THE PERSONAL FACTOR: DON YOST AND THE (LACK OF) PROGRESS IN INORGANIC CHEMISTRY AT CALTECH, 1920-1965

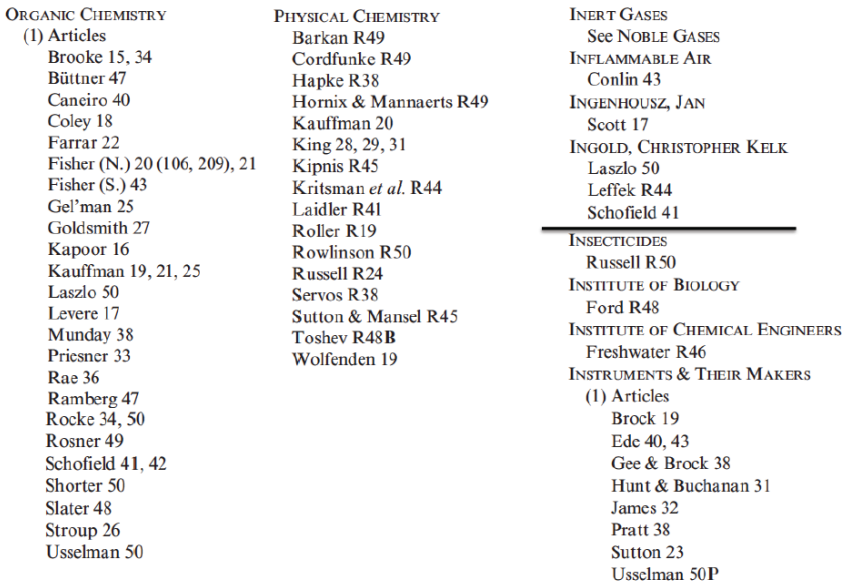
Jay A. Labinger

Introduction

When A. A. Noyes came to Caltech in 1920, he established a program in chemistry that rapidly attained world-class recognition. However, inorganic chemistry played a relatively minor part in that department, a condition that persisted until the mid-1960s, especially during the post-World War II period. To be sure, the same could be said of most major US academic chemistry departments: as I have documented in a recent book,[1] inorganic chemistry was seriously underrepresented, relative to organic and physical chemistry, until well past mid-century. That claim is supported by a wide variety of metrics, such as number of faculty members, papers in the *Journal of the American Chemical Society*, presentations at national meetings of the ACS, election to membership in the US National Academy of Sciences, *etc.* Many chemists conflated inorganic chemistry with general chemistry, treating it essentially as a non-specialization worthy of little special attention. A typical comment was offered by American inorganic chemist Fred Basolo (who played a major role in changing that state of affairs):

> [In 1943] it was believed that beginning general chemistry covered inorganic chemistry, and that no further course on it need be offered nor was there any reason for doing research in the area.[2]

This attitude persisted for even longer — indeed, arguably still persists — among historians of chemistry. As an illustration, an index to *Ambix* includes some 30 and 20 articles cited under organic and physical chemistry respectively; but none at all for inorganic (Figure 1).



ORGANIC CHEMISTRY
(1) Articles
Brooke 15, 34
Büttner 47
Caneiro 40
Coley 18
Farrar 22
Fisher (N.) 20 (106, 209), 21
Fisher (S.) 43
Gel'man 25
Goldsmith 27
Kapoor 16
Kauffman 19, 21, 25
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Levere 17
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Ramberg 47
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Rosner 49
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Shorter 50
Slater 48
Stroup 26
Usselman 50

PHYSICAL CHEMISTRY
Barkan R49
Cordfunke R49
Hapke R38
Hornix & Mannaerts R49
Kauffman 20
King 28, 29, 31
Kipnis R45
Kritsman *et al.* R44
Laidler R41
Roller R19
Rowlinson R50
Russell R24
Servos R38
Sutton & Mansel R45
Toshev R48**B**
Wolfenden 19

INERT GASES
See NOBLE GASES
INFLAMMABLE AIR
Conlin 43
INGENHOUSZ, JAN
Scott 17
INGOLD, CHRISTOPHER KELK
Laszlo 50
Leffek R44
Schofield 41
INSECTICIDES
Russell R50
INSTITUTE OF BIOLOGY
Ford R48
INSTITUTE OF CHEMICAL ENGINEERS
Freshwater R46
INSTRUMENTS & THEIR MAKERS
(1) Articles
Brock 19
Ede 40, 43
Gee & Brock 38
Hunt & Buchanan 31
James 32
Pratt 38
Sutton 23
Usselman 50**P**

