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# Why Respect for History – and Historical Error – Matters

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**Abstract.** This paper addersses Lawson's puzzlement about the absence of prediction in William Harvey's and Marcello Malpighi's views on capillaries. In addressing the context of that enquiry, it also addresses historiographic versus philosophical models of science, contexts of discovery versus justification, normative versus descriptive interpretations of the nature of science, and the role of simple dichotomies in interpreting scientific methodology – and how all these distinctions may inform science educators.

**Key words:** historiography, context of discovery, context of justification, nature of science

*Nothing's concluded until error's excluded.*

—Proverb

Allchin (2004a) earlier analyzed pseudohistory as pseudoscience, profiling the need for science educators to respect history and responsible historical interpretations in portraying the nature of science. Lawson (2004) has implicitly endorsed this principle by seeking clarity and certainty about several historical facts introduced there. Respecting Lawson's critical eye as an occasion to probe the historical claims more closely for possible error, I here provide a deeper analysis, including fuller documentation. Given a shared respect for history, one can look forward to applying rigorous principles of historical documentation and of robust interpretation from multiple sources. We will not be misled, as a hasty scholar might easily be, by fragmentary quotes, quotes out of context, unreliable secondary sources or hearsay.

In his charitable and thoughtful comments on my work, Lawson (2003, 2004) also seemed quite concerned about establishing the hypothetico-predictive method as an 'essential' – by which, I presume, he means universal – method of science. Of course, my paper was primarily historiographic, not philosophical. My aim was far less grand than Lawson generously suggests. I reported only on specific historical cases discussed earlier by Lawson himself – and, of course, whether some particular overarching theory was commensurate with them. My concerns, then, are about representing history faithfully, drawing from it only conclusions that are warranted by a complete analysis and, finally, using history responsibly in educational settings to profile the nature of science as it is or was (not just as one might *propose* it *ought* to be). Here, I continue my role primarily as a historian, while still heeding important developments in the philosophy of science.

## 1. Harvey and Capillaries

Earlier, I claimed that 'Harvey did not predict capillaries' (Allchin 2004a, p. 182). This historical claim seems to have been contentious. But it is hardly novel or unique to me. I echoed respected historians Yehuda Elkana and June Goodfield (1968).[1] Now, educators may well feel inclined to dismiss this historical squabble as minor and arcane. Yet, delving into the details (as I will show) leads to central lessons about how history is used (or misused) to portray the nature of science.

Did Harvey predict capillaries? Well, the term 'predict', perhaps, may be ambiguous. I follow popular meanings (as clearly indicated by earlier usage and context): that Harvey deduced and believed in the existence of capillaries based on theoretical considerations, even though they were not observable. One may articulate this prediction in a format explicitly dictated by Lawson himself (compare with Lawson 2000, p. 484):

> ***If*** ... the blood flows away from the heart in the arteries, and
>
> ***If*** ... the bloods flows towards the heart in the veins,
>
> ***Then*** ... the arteries and the veins *must* be connected by unseen capillaries. (Allchin 2004a, p. 181)

In this widespread formulation, Harvey would seem to have left a prediction, or (in Popperian mode) a bold conjecture, as an unfinished task that, in many popular histories of science, researchers could later confirm, albeit ironically after his death. These elements have profound rhetorical effect in popular stories: specifically, they convey the power of theoretical deduction over unschooled observation, with the dramatic tension between vulnerable prediction and later confirmation providing a vivid affective reinforcement of vindication. Without these overtones, the notion of prediction in science would not carry such authoritative weight. Predictions are presumably different from 'guesses' or 'possibilities' in (a) expressing beliefs and (b) being partly justified (usually 'deduced' from a general law) relative to possible alternatives. Thus, 'Harvey did not predict capillaries' means:

(1) While Harvey (like his predecessors) may have imagined the possibility of capillaries, he did not view (or claim) their existence as a necessary part of his theory of circulation. [prediction as a concept deduced from first principles]
(2) Harvey did not believe or claim that capillaries existed, despite his failure to observe them directly. [prediction as an observable phenomenon inferred independently from theoretical principles]
(3) Harvey did not claim that capillaries would be found, nor leave explicit hints or clues leading later researchers to expect them or guiding them to find them. [prediction as anticipated finding or result, or falsifiable forecast]

I believe this fairly reflects the sense, in which claims about Harvey's predictions also appeared in Lawson (2000), Lewis (1988), Elkana and Goodfield (1968), several prominent websites (Allchin 2005), and elsewhere. This details the concrete meaning of my historical claim.

Let us consider each claim in turn (examining each dimension of meaning in 'prediction'). First, Lawson contends strongly that Harvey imagined the possibility of connections between the blood vessels (2004, pp. 600–601) – and I agree. So did Erisastratus. So did Galen. So what? Merely speculating about capillaries was certainly not implied by my (or other's) stronger use of the term 'prediction' (as in 'a concept entailed by circulation'), and so this small fact is irrelvant to my claims. Moreover, capillaries had been envisioned *without* Harvey's concept of circulation as a hypothesis, so his mention of the possibility is neither novel, unique nor special. Still, *Galen's* earlier view of anastamoses did prove significant for Harvey (more below). Lawson seems to present a speculative possibility as a 'prediction'. If it is indeed a 'prediction', it is a prediction in a very weak sense. It is not specific. It is not definite or concrete. It is not explanatory. It certainly bears no justificatory weight. It is, rather, an alternative guess: an imaginative possibility. Yet the 'prediction' in this odd sense seems crucial to Lawson – and I will return to it below in the discussion of 'Knowing Where to Look'.

