turned the tide.

The story begins in the autumn of 1939, when the Home Office and the War Office were anxious to avoid a repetition of the wholesale internment of nearly thirty thousand mostly harmless Germans in squalid prison camps which had taken place during the First World War. The Home Secretary, Sir John Anderson, therefore established tribunals that classified Germans and Austrians as refugees from Nazi oppression, and ordered the internment only of those thought to be loyal to the Nazi regime.

On April 9, 1940, German forces invaded Norway, supposedly helped by a Fifth Column of Norwegian Nazis and by German spies posing as refugees. A month later, the Germans invaded Holland and Belgium, and Winston Churchill replaced Neville Chamberlain as Prime Minister. Churchill held his first Cabinet meeting on May 11th. At the insistence of the Chiefs of Staff, the reluctant Anderson was asked to abandon his enlightened policy and to intern all male Germans and Austrians living near the coasts that were threatened by invasion. A few days later, Sir Nevile Bland, the British Ambassador at The Hague, returned to London with alarming stories of treachery by German civilians in Holland. His photograph shows him supercilious and vacant, like a figure out of Evelyn Waugh's farcical novels about the British upper class. He realized that his important hour had come, and, at the end of May, he solemnly warned the nation in a radio broadcast, "It is not the German and Austrian who is found out who is the danger. It is the one, whether man or woman, who is too clever to be found out." Having pondered this profound truth, the Chiefs of Staff warned the Cabinet that "alien refugees [are] a most dangerous source of subversive activity," recommending that all should be interned. "The most ruthless action should be taken to eliminate any chances of Fifth-Column activities." On May 24th, Churchill told the Cabinet that he was in favor of removing all internees from the United Kingdom. Newfoundland and St. Helena were two of the inhospitable places to which Churchill proposed we should be banished. General Jan Smuts managed to do one better by suggesting the Falkland Islands instead. On June 10th, when Italy de-

clared war, Churchill ordered the Home Office to "collar the lot" of Italians living in Britain.

Among four thousand Italians interned during the succeeding two weeks, and among those supposedly most dangerous ones later selected for deportation overseas, were H. Zavattoni, the banquet manager at the Savoy Hotel, who had worked there since 1906; D. Anzani, the secretary of the anti-Fascist Italian League of the Rights of Man; Piero Salerni, an engineer urgently needed by the Ministry of Aircraft Production; and Alberto Loria, a Jew who had come to Britain in 1911. All except Loria were drowned on the Arandora Star.

In obedience to the Chiefs of Staff directive, the War Office ordered that those who had survived the torpedoing of the Arandora Star be reëmbarked a few days later on the Dunera, a ship bound for Australia. Among those guarding the internees as they boarded the Dunera at Liverpool Harbor was a young soldier named Merlin Scott. That night, he wrote a letter home. "I thought the Italian survivors were treated abominably—and now they've all been sent to sea again," his letter said. "The one thing nearly all were dreading, having lost fathers, brothers, etc. the first time. . . . Masses of their stuff—clothes etc. was simply taken away from them and thrown into piles out in the rain and they were allowed only a handful of things. Needless to say, various people, including policemen! started helping themselves to what had been left behind. They were then hounded up the gangway and pushed along with bayonets, with people jeering at them. . . . Masses of telegrams came for them from relatives nearly all just saying 'Thank God you are safe,' and they were not allowed to see them." The telegrams "had to go to a Censor's Office . . . Some of them said they had no mail for six weeks." Shortly after the Dunera left harbor, a German submarine fired two torpedoes at it, but the Dunera happened to change course, and the torpedoes missed the ship by about a hundred yards.

Merlin Scott's father was an assistant under-secretary at the Foreign Office. His son's letter made the rounds of the office and was shown to Lord Halifax, the Foreign Secretary. He forwarded it to Sir John Anderson, the Home Secretary, together with a memorandum expressing concern about the bad effect that such inhumanity would have on public opinion at home and in the United States. Halifax and Anderson won over Chamberlain, who until then had been the chief executor of Churchill's deportation policy, and on July 18th, only a week after Scott had written his letter, Chamberlain persuaded the Cabinet that "persons who were known to be actively hostile to the present regimes in Germany and Italy, or whom for other sufficient reasons it was undesirable to keep in internment, should be released." The Cabinet also agreed that the "internal management, though not the safeguarding," of the internment camps should be transferred from the War Office to the Home Office. The deportations were stopped.

The Canadian government at first stonewalled Paterson's proposal to release in Canada those refugees who did not want to return to England, and the American State Department refused to admit even those refugees who had already held immigration visas before they were interned. Early in 1941, Ruth Draper, the great diseuse, gave one of her heartwarming performances in Ottawa for the Canadian Red Cross. Afterward, the Prime Minister asked her what Canada could do in return. She told him, "There is a young innocent boy, whom I have known since he was a baby, being held in one of your internment camps behind barbed wire, without offense, without a trial." The Prime Minister ordered the boy's release, and his decision opened the door for others. When I paid my return visit to Canada, in October of 1943, the last internment camp had just been dismantled. The Gillmans' book shows that even in wartime one person's compassion can sometimes prevail against hardened politicians and the military.

As far as I know, historical research has found no substance in the ugly rumors of spying by Germans who posed as refugees, either in Norway or in Holland; nor was there ever a case of a German or Austrian refugee in Britain who aided the enemy. Merlin Scott, whose letter saved so many Italians in Britain from internment and deportation, was killed by the Italians in Libya during the first British advance, early in 1941.

—M. F. PERUTZ

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Now inside the orbit of Jupiter, amateur astronomers with small telescopes will be able to see Halley's comet this fall.—*San Diego Union.*

If they hold their breath.

The Pioneer Defended

The Private Science of Louis Pasteur
by Gerald L. Geison.
Princeton University Press,
378 pp., $29.95

M.F. Perutz

> *"There is a real world independent of our senses; the laws of nature were not invented by man, but forced upon him by that natural world. They are the expression of a rational world order."*
>
> —Max Planck,
> *The Philosophy of Physics*

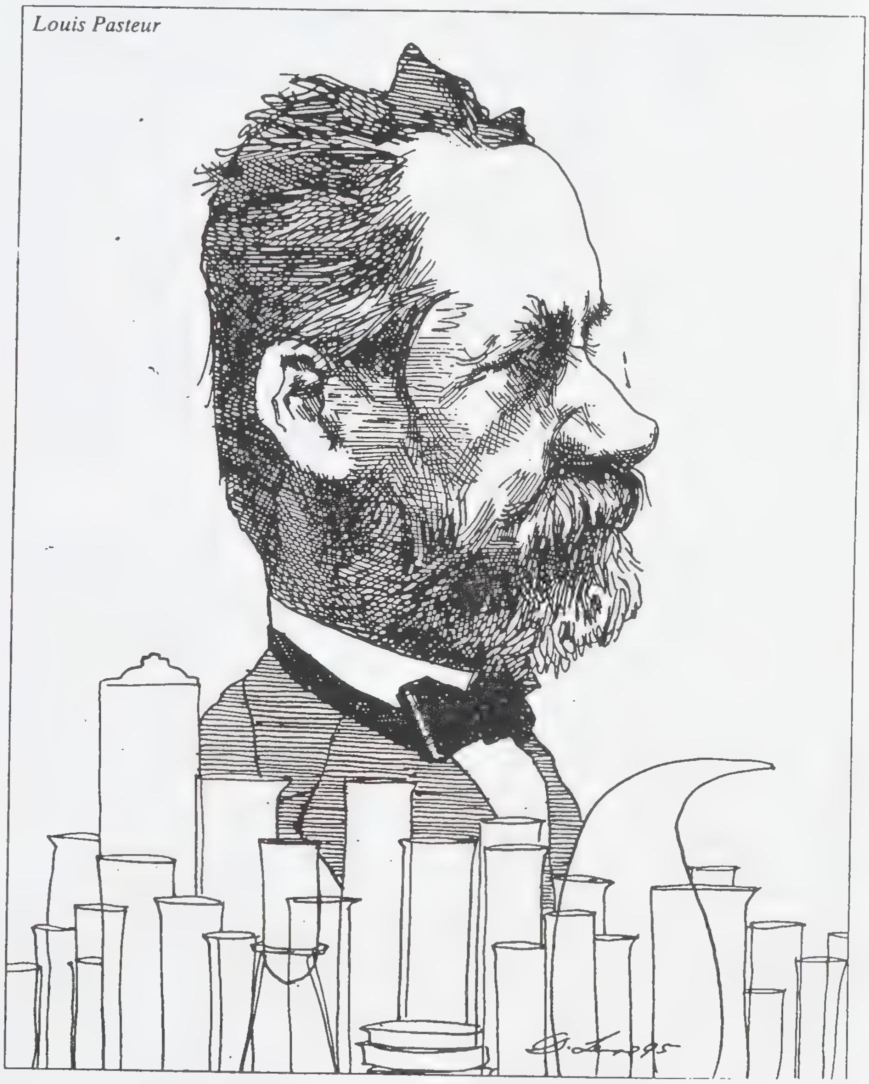
Louis Pasteur was the father of modern hygiene, public health, and much of modern medicine. He was born in 1822 at Dole, halfway between Dijon and Besançon in eastern France, where his father owned and ran a small tannery. He attended school in nearby Arbois, obtained his first science degrees in Besançon, and in 1847 graduated with a doctorate in science from the Ecole Normale Supérieure in Paris. Scientists at that time believed that the fermentation of grapes, or the souring of milk, or the putrefaction of meat, were all purely chemical processes unrelated to microorganisms.

The causes of infectious diseases were unknown. Malaria was believed to arise from "miasmas" emanating from swampy ground; outbreaks of plague were attributed to unfavorable constellations, to comets, to the wrath of God, or even to the poisoning of wells by Jews, who often paid for it with their lives. The "animalcules" first observed by the Dutchman Anton van Leeuwenhoek in the seventeenth century were believed to arise spontaneously in decaying meat or vegetable matter; they had not as yet been connected with disease. In the eighteenth century Edward Jenner had introduced vaccination against smallpox with liquid drawn from the pustules of pox-infected cows, but the infectious agents involved were unknown, and vaccination against other diseases did not exist.

Pasteur revolutionized science by proving that fermentation and putrefaction are organic processes invariably linked to the growth of microorganisms; that these never arise spontaneously from inanimate matter but only by reproduction of their own kind; that they are ubiquitous in the environment, but can be killed by subjecting them to heat, the process now known as pasteurization. He showed that infectious diseases of silkworms, animals, and human beings are caused by microorganisms and he devised ways of preventing them by vaccination. His discoveries inspired Joseph Lister in London to introduce antiseptics into surgery, which reduced mortality to a fraction of what it had been. Shortly before his death in 1895 two of Pasteur's pupils discovered that bubonic plague is caused by bacteria which are transmitted by fleas from dead rats to man, a discovery that helped to eliminate plague from much of the world.

Pasteur led a simple family life and devoted all his time to research. To generations of Frenchmen and to many others, Pasteur's has been the image of the selfless seeker after the truth who was intent on applying his science for the benefit of mankind. In *The Private Science of Louis Pasteur,* Gerald L. Geison, a historian of science, claims to have deconstructed Pasteur, and to have produced "a fuller, deeper and quite different version of the currently dominant image of the great scientist." I propose to deconstruct his deconstruction and restore the rightly dominant image.

Geison analyzes Pasteur's major discoveries: the asymmetry of biological compounds; fermentation; the vaccines against anthrax and rabies; and his demonstration that life is not generated spontaneously from non-living matter. By a painstaking comparison of Pasteur's notebooks with his publications, Geison claims to have found him guilty of deception, of stealing other peoples' ideas, and of unsavory and unethical conduct. Some of these claims are scientifically flawed, while others defy common sense.



Louis Pasteur

Geison's argument follows the line laid down by certain social theorists who assert that scientific results are relative and subjective, because scientists interpret empirical facts in the light of their political and religious beliefs, and under the influence of wider social and cultural pressures.

They allege that instead of admitting their preconceptions, scientists misrepresent their findings as absolute truths in order to establish their power.

Pasteur transformed medicine, but he started out as a chemist and devoted the first ten years of his career to the study of a seemingly recondite matter, the relationship between the crystalline forms of certain salts of tartaric acid, a compound found in wine gone sour, and the effects of solutions containing them on polarized light transmitted through them. Acute observation and brilliant reasoning led him to discover that tartaric acid can exist in two alternative forms which are chemically indistinguishable, but which have their component atoms arranged asymmetrically in space so that they are mirror images of each other, like left and right hands. Since such asymmetries had never been observed in compounds synthesized in the laboratory, Pasteur reasoned that the capacity to produce them must be an intrinsic property of the living cell and soon proved his point with other examples. This was one of the great discoveries in chemistry and immediately established Pasteur's reputation. Since tartaric acid is a product of fermentation, the discovery led him to the study of fermentation as such and to research on disease.

Geison insinuates that Pasteur cheated because the effects on polarized light of his right- and left-handed salts of tartaric acid, which according to Pasteur's interpretation should have been exactly equal and opposite, were in fact slightly different. Geison writes:

> Pasteur minimized the difference—in effect, he explained it away—by pointing to the difficulty of completely separating the two... forms [of crystals]. The deviation would "probably be the same for very well-chosen crystals" he now claimed.

I challenge Geison to go to the chemistry department at Princeton University, where he is a professor of history, and to repeat Pasteur's experiment. He will have a hard time getting as close an agreement between the two measurements as the very skillful Pasteur did. Later experiments by others proved Pasteur's explanation of the small discrepancy exactly right; but it seems that, because references to right and wrong would imply the existence of objective truth, they have been eliminated from the vocabulary of Geison's school of sociologists of science.

Geison also accuses Pasteur of concealing the guidance he had received from his teacher, Auguste Laurent, because such acknowledgment might have implied sympathy with Laurent's radical political views and could have been damaging to Pasteur's career. But Geison's own book shows that Laurent's ideas on the relationship between the crystalline forms of tartaric acid and the transmission of polarized light when they are dissolved were misleading, confirming Pasteur's statement that under Laurent's guidance he "was enveloped by hypotheses without basis." Politics had nothing to do with the judgment, and Geison's accusation of opportunism is unjustified.

Pasteur's turn from molecular asymmetry to fermentation has often been attributed to his having some connections with the brewing industry while he worked at Lille, but Geison writes that Pasteur's notebooks confirm his statement that "the 'inflexible' internal logic of his work" led him to it. Fermentation of grapes produces an alcohol that does not affect polarized light; but Pasteur discovered that other products of fermentation included an alcohol that did. Since he associated that property with living organisms, he concluded that fermentation must be an organic process performed by microorganisms rather than a purely chemical one, as the great chemists Jöns Jakob Berzelius and Justus von Liebig maintained, and he demonstrated this in a series of brilliant experiments which convinced everyone except Liebig, who stuck to his wrong theory.

Pasteur next asked if these microorganisms arose spontaneously from inanimate matter, as was then widely believed. He answered this question in a brilliant lecture in the grand amphitheatre of the Sorbonne before a distinguished audience. After linking spontaneous generation to the kind of materialism in which there is no need for a divine creator, a doctrine abhorrent to himself, and, as he knew, also to the Church and the royal family, he stressed that "neither religion, nor philosophy, nor atheism, nor materialism, nor spiritualism has any place here.... It is a question of fact. I have approached it without preconceived idea." Geison disputes this and alleges that his "approach to the question... was strongly conditioned by an intertwined set of philosophical, religious, and political interests." But he does not give any clear evidence for the claim.

The experiments Pasteur described in that lecture were stimulated in part by Félix-Archimède Pouchet, a biologist in Rouen who claimed that living eggs are generated spontaneously by a "plastic force" in dead plant and animal debris, and that microorganisms arise spontaneously in liquid extracts (or "infusions") made from boiling hay, even when they are exposed to chemically produced, hence sterile, oxygen. By contrast, Pasteur demonstrated that sugared yeast water, boiled briefly, would not ferment when exposed to sterile air. As a final demonstration, he took sterile sugared yeast water to the Mer de Glace above Chamonix and opened the bottles there. As he expected, the air was germ-free and no fermentation occurred. To disprove him, Pouchet took his boiled-hay infusions up a glacier in the Pyrenees and found that they fermented. Pasteur dismissed this finding as the result of sloppy work in preparing the boiled hay—unjustly, as it later turned out.

Pasteur's predecessor, the great naturalist Georges Cuvier, had already disputed the idea of spontaneous generation as unproven, and had associated the idea with the philosophers responsible for the French Revolution, on the ground that it denied the divine creation of life. Later, spontaneous generation became associated with materialism and also with Darwinism. Against this background, Geison castigates Pasteur's conduct in his controversy with Pouchet over fermentation, because some of Pasteur's own attempts at preventing fermentation of sugared yeast water had not succeeded, and many of his other experiments had also failed. According to Geison, he disregarded these failures because his religious and political views prejudiced him against taking them seriously. Geison implies that Pasteur acted dishonestly by not repeating Pouchet's experiment with hay infusions, since, according to the orthodox scientific method as he understands it, a single disproof of a hypothesis invalidates all previous supporting evidence. For his part, Pasteur once remarked wisely: "In the observational sciences, unlike mathematics, the absolutely rigorous demonstration of a negation is impossible." Geison dismisses this as unscientific.

In fact, scientists rarely follow any of the scientific methods that philosophers have prescribed for them. They use their common sense. Having convinced himself by the most rigorous possible experiments that fermentation will not take place under sterile conditions, Pasteur could be confident that any contrary evidence was the result of error and he wasted no time searching for it. He felt sure that the source of the error was bound to emerge eventually. It did indeed fourteen years later when Pouchet's boiled-hay infusions were found to have contained heat-resistant bacterial spores which boiling would have failed to kill. Geison attributes Pasteur's immediate victory over Pouchet to his gift for persuasive advocacy rather than to the intelligent judgment of his audience, which Geison dismisses as a scientific elite in league with Pasteur's quest for power. Now that we have seen that the complexity of life on the atomic scale is vastly greater than that of non-living matter, the idea of spontaneous generation seems even more absurd than it did in Pasteur's time. But this seems not to be apparent to Geison; the line of thought with which he is associated may define such generation as just an alternative paradigm.

Pasteur's discovery that all fermentation was caused by the actions of microorganisms convinced him that the same must be true of contagious diseases. He observed that animals which had recovered from a disease became immune to reinfection by the same disease. From there it was only a short step to the idea that if virulent microorganisms could somehow be reduced in potency, they might serve as vaccines that would make animals immune to infection by fully potent forms of the same organisms.

