On Scientific Evaluation and its relation to Understanding, Imagination, and Taste

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Abstract

This essay addresses the apparent conflict between the desire to view scientific evaluation as objective and the realization that it is inevitably subjective. I first argue that the same holds with respect to understanding, and that the subjective basis of both understanding and evaluation does not contradict their claim to universality (to the extent that such a claim can be made at all). Thus, I believe that the concerns regarding subjectivity are overrated. Next, I point out the significant role that imagination plays (or rather must play) in the evaluation process. Finally, I reject the claim that personal taste plays a significant role in scientific evaluation, arguing that almost all that is attributed to taste is actually not a matter of taste (provided that one uses a reasonable definition of taste).

Contents

1 Introduction

The evaluation of scientific works is an integral part of science.[1] My guess is that many readers will agree with the previous assertion, but interpret it in a more narrow sense than I intended it. Specifically, they may think of the various formal evaluation processes that are an integral part of current academic life (e.g., journal review, hiring and promotion decisions, and grant proposal review), but I am also referring to the informal evaluation that take place whenever scientists decide whether to study and/or teach a specific work.

Scientists are often bothered by the apparent conflict between the desire (or even need) to view scientific evaluation as objective and the realization that it is subjective at least in the sense that such evaluation boils down to experts' opinions (which may vary). They feel uncomfortable about lacking clear guidelines (or recipes) that govern and/or underly such a central scientific activity. This essay is not going to provide such guidelines, and thus may not eliminate the source of their discomfort. Instead, I will try to argue that they should not feel discomfort.

Let me start by pointing out that any creative human activity shares the fate of lacking clear guidelines (or recipes), and this holds, in particular, for learning, studying, and conducting research. Two comments are in place. Firstly, indeed, some advice is often offered regarding some aspects of such activities, but such advice (especially when it is general) is far from anything close to guidelines or recipes. Secondly, as will be argued in this essay, scientific evaluation is a creative activity. Thus, the lack of guidelines regarding scientific evaluation is not an abnormality within the range of scientific activities, but rather conforms to its norm.[2]

In fact, the situation is not radically different when we turn from the notion of evaluation to the notion of *understanding*, which refers to the subjective cognition of facts and theories that are available in the public domain. Here, too, we lack guidelines for gaining understanding or for testing it, although we do spend most of our time in these activities. Indeed, *the problems involved in evaluation are merely a manifestation of the problems involved in understanding*, and so we may start by discussing the latter (which certainly plays a major role in the former).

The understanding's claim to universality. Although our understanding is a result of a subjective process, it *appears* to us as universally valid (i.e., we are forced to view it this way, although we know that our understanding may be wrong). The above claim can be interpreted both as a psychological claim and as a philosophical one. Before trying to justify these claims, let me note that a similar justification can be applied to the analogous claims regarding evaluation.

Viewing the process of gaining understanding as an iterative process in which the current understanding of a rational agent is combined with some publicly available facts and theories to form a new understanding, I claim that the agent must consider the new understanding inevitable. Indeed, any "free choice" made in the process is a choice made by the agent, and so it was determined by the agent's own state of cognition. That is, the *freedom of choice*, which we talk about as

[1] When using the phrase "evaluation of scientific work" I mean the evaluation of their importance, which assumes their validity. See further discussion at the very beginning of Section 3.

[2] Furthermore, in contrast to a common myth, scientific and mathematical fields also lack rules that allow for *absolute* verification of claimed results. In particular, these fields make a host of assumptions before even starting (cf., e.g., the causality postulate or the axioms of mathematics), and so their results are always conditioned on these assumptions. Moreover, the accepted criteria of verification (rigor) change with time (even in mathematics!), and thus the "verified claims" are merely *verified with respect to the level of verification* (rigor) *that is acceptable at the time* (i.e., the contemporary criteria). Lastly, note that there is a gap between the criteria that is held as an ideal (at a time) and the criteria that is typically applied (at the same time), a gap illustrated by the ideal-conforming proof of $1 + 1 = 2$ in Russell and Whitehead's *Principia Mathematica*.

outsiders, *is the agent's freedom from being fully determined by circumstances external to it*. The source of this freedom is the agent's state of cognition, but this state is fixed from the agent's point of view. This does not mean that the agent (Alice) knows beforehand what she is going to do and/or think, but it means that in retrospect *she recognizes her choices as determined by herself*.[3]

The foregoing argument suggests that a rational agent must consider her own understanding as *universally valid*. This is because she reached this understanding by applying her mental faculties (which are universal w.r.t rational beings) to publicly available facts and theories, hereafter referred to as the knowledge base. Based on this consideration she may also expect others to have the same understanding, although in reality this is rarely the case.[4] The latter fact is mainly due to the fact that, typically, different agents have different access to the knowledge base (throughout the *entire* process of gaining understanding),[5] but this does not effect the above consideration: Alice must view her own understanding as forced upon her; she cannot understand things differently than she understand them (at least not at the very same moment). Indeed, I claim that the last assertion means that, in some sense, *Alice may view her understanding as universally valid, and that this sense is the strongest possible available to rational beings that lack a direct access to the absolute truth*.[6] Thus, each agent's understanding carries a claim of universal validity. Applying the same analysis to the notion of scientific evaluation, I propose (as my first thesis) that *a scientific evaluation carries a claim of universal validity*.

The other two main theses made in this essay are less philosophical. But before turning to them, I wish to address a question that may be raised regarding each of these theses: Is it a description of reality or a normative imperative? My brief answer is that they are both. That is, each thesis describes what is involved in a scientific evaluation, whereas the normative part calls for understanding the role of whatever is involved so as to utilize it better. For example, the first thesis asserts that each (subjectively generated) scientific evaluation carries a claim of universal validity, whereas the normative suggestion is not to be perplexed by this but rather focus on correctly applying the relevant mental faculties.

Evaluation combines understanding and imagination. In a nutshell, an evaluation (or judgment) of importance of scientific work W combines an *understanding* of the scientific field to

[3]Indeed, the forgoing paragraph can be interpreted both as a psychological claim and as a philosophical one.

[4]Of course, when two agents discuss their different understandings, the result may be that their individual understandings change. That is, each would reach a new understanding, which is determined by the agent's prior understanding and the contents of the discussion. But, once this new understanding is formed, it is again perceived as universally valid.

