

REMOTE INTERVIEWING AND THE HISTORY OF CHEMISTRY

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Abstract

Several distance interviewing techniques used by the author since the early 1980s are presented along with examples that illustrate various issues dealing with some social aspects of history: the roles of culture, credit, and priority in chemistry. Remote interviewing, especially by email and videoconferencing, are shown to be highly effective and perhaps even the optimum methods for interviewing subjects for research in the history of modern chemistry.

Preface

Every source of data is useful and potentially critical in the study of history of chemistry (HoC). All historians would, or should, agree that if possible, interviewing actors involved in historical events is of critical importance in the study of the history of those events. Indeed, interviewing even witnesses to historical events in science—others in the relevant peer communities—can provide insights and clues that go beyond what is available from the relevant literature. This essay is about distance interviewing methods I have used over the past 40 years, focusing on transformations of methodologies that have occurred over that time in research in the HoC.

Introduction

There is a sweet spot in the study of history of science, where the actors and members of their peer

group—witnesses, even supporting actors to extend the metaphor—are still alive and are sufficiently chronologically distant from the events to feel free, even enlivened and empowered to discuss their experiences. They can even do so with fresh eyes, frequently with sharp memories, and often though not always with receded if not mellowed emotions. With age can come a sense that sharing information from the past is socially and ethically acceptable and even a responsible activity of a scientist. Furthermore, current scientists are especially interested in the history of their fields, especially when they have personal connections with the events, the science and the actors themselves.

The visible portion of the scientific enterprise consists primarily in the published and unpublished documents as well as the inventions and discoveries that have been tangibly memorialized. Enormous as the scientific literature is, what we see is just the tip of the iceberg of the enterprise that is science. A vast percentage of the activities of science is out of sight, and with the death of the actors, becomes lost to time.

For my research over the past 40 years, I have conducted several forms of “remote interviewing” which includes interviewing by letters, fax, and now, especially by email. Rarely have I used on-line video interviews; indeed, in my research and in this paper, I address *only* remote written interviewing. I shall not discuss oral histories, as I have never conducted any—though I have used many in my research to great advantage. Also in this pa-

per, I shall provide examples of remote interviewing over the course of my career that involve the social-cultural context of science. Examples were chosen to illustrate the effect of culture on priority claims and credit, themes that are central to the motivation of scientists and thus to the progress of science (1, 2).

Distance Interviewing by Mail

My first example deals with private agreements among scientists to divvy up research areas, unilaterally contravened representations, multiple simultaneous discoveries, and asymmetric relationships.

In 1980, I was invited to write a chapter on an aspect of reaction kinetics for a book series entitled *Advances in Chemistry*. I decided to include a brief history of the subject in my chapter. I wrote letters to all the leaders in the field, posing questions and requesting photographs. Every single chemist responded including one Nobel laureate, Derek H. R. Barton. Excerpts from Ernest L. Eliel's letters are particularly informative.

Eliel (1921-2008) (Figure 1) was to become a member of the National Academy of Sciences (NAS), president of the American Chemical Society (ACS), and recipient of the Priestley Medal, the highest award in chemistry in the United States. In response to my letter inquiring how he came to discover an important principle in chemistry, Eliel responded on September 22, 1980, by letter. He wrote, in part (3),

The first ideas of conformational analysis in mobile systems surfaced in a paper I wrote in 1952 ... In the summer of 1953 Saul Winstein visited Notre Dame for an extended period as Reilly lecturer and I asked him about the extent of his own interest in doing quantitative work in conformational analysis. At that time Winstein indicated that he was only interested in assessing conformational effects on solvolysis ... In 1954 I did a great deal of thinking about [this subject and have several witnesses to that fact]. The clarification came to me in the late fall of 1954 ... I was rather taken aback when in January 1955 I received a preprint of the famous Winstein/Holness paper. Winstein obviously had changed his mind about strictly working on solvolysis ...

In the early 1950s, Eliel was a recent refugee from Nazi Germany. He had spent most of the war years in Cuba, received his Ph.D. in chemistry in 1948 (University of Illinois) and in the fall of 1948, Eliel became an instructor at Notre Dame University. Thus, when Eliel was misled by Winstein's misrepresentation, he (Eliel) was low on the academic totem pole. On the other hand,

Winstein was entering the height of his powers (he was elected to the NAS in 1955). Eliel held his tongue and did not confront Winstein.