Pasteur and his young collaborator Emile Roux first put this idea into practice in a vaccine against a type of cholera that affects chickens and other domestic birds. Pasteur published his work in 1880, and his new cholera vaccine for poultry became available soon afterward. Pasteur's and Roux's next target was anthrax, which was then decimating French sheep and cattle. The vaccine they developed contained live anthrax bacilli, whose effect was "attenuated" so as to render them non-infectious. This was done either by exposing cultures of virulent anthrax bacilli to air at 42°–43°C—i.e., "oxidizing" them—or subjecting them to the process of "passage," by which one animal that is not susceptible to the disease—say, a mouse—is injected with a small quantity of bacteria, which, after they have multiplied, are injected into a second mouse, and so on.[1] Two of Pasteur's collaborators, Charles Chamberland and Emile Roux, used potassium bichromate, an oxidizing agent, to produce attenuating effects on bacteria that were similar to those from exposure to air, but that were faster and perhaps more drastic. From what we know today, we can conclude that all these treatments probably induced genetic mutations that weakened the bacteria without killing them. Pasteur's competitor Jean-Joseph Henri Toussaint tested other vaccines consisting of anthrax-infected sheep blood that was heated or treated with carbolic acid, the disinfectant which Joseph Lister in London had introduced to kill bacteria. Toussaint's vaccines produced variable results, as indeed Pasteur's did initially.

In 1881, Pasteur's first publications about his air-oxidized anthrax vaccines drew a challenge for a public trial from veterinarians who were upset that a chemist was poaching on their preserve. Twenty-four sheep, one goat, and four cows were given two successive protective vaccinations before the trial; another twenty-four sheep, one goat, and four cows were left unvaccinated. On May 31 all animals received injections of virulent anthrax bacilli. By the day of the public trial, on June 2, all the unvaccinated sheep and the goat were dead and the cows were very sick, while the vaccinated animals were alive and healthy, except for one ewe which died the following day. A post-mortem showed that it carried a fetus that had died about two weeks earlier.

Geison tells us that he found evidence in Pasteur's notebooks that this triumph was achieved not with Pasteur's own air-oxidized vaccine, but with Roux's and Chamberland's bichromate-oxidized vaccine, which Chamberland had attenuated further by three passages through mice. After the trial, however, Pasteur continued to develop the air-oxidized vaccine; it was soon used successfully by farmers throughout the world. By 1894, 3,400,000 sheep had been vaccinated and mortality from anthrax had fallen to 1 percent, compared to 9 percent for unvaccinated sheep.[2]

Pasteur did not make a public statement that the trial vaccine had been oxidized by bichromate; but neither did he claim that it had been oxidated by air. All the same, Geison accuses him of having "actively misrepresented the nature of the vaccine actually used" and of "a significant and undeniable element of deception." (I wonder what difference there is between "misrepresenting" and "actively misrepresenting.") Geison also accuses Pasteur of taking credit that belonged to his competitor Toussaint, because Toussaint was the first to use an antiseptic, such as carbolic acid, to treat the sheep blood used for vaccines, and Geison regards the potassium bichromate used by Pasteur as just an alternative antiseptic.

Both accusations are based on misconceptions. Carbolic acid (phenol, as it is now called) kills bacteria, while Chamberland's bichromate treatment kept them alive, as shown by their subsequent passage through mice. One of the empirical findings of immunology, probably discovered by Pasteur himself, is that live attenuated vaccines are more effective than dead ones. (For example, this is why Sabin's polio vaccine has proved more effective than Salk's.)

Besides, Pasteur pointed out that Toussaint's vaccine would be extremely difficult to adapt for practical use because, unlike his own, it could not be propagated in cultures. Therefore Pasteur owed no intellectual debt to Toussaint. There is, moreover, no qualitative difference between the attenuating mutations induced by exposure to bichromate and those induced by exposure to air; both processes have an oxidizing effect. Hence there was no deception about the nature of the vaccine used. Under the pressure of the public trial, Chamberland and Roux apparently decided that the bichromate vaccine was safer, but Pasteur later preferred his own, air-oxidized vaccine. So what? The charge of "active misrepresentation" is ridiculous, especially since Pasteur's air-oxidized vaccine was used successfully until long afterward.

The next vaccine Pasteur developed, against the rabies virus, was also air-oxidized. In July 1885, a couple from Alsace brought to Pasteur's laboratory their nine-year-old son Joseph Meister, whose hands and legs had been severely bitten fourteen times by a rabid dog. Rabies has a long incubation period, so that a vaccine given soon after infection still stands a good chance of success. Pierre-Victor Galtier's earlier transmission of the infectious agent from rabid dogs to rabbits led Pasteur to the idea of attenuating it by repeated passages through rabbits. Pasteur's young collaborator Emile Roux then thought of attenuating the power of the infection by exposing strips of fresh spinal marrow taken from a rabbit that had died of rabies to dry, sterile air for various lengths of time. A small piece of marrow ground up and suspended in sterilized broth was then used as a vaccine.

Pasteur first gave the boy an injection derived from the most attenuated strips of spinal cord that had been dried longest, and he followed this by injections of less and less attenuated strips of spinal cord that had been dried for successively shorter periods. They saved Joseph Meister's life and also that of a fifteen-year-old shepherd boy, Jean-Baptiste Jupille,

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[1] René J. Dubos, *Louis Pasteur* (Little, Brown, 1950).

[2] Dubos, *Louis Pasteur*, p. 343.

who had been severely bitten while trying to save other boys from being attacked.

Pasteur realized later that Roux's preparation of the vaccine actually killed a progressively greater proportion of the rabies virus instead of merely attenuating its effect; all the same, the vaccine proved effective. In a paper published in 1985 Dr. Hilary Koprowski, formerly president of the Wistar Institute in Philadelphia, wrote:

> Young Meister's treatment took place on 6 July 1885.... By 12 April 1886, 726 people had been treated, 688 after having been bitten by dogs and 38 by wolves. There were four deaths. By 31 October, 2490 had been vaccinated, and since then the Pasteurians have had the last word.... Despite many modifications, confidence in the original product was so strong that it was used until 1953 when the last person was vaccinated at the Pasteur Institute in Paris with Pasteur's original preparation.[3]

Nevertheless, Koprowski has written to me that it is hard to say even today whether Pasteur's vaccine was completely safe for human beings and whether some of the rabies infections that occurred after Pasteur's original vaccination were from the animal bite or from the vaccine. He reports that a vaccine made from rabies virus grown in cultures of human fibroblast (skin cells) has recently proved both efficacious and safe. It is known as the "human diploid vaccine" and is manufactured at the Mérieux Institute in Lyon for use throughout the world.

Dr. Michel Peter and other physicians accused Pasteur, a mere chemist, of having used an insufficiently tested vaccine. Geison eagerly takes up their case and accuses Pasteur of unethical conduct, because "boldly, even recklessly, Pasteur was willing to apply vaccines in the face of ambiguous experimental evidence about their safety or efficacy." Pasteur had indeed obtained variable results when he tried to immunize dogs, beginning with the most briefly dried, and therefore least attenuated and most virulent, strips of spinal cord, and then followed that treatment with injections from spinal cords dried for progressively longer periods. But in a subsequent trial on dogs begun forty days before his vaccinations of Meister, Pasteur had reversed that order, and none of the dogs he treated contracted rabies, despite the virulence of the final injection. Twenty-seven days elapsed between that final injection and the first injection he gave Meister—a period that would have been long enough for the dogs to develop rabies symptoms if Pasteur's second procedure had been faulty. In the face of this evidence, Pasteur would have been timid and heartless to refuse the desperate appeal by Joseph Meister's father. Geison's accusation that Pasteur's successful attempt to save Meister's life was unethical is without foundation.

In Pasteur's day there was no way of being sure either that a suspected dog really had rabies or that it had infected its victim. Pasteur's enemies benefited from these uncertainties, but in 1888 they seem to have been silenced by the report of an English commission which repeated and confirmed the successful vaccinations of dogs and concluded:

> From the evidence of all these facts, we think it certain that the inoculations practiced by M. Pasteur on persons bitten by rabid animals have prevented the occurrence of hydrophobia in a large proportion of those who, if they had not been so inoculated, would have died of that disease. And we believe that the value of his discovery will be found much greater than can be estimated by its present utility, for it shows that it may become possible to avert, by inoculation, even after infection, other diseases besides hydrophobia.[4]

Pasteur had no medical degree and therefore could not carry out the injections of Meister and Jupille himself. They were done not by Pasteur's own medical collaborator, Emile Roux, but by two other doctors. From this fact Geison speculates that Roux refused to give the injections because he considered them unsafe, and that he fell out with Pasteur over this issue. But Geison cites no documentary evidence for this claim.

According to Geison, Pasteur's colleagues supported him against the accusations of Peter and other physicians because Pasteur's treatment of Meister "was a symbolic rallying point in a wider struggle for cultural authority and power.... Critics of Pasteur's treatment for rabies...were...pushed aside in pursuit of a larger project: to secure the cultural domination of modern 'professional science.'" Might they not have been pushed aside because Pasteur's vaccine worked?

It is remarkable that Pasteur achieved his phenomenal practical successes while his theoretical concepts were still far from accurate. At first he thought his live, attenuated vaccines caused immunity by consuming the nutrients in the host, leaving none for virulent bacteria. Later he changed his mind and believed that the attenuated bacteria released a toxin that stopped further bacterial growth, but in 1890 Emil Behring and Shibasaburo Kitasato in Berlin discovered that the toxin was released not by the bacteria but by the defenses of the host animal in the form of antibodies.

Yet Geison has little to say about the correct explanations for the efficacy of vaccines, or for other phenomena, perhaps because his ideological approach denies their very existence. According to him, "Pasteur shared with many of his peers a rather simple-minded and absolutist notion of scientific truth, rarely conceding the possibility of its being multifaceted and relative." According to Geison, Pasteur's "scientific beliefs and modus operandi were sometimes profoundly shaped by his personal concerns, including his political, philosophical and religious instincts," while "...the real individual scientist...tries to navigate a safe passage between the constraints of empirical evidence on the one hand and personal or social interests on the other."

Had Michael Faraday's discovery of electromagnetic induction been "multifaceted and relative," there would be no electric power; had Albert Einstein's concept of the relation between mass and energy and James Chadwick's discovery of the neutron been relative, there would be no nuclear power and no atomic bombs; had Erwin Schrödinger's wave equation been relative, there would be no computers. Nor is there a shred of evidence that any of these scientists, or Ernest Rutherford, or Alexander Fleming, or James Watson, or Francis Crick, had to "navigate a safe passage between the constraints of empirical evidence on the one hand and personal and social concerns on the other." Such concerns may have made Galileo and Darwin hesitate to publish their revolutionary ideas, and they may affect some of today's scientists who try to disentangle the respective influences of nature and nurture on human behavior; but these are exceptions. I cannot think of any Nobel Prize–winning discovery in physics, chemistry, or medicine that was based on anything other than empirical evidence or mathematical insight.

According to Geison, it is now a commonplace among historians and sociologists of science that science, no

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[3] *Bulletin de l'Institut Pasteur*, No. 83 (1985), pp. 301–308.

[4] Dubos, *Louis Pasteur*, p. 352.

less than any other form of culture, depends on rhetorical skills. I have known scientists who possessed great rhetorical skills which failed to conceal the shallowness of their research from their peers. On the other hand, Alexander Fleming's lectures put everyone to sleep, while his discovery of penicillin made him one of this century's most famous scientists. Good research needs no rhetoric, only clarity. The entire approach emphasizing "relative" truth seems to me a piece of humbug masquerading as an academic discipline; it pretends that its practitioners can set themselves up as judges over scientists whose science they fail to understand.

Toppling great men from their pedestals, sometimes on the slenderest of evidence, has become a fashionable and lucrative industry, and a safe one, since they cannot sue because they are dead. Geison is in good company, but he, rather than Pasteur, seems to me guilty of unethical and unsavory conduct when he burrows through Pasteur's notebooks for scraps of supposed wrongdoing and then inflates these out of all proportion, in order to drag Pasteur down. In fact, his evidence is contrived, and does not survive scientific examination.

Pasteur may have been domineering, intolerant, pugnacious, and, in his later years, a hypochondriac who searched every slice of bread for bacteria before eating it; but he was courageous, compassionate, and honest, and his scientific achievements, which have much reduced human suffering, make him one of the greatest benefactors of mankind. Joseph Meister became the proud janitor of the Pasteur Institute in Paris. He killed himself in June 1940 rather than open the crypt where Pasteur lies buried to the invading Germans. In 1922, the French Ambassador to the United States, Jules Jusserand, said in a speech: "In the course of its history, France has produced many great men. There is no one of whom we are prouder than Pasteur.... Some years ago, before the war, a newspaper organized a kind of plebiscite and asked its readers who in their view were France's greatest sons. 2,300,000 replies came, and in this militaristic nation of ours...the emperor Napoleon came seventh and Pasteur came first."[5] □

---

[5] *Bulletin de l'Institut Pasteur*, No. 83 (1985), pp. 301–308.

The New York Review

December 21, 1995

Cambridge 14.4.85

Pls translate
asap = Thanks!
Max
Perutz

Dear Dr [illegible],

Many thanks for your letter of 28 March. I enjoyed [illegible] reflections on the Arab [illegible] from the [illegible] with Egypt, [illegible] they need a large part of [illegible] of the [illegible]

[illegible] helpful for [illegible]

With kind regards to you [illegible]

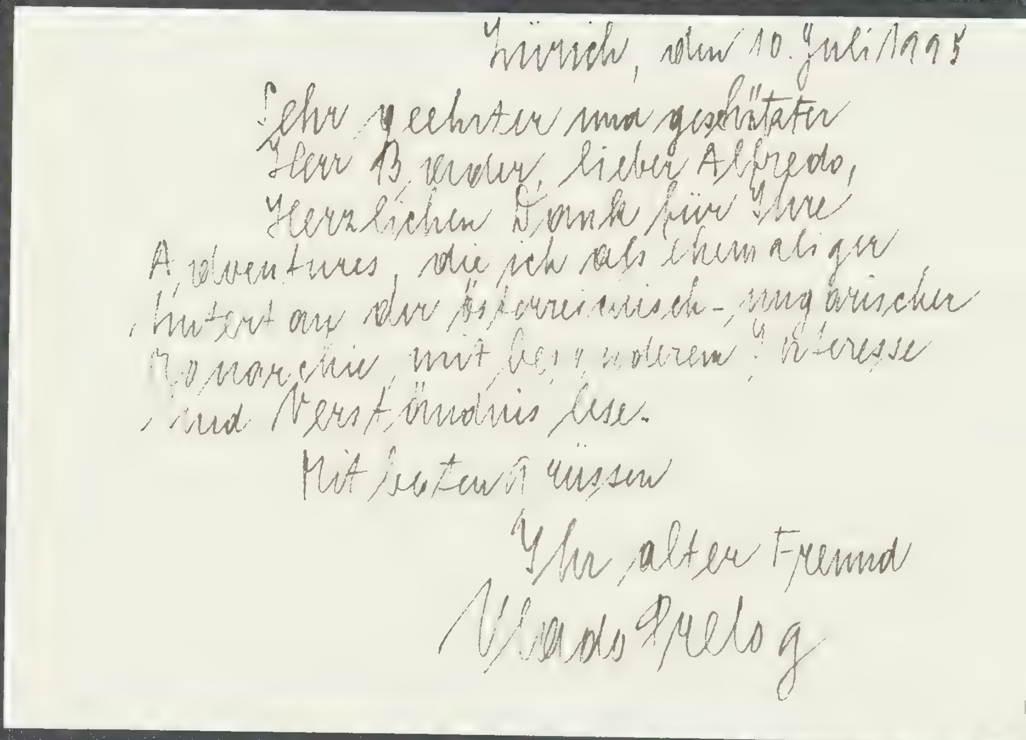
Yours

RIEFVERSAN

File.
V. Prelog

Mr
Alfred Bader
52 Wickham Ave.
Bexhill on Sea
E. Sussex TN39 3ER
England

Laboratorium für org. Chemie
ETH-Zentrum
Universitätstr. 16
CH-8092 Zürich
Prof. Dr. V. Prelog



Zürich, den 10. Juli 1995

Sehr geehrter und geschätzter
Herr Bader, lieber Alfredo,
Herzlichen Dank für Ihre
Adventures, die ich als ehemaliger
Untertan der österreichisch-ungarischen
Monarchie mit besonderem Interesse
und Verständnis lese.

