Bringing *real realism* back home:
a *perspectival* slant

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1. Introduction

When it comes to debates on realism in science, Philip Kitcher's *Real Realism: The Galilean Strategy* (henceforth abbreviated as *RR*) occupies its well-deserved place among my top five must-read articles published in the past forty years or so on the topic (alongside Putnam's *What is realism?*; Boyd's *Realism, anti-foundationalism and the enthusiasm for Natural Kinds*; Laudan's *A refutation of convergent realism*; and Psillos' *The present state of the scientific realism debate*). Personal as this top-five list may be, there is no doubt that *Real Realism* has ushered in a silent revolution. Without much fanfare, it has shown how realism is hard to resist because it "begins at home" and "it never ventures into the metaphysical never-never-lands to which antirealists are so keen to banish their opponents" (*RR*, p. 191). Kitcher has taught us how realism began with homely considerations such as those used by Galileo to persuade the Venetians about the reliability of his telescope to spot ships approaching the harbor. The following step from 'being a reliable naval instrument' to 'being a reliable instrument, in general'—capable of revealing the craters of the Moon, the satellites of Jupiter, and the phases of Venus—was a short one.

The Galilean strategy that Kitcher has so admirably defended in *Real Realism* against both empiricism and constructivism (in their respective semantic and epistemic forms) entice us to a "homely line of thought", and warns us against any "Grand Metaphysical Conclusions". Its impact cannot be underestimated. We all stand on Galileo's shoulders with our defiant trust in science and technology to give us access to nature and its innermost secrets (pace empiricists' intimations against 1-kg mortar and King Kong's ability to break it). More to the point, we all stand on Kitcher's Galilean grid in thinking of realism as a 'homely' enterprise, where a *divide et impera* strategy of 'working posits' and 'idle wheels' can guarantee to the selective realist a cornucopia of past scientific results. Where to go from here?

Closer 'home' is my reply. Whose home? The very home, from which Philip Kitcher began his intellectual journey in the early 1980s, with his reflections on explanatory unification (Kitcher 1981) as the battleground of two grand traditions: the Aristotelian

tradition, whereby "scientists aim to fathom the order of being, an order that is typically opposed to the order of knowing"; and the Humean tradition (continued by Mach, Duhem and the logical empiricists), which, on the contrary, argued for "no joints at which nature can be carved, no objective necessities, no mind-independent causal connections" (Kitcher 1986, p. 202). Against both traditions, Kitcher defended a *via media,* leading out from Kant's writings on the methodology of science onto the philosophy of science. Central to the Kantian project envisaged by the early Kitcher was an analysis of scientific knowledge and objective understanding that "does not depend on any mind-independent notions of causation, natural necessity, or natural kind" (ibid. p. 204). Yet fifteen years later, in *Real Realism* Kitcher took a stance against the Kantian tradition—in its Epistemological Constructivist outfit—holding that "the realists' world is an inaccessible realm of noumena" (*RR*, p. 188).

In this essay, I suggest bringing *real realism* closer home, namely back to its Kantian roots. The very same roots that make real realism a 'homely' kind of realism, against any Grand Metaphysical Conclusions about the world, its causal necessities, and natural kinds. In particular, I suggest reinterpreting a key aspect of real realism—i.e., the notion of success at stake in 'working posits'—along more 'homely' lines, lines that acknowledge historical continuity, conceptual nuances and our role as epistemic agents in assessing success and inferring truth (for some preliminary reflections, see Massimi 2012 and 2014a). The result is a form of *perspectival realism*—to adopt Ron Giere's terminology (2006, 2013)—which is, however, already at a distance from what Giere himself intends by this term (see Massimi 2015). Hence, my very own (loosely Kantian-inspired) *perspectivalist* slant to *real realism*.

Key to the Galilean strategy—as I see it through Kantian lenses (see Massimi 2010)—is not just to deploy the telescope to overcome fictitious boundaries (i.e. those between sea and land, Venice and Amsterdam, Heaven and Earth). But also to approach nature through principles of reason, in one hand, and "experiments thought out in accordance with these principles" in the other hand, "yet in order to be instructed by nature not like a pupil, who has recited to him whatever the teacher wants to say, but like an appointed judge who compels witnesses to answer the questions he puts to them" (Kant 1781/87, Bxiii-xiv). This is how Kant famously portrayed Galileo's contribution to bringing natural science onto the secure path of knowledge "after groping about for so many centuries". It is this *further* Galilean strategy that I turn my attention to here.

