

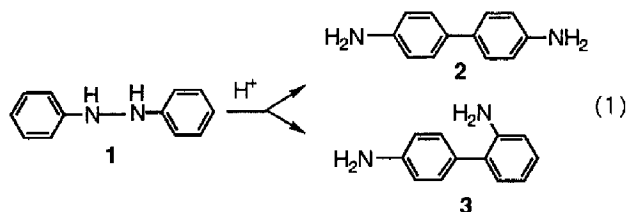
A PERSONAL HISTORY OF THE BENZIDINE REARRANGEMENT

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What I have to say today amounts more to a personal account rather than an honest-to-goodness history of the benzidine rearrangements. Interest in the mechanism of these rearrangements spans this century from the early 1900s to the present day. Prominent in studies of the rearrangements and their mechanisms during these ninety years is the name of the man whose memory we honor today, C. K. Ingold, associated also with his long-time colleague and collaborator E. D. Hughes and their younger coworker Derek Banthorpe. Also prominent in these studies are the names of Paul Jacobson, Michael Dewar, Robert Carlin, Miroslav Vecera, and George Hammond and me. I don't know how or why Carlin and Vecera took up the benzidine rearrangements. Carlin, of course, was much interested in the Fischer indole reaction, and that is connected with benzidine rearrangements, as you know. Possibly, that's how the rearrangements snared his interest. I would guess that Ingold's attraction and that of Dewar developed from their much broader interests in the electronic theories of organic reactions and the mechanisms of all molecular rearrangements. As for me and George Hammond, our interest in benzidine rearrangements was quite unplanned and entirely accidental. This is how it happened.

In early 1948 I went to Iowa State College as a postdoctoral fellow with Henry Gilman. I went there with an arrangement, that I had proposed, to study under his guidance the mechanism of Grignard reactions, particularly those which I called anomalous (enoliza-

pounds) and which I had been studying with E. E. Turner at Bedford College in London during the years 1945-1947. My arrangement with Henry Gilman broke up in the fall, 1948, and I became an independent research associate supported by the Department of Chemistry. In return for that support, I became an instructor of a quiz section in sophomore organic chemistry when the section's graduate-student instructor left Ames on short notice. During one of the quiz-section periods a student asked how the benzidine rearrangement, mentioned only briefly in his book, occurred (Eq. 1) this equation shows two products, as is correct.



The student's book would have shown only the major product, benzidine, 2). Of course, I had no idea, at all. George Hammond had joined Iowa State College in the fall, 1948, bringing to the Department a wonderful freshness and vigor in organic chemistry. I felt sure that George would know the answer to the benzidine question and promised the student that I would find the answer for him. It turned out that Hammond knew no more than I about the rearrangement, and he suggested that I search it out in the literature and report on it at one of

1719), but under conditions corresponding to those of our experiments on simultaneous isomerization, it was not possible to obtain satisfactory direct measurements of velocity, owing to the rapidity of the reactions.

Now, the attempts by Biilmann and Blom to correlate rates and acidity failed(7). But, Holleman and van Loon were explicit about the dependence on acidity in the formation of benzidine (2) from hydrazobenzene (1), and said so with their Eq. 3 (modified slightly here):

$$d[2]/dt = k[HCl]^2 \quad (3)$$

(3) Furthermore, they included a drawing of N,N'-diprotonated hydrazobenzene, leaving no question as to how these earliest of kineticists saw the catalysis of rearrangement(6). It is possible that Ingold and Kidd overlooked the significance of these findings because they were very much more concerned with products. A second criterion for a successful mixture experiment was that the rearrangements be uncomplicated. Each component must give only a benzidine and none of the other possibilities in the family of products. Rearrangements of 4a and 4b satisfied that criterion, except that each also disproportionated. It was, in fact, the disproportionation and a way of avoiding its complications that led Ingold and Kidd to choose the conditions of rearrangement, the conditions that prevented them from measuring rates directly. That is, rearrangement of a mixture of 4a and 4b was carried out in dry ethanol containing hydrogen chloride. Addition of dry ether to the solution after rearrangement precipitated the hydrochlorides of the two benzidines but left the disproportionation products in solution, thus effecting the clean separation desired. The fact that disproportionation occurred impressed Ingold and Kidd. They noted that(5):

We know of no previous record of this reaction, although van Loon (*Rec. Trav. Chim.*, 1904, 23, 162) observed the formation of a mononuclear amine from a hydrazo-compound in the absence of an external reducing agent.

Here, we find the second part of the enigma: The paper in *Recueil* that Ingold and Kidd refer to is, in fact, one by Lobry de Bruyn and Wolff(8) on Tyndall effects. van Loon's paper occurs earlier in the issue(9), and while he does indeed report the formation of mononuclear amine, the emphasis of this paper is on the relative yields of benzidine (2) and diphenylene (3) obtained from 1 and on the rearrangement's kinetic order in acid. van Loon's paper is, in fact, the detailed report of the work described briefly earlier in the *Proceedings*(6), to which Ingold and Kidd also referred(3). van Loon's full paper(9) pre-

sents the kinetic method in great detail (quantitative assay of 2, with time, as its dihydrogen sulfate) and under a variety of conditions, and ends again with the kinetic equation, Eq. 3. It seems extraordinary that Ingold could have overlooked this. Further, van Loon was quite explicit about the mononuclear amine. It was obtained along with azobenene, but only when rearrangement of hydrazobenzene was carried out at elevated temperatures and in relatively concentrated acid. van Loon recognized that the formation of azobenene meant that aniline should be formed. He went on to isolate it, gave the disproportionation reaction for its formation, and characterized it as its derivative benzanilide(9).

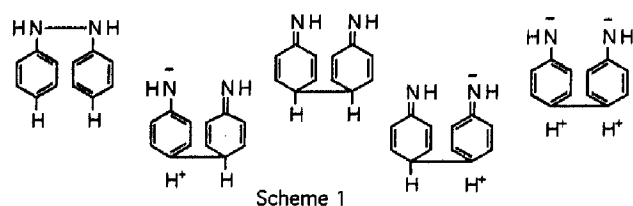
How could Ingold have glossed over van Loon's work? We shall never be able to answer that question. Could he have relied on the reading by the less experienced Kidd? The question haunts us because of how Ingold wrote eight years later about the role of acid in the benzidine rearrangement. I refer here to the paper by Hughes and Ingold on the "Theory of Aromatic Substituents and Rearrangements with Special Reference to the Benzidine Change"(10). This paper is concerned particularly with the difference between Robinson's and Ingold's views of the rearrangement and may well have been written in response to Robinson's sudden (apparently), awakened interest in the benzidine rearrangement(11). Robinson represented rearrangement with a flow of electrons in protonated hydrazobenzene(11). Ingold, however, went to great pains to distance himself from the idea that protonation preceded N-N bond scission. He and Hughes said (2):

We may collect these points as follows: (a) A nearby proton (at least one) disturbs the electronic system of the benzidine (sic) molecule; (b) therefore, this proton (at least) is present in the transition state; that is to say, its coordinates enter into the specification of the state; (c) but its covalent bonding with nitrogen does not precede the N-N heterolysis.

