

## WHY ISN'T NOBLE GAS CHEMISTRY 30 YEARS OLDER? THE FAILED (?) 1933 EXPERIMENT OF YOST AND KAYE

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### Introduction

We know of a number of unsuccessful attempts to synthesize a noble gas compound (1)—probably there were others of which we do *not* know—that preceded the first announcement by Neil Bartlett in 1962 (2), now a little over half a century old. Perhaps the best documented of these was reported in a 1933 paper in *JACS*—a relatively rare instance of publishing of a negative finding!—by Caltech chemistry professor Donald M. Yost and his graduate student Albert L. Kaye (3). After Bartlett's success, which was quickly followed by others, Yost's failure became a subject of some interest, probably due in part to the significant role of Linus Pauling. A variety of explanations have been offered for why Yost was unable to generate any compound of xenon with fluorine—or, perhaps, that he *did* generate something but failed to recognize it. But these would-be explainers appear to have relied mostly on their *recollection* of what Yost and others did; and in many cases that recollection was faulty. Examination of the actual details of Yost's paper, in comparison with those of the later successful reports, shows that the subsequent interpretation by a number of commentators—researchers, reviewers, biographers—has been imprecise, unsupported, or just plain wrong. On the other hand, one particular detail of Yost's experiment, which appears to have gone completely unnoticed, offers a plausible explanation for why Yost did not—though he could well have—beat Bartlett and his contemporaries by nearly 30 years.

### Linus Pauling, Before and After Yost's Experiment

Unquestionably Pauling instigated Yost's work. According to one of Pauling's biographers (4):

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.

Note that Hager, unlike other commentators, is (deliberately?) agnostic with regard to the reasons for the failure. But his facts are well documented. In particular, we have three letters from Pauling to Fred Allen, one of Pauling's former professors who had moved to Purdue, and who possessed a modest supply of xenon. The first requests the loan of a sample (5):

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?

while the second (6) and third (7) report the unhappy results:

At last I can send you some information regarding the xenon experiments, which Dr. Yost has been carrying on, inasmuch as he is thoroughly experienced in the chemistry of the halogens. He found that he could not prepare a compound of xenon with either chlorine or fluorine by any of the means that he tried, and he has now given up the investigation. He and his student, Kaye, have sent a note on the experiments to the Journal of the American Chemical Society, in which they thank you for providing the xenon, I am sorry that the experiments have turned out in this way since I felt confident that xenon would combine with fluorine, at any rate, Yost obtained some red crystals, which he at one time thought contained xenon, but which he later decided were the pink form of hydrogen chloride.

The 70 mm of xenon was the pressure in mm of mercury, the volume of the system being 300 or 400 cc. Hence there was plenty of xenon present. Yost should have mentioned the volume. We still have the xenon, but Yost would like to make another try at preparing a compound (unless you want the xenon returned soon). I still think  $\text{XeF}_6$  [*sic*] should be stable.

Why did Pauling turn to Yost (and Kaye) to test his rather revolutionary idea? Yost (Figure 1) was a (self-described) physical inorganic chemist—indeed, the only inorganic chemist at Caltech at the time—who had considerable experience in (and taste for) gas-phase experimentation with difficult materials, such as chalcogen halides (8) and interhalogen compounds (9); in contrast, Pauling's work at the time (and subsequently) was virtually exclusively devoted to theory and structural determinations. They had already co-authored one paper, on electronegativity and ionic contributions to covalent bond strengths (10), although the paper reads very much like entirely Pauling's ideas, with Yost contributing only some interhalogen bond energy data. To propose a collaboration must have seemed obvious to Pauling.

Albert Kaye arrived at Caltech in September 1932 to begin work on a Ph.D. in *physics*, but apparently chose to work with Yost, a professor in a different department. (Such freedom is still available to Caltech graduate

students.) The *JACS* paper with Yost is the only extant record of his time at Caltech (he also appears as a co-author on several papers on electrochemistry with MIT professor M. de K. Thompson, the earliest dating from 1932; presumably these arose from his undergraduate research); there was no thesis, and he left Caltech after just one year (11). I have not been able to find out anything about his subsequent career.