In his 1965 book on *Conformational Analysis*, Eliel very mildly asserted his priority without engaging Winstein's ethics. Eliel documented his priority over Winstein in a footnote (5),

Following earlier speculations on the conformational behavior of mobile systems (E. L. Eliel, *Experientia*, 9, 91 (1953)), E. L. E., in the fall of 1954, developed the quantitative expression ... and communicated some of the results to Professors W. G. Dauben and D. Y. Curtin (private communications, dated December 20 and December 22, 1954). In January 1955, Professor S. Winstein kindly sent to E. L. Eliel the manuscript [containing the breakthrough Winstein-Holness equation] ... Thus [Eliel's equation and Winstein's equation] were developed independently in the two laboratories ...

From my experience with Eliel and also from seeing several other examples of similar joint-yet independent research strategy planning in letters by such eminent scientists as John D. Roberts, Roger Adams, and William von E. Doering (6), I conclude that:

- Researchers privately, even covertly, divide research programs among themselves with the rationalization that they are reducing waste in granting agency funds, minimizing duplicative research, and protecting students' educational trajectories.
- These agreements are not always maintained. Practical (promotion, funding) and emotional (being scooped) consequences can lead to per-



Figure 1. Ernest Eliel, reportedly about to mail the manuscript for his famous book *Stereochemistry of Carbon Compounds* (4), Notre Dame post office, ca. 1959. Photograph courtesy E. L. Eliel.

manent damage in relationships. An excellent example of an agreement not maintained was one by Geoffrey Wilkinson and Ernest Otto Fisher in the spring of 1954. While they agreed on some division of the periodic table in the study of the sandwich compounds, e.g., ferrocene-type compounds, neither group abided by that agreement (7).

- Sometimes discussions regarding overlapping research interests lead to collaborations rather than competition. As Albert Eschenmoser described the decision to form the Woodward-Eschenmoser collaboration on the total synthesis of vitamin B₁₂, “We decided it was better to collaborate than to compete” (8).
- Asymmetrical relationships can minimize outward conflicts though emotional turmoil can be permanent.
- Strong emotions and previously hidden conflicts can be revealed even during remote interviews.

By 1980 when Eliel wrote that revealing letter to me, he and I had built a close professional relationship. Eliel knew of my chemical research, and we had corresponded frequently. On a trip to Richmond where I live, Eliel had visited me in my home. Faith in one’s interviewer matters. There are many ways to break trust and only one way—eternal vigilance—to maintain trust. I’ve discovered that most individuals want to share, and they are happy to reminisce. They are pleased that someone is interested in them and their lives. It helps if they also recognize your credentials and your knowledge.

Interviewees sometimes say to me, “What I am now going to say is confidential.” I generally agree immediately. Rarely do I ask that interviewees *not* divulge anything that is confidential; sometimes interviewees withdraw that condition, because human beings want to be heard, understood, and valued. Usually, I share advanced drafts of my writings to interviewees for their review, not just as a courtesy, especially if I perceive there are sensitivities involved. And I very much appreciate that opportunity when the roles are reversed.

Only once in my 40 years of interviewing was I told three hours into a major in-person (telephone) interview, “Of course, everything I’ve just told you is confidential.” I responded, “Nothing you’ve just told me is confidential. Confidentiality must be agreed upon *before* an interview, *not after*.” The interviewee agreed immediately, and we continued the interview for another 30 minutes. He wanted to be heard. But I subsequently provided him

two advanced drafts of my manuscript, and I revised my draft based on his corrections and clarifications. That being said, my relationship with that chemist was permanently damaged. I could have, likely should have, responded more softly and reassuringly, not spontaneously like an attorney.

