Alfred Bader fonds

Correspondence

K

1992-2003

Dear Professor Kocovsky,

I enjoyed chatting with you this morning and will call you from Bexhill next Wednesday, May 28th at 10 AM to discuss the e-mail exchange with the Rector at the Masaryk University and about Dr. Jiri Damborsky who is now at the Masaryk University.

With many thanks for all your help I remain

Yours sincerely,
Alfred Bader

Česká společnost chemická
Czech Chemical Society
Novotného lávka 5, CZ-116 68 Praha 1
tel. 02 / 210 82-383, fax 02 / 222 201 84 http://www.csch.cz

Dr. Alfred Bader
924 East Juneau, Astor Hotel - Suite 622
Milwaukee
Wisconsin 53202, USA
Fax: 001 414 277-0709

May 22, 2003

Dear Dr. Bader,

First of all, I would like to inform you that the second winner of The Alfred Bader's Award in Bioorganic and Bioinorganic Chemistry is Dr. Jiří Damborský from Masaryk University in Brno

Dr. Damborský was elected by the Award's Committee on May 20th, winning with 6 votes from 8 cast.

The prize was presented to Dr. Damborský for the series of 56 original papers focused mainly on the study of structure - biological properties relationship. All of these papers were published in highly impacted journals. The prize will be awarded to Dr. Damborský at the conference Advances in Organic, Bioorganic and Pharmaceutical Chemistry organised by Czech Chemical Society in Nymburk on November 2003.

I will inform you in more detail during your visit to Prague on June 16th.

Since your arrival and term of your lecture on Joseph Loschmidt at our faculty (June 16th, 10.00 a.m.) is approaching I would like to make an official announcement, therefor I would be glad if you can give me an exact title of your talk. Your lecture will be held in Jaroslav Heyrovský lecture hall where is excellent acoustics, so that lapel microphone is unnecessary.

Best personal regard also to your wife.

T. Trnka

Prof. Tomáš Trnka
Chairman of the Award Committee

Pavel Kočovský
Sir William Ramsay Professor of Chemistry and Head of the Synthesis Section
Department of Chemistry
University of Glasgow
Glasgow G12 8QQ
United Kingdom
Tel: +44-(0)141-3304199 *Fax:* +44-(0)141-3304888
E-mail: P.Kocovsky@chem.gla.ac.uk
http://www.chem.gla.ac.uk/%7Epavelk/Homepage.html

UNIVERSITY *of* GLASGOW

Given to S. Fissan for Mark Redlich as Chris out of town. To CA 9/29

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

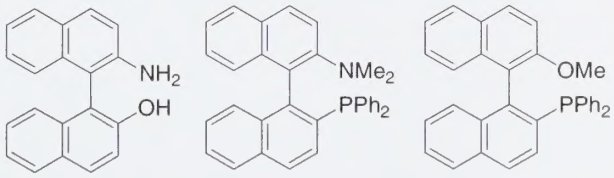
August 19, 2003

Dear Dr. Bader,

I had a recent telephone conversation with an Aldrich salesman and, although he called me in a different matter, I mentioned to him that NOBIN, a chiral ligand which we had introduced some 12 years ago, is now commercially available through a US-based company. Since he was not a chemist, I have serious doubts whether he really got the message and therefore I turn to you.

NOBIN (**1**) was first made by my collaborator Dr. Martin Smrčina (the 2nd recipient of the Bader Award) in Prague in 1990. Over the following years, the support we received for this research was rather sparse owing to one obstacle: the synthesis relied on the coupling of β-naphthol with β-naphthyl amine, the latter compound being a carcinogen. I believed that if we could find good applications for NOBIN, either we or someone else would sooner or later come up with a less risky procedure but British industry remained unimpressed. There were several groups, including Erick Carreira, Steve Buchwald, Amir Hoveyda, Koichi Mikami, and others, who made and used NOBIN and its derivatives in the years following our first papers. Recently, Xumu Zhang showed that NOBIN can be made from BINOL by Bucherer reaction, which I am confident will open the gates for a more common use of NOBIN and ligands that can be prepared from it. One of these is MAP (**2**), first prepared by my student and collaborator Dr. Štěpán Vyskočil (Bader Award in 2002), a line that has then been vigorously pursued by Steve Buchwald. MAP is regarded as a nitrogen analogue of Hayashi's well known MOP (**3**), and has served us and Steve Buchwald great deal in synthetic and mechanistic investigations. An enclosed review, summarizing the details for all of this, has been written upon invitation by the Editors (John Gladysz and Carsten Bolm) for a special issue of *Chem. Rev.* on asymmetric catalysis, published last week (the relevant chapters are: I., II.A., II. E., II.J.1., II.J.3., III.A., III.D., and IV.).

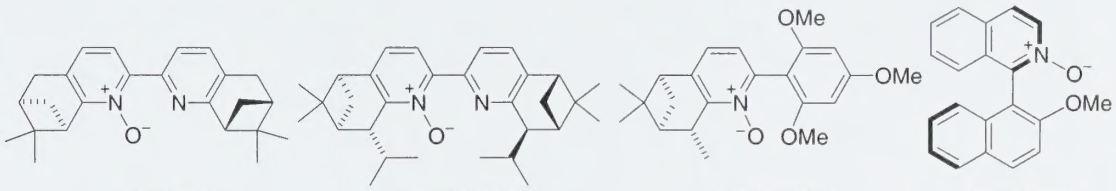
Please note that NOBIN has become commercially available from **Ivy Chiral Chemical Synthesis** (http://www.ivy-chiral.com/index.html), apparently through Zhang's procedure (it's his company!), and this should contribute to its wider use, exactly as I predicted 12 years ago.



1 (NOBIN) **2** (MAP) **3** (MOP)

Another, rather different aspect of our chemistry, revolves around organocatalysis, our currently most vigorously pursued topic. In the last few years, we have developed chiral pyridine *N*-oxides, such as

PINDOX (**4**), *iso*-PINDOX (**5**), METHOX (**6**), and QUINOX (**7**), which are currently the most powerful Lewis basic catalysts for the Sakurai-Hosomi type allylation of aromatic aldehydes (≤98% ee); **4** and **7** have now published (see enclosures). You will be pleased to know that **7** has been made single-handedly by Miss Lenka Dufková, a graduate student at Charles University, whose stay in Glasgow was made possible by your generous support. Filip Teplý and Pavel Herman, who also came to Glasgow as Socrates exchange student, contributed to the development and application of **4** and **5**. Note that this chemistry is being carried out in competition with Scott Denmark, Tamio Hayashi, and several other groups.



4 (PINDOX) **5** (*iso*-PINDOX) **6** (METHOX) **7** (QUINOX)

I wonder whether any of these ligands/catalysts would be of interest to Aldrich. I am confident that if NOBIN (**1**), the first commercially available compound of this series, appeared in the Aldrich catalogue, the chemical community would become more aware of it (especially in conjunction with our recently published review). This may apply to the rest of the set. Therefore, I leave this to your consideration.

I hope we will be able to entertain you here in Glasgow in November.

With warmest regards,

Sincerely yours,

Pavel Kočovský
Sir William Ramsay Professor of Chemistry

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

A Chemist Helping Chemists

August 26, 1996

Professor Dr. Hans Kirchmayr
Technische Universität Wien
Karlsplatz 13
A-1040 Wien
Austria

Dear Professor Kirchmayr:

I still remember with such great pleasure our evening together in Vienna and then particularly your great help about my grandfather's work in your school.

As I wrote to you on August 5th, it would be great if you could send me similar information about Loschmidt's work in your school while he was a student with Meissner and Schrötter in the early 1840s.

Also, I wonder whether I may ask you for help in yet another matter: Dr. Rosner and I are working on an essay about anti-Semitism among chemists in Vienna. We are contrasting the attitudes of Meissner, Schrötter, Loschmidt, and Tschermak.

In this connection, it would be very useful to know when the first Jews were admitted to the Polytechnisches Institut. Of course, it may be that they were admitted right at the beginning, in 1815, but I am not certain. The first Jews were admitted to the University, mainly to study medicine, in 1782.

Now I realize that from the information that you sent me about my grandfather that it doesn't say what his religion was, but clearly a student, Moritz Bader, the son of one Abraham Bader, a merchant in Gaya, was with practical certainty a Jew.

We also know that Loschmidt had a friend, Benedict Margolis, who was a Jew. But this was in the 1840s. Would it be possible for you to check whether among the very first students in 1815 there were Jews, and if not, when the first Jews were admitted, and perhaps even who they were?

Again, with many thanks for your help and best personal regards, I remain,

Yours sincerely,

AB/cw

bc: Professor Robert Rosner

Subject: RE: Your e-mail
Date: Wed, 26 Apr 2000 16:37:03 +0200
From: "akrief" <alain.krief@fundp.ac.be>
To: <baderfa@execpc.com>

Dear Dr Bader
Thank you very much for your answer. Concerning our project of scientific information you mentioned when I phoned you that you knew an organisation which can support this type of education project. If you have such an address, I will be grateful to you if you can let it me know.
Anyhow remember that I will be very happy to have a chance to meet you in Namur once you will be in Europe.
With my best personal regards
Alain K

-----Original Message-----
From: Alfred Bader [mailto:baderfa@execpc.com]
Sent: mercredi 19 avril 2000 7:00
To: Professor Alain Krief
Subject: Your e-mail

Dear Professor Krief,

Thank you for your interesting long e-mail of yesterday.

It seems to me that any effort made by you with other such able chemists like Ian Fleming and J.M. Lehn is likely to be successful.

Also, you are working with some very good industrial chemists such as Dr. Marcel Janssen and so you are likely to be able to raise the necessary funding for this effort.

The manpower you need is not very large and I do not understand why you should need $8 million.

Nor do I understand what kind of help you would like from me. Surely not financial because at my age I do not want to invest in new ventures outside the United States.

I think that the best advice that I can give you is to ask the *Aldrichimica Acta* to publish a really good paper describing Merino. The *Acta*, as you know, is beautifully produced and goes to some 200,000 chemists worldwide.

Thank you for offering to send me your challenging game, MOLEKO, but please don't send it; my grandchildren are not yet big enough to appreciate such games.

With all good wishes I remain

Yours sincerely,
Alfred Bader

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

A Chemist Helping Chemists

May 8, 1996

Mr. Henning Kaaber
Wolff & Kaaber
Rugmarken 24-26
3520 Farum
Danmark

Dear Henning:

Last year, my autobiography, *Adventures of a Chemist Collector*, was published in England, and I would very much like to send copies to you, Niels and Professor Martin Ettlinger. There are good photographs of Niels and Martin in the book, and I think that all of you would be interested in the text.

May I impose on you to send me Niels' and Martin's home addresses. I presume that Martin has retired from his professorship but is still living with his family in Copenhagen.

As I will be leaving for our English home shortly, could you please send your reply to me at White Gables, 2A Holmesdale Road, Bexhill-on-Sea, East Sussex TN39 3QE, England.

Best wishes, as always,

AB/cw

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

Prof. Dr. Hans Kirchmayr
Institüt für Experimentalphysik der
Technischen Universität Wien
A-1090 Wien
Strudlhofgasse 4 / Boltzmanngasse 5
Austria

Dear Professor Dr. Kirchmayr:

I am sorry that a number of trips have delayed my thanking you for your very kind letter of April 2nd.

In the meantime, you will have noted that the title of my talk has been changed somewhat. The reason for that is that I have spoken about Loschmidt in Vienna twice in the last three years - not including the two talks which I gave at the Loschmidt Symposium.

I don't want to bore listeners to death by rehashing old material and hence, hope that you will understand the change suggested on the enclosed.

Isabel and I will be coming to Vienna by car from Pardubice, arriving mid-Tuesday afternoon, June 11th.

Of course, we much look forward to being with you around 4:30 as suggested in your letter.

With all good wishes, I remain,

Yours sincerely,

AB/cw

Enclosure

CHEMISCH-PHYSIKALISCHE GESELLSCHAFT
gegründet 1869 von
H.Hlasiwetz / J.Loschmidt / J.Petzval / J.Stefan

Sekretär: Doz. Dr. Georg REISCHL
A - 1090 Wien, Strudlhofgasse 4 /
Boltzmanngasse 5
Tel: 0222-31367/3053, 3008 - Fax: 3102683

Präsident 1995/96: O.Univ.Prof. Dr. Hans KIRCHMAYR
Institut für Experimentalphysik der Technischen Universität Wien

Herrn
Dr. Alfred BADER
924 East Juneau
Suite 622
M i l w a u k e e
Wisconsin 53202
USA

Wien, 2. April 1996

Sehr geehrter Herr Doktor Bader,

als derzeitiger Vorsitzender der Chemisch-Physikalischen Gesellschaft, der ältesten wissenschaftlichen Gesellschaft Österreichs, danke ich Ihnen sehr herzlich, daß Sie sich bereit erklärt haben, anläßlich Ihres kommenden Wien-Aufenthaltes im Rahmen unserer Gesellschaft über

Loschmidt - der österreichische Faraday
(neueste Ergebnisse der Loschmidt-Forschung)

vorzutragen. Als Termin haben wir **Dienstag, den 11. Juni 1996, um 17.30 Uhr** im Großen Hörsaal des Instituts für Experimentalphysik der Universität Wien, 1090 Wien, Strudlhofgasse 4/Boltzmanngasse 5 vorgemerkt (siehe beiliegenden Entwurf).

Wir freuen uns auf Ihren Vortrag und bitten Sie, wenn es Ihnen möglich ist, uns ein kurzes Abstract Ihres Vortrages zuzusenden, am besten direkt an Christ Langstadlinger, Institut für Experimentalphysik, Universität Wien, A-1090 Wien, Boltzmanngasse 5 oder per Fax: 0043-1-310 26 83.

Wenn es Ihnen möglich ist, am 11. Juni schon ca. eine Stunde vor Beginn Ihres Vortrages bei uns zu sein, dann würden wir uns freuen, mit Kollegen einen Kaffee in unserem Seminarraum (2. Stock, Zimmer 101) zu arrangieren.

Mit nochmals herzlichem Dank und besten Grüßen verbleibe ich

Ihr

Hans Kirchmayr

Hans Kirchmayr

CHEMISCH-PHYSIKALISCHE GESELLSCHAFT

GESELLSCHAFT ÖSTERREICHISCHER CHEMIKER

Ich hoffe Dir gefällt der Text
Grüße
Bobby

EINLADUNG

zum Vortrag von

Dr. Alfred BADER

Gründer und langjähriger Vorsitzender der
Sigma Aldrich Corporation, in Milwaukee, Wisconsin, USA

über

Die Wiederentdeckung der strukturchemischen Arbeiten Coupers und Loschmidts

Richard Anschütz - der Biograph wird zum Detektiv

Der Vortrag findet am

Dienstag, dem 11. Juni 1996, um 17.30 Uhr

im Großen Hörsaal des Instituts für Experimentalphysik der
Universität Wien, 1090 Wien, Strudlhofgasse 4/Boltzmanngasse 5 statt.

Zusammenfassung:
Anschütz, Schüler und Biograph Kekulés, erkannte im Zuge seiner biographischen Arbeiten, daß der schottische Chemiker Archibald Scott Couper wichtige Erkenntnisse der organischen Strukturchemie parallel zu den Arbeiten Kekulés gewonnen hatte. Auch kannte Kekulé Josef Loschmidts **Chemische Studien** von 1861, ohne auf deren große Bedeutung hinzuweisen.

Ohne Anschütz wüßten wir wenig über Coupers und gar nichts über Loschmidts strukturchemische Arbeiten.

Dr. Alfred BADER:

1924	geboren in Wien
	Studium der Chemie an der Queen's University in Kingston, Ontario, Kanada und der Harvard University
1951	Gründer der Aldrich Chemical Company in Milwaukee, Wisconsin
1975	Fusion mit der Sigma Chemical Company, St. Louis, Missouri
1975-1992	President, Chairman and Chairman Emeritus of Sigma-Aldrich

Ehrenbürger der Universität Wien; Honorary Fellow der Royal Society of Chemistry; Stifter des „Josef Loschmidt Preises" für Arbeiten auf dem Gebiet der physikalisch-organischen Chemie der Royal Society of Chemistry

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

A Chemist Helping Chemists

April 29, 1996

Mr. Bruce Kinneberg
President
Ivar Laboratories, Inc.
625 Marina Vista
Martinez, CA 94553

Dear Bruce:

I am sorry that two lecture trips have delayed my responding to your letter of April 12th.

I don't know that I have ever corresponded with a man of your intelligence, and yet had such difficulties in communicating.

After having been thrown out of Sigma-Aldrich in the spring of 1992, I have spent my time three ways: helping a number of small chemical companies, writing and lecturing, and dealing in art.

If you would take the time and get my autobiography from your library, you would see from chapters 12 and 13 that my views of Sigma are not so very different from yours.

Now I just wanted to find out whether you are still in business, as some of the companies I work with might well be able to use your expertise.

With best wishes, I remain,

Yours sincerely,

AB/cw

May 15, 1985

Mr. Bruce Kinneberg
President
Ivar Laboratories Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

In response to your letter of May 1, Dr. Bader is presently in Europe and will return on July 19.

Cordially,

Marilyn Hasmann
Secretary to Dr. Bader

FOODS, DRUGS & FEEDS
PURIFICATION PROCESSES
ENVIRONMENTAL CONTROL
PROCESS DEVELOPMENT

IVAR LABORATORIES, INC.

625 MARINA VISTA
MARTINEZ, CALIFORNIA 94553
PHONE (415) 372-6966

ANALYTICAL METHODS
LABORATORY AUTOMATION
CHEMICAL CONSULTING
SPECIAL PROBLEMS

May 1, 1985

Dr. Alfred Bader
Chairman
Sigma-Aldrich
PO Box 355
Milwaukee, WI 53201

Re: Your letter of 4/17/85 - Sigma's solicitation for patent infringement.

Dear Dr. Bader:

After sending you my letter of 3/28/85 I later reread it and was likewise struck by its uncharacteristically strident tone. I was certain of its appropriateness when written and, if speaking in a seemingly irrational and personally abusive tone is required to get your attention - then so be it. I had previously attempted to calmly and rationally discuss with you what I saw as organizational deterioration at Sigma only to find these mutually serious concerns sidetracked by you into discussions of my most inconsequential products. I note continuing evasiveness as your letter does not directly address the issues of the third page of mine.

I would prefer you not persist in putting your personal reputation on the line with your statement "It is inconceivable to me that anyone at Sigma would engage in fraud..."in the face of my insinuations that both commercial and food and drug fraud may be taking place. As for the latter, our mutual opinions are now somewhat irrelevant as FDA Neuropharmacology in Washington is presently reviewing the situation and will make its own determination. With respect to issues of fraud, add patent fraud. I suggest you read this letter to the very end as I detail the solicitation for patent infringement which took place since my last writing.

As you have returned copies of prior correspondence with the apt and enquiring observation of how kindly I generally express myself, some words of explaination are in order. By analogy, I am responding in the appropriate way for a man reposing on an empty subway car and suddenly finding himself surrounded by a company of fine young bucks with bulgy pockets and who begin a routine of first asking for a cigarette, a dollar, and then a five. If you find being compared with felons unsavory; I respond as a simple and sane man: What looks like a duck, walks like a duck, and quacks like a duck is a duck.

During the four years I have been selling to Sigma I have dealt with a number of purchasing agents including Vern James since 1982. For the size of my

dealings, the number of agents I have dealt with is remarkably large and includes Gelfenbaum, Rossmann, James, Haynes, Steinlage, Baeres, Fagan, Majors, and Gate. Another one named Snow called me in error even as I draft these lines. Your assertion to me a year ago that the department is well managed notwithstanding - it looks to me like a kicked over ant pile where erratic management has resulted in a demoralized and disoriented staff conducting business on short-term considerations in the manner of the lowest human common denominator. Four years ago I felt I was dealing with actual agents who could cooperate, make commitments and special arrangements, and who realized that dealings now have bearing on dealings a year from now. Shaving the size and rate of an immediate transaction was not the be all and end all of their business. Now I see basically order placers who want only to buy small and buy cheap and whose abilities at, say, finding or developing sources of supply look more rediculous by the month. Coherent dealing with price/quality tradeoffs or a patent infringement question seem now beyond their ability. It is not my intent to single out Mr. James for reprisals, but to clarify these generalities with an accurate, detailed account of the latest gaff coming from that office.

Near the end of March I called Vern James to confirm that PO 122926S was out of QC and timely payment expected. During the conversation I enquired if any new or unusual items were on his mind. He indicated he was interested in forskolin and substantial sums of money were involved. As he indicated the material was an isolated natural product from a coleus native to India, and as I have especial interest in natural products as well as a number of good business connections in that part of the world; I was immediately curious. I told him I would look into it and call him back. A literature review was initiated, production possibility looked promising, and I called him back of 4/9/85 to express increasing interest and to confirm his. From my notes, I was told that annual purchase of 5 to 10 grams and possibly more per year was realistic and $5000 per gram was a "good price." My investigations continued.

Into the week of the 19th it had become obvious that, although explosive biochemical interest in this oxygenated diterpene is taking place, Hoechst's extensive initial investigations in the middle and late 1970's had resulted in at least nine foreign patents as well as their current strategic manufacturing and sales position. A serious patent licensing/infringement problem appeared to be involved in entering this market. I again called Mr. James to discuss the situation.

After briefly outlining the problem I attempted to explore options. I asked if relations between Sigma and American Hoechst were amiable and was told yes, but when I suggested I might simply call Hoechst (as for simply negotiating a license) James somberly discouraged any such thing. When I enquired if Sigma would take pains to prevent confrontation with a primary patent holder, such as avoiding strong price competition or avoiding sales relating to pharmaceutical use interests; James was noncommittal. As I pressed for possibilities James' only response was in some detail invoking the memory of Broida and telling me it was and continues to be company policy of longstanding to never disclose purchasing sources!! I was silently but utterly incredulous that your loyal employee, should legal actions be taken, is so confident in your abilities at obstructing justice. Perhaps the man knows more about your upper management than I do? Also perhaps, in fact, Sigma sees small vendors as patsys so hungry for sales that they do your illegal dirty work and then take the consequences (read clause 11 on the back of purchase orders), should such sleazy machinations be legally challenged.

Getting nowhere with patents, I switched the discussion to financial matters. To cover start-up expenses I would need some sort of assurance of seeing around $25,000 in sales (note that prior discussion implied $25,000 to $50,000 a year as realistic). I made clear that this could cover even several years and be contingent on quality approval and absence of unforeseen market changes. Apparently Mr. James has so little authority and such unstable employment prospects that he could not honestly respond that he could "only try" to do this. Wasn't Gelfenbaum fired fired for "trying"? James responded incredulously saying something about such being against policy. I then enquired if anyone else (he had done quite a bit of searching) had expressed any manifest interest like mine. He responded sheepishly in the negative. I next tried to confirm if $4000 to $5000 per gram was a realistic price and James refused to answer. ($2000 to $3000 per gram is current realistic wholesale. I had already seen bait-and-switch with Gate where a $7000 initial tomatidine interest came down to a $3500 purchase order.) James said finally that he would have to call Cal Biochem and I had nothing more to say.

Using my own devices I do now have my own honorable solution to the patent dilemma. My first-line rare plant brokers are researching a source for the coleus. As Sigma acts like a company going out of business, I now must secure other appropriately interested distributors. Thanks for the marketing tip, but Sigma can continue buying retail ($4400 per gram) from Cal Biochem and not waste my time. It is not even convincing that Sigma wants to buy.

Given Sigma's manifest contempt for me and other small suppliers as exhibited in patent strategy; it seems much more likely that they are just looking for a pawn to play off against Cal Biochem to obtain a distributor's discount. I currently have absolutely no reason to now think I would ever offer Sigma forskolin except at arm's length for slightly under the Cal Biochem price - take it or leave it basis.

Dr. Bader, I remain struck by the fact that, while you are a collector of the aesthetic creations of others in art, the business creations over which you preside - your own art - remains so ugly.

Sincerely yours,

Bruce Kinneberg
President
IVAR LABORATORIES INC.

BK/bk.

Dr. Alfred Bader
Chairman

April 17, 1984

Mr. Bruce Kinneberg, President
Ivar Laboratories Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

I have only just returned from a trip abroad and that has delayed my response to your letter of March 28. I have never received a letter quite like it, and I am just relieved that you began by explaining that you had been ill and have only partially recovered. I truly hope that you will get better and will then be able to think in a more balanced manner.

Of course, we accept the cancellation of our orders 516795 and 516796 placed over a year ago.

I do not think that I sanctimoniously or viciously imposed specifications of 97%; rather, I think that it is just a poor business practice to place any orders without specifications. If you had been able to assure us of, say, 95% purity, we would certainly have placed the orders also.

If your anger at Sigma has as much merit as your anger at me, then there is little point arguing: Just get well. It is inconceivable to me that anyone at Sigma would engage in fraud or fraudulent mischief, although all of us will readily admit that we are so busy that we sometimes do not take enough time to explain our requirements.

It has occurred to me that you may not have kept copies of our correspondence, and so I enclose a set, and I would urge you to look at this correspondence when you feel better. Many of us tend to hit irrationally when weak and dejected.

Sincerely,

Alfred Bader
AB:mmh
cc: Mr. L. Curry, Aldrich
Mr. J. Haynes, Sigma
Enclosures

SIGMA-ALDRICH

P.O. Box 355 Milwaukee Wisconsin 53201 USA Telephone (414) 273-3850 Cable Aldrichem TWX 910-262-3052 Telex 26-843

Dr. Alfred Bader
Chairman

April 17, 1984

Mr. Bruce Kinneberg, President
Ivar Laboratories Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

I have only just returned from a trip abroad and that has delayed my response to your letter of March 28. I have never received a letter quite like it, and I am just relieved that you began by explaining that you had been ill and have only partially recovered. I truly hope that you will get better and will then be able to think in a more balanced manner.

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Sincerely,

Alfred Bader

Alfred Bader
AB:mmh
cc: Mr. L. Curry, Aldrich
Mr. J. Haynes, Sigma
Enclosures

SIGMA-ALDRICH

P.O. Box 355 Milwaukee Wisconsin 53201 USA Telephone (414) 273-3850 Cable Aldrichem TWX 910-262-3052 Telex 26-843

FOODS, DRUGS & FEEDS
PURIFICATION PROCESSES
ENVIRONMENTAL CONTROL
PROCESS DEVELOPMENT

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PHONE (415) 372-6966

ANALYTICAL METHODS
LABORATORY AUTOMATION
CHEMICAL CONSULTING
SPECIAL PROBLEMS

March 28, 1985

Dr. Alfred Bader
Chairman
Sigma-Aldrich
PO Box 355
Milwaukee, WI 53201

Re: Your inquiry of 2/14/85 - My refusal of PO's 516795 and 516796.

Dear Dr. Bader:

As I have indicated, I have been primarily occupied for the past year manufacturing various materials for Sigma. The last booked item was shipped 3/6/85. It has been a physically taxing winter and living/working in semi-heated quarters has again taken a toll on my health. I am now but partially recovered from a rather severe respiratory infection of several weeks.

Continuing deteriorating relations with Sigma purchasing - whose behavior you advised me receives your full support - causes me to reevaluate my position with respect to doing business with you and yours. Your above PO 516795 is for material priced below my manufacturing policy cut-off of around $10 per gram. This I explained to you prior to your price dickering and I presently find no good-will basis for making an exception on your behalf. These two orders are for byproducts which have no intrinsic marketing future and, accordingly, must stand on their own financial merits. These are also the smallest orders for custom work I have been tendered from any source in years. As you may see from my letterhead I do not claim to be an organicer. When pressed to define my speciality I say analytical chemist and, in fact, am somewhat bored by manufacturing work. The manufacturing is an intrim preoccupation as is reasonably required to prove that my considerable genius with chemical process development is not the usual vain boasting. When such talents are ignored I do not work as a lab grunt, but make my living as a diesel mechanic, ditch digger, drafting technician, or whatever else is more commonly acknowledged as a useful occupation.

As I recall your purchasing negotiations for the above, after you pressed to reduce my asking prices and we settled on the lower figures you unilaterally and sanctimoneously imposed your 97% purity specifications. When I inquired

if good agreement with literature physical constants such as melting point was what you had in mind, you strenuously objected. Presumably you expected me to develop a suite of purity assays (quantitative primary GC, LC, or TLC assay, solvents assay, and ash and foreign matter assay(s)) to support a piddling sale. The cost of such exteraneous technical services is reasonably $300 to $600 per material. Accepting a $1000 dead-end order with $600 to $1200 overhead is frankly ludicrous. I find it difficult to believe that a man of your experience has no intuitive grasp of what process develop-elements cost and I justifiably perceive a vicious subconscious element in your supposedly "generous offer". These purchase orders are accordingly refused.

I am now advising you and expect my views conveyed to Sigma Purchasing that technical specifications on purchase orders not accompanied by detailed methodology shall be construed as only advisory and not contractual in nature. If Sigma desires arbitrary right of inspection and refusal on custom orders, such may be (obviously dearly) negotiated in the initial price. To reserve the arbitrary right of rejection under the guise of non-conformance with undefined specifications is patently fraud. Although I have hoped that my good faith dealings with Sigma would have prevented confrontation on this issue; one of your bureaucrats saw fit lately to directly threaten me with such fraud (and subsequently had the good sense to back down), so simple good faith is clearly no longer adequate basis for doing business. Sigma's assertions that analytical methods are proprietary is clearly grasping bullshit expedient for fraudulent mischief as detailed above, but most likely is for hiding the obviously patchwork - rediculous to sublime - specification and quality control system from general scrutiny and ridicule.

Specifically, I was threatened with non-payment because some tomatidine by a custom developed process showed - among other things - some 0.3% contaminents by TLC against "zero defect" "specifications". I was the only one in the world who offered to do the job on Sigma's solicitation at a reasonable price - perhaps naively assuming the specifications couldn't really be as stupid as they sounded.

With respect to purity of materials supplied by me to Sigma I characterize their response when materials provided are clearly superior to any previously available (as for hydrastinine chloride and DL-threo-DOPS) as eliciting only greedy silence (and probably covert jeers). The converse (as cited above) in otherwise brilliant execution elicits commercial threats and abuse. The question of product purity is largely hypocritical chatter to hide your own

executive, administrative, and technical shortcomings. I am inclined to think that Sigma must have some quite ignorant quality control executives who are inadequate to the task of slowly rationalizing and tightening all specifications (an unsung and extraordinarily difficult task), who instead create confrontations over nonsense to convince people like you that they are working hard and doing swell.

On the question of who is cheating whom, over the past two years I have been gypped twice in the purchase of grossly defective items from Sigma:

One was a little "tar in a jar" P5755 Protocatechuic Aldehyde, grey xtles which proved to be a useless black tar laced with aluminum chloride/hydroxide and pyridine and methyl pyridin ium chlorides. You should pay whomever makes it double and double the selling price to allow for the expense of the ether extraction (or MEK extraction, if they know what they are doing) needed to correctly purify the reaction product before the mess oxidizes. I ended up making my own from vanillin.