A couple of years after Bartlett and others opened up the field of noble gas chemistry, Pauling reminisced about the failure in the course of a lecture (12)

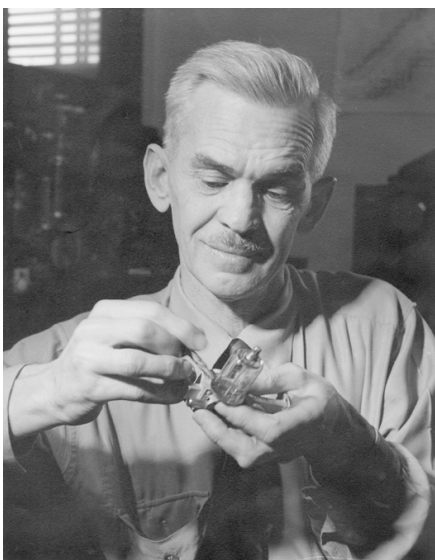
I think it's good to be skeptical about a lot of things. This xenon business was a lesson to me but I don't know just what it taught me. [laughter] In 1932, I think it was, I said that it ought to be possible to make xenon hexafluoride  $\text{XeF}_6$ , to make a hydroxy

compound  $\text{Ag}_2\text{H}_3\text{XeO}_6$ . This has been made, by the way, these compounds have been made -- Professor Yost and a student name Kaye, Albert Kaye. It was hard to get Xenon in those days. My early teacher of physical chemistry of Corvallis had some xenon which he sent to us -- a couple of hundred of milliliters of the gas and it may be that Yost and Kaye really had some xenon fluoride but just failed to observe that they had that when they did this work in '33 and '34.

and again, still later (and with considerably more evident bitterness) during an interview archived at the Chemical Heritage Foundation (13)

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.

Several points should be noted: the possibility, offered as Pauling's own opinion in the first quote and



**Figure 1.** Don Yost, photograph taken in 1948. Courtesy of the Archives, California Institute of Technology.

attributed to Bartlett in the second, that Yost *had* made  $\text{XeF}_2$  but didn't realize it; Pauling's memory—although he acknowledges almost immediately that his memory is unclear—of Yost's having done the experiment in an opaque nickel apparatus; the suggestion by “someone” at Caltech that Yost had never been really committed to the effort and might have been predisposed to failing; and Yost's conclusion (per Pauling) that xenon fluoride could not be made.

### Bartlett and Other Commentators

Neil Bartlett spoke at length about Yost's experiment, long after his own discoveries, in the course of a 1999 interview (14):

Linus was so convinced that there should be some xenon chemistry that he wrote his old teacher Fred Allen, at Purdue, and begged from him a sample of xenon. It was difficult to get samples of xenon in those days. Linus then took xenon to his Caltech colleague Don Yost, who had a student, Kaye, and Yost and Kaye put xenon and fluorine into a quartz bulb. Then they made a mistake. They should have gone out into the Caltech sunlight. Instead, they passed an electric discharge through the bulb and all they got was attack on the quartz container. Quartz is not very resistant to fluorine.... They reported this in the *Journal of the American Chemical Society* in 1933 in a paper on the nonreactivity of xenon with fluorine. That, I suppose, persuaded Pauling that he had been a little too optimistic. The xenon fluorides are close to the limit of thermodynamic stability, after all, and accessible xenon chemistry depends upon that slight stability....

[Hargittai: How did Pauling react, 30 years later, to your discovery?]

