Jun 4th, 11:00 AM - 12:00 PM

The Value of Judgmental Subjectivity

Paula Olmos
Universidad Autónoma de Madrid

The Value of Judgmental Subjectivity

PAULA OLMOS
Department of Linguistics, Modern Languages, Logic and Philosophy of Science
Universidad Autónoma de Madrid
Avda. Francisco Tomás y Valiente, 1. 28049 Madrid
Spain
paula.olmos@uam.es

Abstract: Kuhn (1977) considered that criteria for scientific theory choice function as values and not as rules what implies: i) the debatable character of their attribution, ii) the gradual nature of their compliance and iii) the necessity to weigh them up in a multidimensional values-based judgment. Kuhn also emphasized: 1) the agent-related nature of processes involving the "recognition of values as reasons" and 2) the non-algorithmic and open character of the "justificatory dynamics of science".

Keywords: epistemic values, objectivity, reasons, scientific argument, subjectivity, theory choice, Thomas Kuhn, value-based argument, weighing.

1. Introduction

Thomas Kuhn's "Objectivity, Value Judgment, and Theory Choice" (included in the book *The essential tension*, 1977, pp. 320-339) is still today a major reference in philosophical debates on the justification of scientific claims. The text tried to respond to widespread charges of relativism directed to *The Structure of Scientific Revolutions* (1962, ^{2}1970a), where Kuhn had assumed that the *underdetermination* of scientific theories by empirical evidence (what Kuhn rather restrictedly calls "proof", Cf. Stanford, 2017) called for "techniques of persuasion, or argument and counterargument" (Cf. 1970a, pp. 151-152) in theory choice.

Certainly, this sounded rather more shocking then than it does now. The more than forty years passed since its publication have seen the development of argumentation theory and argumentation studies and it should not be surprising that we are now in a better position to both clarify and assess what Kuhn wanted to say about these problematic questions. Although Kuhn does not use the concepts and tools developed by contemporary argumentation theorist (not even those already available to him at that time), one of the aims of this paper is showing how most of his insights can now be better appreciated by employing them.

Kuhn's "Objectivity…" (1977) extended and deepened the line of response to received criticism he had already outlined in at least two previous texts:

- the 1969 "Postcript", included in *The Structure*'s second edition (1970a) and,
- "Reflections on my Critics", his contribution to the collective volume edited by I. Lakatos & A. Musgrave, *Criticism and the Growth of Knowledge* (1970b).

His aim was now trying to identify the kinds of "argument and counterargument" that were common in scientific controversies and to expose how they did function in a process of theory choice, with the intention of revealing up to what point *rationality* and *objectivity* were still honored. The difficult part was, nevertheless, that in order to do so, he had to make a

conceptual work of *dissociation* (Cf. Tindale 2010) and propose a new way to understand both notions of *rational justification* and *objectivity*.

In this paper, I'll try to analyze Kuhn's ideas and conceptual proposals using, among other things, the tools and framework provided by H. Marraud's "arguments' dialectic" (2015) whose performance for clarifying scientific argument I have already exploited in my recent papers on abduction (2019, forthcoming).

One of the things that become clear once we face Kuhn's text is that the topics mentioned in its title are approached exactly in the reverse order and I'll do the same here. Thus, Kuhn starts by analyzing the kinds of criteria (kinds of reasons) involved in *theory choice*, revisiting a list he had already offered in previous texts and trying to show that those who accuse him of depicting "theory choice" as a non-rational process are misguided. I'll revise these aspects in Section 2 "The Rationality of Theory Choice". Then, Kuhn states that these criteria work not as rules but as *values* (or *maxims* or *norms*, says he, 1977, p. 330). This is the central argumentative point, as what Kuhn is pointing at –and allegedly clarifying with his illustrative examples from the history of science– is precisely "The Argumentative Workings of Values", as I approach the issue in Section 3. Finally, what remains is to vindicate the kind of *objectivity* (if any) that's behind this kind of exchanges. I'll revise Kuhn's suggestions in Section 4 "Objectivity within the Space of Reasons" and Section 5 "The Rational Dynamics of Science".

My main conclusion (expressed by my own title) is that Kuhn's *dissociative* concept of *judgmental subjectivity*, called for to replace an elusive (ideally *a priori* and agent-independent) *objectivity*, is probably his most valuable contribution, opening the possibility for a more dynamical, practical and definitely agent-based understanding of the exchange of reasons in scientific decisions about theoretical choices.

2. The Rationality of Theory Choice

That there is no algorithmic methodology for systematic theory choice within the sciences was, at the same time, the point of departure of Kuhn's ground-breaking proposal and a well-established result accepted, by then, by most philosophers of science (as Kuhn duly acknowledges).[1] The peculiarity of Kuhn's approach, though is that he does not see this as a regrettable condition, "an eliminable imperfection" but as "the essential nature of science" (1977, p. 330). I gladly translate Kuhn's insight here as boldly stating the *argumentative nature* of science.

Empirical proof –i.e. experimental confirmation (in any case gradual) of the observational consequences implied by theories– is never decisive (it *underdetermines* theory choice), but neither are mandatorily decisive, in an absolute way, the value-based "argument and counterargument" explored by Kuhn. There is always room for disagreement. Furthermore, as Kuhn insists, there is always room for reasonable (even scientifically-based) disagreement. Although there are concrete cases where recalcitrant and stubborn positions in scientific matters might be identified and criticized as such (by the majority of a community), this cannot be done in an algorithmic or unanswerable way. The *argumentative* situation (as it happens in legal matters) remains, as science keeps going on, and theories are assessed and accepted as *currently* valid.