The second was of considerably greater financial impact. It was my purchase of over $900.00 worth of manifestly (yellow and gumy by inspection) low grade DL-threo-DOPS for the purpose of developing my own synthysis. After I became a supplier of this to Sigma I was shocked to learn that what I had purchased was, in fact, human investigational drug material manufactured by Sumitomo and distributed by Sigma for that purpose. From what I also gathered from Sigma, the Sumitomo filth had FDA registration with a suite of identity tests and without any type of chromatographic or microbiological purity characterization. Someone had done a real number on the FDA. My simple offering of superior product at a fair price was hardly sufficient inducement to prompt Sigma to give me the business and not continue to split purchases between me and Sumitomo. More explicitly, your so-called pharmaceutical material was only 87.2% optically pure (LC peak area to total peak areas at 280 nm) and only 93.6±3.0% pure by external standard LC against my product. I really wonder if your swell Drug Division folks conveyed this information after I reported it to the FDA or just buried it so as to not offend the good old boys at Sumitomo. Or perhaps who cares - old farts with Parkinson's disease are as good as dead - aren't they?

"It's a pain doing business with Sigma,"

Bruce Kinneberg

Bruce Kinneberg
President
IVAR LABORATORIES INC.

BK/bk.

Chemists Helping Chemists in Research and Industry

P.O. Box 355 Milwaukee Wisconsin 53201 USA Telephone (414) 273-3850 Cable Aldrichem TWX 910-262-3052 Telex 26-843

THIS NUMBER MUST APPEAR ON ALL INVOICES, SHIPPING PAPERS AND CORRESPONDENCE → 516795 1

TO: IVAR LABORATORIES, INC
625 MARINA VISTA
MARTINEZ, CA 94553

ATTN: MR. BRUCE KINNEBERG
PRESIDENT

415-372-6766

SHIP TO → ALDRICH CHEMICAL COMPANY, INC
230 S. EMMBER LANE
MILWAUKEE, WI 53233

DATE	REQUIRED ON OR BEFORE	SHIP VIA	A/C	FOB
2/84	5/1/84	BEST WAY	131	SHIPPING POINT

QUANTITY	ALDRICH NO.	DESCRIPTION	UNIT COST	TOTAL
50-90GM	27592-1	OPIANIC ACID, 97% MIN PURITY 97%	$5.00 PER GM	

CRT

RECEIVED	AMOUNT RECEIVED	BALANCE DUE	DELIVERY INFORMATION OR FOLLOWUP	INVOICE DATE	DUE DATE	FOLLOWUP DATE
			8-27 will ship 9-20		10-1	
			ask Marilyn Mon 10-15			
			10-24 ltr sent ck again →			11/30
			2-14 ... ck. #9 hold for another week	verbal	A.Bader	3-23

ALFRED BADER

Chemists Helping Chemists in Research and Industry

P.O. Box 355 Milwaukee Wisconsin 53201 USA Telephone (414) 273-3850 Cable Aldrichem TWX 910-262-3052 Telex 26-843

THIS NUMBER MUST APPEAR ON ALL INVOICES, SHIPPING PAPERS AND CORRESPONDENCE → 516796 1

IVAR LABORATORIES, INC
625 MARINA VISTA
MARTINEZ, CA 94553

ATTN: MR. BRUCE KINNEBERG
PRESIDENT

415-372-6966

SHIP TO → ALDRICH CHEMICAL COMPANY, INC
230 S. EMMBER LANE
MILWAUKEE, WI 53233

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2/84	5/1/84	~~BEST WAY~~ UPS	131	SHIPPING POINT	

QUANTITY	ALDRICH NO.	DESCRIPTION	UNIT COST	TOTAL
50GM	27594-8	1-CANADINE, 97%	$15.00 PER GM	$750.00
		MIN PURITY 97%		

CRT

DATE RECEIVED	AMOUNT RECEIVED	BALANCE DUE	DELIVERY INFORMATION OR FOLLOWUP	INVOICE DATE	DUE DATE	FOLLOWUP DATE
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			10-24 ltr sent ck again →			11/30
			[illegible] 4 on hold for another week [illegible]			3-23
			Bader returns			4-26

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ALFRED BADER

Chemists Helping Chemists in Research and Industry

aldrich chemical company, inc.

Dr. Alfred Bader
Chairman

February 14, 1985

Mr. Bruce Kinneberg
President
Ivar Laboratories, Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

You will recall that you wrote to me on November 14, 1984, saying that you hoped to be able to deliver the opianic acid and the l-canadine sometime last November. What happened?

Best regards.

Sincerely,

Alfred Bader

AB:mmh

October 24, 1984

Mr. Bruce Kinneberg
President
Ivar Laboratories Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

I am puzzled by your non-delivery of opianic acid and l-canadine ordered from you in March.

Should we cancel the orders?

Best regards,

Alfred Bader

AB:mmh

FOODS, DRUGS & FEEDS
PURIFICATION PROCESSES
ENVIRONMENTAL CONTROL
PROCESS DEVELOPMENT

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ANALYTICAL METHODS
LABORATORY AUTOMATION
CHEMICAL CONSULTING
SPECIAL PROBLEMS

May 1, 1985

Dr. Alfred Bader
Chairman
Sigma-Aldrich
PO Box 355
Milwaukee, WI 53201

Re: Your letter of 4/17/85 - Sigma's solicitation for patent infringement.

Dear Dr. Bader:

After sending you my letter of 3/28/85 I later reread it and was likewise struck by its uncharacteristically strident tone. I was certain of its appropriateness when written and, if speaking in a seemingly irrational and personally abusive tone is required to get your attention - then so be it. I had previously attempted to calmly and rationally discuss with you what I saw as organizational deterioration at Sigma only to find these mutually serious concerns sidetracked by you into discussions of my most inconsequential products. I note continuing evasiveness as your letter does not directly address the issues of the third page of mine.

I would prefer you not persist in putting your personal reputation on the line with your statement "It is inconceivable to me that anyone at Sigma would engage in fraud..."in the face of my insinuations that both commercial and food and drug fraud may be taking place. As for the latter, our mutual opinions are now somewhat irrelevant as FDA Neuropharmacology in Washington is presently reviewing the situation and will make its own determination. With respect to issues of fraud, add patent fraud. I suggest you read this letter to the very end as I detail the solicitation for patent infringement which took place since my last writing.

As you have returned copies of prior correspondence with the apt and enquiring observation of how kindly I generally express myself, some words of explaination are in order. By analogy, I am responding in the appropriate way for a man reposing on an empty subway car and suddenly finding himself surrounded by a company of fine young bucks with bulgy pockets and who begin a routine of first asking for a cigarette, a dollar, and then a five. If you find being compared with felons unsavory; I respond as a simple and sane man: What looks like a duck, walks like a duck, and quacks like a duck is a duck.

During the four years I have been selling to Sigma I have dealt with a number of purchasing agents including Vern James since 1982. For the size of my

dealings, the number of agents I have dealt with is remarkably large and includes Gelfenbaum, Rossmann, James, Haynes, Steinlage, Baeres, Fagan, Majors, and Gate. Another one named Snow called me in error even as I draft these lines. Your assertion to me a year ago that the department is well managed notwithstanding - it looks to me like a kicked over ant pile where erratic management has resulted in a demoralized and disoriented staff conducting business on short-term considerations in the manner of the lowest human common denominator. Four years ago I felt I was dealing with actual agents who could cooperate, make commitments and special arrangements, and who realized that dealings now have bearing on dealings a year from now. Shaving the size and rate of an immediate transaction was not the be all and end all of their business. Now I see basically order placers who want only to buy small and buy cheap and whose abilities at, say, finding or developing sources of supply look more rediculous by the month. Coherent dealing with price/quality tradeoffs or a patent infringement question seem now beyond their ability. It is not my intent to single out Mr. James for reprisals, but to clarify these generalities with an accurate, detailed account of the latest gaff coming from that office.

Near the end of March I called Vern James to confirm that PO 122926S was out of QC and timely payment expected. During the conversation I enquired if any new or unusual items were on his mind. He indicated he was interested in forskolin and substantial sums of money were involved. As he indicated the material was an isolated natural product from a coleus native to India, and as I have especial interest in natural products as well as a number of good business connections in that part of the world; I was immediately curious. I told him I would look into it and call him back. A literature review was initiated, production possibility looked promising, and I called him back of 4/9/85 to express increasing interest and to confirm his. From my notes, I was told that annual purchase of 5 to 10 grams and possibly more per year was realistic and $5000 per gram was a "good price." My investigations continued.

Into the week of the 19th it had become obvious that, although explosive biochemical interest in this oxygenated diterpene is taking place, Hoechst's extensive initial investigations in the middle and late 1970's had resulted in at least nine foreign patents as well as their current strategic manufacturing and sales position. A serious patent licensing/infringement problem appeared to be involved in entering this market. I again called Mr. James to discuss the situation.

After briefly outlining the problem I attempted to explore options. I asked if relations between Sigma and American Hoechst were amiable and was told yes, but when I suggested I might simply call Hoechst (as for simply negotiating a license) James somberly discouraged any such thing. When I enquired if Sigma would take pains to prevent confrontation with a primary patent holder, such as avoiding strong price competition or avoiding sales relating to pharmaceutical use interests; James was noncommittal. As I pressed for possibilities James' only response was in some detail invoking the memory of Broida and telling me it was and continues to be company policy of longstanding to never disclose purchasing sources!! I was silently but utterly incredulous that your loyal employee, should legal actions be taken, is so confident in your abilities at obstructing justice. Perhaps the man knows more about your upper management than I do? Also perhaps, in fact, Sigma sees small vendors as patsys so hungry for sales that they do your illegal dirty work and then take the consequences (read clause 11 on the back of purchase orders), should such sleazy machinations be legally challenged.

Getting nowhere with patents, I switched the discussion to financial matters. To cover start-up expenses I would need some sort of assurance of seeing around $25,000 in sales (note that prior discussion implied $25,000 to $50,000 a year as realistic). I made clear that this could cover even several years and be contingent on quality approval and absence of unforeseen market changes. Apparently Mr. James has so little authority and such unstable employment prospects that he could not honestly respond that he could "only try" to do this. Wasn't Gelfenbaum fired fired for "trying"? James responded incredulously saying something about such being against policy. I then enquired if anyone else (he had done quite a bit of searching) had expressed any manifest interest like mine. He responded sheepishly in the negative. I next tried to confirm if $4000 to $5000 per gram was a realistic price and James refused to answer. ($2000 to $3000 per gram is current realistic wholesale. I had already seen bait-and-switch with Gate where a $7000 initial tomatidine interest came down to a $3500 purchase order.) James said finally that he would have to call Cal Biochem and I had nothing more to say.

Using my own devices I do now have my own honorable solution to the patent dilemma. My first-line rare plant brokers are researching a source for the coleus. As Sigma acts like a company going out of business, I now must secure other appropriately interested distributors. Thanks for the marketing tip, but Sigma can continue buying retail ($4400 per gram) from Cal Biochem and not waste my time. It is not even convincing that Sigma wants to buy.

Given Sigma's manifest contempt for me and other small suppliers as exhibited in patent strategy; it seems much more likely that they are just looking for a pawn to play off against Cal Biochem to obtain a distributor's discount. I currently have absolutely no reason to now think I would ever offer Sigma forskolin except at arm's length for slightly under the Cal Biochem price - take it or leave it basis.

Dr. Bader, I remain struck by the fact that, while you are a collector of the aesthetic creations of others in art, the business creations over which you preside - your own art - remains so ugly.

Sincerely yours,

Bruce Kinneberg
President
IVAR LABORATORIES INC.

BK/bk.

TELEX / FAX MEMO

FAX SENT MAY 6

TO: John Haynes, Sigma, St. Louis

FROM: Dr. Alfred Bader

~~COUNTRY~~ Re: Ivar Laboratories Inc.

DATE: May 6, 1985

15101

FOODS, DRUGS & FEEDS
PURIFICATION PROCESSES
ENVIRONMENTAL CONTROL
PROCESS DEVELOPMENT

IVAR LABORATORIES, INC.

625 MARINA VISTA
MARTINEZ, CALIFORNIA 94553
PHONE (415) 372-6966

ANALYTICAL METHODS
LABORATORY AUTOMATION
CHEMICAL CONSULTING
SPECIAL PROBLEMS

May 1, 1985

1

Dr. Alfred Bader
Chairman
Sigma-Aldrich
PO Box 355
Milwaukee, WI 53201

Re: Your letter of 4/17/85 - Sigma's solicitation for patent infringement.

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PHONE/FAX (510) 372-6966

ANALYTICAL METHODS
LABORATORY AUTOMATION
CHEMICAL CONSULTING
SPECIAL PROBLEMS

April 12, 1996

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

Subject: Your letter of 4/9/96.

Dear Al,

I have assumed these years my last letter ended our correspondence. From the last lines of your current letter I catch a civil mood I cannot ignore.

The first lines, however, are cause for some renewed irritation on my part. It seems that you just do not get it. My primary line of business is not now nor has it ever been manufacturing chemicals. I am not an academic; I am not a distributor of laboratory supplies; I am not a sales agent for Japanese art; and I am not wealthy in either time or money. I do not even like being called a scientist - but I do like the cultural nature of that enterprise and appreciated my recent nomination to the New York Academy of Sciences. In spite of what the letterhead says, I characterize my line as that of chemist and inventor (who has some characteristics and endurance of a yogi.)

I invent things including chemical manufacturing processes. I have developed and own some processes for high value materials into niche markets. I do at times run these processes to make a little money, but the object always has been to maintain interest in my unique materials assuming that sooner or later market fluctuations would turn about 1 in 10 of them into a significant invention. I have four patents issued since 1990. One of these (5,239,819) is energy-related and considered so significant by the NIST last year they recommended it for DoE grant funding. I may even forego that grant. It may be too distracting from some even more recent inventions of greater value.

As far as Sigma-Aldrich goes. I have little interest in further consideration of that venal operation. In Sigma's favor, a very good current client was referred to me by someone there in Purchasing. But what is interesting about people like Cory, or the financial community's shock to learn Aldrich spent nothing on R&D, or even that Sigma's Drug Division made Jack Anderson's column for pushing a quack psoriasis cure? All this is eminently forgettable.

Yes, I am still here.

Sincerely yours,

Bruce Kinneberg

Bruce Kinneberg

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

A Chemist Helping Chemists

April 9, 1996

Mr. Bruce Kinneberg
President
Ivar Laboratories, Inc.
625 Marina Vista
Martinez, CA 94553

Dear Mr. Kinneberg:

Are you still in business making chemicals? If so, please do let me know and send me a list of your products.

You may recall that some eleven years ago, we had some rather strange correspondence. Now, having been dismissed from Sigma-Aldrich, I could write to you much more frankly.

With all good wishes, I remain,

Yours sincerely,

AB/cw

Chemists Helping Chemists in Research and Industry

aldrich chemical company, inc.

Dr. Alfred Bader
Chairman

March 23, 1984

Mr. Bruce Kinneberg
President
Ivar Laboratories, Inc.
625 Marina Vista
Martinez, California 94553

Dear Mr. Kinneberg:

Please accept our sincere thanks for all the time which you spent with us last week Tuesday and Friday.

Of course, one of the first things I did on my return was to check that Sigma has indeed received the 130 grams of Sigma No. D2384 which you sent on February 22. It is in Sigma's QC and payment will be made promptly after it has been approved.

As promised, please find enclosed our order for 50-90 grams of Opianic acid, 97% and 50 grams of 1-canadine, 97%. We have given the first quantity at anywhere between 50 and 90 grams depending on your stock. Please note that these two chemicals should be sent to Aldrich in Milwaukee.

Best personal regards,

Alfred Bader
AB:mmh
Enclosures
cc: Mr. Lee Curry, Aldrich
Mr. John Haynes, Sigma

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BRUCE KINNEBERG

CHEMICAL CONSULTANT

President
Ivar Laboratories, Inc.

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~~Walnut Creek, CA 94596~~
~~(415) 939-1835~~

~~Laboratory~~
~~(415) 783-5865~~

625 Marina Vista, Martinez 94553

MEMO

To Dr. A. Runquist

From M. Hassmann

Date March 12, 1984

Subject New catalog numbers

To be purchased from: Ivar Laboratories, Inc. (see attached)

27592-1

OPIANIC ACID, 97% - 50--90 grams at $5.00/gram
Cross reference as dimethoxy phthalaldehydic acid

27594-8

l-CANADINE, 97% - 50 grams at $15.00/gram
Cross reference as L-tetrahydroberberine

Please see the attached document, the two starred items of the last page.

Thanks.

IVAR LABORATORIES, INC.

625 MARINA VISTA
MARTINEZ, CALIFORNIA 94553
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LABORATORY AUTOMATION
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SPECIAL PROBLEMS

3/6/84, Bruce Kinneberg

Current Products:

H7751	ℓ-β-Hydrastine (high purity)	12.00/gm
# 7876	ℓ-β-Hydrastine Hydrochloride (macrocrystalline)	12.00/gm
H3174	Hydrastinine Chloride (high purity)	19.00/gm
	other salts of the above by request.	
	secondary products from above manufacturing:	
N2	Opianic Acid (needles)	10.00/gm
N2	ℓ-Canadine (ℓ-Tetrahydroberberine)	25.00/gm
	newest product is synthetic:	
D2384	DL-threo-3-(3,4-Dihydroxyphenyl)-Serine	35.00/gm

130g pent
to ℓ 4/22
& Fe 118736 S
300g $30/g

? → 1kg $18000/kg

Golden Seal Alkaloids

Golden seal (Hydrastis canadensis L.) is a small, two leafed plant native to the open woodlands of eastern North America especially in the Ohio River valley from Illinois to Virginia. The small rhizome was used by the Indians for a number of purposes such as yellow fabric dye, skin stain, and mixed with fat as an insect repellant. Investigations of native medical practice later disclosed that infusions were used topically for treatment of skin and mucosal disorders such as ulcers and sore eyes and that it was also taken orally as a digestive tonic (1). The Cherokees used it for cancer (1,2), but apparently did not hold it in high repute for this purpose.

The early isolation (3) of the novel lactonic phthalidetetrahydroisoquinoline alkaloid (-)-β-hydrastine, I , probably prompted over reliance on this substance for explaining the biological activities of the rhizome and for somewhat neglecting the other principal alkaloids berberine, II , and (-)-canadine, III, (4,5). Further work, for example, has firmly established berberine as strongly stimulating bile secretion (6). About eight additional minor alkaloids have been more recently identified (7,8,9).

The rhizome was quickly adopted into American medicine and by 1916 the USP IX listed eight botanical or alkaloid preparations for medical use. In 1933 about 0.11% of all American prescriptions contained such items (10). By the 1940's the number of extravagant clinical claims supported by a mass of ill conceived, inconclusive, or conflicting pharmaceutical studies became overpowering (4,5,11). With the wholesale abandonment of plant drugs from American medicine following the second World War, hydrastis and its alkaloids were also dismissed. With

better drugs available, the use of hydrastine hydrochloride rather than ergot preparations for uterine postpartum hemorrhage or hydrastinine chloride, IV , (salt of a synthetic carbinolamine pseudobase prepared along with opianic acid V , by oxidative scission of hydrastine) rather than digitalis as a cardiac tonic was unjustified (12). In spite of American abandonment, medical interest in hydrastis preparations remains in Europe and in the Americas (13). It has been introduced into the ancient and extant homeopathic medicine of India (14).

Although hydrastis (family Ranunculaceae with buttercups) is not classified with the poppies; its alkaloids closely resemble those of Papaveraceae. Hydrastis alkaloid biosynthesis has received much study (15,16). The poppy alkaloid (-)-α-narcotine , VI , is methoxyhydrastine. Like narcotine, hydrastine shows some inhibition of the cough reflex (17,18,19,20). Perhaps suggested by indigenous medicine, use of hydrastine and berberine in ophthalmological solutions for topical anesthesia, pupil dilation, and fluorescent staining has been patented (21).

A number of recent studies on the biological activity of tetrahydroberberine ((±)-canadine) and (-)-canadine suggest a number of interesting properties not previously supposed. Tetrahydroberberine was found to increase the antimitotic effect of colchicine on fibroblasts <u>in vitro</u>, exhibit analgesic effect in rabbits, and decrease spontaneous activity and potentiate hexobarbital narcosis in mice (6). It antagonized atropine psychosis in dogs (22) and showed tranquilizer activity in rats (23). CNS depression in various species was also confirmed (24). Inhibition of neural adenyl cyclase (25) and hepatic alcohol dehydrogenase (26) has also been studied. Of a group of berberine homologs, (-)-canadine showed

greatest CNS depressant activity (27). A series of substituted tetrahydroberberines was patented for use as tranquilizers (28,29). Although tetrahydroberberine was previously reported to inhibit oxygen uptake in mouse ascite tumor cells; recent testing for antitumor activity against sarcoma-180 mouse ascite tumors was negative, though activity was found in homologs (31).

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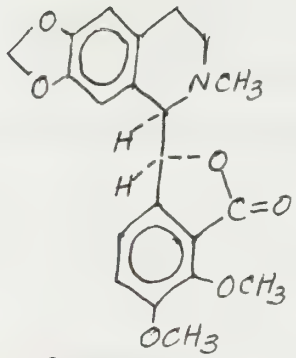
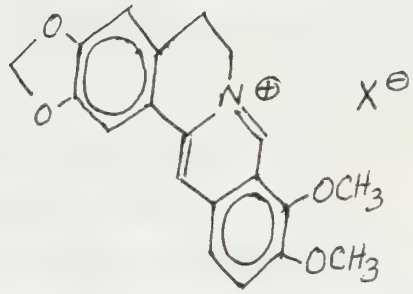
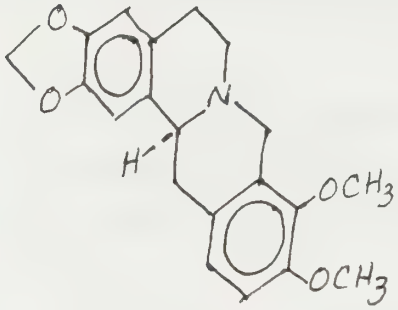
Chemical Structures



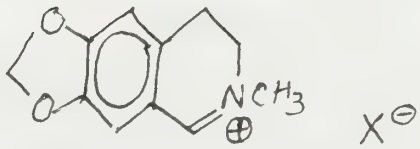
I. (-)-β-Hydrastine
pK_a=6.2, a weak tertiary amine base.



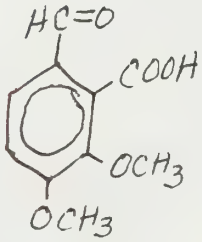
II. (X^-=OH^-) Berberine
pK_a=15.2, an extremely strong hydroxyl base.



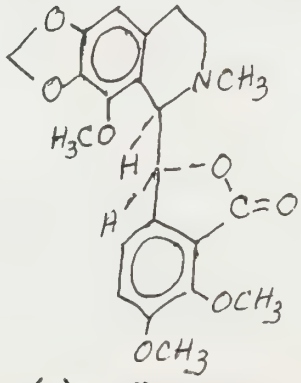
III. (-)-Canadine
((-)-Tetrahydroberberine)
pK_a≈6.8, a weak tertiary amine base.



IV. (X^-=Cl^-) Hydrastinine Chloride
(Hydrastinium Chloride)
(X^-=OH^-) Hydrastinine
pK_a=11.4, a strong hydroxyl base not precipitated from salt solutions by aqueous ammonia.



V. Opianic Acid



VI. (-)-α-Narcotine
(Noscapine)

Copies to:

aldrich chemical company, inc.

CONTACT REPORT

Report No. _____

Date 3/6/84

By GB

Department _____

Other Persons Accompanying:

Persons Interviewed and Position

1. Bruce Kruncberg
2. _____
3. _____
4. _____
5. _____

Company Name and Address:

Wm Laboratories

Send catalog to: 1 2 3 4 5

Add to mailing list: 1 2 3 4 5

REPORT:

ordy opianic acid 97% 50-90g. $5/g

" l-Canadine, 97% 50g. $15/g

Approved by: _____ Date: _____

Form A40 Rev. 12/80

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

January 5, 1995

Via Fax 201 535 8844

Mr. Ira Kukin
Suite 526
5 S Regent Street
Livingston, New Jersey 07039

Dear Ira,

A long trip to England has delayed my thanking you for your most interesting letter of November 22nd.

Isabel and I would love to get together with you. We plan to arrive in New York on Tuesday at noon, January 10th, and then spend Tuesday looking at paintings at Christie's and Sotheby's and attending the sales on Wednesday, Thursday and Friday mornings. We will stay at the St. Moritz Hotel. Early on Friday afternoon we plan to go to spend the Sabbath with an old friend whom I met in Vienna when I was four years of age, and who now lives in Brooklyn. We leave Newark for Milwaukee at 12:15 p.m. on Sunday the 15th.

As yet, we have no plans for Tuesday evening the 10th, although if you were free on Sunday, we could also get together that morning as long as we can be at Newark by 11:00 a.m. or so.

As you perhaps know, we have established four fellowships for students to come to get their PhD in organic chemistry, one at Imperial College in London, and the others at Columbia, Harvard and the University of Pennsylvania. If you son would like some advice about the quality of Czech students, I would suggest that he contact a good friend, a very able working medicinal organic chemist, Dr. Stanislav Radl, whose home address in Prague is Pertoldova 3380, telephone/fax 422 401 0676.

How to find just the right person to work with Apollo in the Czech Republic is quite a challenge. May I suggest that when next you visit there, you meet with an other old friend, Ing. Vladimir Matous, who is one of the leading chemical engineers at a very good synthetic company, VUOS in Pardubice. VUOS does a great deal of contract analytical work and also a great deal of synthetic work, particularly with dyes.

Vladimir is 60 and I sense that he is not totally happy with what is happening at VUOS. Whether he would be interested in working with you, I just don't know. But he is such a competent and giving human being that I am sure that you would enjoy meeting him, and on chatting him I think he will suggest a number of possibilities, competent and good people who might work with you. Matous's home telephone number is 42 40 25493. His English, although not perfect, is really quite good.

We very much look forward to seeing you soon.

Best wishes.

Sincerely,

Olga

P.S. I hope you'll like your photo in my autobiography!

ALFRED BADER FINE ARTS

DR. ALFRED BADER

January 19, 1995

ESTABLISHED 1961

Dr. Ira Kukin
Suite 526
5 S Regent Street
Livingston, New Jersey 07039

Dear Ira,

You must have realized how very much Isabel and I enjoyed our dinner with you and your partner last week.

We very much hope that we will be able to reciprocate your hospitality in Milwaukee before long.

Enclosed, as promised, please find the last annual report of the Helen Bader Foundation, of which my son Daniel is president.

All good wishes.

Sincerely,

Enclosure

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

Apollo

Ira Kukin
Chairman of the Board

Apollo Technologies
International Corp
Suite 526
5 S Regent Street
Livingston, NJ 07039
(201) 535-1515
Fax: (201) 535-8844
Telex: 466351

November 22, 1994

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

Dear Al:

Your letter of October 26, 1994 arrived while I was on an extended trip in the Far East; I am trying to catch up now.

I am very hopeful that we can get together when you come into New York in January for the old master's sales. As of now, I plan to be here through the 17 then I will be away from the 18 to the 25th. I hope this doesn't present a conflict.

With regard to a Czech chemist, I really had in mind, when I wrote to you, a PhD chemist, or equivalent, in medicinal pharmaceutical chemistry as a possible one or two year exchange student or post Doc type to work with my son, Dr. Marrick Kukin, who is head of the Cardiology Heart Failure Institute at Mount Sinai Hospital and Medical School in New York. The idea was nurtured by what our mutual friend Professor Jerrold Meinwald, advised me - i.e., that he brings in one such student from the Czech Republic to work with him at Cornell.

But now something more important has come up, Al. We are expecting a commitment from ČEZ, the large electricity generating company, in the Czech Republic. This would be for Apollo to install its flue gas conditioning system for their boilers, the objective of which is to increase precipitator efficiency. The net result is a reduction of particulate emissions and a reduced stack opacity. The latter are recent requirements of the Ministry of the Environmental Affairs in Czech Republic.

This will means that we have to set up a complete sales, marketing, engineering and technical services company in the Czech Republic and our most urgent need will be for a "hands-on" manager. He should speak English, preferably have a background in engineering, chemistry, technology or industrial management with an excellent prior track record. An "MBA" type of degree might also be suitable.

It is difficult to put out a job description for such a person. As you know from your experiences an intelligent man with a good background, honest, reliable with an excellent work ethic would be preferable to any specific past history. His job will be to maintain liaison for Apollo in the Czech Republic, build an organization and service the accounts. Whereas we ultimately supply the proper chemical antidote to increase the boiler power performance, and we have more than one specific product, it is necessary to package what we are selling. In effect, we become the chemical counterpart to the electrical and mechanical engineering departments at the utility. We custom design the feed equipment, we build, install and maintain it, we carry out the efficiency emissions tests to determine the most cost-effective rates; then we supply a chemical antidote from a wide array of formulations that we have developed over the past 25 years.

The above also explains, Al, what our company does, which is the installation of chemical systems to improve power production and to minimize the most serious air pollutants from the electric utilities, oil-fired and coal-fired boilers.

The company which was founded 35 years ago was sold in 1980 to a Fortune 500 company, formerly Economics Laboratory now Ecolab in Minneapolis. They were unable to carry out the required operations and I eventually ended up buying it back, albeit, I'm operating it on a reduced scale. Most of our sales now are overseas where the environmental condition approximate those that were so prevalent in the U.S. 25 years ago. The most expanding areas are Eastern Europe and the Pacific Far East. Unfortunately, it means too much travel for me.

I selected some brochures that give a background to the company.

I am looking forward to meeting Isabel and you in New York. It would simply be great fun to get together after these many years going back to our student days at Harvard.

Best personal wishes.

Sincerely,

Ira Kukin

IK/mw

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

April 4, 1994

Mr. Ira Kukin
Suite 526
5 S Regent Street
Livingston, New Jersey 07039

Dear Ira,

In going through some old photographs, I came across one taken in front of Converse in 1949--from left to right, Bernie Landau, Leon Mandell, me, and a nice looking fellow on the right who I suspect is you. Is that correct?

Many thanks for your help.

Sincerely,

Enclosure

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

March 21, 1994

Mr. Ira Kukin
Suite 526
5 S Regent Street
Livingston, New Jersey 07039

Dear Ira,

Thank you so much for your letter of March 10th, delayed because you mailed it to Aldrich, from which I was dismissed two years ago. The enclosed explains what happened.

As you will have gathered, my family and I very much like the Czech Republic and the fellowship at Harvard is the fourth we have set up. The others are at Columbia University, University of Pennsylvania and Imperial College in London. We have also set up some fellowships for Czech art historians, and award within the Czech Chemical Society for the best young organic chemist.

We plan to be in Prague in the middle of June. I would love to meet you somewhere, but I don't get to New York very often, just for the big sales of old master paintings in January.

