‘Beyond the pillars’: under what conditions is it appropriate for an epistemic community to accept a novel method as reliable?

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Introduction

How does a community usually rationally agree that a novel scientific instrument is reliable? The community will usually checks the instrument against already-accepted instruments or through intersubjective observation. However, how is it possible for a community to rationally agree that a novel scientific instrument is reliable if no other instruments are available for cross-checking? The only available account of how an instrument can be checked is by appealing to what is what is checkable through public and agreed-upon methods. Consequently, it would seem impossible for any community to accept a scientific instrument that extends beyond already accepted methods.

This puzzle relates to a neglected part of Philip Kitcher's overarching 'Galilean' strategy: he presents a strong companion in guilt argument in how we can reasonably infer that an instrument will continue to be reliable in environments suitably similar to ones in which we accept it as reliable.

However, Kitcher's Galilean strategy does not account for the historical episode in history of science that inspired the very strategy: Galileo and his allies improperly concluded early telescopes were reliable in different conditions and at distances that exceeded those measurable by the unaided human eye. The evidence wasn't available to successfully implement it. It would seem that the Galilean strategy fails when applied to Galileo.

This would be an embarrassing result, however it can be avoided: I introduce an additional companion in guilt argument about the grounds for reasonably inferring the reliability of new instruments: consilience of reports amongst differing types of instruments provides an additional shared condition for accepting that a novel instrument is reliable. This stronger version of the Galilean strategy accounts for the historical episode that provided its name. Furthermore, it fits within general scientific practice about how to accept that new instruments are reliable. Therefore, Kitcher's Galilean strategy is subsumed under a stronger strategy for rational acceptance of novel methods.

1 The vicious circularity of adopting scientific instruments

The Straits of Gibraltar were once thought to be the limits of the known world (Romm 1992, p. 11):

> … the Pillars or Columns of Heracles … became a vivid symbol of the gateway or barrier between inner and outer worlds. For the most part they stood in the Greek imagination as a forbidding *non plus ultra*, a warning … not to proceed any further (Romm 1992, p. 18).

In time, this geographical limit has 'come to stand for the boundary of the human condition itself…' (Romm 1992, p. 18), providing 'a metaphor of restraint and prudence' (Rosenthal 1971, p. 211) from speculation about 'soft places' (Gaiman 1993, p. 17) that are inaccessible to an epistemic community[1]. These geographical boundaries metaphorically track epistemic boundaries: within these boundaries is a familiar 'lived-in space' (Tuan 1978, p. 7), but, as Pindar says, 'What lies beyond [the pillars of Heracles] is impassible for both the wise and the unwise. I shall not try [to go beyond], or I would be a fool' (*Odes*, §3.43–45).

An argument for adopting these epistemic boundaries is expressed as follows: consider how an epistemic community adopts a new scientific instrument. It is unknown whether the beliefs based on its use are accurate. How can the realist argue for the reliability of the instrument while also arguing that beliefs based on its use are not erroneous? This is question-begging: approaching either horn on its own would assume the other horn is correct, but to approach both horns together would justify both horns based on a vicious circle.

In line with this metaphor, we cannot shift these epistemic boundary stones by introducing a new scientific instrument, and crossing these boundaries imposed on us by our sense-organs invites nothing but speculation about 'monsters', myths and fables. Appealing to the apparent reliability of the scientific instrument would be a viciously circular justification. This would license adopting

[1]This warning against investigation outside the known world can be found in, for example, Strabo (1931, §2.5.5, 34) and Pliny (1967, *HN* §2.4), as well as Erasmus (1924, *OE*, p. 359, letter no. 1400), as quoted in Rosenthal (1971, 220, fn. 62).

the reliability of all methods that are not cross-checkable. As Alston (1993, p. 17) says,

> We can just as well say of crystal ball gazing that if it *is* reliable, we can use a track-record argument to show that it is reliable. But when we ask whether one or another source of belief is reliable, we are interested in *discriminating* those that can be reasonably trusted from those that cannot. Hence merely showing that *if* a given source is reliable it can be shown by its record to be reliable, does nothing to indicate that the source belongs to the sheep rather that with the goats.