[1] "Most philosophers of science would, therefore, I think, now regard the sort of algorithm which has traditionally been sought as a not quite attainable ideal" (Kuhn 1977, p. 326).

This is essential in Kuhn's account and something that not everybody welcomed in 1977 (or in 1973, when the lecture that originated the paper took place). In his 1980 review on Kuhn's book, particularly centered on this piece, H. Siegel started by agreeing, to a certain extent, with Kuhn. Siegel quoted at length the following paragraph from the "Postcript", of which the text of "Objectivity…" seems to be a more detailed development, and stated that "This paragraph of Kuhn's seems unexceptionable" (Siegel 1980, p. 364):

> Nothing about that relatively familiar thesis [i.e. no algorithm] implies either that there are no *good reasons* for being persuaded or that these reasons are not ultimately decisive for the group. Nor does it even imply that the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness and the like. What it should suggest, however, is that such reasons function as *values* and that they can thus be differently applied, individually and collectively, by men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision (Kuhn, 1970a, 199-200).

But Siegel finally blamed Kuhn for not trying *to solve* this problem that threats the rationality of scientific justification. If the usual criteria are not enough to close the gap, he should be looking for meta-criteria (preferably "external" to *paradigmatic*, self-referential and self-justifying frameworks) in order to overcome, in a more definitive way, disagreement in scientific controversies.[2]

Siegel specially criticized Kuhn's "reluctance to give up the *incommensurability* thesis" (1980, p. 365) that would prevent effective communication and reasonable dispute between scientists working within different *paradigms*. He, ultimately, considers that Kuhn "cannot have it both ways" (1980, p. 366), in the sense that, if he is honestly looking for criteria and good reasons for, if not settling, at least advancing scientific controversies in a rational way, he cannot, at the same time, insist on the idea of *incommensurable* and *incommunicable* paradigms.

I think Siegel was absolutely right spotting this kind inconsistency in Kuhn, but the truth is that in the text of "Objectivity…" the vocabulary of *incommensurability* is remarkably avoided (though not explicitly denied) and there is even a suggestion towards the end of the text about means other-than-linguistic to overcome problems of incommunicability. I'll come to this later. At this point, I will just mention that even abandoning the *blocking version of incommensurability* –something I'm more than happy to do–, I somewhat understand Kuhn's "reluctance" to embrace a too confident view of the possibilities of finding a, however complicated, model of theory choice that finally would *close the rationality gap* in the way required by his critics. That is why I think some redescriptions of his proposals as leading to a complete traditionally understood methodology as R. Nola's (2000) or those who translate them into Bayesian terms as Salmon's or Earman's (Cf. Farmakis 2008) somehow miss the point.

---

[2] Siegel mentions in this regard the work conducted by Scheffler (1967).

And the point is, as I see it, that the context of scientific controversy remains an *argumentative context* (*vs*. a *demonstrative* one) all through reasonable debates and reasonable theoretical choices made. Those defending an innovative and more fruitful and more coherent scientific theory are reasonable and rational as long as they are prepared to present assessable, field-relevant reasons for their choices and those who resist the new proposal might also be reasonable and rational as long as they do the same. And there is no *a priori*, universally applicable, philosophical way to stop it. Only relatively instable, though often *robust*, assessable as properly discussed, but in the end communal, field-dependent choices based on good reasons.[3]

This, I claim, is not renouncing *rationality* but redefining it with a *dissociative* move. Rational people, according to such move, would be those who "give reasons", "ask for reasons", "look for the best available reasons", "criticize other's reasons", etc. and do so mostly using (and sometimes discussing) the standards of the field they are involved in; not those who *know of the right method* for reaching decisions and apply it. Science (as an *argumentative* field) may have agreed on (relatively permanent and even relatively constitutive) basic criteria that act as *sources of reasonable argument*, but, even then, these are not going to dictate or determine choices but just guide discussions. This would mean to advance a *procedural* or *methodological vs*. an *achievement-centered* notion of *rationality*.

Kuhn is happy to look for, describe and discuss those basic criteria that make of scientific endeavor what it is –at least for now (Cf. Kuhn 1983). But he is also eager to specify the way the items in his non-exhaustive list of criteria work in argumentative exchanges (as *values*, says he, and not as *rules*) in order to keep scientific discussion and controversy *feasible* and *recognizable* but, at the same time, *open*. As is well known, the five criteria listed by Kuhn in "Objectivity…", expressed as desirable characteristics or features *eligible scientific theories* should have, are (Kuhn 1977, pp. 321-322):

1. *Being accurate*: i.e. empirically adequate (especially regarding predictions) according to observation and experimentation.[4]
2. *Being consistent*: both internally (showing absence of contradiction) and externally (proving coherent with other theories and background assumptions).
3. *Having broad scope*: theoretical consequences should be of the kind that makes them extensible to phenomena outside its first realm of inquiry in order to avoid *ad hoc* theories.[5]
4. *Being simple*: its explanatory principles should be kept as simple as possible.
5. *Being fruitful*: it should reveal (or help revealing) new phenomena, enlarge the field of inquiry, open new research perspectives.

---

[3] As philosopher of science A. Cordero has put it: "Science simply did not have the clear and eternal form positivists had imagined it have. Nor, for that matter, did it have any invariant form. If, earlier on, the radicalness of scientific innovation had blended smoothly with the formalist tenets of logical positivism, the mixture fell apart when historical research revealed that science changes, virtually at all levels, as its contents develops. Science, it seemed, was not characterizable in essentialist terms of any kind" (Cordero 1991, pp. 400-401).

[4] This is the most traditionally acknowledged Empiricist criterion and, for some philosophers of science, *the only one*, according to the principle of *Knowledge Empiricism*: "if the data alone do not suffice to determine a theory's truth-value, then nothing does" (Douven 2005, p. 282).