If you every come this way, do visit us.

Best wishes,

Enclosure

Ira Kukin

Suite 526
5 S Regent Street
Livingston, NJ 07039
(201) 535-1515

March 10, 1994
IK: 314297/0394M

Mr. Alfred Bader
1001 West St. Paul Avenue
Milwaukee, WI 52333

Dear Al:

I was delighted to be advised and to read in the Harvard Gazette of your most recent gift to the Chemistry Department.

It was interesting to learn that you are of Czech origin. For the past two years, we tried to establish a business base in the Czech Republic, where we actually opened an office. Unfortunately, the business was too slow in getting off the ground. We temporarily suspended our operations there but with the thought of returning some day. The Czech Republic is on a par with West Germany, perhaps even surpassing West Germany in bringing in the latest advanced technologies in the operation of its electric generating utilities which is the field with which I am primarily concerned.

Within the past year, I also read that you had presented extraordinarily exciting lectures related to your interest in art. I almost attended one of them but unfortunately was out of the country at the time.

I have been hopeful, Al, that our paths would have crossed this past year or so and I am still optimistic that they will. Should you be in the greater New York area, why don't you give me a call. It would be a real pleasure to get together.

Best personal wishes.

Sincerely,

Ira Kukin

Ira Kukin

IK:mh

Dr. Irwin L. Klundt
250 Skyline Drive
Bayfield, CO 81122-9231
Phone (303) 884-9273

February 10, 1993

Editor
Milwaukee Journal
333 W State Street
Milwaukee, WI 53202

Dear Sir:

I have read with interest the article on Alfred Bader that appeared on Sunday November 1, 1992.

As a former vice president of Aldrich and Sigma Aldrich, I agree with Bader's statement any "substantial Wisconsin-friendly influence has disappeared from the company with his departure - warning that the out-of-state management will tilt resources in favor of Missouri plants."

Even when Bader was active with the company, the board of directors had failed to meet in Milwaukee during the past several years. It has been custom to alternate the annual meeting between St. Louis and Milwaukee but I doubt that the meeting to be held in May of 1993 will be in Milwaukee, considering the potential for pointed questions to be asked of management regarding the Bader situation.

All decisions are being make in St. Louis. In my opinion in the last several years no decisions have been made in Milwaukee. The Wisconsin employees of Sigma Aldrich work very hard but are inclined not to offer opinions contrary to St. Louis. Wisconsin employees and stockholders will miss the benefits of interaction with top management.

Bader was a superb ambassador for the corporation, not a threat to anyone in the management chain and has a unique ability to pick new products, which are the mainstay of the growth of the company.

Sincerely,

Irwin L. Klundt, Ph.D.

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

February 17, 1993

Dr. Irwin L. Klundt
250 Skyline Drive
Bayfield
Colorado 91122 9231

Dear Ike,

I appreciate your letter of February 10th to the Milwaukee Journal more than I can tell you.

Let's hope they print it.

Best wishes.

Sincerely,

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

September 30, 1993

KTAV Publishing
900 Jefferson Street
Box 6249
Hoboken, New Jersey 07030 7205

Gentlemen:

I understand that some years ago you re-printed Benno Jacob's commentary on Genesis which was originally published in Germany around 1934.

Could you please tell me the price of your re-print.

Many thanks.

Sincerely,

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

August 9, 1993

Prof. Joseph A. Heppert
Department of Chemistry
University of Kansas
Malott Hall
Lawrence, Kansas 66045 0046

Dear Prof. Heppert:

Your very kind letter of August 2nd arrived at home only on August 7th, and so I will reply to your Chemistry Department in Lawrence.

My calendar tends to fill up very quickly, and as I had not heard from you during the last few months, I thought that you might have lost interest, and my calendar has filled up rather frighteningly. Also, with the great problems of flooding that the midwest has had to contend with, now would not be a good time to travel from here to Lawrence.

I have already agreed to go on an A.C.S. tour through Tennessee in April, but perhaps a visit to several sections in Iowa and Kansas might be feasible for September of next year.

All good wishes.

Sincerely,

August 2, 1993

Dr. Alfred Bader
2961 Shepard Ave.
Milwaukee, WI 53211
FAX 414-962-3822

Dear Dr. Bader:

Since our correspondence last spring, I have been trying to enlist the interest of one of the local sections in Iowa or Missouri to co-sponsor your visit. During late spring and early summer, these sections appeared to be leaderless, perhaps as a result of vacation schedules. I can understand that, more recently, these areas have had the terrible weather in the midwest to contend with. As a result, I have not yet been able to enlist another section to co-sponsor your trip.

My colleague, Linda Stone-Ferrier (Art History) and I are certainly looking forward to sponsoring your visit, although it seems likely that we will have to modify our original plan. We could keep the date for the visit as scheduled, and the K. U. local section will cover travel expenses for yourself and your wife. This would be fine, although I suspect that it may not be as interesting for you as if two local sections were involved. I also worry that travel by road may still be some what disrupted during this period. Otherwise, we could attempt to reschedule your visit during Spring 1994 when travel will be presumably back to normal. This will also give us more time to enlist the involvement of a co-sponsoring section.

Please consider these and any other options that you care to suggest. I will be happy to be of any assistance that I can in making your arrangements. Until August 6, I can be reached by phone at 216-447-5322 or fax at 216-447-5249. After that date, I will be returning to K. U. for the Fall semester. My apologies for these difficulties. I hope that we can still successfully arrange a visit.

Sincerely,

Joseph A. Heppert
Associate Professor
of Chemistry

cc: Dr. Kenneth Ratzloff
Chair, K. U. Local ACS Section

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

April 19, 1993

Professor Joseph A. Heppert
Associate Professor of Chemistry
Chair, K. U. Local Section, A.C.S.
University of Kansas
Malott Hall
Lawrence, Kansas 66045 0046

Dear Professor Heppert:

Thank you so much for your thoughtful letter of April 8th.

Of course I would very much like to be with you and would be happy to give three talks in one day, and I attach a list of talks from which you can pick. Just last week I gave three such talks at Central Michigan University and enclose their schedule.

The problem with visiting Lawrence is twofold. First, while I do not accept honoraria from universities, I do want my travel expenses to be covered, and that is best done with my wife Isabel and I sharing the driving. However, Lawrence is quite a long way, and I am wondering whether you could interest two other A.C.S. sections to invite me also, to share the expenses. The possible sections are Davenport, Kansas City and Des Moines.

Of course, a further difficulty is that driving in the winter would be very chancy, and my calendar is filling up rapidly. However, I have pencilled in three days in September, from the 20th through the 22nd in the hope that you might be able to arrange for talks at three sections during those days.

I look forward to hearing from you.

Sincerely,

Enclosures

The University of Kansas

Chemistry April 8, 1993

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

Dear Dr. Bader:

As chairman of the Kansas University Local Section of the American Chemical Society, I am pleased to invite you to present a seminar at a Section meeting on the K.U. campus during the coming academic year. Our section will sponsor five seminars between September 1993 and April 1994, with the earliest possible openings being in the months of September or November. At present, our seminar schedule during these months is quite open, so we would have a reasonable chance of accommodating any openings in your schedule.

Our interest in having you visit K.U. stems, at least in part, from what we perceive is your ability to speak to subjects that will generate interest in both the scientific and fine arts community. It has been difficult in many ways to bridge the scholarly gap between these disciplines, and we thought that your interests and expertise in both of these areas could create a forum for common interests and discussion. Our plans, if you are interested in accepting our invitation, are to contact the staff at our Helen Spencer Museum about the possibility of jointly sponsoring some events surrounding your visit. This could logically include both a reception/dinner and your talk, although we would really need to contact the Museum in order to gauge their level of commitment to the visit. We have not really tried as a Section to reach out to the arts/humanities in this fashion before, and so are quite excited by this opportunity.

I do hope that you will accept our invitation to visit our institution, and participate in our Local Section activities. Please feel free to contact me by post or phone (913-864-4150) so that we can establish a tentative schedule and initiate contact with our colleagues in the arts. Many thanks for your consideration.

Sincerely,

Joseph A. Heppert
Associate Professor of Chemistry
Chair, K.U. Local Section, A.C.S.

JAH:js

Department of Chemistry • Malott Hall • Lawrence, Kansas 66045-0046 • (913) 864-4670
BITNET: CHEMISTS @ UKANVM; FAX 913-864-5396

Dr. Alfred Bader, Director Emeritus of Sigma Aldrich Chemical Company, Milwaukee, WI

Lecture Series: Central Michigan University Monday, April 12th,1993

Lectures co-sponsored by the Central Michigan University Chemistry Department, the Central Michigan University Art Department and the Midland Chemical Society

Monday, April 12 : 10 - 11 AM / Wightman 142

"The Bible through Dutch Eyes" - an examination of the iconography behind the biblical sources in 17th century Dutch paintings, drawings and prints. Seventeenth century Dutch people were extremely well versed in the Old and New Testaments and drew on these sources in their personal aspirations, and in their delineation of good versus evil in their battles with tyrannical Spain.

Monday, April 12, 3PM / Dow 175

"Josef Loschmidt - The Father of Molecular Modelling" - a discussion of purported discoveries of molecular structure in chemistry by the famous German chemist August Kekule actually being the work of the obscure chemist Joseph Loschmidt, whose manuscript Kekule had read four years before.

Monday evening, April 12, 7:30 PM / Dow 175

"The Adventures of a Chemist Art Collector" - Dr. Bader is a well known collector of 17th century Dutch art. In this address, he will describe how his background in chemistry has assisted him in uncovering rare and original works of art.

An exhibition of the magazine covers of the Sigma Aldrich Chemical Company, featuring reproductions of works of art in Dr. Bader's collections, will accompany his talks in Dow 175.

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

March 15, 1993

Professor Richard S. Givens
Chairman, Department of Chemistry
University of Kansas
Malott Hall
Lawrence, Kansas 66045 0046

Dear Professor Givens:

Thank you for your letter and telephone call.

However, my schedule between now and September is completely filled.

Thank you for your understanding.

Best wishes.

Sincerely,

The University of Kansas

Chemistry

March 8, 1993

Dr. Alfred Bader
940 W. St. Paul Avenue
Milwaukee, WI 53233

Dear Dr. Bader:

I would like to enlist your help with our annual recognition luncheon for outstanding undergraduate and graduate students in chemistry. Would you be willing to speak to a group of 50-60 students and their parents on a topic of you choosing related to chemistry. Your presentation on master painting of alchemists ("The Adventures of a Chemist Collector") would exemplify just the right kind of topic for this diverse audience.

The banquet is held at noon on Saturday, May 1st, in the University of Kansas Memorial Union. The talk is about 30-45 minutes Awards are presented to the students after the luncheon and just prior to the talk.

The Department of Chemistry would pay your expenses plus a small honorarium. We would be pleased to make all local arrangements for you and would enjoy having your spend the Friday prior to the banquet to visit with our faculty and students.

We have had an interesting and diverse group of speakers over the past three decades including Paul Gassman when he was president of ACS, a science editor of the Kansas City Star, several successful graduates of our Department and some of our most distinguished faculty. We would appreciate the opportunity to have your deliver our 1993 Honor's Banquet address.

I will phone you later this week to see if your schedule would permit you to join us May 1st.

With best regards,

Sincerely yours,

Richard S. Givens

Richard S. Givens
Professor and Chairman

RSG/sp

Department of Chemistry • Malott Hall • Lawrence, Kansas 66045-0046 • (913) 864-4670
BITNET: CHEMISTS @ UKANVM; FAX 913-864-5396

37th ANNUAL

AWARD PRESENTATION

HONORS BANQUET

May 2, 1992
12 Noon

Big 8 Room
Kansas Union

The University of Kansas
Department of Chemistry

OUR SPEAKER

Professor Clark Bricker, former faculty member (1963-1983) in the Department of Chemistry, is a native of Shrewsburgy, Pennsylvania. After being graduated from Gettysburg College in 1939, receiving his Master of Science degree from Haverford College and his Ph.D. degree from Princeton in 1944 where he worked for two years on the Manhattan Project, he was employed by the Heyden Chemical Corporation in Garfield, New Jersey. In 1946 he was appointed assistant professor of chemistry at the Johns Hopkins University. Two years later he returned to Princeton where he rose through the academic ranks to full professor in 1960. In 1961, he was appointed Dean of the College and Professor of Chemistry at The College of Wooster in Wooster, Ohio, where he served two years before accepting the invitation to become Director and Coordinator of Freshman Chemistry at the University of Kansas.

During his years at K.U., Professor Bricker developed and coordinated a highly successful program for the introductory chemistry course. It is estimated that over 10,000 students matriculated through the two-semester general chemistry sequence during his tenure as director. Professor Bricker is a popular teacher receiving an impressive number of awards including the national College Teaching Award of the Manufacturing Chemists Association (1968), a Fulbright-Hays Award (1964), the Standard Oil (Indiana) Foundation Award (K.U 1967) and was appointed the first K.U. Endowment Distinguished Teaching Professor (1982). He received an unprecedented four HOPE awards from the senior classes of 1966, 1970, 1979 and 1983.

Professor Bricker has served as a consultant to the NSF-AID program in India, and as the Science Demonstrator at the Research and Development Exhibitions for the USIA in the Soviet Union, Hungary, Yugoslavia and five African countries. He has published over sixty papers on electrochemistry, photochemistry and solar energy, written two laboratory manuals and co-authored a textbook on "Elements of Quantitative Analysis." Ten students completed their Ph.D. degrees under his supervision

Upon retiring from the University of Kansas, Professor Bricker became the Riegel Visiting Professor at Davidson (1983-84), served the College of Arts and Sciences at K.U. in High School–University Relations (1984-85) and was Mitchell Visiting Professor at Trinity University in San Antonio, TX (1985). He continues to participate in teaching and outreach activities for K.U. and the Chemistry Department.

Professor Bricker is currently a Professor Emeritus of Chemistry at K.U

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

October 19, 1992

Professor Wilfred N. Arnold
Department of Biochemistry
School of Medicine
University of Kansas Medical Center
39th and Rainbow Blvd.
Kansas City, Kansas 66160-7421

Dear Professor Arnold:

Please accept my apology that I am responding so late to your interesting letter of last December regarding your book on Vincent van Gogh.

As you will see from the enclosed, I was dismissed from Aldrich, and with that dismissal I doubt that Aldrich will continue to be interested in adding books on paintings to its catalog.

Many thanks for your thoughtfulness.

Sincerely,

Enclosure

The University of Kansas Medical Center

School of Medicine
Department of Biochemistry

Wilfred Niels Arnold Ph.D.
Professor of Biochemistry

December 2, 1991

Dr. Alfred Bader
Chairman
Aldrich Chemical Company
P.O. Box 355
Milwaukee
Wisconsin 53201

Dear Dr. Bader:

I have written a book:

Vincent van Gogh: Chemicals, Crises, and Creativity

which is the culmination of our studies on the artist's medical problems and his interactions with chemicals. I enclose a table of contents which will give you some idea of the coverage. The chemical and medical themes that run through the book are vital to the thesis, but I have attempted to write with graded and progressive development of each topic for the broad audience interested in Vincent van Gogh. You will recall my sending you reprints of papers such as *Vincent van Gogh and the Thujone Connection,* from the primary literature. Back in 1988, we also had a conversation on Karl Spitzweg's *The Alchemist* and stills, in connection with my Scientific American article on *Absinthe*.

We note that your company lists several reference manuals and general interest books in catalog supplements. For example, in *Aldrich 1991, New Products*, two items:

Fluorine: The First Hundred Years [1886-1986] by R.E. Banks et al. and
From Dürer to Rembrandt: Studies in the History of Art by R. Wischnitzer.

caught my attention, and prompted me to ask whether you would be interested in listing my book in 1992. I have mentioning this to the publisher, Birkhäuser Boston Inc. [Springer Verlag]. The Editorial Director, Dr. George Adelman, may communicate with you along these lines in the near future, and hopefully something of mutual interest might be worked out.

Your encouragement in the past has been appreciated and I would welcome your advice on this project.

All the best for the festive season.

Yours sincerely,

Wilf. Arnold

Wilfred N. Arnold Ph.D.
Professor of Biochemistry

WNA:gb
Enclosure

39th and Rainbow Blvd., Kansas City, Kansas 66160-7421
(913) 588-7056

Vincent van Gogh: Chemicals, Crises, and Creativity

Wilfred Niels Arnold, Ph.D.

Professor of Biochemistry

Birkhäuser Verlag
Basel • Boston • Berlin

Vincent van Gogh: Chemicals, Crises and Creativity

Wilfred Niels Arnold

Birkhäuser

Table of Contents (November 29,1991)

Title page		1 page
Dedication		1 page
The Author		1 page
Acknowledgements		1 page
	Preface	2 pages
Chapter 1.	**Introduction**	7 pages
Chapter 2.	**Vita**	52 pages
Chapter 3.	**Illnesses**	28 pages
Chapter 4.	**Absinthe**	35 pages
Chapter 5.	**The Diagnosis**	27 pages
Chapter 6.	**Other Hypotheses**	36 pages
Chapter 7.	**Vincent's Doctors**	22 pages
Chapter 8.	**The Yellow Palette**	25 pages
Chapter 9.	**The Ear-Cutting Affair**	14 pages
Chapter 10.	**Suicide**	22 pages
Chapter 11.	**Creativity and the van Gogh Legacy**	26 pages
	Summary	4 pages
	Burial, Reburial and the Thuja Tree	1 page
	Index	?

Plus reproductions of paintings and drawings, tables, graphs.

EYE volume 5, number 5, 1991

EDITORIAL

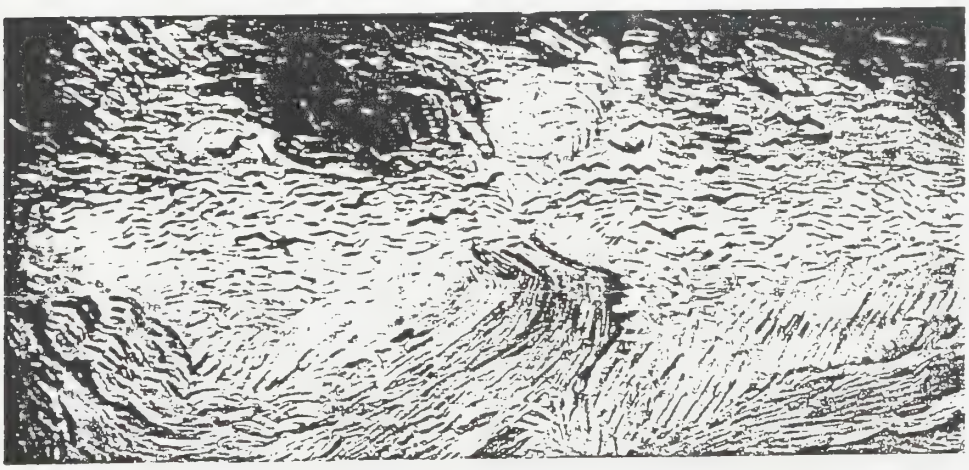
Van Gogh

It is always fun to speculate on the psychopathology of Prime People, the prurience of the private life behind the public face: how much did it contribute to their success or failure. Their morbid pathology can be just as intriguing, and then at least we have firmer grounds on which to base our surmise: especially is this true if we are dealing with underlying factors which can be exactly calibrated, like optics and colour where changes in the artist's rendering can be directly related to a particular experience or pathological state.

Of all the artists whose work has been subjected to such crude analyses, Van Gogh leads the field, both in terms of the number of articles published and the diversity of the attributions. He is also close enough to our time to allow fairly accurate accounts of his maladies and therapies; and, as the most expensive artist in the bona-fide art market and the hottest property in the under-world, he has become a particularly appealing target for a host of enthusiasts with their wayward theories, and to whom the pathos and drama of his last years lend an added fascination.

In the following account, Arnold and Loftus restrict their survey to the various organic explanations of Van Gogh's preference for yellow, and with their magisterial conclusions few will disagree. For it is only when our conjecture abandon the solid shores of science, and float blithely through increasingly speculative waters that the real trouble begins: as Sir Thomas Browne nicely had it, "What song the sirens sang and such-like questions . . . . admit a wide solution".

PATRICK TREVOR-ROPER



Eye (1991) **5**, 503–510

Xanthopsia and van Gogh's Yellow Palette

WILFRED NIELS ARNOLD[1] and LORETTA S. LOFTUS[2]
Kansas City, USA

Summary
A survey of van Gogh's work from 1886 to 1890 indicated that paintings with a yellow dominance were numerous, episodic, and multi-regional. His underlying illness, by his own admission, affected his life and work; furthermore, episodes of malnutrition, substance abuse, environmental exposure, and drug experimentation (all evident from correspondence) exacerbated his condition. Accordingly, we reviewed plausible agents that might have modified the artist's colour perception. Xanthopsia due to overdosage of digitalis or santonin is well documented elsewhere, but evidence of useage of either drug by van Gogh cannot be substantiated. It is unlikely that ageing of the human lens was an influence because of the artist's youth. Sunstroke is too restrictive to fit the multiplicity of regions and motifs. Hallucinations induced by absinthe, the popular liqueur of the period, may explain particular canvases but not the majority of 'high yellow' paintings. Van Gogh's proclivity for exaggerated colours and his embrace of yellow in particular are clear from his letters and, in contradistinction to chemical or physical insults modifying perception, artistic preference is the best working hypothesis to explain the yellow dominance in his palette.

Vincent van Gogh died by suicide in 1890 at the age of 37 years. He was a full-time artist during the last ten years of his life but received little recognition during his lifetime. A wider appreciation of his work was slowly realised during the early years of this century, and has now grown into enormous popularity. There have been numerous attempts to define van Gogh's innovations, to suggest the mechanisms of their genesis, and to identify influences upon his creativity.

It is generally thought that Vincent* had a mental illness, although consensus has not been reached on a specific diagnosis. Most commentators suggest that additional factors exacerbated his condition, even if it were hereditary; for example, Hemphill[1] reached the conclusion that van Gogh was a manic-depressive who developed confusional episodes and fits which were due to excessive consumption of a popular liqueur called absinthe. Several other working hypotheses posit medical problems having some influence on his art; analyses of neurological, nutritional, environmental, and chemical factors have all received some discussion. These elements influenced his life, and some of them probably shortened it, but did they affect his vision, his style, his palette?

Chromatopsias are usually reversible but

*van Gogh preferred the use of the first name professionally; those paintings which he signed (a fraction of the total) were simply inscribed Vincent.

From: [1]Department of Biochemistry and Molecular Biology, University of Kansas Medical Center, Kansas City, Kansas, USA 66103. [2]Department of Medicine, University of Missouri-Kansas City School of Medicine, Kansas City, Missouri, USA 64108.

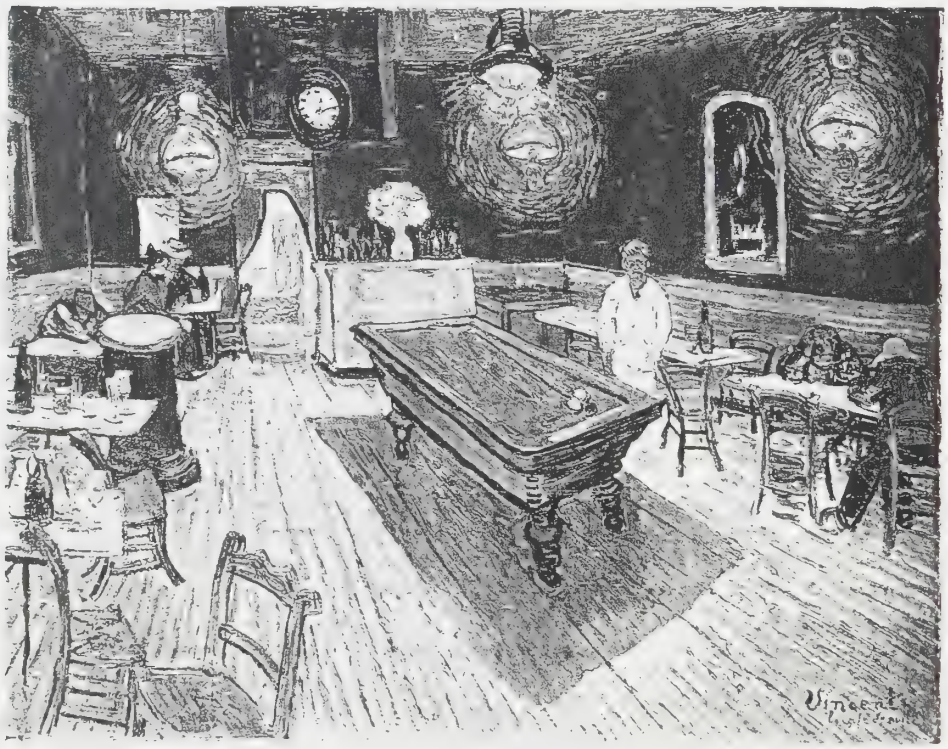
Correspondence to: Wilfred N. Arnold, PhD, Department of Biochemistry and Molecular Biology, University of Kansas Medical Center, Kansas City, USA 66103.

would change one's view of the world and might impress an artist for later rendition on canvas. Lee[2] was the first to propose that van Gogh may have suffered from digitalis intoxication, noticed the golden and coronal symptoms and, when these side effects disappeared, purposefully continued to paint with a yellow dominance. There is no record of Vincent taking the drug. Nevertheless we thought it worthwhile to explore other chemicals which can induce xanthopsia.

The dominance of vibrant yellows and the red end of the spectrum in several paintings after 1886 was declared by the artist himself as a 'high yellow note' (letter 581).[3] Was it a reflection of Vincent's new perception of his subject matter or was it artistic preference? The thrust of this review is to examine existing claims, to explore new possibilities, and to resolve the issue between perception and preference.

Van Gogh's 'high yellow' paintings

From 1886 to 1890 van Gogh produced at least 638 paintings.[4] We have examined 276 paintings from this period.[4–10] These works were produced in Paris (20%), Arles (39%), St. Rémy (24%) and Auvers-sur-Oise (17%). In order to distinguish between realistic representation of a particular scene compared with the same under perception modification it is essential to define '*high yellow*' pictures as not only being rich in yellows, but virtually lacking blues, violets and white. We emphasise that the exclusion of the blues is just as important as the dominance of the yellows in arguments invoking xanthopsia. We found 10% in this category. There are other canvases within this time frame which are rich in yellows, greens, and reds but have a *blue or violet swath;* in this category we found 30%. The warmer hues pervade many of the remainder, but they have a more general, *full-*



Color Plate. *'The Night Café' by Vincent van Gogh, Arles, 1888. Oil on canvas, 72.4 × 92.1 cm. Yale University Art Gallery, New Haven. Bequest of Stephen Carlton Clark.*

spectrum palette that includes the blues. Here we found 161 (60%) of which only 13 were judged to be sombre. [Van Gogh 'touched up' many of his canvases, sometimes months later, and perhaps on occasion towards a more balanced palette, but we have not attempted to evaluate this aspect]. The Table provides examples of each of the three, operationally-defined categories.

In addition, a group of self-portraits was analysed as a separate class based upon Bonafoux's book[11] of 37 canvases by van Gogh. Twenty-eight of them were created in Paris. We deem eight of these portraits to be high yellow paintings and only one, 'Self-portrait dedicated to friend Laval', 1888, was painted in the South. Thus 'high yellow' paintings were not only significant in *number,* but their execution was *episodic,* and they were produced at *multiple locations.* Was this a reflection of an occasional, reversible change in the artist's perception of his subject matter?

Colour perception and ageing

The perception of the full range of colours is subject to a multitude of influences. One natural example concerns ageing. The fraction of incident light energy which reaches the retina decreases with age; this is primarily due to a gradual loss in transmittance by the lens.[12] The attenuation is more pronounced for the shorter wave lengths and is maximal between 450 nm and 470 nm, which are centered about the colour responses of violet and blue respectively, and may be the explanation for less blue and more grey and green in the skies of older landscape painters because of yellowing of the lens.[13] Pokorny *et al.*[14] summarised the available data on healthy eyes of different ages, and with their equations we calculated that the loss in lens transmission between ages 27 and 67 is a significant 28% at 460 nm. But for Vincent, over his artistic career (ca 27–37), the predicted change for healthy eyes is less than 7% , and this degree of 'yellowing' of the lens is insignificant compared with the impact that attends the unnatural condition of xanthopsia, a pathology of the colour sense which elicits yellow vision. Walsh and Hoyt[15] mention at least thirteen chemicals that can cause xanthopsia; some are 20th century drugs, others include digitalis and santonin which were used in France during the period of interest.

Digitalis intoxication

Withering's book (1785) on the medical uses of foxglove (*Digitalis purpurea*) reported yellow vision in patients who received large and sustained doses of foxglove decoctions.[16] Purkinje (1823) described his own symptoms after self-administration of digitalis.[17] Lely and van Enter[18] reported a large-scale digitoxin intoxication in 1969 due to an error in tablet manufacture, and they observed serious eye conditions in 170 (95%) of the patients. As well as episodes of amblyopia and diplopia, and experiences of light flashes and scintillating scotomata, patients and volunteers complained of aberrancies in

Examples of the Three Palettes Each painting is identified by origin, year, title, de la Faille (F) number,[4] and present location. See text for operational definitions of the three categories.

I.	'High Yellow'	
	Paris (1887)	'Sunflowers' F376, Kunstmuseum Bern
	Arles (1888)	'The Night Café' F463, Yale University Art Gallery
	St Rémy (1890)	'Roses' F681, W. Averell & Pamela C. Harriman Collection
	Auvers (1890)	'Trees' F817, Joseph H. Hazen Collection
II.	'Blue Swath'	
	Paris (1887)	'The Bathing Boat' F311, Mellon Collection, Virginia Museum
	Arles (1888)	'Wheat Field' F411, Rijksmuseum Vincent van Gogh, Amsterdam
	St Rémy (1889)	'The Reaper' F618, Rijksmuseum Vincent van Gogh, Amsterdam
	Auvers (1890)	'Sheaves of Wheat' F771, Dallas Museum of Art
III.	'Full Spectrum'	
	Paris (1887)	'Restaurant Interior' F342, Rijksmuseum Kröller-Müller, Otterlo
	Arles (1888)	'Café Terrace by Night' F467, Rijksmuseum Kröller Müller, Otterlo
	St Rémy (1889)	'Starry Night' F612, Museum of Modern Art, New York
	Auvers (1890)	'Crows over the Wheat Field' F779, Rijksmuseum Vincent van Gogh, Amsterdam

colour vision wherein all objects appeared primarily green, yellow, or white; the latter symptoms disappeared in a few days after withdrawal of the drug.[19] There is no unanimity on mechanism, but Gibson *et al.*[20] proposed that the yellow vision of digitoxin toxicity is related to a selective, reversible effect on receptors rather than the optic nerve.[21]

Robertson *et al.*[22] emphasised that patients are generally reluctant to admit to distorted colour vision, thus the statistics are possibly underestimated. Nonetheless a large body of literature covering 200 years supports the fact that digitalis intoxication can induce xanthopsia. Lee[2] put forward the case for Vincent van Gogh. Rentchnick[23] and Lanthony[24] found some support for this working hypothesis and joined Lee[2] in reminding us that digitalis was given for epilepsy, mental illness, and other medical problems in the nineteenth-century (albeit, inappropriately by modern standards). These authors[2,23,24] were intrigued by the sprigs of foxglove that Vincent incorporated into the two 'Portraits of Dr. Gachet', Auvers-sur-Oise, 1890. We are more inclined to interpret the plant as a symbol for the physician rather than as a drug for the artist. Ravin[25] noted that because Gachet was a homeopathic physician he was less likely to have administered a toxic dose of digitalis. In any event van Gogh's last attending physician, Dr. Paul Gachet, was in contact for only two months just before the artist's death. If one of his earlier doctors, e.g. Drs. Peyron (St. Rémy), Rey (Arles), or Gruby (Paris), had been similarly depicted with digitalis, then a more meaningful (but still circumstantial) case could have been made to infer prescription of the drug. There is also the possibility of self-administration by van Gogh although we find nothing about digitalis in his correspondence; he did mention taking potassium bromide at Arles (letter 574) so that he was not averse to talking about medications. Perhaps an indirect case can be made for santonin because it was recommended by Raspail, whose system of medicine was known to the artist [(letter 576), and ref. 26].