This problem is an indiscriminate bomb to any attempt at reasonably ascribing predictive success to scientific theories about unobservables, thus targeting the first half of the 'no miracles' inference 'from predictive success, we can reasonably infer approximate truth'. Therefore, an epistemic community cannot appeal to the predictive success of scientific theories as giving reason to accept a new scientific instrument as reliable. The very predictive success of scientific theories *is at issue*.

Observation, so the anti-realist claims, can only be the accepted reliable method, and any new instrument introduced into a community fails to secure credibility in contexts that are not open to intersubjective observation.

The realist must first diffuse this bomb in a way that does not appeal to a 'no miracles' argument or any variation of it, otherwise the realist's response runs up against a wall: the realist has no question-begging reasons for concluding that scientific theories about unobservables are predictively successful.

2 The Galilean strategy

Here is a proposed solution: rather than overplaying the realist's hand and insisting that this vicious circularity between scientific theories and instruments provides joint grounds for rationally accepting the reliability of scientific instruments *and* the predictive success of scientific theories, the realist can take a different approach: appeal to a number of 'home spun' beliefs that would be irresponsible for the anti-realist to deny, such as 'if something makes the barely perceptible clear and distinct, it is apt to be revealing what is really present when it makes the invisible visible' (Miller 1987, p. 10).

As Miller (1987, pp. 10–11) argues,

> The right defense [of realism] is not a grand argument from a general pattern of success but a series of piecemeal defenses, displaying the power of specific arguments that have actually been compelling for modern scientists, by showing how they rely on appropriate topic-specific truisms as their essential framework.

When addressing the puzzle of the apparent impossibility of 'bootstrapping' (Vogel 2000) to the reliability of new scientific instruments if the scope of what is usually observable to unaided senses with any fidelity goes no further than a few miles. Thus we have the following argument:

1. Observation is reasonably trusted to be reliable

2. Telescopes are at least equally as reliable as observation when aimed at objects on the Earth.

3. There are no known salient differences between the Earth and in the heavens that would adversely affect the reliability of telescopes.

4. It would be absurd to adopt the position that telescopes become unreliable *precisely* at the limits of observation, since it would entail that any increase or decrease in the abilities of an epistemic community to observe affect the reliability of the scientific instrument.

5. Therefore, telescopes can be reasonably trusted to be reliable in some contexts in which we cannot check through observation.

If the critic posits these environmental factors, the critic is committed to the epistemic 'sin' the anti-realist attributed to the realist: appealing to unobservable, hidden causes.

If, however, the critic merely proposed the *possibility* of these hidden environmental factors that routinely lead independent methods into a confluence of error, then the critic has adopted a principle that is equally as effective against other similar cases of consilience: the critic has embraced a form of skepticism that itself indiscriminately bombs any check of the reliability of methods.

Kitcher (2001, 175, ft. 46) concludes his reasoning thus:

> It's possible that … [an anti-realist] might object that I've misdescribed the situation. Galileo didn't show that conclusions about unobservables could be justified through the use of telescopes. Instead he changed the boundaries of the observable. But to make this response would undermine the empiricist strategy. For the notion of the observable has now been detached from the connection with unaided observation, and must now come to something like 'detectable with a reliable instrument.' Realists can sympathize with that notion of observability, formulating their views as claims about the reliability of particular detection devices.

In sum, if the critic of Galileo rejected the apparent reliability of the telescope, two options were available: the critic must resort to arguing for the possibility of a number of unknown and undetectable environmental factors that cast doubt on the reliability of the telescope; or, telescopes are no longer reliable precisely at the limits of the reliability of the unaided eye. The latter option is simply too absurd to accept. In brief,

> Unless we were to adopt some implausible causal theories to the effect that limits of the human sensory system affect the reliability of the inferences, there's just no basis for making the empiricists' preferred judgments about the extent of the legitimacy of the method' (Kitcher 2001)

This leaves the former option. Kitcher does not address it at length; however, were there any reasons to reject it at the time?

3 The terrestrial and celestial spheres

Although the unaided eye has little fidelity at great distances, and cannot pick out purported entities such as sunspots or moons orbiting Jupiter, the eye is still capable of determining the general location and brightness of stars in the sky at considerable distances, so long as their luminosity is great enough. There were reports of two supernovae, one in 1572 by Tycho Brahe and another in 1604.[2] These scientists, so the story goes, '… used the unanticipated appearances to knock down or at least modify the long-lived Aristotelian distinction between incorruptible celestial heavens composed of solid, planet-bearing spheres and a changeable terrestrial realm' (Westman 2011, 19, cf, 33–34) (Cf. Shapin 1998).

