

## Diverse Views in the Assignment of Credit for Research Discoveries

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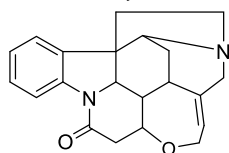
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I am always alert to differences of opinion. So, I immediately reacted to a statement in Albert Padwa's 2007 publication on his total synthesis of strychnine.<sup>1</sup> Padwa wrote,

*... the structural elucidation of strychnine, the most famous of [the Strychnos] alkaloids, by Robinson in 1946.*<sup>1</sup>



1 = Strychnine

My surprise was due to Padwa's claim that it was Robert Robinson and not R. B. Woodward who should be credited for solving the structure of strychnine. My astonishment increased substantially after discussing this distinction with Padwa. I described to him the history of Robinson's and Woodward's publications of the late 1940s. As I will elucidate further below, it was Woodward and not Robinson whose assignment of strychnine's structure was made unambiguously, unequivocally, and irreversibly. But the facts as I presented them to Padwa were unpersuasive to him. Yet as a result of our discussions, I came to understand that there was some validity to Padwa's position.

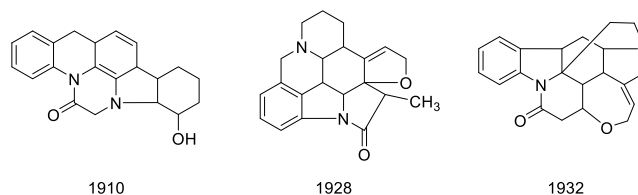
Shortly thereafter, I came across a 1958 memoir written by Rolf Huisgen, the eminent Munich chemist, on the occasion of his 1958 Heinrich Wieland Memorial Lecture. Huisgen wrote:

*"The establishment of the structural formula of strychnine on the basis of the large mass of experimental data is due mainly to Sir Robert Robinson. R. B. Woodward's synthesis corroborated it."*<sup>2</sup>

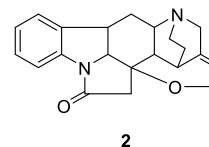
I knew Huisgen well.<sup>3,4</sup> Indeed, I had edited Huisgen's autobiography published by ACS Books.<sup>5</sup> Huisgen was obsessive about data, logic, words, and conclusions.<sup>6</sup> My careful analysis of Huisgen's words suggests that Huisgen had refrained from making a public credit allocation.

Here are the historical facts. The isolation of crystalline strychnine in 1819 by Pierre-Joseph Pelletier and Joseph Bienaimé Caventou<sup>7,8</sup> is of historical importance in that it was the first demonstration that "acid-fixing [alkaloids] are produced in the vegetable kingdom."<sup>9</sup> For the next 130 years, many chemists, including the most elite organic chemists of the field, attempted to determine the structure of strychnine. More than 270 scientific publications appeared before the definitive structure was determined in 1946–1947.<sup>2,9</sup> Between 1910 and 1947, Robinson proposed numerous structures for strychnine, beginning as a graduate student at the University of

Manchester (1906–1909) with his professor William Henry Perkin, Jr.<sup>10</sup> and, ultimately, at Oxford University where Robinson succeeded Perkin as the Waynflete Professor of Chemistry.<sup>11–13</sup> Several examples of Robinson's proposed structures for strychnine are shown immediately below.<sup>11</sup>



In a paper published on January 15, 1946, Robinson proposed the correct structure for strychnine, 1.<sup>14</sup> In a paper published on February 22, 1947, Robinson changed his mind and published yet another (and, of course, incorrect) structure 2.<sup>15</sup>



Then, in a paper published on July 5, 1947, Robinson recanted his incorrect structure 2 and reverted to a number of possibilities, one of those many being 1.<sup>16</sup> In that latter paper, Robinson wrote:

*"We revert, as the best hypothesis to guide future work, to an earlier suggestion. . . . Several slight modifications of this expression [1] are feasible, and these have special advantages and disadvantages which must be discussed at a later date, especially since it is probable that crucial experimental tests can be devised."*<sup>16</sup>

In a one-page communication published on September 1, 1947,<sup>17</sup> Woodward assigned the correct structure to strychnine. On June 1, 1948, Woodward published a definitive nine-page full paper, detailing his reasoning and providing full experimental details of his experiments.<sup>18</sup>

Given these facts, I ask: who deserves credit for determining the structure of strychnine: Robinson or Woodward?

