INHALT:

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LUCA MORETTI: From the Indirect Confirmation of Theories to Theory Unification

BERND PRIEN: Family Resemblances—A Thesis about the Change of Meaning over Time

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WILHELM K. ESSLER: Über die Voraussetzungen der Erfahrungserkenntnis aus der Sicht des Operationalismus
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INHALT

DOUGLAS McDERMID:
The Real World Regained? Searles External Realism Examined ......................... 1

LUCA MORETTI:
From the Indirect Confirmation of Theories to Theory Unification ....................... 10

BERND PRIEN:
Family Resemblances—A Thesis about the Change of Meaning over Time ............. 15

KURT TORELL:
Must Values Have Subjective Existence? ............................................................ 25

WILHELM K. ESSLER:
Über die Voraussetzungen der Erfahrungserkenntnis aus der Sicht des Operationalismus ... 32

IMPRESSUM

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The Real World Regained?
Searle’s External Realism Examined
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Abstract
In Mind, Language, and Society: Philosophy in the Real World, John Searle presents an uncompromising apologia for realism which is distinguished both by its lucidity and by its vigour. His basic strategy is to show that realists have at their disposal the resources needed to refute skeptics who allege that a mind-independent world is unknowable. In what follows, I shall reconstruct Searle’s principal pro-realist argument (Section 3), then argue that it is vitiated by its reliance on two unwarranted assumptions (Sections 4-6). First, however, we need to be sure we understand the position—“external realism”—Searle wants to defend (Section 2).

1 Introduction
In Mind, Language, and Society: Philosophy in the Real World [43] John Searle presents an uncompromising apologia for realism which is distinguished both by its lucidity and by its vigour. As befits a philosopher with a well-known penchant for no-holds-barred polemical exchanges,¹ Searle strenuously defends his brand of external realism against all comers: old-fashioned idealists and phenomenalists, new-fangled anti-realists and neopragmatists, radical social constructivists, perspectivists and postmodernists of all stripes.² His basic strategy is as simple as it is bold: to show that realists have at their disposal the resources needed to refute skeptics who allege that a mind-independent world is effectively an unknowable Ding an sich, or a world forever beyond our ken.

In what follows, I shall reconstruct Searle’s principal pro-realist argument (Section 3), then argue that it is vitiated by its reliance on two unwarranted assumptions (Sections 4-6). First, however, we need to be sure we understand the position—“external realism”—Searle wants to defend (Section 2).

2 What is External Realism?
“Realism” is a notoriously vague and ambiguous term.³ Happily, however, Searle offers us a brief and tolerably clear description of his “external realism”, which he glosses as the view that “there is a real world existing independently of us” [43, p. 14]. Let us then define Searlean external realism as follows:

External Realism (ER) The thesis that “there exists a real world that is totally independent of human beings and of what they think or say about it” [43, p. 13] or, alternatively, that “there is a way that things are independently of our representations” [43, p. 31]. That is, the world is independent of our representations, perceptions, minds, languages, or conceptual schemes.

Given this characterization of ER, it is easy to see why Searle declares it incompatible with “idealism”, a catch-all term Searle uses for views according to which “reality is ultimately [...] constituted by our perceptions and other sorts of representations” [43, p. 16].⁴ Accordingly, Searle urges that Berkeley, Kant, and Hegel are a larger philosophical project: the vindication of what he calls the “Enlightenment vision”, or the view that reality is intelligible and that we can comprehend its nature [43, pp. 2-6]. For a critical discussion of this aspect of Searle’s work, see Rorty [37].

¹Thomas Nagel, in his review of Searle [41], describes Searle as “a dragonslayer by temperament” [32, p. 96]. Similarly, the dust jacket blurb for Searle [40] celebrates the philosopher as “a true prize fighter” who has taken on philosophical opponents as diverse as Derrida and Chomsky.
²For Searle’s views on social constructivism, see [42]. Note that Searle’s defence of external realism is part of a larger philosophical project: the vindication of what he calls the “Enlightenment vision”, or the view that reality is intelligible and that we can comprehend its nature [43, pp. 2-6]. For a critical discussion of this aspect of Searle’s work, see Rorty [37].
³See Lycan [23, pp. 189f] for some witty and astute observations on this topic.
⁴For other ways of understanding idealism, see Rescher [35, Figure 18.1.]
all sworn enemies of ER, as are a number of recent philosophers—Hilary Putnam, Nelson Goodman, Richard Rorty, Thomas Kuhn, and Jacques Derrida—for whom the world is not so much found as it is fashioned by languages, paradigms, categories, or conceptual schemes.

