

From “multiple simultaneous independent discoveries” to the theory of “multiple simultaneous independent errors”: a conduit in science

Jeffrey I. Seeman¹ 

Published online: 13 February 2018

© Springer Science+Business Media B.V., part of Springer Nature 2018

Abstract Multiple simultaneous independent discoveries (MIDs), so well enunciated by Robert K. Merton in the early 1960s but already discussed for several hundreds of years, is a classic concept in the sociology of science. In this paper, the concept of multiple simultaneous independent errors (MIEs) is proposed, analyzed, and discussed. The concept of Selective Pessimistic Induction is proposed and used to connect MIDs with MIEs. Five types of MIEs are discussed: multiple errors in the interpretation of experimental data or computational results; multiple misjudgments of the value of another’s research results or conclusions; multiple cases of false anticipation of achieving a certain experimental result; multiples of ignoring or omitting relevant precedents; and multiple instances of failure due to a not-yet-conceived scientific concept or principle. Causal MIDs and MIEs are those that can be traced directly to antecedent knowledge. Acausal MIDs and MIEs are those involving a consequential and identifiable leap from antecedent knowledge. Examples of causal and acausal MIEs are provided, mostly but not exclusively from the discipline of chemistry. Comparisons are made between MIDs and MIEs. Topics for future research are discussed and implications of these concepts are proposed.

Keywords Multiple independent simultaneous discoveries · Multiples · Singletons · Multiple independent simultaneous errors · Robert K. Merton · Pessimistic Induction

Dedicated to Harriet Zuckerman in honor of her 80th birthday, July 19, 2017 and to Roald Hoffmann in honor of his 80th birthday, July 18, 2017. This paper also celebrates the career of Robert K. Merton (July 4, 1901–February 23, 2003).

✉ Jeffrey I. Seeman
jseeman@richmond.edu

¹ Department of Chemistry, University of Richmond, Richmond, VA 23173, USA

Here again, one instance must stand for many. – Merton (1961)

Introduction

Scientists are hungry for discovery. Recognition of originality is a crucial aspect and driving force in the progress of science. In addition to studying the role of competition and the race to publish first (Merton 1969, 1973), sociologist of science Robert K. Merton (Fig. 1) proposed in 1961 the generalization of multiple independent simultaneous discoveries (MIDs) or “Multiples,” announcing

The strategic fact of the multiple and independent appearance of the same scientific discovery – Merton (1961, 1973).

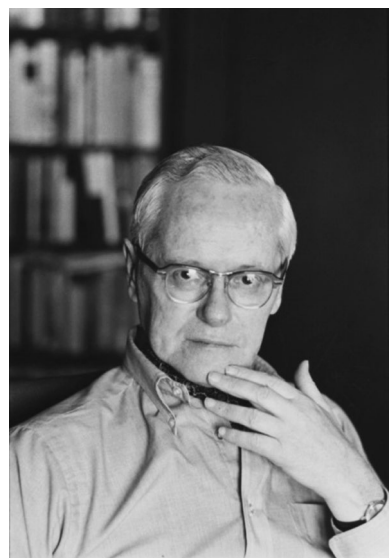
Mostly limiting himself to the period 1820–1920, Merton provided a partial list of no fewer than 20 occasions “on which the fact of multiples with its implications for a theory of scientific development has been noted.” He emphasized the diversity of the examples and posited that they were “independently set forth.” Before Merton, and cited by him among many precedents, Ogburn and Thomas (1922) created a list 148 examples of MIDs. Ogburn and Thomas cited Kroeber’s 1917 paper (Kroeber 1917), as did Merton, which stated

The whole history of inventions is one endless chain of parallel instances ... A volume could be written, with but few years’ toil, filled with endlessly repeating but ever new accumulation of such instances [as MIDs] (Kroeber 1917).

Neither Merton nor I could refrain from an even earlier-than-Merton exposition of the Hypothesis of Multiples, that by Benjamin Franklin, who wrote to the Abbé de la Roche,

I have often noted, in reading the works of M. Helvétius, that, though we were born and brought up in two countries so remote from each other, we have often hit upon

Fig. 1 Robert K. Merton, ca. 1990. Photograph courtesy of and © Jill Krementz and courtesy of the estate of Robert K. Merton, Harriet Zuckerman, Executor



the same thoughts; and it is a reflection very flattering to me that we have loved the same studies and, so far as we have known them, the same friends, and the same women (Smyth 1907).

In spite of the nearly 100 years of use of the Hypothesis of Multiples, including its authoritative and prominent pronouncement by Merton, an important and useful corollary of this concept has yet to be explored, and it is the intent of this publication to do so. This corollary is a close relative of Merton’s Multiples, namely, the Hypothesis of Multiple Independent Simultaneous Errors (MIEs), whereby

“Error” very broadly means a scholarly endeavor in which the investigator(s) commit(s) some type of flaw or miscalculation that leads to an incorrect analysis or conclusion.¹

As for timing of the MIDs and MIEs, Saragoglou et al. present examples of “near-coincident discovery of significant advances” (Saragoglou et al. 2012) by three researchers whose discoveries were in 1939, 1949, and 1959, respectively. The “degree of simultaneity” may be wider than anticipated, especially during time periods when communication was by horseback rather than the internet. In this work, by “simultaneous” (or near-coincident) in timing, we lean on Merton’s caveat when discussing MIDs. Merton posited that the MIDs need not be

chronologically simultaneous... Even discoveries far removed from one another in calendrical time may be instructively construed as ‘simultaneous’ or nearly so in social and cultural time, depending upon the accumulated state of knowledge ... (Merton 1961).²

Numerous examples of MIEs will be presented in the following sections. Beyond establishing the validity of the Principle of Multiple Independent Errors that will stand alongside the Principle of Multiple Independent Discoveries, the focus on discoveries and errors opens many doors of further inquiry and begs connection to several important and more recent themes not previously considered.

There is the general understanding, even within the research chemical community (Laidler 1995), that scientific knowledge is rather unstable over time. Thus, discoveries can subsequently find themselves categorized as mistakes, misinterpretations, misjudgments, or premature conclusions. Indeed, Laudan’s Pessimistic Induction (PI) (Laudan 1981) points out that most theories will be rendered false by later science. To the extent that Pessimistic Induction is valid, then over time, instances of the application of Merton’s Principle of Multiple Independent Discoveries naturally transform themselves into instances of the Principle of Multiple Independent Errors. The concept of Pessimistic Induction will be discussed more fully below, acknowledging the vast literature disputing PI. That being said, the new though rather obvious concept of Selective Pessimistic Induction (sPI) will

¹ It may be that the term “errors” is poorly chosen for this concept. Philosophers speak of theories being false or closer or more distant from the truth (Kitcher 1993). In this paper, the choice of the word “error” was made based on nomenclature simplicity, not on any sophisticated analysis of concepts.

² It may be noted that the majority of examples cited herein come from chemistry, the author’s original educational and research discipline. It is not unusual that in discussions of history and philosophy of science, examples from one discipline will very much outweigh those from other disciplines. For example, all of the examples in Paul Feyerabend’s classic *Against Method* (Feyerabend 1975) come from physics and Gilbert and Mulkay’s classic *Opening Pandora’s Box* (Gilbert and Mulkay 1984) come from biology.

be proposed and used in conjunction with Merton's Multiple Independent Discoveries to lead to the tightly-related concept of Multiple Independent Errors.

Given that discoveries and errors are so tightly interwoven, it will be worthwhile to, at least briefly, examine the four indispensable components of the strong programme as they relate to errors as they do to discoveries. Are failed theories, indeed all "errors" in science, explained exclusively by the biases of the researchers including what are today called "conflicts of interest" while discoveries are not subject to such biases? As will be evident in the many examples below, both the discoveries and the errors are subject to the same social and scientific factors.

As for commonality, it is clear from the rush to discover and publish that errors in science must be just as common as discoveries, if not more so—for there are more ways to formulate hypotheses that are inconsistent with nature than the converse. But, after the community accepts a paradigmatic explanation as truth, or evidence mounts for one description of reality, fewer "errors" will be published. That is, an asymptotic trajectory to the truth will be reached.

Merton believed that multiples were the common pattern in science. In fact, Merton suggested that there likely were few true singleton discoveries (Merton 1961, 1973). Are errors in scientific research these days so pervasive that the phenomenon of Multiple Independent Errors far exceeds that of Multiple Independent Discoveries? If so, it would be ironic, given that MIEs have not been studied to date, while MIDs are now a classical concept in the sociology of science.

Errors and reproducibility are very much connected. In 2005, John Ioannidis published a paper entitled, "Why Most Published Research Findings Are False" (Ioannidis 2005), a theme that continues to this day (Chalmers et al. 2014; Button et al. 2013). These researchers focused primarily on the biomedical field. Beyond errors caused by intentional fabrication and falsification (misconduct of science) and are thus unlikely to be multiples, the broad scientific and medical communities are increasingly concerned about lack of reproducibility (Grens 2014; Leek and Peng 2015; Baker 2016; Bergman and Danheiser 2016; Goodman et al. 2016). If experiments cannot be replicated, then at least one experiment or its description is in error. Even newspapers are publishing articles dealing with the "reproducibility crisis" (van Bavel 2016). And ironically, there is a concern that "reproducible research can be wrong" (Leek and Peng 2015).

In this paper, definitions and examples of multiple causally-independent and acausally-independent errors will be provided. MIDs and MIEs have many similarities beyond their obvious dissimilarity, that being one is a discovery and the other is an error or falseness. The commonalities and dissimilarities between MIDs and MIEs will be discussed in detail. Accepting that errors are just as important and pervasive as "true" discoveries, we will apply Merton's classification to errors.

We had hoped that there would be no literature precedent for the concept of Multiple Independent Errors. Neither Ogburn and Thomas (1922) nor Merton (1961, 1973) had expanded their ideas to include errors. But we were reluctantly prepared to be only the rediscoverers of this concept—to be another example of a MID. If this were to be the case, we would be in good company, since, as enumerated by Merton, many notable scientists had the experience of being second (or later) discoverers though they had originally thought they had been the first to a discovery. This includes such achievers as philosopher August Comte, historian George Sarton, physicist Albert Einstein, and of course, polymath Benjamin Franklin.

Well, that list of rediscoverers now includes this author. After the research for this project was nearly completed, this author came across D. Lamb and S. M. Easton's 1984 book

Multiple Discovery: The Pattern of Scientific Progress (Lamb and Easton 1984). They treat MIEs in one page, buried in the middle of the book, almost as an afterthought. This single appearance has not received any currency and its scope and generality have not been realized hitherto (Woodward and Schramm 1947; Woodward and Hoffmann 1965).

This author rushes to emphasize that the main if not sole theme of this paper is the proposal and justification though many examples of the concept of multiple simultaneous independent errors. The expansion of this concept to its relationship with multiple simultaneous independent discoveries, pessimistic induction, the strong programme, a more erudite discussion of errors and knowledge, and other topics in the philosophy of science is available for subsequent study and discussion by others. I also emphasize that the proposals put forth in this paper were instigated by observations from primarily chemistry, further analyses from all perspectives are welcome and encouraged.

Definitions

It would be fatuous to define “error” as the opposite of “discovery,” the latter as used by Merton, Ogden and Thomas, Kroeber, and the many others who had discussed Multiple Independent Discoveries before them. In part, this is because *none* of these authors actually defined what was meant by the term “discovery.” At some risk, I shall dip my toes cautiously into ‘errors and the sociology of science.’

Scientists deal with many types of errors. For our purposes, these errors are divided into three categories:

1. Misconduct of science errors
2. Experimental errors
3. Human or “honest” errors

Misconduct of science errors are the most serious errors that can be made by a scientist. Briefly, misconduct of science includes intentional and reckless acts of plagiarism, fabrication and falsification (Steneck 2007; White House Office of Science and Technology Policy 2000). Misconduct of science is outside the scope of “error” as used in this work.