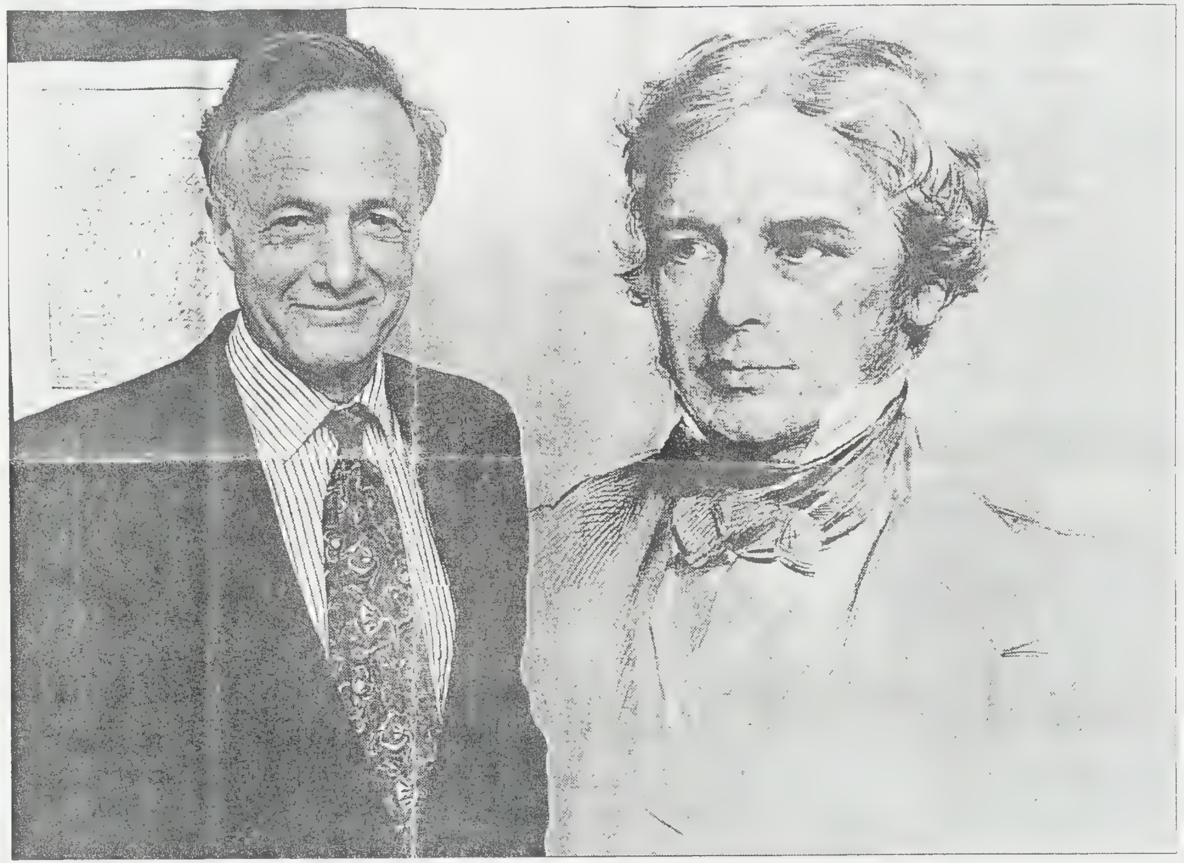
Mit besten Grüssen

Ihr alter Freund
Vlado Prelog

Give free rein to ideas: profits will follow

The Government's White paper on science placed new emphasis on wealth creation and laying down research targets. But the Canadian Nobel Laureate **Prof John Polanyi**, in an address to the Royal Institution this week, highlighted the pitfalls of managing scientific creativity. His talk, entitled 'How discoveries are made, and why it matters', was made at a meeting opened by junior science minister, John Horam. Here is an extract

Picture: ROB JUDGES



Prof Polanyi, pictured with a Michael Faraday display at the Royal Institution. His talk was given during a special celebration to mark the one hundredth Faraday Discussion of the Royal Society of Chemistry

SCIENTIFIC discoveries result from the accumulation of scientific evidence. Only the trained eye can see them gradually taking shape. There is no set level of evidence at which the bell of proof rings. It follows that governments seeking to maximise the return in terms of discoveries per research expenditure will be dependent on the judgment of scientists in applying the accountancy that good business practice requires. The only escape from this "peer review" is into a never-never land of waste.

Discoveries are made gradually and seldom. There is no guarantee that this gradual process, once begun, will continue. Lord Kelvin made the definitive statement on the prevalence of false pregnancies in science. He was asked what, in his long life in science, stood out most clearly. He thought for only a moment before replying: "Failure."

High-gain activities tend to be high-risk, otherwise the world would be even more unstable than it is. But high-risk activities pose a problem for administrators, whose job it is to reduce waste and therefore risk.

Stalin once summoned the Soviet Union's leading film-maker to the Kremlin to inquire how many films he had made in the past year. "Eighteen," was the impressive response. And how many of those would you regard as having fully succeeded, inquired the great one, ominously. "Three," replied the film-maker (exaggerating, surely). "In that case, next year you will make three films," Stalin said, terminating the interview.

So, which three? In the case of science it would, in my country and in Britain, be those few discoveries that had most clearly contributed to what in previous years we called "socio-economic well-being" and today, more desperate or grasping, we simply call "wealth".

Governments have, however, had much more success applying this type of criterion in retrospect than in prospect. In experiments extending over three-quarters of a century, a number of European nations proved to their people's satisfaction that they were unable to pick out in advance the businesses that would be most profitable. It is a curious fact, however, that if, to the difficulty of predicting what business will yield profits, one adds the further imponderable of predicting what scientific discoveries will benefit what businesses, the whole undertaking appears once more to be, if not feasible, at least saleable. Basic science must, after all, be accountable, and here is a method to make it seem so. The fact that it will replace real criteria of worth, namely scientific achievement, by what must needs be a judgment based on the seat of a bureaucrat's pants, tends to be overlooked.

The problem is more serious than I have yet suggested, for science today is not merely threatened by shaky *post hoc* judgments but also by the *pre hoc* provision of stylistic guidelines. We may not be alone in Canada in having Centres of Excellence at our universities in which the criterion of excellence is clearly stated to count for 20 per cent in the assessment. It is outweighed four to one by ill-defined, because indefinable, criteria having to do with the qualities of management, teamwork and relevance.

And yet, worldwide, the lesson has been learnt, where commerce is concerned, that the risk taker and not the legislator is the best decision-maker. Why is this not also thought to be true in the highly competitive market for new ideas?

Perhaps because governments have yet to be persuaded that the self-regulating system of the market-place for goods has relevance to that for ideas. Does science actually have a "bottom line"? Are international prizes (to give an example) perhaps distributed by lot? Some would be loath to admit it. Is it the case?

CONSIDER the record of 20th-century science, guided so far almost exclusively by scientists on the basis of scientific merit in producing scientific advances of importance. The record is so spectacularly good as to raise fears that we cannot cope with the tidal wave of discovery. It would appear that the criteria by which the scientific community has been determining the profitability of scientific investments are real and effective.

Is there nothing, then, in the management of science that needs improvement? Do we merely need to set our scientists free so that they can make discoveries where nature permits, rather than where governments propose?

No, there is more that we can do. We can further improve the communication between the basic scientist and the applied, the academic laboratory and the market-place for goods.

Much has, however, already been done on these lines in recent years. So much that we must now be careful not to let the applied sector set the agenda for the basic, or we shall find that we have spent the bulk of our intellectual capital in pursuit of near-term profit. The objective must be to use what we know, not to restrict what we know to what we know how to use.

Beyond using what we know lies a further objective: to use it wisely. This will require that we levy a tax of citizenship on knowledge. There must be an obligation on scientists (not all of them always, but many from time to time) to participate in the public debate by which our future is determined.

This is not because scientists are possessed of special wisdom, but because they have a special form of literacy, and every form of literacy carries with it obligations. People who can read, for example, are required to bring warning signs to the attention of those who cannot.

I said that scientists do not have a special wisdom. They do, however, share distinctive values. These values are not so much those of scientists as, more broadly, of scholars. None the less, they constitute a proud element in what we term the scientific method.

Science requires that no nation, religion, ethnic group, class or gender be regarded as having a monopoly on the truth. This remains a highly civilised concept, viewed in the light of today's world.

A FURTHER principle, which flows from the first, is that of civility. Since the truth is a shared concern, there should be no disrespect implied in disagreement. Not only must that be acknowledged, it must be made evident in the way we treat one another.

John Tyndall and Michael Faraday, major scientific competitors, attacked one another's views forcefully in the *Philosophical Magazine* of 1855. Strikingly, the dateline for their conflicting letters was the same: The Royal Institution, London. Conscious of their differing viewpoints, Michael Faraday had invited Tyndall to join him at the Royal Institution some years earlier. There they shared a domicile and a lifelong friendship.

Commenting on such altercations among colleagues, Faraday wrote:

> "Each one gives views and ideas new to the rest. When science is a republic, then it gains; and though I am no republican in other matters, I am in that . . ."

The pursuit of science embodies a central requirement that we note with pride on the occasion of the hundredth discussion bearing Faraday's name: "There must be discussion to show how experience is to be interpreted."

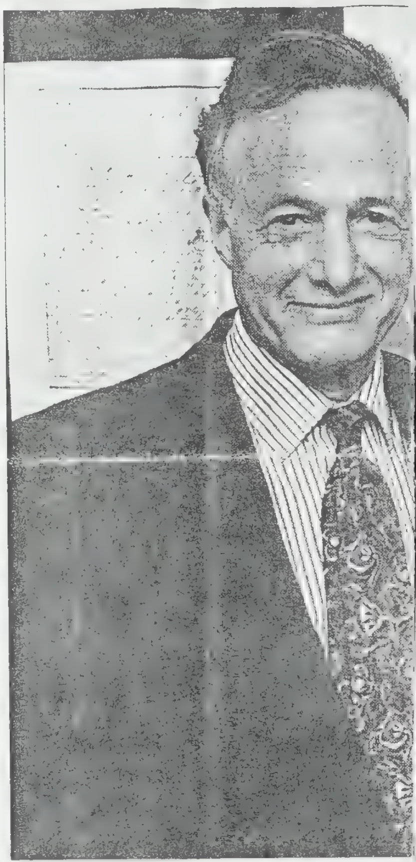
The quotation this time is not, however, from Michael Faraday but from Faraday's contemporary, John Stuart Mill, writing *On Liberty*. What we know about how discoveries are made, matters. It matters to science, and it matters still more beyond science.

Quotations from the writings of Michael Faraday are from Faraday as a Natural Philosopher *by Joseph Agassi, Chicago University Press, 1971*

File: Polanyi

Give free rein to ideas: profits will follow

The Government's White paper on science placed new emphasis on wealth creation and laying down research targets. But the Canadian Nobel Laureate **Prof John Polanyi**, in an address to the Royal Institution this week, highlighted the pitfalls of managing scientific creativity. His talk, entitled 'How discoveries are made, and why it matters', was made at a meeting opened by junior science minister, John Horam. Here is an extract



Prof Polanyi, pictured with a Mich
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File Polanyi

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

A Chemist Helping Chemists

August 21, 1995

Dr. Derek Parker
83, Halford Road
Ickenham
UXBRIDGE, Middlesex UB10 8QA
England

Dear Dr. Parker:

Your thoughtful and most interesting letter of August 14th has been forwarded to me from Herstmonceux Castle.

I so enjoyed your letter, but just don't understand your first sentence: Why could I possibly consider such a letter an impertinence?

For many years, during the early days of Aldrich, I used to visit BDH in London to purchase a number of compounds for the Aldrich catalog. In a way, I was sorry to see the move to Poole and particularly the purchase of BDH by Merck in Darmstadt.

Isabel and I live in Bexhill, usually at the end of November and December and again at the end of June and July; unfortunately, I cannot stay in Britain longer than 90 days - for obvious tax reasons. Should you come to Sussex during our stays in Bexhill, we would love to meet you.

With all good wishes, I remain,

Yours sincerely,

AB/cw

83, Halford Road,
Ickenham,
UXBRIDGE
Middlesex
UB10 8QA

Telephone: (01895) 232073

14th August, 1995

Dr. Alfred Bader,
c/o Queens University,
Herstmonceux Castle
HERSTMONCEUX
E Sussex

Dear Dr Bader,

I hope you do not feel that this letter is an impertinence. You do not know me, but I feel I know you very well. I have just finished reading your book, 'Adventures of a Chemist Collector'. How glad I am that the Editor of 'Chemistry in Britain' published a review of it. Had he not, I might have been deprived of a most enjoyable experience.

The enjoyment has been heightened by the numerous points at which my experiences in life have enabled me to empathise with yours. I am just under a year younger than you. I was trained as a chemicker at University College London, though this was in wartime, and the College was scattered all over Britain. My first job was with the British Drug Houses (before it went to Poole) which as you will know made fine chemicals as well as pharmaceuticals. So I was able easily to follow the chemistry in your book. Since those days I moved away from pure chemistry to polymers, and technology at that. Most of my career was in teaching of one form or another, ultimately ending up in university administration. I was (marginally) more interested in people than science. You managed to hold such interests more or less in balance. Mind you, your book made me intensely glad I had not remained in the fine chemical business. I could never have stood the pace !

I married an Austrian girl. We have been idyllically happy, though the tensions of spanning two cultures have never been far below the surface. I still don't know much German, but I do know what spitzbub means. We have frequent vacations in Austria, of course, and in mainland Europe generally. I used to consider Salzburg to be my favourite city, but it is too full and noisy now. But we do love Prague.

My school was in North London close to a well-known Jewish quarter. Between one fifth and a quarter of the boys at the school were Jews. Most of them were extremely clever, and this might have caused a bit of fricton. It didn't - I realised later that their cleverness was the result of natural selection; they were the sons of Jews who were far-seeing enough and rich enough to escape from Hitler's Europe before the blow fell.

There was no anti-semitism in my school when I was there, and I was glad to hear from Chief Rabbi Jonathan Sacks that there was none in his time at the school, a generation later.

I am a practising Christian since a teen-age 'conversion' (my parents were only nominal 'C of E'), though I have little sympathy with Catholicism, and am very critical of most institutional religion. Since my schooldays I was mildly interested in the Jewish faith, but of recent years I have begun to study it more systematically. We belong to the Council of Christians and Jews, and attend their functions. I, too, love the Bible (by no means only what we call the New Testament), and my respect for the Jewish faith has grown as I have studied the Law and the Prophets. I wish I knew a bit of Hebrew, but I'm probably a bit too long in the tooth to begin to learn it now.

For many years my wife and I used to visit a distant cousin of mine who lived in Bexhill. She had yet more distant relatives who lived in Canada, but who visited her whenever they came to Europe. On these occasions we visited the tourist spots in Sussex including Herstmonceux. The Canadians had two children, whom we met when they were very young, and saw grow up. My cousin has now died, but the Canadians still ring us from Heathrow when they pass through. Last year the boy rang us to say that he was doing a College module at Queen's University and they were sending him to study in England. Said he, "Some rich guy who was interned in Canada during the war and was kindly treated by Queen's has given the College Herstmonceux castle - I am going to study there". Later he invited us down to Herstmonceux, showed us round, and gave us lunch. We were very interested. We didn't then know the whole story, but we do now.

However, unlike you, I know very little about fine art. My education in that field has been sadly neglected. The B and the C, but the A, very little. All the same, you can appreciate how interesting and stimulating I found your book.

Once again I apologise for burdening you with this ego-trip of mine. It is just an expression of my delight with your book. Thank you for writing it. You are still a busy man, and no doubt your secretaries are busy too; so please don't instruct them to send any sort of acknowledgement of this.

Yours very sincerely,

Derek Parker.

(D B V PARKER)
BA, MSc, PhD

PITTSBURGH CONFERENCE
ON ANALYTICAL CHEMISTRY AND APPLIED SPECTROSCOPY (A Non-Profit Corporation)

300 Penn Center Boulevard, Suite 332 • Pittsburgh, PA 15235 USA • Phone: 800-825-3221 or 412-825-3220
Fax: 412-825-3224

May 15, 1995

COPY

Dr. Mildred B. Perry
Program Committee Chairman
1995 Pittsburgh Conference
300 Penn Center Boulevard, Suite 322
Pittsburgh, PA 15235

Dear Dr. Perry:

On the evening of May 10, 1995, I spoke with Dr. Alfred Bader regarding the disputed claims for reimbursement as an invited speaker for the Pittcon '95 Technology Forum Symposium. As you recall, the questionable expenses involved an honorarium request of $1000 and airfare of $377.30 for his wife Isabel.

As I had explained to you earlier, Dr. Bader and I had discussed his wife Isabel's travel to Pittcon '95 in my initial phone conversation with him on April 8, 1994. Considering this as well as Isabel's contribution to the program, we agreed that it was only right to reimburse Isabel's travel expenses. On the subject of an honorarium, we determined that the honorarium offer of $1000 did not come from me or anyone else associated with Pittcon '95, but was instead the result of confusion with another speaking invitation that Dr. Bader received about the same time. Therefore, Dr. Bader agreed to withdraw his honorarium request, and would consider the matter closed upon reimbursement of Isabel's airfare. I strongly recommend that we take this opportunity to resolve this matter. Our conversation was quite amicable and Dr. Bader hoped that we would invite him to speak again at a future Pittsburgh Conference.

Sincerely yours,

Kurt Rothenberger

Kurt Rothenberger
1995 Program Committee

cc: KJM, file

ALFRED BADER FINE ARTS

DR. ALFRED BADER

ESTABLISHED 1961

June 5, 1995

Dr. Russell D. Larsen
Department of Epidemiology
Graduate School of Public Health
University of Pittsburgh
Pittsburgh, PA 15261

Dear Professor Larsen:

Naturally I have been wondering whether you have any plans to come to the Loschmidt Symposium in Vienna later this month. Just in case you have not seen the program, I enclose a copy.

Of course, I have often thought about your most instructive chapter 13 in Professor Wotiz' book, and if fact, I will speak about both the Kindle-Cole Principle and the Matthew Effect.

You may have seen my autobiography, *Adventures of a Chemist Collector*, published by Weidenfeld and distributed in this country by Trafalgar Square, with ISBN 0-297-83461-4. Chapter 16 deals with Loschmidt.

It is conceivable that the American Chemical Society will invite me on a lecture tour on the Pennsylvania/Maryland circuit next April. If so, I would of course be delighted to be able to give both the Loschmidt and the Anschütz talks in Pittsburgh.

With all good wishes, I remain,

Yours sincerely,

AB/cw

Enclosure

By Appointment Only
ASTOR HOTEL SUITE 622
924 EAST JUNEAU AVENUE
MILWAUKEE WISCONSIN USA 53202
TEL 414 277-0730   FAX 414 277-0709

April 13, 1995

L. John Polite, Jr.
Peridot Chemicals (New Jersey), Inc.
1680 Route 23 North
Wayne, NJ 07470

Dear Dr. Polite:

Due to an extended speaking tour, I am only now able to reply to the invitation in your letter of March 29th.

While I would like to be able to attend the reception and award luncheon on Friday, April 21st, I regret that my travel schedule will not permit it.

Please do extend my best wishes to Mr. Sorgenti on his award.

With all good wishes, I remain,

Yours sincerely,

AB/cw

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

March 14, 1995

Mr. Gary Parlin
Johnson & Johnson
1 J & J Plaza
New Brunswick, NJ 08903

Dear Mr. Parlin:

You may recall that we met last December at that happy dinner in Stein am Rhein celebrating our mutual friend, Bert van Deun's many accomplishments.

Bert mentioned to me that J & J, I believe at Noranco, is looking for a really first-class Production Manager.

Just recently, one of the ablest production people I know, Dr. Richard Pariza, got involved in an ugly political hassle at Abbott and was asked to leave after eleven years.

Dr. Pariza earned his Ph.D. from Purdue about twelve years ago and from then on, advanced rapidly in production at Abbott. Until recently, he had some 50 scientists, most of them Ph.D.'s, reporting to him, and he is the man responsible for the smooth production of some of Abbott's most important candidates for the clinic.

I first met Richard, who is now 49, some 25 years ago when he was very interested in chemical production and synthesis. He is one of the brightest and most personable chemists I know.

If you might like to talk to him, his address is 43323 N. Oak Crest Lane, Zion, Illinois 60099, and his home telephone number is 708/746-3530.