[5]In addition, agents may differ in the power of their mental faculties (but not in its basic nature), and disagreement may also arise from the application of the mental faculties (e.g., errors or lack of application). Still, I claim that almost all differences in understanding can be traced to differences in accessibility to the knowledge base.

[6]Philosophically inclined readers may note that the foregoing argument assumes that understanding is at all possible (where the term understanding is endowed with some sense of universal validity). Indeed, a full argument must address this concern, and may justify this assumption by observing that it is implicit in questioning the possibility of *obtaining* (universally valid) understanding. That is, asking "how can we obtain universally valid understanding" assumes a notion of universally valid understanding. The gap between universal validity and the subjective processes (which are supposed to establish it) is bridged by (1) starting from these subjective processes and (2) defining the lowest level of contingency that can be reached from them as (a meaningful notion of) universality. This line of reasoning is reminiscent of Kant's "Copernican Revolution" (which plays a key role in all his critiques), with the crucial difference that Kant's idealistic view ignores the social circumstances by which rational beings are conditioned (and focuses on "categories" that are common to all rational beings). Thus, Kant's account refers to abstract rational beings that have unlimited cognitive power, whereas my account refers to humans that are conditioned by their social setting and are restricted in their access to the public domain (i.e., the knowledge base) as well as in their inference power.

which W belongs with the exercising of *imagination*. I believe that the role of understanding in this process is widely acknowledged, but the fact that imagination plays a crucial role seems to be often ignored. The point is that evaluating the importance of W requires evaluating both its past and future influence on the field. Clearly, *evaluating W's future influence requires imagination* (i.e., imagining the future development of the field). However, *also evaluating past influence requires imagination*, since one may need to imagine the state of the field without W in order to realize which developments were influenced by W (and to what extent).

Both understanding and imagination are subjective, and thus the resulting judgment is subjective. Still this does not mean that the judgment is arbitrary, far from it: As argued above, if these mental powers were honestly and properly applied, then the judge must expect others to agree with the judgment. (N.B.: A proper application requires not forming judgment on things that one does not understand, nor forming judgment without allowing some free play of the imagination.)

Nevertheless, scientists (and more so administrators) are troubled by the subjective basis of judgment and seeks ways to avoid the "curse of subjectivity". Failing to realize that no such way exists, they turn to various statistics with the illusion that these statistics provide objective measures of scientific importance. Interestingly, while they are unwilling to rely on expert opinions (since these are subjective), they are willing to rely on meaningless data (i.e., statistics that lacks a sound connection to what one really cares about).[7]

On taste. The subjective basis of judgment is often misunderstood as implying that judgment is a matter of (or reduces to) arbitrary personal taste. My last thesis explicitly confronts this fallacy, arguing that (1) the role of taste (within the context of scientific evaluation) is greatly overrated, and (2) taste is far from being personal and/or arbitrary. Starting with (2), I claim that taste is mostly a social construct (i.e., it is developed under the heavy influence of various social groups to which people belong), and that it is not arbitrary but rather functional (for these groups and/or their elites). Thus, taste actually represents an alignment with social groups. To establish (1), I claim that, in intellectual contexts, social alignment has little direct effect, whereas its indirect effect is manifested in the adaptation of certain articulated views (which are part of the individual's understanding) not by unarticulated taste. That is, it is true that the social circumstances influence both the person's understanding and the person's taste, but my claim is that *taste plays a minor role in intellectual activities, which are dominated by cognitive processes*.

The rest of this essay. In what follows, I expand, articulate, and provide a wider perspective on the foregoing issues as well as discuss some related issues. I think it is useful to start by reviewing the basic principles (or ideals) of Science, and see what they may say regarding the question of scientific evaluation. This is done in Section 2. Next, in Section 3, I clarify the notion of importance, which is the focus of scientific evaluation (and judgment). The next three sections deal with the three topics discussed above; that is, universal validity (Section 4), the role of understanding and imagination (Section 5), and issues of taste (Section 6). These three sections can be read independently of one another. (Philosophy averse readers may skip Section 4, and just "carry home" the claim that the problems involved in evaluation are merely a manifestation of the problems involved in understanding.) A few additional issues are discussed in Section 7.

On a remotely related essay of mine. In a sense, the current essay compliment my essay "On Struggle and Competition in Scientific Fields" (*SIGACT News*, March 2012).[8] The said essay

[7] See further discussion of this point towards the end of Section 4 (under "Subjectivity").

[8] Also available from my web-page `http://www.wisdom.weizmann.ac.il/~oded/on-struggle.html`

contrasts the actual research activity, which determines importance, with auxiliary attempts to evaluate importance for various purposes, distinguishing legitimate purposes (e.g., circumstances in which scarce resources have to be allocated) from superfluous competitions. The current essay ignores the purpose of the evaluation and focuses on the evaluation itself. As stated up-front, in addition to the allocation of resources (e.g., publication space and personal positions), evaluation takes place also for individual purposes (i.e., deciding what to teach, study, and research). Interestingly, some scientists find it less imposing to apply scientific evaluation for their individual purposes than to apply the same for the allocation of resources. Needless to say, this (understandable) emotional problem has no real justification.

2 The Ideals of Science

Before even starting, I wish to stress that according to my view *ideals are demands posed to the privileged rather than false consolations offered to the underprivileged*. I believe that the bad reputation that ideals have accumulated in history is a direct consequence of their vicious abuse as means of justification for oppression. Thus, ideals have a proven oppressive potential. Still, this does not mean that we should dispose of ideals, because doing so means eliminating the most potent source of (hope for) emancipation and progress. Insisting that ideals be only used as demands directed towards the privileged seems to maintain their progressive (or emancipatory) potential without risking the regressive (or oppressive) danger.

Science is a systematic enterprise aimed at constructing and organizing rational knowledge about the (natural and human) environment. A widely agreed formulation of its basic ideals (or principles) was proposed by Merton in 1957.[9] These ideals include a commitment to *universalism* (of criteria), *communalism* (i.e., disseminating knowledge), *disinterest* (i.e., freedom from external motivations), and *organized skepticism* (i.e., methodological doubt). In the rest of this section, I address the question of what can these ideals tell us with respect to scientific evaluation.

Universalism: This ideal is reflected in the demand that the judgment of a scientific work be based on the universal mental faculties of understanding and imagination (rather than be attributed to vague notions of taste). Indeed, different judges may differ in their understanding and in their imagination, but they are all required to judge according to the best of their understanding and imagination, which in turn stem from a universal source.