Experimental errors include systematic errors, random errors, and blunders such as recording an incorrect value. These are typically mechanically-related or measurement errors, and terms such as accuracy and precision are relevant. One can even obtain a distribution of measurement values of the experimental parameter of interest, e.g., mass, velocity, optical rotation, “area under the curve,” and calculate a mean value and a standard deviation, sometimes referred to as the “standard error.” Generally, MIEs will not involve these types of experimental error.

Nonexperimental, human or “honest” errors involve, for example,

- 3a. Errors in the interpretation of experimental data or computational results;
- 3b. Judgments of the value of another’s research results or conclusions that are inconsistent with the experimental data;
- 3c. False anticipation of being able to achieve a certain experimental result;
- 3d. Ignoring or omitting relevant precedents; and
- 3e. Ignorance of a not-yet-determined law of nature.

We define Multiple Simultaneous Independent Errors as

Near-coincident instances in which two or more individuals (or research groups) independently make the same or nearly the same error.

Examples of MIEs of these five classes (3a–3e) will be presented in the following four sections.

A reviewer asked, “Do you take as a given that the current state of science allows you to know the truth about everything that you discuss so that you can identify falsity when you see it?” Perhaps the best answer to this question is as follows. Scientists do their best, they rigorously apply various methods to obtain, collect, analyze and evaluate their results. Interpretations are presented. Subsequent research may lead to the conclusion that the initial interpretations were inconsistent with the earlier or entire package of experimental data. Further research may lead to yet more refined data and even more modified conclusions. That is the way of science.

All research leans to some extent on a shared common knowledge base, and hence, research projects cannot be entirely, completely independent of the past. To some extent, we all “Stand on the Shoulders of Giants” (Merton 1942, 1965). MIDs and MIEs can be made by individuals or teams who have never communicated, or even know of each other, and yet arrive at identical conclusions without the benefit of anything more than what is commonly known by people skilled in the art and reasonably knowledgeable of the literature. Consider first the scenario where there is no information known by various independent researchers that affirmatively, substantively and consequentially pointed them in the same direction to make the same discoveries (or errors). We then define these MIDs and MIEs to be “acausally-independent.”

Alternatively, MIDs and MIEs can be independently made by others but based very directly on shared information. They may be said to have a causal, though independent, relationship. These MIDs and MIEs will be referred to as “causally-independent.”

The terms “acausal” and “causal” are taken from Carl Jung’s concept of *Synchronicity*, “which holds that events are ‘meaningful coincidences’ if they occur with no causal relationship yet seem to be meaningfully related” (Jung 1973). Jung wrote,

How are we to recognize acausal combinations of events, since it is obviously impossible to examine all chance happenings for their causality? The answer to this is that acausal events may be expected most readily where, on closer reflection, a causal connection appears to be inconceivable (Jung 1973).

Another way of looking at the difference between causal and acausal relationships is to examine the reasoning behind the actions of the scientists. In casual MIDs and MIEs, the reasoning by the independent scientists runs parallel if not identically. They result from the same stimulation(s) and follow a very similar line of logic. In acausal MIDs and MIEs, there is neither a common stimulus nor a common line of logic.

Merton touched on causally-independent and acausally-independent discoveries, but only briefly, and in his examples, Merton did not characterize them as one or the other. Merton wrote,

Discoveries in science are of course not all of a piece. Some flow directly from antecedent knowledge in the sense that they are widely visible implications of what has been done just before. Other discoveries involve more of a leap from antecedent knowledge, and these are perhaps less apt to be actual multiples (Merton 1961).

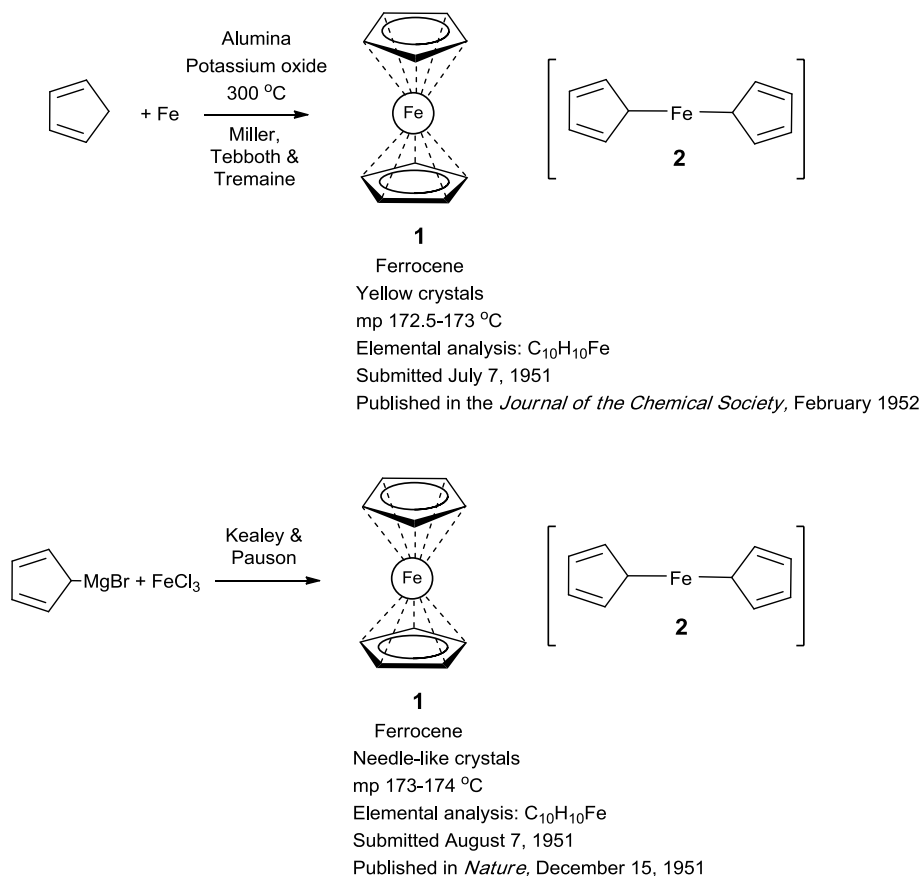


Fig. 2 The first two independent syntheses of ferrocene (**1**) are noteworthy because two research groups both suggested the same incorrect formula **2**

Examples of both acausally-independent and causally-independent multiple errors will be presented below.

Errors in the interpretation of experimental data or computational results

The first example of acausally-independent multiple errors to be discussed herein, that being the discovery and identification of the organometallic compound now known as ferrocene (**1**, see Fig. 2), is scientifically and historically important. This research initiated an entirely new discipline of organic chemistry which has enormously prospered to this day: organometallic complexes.

On July 7, 1951, Miller et al. (1952) of the British Oxygen Company, London, submitted a paper to the British *Journal of the Chemical Society* that was published in February 1952, in which they reported the preparation of a compound having the formula C₁₀H₁₀Fe (see Fig. 2, top equation). Just a month later, on August 7, 1951, John Kealey

and Peter Pauson from Duquesne University in Pittsburgh submitted a paper (Kealy and Pauson 1951) to the journal *Nature* that was published on December 15, 1951 (a month or more earlier than the appearance of the Miller et al. paper), in which they reported the preparation of a compound having the exact same formula $C_{10}H_{10}Fe$ as Miller et al. (see Fig. 2, bottom equation). Miller et al.'s synthetic route was considerably different from that of Kealy and Pauson. Of note is that both the British Oxygen Company's chemists and the Duquesne University chemists proposed *the exact same but erroneous* chemical structure 2. The brackets enclosing structure 2 in Fig. 2 indicate that this proposed structure was, in fact, subsequently shown to be incorrect. These two groups had no knowledge of each other, nor was their research an independent though parallel response to the same chemical stimulus. This is an example of a MIE, in this case, a multiple acausally-independent error.

A reviewer thoughtfully asked the excellent question,

Is the Pauson/Miller ferrocene error acausal because there was no stimulus that led them both to try the reactions they tried, or because there was no obvious cause for them to propose the same erroneous structure?

There was no shared motivation that led both research groups to use the (different) reactions that led to the preparation of the previously unknown compound $C_{10}H_{10}Fe$, which indicates an acausal discovery. Once they experimentally determined that the empirical formula for the compound was $C_{10}H_{10}Fe$, that is, the compound had 10 carbon atoms and 10 hydrogen atoms for every iron atom and had 21 atoms in total, their independent assignment of the same (though erroneous) structure 2 is so reasonable an explanation (based on textbook/common chemical knowledge at that time) that it can be classified as a causal, rather than acausal, MIE. The choice between causal and acausal MIE in this case deserves some further discussion.

In this paper, we will explain the reasoning behind the various errors and the reasoning of the critics who eventually set the record straight, as best as the literature record allows. For convenience in reading this paper, such analyses will be provided in *italics* font and indented.

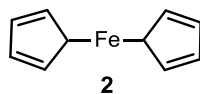
The “linear” structure 2 is the most direct “paper chemical” molecule that contains 10 carbons, 10 hydrogens, and one iron atom made from two cyclopentadienyl-containing



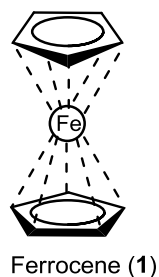
Cyclopentadiene

moieties. (By “paper chemical” is meant the written record of the brainstorming ideas that chemists write, often without serious prioritization of validity. The emphasis is on “brainstorming,” not validity, not even reasonableness. It can also be thought of as gedankenexperiment, a “thought experiment.” The opposite of paper chemistry is experimentation and robust physical and chemical analysis.)

In the cases of Pauson and Kealy & Miller et al., the thought processes were likely as follows: Knowing the empirical formula $C_{10}H_{10}Fe$ from elemental analysis (an experimental technique), how do we put-together a molecule that is two parts “A” and one part “B”, that is, two parts “cyclopentadienyl” and one part iron (Fe) having the formula $C_{10}H_{10}Fe$? The simplest answer is, A-B-A. See structure 2. The problem with



Multiple Independent Errors:
Miller, Teboth and Tremaine
(submitted July 11, 1952)
Kealy and Pauson
(submitted August 7, 1951)



Multiple Independent Discoveries:
Doering (in private discussions with Pauson, September 1951)
Wilkinson (submitted March 24, 1952)
Woodward (submitted March 24, 1952)
Fischer and Pfab (submitted June 20, 1952)
Dunitz and Orgel (submitted July 2, 1952)
Pepinsky (submitted August 12, 1952)

Fig. 3 The originally proposed and incorrect structure **2** of the $C_{10}H_{10}Fe$ compound synthesized for the first time by Miller et al. and Kealy and Pauson. On the right, the correct structure of the $C_{10}H_{10}Fe$ compound **1**, now known as “ferrocene,” as proposed by no less than six independent research groups and scholars. In both structures (the fallacious **2** and the actual compound **1**), each corner of the “pentagons” (except the carbon atoms bonded to the iron (Fe) atom in **2**) represents a carbon atom and a single hydrogen atom

structure 2 is that the isolated compound was found to be stable in the presence of acids and bases, “in striking contrast to the failure of previous investigators to prepare stable organo-iron compounds,” as stated by the first chemists who proposed the alternative and correct structure 1 (Wilkinson et al. 1952) and by the certain instability of compound 2, had it been made, in the presence of aqueous acids and bases.

What is interesting and even confounding is that the correct structure 1 was asserted immediately after the incorrect structural proposals were published. That is, at the time it was proposed, there was no known similar chemical structure to 1. Yet almost immediately, a number of chemists around the world read the Kealy and Pauson and Miller et al. papers, recognized the error, and proposed the correct structure of ferrocene (1) (Fig. 3). These chemists were William von E. Doering (at Columbia University) (Pauson 1955), Geoffrey Wilkinson and R. B. Woodward and colleagues (at Harvard) (Wilkinson et al. 1952), E. O. Fischer and W. Pfab (in Munich) (Fischer and Pfab 1952), and Philip Eiland and Ray Pepinsky (at Penn State) (Eiland and Pepinsky 1952). Dunitz and Orgel (at Oxford) almost immediately thereafter provided X-ray crystallographic and theoretical support for the novel structure.