Nevertheless, Lawson (following Lewis 1988), earlier presented blood flow through capillaries as a *postulate*, not merely a casual or vague prediction, of Harvey's theory of circulation.[2]:

> Postulates of William Harvey's Blood Circulation Theory ...
>
> 4. From the arteries' smallest branches, blood flows through tiny unseen vessels (capillaries) into the smallest veins. (2000, pp. 483, 484) blood circulates by passing from arteries to veins through tiny vessels (William Harvey's circulation theory) (Lawson 2002, p. 16)

In my earlier work, I articulated Harvey's view to the contrary, paradoxically opposite to our modern one: that the tissue of the legs and body is porous, like a sponge, and that blood percolates through it. I presented several references to this idea, but I was surely remiss – given Lawson's avid interest in historical accuracy – in using only paraphrases and isolated terms without providing all the explicit quotes or page references. I provide them here in Table I (further explicit passages from Harvey's correspondence are cited in Elkana & Goodfield 1968). Note how Harvey's statements are *repeated*, scattered *through* his 1628 publication *and echoed again* many years later. Note, too, the *variety of images and phrases* he uses to express the same idea: sponges, pores, springs and rivulets. The repetition and diversity of expression both provide *robust* support for interpreting Harvey's view and its consistency. The historical evidence leads us to conclude (*contra* Lawson 2000, 2002) that Harvey did not rely conceptually on capillaries to complete the circulation.

Second, did Harvey nevertheless believe in capillaries, yet to be observed? Lawson and I seem to agree that Harvey did not *observe* capillaries. In a very explicit passage in his *Second Disquisition to Jean Riolan* (following many years after *De motu cordis*), Harvey reports:

> I myself have pursued this subject of the anastomosis with all the diligence I could command, and have given not a little both of time and labour to the inquiry; but I have never succeeded in tracing any connexion between the arteries and veins by a direct anastomosis of the orifices.
>
> ... by boiling, I have rendered the whole parenchyma of these organs [liver, lungs, spleen and kidneys] so friable that it could be shaken like dust from the fibres, or picked away with a needle, until I could trace the fibres of every subdivision, and see every capillary filament distinctly. I can therefore boldy affirm that there is neither anastomoses of the vena portae with the Cava, or the arteries with the veins, or of the capillary ramification of the biliary ducts, which can be traced through the entire liver, with the veins. (1649a, p. 311)

Harvey did use the word capillary, but clearly to mean only a very fine vessel, or 'filament'. He expressed his concern instead in terms of anastamoses, or direct meetings of the arteries and veins that would close the vesicular circuit.

Having failed to find capillaries, did Harvey believe, on the conviction of theoretical deduction, that they nonetheless existed? This is the crux of the 'prediction' in popular accounts. Lawson provides no document on this score. He shows Harvey rehearsing the various possibilities for his reader. But his *one* quote (Lawson 2004, p. 600) does not show Harvey's belief. Rather, Harvey merely introduces the topic gently to his reader. By contrast, in the passage quoted above Harvey uses the phrase 'boldly affirm', which conveys a strong sense in which Harvey, trusting his ocular demonstration – as he did in so many other cases – not only did not observe anastamoses, but also did not believe they existed.

*Table I.* Some of Harvey's references to blood flow between arteries and veins

| Quote | Reference |
| --- | --- |
| 'The blood percolates the substance of the lungs from the right ventricle of the heart into the pulmonary veins and left ventricle' | 1628, Ch. 7 [chapter title], p. 283 |
| 'That this is possible, and that there is nothing to prevent it from being so, appears when we reflect on the way in which water percolating the earth produces springs and rivulets ...' | 1628, Ch. 7, p. 283 |
| 'The lungs, again, are of a much looser texture, and if compared with the kidneys are absolutely spongy .... in the lungs the blood is forced on by the pulse of the right ventricle, the necessary effect of whose impulse is the distension of the vessels and pores of the lungs.' | 1628, Ch. 7, p. 283 |
| 'And then the lungs, in respiration, are perpetually rising and falling; motions, the effect of which must needs be to open and shut the pores and vessels, precisely as in the case of a sponge ...' | 1628, Ch. 7, p. 283 |
| 'Finally, our position that the blood is continually passing from the right to the left ventricle, from the vena cava into the aorta, through the porous structure of the lungs.' | 1628, Ch. 7, p. 284 |
| The blood 'is sent for distribution to all parts of the body, where it makes its way into the veins and pores of the flesh' | 1628, Ch. 14, p. 296 |
| The blood 'is forced from the capillary veins into the smaller ramifications, and from these into the larger trunks by the motion of the extremities and the compression of the muscles generally' [explaining how the blood is propelled from the periphery to the center – that is, not from blood pressure from the heart's contraction through capillaries] | 1628, Ch. 14, p. 297 |
| The blood, '... that which is contained in the pores or interstices is urged into the smaller veins, from which it passes into the larger vessels' | 1649a, p. 308 |
| 'And then it seems only reasonable to think that the blood in its circuit passes more slowly through the kidneys than through the substance of the heart; more swiftly through the liver than through the kidneys; through the spleen more quickly that through the lungs, and through the lungs more speedily than through any of the other viscera, or the muscles, in proportion always to the denseness or sponginess of the tissue of each,' | 1649b, p. 322 |