He was informed very early on by myself. His reaction was that Yost and Kaye had distinctly missed the boat. He, Pauling, had made the right prediction. Of course, Yost could have suggested to Linus to try it for himself. I never met Don Yost but he writes in an engaging and amusing way. He must have been quite a character.... What we do with fluorine today is done at room temperature by just using photons to dissociate the fluorine. Everything is done in fluorocarbon plastic.... Such containers did not exist in Don Yost's days. He was working in quartz because he knew well that fluorine is extremely difficult to handle in ordinary glass. You could use Pyrex if you got it really dry. Yost probably didn't know that and he probably had some HF in his fluorine.

Bartlett's recollections differ significantly from Pauling's (as well as being much more detailed; it seems likely that

he refreshed his memory by reference to Yost's paper shortly before, or even during, the course of the interviews!), particularly with regard to the reaction vessel: quartz according to Bartlett whereas Pauling remembered (incorrectly, as we shall see) nickel. There is no suggestion of a poor attitude on Yost's part (of course, Bartlett never knew Yost personally, unlike Pauling), nor that he believed (as Pauling says) that Yost had unknowingly made  $\text{XeF}_2$ . Bartlett attributes the failure to two “mistakes”: the use of a quartz apparatus, and the choice of electric discharge rather than photochemical activation.

The latter argument had already been offered in an earlier review (15):

Others also thought of attempting combination of halogens with noble gases in the 1930s, among them Pauling. He was convinced that xenon and fluorine, at least, should react and, at his suggestion, Yost and Kaye tried a discharge experiment. Their failure to gain a definitive result was cruelly unfortunate since, exposure of xenon-fluorine mixtures to sunlight instead of electric discharge, when tried some 30 years later, produced  $\text{XeF}_2$ .

There is an intriguing but unresolved aspect of this quote: did Pauling specifically suggest the discharge method—a mistake, according to Holloway and Bartlett—or just the attempt, leaving the choice of method to Yost? I have not found any support for the former interpretation, or any indication of why Holloway might have said that, if that is indeed what he meant. In any case, as we shall see shortly, it is far from clear that it *was* the wrong choice, no matter who made it.

John Waugh, one of Yost's last graduate students (and by far the best-known, for his subsequent career in NMR), addressed the episode as part of a biography of Yost that he provided for the National Academy of Sciences (16):

The chemistry of rare or “difficult” elements was always a challenge to Yost. Fluorine chemistry was no exception. Indeed, Yost earned an international reputation for his work on the volatile inorganic halides. Apparently the notion arose in the early 1930s (probably from Linus Pauling) that xenon, a “noble gas” guaranteed by all the textbooks to be chemically inert, might form chemical compounds with fluorine, the most electronegative element. Yost (who would not have used the word electronegative) and Albert L. Kaye describe in a 1933 paper a failed attempt to prepare such compounds. Neil Bartlett, who won fame many years later for preparing xenon fluorides, considers it nearly certain that such compounds must

have been created under the conditions used by Yost and Kaye. We can only speculate on the reasons for their negative result on an experiment which might have had a revolutionary effect.

As we have seen, credit for the inspiration definitely, not “probably,” belongs to Pauling. The comment about Yost’s attitude towards electronegativity is interesting in light of a paper he co-authored with Pauling (10), which is thoroughly imbued with the concept; while Yost may well have contributed little to the actual writing, as I suggested, at least at that time he was willing to put his name to an article centered thereupon. I suspect that his strong antipathy—if Waugh is correct about that—developed subsequently, not out of any scientific commitment but more from his personal feelings about Pauling, which will be discussed shortly.

Like Pauling’s biographer Hager, Waugh is here rather circumspect about possible causes for the failure; but in his later personal memoir he permitted himself somewhat more speculation (17):

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.

Again we have a statement that Bartlett believed Yost had in fact succeeded, although Bartlett himself says no such thing in his interview. More importantly, this is the strongest suggestion that Yost’s relationship with Pauling could have contributed to his failure.