A colleague has asked, “What do you do about highly embarrassing or even tragic life events that you’ve learned about a dead person?” A life principle of one of my chemistry heroes Vladimir Prelog was, “*De mortuis nil nisi bonum.*” (“Of the dead, say nothing but good.”) Do we reveal tales of marital infidelity, for example? One must think deeply and question mightily: Was this information ill-gotten? Is it substantiated? Will a true and complete history of science be forsaken with its absence? And does one have—or need to have—permission from a family member to reveal such information? I am just now dealing with one truly tragic life experience and the privacy of the subject and their family.

Communication by letters is slow, even slower today than 40 years ago. But in those days, I found distance interviewing by mail to be effective and inexpensive, and it provided a wonderful collection of autographs of eminent chemists!

Beginning in 1983, I conceived of, organized, contracted with a publisher (the ACS), and edited a series of 20 autobiographies of eminent organic chemists (9-11). Living in seven countries, the authors represented all the major sub-disciplines of organic chemistry. Five were Nobel Prize laureates, and all of them were the elite of their fields. The project entitled *Profiles, Pathways and Dreams* had several goals. One was to illustrate how individual research programs evolved over many decades; the average age of the authors was well over 70. And when taken together, the books revealed how organic chemistry had evolved from the 1940s into the early 1990s. There was great diversity among the authors, in terms of subdiscipline and personality. But not in terms of race and gender, unfortunately. All the authors were Caucasian except two who were Japanese, Tetsuo Nozoe and Koji Nakanishi. All were men. As one of this paper’s reviewers wrote,

I do not think that the author has excluded on purpose minority/female organic chemists. It was the way the field of organic chemistry was at that time, populated mostly with white male scientists.

I appreciate that understanding. That was the way it was. Today, the series would be far more inclusive and diverse.

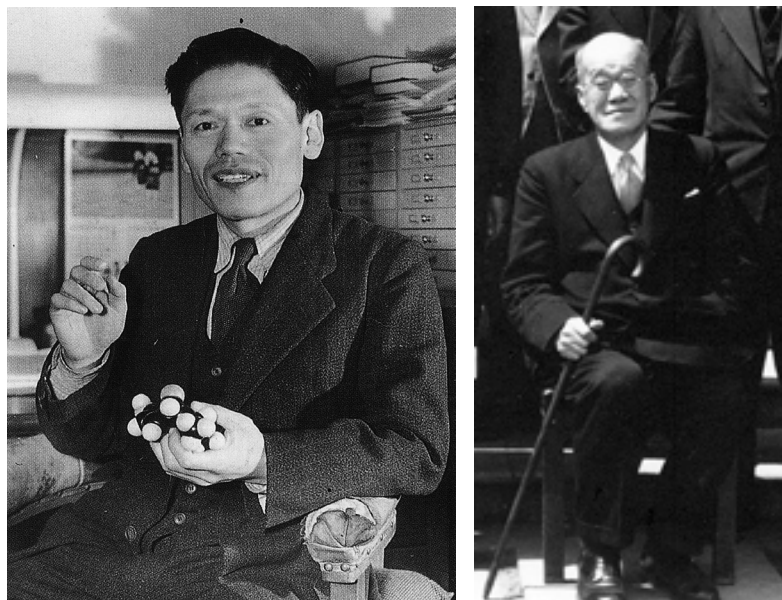


Figure 2. (left) Tetsuo Nozoe, 1952. (right) Riko Majima, 1958. Photographs courtesy T. Nozoe.

Without my request or anticipation, many authors sent me their manuscripts chapter-by-chapter (by mail!) for my review. I rapidly became immersed and excited in these life stories. I constantly asked for more from the authors. I wrote questions in green ink within the white spaces of the typewritten manuscripts. When my commentary was long, I used an early word processor and inserted individual pages within their draft, then sent a collated package back to them. When a revised draft reached me, I would compare it with my annotated version. It was like magic. I was surprised by many discoveries. Many years ago, I concluded:

- An editor is automatically given an aura of respect and influence. An interviewer often has the same standing.
- Reasonable questions will most always be answered.
- Results are proportional to the interviewer's preparation and sometimes persistence.
- An interviewer's job is being the servant of the author.