Still, Lawson contends, did not Harvey's *search* for capillaries indicate an implicit belief in their existence? No. Indeed, Harvey argued *against* anastomoses (Elkana & Goodfield 1968). Galen had argued that blood could pass *from* the veins (where it was produced) *to* the arteries (which, Galen showed, had been misnamed 'air-ducts'. *Galen* thus believed in anastamoses. Harvey argued, rather, for one-way flow in the other direction, *from* arteries *to* veins. He was thus conceptually predisposed to accept his plain observations. It may seem counterintuitive to us, perhaps, that Harvey's investigation may have been motivated in part to *deny* the existence of anastamoses – today's capillaries. Here, approaching history with a strong prediction or expectation may blind one to such fascinating ironies. Did Harvey implicitly *'predict'* capillaries in denying them? Only if one uses the term 'predict' in an extraordinarily weak sense, as noted above: no more than a hint of a possibility. Such a prediction can be neither confirmed nor rejected in the context of a 'planned test', because there is no concrete claim that 'capillaries should eventually be seen' (Lawson 2000, p. 484).

Third, Harvey – apparently quite convinced that anastamoses did not exist – left no entreaty or tantalizing hint to others to continue the search. Lawson presents no such explicit passage from a published work or correspondence. Hence (contingent on unmentioned documents), the evidence does not support the claim that Harvey forecast concretely the eventual observation of capillaries. If others saw in Harvey such a prediction, it would have been based more on their interpretation than on Harvey's intent or language.

Many modern commentators consider the evidence for capillaries as crucially important to Harvey's theory of circulation – that it could not be fully confirmed or accepted until they were observed (whether predicted or not). One may puzzle, then, why Harvey's contemporaries – in particular, his critics – did not regard the status of anastamoses as a significant deficit or threat to his theory (Gregory 2001). They did not see it as crucial because no doubt, like Harvey, they could envision circulation without capillaries as well as capillaries without circulation. They are not logically related. Harvey did have many critics, and someone interested in the nature of science might well investigate and diagnose their reasoning and evidence, rather than assume (based on the *modern* outcome) that they must have been methodologically misguided. The historical response to Harvey's theory provides further context for interpreting science in a perspective very different from our own.

Given Lawson's own respect for history, he will surely honor the testimony of the historical documents: Harvey did not claim that capillaries completed the blood circuit by joining the arteries and veins, as we now claim. He did not even imply that anastamoses were there, although unseen (that is, he did not deny or question his observations based on theoretical deductions about capillaries). He did not leave an unfinished 'planned test' for subsequent investigators (with better instruments, perhaps) to conduct. For Harvey, the matter was settled: there are no capillaries. If there are other relevant historical documents or quotations, I trust Lawson will bring them to our attention.

## 2. Malpighi and Capillaries

Regarding the second claim that Malpighi did not discovery capillaries as a direct result of searching for closure to Harvey's circulation, there is little to add. *All* of Lawson's substantive claims rely solely on an outdated popular source, with a patchwork of quotes lacking both context and full documentation. Consulting Doby (1963), one finds a heroic style that exhibits many telltale warning signs of Whiggish pseudohistory (Allchin 2003a, 2004a, p. 193):

Romanticism
Flawless personalities
Monumental, single-handed discoveries
'Eureka'-type insight
Sense of the inevitable (plot trajectory)
Absence of any error
Unproblematic interpretation of evidence
General oversimplification or idealization
No cultural or social setting
No human contingency
No alternative ideas

Lawson's claims conflict with the primary sources already cited and no reasons are offered to doubt them. Instead, Lawson acknowledges his conditional debt – '*if* Doby is correct' – and then declares, 'Of course, I believe Doby is right' (p. 602). Rhetorically, Lawson's argument relies on repetition and appeal to authority, rather than additional depth or rigor of evidence. The interested reader may consult Malpighi's original epistle (in translation) (Malpighi [1661] 1929). The relevant investigations by Malpighi *on the structure of the lungs* are described in several months of correspondence between Malpighi and his mentor, Alfonso Borelli, showing how observations of blood vessels grew out of finding that air sacs in the lungs were closed. A detailed (and fascinating) account is provided by Adelmann (1966, I, pp. 171–198), who reviews the developing discourse *letter by letter*.

Lawson suggests that Malpighi would have read Harvey, and I suspect that he probably did. As I have noted, Harvey made no explicit predictions to guide later anatomists. Malpighi's efforts on his behalf would be secondary, if demonstrably explicit. I invite Lawson to present *concrete documentary evidence* of this, should it be available to him. Surely, readers of this journal are entitled to accurate information, including excerpts from and citations to *original sources* where appropriate.[3]

### 3. 'Knowing Where to Look'

So, why all the historical fussing? Is it to ascertain "the" nature of science? Not for me, at least. My papers were about the appropriate use – and misuse—of history in science education. My topics were: false history, pseudohistory, appropriate history, *approrpriated* history, history of pseudoscience, and shoehorning history into particular philosophical conceptions of science. My papers were not about the nature of science at all, but about *how history is used* – in this case, *for the purpose of advancing for educators a claim about* the nature of science. The historical errors are, in many ways, incidental. They would be trivial and inconsequential were it not for how the errors are used in a larger context. False history is not always ideologically laden pseudohistory. Getting the history "right" is just the baseline reference for analyzing *how*, and thus *why*, someone seems to get it "wrong". Lawson's historical errors are not uncommon. Yet errors – especiallly for so careful and widely regarded a scholar as Lawson – can be valuable clues (puzzles, perhaps) for probing potentially deeper flaws in our thinking. Lawson's supplemental commentaries (2003, 2004) offer further clues about how and why the errors about Harvey and Malpighi emerged. They hold potentially powerful lessons for science educators.