With all good wishes, I remain,

Yours sincerely,

AB/cw

bcc's to: Bert van Deun and Richard Pariza

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

July 13, 1994

Dr. Richard Pariza
43323 N. Oak Crest Lane
Zion, Illinois 60099

Dear Richard,

A trip to Europe has delayed my thanking you for your fax of June 27th.

I don't think that anyone could present the problem more clearly than you do in your first paragraph.

I knew Don Hedberg reasonably well because Aldrich bought a fair amount from Lab Safety Supply before we started our own lab supply operation.

It is always such fun to chat with you, and I hope you will visit us soon.

Best wishes.

Sincerely,

[illegible]

[illegible] Since I had not heard from them in time [illegible] a small part of [illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] announcement of the [illegible] highly qualified [illegible]

[illegible]

Thank you [illegible]

[illegible]

[illegible]

Dear Alfred:

I appreciate your kind letter of support for my application at Carthage College. Since I had not heard from them in some time, I called Dr. Brawner. The enclosed letter came a few days later. Dr. Brawner seemed to be looking for someone who could set up seminars and arrange for students to interact with [local] entrepreneurs. That would only be a small part of my vision. I think much more could be done from the position they are offering. (I was going to say "...from the position they are creating...", but whoever fills the position will create it.) A major problem in American business is the lack of entrepreneurism in large corporations. The business school idea of low level employee "empowerment" is only given lip service: risk-taking and involvement in all aspects of the success of a project are discouraged by entrenched bureaucracy, formalized goal setting processes, and politics. These issues should be a major part of the student's curriculum since many will end up employed by large corporations. Furthermore, successful small companies tend to grow, and if these issues are not well understood, such problems will limit their potential.

I need not preach to you. It seems to me that Tom Cori creates such problems at Sigma-Aldrich with his smothering management style. You understand these ideas better than anyone. I just wanted to comment on the direction the Carthage people seem to be taking. Separately I will send you a copy of "The Carthaginian" that describes the endowment. My wife saw it in the church office, and it is too large to fax. Donald Hedberg was an instructor at the University of Illinois-Chicago, president of Science Related Materials in Janesville, and started and ran Lab Safety Supply, Inc. His daughter is now president of Lab Safety Supply.

This is a disappointment, but I'm sure there will be other opportunities. I also want to thank you for the letter you sent the fellow at Fisher Scientific. I hope we can get together again, soon. I missed seeing Elisabeth on my recent visit, and I send my greetings to her.

Best personal regards,

Richard

Richard

Carthage

Division of the Natural Sciences

June 15, 1994

Dr. Richard Pariza
43323 Oakcrest Lane North
Zion, IL 60099-9413

Dear Dr. Pariza;

Thank you for your interest in Carthage College and The Hedberg Distinguished Professorship for Entrepreneurial Studies in the Sciences. The response to our announcement of this new and exciting faculty position has resulted in a large pool of very highly qualified individuals. The Search Committee has had a difficult time narrowing the list. However, after reviewing each file the Committee conducted telephone interviews and selected several individuals for on-site interviews. We are currently in the process of conducting the interviews.

It is anticipated that the new faculty member will be selected from those currently being interviewed. However, if the Committee feels that it is necessary to return to the list of applicants, the Chairperson of the Committee may be in contact with you.

Thank you again for your interest in Carthage College.

Sincerely,

Thomas A. Brawner

Thomas A. Brawner. Ph.D., Chairperson
Search Committee

Carthage College • Kenosha, Wisconsin 53140 • (414) 551-8500

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

June 6, 1994

Mr. Paul M. Meister
Senior Vice President
Chief Financial Officer
Fisher Scientific International Inc.
Liberty Lane
Hampton, New Hampshire 03842

Dear Paul:

I am puzzled not to have received a reply to my letter of January 24th, copy of which I enclose, and I hope that the letter did not get lost in the mails.

Also, I trust that reference to Fisher Scientific in my autobiography is correct.

Since writing to you, I have thought of one other very able chemist who might fit well into your organization. I first met Dr. Richard Pariza in 1965. I was then president and CEO of Aldrich and we were just in the process of going public. It soon became clear to me that Richard was very interested in starting his own fine chemicals company, and we often talked about his plans, usually during long walks through Lake Park in Milwaukee.

In 1969, Richard started Willowbrook Laboratories. At first, Willowbrook did very well, offering all sorts of new organic building blocks and reagents. The company grew, but became somewhat overextended financially, and Richard decided it would be better for him if he went back to school to obtain his PhD, which he did at Purdue, working with one of Purdue's most imaginative organic chemists, Professor Phillip Fuchs.

Richard is one of those rare human beings who combines real imagination for research with entrepreneurial talent and a great deal of enthusiasm. If the investors in Willowbrook had not tried to expand too quickly, it would today be a major factor in the fine chemical field. Richard is truly an inspiring individual, and I wish that circumstances would have been right for him to join Aldrich and then Sigma-Aldrich.

He is presently at Abbott and I am not certain that Abbott appreciates his entrepreneurial ability.

All good wishes.

Sincerely,

Enclosure

FAX FROM

DR. ALFRED R. BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone 414-277-0730
Fax No. 414-277-0709

To: Dr. Richard Pariza

Please call me this afternoon at my office or this evening at home, 414 962 5169

Thanks

Alfred

5/18

708 938 6370

ALFRED BADER FINE ARTS

DR. ALFRED BADER

ESTABLISHED 1961

May 13, 1994

Professor Thomas Brawner
Department of Biology
Carthage College
Kenosha, Wisconsin 53140 1994

Dear Professor Brawner:

Dr. Richard Pariza has applied to Carthage College for your newly established Donald D. Hedberg Distinguished Professorship for Entrepreneurial Studies in the Sciences.

Please allow me to tell you about Richard. I first met him almost 30 years ago, in 1965. I was then the president and CEO of Aldrich Chemical Company and we were just in the process of going public. It soon became clear to me that Richard was very interested in starting his own fine chemicals company, and we often talked about his plans, usually during long walks through Lake Park in Milwaukee.

In 1969, Richard started the company he had been thinking about, the Willowbrook Laboratories, first in Waukesha and then moving to a chemical plant originally built by Schlitz Brewing Company in Sheboygan. At first, Willowbrook did very well, offering all sorts of new organic building blocks and reagents.

My friendship with Richard continued and I tried very hard to have Aldrich become Willowbrook's good customer. Willowbrook grew, but became somewhat overextended financially, and Richard decided it would be better for him if he went back to school to obtain his PhD. He attended Purdue University and worked with one of Purdue's most imaginative organic chemists, Professor Phillip Fuchs. I kept in touch with Richard, both during his studies at Purdue and later during his work in production at Abbott Laboratories.

Richard is one of those rare human beings who combines real imagination for research with entrepreneurial talent and a great deal of enthusiasm. If only the investors in Willowbrook Laboratories had not tried to expand too quickly, Willowbrook would today be a major factor in the fine chemical field.

By Appointment Only
ASTOR HOTEL SUITE 622
924 EAST JUNEAU AVENUE
MILWAUKEE WISCONSIN USA 53202
TEL 414 277-0730   FAX 414 277-0709

Professor Thomas Brawner
Caerthage College
May 13, 1994
Page Two

On a personal basis, I have always enjoyed talking with Richard and thinking aloud with him about his plans, hopes and dreams. He is truly an inspiring individual, and I wish that circumstances would have been right for Richard to join Aldrich and then Sigma-Aldrich.

There cannot be many scientists around the country as well trained for your exciting new professorship as Richard Pariza.

Please don't hesitate to contact me if you have any further questions. My office telephone number is 1-277-0730, my office fax is 1-277-0709 and my home telephone number is 1-962-5169.

Sincerely,

FAX FROM

DR. ALFRED R. BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone 414-277-0730
Fax No. 414-277-0709

708 938 6070

5/17/94

To: Dr Richard Pariza

Began 1965

Please call me.

I need a couple of dates

1969

when did you found Willowbrook?

Sept 1975

Purdue?

1978
1983

Pariza
home
43323 N Oak Crest Lane
Zion, Ill
60099

Best wishes

PS I'll be in my office till noon to-day, then from 2 to 5 pm.

[illegible]
[illegible]
[illegible]
[illegible]

[illegible]

[illegible]
[illegible]

[illegible] [illegible]

[illegible] [illegible]

[illegible] [illegible]

[illegible] [illegible]

[illegible] [illegible]

[illegible] [illegible]

[illegible] [illegible]
[illegible]
[illegible]
[illegible]
[illegible]
[illegible]

FAX FROM

DR. ALFRED R. BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone 414-277-0730
Fax No. 414-277-0709

April 7, 1994

To: Richard Pariza
Abbott Laboratories
Chemical Development

FAX: 708 938 6370

Dear Richard:

Thank you for your fax.

That ad appears handwritten for you!

Let me know if I can be of any help, and we sure look forward to seeing you on May 19th.

Best wishes,

PCR®

January 27, 1994

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

Dear Alfred:

I am excited to hear that you will be in Gainesville on Thursday and Friday. My wife, Bonita and I would be very pleased if you and Isabel could join us for dinner one night during your stay.

In addition, on behalf of PCR, I would like to invite you to tour our facilities and meet with the chemists. Please let me know if I can arrange a time for you to visit the plant. I can be reached at 1-800-331-6313, ext. 206 (voice mail), or ext. 0 and the operator can have me paged.

Regarding the excerpt of the autobiography, I have checked with Keith Baucom and Max Petzold. Max does not believe that Stafford was at PCR until 1974, but will check with W.A. Dinkins, the former President, and will hopefully have an answer by next week. Both Keith and Max agreed the rest of the details are accurate. The book is a wonderful idea.

Sincerely yours,

Paul W Kremer

Paul W. Kremer,
Plant Manager

\pjs MANF 94-435

PCR INCORPORATED
A PCR GROUP, INC. COMPANY
P.O Box 1466, Gainesville, Florida 32602
904-376-8246 TWX 810-825-6342 FAX 904-371-6246

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

January 21, 1994

Dr. Paul Cramer
PCR Inc.
P.O. Box 1466
Gainesville, Florida 32602

Dear Paul,

Isabel and I much look forward to being in Gainesville, speaking twice in the chemistry department of the university on Thursday and Friday, February 3rd and 4th.

Of course I very much look forward to being there, and hope to have a chance to chat with you.

I am just finishing my autobiography in which I have written some details of that curious incident between PCR and Aldrich. I enclose excerpt from the draft of the autobiography, and I would appreciate your checking the historical details of PCR to make sure that I have not made any mistakes.

Best personal regards.

Sincerely,

Enclosure

6/93

ALFRED BADER FINE ARTS

DR. ALFRED BADER

ESTABLISHED 1961

February 1, 1993

Mr. George Patterson
Vice President
Rollins Hudig Hall of Wisconsin, Inc.
Two Plaza East, Suite 450
330 East Kilbourn Avenue
Milwaukee, Wisconsin 53202 3179

Dear Mr. Patterson:

Thank you for your thoughtful letter of January 14th.

It would indeed give me great pleasure if we could meet personally.

Best regards.

Sincerely,

Alfred Bader

Dr Bader,

Would you like to have lunch at Nantucket Shores sometime in July. Thanks again for all your help with Alzheimers.

George Patterson

By Appointment Only
ASTOR HOTEL SUITE 622
924 EAST JUNEAU AVENUE
MILWAUKEE WISCONSIN USA 53202
TEL 414 277-0730 FAX 414 277-0709

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Best regards.

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ASTOR HOTEL SUITE 622
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TEL 414 277-0730 FAX 414 277-0709

RHH ROLLINS HUDIG HALL

Rollins Hudig Hall of Wisconsin, Inc.
Two Plaza East, Suite 450, 330 East Kilbourn Avenue
Milwaukee, WI 53202-3179
414/271-6420 800/556-5115 Telefax 414/271-4103

January 14, 1993

Dr. Alfred Bader
2961 N. Shepherd Ave.
Milwaukee, WI 53211

Dr. Bader:

I had written you earlier in regards to the many changes going on at Sigma-Aldrich in regards to decision making being transferred to St. Louis. I did end up losing the Workers' Compensation Insurance based on all insurance being consolidated in St. Louis.

My second reason for writing is to thank you and your wife for the generous donations you have made in researching Alzheimer's Disease. My wife died of genetic Alzheimer's in her mid 30's two years ago. I have two children who both have a 50% chance of having this disease, so I am very interested in the research that continues to go on. We are fortunate in the Milwaukee community to have people like you and your wife that are so committed in trying to find a cure for this dreadful disease. I'd like to get together with you for lunch if you have time in the future.

Sincerely,

George Patterson
Vice President

/pb

12/31

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

November 7, 1992

Mr. George Patterson
Vice President
Rollins Burdick Hunter of Wisconsin, Inc.
Two Plaza East, Suite 450
330 East Kilbourn Avenue
Milwaukee, Wisconsin 53202

Dear Mr. Patterson:

I am just on my way to Europe, but I want to reply very quickly to your moving letter of November 4th, just received.

Yours is just one of a great many examples, for practically all present management decisions of Sigma-Aldrich are made in St. Louis. This wouldn't be particularly serious if we were dealing with caring human beings. Unfortunately, that is not the case, and I fear that all of our companies in St. Louis, Milwaukee, etc., will suffer in the long run.

My wife and I are returning from Euope at the end of December, and I will be happy to chat with you about all this at that time.

Best regards,

(Dictated by Dr. Bader and
signed in his absence)

Rollins Burdick Hunter of Wisconsin, Inc.
Two Plaza East, Suite 450
330 East Kilbourn Avenue, Milwaukee, WI 53202
414 271-6420 800 556-5115 Telefax 414 271-4103

ROLLINS BURDICK
HUNTER

November 4, 1992

Mr. Alfred Bader
2961 North Shepherd Ave.
Milwaukee, WI 53211

Re: Sigma/Aldrich Management Changes

Mr. Bader:

I read with interest the article in Sunday's Milwaukee Journal (11/1/92). Your comments are very appropriate in view of what I have seen happen to insurance-making decisions over the past couple of years. Aldrich Chemical in the past had made many of their own insurance decisions. Over the past couple of years, more of these decisions have been centralized in St. Louis. I have written the Workers' Compensation for the entire organization since 1987, and have been involved with Aldrich since 1985 in regard to their Workers' Compensation coverage. This year, the risk manager, Larry Roeder, decided to consolidate all coverages with a St. Louis broker, Marsh & McLennan. They received an Agent of Record letter for the company I had originally put the entire corporation with in 1987. Insurance decisions are just another example of the control that seems to be leaving the Milwaukee area.

I would be very interested in discussing these issues with you at your convenience.

Sincerely,

George

George Patterson
Vice President

/jae

-FAX FROM

DR. ALFRED R. BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone 414-277-0730
Fax No. 414-277-0709

To: Dr. Kalsyan
Peptosyn

Fax 914 779 8817
3 pages

Peptide buyer at Sigma is Jackie Friedman

I attach pages from Synthetech annual report showing address etc

What are you charging for naphthophalein?

Is it the α-isomer?

Buyer at <u>Aldrich</u> for such dyes is Pat Trainor, fax 414 287 4059. If you can fax him <u>to-day</u>, it would be helpful.

Best wishes

Alfred Bader

10/11/94

TEL. 212-888-3345
FAX 212-888-0695

BERNARD G. PALITZ
CHAIRMAN OF THE BOARD

FINANCIAL FEDERAL CORPORATION
400 PARK AVENUE
NEW YORK, N.Y. 10022

FINANCIAL FEDERAL CORPORATION

Is Pleased to Announce

The Relocation of Its New York Offices,

Effective April 26, 1993,

to

400 Park Avenue - 8th Floor
New York, NY 10022

Tel: (212) 888-3344 Fax: (212) 888-0695

PEPTOSYN, INC.

May 25, 1993

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 55211

Dear Dr. Bader:

This is just a brief note to bring you up to date with regard to our progress at Peptosyn. We are very grateful to you for introducing Max Gergel who has been working very hard in order to introduce us to potential customers and organizations that might be interested in our products.

So far we have established an ongoing relationship with Susan Cohen of Crescent Chemical, and we have signed a confidentiality agreement with Hoffmann-LaRoche through Dr. Arthur Felix. We are planning to meet with Dr. Felix soon to explore areas of common interest. We have also met with Dr. Eugene Oliveto who has many contacts. As a result of these activities, we have received several orders and are working on their production. No news from Sigma!

We would like to invite you to come to New York and visit us as our guest. We are looking forward to continue receiving your guidance as always.

With kindest personal regards to you and Mrs. Bader.

Iraj and Parviz

cc: Max Gergel

[illegible]

PEPTOSYN, INC.

April 7, 1993

Dr. Irwin L. Klundt
Starwood Consulting
250 Skyline Drive
Bayfield, Colorado 81122

Dear Dr. Klundt:

I look forward to meeting you. Max has told me many things about you and the help you are prepared to give our little company. To help compensate for the time you have spent and will spend, we are prepared to give a "family discount" to people with whom you are associated who can help us sell our products. If you are ever in the New York area, you would be a very welcome guest to visit and to be our guest for lunch or dinner. We well realize the great potential help you can be to us. Let's make it a "two-way street."

Thank you again for all your help.

Sincerely,

Parviz Lalezari, M.D.

Parviz Lalezari, M.D.

Attachment

cc: Dr. Alfred Bader
Dr. Charles Wilkie
Mr. Max Gergel

[illegible]

PEPTOSYN, INC.

April 13, 1993

Dr. Charles A. Wilkie
Professor of Chemistry
Marquette University
Milwaukee, Wisconsin 53233

Dear Dr. Wilkie:

I am writing this letter as suggested by Mr. Max Gergel, to introduce our new company, Peptosyn, Inc. in the hope that we can establish a mutually beneficial collaboration.

Attached please find a copy of our brochure and a list of compounds and prices which represent examples of products Peptosyn, Inc. offers. In general, it is our intention to offer, at large quantities, not only peptides and other compounds that heretofore have been offered by others, but also to solicit requests to synthesize compounds not listed or produced by others at prices you will find most attractive. I have to add that we are now extending these products to include peptides and derivatives with up to twelve amino acids. The attached brochure, of course, is preliminary, and we plan to update it soon with the addition of Max Gergel's name as our Sales Manager.

We would like to have the opportunity of having you visit our facility and meet our people in Tuckahoe if you are in the area.

Looking forward to hearing from you and thank you for your interest in Peptosyn, Inc.

Sincerely,

Parviz Lalezari, MD

Parviz Lalezari, M.D.

Attachment

cc: Dr. Alfred Bader
Dr. Irwin Klundt
Mr. Max Gergel

PEPTOSYN, INC.