Why, then, did so many other chemists arrive at such a unique structure so quickly? I posed this very question to Jack Dunitz recently. Dunitz, who published on this structure with Orgel in 1952, responded, “Imagination, Sir, Imagination” (Dunitz 2017). The fact that so many chemists independently solved the structure problem indicates that there was sufficient chemical knowledge at this time (1951/1952) to recognize that no then-known chemical structure could be written that accounted for the analytical data and chemical properties of the $C_{10}H_{10}Fe$ molecule. A novel structural type was therefore required.

In this time period, chemists were beginning to understand that the simple valence bonding concepts built from the days of G. N. Lewis were insufficient to explain all of chemistry. For example, in the late 1940s, three-atom two-bond struc-

tures were proposed by Saul Winstein to explain the extremely high solvolysis rate of 2-exo-norbornyl tosylate (i.e., the nonclassical carbocation) and by H. C. Longuet-Higgins used a similar structural type of explain the structure of diborane. In each of those cases, as well as for ferrocene, novel experimental observations and conceptual jumps led to an entirely new fields of chemistry.

The discovery of the unique sandwich structure of ferrocene immediately ushered in an entirely novel area of organic chemistry, which led to the 1973 Nobel Prize in Chemistry for Wilkinson and Fischer. Why the Nobels went just to these two chemists and not any of the others is a story worth hearing and previously, though only partially, told (Zydowsky 2000; Laszlo and Hoffmann 2000; Seeman and Cantrill 2016).

Thus we have an example of multiple simultaneous acausal-independent errors being corrected by multiple simultaneous causal-independent discoveries.

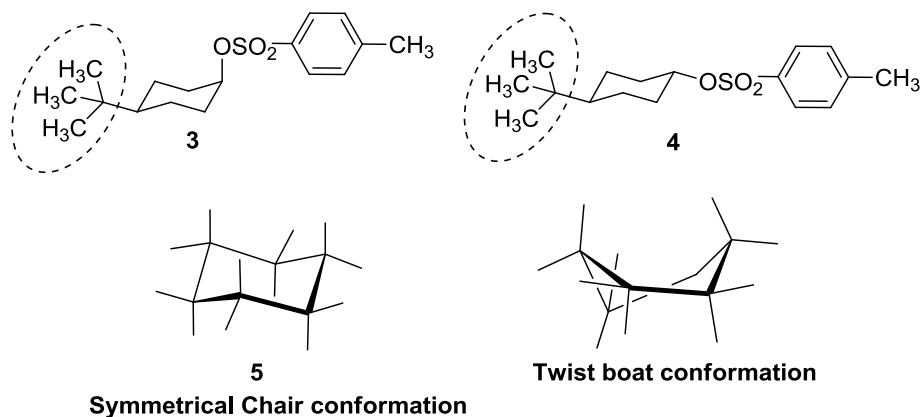
Roald Hoffmann, the noted theoretical chemist, poet, playwright (*including the play "Oxygen" written with Carl Djerassi*), and recipient of the 1981 Nobel Prize in Chemistry, reviewed this manuscript and wrote,

Let me take a contrarian view of the ferrocene story. What's important is that Kealey and Pauson and Miller, Tebboth and Tremane MADE the molecule, not that they assigned the erroneous structure. How long would the world have waited for ferrocene if they had not made it? Three-to-eight years, I am guessing. I view this is a case of experimental MID, accompanied by a theoretical/conceptual MIE. Other examples are Scheele and Priestley on oxygen. They believed they had made something, but interpreted it as dephlogisticated air (Priestley). Only Lavoisier saw it right. It was not yet time to think of it as monoatomic, diatomic etc.

I would suggest further that the theoretical MIE that Kealey and Pauson and Miller et al. made was an inducement for what you call the MID in this story. If these authors had not made the molecule and interpreted its structure wrongly, others would not have been inclined so quickly to posit the right structure (Hoffmann 2017a).

Another example of multiple simultaneous acausal-independent errors, also from organic chemistry, comes from the ideas of Winstein and Holness (1955) at UCLA and subsequently a member of the National Academy of Sciences, in 1955, and of Eliel and Ro (1956), then at Notre Dame and subsequently also a member of the National Academy of Sciences, in 1956. They both employed the *tert*-butyl group, the encircled atoms in structures **3** and **4** to keep the six-membered cyclohexane rings locked into the geometry depicted in structure **5**. What neither realized is that the *tert*-butyl group would alter the three-dimensional shape of the cyclohexane ring, invalidating their method.

What neither Eliel nor Winstein anticipated, nor could have anticipated, was that the tert-butyl group would distort, or modify, the symmetrical chair-like geometry shown in 5, such that the so-called model compounds had different geometries from the test substances and therefore were not viable controls. Distortions such as the twist boat conformation are possible. Thus, the so-called "chemical reaction method of conformational analysis" was and is not valid. This is another example of a multiple acausal-independent simultaneous errors. The reason for the errors was that neither Eliel nor Winstein anticipated that a tert-butyl group would distort the geometry of the ring, a fact established by a number of researchers subsequent to the papers by Eliel and Winstein, one of which was by Eliel himself (Seeman 1983).



In the first half of the nineteenth century, chemists used what is called today the “conventional relative atomic weights” of the elements, i.e., H = 1, C = 6, and O = 8. In 1818 Jöns Jacob Berzelius proposed the alternative relative atomic weights of H = 1, C = 12 and O = 16. The uncertainty was caused by the fact that certain elements, notably hydrogen, nitrogen and oxygen, are found in nature as diatomic molecules and not individual atoms (that is, as H₂, N₂ and O₂ rather than H and N and O; and that water is not OH but HOH or more commonly H₂O). As a consequence of the uncertainty over atomic weights, the field of chemistry suffered greatly by its inability to establish with general acceptance the empirical formula (i.e., the composition or relative number of each atom) of many organic compounds. This is an extreme example of MIEs resulting from accepting a single fallacious idea, in this instance, a foundational idea.

At least one example of MIEs had fatal consequences. *Strychnos nux vomica* L. is a deciduous tree native to India and Southeast Asia and is the source of strychnine, obtained from the seeds from the tree’s fruit. It was known from at least the Middle Ages (Buckingham 2007) that the powdered nuts were dangerous and even poisonous. These warnings were ignored by the French physician Pierre-Éloi Fouquier. In the first half of the nineteenth century, Fouquier and then others promulgated the myth that these powdered nuts and ultimately their major constituent, the compound strychnine, had beneficial medicinal properties—which of course, was *deadly false*—and led to decades of harm and death to untold numbers of human patients. This is an example of an extraordinarily sad MIE. Only following a super-abundance of clinical data was the use of strychnine as a tonic and central nervous stimulant stopped.

Erroneous judgments of the value of another’s research results or conclusions

I have the impression that revolutionary discoveries in science have always met a combination of skepticism and enthusiasm (Campanario and Acedo 2007). – Juan Miguel Campanario

Consider instances where several journal peer reviewers independently recommend rejection of a manuscript; that manuscript is rejected for publication but it is ultimately published in another journal; and the resulting publication is subsequently established to be of great significance. In his book *Of Flies, Mice and Men*, Nobel Laureate Jacob (1998)

cites a number of concepts that were initially rejected for publication and ultimately led to Nobel Prizes for their discoverers. For example, Richard Ernst's paper on nuclear magnetic resonance was rejected twice by the *Journal of Chemical Physics* before it was published in the *Review of Scientific Instrument*; Ernst received the 1991 Nobel Prize in Chemistry for that work. The September 29, 1955 rejection letter to Solomon A. Berson (and Rosalind Yalow) from the editor of *The Journal of Clinical Investigation* said about their new technique radioimmunoassay (RIA),

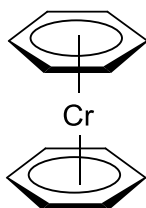
The experts in the field have been particularly emphatic in rejecting your positive statement that ... They believe that you have not demonstrated ... (Jacob 1998).

Yalow received the 1977 Nobel Prize in Physiology or Medicine for RIA.

When, according to his 2002 Nobel Prize citation, John Fenn first published his work on the “development of soft desorption ionization methods for mass spectrometric analyses of biological molecules” in the mid-1970s, Fenn said,

People [said] ‘It’s impossible for this man to get these results from this equipment ... everybody said it was hopeless ... (Fenn and El-Shall 2009).

One of the clearest stories on erroneous reviews blocking publication is that of Harold Zeiss's work on bis(benzene)chromium (**6**), a “sandwich compound” just like ferrocene (**1**) (Seyferth 2002a, b; Werner 2009; Zeiss and Tsutsui 1957). The reviewers just did not believe the proposed structure.



6

Bis(benzene)chromium

Resistance to scientific discovery (Campanario 1995; Campanario and Acedo 2007) and “rejecting and resisting Nobel class discoveries” (Campanario 2009) are numerous and have been well studied.

As exemplified in the above text, the declaration of a new concept can be met with disbelief and resistance, especially when the new concept is particularly novel, and most especially from those who hold current theories as gospel, even when all are following “normal science” as identified by Kuhn (1972). There may well be a demand by journal reviewers and editors for additional supporting data or replication of the results in independent laboratories. In some instances, the results will lay fallow for many years until the time is ripe for their appearance and use. Scientists who are sufficiently influential to retard the publication of novel ideas, not based on scientific data or robust analysis but rather based on emotional or non-scientific factors, are participants in multiple simultaneous independent errors. This also extends to the withholding of major awards such as the Nobel Prize to deserving scientists (Friedman 2001; Seeman 2017b).

The specific reasons for rejecting novel ideas are as varied as the ideas themselves. In the case of John Fenn's electrospray ionization, critics did not believe that ions of macromolecules could be produced because it was assumed that such molecules, when ionized, would break apart into smaller fragments. Macromolecules are very large molecules, such as proteins, having thousands or more atoms. The application of electrospray ionization to biomolecules rapidly took off, an example where the utility was of such great importance that disbelievers were quickly silenced by results reported around the world.

The reasons for the delay in the publication of Harold Zeiss' determination of the structure of bis(benzene)chromium (6) are identical to those experienced by John Fenn, as described above. To quote Zeiss in his eventually published 1957 breakthrough paper in the Journal of the American Chemical Society, “The subject matter of this paper was presented in 1954 [at an American Chemical Society meeting] and also submitted to This Journal as a preliminary Communication. The paper, however, was rejected by the referees chiefly on the grounds of insufficient evidence for our (with Professor L. Onsager) proposal of the π -complexed biconoidal structure” (Zeiss and Tsutsui 1957).

As Merton pointed out (Merton 1969, 1973), priority and recognition are two primary motivational drivers for scientists. Zeiss exhibited this when he pointed out that E. O. Fischer's 1955 paper appeared before his paper. Zeiss was prevented by the editor and reviewers from his paper appearing in the scientific literature first. Zeiss carefully and elegantly alerted the scientific community and the history of chemistry to his priority claim in his eventually published paper, “This chronology is given here in an attempt to reduce the confusion which has arisen and is not to be construed as a detraction from the elegant method of preparation and structural proofs devised by Dr. Fischer and his colleagues” (Zeiss and Tsutsui 1957).

Consider the following situation. First, the editor(s) and reviewer(s) of a journal delay or reject a submission of an extremely novel finding because of several independent reviews of the claims of the author(s) (a MIE). Second, subsequently but reasonably similarly in time (a MID), other scientists submit their manuscripts to different journals and their papers are accepted and published immediately. The first authors are now deprived of their claim of priority. And the greater the importance of the finding, the more likely that there will be disbelief and rejection by some reviewers, thereby increasing the likelihood of priority misunderstandings.

There is another temporal phenomenon that illustrates priority issues. These are the so-called “sleeping beauties,” which are publications whose importance is not recognized until often many years after publication (Ke et al. 2015). Sleeping beauties occur when an entire community fails to recognize the value of a discovery, thus being examples of multiple causally-dependent errors.

The failure of biologists to understand and incorporate Mendel's published ideas for several decades after their publication is an example of MIEs. That Hugo de Vries, Carl Correns, William Bateson, and Erich von Tschermak among others “independently and ‘within a few weeks of each other,’ discovered the discovery of Mendel” (Ogburn and Thomas 1922) is an example of MIEs followed by a MID.

