

A Claim on the Development of the Frontier Orbital Explanation of Electrocyclic Reactions

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An important discovery took place in the period 1964–1969—it was the elaboration of the complex of ideas known variously as “the conservation of orbital symmetry” or “orbital symmetry control.” The stereochemistry and ease (or difficulty) of the elementary steps of many organic reactions found an explanation in the phase relationships of their molecular orbitals.

It was my fortune to collaborate with R. B. Woodward in this story. Our published work consisted of a series of five communications to the Editor of *J. Am. Chem. Soc.* in 1965,^[1] a paper in *Accounts of Chemical Research*,^[2] and, importantly, a final exposition of the ideas in an article of some length in *Angewandte Chemie*.^[3]

Woodward and I used a variety of theoretical approaches in our work—from simple frontier orbital (HOMO/LUMO) arguments, through orbital correlation diagrams, interaction diagrams, and perturbation theory arguments, to detailed molecular orbital calculations. There were good reasons not to use a single approach—symmetry was a way in to the solution, but its unthinking use would have imprisoned us. Still, there was a leitmotif in our work: orbitals and their symmetries mattered, and so did the number of electrons.

Woodward and I were lucky—we not only rationalized a large, piecewise

puzzling, body of chemistry, but also were able to make predictions that were verifiable by a community eminently capable and interested in probing experimentally the consequences of the theory. Orbital symmetry considerations were expeditiously tested; they were found to be portable and productive. The 1981 Nobel Prize in chemistry was awarded to Kenichi Fukui and me for this work; had R. B. Woodward lived, there isn't the least doubt that he would have shared in this recognition.

E. J. Corey's Public Claim

In his 2004 Priestley Medal address, published in *Chemical and Engineering News*,^[4] E. J. Corey writes,

“On May 4, 1964, I suggested to my colleague R. B. Woodward a simple explanation involving the symmetry of the perturbed (HOMO) molecular orbitals for the stereoselective cyclobutene/1,3-butadiene and 1,3,5-hexatriene/cyclohexadiene conversions that provided the basis for the further development of these ideas into what became known as the Woodward–Hoffmann rules.”

The same sentence appears in a 2004 *Journal of Organic Chemistry* perspective by E. J. Corey, “Impossible Dreams”.^[5] This terse reminiscence of a conversation, and a claim of its significance has given rise to considerable discussion and speculation in the community.

Let me recount what I know, remember (and don't remember), and what I infer about the events 40 years ago surrounding this claim. I cannot

speak about the contents of the conversation because I was unaware of it at the time; given that Woodward is no longer alive, we only have Corey's report on it. But in my opinion, the part of E. J. Corey's published statement that reads “...that provided the basis for the further development of these ideas” is not right in its characterization of an episode in an important discovery.

Corey's Claim, Made in Prior Conversations or Exchanges with Me on the Subject

In a visit to Cornell in the 1970s, Corey told me that he made my career by telling R. B. Woodward the HOMO explanation in 1964. To my knowledge, this was the first time he made this direct claim to me. I was puzzled, and I think I ignored the comment.

On November 2, 1981, after the Nobel Prize in Chemistry to Kenichi Fukui and me was announced, Corey wrote me a letter stating his claim in substantial detail. The original of this letter is deposited in my papers in the Cornell University Library (available to the public).^[6] In the letter Corey recounts a conversation he had with Woodward on the evening of May 4, 1964, in which he told Woodward, who posed the problem of the electrocyclic reactions to him (and who seemed, according to Corey, more interested in geometrical explanations), that the explanation might be found in the symmetry properties of the orbitals involved. Corey continues his account by relating how the next day Woodward appeared in Corey's office and told the same

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explanation briefly to Douglas Applequist (a visitor to Harvard that semester) in Corey's presence, as his (Woodward's) "new idea." Corey goes on in his letter to say:

"...the fact is I conceived of the idea, not Bob. In a manner of which few would be capable he pirated the idea, evidently preferring that over my good will. Even more incredible than what Bob did was how he did it."

In his November 2, 1981 letter Corey expressed concern that I would tell the orbital symmetry story in my Nobel Lecture (I had already chosen to talk about something new, the isolobal analogy). And he wanted me to tell people of his, Corey's, contribution.