What led (causally) to Lawson's errors? Even without a hypothesis, one might search his texts for clues. Historians are trained to read historical texts 'sympathetically' – trying to interpret and make sense of an author's claims, however strange or alien (or "wrong"!) they may seem to the modern reader. Let us adopt this strategy now – acknowledging the risk of error – in trying to interpret Lawson's claims, motives, interests and intellectual context.

Lawson's strident tone certainly suggests to the naive reader the author's passionate belief. Yet, the tone becomes especially acute in particular criticisms. Lawson's bête noire would seem to be the inductivist (read 'naive enumerative/Baconian inductivist', *not* the analogist or statistical thinker). The inductivist's fatal flaw, as Lawson repeatedly presents it, is that without a hypothesis, 'he will not know where to look', nor 'what to look for'

(2002, pp. 17–18; 2003, pp. 334–335; 2004, pp. 601, 603–604). Lawson raises a fine question indeed: how *do* scientists know where to look? What can profitably guide search, perhaps even when investigators cannot anticipate particular fiindings? Enter the HP "method". This helps solve Lawson's problem by showing how to generate a prediction that informs the investigator just where to look. In the traditional HD method, as classically articulated by Hempel (1966) (whom Lawson presents as an authority), one predicts an observation using a nominological deduction, applying a generalized law (hypothesis) to a particular case. The deduction specifies *what* to look for. It sketches the 'planned test'. Of course, specifying *what* to look for is subtly different than specifying *where* to look or *how to observe it*. Is it possible to isolate more clearly the role of a hypothesis in 'knowing where to look'?

Consider the historical case of Harvey again. Harvey had demonstrated that blood flows out through the arteries and back through the veins. But, now suppose that *he has no causal hypothesis*. He cannot guess how blood flows between them. Is he helpless? Does he not know where to look? Would looking between the arteries and veins really betray a clandestine causal hypothesis, as required by Lawson's ***if-then*** scheme? I leave the reader to puzzle this out.

Consider another case, also discussed at length by Lawson (2002): Galileo's discovery of the moons of Jupiter. Here, the traditional HD format might frame a profound prediction, based on a Copernican view:

***If*** ... the Earth is a planet orbiting the Sun [Copernican theory],

***and*** ... it also has its own orbiting Moon [uncontroversial observation],

***then*** ... other Planets may have their own orbiting moons [prediction].

This certainly resembles the reasoning that Galileo would *later* use to frame the significance of Jupiter's moon and so try argue for Copernicanism. Did such a prediction actually guide Galileo by informing him 'where to look'? Did it help him conceive a 'planned test' and then build a telescope to find the answer? While this plausible line of reasoning is easily reconstructed in retrospect, there is no historical evidence that Galileo developed such an explicit prediction and then built the telescope and then directed it at each Planet in turn specifically to search for the predicted moons. Lawson seems to 'readily acknowledge this' in ad hoc retrospect (2003, p. 336). That is, we should adopt a common sense interpretation of how Galileo found new astronomical bodies near Jupiter and then watched them change position on successive nights. With no hypothesis, then, *how did Galileo 'know where to look' to discover Jupiter's moons?* Lawson's account of this episode is incomplete here just where he indicates it is most important.

Lawson's position about 'knowing where to look' may seem quite paradoxical, if not equivocal, even to the sympathetic reader. At one point he asserts that 'scientists [armed with predictions] are not randomly exploring nature with some dim hope of finding something of value' (2004, p. 600). Yet, only a page away he contends that 'while exploring nature (explorations that may resemble random walks) scientists sometimes make puzzling observations' (p. 599). So, sometimes scientists' work is random, sometimes not. This would hardly seem to help the student as a would-be scientist any more than a hearty 'good luck', which Lawson explicitly disdains elsewhere (p. 604). How can the reader resolve this?

Randomness, in the sense of *not* 'knowing where to look', does seem to function productively for Lawson in one particular context: namely, *in yielding puzzling observations*. Lawson acknowledges that puzzling observations are integral to science. Indeed, they seem to be where it all begins. Puzzling observations seem far from peripheral. They are foundational. Yet Lawson gives no account of puzzling observations, especially in his cognitive/neurological models. Echoing Lawson's key question for inductivists: *without a hypothesis, how can a scientist 'know where to look' ... for puzzling observations?* How can they know 'what to look for'? Here, ironically perhaps, Lawson leaves the scientist with no more methodological guidance than 'blind search and good fortune', or a 'random walk'. Lawson's interpretation of scientific method is decidedly incomplete without a fuller account of puzzling observations. What makes one observation puzzling in contrast to another? Why are some scientists puzzled, while others are not? Methodologically, are scientists allowed to make puzzling observations while conducting a 'planned test', or is that a violation of the norm of *prediction-oriented* observation? Is encountering a puzzle accidentally while testing really any different than relying on chance or random observations without tests, only hunches? If a scientist has no puzzling observation to begin with, what then? Does science halt? Are there no 'methods' for generating puzzling observations, cousins of methods for generating alternative hypotheses? (Indeed, what are the methods for generating alternative hypotheses? Can one specify creativity in a list of steps as one does for HP reasoning? How does analogy work, for example, especially according to neurological models? Without a hypothesis already in place, how does one know what to make an analogy with? How does one identify a fruitful basis for analogical similarity? Given an unlimited capacity for similarity and analogy, how does a scientist know which to pursue? Isn't this just as problematic as 'knowing where to look' or 'what to look for'?) On all these questions relevant to understanding the process and practice of science, especially its imaginative, creative elements, Lawson provides no more than a promissory note. One might expect, then, that