[5] This *desideratum* responds to the ideal of *scientific unification* and aims at avoiding the multiplication of explanatory principles (Kitcher 1989).

Even if Kuhn's text does not say it explicitly (although it is rather implied by the term "choice" and by his examples), it seems clear that, in general circumstances, these criteria cannot be just applied *qualitatively*. This is not just a *checklist* that theories must go through. We will typically be in a gradual and *comparative* situation.[6] The idea is to choose the theory that is deemed *more* precise, coherent, ample, simple and (or) fruitful *among the available alternatives*. What Kuhn does emphasize, instead (as he already did in the "Postcript"), is the problematic character of the operative, i.e. *argumentative*, use of such criteria. He mentions two distinct problems (Kuhn 1977, p. 322):

a) "[c]riteria are imprecise: individuals may legitimately differ about their application to concrete cases";
b) "[w]hen deployed together, they repeatedly prove to conflict with one another".

Surprisingly enough, Kuhn says that these difficulties are "relatively familiar" so that he will "devote little time to their elaboration" (Kuhn 1977, p. 322). They are, of course, in the sense that we find them everywhere in our daily lives, but this doesn't mean that they cannot be better clarified. I claim that the examples from the history of science that Kuhn offers in order to just "briefly illustrate" these problems merit some analysis on our part with the help of our contemporary *argumentative* tools.

Because it is precisely in them that Kuhn's final suggestion that the criteria work, (i.e. *argumentatively speaking*) as *values* and not as *rules* is, in practical terms, though perhaps more ostensibly than explicitly, exposed.

3. The Argumentative Working of Values

So we have problem a), *imprecise criteria*, and problem b), *conflicting criteria*. Regarding problem a), Kuhn assumes that there is, again, no algorithm, no univocal rule to *attribute* to a scientific theory the kind of characteristics listed by him. Not even *comparatively* speaking. Thus, comparative attribution of these *desiderata* (as Kuhn calls them in other texts, Cf. Kuhn 1983) is the first *argumentative situation* he describes. Reasons, i.e. substantive reasons, based on *substantive warrants* (not on purely semantic or formal rules)[7] must be adduced for such comparative attributions, and this means that there might be controversies between *opposing* but, in principle, *likely reasonable* comparative attributions.

His example to illustrate this problem is the conflict (end of 18th c.) between the "theory of phlogiston" (defended by Georg E. Stahl) and the "theory of oxygen" (defended by Antoine Lavoisier) in the chemistry of combustion, regarding the first criterion, *accuracy*.

[6] This is not incompatible with there being some threshold of compliance that would simply exclude some manifestly implausible or inadequate theories.

[7] In another paper (Olmos forthcoming 2), I attribute to such *substantive warrants* the following two properties (insofar as they are *verbalized* or made explicit, something that does not always happen or need to happen): i) their *general* (or regular) but typically *non-universal* character (i.e. their not being universally quantified statements) and ii) the *substantive* as opposed to *formal* nature of the relation they express (so that they always mention a respect, a concept, containing the alleged *kind of link* between reason and claim that goes beyond formal derivation).

According to Kuhn, both theories presented a certain degree of *accuracy* –in connection with different aspects of the phenomena involved– that made reasonable the comparative attribution of the criterion in favor of one or of the other, thus:

	Oxygen theory correctly predicts (or explains) **weigh relations** in chemical reactions. Phlogiston theory does not.
The *accuracy* of a scientific theory is attributed on the basis of its predictive (or explanatory) capacity:	**So**
	Oxygen theory is more *accurate* than phlogiston theory.

	Phlogiston theory correctly predicts (or explains) the **chemical similarity between metals**. Oxygen theory does not.
The *accuracy* of a scientific theory is attributed on the basis of its predictive (or explanatory) capacity:	**So**
	Phlogiston theory is more *accurate* than oxygen theory

Acknowledging the rational (and even field-adequate) character of *both* initial attributions, in a situation like this, what rationality dictates is that we keep looking for further criteria to ground our choice (as Siegel sensibly suggested). As Kuhn points out:

> [a] scientist would need to decide the area in which accuracy was more significant. About that matter chemists could and did differ without violating any of the criteria outlined above, or any others yet to be suggested. (Kuhn 1977, p. 323).

Kuhn is right but, stopping here, he does not specify that those who make such decisions will *typically*, in a public and collective sphere as is that of science, be required to offer reasons to back them too, in a *second argumentative situation* that calls for meta-argumentative weighing (Olmos 2016).

I will suggest here that a possible (just one possible) weighing judgment about "the significance of one area above the other", *in this particular case*, could be based on the appreciation of the measurable (i.e. quantitative, metric concepts) over the qualitatively appreciated (i.e. qualitative concepts) that characterizes certain developments in modern (and contemporary) science. This kind of balance between opposing arguments can be diagrammed thus:[8]

[8] *Measurability* (using metric concepts) could be another of those criteria "yet to be suggested" as a criterion for eligible scientific theories –it has been, in fact, by some philosophers of science. Such use of one criterion or *desideratum* to weigh up conflicting attributions of *another* criterion, or conflicts between *different criteria*, will be discussed later.