I cannot do justice to the breadth of the philosophical arguments that Kitcher's real realism has put forward. And much as I'd like to discuss Kitcher's articulated response both to the epistemological empiricism of van Fraassenean flavor and to the epistemological constructivism of Kantian descent,[1] I have to leave those for another occasion. For here I concentrate on Kitcher's influential response against the "blockish holism" of epistemological empiricism in its historical form (best expressed by Laudan 1981), which seems to assume that "a theory is false because it is not entirely true" (*RR,* p. 170). In reply, real realism insists "that the past successes stem from parts of the theories that are approximately correct", namely from those hypotheses that are genuinely put to work (i.e. that characterize "working posits"), and are as such "approximately true".

In Section 2, I review Kitcher's famous distinction between working posits and idle wheels in the context of his realist defense against the challenge coming from the history of science. In Section 3, I focus on the notion of scientific success and distinguish between two variants—i.e. success 'from within' and success 'from above'. Finally, I suggest a perspectivalist slant to real realism in the form of a notion of success 'from within', able to assess success from a human vantage point and to capture truth across scientific perspectives (Section 4). I conclude by considering possible objections and replies (Section 5) to the perspectival view canvassed in Section 4.

2. Against the "blockish holism" of epistemological empiricism: 'working posits' and theoretical excrescences

A powerful line of argument against realism has traditionally rehearsed a seemingly compelling historical point against the "success to truth" inference. Namely, that similar inferences "made by our predecessors would have issued in conclusions we now take to be quite wrong" (*RR,* p. 168). Let us leave here aside whether the list of past successful-yet-false theories reflect historical records; or abide instead by the antirealist inclination to inflate examples. There are undoubtedly "prominent cases from the history of science in which views we now take to be false were genuinely successful by anyone's standards" (ibid.). Not surprisingly perhaps, Fresnel's wave theory of light is one of such favorite examples. No matter how false the ether theory is, Fresnel's ability to use his mathematical

[1] Against the epistemological constructivists of Kantian descent invoking a "distinction between objects-as-experienced and objects-in-themselves" (RR, p. 189), real realism responds that "the objects we claim to represent accurately are not mysterious noumena but, in many cases, the things with which we interact all the time" (RR, p. 189)

equations to predict a bright spot in the middle of a dark shade won sceptics like Poisson among the Paris Academy of Sciences, and belies epistemological empiricists' objection to realism. Championed by structural realists, Fresnel's wave theory of light is—to the eyes of real realists—successful not in virtue of a tenuous distinction between structure and substance (pace structural realists). Instead, its success was achieved via "approximately true descriptions of some of the features of light waves (the mathematical accounts) while being wrong about others" (i.e. how light waves propagate in the ether). The real realist sees Fresnel as employing many tokens of 'light wave' to refer to electromagnetic waves and "as saying a large number of approximately true things about the properties of electromagnetic waves of the appropriate type", despite the false opinion about the propagation of the waves through an elastic ether (*RR,* p. 170).

Against the blockish holism of the antirealist that would invite us to regard as false a past theory that is no longer true by our own standards, the real realist recommends a *divide et impera* approach. "Working posits" are approximately true as long as they explain why past theories were successful (to the extent that they were); while "idle wheels" are "theoretical excrescences that are incorrect" and often entangled with working posits. Fresnel might not have distinguished between the two; but it is not a foregone conclusion that it would have been impossible for him to do so. Contemporary selective realists have made their own the real realist's distinction between working posits and idle wheels. And the distinction continues to be, in my view, one of the most persuasive replies against antirealist challenges coming from the history of science.

A difficulty still awaits. For the objection against the structural realist—i.e. that the structure / substance dichotomy cannot easily be exported to other examples—can similarly be leveled against the real realist. Consider, for example, Aristotle's theory of free fall as accelerated motion towards a natural place. The theory was undoubtedly successful by its own lights at the time, and it provided a springboard for Medieval commentators (from Simplicius to Hipparchus and the Arabic commentators), whose views fed into the impetus theory of Buridan and Oresme, and ultimately into Galileo's early Pisan studies on free fall (see Massimi 2010, and 2014b). What are the working posits in Aristotle's theory of free fall? And where do theoretical excrescences begin? Was Aristotle's hypothesis that bodies get heavier nearer the Earth an 'idle wheel'? Well, it provided an explanation—in Aristotle's own scientific perspective—of why free falling bodies accelerate (as opposed to decelerate or move with constant speed) when moving towards their natural place (where he assumed bodies would regain their 'form'). Moreover, it

suggested that there might have been forces acting on the body and pulling it either towards its natural place or in some different direction (what Avicenna and Abū'l-Barakāt called natural and violent *mail*; and Buridan called impetus as an intrinsic force due to a natural gravity; which was in turn the ancestor of the early Galileo's *gravitas* as a weight-related concept; and ultimately of Newton's gravitational mass). Was there *anything* approximately (or even *remotely*) true in Aristotle's theory? What has gone wrong with this example?