Raspail

Francois-Vincent Raspail (1794–1878), a unique, popular, and influential figure in 19th century France, had already made appreciable contributions to chemistry, biology, and particularly histochemistry when he turned his attention to the health and welfare of the general population. His system stressed hygiene, focused on common medical problems, and depended on a few chemicals such as camphor (unfortunately touted as a panacea) and a handful of crude plant extracts. He published 'Manuel Annuaire de la Santé' in pocket-book format starting in 1845, and sales averaged over a hundred copies a day in the first five years.[27] It was repeated every year but two until his death, and then it was continued by his descendants till 1935. 'How can I consider myself a doctor when all the world is going to become, without much pain, as learned as I', wrote Raspail on the title page of the Manuel,[28] to the chagrin of most other physicians of the day. Raspail's contentiousness extended into politics, which landed him in jail for more than two years and exile for nine, but his publications, speeches and ministrations elevated his recognition to a household name.

Vincent was preoccupied with his own health and even wrote to his brother, as early as July 1880, about the wisdom of self-study in medicine (letter 133), and expressed some organised scepticism about prevailing norms of health care (letter 148). Attraction to the low-cost, home-remedies of Raspail (letter 576) by someone of Vincent's demeanour is easily understood. Indeed, he lauded Raspail and incorporated an image of the Manuel into 'Still Life: Drawing Board with Onions', which was painted between January 8 and 17, 1889. During the same period Vincent wrote about his enthusiasm for camphor in overcoming insomnia (letter 570); this was surely inspired by the book, although Vincent's reckless 'very, very strong dose . . . in my pillow and mattress' can be compared with Raspail's directive of chewing about 50 mg at night.

Van Gogh had a copy of the Manuel, considered it worthy of a legible title in his painting, and joined thousands of compatriots in consulting this book of home remedies. We do not know which edition Vincent had, but we consulted the 1863, '72 and '86 versions

wherein we found the same entry (pp 133–4 of the 1886 edition[28]) on semen-contra (called santonica in the 1886 U.S. Dispensatory[29]), which contained santonin as the active principle.

Santonin

This sesquiterpene lactone is found in several *Artemisia* species although the commercial source is primarily *A. maritima*.[30] Santonin, as a crude decoction or powder and later as pure crystals, was used effectively as an anthelmintic for several centuries until displaced by drugs with less side effects. Xanthopsia due to santonin overdosage was probably encountered almost as soon as its efficacy against ascariasis was established and was documented[31] as early as 1806. It may also be important for the present discussion to note that while bright objects are yellow, dark surfaces sometimes have a violet appearance under santonin xanthopsia.[32,33] The visual symptoms may occur in the absence of all other toxic manifestations, which in extreme cases include convulsions.[33]

A single dose of 200 mg santonin (about the upper limit of the therapeutic range) is sufficient to cause yellow vision in some individuals after two hours, and with 500 mg or more the effect is noted in a half hour.[32] Duke-Elder[19] reported that doses as small as 100 mg santonin commonly caused yellow vision. Raspail suggested up to 2 g daily of *semen-contra*, the unexpanded flora heads of *A. maritima*, as often as required; this would have been equivalent to about 80 mg santonin according to our calculation, although the concentration would have varied with the dried herb. More importantly, Raspail's direction, 'a pinch with three fingers equals one gram,' is a bit imprecise. The large variations in actual amount taken, the small difference between therapeutic and toxic doses, and individual differences in sensitivity to santonin are sufficient to account for the incidence of intoxication.[32]

The drug was also taken in the 19th century as a preventative medicine or in response to vague and indefinite diagnoses. More recently, documented examples involved individuals suffering dyspepsia and other gastrointestinal complaints who self-administered,[34] or were given by relatives,[35] santonin-containing preparations. Van Gogh frequently complained of gastrointestinal problems and may have suspected worms; his penchant for excess, as exemplified with camphor, may have led to overdosage on santonin. Arnold has suggested that van Gogh's affinity for absinthe developed into a pica for terpenes, the documented examples being thujone, camphor, and pinene.[36]

Absinthe

Indulgence in alcohol adding to van Gogh's illness and the injurious effects of absinthe in particular have been emphasised.[1,36,37] This liqueur was very popular in 19th century France; the per capita consumption was particularly high in Paris and Arles in van Gogh's time,[38] and there are several indications that he developed an affinity for it. He also painted 'Absinthe Glass and Carafe', Paris, 1887, and included a green absinthe bottle into 'Still Life: Drawing Board with Onions', Arles, 1889. Vincent said that he painted 'The Night Café' on the spot, staying up three nights in a row and sleeping during the day (letter 533). It is tempting to speculate that he consumed absinthe during the execution of this painting; he certainly had access, and the landlord was apparently pleased with the whole event. Vincent surmised that Tersteeg (an Art Gallery manager in Holland) would surely judge Vincent as having 'delirium tremens' while painting this picture (letter 534). Apart from the possibility of special cases such as this we do not imply that van Gogh painted while intoxicated. We feel that his creations occurred while lucid, but that novel experiences of relative sizes, shapes, and colours perceived under the influence of absinthe may have been recalled later and incorporated into new and daring compositions, perspectives, and palettes.

Absinthe contained high concentrations of both alcohol and essential oils from herbs; the latter contributed to flavour, fragrance, and toxicity.[39] The most deleterious constituent was thujone, which can cause auditory and visual hallucinations (described as vivid and terrifying), convulsions, and eventually irreversible neurological damage.[40,41] In the 1920s and 30s, thujone- and camphor-induced con-

vulsions were studied as models for epilepsy.[39] Subsequently camphor, pentylenetetrazole (Metrazole), and hexafluorodiethyl ether (Indoklon) were used successfully as pharmacologic agents to induce convulsive seizures in certain cases of schizophrenia, i.e. chemical forerunners of electroconvulsive therapy.[39,42] Therapeutic doses of Metrazole commonly evoked xanthopsia[43] or varicoloured visual hallucinations[44] in the preconvulsant period.

Notwithstanding chemical differences between Metrazole, camphor, thujone, and santonin, all can act as convulsants; that Metrazole and santonin cause xanthopsia is also intriguing. Amblyopia and optic neuritis were reported under absinthism,[33] but chromatopsia was not encountered in our literature search. Hilbert wrote about a young, neurasthenic, male patient who suffered a three hour bout of xanthopsia after an alcoholic excess, but the liquor was not specified.[45]

The herbs incorporated into absinthe varied with the manufacturer but always included wormwood (*Artemisia absinthium*) and Roman wormwood (*Artemisia pontica*),[39] and we wondered whether they contained santonin (cf. *Artemisia maritima*). The procedure used, as well as the low concentration of santonin reported for *A. absinthium*, excused that species; our own analyses of an alcoholic extract of *A. pontica* (i.e. simulation of the secondary process in absinthe manufacture) indicated that the amount of santonin was insignificant.[46] Nonetheless, van Gogh's over indulgence in absinthe,[36] and an increased susceptibility to its toxic effects due to inadequate diet,[39] may have influenced his perception. According to Signac, 'Though he [Vincent] ate hardly anything, what he drank was always too much . . . after spending the whole day in the blazing sun [painting] . . . the absinthes and brandies would follow each other in quick succession.'[47] We are also reminded of Vincent telling his brother, about his early months in Arles, 'Dr. Rey says that . . . I kept myself going on coffee and alcohol . . . it is true that to attain the *high yellow note* that I attained last summer, I really had to be pretty well keyed up' (letter 581). 'Now if I recover, I must begin again, and I shall not again reach the heights to which sickness partially led me' (letter 570).

'Too much sun for a nordic head'

We quote from the mid-thirties, doctoral dissertation of Beer[48] who attributed the diagnosis of sunstroke to Dr. Paul Gachet, without documentation. Our survey indicated that 'high yellow' paintings were not restricted to the south and included some indoor scenes, still lifes, and self-portraits so that sunstroke or heat exhaustion could not be the sole contributing factor.

Nevertheless Vincent remarked upon being 'dazed with the sun,' (letter 512) that 'beats down on one's head . . . [and] makes one crazy' (letter B 15). He noted that sunburn was common around Arles (letter 498a), and commented upon his own sometimes reckless exposure (letter B 7). The syndrome has been recognised since biblical times. According to an 1886 Handbook, vision is sometimes affected by heatstroke; sixty cases of chromatopsia were observed in New York City about this time.[49] It is curious that more recent reviews[50] of heatstroke do not mention chromatopsia; perhaps the syndrome is included under delirium or goes unreported because of the stigma attached to admitting to hallucinations.[22]

Duke-Elder[19] and Carroll[51] were of the opinion that the visual disturbances due to digitalis intoxication, and some other chemically invoked chromatopsias, are actually hallucinations, i.e. due to central function impairment caused by the drug. This working hypothesis has the charm of being able to embrace such disparate causes of xanthopsia as digitalis, santonin, and sunstroke but otherwise remains in conflict with hypotheses that depend more on specific chemical or physical effects on photoreceptors.[20]

We encountered other chemical and environmental causes of xanthopsia,[15,33] but they were dismissed because they were either isolated and exotic (e.g. picric acid, chromic acid, carbon disulfide, oil of wintergreen) or seemed to be irrelevant to Vincent's lifetime (e.g. amyl nitrite, streptomycin, sulfonamides, DDT, quinacrine, thiazide diuretics). Those chemical and physical factors which we have discussed are at least reasonable can-

didates for causing confusion or distortion in colour perception.

Vincent's preference

We turn now to 'artistic preference' and the key distinction in our operational definition is that the artist views the motif accurately but elects to depart from realistic depiction of colours on the canvas. That the artist's choice of pigments be conscious, accurate and uninfluenced by external factors is central to this argument. In 1886 van Gogh declared that, 'true drawing is modelling with colour' (letter 459a). Later , in 'La Berceuse' ('Woman Rocking the Cradle'), Arles, 1889, he strove for facial modelling by 'naturally broken tones' upon a 'complexion [of] chrome yellow' and hoped, above all, to paint a 'lullaby in colours' (letter 571a). Here, and elsewhere, the artist invokes a deeper emotional language as well as a new visual technique; the preoccupation is with colour. Furthermore, Vincent had earlier embraced 'the artist's liberty to exaggerate, to create . . . a world more beautiful, more simple, more consoling than ours'; in this artistic licence he claimed no originality but embellished a prefatory statement from Maupassant's 'Pierre et Jean' (letter 470). And elsewhere there is ample evidence from van Gogh himself to indicate that he was prepared occasionally to depart from 'exact' colour (letter 533). His colour selections for 'The Night Café' were subsequently described by Vincent as achieving an '[expression of] the terrible passions of humanity by means of red and green' (letter 533). The disparate reds, greens, orange, and yellows meant, 'an atmosphere like a devil's furnace' (letter 534). It is interesting to compare Gauguin's rendition of the same room, 'In an Arles Café', 1888, for the application of a cooler palette.

Van Gogh apparently found the Midi a perfect environment for his study of colour. Although the subjects of people and landscapes were similar in character to Holland, 'the difference [was] in the colour' (letter 488). 'How lovely yellow is! And how much better I shall [later] see the North!' (letter 522). His preference can be gauged from letters (1887–1890) wherein he mentions the yellow of his surroundings more than any other colour. Yellow also had a special symbolism for Vincent: 'The Wheatfield behind Saint Paul's Hospital with a Reaper,' 1889, a work predominantly in yellow, was described as, 'a vague figure fighting like a devil . . . the image of death . . . [and] humanity might be the wheat he is reaping' (letter 604). But, most important for Vincent, 'there is nothing sad in [this] death, [because] it goes its way in broad daylight . . . with a light of pure gold' (letter 604). This is reminiscent of Goethe's conviction that the colours of yellow, orange, and cinnabar (vermillion) evoke quick, lively, aspiring feelings.[52] Later van Gogh hesitated over the colour rendition of this picture, but 'preferred the canvas done from nature [over a later copy] . . . [because it] makes me recall the furnace of summer . . . it is not so exaggerated after all' (letter 608).

Conclusion

An extensive search of the literature revealed several chemical and physical precipitants of xanthopsia. Some of these are reasonable candidates for Vincent van Gogh, but none can be supported in depth. We obviated natural ageing of the lens because of Vincent's youth and short career span. Digitalis intoxication has sufficient medical underpinning but lacks usage documentation. They physiological case for overdosage on santonin is as good or better than that for digitalis, but the evidence is still circumstantial (Raspail's book and the terpene connection). Other chemical insults are known to induce xanthopsia, but no relationship could be established with van Gogh. Sunstroke does not fit the multiplicity of motifs or locations for the yellow paintings. Only a minority of the paintings may have been directly influenced by absinthe-induced hallucinations. Irreversible brain damage from absinthism is not supported by the episodic nature of the yellow paintings. Artistic preference remains the best working hypothesis to explain the yellow dominance in his palette, a position which is upheld by his stated philosophy.

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

March 1, 1993

Dr. Raymond D. Kimbrough, Jr.
165 Avery Drive
Atlanta, Georgia 30309

Dear Kip:

It is so nice to hear from you, and I want to thank you most sincerely for your kind invitation to stay with you in Atlanta.

Isabel and I would love to stay with you, but we are wondering whether that might be possible on the night of Thursday, March 25, rather than Saturday the 20th. We are arriving in Atlanta on the 20th but then plan to drive straight from Atlanta to Columbia, South Carolina because I will be giving several talks there. On Tuesday the 23rd, I am to speak in Charlotte, on the 24th in Athens, and on the 25th to the A.C.S. in Atlanta. We do not plan to fly from Atlanta to Milwaukee until 3:21 p.m. on Friday the 26th.

I think you might enjoy coming to my talk to the A.C.S. on Thursday evening; it will be on the work of Josef Loschmidt who is truly the father of molecular modelling.

Of course if our staying with you that Thursday night is not convenient, we still hope to have a chance to spend a little time with you and Helen.

Best wishes.

Sincerely,

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

February 11, 1993

Dr. Raymond D. Kimbrough, Jr.
165 Avery Drive
Atlanta, Georgia 30309

Dear Kip:

Isabel and I look forward to going on an A.C.S. tour from Atlanta to Columbia, South Carolina and then back to Atlanta. We plan to arrive in Atlanta on Saturday, March 20th, and then drive from Atlanta to Columbia and from there to the other sections ending up back in Atlanta, speaking to the A.C.S. there on Thursday, March 25th.

It would give us great pleasure if we could get together either on the 20th or the 25th.

Best personal regards.

Sincerely,

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

March 30, 1993

Professor R. D. Kimbrough, Jr.
165 Avery Drive
Atlanta, Georgia 30309

Dear Kip:

Please accept Isabel's and my thanks for your wonderful hospitality last week. We so enjoyed being with you and Helen, and we particularly appreciated your taking so much of a rainy day to look for paintings with us.

All good wishes.

Sincerely,

165 Amry Dr.
Atlanta, GA 30309

9 March 1993

Dear Alfred,

The night of Thursday, March 25th will be fine. I had planned to go to the ACS meeting.

The drive from Athens to Atlanta is just under 2 hours. Let me know when on Thursday you will be in Atlanta. I'm in the phone book, (404) 875-1707. I have an answering machine, so you could leave a message.

We are looking forward to seeing you and Isabel on the 25th of March.

Sincerely,

Kip

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

March 15, 1993

Dr. Raymond D. Kimbrough, Jr.
165 Avery Drive
Atlanta, Georgia 30309

Dear Kip:

I am so happy that we can stay with you the night of Thursday, March 25th, provided that the weather will have softened sufficiently so that we can visit Georgia in the first place.

I plan to spend quite a bit of time with Professor Pelletier on Wednesday morning, and as I understand that the drive from Athens to Atlanta is less than two hours, we hope to be with you early that Thursday afternoon. Perhaps you might have time to show us some antique stores, either on Thursday afternoon or on Friday morning.

The curator of old master paintings at the High Museum in Atlanta, Ronnie Baer, was at one time a Bader Fellow in Holland, studying Dutch art history. She is considering inviting me to speak at the High Museum, but I do not yet know when. In any case, I do hope we will have enough time for that and finding some good paintings.

Best wishes.

As always,

165 Avery Dr.
Atlanta, GA 30309

Feb. 23, 1993

Dear Alfred,

It is great that you and Isabel will be in Atlanta next month. I hope you will be able to spend the night of March 20th with me here as you did before.

Since you were last here, I have gotten divorced and I am engaged to Helen, whom you will meet when you are here.

I assume that you will get a car at the airport so I am enclosing directions from the

airport to my house.

Let me know when on the 20th you will arrive. If you get here early perhaps we can visit an antique warehouse. I remember your surprise in an Atlanta warehouse when you found that a painting you thought worth buying was only $18. That was in 1972, I think. That warehouse is gone but I know of some others.

Helen and I look forward to seeing you and Isabel on March 20th.

Sincerely,

Kip

From the Atlanta Airport to Kip's house:

Take I-85 north from the airport
(to Atlanta)
go past downtown (on your left)
exit 10th St (from the right lane
of I-85)
turn ~~left~~ right on 10th St.
turn left at the 5th Traffic light
(Piedmont, 0.6 miles)
go 1.0 miles on Piedmont
Turn left on Avery
go 0.4 miles to 165 Avery
(on the right)

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

March 1, 1993

Dr. Raymond D. Kimbrough, Jr.
165 Avery Drive
Atlanta, Georgia 30309

Dear Kip:

It is so nice to hear from you, and I want to thank you most sincerely for your kind invitation to stay with you in Atlanta.

Isabel and I would love to stay with you, but we are wondering whether that might be possible on the night of Thursday, March 25, rather than Saturday the 20th. We are arriving in Atlanta on the 20th but then plan to drive straight from Atlanta to Columbia, South Carolina because I will be giving several talks there. On Tuesday the 23rd, I am to speak in Charlotte, on the 24th in Athens, and on the 25th to the A.C.S. in Atlanta. We do not plan to fly from Atlanta to Milwaukee until 3:21 p.m. on Friday the 26th.

I think you might enjoy coming to my talk to the A.C.S. on Thursday evening; it will be on the work of Josef Loschmidt who is truly the father of molecular modelling.

Of course if our staying with you that Thursday night is not convenient, we still hope to have a chance to spend a little time with you and Helen.

Best wishes.

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

Date: September 16, 1992

To: Dr. Mayer Goren - 303 398 1806
Dr. Irwin Klundt - 303 247 7310

The best travel schedule I can see avoids Chicago and gives me some extra time with you, Ike:

October 31 Sat	Milwaukee-Denver		CO	4:12 - 5:43 p.m.	
November 3 Tues	Denver-Durango	2163	CO	7:25 - 9:00 p.m.	$390.00/ea
November 5 Thurs	Durango-Denver	2150	CO	6:45 - 8:06 a.m.	
	Denver-Milwaukee	562	CO	10:36 - 1:58 p.m.	

The total package is $390/each. The cost of Milwaukee-Denver-Milwaukee is $193/each.

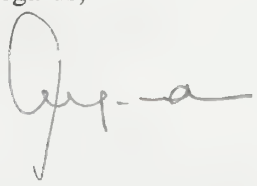
IKE: Please let me know whether you will provide me with travel vouchers for Denver-Durango-Denver, or whether your school will reimburse me for the difference between the round trip fare of $390/each and the Milwaukee fare of $193/each. If you send me travel vouchers, will we be certain to get on the two flights to and from Durango, above.

MAYER: The talk on Loschmidt will really be of great interest to all chemists, and I would much prefer to give that talk in the Chemistry Department.

In the announcements, please refer to me as the retired former CEO and Chairman of Sigma-Aldrich.

Please let me know whether these flight arrangements are satisfactory, and I will book them shortly.

Best regards,



Irwin L. Klundt
250 Skyline Drive
Bayfield, CO 81122

(303)884-9273

January 9, 1992

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

Dear Alfred:

It was great to talk to you last night. I am disturbed by Tom asking you not to maintain your office at Aldrich. There are a number of people at Aldrich and at Sigma that are upset with the way management is treating people. Both companies used to be a place that people were proud to represent and felt that they had found a life long place of employment. This is not so today. There is a lot of unrest among the staff regarding the atmosphere that has been created by "upper management". No one seems to take the time to develop and train the staff below them. The ability to develop oneself professionally does not exist at Sigma Aldrich any longer. You indicated that there were now three of us that were no longer appreciated, but the list goes beyond that. I don't think Tom appreciates anyone. He is only interested in having his way and in the bottom line. I realize that he is running a business, but there is no interest in science and the fact that the customers are scientists.

I miss working with the customers, training the young staff to do more than I was able to do and finding ways to grow the business. However, as you indicated, one has to feel appreciated, and it had become obvious to me during the last years I was active at Aldrich that there was very little appreciation of my efforts.

Aldrich is thought of very highly in the field. I am enjoying teaching and continue to talk about Aldrich where ever I go. I am looking for ways to become active in chemistry again.

I will see if there is a way to get you invited to speak at Fort Lewis College. It is a small liberal arts school, enrollment of 4,000, that does not have a lot of money to pay for seminar speakers. The chemistry department is very strong and has won numerous awards and grants for its teaching ability. Possibly if you are going to be in Denver or on an ACS speaking tour, we could arrange for you to come to Durango for a day. It would be a great honor to have someone of your stature speak to the students and faculty.

Trudy is interested in learning more about the Dutch painting that you varnished for us. Could you let us know how we might go about finding out more information about the painting? We still owe you a dinner for your efforts. I hope you can find the time to come to the southwest for a visit.

Good luck with your discussions at the Sigma Aldrich board meeting. I would appreciate it if you would let me know the outcome of your talk.

Thanks again for all of your help throughout my career. It is my pleasure to know you and to have worked for and with you.

Sincerely,

Ike

Ike
8/19 OK

Wed Nov 4

Dr. Irwin L. Klundt
250 Skyline Drive
Bayfield, CO 81122-9231
Phone (303) 884-9273

December 23, 1992

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

Dear Alfred:

It is hard for me to believe that it has been over a month since you were here. I would like to thank you again for the visit. The students and faculty are still talking about your lectures.

We received the lovely note from Isabell and were glad to hear that you two are both feeling better.

I think this is a great chemistry department and I have enclosed some information about it, the letter from the Research Corporation and a copy of the Alumni publication (see page 20). I also have copies of the correspondence regarding Max Gergel and an article that Dr. Ron Estler of our department found on NIR (near-infrared reflectography used to look at paintings).

I did not get a chance to write down Ralph's address or to ask you if it were true that George Garian was no longer with Makor. If you should have George's home address I would like to write to him.

I talked to Harvey Hopps last week and he asked that I pass on his regards to you and ask if you would consider running for ACS President again.

I have drafted a letter to the editor of the Milwaukee Journal and will try to get it mailed soon.

Trudy and I will be in Milwaukee in late June and will let you know the exact date when we have finalized our plans.

Thank you again for all that you did.

Best regards,

Irv

Rijks*universiteit* Groningen

Faculteit der Wijsbegeerte

Filosofisch Instituut
A-weg 30
9718 CW Groningen
Telefoon (050) 63 61 61
Telefax (050) 63 61 60

Dr. A. Rader
[illegible]
Rosehill-on-Sea
East Sussex TN39 4ER
England

Datum: 3 July 1995

Uw kenmerk:

Ons kenmerk:

Onderwerp:

Dear Mr Rader,

Thank you so much for the copy of your forthcoming Kierkegaard Address. I have the pleasure to send you herewith a copy of the article you kindly sent us for the Kierkegaard Conference in Milwaukee.

With best wishes

Yours sincerely,

Hans Kuenen

University of Groningen

Faculty
of Philosophy

Professor A. Bader
Aldrich Chemical Company
P.O. Box 355
Milwaukee
Wisconsin 53201
Verenigde Staten

A-weg 30
9718 cw Groningen
The Netherlands
Telephone + 31 50 63 61 61
Telefax + 31 50 63 61 60

Date July 3, 1995. Our ref. Your ref.

Subject

Dear Professor Bader,

With the best remembrances of the Loschmidt memorial symposium I herewith send you a copy of my 'letter to the editor' in Chemisch Weekblad of March 19, 1992 (together with an English translation), in which I gladly made reference to the splendid catalogues of Aldrich Chemical Company. In my opinion your activities in the field of Dutch and Flemish paintings symbolize the wished for attitude of chemists, that proudly claim their place in the realm of culture. I was therefore really delighted to see your being honoured by the University of Vienna and, more personally, to make your acquaintance.

Perhaps I may ask your advice for a problem in the field of Dutch and Flemish art of the XVIIth century. I am preparing a (rather thick) book on the history of the concept of molecule. It concerns a project supported by the Royal Netherlands Academy of Arts and Sciences. Of course, Loschmidt will feature prominently in it, but also the Dutch natural philosopher Isaac Beeckman (1588-1637), who, as far as I have been able to establish in my doctoral thesis (Paris, 1983), was the inventor of the notion of 'molecule' (about 1620). Ever since 1983 I have been busy in finding souvenirs of Beeckman, who directed the Latin School of Dordrecht (1620-1637) and was a personal friend of René Descartes and Pierre Gassendi (the maker of the word 'molecule'; ca.1635). I am particularly looking for a complete copy of his doctoral thesis (Caen, France, 1618: De febre tertiana intermittente; there is an incomplete copy in the British Library, London) and a portrait. Therefore I visited all the principal libraries of north-western Europe and wrote to the other important libraries of Europe (including Russia) and the United States. If I didn't succeed in finding a portrait and a copy of the thesis, I nonetheless happened to find a copy of the auction catalogue of his book collection (dated July 14, 1637; Biblioteca Angelica, Rome), perfectly

unknown until then. Now I wondered, as to the portrait, if you could be so kind as to give me some advice concerning what to do and how to proceed. Thank you so much in advance for your courtesy !

With best wishes for your generous work, I remain,

Yours sincerely,

H.H. Kubbinga

Dr. H.H. Kubbinga.

Chemisch Weekblad 88 (12) 90 (1992)
(19 maart 1992)

H.H. Kubbinga in dit CW

De KNCV zou moeten stimuleren dan kunstenaars de scheikunde tot onderwerp kiezen, en wel actief, door het uitloven van prijzen.

REAKTIES

De 'filosofie' van de chemie

Het vak scheikunde heeft het zwaar te verduren. Verbetering van het imago dient echter mijns inziens niet bij het onderwijs te beginnen, dit in tegenstelling tot wat in het Chemisch Magazine van maart 1992 als stelling wordt gepresenteerd. De chemici in het algemeen en hun vakverenigingen in het bijzonder zullen zich direct tot al hun medeburgers moeten richten. Dit kan bij voorbeeld door populariserende artikelen in de landelijke pers, maar ook door gepaste aandacht voor het verleden.

Aan dit laatste hapert het nogal eens. Neem de herdenking van van 't Hoffs Nobelprijs. Hij krijgt zijn postzegel, dat wel, maar op een ongepast tijdstip, de negentigste verjaardag. Bovendien: om goed te laten uitkomen hoe vre-se-lijk gewoon hij in wezen was, werd hij afgebeeld achter zijn typemachine. Geen betere methode, dunkt me, om eventuele Nobel-aspiraties bij aankomende chemici - zeg, op het niveau van de 3e klas - in de kiem te smoren. Geen betere methode ook om de status van het vak temidden der andere vakken te ondergraven. De 'filosofie' achter de scheikunde lijkt ernstig verwaarloosd. Het is die 'filosofie' die aangeeft hoe de scheikunde zich verhoudt tot andere wetenschappen en tot andere cultuur uitingen.

Robert Boyle zei het eens zo in zijn werk The sceptical chymist (1661):

"And though I am unwilling to deny, that 'tis difficult for a man to be an Accomplisht Naturalist, that is a stranger to Chymistry, yet I look upon the common Operations and practices of Chymists, almost as I do on the Letters of the Alphabet, without whose knowledge 'tis very hard for a man to become a Philosopher; and yet that knowledge is very far from being sufficient to make him One."

Wil de scheikunde enige indruk maken buiten de eigen kring en de broodnodige studenten trekken dan moet zij respect afdwingen, niet alleen door haar onbetwist gunstige resultaten (Pimentel, Selinger, Atkins), maar ook door haar belangstelling voor andere takken van cultuur. Zij moet bewijzen dat zij integraal deel uitmaakt van die cultuur en de haar toekomende plaats ook feitelijk opeisen.

De IUPAC c.q. de KNCV als haar vertegenwoordigster zou moeten stimuleren dat kunstenaars (literatoren, schilders, componisten, enz.) de scheikunde tot onderwerp kiezen, en wel actief, door het uitloven van prijzen. Te denken valt aan een concours voor een toneelstuk over een (bio-) chemisch gezien heet hangijzer met iemand als W.F. Hermans als extern beoordelaar (b.v. over het in kaart brengen van het menselijk erfelijk materiaal en eventuele consequenties).

Als voorbeeld bij uitstek mag Brechts Leben des Galilei dienen, dat op dit moment (maart 1992) in Parijs weer eens triomfen viert. De herdenking van Faraday eind september, in Museum Boerhaave, door het gezelschap Pandemonia (Science Theater Nederland) ging al een eind in de goede richting. De KNCV zou echter niet eerder mogen rusten dan dat zo'n toneelstuk ook in de Amsterdamse Stadschouwburg wordt opgevoerd. Het kan dan nooit lang meer duren of er is geen huis meer in Nederland dat zich niet laat voorstaan op zijn scheikundedoos of niet de laatste Aldrich catalogus onopvallend midden op de lectuurtafel heeft liggen; als het zover is zal het gesprek even verderop, aan de borreltafel, ongetwijfeld worden beheerst door de studieprestaties van de scheikundestudenten onder de diverse familieleden. Een adembenemend vooruitzicht, lijkt me. In 2001 kan dan het 'echte' eeuwfeest van van 't Hoffs Nobelprijs luister worden bijgezet met een postzegel met daarop slechts afgedrukt $d\ln K/dT = (A/T^2) + B$, want tegen die tijd weet iedereen dat hij meer was dan alleen een voortreffelijke typist. ●

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Chemisch Weekblad 88 (12) p.90 (1992), dated March 19, 1992.

