

**The Royal Society of Chemistry Historical Group (RSCHG)
Occasional Paper No. 10**

**The Ninth Wheeler Lecture
Burlington House, London
Presented on May 10, 2017**

**Woodward's Unpublished Letters:
Revealing, Commanding and Elegant. Part 2^{†,#}**

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Abstract: A collection of excerpts from letters written by R. B. Woodward to his friends, colleagues and others is presented. These are representative of his lengthy correspondence and illustrate many aspects of his personality and philosophies of life.

[†] Dedicated to the memory of Edgar Philip Wheeler, of whom too little is known but whose bequest adds continuing light and attention on a subject he favored, namely the history of chemistry. For a short biography of Wheeler, see the report by Bailey and Watson.¹

[#] This paper is a sequel to seven previously published papers in the "Words" series: "Gilbert Stork: In his Own Words and in the Musings of His Friends"² "Carl Djerassi: In His Own Words"³, "John D. Roberts: In His Own Words and Those of His Friends"⁴ and "Woodward's Words: Elegant and Commanding"⁵ and two related papers published in the IUPAC newsmagazine *Chemistry International*.^{6,7} Those papers celebrated Stork's and Djerassi's 90th birthdays, Roberts's 97th birthday, and the prose from R. B. Woodward's scientific publications. This paper celebrates the "literature" of R. B. Woodward as found in his unpublished letters and is Part 2 to its "academic twin," a paper recently published by this author on the same subject in *Helvetica Chim Acta*.⁸

Introduction

I happily begin my Wheeler Lecture by thanking the Historical Group of The Royal Society of Chemistry for their kindness and generosity in presenting me this, the Ninth Edgar Philip Wheeler Lectureship. As I look around this room in the glorious Burlington House, I feel the same warm glow as when I entered Burlington House for the first time just a few minutes ago. It is the glow of scholarship, tradition, achievement, and the gathering of scholarship at its best. This has been the meeting place of over 150 years of scientific and artistic output. Being here on this special day, with many of my friends and heroes, I am truly filled with joy and will leave today with a reinforced, even supererogatory commitment to my own scholarship. Thank you.

I must also mention that, as I was preparing for my two lectures this afternoon – one on Woodward's Unpublished Letters, the subject of this paper, and the second on the Development of the Woodward-Hoffmann Rules, a topic on which I have already published⁹ and will publish more in the coming several years – I was reminded of a wonderful photograph of R. B. Woodward that I saw recently for the first time. I emphasize "for the first time" because I've been collecting photographs, anecdotes, and historical data on Woodward for well over a decade. Every new photograph or story is an event of some magnitude in my professional life. That photograph shows Woodward, holding a glass of champagne with the widest of grins, brimming with joy. To be with you today here in London at the Burlington House, I feel similarly. Thank you.

The Wheeler Lectureship and the RSC Historical Group's Occasional Papers

With the help of Peter J. T. Morris, I have gathered together in Table 1 the data on all the previous Wheeler Lectures and the Occasional Papers, including educational backgrounds and primary professional affiliations. The first two Occasional Papers were written by Dame Mary Archer and Robert Anderson in 1997 and 2000, respectively. The third Occasional Paper was written by Seymour Mautskopf in February 2003. Mautskopf was also the first recipient of the Edgar Philip Wheeler Lectureship, which he presented a few months earlier on November 8, 2002, in Waltham Abbey. Robert Anderson gave the second Wheeler Lecture in April 2004 in Leiden, the only instance where this Lecture was presented outside the United Kingdom. Furthermore, Anderson's second Wheeler Lecture was published in 2006 in *Ambix*.¹⁰ Thereafter, the Wheeler Lecture and the Occasional Papers were directly and almost perfectly chronologically linked. "Table 1" notwithstanding, Morris has informed me that "the Occasional Papers have

nothing to do with the Wheeler Lectures except to serve as a vehicle for their publication. The series was not originally conceived with the Wheeler Lecture in mind, but rather arose out of the necessity to find some way of publishing Mary Archer's interview of Emeleus."¹¹

Figures 1-8 show portraits of the first eight Wheeler Lecturers. Figure 9 is a group photo with the Ninth Wheeler Lecturer and the other symposium speakers taken shortly before that Wheeler Lecture was presented on May 10, 2017, at Burlington House, London.



Figure 1. Seymour H. Mauskopf, the First Wheeler Lecturer, and his wife Josephine, Cinque Terre, Italy, 2004. Photograph courtesy S. Mauskopf.



Figure 2. Robert Anderson, the Second Wheeler Lecturer, Richmond, Virginia, October, 2016. Photograph by Troy Wilkinson courtesy Virginia Museum of History & Culture, Richmond, Virginia.



Figure 3. David Knight, the Third Wheeler Lecturer, receiving the Edelstein Award of the Division of History of Chemistry of the ACS, from Dave Abrahams, New York City, September 9, 2003. Photograph courtesy J. I. Seeman.



Figure 4. William H. Brock, the Fourth Wheeler Lecturer, 1994.
Photograph courtesy W. H. Brock.

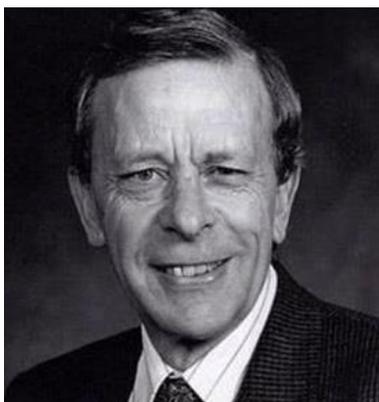


Figure 5. Colin Russell, the Fifth Wheeler Lecturer.
Photograph courtesy The Royal Society of Chemistry.