In the same paper, Hughes and Ingold present drawings of electronic structures that could represent states between hydrazobenzene and benzidine, and they say of these structures (Scheme 1)(13):

As we do not know yet whether one or two adding protons are included in this state (the *p*-benzidine transition state) we shall omit them ...

It is hard to understand how Ingold could say this eight years after having referred(3) to van Loon's papers. No mention is made of these papers in the analysis by Hughes and Ingold(10).



Scheme 1

If I seem to dwell at unseemingly length on van Loon's papers, it is not for the purpose of uncovering a flaw in Ingold's benzidine work, but to set the following historical point. None of the writers, not just Ingold, of the early years appears to have paid any attention to van Loon's second-order acid dependence. Had they done so, in fact, the history of the benzidine rearrangements would certainly have taken a different turn, and it is certain, too, that neither Hammond nor I would have had any part in it.

It is remarkable that as early as 1918 the Robinsons were writing about the mechanism of the benzidine rearrangement in connection with their interest in the Fischer indole reaction and its role in their preparation of tetraphenylpyrrole(14). They ruled out the possibility, voiced occasionally at that time, that *p*-aminodiphenylamine (i.e., *p*-semidine) was an intermediate in the formation of benzidine, by showing that *p*-semidine was unaffected by being heated with concentrated hydrochloric acid. They fitted the mechanism of the benzidine rearrangement into a generalized scheme covering other "enimic to enamic" rearrangements without being specific about protonation.

I am not aware that Robinson engaged directly in any experimental work on the benzidine rearrangement. Yet, remarkably, his address as President of the Chemical Society in 1941 was devoted largely to the mechanism(11). In that address he formulated the, now, curious alternating flow of electrons, to which I referred above, beginning with monoprotonated hydrazobenzene, from which Hughes and Ingold distanced themselves as noted above. Insofar as this history is concerned, Robinson's monoprotonation(15), which was continued by all writers up to 1950, is all the more remarkable, because he, like others, referred to Jacobson's exhaustive summary without, apparently, reading or heeding its last paragraph. Robinson called attention to Jacobson's summary in the following way(16):

Paul Jacobson, who devoted a large part of his experimental work to the study of the benzidine and semidine transformations of substituted hydrazobenzenes has published a summary of everything known on the subject in 1922 (*Annalen*, 428, 76) together with a discussion of the course of the

reactions. In view of this it is unnecessary to repeat the historical account....

In Jacobson's historical account van Loon's kinetic work is recorded but second-order acid dependence is not disclosed. Jacobson's failure to state the kinetics of rearrangement explicitly is another benzidine enigma. Possibly, he neglected the specifics of the kinetics because of his lack of kinetics training to which he refers. Certainly, had Jacobson dwelt on the kinetics and their meaning, the history of the rearrangements must have been different. This is what Jacobson said(17):

Ein weiterer Einblick—besonders auch in Bezug auf die Art, in welcher die als Hervorrufener der Umlagerung wirkenden Säuren an dem Vorgang teilnehmen—dürfte in erster Linie durch reaktionskinetische Erforschung der Umlagerung zu erwarten sein, auf die ich wegen Mangel an Zeit und Schulung verzichten musste. In dieser Richtung liegen einstweilen (ref. 2) nur einige in Holleman's Laboratorium durchgeführte Versuche von van Loon (ref. 3) vor; aus ihnen wird der Schluss gezogen dass die Geschwindigkeit, mit der verschiedenen Säuren die Umlagerung von Hydrazobenzol bewirken, durch ihre Ionization bestimmt wird.

If only Robinson had followed up Jacobson's lead, and had found van Loon's paper, how different the history would have been.

It was left to Hughes and Ingold to call all of us to order by pointing out in 1952 that Hammond and I in 1950 had only re-discovered what van Loon had reported in 1904(18). I remember feeling chagrined at reading that 1952 review, because it made me feel that I had missed the boat in 1949, and that the more perceptive English group had caught it in 1952. In fact, the failure of Hammond and me to find van Loon's work indicates how peripheral was our interest in the rearrangement in 1949. I made no further literature search after finding Dewar's paper(1), relying on his account of the rearrangement; and even after I took up benzidine chemistry again in the late 1950s, I still failed to read the literature carefully, so that I was unaware until going over Ingold and Kidd much, much later that, in some way, Ingold himself had then missed the same boat. Strangely, Carlin, a very careful experimenter, referred to van Loon's work, not for its kinetic import but for information on products(19). In turn, Dewar(20), in support of his π -complex theory of the rearrangement, referred to this information on van Loon's products in Carlin's paper, but apparently did not check into van Loon's paper itself. The consequences for the π -complex theory, based therefore on monoprotonated hydrazoarenes, were se-

vere. That story has been recounted elsewhere and need not be retold here(21).

Between 1933 and 1957(22) Ingold published no experimental work on the benzidine rearrangement. During those years appeared an outpouring of papers on mechanistic organic chemistry, particularly on nucleophilic substitution, elimination, and aromatic substitution, especially nitration. Interest in the benzidine rearrangement appears to have been peripheral until its grand resurgence in the early 1960s. The paper in 1957, bearing also the name (C.A. Bunton) of one of the contributors to this Symposium, established the relationship between rate of rearrangement and acidity function. It established also that acid catalysis was specific rather than general. This paper, then, was the forerunner of the series of 15 experimental papers in the *Journal of the Chemical Society* spanning the years 1962-1964, that were to follow from the group of Hughes, Ingold, Banthorpe, and coworkers. These papers reported the detailed kinetics and product studies of rearrangements of a variety of hydrazoarenes, chosen so as to illustrate the scope in kinetics in benzidine rearrangements, then being uncovered at UCL. The UCL group was primed by Carlin's discovery of mixed order (1.6) acid catalysis to show that some hydrazoarenes rearranged under second-order acid catalysis (e.g., hydrazobenzene) and some by first-order catalysis (e.g., 1,1'-hydrazonaphthalene), while others could exhibit both orders, depending on the acidity of the solution. The UCL group found the solvent, ring-deuterium isotope, and salt effects that characterized the course of protonation at nitrogen and the polar nature of the transition state in rearrangement. Discovering the duality of acid catalysis must have been particularly pleasing to Ingold in that it resembled an earlier duality in mechanistic pathways he had discovered, namely the S_N1/S_N2 and $E1/E2$ reactions. The series of investigations included thermal rearrangements, whose kinetics I had studied myself a little earlier and to which I refer later. The series represented the completion of the development of the polar-transition-state (PTS) theory of rearrangement, the beginnings of which can be seen in the paper by Hughes and Ingold in 1941(10). Part XV of the series, called "A Collective Discussion," is 37 pages long(23). It summarizes the results and meaning of the preceding parts in the series and is also an analysis of other theories and their shortcomings. The massive undertaking was designed to support the PTS theory, showing how it accounted not only for ways in which substituents affected the kinetics of rearrangements, but also, with the exception of the *p*-semidine rearrangement (of

which more is said later), the types of products formed. This, then, was the end of Ingold's search for an answer to the mechanism of the benzidine rearrangements; that is, the establishment of the PTS theory. Further experimental work continued at UCL but mainly in the hands of Derek Banthorpe. In all, the series reached to Part XXVII in 1973(24). Ingold's participation in the last papers was small, there being three papers bearing his name, in 1967(25) and 1968(26). It is sobering to recognize that the benzidine papers were published when Ingold was in his 70s and after he had officially retired in 1961. Ingold and I were corresponding about the rearrangements in 1967. On June 20 of that year he wrote(27):

When I wrote the other day, I had not seen Banthorpe (who is largely concerned with biosynthetic work now-a-days)(28) and did not know (or had forgotten) that, during the year in which I have been withdrawn from the benzidine problem, he has been going on with it.....we have more rate-product studies than I told you of I don't think the new data alter my general appraisal of the position, as I wrote it in my letter (June 7). But as the area of consistency is larger than I thought it was, I feel rather more assurance than I did.