The 'philosophy' of chemistry

Chemistry as a discipline lives hard times. However improving its image should not begin, in my opinion, with the educational system, contrary to the suggestion presented in Chemisch Magazine of March 1992. The chemists in general and their unions in particular shall have to address directly all of their fellow citizens. This may be done by popularizing articles in the national news papers, but also by properly expressed interest in the past.

As to the latter point, there regularly is something wrong. Take for instance the commemoration of Van 't Hoff's Noble Prize. He has got his stamp indeed, but at an improper moment, the ninetieth aniversary. Moreover: in order to show clearly how ter-ri-bly normal he in essence was, Van 't Hoff has been represented behind his typewriter. No better method, in my view, to stifle in its birth eventual Nobel aspirations among freshmen-chemists - say, at the level of mid-High School. No better way either to undermine the status of the discipline among the other disciplines. The 'philosophy' of chemistry seems seriously neglected. It is that 'philosophy' that indicates how chemistry is related to other sciences and other cultural expressions.

Robert Boyle, in his The sceptical chymist, once said it in the following way:

> "And though I am unwilling to deny, that 'tis difficult for a man to be an Accomplisht Naturalist, that is a stranger to Chymistry, yet I look upon the common Operations and practices of Chymists, almost as I do on the Letters of the Alphabet, without whose knowledge 'tis very hard for a man to become a Philosopher; and yet that knowledge is very far from being sufficient to make him one."

When chemistry wishes to make a good impression outside the own circle and to attract the badly needed students, then it has to command respect, not only by its unchallenged favorable results (Pimentel, Selinger, Atkins), but also by its attention for other branches of culture. It has to prove that it is an integral part of that culture and indeed claim the place that is its due.

The IUPAC and the KNCV as its Dutch representative ought to stimulate that artists (litterary authors, painters, composers, etc.) chose chemistry as their subject, and so actively, by putting up prizes. One could think of a competition for a theatre play about a (bio)chemical hot item with someone like W.F. Hermans (*) as an external judge (e.g. about the mapping of the human genome and eventual consequences).

As an unrivalled example may serve Bertold Brechts's Leben des Galilei that just at this moment (March 1992) is triumphantly staged in Paris. The commemoration of Faraday in Boerhaave Museum [Leiden] by the troupe Pandemonia (Science Theater Nederland) did go in the good direction. However the KNCV

shouldn't rest before such a play is staged in the City Theatre of Amsterdam. Then it can't last very long before any house in the Netherlands boasts of its own chemistry kit and has the last Aldrich catalogue in-ad-ver-tent-ly in the midst of the reading-table. When it is so far, the discussion among the dram-drinkers, a table farther, is doubtless dominated by the study results of the chemistry students among the various members of the family. A breath-taking propspect in my view. Then, in 2001, the real centenary of Van 't Hoff's Nobel Prize can be commemorated by a stamp featuring only $d\ln K/dT = (A/T^2) + B$, for at that time anyone knows that he was more than an excellent typist.

H.H. Kubbinga

(*) Willem Frederik Hermans was one of our celebrated authors (he died a month ago).

HENK H. KUBBINGA

La théorie de la matière de Boscovich: l'atomisme de points et le concept d' «individu substantial»

Estratto da

R.J. Boscovich
Vita e attività scientifica
His life and scientific work

A cura di Piers Bursill-Hall

Roma, Istituto della Enciclopedia Italiana, 1993

HENK H. KUBBINGA

La théorie de la matière de Boscovich: l'atomisme de points et le concept d'«individu substantiel»

1. *Introduction*

La théorie de la matière du R.P. Roger Boscovich S.J. a été le sujet de plusieurs études approfondies. Mentionnons d'abord la contribution de Lancelot Law Whyte à l'ouvrage *Roger Joseph Boscovich S.J., F.R.S., 1711-1787: Studies of His Life and Work on the 250th Anniversary of His Birth* (Londres, 1961), contribution intitulée *Boscovich's Atomism*. Ce même ouvrage contient également un article de M. Williams sur *Boscovich and the British Chemists*. L'article en question nous semble plus riche encore que le titre ne suggère, car son auteur donne une analyse très réfléchie des implications chimiques de la théorie du savant jésuite, avant d'entrer dans le détail des réminiscences de cette théorie dans les travaux de Priestley, de Davy et de Faraday [1].

D'un intérêt particulier dans ce contexte est l'éloge que Mme Paušek-Baždar, dans les *Annales de l'Institut français de Zagreb* de 1977-1982 (Zagreb, 1983), a consacré à *Les idées de Boskovich sur la chimie* (traduction française par Mme Vlasta Matic). Il s'agit, à notre opinion, d'un éloge des plus meurtriers. Sans aucune hésitation l'auteur trouve dans les énoncés de Boscovich une première ébauche de la théorie moderne d'orbitaux moléculaires pour les électrons d'une molécule. Au même titre on pourrait lui attribuer l'honneur d'avoir formulé la base théorique de l'imprimante matricielle, en suggérant qu'on peut composer des lettres à partir de points. Les publications nommées jusqu'ici se concentrent sur les aspects de la théorie dite atomisme de points ayant trait à la structure de la matière phénoménale. M Ivanović, dans sa communication au congrès commémoratif de Milan (1962), a osé franchir ces limites apparemment dictées par l'état actuel des sciences exactes et fait ressortir la perspective cosmologique dans laquelle Boscovich

* Cette étude a été réalisée grâce à une bourse de l'*Académie Royale Néerlandaise des Sciences et des Lettres*.

[1] Pour ce qui concerne l'influence de Boscovich sur le développement ultérieur des sciences naturelles, voir R.E. Schofield, *Boscovich and Priestley's Theory of Matter*, in L.L. Whyte [1961], pp. 168-172, I.B. Cohen, *Franklin and Boscovich*, in *Actes* [1962], pp. 131-136, M. Gliozzi, *La costituzione della materia nella concezione di Boscovich e Faraday*, in *Atti* [1963], pp. 115-119, R. Kargon [1964], *idem* [1965], R. Siegfried [1967], J. Brookes Spencer [1967], M.A. Sutton [1971] et A. Rossi [1976].

avait conçu sa doctrine [2]. Signalons, enfin, l'étude de M Stiegler de la filiation de l'idée centrale de l'atomisme de points, à savoir l'hypothèse d'atomes inétendus [3]. M Stiegler a établi nettement que Boscovich, loin d'être un novateur acharné, fit partie de toute une tradition, remontant même à Zénon d'Elée.

Dans notre présente communication nous nous proposons de donner d'abord un aperçu des différentes orientations dans la théorie de la matière au milieu du XVIIIe siècle. Les grands noms sont ceux de Newton et de Stahl [4]. La doctrine newtonienne, telle qu'elle se présente dans les *Philosophiae naturalis principia mathematica* (1687) et dans l'*Opticks* (1704), est d'ailleurs généralement connue, tout particulièrement grâce à M Thackray, dont l'ouvrage *Atoms and Powers - An essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass., 1970) domine encore l'historiographie dans ce domaine. Cependant, des précisions importantes, fondées sur une étude minutieuse des manuscrits inédits, ont été apportées par Mme Dobbs et Mme Figala. Nous parlons des écrits alchimiques de Newton, sauvés de la diaspora par l'économiste anglais J.M. Keynes (1883-1936) au cours des années 1930. Schématiquement parlé, on peut soutenir que Mme Dobbs, dans son livre impressionant sur *The Foundations of Newton's Alchemy or 'The Hunting of the Greene Lyon'* (Cambridge, 1975), a saisi surtout le côté qualitatif de l'alchimie newtonienne, tandis que Mme Figala, de sa part, a mis en lumière plutôt le côté quantitatif de cette doctrine. Mme Figala a su démontrer, en outre, — et bien d'une manière parfaitement convaincante — le lien très étroit entre cette même doctrine et la théorie dite coque de noix, que Newton, selon toute vraisemblance, n'a pas risquée promulguer plutôt que dans la deuxième édition anglaise de l'*Opticks*, donc dans celle de 1718 [5].

L'autre orientation qu'avait prise la théorie de la matière dans la première moitié du XVIIIe siècle est personnifiée par le chimiste et médecin allemand Georg Ernst Stahl (1659-1734). Or il nous a paru que la doctrine exceptionnelle de Stahl, dont les détails n'ont été décélés que dernièrement, doit être regardée comme une étape

[2] *Atti* [1963].

[3] K.D. Stiegler [1980], pp. 327-355.

[4] La première fois que les noms de Newton et de Stahl furent liés ensemble était dans un ouvrage de caractère physico-chimique intitulé *Nouveau cours de chymie, suivant les principes de Newton et de Sthall. Avec un discours historique sur l'origine et le progrez de la chymie*, Paris, 1723. Ce livre parut sans nom d'auteur, mais il est généralement attribué à Jean-Baptiste Sénac (ca. 1693-1770). Contrairement à ce que le titre veut fair croire on ne peut guère parler d'une synthèse d'ordre newtonio-stahlien. Cf. H.H. Kubbinga, *Le développement historique du concept de «molécule» dans les sciences de la nature jusqu'à la fin du XVIIIe siècle*, thèse pour le doctorat de 3e cycle (Ecole des Hautes Etudes en Sciences Sociales), Paris, 1983, pp. 152-153. Voir aussi: H. Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique*, Paris, 1930 (nouveau tirage, Paris, 1974).

[5] K. Figala, *Die «Kompositionshierarchie» der Materie — Newtons quantitative Theorie und Interpretation der qualitativen Alchemie*, thèse pour le doctorat d'état, Munich, 1977; *idem*, *Newton as Alchemist*, in «History of Science», XV (1977), pp. 102-137 (il s'agit d'un compte rendu de *The Foundations of Newton's Alchemy or 'The Hunting of the Green Lyon'*, Cambridge, 1975, pp. 217-221); *idem*, *Die exakte Alchemie von Isaac Newton — Seine «gesetzmässige» Interpretation der Alchemie dargestellt am Beispiel einiger ihn beeinflussender Autoren*, in «Verhandlungen der Naturforschenden-Gesellschaft Basel», XCIV (1984), pp. 157-228.

majeure dans le développement de la théorie de la matière [6]. A l'exception de Hélène Metzger (1889-1944) [7], les historiens des sciences n'ont jugé Stahl jusqu'ici que par les yeux de Lavoisier, c'est-à-dire: ils n'ont considéré que la théorie du phlogistique. Sans tenir compte du fait que le feu principe ne prenait qu'une place de moindre importance dans la vaste doctrine stahlienne, ils ont condamné Stahl, parfois même sévèrement [8]. Or nous avons pu prouver que la théorie moléculaire actuelle, l'une des notions-clefs des sciences exactes modernes [9], est une descendante directe des vues de Stahl et, d'autre part, que celles-ci, d'une manière pas encore dévoilée, remontent aux premières théories proprement dites «moléculaires» [10], à savoir celles d'Isaac Beeckman (1588-1637) (1620) et de Sébastien Basson (fl.fin du XVIe-début du XVIIe siècle) (1621) [11].

La grande innovation qu'ont avancée Basson et Beeckman se laisse résumer dans le concept d'«individu substantiel» comme la condition nécessaire et suffisante pour l'existence d'une espèce de substance. C'est en effet à ce concept que la plupart des idées préponderantes du XVIIe siècle concernant la nature de la matière sont redevables. Il suffit ici de mentionner Descartes, Sennert, Lamy, Rohault, Lémery, Borelli et Huygens, qui ont tous prôné une théorie de la matière dans laquelle le concept d'«individu substantiel» a joué un rôle inspirateur. Très illustratif par ailleurs pour la richesse en connotations propre à ce concept est la doctrine des soi-disant monades de Leibniz. C'est dans cette doctrine que se confondent bon nombre d'idées fondamentales de la physique, de la chimie, de la biologie, des mathématiques, voire de la sociologie, de la psychologie, de la politologie et de la théologie. Quoique la doctrine leibnizienne n'ait joué qu'un rôle de second plan dans le développement ultérieur de la théorie de la matière — c'est Stahl et Newton qui le déterminaient — elle a tout de même pu influencer la pensée de Boscovich, ce que ce dernier reconnut dès le premier paragraphe de sa *Theoria philosophiae naturalis*.

[6] H.H. Kubbinga, *Le développement historique* ..., cit.

[7] Cf. K. Figala, *Die «Kompositionshierarchie»* ..., cit.; *idem*, *Die exakte Alchemie* ..., cit.

[8] J.R. Partington, dans son *A History of Chemistry*, London-New York, vol. II, 1961, chap. XVIII, ne discute que la théorie du phlogistique. Au congrès «Georg Ernst Stahl (1659-1734) und seine Zeit» (Halle, du 23 au 25 février 1984) la théorie de la matière du commémoré n'a pas été discutée pour soi.

[9] Il est à noter que la doctrine moderne de la matière reconnaît dans une première approximation *deux* théories complémentaires, à savoir la théorie *moléculaire* et la théorie dite de *réseau cristallin*. A l'état solide, les deux théories coïncident.

[10] Le mot de «molécule» fut introduit par Pierre Gassendi au début de 1637, comme l'a établi O.R. Bloch, voir O.R. Bloch, *La philosophie de Gassendi. Nominalisme, matérialisme et métaphysique*, La Haye, 1971, p. 259.

[11] H.H. Kubbinga, *Les premières théories «moléculaires»: Isaac Beeckman (1620) et Sébastien Basson (1621). Le concept d'«individu substantiel» et d'«espèce substantielle»*, in «Revue d'Histoire des Sciences», XXXVII (1984), pp. 215-233; *idem*, *La première spécification «moléculaire» de l'atomisme épicurien: Isaac Beeckman (1620) et le concept d'«individu substantiel»*, in «Lias», XI (1984), pp. 287-306; *idem*, *Isaac Beeckman (1588-1637) en de molecularisering van de microcosmos. Een aspect van de 'mechanisering van het wereldbeeld'*[*Isaac Beeckman (1588-1637) et la molécularisation du microcosme. Un aspect de la 'mécanisation de l'image du monde'*], in «De zeventiende eeuw [Pays-Bas]», II (1986), pp. 59-79 (avec un résumé en francais).

Boscovich est effectivement clair quant à ses guides dans l'élaboration de son nouveau système: ce sont Newton et Leibniz [12]. Stahl n'y est pour rien. Son nom fait entièrement défaut, ce qui est paradoxal sachant sa position directrice parmi les chimistes et les minéralogistes et d'autant plus, puisque certaines idées de Boscovich se rapprochent de beaucoup de la doctrine stahlienne dans sa forme achevée. Nous en reparlerons ci-après.

Dans ce qui suit nous commencerons alors avec un exposé sommaire des vues de Stahl, de Leibniz et de Newton et bien dans un ordre chronologique, ce qui permet de mieux cerner le propre de chaque doctrine. Dans la perspective ainsi créée, nous analyserons ensuite la théorie de la matière de Boscovich, pour clore notre présentation avec quelques remarques sur la position de cette théorie dans l'histoire des sciences.

2. *Stahl, Leibniz et Newton: le concept d'«individu substantiel»*

A partir de 1683, Stahl a développé une théorie de la matiére partant de la reconnaissance qu'il y existe, dans la nature, une hiérarchie de substances, l'une substance étant plus complexe que l'autre, ainsi que l'analyse chimique le révèle [13]. L'idée que les corps phénoménaux, tel que l'eau, l'or et le cinabre, ne sont pas tous d'une pareille complexité de composition n'était par ailleurs aucunément neuve. Les alchimistes du Moyen Age déjà s'étaient aperçus que la décomposition du cinabre donne le mercure et le soufre et que, inversément, la combinaison de soufre et de mercure donne naissance au cinabre. Relevons aussi les travaux du médecin Angelo Sala (1576-1637). Comme M Hooykaas l'a mis en évidence, Sala a largement contribué à l'établissement de l'idée de la complexité relative des corps phénoménaux [14]. Celle-ci se confirma d'ailleurs nettement au cours du XVIIe siècle. Elle se transformait même, petit à petit, en l'idée d'une hiérarchie dans la complexité des corps. Dans la seconde moitié du XVIIe siècle on se vit obligé, ainsi, de concevoir une terminologie spéciale pour exprimer les différents rangs dans cette hiérarchie. Le chimiste Johann Joachim Becher (1635-1682) parlait, par exemple, de corps simples, de corps composés, de corps décomposés [15] et de corps

[12] R.J. Boscovich [1763], par. 1, p. 1; *idem*, [1759b], par. I, p. 1.

[13] Cf. H.H. Kubbinga, *Le développement historique* ..., cit., par. 4.4, pp. 133-147. Voir aussi: H.H. Kubbinga, *Hélène Metzger et la théorie corpusculaire des stahliens au XVIIIe siècle*, in G. Freudenthal (éd.), *Etudes sur Hélène Metzger*, in «Corpus», VIII-IX (1988), pp. 59-66.

[14] R. Hooykaas, *Het begrip element in zijn historisch-wijsgeerige ontwikkeling* [*Le concept d'élément, son développement historico-philosophique*], thèse, Utrecht, 1933 (avec un résumé en français; une édition xérox de la traduction anglaise de cet ouvrage est disponible). Voir tout spécialement pp. 145-159.

[15] Le substantif «décomposé» vient du latin «*decompositum*», qui, lui, est une contraction de l'expression «*compositum de composito*»; il indique, par conséquent, un corps *plus* complexe qu'un «composé». Signalons que le mot de «décomposition» au sens d'un processus chimique qui donne des produits *moins* complexes que la substance de départ, a une autre etymologie. Il vient du latin «*decompositio*», qui est une contraction du préfixe «dis», indiquant l'éloignement, la perte, et du substantif «*compositio*». Delà sa connotation d'«analyse» ou de «résolution».

surdécomposés, en quelle série la complexité est supposée de croître [16]. Les corps simples constituent alors les corps composés, tandis que ces derniers forment entre eux les corps décomposés, etc. Becher établissait un parallèle entre ces niveaux de complexité et les degrés linguistiques de comparaison: le positif, le comparatif et le superlatif. On peut trouver une idée semblable chez Robert Boyle (1627-1691), qui en fit allusion dans son chef-d'oeuvre *The Sceptical Chymist* (Londres, 1661).

Georg Ernst Stahl, grand amateur des idées de Becher, reprit la division de ce dernier, tout en y ajoutant une interprétation rigoureusement «moléculaire». Pour lui, un corps perceptible tel que l'alun, par exemple, n'est en dernier ressort qu'un «agrégat» — c'est-à-dire, un amas de «molécules», selon ses propres mots. En 1697, dans l'ouvrage *Zymotechnia fundamentalis* [...], il distingue «molécules» de différents ordres de complexité, en se servant de la terminologie becherienne. Ainsi, les «atomes» des quatre principes admis (l'eau et les trois espèces de terre) constituent les «molécules» des soi-disant *mixta*, alors que ces dernières sont à la base des «molécules» des corps dits *composita*. Il importe de se réaliser ici que, chez Stahl, les termes *mixtum, compositum* et *decompositum* ne se réfèrent pas uniquement à une quantité perceptible d'un corps, mais aussi aux corpuscules caractéristiques de ce corps, c'est-à-dire, à ses «molécules» [17]. Une telle «molécule» était définie «absolument petite» et «physiquement indivisible», c'est-à-dire indivisible par rapport à son espèce. Aussitôt qu'on divise une telle «molécule» en parties, elle perd sa nature et il en résulte, par exemple, deux nouveaux types de «molécules» d'ordre inférieur quant à leur complexité et représentant chacun une certaine espèce de substance. Stahl a pris soin de développer tout un appareil de concepts pour canoniser sa théorie, de la portée de laquelle il était très conscient et à juste titre, selon notre opinion. Citons les petits traités *De differentia mixti, texti, aggregati, individui* de 1700 et *De divisionis et diffissionis differentia* de

[16] J.J. Becher, *Actorum laboratori chymici monacensis, seu Physicae subterraneae libri duo*, Francofurti, 1669, p. 617.

[17] La naissance du concept d'«individu substantiel» menait à un changement dans la notion de «substance» ou de «corps». Pour les atomistes comme pour les aristotéliciens, tout objet phénoménale était une «substance» ou un «corps». Les aristotéliciens croyaient qu'un tel objet était une unité de «matière» et de «forme», tandis que les atomistes, eux, le concevaient comme une unité d'un certain nombre d'atomes de certaines espèces arrangés dans une structure spatiale spécifique. Pour tous deux camps cependant un objet phénoménale était indivisible quant à son espèce, un lévrier ou un poirier autant qu'un cristal d'alun ou une pépite d'or. On savait bien, naturellement, qu'on pouvait diviser le cristal d'alun ou la pépite d'or sans que les qualités chimiques, pour ainsi dire, disparaissaient. Le grand problème était de définir la limite inférieure en deçà de laquelle une quantité d'alun ou d'or cessait d'être d'alun ou d'or. Ce problème a joué à partir d'Aristote, durant tout le Moyen Age. Ni les philosophes, ni les alchimistes ont réussi à définir cette limite de façon consistente. On trouve la première solution adéquate chez Beeckman et chez Basson. Depuis, les notions de «substance» et de «corps» s'appliquent plutôt à la particule caractéristique définissant cette limite inférieure, qu'à des amas de ces particules. Il est curieux de constater dans ce contexte que dans l'Allemand et dans le Néerlandais, on connaît aujourd'hui un terme spécial pour un amas: les équivalents de «substance» et de «corps» («*Selbständigkeit*» et «*Körper*», et «*zelfstandigheid*» et «*lichaam*», respectivement) sont devenus obsolètes dans les manuels de chimie. Depuis environ les premières décennies de notre siècle on ne parle que de «*Stoff*» et «*stof*», respectivement. Dans l'Anglais, le Français et apparemment aussi dans l'Italien et le Serbo-croate l'ambiguité persiste.

1703 [18]. Dans le premier il discute fort systématiquement les différences entre «molécules» et animaux en faisant valoir que les premières, qu'il appelle également «individus physiques» (*individua physica*), n'ont pas le «but organique» (*scopum organicum*) des derniers. Les «molécules» ne seraient assujetties qu'à la seule «nécéssité de la matière» (*necessitas materiae*), c'est-à-dire: à la forme et à la grandeur des particules ultimes, les «atomes». Dans le deuxième traité Stahl considère la différence qui est entre la division mathématique et la division physique et dit que la première est illimitée, tandis que la seconde est justement limitée par la nature de la substance sous considération, qui chez Stahl, comme nous venons de le voir, s'exprime dans sa «molécule». Alors le terme «*divisio*» est réservé pour la division mathématique, celui de «*diffissio*» (dérivé de «*diffindo*», au sens de fendre ou séparer en deux) pour la division physique. Une «*diffissio*» s'opère, par exemple, lorsqu'un corps se dissoud dans un liquide: ses «molécules» se détachent les unes des autres et se répandent — intactes donc! — sur tout le volume disponible.

Il va presque sans dire que les notions décrites jusqu'ici en impliquent d'autres, telle que celle de «corps pur» et celle de «mélange mécanique», dont l'importance pour le développement ultérieur de la chimie ne saurait guère être surestimée. Certes, Stahl les connaissait. Dès 1703, en effet, il eut une philosophie entière de la matière phénoménale. Une étude comparative des ouvrages de Stahl parus dans la première moitié du XVIIIe siècle et leurs différentes éditions nous a appris encore plus que cela. Il nous a paru que Stahl, au fur et à mesure que sa philosophie de la matière s'épanouit, réduisit l'appareil terminologique au minimum absolument indispensable. Pour lui, dans les *Fundamenta chymiae dogmaticae et experimentales* [19], il n'existe dans la nature inorganique sinon des agrégats «homogènes» et «hétérogènes» [20], c'est-à-dire, selon les expressions modernes: des «corps purs» et des «mélanges mécaniques». S'il s'agit d'un agrégat «homogène», on ne peut avoir affaire qu'à un «corps mixte» ou bien à un «corps composé», toute distinction dans la mesure de complexité de ce dernier étant abandonnée [21]. Les molécules des «corps mixtes» consisteraient en les atomes [22] des principes; celles des «corps composés» en

[18] Les deux traités parurent dans le recueil G.E. Stahl, *Observationes physico-chymico-medicae curiosae* [...], Halle, 1709.

[19] G.E. Stahl, *Fundamenta chymiae dogmaticae et experimentalis* [...], Norembergae, 1723. Nous citons d'après la deuxième édition, celle de 1746 (Nuremberg).

[20] G.E. Stahl, *Fundamenta chymiae* ..., cit., pp. 7-8.

[21] *Ibidem*, pp. 6-7. Dans l'introduction (*Proemium*, par. 1, p. 1) la chimie est définie ainsi: «Chymia, alias Alchymia et Spagirica, est ars corpora vel mixta vel composita, vel aggregata etiam, in principia sua resolvendi, aut ex principiis in talia combinandi».

[22] Pour raisons de clarté nous nous servons dans le texte des mots d'«atome» et de «molécule» toujours dans la même acception. Chez Stahl, à vrai dire, il y a des virements. Pour ce que nous autres modernes aurions appelé une «molécule», il usa, en 1696, du même mot, tandis qu'en 1700 il se servit, par contre, du mot «atome» pour désigner la même chose. Ce changement de terminologie est du reste bien justifiable, puisque le concept d'«individuum physicum» auquel le mot d'«atome» se rattache en 1700 implique l'indivisibilité quant à l'espèce. Il demeure qu'il faudrait distinguer dans ce cas les «atomes» des principes (et par cela même noncomposés) des «atomes» des mixtes et des composés. Apparemment Stahl n'était pas encore content. En 1703, il introduisit le couple de concepts «partie constitutive» et «partie intégrante»: le premier se réfère aux composants d'un «individu substantiel», le dernier à cet «individu substantiel» lui-même en tant que constituant d'un

«particules moins simples» (*partes minus simplices*) [23]. Ceci n'empêche qu'il lui fallait avoir recours à une distinction nouvelle, à savoir entre «principes physiques», absolument insécables au sens de l'atomisme antique, et «principes chimiques», insécables pour tout moyen chimique disponible [24]; les «principes physiques» sont donc les atomes qui composent les molécules des «principes chimiques», qui, eux, constituent les molécules plus grandes des «corps composés». Nous verrons par la suite que des idées semblables ont occupé Boscovich.

Dans cette doctrine aussi vaste que géniale le phlogistique, ce fameux feu principe et cible privilégiée de Lavoisier dans les années 1780, n'est qu'un tout petit détail. Il n'est que l'un des quatre «principes physiques», donc l'une des quatre espèces d'atomes initialement reconnues par Stahl.

Retenez de ce que nous venons de dire le concept d'«*individuum physicum*», dont Stahl se servit en 1700, dans l'ouvrage chimique autant qu'épistémologique sur les différences entre choses agrégées et choses individuelles et entre molécules et animaux. En ce concept nous reconnaîtrons immédiatement une manifestation de l'idée d'«individu substantiel», cette précieuse découverte du XVIIe siècle qui définit la condition à la fois nécessaire et suffisante pour l'existence d'une espèce de substance phénoménale. Les «particules» de René Descartes et siens, autant que les «partes integrantes» de Guillaume Lamy (1644-1682) (1669), les «*minima sui generis*» de Daniel Sennert (1572-1637) (1636), les «*minima*» de Basson (1621) et les «*homogenea physica*» de Beeckman (1620) témoignent abondamment de la richesse de cette notion-clef. Mais également — et ceci importe beaucoup dans notre communication — les monades de Leibniz, ce dernier étant, comme nous l'avons vu, l'un des inspirateurs du savant jésuite. En ce qui concerne le calcul à l'aide d'unités infinitésimales Leibniz était, on le sait bien, un élève directe de Christiaan Huygens (1629-1695), le *primus inter pares* de l'Académie Royale des Sciences de Paris. Malgré ses réticences devant le cartésianisme, Leibniz, quant aux conséquences de ses découvertes en mathématique pour la théorie de la matière, appartient donc nettement à la tradition cartésienne, celle des «particules». De fait, c'est surtout dans l'oeuvre de Leibniz que se font jour les interdépendances des mathématiques et des sciences naturelles.

A partir de 1675, Leibniz développa les bases du calcul différentiel et intégral, qu'il publia sous forme de deux articles dans les *Acta eruditorum* de 1684 et 1686, respectivement. Que la théorie des infiniment petits n'était point le résultat d'un développement exclusivement mathématique, mais qu'elle avait dès l'abord une signification beaucoup plus générale, découle sans contredit d'un examen attentif de la doctrine de monades. Leibniz transforma cette conception progressivement

«agrègat». Le même problème s'est présenté par ailleurs plus tard à John Dalton (1766-1844), qui se décida, en 1810, en faveur du couple «simple atom» et «compound atom». La terminologie actuelle est le fruit d'un développement qui culmina dans le célèbre Congrès de Karlsruhe (1860), où les chimistes européens se mirent d'accord sur les fondements de leur discipline.

[23] G.E. Stahl, *Fundamenta chymiae* ..., cit., p. 8.

[24] *Ibidem*, p. 3.

en système. Il débuta avec le *Discours de métaphysique* (1686); puis il développa son *Système nouveau de la nature et de la communication des substances* (1695), pour achever sa doctrine dans les *Principes de la nature et de la grâce, fondés en raison* (1714) et la célèbre *Monadologie* (1714-1715) [25].

Jalabert, dans sa thèse de doctorat sur *La théorie leibnizienne de la substance et ses rapports avec la notion de temps* (Paris, 1946), a déjà fait remarquer le caractère *a posteriori* des deux ouvrages principaux, qui concernent, selon ses mots, plutôt la théorie physique de Leibniz sur la nature de la substance [26]. C'est pour cela que nous nous référons ci-après le plus souvent à eux [27].

Esquissons maintenant les grands traits de la doctrine leibnizienne et citons pour cela comme introduction les trois premiers paragraphes de la *Monadologie* intégralement [28]:

> 1.La Monade, dont nous parlerons icy, n'est autre chose, qu'une substance simple, qui entre dans les composés; simple, c'est à dire, sans parties.
> 2. Et il faut qu'il y ait des substances simples, puisqu'il y a des composés; car le composé n'est autre chose, qu'un amas, ou aggregatum des simples.
> 3. Or là, où il n'y a point de parties, il n'y a ny étendue, ny figure, ny divisibilité possible. Et ces Monades sont les veritables Atomes de la Nature, et en un mot les Elemens des choses.