3. Success 'from above' and success 'from within'. A further thought on Galileo's strategy

Here is a possible diagnosis. The real realist has employed too stringent a criterion in the *divide et impera* strategy: a criterion of success 'from above' rather than 'from within'. In Fresnel's case, current electromagnetic theory provides the criterion of success to discern between the 'working posits' of Fresnel's theory (i.e. the equations for polarization by reflection, which still bear his name) and the 'idle wheels' of the ether theory (long gone from contemporary textbooks). In Aristotle's case, the criterion of success 'from above' cannot similarly be deployed to distinguish between working posits and idle wheels. Too many centuries separate the Galilean-Newtonian theory of free fall from Aristotle's; and all the conceptual nuances, small theoretical steps, and turning corners that the notion of 'free fall' underwent in that span have long been forgotten (were not for the assiduous work of dedicated historians of science).

Thus, I contend, it is not the case that Aristotle's theory is less promising than Fresnel's, when it comes to identifying parts of the theory that are essential to success (working posits), and hence approximately true. Scientists at Hipparchus' time (or even at Buridan's time) might still have been able to identify such parts. But *we* no longer are, because two millennia separate us from Aristotle. Thus, my diagnosis suggests, there is nothing wrong with the real realist's *divide et impera* strategy. What has gone wrong instead in the example of Aristotle's 'free fall' is the real realist's tacit appeal to the scientific realist's criterion of success 'from above', i.e. from *our very own current vantage point* as if that vantage point were the best one to assess the past; or, the one that provides a royal road to Truth with capital T.

The perspectival realist (of Kantian leaning) enters the scene. For the perspectival realist (of the kind I like) would rejoin that there is no privileged vantage point from which to assess scientific claims of the past. Our current vantage point is not a disguised Nagelian

view from nowhere, providing special epistemic standards for assessing the past or a privileged access to the ontology of nature. Our current scientific perspective is only one among many that our ancestors have happened to occupy, and from which failure and success get evaluated. Homely, perspectival considerations of this kind invite us to embrace a more modest criterion of success 'from within', when it comes to discerning between 'working posits' and 'idle wheels'.

The perspectival realist may adopt herself a Galilean strategy, namely the one adopted by Galileo in his early treatment of free fall in the Pisan treatise *De Motu antiquiora* (ca. 1590s), before Galileo discovered the law of free fall ($s : t^2$). Against Aristotle's cause of motion (i.e. motion towards a natural place), Galileo looked for the 'true cause' (*vera causa*) of accelerated motion in an Archimedean explanation of buoyancy, whereby it is the ratio between weight / volume of the body over weight / volume of the medium that explained whether bodies move up or move down. But the Archimedean analogy with buoyancy could only explain uniform (not accelerated) motion. Hence, Galileo had to resort to the Medieval impetus theory of Buridan and Oresme in thinking of a weight-related concept of 'gravity' (*gravitas*) as an *internal static force* that would decay during the free fall. Galileo's *momentum gravitatis* (sometimes also referred to as *impeto*) is already at a distance from Medieval impetus theory, as it is from Newton's gravity, understood as an external impressed force acting at a distance between two bodies. Galileo's gravity is still a weight-related internal force, compared to Newton's thoroughly dynamical concept of 'gravity'. Yet, Galileo had to re-think the Medieval concept of an internal force, and make it obey 'indubitable principles' so as to demonstrate the law of free fall (for full details of this story, I refer the reader to Massimi 2010).

Galileo's kinematic studies exemplify the perspectivalist strategy of engaging with the past 'from within', rather than 'from above'. Working with the Aristotelian tradition that goes from Hipparchus to the Arabic commentators and Buridan's and Oresme's impetus theory, Galileo could operate within well-trodden paths. He could resort to Archimedes' buoyancy, and Hipparchus's theory of free fall, and introduce gradual changes to key concepts. For example, the change from impetus as an internal force propelling a body, to 'momento' (*momentum gravitatis*) as an internal force that after having propelled the body would gradually decay, causing the body to acquire degrees of speed (*celeritatis momenta*) in its descent. Galileo's breakthrough about free fall did not happen by debunking the Aristotelian tradition (pace Galileo's own rhetoric against Simplicius in *Two New Sciences*). Nor did it happen by selecting working posits in the Aristotelian tradition: for

even Archimedean buoyancy, or Hipparchus' theory were inextricably entangled with 'idle wheels' and not amenable to being imported *tout court* into the Galilean story.

