

BASE HYDROLYSIS OF COBALT(III) AMMINES

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I was pleased to be invited by Professor D. A. Davenport to present a paper at the symposium C. K. Ingold: Master and Mandarin of Physical Organic Chemistry to honor Professor Sir Christopher Ingold on the centennial year of his birth. The chemistry community recalls that he was one of the giants of physical organic chemistry, but few chemists realize he also made an important contribution to physical inorganic chemistry. As the token inorganic chemist on the symposium program, it is my job to review briefly the inorganic papers of Ingold and coworkers and to present the saga of this research done at University College London (UCL) and that done independently at Northwestern University (NU). The research involved had to do with the kinetics and mechanisms of ligand substitution of octahedral metal complexes, specifically of cobalt(III) ammines. The two research groups agreed on several of the experimental observations made, but they parted company when it came to the interpretation of the second-order rate law for the base hydrolysis of cobalt(III) ammines. It is primarily this difference that is discussed in this article.

It is not known to me what prompted Ingold to take an interest in the kinetics and mechanisms of ligand substitution of octahedral metal complexes, but perhaps it was because of the influence of the late Professor Sir Ronald Nyholm who had left Australia to join the faculty at UCL. This would be reminiscent of my arrival in 1946 at NU and being able to persuade Professor Ralph G. Pearson, then a physical organic chemist, to join forces and become a physical inorganic chemist. In fact, the initial paper on this subject by Ingold was coauthored

by Nyholm. This paper was concerned with stereochemical changes accompanying ligand substitutions of cobalt(III) complexes, and it was indicated that kinetic studies would be necessary to determine the mechanisms of substitution (S_N1 or S_N2). Nyholm was not comfortable with studies of kinetics and mechanisms, being primarily interested in the syntheses and characterizations of metal complexes, so he did not tend to coauthor the Ingold kinetics papers.

Ingold published five papers on this subject during 1953-56, and finally summarized their research in a small (52 pages) book, the *Weizmann Memorial Lectures*(2) in May, 1958. Ingold states in the preface of the book:

The object of the lectures recorded in this book was to point out that the first attempts are being made to start the development of a corresponding extension of scope (beyond mechanisms of organic reactions) in the very much more diversified field of inorganic chemistry, which hitherto has been essentially restricted, as organic chemistry used to be, to the study of structure.

He was certainly correct that the time had arrived when inorganic chemists could address the questions of mechanisms of ligand substitution in octahedral and square planar metal complexes(3) using approaches similar to those of organic chemists to probe mechanisms of substitution at tetrahedral carbon. Who was better qualified to initiate such studies than Ingold, one of the founding fathers of physical organic chemistry who invented(4) the now classical nomenclature and symbolism S_N1 and S_N2 ?

Ingold's first paper(1) on the subject stressed the importance of stereochemical changes accompanying ligand substitution in the assignment of reaction mechanisms. For substitution at tetrahedral carbon it is well known that an S_N2 reaction takes place with inversion of configuration. The situation is not as simple for S_N2 reactions of octahedral systems, and Ingold described this in terms of *edge displacement* (Fig. 1).

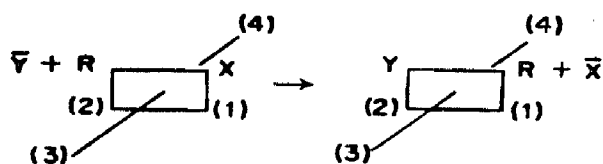
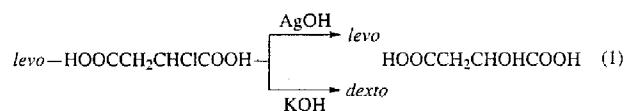
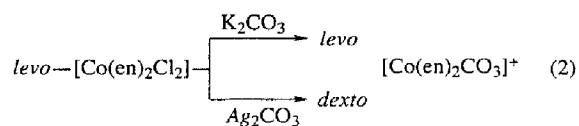


Figure 1 Edge displacement pathway of an S_N2 octahedral substitution.

According to this description the stereochemical change that takes place does not depend on groups Y, R, and X directly involved, but on the position of a marker group A. If A is at (1) the change is *cis* \rightarrow *trans*, if at (2) *trans* \rightarrow *cis*, and if at (3) or (4) either *D* \rightarrow *L* or *L* \rightarrow *D*. This corresponds to a *trans*-attack via a pentagonal bipyramid intermediate(5), whereas a nonedge displacement process with retention of configuration corresponds to a *cis*-attack. Walden(6) in 1896 discovered an inversion, now given his name, in one of the steps of the reaction of chlorosuccinic acid to form malic acid (Eq. 1).