Figure 1. Excerpts from the index to 1937-2003 issues of *Ambix* (2004)

At Caltech, though, the second-class status of inorganic chemistry was *not* simply a manifestation of general trends. To a substantial degree it can be largely connected to a single individual, the department's (lone) inorganic chemist, Don Yost (Figure 2). An extensive picture of Yost's personal and professional

life may be gleaned from the impressively large collection of papers that he deposited in the Caltech archives, as well as from reminiscences of his colleagues and students. Yost was undeniably an accomplished and respected inorganic chemist, who made significant contributions to the field. Nonetheless, his personality, attitudes, and behavior — especially his relationship with Linus Pauling, the major figure of power in the department during most of this period — were key factors in retarding the growth of inorganic chemistry at Caltech. It has even been suggested that they were in part responsible for the failure to achieve a major breakthrough— the first preparation of an inert gas compound — nearly thirty years ahead of Neil Bartlett, a missed opportunity that would surely have substantially altered the subsequent course of inorganic chemistry both at Caltech and throughout the world.

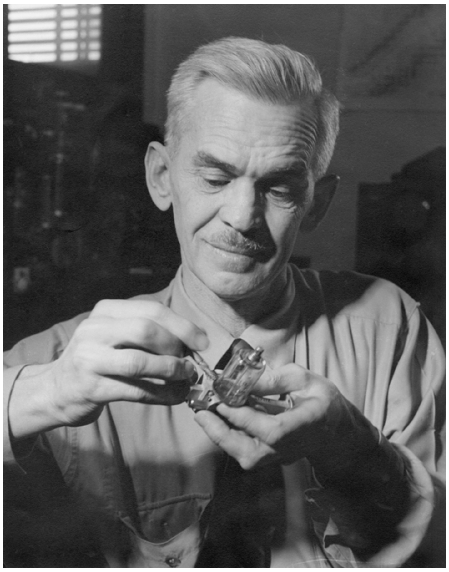

Figure 2. Don Yost in 1948. Courtesy of the Archives, California Institute of Technology.

Yost, Pauling, and inorganic chemistry at Caltech

The 1920 Caltech catalog mentions no professorial faculty member (there was one instructor) identified as an inorganic chemist, and although the instructional listings include a two-term course titled “Inorganic Chemistry,” the description makes it clear that it was the general chemistry course taken by all Caltech students in their freshman year — a manifestation of the common tendency to conflate inorganic and general chemistry. The explicit appearance of inorganic chemistry — both in teaching and research — dates from 1926, the year Yost was appointed to the faculty.

Don Merlin Lee Yost (1893-1977) came to Caltech as a graduate student in 1924, and completed his Ph.D. with Noyes in two years; his thesis, very much in the mainstream of inorganic chemistry (which Noyes was not), was titled "The Mechanisms and Rates of Certain Oxidation-Reduction Reactions in Aqueous Solution. The Existence of Trivalent Silver." For the next 15 years he carried out an active program in physical inorganic chemistry, eventually yielding over 100 published papers, as well as introducing and teaching advanced undergraduate and graduate courses in inorganic chemistry. His work was considerably interrupted by World War II, during which Yost participated extensively in the war effort until he became seriously ill. Afterwards he returned to active research, but only for a few years: he stopped taking graduate students in the early 1950s, and his last (and most notable) two students, John Waugh and James Shoolery, worked not in inorganic chemistry but on (self-initiated) projects in NMR and microwave spectroscopy, respectively. By the mid 1950s he had essentially withdrawn from active chemical research, turning his scholarly interests to Boolean algebra. He retired from Caltech in 1964.[3]

Temporally, Linus Pauling's career at Caltech closely matched Yost's: Pauling started as a graduate student at Caltech (in 1922) and after receiving his degree (following a lengthy sojourn in Europe) joined the faculty in 1927, leaving in 1963.[4] In just about every other way, though, they were very different. Despite their similar origins (both having grown up in the then still somewhat wild West — Pauling in Oregon, Yost in Idaho), they exhibited widely divergent personality traits: Pauling avidly sought visibility and leadership roles, both at Caltech and in the worldwide research community, while Yost was a much more private sort. Nonetheless, initially they apparently got along well enough, as indicated by a 1931 letter of Pauling's:

> If I were to come to M.I.T., I should desire an appointment in physics or in physics and chemistry. And yet I am really not very much interested in physics, but rather in what may be called structural chemistry, and so I prefer being in a chemistry department. Here there are several men in our chemistry department whose interests touch on mine — Tolman, Badger, Dickinson, and Yost especially.[5]

and a joint 1932 paper,[6] not to mention their collaboration on the quest for a xenon compound (*vide infra*). But signs of friction (to say the least) appeared soon thereafter. Pauling's 1944 report on the "Present State and Future Prospects" of the Caltech chemistry division completely omitted Yost's name and any mention of coursework or research in inorganic chemistry.[7] Yost returned the favor in a 1958 memo (by which time Pauling had stepped down — under some duress resulting from his political activities — as chair), summarizing his view of the historical highlights of Caltech chemistry. His list included 14 areas of excellence and 20+ names; Pauling was not mentioned except for an (unmistakable) allusion: "There are reptiles who would induce us to believe that Chemistry was devoted chiefly to molecular structure."[8] Likewise: "The unnamed person…is extremely naïve and, as two Deans put it, pathologically single tracked and nuts."[9]

Figure 3. Faculty of the Division of Chemistry and Chemical Engineering at Caltech, 1950. Don Yost is at the left end of the third row; Linus Pauling is third from left in the second row. Courtesy of the Archives, California Institute of Technology.

To be sure, Yost's curmudgeonly attitude was not reserved for Pauling: aside from a small circle of intensely loyal former students, colleagues, and friends (the so-called "Iron Nail Club"[3]), he displayed it towards almost any topic or group: inorganic chemistry at Caltech;

> There isn't much inorganic chemistry being done here at this Institute these days. The reasons are various, but some of them were due to struggles (competitions). First there was the struggle between professors and

> bookkeepers; the bookkeepers won. Then there was the struggle about admitting girls to the graduate school; the girls won. Next came the struggle between organic-biochemistry and physical-inorganic chemistry; the organikers won. Along with these struggles came one between professors and politicians; the politicians have almost won. Being of a scholarly turn of mind I side-stepped the struggles by turning to mathematics."[10]

research at his undergraduate alma mater UC Berkeley;

> It was a pleasant surprise to have your letter of August 26 and to learn that someone at Berkeley is still interested in chemistry. I hope you will forgive me when I say that I had supposed that my alma mater had dropped out of the picture completely, as far as chemistry is concerned. Matter of fact, I got the impression that the whole staff was on some Pacific island gambolling, with vine leaves in their hair, among the palm trees and around huge casks filled with gold.[11]

the coordination chemistry community in general (despite his having signed a contract for a book on the subject, which was never completed);

> There has been a vast amount of work done on coordination compounds, but when you come right down to it none of it goes much further than Werner left the subject.[12]

and even himself.

> For one who can get up a lot of steam about scholarly subject matter but who does not like to teach classes, I get into the most remarkable predicaments of anyone I know. I'm not a good teacher, and I often marvel about how I happened to get into such a profession. But its (*sic*) too late to do anything about it now.[13]

But the Pauling-Yost friction was clearly something special. Minutes of chemistry divisional meetings show Yost having a near-perfect non-attendance record from the early 1950s until Pauling was replaced as chair by Ernest Swift; and it was well known among faculty that Yost would automatically oppose anything proposed by Pauling.[1] Pauling (who as chair had virtually complete control over hiring) took no action to strengthen inorganic chemistry within the department: when organic chemist John Roberts, who had recently moved from MIT, suggested approaching Henry Taube, a rising start in the field, Pauling expressed no interest whatsoever.[14] While other factors may well have been involved, it seems virtually certain that the poisonous relationship between Yost and Pauling played a role in that stance. Not until Pauling left (in 1963) and Yost retired (in 1964, though as noted above he had long been inactive in the field by then) was a move made to resuscitate inorganic chemistry, by recruiting Harry Gray from Columbia. Since then, to be sure, Caltech has built up one of the top two or three inorganic programs in the country; but the process didn't begin until the personal factor was eliminated.

The xenon story

In 1932 Pauling developed the idea, based on his theoretical work, that the "inert" gas xenon could react with fluorine, and — after borrowing a sample from his former professor Fred Allen — arranged with Yost to carry out some experiments. They did not succeed.