Lawson might respect, rather than criticize, efforts to address this critical deficit of his theory. Historians, of course, consider *all* of the scientist's relevant thinking as part of the process. They document the many contingencies involved in shaping what individuals think. Historians do not prejudicially exclude a role for happenstance or randomness in science, if the historical evidence leads them there. For Lawson, the problem of 'knowing where to look' is solved by a hypothesis. But, a hypothesis seems to rely on both a prior puzzling observation and a process for generating hypotheses, both of which remain unexplained and apparently beyond or without strict method.

A prediction or expectation may sometimes serve scientists – or historians – in suggesting 'where to look' and by guiding them to new observations. But, it may also cripple them. In a common saying, you see only what you want to see. Or, as a philosopher of science (aware of theory-laden observations) may express it with a dash of humor: 'I'll *see* it when I *believe* it'. Directed observation leaves blindspots. And blindspots are opportunities ripe for error. Historians thus approach their field cautiously, well aware of the cognitive and psychological pitfalls of bias and of missing potentially relevant historical information. As observers, humans are prone to errors. Indeed, *the* primary source of error is probably our ability to filter perceptions and selectively shape the interpretation of sense data according to prior schema (e.g., Brush 2004, p. 199, n.3; Sunderland 1992; Glovich l991). Hypotheses and predictions are powerful cognitive agents, not just in seeing things, but also in *not seeing* other things that are plainly there to be seen. Historians have richly documented how scientists in the past have succumbed to such errors [see crude but synoptic surveys by Youngston (1998) and Gratzer (2000)]. 'Knowing where to look' *also* seems to guide researchers away from places where they may profit from looking. Yet historians see how happenstance and new contexts wake them occasionally from their conceptual habituation. A normative system or model of investigation that relied too heavily on prediction would also be one that institutionalizes disposition for error. The scientist who takes the shortest route between a puzzling observation and a consistent explanation – and stops – can never be secure about which belief is ultimately reliable, which unreliable. And this, I am guessing, matters very much to thinkers like Lawson. So it is striking, given Lawson's own interest in cognitive mechanisms (2002, 2003, pp. 331–333), that he omits discussion of these widespread and fundamental flaws in our ability to think effectively.[4]

How might this matter more broadly? Consider the question, posed internationally late in 2002: did Iraq possess weapons of mass destruction (WMD)? The President of the United States predicted 'yes.' In retrospect (we are privileged to know), the answer was ‘no’. The evidence was thin and yet the U.S. Administration presented it as support for their theory of WMDs, because it matched ‘what they were looking for’. Critics and skeptics were brushed aside: an HP-style thinker doesn’t need critics, only confirmation. The U.S. Administration thereby claimed a war was justified, although it was based on faulty evidential reasoning and despite error-analytical criticism by the world community. I repeat:

> History is valuable, rather, for showing students how they might *challenge* the “obvious”. Educators may help them probe evidential claims and show them how historically, with further evidence, later scientists found them ultimately to be without merit. Indeed, the very understanding that something may appear reasonable until it is considered more deeply, is a powerful lesson worth offering to anyone. (Allchin 2004a, p. 191)

That is why historical error matters: it models for students how even our scientific thinking can go awry, even more when relying exclusively on HD styles of thinking for ‘knowing where to look’.

## 4. Discovery, Justification, Explanation and History

Other textual clues may lead the reader deeper into the sub-structure of Lawson’s historical errors. The perceptive reader will have noticed that Lawson’s argument focuses on ‘discovery’, yet concludes with almost virulent rhetoric about ‘why some of these [past] ideas [in science] are still accepted while others have been discarded’ (p. 604). Elsewhere, Lawson contends that ‘the key aspect of scientific discovery is the generation and test of explanations’ and, further, that ‘the key aspect that separates scientific discovery from other human endeavors is the act of explanation’ (Lawson 2003, p. 336). Borrowing a traditional concept, one may interpret Lawson as not just characterizing ‘the nature of science’, but endorsing a view that explanation and testing are a *demarcation criterion* for science. Thus, Lawson seems to cast my work as having crossed a critical line of legitimacy and credibility by forsaking science altogether.

Lawson’s ultimate target seems further to be the product and its *justification*, not the process of discovery broadly (semantics about ‘discovery’ aside; Lawson 2003, pp. 336–337). Narrowing the domain of science to explanation and testing, however, is problematic historiographically (even if one were to deem them distinctive features of science). For a historian, focusing just on planned tests excludes much of scientists’ activity that seems relevant to their achievements – how they reach solutions prospectively, not merely how they (or we) justify them in retrospect. Traditionally, philosophers have distinguished between a *context of discovery* and a *context of justification* (or, alternatively, a context of pursuit and a context of acceptance). The two contexts would seem to be complementary.