April 7, 1993

Dr. Charles A. Wilkie
Professor of Chemistry
Marquette University
Milwaukee, Wisconsin 53233

Dear Chuck:

It was as always a pleasure to see you. I am going to discuss with Dr. John Wasson of Advanced Material, to whom I am sending a copy of this letter, a carborane project in which the two Russian Scientists could no doubt help us. I will keep in mind a possible visit to Marquette in late May to see you and your guest. If at any time you might recommend me as a seminar speaker anywhere in Wisconsin not too far from Milwaukee, Alfred Bader says he will want to make the introductions and hear the talk. Janssen will pick up the airfare and the University needs only pay for the food and lodging. I'll pay you part of a good dinner and bottle of wine! My hopefully close association with Ike Klundt may open some doors for both of us - let's say all three of us.

You will get a letter from Dr. Parviz Lalezari of Peptosyn, Inc., whom I mentioned to you, and whom I met through Alfred. It was great to see you and Harris Lehrer of Spectrum was so happy to meet you. All best wishes.

Sincerely yours,

Max Gergel

Max G. Gergel

cc: Dr. Alfred Bader
    Dr. Harris Lehrer
    Dr. Ike Klundt

[illegible]

PEPTOSYN, INC.

April 7, 1993

Dr. Frederick Zucker
President
Fluka Chemical Corp.
980 South Second Street
Ronkonkoma, New York 11779

Dear Dr. Zucker:

Max has written me about Fluka's interest in peptides and we are prepared to send our updated catalogs to you when available. We would also be pleased to send you quotations on any items of interest to you at attractive prices. Dr. Bader, as Max may have told you, was instrumental in bringing us in contact with Max and he has passed on to us your favorable comments on our product line. I will look forward to speaking with you and arrange for future collaboration.

Sincerely,

Parviz Lalezari, M.D.

Parviz Lalezari, M.D.

cc: Dr. Alfred Bader
Max Gergel

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

February 24, 1993

Dr. Parviz Lalezari
Peptosyn, Inc.
80 Yonkers Avenue
P.O. Box 394
Tuckahoe, New York 10707

Dear Dr. Lalezari:

Thank you for your letter of February 19th.

I think that your letters to Synthetec, Dr. Fagan, Paul Berg and Steven Woodland are fine, but the key question now is how to persuade Jackie Friedman in St. Louis to become your customer.

I have asked a good friend of mine in purchasing at Aldrich to see what he advises, and as soon as I hear from him I will let you know.

I do not think that sending free samples is the right way to go, although a sample of the repurified tetra-peptide which Sigma suggestion would be appropriate.

Best regards.

Sincerely,

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

February 24, 1993

Mr. Patrick Trainor
4800 Hunting Park Drive
Franklin, Wisconsin 53132

Dear Pat:

Please look over the correspondence with Peptosyn, enclosed.

Let me say, first of all, I have no commercial interest in Peptosyn other than wanting to help a small, young company get started by people who seem quite competent.

Certainly if I call Jackie Friedman it might actually be counterproductive, because if Jackie mentions in St. Louis that Bader recommended Peptosyn, Tom Cori might well say that Sigma should never buy from Peptosyn again.

What do you recommend? Please call me at your convenience.

Best wishes, and many thanks.

Sincerely,

Enclosures

[illegible]

February 19, 1993

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, WI 53211

Dear Dr. Bader:

Thank you very much for your kind letter of February 10 and your genuine interest in our success. We especially appreciate your contacting your friends on our behalf, and your efforts to re-establish our communication with Jackie Friedman.

In the attached copy, we would like to present a few examples of several peptides we have recently synthesized with their HPLC data. As you can see, purities of these preparations range between 98% to 99%. As you are aware, purities of compounds offered by Sigma range between 97% to 98%. Among our preparations, please note the heptapeptide, GRGDSPC, which is a fibronectin-related compound and was synthesized for Immucore with a 98.776% purity. This compound is not in the Sigma catalog. Another compound of interest is the tetrapeptide, Phe-Gly-Phe-Gly, which we synthesized for Sigma, had a purity of 97%, but was rejected by Jackie. Subsequently, we recrystallized the preparation and increased its purity to 99%. We are quite pleased about these qualities, which should give us and our potential customers the confidence we need in marketing our products.

Attached also please find a copy of the letter we mailed to Jackie. As you had suggested, two weeks later I placed a call to her, but she did not respond. The question is how to proceed. You suggested that we should send her one product of real interest to Sigma. At this point, we do not know what this product is? Should we send the re-purified Phe-Gly-Phe-Gly, our new heptapeptide, or a few samples for free? We would appreciate your advice.

We are in the process of contacting other individuals in the list you gave us during our January meeting.

With kindest personal regards.

Sincerely,

Parviz

Parviz Lalezari, M.D.

Attachments

Attachment

HPLC Data on Examples of Peptides

Synthesized at Peptosyn

Catalog No.	Compound	HPLC Purity
PH-4141	Phe-Gly-Phe-Gly	99.93%*
G-6273	Gly-Arg-Gly-Asp-Ser-Pro	97.80
P-2081	Pro-Asn	97.64
GU-3002	Glu-Glu	98.61
L-3642	Leu-Asn	98.20
G-7023	Gly-Arg-Gly-Asp-Ser-Pro-Cys	98.776
A-2044	B-Ala-Phe	99.9

* - The actual HPLC print-out for this product is attached

```
CH. 1 C.S 10.00 ATT  8  OFFS   5  02/19/93  14:03
```

```
D-2500                                                 02/19/93  14:03

METHOD: FGFG            TAG:      7  CH: 1

FILE: 2  CALC-METHOD: AREA%    TABLE:    0  CONC: AREA

  NO.      RT         AREA       CONC  BC
    1    6.12      2896428     99.937  BV
    2    7.58          927      0.032 TBB
    3    7.90          892      0.031 TBB
TOTAL
                   2898247    100.000
PEAK REJ :               0
```

PEPTOSYN, INC.

January 22, 1993

Ms. Jackie Friedman
Sigma Chemical Company
3050 Spruce Street
St. Louis, MO 63103

Dear Jackie:

First, let me thank you for your continuous help and advice which has been most valuable in making Peptosyn, Inc. a successful operation. Your critical evaluation of the few products we had sent to Sigma was essential for us to realize the importance of a stringent quality control process. I am pleased to inform you that we have now completed this task and are ready to provide you with fully-controlled products. The quality control processes we have implemented can be summarized as follows:

- We are using only the highest quality starting materials.
- The structure of all synthesized peptides and products are monitored and confirmed by FTIR, Polarimeteric and NMR analyses, determination of melting points, and in many cases by elemental analysis.
- The purity of each product is first checked by thin layer chromatography. Products with any degree of impurity are subjected to re-crystallization and re-purification, and the final preparations are tested by HPLC for confirmation of their purity. The analytical data would be available to be sent with each shipment.

Attached please find two separate lists of compounds that are now readily available for your consideration. First is a list of 18 compounds from the Sigma catalog (pfs) with the prices we offer. All these have purities identical or better than your compounds. The second list contains a number of products that are not in your catalog and can be offered (by you) to your customers.

86 YONKERS AVENUE • P.O. BOX 394 • TUCKAHOE, NEW YORK 10707 • (914) 779-8892 • FAX (914) 779-8817

I would appreciate it if you could review these lists and let us know if you have any comments. We are looking forward to the resumption of our collaboration, which we believe would be mutually beneficial. I thank you again for your help.

Sincerely,

Parviz Lalezari, M.D.

Attachments

Attachment: List I

Peptide	Sigma Catalog #	Peptosyn Catalog #	Sigma Price	Peptosyn Price
N-t-Boc-Met-Leu-Phe	B0511	M4310	100mg $115.50	1g $ 500.00
L-Tyrosine-7-Amido-4-methyl coumarin	T2141	T2821	50mg $ 78.55	1g $ 850.00
Val-Ala-Ala-Phe	V8251	V4125	100mg $ 49.15	5g $1000.00
Leu-Ser-Phe	L8140	L3384	250mg $103.20	5g $1000.00
Val-Trp	V2000	V2886	100mg $ 26.20	10g $1250.00
Leu-Asn	L064	L2370	250mg $ 68.85	10g $1500.00
Pro-Asn	P2913	P2870	250mg $ 68.85	10g $1500.00
Thr-Val-Leu	T3390	T3824	100mg $ 65.00	5g $1600.00
Ser-Tyr	S8258	S2998	1g $130.60	10g $ 850.00
Thr-Ser	T3150	T2187	250mg $137.60	5g $1500.00
Val-Pro-Leu	V3255	V3757	250mg $220.10	1g $ 500.00
Ile-Pro-Ile	I9759	I3646	25mg $ 49.00	1g $ 750.00
Phe-Gly-Gly-Phe	P3626	P4620	100mg $ 53.40	10g $1800.00
Pro-Gly-Gly	P9382	P3822	5g $220.00	50g $1000.00
Val-Gln	V5252	V2390	250mg $ 86.50	10g $1500.00
B-Ala-Phe	A1416	A2214	1g $136.20	10g $ 700.00
N-t-Boc-Gly-Pro	B9260	G3209	1g $ 50.00	20g $ 550.00
Ile-Asn	I3635	I2742	500mg $123.85	10g $1200.00

Attachment: List II

Lipidoyl Amino Acids:

Lipide residue: C_6-C_{18} Saturated and unsaturated fatty acids

Amino Acids: Ala, B-Ala, Asp, Asn, Gln, Glu, Gly, Ile, Lys (dilipidoyl), Met, Orn (dilipidoyl), Phe, Pro, Pyr, Ser, Thr and Val.

Price:		
	1g	$ 25.00
	5g	$ 85.00
	25g	$275.00

Literature available upon request.

Please compare our prices with Novabiochem.

PEPTOSYN, INC.

February 17, 1993

Mr. Paul C. Ahrens
President and CEO
Synthetech, Inc.
P.O. Box 646
Albany, Oregon 97321

Dear Mr. Ahrens:

Dr. Alfred Bader has suggested that I write this letter and introduce Peptosyn to you. We would like to explore the possibility of establishing a close collaboration between Peptosyn and your company. As indicated in the attached material, Peptosyn is a new company, the objective of which is to provide peptides and other chemicals to interested scientific and commercial organizations.

Peptosyn's strength is in our novel technology which enables us to synthesize peptides and related compounds rapidly, in large quantities and at low costs. We have now established a fully-equipped facility for organic synthesis and employ most stringent quality-control processes to ensure the production of the highest quality compounds. Examples of products and services we provide, and our low prices, are given in the attached folder.

We would appreciate it greatly if you would review the attached material and inform us of which areas might be of interest to you. We would be pleased to provide additional information on all of our products, and look forward to being of service to you in the future.

Sincerely,

Parviz Lalezari MD

Parviz Lalezari, M.D.

Attachments

bcc: Dr. Alfred Bader

PEPTOSYN, INC.

February 17, 1993

Dr. Dan Fagan
General Manager of Peptides
Mallinckrodt Specialty Chemicals Co.
16305 Swingley Ridge Drive
Chesterfield, MO 63017

Dear Dr. Fagan:

Dr. Alfred Bader has suggested that I write this letter and introduce Peptosyn to you. We would like to explore the possibility of establishing a close collaboration between Peptosyn and your company. As indicated in the attached material, Peptosyn is a new company, the objective of which is to provide peptides and other chemicals to interested scientific and commercial organizations.

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Sincerely,

Parviz Lalezari, MD

Parviz Lalezari, M.D.

Attachments

bcc: Dr. Alfred Bader

56 [illegible] AVENUE • [illegible]

PEPTOSYN, INC.

February 17, 1993

Mr. Paul Burg, CEO
Spectrum Chemical
Manufacturing Corp.
14422 South San Pedro Street
Gardena, California 90248

Dear Mr. Burg:

Dr. Alfred Bader has suggested that I write this letter and introduce Peptosyn to you. We would like to explore the possibility of establishing a close collaboration between Peptosyn and your company. As indicated in the attached material, Peptosyn is a new company, the objective of which is to provide peptides and other chemicals to interested scientific and commercial organizations.

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We would appreciate it greatly if you would review the attached material and inform us of which areas might be of interest to you. We would be pleased to provide additional information on all of our products, and look forward to being of service to you in the future.

Sincerely,

Peter Borducci
Executive Vice President

Attachments

bcc: Dr. Alfred Bader

86 YONKERS AVENUE • P.O. BOX 394 • TUCKAHOE, NEW YORK 10707 • (914) 779-8892 • FAX (914) 779-8817

PEPTOSYN, INC.

February 17, 1993

Mr. Steven Woodland
Lancaster Synthesis
MTM Research Chemicals
Eastgate White Lund
Morecambe, Lancs. LA3 3DY
England

Dear Mr. Woodland:

Dr. Alfred Bader has suggested that I write this letter and introduce Peptosyn to you. We would like to explore the possibility of establishing a close collaboration between Peptosyn and your company. As indicated in the attached material, Peptosyn is a new company, the objective of which is to provide peptides and other chemicals to interested scientific and commercial organizations.

Peptosyn's strength is in our novel technology which enables us to synthesize peptides and related compounds rapidly, in large quantities and at low costs. We have now established a fully-equipped facility for organic synthesis and employ most stringent quality-control processes to ensure the production of the highest quality compounds. Examples of products and services we provide, and our low prices, are given in the attached folder.

We would appreciate it greatly if you would review the attached material and inform us of which areas might be of interest to you. We would be pleased to provide additional information on all of our products, and look forward to being of service to you in the future.

Sincerely,

Peter Borducci
Executive Vice President

Attachments

bcc: Dr. Alfred Bader

36 YONKERS AVENUE • P.O. BOX 394 • TUCKAHOE, NEW YORK 10707 • (914) 779-8892 • FAX (914) 779-881

PEPTOSYN INC

February 11, 1993

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, WI 53211

Dear Dr. Bader:

As requested by Dr. Parviz Lalezari, I am enclosing 20 complete packages of our presentation brochures for your use in conjunction with the catalogs.

Thank you for all your help.

Sincerely,

Laurie Thomasset

Laurie Thomasset
Secretary

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

February 10, 1993

Dr. Parviz Lalezari
Peptosyn, Inc.
86 Yonkers Avenue
P.O. Box 394
Tuckahoe, New York 10707

Dear Dr. Lalezari:

I must tell you first of all that I was really taken aback to receive your Federal Express package on Saturday. Why waste money on Federal Express when ordinary mail would have taken a day or two longer and cost one tenth.

As you will see from the enclosed, I have already sent copies of your price list to a number of potential customers, so, for instance, Mr. P. Geuens the Manager of Janssen Chimica.

I have asked a good friend of min at Aldrich, in purchasing, to talk at length about your product line with Jackie Friedman at Sigma. Allow me to be frank in the hope that it will be constructions: she is thoroughly disappointed in your company and her first reaction was that she did not want to have anything further to do with you. The only advice I can really give is that you apologize profusely and offer her just one product really of interest to Sigma at a particularly good price. If she then does order, do make sure that it is of good quality and that you keep your delivery promise.

All good wishes.

Sincerely,

Enclosure

PEPTOSYN, INC.

February 5, 1993

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, WI 53211

Dear Dr. Bader:

First let me thank you for the opportunity of the most enjoyable and constructive meeting we had with you and Mrs. Bader in New York on January 14, 1993. We consider your advice invaluable, and we are most grateful to you for your genuine interest.

As per your recommendation, we are sending 20 copies of our "catalog" for you to use as you see fit.

To start implementing your other suggestions, we have prepared a lengthy letter to Jackie Friedman which is being mailed. We are also in the process of contacting all other individuals you mentioned, and will be glad to send you copies of our correspondence, if you feel this would be helpful. Certainly, we will contact you after we receive their responses.

With best wishes to you and Mrs. Bader.

Sincerely,

Parviz Lalezari, M.D.

Parviz Lalezari, M.D.

Enclosures

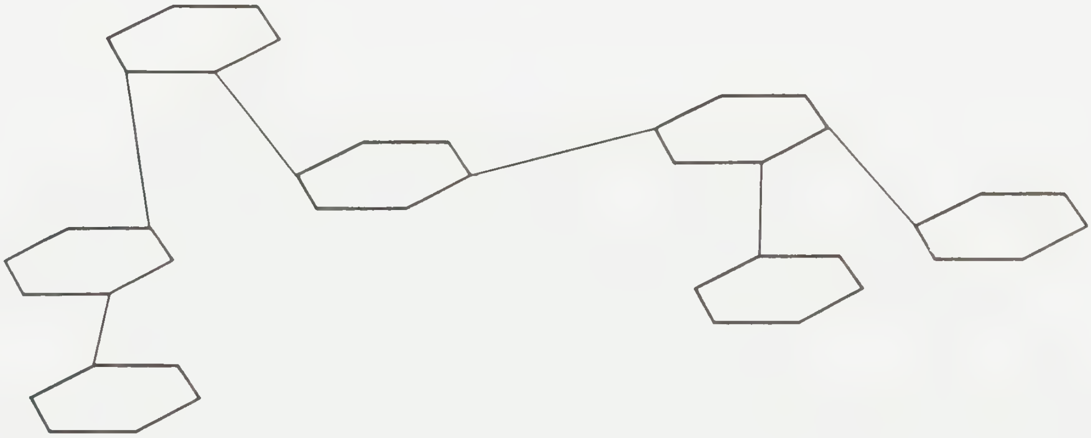
PEPTOSYN, INC.

PRODUCTS AND PRICE LIST

Special Offers *Spring 1993*

This price list reflects a series of special offerings and is only a part of Peptosyn's products.

All Peptides and Chemicals synthesized at Peptosyn are controlled by Analytical and Spectroscopical methods including HPLC. HPLC data sheet will be provided with each delivery. Other analytical information such as UV, NMR, FTIR spectra will be made available upon request.



