# Insights and Blind Spots: The Conundrum of Bias

Douglas Allchin

**Abstract.** I explore the role of cognitive perspectives, as biases, in fostering (beneficial) insights, on the one hand, and (deleterious) blind spots, on the other. Historical examples help demonstrate how—counterintuitively, perhaps—insights and blind spots can arise from the very same perspectival bias. While philosophers widely acknowledge the importance of unique perspectives in leading to discovery and bias in contributing to errors, there seems little recognition that these tendencies can be coupled together. Skepticism is best viewed not as an individual form of self-doubt, but as a social level phenomenon, based on criticism among contrasting perspectives. Reciprocal criticism from complementary perspectives helps in managing the dual effects of such bias.

**Keywords**: bias, pespective, discovery, error, social epistemology, skepticism

## *1. Introduction: Insights and Blind Spots*

Philosophers of science, in efforts to support more effective scientific practice, have sought to articulate methods to promote scientific discovery on the one hand, and to reduce error on the other. Here, I address how these two aspects of science may have an intimate but ironic relationship.

Scientists encounter many sources of error (or error types), spanning observational, conceptual and social dimensions (e.g., Allchin 2001). Prominent among the errors are various cognitive orientations that bias perception, the collection of representative data, or the interpretation of results (e.g., Benson & Manoogian 2018; Piattelli-Palmarini 1994). Using various personal and cultural perspectives, scientists focus on some things, and exclude others (e.g., Giere 2006). They highlight some phenomena or evidence as relevant or salient, and others as peripheral or insignificant. The biases include (at least): observer bias, availability bias, theoretical bias, confirmation bias, gender bias, racial bias, class bias, and other forms of cultural bias (e.g., Collaboration of Bias, 2020; Sackett 1979). With only selective evidence, conclusions may become skewed, misleading and/or wrong. Even worse, perhaps, confirmation bias may further help the errors escape detection (e.g., Gilovich 1991; Nickerson 1998). That is, biases of various sorts generate unwelcome *blind spots*.

Bias thus seems anathema to good science. The various biases confound the development of reliable knowledge. Because perspectival bias can foster unproductive or wayward lines of reasoning— or outright error— philosophers have been conventionally concerned with curbing or even trying to eliminate such bias (see also below). The normative ideal is to steer clear of personal idiosyncrasies, either through individual rigor or collective critical discourse.

Many philosophers are concerned, alternatively, with how to support the creative dimension of science. How do fruitful new ideas originate? While the prospect of any formal "logic of discovery" has generally been abandoned, some philosophers (along with many historians of science) remain fascinated with trying to identify the factors that have fostered important insights in the past, and that may reflect general principles of innovation (e.g., Gleick 1992; Johnson 2009; Kohn 1985; Ness 2012). Simonton (2004) synthesized the literature on creativity in science, and identified four main theories: logic, genius, Zeitgeist, and chance (or what a historian might label as *contingency*). Each is a way to account for particular ways of recombining ideas—from personal experience, professional background, unanticipated encounters, available cultural metaphors, cross-disciplinary connections, and so forth—into a unique constellation that constitutes a substantially new way of seeing things. Here, too, we encounter a role for particular perspectives: that construe certain elements as relevant, highlighting a definitive pattern, while obscuring other elements as irrelevant background. Namely, they function just as perspectival frames do in cases of error (described above). On these occasions, however, biases seem to lead to *insights*.

The contrasting outcomes of bias—both positive and negative—may seem a conundrum. But philosophers may easily imagine a prospective resolution by carefully differentiating the two types of cases, sorting perspectives according to whether each contributes to science or leads one astray. Accordingly, philosophers tend to celebrate those biases that lead to discovery, while regretting those that lead to error: for example, by celebrating the creative genius of a Darwin or Einstein, or denouncing the undesirable specter of Colonialist or gender bias. The assumption is that any particular bias may be regarded as *either* productive or detrimental, *either* as a "resource" or a source of "error" (e.g., Anderson 2020, §6). The philosophical challenge would seem to be finding a general rule that can distinguish the respective types of bias, towards guiding more effective science, promoting insights and avoiding blind spots.

Here, however, I show how both insights and blind spots can arise *from the very same perspectival bias*. Context matters. While such cases may not seem beyond possibility according to conventional views, philosophers seem to respond to them as curious accidents. Yet such instances hold potentially potent lessons. They indicate that the prospect of trying to categorize and characterize "negative" perspectival biases and develop methodological strategies to avoid them may be misconceived. The same bias may, on another occasion, prove fruitful. The programmatic effort to eliminate bias (or at least certain types of biases) as necessarily detrimental would seem ill advised. The sources of error are also, in other contexts, sources of innovation. The cost of discovery seems to be the risk of error.

In this paper, I explore the general philosophical question of perspectival bias through a constellation of selected cases, echoing the form of argumentation that in the 1970s, 80s and 90s originally instilled appreciation of the very notion of bias in science: by demonstration, through a suite of historical examples (e.g., Gould 1981; Mackenzie 1990; Schiebinger 1993; Shapin &

Schaffer 1985). As part of a larger project, I have documented and analyzed over 200 cases of error in science. Here I document numerous striking examples from among them where the source of error on one occasion also led to a significant discovery on another, all based on the very same individual perspective, or "bias" (Section 3). Next, I describe a series of related episodes showing how verdicts (or attributions) of a scientists's "bias" varied from one case to another (Section 4). Third, I review how the tools of social epistemology that originated in Merton's sociological norms help manage errors. I confront the problem of whether social epistemologists have fully demonstrated that bias is remedied through complementary perspectives, in contrast to the asymmetric exercise of superior, or privileged, perspectives. This leads me to discuss a few cases where error correction was reciprocal, underscoring the role of symmetry, based on complementarity (Section 5). To begin, however, I delve further into current philosophical views on bias and perspectives (Section 2).

## *2. The Nature of Bias and Perspectives*

My focus here is on insights and blind spots, on discovery and error, and how they sometimes originate in similar ways. They are linked by what I am calling bias, or perhaps, more precisely, perspectival bias. The term 'bias' permeates philosophical discourse, yet ironically it seems that there is no standard benchmark conception, commonly accepted account, or "textbook" definition (e.g., Aronson 2017). It seems prudent, therefore, to clarify the meaning of the term (and the concept it denotes) as I use it here.

Lively discussion of bias can be found among practicing scientists, especially in clinical medicine and the experimental sciences, which rely heavily on sampling and statistical reasoning (e.g. Aronson 2017; Chavalarias and Ioannidis 2010). A widely cited defintion is Sackett's (1979): bias is "any process at any stage of inference which tends to produce results or conclusions that differ systematically from the truth" (p. 60). It reflects, as Aronson (2017) notes, several common themes in conceptualizing bias: distortion, systematic error, and deviation from the truth. The effects of bias are thus almost universally (or by default) construed as negative, leading *away* from reliable knowledge. Biases are habitually associated with blind spots.