3 The Vindication of External Realism: Searle’s Master Argument

Having elucidated the content of ER, we are now in a position to scrutinize Searle’s attempted vindication of it. We may reconstruct his argument—which I shall call Searle’s “Master Argument”—as follows:

The Master Argument:

(P1) If there are no cogent arguments against ER, we are justified in accepting ER.

(P2) If ER is not vulnerable to skepticism about our knowledge of the external world, then there are no cogent arguments against ER.

(P3) ER is vulnerable to skepticism only if direct realism is false.

(P4) Direct realism is true.

∴ (C) We are justified in accepting ER.

(P1-P4)

Because the Master Argument is formally valid, the only question is whether its premises are true. Since the remainder of this essay will be given over to a critical examination of premises (P3) and (P4), I shall say very little about (P1) and (P2) here. Nevertheless, a few brief comments about this latter pair of assumptions are in order.

We may begin with (P1). What grounds this premise, ultimately, is Searle’s conviction that “[e]xternal realism is not a theory” [43, p. 32] but is instead what he calls a “default position”, that is, a fundamental presupposition of inquiry and discourse which we hold prereflectively and which forms part of the so-called “Background” of our thought and language [43, p. 9f]. Although Searle concedes that not all default positions are true [43, p. 11], he maintains that there is a prima facie presumption in favour of them, so that “any departure from them requires […] a convincing argument” [43, p. 11]. And once we allow that those who oppose a default position must shoulder the burden of proof, we are well on our way to (P1), provided we grant ER is in fact a default position.

Two points need to be made about (P2). First, Searle is not saying that the epistemological objection to ER is the only argument against it. On the contrary: he discusses a range of stock anti-realist arguments, which he rapidly dissects and deftly counters. What Searle stresses, however, is that epistemological considerations have long led philosophers to jettison realism—so much so, in fact, that he regards the complaint that realism renders reality unknowable as the main argument against ER [43, p. 26f]. And—this brings us to our second point—this observation seems just: the accusation that realism puts the world permanently beyond our ken was made by old-fashioned idealists such as Berkeley [7, §4, §18-§20], Kant [19, A367ff], Fichte [14, p. 45], and Schopenhauer [39, II: I, 10], and has been revived by Rorty, Putnam, and Goodman. In light of this, (P2) is not without a certain plausibility.

That is all I intend to say about (P1) and (P2). While I do not deny that there may be problems with these two premises, I propose to grant them, at least for the sake of argument, and focus instead on premises (P3) and (P4). The latter two assumptions will give Searle’s critics more than enough ammunition to use against him—or so, at any rate, I shall now argue.

4 Skepticism, Direct Realism and Representationalism

Let’s begin with (P3), which is built around a familiar intuition—namely, that the skeptic can

5 For a broadly similar way of understanding of realism, see Nagel [31, 33].

6 He reviews four anti-realists arguments in all; cf. [43, pp. 20-26]. Several of these arguments are discussed in greater detail by Alston [1], whom Searle cites with approval [43, p. 168].

7 For a discussion of this argument in Rorty, Putnam, Goodman, Kuhn, and others, see McDermid [25].
succeed only if he can discredit direct realism. What is meant here by “direct realism”, however? What I shall call generic direct realism can be represented as the conjunction of three theses:

**Generic direct realism (GDR):**

1. that physical objects are mind-independent;
2. that physical objects are perceivable and knowable; and
3. that we can perceive these objects directly, or without epistemic intermediaries (i.e., there is no subjective representation or private mental object which we must perceive in order to perceive a physical object).