Both seem 'essential' to describing science fully. Lawson seems to focus, however, just on justification (2003, p. 336). For Lawson (as profiled above), the context of discovery seems to reduce to no more than a 'puzzling observation' and an unexplained generation of explanations. 'Observation of an initial puzzling observation is crucial', Lawson states, '... But, the observation of something puzzling is not enough' (2004, p. 336). Everything turns on planned tests and justifying explanations. Thus, anyone who finds that puzzling observations may involve happenstance or chance and deems this part of science has, according to Lawson, abandoned all sense of empirical evidence and adopted a relativist stance regarding scientific claims (2004, Conclusion). Applying the discovery/justification distinction, one may see that Lawson's appraisal rests on his unstated demarcation criterion that – solely by definition – excludes discovery as a part of science proper. 'Method' applies only to justification through 'planned tests'. All else is non-scientific or anti-scientific. Of course, this make it difficult if not impossible to analyze or discuss as a part of science the source of puzzling observations, predictions or any feature within a context of discovery. Here, Lawson's *historical* errors about the discovery of capillaries can be traced to a *philosophical* position about demarcating science. In an imbalanced view, the context of justification (namely, testing explanations) eclipses the complementary context of discovery. The consequence of a parochial characterization of the 'nature of science' and 'scientific method' is that it forsakes the complete *process* of science, whereby discoveries are ultimately made. Educators interested in conveying the nature of science or in teaching skills in the process of science may thus be reminded here to address both the exploratory *and* justificatory dimensions of science.

Many philosophers of science now regard the discovery/justification distinction (and its successor, pursuit/acceptance) as far too crude to characterize science effectively. One may too easily partition the process of science into an irrational "before" and a rational "after": *first*, randomly develop an explanation, *then* test it. Or: *first* think creatively, blind to the outcome, *then* apply rigor in testing and observation. Many philosophers reject the strict dichotomy because divergent, creative elements as well as selective, evaluative elements seem to interact closely *in all aspects* of the process – at least when viewed historically or through cases studies of actual practice. Justification may be sought while developing hypotheses. Tests may change despite initial plans. Unexpected results may suggest novel interpretations. For example, concepts are rarely generated blindly. They may highlight or be generalized from certain observations deemed especially relevant or representative. Scientists may also assess the plausibility or promise of theories, even before any planned test' (Whitt 1992). Even *having developed* a hypothesis – as Lawson himself points out – one still needs to imagine appropriate observational or experimental circumstances to collect the relevant information, unobscured by other information. This is not specified by the hypothesis: how does the researcher 'know what to do' or 'where to look'? Even what one researcher called 'the most beautiful experiment in biology' did not come about by direct method, but involved numerous chance events and an ultimately convoluted history (Holmes 2001). Interpreting results may also involve creativity. Not in the sense of fantasizing or disregarding the results themselves, but in configuring the data into meaningful patterns. Some great discoveries – notably Darwin's concept of evolution by natural selection and Einstein's theory of relativity – were largely synthetic (Janssen 2002). Anomalous experimental results may indicate that a theoretical expectation is broken, but they do not always indicate 'where to look' among the numerous assumptions, background conditions or concepts (or experimental conditions) to find a solution or replacement theory. Yet there are strategies (not quite 'methods') for resolving anomalies (Darden 1991). Investigations that are not theory-driven, but rather involve exploring 'experimental systems' move forward by analysis and imaginative tinkering, rather than bouts of proposal/disposal (Rheinberger 1997). False models may be pursued deliberately and heuristically, with no investment in confirming them (Wimsatt 1987). The problem is that *in all these creative processes*, the investigator *does not know where to look*. Lawson's focus on justification thus fails to characterize the nature of science fully. Further, what he presents as the central problem of 'knowing where to look' is not solved by HD (or HP) because discovery and justification are so intimately mixed.

Lawson's belated emphasis on the importance of explanation (2003, p. 336; 2004, p. 174) is telling. That is, hypothetico-deductive reasoning is highlighted as 'essential' to *explanations* and to the logical format *of evidence*. 'Knowing where to look', then, seems more about abstractly characterizing *evidential relevance* than providing methodological advice in *designing tests*. Objections to the HD characterization of scientific explanation are hardly scarce. However, assigning HD/HP to a philosophical position on scientific explanation could at least free Lawson from erroneous claims about the *practice* of science and the *historical process* by which such explanations are assembled.

Lawson's historical errors emerge, then, when he tries to import what he considers the *ultimate structure* of an explanatory argument into the *process of reasoning* toward it. Retrospect and prospect are confused. Indeed, Peter Medawar, characterized by Lawson (2003, p. 334) as a "well known and widely respected" philosopher, understood the difference well. He famously portrayed the hypothetico-deductive format of a scientific paper as a 'lie'. Scientists now standardly present scientific arguments and evidence in an HD framework. But, as Medawar noted (1964), HD misrepresents scientific thinking and what goes on in the lab. Medawar should know: he was primarily a scientist and shared the 1960 Nobel Prize in Medicine. (Indeed, this distinction may be one reason for students' difficulty in writing lab reports: they try to write a narrative of their experience, rather than structure the evidence in an abstracted argument.) Failing to apply this distinction, Lawson seems ultimately to conflate ideals and endpoints with actual process. Philosophy substitutes inappropriately for history. Analysis of his historical errors are key to showing that his reconstructions are just that: philosophically shoehorned reconstructions that do not reflect the history of the actual discovery itself (Allchin 2003b, 2006). For my part, I am interested in understanding and teaching about how great scientific discoveries actually happened, rather than how they "should" have happened instead.