In chemistry, one of the best examples of a sleeping beauty is the work of Erich Hückel in the 1930s on aromaticity and molecular orbital theory for unsaturated organic compounds (Hückel 1931, 1935, 1937a, b, 1938). Hückel developed what is today considered a classic theory for the determination of the properties of conjugated ‘aromatic’ systems

like benzene. However, the Hückel molecular orbital method (HMO) was not used until the early 1950s, 20 years later. HMO theory has made many contributions to the understanding of organic chemistry.

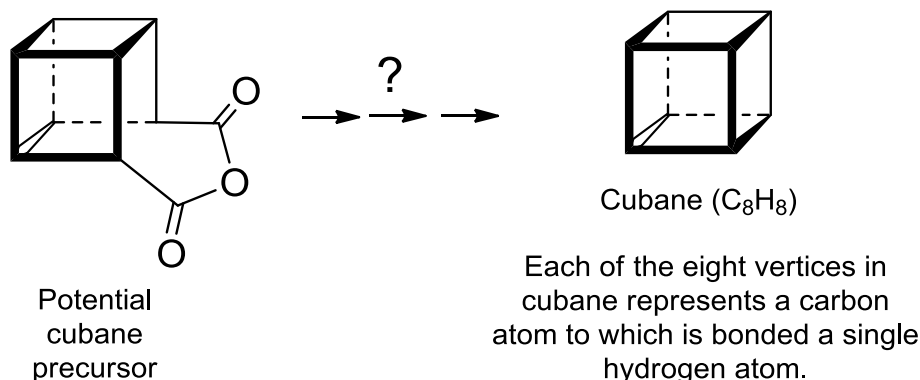
Why do entire communities ignore important scientific results thereby creating sleeping beauties? Ke et al. lists the top 15 sleeping beauties covering a wide range of physical sciences that have an average 69.6 ± 18.2 years in “awakening years” (Ke et al. 2015). In Ke et al.’s study, the top three disciplines producing sleeping beauties are “physics, multidisciplinary,” “chemistry, multidisciplinary” and “multi-disciplinary sciences.” These authors point out that “journals in the multidisciplinary sciences subject category are more likely to attract publications that become field-defining even decades after their appearance” (Ke et al. 2015).

In the case of the Hückel molecular orbital theory, the prime uses would ultimately be organic chemists who are notoriously shy of complex mathematics. The Hückel papers, however, were highly mathematical in their content and written mostly in German in German physics and physical chemical journals not typically read by organic chemists. In the period 1930–1960, German organic chemists were focused primarily on synthesis and less on mechanistic organic chemistry, for which the HMO theory would be useful. Thus, the German organic chemists did not even read these papers, nor did organic chemists from other countries (Weininger 2018). However, in the late 1940s, the field of aromaticity and nonbenzenoid aromatic compounds began to flourish (Ginsburg 1959), and the Hückel theory came to the attention of organic chemists, who immediately jumped onto it in the early 1950s.

False anticipation of being able to achieve a certain experimental result

We define “error of experimental result anticipation” as when the experimenter anticipates a particular result based on previous experimental or theoretical considerations or well-grounded, or not so well-grounded intuition, and that anticipation fails to materialize.

One example of a multiple simultaneous independent error of anticipation will be presented. In the 1960s, chemists became very interested in what is called “small ring chemistry.” One very special small ring compound of theoretical and dramatic interest was and continues to be the eight-carbon atom Platonic hydrocarbon cubane (see structure below). The structure of cubane is well anticipated by its very name! Chemists will appreciate what non-chemists may not (Woodward 1963): prior to its preparation, the possibility of its very existence was uncertain because of extremely challenging energetics—and that uncertainty was only resolved when it was made and isolated by Eaton and Cole (1964a, b) at the University of Chicago in 1964.



In a letter dated May 29, 1966, future Nobel Laureate Jean-Marie Lehn (Strasbourg) wrote his friend and also future chemistry Nobel Laureate Roald Hoffmann (Cornell) about his (Lehn's) attempt to synthesize cubane. As shown in Fig. 4, in mid-1966 Lehn and his students were one step away from cubane. Lehn's letter says in part, "We are one step from cubane, starting with its 'valence isomer' Cyclooctatetraene. Unfortunately, [Sartoru] Masamune [at MIT] seems also be on this problem!" (Lehn 1966).

At the same time, unbeknownst to Lehn and Hoffmann, in the late 1960s, William G. Dauben and his student Charles Schallhorn at the University of California, Berkeley, were attempting to synthesize cubane using the same precursor. Neither Lehn nor Dauben and their respective students were able to achieve the very last step. However, Satoro Masamune and his students at the University of Alberta in late December 1966 reported the formal total synthesis of cubane following another set of conditions from the same cubane precursor (Chin et al. 1966). Thus, the Dauben group and the Lehn group were part of a multiple simultaneous independent error of anticipation. That is, they anticipated that a set of reaction conditions would lead to the same unrealized-for-them result, the synthesis of cubane. It was left for others to achieve that result.

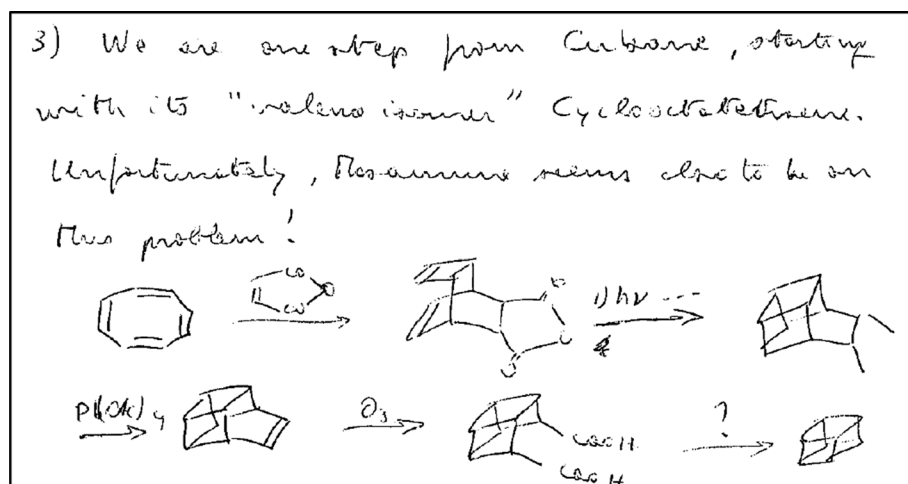


Fig. 4 An excerpt from Jean-Marie Lehn's May 29, 1966 letter to Roald Hoffmann (Lehn 1966)

In synthetic chemistry, paper analogies, i.e., Gedanken experiments or thought experiments, do not necessarily work in the chemical laboratory. That is, there are many known reaction types. For example, the conversion of an ester to an acid, or an acid to an ester, are known reactions that have been found to occur in thousands of cases. However, that does not mean that every attempt to convert an ester to an acid or an acid to an ester will be successful. For various reactions, even highly anticipated reactions do not always occur in the expected fashion. The uncooperative compound may possess structural features that cause unexpected or even somewhat anticipated deviances from the norm. In such instances, synthetic chemists look for work-arounds, that is, other approaches that will circumvent the previously unanticipated roadblocks. Sometimes this becomes a matter of trial and error. In certain cases, several members of a research group may spend an entire year or more attempting an unobliging reaction to occur, trying many approaches before success. Sometimes the project needs a “new hand,” that is, a different chemist.

Perhaps the greatest example of a MSE in the category of false anticipation of being able to achieve a certain experimental result were the alchemists who tried to transmute base metals such as lead to the precious metal gold (though ultimately nuclear chemistry in the twentieth century demonstrated that in special cases, one element can be converted to another, including the transmutation of bismuth to gold (Matson 2014) though not in the quantities of interest to jewelers).

Another example of false anticipation has been the search for a chemical element lighter than hydrogen, even though such an element is impossible to conform to today's understanding of atomic structure. One of these chemists was the great Dmitri Mendéleev, the inventor of the periodic table (Bensaude-Vincent 1982). Another was the American chemist William Draper Harkins (Fontani et al. 2015). While it would be easy to call such an idea folly, Harkins was a renowned researcher. He predicted the existence of the neutron (also predicted independently and simultaneously by Ernest Rutherford, an example of a MID) 12 years before it was experimentally discovered by James Chadwick (in Rutherford's laboratory in Cambridge). Together with Martin Kamen, Harkins built a cyclotron which was used in the Manhattan Project at Chicago during World War II. Harkins was the first to propose the principle of nuclear fusion. Further, Harkins and two others (W. G. Hardy and I. Langmuir) independently proposed the theory of molecular orientation in surfaces (Mulliken 1975), another example of a MID. In the mid-1920s, Andreas von Antropoff in Germany proposed a new form of matter comprised solely of neutrons. And the eminent physicist de Broglie attempted to insert another subatomic particle into the periodic table. According to a recent book on *The Lost Elements* (Fontani et al. 2015),

this idea [of an atom lighter than hydrogen] has never had any reliable experimental confirmation, has not fallen into oblivion and even today has some supporters (Fontani et al. 2015).

The latter statement (i.e., “even today”) was unfortunately made without any literature references. Nonetheless, it may be that Harkins, von Antropoff, de Broglie and others may ultimately be judged as promoting a MIE.

Ignoring or omitting relevant precedents: errors of omission

Errors of omission are another class of MIEs. In their article *Crocker, Not Armit and Robinson, Begat the Six Aromatic Electrons*, Alexandru Balaban et al. pointed out that “text-book writers as well as the general chemical public” (Balaban et al. 2005) failed to credit Ernest C. Crocker, who is “almost completely unrecognized” for being the first to suggest the six-electron aromaticity concept for benzene and related compounds. Thus, the continued omission of the same critical, specific facts by authors of different textbooks is an example of MSEs. This is related to Robert K. Merton’s “obliteration by incorporation” (Merton 1988, 1995). These are instances when certain ideas become so universal that their originators are no longer cited. Such behavior can lead to false attributions of priority and emotional distress by the ignored scientists and errors in the history of science.

Another class of errors of omission includes the behavior pattern of some scientists when they transform themselves into advocates in political, social, public health or economic matters. There is a tendency by some scientist-advocates to omit citations that disagree with their position. Two notable examples of this are multiple academic and medical advocates for (or against) sugar (Schillinger et al. 2016) and for (or against) tobacco smoking (Boyd and Bero 2007; Odierna et al. 2013; Glantz and Bero 1994; Misakian and Bero 1998; Flanigan 2016). Clearly, the exclusion of legitimate citations because they disagree with one’s position is inappropriate, at best, and falsification of the literature, i.e., misconduct of science, at worst. In all such instances, there are multiple errors.

Recently, the U.S. National Academy of Sciences, Engineering and Medicine (NAS) received considerable negative media attention for potential conflict of interest because several members of a panel they chose to advise on biotechnology matters had strong ties to the biotechnology industry. Instances of this type of behavior may fall dangerously close to collusion rather than causally-independent multiples. Failure to reveal potential conflicts of interest by several panel members is a multiple error of omission (Strom 2016). But excluding a scientist entirely on the basis of his or her current or previous employment may well be an example of McCarthyism in science (Rothman 1993; Seeman 2017a).

Unknowing of a not-yet-determined law of nature

Prime sources of MIEs are judgments made on the basis of incomplete knowledge, especially if that knowledge has yet to be unearthed or as yet unpublished knowledge. Incomplete knowledge can also be a result of poor literature searches or reckless behavior regarding the literature. For example, scientists made many errors because they lived decades before Albert Einstein proposed the theories of general relativity and special relativity. All attempts to explain the source of the energy from the sun were hopeless before Einstein’s equation $E = mc^2$, Hans Bethe’s delineation of nuclear reactions in the sun, indeed before the chemical composition of the sun.