The view that any particlar bias inherently threatens reliability is further echoed in the work of philosophers, in their articulation of various methods to "correct" for bias. For example, observer bias may be remedied by blinding protocols. Confounding bias may be identified and regulated through the use of controls. Sampling bias can be reduced through randomization. Publication bias may be addressed by various proposed incentives and by more blinding in the editorial process. Gender and racial bias can be exposed (and presumably resolved) through critical discourse. Again, philosophers tend to view bias chiefly as a problem, best eliminated.

My concern, however, is specifically the "bias" that emerges through the adoption of particular perspectives—the notion of perspectives most notably described by Giere (2006). Here, a perspective is a way of framing: specifying the features and style of a representation or interpretation, of sampling (or collecting relevant evidence), or of inference from one claim to a prospectively valid alternative. (In mechanical drawing or art, for example, a perspective literally establishes a focal point, an orientation, including a horizon line, and the spatial geometry for projecting spatial information onto a planar surface, without dictating any of the content per se.) There is also, literally, the frame, which (again, literally) both includes and excludes. Namely, the particularities of a perspective highlight certain features. At the same time, they send other features into the background, blind the thinker to certain lines of reasoning, or accept certain assumptions as warranted without full justification. The upshot is that some things are highlighted and clearly delineated, while others are excluded or obscured. Ironically, perhaps, a perspective yields both insights and blind spots simultaneously.[1]

The notion of perspectives has perhaps been most thoroughly developed in the context of maps (e.g, Giere 2006; Turnbull 1989; as early as Ziman, 1978, and echoed as recently as Winther, 2020). Maps exhibit perspectives, hence are selective, conventional, contextual, and potentially laden with instrumental value (Turnbull 1979, 61). A perspective, as exemplified in maps, provides partial knowledge. It functions to elucidate some features, while disregarding others that would only be distracting and stymie the effective retrieval of the intended information. Because it is incomplete, a map (or a perspective) potentially can provide a misleading image if not applied appropriately. As Monmonier notes, “Not only is it easy to lie with maps. It is essential” (1991, 1).

What is true for maps may apply equally to models, heuristics, and analogies. All embody perspectives. And all are selective, conventional, contextual and subject to erroneous conclusions if misinterpreted. Here, philosophers may be more familiar with and acknowledge the bivalent nature of perspectival bias: supporting both insights and blind spots. Analogies, for instance, are well known to have both positive and negative aspects, and heuristics, as valuable as they may be, are known to have systematic points of failure, leaving heuristic gaps. Here, too, one may situate the view of some feminist epistemologists (e.g., Anderson 2019) that some biases may be “partial” or “limiting”: able to serve as a resource, instead of a source of error. (The diagnostic question of how to distinguish such limiting biases from erroneous biases, however, seems left unanswered.) Biases, at least of the perspectival kind, may potentially contribute positively or negatively with respect to constructing reliable knowledge.

Managing perspectival bias poses a pragmatic challenge, of course. How do scientists negotiate their way through the respective insights and blind spots, sorting reliable knowledge from misleading dreck, filtering the fruitful from the frivolous? Social epistemologists credit the “organized skepticism” of a diverse critical community (e.g., Hull 1988; Merton 1973; O’Connor, Goldberg & Goldman, 2024; Oreskes 2019; Ziman 2000). A key element in regulating error is afforded to complementary perspectives (each with their own bias, perhaps), about which more below in Section 5.

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[1]This account of perspectives and framing notably escapes the problem of attributing bias to values or to strong motivational reasoning. It allows thinking about bias independently of the controversies surrounding the ineliminable role of values in shaping scientific content knowledge (e.g., Giere 2006), without discounting the possibility that other instances of bias may indeed reflect such cognitive processes.

To summarize: biases, both informative and misleading, arise from perspectives. Perspectives, here, include the framing dimension of maps, models, heuristics and analogies. They apply to representations, or interpretations, as well as to inferences, and data- or evidence-collecting methodologies. All exhibit selectivity, contextuality, and conventionality. These features contribute to uneven attention to phenomena. As a result, some pehnomema and observations gain prominence and clarity, while others recede into the shadows. Perspectival baises can (theoretically) foster both insights and blind spots simultaneously. Of course, this discussion clarifies only the use of terms. It does not speak directly to actual scientific practices, which requires (like any scientific claim) concrete evidence: here, from historical or contemporary cases as validation, a requisite task to which I now turn.

## *3. Perspectival Bias: Bane, Benefit, or Both?*

In the previous section, I sketched the nature of perspectives and biases. Of course, no respectable philosophy will rely solely or even primarily on stipulative definitions or abstract argument. What ultimately matters is whether these philosophical interpretations or characterizations reflect the concrete reality of scientific practice: what is the evidence by which the conceptions are tested and their validity measured? Hence, in the remainder of this paper, I will focus primarily on historical cases which probe these views. In this section, I present a handful of cases where (typically) the same scientist exhibited an insight on one occasion, and a blind spot on another, all based on the same perspective. While these cases are known, the poignant juxtapositions I describe here seem to have escape significant philosophical notice. The historical details are essential for revealing just how perspectives function.

### *Michael Faraday, Sandemanian*

I begin with one of the titans of 19th-century science, Michael Faraday. Some readers may know that Faraday was a member of a small Protestant sect in Britain, the Sandemanians. They espoused the harmony and unity of nature. That worldview motivated and guided Faraday's scientific investigations (Cantor, 1991). He sought especially a unifying relationship between the forces of nature. Notably, that led to the discovery of electromagnetic induction. The Sandemanian belief in symmetry in nature further helped guide Faraday in establishing the reciprocal relationship. This discovery later led to electric generators and electric motors. Those were important *insights*, partly afforded by a particular religious perspective.

The very same beliefs led Faraday to investigate other relationships among natural forces, which he believed must be unified in God's world. For example, he tried to find how electricity affected polarized light or produced heat. In 1828, he projected a solar spectrum on a copper plate, expecting to show how light could induce electricity. Those investigations did not yield the same positive results as his electromagnetic research.

Even more impressive were studies done over three decades on the relationship between electricity and gravity. Faraday was convinced that a *gravielectric* effect was waiting to be demonstrated, just like the once unknown electromagnetic effect. He recorded in his diary on March 19, 1849:

> Gravity. Surely this force must be capable of an experimental relationship to Electricity, Magnetism and the other forces, so as to bind it up with them in reciprocal action and equivalent effect. Consider for a moment how to set about touching this matter by facts and trial.

By later that summer Faraday was conducting experiments. He adapted his earlier work on electromagnetic effects, attaching a galvanometer to a wire wound around a coil, which would now experience gravitational free fall. But he failed to document any gravielectric effect.

Apparently undeterred, by September he was at work on yet another test. Again, using earlier successful work on as a model, he dropped a metal bar through a wire helix. Again, no success. And again, Faraday retained the same conviction that had promoted all his earlier work.