Bailar(7) in 1934 discovered what he termed the first "Walden inversion" reaction of inorganic chemistry (Eq. 2).



Ingold(1,2) suggested it is misleading to refer to this as a Walden inversion because it implies a geometrical inversion of octahedral valencies analogous to that of tetrahedral valencies. This one example can serve to illustrate the greater complexities of displacement reactions in octahedral systems over those at tetrahedral carbon.

Furthermore S_N1 reactions for octahedral substitution also permit a choice of pathways that account for a variety of stereochemical changes. Research groups at

both UCL and NU independently arrived at the same interpretation of rearrangements that may occur during ligand substitution of octahedral systems by an S_N1 mechanism (Fig. 2).

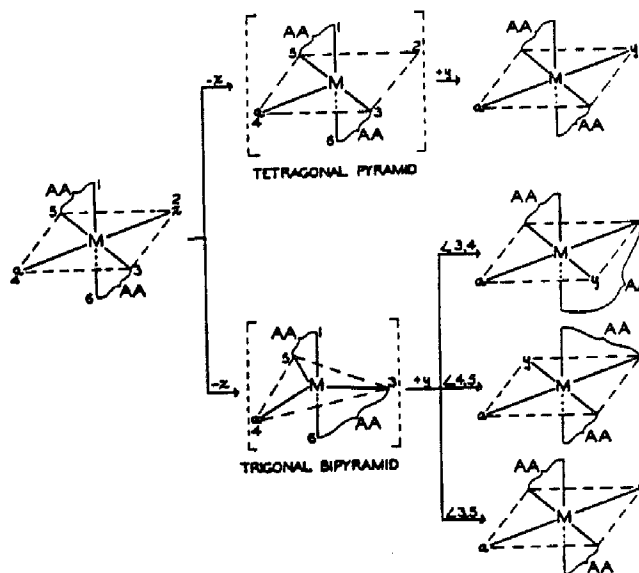
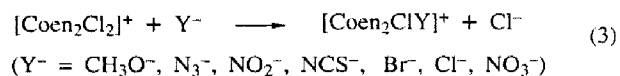


Figure 2 Possible pathways of an S_N1 octahedral substitution.

The rearrangements proposed for S_N2 and for S_N1 mechanisms were used with modest success in an attempt to account for observations reported earlier by the "father of coordination chemistry," Professor Alfred Werner(8). More quantitative spectroscopic data came from the UCL laboratory(9) and from our laboratory(10). Authors of our paper included first names, considered trivial by some English standards; and we later learned that this publication was called by Ingold the "Fred, Bob, and Ralph" paper. The final conclusion of both research groups was that the approaches used could account for stereochemical changes of reactions of octahedral cobalt(III) complexes, but the information obtained was not as diagnostic of mechanism as it was for reactions of tetrahedral carbon compounds.

The second paper in this series by Ingold(11) reported details of a kinetic study of some reactions in methanol of $[\text{Coen}_2\text{Cl}_2]^+$ (Eq. 3)



Methanol was chosen as a solvent because it has much less tendency to coordinate to cobalt than does water,

which was known(12) to complicate such a study. The summary of this investigation is illustrated in Fig. 3.

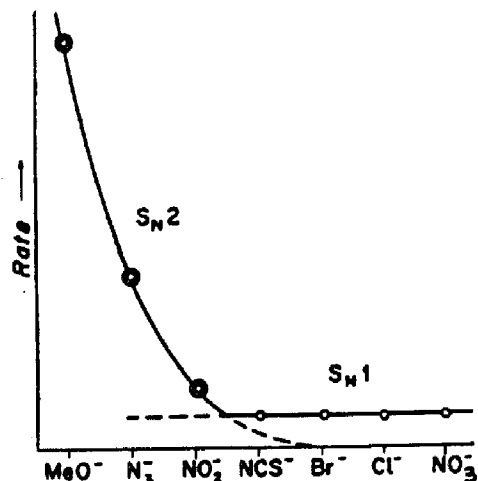
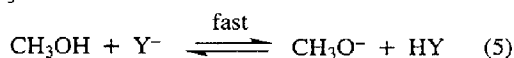


Figure 3 Rates of substitution of one chloride ion from *cis*-[Coen₂Cl₂]⁺ with different reagents in nonbuffered methanol solutions (Eq. 3). See text for assignment of an S_N1CB mechanism to the reagents CH₃O⁻, N₃⁻ and NO₂⁻.