In 1915 Albert Einstein’s theory of general relativity exactly predicted the wobble in Mercury’s orbit, which had been observed for decades previously. In the nineteenth century, during the search for planets and an understanding of our solar system, the French mathematician Urbain Le Verrier proposed that Mercury’s wobble was caused by a hypothetical planet whose orbit was between the orbit of Mercury and the sun. Le Verrier even named this hypothetical planet Vulcan. Mercury’s wobble and Le Verrier’s pronouncement set off a flurry of professional and amateur astronomers to find Vulcan, and a very

large number of announcements—false alarms, so to speak—were made, claiming to have observed this new planet. Thirty-eight years after Le Verrier’s death in 1877 was the actual explanation for this wobble obtained. There was and is no planet Vulkan. But there is general relativity. This story is told in detail in Thomas Levenson’s book (Levenson 2015) *The Hunt for Vulcan*.

Consequences of MIEs

Errors in the scientific literature can have serious scientific and societal impact, especially those involving misconduct of science (Calabrese 2017). The entire scientific community is currently grappling with how to minimize “the afterlife of bad science” (Teixeira da Silva and Dobránszki 2017). But these types of errors are in form and in consequence different from errors due to the temporal instability of knowledge.

MIDs can rightfully provide support for a particular hypothesis. In contrast, MIEs can confusingly give succor to a false hypothesis or to an idea that eventually may be established as inconsistent with the experimental data. The process of legitimizing or delegitimizing scientific hypotheses is enormously important. The more independent replications there are (in terms of who performs the experiments and in multiple laboratories) and the more confirmation-related experiments testing the hypothesis from alternative directions, the more secure (or the closest to the truth) is the hypothesis. Of course, the caveat remains: repeating an error in logic or an error in an experiment or accepting scientific misconduct as valid can lead to waste in time and resources.

MIDs have the potential to advance or even transform a discipline or initiate a new one. That change may be delayed until the time is ripe, e.g., sleeping beauties. MIEs can also change the field, by stimulating more work, especially by those who anticipate that there is an error to be overcome (and scientific fortunes to be made). See the ferrocene story, told above.

Greater awareness of discoveries save money since researchers can be more effective in their endeavors, either by shifting to other projects, enhancing their research, or more rapidly jumping to the new state-of-the-art. But a greater awareness of errors that are not yet debunked (but are still believed to be discoveries) can cost resources. Other researches may begin relying on such misrepresentations or misunderstandings and building upon them.

Commonalities between MIDs and MIEs

Mendelssohn described MIDs as due to

an organic growth which inevitably will produce the discoveries required by each development stage (Mendelssohn 1966).

Merton wrote that MIDs are

virtually inevitable as certain kinds of knowledge accumulated in the cultural heritage and as social developments directed the attention of investigators to particular problems (Merton 1973).

I claim that MIEs have the same temporal underpinnings as MIDs. In addition, I posit that many attributes of MIDs and MIEs are identical. Fifteen will be identified herein.

1. First, we speak to the existence and frequency of MIEs. To the extent that there are many MIDs, there are surely as many, if not more, MIEs. Indeed, there is every reason to anticipate that Multiple Independent Errors are as common as if not more common than Multiple Independent Discoveries. As stated above, there are more ways to be inconsistent with an eventually established theory than to be consistent with that theory. MIEs are also far less preserved in the permanent record and thus largely unrecognized as such, but that doesn't erase their occurrence—just their documentation. And to the extent that Selective Pessimistic Induction is valid (see [“Errors in science and their sociological dimension”](#) below), MIEs will be numerous.
2. During the initial phases or steps of a research process, one cannot know whether a project will be fruitful or not, or be “in error” or a discovery. The researchers themselves may not realize the tenuousness of their work. Peer review and editorial review may, or may not limit publication. This is true for both MIDs and MIEs.
3. Both MIDs and MIEs can fall within the same range of “independence” and “near-coincident in time” as discussed in the above section. Both MIDs and MIEs originate within science.
4. MIEs that are based on faulty analyses or faulty explanations or conclusions can share valid experimental methods with MIDs.
5. Both MIDs and MIEs can engender support from a large numbers of scientists, or disdain from a large number of scientists. Given the rush for priority among scientists, premature MIDs and too-frequent MIEs may occur.
6. The probability of both MIDs and MIEs can be enhanced by a lack of knowledge and familiarity with the state of the art. Polanyi (1963) pointed out that very novel ideas are often opposed by those holding strongly to the state of the art. Sometimes those novel ideas lead to MIDs; other times to MIEs.
7. Lamb and Easton (1984) characterized a number of MIDs as “unpredictable” or “accidental” or “missed multiples” (because the protagonists, at first acting independently, decided to collaborate) or “resisted multiples” (those multiples which were not published). Each of these types could easily be either MIDs or MIEs. These authors also proposed that MIDs “provide a criterion of plausibility, an important indication that one is heading in the right direction, that is where science appears to be going” (Lamb and Easton 1984). But MIEs also may well suggest plausibility until and unless they are shown to be false. As discussed above, replicating an experiment with errors in logic can lead to false positives and MIEs.
8. As will any new and controversial discovery, scientists will line up in favor of and against any new and noteworthy idea. One side or both may be involved in MIEs while only one side can claim a part of a MID although another side may dispute the discovery, thus being part of a MIE.
9. For both discoveries and errors, the dates of inception are not always unambiguously known or even knowable.
10. MIDs, as for all scientific discoveries, depend on the state of the art as well as the readiness to accept new ideas and developments. If anything, MIEs depend even more on the readiness to accept new—and in this instance, erroneous—ideas and developments. Perhaps also MIEs will more likely flourish in a rapidly-developing field where new ideas are popping up regularly, unpredictably and with the enthusiasm that normally occurs in a rapidly evolving field.
11. It is likely that serendipity plays a role for both MIDs and MIEs.
12. Scientists are certainly concerned that their discoveries will be published by others prior to their own publication, thereby taking away from them some degree of priority

and prestige. Because errors are not characterized as errors until they are discovered to be such—prior to that, errors are characterized as discoveries—there may well be as much of a rush to publish errors as to publish discoveries. And there may well be enhanced reputations for both MIDs and MIEs (prior to the errors being demonstrated to be such).

13. There surely are countless instances of both MIDs and MIEs unrecorded in the open literature, buried in the records of researchers, in the laboratory notebooks and internal reports of student in academia and scientists in all professions and at all levels of performance.
14. An unusual example of a commonality of MIDs and MIEs is their occurrence in one and the same event. Thus, the MIEs in the proposals of the structure of ferrocene discussed above were immediately followed by the MIDs of the correct structure of ferrocene. Indeed, it is likely that other MIEs were and in the future will be rapidly followed by corrective MIDs (which may well become MIEs, c.f., Selective Pessimistic Induction in “[Errors in science and their sociological dimension](#)”).

Dissimilarities between MIDs and MIEs

In contrast to the many similarities between MIDs and MIEs described in the section immediately above, there are some, but far fewer, dissimilarities. Five dissimilarities follow.

1. As mentioned above, discoveries are made public when the discoverers deem the time is ripe to do so. If discovered in time, errors may well be left unpublished.
2. Scientists understand that their discoveries may well have been observed before them and even published, though unknown, to them. Scientists, however, are far less likely to know of or anticipate that errors they have made were made by other scientists before them or will be made by other scientists after them.
3. To the extent that scientific discoveries are inevitable (Ogburn and Thomas 1922; Soler 2008), and Merton (1961) believes otherwise, this author doubts that *all* MIEs are inevitable.
4. Merton reported unpublished results with Elinor Barber on the intensive examination of 264 MIDs (Merton 1961). Merton noted that these cases extend back to the seventeenth century, and that there was a continuous decline with regard to *contested claims of priority*: 92% in the seventeenth century; 72% in the eighteenth century; 74% in the first half of the nineteenth century; 59% in the second half of the nineteenth century; and 33% in the first half of the twentieth century. Make no mistake: credit remains a very important component of modern science (Seeman and House 2010a, b, 2015; House and Seeman 2010). In contrast, it is unlikely that any scientist will make a claim of priority of an error shared with other researchers or will continue to pursue a priority claim once the error has been revealed.
5. Similarly, Merton teaches that the publication of a discovery forestalls the discovery by other scientists due to the blockage of multiple publications by journal policy or the scientist’s voluntary retreat from such projects that would have been pursued had earlier disclosure had not been made. This is certainly not true for organic synthesis. Over the years, there have been numerous total syntheses of important natural products and pharmaceutically important drugs, such as taxol (Anonymous 2018) or strychnine

(Cannon and Overman 2012; Shibasaki and Ohshima 2007). Of course, these are not identical synthetic routes but alternatives with either improved efficiencies or with novel methodologies. This is not the case with MIEs, as journals discourage publication of negative findings, which include errors.

The analysis of errors

Progress in science is typically measured in terms of new discoveries, applications of research achievements to the good of humankind, and on a personal level, peer and public recognition, receipt of tenure, increased amount of laboratory space and facilities, more students, and enhanced research funding. In contrast, scientists are neither eager to display nor to bring further attention to their own errors.

What about retrospective analyses of the research endeavor? How often do research directors, with or without the participation of their research group, engage in retrospective analysis of his/her research group's productivity? I suspect hardly ever; indeed, neither I nor the several colleagues I have spoken to have participated in a retrospective analysis of a past year's research program. Funding agencies rarely, if ever, review the past proposals to determine if their funding objectives were met or if the funding agency's program objectives were met. Do academic institutions periodically review their departmental hiring practices alongside their departmental achievements relative to university goals? If retrospective analyses are performed at all, it is likely done by commercial publishers or institutions under stress, for example, chemical societies concerned about loss in membership.

Historians, sociologists and philosophers of science may be the predominate students of error. From a philosophical perspective, some scholars consider that all theories are tentative and will be shown to be incomplete or in error over time. This author has studied the inability of organic chemists in the late 1950s and early 1960s to solve the so-called “no-mechanism” problem, that is, the mechanism(s) of pericyclic reactions in organic chemistry until the publications of Woodward and Hoffmann (1969) in the mid- to late-1960s. This led to the 1981 Nobel Prize in Chemistry for Hoffmann (Woodward, who died in 1979, received the 1965 Nobel Prize in Chemistry for other research). The reasons that an entire community of organic chemists were left in the dark, publishing erroneous suggestions (or empty explanations) for years, until Woodward and Hoffmann published their theory, deal mostly with traits such as narrow intellectual focus, failure to see or be interested in the big picture, or misjudgment as to the importance of the problem. This type of error analysis provides a unique understanding of science practice. Further studies in correction and progress in science are eagerly awaited.

Learning from the instability of knowledge

Are there some new things we learn to ask of “wrong” explanations that we would not ask of “real” discoveries? Is there anything to be learned from an analysis of “wrong” explanations, incorrect views of observation?

Progress in science is typically measured in terms of new discoveries, progress in ongoing research endeavors, and improvement in understanding. And perhaps progress is also measured in terms of translational research, namely the conversion of research into tangible, useful human and environmental outcomes. As indicated above, the intentional

publication of errors or non-reproducible research is highly discouraged by the scientific community. Furthermore, scientists are not eager to display or bring further attention to their errors.

Kuhn and also Feyerabend took the position that no theory is ever consistent with all the relevant facts (Feyerabend 1975) (see the discussion of Pessimistic Induction below). To the extent that scientists adopt theories that are faulty, or use theories in ways that are incompatible with the relevant data, these could be instances of MSEs.

Learning from errors is much more than simply not ever making the same error again, or helping others not make that error. I refer to retrospective analyses of the research endeavor from a funding perspective. How often do research directors, with or without the participation of their research group, engage in retrospective analysis of his/her research group's productivity? I suspect hardly ever; indeed, I have asked several colleagues, they have reported never engaging in themselves in retrospective analysis of their errors (or failed research projects) nor knowing such behavior by their colleagues. Funding agencies rarely, if ever, review the past proposals to determine if their funding objectives are being met let alone being optimized even though they demand annual progress reports. Do academic institutions periodically review their departmental hiring practices alongside their departmental achievements relative to university goals? If retrospective analyses are performed at all, it is likely done by commercial publishers or institutions under stress, for example, chemical societies concerned about loss in membership.