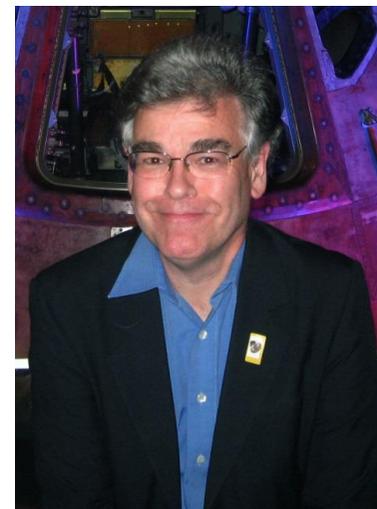


Figure 6. Peter Morris, the Sixth Wheeler Lecturer.
Photograph courtesy P. Morris.



Figure 7. Anthony S. Travis, the Seventh Wheeler Lecturer, receiving the Edelstein Award, flanked by Arnold Thackray (left) and Roger Egolf, then Chair of HIST, Boston, MA, August 21, 2007.
Photograph courtesy J. I. Seeman.

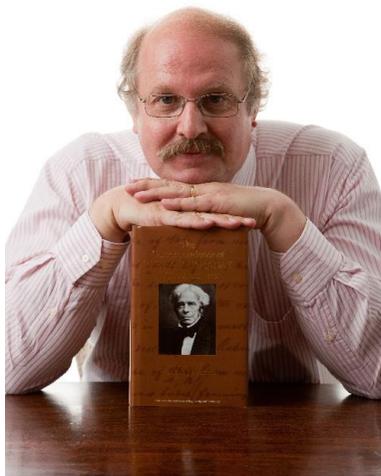


Figure 8. Frank A. J. L. James, the Eighth Wheeler Lecturer, with one of the six volumes of *The Correspondence of Michael Faraday*, which he edited between 1991 and 2012. Photograph reproduced by courtesy of the Royal Institution of Great Britain



Figure 9. Speakers at the Wheeler Award Symposium entitled “The Centenary of Robert Burns Woodward (1917-1979),” Wednesday, May 10,

2017, Burlington House, Piccadilly, London. (Left to Right) Mark Whiting (University of Bristol, Woodward postdoctoral student, early 1950s); Pierre Laszlo (Ecole Polytechnique, Palaiseau, France and University of Liège, Belgium); Anthony G. M. Barrett, FRS, FMedSci (Imperial College London); Jeffrey I. Seeman, the Ninth Wheeler Lecturer (University of Richmond, USA); John Hudson (Chair, Historical Group of The Royal Society of Chemistry); Stanley Roberts (Manchester University, Woodward postdoctoral student, mid-1960s), and Peter Morris (Science Museum, London). An image of R. B. Woodward looms somewhat solemnly from behind and over the speakers, appropriately so, given the theme of that afternoon’s symposium. Photograph courtesy of Rav Rattan, Royal Society of Chemistry.

Perhaps not entirely coincidentally but certainly worthy of note, all but one of the previous Wheeler Lecturers are recipients of the highest award given by the Division of History of Chemistry (HIST) of the American Chemical Society (ACS). Also not entirely coincidentally but worthy of note, in all but one case, these individuals received the HIST award *before* receiving the Wheeler Lectureship and invitation to write an Occasional Paper. The latter observation may be due to the fact that the HIST award was created first, by nearly 50 years, providing a head start in selecting awardees.

Since the HIST award is presented by your American cousins, perhaps a few sentences about the HIST award are in order. For the first 45 awardees (from 1956 – 2001), the HIST award was called the Dexter Award, named after the Dexter Chemical Company. The Dexter Chemical Company was started and owned by Sidney M. Edelstein, who served as Secretary/Treasurer of HIST from 1948 – 1965. Edelstein was a philanthropist who funded many pursuits dealing with the history of chemistry. These included the Sidney M. Edelstein Center at The Hebrew University, the Edelstein International Fellowship program at the Chemical Heritage Foundation, and the Dexter Award for lifetime achievement in the history of chemistry, administered by HIST. After the sale of the Dexter Chemical Company, Sidney Edelstein’s family funded the Dexter Award’s successor, the Edelstein Award for lifetime achievement in the history of chemistry, also administered by HIST (from 2002 – 2009). This award was not given in 2010 and 2011. After this two-year hiatus, a major new source of funding was obtained, and this award is now called the HIST Award for Outstanding Achievement in the History of Chemistry.

Table 1. Edgar Philip Wheeler Lectureships and Occasional Papers of the Historical Group of The Royal Society of Chemistry and Dexter/Edelstein/HIST awardees of the Division of the History of Chemistry of the American Chemical Society^a