Thus, the benzidine story for Ingold came to an end essentially in 1964 with the PTS theory, in his 71st year(29).

In July, 1963, the IUPAC meeting on Pure and Applied Chemistry was held in London. I had been looking forward to seeing the UCL group and talking about the rearrangements; but Hughes died in June, 1963, Banthorpe was on holiday while the IUPAC meeting was on, and Ingold's time was sought by many international visitors. At a pub lunch, our only meeting, he asked, "What is the remaining question to be answered in benzidine rearrangements?" Even after 20 years I was still somewhat intimidated by the residue of awe inculcated in me in my two undergraduate years at Aberystwyth(30) and sensed that Ingold was asking a question to which he already had an answer. My answer was pragmatic: to find how disproportionation fitted with rearrangement. His answer, which indeed he had had all along was: to know what the transition state was like. This was the question he had posed at the "Symposium on The Transition State" in Sheffield in April, 1962, at which he displayed his now famed drawing of a transition state hidden in the clouds (Fig. 1)(31). In the printed version he wrote(32):

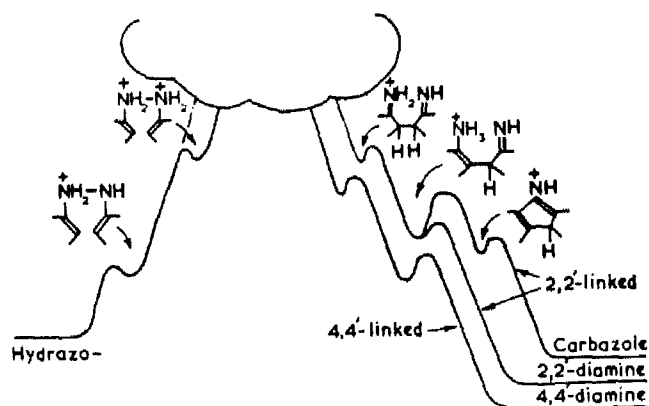


Figure 1. Reproduction (with permission of the Royal Society of Chemistry) of Fig. 8, Ref. 31, showing Ingold's cloud over the transition state of the rearrangement of hydrazobenzene.

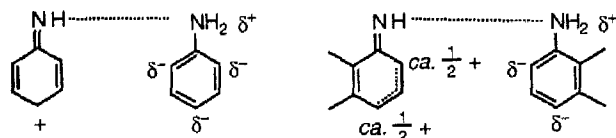
Fortified by these reminders of partly solved problems, let us now face an unsolved one, that of the mechanism of the benzidine rearrangement, the transition states of which must be like nothing else in this world.

He summarized what was then known and outlined how it could be explained with polar transition states. He concluded(33):

I do not change my view that the problem of the transition state of the benzidine rearrangement is unsolved. I have offered certain tentative interpretations only in order to show how it may be possible to fit known facts into a picture. To establish the unique correctness of the picture is another matter.

The unique correctness, from his point of view, became the PTS theory. According to the theory, the transition state of each rearrangement became polar by the heterolysis of the N-N bond of the hydrazoarene. Heterolysis for first-order-acid cases was initiated by monoprotection at one of the nitrogen atoms and led to two dissimilar halves of the PTS, one bearing positive charge and the other being neutral. Which half bore the charge depended on the relative basicities of the two nitrogen atoms and the relative capabilities the rings had to delocalize the charge. These relative conditions were controlled by substituents in the rings. In second-order-acid cases the dominant role in causing heterolysis was attributed to the first of the two protons. Nevertheless, pre-equilibrium diprotonation was called for by the solvent deuterium kinetic isotope effect and the acidity function dependence at high acidities, which

the UCL group had discovered. An important criterion was that both protons were in place in the transition state. Again, heterolysis was such that one half of the TS bore two positive charges and the other half had the character of a primary amine. In the PTS theory, product type (benzidine, diphenylene, semidine) was determined by the dispositions of polarity in the rings, the dispositions being affected by the placement and nature of substituents. Examples of Ingold's TS drawings(23) are shown in Scheme 2(34).

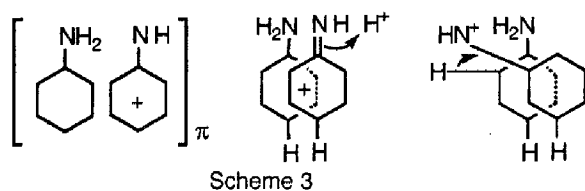


Scheme 2

The PTS theory was the most comprehensive of all of the theories that had appeared on the mechanism of the benzidine rearrangements. The earliest of theories proposed, for example, by Tichwinsky(35) and by Stieglitz(36) called for scission of the N-N bond into two fragments, either with or without prior protonation, and the recombination of the fragments. This intermolecularity was essentially disposed of by Jacobson, who quoted thereto his 65 examples of rearrangement of unsymmetrical hydrazo compounds, not one of which had given evidence of crossed products(4). Jacobson came down on the side of intramolecular rearrangement without understanding fully how it occurred. The formation of benzidine from hydrazobenzene was described by him as nothing more than an exchange reaction in which two *para* hydrogens changed place intramolecularly with a single bond. It is interesting to see Jacobson's diagram for this exchange and compare it with our modern representation of a sigmatropic shift. Jacobson represented the *para* hydrogens as migrating to the nitrogen atoms at the time of forming the new single bond. Analogously, the semidine rearrangement was represented as the intramolecular replacement of a *para* hydrogen by a phenylimino group, the hydrogen migrating at the same time to the second nitrogen atom. The role of the acid was not explained, however, nor was protonation shown. Jacobson was candid about his interpretation, moreover, acknowledging that the objection could be raised that it was not a clarification but only a rephrasing of the facts. It was this open appraisal of his position that he followed with his observation, that is quoted earlier, on the need for kinetics.

Some years before Ingold's interest in the benzidine rearrangements was reawakened, a simple and very clever explanation for them was given by Dewar. This

came to be known as Dewar's π -complex theory of the rearrangement. It was first proposed in 1945(39) and incorporated a year later in a general theory of a number of related rearrangements(20). The π -complex theory was initially very attractive. The basic idea of it was that the N-N bond of a protonated hydrazoarene broke heterolytically and led intramolecularly into a complex of the two halves. These could then rotate, depending on their structure, to the conformation favoring product formation. Dewar's earliest presentation(39) of this idea is shown in Scheme 3.