Distance Interviewing by Fax

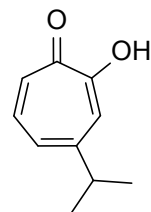
Distance interviewing is generally necessary for individuals who live in countries quite distant from the interviewee. But interviewing such individuals also comes with other challenges: language and cultural considerations. Fortunately, English seems to have suc-

cessfully become the *lingua franca* (12-14). But differences in cultural norms can affect both science and scientists.

In the West, little is known of the history of Asian chemistry. For example, the identities and life stories of the foundational Japanese chemists are relatively unknown outside of Japan. Many Japanese historians of chemistry often write in Japanese (15-17). Even some of the great Japanese chemists, e.g., Kenichi Fukui, wrote their autobiographies in Japanese (18), and unfortunately, it is only in their first language that those autobiographies were published and accessible only to readers of East Asian languages. Was Fukui's autobiography, for example, considered by the publisher not to have a market outside of Japan? Erich Hückel's autobiography (19) remains solely in German. But Richard Willstätter's autobiography (20) was translated from German to English. All the autobiographies in the

Profiles, Pathways and Dreams series were published in English (though Vladimir Prelog's autobiography (21) was initially written in German by Prelog and translated into English by David Ginsburg and O. T. Benfey), an example of the application of a *lingua franca* and perhaps also the fact that the publisher was the American Chemical Society.

As mentioned above, one of the *Profiles* authors was Nozoe (1902-1996) (Figure 2), the doyen of late 20th century Japanese chemistry. Nozoe was first to discover the structure of a non-benzenoid aromaticity (hinokitiol), though his achievement was published in Japanese in the early 1940s and so was recognized in Europe and America only *after* Michael Dewar in England and Holgar Erdtman in Sweden had made their simultaneous independent discoveries in the late 1940s (22) Nozoe's professor in the early 1920s was Riko Majima (Figure 2) (23, 24). Majima (1874-1962) was the seminal Japanese chemist from whom the first generation of major Japanese organic chemists was produced.



Hinokitiol

I asked Nozoe to provide a listing of Majima's most successful and consequential students (first generation Majima descendants) and then a listing of the most important students of those professors (second generation Majima descendants). This is equivalent to family genealogical trees that trace one's family's ancestors. Within a short time, I received a fax containing Nozoe's derivation of Majima's family tree. A day or two later, I received a fax from Nozoe, that he was deleting the Majima family tree from his autobiography.

Nozoe had shared the family tree with some of his colleagues and was very strongly advised *not* to publish it. As Nozoe could not list every student of Majima's, by including some names and excluding others, Nozoe would be making public judgements about academic caliber. This would be a significant taboo within the Japanese culture which values harmony, group (which equals "family") loyalty, and the maintenance of good human relations (25-27).

What was I to do? Nozoe, then in his late 80s, was the last living person who could authoritatively document the record of early 20th century Japanese organic chemistry. It was now or never. I explained this conundrum to Nozoe. Multiple faxes went back and forth between us. Which would Nozoe choose: history of chemistry or the maintenance of cultural norms? Ultimately a very brave Nozoe chose HoC.

- For Nozoe to include the Majima family tree required Nozoe to face squarely his place in the HoC as well as his obligations to the culture in which he lived.
- With the development of mutual trust, the bond between interviewer and interviewee can rapidly become intimate. Responsibilities emerge for the editor to protect the author. Indeed, an interviewer must commit to an Editor's Hippocratic-like Oath: to do no harm, to protect the interviewee (and the author).
- My often almost daily conversations with Nozoe via fax built this trust and negotiated complicated issues smoothly and diplomatically. Thirty years after the publication of Nozoe's autobiography and 25 years after his death, we have this important documentation of the leaders of early 20th century Japanese organic chemistry.

For 40 years, from 1953 to 1992, Nozoe collected autographs and other writings during his worldwide travels. Many of these were published in his autobiography (22). You can watch a short video produced by Carman

Drahl and *Chemical & Engineering News* which includes short interviews of Roald Hoffmann and Carl Djerassi and me (!) discussing the Nozoe autograph books:

<https://www.youtube.com/watch?v=pqswFQrUdw8>.