Communalism: This ideal is reflected in the demand that the judgment of a scientific work be based on the work's influence on the relevant research community, which means that knowledge contained in the work has to be disseminated. Specifically, this ideal is reflected in the definition of importance, which focuses on such influence (see Section 3).

Disinterest: This ideal is reflected in the demand that the judgment of a scientific work be based on the work's benefit to the field, which in turn is defined as interest in a certain type of questions. Any other interest is viewed as external, and disinterest means freedom from any such external interest. Hence, this ideal is also reflected in the definition of importance, which ignores external interests and influences.

[9] The terms in italics are due to Merton, whereas I assume responsibility for the clarifications and/or interpretations provided in parentheses.

Organized skepticism: Admittedly, this ideal is less obvious to apply in the context of judgment of scientific works. Still, it can be fruitfully applied if methodological doubt is understood to stand for questioning our own understanding, which includes the "common wisdoms" of the field. It becomes less fruitful if understood as promoting doubt in the new ideas presented in the new work. There is no need to promote such doubts, since they are anyhow the normal reaction to anything new. Instead, doubts should be promoted with respect to this normal reaction.

Originality (sometimes also listed among the ideals): The ideal of originality (which is associated with novelty and progress) further supports the demand to be open to new ideas. Note that the demand is not that the evaluation should be original, but rather that the evaluation should welcome and encourage originality (i.e., it should credit originality in the evaluated work).

3 Importance

Scientific evaluation, in the sense discussed in the essay, is concerned with the *importance* of scientific work, not with their correctness, which is taken for granted here.[10] (Indeed, one may use the term "evaluation" also with respect to the verification of validity, but then it will be distinct from the evaluation of importance, which is the topic of this essay.)

The importance of a scientific work (within a field) is proportional to this work's *influence on the development of the field*, where the influence may be direct and/or indirect. Direct influence consists of *explicit use* of the work in subsequence work as well as explicit use of works that are directly and significantly influenced by the original work. Indirect influence includes a wide range of possible ways in which a scientific work may influence subsequent research, *including a revision of the interpretation of the current contents of the field* (i.e., the way some element of contents is understood by some individual scientists or even by the research community at large). Note that the contents of the field includes the known facts, theories, paradigms, and techniques as well as the set of problems that the field faces. Thus, an indirect influence may take the form of a change in the way known elements and/or challenges are understood. Such influence is typically indirect, because it may not modify the formal contents of these elements (and is thus not used explicitly), but rather the way they are understood. Consequently, such an influence may be hard to trace.

I wish to stress that works of greater importance tend to have more indirect than direct influence, especially when their main contribution is conceptual.[11] This is because a significant conceptual contribution revises the way that the community understands some of the contents of the field. Such a revision occurs in the scientists' minds, and may be communicated in surveys and/or books, but it is typically not stated explicitly in subsequent works (i.e., in scientific articles, one rarely sees a sentence like "Our view of this problem has been greatly influenced by ..."). Furthermore, the authors of subsequent works may not even be aware of the influence of a past work on the way a problem is currently presented. In contrast, when one uses a result of a prior work, one makes explicit reference to that result (even when one does not cite the original work (which may

[10] Typically, the correctness of a scientific work is not disputed. The exceptional cases occur mostly in disciplines (and periods) in which there exists an active conflict between alternative conceptual framework and/or paradigms. (Such a conflict may either be temporary, as in the case that the discipline undergoes a paradigm shift, or be persistent, which may eventually lead to a division of the discipline.)

[11] By a conceptual contribution I mean not only the introduction of conceptual frameworks and techniques, but also a revision of the conceptual landscape by technical results. Let me clarify the latter point by indicating that in many cases a fundamentally important result influences the field not merely by making the result available as a tool, but rather by changing the perspective on other results and problems (e.g., a new result may change the perceived importance of prior works, eliminate some past problems, and give rise to some new problems).

be the case when the result is currently taken for granted)). The foregoing in reflected not only in conventions of scientific citations but also in the consciousness of the scientists. In both cases, direct influence is much more apparent than indirect influence, and in particular the latter does not appear in any citation statistics.

The difference between direct and indirect influence can also be demonstrated by considering expository work (i.e., surveys and books). Typically, such works do not present any new contents, but rather offer an organization, structuring, and interpretation of known contents. Such expositions may have a significant influence, but this influence is mainly an indirect one (since it amounts to influencing the understanding of the existing contents). Hence, such works may have significant indirect influence and little (if any) direct influence. Nevertheless, when such expositions are cited, typically this is not done for acknowledging the insights that they offer (which may be their most important contribution), but rather as an easy way of providing a reference to some standard element of contents (to which they might have contributed nothing).

The importance of a scientific work evolves with time, but the evaluation is done at a specific point of time. Such an evaluation may refer either to the proven importance (i.e., the effect that the work already had in the past) or to the potential importance (i.e., the effect that the work is believe to have in the future). Hence, the importance of a work (defined as its accumulative influence) never decreases with time (even when the related research direction is later proved to be non-fruitful).[12] Each work starts with zero proven importance, although even at this starting point its potential importance may be soundly evaluated (or just speculated) to be significant. With time, the potential importance of a work may (or may not) materialize; and, typically, at some point, one stops talking of its potential importance and refers only to its (proven) importance, since at that time the actual importance dominates the potential importance. (Fundamentally important works escape the latter fate; they always have a potential importance that significantly exceeds their current importance, since they are believed to influence also much future work (which is clearly a pure potential).)

In this context, one may talk of the trajectory of importance (at a certain time) defined to be the rate of growth of importance (at that time).[13] Indeed, fundamentally important work maintain a steep trajectory of importance over time. In contrast, the trajectory of importance of a work may decrease and even become zero (e.g., when the community perceives the related research directions as reaching a dead-end).

Although the notion of importance is well-defined (theoretically), evaluating importance (in practice) is highly non-trivial. Needless to say, evaluating the future importance of a work (i.e., evaluating the potential importance of a work) is even harder. Indeed, these evaluation tasks are the focus of the current essay.

A parenthetical comment. The foregoing paragraphs refer to the importance of a work *within a given field*, as reflected in the focus on the influence on the development of the field. This notion

[12] The fact that a scientific work was useful in the past is not eliminated by the fact that the directions of research that used it were proven to lead to a dead-end (i.e., are not useful). In other words, the history of the field is always present in it (and, in fact, this past – including its dead-ends and errors – is the basis of the present). On the other hand, a work that is useful to directions of research that continue to flourish (i.e., are useful) grows in importance with time (because it gets used, at least, indirectly by more and more works).