The theory assumed at the start that monoprotonated hydrazobenzene rearranged(20,39), and this assumption was maintained in Dewar's experimental work by his failing to measure acid dependence in the rearrangements of hydrazobenzene itself and three other hydrazobenzenes(1). The finding of second-order acid dependence by Hammond and me in 1950(40), and a little later by Carlin(41) was bad news for the π -complex theory. This was unfortunate, in a sense, because an intrinsically clever idea received the criticism due only to one of its faulty props. The story of the succession of flaws in parts of the theory has been described already(21). The theory received a pummeling because of them but continued to be promulgated by Dewar after being shorn of faults. As far as I can tell, the last of Dewar's writings on the π -complex theory of the benzidine rearrangement was in 1965(42).

A major supporter of an intermediate-complex (π - or molecular) theory was Vecera, who published a substantial amount of experimental work, initially on the chromatographic analysis of products(43) and later on the mechanism of rearrangement; but the sparse attention paid to his work by others does not reflect its wide scope. Possibly, the reason is that the publications were mainly in Czech and in German. The last of the series of papers, published, this time in English(44), was aimed specifically at expressing the difference of opinions and conclusions the Czech group had from those of Ingold and his coworkers, which had been laid out in great detail in the summary paper(23) mentioned earlier. The Czech view was that the major driving force for rearrangement was the cleavage of the N-N bond and the resonance (delocalization) energy of the fragments. It

was irrelevant to consider the way in which the N-N bond cleaved (that is, homo- or heterolytically) because complexation via overlapping π -orbitals accompanied cleavage. The essence of the Czech view, then, was in opposition to the PTS theory, first in its view of two-proton catalysis and next in favor of one calling for some intermediate complex. It was not that the group had compelling new evidence to support an intermediate-complex theory, but that it was, for them, the better way of understanding the rearrangements.

Here, then, a sort of plateau was reached in the years 1964-1966, in which the choice of PTS versus intermediate-complex theories became a matter of opinion. Dewar and Marchand put it aptly in 1965(42) in saying that kinetic evidence could throw no light on the nature of intermediates in the reactions.

Now, the PTS and π -complex theories had one thing in common, namely that each had one view and one only of all of the rearrangements. In the PTS case, all rearrangements were one-step in type; that is, concerted in today's terminology. In contrast, the π -complex theory showed all rearrangements as being two-step in type; that is, a π -complex was formed in a rate-determining step and, after the appropriate rotational adjustment, collapsed in a fast step to product.

Distinction between these two possible pathways could not be made with any of the great body of experimental evidence that had so far been assembled. It was made only later with work on heavy-atom kinetic isotope effects (KIE) at Texas Tech. I can finish this account, then, by returning to where benzidine rearrangements and I parted company in 1949.

During 1949-1951 I had a postdoctoral fellowship with Carl Niemann at Caltech. Failing to get a teaching position, I joined the research laboratories of the U. S. Rubber Co., in Passaic, NJ, in late 1951, and spent three happy years there, until the fall, 1954. The urge to "do my own thing" took me to the small and unknown Texas Technological College (now Texas Tech University) in September, 1954. The head of the (then) Chemistry and Chemical Engineering Department, Joe Dennis, had decided views on building a research and graduate department. It was not a publish-or-perish department and there was no such thing as tenure (that came later, and I received tenure by "grandfathering"); but there was a moral obligation to begin a research program. One of my first efforts in this direction was to study the oxygen oxidation of alkenes in acetic anhydride that I had discovered at U. S. Rubber(45). The reaction encompassed a Criegee rearrangement and that led me, at Texas Tech, to study the decomposition of acetyl peroxide in

cycloalkenes. Out of that came the discovery that, as unstable as it may be, the acetoxy radical could be trapped by an alkene(46). At the same time I returned to the benzidine rearrangement and Jacobson's wonderful summary(4). In that summary Jacobson listed other agents, beside mineral acid, that cause benzidine rearrangements; and I was struck by his reference of the, until then, sole observation of a rearrangement caused by heating with base. This was the work of Meisemheimer and Witte, who had rearranged 2,2'-hydrazonaphthalene by heating it in alcoholic sodium hydroxide(47). A search of the literature showed, however, that Krolik and Lukashevich had pinpointed the rearrangement as being thermal and not in need of the base(48). I decided then to begin studying the mechanism of that rearrangement. My notebooks show that I began the work on February 4, 1955, first with hydrazobenzene, whose study was fruitless, and next on March 3 with 2,2'-hydrazonaphthalene in ethanol. The first-order kinetics were soon uncovered and led to further study with a series of solvents(49). Thereafter the work was taken over by my first graduate students, John Trisler and Robert Snell(50). It was never my intention to return to acid-catalyzed rearrangements, because I was now becoming enmeshed in a new field for me, radical ions and esr spectroscopy. Yet, when papers began coming out from Ingold's group and the battle lines between them and Dewar were being drawn, I was attracted to join the fray. I did not begin a systematic study in the sense of the UCL one. At that time, I felt more in tune with the π -complex than the PTS theory, but began checking out the cases that were crucial to arguments for the both of them, but which had never been investigated properly. In time, for example, it was found by Tommy Chamness with 4,4'-di-*tert*-butylhydrazobenzene(51) and by Jim Stanley with 4,4'-hydrazobiphenyl(52) that these bulky hydrazo compounds rearranged and by second-order acid catalysis, contrary to what Dewar was saying about them and their consequent role in the π -complex theory.

Insofar as the PTS theory was concerned, we took up at Texas Tech the only class of rearrangements, the *p*-semidine, that had been deliberately excluded from consideration by Banthorpe, Hughes, and Ingold(53). We thought it odd that the *p*-semidines had not been examined at UCL. The reason for the exclusion was their acceptance(53) of an earlier, faulty proposal by Hammick and Munro(54), that heavy-metal ions were necessary to effect *p*-semidine rearrangements. The reason for this proposal was that it was thought, also erroneously, that all the rearrangements listed by Jacobson

had been carried out by starting with an azoarene and reducing it with metal and acid to the hydrazoarene, which then rearranged. Hammick and Munro tried to find whether *p*-semidine rearrangements could occur without following that procedure, but unfortunately chose a rather complicated case as the test, the rearrangement of *p*-ethoxyhydrazobenzene. The *p*-semidine product of that rearrangement is so sensitive to oxidation that Hammick and Munro failed to isolate it and concluded, therefore, that it had not been formed, thus supporting the idea that a metal ion should have been used. But, Charles Baldwin and Harvey Harris showed at Texas Tech, that, if handled properly, the rearrangements of *p*-ethoxy- and *p*-methoxyhydrazobenzene took place in the absence of heavy-metal ions(55). Ingold and I corresponded about the *p*-semidine rearrangement. On May 22, 1967, I wrote about our work on the *p*-ethoxy- and *p*-methoxy cases at Texas Tech(56):

If the *p*-semidine rearrangement turns out to be a valid intramolecular acid-catalyzed one, for which assistance by a metal ion is not needed, I would say that this rearrangement would best be interpreted as going through an intermediate other than the common place ones. I do not see how the breaking of the N-N bond and formation of the *p*-C-N bond could be concerted and so be represented with one transition state. Perhaps I am not being sufficiently imaginative over this. The *p*-semidine rearrangement (if intramolecular, etc.) would be similar to the quinamine rearrangement, and I have the same feeling about requiring an intermediate in that one(57).