Instead, the Galilean *kinematic* strategy consisted in small theoretical steps and subtle conceptual nuances that ultimately allowed Galileo to turn the corner from the Aristotelian tradition. Galileo's ability to interrogate nature with principles of reason on the one hand (i.e. the indubitable principles from which he demonstrated the law of free fall), and with experiments thought out in accordance with these principles, on the other hand (i.e. both thought experiments with chords and real ones with inclined planes), made the revolutionary shift possible. The perspectival realist can appeal to *this* Galilean strategy to bring the real realist's notion of success home: from 'above' to 'within'.

4. Success and truth across scientific perspectives

But how should the perspectivalist notion of success 'from within' be understood? So far, I have simply suggested that it should not be understood as the ability of inquirers to identify parts of a theory that are essential to success, and hence approximately true. But that can hardly be enough to understand the perspectivalist move that I am here offering to real realism. We need to unpack the slogan.

In what follows, I take my cue from broader discussions on perspectivalism in contemporary epistemology,[2] to propose that success 'from within' should be understood as success with respect to standards of performance-adequacy appropriate to the scientific perspective of the inquirer *when assessed from the point of view of another (either diachronically subsequent or synchronically rival) scientific perspective*.

Given the Aristotelian–Archimedean perspective, and the available evidence for free fall, Galileo could conclude that the Aristotelians failed to satisfy standards of performance-adequacy appropriate to *their own* perspective in the explanation of the phenomenon. For example, Aristotle's theory could not explain the precise mechanism

[2] See for example, Sosa's perspectival coherentism (as part of his virtue perspectivism in Sosa 1991), where the justification for beliefs is a matter of perspectival coherence. Along similar lines for perspectival justification for beliefs, see Haack (1993), and Rosenberg (2002), p. 149: "the reason that we correctly judge that S does not know that p is that, given our richer informational state, we recognize that what we are (stipulatively) entitled to take to be S's epistemic circumstances demand a higher level of scrutiny than we are supposing S himself to have exercised. S therefore, has not satisfied what, from our perspective, are the standards of performance-adequacy appropriate to his epistemic circumstances, and hence, from our epistemic perspective, we judge that, despite his not having acted irresponsibly given the information available to him (judged from his own legitimate perspective on his epistemic circumstances), he has not justifiably come to believe that p". In what follows, I develop Rosenberg's appeal to standards of performance-adequacy to elaborate a perspectivalist notion of *success from within*.

through which equal degrees of speed accrued during the descent, and hence why motion towards a natural place was *uniformly* accelerated motion. The Aristotelians could not obviously be blamed for having believed what they believed about free fall (i.e. that it was motion towards a natural place) given their own scientific perspective. And the proposition that free fall was motion towards a natural place cannot be regarded as relatively true (i.e., true for the Aristotelians, but false for Galileo), on pain of abandoning realism altogether for relativism about truth.

Scientific perspectives—I suggest— provide *contexts of assessments* for scientific claims. Galileo could assess the Aristotelian claims about free fall and find them lacking in satisfying what—from Galileo's own perspective—were the standards of performance-adequacy *appropriate to the Aristotelian epistemic context* (e.g. how come free fall as motion towards a natural place is accelerated motion and not uniform motion as one should expect from the analogy with Archimedes' buoyancy?). In answering these questions, Galileo came eventually to establish a new scientific perspective, from which it became possible to evaluate new claims about free fall. Galileo's scientific perspective, in turn, can be found as lacking in satisfying what—from our own current perspective—are the standards of performance-adequacy appropriate to the Galilean-Newtonian epistemic context (e.g. how to think of Galilean free fall when sense-impressions about the free mobility of rigid bodies and paths of light rays get called into questions, as they were with Helmholtz's mirror sphere thought-experiment? What becomes of the Galilean-Newtonian gravity in a non-Euclidean space?).

Success 'from within' is then the ability of a theory to *perform adequately* with respect to standards that are appropriate to the theory's wider epistemic context—or scientific perspective, as I prefer to call it—when *assessed from the point of view not just of the scientific perspective at stake, but, crucially, from the point of view of other scientific perspectives*. Building on recent important work in epistemology,[3] I suggest the following definition.