[13] Often, when researchers talk of "importance" they actually mean what I have defined as the trajectory of importance. This may be reflected in phrases such as "the importance of X for current research", which seems to correspond to the current (or recent past) value of the trajectory of the importance of X. Regardless of its name, the notion of the trajectory of importance is related to the yet immaterialized potential importance (i.e., to the future influence of the work).

of importance is the one being evaluated by scientists (who belong to the field), and it ignores the question of the importance of the field to society at large. By the latter question I refer not only to the principled importance of the field, but also (or actually mostly) to the question of the importance of the current activity in the field (when compared to an alternative hypothetical direction that the field could have taken). This question is avoided by our definition of importance, which refers to influence on the given research activity and speculations regarding its evolution (which are all based on its current state), but does not relate to whether or not an alternative research activity (or program) would have been more valuable and/or fruitful for the field.[14] Recall that evaluating the importance of a work within a given field, which is the focus of the current essay, is sufficiently hard. Requiring such an evaluation to take into account the value of the current research activity in the field (when compared to unavailable alternatives) would have made the task an impossible one. (Indeed, the latter comparison is impossible to make when the alternatives are not available.)

4 Subjective Universality

Why do we consider a specific scientific work important and/or interesting? How do we justify our judgment? Why do others share our opinion or disagree with it?

Asking these questions indicates that we believe that judgment regarding such matters ought to carry a universal claim of validity, whereas we are unaware of any universal rules of determining importance (i.e., rules that can be used as a basis for such a judgment). I do not believe that one can change this state of affairs, but I believe that it makes sense to try to clarify the situation and argue that we should not be too perplexed by it.

A bewildering detour (into aesthetics), which may be skipped. Before attempting the foregoing, it may be useful to note that Kant was faced with a far more puzzling problem when developing his *Critique of Aesthetic Judgment*, since his starting point was that universal rules of beauty do not exist.[15] Nevertheless, Kant was able to resolve the problem of universality versus subjectivity in judgments of beauty, to his own satisfaction (in 1790). Specifically, he argued that a *claim to universal validity* (regarding judgments of beauty) is possible without rules that govern the validity of specific claims; that a claim to universal validity is based on verifying that certain conditions were satisfied in the process of forming judgment, but this does not guarantee that the conclusion reached will be agreeable to others. More concretely, under certain conditions, we

[14] The notion of a research program and the question of the extent to which it is valuable and/or fruitful and/or progressive was put forward by Lakatos in the 1960s. A research program is a sequence of theories that share a conceptual core and it is progressive to the extent that the theories in the sequence make progress. This progress may be empirical (i.e., providing explanations and/or predictions for a wider scope of phenomena), conceptual (e.g., introducing new concepts, richer structures, or simplifications of prior structures), or technological. A degenerated program offers no real progress, and tends to bypass (or avoids) difficulties by declaring them to be "none of our problem". Lakatos argues that, typically, the question of whether a research program is progressive or degenerated can only be answered in retrospect (when compared with alternatives that are not available in real time). Specifically, a research program may appear to be progressive, whereas it is actually in degenerated.

[15] Here I assume that most readers believe that universal criteria regarding the importance of scientific work do exist (or may exist in principle), but are merely unknown to them and/or are inapplicable by them. I guess that few readers believe that such criteria cannot exist in principle (regardless of their complexity and/or the feasibility of finding them); such readers would be at a similar situation as Kant was with respect to judgments of beauty. Personally, I believe that the rules of determining importance are implied by the definition of importance, but are infeasible to apply. Specifically, an absolute understanding of the environment and of the knowledge base at any point in time (plus a perfect power of imagination) allows for determining the importance of each scientific work.

may form judgments and expect others to agree with us, although we have no way of "leading" them to agree with us. The latter impossibility (of guaranteeing agreement with others) is a direct consequence of the lack of universal rules determining beauty, whereas the possibility of expecting agreement is based on our knowledge that we satisfied the conditions.

Back from the detour. Likewise, all that this section will offer is a discussion of what is involved in forming judgment regarding importance, and when we can expect and/or demand agreement, although we may not be able to lead others to agreement. The impossibility of guaranteeing that agreement be reached is a consequence of our lack of knowledge of universal rules determining importance, whereas the possibility of expecting agreement is based on our knowledge that we satisfied certain conditions in the process of forming our judgment. Following Kant's "Copernican Revolution" (cf., his critiques), let us see how this is possible.

As hinted above, the starting point is the question itself (i.e., the question of the possibility of *obtaining* universally valid judgments of scientific work). This question indicates that we have a notion of such (universally valid) judgments, while we are aware of the subjective nature of the process involved in forming judgment. The contents of the question boils down to asking how can a subjective process yield a result that may be perceived as universally valid. In a nutshell, the answer is that if the process consists of applying mental faculties that are common to all rational beings (i.e., understanding and imagination), then we may perceive its result as universally valid, although the result depends not only on our mental faculties but also on the contents of our cognition (i.e., what we understand, which also determines what we can imagine).

Indeed, while we are aware that what we understand may be different from what others understand, we have no choice but to perceive what we understand as *forced* upon us, and thus as universally valid. Thus, we may perceive a judgment that we reach, by applying our mental faculties to what we understand, as being universally valid. Let me stress that the actual judgment is not necessarily correct (e.g., we may lack information and/or understanding or just make an error), but we have no choice but to perceive it as universally valid. It is universally valid *to the highest extent possible in reality*: Each rational being that understands the same as we understand would have reached the same conclusion. Disagreements arise either from different understandings or from failures to properly apply the mental faculties.[16]

In other words, the universal claim to validity of judgment does not guarantee that its contents is correct, because the contents may be based on lack of understanding or on a failure in the mental process. But this possible deficiency or failure does not provide reasonable ground for objecting the universal claim to validity of judgment, because in the same way one may object any claim to truth (made by any human). Indeed, our claims are always subjected to (and conditioned on) the best of our understanding.