Ingold replied on June 6(60):

You were right to feel that we cannot get rid of the para-semidine problem as easily as Hammick and Munro wanted to. However, I find no great difficulty in picturing a transition state with an N-to-para polar bond of about 3A long. There would be plenty of strength in such a bond and direction is no problem.

Ingold's letter included tables of rate data, including those for *p*-methoxy- and some other *p*-substituted hydrazobenzenes. Nevertheless, I was disappointed in the UCL group for writing an updated history, with the *p*-semidine rearrangement as a chief new point(25), without acknowledging Baldwin's and Harris's work, and for Banthorpe and Cooper's publishing the *p*-methoxy work as the first recorded one-proton conversion of a *p*-semidine(61).

Very little more of consequence was done at Texas Tech with acid-catalyzed rearrangements in those years. Work was carried out on photo-benzidine rearrangements, however, since that fitted in more with my inter-

ests in radical ions and esr(62). Thus, acid-catalyzed rearrangements were abandoned at Texas Tech in the late 1960s. I felt then that nothing new could be done in solving the benzidine mechanism until someone came along with an entirely new approach. It was certainly not in my thoughts that it would come along at Texas Tech. But, that is what happened and also in a more-or-less accidental way. I met Prof. C. A. Bunton(63) by chance at a meeting of the American Chemical Society and we fell into a discussion of benzidine rearrangements. Bunton had continued his interest in them since leaving England but not in a major way(64). Our discussion turned to one's not being certain that N-N bond breaking was rate-limiting, and that measuring the nitrogen KIE would give the answer. At that time I did not know how to go about measuring a nitrogen KIE, so Bunton suggested that I solicit the help of Prof. A. N. Bourns at McMaster University, a recognized expert in measuring KIE with isotope-ratio mass spectrometric data. That suggestion bore fruit, and eventually with the help of one of Prof. Bourns' former co-workers, Dr. Peter Schmid, the nitrogen KIE was at last obtained(65). When the nitrogen KIE result was published I heard from an old friend, Prof. Harold Kwart, who pointed out that he could measure heavy atom KIE without needing to use isotope-ratio mass spectrometry, but, instead, by using whole molecule-ion quadrupole mass spectrometry(66). We undertook to do that with the rearrangement of hydrazobenzene, but this time to do it properly; that is, on the separated products, benzidine and diphenylene. At about the same time, I solicited the help of another old friend from Gordon Conference days, Dr. Clair Collins, an expert in measuring ^{14}C KIE, at Oak Ridge National Laboratories(69). Our collaborative results were, to me, astonishing. They showed that benzidine formation was a concerted process whereas diphenylene formation was not(70,71). It may seem odd for me to be saying that I was astonished, because as soon as the results were obtained we could see that is exactly what they should have been. Benzidine formation is a [5,5]-sigmatropic shift, with which the KIE were consistent. That being so, the diphenylene formation could not be concerted because it is a [3,5]-sigmatropic shift, forbidden by orbital-symmetry constraints from being concerted, unless antarafacially.

The hydrazobenzene results led us at Texas Tech to go further with *o*- and *p*-semidine and *o*-benzidine rearrangements (in the naphthalene series), with thermal rearrangements, and with the disproportionation reaction. Successful characterization of pertinent examples encouraged us to extend the mass spectrometric and scin-

tillation techniques to solving questions about other rearrangements, such as the Claisen and quinamine, and to continue now with Diels-Alder reactions. Thus, the question that an undergraduate asked over 40 years ago, and which affected my research life so dramatically, can now be answered more-or-less reasonably.

It is amusing now to look back and wonder how all of us, who were the major workers in the benzidine rearrangements, could have missed the connection between them and the blossoming of ideas in pericyclic reactions. Perhaps we were too far in the woods to see the trees.

The first papers by Woodward and Hoffmann on orbital-symmetry control of pericyclic reactions appeared in 1965(72). At about that time Ingold's interest in benzidine rearrangements was diminishing, and it may be understandable that he did not see or comment on their relationship to sigmatropic shifts. It would be interesting to know why others in the field failed to see or comment, too. For my own part, I can believe only that I was not sufficiently alert. The first workers to suggest that benzidine formation may be a [5,5]-sigmatropic shift were, in fact, not benzidine practitioners at all, but those very active indeed in Claisen rearrangements. From H. Schmid's laboratory in Switzerland came a succession of truly beautiful papers on these rearrangements, crammed full of splendidly worked out examples. Schmid's group applied frontier molecular orbital treatment to those rearrangements and noted that benzidine formation might be included among [5,5]-sigmatropic shifts in charged systems(73). Schmid went no further than that. A more extensive recognition that benzidine formation was an allowed [5,5]-sigmatropic shift, whereas other products come from rearrangements that are formally forbidden, is to be found in the 1971 book by Alder, Baker, and Brown(74). I was not aware of this perceptive writing until almost 20 years later and can only admit to another lapse in alertness. It is apparent that Alder and his colleagues must have written about the rearrangements some time before 1971(75). In a brief review of the rearrangements(77) I described Schmid's view(73) but even then did not appreciate fully that the answer to the benzidine rearrangements lay in it.

A summary of the KIE results for the rearrangements of hydrazobenzene, 4-methoxyhydrazobenzene (an allowed *p*-semidine rearrangement), 2,2'-dimethoxyhydrazobenzene (a benzidine rearrangement), 2,2'-hydrazonaphthalene, *N*-2-naphthyl-*N'*-phenylhydrazine (each *o*-benzidine rearrangements), 4,4'-dichlorohydrazobenzene (an *o*- and a

p-semidine rearrangement with loss of Cl), and the disproportionation of 4,4'-diiodohydrazobenzene has been given earlier and need not be repeated here(21). After that summary, however, the nitrogen and carbon KIE for the rearrangement of hydrazobenzene were re-measured and are given here in Table I. The motive for re-measuring these KIE lay in our intention of measuring KIE for rearrangements of [1-¹⁴C]- and [1,1'-¹³C₂]hydrazobenzene and our wanting to have for comparison contemporary measurements for [4-¹⁴C]- and [4,4'-¹³C₂]labelling. We recognized that the earlier KIE, determined on benzidine and diphenylene themselves(70,71), might be quantitatively wrong.

The 1,1'-positions of hydrazobenzene are not directly involved in bond breaking and forming, so that ordinarily one would not make these positions candidates for KIE measurements. We had found, however, that labelling the C-1 and C-β positions of allyl phenyl ether, also not formally involved in bonding, gave rise to significant KIE in its rearrangement to 2-allylphenol(78). The KIE arise from the motion of these atoms in the six-atom array of a [3,3]-sigmatropic shift. We felt, therefore, that analogous, coupled motion of the 1,1'-positions of rearranging hydrazobenzene might be discernable with KIE measurements. Our data for both benzidine and diphenylene formation are given.