In November, 1850, he echoed his “strong feeling” that the effect would yet be discovered. Indeed, Faraday continued to reflect on the problem. Apparently undeterred, he returned to experiments yet again in 1859, anticipating that a longer free fall distance, made possible in a lead shot tower, would reveal the effect. Throughout all his gravielectric investigations, Faraday applied the same faith in the unity of nature that led to his highly celebrated electromagnetic discoveries. But here his convictions of a gravielectric effect were in error. The same bias yielded both insight and blind spot.

### *Charles Darwin, Uniformitarian*

Next, consider the equally renowned Charles Darwin. Darwin’s very first scientific paper, in 1837, was a theory on the formation of coral atolls. Darwin had been greatly influenced by Charles Lyell’s principle of uniformitarianism. Using that lens, or bias, Darwin reasoned that coral reefs formed along the shores of mountainous islands, which then gradually eroded, leaving hollow rings. It was an act of sweeping historical imagination based on observational fragments about coral growth and location. The idea proved correct — and it helped launch Darwin’s scientific career.

Darwin continued to apply Lyell’s large-scale gradualist thinking in theorizing about other geological formations. In his next effort in 1839, he tackled the “parallel roads of Glen Roy,” a well known series of stony ledges lining a valley in Scotland. Here, Darwin imagined that they were the debris of successively lower shorelines, left by a receding ocean over a large span of geological time. Here, he was wrong. The ledges were glacial moraines, left by a retreating glacier, not an ocean. Darwin, to his credit, acknowledged his “great blunder” when Louis Agassiz’s theory of glaciation and ice ages gained prominence. In both these cases—one insightful discovery, one error—Darwin relied on the same Lyellian perspective, which biased his reasoning (adapted from Author; see also Browne, 1995, 376-378, 431-433; Rudwick, 1974). But the bias was not uniformly negative, as philosophers typically contend (Section 2).

### *Charles Barkla, Nobelist*

Next, consider Charles Glover Barkla's Nobel-Prize winning work on X ray fluorescence in the early 1900s. Each atomic element absorbs and releases X rays in a distinct pattern. Barkla's extensive documentation of the angles and wavelengths intersected with Henry Moseley's work identifying elements based on their atomic number. That dimension of atomic *structure* was more exact than identifying elements based on their chemical properties, or by their atomic weight — the features that Mendeleev had used to organize the periodic table. Moseley's work, in turn, helped identify "holes" and mistakes in the sequence of the elements, and provided a deeper explanation for the ordering of the atomic elements.

Barkla's original work focused on two series of emissions from each element, labeled the K series and the L series. But later Barkla reported finding a third series, which he called the J series. He pursued those results for two decades. After years of work, he wrote to a colleague "Yes! the J-phenomenon is very interesting and is so fundamentally new, but it may take a generation to work it out thoroughly. ... I am convinced that it is of the very greatest importance." Alas, the J series was eventually determined by others to be "echoes" of the original series — artifacts of the Compton effect. But Barkla's perspective was limiting. He admired J.J. Thomson and held to classical views about the preservation of energy in the scattering of X rays. He refused to consider quantum principles as inherent in what he observed, as described by Compton. He thus held steadfast to his ideas and original experimental methods, a bias which had served him well at first. Ultimately, he was ill equipped to fully investigate the phenomena he labeled the J series, or to interpret it effectively. Barkla's perspective led to insights at first, but then, later, to a major blind spot and corresponding error (Allen, 1947, quote on p. 356; Trimble, 2017; Wynne, 1976).

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These three examples show how the very same principle can guide reasoning in different cases, fruitful on one occasion, not so in another. At the same time, one may well contend that in these three examples, the idiosyncratic pattern of reasoning that led to error was individual, and not shared by others, hence easily discounted. However, in the next three cases, the erroneous conclusions were far more consequential.

### *Urbain Le Verrier and Anomalous Planetary Orbits*

French astronomer Urbain Le Verrier is generally celebrated by philosophers for his successful prediction of Neptune, based on the anomalies in the orbit of Uranus (e.g., Hanson 2020; Losee 2005, 142). But the same considerations led him to predict another planet, named Vulcan, which spurred earnest search for nearly two decades. But the presumed justification for that second planet's existence was an error (Levenson, 2015).

Both occasions hinged on applying Newtonian mechanics to planetary orbits. The first was based on an anomaly in the orbit of Uranus. After its discovery in 1781, subsequent observations charted Uranus's orbit around the Sun. But its pathway did not accord with the calculations based on Newton's theoretical equations. By 1821, there was no escape in hoping that more measurements might resolve the dilemma. Several alternatives were proposed to explain the conspicuous incongruence.

About two decades later, Le Verrier adopted the assumption that another massive body might be lurking nearby, exerting additional gravity on Uranus, perturbing its orbit. He completed the complex calculations, making a precise prediction of where it should be, according to Newton's equations. After failing to inspire his colleagues in Paris, Le Verrier wrote to a German astronomer. Within hours of receiving the letter, he found the new planet just about where it "should" have been: the discovery of Neptune in 1846. The anomaly was resolved. With it, as Norwood Russell Hanson proclaimed, Le Verrier triumphantly "carried Newtonian mechanics into the brightest heaven of scientific achievement" (2020, p. 130).

However, another orbital mismatch soon arose. Its resolution was not so accommodating for Newton's theory. At virtually the same time as the discovery of Neptune, a rare transit of Mercury across the Sun had allowed precise measurements of its movement. According to the latest calculations — now taking the gravitational pull of nearby Venus into account — Mercury was 17 second behind schedule. In context of the precision available, that was problematic. Later, the problem was refined. Mercury's orbit had a "wobble" (its perihelion precesses). Le Verrier now calculated the relative gravitational influence of other planets on Mercury: Venus could account for half of the wobble, Jupiter another quarter, and Earth about 15%. That left 7% of the total discrepancy unexplained. A new planetary orbit anomaly. At least a solution seemed conveniently at hand: based on experience with Uranus and Neptune, there must be another planet in the vicinity of Mercury. In 1860, Le Verrier named it Vulcan.

Other astronomers seemed to agree. And the search began. Yet many promising efforts failed. Some prospects turned out to be sunspots; others, known stars. Yet other reports could not be confirmed. So, despite persistent efforts, it could not be found. Vulcan (the solution to Mercury's anomaly) became, ironically, the anomaly itself.

Nearly two decades later, after exceptional opportunities in 1878 yielded no concrete observations, confidence in ever finding Vulcan waned and it gradually dwindled to the status of an error. Ultimately, in 1915, Einstein's general theory of relativity explained Mercury's puzzling orbit. This time, Newtonian mechanics did indeed prove wrong

Le Verrier's method for predicting planets based on anomalous orbits, which had proved so remarkable in the case of discovering Neptune, failed in the case of Vulcan. Insight and blind spot, coupled together by a shared perspectival bias.