All apparently reliable eyewitnesses reported the same sudden brightness in the sky at the *same* time and in the *same* location in the sky.[3] All apparently reliable eyewitnesses reported the same sudden brightness in the sky at the *same* time and in the *same* location in the sky.

It would be absurd to deny the credibility of these reports. This unexpected and novel brightness was not a mass hallucination: 'the initial challenge originated from a contextual, nonhuman source: natural events in the "out there"… actually impinged on the perceptual apparatus of those who claimed to have observed something new' (Westman 2011, p. 19). If members of a community independently attest to the occurrence of an event or observation of an object on *Earth*, their mutual testimony was sufficient for the scientific community to agree that they had witnessed two events that occurred in the heavens above, and Galileo and his critics both accepted the limits of observation to extend beyond the terrestrial sphere.

[2]This supernova was observed unaided by a telescope by Baldassare Capra, Simon Mayr and Camillo Sasso on 10 October (Shea 2005).

[3]The number of reported observations skyrocketed: the 'new star' was then confirmed again on 15 October, this time larger and more luminous than before (Shea 2005). Furthermore, Righini (1978, p. 14) further argues that earlier reports were made of the supernova on 8 and 9 October by Anton Lorenzo Poliziano, and the professor of mathematics at Pisa, Antonio Santucci. Galileo himself recorded observing the supernova on 28 October. As Galileo reports, 'First small and weak, it became huge within a few days…' (*Opere*, 277, vol. 2).

Within six months reported observations without the aid of a telescope were recorded all across Europe (Shea 2005) and possibly even incorporated into works of contemporary literature, e.g. Shakespeare's *Hamlet*, Act. 1, Scene 1, (Olson, Olson, and Doescher 1998; Gingerich 1981).

3.1 Lorenzini's objection

However, these reports were not enough to infer more than that many people had witnessed two bright lights. They did not answer *at what distance* the supernovae were at. Antonio Lorenzini (1605) declared in response to the reports of the supernova that since there could be no new stars in the heavens, the sudden brightness in the night sky could only have occurred within the distance between the Earth and the planetary spheres (Shea 2005).

There was an accepted discontinuity in the structure of the environment that could very well have adversely affected the reliability of scientific instruments. The sublunary world was supposedly imperfect and the celestial sphere perfect (Grant 1994). This was the result of adopting an Aristotelian version of physics, 'a complex and tight conceptual scheme' (Rovelli 2014) that accounted for all the available evidence at the time.[4] In other words, if it were the case that the celestial spheres were perfect, any purported detection of some imperfection in the heavens (e.g. a change in the luminosity of stars, new stars appearing in the heavens, or sunspots) could be reasonably dismissed as a sublunar event, rather than a genuine detection of imperfection in the heavens.

While there had been communal observation, observation *alone* did not call into question the imperfection of the heavens, since the new light in the sky could very well be *within* the orbit of the planetary spheres, not outside its orbit. In short, Galileo's critics rightly worried that Galileo and his allies had misidentified a comet as a 'new star'.

Any attempt to establish the imperfection of the celestial spheres would have to bottom out with observation, but Lorenzini produced a well-entrenched explanation for the phenomenon that was not amenable to observation, since eyesight alone could not determine the distance of faraway objects with any precision. In effect, there was a well-entrenched and highly supported salient distinction between the terrestrial and celestial spheres.

[4]Cf. 'the beautiful order of the heavenly bodies' (Digges 1573, A1v); 'the unchanging pure aether' (Digges 1573, A2r); 'no substantial change in the heavens' (Digges 1573, p. L2v). From (Johnson and Larkey 1934).

3.2 Responding to Lorenzini's objection: introducing consilience

This problem, however, was short lived, since while Lorenzini's reply rests on the failure of eyesight to reliably determine distance with precision, there were scientific instruments available that could reliably determine *relative* distance, and on occasion determined exact distance on Earth with great precision: methods of determining stellar parallax showed that the two supernovae in the sky could *not* be anywhere but beyond the planetary spheres.

The 'astronomer's staff' (or 'stave') and other methods for measuring parallax on the battlefield had incredible overlap with other measurements of distance, and in fact frequently supplanted these measurements on the grounds that, if used correctly, and in the appropriate contexts, it was far more reliable than previous methods. Furthermore, the introduction of the telescope provided a further expansion of available types of tools.