Lastly, what did Yost himself have to say? There is a huge collection of Yost’s papers in the Caltech archives but, unfortunately, they only date back to around 1940, so there is no contemporaneous documentation, and no commentary in any of his later personal papers. He did write a brief introductory article (18) to a 1963 monograph, a collection of articles on the then-new field of noble gas chemistry, which included the following:

My sole excuse for being a contributor to this important book rests in the fact that, as mentioned above, Albert Kaye, then a graduate student, and I tried without success to bring about reaction between xenon and both chlorine and fluorine. . . . We were on our own throughout. We constructed our own apparatus, blew our own glass, and used cast off Ford coils as a source of high potentials. . . . Our sole supply of xenon was some 200 cc. at less than one-half atmosphere pressure which had been kindly loaned to us by Dr. Fredrick John Allen of Purdue. Furthermore, we had

to construct and operate our own (temperamental) fluorine generator. . . . The techniques (or art) of handling fluorine and its generators were in primitive stages of development. . . . There may, of course, be serpents who will say, and possibly with some reason, that if in spite of undeveloped techniques we had worked harder and more exhaustively we would have succeeded in preparing one or more xenon fluorides. But the simple fact is that we didn’t succeed. . . . Mr. Kaye and I will have to rest content with the fringe virtue of having said in print that we hadn’t proved by our experiments that a xenon fluoride was incapable of existing.

One can’t help noticing the absence of any mention of Pauling: in Yost’s account the xenon was loaned to “us,” not Pauling. By this time Yost could hardly bring himself to use Pauling’s name, often using phrases such as “the unnamed person (19).” (Actually Pauling probably *is* mentioned, in a way: I have little doubt that “serpent” was intended to refer to him.) But aside from that, Yost notes (correctly) that the paper explicitly left open the possibility of the existence of xenon compounds, whereas Pauling’s reminiscences (see above) implied Yost had concluded they could *not* be made. Yost puts his failure down to “undeveloped techniques,” particularly, it would seem, the difficulty of working with  $F_2$ .

### Yost and Pauling

Is there any support for the suggestion, made most explicitly by Waugh but also detectable in other commentaries, that the personal relationship between Yost and Pauling played any causal role in Yost’s failure? There is no question that Yost and Pauling developed an intense dislike for one another at some point in time, as I have documented, based on the Yost archives (19). Hager suggests it began when Pauling assumed the chairmanship in 1937, and began to favor his own field of structural chemistry as well as biochemistry at the expense of physical and inorganic chemistry, although the discord may well have been exacerbated when Bartlett’s discovery reawakened Pauling’s memory of Yost’s failed effort (20):

Don Yost, a highly opinionated, individualistic inorganic chemist, became “somewhat antagonistic to me,” Pauling remembered, in part because of his unhappiness at the shift in emphasis away from his field and perhaps in part because of lingering bad feelings over a fiasco in which Yost failed to find the xenon compounds that Pauling had predicted should exist. Yost stayed at Caltech until he retired, often at loggerheads with Pauling, the sole dissenting vote



in many divisional decisions, his rancor barely concealed and growing to the point where he and Pauling sometimes stopped speaking to each other entirely.

The antagonism was full-blown by the 1940s—and was surely not all one-sided, as Hager's quote might imply: Pauling completely, and clearly deliberately, omitted Yost from a listing of chemistry faculty in a 1944 memo (19). Yost later returned the favor when in 1958, after Pauling had left Caltech, his successor as chair, Ernest Swift, requested a historical summary of "Departmental Accomplishments" from each faculty member. Yost provided a list (21) of some 14 areas of excellence and 20 or so names, with Pauling left out entirely—except for the sentence, "There are reptiles who would induce us to believe that Chemistry was devoted chiefly to molecular structure," a clear allusion to the unnamed enemy (see "serpent" above).

But there is no evidence that I can find of hostility dating as far back as 1932-33. The only significant earlier reference to Yost in the Pauling archives is in a 1931 letter to John Slater, declining an invitation to move to MIT (22):

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.