### *Spectroscopy and New Elements*

Next, consider the "non-discovery" of two elements. In the early 1800s, chemists learned that elements emit distinctive colors. With the development of the spectroscope by Robert Bunsen and Gustav Kirchoff in 1860, those colors could be associated with distinctive wavelengths. Spectroscopy thus became a powerful form of chemical analysis: for identifying elements in unknown samples. It also quickly became a tool for discovering new elements: cesium, rubidium and thallium (each named, notably, for their distinctive spectral color) and, later, scandium. Helium was identified in 1868 on the basis of unfamiliar spectra lines from the Sun ("helios"), and within three decades it was found on Earth.

In 1864, William Huggins found that light from the Cat's-Eye nebula (seen in the constellation Draco) was found to exhibit yet unknown spectra lines. Following the pattern of spectroscopic reasoning, he attributed that to a yet-unknown element. It was given the name (appropriately enough) *nebulium*.

Five years later, during a solar eclipse, Charles Young and William Harkness each observed other anomalous spectra lines in light from the Sun's corona: evidence of yet another element. Its name? Yes: *coronium*.

Yet they do not exist. 1927, Ira Bowen showed that the particular lines of "nebulium" were emitted by highly ionized forms of oxygen and nitrogen (as yet unrecognized). In the 1930s—after decades of confirming and refining the measurements of the "coronium" spectra lines— the lines were traced to highly ionized forms of iron and nickel—at temperatures that were simply not precedented on Earth. So the elements quietly disappeared from textbooks and chemical discourse. Relying on a narrow spectroscopic perspective alone had yielded the discovery of new elements on many occasions, and on others, using precisely the same reasoning, predicted phantom elements: errors (Bowen 1927; Fontani, Costa & Orna 2015).

### *Isaac Newton and Chemical Affinities*

Finally, consider Isaac Newton's notion of chemical affinities, wherein he adapted his insights on gravitational attraction to conceptualizing matter as particles held together by analogous forces on an atomic scale. Newton's monumental credibility, in particular, gave legitimacy to the idea, which guided work for nearly a century, before it was abandoned as unworkable and erroneously conceived (Brock 1992; Holmes 1962; Hornix 1988; Kim 2013).

Newton's law of gravitation was an important element in his mechanics. And, as the prediction of Neptune notably demonstrated, it was enormously powerful and effective. Newton also conceived matter as composed of corpuscles and imagined them in the same context. In an addendum to his 1717 *Opticks*, Newton postulated that a microgravity-like force might characterize the interactions of atomic particles. Each substance, he proposed, had a characteristic attractive force—its *affinity*—which determined how it reacted with other substances. He illustrated this with a series of replacement reactions, showing the relative power of each substance to displace others when combined with nitric acid in solution. That is, Newton envisioned simple quantitative laws governing chemical reactions—an atomic physics, of sorts.

Others followed Newton's lead, treating his list of replacement reactions as an exemplar (or paradigm, in a narrow Kuhnian sense). In 1718, Newton's table was expanded to include other substances, each adding another column to an emerging affinity table, now with 16 columns. Then, yet more columns were added. More substances also meant adding new rows to each column. It became a useful summary table — displayed on the walls of many laboratories — for what chemical reactions to expect when combining particular substances. Over the century, the affinity tables became more complex. By 1783, Torbern Bergman's grand version encompassed 59 columns.

However, the order of replacement was not always the same from column to another. The sequence also varied based on whether the substances were mixed in water (wet) or were heated (dry). Results varied with temperature. The concentration, or saturation, of the substances mattered in some cases. Later, the role of volatility and solubility were noted. Additional substances (mixtures of four or more) exhibited further complications. In short, over time, understanding of context riddled the table with anomalies. All these informed chemical practice, but the prospects of fulfilling Newton's vision by establishing simple laws of affinity became increasingly problematic.

Nevertheless, Newton's perspective, especially of *quantifying* affinities, continued to bias research. Mid-century, Comte de Buffon helped articulate the basic view: "The laws of affinity ... are the same with that general law by which the celestial bodies act upon one another. The exertions are mutual, and proportional to their masses and distances" (quoted in Thackray, 1970, p. 159). In 1758 the Academy of Rouen offered a prize for the best essay on affinities. The prize was divided. George Louis Le Sage drew the analogy with gravity, but to maintain consistency among his observations, he had to suggest that attractions depended on density, not absolute masses. Jean Philippe de Limbourg, for his part, appealed to three factors in attraction (not one), but even so had to acknowledge many contradictions and exceptions. Despite the difficulties, both invoked Newton's authority and used Newtonian language. In 1772, French chemist Guyton de Morveau echoed the basic perspective again, appealing to Newton's inverse square law of gravitation, while endeavoring (unsuccessfully) to quantify affinities in a consistent way. In Ireland, Richard Kirwan, too, tried to measure affinities. His efforts earned him the prestigious Copley Medal from the Royal Society in 1782. But he also struggled to identify which factors were relevant to "that power by which the invisible particles of different bodies intermix and unite" (quoted in Kim, 2003, p. 274). Was it specific gravity? Concentration? Saturation? As exemplified in these efforts over many decades, Newton's perspective remained a benchmark for comparison, biasing thinking.

None of the efforts at quantification or explaining affinities succeeded. Eventually, after nearly a century of work, Newton's concept was abandoned. Chemists shifted to quantifying the relative proportions of each substance that would be required to react with each other: a more practical measure for the laboratories in pharmacy and industry. Later, with the emergence of electrochemistry (and, with it, electrolysis), chemists began to conceive chemical combination instead in terms of the attraction between positive and negative charges. Newton's framing of unique, quantifiable forces, successful for gravity and celestial mechanics, was in error when applid on a microscale to chemistry.

*4. The Context-Dependence of Bias*

The implicit assumption among many philosophers is that bias harms scientific reasoning and easily leads to error (Section 2). The corresponding notion is that anyone who finds and corrects such an error has a more privileged perspective, is inherently more objective, or has demonstrated the ability to exercise skepticism more productively: by virtue of their action, they exhibit more fully the ideals of individual rationality in science. There is little consideration of context or its epistemic significance.

For example, in this view, one is liable to say that the advocates of N-rays (one of the more notorious cases of human observational error in science) were *self-deluded* (e.g., attributions made by Dolby, 1996; Langmuir, 1953, 1989; Rousseau, 1992; Turro, 1999*)*. Robert Wood (who exposed the sad tale) was, by contrast, a champion of objectivity (e.g. Gratzer, 2000). Likewise, critics, like Irving Langmuir, who chastised the likes of Fred Allison for his eponymous magneto-optic effect, are the rare but essential champions of (true) science. These philosophical assessments, however, often cherry-pick the historical evidence, which is used to illustrate rather than to probe and test their philosophical views. A more complete account of the historical details yields a quite different conclusion.