We now had three methods available: the unaided eye, the telescope and the astronomer's stave. All three independently said the supernovae occurred at a distance putting them within the celestial spheres. But if so, then the celestial spheres were imperfect, thus the third premise–there were no known salient differences between the Earth and heavens that would adversely affect the telescope, unaided eye and astronomer's stave. And therefore the Galilean strategy can proceed.

4 A twist

This approach, however, would appear at first glance to be pushing back the issue over the reasonableness of adopting the reliability of the telescope to *another* scientific instrument that was previously accepted as reliable within a limited domain: the reliability of measuring stellar parallax could very well be called into question when directed at the heavens, preserving the perfection of the celestial spheres.

A critic of Galileo, Martin Horky, said, 'Below it works wonders' but refused to admit to its apparent reliability when directed above (Kitcher 2001, p. 173). While Horky's criticism was unprincipled, Cesare Cremonini's criticism was not: he 'argued that it was bad physics to extrapolate from objects close at hand (and thus observable by the senses) to very distant objects' (as quoted in Muir 2007, p. 52). Cremonini continued: 'Although one may be instructed by ingenuity or by logic, unless he has also experienced the thing he is to judge, he will not be able to exercise judgment', (as quoted in Muir 2007, p. 49). Drake (1977, p. 22) notes, 'Cremonini's position was in fact quite sound, though it lost out in the later science of the seventeenth century'.

Generally, if there are no ways to check the trustworthiness of a method, any inquiry beyond the 'safe harbour' is possible in principle, but remains questionable *in practice*: it did not resolve the critic's worry over the apparent impossibility of determining in which contexts the telescope was no longer reliable. We are faced with the problem that the telescope and astronomer's stave were not fully independent of the human eye; in fact, both relied on it.

One initially plausible approach to this problem would be to appeal to features of the scientific instruments in question. After all, we frequently and consistently discover a number of pertinent distinctions in our environment that affect scientific instruments. For example, water boils at 95 degrees celsius in Denver, Colorado but at 100 degrees celsius at sea level. If a scientific instrument should demonstrably fail in this new context, this indicates unreliability as a result of some feature of this new environment adversely affecting the reliability of the instrument. Take the freezing of mercury at certain temperatures as an example:

> The immediate and easy lesson from the story of freezing mercury is that unexpected things can and do happen when we go beyond the realms of phenomena that are familiar to us… Bentham compared this with the tale of the Dutch voyagers (reported by John Locke), who were denounced by the king of Siam 'with a laugh of scorn' when they told him that in the Netherlands water would become solid in the winter so that people and even wagons could travel on it. Locke's story may be apocryphal, but the philosophical point stands. (Chang 2004, p. 106)

The realist could presumably appeal to the features of the scientific instrument in question to determine whether it is trustworthy: if in new contexts the instrument does not demonstrably fail, the realist is licensed to infer that it continues to be trustworthy. However, what should count as failure?

It would be absurd to conclude, for example, that since a watch is not broken that it accurately reflects the time. Similarly, since methods of measuring stellar parallax all relied on the same type of instrument and all telescopes operated according to the same principles, both could very well be systematically unreliable in ways that could not yet be detected. In fact, this is precisely the worry Cremonini had: the standard of appealing to no obvious failures of a watch equally licenses inferences to the accuracy of both accurate watches and inaccurate watches. Similarly, the telescope and human eye both were unreliable at a certain distance, but what distance was it, exactly? Was it at a distance close to the demarcation line between the celestial and terrestrial spheres?

In summation, a determination of whether an instrument is reliable or not only runs in one direction: *we only learn if scientific instruments demonstrably fail to be reliable in some unexplored context*. But if there existed a distinction between the terrestrial and celestial spheres, then the instruments were unreliable. Furthermore, without a way to determine a limit of reliability for the telescope and astronomer's stave, then there was no way to determine whether the instruments were reliable at the distance between the purported spheres.

The reliability of the telescope could have been adversely affected by the salient differences between the celestial and terrestrial realms or the reliability of the telescope could have ended at some unspecified point. Thus it was premature to conclude the astronomer's stave and observation revealed that both the celestial and terrestrial realms were imperfect based on its measurement of the distance of the two 'new stars'.