> I should like to do some work (with Professor Yost) in an attempt to prepare certain compounds of Xenon suggested by theoretical arguments. No doubt your xenon is precious; if, however, you could lend us 10 cc. or so (of not necessarily pure stuff), we would try to return it to you either as such or in some compound (I hope), and we would be properly grateful. If this is asking too much, or if you can't lend it, could you give us advice as to where we might possibly obtain some?[15]

> One daring prediction Pauling made was that fluorine was so electronegative it would form compounds even with an inert gas like xenon….Pauling managed to obtain a little of it from a colleague and gave it to Yost, who worked through the summer of 1933 searching for the predicted compounds. He failed to find any — a failure that Pauling found both confusing and galling. The reasons for Yost's inability to find what he was looking for are uncertain.[16]

Yost (working with his graduate student Albert Kaye) published the negative findings in a 1933 *Journal of the American Chemical Society* article, describing their procedures in considerable detail, and concluding "It cannot be said that definite evidence for compound formation was found. It does not follow, of course, that xenon fluoride is incapable of existing."[17] After Neil Bartlett reported the first compound of xenon in 1962,[18] followed in short order by a large number of further publications from other groups, some commentators — less agnostic than Hager in the quote above — proposed possible reasons for Yost's failure. Most of these were technical — choosing the wrong material (quartz) for the reaction vessel; employing the wrong means of initiating reaction (electrical discharge instead of light); even that they did succeed and just failed to recognize it — but Pauling, as well as his National Academy of Sciences biographer Waugh, thought that the relationship between Yost and Pauling might well have been a factor:

> Freddy [Allen] sent me about 200 milliliters of gaseous xenon for the experiment. I wasn't the experimenter. I got Don Yost to try to make it, and he reported that he didn't succeed. He had a nickel apparatus, and he couldn't see inside it. The man who later made the xenon compounds….said he was sure that Yost had made xenon difluoride, but had failed to recognize that he had. I think he may have been measuring the change in pressure but I don't remember just how the experiment was carried out. So Yost reported in a paper that you couldn't make xenon fluoride….Someone at CIT said that he thought this was about the most unenthusiastic investigator who ever carried out an investigation. I judge that Don did this just because I asked him to, but perhaps he was convinced that it would be a failure.[19]

> I signed up for research with Don Yost, who was a colorful and crusty character, and a man of catholic interests….In 1933, at Pauling's instigation, Yost and A.L. Kaye had tried to make xenon halides but failed. That may have been because Yost did not like Pauling very well; Neil Bartlett once told me that Yost's experiment must have created xenon compounds.[20]

I have discussed the above-mentioned technical interpretations elsewhere,[21] and will not repeat the details here. In brief, while *none* of the prior suggestions offered hold up, being based on imprecise understanding and/or memory of what Yost and Kaye (as well as other researchers) actually did, there *is* a plausible explanation for Yost's failure: a combination of too-low Xe pressure and too-short reaction times, neither of which can be fairly called a mistake, given the limited understanding that was available about reaction mechanisms. Given that, along with the (circumstantial) evidence of reasonably good relations between Yost and Pauling at the time of the work described earlier, it seems to me hard to sustain an argument that the personal factor could have played a significant role here. But it clearly *did* play such a role in the course of the evolution of inorganic chemistry at Caltech; and even the suggestion that it *could* have been a factor in what would have been such a landmark event for the field as a whole is quite striking.

References

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[9] Letter from Don M. Yost to Harold S. Johnston, August 27, 1958. Courtesy of the Archives, California Institute of Technology.
[10] Letter from Don M. Yost to Ludwig F. Audrieth, November 21, 1963. Courtesy of the Archives, California Institute of Technology.

[11] Letter from Don M. Yost to Leo Brewer, August 31, 1954. Courtesy of the Archives, California Institute of Technology.
[12] Letter from Don M. Yost to Lindsay Helmholtz, March 30, 1964. Courtesy of the Archives, California Institute of Technology.
[13] Letter from Don M. Yost to Rev. Joseph Duke, October 30, 1959. Courtesy of the Archives, California Institute of Technology.
[14] Fred C. Anson, personal communication, 2012.
[15] Letter from Linus Pauling to Fred Allen, September 13, 1932. Courtesy of the Ava Helen and Linus Pauling Papers, Oregon State University Libraries.
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[17] Don M. Yost and Albert L. Kaye, "An Attempt to Prepare a Chloride or Fluoride of Xenon," *J. Am. Chem. Soc.* 55 (1933): 3890-3892.
[18] Neil Bartlett, "Xenon Hexafluoroplatinate(V) $Xe^{+}[PtF_6]^{-}$," *Proc. Chem. Soc. London* (1962): 218.
[19] Linus Pauling, interview by Jeffrey L. Sturchio at Executive Tower Inn, Denver, Colorado, 6 April 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0067).
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