Between 2012 and 2015, *The Chemical Record*, a Wiley-VCH journal published for the Chemical Society of Japan, published nearly 1200 pages of Nozoe's travelling autograph books in 15 consecutive issues of the journal (28). This idea was imagined by Eva Wille, a Wiley-VCH senior executive, to whom I actually "pitched" the idea of publishing all Nozoe's autograph books in a single volume. A strong relationship between authors, editors and publishers is critical, not just to "sell a book idea" but to bring it to its maximum fruition. The publication of the Nozoe autograph books was due to my relationship with Wille—and that it was a grand idea to do so!

Distance Interviewing by e-Mail

Distance interviewing has morphed considerably over the past 20 years. Letters and fax have been replaced by email. I shall give several examples of the use of email interviewing to determine the priority of achievements dealing with the development of the Woodward-Hoffmann rules published in 1965 by R. B. Woodward and Hoffmann (29-33). In 1981, Hoffmann received the Nobel Prize in Chemistry for this achievement. Had Woodward not died in 1979, he would have received his second Nobel Prize. In 1965 Woodward received a Nobel Prize in Chemistry for his total syntheses of complex natural products.

First, I shall discuss the Charles H. DePuy (Figure 3) story. In 2016, Veronica Bierbaum and Robert Damrauer, two professors at the University of Colorado, published a biographical memoir for the NAS of their former University of Colorado colleague Charles DePuy (1927-2013). Within their memoir, Bierbaum and Damrauer wrote (34),

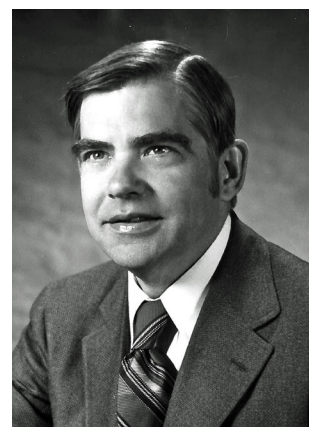


Figure 3. Charles DePuy, ca. 1965. Photograph courtesy C. DePuy.

[DePuy] did comment to friends that such ideas [the stereochemistry of ring-opening in cyclopropanol

solvolyses] had been raised with Roald Hoffmann at a meeting in October 1964, but they had not been attributed to Chuck in Woodward-Hoffmann papers until sometime later.

The implication is that Woodward and Hoffmann slighted DePuy. From my email interviews with Hoffmann, I knew the story was more complicated than what appeared in the NAS memoir. In fact, while Woodward and Hoffmann did fail to credit DePuy in their first 1965 paper (29), they cited DePuy fully in their reviews of the topic in 1968 (35) and in 1969 (36). In an email interview with me (37), Hoffmann acknowledged that he and Woodward

did not credit him properly. He complained, rightfully. We made up for it in [subsequent papers].

Recently I emailed Bierbaum and Damrauer. I asked them several questions.

JIS [Seeman]: By your inclusion of this matter in your memoir, are you implying that Chuck held any, even the smallest, degree of dissatisfaction with Woodward and/or Hoffmann regarding attribution?

B&D [Bierbaum and Damrauer]: Yes (38).

JIS: If yes, then was this your attempt to further set the record straight?

B&D: Yes (38).

Fortunately, I had interviewed DePuy on this credit matter. On July 17, 2011, DePuy wrote in an email (39),

To be perfectly honest, I did not notice until many years later that the cyclopropyl case was covered in the initial [1965] Woodward-Hoffmann communication. I guess I thought I knew what the communication was about and did not read down to the last line. I did notice their acknowledgement in their [1969] book, which seemed strange but welcome. Still, I do not believe that anyone behaved at all unethically and my hearing about the W-H rules at the [October 1964] Natick meeting gave me wonderful ideas for some very interesting chemistry.

Thus, Bierbaum and Damrauer's claim on behalf of DePuy against Woodward and Hoffmann in the NAS memoir is inconsistent with DePuy's position. I felt I had an obligation to help set the record straight. I wrote to them both. In a prompt email response, Bierbaum and Damrauer revised their understanding, writing to me (38),

We were not directly involved in the W-H issues and so don't have additional information. It's good that you were able to interact directly with Chuck before his passing; your reporting of these communications in your *Journal of Organic Chemistry* paper is the best reflection of Chuck's thoughts on the matter.