A scientific claim (*SC*) meets the criterion of success 'from within' iff:

(a) *SC* expresses a proposition *p* at scientific perspective SP_1

[3] Here I want to latch onto the helpful distinction between context of use and context of assessment in discussions on relativized truth and faultless disagreement—see MacFarlane (2005), (2012); and Marques (2014), among many others. By contrast with MacFarlane, I will not be using this distinction to defend any notion of relativized truth. Instead, I make use of MacFarlane's distinction between context of use and context of assessment to provide a notion of success in science that does not beg the question for scientific realism (i.e. that does not judge past theories on the basis of our current successful theories).

(b) *p* is true (i.e. corresponds to states of affairs in nature) *and* meets standards of performance-adequacy in SP_1 when assessed from other scientific perspectives SP_2, SP_3, SP_4 ...

This definition of success 'from within' vindicates the real realist's expectations for successful posits to track truths in nature (via the first part of premise b.). It is *perspectival* in giving up on any Nagelian view from nowhere and in not taking our currently successful scientific claims as the gold standard for assessing past failures and successes.

Success 'from within' is kosher to the Kantian spirit of perspectivalism in giving due consideration to epistemic agents' (or I should say, scientific communities') commitment to scientific claims (without dismissing them out of hand as sheer errors of the past). Success 'from within' does justice to the historian's anti-Whiggish plea for judging past theories in their own terms and *by their own standards* (not by ours) *when assessed from the point of view of other (diachronically subsequent or synchronically rival) scientific perspectives*. At the same time, it avoids the perils of truth relativism by anchoring success to the truth of perspective-independent states of affairs (i.e. that free fall is accelerated motion is a perspective-independent state of affair that either holds in nature or does not; that accelerated motion is in turn motion towards a natural place as opposed to motion due to an impressed force of gravity is a scientific claim that can be assessed within a given epistemic context or perspective, with its standards of performance-adequancy and so forth).

For example, it is not enough for Aristotelians to be satisfied with their own theory of free fall, for it to count as successful. Scientific communities cannot ratify their own success, if their practices are not also deemed as successful by other communities of inquirers. Yet other communities cannot smuggle in their own standards of performance-adequacy when evaluating other (past or rival) theories.[4]

It was possible for Galileo to assess Aristotle's theory of free fall by Aristotle's own standards (expressed by Simplicius in *Two New Sciences*) and conclude about its inadequacy. As it was possible for William Thomson (later known as Lord Kelvin) in 1847 to assess Carnot's cycle by Carnot's own standards (which included conservation of caloric) and conclude about its inadequacy (when combined with Joule's claim that a quantity of heat

[4] On closer reflection, this is what is to be expected from Kuhnian anomalies and periods of crisis. Anomalies reveal cracks into well-established and well-trodden paradigms, by revealing the inability of the paradigm to handle *in its own terms* an increasing number of persistent problems.

proportional to the mechanical work produced must be consumed in a paddle-wheel experiments).[5]

These examples show important features about the definition of success 'from within' I just gave:

(i) The relevant standards of performance-adequacy for scientific claims are settled in the original *context of use*, i.e. in the scientific perspective in which the claim is first formulated and advanced.

(ii) Subsequent perspectives provide *contexts of assessment* from which it is still possible to evaluate past scientific claims *by their own original standards.*

(iii) Given the richer informational content available to subsequent perspectives, it may be possible for later assessors to regard the performance-adequacy of past claims as lacking in some respects; hence, it is possible for later assessors to either retain or withdraw (in whole or in part) past scientific claims on the basis of their continuing performance-adequacy.

Success 'from within' becomes then a commitment that a community of epistemic agents undertakes to *retain* past scientific claims when their performance-adequacy continues to be regarded as satisfactory, from the point of view of later scientific perspectives. Success 'from within' bears important similarities with the real realist's working posits. Both react against the "blockish holism" of epistemological empiricism. Against scientific realism, they both invite us to a more nuanced re-appraisal of past theories. Against structural realism, they both refrain from cashing out success in terms of structure vs. substance. And both equally stress what might be called the *enactive* nature of scientific success: success is whatever works, or continues to perform adequately.