This letter, coming at the time that it did, and with such claims, disturbed me greatly. I answered it on November 8, 1981.^[6] I said that I would not tell people of Corey's claim—he chose not to do so himself in public (or to Woodward); it would be unfair to Woodward for me to do so then, in 1981, after his death in 1979.

Corey then wrote another letter to me (November 16, 1981)^[6] in which he reiterated and escalated his claims, in a manner that I felt as intimidating to threatening. Here are two excerpts from that letter, which convey its tone:

"You cannot deny that despite the possibility of appalling dishonesty at the roots of your collaboration with Bob, you elected to close your mind. The two of you in effect 'stonewalled' my contribution so completely that you chose not even to make a perfunctory acknowledgement in your papers."

and

"Roald, please consider that history may not deal leniently in this matter, taking seriously the possibility not only of Bob's dishonesty, but of your own not unwitting participation in the extension of fraud."

In the spring of 1984, I visited Harvard for a semester. During that time Corey and I spoke for a couple of hours on the issue. He then sent me another letter on April 29, 1984^[6] in which he gives as the reason that he did not publish his claim in Woodward's lifetime as follows:

"I am amazed that you would think it possible that I would consider doing anything against Harvard, to which I was and am so devoted."

In his first letter to me on November 2, 1981, Corey gave another reason for not writing himself of the way he perceived things had happened:

"I had hoped that Bob himself would do it as he grew older, more considerate, and more sensitive to his own conscience."

To the best of my knowledge, Corey never made an attempt to set the record straight (according to his own perceptions) by speaking of it to Woodward in the 15 years between 1964 and 1979, when Woodward died. Or by publishing his claim. Corey made no public claims, as far as I know, until the statement in his Priestley Medal address in 2004.

Corey did talk to others at Harvard and elsewhere of the matter. In his November 2, 1981 letter to me, Corey writes that he told his story to E. B. Wilson in 1964. And I know he discussed the matter subsequently with Jeremy Knowles. Frank Westheimer and Elkan Blout knew about Corey's claim and agonized over the question for years while composing a memorial notice on Woodward. In the end, despite my objections, Elkan Blout included in his 2001 biographical memoir for Woodward the following sentence: *"In 1964, after a brief discussion with E. J. Corey and others of his thoughts and his chemical results, Woodward was able to formulate his important ideas in this area."*^[7]

Scientific and Personal Background to 1964 Events

To set the stage for my recollections of the period, let me recount the story of my relationship with E. J. Corey and R. B. Woodward.

I received my Ph.D. in 1962 with W. N. Lipscomb, Jr. and M. P. Gouterman, and began a Junior Fellowship in the summer of 1962. I was given an office in the basement of Conant Hall, a few doors down from Corey's office in Converse Hall. And I began to learn organic chemistry, applying the extend-

ed Hückel method that a group of us developed in the Lipscomb group to organic molecules. When I finished the first extended Hückel paper on hydrocarbons I sent a preprint (this was early 1963) to the organic chemists at Harvard—Woodward, Corey, Bartlett, and Westheimer—and then set up appointments to see all of them. This was the first time (after five years at Harvard) that I spoke to Woodward about science.

Of the people I saw, Corey responded most positively to my growing interest in organic chemistry. I was down the hall from him, and as busy as he was, he always welcomed me to talk. By telling me of hot subjects—nonclassical carbonium ions and organic photochemistry—he helped draw me into organic chemistry.

I began to go to organic seminars and went to one of the legendary evening Woodward meetings. In the spring of 1964, one of Corey's colleagues from Illinois, Douglas Applequist, visited Harvard for the semester. He gave a course on strained molecules which I listened to with great interest. Applequist and I talked to each other a lot. I was in the midst of discovering for myself the full beauty of organic chemistry.

My Recollections of the Specific Period in 1964

The simple frontier orbital explanation of the stereochemistry of electrocyclic reactions did not originate with me. It was brought to me by Woodward. Let me outline what I have a record of, what I remember, and what I don't recall, concerning my introduction to that frontier orbital explanation. And how my collaboration with Woodward began.