Edgar Philip Wheeler Lectureship		Author – Awardee <i>Title of the Wheeler Lecture and/or the Occasional Paper (Publication Date)</i>	Occasional Paper No.	Year of Dexter, Edelstein or HIST Award	Summary of Educational Background and Primary Professional Affiliation(s)
No.	Date and Location				
	March 7, 1989	Dame Mary D. Archer, DBE <i>An Interview with Professor Harry Julius Emeléus (1903-1993)</i> (July 1997)	1	---	M.A. and B.Sc. in chemistry, Oxford University. PhD in chemistry, Imperial College. Career: National Energy Foundation; NHS Trust, Chair of trustees, National Science Museum Group.
	April 11, 1995	Robert G. W. Anderson <i>Chemie to Chemistry at Edinburgh</i> (June 2000)	2	1986	M.A. and B.Sc. in chemistry, Oxford University. D.Phil. in chemistry, Oxford. Career: Science Museum, National Museums of Scotland, British Museum, Chemical Heritage Foundation.
1 st	November 8, 2002 Waltham Abbey	Seymour H. Mauskopf <i>Long Delayed Dream: Frederick Abel and Smokeless Powder</i> (February 2003)	3	1998	B.A. in history, Cornell University. M.A. in history, Ph.D. in history of science, Princeton University. Career: Department of History of Science, Duke University

Edgar Philip Wheeler Lectureship		Author – Awardee <i>Title of the Wheeler Lecture and/or the Occasional Paper (Publication Date)</i>	Occasional Paper No.	Year of Dexter, Edelstein or HIST Award	Summary of Educational Background and Primary Professional Affiliation(s)
No.	Date and Location				
2 nd	April 23, 2004 Leiden	Robert G. W. Anderson <i>Boerhaave to Black: The Evolution of Chemistry Teaching</i>	<i>Ambix</i> ¹⁰	1986	See two rows above.
3 rd	March 22, 2007, Burlington House	David Knight <i>Davy and the placing of potassium among the elements</i> (August 2007)	4	2003	M.A. in chemistry, Oxford. D. Phil. in history of chemistry, Oxford. Career: Philosophy Department, University of Durham.
4 th	March 22, 2007, Burlington House	William H. Brock <i>Radiant Spectroscopy. The Rare Earth Crusade</i> (August 2007)	5	1995	University College London, first chemistry, then history of chemistry. Ph.D. in the history and philosophy of science, Leicester. Career: Department of History and Philosophy, University of Leicester.

Edgar Philip Wheeler Lectureship		Author – Awardee <i>Title of the Wheeler Lecture and/or the Occasional Paper</i> (Publication Date)	Occasional Paper No.	Year of Dexter, Edelstein or HIST Award	Summary of Educational Background and Primary Professional Affiliation(s)
No.	Date and Location				
5 th	March 20, 2009, Burlington House	Colin A. Russell <i>Frankland — the First Organometallic Chemist</i> (also referred to as <i>The Origins of Organometallic Chemistry</i> ¹² (August 2009)	6	1990	External B.Sc. in chemistry at University College, Hull (now University of Hull). M.Sc., Ph.D. in the history and philosophy of science, University College, London. Career: History and Philosophy of Science and Technology, Open University, among other institutions.
6 th	May 17, 2013 Burlington House	Peter J. T. Morris <i>Robert Burns Woodward in His Own Words</i> (April 2017)	9	2006	M.A. in chemistry, Oxford, with a Part Two in history of chemistry. D.Phil. in modern history, Oxford. Science Museum, London.
7 th	October 22, 2014, Burlington House	Anthony S. Travis <i>Nitrogen, Novel High-Pressure Chemistry, and the German War Effort (1900-1918)</i> (April 2015)	7	2007	B.Sc. and Ph.D. in chemistry, Birkbeck College, University of London. Career: Sidney M. Edelstein Center for the History and Philosophy of Science, Technology and Medicine, The Hebrew University of Jerusalem and Leo Baeck Institute.

Edgar Philip Wheeler Lectureship		Author – Awardee <i>Title of the Wheeler Lecture and/or the Occasional Paper</i> (Publication Date)	Occasional Paper No.	Year of Dexter, Edelstein or HIST Award	Summary of Educational Background and Primary Professional affiliation(s)
No.	Date and Location				
8 th	October 12, 2015 Royal Institution	Frank A. J. L. James <i>'the first example . . . of an extensive scheme of pure scientific medical investigation'. Thomas Beddoes and the Medical Pneumatic Institution in Bristol, 1794 to 1799</i> (November 2016)	8	---	M.Sc. and DIC and Ph.D. in history of science, Imperial College, London University. Professor of the History of Science, Royal Institution and Professor of the History of Science, University College London.
9 th	May 10, 2017 Burlington House	Jeffrey I. Seeman <i>R. B. Woodward's Unpublished Letters and His Lighter Side</i> ^{bs} (April 2018) and <i>Woodward, Hoffmann & Corey and the Rashomon Effect</i> ^{pd}	10	2017	B.S. in chemistry, Stevens Institute of Technology. Ph.D. in chemistry, University of California Berkeley. Industrial research chemist, Department of Chemistry, University of Richmond.

^a Date from <http://www.sbcs.qmul.ac.uk/rschg/OccPapers/> and <http://www.scs.illinois.edu/~mainzv/HIST/awards/index.php> (both accessed January 2, 2018).

^b Title of two lectures were presented by Seeman on May 10, 2017 at Burlington House at a symposium entitled *The Centenary of Robert Burns Woodward (1917-1979)*.

^c Theme of this Occasional Paper, which has a slightly different title. ^d Title of the Wheeler Lecture.

Sincere Words of Thanks

I very much thank my host for the Wheeler Lecture day, Peter Morris. Peter is one of my heroes and my friend. He, too, has been studying Woodward for many years. One of his many accomplishments is a spectacular book, for me a pleasure book and a reference book, written with and co-edited by another dear friend, O. Theodor Benfey. That book, *Robert Burns Woodward. Architect and Artist in the World of Molecules*,¹³ was published over 15 years ago by the Chemical Heritage Foundation. I use that book often, and I am continually pleased with how rich that volume is, in information about Woodward and his era.