Multiples and the strong program

The “sociology of error” became important in social studies of science after its use by David Bloor in his 1976 book *Knowledge and Social Imagery* (Bloor 1991), a key text in the “strong program” in the sociology of knowledge. We very briefly examine the four indispensable components of the strong program following Bloor's conceptual scheme as they relate to *both* discoveries, according to Bloor, and errors, new in this work.

- *Causality* Both asserted discoveries and errors should be interrogated regarding their psychological, social and cultural underpinnings.
- *Impartiality* All knowledge claims, be they ultimately in the positive (discoveries) or in the negative (errors), should be fully and completely examined without bias.
- *Symmetry* The same type of experimental and theoretical examinations should be applied to successful (discoveries) or unsuccessful (errors) knowledge claims.
- *Reflexivity* Both discoveries and errors are the norms of science, and hence, their examination and consequences are applicable to the sociology (and philosophy) of science.

The transformation of MIDs into MSEs via Selective Pessimistic Induction

In 1905, Poincaré (1952) and in 1981, Laudan (1981) proposed the theory of Pessimistic Induction (PI) over scientific theories. PI states that “according to our present vantage point, most past scientific theories are false; but there's nothing special about our present vantage point, and so, from any particular vantage point p , most scientific theories that were extant prior to p will be regarded as false by those at p . Generalizing from these

cases, we should conclude that our current theories are false as well.” To the extent that Pessimistic Induction is valid, then the Theory of Multiple Simultaneous Discoveries leads directly to the Theory of Multiple Simultaneous Errors.

While there has been some recent support for Pessimistic Induction (Wray 2013, 2015; Stanford 2001, 2006), there has been an abundance of criticism of the theory from many perspectives (Mizrahi 2013; Fahrback 2011; Park 2016; Doppelt 2007a, b; Roush 2010, 2015; Park 2011, 2014; Lewis 2001). But in the eagerness to discredit Pessimistic Induction and preserve Scientific Realism, the possibility of a partial validity of Pessimistic Induction has been apparently unappreciated. We propose the theory of Selective Pessimistic Induction (sPI) following the model of Selective- or Semi-Realism described in detail by Chakravartty (2017a, b) and others (Cordero 2015, 2017; Psillos 2017; Badino 2017) and relying on the careful division of cases by specific factors (Roush 2010; Badino 2017).

In sPI, it is recognized that some past theories are false, and following the logic in the immediately above paragraph, some of our current theories are false as well. At this time, we make no proposal as to unifying factors which distinguish those theories that were or will be shown to be in error in contrast to those which remain, at any particular point of time, unchallenged. To analyze and flush out the concept of sPI in more detail is beyond the scope of this paper. And in no way does the validity or not of PI or sPI diminish the validity of MSEs.

Perhaps most illuminating and relevant to the topic of this paper on multiple independent simultaneous errors is Wray’s observation, who used the qualitative term “many” twice, that

Given that *many* past successful theories have been discovered to be false, it seems reasonable to assume that *many* of the successful theories we currently accept will also be shown to be false in the future (Wray 2013) [my ital].

In Selective Realism, one is searching for those factors and those theoretical posits that are true or approximately true (Cordero 2015; Badino 2017). In Selective Pessimistic Induction, one is focusing on those theories which have been or will eventually be found to be false or approximately false without worrying that many theories remain essentially untarnished. This approach may be called Selective Pessimism (Chakravartty 2007) though practicing scientists over the centuries have so much experienced the fragility of theories that the term “pessimism” may be overly negative.

We suggest the use of Selective Pessimistic Induction as to include those past, present and future theories that were or will eventually be shown to be false or incomplete in some fashion, i.e., considerably distant from the truth. We propose that Selective Pessimistic Induction can be used and still be completely consistent with the rationality of science in relation to its history (Roush 2015). We believe that there would be no need to attack Selective Pessimistic Induction if one accepts Semi-Realism (Chakravartty 2017c, d; Roush 2015; Cordero 2017).

In summary, it is true that many successful theories of the past have been shown to be false. It is also almost certain that some of today’s successful theories will also be shown to be false or at least modified in one way or another in the future. It is almost certainly true that at least some of tomorrow’s successful theories will be shown to be partly incorrect or not completely correct at a still later time.

Rarely does the philosophy and sociology of science bridge into the practice of science as experienced by scientists. The chemists Polanyi (1969), Berson (2003) and Roald Hoffmann are exceptions (Hoffmann 2012, 2015; Hoffmann et al. 1997). Hoffmann, the noted chemical theoretician and recipient of the 1981 Nobel Prize in Chemistry, has said that

I think error can be adequately defined for experiment – the molecule is made or not made, the melting point is x and not y . As for anything, there may be exceptions – the molecule made may not be the molecule that an author thinks he or she made. Melting points differ for polymorphs – i.e., the molecule may be the same, but its crystal structure different for one form from another. What is an error in theory is more difficult to define, as all theoretical explanations have to be viewed as provisional (Hoffmann 2017b).

Errors in science and their sociological dimension

There is much to be said about the sociological considerations dealing with the conception of errors, their codification within certain communities, and their lifespan and eventualities. Just as Ludwik Fleck discussed the difficulties of “thought communities” and “thought collectives” (Fleck 1979) in communicating scientific ideas amongst each other, imagine the consequences when those very ideas are erroneous—as many will be, as discussed in the sections above. Also, T. S. Kuhn pointed out the need to consider “the epistemic distinction between knowledge and belief” (Kuhn 1979).

Another matter of sociology in science is the reinforcement of error, as in the reinforcement of discovery. In 1935, Fleck wrote,

Once a statement is published it constitutes part of the social forces which form concepts and create habits of thought. Together with all other statements it determines ‘what cannot be through in any other way.’ Even if a particular statement is contested, we grow up with its uncertainty which, circulating in society, reinforces its social effect. (Kuhn 1979)

Fleck’s ideas of the collective holding tightly to its ideas is related to the more recently described cognitive biases such as confirmation bias and desirability bias (Tappin et al. 2017) These can support MSEs as well as MSDs.

Questions

Regarding the nature of MIEs, a number of questions come to mind that are topics for further research and consideration.

- Merton (1973) believed that essentially all discoveries are multiples, including those that on the surface appear to be singletons. Is the same true for errors?
- Do MIEs and well as MIDs lead to scientific advances more quickly?
- Are some disciplines more likely to have MIDs than MIEs, or the converse?
- Merton (1961, 1973) rejected statistical and psychological explanations in favor of sociological underpinnings for MIDs. Is the same true for MIEs?
- Are there some new things we learn to ask of “wrong” explanations that we would not ask of “real” discoveries?
- What is the relationship between the correction of errors and the resolution of controversies?

- Could a MIE (or even a singleton error) be later found to be *not in error*? As knowledge is known to be unstable over time, why might not misinterpretations or other errors in knowledge be unstable in time?

Conclusions

In his 1961 essay on multiple independent simultaneous discoveries, Merton pointed out that, while a number of scholars had concluded that MIDs were

odd or curious or remarkable, the pattern of independent multiple discoveries in science is in principle the dominant pattern, rather than a subsidiary one ... the hypothesis [Merton proposes] states that all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons (Merton 1961).

Because of the importance of multiple independent discoveries in the sociology of science, and because this concept has been known for nearly 200 years (Merton 1961), it is remarkable that its sister concept—multiple independent errors—has not been proposed and analyzed in detail. One can apply to such multiple errors classifying criteria similar to those Merton found useful for understanding multiple discoveries.

Of particular note is the application of the theory of Pessimistic Induction, or perhaps more likely the theory of Selective Pessimistic Induction, to Multiples. Pessimistic Induction proposes that since past successful scientific theories have been found to be false, there is reason to believe that currently successful theories will also be shown to be false. There is no doubt that significant discontinuity is a fact of science, to only a very minor extent exemplified in the various historical narratives discussed above. To the extent that Pessimistic Induction is valid, then Merton's Multiple Independent Discoveries becomes, in time, Multiple Independent Errors. To summarize, to the extent (a) that all theoretical explanations *are* provisional; (b) that new theories will constantly be generated (Optimistic Induction); (c) that some degree of Pessimistic Induction is valid and (d) that Merton's postulate that all discoveries are indeed multiples, then three conclusions logically follow:

Many of the past's MIDs are today's MSEs.

Some of today's MIDs will be tomorrow's MSEs.

Some of tomorrow's MIDs will become MSEs at a still later time.

To readers who find fault with the concept of Selective Pessimistic Induction, I urge the rapid disconnection of that idea from the proposal of MIEs. Such a tie may or may not be useful but in any event, it is not necessary for the promulgation of the concept of MIEs.

Evidence has been presented herein that MIEs are a norm in science, just as MIDs are a norm. Ironically, to the extent that MIEs are the norm, if there is any solace in committing an error, it may be some comfort that others have made or will repeat that error.

Two characteristics particularize both MIDs and MIEs: the extent of simultaneity and the degree of independence. For MIEs, Merton's proposal for MIDs is quite apt: simultaneity may be in calendrical, social or cultural (including intellectual) time. In this paper, we propose that “independence” for both MIDs and MIEs can be causal or acausal. For MIEs, examples for both causal and acausal independence have been provided. In this paper, we have applied multiple errors classifying criteria similar to those Merton found useful for understanding multiple true discoveries. And there are many similarities and some differences.

It is interesting to look at the opposite of what people like and praise, namely, claims of discovery that in fact have subsequently been proven to be inconsistent with currently known facts or subsequent discoveries. To the extent that scientists follow the same rules and patterns of behavior in their practice of science as proposed over 30 years ago by Collins (1985), it is not surprising and should be expected that MIEs would be as prevalent as, if not more prevalent than, MIDs. One might think scientists wouldn't be able to publish so many ideas that later had to be discarded or seriously modified. But since the publication of errors, not known to be such during the peer review stage, is driven by the same forces that drive publication of discoveries, the literature is strewn with multiple independent errors. Errors—except those related to misconduct of science—are not to be ridiculed, for they testify to our humanity and to our desire to understand. Indeed, if discoveries are merely approximations of the truth, then with time and added research, closer approximations of the truth result (i.e., the magnitude of error decreases).

When the degree of causality for either a discovery or an error is high, for example, colleagues discussing a topic simultaneously having a moment of sudden but identical insight or discovery—a Eureka moment—it may well be argued that these are not truly independent. But even then, if the intellectual gap is large, the notwithstanding shared knowledge also being large, this may well be classified as independent.

MIEs can occur whenever there is a difference of opinion involving multiple individuals. These perhaps are most clearly illustrated from any of the many well-publicized controversies in science. Such examples do not trivialize the concept of MIEs any more than the grouping of individuals who are on the right side of a controversy trivializes their being examples of MIDs.

Perhaps the use of the word “error” is too harsh and maybe not even applicable in some of the cases we have discussed and others that fall within the four corners of Multiple Independent Errors. In 2016, Wray characterized such theories as “discarded” rather than “erroneous” (Wray 2016). However, “Multiple Independent Discards” doesn't seem as illustrative nor flow as easily off the tongue as does “Multiple Independent Errors.” No scholarship nor other justification underlies the choice of the term “error.” Furthermore, as pointed out by Geoffrey Blumenthal, in the context of some of the philosophical and sociological concepts briefly mentioned herein, “any proposal that there are definitely-identifiable ‘errors’ tends to be labelled with perjorative adjectives or phrases.” Errors which have no scientific significance can remain unchallenged forever while those otherwise will be addressed in the light of future objectivity. As in the example of ferrocene discussed at great length above, a wide and brilliant light was shown on the structural error, corrections were rapidly published, and an entire new subdiscipline of chemistry was formed and blossomed, even to this day. In this sense, the “error” was only briefly retrogressive if at all; the net consequence was constructively consequential. As Blumenthal further stated, “It would be beneficial to classify types of correction of error including MIEs.” This paper has not intentionally underemphasized the correction of error, a topic that awaits a time in the future.

Notably Merton also characterized MIDs are a “strategic fact ... at the root of a sociological theory of the development of science” (Merton 1961). The examples and analyses reported herein assert that MIEs with MIDs are both strategic facts, at the root of a sociological theory of the development of science.