Table I. KIE for Rearrangement of Hydrazobenzene (1) to Benzidine (2) and Diphenylene (3)(79)

Isotope, Position	KIE ^a	
	2	3
1,1'- ¹³ C ₂ ^b	1.0035 ± 0.0010	1.0000 ± 0.0026
1- ¹⁵ C	1.0051 ± 0.0017	0.9988 ± 0.0017
4,4'- ¹³ C ₂ ^b	1.0127 ± 0.0011	1.0121 ± 0.0008
4- ¹⁵ C	1.0121 ± 0.0008	1.0010 ± 0.0010
¹⁵ N, ¹⁵ N ^b	1.0410 ± 0.0009	1.0367 ± 0.0009

a) All measurements were made on the bis(trifluoroacetyl) derivatives.

b) For two atoms.

The results in Table I from labelling at the 1,1'-positions show KIE for forming 2 but none for forming 3. That is, the coupled motion of the 1,1' carbon atoms, anticipated for the ten-atom array in a [5,5]-pericyclic reaction, is detectable. In contrast coupled motion for diphenylene formation cannot be detected. The KIE at the 1- and 1,1'-positions for 2 are very small and not precise. By this I mean that the logarithmic ratio (eq. 4)

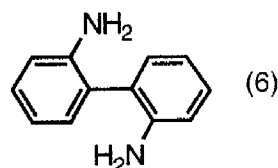
$$\ln(k^{14}\text{C}/k^{12}\text{C}) \ln(k^{13}\text{C}/k^{12}\text{C}) 1.9 \quad (4)$$

expected for ¹⁴C and ¹³C KIE (single atoms) is not met by our results. The precision of our measurements, for

¹³C probably, was, we think, not good enough. Yet, the motion we sought was found. The results for labelling at the *para* carbon atoms confirmed our earlier finding, that formation of 2 is concerted while formation of 3 is not. The new measurements for the *para* carbons do fit eq. 4, whereas our earlier ones with 2 itself did not(71). It is notable that the nitrogen KIE (Table I) are not much different from each other. They tell us that there is not much difference in the breaking of the N-N bond in the transition states for the two products, but for a reason still unknown, 16% (the yield of 3) of the molecules do not follow a pericyclic path. Nevertheless, they do not enter an intermolecular path either, and perhaps, after all, a π -complex prevails for this pathway. Thus, here we may have the pathways of both earlier theorists, the concerted (Ingold) for 2 and π -complex (Dewar) for 3.

We now know, then, that the benzidine rearrangements follow the dictates of sigmatropic reactions. This still does not lift Ingold's cloud completely from obscuring the transition state(s). We do not know the geometry of the TS in concerted rearrangement, and we do not know what maintains intramolecularity in non-concerted ones. Perhaps this is as far as we shall get—until someone comes along with a newer approach.

One of the puzzles of benzidine formation is that benzidine and not 2,2'-diaminobiphenyl (6, an *o*-benzidine) is formed. Formation of 6 is an allowed reaction, a [3,3]-sigmatropic shift. That type shift occurs in the rearrangement of hydrazonaphthalenes, both under acid catalysis and by heating(67,80). At one time it was suggested, briefly, that benzidine was formed by a succession of migrations, first to the *ortho* and next to the *para* positions(81). That suggestion was made when the so-called "cartwheel" mechanism of the nitramine rearrangement was popular. Had the double migration, now recognized as double [3,3]-shifts, prevailed, it would have been remarkable that it could compete almost exclusively with aromatization by loss of protons leading to 6. Later, my colleagues and I were able to exclude



the double migration by finding no KIE for forming 2 from [2,2',6,6'-¹³C₄]1(84). We do not really know why 6 is not formed, although I have suggested earlier that either the bent cyclohexadienyl shape of the rings in the diprotonated TS does not allow for the close-enough

approach of the *ortho* carbons, or that charge and electron distributions, by analogy with spin distributions in the aniline cation radical, call for preferred bonding at the *para* positions(71). If the latter is correct, it will correspond with Ingold's PTS.

I will end this personal account by returning to van Loon and my own most recent and perhaps last experimental work on the benzidine rearrangement. In the many separations of **2** and **3** that accompanied measuring the KIE, Dr. Subotkowski perforce had the ratios of **2/3**. These were uniformly 84:16(79), rather than the 70:30 so often quoted(85). In his first report, with Lobry De Bruyn (see Ref. 6), and in his full paper(9), van Loon recorded that in 0.1 N HCl and 50% ethanol 84% of benzidine was formed. He did not report the amount of diphenylene, that being too difficult to obtain. I take my hat off to van Loon who was 50 years ahead of us in kinetics and 90 years in yields. The same hat I doff to C. K. Ingold and M. J. S. Dewar, whose originality in ideas set them apart from all other practitioners and goaded us into action.

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- Ref. 11, p. 220.
- Ref. 4, p. 121: "Further insight - particularly regarding the way acids cause the rearrangement - should be anticipated primarily from kinetic studies of the rearrangement, which I must forgo because of lack of time and training. For the time being (Ref. 2), the only completed effort in this direction is by van Loon (Ref. 3), from Holleman's laboratory; from it the conclusion is drawn