Unlike Searle, who is prepared to endorse theses (1), (2), and (3), his skeptical adversary admits only thesis (1), denying (2) and (3). More specifically, the skeptic’s rejection of (2) is supposedly grounded in his denial of (3), just as Searle’s affirmation of (2) is rooted in his commitment to (3). For both sides, then, it appears that the fundamental issue is whether (3)—the thesis that “[w]e have direct perceptual access to [the] world through our senses” [43, p. 10]—is to be affirmed or denied.

4.1 Skepticism and Direct Realism

Bearing all this in mind, we are now in a position to take a closer look at (P3), which states that the falsity of GDR is a sine qua non or necessary condition for generating skepticism. According to (P3), the skeptic must reject GDR in favour of the view that perceptual experience forms a “veil of ideas” that interposes itself between mind and world. But must the skeptic be so committed?

That is very far from obvious. Recall that the skeptic’s initial move is simply to remind us that our beliefs about the way the world is depend epistemically on the testimony of our senses, inasmuch the way things appear to us functions as a source of evidence or justification. Since sensory experience is thus supposed to confer warrant upon our beliefs, those beliefs cannot amount to knowledge unless appearances actually are a trustworthy or reliable guide to reality; the mere fact that there seems to be a book in front of S is not a reason for S to believe that there really is an book unless S is right in assuming that how things look can (often or usually) yield the truth about how they actually are. Since, however, the true relation between appearance and reality cannot be known unless reality itself is already known, any reply to the skeptic that appeals to that relation is impermissibly circular, because it presupposes the very thing the skeptic denies—namely, knowledge of the way the world is. Or, to put the matter in a slightly different way: if my belief that, say, there is a green mug on the desk is ultimately based on my sensory experience, and if it is always possible that my experience is non-veridical or delusive (as in a dream or hallucination), then how can such a belief be warranted and justified so as to amount to knowledge?

It will readily be seen that, whatever its weaknesses, the above argument for skepticism makes no explicit appeal to a “veil of perception” doctrine. Yet what, it will be asked, about the references to “sensory experience”, “appearances”, and “the testimony of the senses”? The answer is that such talk need not be construed in terms of a theory which holds that the direct objects of perceptual awareness are subjective representations (or “ideas”, “impressions”, or “sense-data”). Indeed, the skeptic’s invocation of “sensory experience” only requires us to concede a pair of platitudes. Let us look briefly at both.

4.2 Two Platitudes

i. The first platitude is a banal distinction between appearance and reality, according to which things are not always as they seem, and perception is not necessarily veridical. Now, since any remotely credible philosophical theory of perception—be it direct realism, a sense datum theory, or an adversarial account—must find a way to accommodate
this distinction, Searle must think the direct realism he favours can make sense of this elementary contrast between appearance and reality\textsuperscript{11}, in which case he surely cannot quarrel with the skeptic’s invocation of it.

ii. The second platitude is that any knowledge of the external world we possess is mediated by our senses, in the sense that perceptual experience is a source of evidence or epistemic justification.\textsuperscript{12} Note, however, that there is no inconsistency between affirming the minimal empiricism this doctrine embodies and denying the doctrine of representationalism, which affirms that all we know directly are our perceptions, understood as a medium of private Vorstellungen epistemically prior to public material objects.\textsuperscript{13} Once we have distinguished minimal empiricism from representationalism and seen that the former doctrine does not entail the latter, there seems no good reason to maintain that the case for skepticism cannot get off the ground unless thesis (3) of GDR is false. Hence it would seem a grave mistake to say, with Searle, that “[o]nce we reject the idea that all we ever perceive are our own perceptions, then we have no epistemic basis for denying external realism” [43, p. 31].

5 Direct Realism Revisited

If the arguments advanced in the last section are basically on the right track, then we have good reason to regard Searle’s premise (P3) as ill-founded. We now need to determine whether we are entitled to entertain similar reservations about (P4).

Note that Searle advances no positive argument in favour of (P4). (This is not surprising, of course, since direct realism is included on his list of default positions) [43, p. 10]. Instead, he offers brief rebuttals to what he calls “the two most famous refutations” of GDR [43, p. 28]. Let us carefully consider what he has to say about each.