*From N-Rays to the Allison Effect to Cloud-Seeding*

We begin with the well known case of N-rays in the early 1900s (Nye, 1980). René Blondot claimed to have detected a new kind of radiation. It was confirmed by the observations of many French colleagues. However, the images on the screen were very faint, and not everyone attested to seeing them. Did the concurrence among many observers bolster their legitimacy, or was the phenomenon an artifact of communal expectations and subtle, but shared perceptual bias? The key factor in answering that question was someone who did not share the French physicists' perspective.

Enter Robert Wood, visiting from America. Wood could not see the rays, and his posture quickly turned to one of probing the circumstances. He mischievously tampered with the experimental set-up afforded by the secrecy of the dark room—adopting questionable professional ethics, but certainly achieving the blinded conditions for testing the observers themselves with suitable controls. The observers did not pass the test. Many historical commentators credit Wood for exhibiting a more rational, skeptical stance. But that judgment seems largely shaped by the outcome ("N-rays were imaginary, so *of course* Blondot and others must have been prejudiced, inferior scientists—and the debunker, a hero"?). It seems more appropriate to say that Wood simply brought to the occasion an alternative viewpoint which fostered collecting "deeper evidence" than what was already available. It was the concrete evidence from blinded observation—not some abstract measure of "rationality"—that helped isolate a hidden source of error.

One of the great ironies of the N-rays affair is that Blondot's "debunker," Robert Wood, himself came to endorse erroneous claims of a quite similar nature decades later. There, he was not so critical. In 1930 Fred Allison had apparently developed a powerful method of chemical analysis that could detect subtle variations of composition based on an element's atomic weight, even at low concentrations. The research had begun as a somewhat routine investigation into the Faraday effect (the ability of an ionic solution to rotate polarized light in a magnetic field). Chemists were eager to know about any time delay, which might provide clues about the atomic nature of the phenomenon. Allison's strategy involved using two complementary samples whose rotational effect could cancel each other, but would be induced at slightly staggered times. The amount of light transmitted through the apparatus would vary with the samples' respective time delays. Like the N-rays set-up, Allison's apparatus also involved a spark generator. It emitted short bursts of light as the primary light signal, which simultaneously triggered the electromagnetic field around the samples. The experimenter could vary the length of the electric wires, and thus the time difference between the two samples, and search for the critical time that minimized the intensity of the light signal. It may sound complicated, but it was simple enough that others, including many graduate students, were able to copy it and study the "Allison effect" for themselves.

After setting up the basic apparatus, Allison investigated a diversity of samples, documenting the time delays for each. He soon concluded that the delay time varied with the atomic weight of the element, reflected in smooth graphs comparing a series of related compounds. In 1931, he encountered results that indicated hydrogen had an unknown second, heavier isotope (depicted in graphs slightly displaced from the previous ones). The following year Harold Urey announced spectroscopic evidence for the same—an apparent validation of Allison's method. Encouraged, Allison reasoned that his method could detect the presence of new elements, marked indirectly by unfamiliar atomic weights. He went searching for element 87, and found evidence of it in ores containing (as one might expect) the related element, cesium. He named it virginium. Then followed the apparent discovery of element 85, which he named alabamine. The periodic table was duly revised in textbooks and classroom posters. Ironically, on this occasion, Robert Wood (Blondot's critic) summarized Allison's work favorably in the 1934 edition of his popular textbook, *Physical Optics*.

Many chemists replicated Allison's findings, and even announced the discovery of other new elements. However, sometimes they could later not repeat their own initial successes, or they encountered puzzling results. For example, Francis Slack observed the critical minima in arbitrary places on the scale, not just at the values determined earlier by others. Then, like Wood, he began to replace or remove various parts of the apparatus—with no apparent effect. He tried a constant source of illumination—which should have eliminated any punctuated delay—and found no change. Independently, two other labs also encountered many minima, randomly distributed. Both also introduced new methods for assessing the brightness of the light. They split the light beam from the original spark: one went through the samples, while the other served as a unmodified reference, for comparison (if the spark should flicker, say). They also used photographs or photometric technology to measure the difference in light intensity. Both labs documented variation in the intensity of the spark that could occasionally (but unpredictably) decrease the signal intensity and that, without the direct comparison, could easily have been mistaken as caused by the samples. Slack added how, during his own visit to Allison's lab, the assistant's informal chatter had included subtle indications about where he expected the critical minima. Those comments could easily coax an observer into how to interpret the very weak signals. Ultimately, the Allison magneto-optic method of analysis proved an error. Research on it was abandoned. It originated in a diabolical combination of random variability in the spark generator, faint light signals at the physiological limits of human vision, and the psychological power of theoretical expectations and of subtle cues that could unconsciously shape basic sensory perceptions. Under these conditions, the human observer was simply an unreliable instrument, and a source of observational error (Farwell & Hawkes, 1935; MacPherson, 1934; Slack 1935; Woodriff, 1935). In this case, Wood, was *not* the "rational skeptic." Rather, he ironically endorsed the phenomenon that later proved illusory.

Years later, Nobel Prize-winning chemist Irving Langmuir (1953, 1989) would characterize these two episodes (Blondot and Allison) as examples of "pathological science." He portrayed them as patently "bad science" — lapses of a skeptical attitude. He warned against the dangers of psychological bias in science: "wishful thinking," "fantastic theories," "subjective" phenomena, "ad hoc" excuses, and the like. Langmuir's verdict of "self-delusion" in such cases has been widely echoed by others (e.g., Dolby, 1996; Gaby, 2002; Gratzer, 2000; Rousseau, 1992; Turro, 1999). They follow the pattern of disparaging the scientists who erred as inherently less-than-competent individuals. For them, error inevitably results from "unscientific" attitudes, not just from bad luck or the vagaries of individual blind spots.

Ironically, Langmuir's own research would later exhibit the same sort of blind spot. He seemed to readily attribute error to others, while claiming error-correcting skills for himself. Yet he had his own Achilles heel, once featured on the cover of *Time* magazine (August 28, 1950): cloud-seeding. Ironically, Langmuir himself succumbed to the very same error he disparaged in others.

Langmuir certainly earned his reputation as a titan of research and development in chemistry and physics in the 1920s-40s. His achievements are numerous. He originally gained renown for his work on the chemistry of adsorption on surfaces — the occasion for the 1932 Nobel Prize in Physics. His extensive ongoing work in that field has been recognized with an eponymous unit of measure (in ultra-high vacuum physics) and the naming of the American Chemical Society's premier journal on such topics. His work led to the understanding of *Langmuir* waves and *Langmuir* probes (plasma physics), *Langmuir* circulation (ocean currents), *Langmuir*-Taylor detectors (ionizing beams), and many other eponymous phenomena and instruments (24 total are listed by Wikipedia). One might imagine that Langmuir developed a sense of self-confidence that his insights could help master any problem he encountered, even if venturing into a new field.