In practical terms, the foregoing implies that scientific disagreements (and in particular in judgments of importance) can always be traced either to different understandings or to errors (i.e., failures in proper application of mental faculties). The former case is far more common. This suggests that disagreements could be eliminated, in principle, provided that differences in understanding can be eliminated. But, as hinted in the introduction, I do not think that we can obtain universal (and feasible) rules for eliminating and/or reconciling differences in understanding. To summarize, I claim that differences in scientific evaluation are similar to differences in scientific understanding; the former are merely a manifestation of the latter.

[16] As argued in Section 6, the common attribution of disagreements regarding scientific judgment to differences in taste is misguided: Almost all that is attributed to taste is actually a matter of understanding.

Subjectivity. Note that the aforementioned claim to universal validity does not free us from the "curse of subjectivity" (so feared by scientists). In contrast, the foregoing clearly states that our judgment depends on our understanding and imagination, which are both of subjective nature. Still this does not mean that the judgment is arbitrary, far from it: As further discussed in Section 5, the judgment is determined by the judge's understanding (and imagination), which arise from the application of universal mental faculties to the public knowledge base. The subjective basis (or dimension) of judgment arises from the different circumstances in which different individuals obtain their understanding, since these circumstances greatly influence their access to the public knowledge base.[17] In contrast to common opinions, this subjective basis (of scientific judgment) has little to do with personal taste. Specifically, as argued in Section 6, (1) the role of taste (within the context of judging specific scientific works) is greatly overrated, and (2) taste is far from being personal and/or arbitrary.

As stated above, scientists feel very uncomfortable with the subjective nature of scientific evaluation and seek to escape it (i.e., replace it by some objective method). The same and more holds with respect to administrators (since they have no chance of obtaining an evaluation by their own understanding of the field). Their search for an objective replacement reminds me of the search for the *philosopher's stone*, with the difference that failures are less easy to point out (in lack of objective methods for pointing them out...).[18] The currently popular suggestions are various bibliographic statistics. Needless to say, each of these measures is objective; that is, it will yield the same value for any given work, when applied by any administrator (who knows how to apply it).[19] But what do these measures actually measure (other than the statistics that underlies their definition)? Needless to say, the promoters of these measures would like to argue that these measures reflect scientific importance, but in order to make the case they must first provide a reasonable definition of importance (e.g., akin to the one provided here) and then show that their statistical measure reflects importance. However, a definition of scientific importance will have to reflect importance as evaluated by the scientific community, which would bring these advocates back to the domain of subjectivity.[20]

Back to universality without rules. Turning back to the universality of judgments of importance, we note that while these judgments do not follow rules they often refer to examples, which are viewed as exemplars of perfect cases regarding importance.[21] These cases are established as perfect exemplars without referring to rules that may explain why they constitute perfect cases (regarding importance). Furthermore, a posteriori, these exemplars act as yardsticks (although such an action cannot be perceived as a real recipe).

[17] See further discussion of "the social angle" in Section 5.

[18] Indeed, the promoters of the various objective alternatives are not impressed by scientists' claims that these alternatives fail to measure the actual importance of scientific work, since they view these claims as subjective. In insisting on objective refutations, these promoters ask for a philosopher's stone that would demonstrate that their own stone can not turn cheap metal into gold.

[19] Indeed, this subjective condition (i.e., the 'know how') is often ignored.

[20] Let me mention that more sophisticated advocates of bibliographic statistics may argue that they do not intend to replace subjective evaluation but rather reduce its amount. For example, they may suggest a two-step process in which first a bibliographic measure is calibrated to provide a good correlation with importance (as defined here), and next the measure is used to replace the traditional evaluation of scientific work. I doubt one can obtain a sufficiently good correlation for such an application, bearing in mind that we need very high correlation levels (e.g., when deciding hiring or promotion).

[21] My discussion of exemplars of importance mimics Kant's discussion of exemplars of beauty in his *Critique of Aesthetic Judgment*.

A parenthetical comment. The foregoing discussion referred to hypothetical rules for determining importance and blurred the distinction between rules for reaching judgment and rules for verifying it. The distinction was immaterial since we believe that there are no effective rules for neither, and our focus was on what can be said under such circumstances. Still, the lack of rules for verification (of importance) is actually the more disturbing phenomenon, since the lack of rules for reaching judgment is akin to the lack of rules for many other creative activities.

5 Understanding and Imagination

Having argued that scientific evaluation is possible (in the sense that it may claim universal validity), we turn to the question of how is it formed. As hinted before, such an evaluation (as any cognition process) calls for a combined action of understanding and imagination on the given material (i.e., the scientific work). The role of understanding in the process is quite obvious, and so I stress the role of imagination. Let me briefly discuss this role before turning to a more comprehensive discussion.

Obviously, imagination plays a key role in trying to evaluate the potential influence of a given work on future research (as required in the evaluation of potential importance); that is, future research must be imagined. It is less obvious that imagination plays a key role also in evaluating the past influence of a given work (as required in the evaluation of proven importance), yet this is the case because such an evaluation requires taking into account indirect influences and not only direct ones. Tracing indirect influences of work W requires imagining an alternative world in which W does not exist, and asking how would such a world be different (e.g., which works that exist in the current world would not even exist in the alternative world, and how would works that would also exist in the alternative world be different). Furthermore, even when evaluating direct influences, one may need to imagine an alternative world in order to grasp the full magnitude of the direct influence.

Indeed, I claim that the role of imagination in the evaluation of scientific work is underrated, and a good question to ask is why is this the case and what are the consequences of this underrating. Let me postpone these questions for the time being.

Understanding. By understanding I mean the internalization of publicly known facts and theories, hereafter referred to as the knowledge base. In contrast to common views, the knowledge base does not consist mainly of raw facts, but is actually dominated by things (mostly theories) that we heard from others or read in books, articles, manuals, and private memos. Actually, the knowledge base contains very few raw facts; what we know is almost solely obtained from (oral or written) texts.[22] Anyhow, our understanding is the result of our mental processing of a tiny portion of the knowledge base, where this portion was selected by us.[23] Thus, our current understanding is a combination (or a synthesis) of our prior understanding and the knowledge base. (Actually, imagination often plays a role in this synthesis, since the prior understanding and the new data

[22] Even in experimental sciences, one never sees raw facts, but rather gets a combination of sensations with (private or public) concepts that interpret these sensations. Here I refer not only to theories that offer predictions, but rather to theories about the experiment itself (including theories regarding the experimenting apparatus). For example, when you look in a microscope you interpret what you see according to a theory that refers to the microscope. (Actually, even when you look at objects with your bare eyes, you are employing a theory about the relation between your recorded image and the three-dimensional source of that image.)