The above historical episode, in brief, presents a case of burden-shifting: Galileo and his allies attempted to shifting the burden from the realist to the anti-realist's shoulders, in line with Kitcher's Galilean strategy. But Galileo's critics were able to shift the burden back to the realist: Galileo still needed to provide a *reason* for accepting the reliability of the telescope, unaided eye and astronomer's stave; otherwise, there was no reason to abandon the well-entrenched theory that there were two differing spheres.

On this point, Kitcher's paradigmatic example of the Galilean strategy in action falls short of providing additional grounds for accepting a novel scientific instrument beyond this companion in guilt argument. Adopting a realist position does not come for free; it must first be *earned* by explaining why we should reasonably come to accept a novel instrument.

4.1 A response: shared grounds of consilience of types

What grounds are there for reasonably inferring that a method is trustworthy for observables? Communal checking of putative exceptional abilities by appealing to other accepted methods is the only available check. Consilience is also the only available check of the average abilities of an epistemic community, as well as below-average abilities. The methods of an epistemic community are trustworthy when their results align themselves with the testimony of other members of the community that use these same methods, for example, 'It is *absurd* … to suppose that someone could see the very same afterimage that I see, or feel the very same pain that I feel… However, to say that I see a thing to be met in space, such as a hand or a soap-bubble is to say that it might have been perceived by others as well as by myself' (Morris and Preti 2015, p. 3).

Consilience of methods usually provides the reasons for accepting the reliability of the sense-organs of others, but consilience between our *own* sense-organs provides the grounds for accepting their reliability. The only other explanation available for how each of these independent methods produces a coherent and mutually supporting web of beliefs would be that each method only *appears* to track some aspect of the subject of my purported causal interactions, but I am being systematically mislead in everyday circumstances.

Consider, for example, a lone individual that insists that a putative event or entity is independent of their private experience–in other words, they claim to have exceptional abilities to see ghosts or speak with the departed, the ability to read minds, to move objects without touching them, or to have extrasensory and precognitive abilities, e.g. a 'spider-sense' (Lee and Ditko 2013).

It would do no good to appeal to others that claim to see ghosts or speak with the departed. After all, that is to appeal to the very type of method that is in question, hence it is question-begging. But if many different people claimed, independently from one another, that they received the same series of messages from the beyond, this is a different story.

The problem of determining if these individuals have these abilities is directly comparable to the introduction of new scientific instruments into an epistemic community. In fact, both are special instances of the apparent paradox that there is no way of assessing the reliability of untested methods that cannot be intersubjectively checked. Answering this question provides an answer to the general question.

How can we answer this question? First, consider the principled *rejection* of untested methods:

One classic method relies on finding some agreed-upon test, that is, a comparison between the new method and mutually supporting, independent types of methods from members of the epistemic community to show that the purported new ability or method systematically fails to cohere with a number of accepted *types* of methods.

Another method relies on using different *token* instruments to see if they provide differing results: if a number of dowsers, for example, each continue to fail to locate precious metals or water in a double-blinded trial, the reports from different token instruments are not in accordance with one another above chance.

But, again, these methods do not work when extended beyond observation. That is the very problem that Galileo faced: each token telescope, eye and astronomer's stave were in agreement with one another; each type of instrument was in agreement with one another. And yet Galileo's critics still had an iron-tight argument.

Here is one solution that shifts the burden on to Galileo's critic: what of different token and types of *tests*? It is one thing if two or three people report the same message received from spirits–perhaps it is merely a matter of chance; it is *another* thing entirely if they each repeatedly and independently report the exact same messages from beyond. This requires further examination.

Galileo and his allies had *two* token supernovae, each measured precisely. Two different token tests are better than one, and each showed (that is, if the instruments were reliable) that the two supernovae occurred in the celestial spheres.

Furthermore, there were different *types* of test available: Father Christoph

Clavius SJ 'said that if the telescope revealed four new "planets" around Jupiter to Galileo, then Galileo must have put them in the telescope to begin with' (as quoted in Van Helden 2010, p. 195) Two months later, Clavius accepted the reliability of the telescope (*Opere*). But why? Here is one explanation: different types of instruments kept reporting the same results for different types of tests.