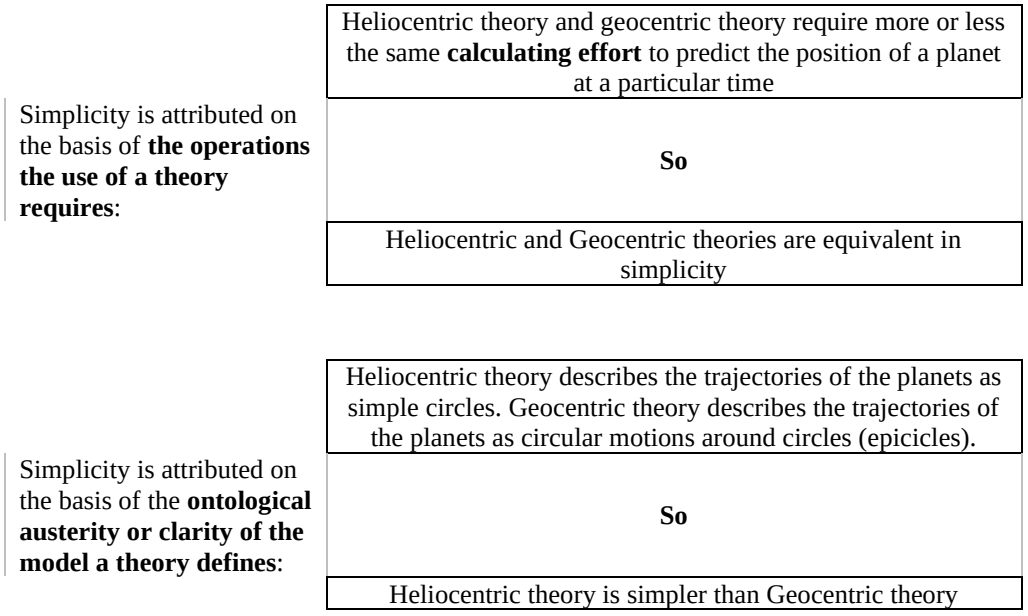
	Weigh relations in chemical reactions are measurable. Chemical similarity between metals is a qualitative concept.				
Metric concepts (magnitudes) characterize *advanced science*:	**So**				
		Oxygen theory correctly predicts (or explains) weigh relations in chemical reactions. Phlogiston theory does not.		Phlogiston theory correctly predicts (or explains) the chemical similarity between metals. Oxygen theory does not.	
	The *accuracy* of a scientific theory is measured by its predictive (or explanatory) capacity:	**So**	>	**So**	The *accuracy* of a scientific theory is measured by its predictive (or explanatory) capacity:
		Oxygen theory is more *accurate* than phlogiston theory.		Phlogiston theory is more *accurate* than oxygen theory	
	So				
	The kind of accuracy shown by oxygen theory seems more promising for scientific development than the kind of accuracy showed by phlogiston theory.				
	So				
	I (*Lavoisier* speaks) choose oxygen theory and recommend that choice				

Notice the agent-related character of the conclusion and its *practical* nature as a *personal* decision and a recommendation to the relevant community. It is *human agents* that recognize non-formal, non-purely-semantic reasons for a conclusion[9] and that propose them *to be discussed* and *assessed* by other human agents.

And yet, in this case, we are still lucky. We have been able to reconstruct the conflicting comparative attributions of *accuracy* as based *on the same warrant* (added by me, but implied by Kuhn's way of presenting the case), what makes them more easily comparable by a rather simple *balance judgment*. But Kuhn is conscious that this is not always so easy. Problem a), imprecision, does not only affect the way we comparatively attribute one criterion, understood in a single, agreed-on, way. It might also be the outcome of different conceptions of (or different ways of attributing) what is deemed to be the *same* criterion.

[9] What J. Woods (2016, p. 101) conceptualizes as properly understood *inferences*, insisting on "the depth of the implication-inference divide" and the distinction between the *logical space* of relations between statements and the *psychological space* of consequence recognition and drawing: "If consequence-having obtains in logical space, consequence-spotting occurs elsewhere. It occurs in psychological space – in the spotter's head, indeed in his "recognition subspace", as we might say. If so, consequence-drawing likewise occurs in psychological space, in a sub-region of psychological space which I'll call his "inference subspace", within which consequences are believed for a reason, and the reason is supplied by the premisses from which that conclusion follows. It is easy to see that consequence-spotting and consequence-drawing are natural processes. When they occur they do so on the four-dimensional wordline of some or other individual".

This is what happens in another of Kuhn's historical examples: Heliocentric (Copernicus') *vs*. Geocentric (Ptolemy's) astronomical systems. He will use this example mainly to illustrate problem b), i.e. conflicting criteria (namely the conflict between *simplicity* and *consistency*), but he further acknowledges that *simplicity* itself might be comparably attributed on also *conflicting grounds* thus:



According to Kuhn's presentation of the historical case, the *conflicting grounds* (different warrants) present here a final result in which there is, at least superficially, one *tie* and one *win* for Heliocentrism. We could say that Heliocentrism takes the overall *win at simplicity*. But it is not difficult to imagine that conflicting grounds could issue two opposite assessments (as in the case of the oxygen and the phlogiston). The looked for meta-criterion, then, would have to weigh *what's different* in both (the warrants, in this case, not the way the same warrant allows for conflicting comparative attributions).