[23] Our selection may be affected by various social circumstances, which are further discuss below (cf., "the social angle").

rarely fit perfectly and some "free play" is needed to fit them together.[24] Still "understanding" refers to the result of the synthesis, whereas the process of forming understanding involves also the action of imagination.)

Before moving to a review of imagination and its role in forming judgments, let me note that understanding is subjective, although it is aimed at universal validity. Our new understanding is the result of applying our mental processes to the knowledge base, under the supervision and interpretation of our prior understanding. Hence, our new understanding is not fully determined by the knowledge base. Furthermore, this iterated process (of gaining understanding) is influenced not only by our mental faculties but also by our social circumstances, which may limit our access to some portions of the knowledge base or encourage our access to other portions. However, the circumstantial aspect of this process is not perceived by us; we are forced to view the process as determined, since what we understand appears as forced on us. We cannot view our understanding otherwise (because each thought views itself as inevitable).[25]

Imagination. In contrast to our understanding, which is always rooted in the (actual) past, our imagination often refers to the future (or to a hypothetical past). Still, our imagination is based on our understanding; that is, the range of possibilities is determined by our understanding and our imagination is free only within these limits (where these limits also play a positive role, because (inconceivable) total freedom is not very fruitful).[26] On the other hand, imagination is pivotal for forming understanding, and understanding crystallizes the past action of imagination. In the context of judgment of scientific work, imagination is limited to the evaluation of the (potential and actual) influence of the given work, and is not required to invent alternatives to that work. Let me elaborate a bit.

In the context of the evaluation of scientific work, the role of imagination is to answer hypothetical questions regarding the future and the past. Starting with the future, the question is what is the possible influence that the given work may have on the future development of the field. This requires not only understanding what the work says and where the field stands right now, but also detaching these understandings from their concrete contents and allowing them to take more vague (and undetermined) forms. Such an (imaginative) "abstraction" of our understanding allows for estimating how potentially useful the given work is for the possible development of the field. (Yes, I am aware that the above sounds extremely lofty and vague; as I claimed up-front, there are no recipes regarding the evaluation of scientific work, and the above description of part of the role of imagination in it is merely an illustration of this claim.)

Turning to the past, the question posed by a scientific evaluation is to what extent did the given work influence the development of the field. This requires understanding not only what the work says and where the field stands right now, but also where it stood prior to that work and how it would have developed without it. Obtaining such an understanding is highly non-trivial, since the actual past and present are embedded in our understanding and so imagination is required in order to move from the actual past and present into a hypothetical one. The imagining of such a hypothetical reality is similar in nature to the imagining of the future.

[24] Hence, any non-trivial cognition process (let alone a creative one) calls for the combination of (1) existing understanding (say, concepts), (2) given external sensations (say, experiences), and (3) imagination (say, free play of undetermined and/or possible concepts).

[25] The thought is free a priori but determined a posteriori. Beforehand, we do sense a freedom regarding the future contents of our next thought, let alone the very decision whether to think at all. But looking backwards, (at least) the contents of the thought appears to us as being determined by our prior state of understanding.

[26] Imagination is not a tactful expression for arbitrariness. Imagination is always employed within given constraints, and it is fruitful with respect to a situation when it relates to the situation.

Let me comment that a rigid understanding of the current contents of the field hinders the evaluation of new works, since it does not allow room for imagining future developments. (A similar comment applies with respect to the history of the field and the evaluation of the influence of a past work on it.) In contrast, a deeper understanding of the field, which is bound to be flexible (rather than rigid), assist in the task of evaluating work in it.

The social angle. As mentioned in the discussion of understanding, our social circumstances may limit our access to some portions of the knowledge base and/or encourage our access to other portions. These social circumstances are most influential in the early stages of constructing our understanding (i.e., in our primary education), but they also influence later stages including our undergraduate and graduate studies as well as the rest of our research career. Different social circumstances offer different access to facts and theories, regardless of whether these are communicated orally or in writing. In particular, different (institutional) schools may endow their graduates with different understanding of the same field. The research community (e.g., the peer group) also plays a role, by highlighting a subset of the facts and theories as well as common interpretations and opinions. All of this is affecting our understanding, but I view these effects as being integrated in our understanding.

A social phenomenon that deserve a separate discussion is the appeal to authority (e.g., "X is important because A said so" or "solving problem P is important because it was raised by A"). Such behavior should be viewed as delegation of judgment to a chosen authority, where the delegator's understanding is reflected in the choice of the authority. Needless to say, such delegation should be avoided whenever one can judge on one's own, but delegation may be a wise decision if one realizes that one lacks necessary understanding. The delegation of judgment may be direct, as when the authority Alice is asked by Bob for an evaluation (and Alice's evaluation is then used as a basis of Bob's evaluation), or indirect as when Bob refers in his evaluation to opinions attributed to Alice. But in both cases, it is not that Bob submit to Alice's taste (or that Bob's taste is to follow Alice), he submits to her evaluation. That is, the delegation is on the level of cognition and understanding, not on the level of taste and personal biases.

Let me now return to the question of why is it common to underestimate the role of imagination in the evaluation process. In my opinion, the primary force acting in this direction is the commitment of the scientific culture to objectivity and its strong aversion towards subjective phenomena, which are so central to the action of the imagination. (It is true that the same may be said of creativity, which is worshiped by the scientific community, but in this case the subjective process can be totally decoupled from its objective result, and so the aversion does not seem to be active.) Note that the aversion towards subjective phenomena is not a personal tendency that develops spontaneously among scientists, but is rather a socialized attitude (i.e., it is socialized during the process of becoming a member of the scientific community). A second force that acts to underestimate and undermine the role of imagination in scientific evaluation is the temporal structure of the scientific field; that is, the conservative strategies employed by the elites (i.e., those who hold positions of prestige and/or power) tend to favor conservative evaluation procedures, which are associated with a lesser role for imagination.

The latter assertion also suggests what happens when imagination is undermined in the evaluation process: Indeed, the consequence is that the evaluation is conservative, which means that it tends to consider only (potential and past) *influences that are clearly visible*, which in particular means that these influences refer to elements of contents (e.g., results) that *are in the mainstream of the current state of the field*. Furthermore, works that relate to the mainstream contents will be assigned additional credit for just being so. Consequently, *importance will be evaluated based on a*

rigid understanding of the current state of the field, while failing to account for its dynamic nature (e.g., the fact that the contents of the mainstream may change).