The experimental results showed that for some reagents (NCS⁻, Br⁻, Cl⁻, NO₃⁻) the rate is first-order in concentration of substrate but zero-order in concentration of reagent. The S_N1 mechanism of substitution was assigned for these poor nucleophiles. A second-order rate law (Eq. 4)

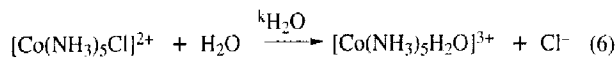
$$-\frac{d[\text{Coen}_2\text{Cl}_2]^+}{dt} = k_2[\text{Coen}_2\text{Cl}_2]^+[\text{Y}^-] \quad (4)$$

was observed for the reagents CH₃O⁻, N₃⁻, and NO₂⁻. It was suggested that these stronger nucleophiles react by an S_N2 mechanism. Later(13) we were able to show that the reactivities of N₃⁻ and NO₂⁻ are due to their greater proton basicities which produce catalytic amounts of CH₃O⁻ (Eq. 5).



As mentioned near the end of this article, had the UCL group buffered their solutions they would have noted that the rates of reaction are zero-order in concentrations of either N₃⁻ or NO₂⁻. Here then is the beginning of the saga of the polemic between the research groups at UCL and at NU. What follows is a brief account of our work on the base hydrolysis of cobalt(III) amines, leading finally to experiments that conclusively disprove the S_N2 mechanism proposed by Ingold.

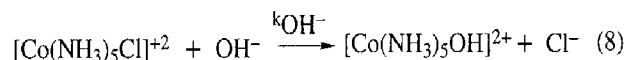
Cobalt(III) complexes are often water-soluble, and at pH less than 3 or 4 some of the complexes slowly react with water to form an aquo complex (Eq. 6).



At higher pH the rates are much faster (even as much as 10 times faster than aquation) and the rate law is second-order (Eq. 7).

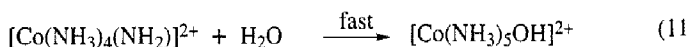
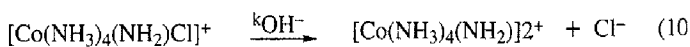
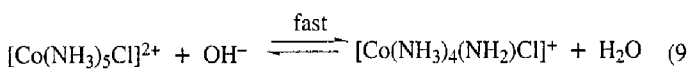
$$-\frac{d[\text{Co}(\text{NH}_3)_5\text{Cl}]^{2+}}{dt} = k_{\text{OH}^-}[\text{Co}(\text{NH}_3)_5\text{Cl}]^{2+}[\text{OH}^-] \quad (7)$$

Such a reaction (Eq. 8) is referred to as *base hydrolysis*(3).



Since OH⁻ is the strongest base possible in water and since it is a strong nucleophile, it is understandable that Ingold would suggest the reaction takes place by an S_N2 pathway. In spite of this, at NU we were aware of work by Brønsted(14) and the suggestion by Garrick(15) of an alternative mechanism also consistent with the observed rate law (Eq. 7), along with other qualitative observations in our laboratory that prompted us to investigate further the mechanism of base hydrolysis of cobalt(III) amines.

Brønsted(14) reported that the hydrolysis of [Co(NH₃)₅NO₃]²⁺ is independent of pH below 3; however, that of [Co(NH₃)₄(H₂O)NO₃]²⁺ is dependent on [H⁺] even below pH 3. He suggested that [Co(NH₃)₅NO₃]²⁺ does not dissociate in acid to form its conjugate base, whereas [Co(NH₃)₄(H₂O)NO₃]²⁺ is a sufficiently strong acid to form rapidly equilibrium amounts of the conjugate base [Co(NH₃)₄(OH)NO₃]⁺ which is much more substitution-labile than its parent aquo complex. Garrick(15) reported in 1937 that the second-order rate law (Eq. 7) for the base hydrolysis of [Co(NH₃)₅Cl]²⁺ was consistent with what is called an S_N1CB mechanism (substitution, nucleophilic, unimolecular, conjugate base) (Eqs. 9, 10, 11).



Garrick did not report any attempts to test his proposed mechanism, but we felt it worthy of being investigated.

Although a reaction mechanism, like any scientific theory, cannot be proved correct, it can be disproved.