So you see how very interconnected are the chemistry and history of chemistry communities. Even more so, several years ago, Peter and I quite independently were writing papers on *Woodward's Words*. When I discovered this remarkable coincidence, I suggested to Peter that we collaborate and write one single paper. But because of distance and other practical matters, this idea was not meant to be. Perhaps that is just as well, as today we have several papers in the literature rather than just one. Peter's paper is now available as The Royal Society of Chemistry's Historical Group Occasional Paper No. 9.¹⁴ Mine can be found in the chemical literature⁵⁻⁷ and in this Occasional Paper No. 10.

The Ninth Wheeler Lecture

After such a lengthy introduction and nine figures, it is time for us to turn to R. B. Woodward (Figure 10) and his words. I should like to begin my paper by saying that

“every privilege carries with it some kind of responsibility, and I have, at this moment, a responsibility which I cannot but regard as a heavy one – that of presenting to all of you a lecture [actually, two lectures and a “playlet” with Tony Barrett of Imperial College joining the Wheeler Lecturer], appropriate to the occasion, and it may be hoped, of some general interest to an audience among whose members there must certainly be a wide diversity in background.

“WELL, THEN, here I stand with the problem still before me. WHAT TO SAY? And now, the solution. I present it with considerable diffidence, since what I am about to describe is highly personal, idiosyncratic, and very far removed from the kind of lecture with which I am familiar!”^{14,15}

These words, of course, were not mine, at least not originally! They were first spoken by R. B. Woodward on August 28, 1973, in his Arthur C. Cope Award address at the American Chemical Society National Meeting.

Woodward shared the first Cope Award with his collaborator Roald Hoffmann. These words of Woodward's were subsequently recited by Peter Morris on May 17, 2013, on the occasion of his Wheeler Lectureship. I find it particularly fitting that I begin my paper with those same words, too, as an acknowledgment to Woodward, Morris, and a co-conspirator for a portion of the performance, Tony Barrett.



Figure 10. Woodward in his Harvard office, 1978.
Photograph courtesy Ian Fleming

Part 1 of “Woodward's Unpublished Letters: Revealing, Commanding and Elegant” recently appeared in *Helvetica Chimica Acta*⁸ With the exception of a few sentences, none of the excerpts herein appeared in Part 1 of this two-part series.

Woodward's Communication Timing

In a number of letters, Woodward characterized himself as a tardy correspondent. His apologies were often quite elegant and never duplicates of a previous apology. Here are two examples. Another follows later in this paper.

To Eugene van Tamelen at Stanford University, 1970. Van Tamelen, who began his academic career at the University of Wisconsin, was enticed to Stanford when William S. Johnson, another leading organic chemist at Wisconsin, and Carl Djerassi began to build up Stanford's Department of Chemistry in the late 1950s. Van Tamelen's organic chemistry research was divided into two major areas, the synthesis of novel small ring compounds and biomimetic synthesis. He was the first to identify squalene oxide as the precursor in the biosynthesis of cholesterol.

"You are certainly familiar with my incapacity in the matter of discharging my responsibilities in respect to correspondence, and will accept my apology for this very tardy reply."¹⁶

Letter to R. J. Eastman, Stanford University, 1947. Eastman had been a graduate student with Woodward at Harvard prior to taking an academic position at Stanford.

"Knowing me as you do, and something of the manner in which my time is spent, you will not measure my concern and interest in you and your affairs by the long interval which elapses between the receipt of your much appreciated letters, and my replies. It really is wonderful to hear from you, and particularly so to hear how much you enjoy your present station. I cannot tell you how glad I am that you have found a place in which you are so happy, both professionally and otherwise; I hardly need say that you deserve your good fortune. . . . And now to the business you mentioned. The situation on the diazo-coupling reaction is very simple. I have no objection (in fact, I think you should) to your considering that work entirely your own, and publishing it when and in whatever form you wish. Incidentally, I should be interested in hearing of your new results along that line. . . . go ahead full steam; in that sphere I'm just a very interested spectator."¹⁷

Woodward's Self-characterization and Sense of Humor

From Woodward's introduction of William von Eggers Doering on the occasion of Doering's receipt of the Richards Medal, April 9, 1970. Doering was a graduate student of R. P. Linstead's at Harvard and then worked with Woodward (1943-1944) on the total synthesis of quinine. In 1944, Doering joined Columbia University in his first academic appointment. He became a leader in physical organic chemistry studying primarily valence isomerizations.

"My personal investigation of Doering's career at Belmont Hill [School] has revealed that he was unable to distinguish himself by getting less than a perfect academic record. . . . When, after completing his Ph.D. research

[with R. P. Linstead at Harvard], and having spent some time working with Professor Fieser, Doering decided to cast his lot with mine, Lilli [Schwenk] took a distinctly jaundiced view of his move. . . . The completion of the synthesis of quinine attracted a certain amount of notoriety. At that precise time I became the victim, or beneficiary, of some of the lurid aspects of one of the earlier instances of a not inconsiderable train of melancholy events which have molded my character, or vice versa. In consequence, Doering had to bear the brunt of dealing with our public relations, which he did with aplomb, charm, and revealingly, I think, a certain amount of pleasure."¹⁸



Figure 11. Woodward and his close friend Bill Doering at Woodward's 60th birthday party, April 1977. Photograph courtesy Lauri Robertson.