Woodward describes in two lectures the chemical and historical background of the advent of the orbital symmetry rules. In the first, his 1966 Sheffield lecture,^[8a] he uses the generic "we"; in his second account, the 1973 Cope Award Lecture (published long after, from a copy of a handwritten text by R. B. Woodward),^[8b] there is a direct first person account, as follows:

"I REMEMBER [capitals in the original] very clearly—and it still sur-

prises me somewhat—that the crucial flash of enlightenment came to me in algebraic, rather than in pictorial or geometric [all underlining is in original text] form. Out of the blue, it occurred to me that the coefficients of the terminal terms in the mathematical expression representing the highest occupied molecular orbital of butadiene were of opposite sign, while those of the corresponding expression for hexatriene possessed the same sign. From here it was but a short step to the geometric, and more obviously chemically relevant, view that in the internal cyclisation of a diene, the top face of one terminal atom should attack the bottom face of the other, while in the triene case, the formation of a new bond should involve the top (or *pari passu*, the bottom) faces of both terminal atoms.”

In this period I kept a hard-bound notebook. But I did not keep it carefully, with only occasional dates written in. There is in the notebook an April 17 date for a seminar by George Olah, and the next date in the notebook is May 5,

1964 (Figure 1) the day after the Corey and Woodward conversation (as reported by Corey).

The May 5 entry (p. 80; the entire notebook, covering roughly the period March to June 1964, is in the Cornell University Library, collection cited^[6]) says “Talk with Woodward and Applequist,” and below it contains the stereochemical essence of the four- and six-electron electrocyclic reaction, without orbitals. But clearly orbitals and their phases are in the discussion, because below these reactions I sketch the cyclopropyl to allyl opening and draw the lowest allyl orbital as presumably controlling the disrotatory motion in the cation.

This is the first mention of electrocyclic reactions in my notebook. But I believe I heard of the HOMO symmetry argument earlier (and prior to May 4, the date of the Corey–Woodward meeting) in—I think—a conversation I had with D. E. Applequist, who recounted it as an explanation that Woodward suggested and asked me for my reaction to

it. I have no written record of that early conversation. It must be said, however, that Applequist does not think he had such a conversation with me prior to May 5, 1964.^[9]

Returning to my notebook, after some thoughts on cyclopropyl cation, two pages later I sketch out a plan for studying what would later be called con- and disrotatory motion. These preoccupy me for quite a few pages/days (no further dates appear in the notebook until June 9, 1964); the discussion does not focus on orbitals but is “computational”. It is pretty well summarized in our first paper. At this point I was still a computational chemist, and even though I used orbitals and perturbation theory, explanations constructed in frontier orbital language were not yet a common part of my work and thought. They quickly became so. In fact, this—a recognition of the force of simple bonding arguments, and their utility in a theory–experiment dialogue—was on a personal level the main consequence for me of the orbital symmetry story.

Here’s how Woodward described the beginning of our interaction, in his Cope Award Lecture:

“AFTER a brief false start in extending these ideas, attributable clearly to my *gaucherie* in the details of quantum chemistry, I very soon realized that I needed more help than was available in my immediate circle, and I sought out Roald Hoffmann... I told him my story, and then, essentially, put to him the question ‘Can you make this respectable in more sophisticated theoretical terms?’”^[8b]

Woodward and I began to talk in that period. An important conversation I recall took place after a seminar by W. von E. Doering. By that time I had done the calculations and could confirm the ground- and excited-state stereochemical preferences. The electrocyclic reaction paper was submitted on November 30, 1964, and published in the January 20, 1965 issue of the *J. Am. Chem. Soc.*^[6a] Then R.B. Woodward and I went on to the rest of our orbital symmetry work. There was a lot to do...