This work also points to the value and much untapped research territory in the history, sociology and philosophy of science, namely the study of erroneous claims and theories, their effect on on-going science, their modes of detection and correction or improvement,

the speed and nature of their correction, and whether erroneous theories are treated in any fashion differently than longer lasting, closer to the truth theories.

It is interesting and fascinating to examine the opposite of what people like and praise—discovery, namely claims of discovery that in fact have proven wrong. It turns out, to nobody's surprise, that there are many such, for there is any infinity of ways of being wrong, and few ways of being right or few ways to be closer to the truth. Furthermore, the application of Selective Pessimistic Induction explains the transformation of many discoveries into errors., One might think that scientists wouldn't be able to publish so many wrong ideas, given the watchful eye of reviewers and journal editors and ultimately the reading community, but since publication of errors (not yet known to be such) is driven by the same forces that drive publication of correct results or state-of-the-art theories, the literature is strewn with multiple independent errors. Scientists, reviewers and journal editors are not to be ridiculed—they testify to our desire to understand the complexities of nature and of our humanity.

Acknowledgements The author thanks Robert Anderson, Carin Berkowitz, Geoffrey Blumenthal (whose lengthy and scholarly analysis and suggestions arrived only after the paper was in production and thus his input could only partially though usefully be incorporated), Stuart Cantrill, Richard Carchman, Anjan Chakravarty, Amihud Gilead, Roald Hoffmann, Jay Labinger, Seymour Mauskopf, Brannon McDaniel, Lissa Roberts, Eric Scerri, Harriet Zuckerman and one anonymous reviewer for very helpful discussions.

References

- Anonymous. <https://en.wikipedia.org/wiki/Paclitaxel#Synthesis> (2018). Accessed 31 Jan 2018
- Badino, M.: How to make selective realism more selective (and more realist too). <http://philsci-archive.pitt.edu/12523/> (2017). Accessed 31 Jan 2018
- Baker, M.: Is there a reproducibility crisis? *Nature* **533**, 452–454 (2016)
- Balaban, A.T., Schleyer, P.V.R., Rzepa, H.S.: Crocker, not Armit and Robinson, begat the six aromatic electrons. *Chem. Rev.* **105**, 3436–3447 (2005)
- Bensaude-Vincent, B.: L'éther, élément chimique: un essai malheureux de ménélaüs? *Brit. J. Hist. Sci.* **15**, 183–188 (1982)
- Bergman, R.G., Danheiser, R.L.: Reproducibility in chemical research. *Angew. Chem. Int. Ed.* **55**, 12548–12549 (2016). <https://doi.org/10.1002/anie.201606591>
- Berson, J.A.: *Chemical Discovery and the Logicians' Program*. Wiley-VCH, Weinheim (2003)
- Bloor, D.: *Knowledge and Social Imagery*. University of Chicago Press, Chicago (1991)
- Boyd, E.A., Bero, L.A.: Defining financial conflicts and managing research relationships: an analysis of university conflict of interest committee decisions. *Sci. Eng. Ethics* **13**, 415–435 (2007)
- Buckingham, J.: *Bitter Nemesis: The Intimate History of Strychnine*. CRC/Taylor & Francis, Boca Raton (2007)
- Button, K.S., Ioannidis, J.P.A., Mokrysz, C., Nosek, B.A., Flint, J., Robinson, E.S.J., Munafò, M.R.: Power failure: why small sample size undermines the reliability of neuroscience. *Nat. Rev. Neurosci.* **14**, 365 (2013). <https://doi.org/10.1038/nrn3475-c4>
- Calabrese, E.J.: Societal threats from ideologically driven science. *Acad. Quest.* **30**, 405–418 (2017). <https://doi.org/10.1007/s12129-017-9660-6>
- Campanario, J.M.: Commentary: on influential books and journal articles initially rejected because of negative referees' evaluations. *Sci. Commun.* **16**, 304–325 (1995). <https://doi.org/10.1177/1075547095016003004>
- Campanario, J.M.: Rejecting and resisting Nobel class discoveries: accounts by Nobel Laureates. *Scientometrics* **81**, 549–565 (2009)
- Campanario, J.M., Acedo, E.: Rejecting highly cited papers: the views of scientists who encounter resistance to their discoveries from other scientists. *J. Assoc. Inf. Sci. Technol.* **58**, 734–743 (2007)
- Cannon, J.S., Overman, L.E.: Is there no end to the total syntheses of strychnine? Lessons learned in strategy and tactics in total synthesis. *Angew. Chem. Int. Ed.* **51**, 2–26 (2012)
- Chakravarty, A.: *A Metaphysics for Scientific Realism*. Cambridge University Press, Cambridge (2007)

- Chakravartty, A.: Case studies, selective realism, and historical evidence. In: Massimi, M., Romeijn, J.-W., Schurz, G. (eds.) *EPSA15 Selected Papers: The 5th conference of the European Philosophy of Science Association in Düsseldorf*, pp. 13–23. Springer, Cham (2017a)
- Chakravartty, A.: Scientific realism. <https://plato.stanford.edu/entries/scientific-realism/> (2017b). Accessed 31 Jan 2018
- Chakravartty, A.: Scientific realism. <https://plato.stanford.edu/entries/scientific-realism/> (2017c). Accessed 31 Jan 2018
- Chakravartty, A.: email to Seeman, J.I., West Lafayette, Indiana (2017d)
- Chalmers, I., Bracken, M.B., Djulbegovic, D., Garattini, S., Grant, J., Gülmezoglu, A.M., Howells, D.W., Ioannidis, J.P.A., Oliver, S.: Research: increasing value, reducing waste 1. How to increase value and reduce waste when research priorities are set. *Lancet* **383**, 155–165 (2014)
- Chin, C.G., Cuts, H.W., Masamune, S.: Strained systems: cubane. *Chem. Commun. (Lond.)* 880–881 (1966). <https://doi.org/10.1039/c19660000880>
- Collins, H.M.: *Changing Order: Replication and Induction in Scientific Practice*. Sage, London (1985)
- Cordero, A.: On scientific realism and naturalism. *J. Philos. Res.* **40**, 31–43 (2015)
- Cordero, A.: Eight myths about scientific realism. <http://philsci-archive.pitt.edu/12533/> (2017). Accessed 31 January 2018
- Doppelt, G.: Reconstructing scientific realism to rebut the pessimistic meta-induction. *Philos. Sci.* **74**, 35–47 (2007a)
- Doppelt, G.: Reconstructing scientific realism to rebut the pessimistic meta-induction. *Philos. Sci.* **74**, 96–118 (2007b). <https://doi.org/10.1086/520685>. Accessed 31 Jan 2018
- Dunitz, J.: email to Seeman, J.I., Zürich, Switzerland (2017)
- Eaton, P.E., Cole, T.W.: Cubane. *J. Am. Chem. Soc.* **86**, 6719–6745 (1964a)
- Eaton, P.E., Cole, T.W.: The Cubane system. *J. Am. Chem. Soc.* **86**, 962–964 (1964b)
- Eiland, P.F., Pepinsky, R.: X-ray examination of iron biscyclopentadienyl. *J. Am. Chem. Soc.* **74**, 4971 (1952)
- Eliel, E.L., Ro, R.S.: Conformational effects in S_N2 reactions. *Chem. Ind. (Lond.)* **14**, 251–252 (1956)
- Fahrbach, L.: How the growth of science ends theory change. *Synthese* **180**, 139–155 (2011)
- Fenn, J.B., El-Shall, M.S.: A conversation with John B. Fenn. *Ann. Rev.* **2**, 1–11 (2009)
- Feyerabend, P.: *Against Method, Outline of an Anarchistic Theory of Knowledge*. Redwood Burn Ltd/Trowbridge & Esher, London (1975)
- Fischer, E.O., Pfab, W.: Cyclopentadienmetallkomplexe, ein Neuer Typ Metallorganischer Verbindungen. *Z. Naturforsch. B* **7**, 377–379 (1952)
- Flanigan, J.: Double standards and arguments for tobacco regulation. *J. Med. Ethics* **42**(May), 305–311 (2016)
- Fleck, L. (Edited by Trenn, T.J., Merton, R.K.; translated by Bradley, F., Trenn, T.J.): *Genesis and Development of a Scientific Fact*. University of Chicago Press, Chicago (1979)
- Fontani, M., Costa, M., Orna, M.V.: *The Lost Elements: The Periodic Table's Shadow Side*. Oxford University Press, Oxford (2015)
- Friedman, R.M.: *The Politics of Excellence: Behind the Nobel Prize in Science*. W. H. Freeman/Times Books, New York (2001)
- Gilbert, N., Mulkay, M.: *Opening Pandora's Box*. Cambridge University Press, Cambridge (1984)
- Ginsburg, D. (ed.): *Non-Benzenoid Aromatic Compounds*. Interscience Publishers, New York (1959)
- Glanz, S., Bero, L.A.: Inappropriate and appropriate selection of 'peers' in grant review. *J. Am. Med. Assoc.* **272**, 114–116 (1994)
- Goodman, S.N., Fanelli, D., Ioannidis, J.P.A.: What does research reproducibility mean? *Sci. Trans. Med.* **8**, 341ps12 (2016)
- Grens, K.: The rules of replication. *Scientist* **28**, 71–73 (2014)
- Hoffmann, R.: Science and ethics: a marriage of necessity and choice for this millennium. In: Kovac, J., Weisberg, M. (eds.) *Roald Hoffmann on the Philosophy, Art, and Science of Chemistry*. Oxford University Press, Oxford (2012)
- Hoffmann, R.: Tension in chemistry and its contents. *Account. Res.* **22**, 330–345 (2015)
- Hoffmann, R.: email to Seeman, J.I., Ithaca, New York (2017a)
- Hoffmann, R.: email to Seeman, J.I., Ithaca, New York (2017b)
- Hoffmann, R., Minkin, V.I., Carpenter, B.K.: Ockham's razor and chemistry. *Hyle* **3**, 3–28 (1997)
- House, M.C., Seeman, J.I.: Credit and authorship practices. Educational and environmental influences. *Account. Res.* **17**, 223–256 (2010)
- Hückel, E.: Quantentheoretische Beiträge zum Benzolproblem. I. Die Elektronenkonfiguration des benzols und verwandter verbindungen. *Z. Phys.* **70**, 204–286 (1931)