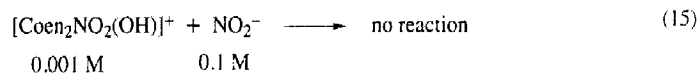
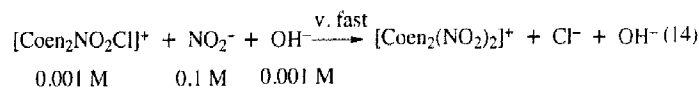
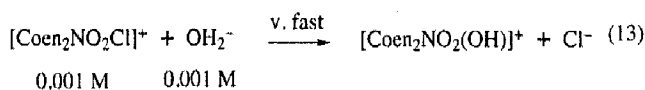
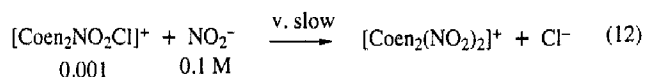
For example, this S_N1CB mechanism requires that 1) the complex have a $pK_a > 14$, 2) the complex undergo H—D exchange in D_2O faster than its rate of base hydrolysis, 3) OH^- not be a good reagent if the complex has ligands with no N—H bonds, and 4) the reaction be specifically OH^- catalyzed. All four of these requirements stood up to the tests applied; had any one of them failed, the mechanism would have had to be discarded or modified.

1) A $pK_a = 11$ was reported(16) for $[Co(NH_3)_6]^{3+}$ and a $pK_a > 14$ for $[Co(NH_3)_5Cl]^{2+}$ was indicated because it could not be determined in water. 2) Adamson and I(17) determined that the rate of H-D exchange of $[Co(NH_3)_5Cl]^{2+}$ in D_2O is about 10^6 times faster than its rate of base hydrolysis. 3) We found(18) that OH^- concentration had no effect on the rates of solvolysis of three different pyridine complexes $[Co(x-py)_4Cl_2]^+$ which do not have any N—H bonds. 4) The rate of release of Cl^- from an acidified water solution of $[Coen_2NO_2Cl]^+$ is independent of the concentrations(12) or even the presence of added NO_2^- , N_3^- , or NCS^- .

In spite of all of our efforts to test the S_N1CB mechanism, Ingold stated(2):

Before summarizing the main points of evidence (reported in support of S_N1CB), let me clear away the irrelevancies which have been introduced by remarking that no conclusion can be reached by altering the constitution of the complex by changing the ligands or the metal and discussing the consequent changes in absolute rate. There are so many factors affecting rate, most of which cannot be relatively weighed, that it is always possible plausibly to rationalize observations of this character. I know of no such significant observations that cannot quite reasonably be discussed on the basis of mechanism S_N2 .

Confronted with the cleverness of Ingold in rationalizing our experiments in support of an S_N2 mechanism for base hydrolysis of cobalt(III) amines, we were driven to the necessity of proving that the S_N2 mechanism is wrong. This was achieved(19) by using dimethyl sulfoxide as a solvent and observing the following reactions of $[Coen_2NO_2Cl]^+$ (Eqs. 12-15).



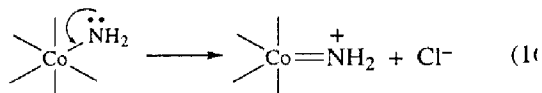
If the S_N2 reaction were correct then the role of OH^- would be to attack cobalt and form $[Coen_2NO_2(OH)]^+$ (Eq. 13). Clearly that is not the role of OH^- , because $[Coen_2NO_2(OH)]^+$ does not react with NO_2^- (Eq. 15). However, a mixture of $[Coen_2NO_2Cl]$, NO_2^- , and OH^- readily afford $[Coen_2(NO_2)_2]^+$ which must occur without the intermediate formation of $[Coen_2NO_2(OH)]^+$ required, were this to involve an S_N2 pathway. The role of the OH^- here is in keeping with an S_N1CB mechanism. Although these experiments cannot prove the S_N1CB mechanism is correct, they surely prove the S_N2 mechanism is wrong. Just prior to the publication of these results in 1960, I gave a guest seminar at UCL and visited with Ingold in his office. There behind closed doors we discussed our differences, and I stressed our unpublished findings in DMSO solution that disproved his S_N2 mechanism. He was courteous, kind, and listened carefully; but at the end he looked at me and said, "...but, Fred, DMSO is not water." Fortunately, three years later Green and Taube(20), using ^{18}O -labeling in much the same experiment, showed it was not OH^- that attacked the cobalt as required by an S_N2 mechanism, but instead the labeled-O in the product $[Co(NH_3)_5OH]^{2+}$ was derived from H_2O . A few years later a series of competition experiments(21) provided the ratio of products $[Co(NH_3)_5Y]^{2+}/[Co(NH_3)_5H_2O]^{3+}$ for the reactions of $[Co(NH_3)_5X]^{2+}$ in water solution in the presence of OH^- and Y^- , where $Y^- = NO_2^-, N_3^-, NCS^-$ and $X = Cl^-, Br^-, I^-, NO_3^-$. The results were consistent with an S_N1CB mechanism. Hundreds, perhaps even thousands, of papers have been published on the base hydrolysis of metal amines which report results in keeping with the S_N1CB mechanism, perhaps somewhat modified because of the presence of ion pairs(22).