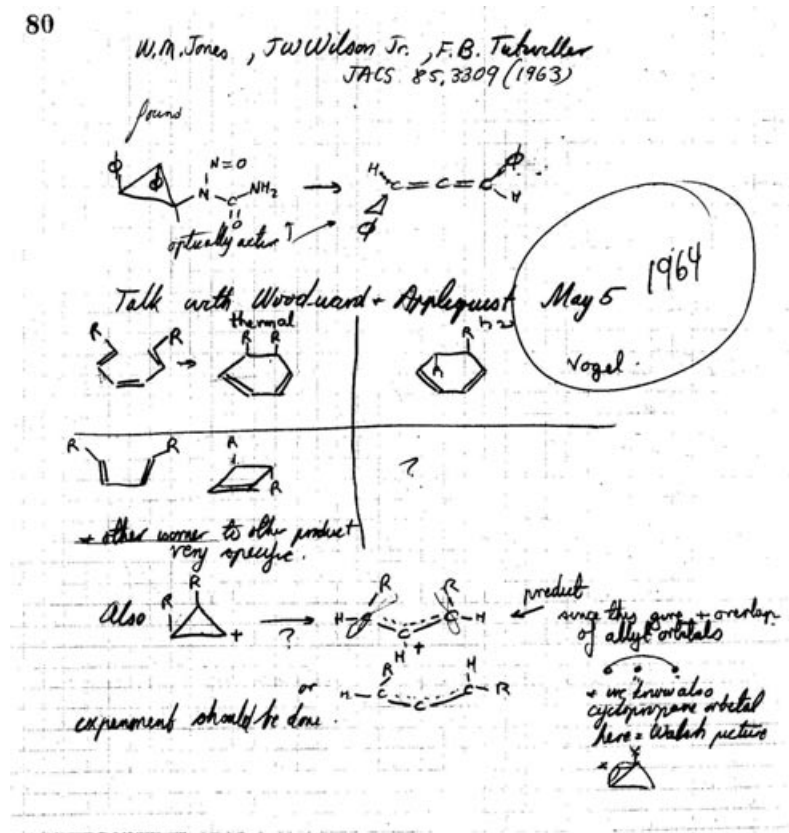


Figure 1. May 5, 1964, entry in Roald Hoffmann’s notebook. The year in the date was written in some time later.

Corey's Role in the Orbital Symmetry Story and Woodward's Denial of It

In the letter to me of November 16, 1981, E. J. Corey says that he told me in May 1964 that he had suggested to R. B. Woodward the HOMO explanation for the stereochemistry of electrocyclic reactions, and that Bob had taken over the idea from him. I spoke to Corey often in that period, but I have no recollection of such a conversation; I think Corey's statement would have been so striking (especially the part about Woodward taking the idea from Corey) that I would have remembered it.

Nevertheless, there *was* an idea in my mind then that Corey had a stake in the electrocyclic reaction story; my reconstruction today is that in the late spring and summer of 1964 Corey kept talking to me as if he did have such a role, as if the electrocyclic reaction research were joint work between Corey, Woodward, and me. Eventually, I was sufficiently puzzled by this that I did speak to Woodward. I do not remember when that was; I suspect it was later, in the fall of 1964, when we wrote our first paper.

When Woodward and I spoke of the matter—again, I cannot remember when it was—I asked him directly if Corey had a role in this work. He said “no”—this was our (R.B.W. and R.H.) work.

I did not question Woodward further. In retrospect, I regret not doing so, and I regret not asking Corey more directly to tell me his role in the work. R. B. Woodward was 47, the top synthetic organic chemist in the world. E. J. Corey was 36, a full Professor at Harvard. In May 1964 I was 26, a new Ph.D., a Junior Fellow to be sure, but just starting out in my career.

In the more than a year that I remained at Harvard (I left in June 1965), I kept on talking to Corey and learned a lot from him. I do not remember any cooling of our relationship, though I certainly spent more and more time with Woodward. Corey, in his correspondence with me, disagrees, saying that our previously frequent conversations stopped after I began working with Woodward.

My Perception of What Happened

I have no reason to doubt Corey's statement that there was a conversation between Corey and Woodward on May 4, 1964, even though we have only Corey's account of it.

But, I also do not think that the conversation “*provided the basis for the further development of these ideas into what became known as the Woodward–Hoffmann rules*,” as Corey writes. Here are some factors that go into my reasoning:

- Woodward had a habit of posing to people problems of great interest. And also of posing such problems as puzzles, even if he had the solution. Specifically, we know that he talked about the electrocyclic conundrum—to H.C. Longuet-Higgins, and Lionel Salem, for instance—as it was taking shape experimentally in the work of others and in the vitamin B₁₂ synthesis he was pursuing in collaboration with A. Eschenmoser's group.
- I think Woodward was perfectly capable of coming up with the frontier orbital explanation by himself. Though he began his career with VB arguments, I found Woodward in 1964 pretty well-versed in MO ideas. Woodward had learned a lot from William Moffitt, around the ferrocene and octant rule stories. Orbital ideas were in the air, the classic Streitwieser and Roberts books widely read, the success of the Hückel rule amply demonstrated. There was also the L. J. Oosterhoff “forerunner” (see below), which Woodward knew about.