Woodward's Early View of Credit and Documentation

Prior to the advent of the on-line version of Chemical Abstracts, a CA search often involved a step in which one followed a path involving the last name of the first author of a paper. A literature search was often easier when the first named author was a well-recognized individual. It is this aspect of literature searching that Woodward refers to in the following excerpt from a letter to Doering at Columbia University, 1945.

“For goodness’ sake, change ‘Chanley and Doering’ to ‘Doering and Chanley’. You know as well as I do that papers are listed under the name of the first author, and you must have been very annoyed many times to have to try to find a paper known to you to have been written by a fairly well-known investigator only to find that it is listed under the name of his little-known collaborator. It is only courtesy to the reading public to put your own name first on all your papers unless very special circumstances demand clearly the other course. Besides, need you start in with false modesty at this early stage of the game?”¹⁹

*That paper “The Autoxidation of Quinone” was published in 1946 with Doering as the first author.*²⁰

Letter of Recommendation

Letter to the Department Chair of a Major University, 1957.

“I have learned that [name expunged] is, for various reasons, not a candidate [for a staff position in your university]. Before receiving that intelligence, I devoted some thought to the possibilities inherent in his incumbency, and I am not sure that you should be thoroughly disappointed in the fact that he is not available. Of his very great energy, outstanding organizational talents, and present deep interest in matters chemical, there can be not the slightest doubt. Perhaps these qualities should be quite sufficient; certainly at the present time, the prospect would appear excellent that he will make a great, and perhaps massive, contribution to chemistry. None the less, I have certain reservations. It is possible to feel that his approach to his work has lacked certain intellectual and scholarly attributes which may be of definitive importance in determining the depth, scope, and continuity of a man’s contributions. It is possible that this lack in the long run could operate to the detriment of a department over which he has control.”²¹

It would be interesting to analyze, with the benefit of hindsight, the extent to which Woodward’s judgement was valid. But given the critical nature of Woodward’s prediction of this non-candidate’s subsequent performance as an academic chemist, such an analysis will not be examined herein.

On Behalf of a German Immigrant, 1941

Letter to the U.S. Department of Immigration, Washington, D.C., July 5, 1941.

“It has been called to my attention that Mr. Ernst Berliner, a German refugee student in this laboratory, desires to take up permanent residence in this country, with the intention of becoming an American citizen. After very considerable difficulties in leaving Germany . . . Furthermore, my personal

knowledge has convinced me that as a result of his experiences in Germany, as well as his innate respect for the things for which America stands, there is no doubt whatsoever of his loyalty or his wholehearted sympathy with American ideals. In short, I am sure that Mr. Berliner would make a loyal, valuable, capable citizen, and I should like to request that everything possible may be done to help him attain his goal.”²²

Indeed, Berliner (1915 – 2008) did become a productive American citizen. After receiving his Ph.D. from Harvard in 1943, he taught at Bryn Mawr College from 1944 until his retirement in 1985. Many chemists studied with Berliner including Marjorie Caserio who received her Ph.D. from Bryn Mawr with Berliner as her advisor. Caserio then moved to Caltech as a postdoctoral student with John D. Roberts and subsequently became Roberts’s research associate. Caserio ultimately became an eminent professor of chemistry at the University of California Irvine before becoming a university administrator including Interim Chancellor at the University of California San Diego.

Response to an Idea

*Letter to Woodward’s former postdoctoral student Jerome A. Berson, University of Southern California (USC), 1956 (Figure 12). Berson received his Ph.D. with Doering at Columbia University. His first academic appointment was at USC followed by positions at the University of Wisconsin and Yale University. Berson was a leading physical organic chemist who published what many consider to be the most significant experimental test of the Woodward-Hoffmann rules.*²³



Figure 12. Jerome Berson, ca. January 1950, Cambridge, MA.
Photograph courtesy J. Berson

“I was delighted to hear from you, and I have derived much pleasure from considering your very ingenious scheme for building up alkaloid structures from polyketo acids. As you note, your scheme is certainly an amusing exercise, and is sufficiently pretty as to make me hesitant to throw cold water on it as a contendant for a place in the order of Nature.

“None the less, I feel that I must do so. In the first place, while I realize that in so doing I may be simply falling prey to the error of protecting the vested interests [apparently Woodward had an idea that he felt was superior to Berson’s proposal], I cannot but feel that the very different schemes involving amino acid ‘equivalents’ are so strongly supported by all of the available inferential evidence that there is no profit to be had in looking for alternatives . . . ”²⁴

Response for a Request for Information from Harvard Colleagues

According to Wikipedia, “Samuel Andrew Stouffer was a prominent American sociologist and developer of survey research techniques. Stouffer spent much of his career attempting to answer the fundamental question – How does one measure an attitude?” On May 23, 1957, Stouffer, working at the Laboratory of Social Relations, Harvard University, wrote Woodward a reminder letter, asking for Woodward’s response to a questionnaire.

“Knowing how busy everybody is at this time of year, I feel embarrassed in writing you this begging letter. However, our record does not show that we have received the little questionnaire on the ‘intellectual’ and the ‘anti-intellectual’ which my assistant left at your office some time ago.

“We need your help badly. All but a small minority of professors on our list have now responded – including some who did so in spite of expressed skepticism about the value or validity of such an inquiry.

“A main objective of the study is to ascertain the range, variety, and nuances of viewpoints from a truly representative sample of professors from all disciplines. Some of those not yet heard from may have viewpoints quite different in various ways from the viewpoints already expressed. This possibility could cloud our interpretation of the data. The effort already generously expended by so many of our colleagues would, of course, be jeopardized by such a hazard.