Although Ingold never did state in print that the S_N2 mechanism was disproved, his former student, co-author, and then faculty colleague at UCL, Professor Martin L. Tobe, who continued to do elegant research on this problem, did state(23):

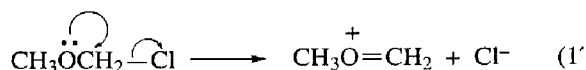
Probably the closest approach to the correct mechanism is that developed by Basolo and Pearson, who used a suggestion by Garrick that the role of hydroxide was essentially catalytic and that it served to generate a dissociatively reactive entity by removing a

proton from the substrate....., although it ought to be borne in mind that base hydrolysis might take place by more than one mechanism.

One issue to address briefly is why the conjugate base $[\text{Co}(\text{NH}_3)_4\text{NH}_2\text{Cl}]^+$ is so much more reactive than are corresponding complexes $[\text{Co}(\text{NH}_3)_4\text{YCl}]^+$ ($\text{Y}^- = \text{Cl}^-, \text{OH}^-, \text{NCS}^-$). We suggested(24) this is because NH_2^- is such a strong base that it stabilizes the cobalt(III) ion and promotes a dissociative reaction (Eq. 16),



similar to stabilizing the carbocation believed responsible for the high reactivity of α -haloethers (Eq. 17).



Furthermore, efficient NH_2^- to Co π - donation requires formation of a trigonal bipyramid intermediate which leads to stereochemical change during reaction that is observed experimentally.

Finally, reference is made to Fig. 3 and the indication that the strongly nucleophilic reagents N_3^- and NO_2^- react with $[\text{Coen}_2\text{Cl}_2]^+$ by an $\text{S}_{\text{N}}2$ mechanism. We repeated these studies in methanol solution and also added the reagent acetate ion. Our results(13) in buffered solutions showed conclusively that the rates did not depend on the concentrations of the entering ligand, but only on the concentrations of CH_3O^- rapidly generated by these basic reagents (Eq. 5). Thus the role of CH_3O^- in methanol solution is similar to that of OH^- in water and in DMSO, and the results support an $\text{S}_{\text{N}}1\text{CB}$ mechanism(3).

To my knowledge, the final words in print by Ingold on this subject are contained in his booklet on his *Weizmann Lectures* given in May, 1958. There he states(2):

...yet it must be confessed that the first five years (1953-58) of published work consciously directed to the disclosure of mechanism in substitution at octahedral cobalt by the correlated study of kinetics and products has yielded little in the way of fully proved conclusions... I do not dare to hope that the interpretations I have suggested are correct in their entirety. But I do believe them sufficiently to think that they are more likely to be modified than scrapped.

Modified they were, but scrapped not because the outstanding insight Ingold possessed about kinetics and

mechanisms of chemical reactions was something very much needed by inorganic chemistry and inorganic chemists. Shoppee in his biographical memoirs wrote(25):

Ingold had extraordinary imagination, insight, initiative, and ingenuity... he possessed one of the greatest intellects in chemistry of the 20th century, and he added a new dimension to organic chemistry.

Pearson, my former colleague and collaborator on research dealing with the base hydrolysis of cobalt(III) complexes, states(26):

I wholeheartedly agree with these remarks (above of Shoppee) and I will add that he also was a great stimulus to inorganic chemistry. The very fact that he interested himself in the subject of inorganic reaction mechanisms added great stature to the topic. Both Basolo and I recall the sparse audiences to which we would lecture prior to 1953, and the crowds we drew after Ingold's entry into the field.

I strongly second everything stated by Pearson, and hasten to add that the polemic we, as yet untenured faculty at NU, had with Ingold focussed attention on our research and helped make NU one of the best departments of inorganic chemistry in the USA. Needless to say, Pearson and I are very grateful to Ingold and to Nyholm for helping us get off to a good start with our careers in inorganic chemistry.

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