Investigating the context of Lawson's history thus leads the science educator, wary of error, to several conclusions:

- Although justification (evidence, testing, etc.) is integral to science, the *process* of science viewed as a whole also includes the complementary elements of discovery.
- While science may be conceived abstractly in terms of elements of discovery and of justification, divergent and convergent thinking are intimately interwoven in practice.
- Philosophical accounts of scientific explanation are not historical accounts of the process of science. Scientific arguments are not narratives of science. Rational reconstructions are not history.
- Be wary of simple (or simplistic) demarcation criteria.
- Errors occur despite good intentions and proper method. Good scientists acknowledge them and endeavor to remedy them.

While one might learn these lessons in many ways, seeing how factual errors in interpreting history can emerge underscores their importance, while illustrating how such lessons may become invisible to some educators.

## 5. Nature of Science: Normative or Descriptive? – *or Both?*

Educators may find one more distinction of value in interpreting the source of Lawson's historical errors. The phrase 'nature of science' is precipitously ambiguous. Is it normative, in the sense of what science ideally ought to be? – Or is it descriptive, in the sense of what science actually is in practice – or has proven to be in the past?

Lawson's effort is decidedly normative. The scientific method is *idealized*, not merely generalized. Science is not *described* as it is actually practiced. Indeed, scientific discoveries are repeatedly '*reconstructed*', frequently '*imagining*' how scientists of the past thought. When scientists' reasoning does not conform to some neurological or philosophical model which dictates how they *ought* to have thought, then the historical reasoning is adjusted to fit the model, regardless of the historical evidence. When one uses the context of justification or acceptance to demarcate science, normative models naturally take precedence.

My analysis, of course, has focused on respecting history. Claims (or hypotheses, if you will) about the nature of science, I contend, must be authentic to history. They should reflect the historical evidence (historical tests). Notably, this also includes the context of discovery or pursuit as equally relevant. Actual practice, and the discoveries that arise from it, matter. Accordingly, one might support teaching nature of science descriptively.

Someone predisposed to sharp dichotomies might cast this as an either-or choice: Does one teach science as it should be *OR* teach history of science descriptively? Alternatively, one may reject the question. Why regard this as an either-or choice? A science instructor may teach *both*. Science instructors probably *should* teach both (Allchin 2004b).

The dichotomy between normative and descriptive approaches to the nature of science does not imply, however, simply dividing the labor between philosophers and historians. One may easily imagine a philosopher purporting to divine the "true" nature of science, while relegating historians to merely document how real scientists conform to or deviate from the norm. Privileging one account versus another does not contribute to profiling the nature of science. Each must be responsible to the other.

Lawson might well present HD/HP as an exclusively normative account that addresses the particular problems of explanation and evidence. However, there would be no need to mention Harvey, Malpighi or their reasoning. One could easily rely on a fictitious scenario for explaining capillaries, knowing what we know now. The history would be irrelevant. But, the absence of a role for history in this normative context highlights just how the history functions for Lawson in his numerous presentations in this journal. Namely, the history becomes part of the context of justification for Lawson's explanations *about science*. History is enlisted to give the normatively derived account the semblance of transparent description. It is part of an effort to make the assumptions about the neurological models and demarcation criteria appear objective and thus beyond question or analysis. The history is presented to show how science *is*, not how one person *interprets* how it *ought* to be. Lawson's minor, almost trivial errors about the history of capillaries thus signal a much deeper error of substantial importance to educators, what I characterized earlier as pseudohistory: Lawson tries to appropriate erroneous descriptions of history as "facts" to support an interpretation of science which is, ultimately, a normative ideology and value judgment. Respect for historical facts about science matter no less than respect for facts within science, lest we succumb to error.[5]

## 6. On Historiographic Models: HP '*Versus*' Inductivism?

Lawson's rhetoric also includes bold claims about my beliefs. Presumably, I advocate 'a blind, brute force, inductivist version of science' (p. 601), where 'science is primarily a game of blind search and good fortune' (p. 604) and so I present Malpighi as 'a man blindly searching and using induction' (p. 602). My 'inductivist view' (pp. 601, 604) apparently implies chaos in research as well as relativism about all past scientific claims: that, on my views, one could not explain 'why some of these [scientific] ideas are still accepted while others have been discarded' (p. 604). In short, I am everything that virtuous HP is not. While some may well be honored by such epithets, I must decline the attributions, even to the extent of not having a 'theory' as Lawson so generously allows (Brush 2004, p. 198). As I noted at the outset, my papers were about use of history, not strictly the nature of science – that is, they were historiographic, not primarily philosophical.

I aimed, far more modestly, to examine characterizations of individual *historical episodes* as *instances* of science. Lawson may have missed that earlier I endorsed:

> the importance of teaching hypothetical reasoning to students, as one among *many modes of thinking* – and one which students often find difficult. But, no one needs history to support this claim. Nor would anyone likely deny that *some scientists* on *some occasions* have profited from hypothetical reasoning, especially in testing tentative ideas.

I did contend, nonetheless, that:

> these piecemeal positions do not amount to a conclusion about one exclusive, monumental, algorithmic method of science. (Allchin 2003b, p. 326)

Indeed, by quickly perusing the articles Lawson addresses, one may find that I identified *historical* use of many different methods, or 'tools' in the scientist's toolbox (Wivagg & Allchin 2002), as detailed in Table II. Of course, these come from only a handful of historical cases, so the list can hardly be exhaustive. Moreover, they were presented as no more than case-specific alternatives to an imputed algorithmic and universal HD/HP.[6] They hardly coalesce into a 'theory' (Lawson 2003, p. 335). They are descriptive, not normative. Still, one cannot discount that they did, historically, contribute to discovery. More important, these characterizations of scientists in action hardly reduce to HP *versus* induction. As Table II might indicate, that dichotomy is woefully simplistic.