Dans le *Discours de métaphysique*, que nous laisserons ici de côté pour le reste, il parle de «substances individuelles» [29]; ailleurs, dans les *Principes de la nature*, il les appelle également «centres substantiels» [30]. Partout, dans la *Monadologie* comme dans les *Principes de la nature*, l'inspiration biologique — ou, si l'on veut, logique — est évidente; partout il est clair que c'est l'homme lui-même qui a servi Leibniz de modèle pour caractériser la monade. Toute qualité de cette dernière est déterminée par rapport à l'homme, ou, dans un sens plus large, à l'animal en général. Même la variabilité propre à l'homme et aux animaux de même espèce est attribuée aux monades [31].

[25] Voir, entre autres O. Ruf, *Die Eins und die Einheit bei Leibniz - Eine Untersuchung zur Monadenlehre*, Meisenheim am Glan, 1973. Voir spécialement: chap. II, *Die Verdeutlichung der monadischen Einheit im Durchgang durch die Kritik an Descartes und am Atomismus*, pp. 61-103; M. Cariou, *L'atomisme — trois essais*, Paris, 1978. Voir spécialement le deuxième essai intitulé *Leibniz et l'atomisme antique*, pp. 65-139; R. Böhle, *Der Begriff des Individuums bei Leibniz*, («Monographien zur philosophischen Forschung», 161), Meisenheim am Glan, 1978; J. Mittelstrass, *Substance and its Concept in Leibniz*, in «Studia Leibnitiana», IX (1981), pp. 147-158 [contenant les actes du symposium «Truth, Knowledge and Reality - Inquiries into the Foundations of Seventeenth Century Rationalism», Reading, du 17 au 23 juillet 1979].

[26] J. Jalabert, *La théorie leibnizienne de la substance et ses rapports avec la notion de temps*, Paris, 1946, pp. 16-17.

[27] Nous citons d'après l'édition G.W. Leibniz, *Die philosophischen Schriften*, hrsg. von E.J. Gerhardt, 7 voll., Berlin, 1875-1890 (réédition Hildesheim-New York, 1978).

[28] *Ibidem*, vol. VI, p. 607.

[29] *Ibidem*, vol. IV, p. 432.

[30] *Ibidem*, vol. VI, p. 603.

[31] *Ibidem*, vol. VI, p. 608 (par. 9).

> Il faut même que chaque monade soit différente de chaque autre. Car il n'y a jamais dans la nature deux Etres, qui soyent parfaitement l'un comme l'autre, et où il ne soit possible de trouver une différence interne, ou fondée sur une dénomination intrinsèque.

Il est vrai que cette variabilité n'est pas un privilège des êtres vivants. A proprement parler, elle concerne tout objet appartenant à une certaine espèce, comme les grains de sable ou les feuilles d'un arbre ou quoi que ce soit. En tout état de cause, ce sont les implications d'ordre physico-chimique qui nous intéressent ici particulièrement. Or selon Leibniz, toute quantité d'or, d'eau ou d'alun, par exemple, doit être considérée agrégat de monades spécifiques [32]. Ailleurs, il décrit un tel agrégat [33]: «comme une armée ou un troupeau, ou comme un etang plein de poissons».

Une fois de plus, l'inspiration biologique est manifeste et il est évident, en outre, que les monades sont de véritables «individus substantiels». Pourtant, la praticabilité de la doctrine de monades en matières physico-chimiques était assez limitée, en particulier par une surcharge d'hypothèses supplémentaires, telle que la divisibilité à l'infini [34].

Ayant discuté les vues de Stahl et de Leibniz, il nous reste encore à analyser la théorie de Newton sur la structure de la matière phénoménale [35], avant d'être à même de percevoir la pensée de Boscovich. Newton était atomiste, on le sait bien [36]. C'est surtout dans la remarquable Query 31, référence très chérie de Boscovich, qu'il développait ses idées là-dessus. Voyons d'abord comment il concevait ses atomes [37]:

> it seems probable to me, that God in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such

[32] *Ibidem*, vol. IV, p. 494.

[33] *Ibidem*, vol. IV, p. 482.

[34] *Ibidem*, vol. VI, p. 618, par 67: «Chaque portion de la matière peut être conçue comme un jardin plein de plantes, et comme un étang plein de poissons. Mais chaque rameau de la plante, chaque membre de l'Animal, chaque goutte de ses humeurs est encor un tel jardin ou un tel étang».

[35] H.H. Kubbinga, *La théorie de la matière de Newton*, in *Newton's Scientific and Philosophical Legacy*, ed. by P.B. Scheurer, G. Debrock, Dordrecht, 1988, pp. 321-341.

[36] Sur l'atomisme de Newton, voir aussi: Note 5 et I.B. Cohen, *Franklin and Newton: an Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example thereof*, Philadelphia, 1956 (réédition Cambridge (Mass.), 1966); H. Guerlac, *Newton et Epicure*, conférence au Palais de la Découverte, Paris, 1963. A.R. Hall, M. Boas Hall, *Newton's Theory of Matter*, in «Isis», (1960), pp. 131-144; R.H. Kargon, *Atomism in England from Harriot to Newton*, Oxford, 1966; E. McMullin, *Newton on Matter and Activity*, Notre Dame, 1978; M. Boas-Hall, *Newton and his Theory of Matter in the Eighteenth Century*, in «Vistas in astronomy», XXII (1978), pp. 453-459 [contenant les actes du congrès «*Newton and the Enlightenment*», Cagliari, du 3 au 5 octobre 1977]; B.J.T. Dobbs, *Newton's Alchemy and His Theory of Matter*, in «Isis», LXXIII (1982), pp. 511-528.

[37] I. Newton, *Opticks: or, a Treatise of the Reflections, Refractions, Inflections and Colours of Light*, London, 1704. Nous citons d'après la 4e édition (London, 1730) (réédition New York, 1952); Livre III, Partie I, Query 31, p. 400.

other Properties, and in such Proportion to Space, as most conduced to the End for which he form'd them.

Ce sont ces atomes qui forment des entités composées, qui, elles, constituent des entités plus complexes encore et ainsi de suite. A un moment donné, on arrivera aux particules qui occasionnent les réactions chimiques et qui causent les couleurs des corps [38]. Selon Newton, toute matière phénoménale est un agrégat de ces particules, qu'il appelle parfois «particules de l'ultime composition» (*particles of the ultimate composition*) [39]. Chaque substance est supposée d'avoir ses propres «particules de l'ultime composition»; elles sont donc spécifiques. Encore, elles sont d'une extrême porosité, ce que Newton déduit d'un bon nombre de phénomènes, de la transparence de l'eau pour la lumière jusqu'à la transparence de l'or — dix-neuf fois plus dense que l'eau — pour le magnétisme. Il évoque même la transparence de l'or pour l'eau, quoiqu'il ait pris soin d'y ajouter: «*I have been inform'd by an Eye witness*» [40]. A la rigueur et malgré beaucoup d'autres apparences, la matière perceptible serait donc, à en croire Newton, presqu'un vide par rapport à la solidité des atomes.

Newton n'hésite pas à donner une impression quantitative de sa conception [41]. Selon son avis, les atomes prennent dans les soi-disant «particules de la première composition» (*particles of the first composition*) la moitié de l'espace disponible, l'autre moitié demeurant vide. Ceci vaudra *mutatis mutandis* pour tout niveau dans la hiérarchie particulaire. Ainsi, les particules du deuxième niveau de complexité consistent en «particules de la première composition» et en espace vide dans la même proportion volumique de 1 : 1, *et caetera*. En montant l'échelle Newton calcule pour chaque niveau la proportion volumique entre l'espace occupé par les atomes et l'espace laissé vide. Pour les atomes eux-mêmes cette proportion est naturellement de $\infty : 1$. Les «particules de la première composition», elles, connaissent une proportion matière : vide de 1 : 1. Pour les particules du niveau suivant cette proportion égale 1 : 4 et ainsi de suite. La valeur que prend la proportion matière: vide paraît diminuer rapidement. Pour les «particules de la cinquième composition», par exemple, Newton calcule que l'espace vide est 63 fois plus grand que l'espace occupé par les atomes. En général, la proportion matière : vide suit la formule $1 : (2^n - 1)$, où n représente le numéro du niveau de complexité, les atomes eux-mêmes correspondant à $n = 0$.

Joseph Priestley (1733-1804) a fait ressortir, à propos de l'immense nullité de la matière dans la théorie de Newton, qu'il se peut fort bien que toute la matière de l'univers entier puisse être enfermée dans un seul coque de noix. M Thackray a repris cette image si expressive pour caractériser la doctrine newtonienne de la matière [42]. En feignant une forme cubique pour les atomes, M Thackray a aussi

[38] *Ibidem*, Livre II, Partie III, Proposition VIII, p. 268; cf. *ibidem*, Livre III, Partie I, Query 31, p. 394.

[39] I. Newton, *Some Thoughts about the Nature of Acids*, in *The Correspondence of Isaac Newton*, Cambridge, vol. III, 1961, pp. 257-258.

[40] I. Newton, *Opticks* ..., cit., Livre II, Partie III, Proposition VIII, p. 267.

[41] *Ibidem*, Livre II, Partie III, Proposition VIII, pp. 268-269.

[42] Voir aussi: A. Thackray, *'Matter in a Nut-shell': Newton's Opticks and Eighteenth Century Chemistry*, in «Ambix», XV (1968), pp. 29-53.

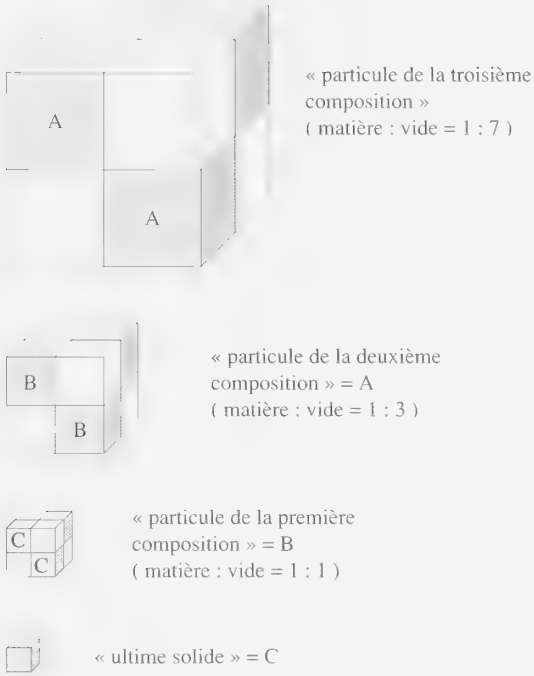


Fig. 1 - La théorie de la matière de Newton selon l'interprétation de M Thackray (voir la note 43). En supposant une forme cubique pour tous les «ultimes solides», on peut s'imaginer des particules toujours plus grandes, dans chacune desquelles la proportion volumique de sous-particules : vide est de 1 : 1. La théorie «coque de noix» peut être considérée, à notre opinion, comme la première théorie dite *fractale* de la structure de la matière, puisque sur tout niveau dans la hiérarchie particulaire l'expression mathématique est la même. Cf. B. Mandelbrot, *The Fractal Geometry of Nature* (New York, 1983).

su développer un modèle des plus ingénieux pour éclaircir cette doctrine (voir la Figure 1) [43].

En ce qui concerne la théorie de la matière, Boscovich ne se réfère qu'à l'*Opticks* et aux *Principia*. C'est pour cela que nous omettrons ici quelques points indécis dans la doctrine de Newton, qui concernent en particulier le petit traité *De natura acidorum*. Ce dernier a connu plusieurs versions et celles-ci ont été le sujet d'une étude minutieuse de Mme Dobbs, à laquelle nous renvoyons très volontiers [44]. Pour une meilleur compréhension de la théorie «coque de noix», théorie qui est à la base de la doctrine de Boscovich, force est de stipuler succinctement les résultats des recherches de Mme Figala. Or Mme Figala a manifestement réussi à démontrer

[43] A. Thackray, *Atoms and Powers - An Essay on Newtonian Matter-Theory and the Development of Chemistry*, Cambridge (Mass.), 1970, p. 64.
[44] B.J.T. Dobbs, *The Foundations* ..., cit., pp. 217-221.

que la théorie «coque de noix» doit être interprétée en termes de l'alchimie newtonienne, ce qui revient à dire que, chez Newton, l'alchimie n'est plus censée être la chimère d'autrefois: elle est devenue, chez lui, science véritablement exacte. Newton en était parfaitement convaincu, n'en doutons pas. Il n'en reste pas moins vrai cependant qu'il a soigneusement évité de rendre public le moindre détail de ses activités dans ce domaine. Jusqu'à les dernières décennies de notre époque les historiens des sciences, accablés par cette connaissance qui s'opposait si vivement à l'image favorite de Newton créateur exclusif de la science positive, l'ont suivi dans sa discrétion et se sont tus. Enfin, les résultats de Mme Figala impliquent non seulement une retouche fondamentale de cette image, mais également une nette revalorisation du status épistémologique de l'alchimie elle-même [45]. A notre opinion, on ne saurait par trop apprécier ce développement.

Ainsi, en s'appuyant sur de données numériques procurées par John Freind (1675-1728) et par Newton lui- même, Mme Figala a reconstruit l'interprétation newtonienne de la théorie «coque de nois» en termes de densité relative. Chaque substance phénoménale serait alors un agrégat de «particules de l'ultime composition» propre à son espèce et, en plus, caractérisée par une densité relative — mesurée par rapport à l'eau — de valeur spécifique. En outre, cette densité — qualité intensive, rappelons-le — est liée directement à un certain niveau dans l'échelle exprimée par la proportion matière : vide = $1 : (2^n - 1)$.

Comme nous l'avons indiqué auparavant, l'eau est le corps de référence dans ce système. Newton lui attribuait une proportion matière : vide de 1 : 65, correspondant à peu près à $n = 6$, sans qu'il précisait pour autant comment il était arrivé à justement cette valeur [46]. Cependant, en posant cette valeur, il était en état, d'après la reconstruction de Mme Figala, de calculer la proportion matière : vide pour toute substance phénoménale, à condition que la densité de cette dernière par rapport à l'eau soit connue. L'or, par exemple, étant 19,2 fois plus pesant que l'eau, contiendra logiquement 19,2 fois plus de matière. La fraction du volume total occupée par les atomes sera donc $19{,}2/(65 + 1) = 0.291$ et celle du vide $(66 - 19{,}2)/(65 + 1) = 1 - 0.291 = 0.709$. Ainsi, la proportion volumique matière : vide de l'or sera de $0.291 : 0.709$, si bien que $1 : (2^n - 1) = 0.291 : 0.709 = 2.44$, d'où $n = 1.78$. Ceci veut dire que l'or s'insérera dans l'échelle de complexité entre $n = 1$ (avec la proportion de 1 : 1) et $n = 2$ (avec la proportion de 1 : 3) (voir la Fig. 2).

[45] L'ouvrage de référence en ce qui concerne l'alchimie et son histoire est encore toujours E.O. von Lippmann, *Entstehung und Ausbreitung der Alchemie*, Berlin, 1919 (réédition, Hildesheim-New York, 1978). Or, là où von Lippmann discute la théorie soufre-mercure, qui jouait un rôle crucial dans l'alchimie du Moyen Age, il fait ressortir que celle-ci «*[erscheint] vom chemischen Standpunkte aus ganz unbegreiflich, ja völlig widersinnig*» (op. cit., p. 488). Hooykaas a établi, par contre, que c'est justement dans cette théorie que l'alchimie atteignit son niveau le plus haut. Surtout dans l'oeuvre de Geber (fin du XIIIe s.) la théorie soufre-mercure — et avec elle l'alchimie entière — devint une théorie véritablement scientifique, du moins pour l'état des connaissances à la fin du XIIIe siècle (op. cit., pp. 44-5). Les trouvailles de M Hooykaas et de Mme Figala justifient, à notre opinion, une nouvelle mise en cause du statut épistémologique de l'alchimie au cours des siècles.

[46] Communication privée de Mme Figala (Université Technique de Munich).

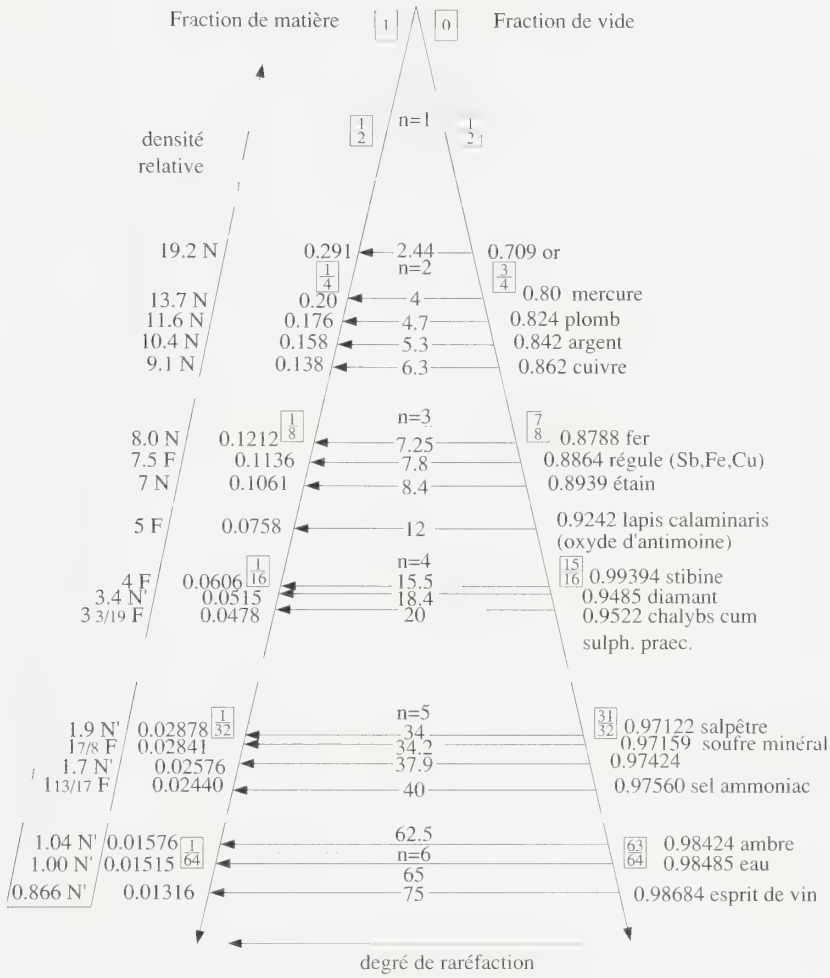


Fig. 2 - Schéma stipulant ce que Mme Figala appelle l'«arbre généalogique» des substances phenoménales dans la théorie de Newton (5). Le tronc commun de toutes les substances correspond à n = 1. Pour Newton, le corps avec la plus grande densité relative était l'or. Ainsi, si pour l'eau la proportion de matière : vide est de 1 : 65, la fraction du volume total occupée par les atomes sera de 1 : (65 + 1) = 1 : 66. Dans le cas de l'or, qui est 19.2 fois plus pesant que l'eau, cette fraction sera de 19.2 : 66 = 0.291 : 1. La fraction du volume total entièrement vide sera donc de (1 – 0.291) : 1 = 0.709 : 1. La proportion volumique matière : vide sera alors de 0.291 : 0.709, si bien que 1 : (2^n – 1) = 0.291 : 0.709 = 2.44, d'où n = 1.78. Dans la figure l'or se retrouve donc un peu au-dessus de n = 2. L'eau avec la fraction matière de 1 : 66 et la fraction vide de 65 : 66, se situera légèrement au dessous de n = 6. On peut dire qu'à mesure que la densité diminue, les dimensions des particules concernées augmentent. Ainsi, l'air s'insérera entre n = 15 et n = 16. La substance la plus complexe et, par conséquent, la moins dense serait, selon Newton, l'éther; elle aurait n = 37.

Il y a plus. Selon l'alchimie newtonienne toutes les substances de l'univers sont congénères, tout corps étant à même de se transformer en tout autre. Cette transformation s'effectuera, d'après Newton, par le démontage partiel des «particules de l'ultime composition», les atomes des niveaux plus hauts étant successivement ôtés, jusqu'à ce qu'on arrivera au niveau de la «particule de la première composition», qui serait la même pour toutes les substances. Une fois arrivé à ce niveau, l'échelle est remontée, les atomes propres au nouveau corps se mettant successivement à leur place. Sur ce point, il faut bien se rendre compte du fait que Newton admettait l'existence d'atomes de différentes formes et de différentes grandeurs. Ainsi, dans la pensée, nous pouvons nous imaginer déjà toute une classe d'entités constituées de cette «particule de la première composition» et un seul atome surajouté, pourvu évidemment que la construction de l'édifice suivra la prescription de Newton, qui exige pour chaque niveau une proportion volumique de 1:1 pour les particules de l'avant dernier niveau et le vide. Encore dans la pensée, on peut distinguer pour chacune des entités décrites toute une famille d'entités dérivées dans lesquelles un deuxième atome a été introduit, et ainsi de suite.

Une réaction chimique revient donc, selon la théorie «coque de noix», au démontage des «particules de l'ultime composition» du (ou des) corps de départ par l'enlèvement de tous les atomes superflus. Une fois arrivé au tronc commun aux particules de départ et à celles du produit, l'édification des nouvelles particules s'opérera. Le cas échéant, il faudra descendre toute l'échelle, jusqu'aux «particules de la première composition», avant de remonter. Remarquons, enfin, que, selon Newton, à mesure que les particules sont plus complexes, leur stabilité diminue [47].

Ayant exposé l'acheminement d'une réaction chimique d'après la théorie «coque de noix», il nous faut voir, pour conclure, comment Newton s'imaginait la conduite des «particules de l'ultime composition» — disons, des molécules —, les unes vis-à-vis des autres. Or, selon la Query 31, il y a deux possibilités: ou bien il n'y a qu'une seule force attractive dont l'intensité dépend de la nature des particules, ou bien il y en a deux, l'une attractive et l'autre répulsive, dont la première diminue en fonction de la distance, pour devenir zéro là où la dernière commence. Ainsi, la dissolution d'un sel dans une quantité d'eau s'expliquera de deux manières. Ou bien par l'hypothèse que l'attraction qu'exercent les particules de l'eau sur les particules du sel est plus forte que l'attraction de ces dernières les unes sur les autres; ou bien par l'hypothèse que les particules salines se repoussent mutuellement, mais sont attirées par les particules du dissolvant. Toujours est-il que Newton ne s'est pas décidé. Ce qui importe ici c'est, tout de même, qu'il considérait ses «particules de l'ultime composition» comme de vrais «individus substantiels». Elles sont en effet spécifiques pour une substance phénoménale, dans ce sens que leur espèce est définie par le nombre et la nature des atomes, ainsi que par la structure spatiale dans laquelle ces atomes — selon les prescriptions de la théorie «coque de noix» — sont unis.

Il nous reste encore de dresser un bilan, de montrer comment les savants du XVIIIe siècle ont apprécié les doctrines mises en cause, c'est-à-dire, celle de Newton,

[47] I. Newton, *Opticks* ..., cit., Livre III, Partie I, Query 31, p. 394.

celle de Leibniz et celle de Stahl. Cette tâche n'est d'ailleurs pas si lourde que ça, comme nous avons le témoignage d'un illustre contemporain, de Gabriel-François Venel (1723-1775), pour être exact. C'est Venel qui a rédigé l'article *Chymie* dans l'*Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers* [...] de Diderot et d'Alembert. Or, l'article de Venel fut inséré dans le troisième tome, qui vit le jour en 1753, donc à l'époque même où Boscovich développa les idées qu'il devait résumer, en 1758, dans la *Philosophiae naturalis theoria redacta ad unicam legem virium in natura existentium*.

Il apparaît que Venel ne reconnaît que l'autorité de Newton et de Stahl; Leibniz n'est mentionné qu'en passant. Venel traduisait sans nul doute le sentiment général lorsqu'il couronnait les *Principia* de Newton comme le produit du «génie physicien porté peut-être au plus haut degré où l'humanité puisse atteindre», tout en reconnaissant le *Specimen Beccherianum* de Stahl comme «l'extrême correspondant du génie chimiste» [48]. La physique serait, selon l'avis de Venel, la science de la conduite des agrégats, les uns vis-à-vis des autres, tandis que la chimie serait celle des molécules et des relations intermoléculaires [49]. Il faut donc distinguer, pour ainsi dire, la mécanique moléculaire de la mécanique céleste. Pour Venel, la physique se rapporte à la chimie comme les mathématiques aux langues, comme l'abstrait au concret, ou bien comme l'absolu au relatif.

Le bilan fait par Venel a été confirmé par Thackray dans sa monographie. Thackray s'est vu obligé de conclure que la théorie «coque de noix» n'a guère contribué à l'avancement de la théorie de la matière au XVIIIe siècle [50]. Il lui a paru, par contre, que l'idée d'une attraction intermoléculaire — ou éventuellement une combinaison d'attraction et de répulsion — a su séduire la majorité des théoriciens de la matière. Les minéralogistes et les chimistes, eux, adhéraient à la doctrine de la matière de Stahl, les physiciens se fiant le plus souvent à celle de Newton. Noblesse obligeait!

3. *Boscovich: l'atomisme de points*

Comme il l'indique lui-même dans le premier paragraphe de la *Theoria*, le problème de la matière et des forces a occupé Boscovich à partir de 1745. Quelques-unes des dissertations qu'il lui fallait composer dans sa fonction de professeur de mathématiques au Collegium Romanum y étaient effectivement consacrées. Citons les dissertations *De viribus vivis* (1745), *De lumine* (1748), *De materiae divisibilitate et de principiis corporum* (1748), *De continuitatis lege et ejus consectariis pertinentibus ad prima materiae elementa eorumque vires* (1754) et *De lege virium in natura existentium* (1755). Une mention spéciale est due au commentaire que Boscovich avait rédigé sur l'énorme poème didactique de son confrère et compatriote le R.P. Bénédicte Stay S.J. (1714-1801) sur la philosophie naturelle contempo-

[48] G.F. Venel, article *Chymie*, dans l'*Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers*, vol. III, Paris, 1753, p. 416b.
[49] *Ibidem*, p. 416a, b.
[50] A. Thackray, *Atoms and Powers* ..., cit., pp. 23-24, 45, 187-188 et 204.

raine [51]. Qu'il suffise ici de dire qu'il s'agit d'un panégyrique du newtonianisme, dont le premier tome parut en 1755, à Rome [52].

Les énoncés éparses ne sont mises en système que dans la *Philosophiae naturalis theoria redacta ad unicam legem virium in natura existentium*, qui vit le jour en 1758, à Vienne. Du vivant de Boscovich, cette édition connut deux réimpressions, à savoir en 1759 et 1764, toutes deux également à Vienne. Une édition corrigée et augmentée par les soins de Boscovich lui-même parut en 1763, à Vénise [53]. Par la suite nous nous référons principalement à l'édition de Vénise, que nous avons comparé par ailleurs, pour ce qui concerne les fragments cruciaux, avec l'édition Vienne, 1759. Généralement parlé on peut dire qu'il n'y a pas de divergences importantes. Le plus souvent les textes sont identiques.

Abordons maintenant l'analyse de la doctrine de la matière de Boscovich avec une attention spéciale pour tout ce qui touche au concept d'«individu substantiel». Alors, dès les premiers paragraphes Boscovich tire quelques lignes de démarcation entre les doctrines de Newton et de Leibniz, d'une part, et la sienne, d'autre. A la théorie de Leibniz il emprunte l'idée d'«éléments premiers absolument simples et inétendus» (*elementa prima simplicia ac prorsus inextensa*), qu'il appelle «points» (*puncta*); à Newton la conception de «forces mutuelles» (*vires mutuas*), à savoir l'attraction et la répulsion, dont l'une commence là où l'autre est réduite à zéro. A tous deux il emprunte l'idée que tout changement dans la position d'un point influence l'état de tous les autres points de l'univers, combien éloignés qu'ils soient (*utcunque remota*) [54]. A la différence de Leibniz, Boscovich admet qu'aucun continuum ne saurait se former à partir d'entités inétendues qui se touchent. Selon Boscovich, la contiguïté de ces entités inétendues impliquerait, dans ce cas, l'interpénétration de ces entités. Il faut donc qu'il y ait une distance entre les points. En plus, les points sont identiques entre eux, contrairement à ce qu'a soutenu Leibniz pour les monades. Une «analogie de la nature» (*analogia naturae*), selon l'expression typiquement newtonienne, a conduit Boscovich à cette conclusion. Après tout, dit-il à peu près, c'est l'analyse chimique qui enseigne qu'à mesure qu'elle progresse, le nombre de principes diminue continûment, alors qu'ils se ressemblent de plus en plus. Une analyse plus poussée encore mènera sans doute à un nombre de principes toujours plus petit et à une ressemblance toujours plus grande. A

[51] Stay était un grand amateur du genre littéraire de poème didactique. En 1744 parut à son insu, par les soins de son frère Christophore, l'ouvrage *Philosophiae a Benedicto Stay ragusino versibus traditae libri sex* (Vénise); cet ouvrage connut une deuxième édition augmentée et corrigée en 1747 (Rome). L'auteur donne une interprétation poétique de la philosophie de René Descartes, à l'instar de Lucrèce Tite Carus, qui dans ses *De rerum natura libri sex* avait rendu hommage à Epicure et à sa philosophie de la nature. Le style est parfaitement lucrétien, quoique le contenu soit diamétralement opposé à l'atomisme du philosophe romain. Sur la philosophie de Lucrèce, Christophore Stay, qui se cacha derrière ses initiaux, remarqua dans l'introduction à la première édition (p. 5): «Verumtamen, quo magis hanc in illo praestantiam admiramur, accidit, ut eo molestius feramus, tantam elegantiam tam absurdis sententiis adhibitam esse, et optima erat ratio, et praeclare ad juvandum instituta, eam opinionibus nequissimis esse depravatam».

[52] R.J. Boscovich [1755a], [1760a], [1792].

[53] Voir la bibliographie dans L.L. Whyte [1961], pp. 214 et suiv.

[54] *Theoria* [1763], par. 2, pp. 1-2; *Theoria* [1759b], par. II, p. 1.

la limite, il n'en restera qu'une seule espèce d'entités identiques entre elles, à savoir les points. C'est le principal du message de Boscovich [55]. Ailleurs, pour préciser sa pensée, il reprend l'analogie ancienne de l'alphabet. La nature est comparée avec une grande bibliothèque, dont une première étude révèlera qu'elle consiste en ouvrages en quelques langues différentes, imprimés en caractères semblables. Le chercheur imaginaire qui a trouvé cela tentera après de classer ces ouvrages selon leur langue, puis il dressera des dictionnaires, pour chaque langue un. Ensuite, il trouvera que tous les mots dans ces dictionnaires sont composés des mêmes lettres du même alphabet et ceci sera le dernier terme de son analyse, pour peu qu'il ne se servira pas d'un miscroscope. Seulement dans le cas où il utilisera un tel instrument il découvrira les entités encore plus semblables qui constituent ces caractères, à savoir les ronds points identiques, dont l'arrangement spatial détermine la nature d'une lettre [56].