- Hückel, E.: Grundzuge der theorie ungesättigter und aromatischer verbindungen. *Z. Elektro. Angew. Phys. Chem.* **43**, 752–788 (1937a)
- Hückel, E.: Grundzuge der theorie ungesättigter und aromatischer verbindungen. *Z. Elektro. Angew. phys. Chem.* **43**, 827–849 (1937b)
- Hückel, E.: Grundzüge der Theorie ungesättigter und aromatischer Verbindungen. Verlag Chemie, Berlin (1938)
- Hückel, E.: Aromatic and unsaturated molecules. Contributions to the problem of their constitution and properties. In: International Conference on Physics. Papers & Discussions. A Joint Conference Organized by the International Union of Pure and Applied Physics and The Physical Society. The Solid State of Matter, vol. 2, pp. 9–35. The Physical Society, London (1935)
- Ioannidis, J.P.A.: Why most published research findings are false. *PLoS Med.* **2**(8), e1124 (pp. 1–6) (2005)
- Jacob, F.: Of Flies, Mice and Men. Harvard University Press, Cambridge (1998)
- Jung, C.: Synchronicity: An Acausal Connecting Principle. Princeton University Press, Princeton (1973)
- Ke, Q., Ferrara, E., Radicchi, F., Flammini, A.: Defining and identifying Sleeping Beauties in science. *Proc. Natl. Acad. Sci. USA* **112**, 7426–7431 (2015)
- Kealy, T.J., Pauson, P.L.: A new type of organo-iron compound. *Nature (London)* **168**, 1039–1040 (1951)
- Kitcher, P.: The Advancement of Science. Science Without Legend, Objectivity Without Illusions. Oxford University Press, Oxford (1993)
- Kroeber, A.L.: The superorganic. *Am. Anthropol.* **19**, 163–213 (1917)
- Kuhn, T.: The Structure of Scientific Revolutions. University of Chicago Press, Chicago (1972)
- Kuhn, T.S.: Preface. In: Fleck, L. (Edited by Trenn, T.J., Merton, R.K.; translated by Bradley, F., Trenn, T.J.) (ed.) Genesis and Development of a Scientific Fact by L. Fleck, pp. viii–x. University of Chicago Press, Chicago (1979)
- Laidler, K.J.: Lessons from the history of chemistry. *Acc. Chem. Res.* **28**, 187–192 (1995). <https://doi.org/10.1021/ar00052a004>
- Lamb, D., Easton, S.M.: Multiple Discovery: The Pattern of Scientific Progress. Avebury Publishing Co., Towbridge, Wiltshire (1984)
- Laszlo, P., Hoffmann, R.: Ferrocene: ironclad history or Rashomon Tale? *Angew. Chem. Int. Ed.* **39**, 123–124 (2000)
- Laudan, L.: A confutation of convergent realism. *Philos. Sci.* **48**, 19–49 (1981)
- Leek, J.T., Peng, R.D.: Opinion: Reproducible research can still be wrong: adopting a prevention approach. *Proc. Natl. Acad. Sci. USA* **112**, 1645–1646 (2015)
- Lehn, J.-M.: Letter to Hoffmann, R., Strasbourg, France (1966)
- Levenson, T.: The Hunt for Vulcan. How Albert Einstein Destroyed a Planet and Deciphered the Universe. Head of Zeus, London (2015)
- Lewis, P.J.: Why the pessimistic induction is a fallacy. *Synthese* **129**, 371–380 (2001)
- Matson, B. Fact or fiction? Lead can be turned into gold. <https://www.scientificamerican.com/article/fact-or-fiction-lead-can-be-turned-into-gold/> (2014). Accessed 31 Jan 2018
- Mendelssohn, K.: The Quest for Absolute Zero. Weidenfeld and Nicholson, London (1966)
- Merton, R.K.: A note on science and democracy. *J. Leg. Polit. Soc.* **1**, 115–126 (1942)
- Merton, R.K.: Singletons and multiples in scientific discovery: a chapter in the sociology of science. *Proc. Am. Philos. Soc.* **105**, 470–486 (1961)
- Merton, R.K.: On the Shoulders of Giants. A Shandean Postscript. Harcourt Brace & World, New York (1965)
- Merton, R.K.: Behavior patterns of scientists. *Am. Sci.* **57**, 1–23 (1969)
- Merton, R.K.: The Sociology of Science. Theoretical and Empirical Investigations. University of Chicago Press, Chicago (1973)
- Merton, R.K.: The Matthew effect in science, II. Cumulative advantage and the symbolism of intellectual property. *Isis* **79**, 606–623 (1988)
- Merton, R.K.: The Thomas Theorem and the Matthew Effect. *Soc. Forces* **74**, 379–424 (1995)
- Miller, S.A., Tebbboth, J.A., Tremaine, J.F.: Dicyclopentadienyl iron. *J. Chem. Soc.*, 632–635 (1952)
- Misakian, A.L., Bero, L.A.: Publication bias and research on passive smoking. *J. Am. Med. Assoc.* **280**, 250–253 (1998)
- Mizrahi, M.: The pessimistic induction: a bad argument gone too far. *Synthese* **190**, 3209–3226 (2013)
- Mulliken, R.S.: William Draper Harkins, 1873–1951. *Biog. Mem. Nat. Acad. Sci.* **47**, 48–81 (1975)
- Odierna, D.H., Forsyth, S.F., White, J., Bero, L.A.: The cycle of bias in health research: a framework and toolbox for critical appraisal training. *Account. Res.* **20**, 127–141 (2013)
- Ogburn, W.F., Thomas, D.F.: Are inventions inevitable? *Polit. Sci. Q.* **37**(March), 83–98 (1922)
- Park, S.: A confutation of the pessimistic induction. *J. Gen. Philos. Sci.* **42**, 75–84 (2011)
- Park, S.: A pessimistic induction against scientific antirealism. *Organon F21*, 3–21 (2014)

- Park, S.: Why should we be pessimistic about antirealists and pessimists? *Found. Sci.* **22**, 613–625 (2016). <https://doi.org/10.1007/s10699-016-9490-y>
- Pauson, P.L.: Ferrocene and related compounds. *Q. Rev. Chem. Soc.* **9**, 391–414 (1955)
- Poincaré, H.: *Science and Hypothesis* (Reprint of 1905 Book). Dover, New York (1952)
- Polanyi, M.: The potential theory of adsorption. *Science* **141**, 1010–1013 (1963)
- Polanyi, M.: The logic of tacit inference. In: Greene, M. (ed.) *Knowing and Being: Essays by Michael Polanyi*, pp. 140–144. University of Chicago Press, Chicago (1969)
- Psillos, S.: The Realist Turn in the Philosophy of Science. <http://philsci-archive.pitt.edu/12440/> (2017). Accessed 31 Jan 2018
- Rothman, K.J.: Conflict of interest. The New McCarthyism in Science. *J. Am. Med. Assoc.* **269**, 2782–2784 (1993)
- Roush, S.: The rationality of science in relation to its history. In: Devlin, W.J., Bokulich, A. (eds.) *Kuhn's Structure of Scientific Revolutions-50 Years On*. Boston Studies in the Philosophy and History of Science, vol. 311. Springer, Boston (2015)
- Roush, S.: Optimism about the pessimistic induction. In: Magnus, P.D., Busch, J. (eds.) *New Waves in Philosophy of Science*, Palgrave MacMillan, London (2010)
- Sarafoglou, N., Kafatos, M., Beall, J.H.: Simultaneity in the scientific enterprise. *Stud. Sociol. Sci.* **3**, 20–30 (2012)
- Schillinger, D., Tran, J., Mangurian, C., Kearns, C.: Do sugar-sweetened beverages cause obesity and diabetes? Industry and the manufacture of scientific controversy. *Ann. Intern. Med.* **165**, 895–897 (2016). <https://doi.org/10.7326/L16-0534>
- Seeman, J.I.: Effect of conformational change on reactivity in organic chemistry. Evaluations, applications, and extensions of Curtin-Hammett/Winstein-Holness Kinetics. *Chem. Rev.* **83**, 83–134 (1983)
- Seeman, J.I.: Moving beyond insularity in the history, philosophy, and sociology of chemistry. *Found. Chem.* (2017a). <https://doi.org/10.1007/s10698-017-9290-7>
- Seeman, J.I.: Second-guessing the Nobel Prize committee for chemistry. In: Strom, E.T., Mainz, V. (eds.) *The Posthumous Nobel Prize in Chemistry. Correcting the Errors and Oversights of the Nobel Prize Committee*, pp. 9–20. American Chemical Society, Washington (2017b)
- Seeman, J.I., Cantrill, S.: Wrong but seminal. *Nat. Chem.* **8**, 193–200 (2016)
- Seeman, J.I., House, M.C.: Influences on authorship issues. An evaluation of giving credit. *Account. Res.* **17**, 146–169 (2010a)
- Seeman, J.I., House, M.C.: Influences on authorship issues. An evaluation of receiving, not receiving, and rejecting credit. *Account. Res.* **17**, 176–197 (2010b)
- Seeman, J.I., House, M.C.: Authorship issues and conflict in the U.S. Academic Chemical Community. *Account. Res.* **22**, 346–383 (2015). <https://doi.org/10.1080/08989621.2015.1047707>
- Seyferth, D.: Bis(benzene)chromium. 1. Franz Hein at the University of Leipzig and Harold Zeiss and Minoru Tsutsui at Yale. *Organometallics* **21**, 1520–1530 (2002a)
- Seyferth, D.: Bis(benzene)chromium. 2. Its Discovery by E. O. Fischer and W. Hafner and subsequent work by the research groups of E. O. Fischer, H. H. Zeiss, F. Hein, C. Elschenbroich, and Others. *Organometallics* **21**, 2800–2820 (2002b)
- Shibasaki, M., Ohshima, T.: Recent studies on the synthesis of strychnine. In: *The Alkaloids: Chemistry and Biology*. Elsevier, Amsterdam (2007)
- Smyth, A.H.: *The writings of Benjamin Franklin*, vol. 7. Macmillan, New York (1907)
- Soler, L.: Are the results of our science contingent or inevitable? *Stud. Hist. Philos. Sci.* **39**, 221–229 (2008)
- Stanford, P.K.: Refusing the Devil's Bargain: what kind of underdetermination should we take seriously? *Philos. Sci.* **68**(Proceedings), S1–S12 (2001)
- Stanford, P.K.: *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*. Oxford University Press, Oxford (2006)
- Steneck, N.H.: *Introduction to the Responsible Conduct of Research*. U.S. Department of Health and Human Services, Washington (2007)
- Strom, S.: U.S. Panel under fire for its ties to biotechs. In: *The New York Times*. vol. 165, pp. B1, B4. New York (2016)
- Tappin, B., van der Leer, L., McKay, R.: The heart trumps the head: desirability bias in political belief revision. *J. Exp. Psychol. Gen.* **146**, 1143–1149 (2017)
- Teixeira da Silva, J.A., Dobránszki, J.: Compounding error: the afterlife of bad science. *Acad. Quest.* **30**, 65–72 (2017). <https://doi.org/10.1007/s12129-017-9621-0>
- van Bavel, J.: Why do so many studies fail to replicate? In: *The New York Times*. vol. 165, p. SR10 (2016)
- Weininger, S. J.: (2018) Delayed Reaction: The Tardy Embrace of Physical Organic Chemistry by the German Chemical Community. *Ambix* **65**, 1–24. (In Press)

- Werner, H.: Landmarks in Organo-Transition Metal Chemistry. A Personal View. Profiles in Inorganic Chemistry. Springer, Berlin (2009)
- White House Office of Science and Technology Policy: Federal Policy on Research Misconduct. 76260-76264; http://ori.dhhs.gov/policies/fed_research_misconduct.shtml, <http://ori.hhs.gov/federal-research-misconduct-policy> (2000). Accessed 4 June 2015
- Wilkinson, G., Rosenblum, M., Whiting, M.C., Woodward, R.B.: The structure of iron *bis*-cyclopentadienyl. J. Am. Chem. Soc. **74**, 2125–2126 (1952)
- Winstein, S., Holness, N.J.: Neighboring carbon and hydrogen. XIX. *t*-Butylcyclohexyl derivatives. Quantitative conformational analysis. J. Am. Chem. Soc. **77**, 5562–5578 (1955)
- Woodward, R.B.: Art and science in the synthesis of organic compounds. In: O'Connor, M. (ed.) Pointers and Pathways in Research: Six Lectures in the Fields of Organic Chemistry and Medicine, pp. 23–41. CIBA of India Limited, Bombay (1963)
- Woodward, R.B., Hoffmann, R.: Stereochemistry of electrocyclic reactions. J. Am. Chem. Soc. **87**, 395–397 (1965)
- Woodward, R.B., Hoffmann, R.: The conservation of orbital symmetry. Angew. Chem. Int. Ed. **8**, 781–853 (1969)
- Woodward, R.B., Schramm, C.H.: Synthesis of protein analogs. J. Am. Chem. Soc. **69**, 1551–1552 (1947)
- Wray, K.B.: The pessimistic induction and the exponential growth of science reassessed. Synthese **190**, 4321–4330 (2013)
- Wray, K.B.: Pessimistic inductions: four varieties. Int. Stud. Philos. Sci. **29**, 61–73 (2015)
- Wray, K.B.: Discarded theories: the role of changing interests. Synthese, 1–17 (2016). <https://doi.org/10.1007/s11229-016-1058-4>
- Zeiss, H.H., Tsutsui, M.: π -complexes of the transition metals. I. Hein's polyaromatic chromium compounds 1,2. J. Am. Chem. Soc. **79**, 3062–3066 (1957). <https://doi.org/10.1021/ja01569a019>
- Zydowsky, T.M.: Of sandwiches and Nobel Prizes: Robert Burns Woodward. Chem. Intell. **6**, 29–34 (2000)