[5] Following up on Thomson, in 1850 Rudolf Clausius laid the foundations of thermodynamics (let us call it SP_2) by reconciling Carnot's cycle with Joule's ideas. For Clausius envisaged that it was possible to retain Carnot's idea that heat passes from a hot reservoir to a cold one whenever mechanical work is done in a cyclic process, while also abandoning Carnot's additional claim about conservation of caloric. The second law of thermodynamics was born: in any cyclic transformation of thermal energy into mechanical energy, a portion of heat gets dissipated irreversibly (pace caloric theory). Subsequent perspectives, such as Maxwell-Boltzmann statistical mechanics (let us call it SP_3) were still able to evaluate the truth of Carnot's cycle and its ability to meet standards of performance-adequacy at its own time (i.e. measuring engines' efficacy in producing mechanical work). Yet in the light of the richer informational content available to Maxwell and Boltzmann (after Clausius' introduction of entropy), Carnot's overall claim was deemed as requiring a higher level of scrutiny (especially about conservation of caloric and the nature of heat) than Carnot himself could have possibly exercised in the early nineteenth century.

Yet real realists and perspectival realists differ when it comes to the notion of success. For real realists deploy working posits to identify hypotheses that are approximately true by the criterion of success 'from above'. Whereas perspectival realists of the kind I like would urge to deploy the aforementioned success 'from within' to identify scientific claims that—by being *justifiably retained* in the shift from the original perspective / context of use to another perspective/ context of assessment—we have reasons for thinking of as true (to the best of our knowledge). The perspectival slant I am offering to the real realist's 'working posits' is then in terms of commitment of a scientific community to assess and justifiably retain past scientific claims whenever their performance-adequacy continues to be deemed as satisfactory by their own original standards, when assessed *from the vantage point of other perspectives*.

On this perspectival reading, truth is not an *ex post-facto* explanation of the ongoing success of some scientific claims. Instead, truth is built into the aforementioned definition from the ground up, with premise (b) "*p* is true". It is the truth of the propositional content of a scientific claim *and* the ability of the claim to meet standards of performance-adequacy at SP_1 when assessed from other scientific perspectives that ultimately grounds success 'from within'. And I do not mean that it *explanatorily* grounds it. I mean instead that it *ontologically* grounds it. If the propositional content of the claim were false by realist lights (i.e. if there were no such a thing as *p* in nature), even if the claim were hypothetically able to meet standards of performance-adequacy when assessed from other perspectives, the claim would not qualify as successful under the aforementioned criterion of success 'from within'.

Imagine a scientific community in the eighteenth century, who could have built a perfectly consistent scientific system around caloric to advance various claims about the production of mechanical work, thermal expansion, and matter's states of aggregation, among others. Our best eighteenth century scientists failed to distinguish among states of aggregation as physical in nature (and went on to identify water as a *liquid* chemical substance; see Kuhn 1990). But let us assume that our hypothetical community can do better than our own Lavoisier, Dalton, and Carnot and come up with a perfectly good system of knowledge around caloric that meets their own standards of performance-adequacy at the time.

For example, such system proves consistent when offering explanations in terms of caloric for matter's states of aggregation and the production of mechanical work; it gives simple and elegant accounts of how caloric (by being released and absorbed) underlies all

these phenomena; it seems accurate with respect to the evidence available to the community at the time; and so on. Should not we judge—from our own current perspective—such community as having met its own standards of performance-adequacy? Should not we assess its scientific claims as being successful, despite their propositional contents (in terms of caloric) being false? More to the point, who are *we* to conclude that their propositional content is indeed *false*? Are not we re-introducing a much-dreaded view from nowhere to reach such cross-perspectival Grand Metaphysical Conclusion?

5. Objections and replies

Not so fast. What needs be considered in this imaginary case is whether positing caloric can indeed give rise to such a perfect system of knowledge able to meet all the aforementioned standards by the light of the hypothetical eighteenth century community. I contend that it cannot. *Consistent explanations,* first. Assuming caloric as an imponderable fluid—as eighteenth century scientists did, and our hypothetical scientists would also presumably do—would immediately pose severe challenges to any attempt to provide a consistent explanation of mechanical work and states of aggregation. Mechanical work would require caloric to be consumed (pace conservation of caloric); as much as turning water into ice would require removing caloric (qua a shell of imponderable fluid surrounding water's particles) and yet expanding the overall volume. How can water's particles lose part of their volume (by releasing caloric), while also expanding their overall volume? Caloric does not seem to license consistent explanations.