Les livres de la bibliothèque seraient les objets dans la nature (*corpora*), les langues représentant les règnes. L'analyse chimique indiquera les «principes» (*principia*) dont la difformité est moins grande que celle des livres, à savoir les «mots» (*voces*). Ces «mots» sont mis en parallèle avec les nombreuses espèces de sels, de terre et d'huiles, que l'analyse chimique trouve dans les différents corps naturels. Une analyse ultérieure (*ulterior analysis*) démontrera, selon Boscovich, l'existence de corps comparables aux lettres constituant les «mots», tandis que l'analyse ultime donnera, enfin, les points [57].

Les points sont considérés centres de force, cette force étant alternativement attractive et répulsive en fonction de la distance. A de petites distances il est question de répulsion, dont l'intensité augmente à l'infini à mesure que la distance tend à zéro. Au contraire, lorsque la distance devient plus grande, la répulsion diminue en intensité et se change, en passant par un point neutre, en une attraction toujours plus grande. Après un maximum dans l'attraction, celle-ci diminue, devient neutre et se transforme, ensuite, en répulsion, qui, elle aussi, passe par un maximum. Ainsi, à des distances moyennes, l'attraction et la répulsion se succèdent, la seule force admise étant conçue, ici, comme une fonction algébrique sinusoïde. Enfin, à partir d'une certaine distance, l'attraction, ayant passé son dernier maximum, ne devient plus répulsion, mais s'approchera asymptotiquement de la neutralité. Ici, nous nous occuperons pas tellement des détails de cette doctrine de force unique. Il nous suffit de dire, que, grâce à cette force, les points peuvent former des entités plus complexes, en s'arrangeant en certains nombres à des distances et dans une structure spatiale déterminée. Ainsi résultent des «petites masses» (*massulae*) [58], dont le comportement des unes vis-à-vis des autres dépend en quelque sorte de la sommation des forces des points constituants. La force résultante de la nouvelle «petite masse» fait donc que la distribution spatiale des positions d'attraction et

[55] *Theoria* [1763], par. 3, p. 2; cf. par. 98 et 99, pp. 44-45. *Theoria* [1759b], par. III, p. 2; cf. par. XCVII et XCVIII, pp. 49-50.

[56] *Theoria* [1763], par. 98, pp. 44-45. *Theoria* [1759b], par. XCVII, pp. 49-50.

[57] *Theoria* [1763], par. 99, p. 45. *Theoria* [1759b], par. XCVIII, p. 50.

[58] *Theoria* [1763], par. 212, pp. 97-98. *Theoria* [1759b], par. CCXI, p. 109. N.B. Le mot de «molécule» n'est pas dans la *Theoria*.

de répulsion autour de cette «petite masse» n'est pas nécessairement symmétrique, chaque type de «petite masse» étant caractérisé par sa propre distribution [59]. A de grandes distances, ces «petites masses» s'attirent mutuellement, l'attraction diminuant avec la distance selon la loi des carrés inverses de Newton, sans devenir jamais zéro. A de petites distances cependant la force résultante ou, autrement dit, la distribution des positions d'attraction et de répulsion, déterminera les qualités sensibles d'un certain type de «petite masse», telles que le goût et l'odeur, ainsi que toutes les propriétés chimiques [60]. Cette distribution explique également que les «petites masses», que Boscovich appelle ailleurs «particules» (*particula*), ne s'agrégent que sous forme d'entités de figure bien définie, telles que les cristaux, les plantes et les animaux (*non nisi certo ordine*) [61]. A l'instar de Newton, Boscovich admet également l'existence de toute une hiérarchie de «particules», qu'il visualise ainsi, à titre d'exemple: à partir de quatre points une particule pyramidale régulière se formera, alors que quatre de ces particules pyramidales constitueront une pyramide encore plus grande, et ainsi de suite (voir la Figure 3) [62].

A côté des points, il y a donc des particules du «premier ordre», du «deuxième ordre», du «troisième ordre», *et caetera*. Tout comme chez Newton, on arrivera, à un moment donné, aux particules «dont dépendent les opérations chimiques» (*ex quibus pendent chemicae operationes*) [63]; ce sont elles aussi qui déterminent les propriétés optiques d'un corps. De toute évidence, Boscovich les conçoit comme des «individus substantiels», dans chacun desquels l'espèce d'un corps se reflète [64]. Sur ce point il se conforme donc nettement à la tradition. En effet, comme nous l'avons vu, Newton, Stahl et Leibniz s'accordaient ici, malgré toutes divergences.

Il y a donc, dans le monde inorganique, tout une hiérarchie de particules caractérisée par une complexité croissante; la structure pyramidale discutée ci-dessus n'est qu'un exemple. Là où Boscovich admet qu'à mesure que la complexité est plus grande, la stabilité des particules diminue, il souscrit également à l'opinion de Newton [65]. Toutefois il considère, à la fin de la *Theoria*, l'existence éventuelle, dans la hiérarchie, de particules stables à défier toute force naturelle [66]. Ceci lui

[59] *Theoria* [1763], par. 212, p. 97: «in eadem constitutione massulae pro diversis directionibus admodum diversae sint vires pro eadem etiam distantia a medio»; cf. *Theoria* [1759b], par. CCXI, p. 109; *Theoria* [1763], par. 92, p. 42: «Diversitas autem legum virium pro diversis particulis, et pro diversis respectu ejusdem particulae directionibus, habetur utique ex diverso numero, et positione punctorum eam componentium»; cf. *Theoria* [1759b], par. XCI, p. 46.

[60] *Theoria* [1763], par. 213, p. 98, *Theoria* [1759b], par. CCXII, p. 110.

[61] *Theoria* [1763], par. 439, pp. 200-201; *Theoria* [1759b], par. CCCCXXXIV, p. 225.

[62] Cfr. B. Mandelbrot, *The Fractal Geometry of Nature*, New York, 1983; Figure 143, p. 142; voir aussi: M. Calvesi, M. Emmer et collaborateurs, *I frattali - la geometria dell'irregolare*, Roma, 1988, p. 111 (La piramide transfinita di L. Safaro, 1964). Nous remercions très volontiers Mme S. Rubcich (Istituto della Enciclopedia Italiana) de sa précieuse coopération.

[63] *Theoria* [1763], par. 111. *Theoria* [1759b], par. CCXXXVIII, pp. 124-125.

[64] Voir aussi ci-dessous la note 82.

[65] *Theoria* [1763], par. 239, p. 111; par. 398, pp. 181-182; par. 424, p. 194. *Theoria* [1759b], par. CCXXXVIII, p. 125; par. CCCXCIII, pp. 203-204; par. CCCCXIX, p. 218.

[66] *Theoria* [1763], par. 521, p. 245; cf. par. 421, pp. 192-193. *Theoria* [1759b], par. DXVI, p. 277; cf. par. CCCCXVI, pp. 216-217.

Fig. 3 - La figure montre une structure pyramidale de la matière considérée par Boscovich, à l'instar de la théorie «coque de noix» de Newton. La nature *fractale* de cette structure est très claire.

permet d'opposer «*elementa*» et «*mixta*», autrement dit «éléments» et «composés». Les premiers peuvent être tenus «pour simples, et inaltérables par la force de la nature» (*pro simplicibus, et inalterabilibus vi naturae*); leurs particules sont les constituants des particules des derniers; «dans toutes les conversions [elles] ne sont jamais détruites» (*in omnibus permutationibus [...] nunquam corrumpantur*). Une réaction chimique revient, dans ce contexte, à un changement dans la composition de particules d'un ordre supérieur, par un changement dans l'arrangement des particules qui les constituent et qui sont donc d'un ordre moins élevé [67]. Ainsi,

[67] *Theoria* [1763], par. 521, pp. 245-246; cf. par. 398, p. 182 et par. 463, p. 213. *Theoria* [1759b], par. DXVI; p. 277; cf. par. CCCXCIII, pp. 203-204 et par. CCCCLVIII, pp. 240-241.

Boscovich reprend la thèse ancienne des quatre éléments qui, pour lui, serait la base de la chimie [68]. Il est à noter que, au fond, nombreux chimistes contemporains y adhéraient également, combien modernes qu'ils fussent peut-être pour le reste de leurs vues [69].

Enfin, selon Boscovich, la doctrine d'«*elementa*» et de «*mixta*» nous permettra de distinguer le genre et l'espèce d'un agrégat de ces particules. La classification qu'il propose n'est d'ailleurs pas tellement pratique. Il discute, par exemple, l'existence d'un corps avec toutes les qualités de l'or (couleur jaune, grande pesanteur, ductilité, etc.), sauf la dissolubilité dans l'eau régale. Ce corps n'est pas digne du nom d'«or», à moins qu'il ne soit pas question d'une relation avec l'or commun telle que la suivante: si ce corps, quoiqu'indissoluble dans l'eau régale, se dissoud dans un liquide qui ne dissoud pas l'or, et que les quantités du dissolvant et du dissolu sont comparables dans les deux cas, on peut parler de deux «espèces» d'or [70]. Enfin, ici comme parfois ailleurs, Boscovich cache des idées en principe très fructueuses sous un pêle-mêle de subtilités scolastiques, ce qui est d'autant plus vexant, par exemple, lorsqu'il tâche de convaincre ses lecteurs que le problème du son est «très difficile, si l'on devait le traiter sans principes subsidiaires ou hypothèses gratuites» [71]. Comme si la *Theoria* est étrange à toute spéculation, il abandonne justement ce problème.

Dans les derniers paragraphes de la *Theoria* Boscovich s'efforce de rendre vraisemblable que l'atomisme de points avec sa loi des forces s'accorde fort bien avec les idées de Leibniz et de Newton — ce qui n'étonne pas! — ainsi qu'avec celles des péripatéticiens et celles des occasionalistes [72]; ailleurs il avait déjà insisté sur la possibilité d'enrichir l'atomisme de Gassendi et successeurs avec sa conception de force [73]. Or, sachant les hautes responsabilités que la Compagnie de Jésus lui avait confiées au Collegio Romano, il n'est pas entièrement inattendu que les grands problèmes apparaissent être la doctrine de l'hylémorphisme en général et du dogme de la transsubstantiation en particulier.

L'hylémorphisme fit partie de la philosophie naturelle développée par Aristote. Cette doctrine fut reprise au cours du XIIIe siècle par St. Thomas d'Aquin (c. 1225-1274) et adaptée à servir de fondement pour la théologie chrétienne. Sanctifié en 1323 par Jean XXII, Thomas fut nommé docteur de l'Eglise par Pie V en 1567. Au cours du concile de Trente (1554-1563), c'est la philosophie péripatéticienne dans une élaboration thomiste, qui a dominé, tout spécialement lors de la formulation des onze canones concernant l'Eucharistie. Pendant l'Eucharistie, on le sait, le pain et le vin sont changés en le corps et le sang du Christ, comme

[68] *Theoria* [1763], par. 450, pp. 206-207. *Theoria* [1759b], CCCCXLV, p. 232.

[69] Voir la note 5 p. 147.

[70] *Theoria* [1763], par. 524, p. 247. *Theoria* [1759b], par. DXIX, pp. 278-279.

[71] *Theoria* [1763], par. 504, p. 235. «est admodum ardua si sine subsidiariis principiis, & gratuitis hypothesibus tractari debeat». Cf. *Theoria* [1759b], par. CCCCXCIX, pp. 264-265.

[72] *Theoria* [1763], par. 516, p. 243. *Theoria* [1759b], par. DXI, p. 274.

[73] *Theoria* [1763], par. 440, pp. 201-202. *Theoria* [1759b], par. CCCCXXXV, pp. 226-227.

à l'occasion de La Cène. Ce changement est connu sous le nom de «transsubstantiation» depuis Hildebert de Lavardin (1057-1136). Après le concile tridentin le dogme de la transsubstantiation a joué, dans l'Eglise, le rôle de pierre de touche pour déterminer l'orthodoxie de toute théorie de la matière, nouvelle ou ancienne [74]. Pour Boscovich, comme autrefois pour Descartes [75], il était, par conséquent, chose impérieuse de mettre sa nouvelle théorie à l'épreuve, surtout comme l'hylémorphisme fit encore toujours une partie intégrante de l'enseignement contemporain au Collège Romain. Par la suite, nous résumons les thèses principales de la doctrine de l'hylémorphisme et du dogme de la transsubstantiation, pour voir après ce qu'en dit Boscovich.

Dans la *Physique* (I.vii) Aristote avait développé ses vues sur les changements naturels en général. C'est là qu'il stipulait qu'il faut considérer un changement quelconque comme la transition d'un objet d'un état sans une certaine qualité à l'état avec cette qualité. Dans cette transition il y a quelque chose qui subit le changement proprement dit et c'est ce qu'Aristote appelle l'«*hylè*». Outre ce principe sous-jacent, il y a la «*morphè*» ou l'«*eidos*», qui au cours du changement se réalise dans tout objet naturel. Cette «*morphè*» est en quelque sorte la sommation des propriétés spécifiques d'un objet. Tout objet, appelé «*ousia*» par Aristote, est donc une unité de l'«*hylè*» et de la «*morphè*». Depuis le Moyen Age, une telle «*ousia*» était connue sous le nom de «*substantia*». Enfin, dans les termes modernes, tout objet naturel est donc considéré «substance», un animal pas moins qu'une plante ou un cristal [76]. Le changement que subit une telle «substance» est, pour ainsi dire, le développement qu'elle parcourt ou, plus précisément, la transition de sa puissance à l'acte. Ici, comme partout ailleurs, l'inspiration biologique de l'hylémorphisme est très visible.

Remarquons qu'Aristote, dans l'ouvrage *De la génération et de la corruption* (I,x), distingue différents modes de changement matériel, à savoir [77]: 1. la «*mixis*» ou, chez Thomas, la «*mixtio*»; 2. la combinaison de «*genesis kai phthoras*» ou, chez Thomas, de «*génératio et corruptio*»; 3. un processus sans nom propre, appelé «*augmentum*» par Thomas; 4. un autre processus sans nom, que Thomas appelle «*alteratio*». La «*mixtio*» est ce qui se passe lorsque deux corps de qualités contraires mais plus ou moins équipotentes sont mélangés; il en résulte un nouveau corps foncièrement homogène, dans lequel les qualités se sont moyennées, en quelque sorte. Dans le cas où l'on mélange une grande quantité de l'un corps avec une petite quantité de l'autre, le dernier est converti entièrement dans la nature du premier; on parle dans ce cas de «*corruptio*» (du dernier corps) et de «*generatio*»

[74] P. Redondi, *Galileo eretico*, Torino, 1983; traduit de l'italien par Mme M. Aymard sous le titre *Galilée hérétique*, Paris, 1985.

[75] J.R. Armogathe, *Theologia cartesiana. L'explication physique de l'Eucharistie chez Descartes et dom Desgabets*, La Haye, 1977.

[76] Cf. ci-dessus la note 17.

[77] Pour le texte grec, la traduction latine (de Guillaume de Moerbeke, c. 1220/35-1286) et le commentaire de Thomas Aquinas, voir *St. Thomae Aquinatis opera omnia, iussu Leonis XIII edita*, Romae, vol. II, 1882. Pour les différents modes de changement matériel, voir spécialement lectio 24 et 25, pp. XVIII-XXIII.

(du premier). Ce que Thomas appelle «*augmentum*» revient à l'incorporation de la nourriture par le corps d'un être vivant, tandis qu'une «*alteratio*» serait le changement de figure d'une substance donnée (par exemple, une quantité de cire qui acquiert une autre figure).

Dans la messe, pendant la consécration le prêtre fait changer le pain et le vin en le corps et le sang de Jésus Christ, en répétant les mots que Jésus lui-même aurait prononcés à La Cène et qui, depuis l'établissement de la tradition latine — c'est-à-dire depuis les années 370, sous le pontificat de Damasus I — sont représentés ainsi: «*Hoc est enim corpus meum*». Le grand problème a toujours été l'interprétation de la forme verbale «*est*» dans cette phrase. Au cours des siècles, beaucoup de mouvements oppositionnels s'etaient crus obligés de formuler une propre interprétation de ce mot-clef. Sa signification précise a joué également un rôle important pendant la Réforme et c'est pour cela que le concile de Trente y a consacrée une session entière, la treizième, en date du 11 octobre 1551. A cette occasion le dogme de la transsubstantiation fut accepté comme fondement de la doctrine de l'Eucharistie. Les canones principaux qui le concernent sont ainsi [78]:

> 1. Si quelqu'un nie, que dans le très saint sacrement de l'Eucharistie, soient contenus vraiment, réellement et substantiellement le corps et le sang avec l'âme et la divinité de Notre-Seigneur Jésus Christ, et par conséquent le Christ tout entier; mais s'il prétend qu'ils n'y sont qu'en signe ou en figure ou par leur vertu, qu'il soit anathème.
> 2. Si quelqu'un dit que, dans le très saint sacrement de l'Eucharistie, il reste la substance du pain et du vin avec le corps et le sang de Notre-Seigneur Jésus Christ, et nie cette merveilleuse et unique conversion de toute la substance du pain au corps et de toute la substance du vin au sang, qui ne laisse subsister que les apparences du pain et du vin, conversion que l'Eglise catholique appelle du nom très approprié de transsubstantiation, qu'il soit anathème. [...]

[78] Pour le texte latin des canones, voir *Concilium tridentinum diariorium actorum epistularum tractatum. Nova collectio*, edidit Societas Goerresiana, tome VI (Actorum pars quarta), vol. I, Freiburg im Breisgau, 1961, pp. 203-204: «1. Si quis negaverit, in sanctissimo Eucharistiae sacramento contineri vere, realiter et substantialiter corpus et sanguinem una cum anima et divinitate Domini nostri Iesu Christi, ac proinde totum Christum, sed dixerit tantummodo esse in eo ut in signo vel figura aut virtute: anathema sit. 2. Si quis dixerit, in sacrosancto Eucharistiae sacramento remanere substantiam panis et vini una cum corpore et sanguine Domini nostri Iesu Christi, negaveritque mirabilem illam et singularem conversionem totius substantiae panis in corpus et totius substantiae vini in sanguinem, manentibus duntaxat speciebus panis et vini, quam quidem conversionem catholica ecclesia aptissime transsubstantiationem appellat: anathema sit. [...] 8. Si quis dixerit, Christum in Eucharistia exhibitum spiritualiter tantum manducari et non etiam sacramentaliter ac realiter: anathema sit». Pour la traduction française des canones, voir A. Michel, *Les décrets du concile de Trente*, partie I, Paris, 1938, in C.J. Hefele, J. Hergenroether, H. Leclerq, *Histoire des conciles d'après les documents originaux*, vol. X, p. 271 (canon 1), p. 273 (canon 2), p. 280 (canon 8). Dans le canon 1 le texte latin utilisé par Michel dévie de celui présenté par la Société Görres; c'est pour cela que nous avons adapté la traduction.

8. Si quelqu'un dit que le Christ présenté dans l'Eucharistie, est mangé spirituellement, et non pas aussi sacramentellement et réellement, qu'il soit anathème.

Abstraction faite des problèmes concernant l'utraquisme [79], l'explication de ces canones selon l'orthodoxie hylémorphiste est ainsi. Avant la consécration la hostie n'est qu'un morceau de pain, avec toutes les qualités du pain. La hostie est une «substance» et en tant que telle, une unité de «matière» et de «forme». Comme toute autre «substance», elle consiste en les quatre éléments, dans une certaine proportion; ceux-ci n'y sont qu'en puissance, la «substance» étant foncièrement homogène. La nature de la «substance» est connaissable à travers les qualités perceptibles. La somme de ces qualités n'est tout de même pas identique à cette nature de par leur variation; elle constitue la soi-disant «forme accidentelle». Les qualités essentielles constituent la forme dite «essentielle». Au cours de la transsubstantiation, la substance du pain devenant la substance du Christ, cette «forme accidentelle» demeure, alors que la «forme essentielle» du pain périt et celle du Christ surgit. Après la consécration, la hostie «*est*», par conséquent, le corps du Christ, malgré toutes les apparences, c'est-à-dire, malgré toutes les qualités accidentelles du pain. *Mutatis mutandis*, ceci vaudra également pour le vin.

Il est évident qu'une théorie de la matière telle que l'atomisme, qui admet l'existence d'entités inaltérables, éprouvera de l'embarras devant le problème de la transsubstantiation. Selon l'atomisme, en effet, une «substance» est une unité d'un certain nombre d'atomes de certaines espèces, arrangés dans une structure spatiale caractéristique. Une conversion substantielle s'opérera, par conséquent, par l'addition et/ou la substraction d'atomes et/ou par un changement dans leur arrangement dans l'espace. Pour devenir le Christ il faudrait donc que les atomes du pain de la hostie sont échangés contre les atomes qui constituaient autrefois le corps du Christ, ce qui amène à des contradictions fondamentales. Bref: l'atomisme classique était dans l'impossibilité de rendre compte du dogme de la transsubstantiation. En plus, les objections des Pères Lactance et Augustin étaient encore valables. C'est pour cela que, depuis le concile de Trente, l'atomisme était encore plus suspect qu'il ne l'était déjà auparavant et ceci d'autant plus parce qu'il venait de reprendre son essor en tant qu'hypothèse de travail dans les sciences de la nature.

Au XVIIIe siècle cette situation n'avait guère changé. Du point de vue de l'Eglise, elle s'était aggravée même, car entre-temps l'atomisme était devenu l'une des idées directrices dans les sciences. Du même point de vue, la tentative de Boscovich de réconcilier le dogme ecclésiastique avec l'état actuel de la science de son époque par une nouvelle théorie atomistique, ne pouvait être jugée autrement que téméraire et par-là même tout à fait superflue. Son zèle devait également inquiéter ses supérieurs de la Compagnie de Jésus pour des raisons de politique intra-ecclésiastique: après tout, ce sont deux Pères de la Société qui s'étaient dinstingués

[79] L'«utraquisme» concernait une doctrine très populaire au XVIe sècle selon laquelle le sacrement de l'Eucharistie doit être administré aux fidèles sous les deux espèces. Après une ample discussion le concile de Trente se décidait contre cette doctrine.

lors de la formulation des canones principaux concernant l'Eucharistie, au cours de la XIIIe session du Concile de Trente [80].

Etudions donc ce qu'en dit Boscovich. Remarquons d'abord que, quant à la forme de sa justification, le savant jésuite choissit une confrontation indirecte. Ainsi, le terme théologique de «transsubstantiation» n'est pas dans la *Theoria*. Boscovich dit simplement que les péripatéticiens peuvent éventuellement assimiler ses points à l'«*hylè*» aristotélicienne et soutenir, que la loi des forces propre à un point ou à un groupement de points est la cause derrière les propriétés essentielles, à savoir la forme substantielle, la «*morphè*» d'Aristote [81]. En plus, les péripatéticiens, «s'ils voulaient» (*si velint*), seraient en droit d'admettre que les propriétés accidentelles d'une substance puissent subsister si la substance elle-même a péri. Ceci veut dire qu'une chose individuelle — c'est-à-dire une substance — peut être convertie en une autre chose individuelle sans que les apparences de la première chose disparaissent: par conséquent, pour les sens humains il n'y a aucun changement à enrégistrer. C'est précisément ce qui se passe lors de la soi-disant «transsubstantiation», mais Boscovich ne se laisse pas amener à une analyse appronfondie. Non seulement il évite l'usage du terme théologique, mais il renonce également à développer une explication adéquate. Il considère apparemment que c'est un problème qui relève uniquement de la théologie et ce qu'il fait simplement c'est suggérer aux théologiens une nouvelle voie. Dans le même but il y ajoute qu'en cas de besoin on pourrait même admettre plusieurs espèces de points, assujetties à plusieurs lois de forces. Ansi, les péripatéticiens — lisez: les théologiens — «s'ils voulaient» (*si velint*), pourraient admettre l'existence d'une matière parfaitement homogène et des formes substantielles d'ordre diverse [82]. Il se pourrait même qu'il y a différents mondes matériels et sensibles dans le même espace, des mondes entre lesquels il n'y a pas de communication de par leurs profondes différences [83].

Toutefois, dans notre monde phénoménal la nature d'un corps est déterminée par le nombre et la disposition spatiale des points [84]:

> Etant donnés le nombre et la disposition des points dans une masse donnée, l'origine de toutes les propriétés qu'a cette masse elle-même, ainsi que l'origine de tous les rapports que celle-ci doit avoir avec d'autres masses — rapports qui détermineront les modalités [quantitative et qualitatives] des combinaisons — et la cause de tous les changements qu'elle peut subir, sont connues.

[80] H. Jedin, *Geschichte des Konzils von Trient*, vol. III, Freiburg-Bâle-Vienne, 1970, p. 270.

[81] *Theoria* [1763], par. 516, p. 243; *Theoria* [1759b], par. DXI, p. 274.

[82] *Theoria* [1763], par. 517, pp. 243-244; *Theoria* [1759b], par. DXII, pp. 274-275.

[83] *Theoria* [1763], par. 518, p. 244; *Theoria* [1759b], par. DXIII, pp. 275-276.

[84] *Theoria* [1763], par. 519, pp. 244-245: «Dato numero et dispositione punctorum in data massa, datur radix omnium proprietatum, quas habet eadem massa in se, et omnium relationum, quas eadem habere debet cum aliis massis, quas nimirum determinabunt numeri, et combinationes, ac motus earum, et datur radix omnium mutationum, quae ipsi possunt accidere» (cf. *Theoria* [1759b], par. DXIV, p. 276).

Boscovich distingue les propriétés essentielles d'un corps de ses propriétés accidentelles: la combinaison des qualités les plus constantes (fluidité, élasticité, mollesse, goût, odeur, couleur, etc.) déterminera l'espèce. L'admixtion de corps étrangers donne lieu à des qualités variables et partant accidentelles [85]. A base de ce couple de concepts Boscovich distingue nommément deux sortes de conversions matérielles: la «*transformatio*» et l'«*alteratio*». Une «*transformatio*» concerne un changement dans les propriétés spécifiques et constantes. Une telle conversion s'opérera, selon Boscovich, toujours de la même manière, c'est-à-dire, dans des conditions semblables. Elle affecte la «structure» (*textus*) des particules. Le (ou les) corps de départ périt (périssent) et est (sont) transformé(s) en un ou plusieurs nouveaux corps. Si plusieurs corps de départ se combinent par une nouvelle liaison entre leurs particules, il en résulte un nouveau «*mixtum*»; si, inversement, les constituants des particules d'un «*mixtum*» sont résolus, ils entrent dans une ou plusieurs nouvelles compositions. Dans les deux cas il est question d'une transformation.

Une «*alteratio*» serait, d'après Boscovich, un processus s'effectuant sous permanence des qualités spécifiques. Il cite l'exemple du figement et celui de l'ébullition. Avec une allusion très évidente à Thomas et Aristote, il compare ces transitions avec les changements de figure que peut subir le cire. Dans la doctrine de l'hylémorphisme cependant la transition de l'eau en vapeur avait toujours été l'exemple d'une corruption (de l'eau) suivie d'une génération (du vapeur), l'eau et le vapeur étant considérés comme des substances différentes. Mais Boscovich ne discute point les topiques de la philosophie scolastique. Ce qu'il offre à ses lecteurs, c'est en quelque sorte une terminologie scolastique pour couvrir les acquis de la science de son époque, sans corrompre le moindre détail de cette science contemporaine. Delà le terme d'«*alteratio*» pour ce que Venel, dans l'*Encyclopédie*, aurait regardé comme un processus exclusivement physique, et le terme de «*transformatio*» pour un processus ultimement chimique. Delà les concepts de propriétés essentielles et propriétés accidentelles pour distinguer une substance pure d'une substance contaminée, autrement dit pour différencier entre «corps purs» et «mélanges mécaniques», comme il était devenu la coutume dans la tradition stahlienne.

4. *Conclusions*

Quant à la théorie de la matière Boscovich offre une accrétion des vues de Newton et de Leibniz autour d'un noyau d'idées propres à lui même (atomes points; loi des forces). Quoique sa terminologie est d'origine scolastique, les idées défendues sont les siens ou celles de la science naturelle de son époque, ces dernières, pour ce qui concerne la physico-chimie de la matière, remontant pour la plupart à Stahl (*elementa*, *mixta*; *alteratio*, *transformatio*). Ni le nom de Stahl, ni celui de son protagoniste Venel, figure d'ailleurs dans la *Theoria*.

[85] *Theoria* [1763], par. 520, p. 245; *Theoria* [1759b], par. DXV, pp. 276-277.

Il n'est donc pas question d'une tentative de sauvetage de l'hylémorphisme. Par contre, ce que Boscovich propose est considéré par lui même comme une solution adéquate de nombreux problèmes fondamentaux présentés par la science contemporaine (qualités perceptibles des corps et leur comportement physico-chimique; figure spécifique des cristaux, des animaux et des plantes). Il est équitable de constater ici l'originalité et la fécondité éventuelle de l'atomisme de points. Il ne faut pas perdre de vue cependant qu'il s'agit d'une théorie qualitative: Boscovich ne donne pas une fonction algébrique vérifiable pour sa loi de forces, mais se borne à un traitement descriptif — parfois excessivement spéculatif —, tout à fait comme son idole Newton dans la Query 31 de l'*Opticks*. Par rapport à l'*Opticks*, du reste, le style aussi obsolète que parfois orgueilleux de la *Theoria* saute aux yeux.

Les temps avaient changé. Encore les présages étaient mauvaises. L'année même de la parution de la *Theoria* était aussi celle de la réapparution prédite du comète de Halley, phénomène qui ne tardait pas à catalyser la mathématisation des sciences de la nature. On allait à la recherche d'une mathématique universelle. C'est pour cela que l'aspiration de Boscovich à une théorie universelle de la matière devait s'avorter.