86 YONKERS AVENUE • P.O. BOX 394 • TUCKAHOE, NEW YORK 10707 • (914) 779-8892 • FAX (914) 779-8817

Z-PEPTIDES

C5160	Z-Ala-Ala-Leu-p-Nitroanilide	50mg	31.50
		250mg	105.00
C3100	Z-Ala-Pro	500mg	18.00
		1g	32.00
		5g	128.00
C3430	Z-Gly-Met	1g	12.00
		5g	55.00
		25g	200.00
C6282	Z-Gly-Pro-Phe-Pro-Leu	100mg	88.00
		250mg	205.00
C2380	Z-Phe-Ala	1g	32.00
		5g	110.00
		25g	400.00
C4883	Z-D-Phe-Phe-Gly	25mg	45.00
		250mg	250.00
C4730	Z-Pro-Leu-Gly	100mg	12.00
		1g	89.00
C3710	Z-Pro-Phe	1g	12.00
		5g	55.00
C4932	Z-Sar-Pro-Arg	100mg	28.00
		500mg	110.00
C4902	Z-Val-Gly-OMe	1g	18.50
C4602	Z-Val-Tyr-OMe	1g	18.00
		5g	70.00

CHEMOTACTIC PEPTIDES

A4755	N-Acetyl-Met-Leu-Phe	50mg	60.00
		250mg	210.00
B4301	Boc-Met-Leu-Phe	100mg	85.00
B4102	Boc-Nle-Leu-Phe	100mg	85.00
F4051	N-Formyl-Met-Leu-Phe	100mg	45.00
		500mg	200.00
F5008	N-Formyl-Met-Leu-Phe-Benzylamide	25mg	75.00
		100mg	225.00
F5311	N-Formyl-Met-Leu-Phe-Phe	25mg	45.00
		100mg	135.00
F3650	N-Formyl-Met-Phe	500mg	88.00
F4083	N-Formyl-Met-Phe-Met	25mg	25.00
F4055	N-Formyl-Nle-Leu-Phe	100mg	55.00
		500mg	235.00
F7096	N-Formyl-Nle-Leu-Phe-Nle-Tyr-Lys	100mg	260.00
		500mg	950.00
V4236	Val-Gly-Ser-Glu	25mg	16.00
		250mg	120.00

FLUOROGENIC 7-AMINO-4-METHYLCOUMARIN (AMC) AMIDES OF AMINO ACIDS

A2011	L-Ala-AMC	100mg	45.00
		250mg	100.00
A2032	L-Arg-AMC	100mg	89.00
		250mg	200.00
A2006	L-Asp-AMC	25mg	28.00
		100mg	95.00
G2014	L-Glu-γ-AMC	25mg	35.00
		100mg	120.00

G2001	Gly-AMC	100mg	68.00
		250mg	150.00
I2012	L-Ile-AMC	25mg	25.00
		100mg	75.00
L2003	L-Leu-AMC	25mg	25.00
		100mg	75.00
P2010	L-Phe-AMC	25mg	25.00
		250mg	175.00
P2115	L-Pro-AMC	25mg	25.00
		100mg	85.00
S2301	L-Ser-AMC	25mg	45.00
		100mg	125.00
T2501	L-Thr-AMC	25mg	45.00
		100mg	125.00
T2901	L-Tyr-AMC	50mg	60.00
		250mg	230.00
V2611	L-Val-AMC	25mg	25.00
		100mg	76.00

FLUOROGENIC PEPTIDES

S3911	Ser-Tyr-AMC	100mg	95.00
		250mg	250.00
S5189	Suc-Ala-Pro-Ala-AMC	25mg	45.00
		100mg	160.00
S4191	Suc-Leu-Tyr-AMC	50mg	45.00
		100mg	80.00

N-FORMYL AMINO ACIDS

F2112	Formyl-Asn	1g	15.00
		10g	120.00
F2314	Formyl-Asp	500mg	6.00
		10g	90.00
F2411	Formyl-Gln	1g	16.00
		10g	125.00
F2511	Formyl-Glu	1g	16.00
		10g	125.00
F2011	Formyl-Gly	1g	6.00
		10g	40.00

PEPTIDES

P2381	Leu-Trp	100mg	15.00
		1g	75.00
P3393	Leu-Val-Leu	100mg	40.00
		1g	180.00
P4638	Phe-Gln-Gly-Pro	250mg	155.00
		1g	390.00
P4224	Phe-Gly-Gly-Phe	100mg	36.00
		1g	285.00
P4242	Phe-Gly-Phe-Gly	100mg	35.00
		1g	260.00
P2081	Pro-Asn	250mg	46.00
		1g	140.00
P2056	Pro-Gln	1g	75.00
P2010	Pro-Ile	1g	32.00
		10g	210.00
P3022	Pro-Gly-Gly	1g	32.00

P4322	Pro-Leu-Gly-Gly	1g	220.00
		5g	720.00
P2008	Pro-Trp	1g	58.00
P2081	Ser-Asn	250mg	60.00
		1g	180.00
P2091	Thr-β-Ala	250mg	65.00
P2094	Thr-Gln	250mg	85.00
P2097	Thr-Ser	1g	260.00
P3973	Thr-Val-Leu	100mg	35.00
		500mg	125.00
P2980	Val-Trp	100mg	20.00
		1g	165.00

LIPIDOYL AMINO ACIDS

Lipide Residue: C_6-C_{18}

Hexanoyl, Octanoyl, Decanoyl, Lauroyl, Myristoyl, Palmitoyl and Stearoyl

Amino Acids: All usual amino acids. Lipidoyl di-amino acids such as lysine are dilipoidyl compounds.

Ex: α-ω-Di Lauroyl-L-Lysine

1g	35.00
5g	100.00
25g	300.00

When ordering, please specify the lipid(s) and amino acid(s) desired.

BOC PEPTIDES

B2563	Boc-Gln-Pro	100mg	25.00
		500mg	95.00
B4326	Boc-Gly-Phe-Benzylester	1g	30.00
		5g	120.00
B2839	Boc-Gly-Pro	1g	35.00
B2311	Boc-Ile-Gly	1g	23.00
		5g	90.00
B3730	Boc-Phe-Phe-Gly	250mg	45.00
		500mg	85.00
B2620	Boc-Pro-Pro	100mg	30.00
		1g	180.00
B5311	Boc-Val-Leu-Gly-Arg	50mg	50.00
		250mg	200.00

NOTES

Prices are in U.S. Dollars. Bulk quantities will be provided with additional discounts.

Peptosyn is not responsible for any patent infringements that might occur with the use of the products listed in our catalog.

The products offered by Peptosyn are for research and laboratory use only.

JANSSEN
CHIMICA
Janssen Pharmaceuticalaan 3 - B-2440 Geel, Belgium - Tel.: 014/60.42.00 - Telex: 34.103 - Telefax: 014/60.42.20

Mr. VAN DEUN BERT
45 FOXCROFT DRIVE
PRINCETON, NEW JERSEY 08540
USA

GEEL, 25.01.93.

Dear Bert,

Thank you for sending me the catalogs of Fine Tech and Peptosyn.

We are now evalutating which products could be of interest to put them in our catalog.

Thank you for thinking of Janssen Chimica.

Best regards.

Yours Sincerely,

P.Geuens

Beste groeten aan Mia

cc. Dr. A.Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin
U.S.A.

Chemical division of Janssen Pharmaceutica N.V.
Registered office: Turnhoutseweg 30, B-2340 Beerse Belgium
H.R.T. 4203, VAT: 403.834.160
P.R.: 000-0003331-33

Johnson & Johnson

NEW BRUNSWICK, N. J.

COPY

January 15, 1993

Mr. M. Baronian

Enclosed is a copy of correspondence between Alfred Bader and Peptosyn, a company in New York that, as its name suggests, specializes in the synthesis of peptides.

I don't know whether Erich knows of this company, but as Alfred talked so highly about them, I thought he should peruse their price list, considering your great interest in peptides.

Best regards.

Bert Van Deun

s

enclosure

cc: Dr. A. Bader ✓
Mr. P. Geuens

YF

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

January 7, 1993

Via Fax 914 779 8817

Dr. Parviz Lalezari
Peptosyn, Inc.
86 Yonkers Avenue
P.O. Box 394
Tuckahoe, New York 10707

Dear Dr. Lalezari:

Thank you so much for your most interesting letter of January 2nd and your price list, which I very much look forward to discussing with you.

As agreed, we will meet at Christie's in New York, 502 Park Avenue, at 1:00 p.m. on Thursday, January 14th. However, it seems to me that it is far more important that we have a good long talk than that I look at your laboratory. Hence, let's cancel the drive to Tuckahoe and go somewhere in Manhattan, near Christie's, where we can just talk for two hours. I really cannot give you more than that time, but two hours should suffice for me to make all sorts of suggestions which I hope you will find helpful.

Do you know Synthetec, whose 6-month statement I enclose. It might be a very good idea for you to consider working with them. You are not really competitors, but would complement each other's product lines.

I look forward to seeing you a week from today.

Best wishes,

Alfred Bader

Enclosure

PEPTOSYN, INC.

January 2, 1993

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, WI 53211

Dear Dr. Bader,

Iraj and I would like to thank you for agreeing to visit our facility on January 14, 1993. In preparation for this meeting and in order to save some time, I am taking the liberty of sending you the enclosed material for your review and evaluation.

As you are aware, after several years of deliberation, we have finally established our laboratories, and are ready to market our products. I believe our assets are:

- A novel technology which enables us to synthesize various peptides and related compounds at low cost. For example, we can produce peptides such as Oxytocin, Desmopressin and Vasopressin in quantities as large as the market can absorb.

- A laboratory facility in a 4,800 sq. ft. area in an excellent neighborhood, fully equipped for organic synthesis. Our equipment includes NMR, Polarimeter, HPLC, FTIR UV and Visible Light Spectrophotometer, etc.

The issues we would like to present during your visit are:

1. What would be the best approach to reach major customers such as Sigma, Aldrich and others? We have prepared an introductory "catalog" and a pamphlet for distribution, a draft of which is attached. The prices offered are in the range of 20%-40% of those in the Sigma Catalog. Is this O.K.?

2. Should we try to reach the consumers directly? If so, what would be the best approach?

3. Do you know any qualified individual who may be interested in working with us in Sales and being our representative in the industry?

[illegible]

4. Are there any products, in large demand, we should produce?

5. You had advised me to contact three individuals, namely Dr. Fagan of Mallinckrodt, Dr. Felix of Hoffmann-La Roche and Dr. Hummel of Sigma.

   Dr. Fagan has informed me that his primary plan involves systhesis of Desmopressin, and that he is not interested in other peptides. It appears to me that in fact he is our competitor. Do you see any possibility for collaboration?

   Dr. Felix of Hoffman-La Roche told me that his needs were limited, and that he had the ability to synthesize his own needs.

   I have not contacted Dr. Hummel as yet. We felt Jackie Friedman, with whom we have had some dealings, may resent it if we went over her head, at least at this time. Another reason was a set-back we had with Sigma: Before we had our HPLC quality control system in operation, we prepared five products for Sigma. Unfortunately, three of these had 97%-98% purity and were not accepted. As a result of this shortcoming, we decided to suspend our dealings until all products were HPLC-controlled and met the best standards of quality. I am pleased to tell you that we have now reached this goal. Of course, we are concerned that there may be other problems that we may be unaware of.

Finally, I would like you to know that we are most appreciative of your willingness to take time and guide us in the right direction despite your busy schedule. Since the list of our "demands" is embarrassingly large, I am wondering if it would be possible for you to extend your time with us? Please let me know if this is a possibility so that we can make appropriate arrangements. I do understand that some of the issues can be presented and discussed by phone.

With kindest personal regards and best wishes for the New Year.

Sincerely,

Parviz

Parviz Lalezari, M.D.

Attachment

PEPTOSYN, INC.

PRODUCTS AND PRICE LIST

SPECIAL OFFERS

This price list reflects a series of special offerings and is only a part of Peptosyn's products.

All Peptides and Chemicals synthesized at Peptosyn are controlled by Analytical and Spectroscopical methods including HPLC. HPLC data sheet will be provided with each delivery. Other analytical information such as UV, NMR, FTIR spectra will be made available upon request.

NOTES

Prices are in U.S. Dollars. Bulk quantities will be provided with additional discounts.

Peptosyn is not responsible for any patent infringements that might occur with the use of the products listed in our catalog.

The products offered by Peptosyn are for research and laboratory use only.

Z-PEPTIDES

C5160	Z-Ala-Ala-Leu-P-nitroanilide	50mg	31.50
		250mg	105.00
C3100	Z-Ala-Pro	500mg	18.00
		1g	32.00
		5g	128.00
C3430	Z-Gly-Met	1g	12.00
		5g	55.00
		25g	200.00
C6282	Z-Gly-Pro-Phe-Pro-Leu	100mg	88.00
		250mg	205.00
C2380	Z-Phe-Ala	1g	32.00
		5g	110.00
		25g	400.00
C4883	Z-D-Phe-Phe-Gly	25mg	45.00
		250mg	250.00
C4730	Z-Pro-Leu-Gly	100mg	12.00
		1g	89.00
C3710	Z-Pro-Phe	1g	12.00
		5g	55.00
C4932	Z-Sar-Pro-Arg	100mg	28.00
		500mg	110.00
C4902	Z-Val-Gly-OMe	1g	18.50
C4602	Z-Val-Tyr-OMe	1g	18.00
		5g	70.00

CHEMOTATIC PEPTIDES

A4755	N-Acetyl-Met-Leu-Phe	50mg	60.00
		250mg	210.00
B4301	Boc-Met-Leu-Phe	100mg	85.00
B4102	Boc-Nle-Leu-Phe	100mg	85.00
F4051	N-Formyl-Met-Leu-Phe	100mg	45.00
		500mg	200.00
F5008	N-Formyl-Met-Leu-Phe-Benzylamide	25mg	75.00
		100mg	225.00
F5311	N-Formyl-Met-Leu-Phe-Phe	25mg	45.00
		100mg	135.00
F3650	N-Formyl-Met-Phe	500mg	88.00
F4083	N-Formyl-Met-Phe-Met	25mg	25.00
F4055	N-Formyl-Nle-Leu-Phe	100mg	55.00
		500mg	235.00
F7096	N-Formyl-Nle-Leu-Phe-Nle-Tyr-Lys	100mg	260.00
		500mg	950.00
V4236	Val-Gly-Ser-Glu	25mg	16.00
		250mg	120.00

FLUOROGENIC 7-AMINO-4-METHYLCOUMARIN (AMC) AMIDES OF AMINO ACIDS

A2011	L-Ala-AMC	100mg	45.00
		250mg	100.00
A2032	L-Arg-AMC	100mg	89.00
		250mg	200.00
A2006	L-Asp-AMC	25mg	28.00
		100mg	95.00
G2014	L-Glu-γ-AMC	25mg	35.00
		100mg	120.00
G2001	Gly-AMC	100mg	68.00
		250mg	150.00
I2012	L-Ile-AMC	25mg	25.00
		100mg	75.00
L2003	L-Leu-AMC	25mg	25.00
		100mg	75.00
P2010	L-Phe-AMC	25mg	25.00
		250mg	175.00
P2115	L-Pro-AMC	25mg	25.00
		100mg	85.00
S2301	L-Ser-AMC	25mg	45.00
		100mg	125.00
T2501	L-Thr-AMC	25mg	45.00
		100mg	125.00
T2901	L-Tyr-AMC	50mg	60.00
		250mg	230.00
V2611	L-Val-AMC	25mg	25.00
		100mg	76.00

FLUOROGENIC PEPTIDES

S3911	Ser-Tyr-AMC	100mg	95.00
		250mg	250.00
S5189	Suc-Ala-Pro-Ala-AMC	25mg	45.00
		100mg	160.00
S4191	Suc-Leu-Tyr-AMC	50mg	45.00
		100mg	80.00

N-FORMYL AMINO ACIDS AND PEPTIDES

F2112	Formyl-Asn	1g	15.00
		10g	120.00
F2314	Formyl-Asp	500mg	6.00
		10g	90.00
F2411	Formyl-Gln	1g	16.00
		10g	125.00
F2511	Formyl-Glu	1g	16.00
		10g	125.00
F2011	Formyl-Gly	1g	6.00
		10g	40.00

F2666	Formyl-Lys	250mg 1g 10g	8.00 20.00 175.00
F2411	Formyl-Met	250mg 1g 10g	4.00 10.00 70.00
F2152	Formyl-Phe	250mg 1g 10g	7.50 13.00 100.00
F2803	Formyl-Tyr	1g 10g	15.00 120.00
F2911	Formyl-Val	1g 10g	6.50 52.00
F3215	Formyl-Met-β-Naphthylamide	5mg 100mg	8.00 45.00
F3218	Formyl-Met P-Nitroanilide	5mg 100mg	8.00 95.00
F3252	Formyl-Val-Val	250mg 1g	30.00 95.00
F3414	Formyl-Leu-Phe	250mg 1g	30.00 95.00

PEPTIDES

P4515	Ala-Leu-Ala-Leu	50mg 100mg	80.00 150.00
P2114	β-Ala-Phe	1g 10g	60.00 450.00
P3211	Arg-Gly-Asp	25mg 250mg	38.00 280.00
P4218	Arg-Gly-Asp-Ser	25mg 100mg	92.00 335.00
P2620	Glu-Glu	500mg 1g 5g	45.00 75.00 300.00
P4433	Gly-Leu-Gly-Gly	250mg 1g	55.00 175.00
P3343	Gly-Leu-Phe	100mg 500mg	33.00 188.00
P3371	Gly-Phe-Arg	100mg 1g	80.00 400.00
P3471	Gly-Pro-Arg	100mg 1g	80.00 400.00
P3272	Ile-Pro-Ile	25mg 100mg 500mg	30.00 70.00 255.00
P2317	Leu-Met	100mg 1g	12.50 79.50
P2318	Leu-Phe	250mg 1g	9.00 28.00
P2372	Leu-Ser	100mg 1g	11.00 65.00

P2381	Leu-Trp	100mg	15.00
		1g	75.00
P3393	Leu-Val-Leu	100mg	40.00
		1g	180.00
P4638	Phe-Gln-Gly-Pro	250mg	155.00
		1g	390.00
P4224	Phe-Gly-Gly-Phe	100mg	36.00
		1g	285.00
P4242	Phe-Gly-Phe-Gly	100mg	35.00
		1g	260.00
P2081	Pro-Asn	250mg	46.00
		1g	140.00
P2056	Pro-Gln	1g	75.00
P2010	Pro-Ile	1g	32.00
		10g	210.00
P3022	Pro-Gly-Gly	1g	32.00
P4322	Pro-Leu-Gly-Gly	1g	220.00
		5g	720.00
P2008	Pro-Trp	1g	58.00
P2081	Ser-Asn	250mg	60.00
		1g	180.00
P2091	Thr-β-Ala	250mg	65.00
P2094	Thr-Gln	250mg	85.00
P2097	Thr-Ser	1g	260.00
P3973	Thr-Val-Leu	100mg	35.00
		500mg	125.00
P2980	Val-Trp	100mg	20.00
		1g	165.00

LIPIDOYL AMINO ACIDS

Lipide Residue: C_6- C_{18}

Hexanoyl, Octanoyl, Decanoyl, Lauroyl, Myristoyl, Palmitoyl and Stearoyl

Amino Acids: All usual amino acids. Lipidoyl di-amino acids such as lysine are dilipoidyl compounds. Ex: α-ω-Di Lauroyl-L-Lysine

1g	35.00
5g	100.00
25g	300.00

When ordering, please specify the lipid(s) and amino acid(s) desired.