But their internet-available memoir stands unchanged (34) though this paper helps to set the record straight.

Next I shall tell a story involving the photochemist Howard E. Zimmerman (1926-2012) (Figure 4). Zimmerman, a former student of Woodward's, was a professor of chemistry at the University of Wisconsin for many decades. In his 2014 biographical memoir of Zimmerman also for the NAS, Richard S. Givens, a former graduate student of Zimmerman's and professor at the University of Kansas, wrote that in 1961 (40)

Howard [Zimmerman] was among the first to employ orbital *correlation diagrams* in assessing "allowed" and "forbidden" pathways for reaction processes controlled by orbital symmetry ... [emphasis added]

In their second 1965 communication (30), Woodward and Hoffmann used *correlation diagrams* as the theoretical basis for their proposed mechanism for cycloadditions but did not cite Zimmerman.

In 1961, Zimmerman and Arnold Zwiig derived a "molecular orbital reaction diagram" without symmetry assignments—a required component of correlation diagrams—and used LCAO-MO calculations as supporting evidence for the mechanism of several carbanion rearrangements (41). In his June 5, 2020, email responding to this author's email of June 4, 2020, about correlation diagrams, Givens corrected himself (42),

In answer to your question, the paper you cited for Zimmerman and Zwiig does not involve *correlation diagrams* but rather, questions of orbital overlap and electron population to determine ... [emphasis added]



Figure 4. Howard E. Zimmerman, Madison, WI, ca. 1980. In this photograph, Zimmerman is shown before a map of the world in which he placed brightly colored push pins to mark the location of his former students who held academic positions.

Thus, Givens agreed with my analysis: Zimmerman's 1961 paper with Zweig (41) did not include correlation diagrams.

I actually had interviewed Zimmerman quite extensively shortly before his death. Among many responses to me, in an August 7, 2011, email, Zimmerman described his own contributions to the field (43):

It was tragic for me that I had not seen the generality of Arnie Zweig's *correlation diagram* other than in the paper [*sic*] we did make clear that occupied bonding MO's going antibonding impede the reactions ... I have no claim on early discovery... My 1966 Möbius-Hückel article is where my interest begins. [emphasis added]

Zimmerman thus acknowledged that he had no claim, but he mischaracterized his 1961 publication. Zimmerman also believed he had presented a correlation diagram, and indeed, Zimmerman and Zweig were tantalizing close to have done so. But they did not.

I conclude,

- As scientists well know, science is complex, and so is writing about science. Often only subject-matter experts can examine the historical record in order to characterize correctly the science.
- Errors in attribution are easily made and hardly ever corrected. These are typically neither intentional nor deceitful. Nonetheless, attribution errors often remain permanently in the record, as the last statement of priority.
- Errors in remembering and characterizing one's work are often made. These are not always intentional. Robert K. Merton, the great sociologist of science and one of my heroes, used the term cryptomnesia (2, 44). According to Wikipedia, "Cryptomnesia occurs when a forgotten memory returns without its being recognized as such by the subject."
- Authors of biographical memoirs are often "academically-related" to the examinee, as former students or colleagues are most apt to write such treatises (and are most apt to be invited to do so). Various biases are possible if not likely, e.g., favoritism bias and conflict of interest bias. In these instances, attributions of credit must be made very carefully by the authors and considered very carefully by readers, if they are even aware of the familial relationship.

- As Merton discussed in detail, credit is a primary motive among scientists and an important issue for the history of science (2, 45). Great care must be done by biographers and others who attribute credit in their writings.
- Emailing is a rapid and highly interactive mode of distance interviewing.

Additional Observations and Conclusions

I have discovered that some individuals prefer telephone interviews to questions by email. There is little hope to convince a determined telephone-interviewee to do otherwise. Recording such a telephone interview is optimum. Otherwise, inaccuracies can enter the historical record. I much prefer distance interviewing by email. Interviewing by email is not unlike traditional letter correspondence or even oral histories regarding the interviewees' freedom to craft their narratives.

A reviewer has asked,

How do we judge the validity of eye-witness reports?
How do historians treat the biases of the interviewer and the interviewee?