That certainly *sounds* as though they still got along well enough in 1931. When we also consider the co-authored paper of 1932 and, not least, that Pauling turned to Yost to try his great xenon idea and that Yost (apparently) readily agreed, it is hard to see any justification for including a personal factor in accounting for the failure.

### What Yost (and Others) Really Did

If we rule out the personal factor, Yost's failure would have to be ascribed to one or more technical deficiencies or errors. As we've seen, the various suggestions offered—wrong materials, use of discharge instead of light, impure reagents—were often based on fuzzy and even contradictory recollections of the actual experiments done by Yost, as well as the successful ones that followed Bartlett's breakthrough. What are the facts?

Yost and Kaye's first attempts were with  $\text{Cl}_2$ ; most probably it was more readily available than  $\text{F}_2$ . That work was carried out *both* in quartz and in Pyrex, and they tried

*both* light and electric discharge to initiate reaction. Not surprisingly (from what we now know), nothing happened (except for formation of a small amount of the colored modification of HCl mentioned by Pauling) (3):

A mixture of xenon (70 mm.) and chlorine (225 mm.) contained in a quartz tube was irradiated with the light from a mercury arc in Pyrex and in quartz, and from a carbon arc.... In no case was any pressure change observed.... The mixture was next transferred to a Pyrex bulb equipped with tungsten electrodes. After sparking for varying periods of time and then condensing the contents of the bulb ... the red solid noted above appeared.... No definite evidence for the presence of a xenon chloride was found.

They then turned to fluorine; but here they worked *only* in quartz, not Pyrex, and with electric discharge, *not* with light (3):

It was found that fluorine, carefully freed from hydrogen fluoride, could be kept for an indefinite period in quartz bulbs which had been baked out under vacuum.... A mixture of some 600 mm. of fluorine and 30 mm. of xenon was prepared in an all-quartz apparatus provided with copper electrodes.... A high voltage (30 k.v.) discharge was then passed through the mixed gases for varying intervals of time. The contents of the apparatus were condensed out frequently, but the appearance and properties were no different from those observed before applying the discharge. It was noted that the side tube was appreciably attacked after some time, and this might be due to the action of a reactive xenon fluoride.... 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.

In hindsight, it seems strange that Yost and Kaye used a wider range of experimental conditions with chlorine than fluorine. Yost surely understood (for example, from his joint paper with Pauling (10)) that a fluoride was much more likely to be stable. It is true that preparing the  $\text{F}_2$  sample entailed more work; but the  $\text{Cl}_2$  experiments were all done on a single preparation of mixed gases, so that shouldn't have been an issue. Why did they use only quartz? Bartlett's suggestion, that Yost didn't realize fluorine could be handled safely in Pyrex, seems reasonable. More importantly, why didn't they try light irradiation? I can only surmise that the fact that  $\text{F}_2$  is colorless, unlike  $\text{Cl}_2$ , might have made them think that photochemistry would be less apt to succeed with the former.

Whatever the reasons, the narrative *up to this point* appears consistent with blaming the choice of electric discharge rather than light as the prime cause for failure, as did Bartlett and Holloway. Formation of  $\text{XeF}_2$  from Xe

and  $F_2$  under the action of light from a mercury arc lamp was reported shortly after Bartlett's initial paper (23). A couple of years later the experiment that Bartlett said Yost *should* have tried—exposure to ambient sunlight (and in Pyrex, to boot)—also succeeded (24). Bartlett's further suggestion, that the choice of a quartz vessel and the possible presence of HF could have also been problematic, is likewise not inconsistent.