After World War II, Langmuir turned his attention from icing on the surface of airplane wings to the related problem of rain formation. His assistant, working with a home freezer in the lab, stumbled upon a role for dry ice. It dramatically turned condensed water vapor—an artificial cloud—into indoor snow. Soon, Langmuir was at a nearby airfield, sending up a plane to inject dry ice pellets into a nearby cloud to make it rain. Langmuir was impressed by the results, although professional meteorologists far less so. For example, the snow all evaporated before reaching the ground. Still, it made news headlines. Thinking again in terms of surface chemistry, Langmuir and his assistant began looking for a chemical with a crystalline structure similar to ice, which could serve as a seed for crystallizing water and thus coax clouds to release their stored moisture. That led to silver iodide as an alternative method for cloud seeding. More tests, more rain.

However, Langmuir had ambitious visions for controlling the weather. The following year he was working with the U.S. military in an effort to neutralize hurricanes. In October, 1947, with the help of a sturdy Air Force B-17 bomber, 180 pounds of dry ice were dumped at the edge of the eye of a hurricane heading out to sea. The hurricane veered and headed back to shore, stronger than ever. In retrospect, there was no firm evidence that the cloud-seeding had changed the course of nature. The weather system was too complex and Langmuir had not considered adequate controls to support any definitive conclusions. So, Langmuir was wrong. He had made some grandiose claims, but they far outstripped the available justification, both conceptually and experimentally.

Undeterred by the hurricane failure and other inherent uncertainties, Langmuir avidly *continued* to pursue his surface-chemistry perspective and his cloud-seeding research. A new endeavor, Project Cirrus, funded by the Army, moved to New Mexico, where the arid climate provided some hope, at least, of more controlled tests. In 1949, Langmuir observed one thunderstorm that he claimed he had initiated. Meteorologists later attributed it to a naturally occurring weather front that moved in from the Gulf of Mexico at the same time. Later, he claimed that his actions in New Mexico had changed rain patterns in Ohio several days later. Despite cautions from his colleagues, he published the statistics in *Science* magazine, hoping to claim credit for large scale influence of the weather. But others did not interpret the correlation as evidence of causation.

Research on cloud seeding waned over the next decade. Still, even until his death in 1957, Langmuir never wavered in his beliefs that the concrete promise of cloud-seeding, based on surface chemistry, was fully justified (Chew, 1987; Kean, 2017; Langmuir 1950, 1951). . Langmuir's source of insight was also, simultaneously, his blind spot.

Langmuir's story includes an ironic and cautionary epilog. In 1953 in a presentation at Princeton University, Langmuir proposed the notion of "pathological science." He did not neatly define the concept, but was clearly inspired by concern for fellow researchers who seemed to succumb unwittingly to motivated reasoning, rationalization, and other cognitive errors. "These are cases," Langmuir commented, "where there is no dishonesty involved but where people are tricked into false results by a lack of understanding about what human beings can do to themselves in the way of being led astray by subjective effects, wishful thinking or threshold interactions." "Criticisms," he continued, "are met by ad hoc excuses thought up on the spur of the moment. They always had an answer — always." Langmuir (1953) was clearly intrigued by the cognitive lapses: "To me, the thing is extremely interesting, that men, perfectly honest, enthusiastic over their work, can so completely fool themselves."

Sadly, of course, this syndrome of short-sightedness largely described his own work on cloud-seeding. His enthusiasm had always been ahead of the data. He inevitably highlighted favorable results, while peripheralizing apparent anomalies and ambiguous data. When others politely challenged his conclusions, he always found reasons to discount their criticism. For instance, he would appeal to laboratory models and data to justify his claims, even when real-world circumstances were far more complex. The alternative interpretations from professional meteorologists were merely plausible, not necessarily proven, he contended. And so on. Langmuir had a philosophical blind spot, as well as a scientific one.

Ultimately, this series of cases—cascading from Blondot (N-rays) and Allison (magneto-optic effect) to Wood (textbook) and Langmuir (cloud seeding)— helps demonstrate that error is not corrected by some scientist's superior objectivity or rigorous methodology, but by contingent, alternative perspectives. Insight on one occasion was coupled with blindness on another. The value of each perspective depended on the context of the case.

## *5. Managing Bias: Complementarity Perspectives and Reciprocal Criticism*

The conundrum of bias—that one and the same perspective can yield both insights and blind spots—means that striving to eliminate perspectival biases (construed as harmful) in order to reduce errors will not necessarily benefit science. Some "biases" can be fruitful. At the same time, particular biases may still introduce error. As noted earlier, the cost of discovery seems to be the risk of error. Scientists must nonetheless *manage* their perspectival biases both to identify errors as errors and to amplify overlooked insights.

The strategy is already familiar to social epistemologists. Namely, as described by Robert Merton (1973), the "growth of reliable knowledge" through science depends on "organized skepticism." That is, skepticism should be conceptualized at the *sociological* level: not in terms of solopsistic Cartesian doubt or blind critique (the conventional images, based on rationality as an individual quality). Rather, skepticism is manifested socially as the interactions between contrasting perspectives. Cognitive science certainly indicates that one cannot expect individuals to rigorously question their own work. That task falls on others. As Hull (1988) noted, under appropriate motivational conditions, individuals will challenge each other's claims and hold them to higher standards of proof than they might otherwise accept. Skepticism is best conceived as an emergent social phenomenon.

The conundrum of bias is accommodated through effective mutual criticism—Merton's "unending exchange of critical judgment." Yet it depends on capitalizing on different perspectives, or alternate biases. Social epistemologists in particular, inspired by feminist critiques, have underscored especially the role of *complementary* perspectives (Harding [1991] speaks in terms of contrasting "standpoints," while Solomon [2001] refers to the balance of cognitive "decision vectors"; and Haraway [1989] highlights the dialogic insights of "ironic diptychs”). For example, a community without the requisite sampling of perspectives can thereby succumb to *communal* confirmation bias or *communal* cultural bias: the absence of appropriate epistemic checks and balances.

Toward that end, many feminist philosophers and others have argued—through case studies—that the mere introduction of a complementary perspective seemed sufficient to resolve various instances of gender bias (e.g., Gilmer 1993; Haraway, 1989; Spanier 1995; Tang-Martínez, 2020). However (and this is precisely where history matters), the historical evidence in many such cases is incomplete. To isolate complementary perspectives as the relevant variable, appropriate controls are needed, to rule out other interpretations. For example, one may well conclude that the critic’s perspective was simply more fully informed, and thus epistemically privileged. While feminist philosophers of science have paraded case after case of egregious androcentric gender bias (as a source of error), they have been less forthcoming in describing cases of error based on gynocentric gender bias, portraying it instead as a “resource” (e.g., Anderson, 2020). Indeed, many case presentations leave one with the impression that the feminine (or gynocentric) perspective was *superior* to, *not merely complementary* to, the androcentric one. The implicit implication is often that the feminist critiques transcended and trumped the gender bias of the male researchers, rather than exhibit their own, alternate bias.