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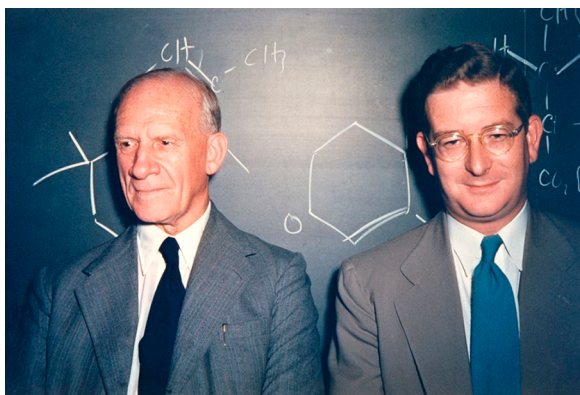
Scientists consider receipt of appropriate credit for their accomplishments central to their profession and a prime motivator for their research, but the determination of credit can be complicated by many interrelated factors. Unfortunately, there is little generally accepted guidance regarding credit allocation for historical events even though there is substantial literature and published standards regarding authorship criteria within one's own research project.<sup>19–21</sup>

I wondered what a larger sampling of chemists would reveal about who deserves credit for determining the structure of strychnine: Robinson or Woodward? About a decade ago, my colleague Mark C. House, who is an expert in marketing and opinion polling, and I began to conduct surveys of academic chemists and other professionals in Ph.D.-granting institutions in the U.S. The primary goal of that research was and continues to be to identify and quantify the experiences and perspectives of chemists on topics dealing with responsible conduct of research (RCR) and ethics in science.<sup>10,22–26</sup>

In a recent survey, House and I designed a question that represents the Robinson–Woodward strychnine credit issue, though we hid the names from the respondents so as to avoid any bias. We asked: Who deserves credit for a scientific discovery (or an explanation for a natural phenomenon): the first person (“Professor A”) who made the discovery (or explanation) but who subsequently abandoned that discovery (or explanation) in favor of an alternative but then reversed course though in an ambiguous and equivocal fashion or another individual (“Professor B”) who, after the above abandonment and the ambiguous revival, proposed the correct discovery (or explanation) and never recanted this proposal or both?

Fourteen percent of the academic chemists who responded to the survey reported that they would give full credit to “Professor A”(Robinson), 21% to “Professor B” (Woodward), and 65% to both. There was no like-minded, concordant opinion as to who should receive the credit for determining the structure of the natural product. The absolute majority of the respondents felt that both Professor A and Professor B should share the credit.

Had this survey and its conclusion been published during his lifetime, Robinson would certainly have been angered (Figure 1). It might have mattered less to Woodward. In the late



**Figure 1.** Sir Robert Robinson (left) and R. B. Woodward, ca. 1952. Their animosity is clearly evident from their facial expressions and their posture. Credit is due to the photographer, John D. Roberts, that these two chemists would even agree to pose next to each other in the early 1950s. Photograph courtesy of J. D. Roberts.

1940s, Woodward had not yet reached his prime and was certainly concerned about his future, perhaps not necessarily at Harvard but certainly in the eyes of his peers. Recall that Woodward had been promoted to Associate Professor only in 1946. Woodward was also sensitive to credit issues as revealed years later in his letter to the Nobel Committee for Chemistry<sup>27</sup> when the 1973 Nobel Prize was awarded to Ernst Otto Fischer and Geoffrey Wilkinson “for their pioneering work, performed independently, on the chemistry of the organometallic, so called sandwich compounds” without mention that it was Woodward who first published the correct structure of ferrocene with Wilkinson in 1952.<sup>28</sup>