5.1 The Argument from Science

Searle has no trouble disposing of the first would-be refutation: the so-called “argument from science” [43, p. 28f].\textsuperscript{14} Here the idea is that since perceptual experience of material objects is the last link in a lengthy causal chain—a chain which stretches all the way from the object which stimulates my sense organs to certain goings-on in my brain and central nervous system—perception cannot be direct or immediate. The argument runs as follows:

\[ \text{(1) Perceptual experience (e.g. seeing a tree) is (causally) mediated by a lengthy and complex series of neurophysiological processes.} \]

\[ \therefore (C) \text{ GDR is false: perception of the physical world cannot be direct.} \]

Searle points out, rightly, that this is a blatant non sequitur:

“This argument seems to me fallacious. From the fact that I can give a causal account of how it comes about that I see the real world, it doesn’t follow that I don’t see the real world [. . . ] There is no inconsistency between asserting, on the one hand, “I directly perceive a tree,” and asserting, on the other, “There is a sequence of physical and neurobiological events that produce in me the experience I describe as “seeing the tree”” ” [43, p. 28f].

Expressed in terms of our reconstruction, Searle’s complaint is that the move from (1)—the claim that perception is causally mediated—to (C)—the claim that perception of the physical world is epistemically mediated (in the sense that my perceptual awareness of physical objects ultimately depends upon my perception of my subjective rep-

\textsuperscript{11} For more on this point, see the arguments against direct realism described in Section 6.

\textsuperscript{12} Some philosophers—Rorty [36], Davidson [11]—have denied this and have held that although experience causes our beliefs, it cannot serve as a source of evidence for them; for critical commentary, see McDermid [25, 27]. But Searle does not take this line.

\textsuperscript{13} I take the term “minimal empiricism” from McDowell [29].

\textsuperscript{14} Incidentally, it is rather odd to give this argument as much importance as Searle does, since there are several other objections to GDR which have been more prominent historically. See Section 6, below.
resentational states)—is invalid. Once we distinguish causal mediation from its epistemic counterpart, we can see that the Argument from Science poses no threat to the direct realist, whose position would be fatally compromised only by the introduction of epistemic intermediaries (i.e. – by the admission that the immediate objects of perceptual acquaintance are mental representations).

Searle’s reply to this version of the Argument from Science therefore seems absolutely correct. Nevertheless, we may well wonder if he hasn’t made things too easy for himself by focussing exclusively on a rather weak version of the argument. For we can, I think, easily imagine a superior version of the Argument from Science which, while eschewing the clumsy and straightforward conflation of causal with epistemic mediation, nevertheless aspires to show that direct realism cannot be taken seriously once we have a detailed scientific understanding of the specific ways in which our perception of the physical world is causally mediated by neurophysiological processes.

Here is a sketch of one way such an argument might unfold. If we are persuaded by the work of psychologists of perception (such as that discussed in [21] and [44]) or neurophysiologists such as J.C. Eccles [13] we will readily appreciate the extent to which perceptual output—say, the visual experience I have when I look at a Necker cube or at one of Rubin’s figure / ground reversing figures—is underdetermined by raw sensory input or stimuli. This makes it possible to argue that perceptions of the physical world are not ready-made or given to the mind but are instead constructed or assembled by a set of complicated processes—processes which, far from merely receiving and transmitting the information contained in the raw data of sensation, actively interpret such data by synthesizing it and projecting an order onto it. If this is indeed the case—and whether it is the case is plainly an empirical question—then direct realism is put on the defensive; for how can the immediate objects of perception be reckoned wholly objective or mind-independent if they are inescapably conditioned or constituted by such subject-derived processes?

Whatever the merits of this revised version of the Argument from Science are—and obviously we lack space to discuss them in any meaningful way here—it is crucially important to see that, unlike the simple-minded version scouted by Searle, the revised version does not contend that perception of the physical world must be epistemically mediated simply because it is causally mediated. Instead, it maintains that direct realism is untenable because the scientific study of perception indicates that perception’s immediate objects are partially formed or constituted by certain neurophysiological and psychological processes. Hence Searle’s sole objection to the original, weaker version of the argument is not sufficient to refute the modified version sketched in the preceding paragraph.