BOC PEPTIDES

B2563	Boc-Gln-Pro	100mg	25.00
		500mg	95.00
B4326	Boc-Gly-Phe-Benzylester	1g	30.00
		5g	120.00

B2839	Boc-Gly-Pro	1g	35.00
B2311	Boc-Ile-Gly	1g	23.00
		5g	90.00
B3730	Boc-Phe-Phe-Gly	250mg	45.00
		500mg	85.00
B2620	Boc-Pro-Pro	100mg	30.00
		1g	180.00
B5311	Boc-Val-Leu-Gly-Arg	50mg	50.00
		250mg	200.00

Central Research Division
Pfizer Inc
Eastern Point Road
Groton, CT 06340
Tel 203 441 4669 Fax 203 441 4111

Pfizer

Central Research

Fredric J. Vinick, Ph.D.
Director
New Leads/Structural Chemistry

May 19, 1994

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

Dear Alfred:

Thanks for your kind letter. I have enclosed the original *Science* paper which describes the discovery of the first quinuclidine SP antagonists - thought I had included it before but maybe not. Keep in touch!

Fondly,

Fredric J. Vinick (dg)
Fredric J. Vinick

:dg

Enclosure

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

May 16, 1994

Dr. Fredric J. Vinick, Director
New Leads/Structural Chemistry
Central Research Division
Pfizer Inc.
Eastern Point Road
Groton, Connecticut 06340

Dear Fred,

I was particularly happy to have your letter of May 10th with that fitting title "Director, New Leads/Structural Chemistry" on your stationery. What a lovely title, and so fitting.

The lead paper which you so kindly sent to me is the one by J. A. Lowe, et al. in J.Med.Chem. 35, 2591 (1992). But I am certain that I saw an earlier paper which was the very first describing in some detail the lead from our library to you. Would it be possible to send me that very first paper?

All good wishes.

Sincerely,

Central Research Division
Pfizer Inc
Eastern Point Road
Groton, CT 06340
Tel 203 441 4669 Fax 203 441 4111

Central Research

Fredric J. Vinick, Ph.D.
Director
New Leads/Structural Chemistry

May 10, 1994

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

Dear Alfred:

It was a rare pleasure hearing from you once again. I have enclosed reprints of five papers dealing with the quinuclidine substance P antagonists. Hopefully they will be of some use to your autobiographical efforts.

Best of luck with everything. Please keep in touch!

Sincerely,

Fredric J. Vinick

:dg

Enclosures

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

April 27, 1994

Dr. Fred Vinick
Pfizer Inc.
Central Research Laboratories
Department of Medicinal Chemistry
P.O. Box 307
Groton, Connecticut 06340

Dear Fred,

I haven't heard from you for a very long time and hope you are well.

I am just working on my autobiography which I hope to complete this summer.

Could you please send me some information about what happened to that quinuclidine lead. Did Pfizer actually get a product, or did it die in the clinic?

I would appreciate any publication on this that you could send me for inclusion in this paragraph.

All good wishes.

Sincerely,

Enclosure

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

October 18, 1993

Mr. and Mrs. H. Pinkalla
P.O. Box 28817
Greenfield, Wisconsin 53220

Dear Emily and Ham,

Thank you for your gracious note of October 7th, received only today.

As I already mentioned on the phone, you have my permission to print that talk given at the SEED luncheon.

Best wishes,

EMILY AND
HAMILTON A. PINKALLA
P.O. Box 28817
Greenfield, WI 53220

October 7, 1993

Dr. A. Bader
2961 No. Sheppard avenue
Milwaukee, WI 53209

Dear Al:

Thank you for your "ALMEMO" birthday greeting. Your enclosure of copies of Dr. Brennan's letter and your talk on Project Seed at the ACS National meeting in Chicago were most welcome.

Emily and I would like to have your permission to include your speech in the October Chem-Vets Newsletter.

Your thoughts, ideas and personal philisophy re: chemistry and the chemist's contribution and responsibility to life on this earth, are, and should be, of interest to every chemist.

And you are to be commended for your sponsorship and work to promote this project and your philosophies.

We will appreciate your comments.

And when you have a day to spare, let's have lunch or dinner at the Wisconsin Club so we can expand and exchange some ideas.

SINCERELY,

Emily and Ham

TELEPHONE 543-4714 HAMILTON
543-4768 EMILY
MOBILE 581-4719

Dr. Alfred R. Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

August 24, 1993

Mr. and Mrs. H. Pinkalla
P.O. Box 28817
Greenfield, Wisconsin 53220

Dear Emily and Ham,

Thank you for your kind invitation to join you in celebrating Ham's 85th birthday on Saturday, September 25th.

I wish we could come, but that is the Day of Atonement, the one day in the year when we don't eat. I do hope that we will all be around for you to invite us to Ham's 90th birthday.

As you perhaps know, I keep busy despite being dismissed by Sigma-Aldrich. Yesterday, I gave a talk on Loschmidt at the A.C.S. and then a speech after the luncheon celebrating 25 years of Project SEED.

The enclosed will tell you a little bit about what else I have been up to.

Fond regards and all good wishes,

Enclosure

Please join us in celebrating
Hamilton's 85th Birthday
Saturday, September 25, 1993
at six thirty in the evening.
Cocktails, Dinner and Socializing

The Wisconsin Club
900 West Wisconsin Avenue
Milwaukee, Wisconsin

Emily and Hamilton Pinkalla
Please respond no later than September 4
Please no gifts

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

February 4, 1993

Mr. and Mrs. H. Pinkalla
P.O. Box 28817
Greenfield, Wisconsin 53220

Dear Emily and Ham,

Thank you so much for Newsletter, Volume 3, No. 1.

I wonder how many readers have pointed out to you that Dr. Helen Free is not the first woman president of the American Chemical Society. At least one other, and perhaps the first, was Professor Anna Harrison in the Chemistry Department at Mount Holyoke College. Anna is my good friend, and I enclose introduction to a catalog of paintings at Mount Holyoke, dedicated to her. Yet another woman, Dr. Mary Good of Allied Chemical, served as president of the A.C.S. three or four years ago.

All good wishes.

Sincerely,

Enclosure

Dr. Alfred Bader
2961 North Shepard Avenue
Milwaukee, Wisconsin 53211

January 18, 1993

Mr. and Mrs. H. Pinkalla
P.O. Box 28817
Greenfield, Wisconsin 53220

Dear Emily and Ham,

I have received several most interesting letters from you, mainly relating to ChemVets.

Isabel and I have been travelling a good deal, but between now and early May we will be travelling one week each month. So, I very much hope that we can get together and exchange notes about our activities.

Best wishes.

As always,

EMILY AND
HAMILTON A. PINKALLA
P.O. Box 28817
Greenfield, WI 53220

December 3, 1992

TO: Dr.A. Bader

Dear Al:

Seeing your picture in the "Chemical Heritage" and the tribute the publication paid to you and Isabel, for your part in making the Robert Burns Woodward symposium a success, reminded Emily and me that Milwaukee has a truly creative "organiker" in Alfred Bader in our midst.

Emily and I are still looking forward to haveing a lunch or dinner with you and Mrs. Bader. We can talk about the Chem Vets and your ideas about what we can do to more effectively channel the talents and efforts of retired chemists to interest our young people in chemistry and the sciences.

When you have a day free, please call us.

Sincerely,

Emily and Hamilton

TELEPHONE 543-4714 HAMILTON
543-4768 EMILY
MOBILE 581-4719

PLEASE ADDRESS REPLY TO:

EMILY AND
HAMILTON A. PINKALLA
P.O. BOX 28817
Greenfield, Wisconsin 53220

CHEM-VETS NEWSLETTER

Vol. 3 No. 1

January, 1993

The holidays have come and gone and the New Year (1993) is upon us.

* * * * *

We are in receipt of a very nice letter from Sue Roethel, Manager of the Office of Local Section Activities of the American Chemical Society (ACS), Washington DC.

Sue coordinates the activities of the local sections, including Milwaukee, and is on the mailing list to receive the Chem-Vets Newsletter.

She is doing a great job and we sincerely appreciate her letter and the comments contained therein.

Sue wrote, "she likes the Health Watch column" in the newsletter and hopes that our non-retired members will read it also as it applies to chemists of all ages.

Thank you Sue. Please send us your comments whenever you think they are appropriate.

* * * * *

Getting to Know You

Beginning in February of this year Emily and I will begin a new program called, "Getting to Know You."

This program will consist of telephone calls to randomly selected Mr. and Mrs. Chem-Vet, whom we will write to have lunch with us.

We hope that these luncheon visits will enable you to give us your thoughts, ideas and suggestions to make Chem-Vets more effective, more meaningful and more productive as an organization and activity. Over time we hope to meet each one of you through these personal visits.

We tried this on an experimental basis with Dr. and Mrs. Thomas H. Hartzell of Sheboygan. It was indeed a very pleasant and rewarding experience, especially in terms of the new ideas generated and in the new friendship it created.

It is hoped that a by-product of this program will be an interaction and friendship between individual Chem-Vets as well.

* * * * *

Health Watch

Our Health Watch for the month of January is Staying Well and Avoiding Illness in 1993.

* * * * *

New President of the ACS

Dr. Helen M. Free is the new President of the ACS. I believe that history has been made. This the first time that we have a woman president.

A letter, a simple post card or a more formal card of congratulations and best wishes would indeed be very appropriate from each and every Chem-Vet. Write to our "First Lady of Chemistry."

Helen M. Free, President
American Chemical Society
1155 Sixteenth Street N.W.
Washington DC 20036

* * * * *

Dr. Alfred Bader
924 East Juneau, Suite 622
Milwaukee, Wisconsin 53202
Phone: 414/277-0730
Fax: 414/277-0709

A Chemist Helping Chemists

October 16, 1996

Dr. Richard Pariza
CP Consulting, Inc.
43323 North Oakcrest Lane
Zion, IL 60099-1258

Dear Richard,

Thank you for your gracious letter of October 7th.

Isabel and I certainly enjoyed ourselves in Chicago, particularly of course meeting so many old friends. And now I have to thank you for those beautiful photographs.

I do hope that you will be able to obtain a quotation for the potasium salt of acetylenedicarboxylic acid. Both Cilag and Aldrich will be interested and surely the details of the preparation which I sent show that it is not very difficult. Of course I trust that the supplier will reserve a commission for you.

Isabel and I will be gone from Milwaukee almost all of the time between now and January 6th but the balance of January is open and I very much hope that you will come and visit us.

Best personal regards.

Sincerely,

AB/lh

20th
IUPAC Symposium
ON THE
CHEMISTRY
OF NATURAL
PRODUCTS

CP Consulting, Inc.
43323 Oakcrest Lane North
Zion, IL 60099-9413
Phone: (847) 872-6925
Fax: (847) 872-6920

October 7, 1996

Dr. Alfred Bader
Alfred Bader Fine Arts
Suite 622
924 East Juneau Avenue
Milwaukee, WI 53202

Dear Alfred:

I want to again thank you and Isabel for coming to Chicago to greet the speakers and discussion leaders at our dinner on September 18. Many of them told me they were delighted to see you. Several people said that your talk was truly outstanding, and was among the best of our symposium. I wholeheartedly agree. Let me assure you that this is high praise considering the many distinguished speakers we were fortunate to get. As Sir Derek said, "I came expecting to attend a meeting, but I found myself at an event!"

I hope that we can get together soon to discuss several things. Mukund and I would like to work with you, as we have with the Cilag/Herdillia order. We are also looking into production of acetylene dicarboxylic acid in India. As you advised me well over a year ago, a consulting business grows very slowly. We have several clients who expect instant responses from us, but are dreadfully slow to pay their bills. One in particular owes us a very large sum in fees and expenses, and is causing severe cash flow problems for both Mukund and me. They are a small company, so the problem is if we get too nasty, or refuse to continue helping them, we will probably never get paid anything. Furthermore, I am astounded at how rude businessmen are these days. With all the new electronic ways we have to communicate with each other, many key people simply do not return phone calls or answer letters (E-mails, faxes, etc.) I am not just referring to IUPAC fund-raising letters. The information age has numbed many people to the content of what is being communicated. They have become slaves to their voice-mail, pagers, E-mail, and faxes, and are drowning in information without understanding, knowledge without the wisdom to use it constructively. I sincerely hope our *IUPAC Symposium* served to present new ideas and inspire the delegates, not just offer them more data and facts. Such meetings also allow people to meet, face-to-face, strengthening the sorts of friendships you have uniquely cultivated over many years of diligence. At the closing ceremony, after Jean-Marie Lehn's brilliant lecture, I told those few who were still there that as wisdom translates knowledge into understanding, genius transforms understanding into beauty. How well you must know that from your studies of art, the Bible, and now the history of chemical discoveries and insights!

I recently bought some artwork at an auction, and I would like your opinion of it. As you know I have always enjoyed fine art, and I would like to continue to collect, however at a much more modest level than yourself. I will bring some along when we visit.

Please give my best to Isabel. Thank you again for your delightful and thought-provoking lecture at our speakers' dinner. It was scholarly yet anything but boring! I hope you enjoy the enclosed pictures. I look forward to seeing you soon. Best personal regards.

Sincerely yours,

Richard J. Pariza

FAX FROM

DR. ALFRED BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone: 414/277-0730
Fax: 414/277-0709

May 22, 1996

Page 1 of 3

To: Dr. Mukund S. Chorghade
Fax: 708/360-9175

Dear Dr. Chorghade:

I am sorry to have to tell you that your letter of May 15th arrived here only today, and I must reply very quickly as I am leaving for a two-month trip to Europe this coming Saturday, May 25th.

I have not received the detailed schedule of the scientific and social programs, nor the registration form, nor have I heard from Dr. P.D. Gujral, the IUPAC Secretary. The only communication I have had has been with Dr. Richard Pariza, and I attach my letter to him of July 31, 1995.

You will note that I agreed to be your after-dinner speaker on Tuesday evening, September 17th, and I presume that for an after-dinner speech, you require neither an abstract nor a four-to-six page article for publication. Please do let me know that I am correct in thinking this.

I sent Dr. Pariza a copy of my CV and now attach another copy.

The title of my after-dinner speech will be *"A Chemist Turns Detective: Richard Anschütz Discovers the Work of Archibald Scott Couper and Josef Loschmidt"*.

As I am leaving on Saturday, I would appreciate your prompt reply.

With many thanks for your help and best regards, I remain,

Yours sincerely,

Alfred Bader

AB/cw

c: Dr. Richard Pariza (fax: 847/872-6920) ✓

Thursday
Best regards
Alfred

FAX FROM

DR. ALFRED BADER
Suite 622
924 East Juneau Avenue
Milwaukee, Wisconsin 53202
Telephone: 414/277-0730
Fax: 414/277-0709

March 7, 1996

To: Dr. Richard Pariza
Fax: 847/872-6920

Dear Richard:

I am happy to be able to tell you that I just learned from Cilag that they have placed a trial order for 200 kilos of 2,6-diisopropylphenol with Herdillia.

It takes large companies like J&J - of which Cilag is part - a long time to decide, but all is well that ends well. Hopefully, the 200 kilos will be satisfactory, and Cilag will then order more.

Do you perchance know of any possible supplier for acetylenedicarboxylic acid potassium salt?

Many thanks for all your help and best regards, as always,

AB/cw

CP Consulting, Inc.
43323 Oakcrest Lane North
Zion, IL 60099-9413
Phone: (847) 872-6925
Fax: (847) 872-6920

February 28, 1996

Chw: Record

Dr. Alfred Bader
Suite 622
924 East Juneau Avenue
Milwaukee, WI 53202

By Fax

Dear Alfred:

I received your recent letter by mail. Perhaps the difficulty in sending it by Fax was due to our Area Code change in January. Both the new and old codes are supposed to work for three months, but many others have experienced difficulty. Please note the new area code (847) as well as my dedicated fax line: (847) 872-6920. The ACS sent me a fancy fax machine to use for the process journal, and it needs a dedicated line. I still have my old machine on the voice line: (847) 872-6925.

We have checked on acetylenedicarboxylic acid potassium salt, but have not yet found a supplier in India. Thank you for the inquiry. Herdillia could be a supplier, but we are reluctant to contact them because of the strange situation with the 2,6-diisopropylphenol request. We are very puzzled by the lack of response by Cilag to several Herdillia faxes. I have attached some correspondence. It is my understanding that Herdillia made a satisfactory sample and acceptable quotation, but has not been able to get any response from Cilag. Perhaps you can look into this and get a reply.

Please give my warmest wishes to Isabel. I look forward to visiting with you both soon. Best personal regards.

Sincerely yours,

Richard

Richard J. Pariza, Ph.D.
Pager: (800) 631-2281

WPDOC:BADER3

FAX FROM [illegible]

[illegible] International, Inc.

[illegible]

[illegible]

2011

[illegible]

4 12

[illegible]

[illegible] First of all, I heard [illegible] Chemical Development [illegible] in Pearl River, New [illegible] think about it [illegible] as a reference. I think that I could really help the [illegible] appears [illegible] scientific excellence in chemical development is a weakness for them. [illegible] job description. It sounds like a great opportunity.

Secondly, Dr. Dieter Best of CarboGen Laboratories contacted me. Initially he wanted me to put him in touch with some key people at major pharmaceutical companies, which I did. I have helped him get several new customers. He called recently [illegible] his expansion in A[illegible] has resulted in a severe shortage of working capital for CarboGen. He is very anxious to find some partners to invest and bring his capitalization in line with what the Swiss banks are demanding. Despite several new orders and a successful record of profitable sales, he has had to lay off several key people and close his [illegible] I was extremely impressed with CarboGen's technical competence, integrity, and professionalism when I dealt with them at Abbott. I told Dieter that I would contact you and suggest that you might visit him in Switzerland if you have the time and interest. [illegible] investors or partners. Dieter, Mukund Chorghade and I had discussed the possibility of forming "CarboGen USA" and even "CarboGen India" in partnership with Dr. [illegible] Rao. I think such collaborations could be very succ[illegible]

If you are [illegible] numbers. He would [illegible] I would also be happy to discuss th[illegible]

[illegible]

Howe-Lewis International, Inc.