Simplicity next (a notoriously slippery standard, if any). Would caloric provide a simple and elegant account of various phenomena? Caloric could be squeezed out of particles' volumes (assuming a Daltonian model) and get re-attached to them at ease. As simple as that? Well, assuming some mechanism was in place to explain what held caloric attached to the particles of matter; what had the power to detach it from matter and re-attach it to it at will, and so forth. Perhaps some attractive and repulsive forces might do the trick. Or, perhaps electrical fluids. Or, some ethereal substratum (along the lines of Kant's matter of heat). Simplicity is not within easy reach. A complex story would need to be told about the mechanisms underlying caloric's behavior in all these phenomena, mechanisms that can potentially be at odds with each other.

Perhaps *accuracy* with the available evidence fares better than consistency and simplicity when it comes to standards of performance-adequacy. Assuming our hypothetical community has produced a system of scientific claims that are accurate by the

experimental standards available to the community at the time. Such claims must surely be regarded as successful (no matter how false caloric is from our own current vantage point). An analogy may perhaps help here. Suppose I have an accurate story about hedgehogs living in my garden and creeping out at night to collect the mulberries that have fallen on the ground. My story is so accurate that tells me with precision that hedgehogs come at night, between 1am and 2am, from the far right corner of the garden, behind the hedge and collect only the purplest mulberries they can get their spiky backs on. So my available evidence of red mulberries on the ground seems to support the accuracy of my story.

But is accuracy such a malleable standard? Surely, even my garden hedgehogs would have to respond to some mundane questions: do they come out every night? From 1pm or from 2pm? What about the purple spots on the ground that look like old mouldy mulberries? Accuracy (be it the accuracy of a measurement or the accuracy of a theory) comes always in tandem with other standards, such as consistency, fruitfulness, explanatory power, and so on. Our imagined eighteenth century caloric supporters would have to tell a pretty convincing story about how their scientific claims involving caloric were accurate over and above fitting a sample of observed regularities (especially if such sample proved in conflict with others, and the caloric mechanisms envisaged in each case were in contradiction with each other and hard to pin down).

Whatever the standards of this hypothetical eighteenth century community could have been (the list above, of Kuhnian flavor, is only illustrative and is not meant to be exhaustive), the above examples should make it clear that building a system of scientific claims on an ontologically false ground is not going to go very far. The system of claims would soon fail *by its very own standards of performance-adequacy*. And we do not have to resort to hypothetical scenarios. Real historical communities, who entertained standards similar to the ones listed above, came to realize the inadequacy of the caloric theory in the nineteenth century.

Let us take stock. A critic was envisaged that challenged the criterion of success 'from within' on the ground that as long as a community can justifiably be regarded as meeting its own standards of performance-adequacy, the scientific claims advanced on behalf of such standards should count as successful (despite their propositional contents being false). This objection attacks the realist component in my definition of success 'from within', namely premise (b) that says that "p is true". For it seems possible for p to be false and yet still meeting standards of adequacy in SP_1 that make scientific claims about p count as successful. In response, I have shown how if p were false, it would prove in practice

impossible to justifiably meet standards of performance-adequacy in SP_1. And for good reasons too: *ex falso quodlibet*. An ontologically false ground (e.g. caloric) cannot possibly license scientific claims that are arguably consistent, simple, accurate, and so on (unless inconsistency, inaccuracy and so forth are themselves acceptable standards within a particular scientific community).[6] Thus, I conclude that if the propositional content of the scientific claim were false by realist lights (i.e. if there were no such a thing as *p* in nature), it would in practice be impossible for the claim to meet standards of performance-adequacy in its own scientific perspective (and even more so when assessed from other perspectives). The claim would not meet the criterion of success 'from within'.

But is it 'being true' by realist lights *enough* to secure success 'from within'? Are the standards of performance-adequacy themselves idle wheels? Here a different critic is envisaged, who may retort that real realist's working posits ultimately underpin the truth of *p* and my aforementioned definition of success 'from within' collapses onto the real realist's success 'from above', at a closer inspection. The critic may insist that Fresnel's theory worked and proved successful not because it met standards of adequacy at Fresnel's time (e.g. it was fruitful in predicting novel phenomena; it seemed accurate in explaining diffraction; and so forth); but because Fresnel's 'light wave' referred to electromagnetic waves of high frequency. Or better—the critic may continue—Fresnel's theory met those standards *because* its working posits (i.e., electromagnetic waves) were true. And to emphasis the idleness of the standards of adequacy themselves, one could easily invoke consistency with the ether theory as an example (yes, Fresnel's theory was consistent with popular ether theories at the time; yet consistency *in and of itself* does not cut any ice for the success of Fresnel's theory).