However, there is one more piece of the story that refutes all of that! Bartlett and Holloway apparently had forgotten (or never realized?) that the *first* (25) reported preparation of  $XeF_2$ , by Hoppe and coworkers (26), was achieved using Yost and Kaye's methodology:

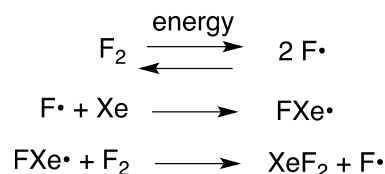
Commercial xenon ... was mixed with carefully purified fluorine (prepared electrolytically) in a Xe: $F_2$  proportion of 1:2 by volume in a sealed quartz vessel and subjected at room temperature to discharges from an induction coil.... After a short interval, a fall in pressure occurred, after several (up to 10) hours, it corresponded at maximum to a 50% decrease in volume of the original quantity of gas. Simultaneously a colorless crystalline condensate was formed on a cold finger.... The analytical data show that its overall composition corresponds approximately to the empirical formula  $XeF_2$ .

As can be seen, Hoppe used activation by discharge, not light; he worked in a quartz apparatus, not Pyrex; he used home-made  $F_2$ , not a commercial sample; and he monitored the reaction by attempting to condense out product. All of that is (almost: see below) *exactly* what Yost and Kaye tried (27)! Clearly, then, most of the prior explanations are *not* correct: the "mistakes" cited—failure to use light, working in quartz—were not mistakes at all. As for the possible presence of HF, Yost and Kaye explicitly comment that they "carefully" removed it, and there is no reason to believe they were less capable than Hoppe in this regard. Furthermore, if any appreciable amount of HF *had* been present, attack on the vessel walls should have been visible from the very beginning of the experiment, not just "after some time."

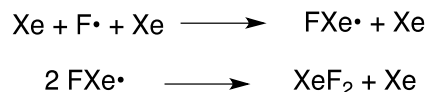
So what *was* the key difference between Hoppe's success and Yost's failure? Only one is apparent: the composition of the gas mixture. Hoppe used 1:2 Xe: $F_2$ , at a total pressure around 1 atm (26), corresponding to partial pressures of Xe and  $F_2$  around 250 mm and 500 mm, respectively. That's fairly typical of other contemporary studies: for example, Streng and Streng used 350 mm Xe and 374 mm  $F_2$ , around 1:1 (24). In contrast, Yost and Kaye used 30 mm and 600 mm: more  $F_2$  and *much* less Xe, a ratio of 1:20.

Again, we may wonder why they made that choice, especially given that making the  $F_2$  was a major challenge; note (see above) that they used a good deal more Xe and less halogen for the chlorine experiment, 70 mm and 225 mm respectively. Could they have been influenced by Pauling here? Recall that he had predicted  $XeF_6$  as his expected product.

But the central question is, could this have made a major difference? Almost certainly so. The mechanism of the reaction of Xe with  $F_2$  is complex, but studies agree that the reaction begins with dissociation of  $F_2$  to  $2 F\cdot$ , with the latter adding to Xe to form  $FXe\cdot$ ; there are several ways to go on from there (28,29). Assume that the electric discharge method generates radicals at a given rate; that the rate is similar between Yost's and Hoppe's work (of course we can't know for certain just how the experimental setups compare in this regard); and that the following (much oversimplified) discharge-initiated chain mechanism operates:



In this model, application of the steady-state approximation (in  $F\cdot$ ) predicts that the rate of formation of  $XeF_2$  will be proportional to Xe pressure, so Yost and Kaye, using around 1/8 or 1/10 the Xe pressures of Hoppe or Streng and Streng, should have generated product at a rate around 8-10 times slower. In fact, that is almost certainly an overly *generous* estimate: mechanistic studies indicate that at higher Xe pressures reactions involving two Xe's start to become important (27):



That implies a dependence on Xe pressure that should be *greater* than first-order, and hence Yost and Kaye should have produced  $XeF_2$  more than an order of magnitude more slowly (everything else being equal) than did Hoppe or Streng and Streng. Hoppe saw significant product after a few hours; Streng and Streng, on the second day (of course, using exposure to sunlight they could only operate during daytime). Hence Yost and Kaye would have needed to run for at least a day, probably much longer, to see much of anything. Did they? They only mention "varying intervals of time," but it seems unlikely they would have carried on for multiple days.