“We hope you will help us avoid this contingency . . . ”²⁵

Woodward responded that very same day,

“Much as I should like to cooperate with your program through filling out one or another of the two copies of the ‘little questionnaire’ (Heaven preserve us from big ones) which you sent to me recently, I regret very much that I am forced to return them to you, unfilled.

“To have been forced to reach this decision is the more regrettable since I have glanced briefly at your questionnaire, and I know that I should derive much enjoyment from completing it. I know also that I am incapable of doing that kind of thing hastily, haphazardly, or without careful and concentrated thought, which would in the case at hand consume a great many hours of my time. These are hours which I do not have.”²⁶

Letter to Professor Louis Caryl Graton, American geologist, chemist and educator and professor of Mining Geology at Harvard, 1963. Unfortunately, the questions and problems put forth by Professor Graton are not available to share with the reader.

“I have no doubt that the questions put and problems raised in your letters of April seventeenth and May eleventh are of great interest and importance, but I am much less sanguine about my capacity or competence to deliver any meaningful opinion about them, and even less so about the amount of time I might be able to find in which to give them any thoughtful consideration.

“Consequently, may I thank you for the compliment of your having asked me to think about the things which interest you, and express my regret that I shall not be able to do so.”²⁷

Letter to Mr. Alfred M. Ajami, The Office of Tests, Harvard College, 1972. Unfortunately, as in the case of Louis Graton immediately above, the requests put forth by Mr. Ajami are not available to share with the reader.

“I do wish I were able to help in the project outlined in your letter of February twenty-fourth. But the muse is not upon me, and it is unlikely to be before I leave shortly for Switzerland. In its absence, the kind of thing you request is far from the kind of thing I am able to do in ‘a few hours,’ and I know that I shall not be able to find the much more extended period of time needed before my departure.”²⁸

Woodward’s Explanation of an Orbital Symmetry Issue

Letter to H. G. Heller, Université de Genève, 1972.

“I do hope I am not being unkind in suggesting that the inclusion of category C in your analysis of 1,3 shifts is supererogatory, or perhaps even misleading, because any ‘1,3 shift’ which satisfies the prescribed conditions will be built into some special structure, and proper analysis will give the correct result, without any elaboration or modification. Perhaps I have misunderstood your position; if so, please let me know.”²⁹

“The Problem is Dormant”

Letter to G. B. Gill, Nottingham, England, January 7, 1970. Note that Woodward’s response was written, indeed, over six months after his receipt of Gill’s letter.

“I am very sorry that various special circumstances here in Cambridge during the past six months have made it impossible for me to reply sooner to your letter of May twenty-eight. May I now make this tardy reply to the questions you raise.

“One of the main stimuli in our synthesizing triquinacene (Figure 13) [*Woodward’s reported synthesis was in 1964*³⁰] was our hope that we might be able to bring about dimerization of the triene to dodecahedrane. In the event we have not pursued the dimerization vigorously, but the experiments we have done have not as yet brought any encouraging results. Irradiation, for example, leads only to very efficient polymerization. A very real problem would appear to be that approach of two molecules of the triene to one another, each from the concave (endo) side, is sterically a very unfavorable process. At the present time the problem is dormant; I am awaiting the arrival of a good idea about how to proceed further.”³¹

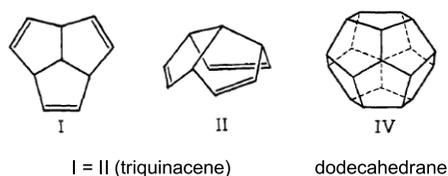


Figure 13. Structures of triquinacene and dodecahedrane, reproduced from R. B. Woodward, T. Fukunaga, and R. C. Kelly’s 1964 paper [Woodward, R. B.; Fukunaga, T.; Kelly, R. C. *J. Am. Chem. Soc.* 1964, **86**, 3162-3164] which reported the synthesis of triquinacene. It was Woodward’s idea, never realized, to synthesize dodecahedrane via the dimerization of triquinacene, a reaction which has not yet been achieved.

Twenty three years later, in 1993, Bertz, Cook *et al.*³² published a full paper on their unsuccessful attempts to dimerize triquinacene using high pressure “(up to 17 GPa) with or without photochemistry (248 and 308 nm, 5GPa).”³² Of course, their first reference was to Woodward, Fukunaga and Kelly’s 1964 paper on this topic.³⁰

On Responding to an Asserted Literature Citation Slight

Letter to Eugene E. van Tamelen, Stanford University, 1970 who had written Woodward, expressing concern that some of his [van Tamelen’s] published work had not been sufficiently reported in Woodward and Hoffmann’s *Angewandte Chemie* paper on the “Conservation of Orbital Symmetry.”³³

“The 9,10-dihydronaphthalene/cyclodecapentaene story [Figure 14]. I have re-read the relevant section several times carefully and have asked Roald

[Hoffmann] to do the same. I have further asked unprejudiced and unknowledgeable volunteers to examine the section. None among these find the slightest intimation that your ‘previous disclosures were depreciated.’ The relevant references are there, available for anyone to read; we make no judgement about the relative merits of the claims made in them; we do not even state that there is a controversy. To the extent that there is one, no doubt you, [Saturo] Masamune, other interested parties, and posterity will settle it.”¹⁶

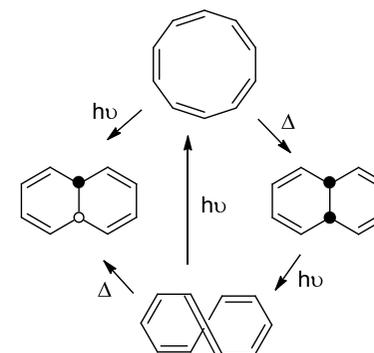


Figure 14. A series of reactions discovered by van Tamelen and Burkoth [*J. Am. Chem. Soc.* 1967, **89**, 151-152] illustrating the thermal-to-photochemical stereochemical alternating effects that serve as a fundamental hallmark of the Woodward-Hoffmann rules [*Angew. Chem. Int. Ed.* 1969, **8**, 781-853].