[illegible]

[illegible]

[illegible]

[illegible]

ALFRED BADER FINE ARTS

DR. ALFRED BADER

ESTABLISHED 1961

July 14, 1995

Dr. Richard Pariza
43323 North Oakcrest Lane
Zion, IL 60099-1258

Dear Dr. Pariza:

Dr. Bader is currently traveling in England through the end of July, but he has asked me to forward a copy of the enclosed fax from Dr. Hammer at Cilag and to ask you if you can urge your Indian friends to speed up the quote.

Best wishes,

Cheryl Weiss
Office Manager

Enclosure

By Appointment Only
ASTOR HOTEL SUITE 622
924 EAST JUNEAU AVENUE
MILWAUKEE WISCONSIN USA 53202
TEL 414 277-0730 FAX 414 277-0709

CILAG LTD
Hochstrasse 201/209
CH-8201 Schaffhausen
Switzerland

TELEFAX

From: *Dr. E. Hammer/as*
Fax-No.: (41) 53/829 443
(Direct fax No Chem Op)
Date: *July 10, 1995*
Pages: 1
including this cover page

To: *Dr. Alfred R. Bader*
Company: *Suite 622*
Milwaukee / USA
Fax-No.: *001'414/277 0709*
Please provide copies for:

Message:

Dear Alfred,

The information from India for possible supply of Diisopropylphenol needs more time as expected. The company confirmed but we are waiting for

samples, specification and price.

Where do you stay with your book? Is it published? We expect the ordered examples. I am very interested in your modern history of chemistry.

Kind regards

CILAG AG

Dr. E. Hammer

PS: *Aldrich Chemical prepare an offer for final steps made in their plant.*

RICHARD J. PARIZA

43323 Oakcrest Lane North
Zion, Illinois 60099
Phone: (708) 746-3530
Fax/Phone: (708) 872-6925

EDUCATION

Ph.D. in Organic Chemistry, December, 1983
Purdue University. Analytical minor.

B.S. in Chemistry, December, 1976
University of Wisconsin-Milwaukee.

EMPLOYMENT

1969-1975 President and founder of Willow Brook Laboratories, Inc., Waukesha, Wisconsin. Directed up to 15 employees, several with advanced degrees, in research and the manufacture of fine organic chemicals. Production was from bench scale to 500 gallon Pfaudler equipment; over 150 products offered. Handled administration, research and sales

1980-1983 Head graduate instructor for organic chemistry course at Purdue University, West Lafayette, Indiana. Graduate instructor for graduate level synthetic and spectroscopy courses.

8/83-6/89 Abbott Laboratories, Abbott Park, Illinois. Rose to Manager, Chemical Services in the Pharmaceutical Products Division (PPD). Started the process research function in PPD; designed and equipped two laboratory modules; interviewed and hired the professional staff. Responsible for 15 chemists (7 with Ph.D.'s) in Process Research, Organic Preparations, and High Pressure sections: handled budget, scheduling, professional development. Hands-on work included developing a new synthesis of a cardiovascular drug candidate (for which a cash award was received), pioneering two new series for the Macrolide Discovery team, and developing new chemistry for several lead compounds in other projects.

6/89-4/95 Abbott Laboratories, North Chicago, Illinois. Manager, Process Research in the Chemical and Agricultural Products Division (CAPD). Started the process research function in CAPD, initially retaining strategic responsibility for PPD Process Research (the PPD department has grown

to 23 scientists, with 15 Ph.D.'s). Personally recruited and hired professional staff from core of 5 scientists to current 48 (35 Ph.D.'s) and several openings. Responsible for process research on new R&D candidates from pre-IND to near NDA for PPD: budgets and schedules are negotiated with R&D management; scientists from Discovery, PPD and CAPD process groups are assigned and coordinated; resources (analytical support, pilot plant time, raw materials, consultants, etc.) are arranged for and coordinated. Full budget, strategic planning and professional development responsibilities. Overall responsibility for safety and cGMP compliance of work within department; accountable for EPA and employee exposure issues. Additional projects include scope and justification of new $40MM pilot plant for CAPD and $3MM Laboratory and Kilo Lab for PPD, strategic presentations to top corporate management, and improving relations between PPD and CAPD. Vendor development activities including many trips, foreign and domestic, to evaluate suppliers, arrange for technology transfer, long range planning, etc. Current annual budget of ca. $20MM.

PUBLICATIONS

R.J. Pariza and P.L. Fuchs: J. Org. Chem. (1983), **48**, 2304.

R.J. Pariza and P.L. Fuchs: J. Org. Chem. (1983), **48**, 2306.

R.J. Pariza, F. Kuo, and P.L. Fuchs: Synth. Commun., (1983), **13**, 243.

J.J. Plattner, A.K.L. Fung, J.A. Parks, R.J. Pariza, S.R.Crowley, A.G. Pernet, P.R. Bunnell and P.W.Dodge: J. Med. Chem., (1984), **27**, 1016.

P.R. Bunnell, J.J. Plattner, A.K.L. Fung, R.J. Pariza and P.W. Dodge, in: "Diuretics", Jules B. Puschett, Editor, Elsevier, 1984, 374-381.

R.J. Pariza and P.L. Fuchs: J. Org. Chem., (1985), **50**, 4252.

C.N. Hsiao, L. Bhagavatula, and R.J. Pariza: Synth. Commun. (1990), **20**, 1687.

D.J. Krysan, A.R. Haight, J.E. Lallaman D.C. Langridge, J.A. Menzia, B.A. Narayanan, R.J. Pariza, D.S. Reno, T.W. Rockway, T.L. Stuk, and J.H. Tien: Org. Preparations and Procedures Int., (1993), **25**, 437.

D. S. Reno and R. J. Pariza: "Phenylvinyl sulphide", Organic Synthesis, (1995), submitted.

M.S. Chorghade, D.H. Dolphin, D.R. Hill, E.C. Lee, L.-Y. Zhang

and R.J. Pariza: "Metalloporphyrins as Chemical Mimics of Cytochrome P-450 Systems", Pure & Appl. Chem,, (1995), in press.

M.S. Chorghade, E.C. Lee, and R.J. Pariza, Tying a GABA Between Copenhagen and Chicago, "Advances in Medicinal Chemistry", Bruce and Cynthia Maryanoff, Eds., JAI Press, 1995, in press.

A.V. Rama Rao, M. K. Gurjar, S. Pai, R.J. Pariza and M.S. Chorghade: "Synthesis of a Novel C2-Symmetrical (2S,5S)-2,5-Bis-[(1,1-dimethylethoxy)carbonylamino]-1,6-diphenylhex-3-ene: Applications in the Synthesis of Potential HIV Protease Inhibitors, Tet. Lett., (1995), in press.

PRESENTATIONS

"Pharmaceutical Chemists in the 1990's" Invited, in "Symposium on Chemistry in the 1990's" Division of Professional Relations, 190th ACS National Meeting, 1985, Chicago.

"The Chemistry of a Benzisoxazole Diuretic" 20th Great Lakes Regional Meeting of the ACS, 1986, Milwaukee.

"Chemical Modifications of Erythromycin" Invited, in: "Symposium on Recent Developments in Macrolide Antibiotics", 27th Interscience Conference on Antimicrobial Agents and Chemotherapy, 1987, New York.

"The Acyloin Reaction" Invited, Gordon Research Conference on Organic Reactions and Processes, 1989, New Hampton, NH.

"New Challenges for an Old Profession" Invited, in "Chemistry for the Year 2000" Milwaukee Section of the ACS Spring Symposium, 1990, Marquette University.

"HIV Protease Inhibitors: The Chemistry of New Hope" Invited, in: "Symposium on Antivirals" 204th ACS National Meeting, 1992, Washington , D.C.

"Toward a Better Understanding Of Solvent Interactions" Invited, 15th Gulf Coast Chemistry Conference, 1992, Pensacola Beach, Fl.

19th IUPAC Symposium on the Chemistry of Natural Products, Invited speaker, Karachi, Pakistan, January 16-20, 1994.

INVITED RESEARCH LECTURES AT UNIVERSITIES and INSTITUTES

Emory University, Atlanta, GA (Hosted by Professor Dennis Liotta), April 3, 1990.

Florida State University, Tallahassee, FL (Hosted by Professor

Robert Holton), November 7, 1990.

Case Western Reserve University. Clevelend, OH (Hosted by Professor Anthony Pearson), February 28, 1991.

University of Chicago, Chicago, IL (Hosted by Professor David Lynn), February 20, 1992.

Scripps Research Institute, La Jolla, CA (Hosted by Professor Chi-Huey Wong), October 8, 1992.

University Louis Pasteur, Institut Le Bel, Strasbourg, France (Hosted by Professor Jean-Marie Lehn), October 27, 1993.

Indian Institute of Chemical Technology, Hyderabad, India, (Hosted by the Institute Director, Professor Rama Rao), January 24, 1994

University Department of Chemical Technology Golden Jubilee Visiting Fellowship Lecture, University of Bombay, India, (Hosted by the Director, Professor M. M. Sharma), January 28, 1994.

National Chemistry Laboratories, Puna, India, (Hosted by the Director, Professor R. A. Mashelkar), January 31, 1994.

University of Arizona, Tucson, AZ (Hosted by Professor Jacquelyn Gervay), October 6, 1994.

Northwestern University, Evanston, IL (Hosted by Professor Frank McDonald), October 27, 1994.

Stanford University, Palo Alto, CA (Hosted by Professor Dale Drueckhammer), March 29, 1995.

Marquette University, Milwaukee, WI (Hosted by Professor William Donaldson), April 21, 1995.

US PATENTS

Pariza, R.J., Maring, C.J., Lartey, P.A., Klein, L.L.: "New 9-R-aza-cyclic Erythromycin Derivatives..." US# 5,075,289 (12/24/91).

Norbeck, D.W., Plattner, J.J., Rosen, T.J., Pariza, R.J., Sowin, T.J., Garmaise, D.L., Hannick, S.M.: "New N-cyclobutyl analogs of pyrimidine nucleosides with antiviral and antitumour activities..." US# 5,153,352 (10/06/92).

Doherty, E.M., Hannick, S.M., Pariza, R.J., Sowin, T.J.: "Process for preparing substituted cyclobutane purines." US #5,235,052 (8/10/93).

Garmaise, D.L., Hannick, S.M., Norbeck, D.W., Pariza, R.J., Plattner, J.J., Rosen, T.J., Sowin, T.J.: "Carbocyclic Nucleoside analogs." US# 5,246,931 (9/21/93).

Pariza, R.J., Hannick, S.M. Sowin, T.J., Doherty, E.M.: "Process for preparing Substituted Cyclobutanes" US# 5,312,963 (5/17/94).

Pariza, R.J., et. al., US# 5,399,775 (3/21/95).

Several other patents filed, some with foreign issue. Several additional publications in preparation.

OTHER PROFESSIONAL ACTIVITIES

Elected Chairman for the 1995 Gordon Research Conference on Organic Reactions and Processes, to be held July 16-21, 1995; Vice-Chairman for 1994 Conference.

Founder, charter member of Steering Committee for the "Midwest Pharmaceutical Process Chemistry Consortium". The first annual meeting was held at Eli Lilly, October 11 and 12, 1993, and attracted nearly 200 participants. Six scientific lectures and 32 posters were presented, and a GMP workshop was held. Co-Chairman of the Second annual meeting held at Abbott, October 13-14, 1994, attracting 300 scientists. The Third annual meeting is scheduled for Upjohn, Kalamazoo, MI, October 12-13, 1995.

Chairman of the Organizing Committee for the "20th IUPAC Symposium on the Chemistry of Natural Products" to be held in Chicago, September 15-20, 1996. Over 1,500 distinguished scientists from around the world are expected to attend. Speakers who have already accepted include top academic and industrial scientists, and several Nobel Laureates.

Member of an American Chemical Society Task Force to evaluate the creation of a new Process Research Journal, 1993-1994. Chairman of the search committee for the Editor of the Journal, 1995. The ACS plans to launch the new journal in mid-1995. The Journal has been fully approved by the ACS Board of Directors, and will be cosponsored by the UK Royal Society of Chemistry.

Member of the American Chemical Society, including Organic and Medicinal Sections, and the Chicago Local Section.

Member of the International Union of Pure and Applied Chemistry (IUPAC).

ADDITIONAL INFORMATION and REFERENCES

Available on Request

Chemists Helping Chemists in Research and Industry

aldrich chemical company, inc.

Dr. Alfred Bader
Chairman

February 11, 1988

Dr. Robert Levine
Pittsburgh Science Institute
635 Ridge Avenue
Pittsburgh, Pennsylvania 15212-6099

Dear Bob:

I am happy to know from your delightful letter of Febrary 5th that you and Dorothy are enjoying retirement and are so very productive.

Regarding your requests, please find enclosed the three Aldrichimica Acta which were published in 1987, as well as a number of reproductions of my paintings. A copy of our latest catalog is going out to you under separate cover, and I have asked that your name be placed onto our Aldrichimica Acta mailing list.

Our mugs are offered on page 43 of the enclosed Acta, and our books on paintings are offered on page 1. Unfortunately, I cannot send these to you as gifts, and I hope that you will understand.

As you include some discussion about the beginnings of Aldrich, the enclosed excerpt from an autobiography may interest you.

May I ask you for a favor, if you will find it easy. The one book from which I have learned than any other is a commentary on Genesis written by a German rabbi, Benno Jakob. His grandson is in Pittsburgh, perhaps at the very Temple in which you are active. Rabbi Jakob's book is very hard to come by, most instructive and in German. Quite a few years ago I sent Rabbi Jakob in Pittsburgh $5000 to help with a translation and publication of Genesis in English. Since then, I have not heard anything whatever, and am wondering whether I will ever see that translation. If you should happen to know Rabbi Jakob, please do ask him.

If you ever have a chance to visit us in Milwaukee, we would love to see you.

Fond regards,

Alfred Bader
AB:mmh
Enclosures

P.O. Box 355, Milwaukee, Wisconsin 53201 USA, Telephone (414) 273-3850, Cable Aldrichem TWX 910-262-3052, Telex 26-843

Chemists Helping Chemists in Research and Industry

aldrich chemical company, inc.

Dr. Alfred Bader
Chairman

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March 10, 1986

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624 5000

Prof. Alan P. Kozikowski
Department of Chemistry
University of Pittsburgh
Chemistry Building
Pittsburgh, Pennsylvania 15260

Dear Prof. Kosikowski:

Thank you for your thoughtful letter of March 5, regarding your exciting work on the phosphoniosilylation of enones. We will consider incorporating this work in one of our advertisements later this year.

I have not been to the Chemistry Department of the University of Pittsburgh for a very long time, and I am wondering whether a visit with you some time in August or September would be convenient. Will you be in Pittsburgh during both months? It would give me great pleasure if I could thank you personally for all your help.

Best regards.

Sincerely,

Alfred Bader

AB:mmh

cc: Dr. A. W. Runquist

Dr. Alfred Bader
Suite 622
924 East Juneau Avenue
Milwaukee, Wis.
Fax No 001-414-277-0709

Lieber Alfred ! Ich sende Dir die beiden übersetzten Abstracts. Ich habe sie etwas gekürzt um möglichst nahe an die vorgeschriebenen 10 Zeilen zu kommen.Bitte laß mich wissen, ob Du damit einverstanden bist.

Die Frage Deiner Autobiographie werde ich demnächst mit einigen Leuten im Ausschuß erörtern.

2 enclosures

Beste Wünsche für 1995

Loschmidt's graphische Formeln von 1861: Vorläufer der modernen Strukturformeln

Alfred Bader

Eine der großen Errungenschaften des 20.Jahrhunderts ist die Aufklärung der genauen Strukturen von Molekülen, die wir hauptsächlich der Röntgenstrukturanalyse und der NMR-Spektroskopie verdanken.

Der erste, der eine Darstellung von Strukturen organischer Moleküle als möglich erachtete, war Josef Loschmidt. Er veröffentlichte seine "Chemischen Studien" 1861, im gleichen Jahr in dem Kekulé erklärte, daß solche Darstellungen unmöglich wären. Loschmidt stellte Doppel- und Dreifachbindungen so dar, wie wir es heute tun, zeichnete in richtiger Weise heterocyclische Verbindungen und Zucker, ebenso Cyclopropan, 20 Jahre vor dessen erstmaliger Synthese. Mehr als 100 Benzolderivate wurden von ihm mit 6 Kohlenstoffatomen in einem Kreis dargestellt.

Loschmidt war seiner Zeit so weit voraus, daß die Bedeutung seiner Arbeit leider erst von Anschütz, 50 Jahre später, richtig verstanden und gewürdigt wurde. Heute stellt sich die Frage, wieso es zu dieser Verzögerung im Erkennen der Bedeutung der Loschmidt'schen Arbeit gekommen ist und wie sich die Wissenschaft entwickelt hätte, wenn es diese Verzögerung nicht gegeben hätte.

Anschütz, Couper und Loschmidt

Alfred Bader

Die größten Durchbrüche in den Grundlagen der organischen Chemie im 19.Jahrhundert waren die Erkenntnisse über die Vierwertigkeit des Kohlenstoffs, über die Kohlenstoff-Kohlenstoff Bindung und daß Benzol 6 Kohlenstoffatome in einem Ring enthält. Alle diese Entdeckungen wurden Kekulé zugeschrieben.

Richard Anschütz, ein Schüler Kekulés und sein Nachfolger auf dem Lehrstuhl für Organische Chemie in Bonn, arbeitete viele Jahre an einer Biographie Kekulés. Bei seinen Nachforschungen machte er zwei bemerkenswerte Entdeckungen. Ein junger Schotte, der in Paris studierte, hatte gleichzeitig mit Kekulé die Vierwertigkeit des Kohlenstoffs und die Möglichkeit der Kohlenstoff-Kohlenstoff Bindung erkannt. Seine Veröffentlichung - vielleicht noch vor Kekulés Arbeit geschrieben - erschien mit Verspätung, weil Professor Wurtz die Übermittlung an die französisbhe Akademie der Wissenschaften verzögert hatte.

Die zweite Entdeckung betraf Loschmidt's Schrift "Chemische Studien", in der Benzol als Kreis mit 6 Kohlenstoffatomen dargestellt wird.

Die Bemühungen von Anschütz, die Leistungen dieser beiden Forscher hervorzuheben, können als Wiedergutmachung für die Versäumnisse seines Lehrers aufgefaßt werden. Ohne Anschütz wüßte man heute wenig von Couper und nichts über Loschmidts Arbeit über die Struktur organischer Verbindungen.

→ FAX 001 414 277 0709