Feminist philosophers seem not to have invested substantial effort in documenting the deficits of feminine bias, nor the value of complementary androcentric-informed corrections to them. Such cases, however, would, ironically, help establish a symmetry and underscore the significance of complementary—not hierarchical—perspectival biases. For example, Rachel Carson’s arguments in *Silent Spring* were partly based on an unwarranted notion of the balance of nature, considered by some as a virtued gendered perspective, yet aptly criticized by male ecologists (Allchin 2014). Or: Margaret Mead’s anthropological work in Samoa (again, hailed for its gendered insights) was put in check by criticism from Derek Freeman (Shankman 2009). Such analyses apply as well to the philosophical interpretations of history. Evelyn Fox Keller’s claim that Barbara McClintock exemplified a special gendered “feeling for the organism” has been shown to exhibit casual disregard for historical facts and testimony (Comfort 2003). Coupling such cases to those of the feminist critiques helps clarify further that perspectival bias can yield *both* blind spots (error) and insights (in the form of error-correction) and that lateral complentarity is a key factor in resolving perspectival bias.

Epistemically, how do we know that one perspective, finding deficits in another, is not merely an inherently more informed, more comprehensive perspective? As in the cases just described, we must document functional symmetry, or reciprocity. In this section, therefore, I present cases where pairs of scientists exhibited complementary blind spots and insights—leading (perhaps ironically) to remedying *each other’s* errors. Each was susceptible to error and, at the same time, each had the potential to reveal errors in the other’s view. This reciprocal relationship underscores the role of complementarity, not any methodologically superior position.

*Debates as Mutual Criticisms, not Either-or Competition*

Scientists with corresponding insights and blind spots can correct the other. This form of checks and balances is especially striking, of course, when the investigators posture themselves as mutually exclusive rivals or lay stake to what are considered to be incompatible claims.

For example, consider the 1908 Nobel Prize in Physiology or Medicine, which was shared for a pair of related discoveries in immunology. Paul Ehrlich had characterized various immune reactions — agglutination, bacteriolysis (via complement) and hemolysis — all chemical in nature. His work embodied the then popular approach based on blood chemistry. At the same time, Ehrlich denigrated cell-oriented perspectives as misguided—his blind spot. He erroneously excluded any role for white blood cells engulfing waste, say, or for immune action mediated by what we now know as T-cells. Ironically, such processes had already been observed and investigated by Ilya (Elie) Metchnikov — who received the other half of the prize. Metchnikov was a champion of the cell-oriented approach that showed the importance of various white blood cells and phagocytes, as the Nobel Committee acknowledged. But Metchnikov, for his part, dismissed the relevance of perspectives based on chemical elements carried in the blood — his complementary blind spot. Both immunological mechanisms — humoral and cellular — are now recognized as important and functionally integrated. Historically, the Nobel Prize committee seemed to conspicuously recognize the respective insights and their respective erroneous overgeneralizations (adapted from Author; Bibel, 1988;  Magner, 2002, 278-285; Silverstein, 1989).

Consider, next, the famous Volta-Galvani debate on “animal electricity” (Kipnis, 1987; Piccolino, 1998). Luigi Galvani observed the twitching of a freshly dissected frog leg stretched between two different metals. As an anatomist, he interpreted it in terms of an “animal electricity.” Alessandro Volta, a physicist, was skeptical of that vitalist notion. He showed that the electric current could be produced with the metals alone, thereby dispensing with the need to appeal to any special organic power or energy—Galvani’s blind spot, perhaps. But in focusing on the physical phenomenon only, Volta simultaneously failed to explain what caused the frog muscle to contract or how nerves worked. That was his blind spot. Galvani was justified in concluding that the movement of the frog’s leg was more than just electricity in action. Electricity was only a stimulus. That implied that nerves must have an electrical nature, although not necessarily by virtue of metals, as in a Voltaic pile, or battery. It would be decades before biologists could articulate what that meant at a cellular level, however. Contrary to Volta’s claims, organisms are more than “mere” physical machines. Two complementary insights, two complementary blind spots.

A similar story can be told for the Great Devonian Controversy in early 19th-century geology (Rudwick, 1985). Henry de la Beche initially reported his very basic field observations about the geological strata in Devon, England. His sequence, however, implied that fossils (from the layer known as the “Coal Measures”) were older than any known previously. That was a significant insight. However, de la Beche was apparently blind to the large scale theoretical implications. For Roderick Murchison, the presence of fossils that old was strictly not possible, based on his own observations of strata of a corresponding position and age. So (he claimed) de la Beche must have been incompetent and made a cardinal error in basic stratigraphy in his elementary observations. At the same time, Murchison had proposed his sharp historical boundary on somewhat arbitrary foundations. That was his blind spot, exposed by de la Beche's mundane field work. Veteran geologist William Buckland was able to reconcile the two views by interposing a new geological period, the Devonian. The fossils were old, yes, but not quite so old as to violate Murchison's views on geological history. Again, complementary insights and blind spots, accommodated in a synthesis.

In these three cases, errors emerged from perspectival blind spots. Error correction did not arise from inherently superior perspectives or deeper objectivity, but through criticism from particular perspectives that compensated for those blind spots, while exhibiting their own biases and errors. Again, the details of historical cases give essential reality to the philosophical musings, which are too vague and suggestive by themselves.

### *Charles Darwin and Alfred Russel Wallace on Human Evolution*

For a more nuanced case, we may turn to the cofounders of the theory of evolution by natural selection: Charles Darwin and Alfred Russel Wallace (Author). Both endorsed the evolution of humans from primates, but whereas Darwin gave it a fully naturalistic explanation, Wallace saw a spiritual element. On the other hand, Darwin's view had racist overtones, which Wallace starkly rejected.

Darwin's view was itself an ironic combination of insight and blind spot, all at once. Darwin reasoned about human descent from his gradualistic perspective, but also in the context of his social status. British society was stratified, and Darwin enjoyed membership in the upper class. He was also a white European at a time when Europeans (notably the British) dominated the globe. This context shaped perceptions of other races, easily construed in a hierarchy. While voyaging on the *Beagle*, for example, Darwin was appalled by the habits of the natives of Tierra del Fuego:

> It was without exception the most curious and interesting spectacle I ever beheld: I could not have believed how wide was the difference between savage and civilized man: it is greater than between a wild and domesticated animal, inasmuch as in man there is a greater power of improvement. (Darwin, 1845, 218)

Improvement there was. One of the Fuegians had been taken to London, educated, and entered into elite society. When he returned, however, he seemed content to revert (as Darwin saw it) to his "primitive" habits. It was all too easy for Darwin to consider racial differences as innate and to rank them on a scale from "savage" to "civilized." That conception proved both fruitful and dramatically misleading.