- that their ionization determines the velocity with which various acids cause the rearrangement of hydrazobenzene." Jacobson's Ref. 2 is to an announcement, by Stieglitz and Curme, Jr. in *Berichte* **1913**, 46, 912, of other rate measurements. Reference 3 is to van Loon in *Rec. Trav. Chim.* **1904**, 23, 62 and to Lapworth in *J. Chem. Soc.* **1908**, 93, 2188. Lapworth's work is a brief qualitative description that the speed at which benzidine sulfate precipitates from a solution of hydrazobenzene in ethanol, acidified with a little sulfuric acid, is inversely proportional to the amount of added water. Ref. 2 is a report of first-order kinetics in the thermal decomposition of hydrazobenzene in ethanol into azobenzene and aniline. Stieglitz and Curme refer briefly to van Loon's work and to their intention to submit later their recent measurements of rates in the acid-catalyzed benzidine rearrangement.
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 27. Letter from Sir Christopher Ingold to H. J. Shine, June 20, 1967.
 28. Banthorpe's switch to biosynthetic work became complete some years later. In response to my enquiring for some additional information about the preparation of D_3PO_2 in the UCL work, he replied in a letter of February 20, 1979, that he had disposed of his laboratory notebooks and gave me helpful information from memory. A clean sweep had been made, evidently.
 29. An indication of his diminishing interest in benzidines may be inferred, also, from his visit to Texas Tech on May 25, 1964, from Vanderbilt, where he was a visitor for several months under the NSF Senior Foreign Scientist Fellowship Program. He wrote, on March 19, 1964, that "in order not to bring coals to Newcastle I think I had better lecture on something else than benzidine rearrangements (which we can still talk about privately). I am warmed up just now on organo-metallic substitutions and could keep going for an hour on that." He lectured on "Substitutions on Organo-metallic Compounds." In 1964 the air journey required changing planes at Love Field. Ingold had a four-hour lay-over and spent the time in the comfortable mezzanine lounge available then, writing on the next edition of his book.
 30. Ingold was a remote figure to me, and maybe to other undergraduates, at that time (1942-1944). I met him and Hughes at the beginning of my two years at the interview all new undergraduates had to have with them before embarking on course work. I blotted my copy book by showing up late, after all interviews were finished, having gone walking on the sea front to kill time. Ingold was testy with me about that. I cannot remember talking to him again except in some group meetings that were held when the students and faculty tried, but failed, to make an arrangement to hold classes during the long summer vacation. Ingold would walk along the long promenade at Aberystwyth but be oblivious to passing students. Hughes had more personal contact with undergraduates, it seems, especially during laboratory periods. Ingold taught, of course, and very well. In contrast, Hughes was a terrible lecturer, reading from his meticulous notes and continuing his reading each period from where he ended the time before.
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- ment." However, Curme(37) reported that the theory had now been abandoned because of work by Wieland and also because of the measurements by Stieglitz and Curme(38), (referred to earlier in this history), of the velocity of the benzidine rearrangement.
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 56. Letter from H. J. Shine to Sir Christopher Ingold, May 22, 1967.
 57. My imaginativeness was indeed puny. Eventually, with heavy atom KIE, Henryk Zmuda showed the *p*-semidine(58), and Bogdan Boduszek the quinamine(59) rearrangement to be concerted.
 58. H. J. Shine, H. Zmuda, H. Kwart, A. G. Horgan, and M. Brechbiel, "Benzidine Rearrangements. 17. The Concerted Nature of the One-Proton *p*-Semidine Rearrangement of 4-Methoxyhydrazobenzene," *J. Am. Chem. Soc.*, **1982**, *104*, 5181-5184.
 59. B. Boduszek and H. J. Shine, "The Mechanism of the Quinamine Rearrangements," *J. Am. Chem. Soc.*, **1988**, *110*, 3247-3252. See also Ref. 73.
 60. Letter from Sir Christopher Ingold to H. J. Shine, June 6, 1967.
 61. D. V. Banthorpe and A. Cooper, "Mechanism of Benzidine and Semidine Rearrangements. Part XVII. Kinetics and Products of the Acid Rearrangements of 2,2'-Dimethoxy- and 4-Methoxy-hydrazobenzene," *J. Chem. Soc.*, **1968**, 605-609. Banthorpe and Cooper acknowledged having heard about the work at Texas Tech. In the sense that theirs was the first recorded one-proton case they were correct. Kinetics were omitted at Texas Tech. More on the *p*-semidine rearrangement is discussed in Ref. 26a.
 62. (a) J.-D. Cheng and H. J. Shine, "Photobenzidine Rearrangements. V. Mechanistic Aspects. Rearrangement of Mixtures of Different N,N'-Dimethylhydrazo Aromatics, and the Nature of the Excited State," *J. Org. Chem.*, **1974**, *39*, 2835-2838. (b) J.-D. Cheng and H. J. Shine, "Benzidine Rearrangements. XIII. The Role of Reductive Scission. Reactions of N,N'-Dimethylhydrazobenzenes in Acid Solutions," *J. Org. Chem.*, **1975**, *40*, 703-710.
 63. Clifford A. Bunton was a lecturer at UCL when I was an undergraduate, in Aberystwyth, so that my acquaintance with him went back many years.
 64. C. A. Bunton and R. J. Rubin, "Benzidine Rearrangement in the Presence and Absence of Micelles. Evidence for Rate-Limiting Proton Transfer," *J. Am. Chem. Soc.*, **1976**, *98*, 4236-4246.
 65. H. J. Shine, G. N. Henderson, A. Cu, and P. Schmid, "Benzidine Rearrangements. 14. The Nitrogen Kinetic Isotope Effect in the Acid-Catalyzed Rearrangement of Hydrazobenzene," *J. Am. Chem. Soc.* **1977**, *99*, 3719-3723. The final answer took a long time to come.
- On October 4, 1974, Bunton wrote: "I will be very interested to know how your ¹⁵N isotope work comes out. Our mechanistic ideas would be very well clarified if we knew with a degree of certainty that N-N bond breaking was rate limiting." A letter of mine to Bunton on November 12, 1975, says, "We have our samples sealed, waiting for mass spectrometry." The samples at that time were, in fact, the mixed hydrochlorides of benzidine and diphenylene. We did not think far enough ahead to separate the products, wanting to know only if we could find a nitrogen KIE for their formation. Thus, the answer we obtained was really for the disappearance of hydrazobenzene. At that time, Peter Schmid was, in fact, a postdoctoral worker with Dr. Keith Ingold in the National Research Council laboratories in Ottawa. Ingold graciously agreed that Schmid could return to McMaster for a month to make our measurements.
66. I had known Harold Kwart since 1951 when I worked for, and he was a consultant for, U. S. Rubber. Our families became friends and we would meet periodically, particularly at Gordon Conferences. Nevertheless, I had not kept up with Kwart's work well enough to know what he had been doing with mass spectrometry and KIE. He had, in fact, pioneered the technique of measuring isotopic ratios with the selected ion monitoring (SIM) mode of a quadrupole mass spectrometer. He worked with a very old Hewlett Packard instrument that belonged to the Chemical Engineering Department at the University of Delaware. My impression, on seeing it, was that Kwart's group was keeping it going with "baling wire" repairs. The first nitrogen KIE on separated benzidine and diphenylene were measured with that instrument. At the time we, in Lubbock, did not know enough about the technique and its demands on sample character to send Kwart anything but benzidine (a solid) and diphenylene (an oil), themselves. Therefore, it was very difficult to handle diphenylene, something we were blissfully unconcerned about in Lubbock. As we learned, when we repeated the work ourselves years later in Lubbock with our own quadrupole mass spectrometer, the early results were correct in finding KIE but quantitatively wrong. My collaboration with Harold Kwart continued until his mass spectrometer became inoperable. At the same time, Kwart became increasingly ill with stomach cancer. We continued our contacts, nevertheless, even during his final days in the Sloan-Kettering Hospital in New York. We spoke almost daily by telephone, always about chemistry, and, on learning that I was planning on trying to measure KIE in photochemical reactions, his last conversation with me was to advise me to read up on the Russian work on magnetic isotope effects. Kwart died on March 31, 1983. When Kwart's mass spectrometer became unusable, he recommended that I seek the help of Prof. Joe San Filippo, Jr., then at Rutgers University. San Filippo had one of the best HP instruments and had it programmed to handle SIM data beautifully. We collaborated for a few years