Woodward’s Conciliatory Tone

Woodward had a lifelong interest in the Diels-Alder reaction beginning in his early teens. In the 1940s, he published a series of paper dealing with the mechanism of this reaction.³⁴⁻³⁷ He then liberally used the Diels-Alder reaction in many of his total syntheses, returning in the late 1950s with several papers with Thomas Katz^{38,39} in which a concerted, nonsynchronous mechanism was proposed, a mechanism which today has received some theoretical support.⁴⁰ In the following excerpts, communications to and from his Harvard colleagues and editor of *JACS* Arthur B. Lamb are shown (from 1942).

On November 11, 1942, Woodward wrote to the Editor of *JACS*,

“I am submitting herewith a Communication entitled ‘The Mechanism of the Diels-Alder Reaction’. I am particularly anxious that publication of this material be expedited, since I noticed to-day in the Proceedings section of the J. C. S. that a paper by Wassermann on this same subject has been

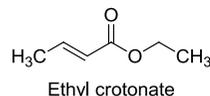
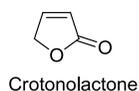
received. I have had this material in mind since June, but the pressure of other work has prevented my getting it written up and sent in, and I would rather not be anticipated."⁴¹

On November 17, 1942, Lamb responded, provisionally accepting Woodward's paper, saying in part,

"If you do not agree with the Referee's recommendation in this particular ['the elimination of the first paragraph'], perhaps there will still be time to argue it out with him by mail if you can make a prompt reply."⁴²

Woodward responded on November 19, 1942,

"I do not agree entirely with the Referee, particularly as regards his statements concerning crotonolactone and ethyl crotonate. However, I feel that he is probably quite correct in pointing out that my first paragraph, considered as an attempt to dispose of the two rival hypotheses, is unconvincing and unjustified. You will imagine that these sentences did not have this purpose; such disposition would take a much longer space, and I should do a rather more convincing job of it. In fact, the opening sentences were intended merely as an introduction; they are not necessary, and in accordance with the opinion of the Referee I have deleted them."⁴³



Woodward's Firm Tone

Woodward's letter to JACS editor Lamb regarding another paper on the Diels-Alder reaction, 1943.

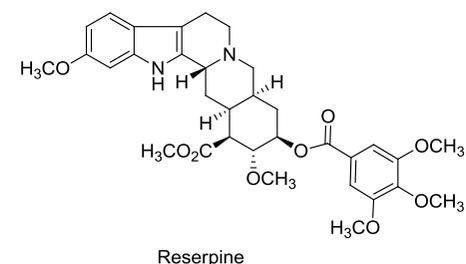
"... These views, which have been elaborated purely from general electronic principles, are in consonance with the facts as observed in our work, namely, in the case of fulvenes, both exo and indo [sic] isomers are obtained, whereas, in the case of cyclopentadiene, only the endo isomer is obtained. The situation thus seems to be: We propose a theory (JACS, 64, 30558 (1942)). We set out to test it. We find some experimental facts which provide some support for the theory in that they can be explained through its use, and not easily in any other way. Can there be any further question but that in the paper describing our experimental work we should call attention to the relevance of our experimental work to the theory which inspired it, and in terms of which the results are rationally interpretable?"⁴⁴



Fulvene

Woodward as Co-Chairman of the Honorary Editorial Board of *Tetrahedron* and *Tetrahedron Letters*

In the early 1950s, Sir Robert Robinson initiated a movement to begin the first international journal of chemistry which, after many negotiations, began publication in 1957 as the journal *Tetrahedron*. The Co-Chairman of *Tetrahedron* were Robinson and Woodward. Two years later, the all-communications journal *Tetrahedron Letters* began publication. Woodward's first publications in these journals were his classic total synthesis of reserpine⁴⁵ in 1958 in *Tetrahedron* and his rebuttal of Michael Dewar's⁴⁶ article on the Diels-Alder reaction in *Tetrahedron Letters* in 1959.³⁸



As part of his role as Co-Chairman of *Tetrahedron*, Woodward was sent Dewar's submission which he (Woodward) considered to be considerably lacking. Together with his student Thomas J. Katz, Woodward constructed his own letter to *Tetrahedron Letters* rebutting Dewar⁴⁶ and presenting his own ideas.³⁸ Later in that same year, Woodward and Katz would more fully describe their mechanistic thinking in a full paper published in *Tetrahedron*.³⁹ An excerpt follows, from Woodward's cover letter to I. Stuart, Sub-editor of Pergamon Press, publisher of *Tetrahedron* and *Tetrahedron Letters*, London, England, 1959. Katz and Woodward's rebuttal was enclosed along with that letter.