One interesting piece of evidence for Woodward's knowledge of molecular orbitals is in a published comment he made after a lecture by Rolf Huisgen in a Welch Foundation Conference in 1961.^[10] Woodward draws quite explicitly the orbitals of a vinyl carbene (albeit without phases), and asks “*Do we have specific orbital geometric requirements [for Huisgen's reaction] and would they in this case preclude the operation of the mechanism which is so general in many of the cases?*”

My strong feeling is that Woodward had the frontier orbital idea before he spoke to Corey. I think that Corey's coming up with a similar way of thinking about the reaction in that May 4 conversation perhaps might have prompted Woodward to push on, and to ask for my assistance with calculations supporting the frontier orbital approach. For some reason, he was unsure of himself (not a characteristic one normally associated with Woodward...) and needed the support of a theoretical chemist.

Going back to the Corey–Woodward meeting of May 4, 1964, it would not be the first time that two people came away from a conversation with diametrically opposed ideas of what was said.

Crediting Suggestions: Then and Now

Assuming that the contents of the May 4, 1964 conversation were as E. J. Corey reports them, then I think that at best Corey has a claim to a rediscovery of the Oosterhoff orbital argument of 1961. In a paper by E. Havinga and J. L. M. A. Schlatmann, the authors write:

As Prof. Oosterhoff pointed out, another factor that possibly contributes to this stereochemical difference between the thermal and photoinduced ring closure may be found in the symmetry characteristics of the highest occupied π orbital of the conjugated hexatriene system.^[11]

The Oosterhoff “forerunner” to the orbital symmetry control ideas is mentioned in a footnote in our first 1965 paper; its role has been discussed by J. A. Berson in his book “Chemical Creativity.”^[12]

In 1963–1964, E. J. Corey and A. G. Hortmann accomplished the total synthesis of dihydrocostunolide. A crucial step involved a stereospecific electrocyclic reaction. The two papers on the work^[13] do not have any reference to an electronic factor in the stereoselectivity. Neither is the Havinga and Schlatmann paper mentioned. The full Corey and Hortmann paper was submitted on August 5, 1965, some months after publication of the first paper by Woodward and me.

The full paper on the dihydrocostunolide synthesis provided a place for Corey to state his electronic explanation. It was an opportunity not taken.

Should Corey's suggestion of the frontier orbital explanation for the stereochemistry of electrocyclic reactions have been credited in our first paper and subsequent ones? I can only speak for myself, and from today's perspective. We all make decisions, some easy, some hard, of what to reference/credit in our papers. If the substance of what E. J. Corey reports was said in that conversation with Woodward is true, then I believe it should have been credited. As Oosterhoff's suggestion was. I would be ready today to regret the omission and apologize to Corey for it. But, I am torn... between the conviction with which Corey tells his story in 1981 and today, torn between that and the impossibility of Woodward commenting on the decision today, as well as Woodward's 1964 denial to me of a role for Corey. With Woodward not alive, and no record except Corey's memory for the essence of that conversation, I just cannot say today that the conversation should have been credited.

A Claim Unfairly Made

The matter should have been argued out in 1964, by Corey persisting in telling Woodward of his (Corey's) perception of his contributions. Seventeen years later, Woodward gone, Corey,

without publishing his account, wanted me to tell others that *his* claim was valid.

People will form their own opinions of Corey's explanation of why he didn't press a public claim (that it would have hurt Harvard, that Woodward would eventually give him credit). Too bad he didn't speak out; had Corey done so in 1964, or in the period 1964–1979, we could have had Woodward's viewpoint.

I believe that E. J. Corey's perception of the consequences of what he told R. B. Woodward is just that—what he, Corey, believes. Based on the arguments I made above, and on what I remember, I don't think that the conversation in question influenced in any significant way the beginning of the orbital symmetry control story. And given that Corey's claim was (and is being) made after Woodward's death—when the claim could have been made for 15 years in Woodward's lifetime—that claim is also deeply and fundamentally unfair.

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