For the sake of the argument, let us imagine that Kuhn would have suggested that (at least at some point in its development) Ptolemy's theory required *less* calculations although demanding a more entangled geometry. We still could attribute to early 17th c. scientists, as Galileo or Kepler, a decision favoring Heliocentrism based on, for example, a choice for realistic *vs*. merely instrumental scientific theories (another *value-like* criterion). I represent such a weighing argument in the following diagram. (Notice, I'm not suggesting the historical accuracy of this line of argument, just trying to illustrate the argumentative workings of such a possibility):

	Simple calculations for predictions are valued for instrumental reasons alone. Geometrical austerity is valued for the ontological plausibility of the models it describes.				
Science aspires to more than instrumental theories (Scientific realism):	**So**				
		Heliocentric theory requires more calculating effort to predict the position of a planet at a particular time than geocentric theory	<	Heliocentric theory describes the trajectories of the planets as simple circles. Geocentric theory describes the trajectories of the planets as circular motions around circles (epicicles).	
	Simplicity is attributed on the basis of the calculations a theory requires:	**So**		**So**	Simplicity is attributed on the basis of geometric austerity:
		Geocentric theory is *simpler* than heliocentric theory		Heliocentric theory is *simpler* than geocentric theory	
	So				
	The kind of simplicity shown by Heliocentrism is more promising for scientific endeavor than the kind of simplicity shown by Geocentrism				
	So				
	I choose Heliocentrism and recommend that choice				

Notice that here a *qualitative* aspect of simplicity is being valued over a *quantitative* one (contrary to what was assumed in the first weighing on oxygen and phlogiston theories) in a somewhat reasonable and recognizable (though not unanswerable) way. This means that if, as suggested in note 5, should measurability (or other value-like criterion) be added to the list, it would not help to close, in any algorithmic way, the *rationality gap*. That measurability is a recognizable scientific criterion does not mean that it is *always* going to play a decisive role (or the same kind of role) in theoretical choices.

But in these comments I'm going rather beyond Kuhn. He really just presents this example assuming that heliocentric theory is finally *simpler* in the proper way and that the real problem arises, in this particular case, from the conflict between *simplicity* and another criterion, namely *consistency*. He states, furthermore, that both theories appeared to be "internally" consistent but that Geocentrism remained more akin to the human perception of a stable planet and more consistent with received theories in physics (about projectiles, water pumping etc.).

So, in addition to the comparative judgments on *simplicity*, 17th century scientist had to count on the following comparative (and in this case *conflicting*) judgment on the *consistency* of both theories:

External consistency is attributed on the basis of compatibility with other accepted theories and background assumptions:	Geocentric theory (stationary Earth) is consistent with human perception and with received physics. Heliocentric theory (moving Earth) is not. **So** Geocentric theory is more "externally" *consistent* than heliocentric theory

The way Kuhn refers to Galileo and Kepler's bold acceptance of Heliocentrism is not initially very clear about the reasons behind their choice. He only says that the simplicity of Copernican theory was "a fact vitally important to the choices made by Kepler and Galileo" (Kuhn 1977, p. 324).

Nevertheless, we may assume, for example (again, no historical accuracy is intended), that their prevision of what this theory could bring in terms of a unified physics for both the Earth and the Heavens was crucial. It seems reasonable enough. This would mean, again, using some of the remaining criteria in the list (either *broad scope* or *fruitfulness*) as a meta-criterion, to "solve" (always in a non-algorithmic way), conflicts created by the clash between other items in the list, thus:

	Geometric simplicity will allow for further developments in understanding the Universe and for the comprehension of earthly phenomena within a more general framework				
The *broader scope and fruitfulness* of a theory make it preferable:	**So**				
		Geocentric theory (stationary Earth) is consistent with human perception and received physics. Heliocentric theory (moving Earth) is not.	<	Heliocentric theory describes the trajectories of the planets as simple circles. Geocentric theory describes the trajectories of the planets as circular motions around circles (epicicles).	
	External consistency is attributed on the basis of compatibility with other accepted theories:	**So**		**So**	Simplicity is attributed on the basis of geometric austerity:
		Geocentric theory is *more "externally" consistent* than heliocentric theory		Heliocentric theory is *simpler* than Geocentric theory	
	So				
	I (Kepler or Galileo) choose Heliocentrism and recommend that choice				

I think it is starting to be clear how this kind of argumentative workings is similar to that of *values* in other kinds of argumentative settings. In, for example, political or legal discussions or, simply, in daily life and quotidian decisions, as Kuhn suggests (1977, p. 330). They clash, they are difficult to order, they might be used to account for choices but then those choices might be contested on their account. They keep demanding from us to offer reasons and meta-reasons in defense of the choices they influence (not determine). And still it is not as if we (or scientists in this case) would have nothing to cling to:

> Values like accuracy, consistency, and scope may prove ambiguous in application, both individually and collectively; they may, that is, be insufficient basis for a *shared* algorithm of choice. But they do specify a great deal: what each scientist must consider in reaching a decision, what he may and may not consider relevant, and what he can legitimately be required to report as the basis for the choice he had made. (Kuhn 1977, p. 331).

We are now in a position to sum up what *functioning as values and not as rules* means *argumentatively speaking*. It means at least that:

i. Their attribution *functions* as a "value judgment" ("value claim", Cf. Schiappa & Nordin 2014, p. 71ss), that is, an evaluative not a factual expression, conveying an *agent-related* positive attitude towards the object to which it is attributed. Arguments supporting such attributions *are to be understood*, therefore, as "evaluative arguments" (not theoretical ones).

ii. Attribution of values is *typically* a *matter of degree*. We should not expect qualitative "value claims" assessable in a bivalent way; we will *always* be in a gradual and comparative situation. Thus, the *norm* associated to a value (its attribution warrant) will typically not be of the kind stating "necessary and sufficient conditions", but substantive grounds for comparative attribution. As we have seen, the *norm* of attribution itself might also have *to be selected* among several recognizable possibilities. It is usually not a settled question how we attribute what we consider a *value*.

iii. Values are associated to contexts and practices where their satisfaction or maximization is assumed as *desirable* or commendable. That means that the positive attitude towards the evaluated object will *typically* be used as a reason for its *eligibility* in what finally becomes a "practical argument" describing and prescribing an action (in this case, a "theoretical choice" that implies a series of practical decisions).