These topics have generated a large and rich scholarship, for they are a very lively concern in the field of history of modern science. Trouble brews when individuals are inconsistent in their own responses or when the stories of different actors do not coincide. This concern is valid for all forms of interviewing. I have faced such instances several times. The following techniques have proven useful in my research: I attempt inquiry via different substantive trajectories; I seek testimony from multiple actors; I relentlessly examine testimony for consistency in responses over time for the same event; and I refer to published and unpublished documentation. Sometimes we simply must accept and document inconsistencies in the record. People are not always consistent in their behaviors.

Life is rich with vagueness (46-48). At such times, distance interviewing by email is especially useful; it is easy to inquire several times, over several days, to multiple individuals. Even faced with Rashomon's tribulations (49, 50), one never, ever alters an interviewee's narrative to fit one's interpretation of events or to be consistent with the memories of others or even to agree with relevant contemporaneous documentation.

There is another aspect of email interviews that ought to be highlighted: Interviews by email easily al-

low one to pursue newly opened areas of interest and clarify any possible ambiguities. All one needs to do is send another email loaded with the appropriate questions. Repeatedly, I find folks all over the world who immediately, happily, and rather spontaneously respond to emails. In my major current project on the history of the development of the Woodward-Hoffmann rules (51), which includes E. J. Corey's claim of plagiarism against Woodward (52, 53), I have hundreds of emails from Hoffmann and tens of emails from others who have been involved in this multidimensional story. The brightest times of my day are when I see a response in my inbox.

Looking Toward the Future

I do believe that modern distance interviewing—by email—is not just easier and faster than previous methods. It also provides more and more rapid opportunities with individuals who share rich historical data and very personal perspectives. I also believe that the future will provide possibilities in distance interviewing far beyond our anticipations. Even today, one can imagine video interview segments being included in journal articles that are accessed on-line—just as I have included a *url* link to a short video about the Nozoe autograph books herein. With character recognition, rapid on-line translations, and artificial intelligence-facilitated capabilities not yet even imagined, the ability to bring HoC to the world's communities at levels from kindergarten to research level is unlimited. Distance interviewing and recording on-line video interviews provide additional, powerful incentives for the study of the history of modern chemistry—a topic mostly avoided by professional historians (11).

Hoffmann has characterized scientists as “scrabblers” (54). I claim that all researchers, including historians of chemistry, are scrabblers. Scrabblers seek more and better information for their research using whatever tools and resources are at their disposal. Some researchers are more enterprising if not enthusiastically ambitious, in their pursuit of information. Others are less so. Such is the way of research and of interviewing.

Coda

An unstated assumption in this paper is that the history of modern chemistry is a legitimate historical endeavor. In another article in this special issue of the *Bulletin* (55), Peter Morris and I have briefly discussed the need for an increased study in the history of modern chemistry—a topic that Morris has spoken of previously

(11). I have been told informally by several historians that science must be at least 25 years old to be considered appropriate for a study of the history of chemistry. In another article in this special issue, Carmen Giunta argues otherwise, and quite persuasively (56). It is quite telling that the philosopher of science Thomas S. Kuhn wrote (57):

[The history of] science should be learned from the textbooks and journals of the period he studies.

Besides using the term “he” to represent historians of chemistry, an affront to today's sensibilities and realities, Kuhn, in his quote, did not consider interviewing scientists who were participants or active observers of the historical events. This is, of course, the entire subject of my present paper and many of my own contributions to the history of chemistry.

I owe a debt to Derek H. R. Barton (1969 Nobel Prize in chemistry); I follow quite willingly Barton's precedent (58) for my use of codas in my own writing.

Coda to a Coda

Since the submission of this paper, I have begun conducting interviews using Zoom and Skype, during which I type my subjects' responses to my questions as if I were a court reporter. Video interviews have been accomplished quite effectively with Roald Hoffmann and Sason Shaik. The interactive nature of such interviews provides for enormous flexibility in topic and for intense focus on issues that arise during the sessions. Hoffmann and I have each drawn graphics and showed these and other documents to each other using the video camera or by concurrent emailing of attachments. These video interviews have lasted from one to two hours and must now be considered another and very effective form of distant interviewing.

Acknowledgments

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