5.2 The Argument from Illusion

Searle is similarly dismissive of the second objection to direct realism: the venerable Argument from Illusion. Here is his statement of it:

“[T]he person who thinks that we directly perceive objects and states of affairs in the world, the naive perceptual realist, cannot deal with the fact that there is no way of distinguishing the case where I really do see objects and states of affairs in the world, the so-called ‘veridical’ case, from the case where I am having some sort of illusion, hallucination, delusion, and so on. Therefore, perceptual realism is false.” [43, p. 29].

To clarify matters, it will once again be helpful to have before us an outline of the argument in question which identifies both its starting-point and its conclusion. Here is the partial reconstruction I propose:

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15For a similar criticism of the Argument from Science, see: [3, 22, 9, 10].
For any veridical perceptual state $P$ (e.g. – $S$ seeing a large red tomato on yonder table), it is possible for there to be a non-veridical or delusive perceptual state $P'$ which is subjectively indistinguishable from $P$ (i.e. – which is phenomenologically as if $S$ were seeing a large red tomato on yonder table, although no such tomato is actually present to $S$).

∴ (C) $gdr$ is false: the immediate objects of veridical perception are not mind-independent physical objects, but mental representations (as in $P'$).

Searle is completely unimpressed by the Argument from Illusion. Why? First, because he thinks it assumes that if there is no qualitative or introspectible difference between my actually seeing a real tree in the quad and my vividly dreaming (or hallucinating) that I am seeing a tree, then we are left with no reliable way to distinguish between veridical perceptual experience and its counterfeit or non-veridical counterpart. He insists, however, that although “[t]he single experience, considered in isolation by itself, is not sufficient to make the distinction between veridical perception and hallucination” [43, p. 31], we can nevertheless distinguish our veridical perceptions from hallucinations and dreams (and thus know when we are actually seeing trees) by appealing to considerations of coherence, or determining how a given experience is related to one’s “network of other experiences” [43, p. 30].

This riposte is totally irrelevant, however. Champions of the Argument from Illusion need not commit themselves to anything like the adventitious epistemological assumption Searle saddles them with, viz., that we cannot know that we are not dreaming but actually seeing a tree unless there is “some internal feature of the experience itself that is sufficient to distinguish the veridical experience from a hallucination of the object” [43, p. 31]. Their initial claim is just that “[t]he single experience […] is not sufficient to make the distinction between veridical perception and hallucination” [43, p. 31]. Since Searle grants this [43, p. 31], his first objection to the argument misses the mark.

This brings us to a second, more promising objection, in which Searle registers his opposition to the idea that $gdr$ “requires that there be a distinction in the qualitative character of my visual experiences between veridical and non-veridical perceptual experiences” [43, p. 31]. It is simply not the case, we are told, that “in order for me to be seeing the object in front of me, there must be some internal feature of the experience itself that is sufficient to distinguish the veridical experience from a hallucination of the object” [43, p. 31]. Here his point, it would appear, is that the claim that the direct object of $P$ is not mind-independent does not actually follow from the claim that $P$ and $P'$ are subjectively indistinguishable (allowing that the immediate objects of awareness in cases like $P'$ are mind-dependent representations). I take it, moreover, that Searle thinks this does not follow because he repudiates the following principle:

(2) If two perceptual states are subjectively indistinguishable, then their immediate objects—what is directly before the mind or immediately present to consciousness in both cases—belong to the same ontological type.

This principle is surely not self-evident, and Searle is not the only philosopher who has viewed it with suspicion [4, 28, 2, 34]. Nevertheless, Searle says next to nothing about why we should reject (2). So his critique of the Argument from Illusion is very far from conclusive.

6 More Opposition to Direct Realism

Even if Searle’s treatment of the Argument from Illusion were satisfactory, however, direct realism would still be exposed to a number of noteworthy objections—objections which Searle fails to consider, let alone refute. To give a sense of the difficulties facing (P4), I shall now outline two classic arguments directed against $gdr$.