iv. Similarly, as theorists of legal argument know well, *value-based practical choices* typically respond to *principles of maximization* (e.g. "Act so as to maximize the satisfaction of values v_1, $v_2 \ldots v_n$") not to strict decision rules that allow for *subsumption* (e.g. "If conditions a, b, c, obtain, do X", Cf. Alexy 1983). That is, actions are selected from a somewhat indeterminate set of available ones as *maximizing* the joint satisfaction of different values. We do not count on simple decision rules prescribing actions based on the objective description of situations. That is a consequence of all the things we have already said together with the basic consideration that the contexts and

practices in which *values* count are typically *multi-value contexts*. If there was just one value, and that value would not be subject to the problems of comparative and conflicting attribution we have described, it would not really *function as a value*. Criteria such as those presented by Kuhn *are not values* in any essential way, they *function as* values. If *accuracy* (the usual suspect) was the only scientific value, as some think it is, it would not function *as a value* anymore. It would just either determine or underdetermine theory choice, but not make room for any discussion. A *value approach* implies a *multiplicity of values* and all the argumentative problems (or exuberance) concerned.

v. Choices based on *value claims* (practical decisions based on reasonable evaluations) call for complicated (sophisticated) argumentation. They typically imply demands for backing (because warrants and warrant choices are not obvious), weighing procedures and are finally open to further demands for better grounding.

That scientific justificatory practice responds to these characteristics is Kuhn's claim. And it challenges certain expectations regarding science as a *demonstrative* (or at least *simply evidentiary*) realm, while it seems to respond better to its characterization as a *forensic activity* (Woods 2017, pp. 143-144). And a highly disputed one, for that matter, that has given place to extensive, time-consuming and necessarily collective controversies that still leave room for further arguments.

This does not mean that any kind of reason and basis will be considered appropriate and relevant in scientific choice, but just that no simple, *allegedly decisive*, logical (formal or semantic) scheme will account for its rationality. This is good news for us because there is a lot of argumentative work to do just in terms of the appropriate description of such practices and the recognition of the field-related norms that govern them. Kuhn's was in any case a step in the right direction.

4. Objectivity within the Space of Reasons

Kuhn finally addresses the crucial issue of *objectivity*. If things stand how he has described them. If scientific choices are based on the debatable attributions of not really well-defined (or variably definable) criteria that *function as values* that, can be just satisfied to a certain (comparable) degree, that clash with each other, and that *argumentatively* justify choices that can still be resisted… where does the supposed *objectivity* of science remain?

Paradoxically enough, Kuhn's answer is that the *objectivity* of science is really based on *subjectivity*. Not any kind of subjectivity but *judgmental subjectivity* (Kuhn 1977, pp. 336ss). This is Kuhn's second *dissociative* move. He supports this idea by exposing the *judgmental* character of value-claims that are not necessarily just a question of *personal taste* (with no further reasons expected) but are nevertheless necessarily a question of *personal judgment* (because it is *people* that judge) for which rational grounds can be (and typically will be) asked:

> What is discussable in my remark [that a certain film is a *potboiler*] is not my characterization of my internal state, my exemplification of taste, but rather my *judgment* that the film was a potboiler. […] scientists may always be asked to explain their choices, to exhibit the bases for their judgments. Such judgments

> are eminently discussable […] If my critics introduce the term subjective in a sense that opposes it to judgmental –thus suggesting that I make theory choice undiscussable, a matter of taste– they have seriously mistaken my position (Kuhn 1977, p. 337).

In empirical science we cannot resort to completely *objectified* agent-independent (formal or semantic) relations between also completely objective contents. Empirical (*vs*. formal) science is not the kind of practice that's determined by logical, impersonal, relations that obtain within a *logical space* (where "there are no people", as J. Woods says, 2016, p. 101). Conclusions drawn (by scientists) in empirical science are instead *guided* by substantive relations (based on warrants) conceived of, recognized and proposed for recognition and acceptance *by agents* and *for other agents* (and related to their personal experiences that make of them rational agents).

Science is assessed in the *public space* of argument, where the *psychological space* of inference, as construed by J. Woods (2016) is either put to communicative work (as in Woods' *inference-based account of reasons*) or is ultimately founded (as in an *argument-based account of reasons*). In either case, as Kuhn insists once and again although using a different vocabulary, all this implies the *agent-related* nature of processes involving the *recognition of values as sources of reasons*. No algorithm is going to take the decision for a scientist. Scientists must move within a *space of reasons* where they are asked to be relevantly *rational* (Cf. Section 2), that is, summon, recognize and give appropriate and field-related reasons and be prepared to substantively respond to other agents' counter-reasons.

Objectivity within the space of reasons is, therefore, a *result* of collective, public discussion, not a prerequisite of the kinds of reasons involved in such a discussion. As a global (rather holistic) *result*, it is founded on the joint concourse of individual, *personal* and *subjective*, but in any case relevantly *judgmental*, contributions.

Again such kind of *objectivity* is not conceptually construed, as a *success term* (or an *achievement-centered* notion) acquiring instead a *procedural* or *methodological* ring. The result of a well conducted scientific discussion (according to the standards of the field) would be *objective* (as a *currently* valid theory is deemed to be objective) and yet subject to further questioning and new value judgments (personally made by human agents) in view of the contributions of other human agents. And, as Kuhn maintains, this is not "an eliminable imperfection" but "the essential nature of science".