A previous survey by House and me revealed other instances in which there was great diversity in responses from the academic chemists. That survey illuminated some of the reasons for the diversity we are herein observing. For example, House and I asked about the credit the respondent would give to an individual who provided a suggestion that permitted the successful completion of the respondent's research project but otherwise had no further contribution to the project.<sup>22</sup> The responses (to provide coauthorship, an acknowledgment, or nothing) varied greatly, in part as a function of the age of the respondents. There was also great diversity regarding the criteria that the respondents cited in their own coauthorship decisions.<sup>23</sup> The survey revealed that the most important influence on credit decisions was “It just seems to be the right thing.” Lowest on the influence scale were the codes provided by the ACS, the NSF or the NIH, or the respondents' own institutions.<sup>23</sup>

Essentially, the survey respondents make up their own authorship criteria *ad hoc*. No doubt there is diversity in the assignment of credit by the senior authors, given the idiosyncratic nature of their decision-making processes. Our survey also found that 50% of respondents had experiences in which they felt they failed to receive the credit they deserved.<sup>10</sup> The conclusion is that those responsible for credit decisions are using criteria that are often different from those used by individuals who are receiving the credit. In sum, these results indicate there are multiple, inconsistent criteria being used within the chemistry community for the allocation of credit.

The eminent philosopher of science Thomas S. Kuhn was conscious of the need for a common lexicon and the value of diversity within the scientific community when he wrote some 50 years ago:

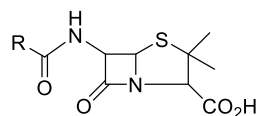
*“Shared values can be important determinants of group behavior even though the members of the group do not all apply them in the same way. . . . Imagine what would happen in the sciences if consistency ceased to be a primary value. [Also] individual variability in the application of shared values may serve functions essential to science. The points at which values must be applied are invariably also those at which risks must be taken.”<sup>29</sup>*

So, we reach an interesting conundrum. Within the science profession, we encourage, reward, and honor individual initiative, imagination, and diversity of backgrounds and opinions. At the same time, some aspects of being a scientist require uniformity in behavior. These include following the codes of conduct and ethical standards of our profession. As we strive to achieve balance between individualism and uniformity—including credit attributions, as revealed in the above discussion—we must rely on the better angels of our nature. And we must act to insure and enhance the common good.<sup>30</sup>

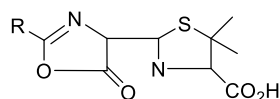
## ■ CODA

Robinson was not one to forget or forgive. He carried to his deathbed<sup>31</sup> his anger at Sir Christopher Ingold for Ingold's theft of Robinson's electronic theory of organic chemistry, as Robinson saw it.<sup>32–36</sup>

The relationship between Robinson and Woodward was more complex, in that it had several phases. Their first battle involved the structure of penicillin. During World War II, Woodward had proposed a  $\beta$ -lactam structure for penicillin, while Robinson dug in his heels with the thiazolidine-oxazolone structure.<sup>37–39</sup> The  $\beta$ -lactam structure was subsequently unequivocally established by Dorothy Crowfoot Hodgkin's X-ray analysis,<sup>40</sup> though Hodgkin's results were not persuasive to Robinson.<sup>41</sup>



Penicillin  
 $\beta$ -lactam structure



Thiazolidine-oxazolone structure  
demanded by Sir Robert Robinson

Their second battle was the structure of strychnine.

Their third battle was on the total synthesis of cholesterol. Woodward was again first.<sup>42–46</sup>

In contrast to the Robinson–Ingold relationship, the relationship between Robinson and Woodward was, in time, transformed into one of both friendship and collaboration with implications that lasted far beyond their own lives. Robinson and Woodward eventually would serve as cochairs of the Honorary Editorial Advisory Boards of *Tetrahedron* and *Tetrahedron Letters* from the first issues of both journals (1957 and 1959, respectively). Robinson and Woodward would work closely together to ensure the success of these, the first international chemistry journals. Clearly, an open mind, a generosity of spirit, and flexibility can reverse self-defeating strategies and help us serve ourselves and our community more wisely.

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## Notes

Views expressed in this editorial are those of the author and not necessarily the views of the ACS.

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