When Darwin began considering human ancestry, he saw immediately that the problem was not primarily anatomical. Humans had long been classified as primates. The challenge was accounting for the origin of mental faculties and moral sensibilities. Darwin's early musings turned to the Fuegian episode. He wrote to himself in the fall of 1838:

> Nearly all will exclaim, your arguments are good but look at the immense difference. between man, —forget the use of language, & judge only by what you see. compare, the Fuegian & Ourang & outang, & dare to say difference so great . . . "Ay Sir there is much in analogy, we never find out." (*M Notebook*, 153)

Darwin essentially cast the Fuegians as intermediates between orangutans and "fully developed" humans, such as himself and his elite British peers. That is, Darwin's view of stratified races facilitated his conception of a transition, linking apes and humans through a series of gradual changes. "Savages" became convenient transitional forms in moral and mental development. That is, Darwin arrived at his insight of human evolution in part because of his cultural view of human races (Browne, 1995, 234-253, 382-383; Herbert, 1974/1977).

The relevance of Darwin's social class in shaping this conclusion comes into relief by comparing him with Wallace. Wallace, by contrast, came from the middle class and worked to earn his living. While collecting in the Malay archipelago, he depended heavily on the local natives. He learned to respect their knowledge and willingness to help. In 1855 he wrote to a friend: "The more I see of uncivilized people, the better I think of human nature and the essential differences between civilized and savage men seem to disappear." For Wallace, if even such "brutes" could show kindness, then all humans seemed to share the same moral sense. He echoed his sentiments in 1873: "We find many broad statements as to the low state of morality and of intellect in all prehistoric men, which facts hardly warrant" (Eiseley, 1961, 303). Wallace, in contrast to Darwin, experienced moral and mental discontinuity between man and beast. Wallace certainly acknowledged that humans had primate ancestry — anatomically. Still, he maintained that the human mind was unique. The human brain, he contended, emerged by some guided process, *not* by natural selection.

Wallace erred in viewing human minds as exceptions to evolution. He never considered, as Darwin did, that morality itself might evolve. At the same time, however, Wallace's views, arising from his social status, helped expose Darwin's unfortunate bias of ranking races hierarchically. Insight here, blind spot there, neatly fitting together as a pair.

Philosophically, cultural context should not really matter to science, perhaps. But here we see vividly how such contexts do matter. Cultural perspectives can foster errors, but also—counterintuitively, perhaps—they can also help remedy such errors. Such intereactions would be hard to predict based on abstract principles, but they can surely be characterized by analyzing the history in philosophical terms.

### *Peter Mitchell and Paul Boyer on Cell Energetics*

My final example involves another pair of Nobel Prize winners whose insights accommodated each other's blind spots—here, on deciphering how cells process energy (adapted from Author; see also Prebble & Weber, 2003).

The first error was made by Paul Boyer in 1963. In the 1950s and 60s biochemists were looking for a set of high-energy molecules that transferred energy from the electron transport chain to the final energy molecule (ATP). After a decade of failed claims from several labs, Boyer employed a technically demanding method using a radioactive tracer. He thereby isolated an intermediate and identified it as phosphohistidine. He reported the apparent triumph in *Science*. Relief rippled through the community. The high profile discovery soon fizzled out, however. When additional controls were applied, Boyer's lab attributed the results to other energy reactions in the cell. A discovery on its own, but not the imagined discovery: a framing error. "I was wrong," Boyer frankly admitted later.

Boyer was actually wrong on two levels at once. Phosphohistidine was not the intermediate. Boyer acknowledged as much. But the very concept of the intermediates, for which everyone had been searching so earnestly, was also mistaken. Boyer soon reached that conclusion, as well. (If he hadn't found the intermediate using his methods, he boldly believed, no one else would.) Boyer hypothesized instead that the energy must be transferred through energized changes in protein shape (like a pair of interacting molecular springs): a confomational perspective, replacing a chemical one. This concept, too, would eventually prove mistaken, at least at this level.

Here, the unexpected solution was introduced by Peter Mitchell. Mitchell was guided in his thinking by a novel principle that he developed while thinking about the transport of ions and substrates across cell membranes. In his conception, chemistry was vectorial, not just scalar. That is, enzymatic reactions included an important spatial dimension. For example, an "ordinary" reaction might span a cell membrane, with reactants on one side and products on the other. The movement of molecular components would be an integral part of the reaction, not recorded in conventional equations of an ordinary chemical perspective. Drawing on a collage of findings, Mitchell conceptualized the intermediate energy state as a proton gradient across a membrane: a novel chemiosmotic perspedctive. That revolutionary idea ultimately earned Mitchell a Nobel Prize in 1978. Mitchell had solved Boyer's error, in a sense.

Mitchell's own claims, however, were hardly free from error. In the first formulation of the theory, for example, the direction of the gradient across the membrane was completely reversed! Mitchell also specified one proton as adequate, when two were needed. Such "minor" errors were soon remedied. But they damaged the credibility of Mitchell's perspective.

Most dramatically, Mitchell had a vision about how ATP was energized from the proton gradient. Using his foundational perspective of vectorial chemistry, he insisted that protons flowed into the interior of the ATP enzyme and there participated directly in forming the high-energy phosphate of ATP. That concept was creative, but never fit comfortably with the data. Here, it was Boyer's concept, rather, that prevailed. Boyer had adapted his ideas on the role of protein shape. He reasoned how ATP formed on the surface of the enzyme, and was then released through an energy-requiring change in the enzyme's shape. The energy was provided by protons, now in accord with Mitchell's overall scheme. The ATP enzyme worked like a miniature molecular motor. Boyer's insights were recognized in a 1997 Nobel Prize. So, on this occasion, Boyer solved Mitchell's error.

Ultimately, Boyer and Mitchell had both been right (on different occasions). And both had been wrong (again, in separate cases). Their insights, arising from separate backgrounds and orientations, neatly complemented each other's blind spots.

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In summary, this handful of cases — Metchnikov and Ehrlich, Galvani and Volta, de la Beche and Murchison, Darwin and Wallace, Boyer and Mitchell — illustrates how the perspectival biases of different scientists can complement each other. One person's blind spot may be the occasion for someone else's insight. *And vice versa*.

## *6. Conclusion*

Many of the themes I have presented here are familiar: the negative role of bias, the constructive role of bias, and the general dynamics of social epistemology. What has been missing in earlier discussions, however, has been a historically well informed philosophical analysis of the intimate link between insights and blind spots in science. Here, I have shown how they can emerge from the very same perspective, or bias. I have further characterized skepticism as a social-level concept: manifested in the critical dialogue of contrasting interpretations. In accord with that view, I have highlighted the role of complementary perspectives (not merely alternative, deeper, or more comprehensive perspectives) in exposing bias, profiling critical missing evidence, and thus remedying errors arising from perspectival bias. I hope to have also underscored that we should not seek to eliminate this form of bias from science, not if we want to foster discovery. But it comes with consequences. The cost of discovery is the risk of error. That is, in essence, the conundrum of bias.

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