- until he begged off because the type of samples we were sending, bis(trifluoroacetyl) derivatives of amines, were deadly to his electron multipliers. Our being cut off from a mass spectrometer finally enabled us to get our own, with Texas Tech University funds, not the high-class model that San Filippo had, but a lesser one that could do the job, however. San Filippo kindly gave us his computer program for handling the thousands of SIM scans. When we updated the computer facility for the mass spectrometer and found that it worked on Pascal rather than HP Basic, we found two hot-shot undergraduates in the Computer Science Department to make the translation for us. Thus, after some years of wandering in the mass spectrometry deserts we were settled in our own promised land. Some further forays into measuring KIE with isotope ratio mass spectrometry were also made. Initially, in 1985-1986, (that is, some years after our work with Peter Schmid in Canada), we had to use a commercial laboratory, Kreuger-Geochron, for the measurements. These were successful for nitrogen KIE, but not ^{13}C KIE, in the rearrangement of N-2-naphthyl-N'-phenylhydrazine(67). The cost of commercial analyses was high, and, although Kreuger-Geochron generously dropped its price for us to \$45/sample, we had to abandon them. I learned a lot about the technique from Mr. Harold Kreuger by letter and 'phone discussions. Fortunately, Texas Tech's Department of Geosciences acquired a state-of-the-art VG-SIRA 12 isotope ratio mass spectrometer, which Dr. Yilmaz learned to use, and in that way obtained nitrogen KIE in a Smiles rearrangement with naturally-abundant starting material (68)
67. (a) H. J. Shine, L. Kupczyk-Subotkowska, and W. Subotkowski, "HeavyAtom Kinetic Isotope Effects in the Acid-Catalyzed Rearrangement of N-2-Naphthyl-N'-phenylhydrazine. Rearrangement is Shown to be a Concerted Process," *J. Am. Chem. Soc.*, **1985**, *107*, 6674-6678. (b) *Ibid.*, **1987**, *109*, 1286; a correction.
 68. I. Yilmaz and H. J. Shine, "Heavy-Atom Kinetic Isotope Effects in the Base-Catalyzed Smiles Rearrangement of N-Methyl-2-(4-nitrophenoxy)ethanamine," *Gazz. Chim. Ital.*, **1989**, *119*, 603-607.
 69. Again, because of our inexperience we sent Collins samples of benzidine and diphenylene, and again the answers were qualitatively right but quantitatively wrong. Collins thrilled us with his phone call about the first benzidine measurements: "Henry, you have a carbon KIE." Following the collaboration with Collins we turned to using scintillation counting. Texas Tech did, in fact, have a counter. Clair Collins died July 27, 1988.
 70. H. J. Shine, H. Zmuda, K. H. Park, H. Kwart, A. G. Horgan, C. Collins, and B. Maxwell, "Mechanism of the Benzidine Rearrangement. Kinetic Isotope Effects and Transition States. Evidence for Concerted Rearrangement," *J. Am. Chem. Soc.*, **1981**, *103*, 955-956.
 71. H. J. Shine, H. Zmuda, K. H. Park, H. Kwart, A. G. Horgan, and M. Brechbiel, "Benzidine Rearrangements. 16. The Use of Heavy-Atom Kinetic Isotope Effects in Solving the Mechanism of the Acid-Catalyzed Rearrangement of Hydrazobenzene. The Concerted Pathway to Benzidine and the Nonconcerted Pathway to Diphenylene." *J. Am. Chem. Soc.*, **1982**, *104*, 2501-2509.
 72. R. B. Woodward and R. Hoffmann, "Selection Rules for Sigmatropic Reactions," *J. Am. Chem. Soc.*, **1965**, *87*, 2511-2513.
 73. Gy. Fräter and H. Schmid, "Thermische Umwandlung von Penta-2,4-dienyl-phenyläthern in 4-(Penta-2,4-dienyl)-phenole; [5s,5s]-sigmatropische Umlagerungen," *Helv. Chim. Acta*, **1970**, *53*, 269-290. The possibility that the quinamine rearrangement is a sigmatropic one is also pointed out in this paper. See Ref. 59. 74. R. W. Alder, R. Baker, and J. M. Brown, *Mechanism in Organic Chemistry*, Wiley-Interscience, London, 1971, pp. 273-274.
 75. Dr. Alder told me(76), in fact, that he included this view of benzidine rearrangements in his University lectures for some years before the book was written.
 76. Meeting of Dr. R. W. Alder and H. J. Shine, University of Bristol, July 19, 1989.
 77. H. J. Shine, "Aromatic Rearrangements," in *MTP Intl. Rev. Sci., Org. Chem.*, Ser. I, H. Zollinger, Ed., **1973**, *3*, 65-103; see p. 84.
 78. L. Kupczyk-Subotkowska, W. Subotkowski, W. H. Saunders, Jr., and H. J. Shine, "Claisen Rearrangement of Allyl Phenyl Ether. 1- ^{14}C and β - ^{14}C Kinetic Isotope Effects. A Clearer View of the Transition Structure," *J. Am. Chem. Soc.*, **1992**, *114*, 3441-3445.
 79. W. Subotkowski, L. Kupczyk-Subotkowska, and H. J. Shine, "The Benzidine and Diphenylene Rearrangements Revisited. 1- ^{14}C and 1,1- $^{13}\text{C}_2$ Kinetic Isotope Effects, Transition State Differences, and Coupled Motion in a 10-Atom Sigmatropic Rearrangement," *J. Am. Chem. Soc.*, **1993**, *115*, 5073-5076.
 80. H. J. Shine, E. Gruszecka, W. Subotkowski, M. Brownawell, and J. San Filippo, Jr., "Heavy-Atom Kinetic Isotope Effects in the Acid-Catalyzed and Thermal Rearrangements of 2,2'-Hydrazonaphthalene. Transition-State Differences in their Concerted Rearrangements," *J. Am. Chem. Soc.*, **1985**, *107*, 3218-3223.
 81. S. Brownstein, C. A. Bunton, and E. D. Hughes, "The Intramolecular Rearrangement of Phenylnitramine and the Benzidine and Semidine Changes," *Chem. Ind. (London)*, **1956**, 981. The intervention of stepwise migrations was part of the earliest speculations on mechanisms of benzidine rearrangements, but not, of course, in the sense of the double migration described here. That is, as mentioned earlier in the discussion of Robinson's work, the *p*-semidine was thought by Chattaway(82) to precede benzidine formation. Analogously, Chattaway proposed that the *o*-semidine might also be an intermediate step on the way to *o*-benzidine, although, he noted, that product had hitherto escaped detection. Fry(83) also made an early speculation that either the *o*- or *p*-semidine

- rearrangement preceded benzidine formation. It is interesting that each of these speculations was made as a by-analogy addendum, to a discussion of another piece of chemistry, the diazoaminobenzene rearrangement in Chattaway's case and the Crum-Brown and Gibson Rule in Fry's.
82. F. D. Chattaway, "The Transformation of Diazoamido-into Aminoazocompounds and of Hydrazobenzene into Benzidine," *Proc. Chem. Soc. (London)*, **1902**, 18, 175-177.
83. H. S. Fry, "Die Konstitution des Benzols vom Standpunkte des Korpuskular-atomistischen Begriffs der positiven und negativen Wertigkeit. I. Eine Interpretation der Regel von Crum Brown und Gibson," *Z. Physikal. Chem.*, **1911**, 76, 385-397.
84. L. Kupczyk-Subotkowska, H. J. Shine, W. Subotkowski, and J. Zygmunt, "Bond-Forming Carbon Kinetic Isotope Effects in Benzidine Rearrangements," *Gazz. Chim. Ital.*, **1987**, 117, 513-516. This paper is dedicated to the memory of Prof. Gabriello Illuminati (1922-1986), whom I met first in 1948 when we were both postdoctoral fellows with Prof. H. Gilman at Iowa State College.
85. The ratio of yields 79:15 is also reported by Vecera for rearrangement in 50% ethanol 0.1 M in HCl at 20. C (Ref. 86, p. 1996).
86. M. Vecera, L. Synek, and V. Sterba, "Über die Umlagerung Aromatischer Hydrazoverbindungen IV. Untersuchung der Sauren Katalyse und des Einflusses des Mediums und der Temperatur bei der Umlagerung von Hydrazobenzol," *Coll. Czech. Chem. Commun.*, **1960**, 25, 1992-2004.

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