6.1 The Time Lag Argument

The following argument is based on the fact that light takes time to travel and runs as follows:
“Suppose S looks up at the night sky and indulges in some star-gazing. Unbeknownst to S, the distant star she is looking at no longer exists, having burnt out one hundred years ago, though its light is reaching Earth only now. Thus although S’s visual experience is just as it would be if the star existed, and although S directly perceives some object, it can’t be the star (unless we wish to deny that the direct objects of perception must exist when perceived). This poses a problem for direct realism, since what holds in the example of the distant star holds also (though far less dramatically) in more mundane cases, such as my claim to see the bureau across the room, in which the time lag or interval is much shorter. Provided there is a time lag, no matter how small, there is reason to say that S is always directly aware of something other than the physical object in question.”

6.2 The Argument from the Scientific Image

The next time-honoured objection to GDR runs as follows: “If it is the material world, and not a more or less subjective representation of it, that we directly apprehend in perception, then what are we to say of the ways in which the scientific image of the world differs from the manifest image? Secondary qualities—colour, for instance—is not a material object. But what normal human perceivers see is in fact coloured. Hence, what I see is not a material object but a mental one, so that the objects of immediate (visual) perception are sense-data.”

6.3 Some Caveats

Now, the point is most emphatically not that the foregoing arguments are knock-down refutations of direct realism. On the contrary, neither of these objections can be reckoned unproblematic, and more work would have to be done to present detailed and well-worked out versions of them. Nevertheless, since each presents a serious challenge to the idea that perception puts us in direct cognitive contact with an extra-mental reality, it seems fair to say that we are not entitled to help ourselves to (P4) until we understand why these arguments go astray (if indeed they do). That, however, is a large subject, about which Searle unfortunately says virtually nothing.

I shall bring my discussion of (P4) to a close with two very brief observations. First, a word to those tempted to defend Searle by pointing out that he regards direct realism as a default position [43, p. 10]. Recall that, by Searle’s own admission, default positions can be given up in the face of “a convincing argument” [43, p. 9]. What the invocation of the Time Lag Argument and the Argument from the Scientific Image does, then, is effectively shift the burden of proof back to Searle.

Second, even if Searle can bear this burden by refuting these (and numerous other) objections to direct realism, note that ER would not thereby be vindicated unless premise (P3) is true. And yet that there is no good reason to think that premise is true—or so we have already argued in Section 4.

7 Conclusion: The Real World Regained?

It is time to take stock. The main moral to be drawn from our discussion is, I submit, that the Master Argument depends on two assumptions Searle has failed to justify. In the absence of some compelling defence of (P3) and (P4), Searle’s “real world”, far from having been regained, seems as remote, elusive, and epistemically inaccessible as ever. This conclusion will be found congenial by external realism’s post-Kantian critics, who will no doubt remind us that knowledge must remain an unfathomable mystery until we acknowledge that empirical reality is shaped and

For an attempt to restore the default position, see Austin [4]—though, curiously, Austin says that he is “not […]—and this is a point to be clear about from the beginning—going to maintain that we ought to be ‘realists’, to embrace, that is, the doctrine that we do perceive material things” [4, p. 3]. Cf. Putnam, who contends that “the objects of (normal ‘veridical’) perception are “external” things, and, more generally, aspects of “external” reality” [34, p. 10].
structured by the mind. Whether we should join them, and thus become anti-realists by default, is a question which deserves separate and sustained consideration. What we can say here, however, is that Searle’s Master Argument, though unsuccessful, has the virtue of revealing the ways in which the issues of realism, perception, and skepticism fit together—as well as some of the ways in which they do not.

References


From the indirect confirmation of theories to theory unification
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Abstract
Theory unification is a central aim of scientific investigation. In this paper, I lay down the sketch of a Bayesian analysis of the virtue of unification that entails that the unification of a theory has direct implications for the confirmation of the theory’s logical consequences and for its prior probability. This shows that scientists do have epistemic, and not just pragmatic, reasons to prefer unified theories to non-unified ones.