6 The Sociology of Taste

In this section, I will argue that taste is mostly determined by social circumstances (but this determination is not arbitrary but rather functional), and that reference to taste does not explain much when it comes to scientific judgments.

Here, a key distinction is drawn between taste and intellectual views and/or principles. Taste is *implicit, non-verbal, vague, and practice-oriented*, whereas views and/or principles are explicit, verbal (or at least verbalizable), and theoretic. Bourdieu has often said that intellectuals tend to make the mistake of intellectualizing the practical behavior of ordinary people, whereas these behaviors are often dominated by taste. My claim is that *we should not vulgarize the behavior of intellectuals by attributing to taste behaviors that are governed by clear views and principles.*

I wish to stress that principles are not necessarily more immune to error than taste; similarly, they are not more immune to disagreement or disapproval. The difference is that principles can be articulated. I will claim that *most of what is attributed to taste, in the context of scientific judgment, should actually be attributed to principles and views*, and that the latter may be conceptualized as part of the scientist's understanding of the field.

Note that I do not claim that taste plays no role in our scientific activity. I just claim that taste plays a very small role in scientific evaluation. In contrast, taste plays a central role in our activity when it comes to the design of illustrative figures, the layout and organization of texts, and the actual structure and prose of texts.

6.1 How is taste determined?

In contrast to a common saying, *taste is not a personal matter*. Taste is largely determined by socialization processes, firstly in the socialization to the society at large and to its primary groups (e.g., family and friends), but also in the socialization into secondary groups such as the profession (or discipline). Indeed, the unique personality of an individual also has an effect, but one should bare in mind that the personality is largely determined by the biography, which in turn is largely determined by the social circumstances.

Thus, the taste of an individual is, to a large extent, common in social groups to which the individual belongs, and indeed this taste is implicitly promoted by these groups. In particular, the "scientific taste" of a scientist is, to a large extent, determined by the scientific field to which he/she belongs. The most dominant component of this taste is an interest in a certain type of questions, but this taste is determined by the very act of joining the field.[27] Thus, there is an agreement within the field on the importance of the field at large. Of course, this leaves much room for disagreement (regarding importance), but (as argued below) these disagreements are not to be attributed to differences in taste.

Before proceeding, note that taste (as determined by socialization) is not arbitrary in any way. The socialized taste is the one that is perceived to be functional (i.e., useful) for the social group or at least for its "ruling elite" (or "ruling class").

[27] Indeed, our decision to join a field is based on our general understanding of the world, but within the field itself the field's importance cannot be addressed (rationally). The field's importance is an axiom of the field, and this axiom is supported by a feeling of self-evidence, which in turn may be conceptualized as taste. The social processes of the field strengthen and promote this feeling.

The foregoing does not contradict the apparent variety in tastes among people (even among members of the same scientific field). The variety arises from the fact that each individual belongs (and/or did belong) to several social groups, and so his/her taste is effected by the tastes of all these groups. Since different individuals belong to different groups, a big variety of tastes arises, but none of these tastes is personal or arbitrary.

6.2 What does taste explain?

Common taste within social groups explains much of the coherence within the group (e.g., it smoothens the interactions among its members). Thus, taste explains and/or reflects much of what is common in the behavior within the group. However, taste explains less when it comes to disagreements (among members of the group), and typically there exist much more clear explanations (for these disagreements). Specifically, a disagreement that is attributed to difference in taste can actually be traced to differences in social alignment (i.e., membership in other social groups), and it is much more enlightening to spell out these differences rather than remain at the vague level of talking about differences in taste. Furthermore, as argued below, in an intellectual setting, such different alignments will typically be articulated and presented as principled views (not as different taste).

Taking a close look at scientific disagreements that can be traced to social alignments, it is useful to distinguish disagreements that can be traced to alignments with groups outside the field, hereafter called externally-induced disagreements, from disagreements that can be traced to alignment with different groups inside the field (or subdivisions of the field), hereafter called subdivision-induced disagreements. (Indeed, it is possible that disagreements can be traced to both types (or to none), but then the analysis applies residually.)

Externally-induced disagreements. In the context of mathematical and exact scientific fields, the relevant social differences are typically related to differences in the scientific culture in which an individual was educated, and it is easy to trace some disagreements to these differences. These differences are viewed as external to the field whenever they refer to a scientific culture that extends beyond the boundaries of the field (e.g., a scientific and cultural tradition of a geographic region or an educational system).[28] While these differences seems to have some effect on scientific judgment, other social differences seem to have little effect (in the context of mathematical and exact scientific fields). In social sciences and humanities, other social differences seem to play a larger role, but in these cases these differences are typically articulated as theories or ideologies (and so I conceptualize them as principled views rather than as taste).

Although some scientific disagreements can be traced to differences in formal education, this essay conceptualizes them as part of the understanding of the graduate (see Section 5). In any case, I believe that the effect of these differences is relatively small, and the essay can be read as an analysis of the residual space (i.e., an analysis of scientific judgment given a fixed social context).

Subdivision-induced disagreements. Looking at different groups within a scientific field, I distinguish groups (i.e., research communities) that are defined by their research interest (i.e., an area of the field) from groups that correspond to a scientific subculture (of the field). By scientific subcultures I mean education and research traditions that vary within the field itself *but are not aligned with an area* (of the field), much like the scientific traditions that are external

[28] In the context of TOC, it is easy to observe differences between an American education and a European education; likewise, between an engineering education, a CS education, and a Mathematical education.

to the field (and were discussed above). In my opinion, the effect of both types of differences in scientific culture are similar (and should be conceptualized as differences in the understanding of the graduate). In contrast, the sub-communities associated with different areas deserve a separated treatment, provided next. (Note, however, that these sub-communities often develop a scientific culture that is distinct from the rest of the field. But also in such a case, it is better to conceptualize the issue as referring to an area, and not to a scientific subculture.)