It is probable that they *did* generate traces of product: the attack on the quartz observed was most likely the result of HF generated by hydrolysis of a very small amount of xenon fluoride. Their experimental procedures should have been good enough to exclude all but a *small* amount of adventitious water; if they *had* kept on for long enough, they might well have scrubbed all the water out of the system and started making observable quantities of product—which their methodology should indeed have permitted them to see.

We have seen that none of the interpretations previously proposed for Yost and Kaye's lack of success are consistent with the record, and that it does not seem fair to call anything they did do a mistake. They certainly *could* have chosen a gas mixture composition that would have been more likely to yield success—it is not at all clear why they did not—but with little or no mechanistic understanding (and how could they have had any, for an as yet unknown reaction?) there would have had no obvious reason to use higher Xe pressures. (Pauling also says, in his third letter to Allen, that he believes “there was plenty of xenon present” (7).) Alternatively, they *could* have carried out the experiment for much longer periods of time—probably days on end would have been needed—but their failure to do so, with nothing (good) visibly happening, is understandable.

It appears that relatively minor adjustments to some experimental details could well have yielded success, but they had no reason to believe the reaction should be so sensitive to their choices. We have to conclude that Yost and Kaye were just unlucky not to have opened up the field of noble gas chemistry, several decades ahead of the actual event.

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20. Ref. 4, p 212.
21. Memo from D. M. Yost to E. H. Swift, Sept. 22, 1958. Courtesy of the Archives, California Institute of Technology.
22. Pauling to J. C. Slater, Feb. 9, 1931. Courtesy AH & LPP.
23. J. L. Weeks, C. L. Chernick, and M. S. Matheson, “Photochemical Preparation of Xenon Difluoride,” *J. Am. Chem. Soc.*, **1962**, *84*, 4612-4613.

24. L. V. Streng and A. G. Streng, "Formation of Xenon Difluoride from Xenon and Oxygen Difluoride or Fluorine in Pyrex Glass at Room Temperature," *Inorg. Chem.*, **1965**, *4*, 1370-1371.
25. Hoppe's paper has a received date of October 8, 1962; the actual work had been done in late July 1962, according to his later commentary (1). Matheson's received date is November 5, 1962.
26. R. Hoppe, W. Dähne, H. Mattauch and K. Rödder, "Fluorination of Xenon," *Angew. Chem. Int. Ed.*, **1962**, *1*, 599.
27. Hoppe's 1962 paper does not mention Yost, but it is only two paragraphs long and cites only a recent report of XeF<sub>4</sub>. His 1964 review article (1) suggests that he was indeed aware of Yost's work, which had led him to believe that high-pressure thermal synthesis might be a more promising approach; however, having no access to the required quantities of either reagent, he resorted to the discharge method. There is no indication of any *deliberate* changes to Yost's experimental design that he thought should give a greater chance of success.
28. I. Messing and A. L. Smith, "Role of Xenon Monofluoride as an Intermediate in the Photochemical Synthesis of Xenon Difluoride in Flash-Photolyzed Xenon-Argon-Fluorine Mixtures," *J. Phys. Chem.*, **1982**, *86*, 927-932.
29. V. S. Pavlenko, S. E. Nalivaiko, V. G. Egorov, and E. B. Gordov, "Laser-Induced Excimer Fluorescence Kinetic Study of the Recombination of Chlorine and Fluorine Atoms in Inert Gases," *Kinet. Catal.*, **1996**, *37*, 304-314; V. S. Pavlenko, S. E. Nalivaiko, V. G. Egorov, O. S. Rzhetskii, E. B. Gordov, and E. B. Kinetika, "Kinetics of the Photoinitiated Fluorination of Xenon," *Khim. Fiz.*, **1998**, *17*, 23-40.

### About the Author

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