5. The Rational Dynamics of Science

Finally, for Kuhn, science and scientific choices are not only *rational* and *objective in spite of* the absence of an algorithmic or quasi-algorithmic methodology, but precisely *thanks to* such an absence. The positive evaluation of this situation for science as a collective endeavor on the part of Kuhn comes from his estimation of "time" (research time, reflection time, discussion time) as a *commodity* that improves scientific validation. The problem with decision algorithms, even of *probabilistic* algorithms, is their *supposedly instantaneous* workings. Good science needs time, as it needs people using that time:

> Copernicus's system, for example, was not more accurate than Ptolemy's until drastically revised by Kepler more than sixty years after Copernicus's death. If Kepler or someone else had not found other reasons to choose heliocentric

> astronomy, those improvements in accuracy would never have been made, and Copernicus's work might have been forgotten (Kuhn 1977, p. 323).

Allowing for time to count is what makes possible the *justificatory dynamics of science*. In Kuhn's terms, standard criteria (i.e. the array of scientific values) should have a role in choices made at the early stages of a theory, and the recognition (by others than the proponent) of this necessarily imperfect compliance with them, as field-relevant guides, is what makes possible the survival of a new idea that may eventually be improved (Kuhn 1977, p. 331-332).

If time is needed –and it has been needed, since science is what we understand it to be–, then scientific methodology and the measure of its correction, i.e. its *rationality*, the *objectivity* of its conclusions, cannot be of any *instantaneous* kind (and this is, precisely, what happens when we philosophically construe them as *success terms*). As Kuhn points out, if scientists would share an algorithmic methodology "all conforming scientists would make the same decision at the same time" (Kuhn 1977, p. 332), and this would give no time either for a prospective (and yet much imperfect) new idea to enter the pool of candidate theories or for the traditional one to yet survive, awaiting new developments.

This high estimation of "time", of "collective work" and also of the scientific "division of labor" is present in the (either simply descriptive or normative) proposals of many contemporary philosophers of science, especially those of a pragmatist inclination (Cf. Hacking 1983, Kitcher 2012). However, the consequences this has for the *argumentative* understanding of *scientific justificatory practice* have not been, in my opinion, sufficiently stressed. And I would add, that Kuhn's old paper is closer to what we need in that direction than other more contemporary suggestions.

Time is also, ultimately, one of Kuhn's *mitigators* of the problems of *incommensurability* between paradigms or disciplinary matrixes (Kuhn 1974). New theories often come with new ideas about how to order and weigh different values –even if we assume that the basic core of scientific values remains constitutive of science. But if everything comes at the same time and offered to be accepted in one pack, it is difficult to avoid problems of circularity and the barrier between those who accept and those who reject the whole pack becomes rather insurmountable. In Kuhn's words:

> Clearly, if such value changes had occurred as rapidly or been as complete as the theory changes to which they are related, then theory choice would be value choice, and neither could provide justification to the other (Kuhn 1977, p. 336).

The other *incommensurability mitigator*, as I understand it, mentioned in "Objectivity…" (in its last paragraph, and with very little elaboration) is the possibility that scientist that do not share a paradigm, a conceptual network or a common vocabulary may nevertheless still "exhibit to each other, not always easily, the concrete technical results achievable by those who practice within each theory" (Kuhn 1977, p. 339).

There could be room, I claim, to render this idea into the framework of "visual argument from ostension" (Marraud 2018), but it is not my intention to do so at this time. My point is, instead, that in his 1977 paper, Kuhn really struggled (even against the usual implications some of his most cherished ideas) to look for ways to account for the *rationality* of scientific choices. If, in Siegel's terms (1980, p. 366) he could not "have it both ways" (see Section 2), it seems that, at least in this paper, he chose to emphasize one of them, and

bring the other to a minimum. And the *way* he chose, even if he did not really make use of argumentative vocabulary, is more than highly compatible with some of our contemporary approaches in argumentation studies.

6. Conclusion: The Value of Judgmental Subjectivity

In this paper I have tried to do two things. One is using some analytical and conceptual tools developed within argumentation theory in order to explore and clarify Kuhn's insights in his well-known 1977 article "Objectivity, Value Judgment and Theory Choice". The other is defending the idea that, beyond the particulars of the set of scientific values proposed by Kuhn and even beyond their *rational* (related to reason-giving or argumentative) workings, it is Kuhn's suggestion that the ultimate normative concept of science is not exactly objectivity but *judgmentability* that deserves our greatest notice.

Such judgmentability is *subjective* insofar as it is agent-related (only human agents *recognize* non-algorithmic reasons and *draw* conclusions thereof) and should be conceptually construed not as a set of *a priori* conditions (of success) a certain content may prove to have for being *judgmental*, but as the condition a certain content *acquires* by being presented for its examination, questioning and judging (by others than its proponent) in the public arena of *argumentative practice*.

This *judgmental subjectivity* is, thus, the decisive value that covers, in fact, all kinds of argumentative practices. Insofar as we understand the scientific way of establishing (still fallible) conclusions as based on collective argumentative practices, *judgmental subjectivity* will be its basic condition.

In a paper critically examining the "practical character of scientific rationality", J. Vega (2011, pp. 145-146) enounces three *normative conditions* regarding the kinds of reasons that we may expect would constitute the "practical identity of science":

a) *Publicity*: any reason adduced within a scientific community must be *publicly* expressed.
b) *Reliability*: all good reasons are linked to processes evaluated according to their *reliability*.
c) *Reflexivity*: scientific community is involved in a collective process, on which its *epistemic authority* relies, carried out through *reflexive strategies* leading to the continuous assessment of the epistemic position from which it exercises its activity.

Conditions a) and c) can be easily understood as demanding an *argumentative* setting (as the one I have depicted here) for scientific choices. Condition b) is more problematic as its compliance could be exactly one of the things that is at stake in scientific argumentation. The options with this condition would be trying to flesh it out in terms of further conditions for *reliability* (as some epistemologists do) or expecting from the observance of conditions a) and c) that scientists themselves would come out with adequate standards. What Kuhn shows us, in any case, is that such standards, even if well established, stabilized, agreed on and honored by the relevant community will not be of the kind to completely determine *the right answer* so as to dispense us from conditions a) and c).