I claim that disagreements that can be traced to belonging to the sub-community associated with an area should not be described as being a matter of taste, because such a description merely clouds the issue at hand. Note that here we are considering a hypothetical disagreement that is supposedly traced to the fact that one judge belongs to the area while the other judge does not belong to it (and we assume that the difference does not refer to their level of understanding).[29] Note that such disagreements can only refer to the question of the (relative) importance of the entire area, because if that question is not in dispute then membership in the area is irrelevant (since we assumed that the difference is not due to a difference in understanding). But when the importance of the area is questioned, then the disagreement is not fundamentally different from a disagreement on the importance of a specific work (given an agreement on the importance of the area). In both cases, we are faced with a disagreement regarding the evaluation of importance of a scientific element (i.e., either an area or a work), which can be traced to difference in understanding (of the field at large or to a difference in imagination). Tracing differences in evaluation to differences in affiliation with the element (i.e., area or work) adds no information, because it just re-affirms the difference in evaluation (i.e., one does not work in an area or performs a specific work unless one find it interesting). Attributing the difference in evaluation to a difference in taste is even worse, because it clouds the differences in understanding.

6.3 Some things that are wrongly attributed to taste

Indeed, much of the contents of Section 6.2 asserts that some disagreements that are commonly attributed to differences in taste actually arise from differences in understanding (and/or differences in imagination). This wrong attribution seems more a behavioral mechanism than a principled view:[30] It seems that some scientists prefer to conclude a dispute by suggesting that "it is a matter of taste" rather than by insisting that the disagreement is due to a difference in understanding (and/or a difference in imagination). This polite resolution has immediate practical benefits, but it carries some long-term dangers (since it undermines the ideal of universalism when applied to importance).

In Section 5 we encountered another false attribution: In contrast to common perceptions, when Bob says that he thinks that X is important because Alice said so (explicitly or implicitly), he is not expressing his taste (in following Alice) nor does he follow Alice's taste; he rather follows Alice's (explicit or implicit) evaluation. In this case, a decision to delegate the evaluation is perceived as a matter of taste, while in fact it is determined by Bob's opinion (which is based on his understanding of the situation at hand) that he better delegate the evaluation to Alice.

[29] Of course, it is likely that a member of the research community associated with an area understands the area better than a non-member, and can apply his/her imagination more effectively. But here we assume that the difference in opinion does not relate to a difference in understanding and/or imagination.

[30] Indeed, it is tempting to suggest that the preference to smoothen disputes by attributing them to taste is a matter of taste.

7 Other issues and the case of TOC

In this section, I wish to discuss a few issues that relate to the standing of a field among all scientific fields. These issues are very relevant to the Theory of Computation (TOC).

The definition of a field: This essay distinguishes fields from areas internal to fields, but this distinction was (intentionally) left undefined. In my opinion, *the transition from an area to a field is gradual, and refers to the level of independence and autonomy of the activities, institutes, and structures of the field/area in question*. Yet, for simplicity, the text adopts a dichotomic view, and adaptations should be made in order to accommodate the spectrum of cases that exist in reality. Specifically, when moving from the status of an area to the status of a field, one should give relatively more weight to the influence on the area/field itself and relatively less weight to the influence on the field/cluster-of-fields to which this area/field belongs. (Apropos, in my opinion, many fields are actually clusters of fields. Examples include Mathematics, Electrical Engineering, Biology, and Computer Science.) This principle will be applied in the discussions that follow.

Relations with other fields: Recall that the definition of importance referred to the influence *on the development of the field*. When a field contains areas that are closely related to other fields (especially, when an area is in the intersection of several fields), scientists find it hard to use this definition of importance, because it is unclear to which field it should be applied. My opinion is that if area A is related to both fields $F1$ and $F2$, then one should evaluate separately the importance of work in A to each of the two fields. In such a case, influence on $F1$ should be evaluated by scientists who understand $F1$, and ditto for $F2$. (Of course, if A is effectively a separate field, then work in A should be evaluated with respect to their influence on A.) In contrast, I am somewhat concerned when somebody tries to resolve the issue by defining $F1$ and $F2$ as consisting of a single field, assuming that this is actually not the case (i.e., most researchers in at least one of these fields do not consider this to be the case). That is, fields should not be treated differently than the way they actually are, even if this would have been more convenience.

Fields split and (less frequently) merge by social decisions of the relevant research communities. Yet, the boundaries of a field are a topic of struggle within it; in particular, there may be a struggle in field $F1$ regarding the question of whether or not to merge with field $F2$. (These struggles evolved mainly via the actual research activity.) Needless to say, the facts that $F1$ and $F2$ are different fields should not prevent researchers in $F1$ from attempting to influence $F2$, but one should distinguish contributions to $F1$ from contributions to $F2$.

In the case of TOC, I believe that there is a struggle about its independent stature versus its integration in Computer Science (CS) at large. The institutional structure indicates that TOC is part of CS, but the internal structures of TOC are autonomous. The relation of TOC with other sciences (e.g., Biology, Physics, and Economics) seem to be at a level of interdisciplinary activities (which may be conceptualized as areas in TOC that also influence these other sciences).

Emerging fields: Typically, new fields emerge by separation from existing fields, where the separation is caused by the development of sufficiently different interests that do not allow the new field to continue to flourish as an area within the older field. Thus, the separation is induced by the contents of the new field, which cannot fit well within the old field. Yet, this separation creates social problems. In particular, the new field has to form its own institutions and define its own identity, independently of the old field. Naturally, this formation process is not completed overnight, and the transition phase is problematic. In particular, there is a tendency to follow the

mold of the old field (or of other fields), whereas the entire purpose of the separation from the old field is to gain autonomy in determining the field's interests, methodology, and structure.[31] In the context of scientific evaluation, this raises a number of difficulties.

Firstly, it may not be clear what the new field is about and it may be harder than usual to imagine its future development. Secondly, there is a tendency to consider importance with respect to the old field rather than focus on importance with respect to the new field. Thirdly, the formal (institutional) evaluation processes of the new field may lack clear values and norms of their own, and they tend to adopt values and norms of the old field (and/or of other model fields) rather than try to adapt these elements to the new field. All these difficulties relate to the social fabric of the new field, which needs to be constructed to fit the field's actual contents and research activity.

Clearly, CS and TOC are new fields. My view is that CS emerged by separation from Electrical Engineering (EE), whereas TOC was formed within CS with a significant influx from Mathematics. Nowadays, TOC appears as having a dual origin in EE and Mathematics, which is somewhat uncommon in the emergence of new fields, and it is often used to explain the conflicting orientations within TOC. In my opinion, the process of forming the identity of TOC is far from completion and many of the aforementioned difficulties are relevant to TOC.

[31] Indeed, a scientific field is not required to follow the mold (or model) of any other scientific field. It is merely required to follow the basic principles of science, discussed in section 2.