DOCUMENTATION

Nouveau : le *Catalogus* [...] *librorum* d'Isaac Beeckman (*)

Récemment nous avons trouvé dans la *Bibliotheca Angelica* (Rome) le catalogue — jusqu'ici inconnu — de la vente aux enchères de la bibliothèque d'Isaac Beeckman. Ce catalogue fut établi quelque temps après la mort du savant, survenue le 19 mai 1637, et porte le titre suivant :

CATALOGUS / Variorum & insignium Librorum Clarrissi- / mi Doctissimique viri / D. ISAACI / BEECKMANNI / Praestantissimi Medici, Philosophi atque Mathe- / matici a [c]utissimi, Sche[<o]lae Du[<o]rdracaenae / Rectoris vigilantissimi. / Quorum Auctio habebitur in aedibus defuncti / ad diem 14 [nombre manuscrit] *Iulij MDCXXXVII.*

Il fait partie d'une liasse de 18 catalogues du même type dressés entre 1636 et 1650 concernant des savants des Pays-Bas. Il s'agit de Johannes Fabritius Deuringh (Franeker, le 16 mars 1646), Petrus Horenius (Groningue, le 31 juillet 1650), Suffridus Sixtinus (Amsterdam, le 7 juin 1650, n.s.), Abrahamus vander Mylius (Dordrecht, le 7 juillet 1637), Nicolaus Mulerius (Groningue, le 23 novembre / 3 décembre 1646), Franciscus Gomarus (Leyde, le 4 octobre, n.s.), Fridericus Spanhemius (Leyde, le 22 novembre 1649, n.s.), Mathaeus Vossius (Amsterdam, le 10/20 mai 1647), Jacobus Rooverius (Dordrecht, le 6 août 1641), Iohannes Elichmannus (Leyde, le 24 mai 1640, n.s.), Guilielmus Staackmans (Franeker, le 24 mars 1645), Rudolphus Schuiring (Gorningue, 1649), Dirck Alberts (Leeuwarden, le 20 juillet 1646), Hemmo Huninga (Groningue, 1650), Justinus van Assche (Rotterdam, le 13 juin et jours sui-

(*) Nous remercions très sincèrement M. E. Coumet (EHESS, Paris), le R.P. P. Costabel (EHESS, Paris) et M. T. Gregory (Lessico Intelletuali Europeo, Rome) de leur bienveillant accueil, ainsi que la direction de la *Bibliotheca Angelica* pour son aide efficace.

vants 1650), Daniel Castellanus (Amsterdam, le 31 mars 1637), Eido Campegius (Franeker, le 25 août 1645) et Daniel Colonius (sans lieu, le 23 septembre 1636, n.s.). La liasse porte sur le dos *Miscellanei CCXCI* ; elle fait partie de la très riche collection de livres léguée par le cardinal Domenico Passionei (1682-1761) à la *Bibliotheca Angelica.*

A première vue, la bibliothèque de Beeckmann ne semble pas avoir été très impressionnante. Elle compte au total 566 livres, dont 161 *libri theologi*, 50 *libri medici*, 82 *libri philosophi*, *historici*, etc., et 273 *miscellanei.* On a l'impression qu'elle a été pillée avant l'établissement du catalogue : il ne reste que très peu de textes mathématiques, et de nombreux ouvrages contemporains font également défaut. Au contraire, la plupart des sources dont Beeckman s'est servi lors de l'élaboration de sa théorie d'*homogenea physica* (1612-1620) semblent avoir survécu au pillage.

Signalons, par ailleurs, que le quatrième centenaire de la naissance de Beeckman a été commémoré, aux Pays-Bas, par une exposition intitulée *Substance et plan : Beeckman et Escher.* Cette exposition, faite sous les auspices de la *Société royale de Chimie des Pays-Bas* à l'occasion de son congrès d'été (Delft, 25 et 26 août 1988), mettait en relief quelques remarquables ressemblances dans les idées de Beeckman et de l'artiste graphique Maurits Cornelis Escher (1898-1972) sur la composition du continu physico-chimique (les substances) et mathématique (le plan), respectivement.

H. H. Kubbinga.

Université de Groningue, EHESS Paris.

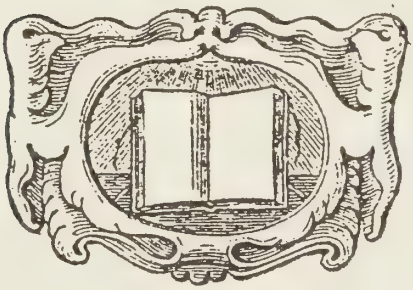
CATALOGVS

Variorum & insignium Librorum Clarissimi Doctissimique viri

D. ISAACI BEECKMANNI

Præstantissimi Medici, Philisophi atque Mathematici autissimi, Schelæ Durdracænæ Rectoris vigilantissimi.

Quorum Auctio habebitur in ædibus defuncti ad diem 1 [illegible] Iulij cIↃ ↃIC XXXVII.



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THE FIRST "MOLECULAR" THEORY (1620): ISAAC BEECKMAN (1588–1637)*

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ABSTRACT

The first "molecular" theory of phenomenal matter (be it avant la lettre) was conceived in the period 1612–1620 by the Dutch natural philosopher Isaac Beeckman (1588–1637). According to Beeckman, all matter is an aggregate of so-called "physical homogenea" ("homogenea physica") of varying stability. So, the homogenea of substances such as metals and medicines are quite stable, while those of light and sound, for instance, are far less so. Beeckman's homogenea are supposed to be composed of four kinds of atoms of one and the same prime matter, these atoms being united in specific numbers into a specific structure. The Epicurus batavus compared his homogenea with biological individuals. Like biological individuals of the same species, similar homogenea are not perfectly identical. They are taken as substantial individuals and manifest as such a variability comparable to that of their biological counterparts. Moreover, as substantial individuals they are the necessary and sufficient condition for the existence of a specific substance. Beeckman's concept of "molecule" can rightly be considered as one of the major achievements of the so-called Scientific Revolution of the 17th century.

INTRODUCTION

In my Ph.D. thesis I studied the historical development of the concept of "molecule" in natural science up to the end of the 18th century [1]. One of the results of my research has been the discovery of the "molecular" theory of the Dutch natural philosopher Isaac Beeckman (1588–1637), apparently the first worth that designation [2]. Beeckman developed his theory in the years 1612–1620, but did not publish it. He only wrote it down in a "scientific diary", which he kept to himself. The only three savants that were allowed to study his manuscripts were René Descartes (1596–1650), Marin Mersenne (1588–1648) and Martinus Hortensius (1605–1639), the first of these being the most influential in the long term. Indeed, in Descartes's works *Les météores* (1637; one of the three "essais" at the end of the *Discours de la méthode*) and *Principia Philosophiae* (1644) a corpuscular theory of matter was propounded that owed its origin to Beeckman's diary. Anyway, after Beeckman's death, in 1637, the

*Avunculo optimo H.H. Fikkers.

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manuscript of the diary was lost and it was only in 1905 that it was found in the archives of the Provincial Library of Zeeland (the actual Zeeuwse Bibliotheek), at Middelburg, Holland. Thanks to the tenacity of its finder, the historian of science Cornelis de Waard (1879–1963), the manuscript appeared in press between 1939 and 1953, in four monumental volumes [3].

Beeckman's revolutionary doctrine was the solution of a key problem in classical and mediaeval natural philosophy, viz. that of the finite or infinite divisibility of phenomenal matter. From a modern point of view it is difficult to understand that ancient natural philosophers were unable to distinguish, in an epistemologically consistent manner, between natural objects such as "a nugget of gold" and "a greyhound", for instance. Both were generally considered as individuals belonging to some kind of species. Of course, one knew that the nugget of gold was divisible, but the crucial problem was how to define the limit below which the nugget stopped being gold. In the history of natural philosophy three traditions can be distinguished that have dealt with this problem: alchemy, hylemorphism and atomism. Though the alchemists had, as Hooykaas demonstrated [4], by far the most sophisticated ideas on the structure of matter in general and of the metals in particular, their solution was inadequate. Where the alchemists fell short, hylemorphism, following Aristotle (384–322 B.C.), and atomism, following Epicurus (341–270 B.C.), were bound to fail. For an atomist and an hylemorphist each natural object was something individual belonging to a certain species. That is to say for both natural philosophers the object loses its nature when it is divided, equally so for a greyhound and a nugget of gold. By a curious coincidence, the first "molecular" theory of matter arose as the fruit of an application of the main tenets of atomism to the doctrine of the elements of hylemorphism.

In the following I present a synopsis of Beeckman's theory. The concluding paragraph offers a brief outline of the further development of the "molecular" theory up to the beginning of the 20th century, when it was unequivocally demonstrated that "molecules" really exist [5].

BEECKMAN'S "HOMOGENEA PHYSICA" AS SUBSTANTIAL INDIVIDUALS

Beeckman's starting point seems to have been his conception of the communication between human individuals and the objects that surround them. This communication would be brought about by more or less stable particles of flavours, odours, sound, light, and the like, in much the same way as modern chemists and biologists imagine the communication between animals and/or men by means of pheromones (between animals of the same species) and allelochemics (between animals of different species) [6]. The particles of light and sound were supposed to be less stable than the other, the latter being able to endure in the form of a "heap" or "aggregate". The characteristic particles of a substance – or a phenomenon, what's in a word?! – were called "physical

homogenea" ("homogenea physica") (14 September 1620; see Fig. 1) [7]. It is against this background that Beeckman's view should be projected for a correct understanding.

In the period between 1612 and 1620 Beeckman elaborated his theory of matter while studying and commenting on the didactical poem *De Rerum Natura* of Titus Lucretius Carus (ca. 95–ca. 55 B.C.) and some works of the physician Galen (129/130–199/200 A.D.). Lucretius's poem can be described as a fascinating panegyric of classical atomism, in a form received from Epicurus. The works of Galen, a true follower of Aristotle, studied by Beeckman concerned primarily the constitution of the human body and the composition and faculties of medicines. That Beeckman studied Galen was otherwise quite natural for a philosopher of the early 17th century: at that time philosophy meant natural philosophy and one was hardly allowed to speculate about nature without a doctorate in medicine. In other words: the study of medicine was the only academic route to natural science.

Beeckman borrowed his ideas about the constitution of the human body from Galen, ideas that were to play an important role in his later theory of matter. According to Galen, the human body is composed of four humours (blood, phlegm, yellow bile and black bile) which, by their proportion, determine the quality of the human being under consideration and by their temporary deviation from a mean value cause the physical affections (hunger, illness, etc.). These four humours are made from four really ultimate elements namely the classical four: Earth, Water, Air and Fire. A determinate proportion between the humours thus corresponds to an equally determinate proportion between the elements. Galen adopted his theory of the elements from Aristotle, who had maintained that all natural substances are composed of them. With Aristotle, Galen believed that all four elements should always be present in any substance whatsoever, and in a characteristic proportion. These ideas were taken up by Beeckman, together with the following:

(1) The specific proportion between the four humours and, ultimately, between the four elements, is subject to some slight variation. This explains why individuals of the same species – and, consequently, with the same mean proprotion – differ inter se and why the bodily condition of one individual is not always the same. Thus, each member of the proportion Earth : Water : Air : Fire has a small domain of variation; this can be expressed as follows
Earth : Water : Air : Fire $= (E \pm a) : (W \pm b) : (A \pm c) : (F \pm d)$ where $E : W : A : F$ represents the mean and/or optimal value of the elemental proportion.

(2) Though the mean and/or optimal humoral or elemental proportion is characterized by a certain domain of variation to explain the variability of individuals, this proportion is discrete, that is to say there is no overlap between the proportions of, for instance, a man and a lion. This becomes evident from the fact that there are no transition forms between biological species, no way in between man and lion. Biological species are, so to say, discrete entities.

(3) Deviations from the mean and/or optimal proportion can be cured, at least in principle, by the addition and/or substraction of short- and over-mea-

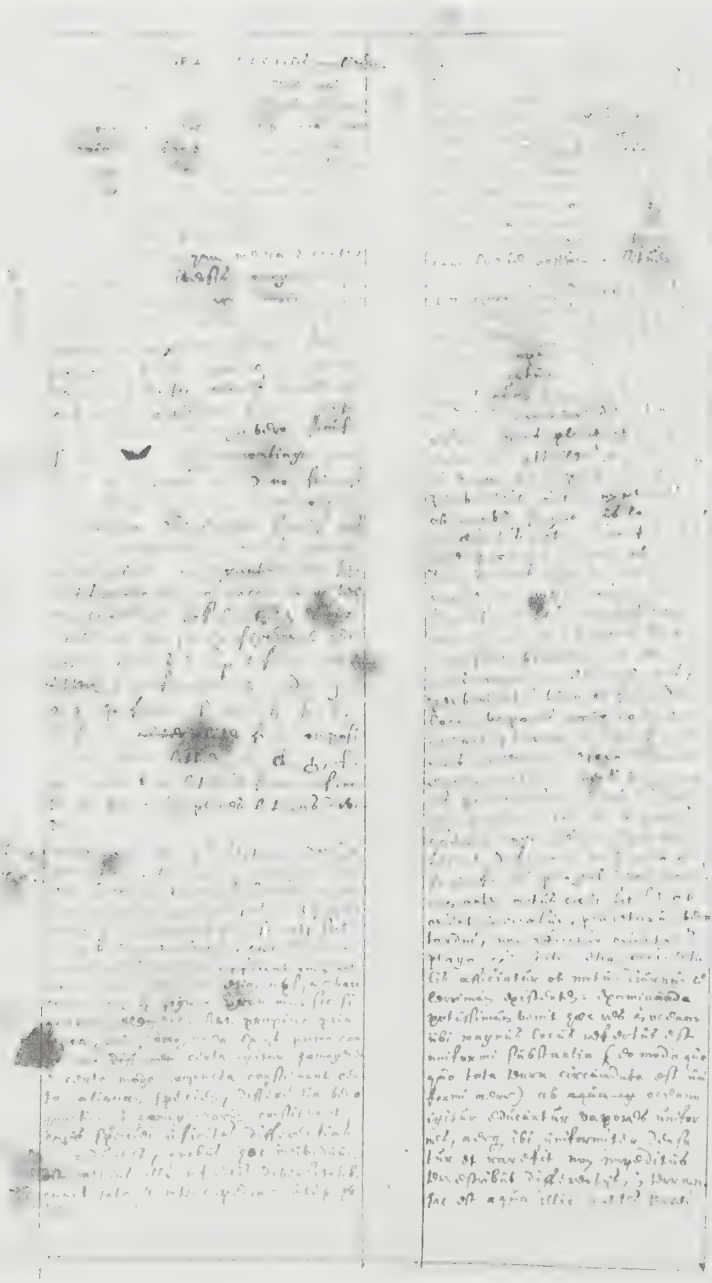

Fig. 1. Photo of folio 146.bis.v. from the manuscript of Beeckman's diary. It contains the crucial fragment of 14 September 1620, in which the Epicurus batavus summarized the main tenets of his revolutionary "molecular" theory (last paragraph column left, first paragraph column right). Reproduced with kind permission of the owner of manuscript, the Zeeuwse Bibliotheek, Middelburg, The Netherlands.

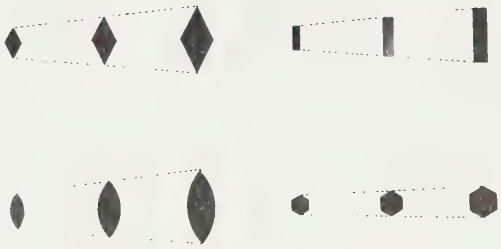

Fig. 2. The four atomic species – Earth, Water, Air and Fire – of Beeckman with their domain of variation. I have reduced the differences between similar atoms to differences in magnitude (the chosen figures are completely arbitrary). Beeckman followed Epicurus (341–270 B.C.) in that he maintained that atoms of the same kind manifest a certain variability, just like grains of corn or shells of molluscs of the same species.

sure, respectively. The working of cures (e.g. blood letting) and remedies should be judged from this point of view.

It is particular to Beeckman that he has attempted to translate Galen's medical views in atomistic terms, which is the more remarkable because Galen himself had been, like Aristotle, a convinced adversary of atomism.

According to Beeckman, the four elements are four kinds of atoms of one and the same prime matter. Each element has its own peculiar figure and size which, however, are not specified by Beeckman. With Lucretius, who followed Epicurus closely, he compares the atoms with macroscopic objects. Like grains of corn or shells of molluscs, Lucretius had suggested, similar atoms are not completely identical. They manifest variability in the only two properties they possess (form and magnitude), in the same way as macroscopic objects of the same species have in all their properties (form, magnitude, colour, length of hair, sound, etc.). For each property there is a certain maximum and minimum value, which, in the case of a biological individual, is reflected in the domain of variation of the elemental proportion. Translated in atomistic terms, this means that for each kind of atom there is also a maximum and a minimum between which figure and magnitude can vary (Fig. 2).

Beeckman's atoms are the ultimate causes of the properties of all substances (colour, taste, smell, etc.), not unlike the modern concept of atoms. However, following the Ancients, Beeckman thought that the observable properties arise from a mixture of a limited number of basic qualities manifested by all substances. That is why he claimed that all four kinds of atoms are necessary to constitute a substance perceptible to the human senses, or, inversely, that any substance whatsoever must contain all kinds of atoms.

At this stage is could be objected that Beeckman's atomistic interpretation of the doctrine of elements of Galen and Aristotle, though perhaps original in itself, is not a real advancement with respect to classical natural philosophy,

Fig. 3. Some types of "physical homogenea" (i.e. molecules) formed from the four elements. Note that all four kinds of atoms are present in each type of homogeneum. The substantial species is defined by the number of atoms of each kind together with the structure in which these atoms are united. The homogenea are considered as substantial individuals, that is: as the necessary and sufficient condition for the existence of a substance.

and this cannot be denied. Indeed, the key problem, that of the finite or infinite divisibility of matter, is not brought nearer to a solution. Moreover, others such as Empedocles (ca. 492–ca. 432 B.C.), Plato (427–348/7 B.C.) and more recently Geber (end of 13th century A.D.) had propounded similar ideas. However, by framing a "molecular" theory on the base of his elemental atomism Beeckman definitely surpassed the Ancients [8]. In a way he thus personifies the ideal type of the Renaissance man depicted by Jakob Burckhardt (1818–1879) in his work *Die Kultur der Renaissance in Italien* (1860). However, though highly individualistic in his diary "imitatio" and "aemulatio" of the scientific forebears, Beeckman was lacking both the selfishness and publicity seeking tendency of the Italian literary renaissancists, and the craving for intellectual recognition manifested by contemporaries such as Galileo Galilei (1564–1642) and René Descartes. If we dare to call Beeckman the Epicurus batavus, therefore, this obvious anachronism is simply an act of historical justice. Let us now analyse first the precise content of the "molecular" atomism that makes him worth this title.

According to Beeckman, the homogenea, i.e. the molecules of the compound bodies and thus of all perceptible matter, are made from the four kinds of atoms. Each type of homogeneum and thus each kind of substance was characterized by specific numbers of all four atomic species and an equally specific spatial arrangement (Fig. 3). No two homogenea of one substance, however, are completely identical: as a consequence of the variability of the atoms, the homogenea too manifest variability (Fig. 4). This appears to be an implication of the fundamental analogy on which Beeckman had built his "molecular" theory, namely the analogy between the extremely small characteristic particle

Fig. 4. Some physical homogenea of the same substance. Beeckman compared such homogenea with biological individuals. Like biological individuals, homogenea manifest variability: no two of them are perfectly identical, because the composing atoms vary slightly. However, the structure is roughly the same.

of a substance and the human, or more generally, the animal body. Apparently, Beeckman took his homogenea in the sense of substantial individuals. They are, in other words, the necessary and sufficient condition for the existence of a substantial species. However, substantial species, in analogy with the animal world, are considered to be discrete entities (Fig. 6). The metals, for instance, are as discrete as animals in the sense that there are no transition forms between any two of them (e.g. tin and copper), as there is none between men and lions. In order to make this plausible, Beeckman compares the atoms with equilateral triangles in their relation to the regular polyhedra. From such triangles only three regular polyhedra can be built: the tetrahedron, the octahedron, and the icosahedron. Indeed, only four, eight or twenty such triangles can give a polyhedron that rightly deserves the name 'regular'. For the same reason, Beeckman argues, atoms are unable to give rise to anything whatsoever; they can only constitute discrete kinds of homogenea, representing discrete substances. Seeming transition forms ought to be regarded, therefore, as mixtures of two or more pure substances; Beeckman mentions the alloys as an example. It is otherwise very enlightening to see how far Beeckman had taken his conception of matter. Four points merit attention: the importance of the structure

Fig. 5. "Isomerism" (avant la lettre) with Beeckman: a change of structure suffices to give another type of homogeneum representing another substantial species.

THESES
DE FEBRE TERTIANA
INTERMITTENTE.
QVAS
DEO OPT. MAX. ANNVENTE

Ex decreto & authoritate ſcholæ Medicæ, & nobilis viri Dionyſij de Vandes, Medici Regij, in alma Cadomenſi Academia facultatis Medicinæ Decani, diſputandas proponit

Iſack Beeckman Mittelburgo-Zeelandus ad diem menſis Septembris, anni 1618.

Pro ſupremo gradu Doctoratus in Medicina conſequendo.

CADOMI,

Ex typographia IACOBI BASSI Regis, & Academiæ Typographi.

Fig. 6. Only two natural philosophical treatises carrying Beeckman's name were published during the 17th century. The first was his doctorate thesis (Caen, France; 6 August, 1618), of which only three pages – of one and the same copy – have survived (The British Library, London; shelf number 1179, d 9 (3)). Its title page is reproduced above (borrowed from ref. 3, volume IV, pp. 42). As the title indicates the theme of the thesis was the so-called quartan fever, some kind of malaria characterized by the occurrence of a paroxysm every fourth (in modern reckoning, every third) day and in Beeckman's time endemic in the marshy districts of western Europe. It is illuminating to see that one of the arguments for the discrete character of substances was inspired by the observation that there exist discrete forms of malaria, each with its own fever periodicity. In much the same way as there is no transition form between quartan and tertian fever (or between men and lions), there is none between, say, gold and silver (ref. 3, volume I, pp. 32–33, fol.157.r-157.v, 21 March 1620; ibid. volume II, pp.127, fol. 146. bis.r.–146.bis.v., 13 September 1620).

of an homogeneum, the precise number of the elements, the bonding of the atoms, and, finally, the cosmological background.

As we saw above, the structure of an homogeneum is, for Beeckman, one of the (two) factors determining its species and he was perfectly conscious of the fact that a change in structure would suffice to make a new type of homogeneum and thus a new kind of substance (Fig. 5). This clearly implies what is called (since Jöns Jacob Berzelius (1779–1848)(1831) [9], "isomerism". Beeckman was not at all dogmatic regarding the precise number of the elements. He considered that his adoption of the classical four was but a concession to natural philosophical tradition and, moreover, a rather unimportant one. Self-confidently he states that he has demonstrated that no more than four elements are necessary to explain all qualities of perceptible bodies. However, another number of quality-causing atomic species would not affect the epistemological value of Beeckman's molecular atomism, provided, of course, that these atomic species were assumed to be the cause behind all phenomenal qualities of matter.

The interconnection of the atoms, point three above, had always been a delicate part of the ancient doctrine of atomism. In antiquity the atoms were supposed to have hooks and eyes, so that they could cohere to form a more or less stable macroscopic object. This image, apparently, did not satisfy Beeckman. In his opinion, atomic bonding is the result of a contact action between the surface of adjacent atoms: they would cohere in the same way as two well-polished marbles brought into contact. As the surface of a particle in proportion to its volume is greater for smaller particles than for bigger ones, Beeckman triumphantly concludes that the cohesion of atoms, i.e. the smallest possible particles, will be maximal.

Like ancient atomism, Beeckman's new "molecular" atomism has cosmological dimensions. A short discussion of this point rounds off the survey. According to Beeckman, the atoms were created by God in the beginning "out of nothing" (ex nihilo). They received on that occasion such a form, magnitude and movement that their conduct – their future doings, so to speak – is lawfully determined. He could thus maintain that figure, magnitude and movement of the atoms reflect God's providence: there is nothing fortuitous in the movements of the atoms and, therefore, in the sequence of events in the cosmos and, by implication, in the course of human life. With Beeckman, who was a tolerant but firm Christian of an orthodox protestant orientation, theology and cosmology are discretely related. His candour in dealing with these delicate problems in the intimacy of his diary contrasts sharply with the cautiousness of more publicity minded contemporaries such as Galileo Galilei (1564–1642) and René Descartes. In Italy, where, in 1600, Giordano Bruno (1548–1600) was burnt at the stake for his atomistic interpretation of nature, atomism was judged heretical, especially since at the Council of Trente (1545–1563) the doctrine of transsubstantiation was made the touchstone of Roman Catholic

7

XX.

Nonnunquam etiam vomitus excitandus, hydrotica, diuretica adhibenda, prout naturæ motus ex proprijs signis præoſtendet.

Corollaria.

Aqua ſuctu ſublata non attrahitur vi vacui, ſed ab aere incumbente in locum vacuum impellitur.

Eſt vacuum rebus intermixtum.

Quas vocant optici ſpecies viſibiles ſunt corpora.

Lapis ex manu emiſſus pergit moueri non propter vim aliquam ipſi accedentem, nec ob fugam vacui, ſed quia non poteſt non perſeuerare in eo motu, quo in ipſa manu exiſtens mouebatur.

Ditonus conſonantia non conſiſtit in proportione 9. ad 8. duplicata. Sed vt 5. ad 4.

Quodlibeta.

Sol movetur & terra quieſcit, aut terra mouetur & ſol quieſcit.

Circulus poteſt perfectè quadrari, aut contra.

FINIS.

Soni materia, quæ auribus ingreditur auditum motura est ille idem numero aer, qui erat in ore loquentis.

Fig. 7. The last page, number 7, of the British Library copy of Beeckman's doctorate thesis. Perhaps as C. de Waard, the editor of Beeckman's diary, suggested (ref. 3, volume IV, p. 42), it was Beeckman's own copy, because the handwriting in the subscription corresponds to that in the diary manuscript, in a fragment about the contents of the thesis (folio 83.recto, 9 July-(midst of August) 1618). The latter handwritten addition is noteworthy. The transcript reads as follows: "Soni materia, quae auribus ingreditur auditum motura est ille idem numero aer, qui erat in ore loquentis". This means: "The matter of sound that enters the ears and will move the auditory organs, is quantitatively equal to (the matter of) the air that was originally in the mouth of the speaker". Stripped of its jargon this says that the mouth of a speaker emits sound homogenea, generated from the homogenea of air by the vocal chords, and these sound homogenea reach the ears of the public where they are registered. It should be remembered that, according to Beeckman, sound belonged, along with light, magnetism, music, and the like, to the so-called infimae species, i.e. the lowest species of individual being, just like the chemical substances. That chemical substances occur as such, i.e. as aggregates of homogenea, is a question of stability. That is to say, the homogenea of sound, etc., are far less stable than those of chemical substances, and so the former do not exist as visible aggregates.

D. ISAACI BEECKMANNI

Medici, & Rectoris apud

DORDRACENOS

MATHEMATICO-
PHYSICARVM

Meditationum, Quæſtionum, ſolutionum

CENTVRIA

TRAIECTI ad RHENUM,

Apud PETRUM DANIELIS SLOOT,
M. DC. XLIV

Ex Bibliotheca Viri Illuſt. Iſaaci Voſſii. (43)

Fig. 8. The title page of the anthology from Beeckman's diary manuscript, posthumously composed by his brother Abraham (1607–1663) (Utrecht, 1644). The only known copy of this booklet was formerly in the possession of Isaac Vossius (1618–1689), a renowned Dutch bibliophile who bequeathed his collection in his last will to the University Library of Leyden. It is still in Leyden (shelf number: 534 F 7) (reproduced with kind permission of the University Library of Leyden). The word homogeneum is used in only one of the hundred problemata dealt with (number 88). Curiously, there is otherwise no reference to the doctrine of homogenea: none of the crucial fragments from the period 1618–1620 was selected.

orthodoxy. It has been maintained, very recently, by the Italian scholar Pietro Redondi, that Galilei's trial of 1633 could have been a fake, organized by pope Urban VIII to prevent his friend Galilei from being condemned, on the initiative of the Collegium Romanum of the Jesuits, to death at the stake for his atomistic views. Now, by that successful manoeuvre of Urban VIII, he was condemned, indeed, but only for his relatively non-malign geokinetic ideas, and not to death, but only to confinement to his house for life [10]. As is generally known, Galilei's vicissitudes of 1633 kept Descartes from publishing his cosmological treatise *Le Monde*, which was planned to appear at that time. If one compares Galilei's rather superficial conception of matter with that of Beeckman, the latter's merit becomes more evident; we already know that Descartes' views owed much to a study of the manuscript of Beeckman's diary, a study dated about 1630.

BEECKMAN IN HISTORICAL PERSPECTIVE: THE RISE OF MOLECULARISM

It turns out that a relatively large number of 17th century natural philosophers adhered to matter theories based on the idea of the substantial individual (e.g. Basso, Descartes, Huygens, Leibniz, and Newton), while others, though apparently familiar with the idea, were more equivocal (e.g. Gassendi and Boyle) [11].

The most fruitful application of the idea of the substantial individual can be found in the works of the German chemist and physician Georg Ernst Stahl (1660–1734), who developed, from 1683 onwards, a matter theory closely related in its starting points to that of Beeckman [12]. Stahl's doctrine influenced the 18th century debate to such an extent that even Lavoisier, his great adversary in the phlogiston battle, agreed with it. However, though the majority of chemists, mineralogists and crystallographers embraced Stahlian molecular atomism in the second half of the 18th century, the attention of these scientists was focused on a relatively small detail of Stahl's vast chemical doctrine. When this detail – the phlogiston theory – was identified by Lavoisier in the 1780s as a "mirror image" of reality and thus erroneous, the whole molecular theory, the core of chemical Stahlism, was abandoned with it.

With the advent of Dalton's ideas, however, Stahlian molecular atomism was reconciled with Lavoisier's doctrine of elements and, moreover, united with Newton's ideas on the interaction of substantial individuals and thus of molecules. This superior blend determined in its turn the 19th-century discussion on the nature of matter. Natural philosophical scruples about the epistemological stature of imperceptibly small, specific, secondary particles, about some inconsistencies in Dalton's premisses, and about notorious experimental stumbling blocks, severely impeded a rapid propagation of Dalton's molecular theory. At last, however, at the famous Congress of Karlsruhe in 1860, the first international chemical congress, a great number of problems were solved, with

especial thanks to the intervention of Stanislao Cannizzaro (1826–1910). After the establishment, at that congress, of a uniform system of atomic weights, the molecular theory started its irresistible advance in natural science as a whole, but particularly in organic chemistry, in those days chemistry's molecular branch par excellence. The image of the concept of molecule became sharper and sharper, by means of ideas of ever increasing resolving power: valency (1850–1860), structure (1860–1870), asymmetric atoms (1876) [13]. In the mean time molecular atomism, especially with the supporting evidence supplied by the kinetic theory of gases, had become nearly a pleonasm. Even the analogy between molecules and biological individuals was rediscovered and some chemists did not shrink from applying Darwin's doctrine of the survival of the fittest (1873) to the behaviour of molecules. It should not be forgotten, however, that there was until the beginning of the 20th century a stubbornly active anti-molecular movement, especially in France and Germany, where the thermodynamists vigorously opposed any deviation from true empiricism and, therefore, combated the molecular theory. The first definite proof of the factual existence of molecules was delivered by Jean Perrin (1870–1942) in 1908, on the basis of phenomena related to Brownian movement. Gradually, the thermodynamists, too, became convinced of the reality of molecules.

It should not be forgotten, that molecular theory is not the only one capable of explaining substantial differences between bodies. The other is lattice theory; according to this solid matter is a specific three-dimensional arrangement of particles with a repetitive character. The first manifestation of this theory is found in the work of the German natural philosopher Joachim Jungius (1587–1657) (ca. 1630). It was revived and refined in the course of the 19th century, especially by crystallographers. It fact, lattice theory is the natural complement to molecular theory; these theories of course coincide in the solid phase.

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