"Thank you for letting me see the copy of M.J.S. Dewar's paper entitled 'Mechanism of the Diels-Alder Reaction'. Unfortunately, Professor Dewar's contribution represents another piece of inconsequencia, which will do *Tetrahedron Letters* little credit. My evaluation of the contribution is simple: it should have been rejected. However, since it has been accepted, and will presumably appear, the accompanying note of my own should probably be published with it, if the record is to be kept straight. I find it a melancholy fact that my maiden appearance in *Tetrahedron Letters* [with Thomas J. Katz]³⁸ is to be associated with so unexciting and relatively tawdry an incident."⁴⁷

Mr. Stuart immediately responded to Woodward, a response which either demonstrates a complete misunderstanding of Woodward's intention and childlike naiveté or provides an example of extraordinarily polished politics.

"I hereby acknowledge with thanks receipt of your communication 'Mechanism of the Diels-Alder Reaction' for publication in Tetrahedron Letters. I am extremely sorry you feel the way you do about Professor Dewar's paper, and perhaps you will be somewhat relieved to hear that your communication will not be appearing alongside his. The reason for this being that Professor Dewar's paper is in [volume 1] No. 4, which will appear next week. Your paper will appear in [volume 1] number 5, which should be out at the end of May."⁴⁸

Woodward was undoubtedly further dismayed by Stuart's letter. Stuart's explanation that Woodward "will be somewhat relieved to hear that your communication will not be appearing alongside his" is surely tongue-in-cheek. Surely Stuart understood from Woodward's letter that his (Woodward's) intent was to have his (Woodward's) paper published directly following Dewar's paper, not in the succeeding issue of the journal.

Woodward's Perfectionism Pushed to the Limits

Another letter to I. Stuart, 1959.

"Some time since I ordered 500 reprints of my paper with Thomas J. Katz 'The Mechanism of the Diels-Alder Reaction' (Tetrahedron, 5, 70 (1959)). I have just received two packages, containing 80 of the reprints ordered. I am much distressed to find that the covers of the reprints contain two errors . . . I have received many requests for reprints of the paper. In any circumstances I should be hesitant to send out examples of such carelessness, but in my capacity as Co-Chairman of the Honorary Editorial Advisory Board of Tetrahedron, I feel that it would create a particularly unfortunate impression for me to do so. On the other hand, the impression which would be created by my failure to accede to the [reprint] requests which have been made would be equally unfortunate . . ."⁴⁹

Conclusions

Woodward's letters can be analyzed in at least three categories: for their entertainment value; for the understanding of Woodward as a person and as a professional; and for their insights into the sociology of science.

First, reading these letters is surely amusing and entertaining. Enough said at this time on that point.

Second, in Part 1⁸ of this two part series, a number of conclusions were made regarding Woodward's personality as reflected in his letters.

Woodward used the "high or grand style" of writing in his letters and lectures and in his research publications. He was an "indifferent correspondent" in terms of response time, though not in terms of the quality of his writing. His correspondence covered a wide range of topics and a wide range of personal styles – from caring and kind to pointed and critical; from deeply serious to outrageously humorous. He could write generous, detailed, helpful letters to teenage budding students, yet could reject requests from Harvard colleagues to participate in academic surveys asserting that he had insufficient time to do so.

Thirdly, regarding the sociology of science, surely correspondence can serve as a rich source of additional data for the study of social relationships and interactions experienced by scientists during the practice of science and scientific research. An optimum source may well be the correspondence between two scientists for as many years as possible, perhaps during a time of a major collaboration, such as during the Eschenmoser-Woodward collaboration on the total synthesis of vitamin B₁₂.⁵⁰⁻⁵⁵ In a future paper, I shall examine the Woodward-Hoffmann correspondence as part of my study on the history of the development of the Woodward-Hoffmann rules, on which one paper has already been published.⁹

In my series of "Words" and "Letters" papers including this paper (see a listing of the previous papers in an early footnote in this paper), excerpts from letters written to and sometimes received from a wide variety of individuals are found. These are not without their value in terms of the sociology of science, though only a brief hint at that will now be provided. A soon-to-be issued collection of letters⁵⁶ written by the theoretical physicist and mathematician Dyson Freeman as "An Autobiography Through Letters" (the book's subtitle) should provide another example of the utility of letters. Here are some of the conclusions we can gather regarding the sociology of science from Woodward's letters, considering the excerpts herein and from Part 1.⁸

- Even renowned scientists are willing to nurture and mentor very young individuals who show a serious, perhaps even quite advanced level, of knowledge and capability. There can also be a more subtle and hidden mentoring among the peer group, in part reflected in their private letters.
- Aspects of the competition, not evident in public venues, can be seen in their letters. Indeed, the letters provide much support for what may be viewed as unsubstantiated generalizations regarding the struggle for priority recognition among scientists.
- There exists an invisible, parallel set of communications between authors and editors, which influence the final acceptance decision of submissions, the timing of publication and the content of the publication.

- The typical competition for visibility and prestige, so very normal in the practice of science,^{57,58} can rise to a very visible though private manifestation even though some data show that disagreements among chemists often are not shared let alone resolved.⁵⁹
- Scientists can be emotionally disturbed by both minor transgressions, e.g., misspellings of a colleague's name or a paper not being cited, and by major issues, e.g., perceptions of plagiarism.
- Permanent close friendships can be formed among two or more scientists, one might call these cliques or even invisible colleges,^{60,61} in which the participants meet primarily at international conferences. This was true especially before the 1970s when even long distance telephone calls were rare and expensive and emails were far in the future. These relationships influenced sharing of students, nominations and voting for awards, and invitations to speak (as plenary lecturers) at international meetings. Private correspondence is often indicative of those close friendships, examples of which appear in previous papers in this series (see references 1-7).

Further elaboration of these topics will appear in future publications by this author.

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