Take the existence of sunspots, for example: Galileo said,

> It proves nothing to say … that it is unbelievable for dark spots to exist in the sun because the sun is a most lucid body. So long as men were in fact obligated to call the sun "most pure and most lucid", no shadows or impurities whatever had been perceived in it; but now it shows itself to us as partly impure and spotty, why should we not call it "spotted and not pure"? For names and attributes must be accommodated to the essence of things, and not the essence to the names, since things come first and names afterwards (Shapin 1998, p. 18).

Yet, the existence of sunspots was *not* conceded by all competent practitioners, since the apparent observation of sunspots could very well be explained as an artefact of the telescope at such distances, or the phenomenon could very well have been terrestrial in origin: 'that no material of ours better imitates the properties of these spots than terrestrial clouds' (as quoted in Drake 1957, p. 99). But different token telescopes continued to make the same reports: there were sunspots. Thus a type of instrument made the same report: the Sun was imperfect. This instrument, when pointed at a different type of subject, the Moon, made the same report as well: the Moon was imperfect. When pointed at the planets, the result was, again, the same: the planets were imperfect. All these different types of test kept saying the same thing: the celestial spheres were imperfect. But if so, then there was no reason to maintain a salient distinction between the Earth and heavens.

The expansion of the scope of the accepted reliability of the telescope continued so long as there were consilience between mutually independent types of tests of token telescopes. With the rejection of any environmental boundary, Galileo and his allies could now reintroduce the astronomer's stave based on the Galilean argument, thus providing a measure of distance.

Galileo's critics were themselves caught in the following problem of a *companion in guilt*: denying that consilience of reports from many different token telescopes about different types of tests, each under different environmental conditions and each giving the same coarse-grained reports (there is imperfection in the heavens) would be absurd. To abandon the argument presented by Galileo and his allies effectively defeated the only agreed-upon method for accepting observables, that is, consilience between types of tests, token instruments, and types of instruments.

If the critic of the reliability of the telescope accepts consilience *only* as providing grounds for accepting sense-organs and other scientific instruments for observables as reliable, and not for scientific instruments immediately outside the scope of what is observable, the critic must explain in which contexts consilience will routinely go awry without appealing to the machinations of unobservable and undetectable features of the instruments and environments. This is to shift the burden back on the shoulders of the anti-realist.

Once telescopes were considered reliable once pointed at the heavens, the next step was to answer the problem of under what conditions reports from the telescope could supplant observation with the unaided eye. Telescopes were more accurate than the unaided eye when aimed at objects on Earth, so it stands to reason that, all else being equal, the unaided eye was unlikely to be more accurate than telescopes when aimed at objects in the heavens.

One needed a suitable test-case, in which the object was close *enough* to be at the far reaches of what was observable with fine-grained detail with the unaided eye, but not *too* far that the reliability of the telescope could reasonably be in doubt. A (relatively) nearby planet would have to do. In 1631, Pierre Gassendi repeatedly observed the transit of Mercury and recored the size as 'entirely paradoxical smallness'. The repots of Gassendi's telescope routinely overlapped with contemporaneous token telescopes, and he furthermore independently reported a number of fine-grained features of planets and moons that were themselves corroborated by different token telescopes.

Gassendi had previously relied on testimony from pretelescopic observations, such as Ptolemy, in forming the belief that Mercury's diameter was one-fifteenth of the diameter of the Sun. As Van Helden (1985, p. 3) optimistically concludes,

> The telescope made it possible to subject this traditional scheme of sizes and distances to scientific scrutiny. With this instrument Gassendi could falsify Ptolemy's estimate of the apparent diameter of Mercury; indeed, he was almost forced to do so against his own will!

Thus, the modified version of Galilean argument–consilience of token instruments if subject to different types of tests shows a type of instrument is reliable within some limited domain. A comparison between different types of instruments and different types of tests checks whether different types of instruments are in accordance with one another.

Employing this method, Galileo and his allies thus inspired measured confidence when previously there was prudence: the 'traditional expression of the limits on knowledge *ne plus ultra*–"no farther"–was defiantly replaced with the modern *plus ultra*–"farther yet"' (Shapin 1998, p. 20). It is appropriate, therefore, that in a case of historical irony,[5] 'The location of the Pillars [of Heracles] later had to be moved to accord with the fact that the Straits themselves had become penetrable' (Romm 1992, p. 17).

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[5] Cf. Strabo 1931; Schulten 1925, §3.5.5.

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