In reply, one may consider what would happen to a lone researcher that gets it right without yet meeting the standards of performance-adequacy of her community at the time (perhaps because such community has not quite got to the stage of precisifying standards able to capture the truth of what the lone researcher has just discovered). Should we conclude that the researcher has been successful? Here, I cannot help but sharing Richard Boyd's negative conclusion,[7] although I give a perspectival gloss to what Boyd portrays as

[6] Here a relativist may come to the fore and make this kind of rejoinder (one is reminded here of the familiar story about the Azande and their witchcraft and how standards of adequacy vary from one epistemic community to another; see Kusch 2002 for a helpful discussion). A discussion of relativism would lead me into a territory further afield from the topic of my essay here, and as such I will not pursue it.

[7] Boyd describes the hypothetical scenario of the lone researcher that gets it right but she does not make any contribution to the reliability of our scientific practice unless her success is also recognized as such by a community (Boyd 2010, pp. 217-8).

the sheer social dimension of scientific inquiry. That '*p* is true' by itself is not sufficient to ontologically ground success, *unless p also* meets standards of performance-adequacy at SP_1 when assessed from another scientific perspective.

Consider the astronomer V. M. Slipher, who at the Lowell Observatory in Arizona, throughout 1912-1917 was able to measure with precision the radial velocity of galaxies and to empirically establish that galaxies were expanding a decade before Hubble found the law for this phenomenon, and at a time when Einstein was introducing his cosmological constant in the equations of general relativity to secure instead a static universe.[8] Slipher was the lone virtuouso experimentalist, who got it right in a scientific perspective dominated by general relativity and Einstein's and de Sitter's interpretation of the field equations as implying static solutions. It was only in 1924 that Friedmann first, and Lemaître then (in 1927) introduced models of general relativity that implied non-static solutions to the field equations, with Hubble later introducing the law to measure the redshifting of galaxies in 1929. Once the idea of an expanding universe became a live option for scientists, it became also possible to go back to Slipher's experimental findings and to assess them as meeting the standards of performance-adequacy of the scientific perspective of the time. For example, their consistency with non-static solutions to Einstein's field equations that Friedmann and other cosmologists were bringing to the fore in the 1920s (despite Einstein's cosmological constant). Using Supernova Ia techniques, current cosmologists can still assess the performance-adequacy of Slipher's findings, despite his pioneering work being overlooked for long time by his own peers who had not yet precisified the standards of performance-adequacy appropriate to their scientific perspective.

To conclude, success 'from within' does not fall back onto success 'from above' because ontologically true grounds *in and of themselves* (without meeting also standards of performance-adequacy at the time) are necessary but not sufficient to license success. Scientific success is what a community of epistemic agents acknowledges and welcomes as such at any given time. The truth of the propositional contents of our scientific claims — the first conjunct in my premise (b)—by itself would grant only a view of success from nowhere, a view that no epistemic community (either here now or back then) would recognize as its own.

Success 'from within' has both a realist and a perspectival component. Correspondence with a mind-independent world of states of affairs, and meeting standards

8 Here I draw on John Peacock's account of this episode in Massimi & Peacock (2014).

of performance-adequacy that can be assessed by epistemic agents across perspectives are both key to the success of our scientific claims (of today and of the past). Success 'from within' is not the success of those, who historically happened to be the winners. But it is instead the product of those, who were responsible for the findings *and* for their meeting standards of performance-adequacy that can *still be assessed by us today*.

6. *Envoi*

Fourteen years after *Real Realism*, we are all heirs of Galileo's strategy. We learned from Kitcher's real realism how to tell apart truth from falsehood; how to discern bits that work from idle wheels; and most of all, how to believe in the reliability of the deliverances of our instruments. Empiricists and constructivists of all stripes owe us an argument for maintaining a justifiable degree of skepticism about science and its success. More to the point, they owe us an argument for justifiably retreating into "metaphysical never-never lands" on the face of so many homely arguments for being realists about the things with which we interact all the time.

For myself and for my generation, *real realism* has enticed us to explore new avenues and encouraged us to appraise success and failure across the history of science in a considerate way. Maybe, success 'from above' should leave room for success 'from within'. We stand on Galileo's shoulders by acknowledging our continuity with the past, and our ability to assess past scientific claims by their own lights and from our current vantage point, a vantage point that is neither metaphysically nor epistemically privileged. That is how in my view a *perspectival* slant can help us bring real realism back to the Kantian home, to which it naturally belongs.

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