*Table II.* Allchin's references to historical examples of features in the process of science

| Method | Reference |
|---|---|
| new instruments as opportunities for new observations and explorations | (2003b, pp. 317, 321) |
| observational heuristics | (2003b, p. 321) |
| noticing | (2003b, pp. 317, 320) |
| "brute" observation [exemplified in anatomical dissection] | (2004a, pp. 181, 182, 183) |
| tentative generalization from a single case or model example | (2003b, pp. 324, 326) |
| arithmetic analysis and enumerative induction | (2003b, p. 322) |
| limited induction across a small number of cases | (2003b, p. 323) |
| "blind" variation and selection (= natural selective reasoning) | (2003b, p. 323) |
| hypothetical reasoning | (2003b, p. 326) |
| analogy | (2003b, pp. 318, 321; 2004a, pp. 182–184) |
| error probes and severe tests | (2003b, pp. 325–326; 2004b, pp. 322, 326) |
| interaction of multiple, contrasting perspectives | (2003b, p. 319) |
| chance, luck, coincidence, happenstance or contingent events | (2003b, p. 321) |
| designing appropriate tests | (2003b, p. 326) |
| evaluating results | (2003b, p. 326) |
| reconstruction of persuasive examples | (2004b, p. 181) |

Given my modest (albeit potentially pluralistic) interpretive posture – and a focus on the appropriate use – and misuse – of history in science education, one may wonder why Lawson characterizes my views in such extreme, monolithic terms. The reader will have to interpret Lawson's reason for transforming my perspective into a handy straw man, in collapsing my wide ranging characterizations into a single notion, in construing these disparate statements as a 'theory' and in calling it, specifically and derisively, 'induction'. Lawson's misleading rendering of Allchin (2003b) is in (Lawson's terms) a puzzling observation in need of a hypothesis or causal explanation.

In any event, *from the historiographic perspective* of my earlier papers, adopting a strict HP *versus* inductivist dichotomy (Lawon 2002, pp. 15–21; Lawson 2003, pp. 331, 335; Lawson 2004, p. 604) impoverishes our ability to interpret the history of science. Historians eschew such bald dichotomies in favor of subtle and nuanced descriptions of what scientists actually do. There is no doubt, for example, that Harvey made a great discovery, of lasting value to science. The challenge for historians, who might thereby inform the teacher interested in the process or nature of science, is discerning precisely how he did so, without prejudicing the case or constraining the methodological possibilities according to some wholly normative philosophical model. Good concrete history describes how scientists do make great discoveries, and sometimes how they fail. History offers prospective models for students to learn – but only if the history is accurate. Ultimately, that is why respect for history matters to science educators.

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## Notes

[1] I find – to my dismay – that Elkana and Goodfield (1968), while cited in Allchin (2004a, p. 182), did not appear in its list of References, although the information was added in proofs. The reference is given in Allchin (2003b, p. 327) and below. Nevertheless, I am surprised that such a thorough scholar as Lawson did not see fit to address the claims and the extensive further evidence presented in their paper.

[2] The perceptive reader will note the unannounced shift from Lawson (2000) to Lawson (2004). In the former, Harvey predicts capillaries as a concrete discovery or theoretical postulate. In the latter, Harvey 'predicts' 'where to look' for blood flow (between the arteries and veins!) and 'predicts' that 'what to look for' may be capillaries, pores, or both – but who is to say one guess is any more justified than another? The first is definite and epistemically risky: capillaries "must" connect arteries and veins (there are no viable alternatives). The revised position is vague and almost uninformative epistemically: there "must" be

anastamoses OR there "must" be pores OR there "must" be both. One might wonder about the meaning of "must" in the disjunction: namely, how can "must" apply to all three simultaneously with any sense of necessity?

[3] Please note that my claims about pseudohistory and the potential misuse of history do not rest on this one case alone, which I used primarily for illustration and *certainly not* as evidence about some singular alternative method of science, despite Lawson's all too generous reading).

[4] Lawson's attention to cognitive models seems to focus on 'information processing' and 'recognition', giving a large role to 'memory' (2002, pp. 10–14; 2003, pp. 332–334). This seems more recall and identification, than concept formation or learning of patterns or ideas which are not yet formed, a more apt basis for a neurological model of scientific *discovery*. See Allchin (2003b, note 2) for references on long-term potentiation as a neurological model of *learning*.

[5] For someone interested in evidence and facts, the testimony of history should matter. The evidence about Harvey's and Malipighi's work on capillaries was incorrectly interpreted. The historical tests of HP failed. Lawson could, within his own system, take the *fact* that Harvey did not predict capillaries as a disconfirmation of his philosophical hypothesis that HP is 'essential' to science'. It could be a 'puzzling observation' that could begin a 'cycle' of new 'hypotheses' and 'prediction' about the nature of science. Educators may examine whether the notion of discrepant events will have any mettle here.

[6] Some of these tools (or methods) may well be viewed, in other contexts, as modes or forms of hypothesis generation. If so, then an account that refers to hypothesis generation without articulating the variants of doing so is, in my view, deeply impoverished.

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