Acknowledgements: This work has been made possible by funds provided by the Spanish Ministry of Science, Innovation and Universities through Research Project PGC-2018-095941B-100, "Argumentative Practices and the Pragmatics of Reasons".

References

Alexy, R. (1989 [1983]). *Teoría de la Argumentación Jurídica: Teoría del discurso racional como teoría de la fundamentación jurídica*. Madrid: Centro de Estudios Constitucionales.

Cordero, A. (1991). Evolutionary Ideas and Contemporary Naturalism. In: E. Agazzi & A. Cordero (Eds.), *Philosophy and the Origin and Evolution of the Universe* (pp. 399-439), Amsterdam: Kluwer Academic Publishers.

Douven, I. (2005). Empirical Equivalence, Explanatory Force, and the Inference to the Best Theory. In: R. Festa, A. Aliseda & J. Peijnenburg (Eds.), *Confirmation, Empirical Progress, and Truth Approximation, Essays in Debate with Theo Kuipers. Volume 1* (pp. 281–313), Leiden: Brill

Farmakis, L (2008). Did Tom Kuhn actually meet Tom Bayes? *Erkenntnis* 68, 41-53.

Hacking, I. (1983). *Representing and Intervening*. Cambridge: CUP.

Kitcher, P. (1989). Explanatory Unification and the Causal Structure of the World. In: P. Kitcher & W. Salmon (Eds.), *Scientific Explanation* (pp.410-505), Minneapolis: University of Minnesota Press.

Kitcher, P. (2012). Towards a Pragmatist Philosophy of Science. *Theoria* 28 (2), 185-231.

Kuhn, T. (1970a). *The Structure of Scientific Revolutions (2nd ed.)*. Chicago: University of Chicago Press.

Kuhn, T. (1970b). Reflections on my critics. In: I. Lakatos & A. Musgrave (Eds.), *Criticism and the Growth of Knowledge* (pp. 231-278), Cambridge: CUP.

Kuhn, T. (1974). Second Thoughts on Paradigms. In: F. Suppe (Ed.), *The Structure of Scientific Theories* (pp. 459-482), Urbana IL: University of Illinois.

Kuhn, T. (1977). Objectivity, Value Judgment and Theory Choice. In: T. Kuhn *The essential tension* (pp. 320-339), Chicago: University of Chicago Press.

Kuhn, T. (1983). Rationality and Theory Choice. *Journal of Philosophy* 80, 563–70.

Marraud, H. (2015). Do Arguers Dream of Logical Standards? Arguers' dialectic vs. Arguments' dialectic. *Revista Iberoamericana de Argumentación* 10, 1-18.

Marraud, H. (2018). Arguments from Ostension. *Argumentation* 32**,** 309–327.

Nola, R. (2000). Saving Kuhn from the Sociologists of Science. *Science and education* 9, 77-90.

Olmos, P. (2016). Meta-argumentation in deliberative discourse: Rhetoric 1360b 05-1365b 21. In: P. Bondy & L. Benacquista (Eds.), *Argumentation, Objectivity, and Bias: Proceedings of the 11th International Conference of the Ontario Society for the Study of Argumentation (OSSA), 18-21 May 2016* (pp. 1-17), Windsor, ON: OSSA. http://scholar.uwindsor.ca/ossaarchive/OSSA11/papersandcommentaries/73

Olmos, P. (2019). Abduction and comparative weighing of explanatory hypotheses. An argumentative approach. *Logic Journal of the IGPL*, jzz038, *https://doi.org/10.1093/jigpal/jzz038*

Olmos, P. (forthcoming). An Argumentative Approach to the Justification of Abduction. En B. Verheij & C. Dutilh Novaes, eds., *Proceedings of the 3rd ECA Conference, Groningen 24-27 July 2019.*

Olmos, P. (forthcoming 2). Commentary on T. Blair's "Is there an informal logic approach to argument?" In: B. Verheij & C. Dutilh Novaes (Eds.), *Proceedings of the 3rd ECA Conference, Groningen 24-27 July 2019.*

Scheffler, I. (1967). *Science and Subjectivity*. Nueva York: Bobbs-Merrill

Siegel, H. (1980). Objectivity, Rationality, Incommensurability, and More (Review of *The Essential Tension*), *The British Journal for the Philosophy of Science* 31/4, 359-375.

Stanford, P.K. (2017). Underdetermination of Scientific Theory. In: E.N. Zalta (Ed.), *The Stanford Encyclopedia of Philosophy* (Winter 2017 Edition), URL: https://plato.stanford.edu/archives/win2017/entries/scientific-underdetermination/.

Tindale, C.W. (2010). Ways of Being Reasonable: Perelman and the Philosophers, *Philosophy and Rhetoric* 43/4, 337-361.

Vega, J. (2011). ¿Es la racionalidad de la ciencia una especie de racionalidad práctica? In: A.R. Pérez Ransanz & A. Velasco Gómez (Eds.), *Racionalidad en ciencia y tecnología* (pp. 135-148), México: Universidad Nacional Autónoma de México.

Woods, J (2016). The fragility of argument. In: F. Paglieri, L. Bonelli and S. Felletti (Eds.), *The Psychology of Argument. Cognitive Approaches to Argumentation and Persuasion* (pp. 99–128), London: College Publications (Studies in Logic 59).

Woods, J. (2017). Reorienting the logic of abduction. In: L. Magnaniy and T. Bertolotti (Eds.), *Springer Handbook of Model-Based Science* (pp. 137-150), Dordrecht: Springer.