1 Introduction
Theory unification is a central aim of scientific investigation; typical examples are the recurring attempts to provide, in physics, a unified theory of both quantum and relativistic phenomena. It is therefore important for the methodology of science to give an account of the property of unification. One possible view is that scientists prefer unified theories to non-unified rivals for just pragmatic reasons (for instance, because unified theories are simpler and thus easier to ‘handle’). Indeed, this seems to follow from van Fraassen’s conception of a theory’s explanatory power and non-empirical epistemic virtues in general (see [8]). Against views of this kind, I will argue that scientists do have epistemic reasons to prefer unified to non-unified theories, as they believe that unification increases their reliance on the scientific statements that they accept. To show it, I will lay down the sketch of a Bayesian analysis of unification that entails that the unification of a theory has direct implications for the confirmation of the theory’s logical consequences and for its prior probability.

Intuitively, a theory is unified if it is not a mere collection of disparate hypotheses but a system of laws and principles that connect to one another. For example, it is intuitive that such principles should be as few as possible (the minimum necessary to explain all relevant evidence). They should be logically consistent with one another and similar to each other in terms of logical structure. My purpose is not to try to justify the correctness of those ascriptions (for instance, from a realist point of view) but, rather, to clarify them by means of a mathematical formulation of their content.

In the next two sections, I will outline an account of unification resting on the explanation of the fact that unified theories are used, in science, for the indirect confirmation of their theoretical and observational consequences, and resulting in a necessary Bayesian condition that a unified theory must meet. In the conclusion, I will present a possible case of theory choice based on the probabilistic consequences of unification.

2 Indirect confirmation and unification
There are direct links between unification and empirical confirmation. These links result, to begin with, in the fact that unified theories allow their theoretical consequence to be indirectly confirmed by observations.

The notion of indirect confirmation can be spelled out as follows: let $T$ be a scientific theory that entails a hypotheses $H$. Let us suppose that $T$ also entails an observational statement $E$, increases their reliance on the scientific statements that they accept. To show it, I will lay down the sketch of a Bayesian analysis of unification that entails that the unification of a theory has direct implications for the confirmation of the theory’s logical consequences and for its prior probability.

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The notion of indirect confirmation can be spelled out as follows: let $T$ be a scientific theory that entails a hypotheses $H$. Let us suppose that $T$ also entails an observational statement $E$,
which is not logically derivable from $H$ (i.e. neither from $H$ alone nor from $H$ in conjunction with a set of auxiliaries).\footnote{Notice that $T$ cannot be considered an auxiliary hypothesis of $H$, as $T$ entails $H$. Moreover, if $E$ is derivable from another theoretical consequence $H^*$ of $T$, $H^*$ cannot be considered an auxiliary hypothesis that enables $H$ to entail $E$. For $H^*$, in this case, entails $E$ also when isolated from $H$.} The advocates of indirect confirmation maintain that, in such a situation, if $E$ confirms $T$, the confirmation can be ‘communicated’ to $H$ through the ‘bridge’ theory $T$. Thus, $E$ indirectly confirms $H$. (See, for instance, [4]). Indirect confirmation has effective uses in science. In physics, a celebrated example is given by the indirect support for the kinematic laws ($H$) of the Theory of Relativity ($T$) obtained by the achieved increase of mass with velocity ($E$). (For this and other examples, see [4] and [3]).

As Okasha [7] has argued, the indiscriminate use of the method of indirect confirmation yields paradoxical conclusions. Suppose in fact that the method is applicable in any case. Namely, to the effect that, given an observational statement $E$ and a non-tautological hypothesis $H$ not entailing $E$, $E$ indirectly confirms $H$ if there is a logically consistent theory $T$ that entails both $E$ and $H$ and is confirmed by $E$. Considering that (on basic hypothetico-deductivist principles) $T$ is confirmed by a logical consequence $E$ if and only if $T$ is logically consistent and $E$ is not a tautology,\footnote{I include under the term ‘tautology’ also conventional definitions and analytic statements.} the above condition can be re-written as follows:

(\textbf{IC1}) If $E$ is a non-tautological observational statement and $H$ is a non-tautological theoretical statement that does not entail (alone and in conjunction with auxiliaries) $E$, $E$ indirectly confirms $H$ if there is a logically consistent theory $T$ that entails $E$ and $H$.

It is easy to show that (IC1) is untenable. Let $E$ be the statement that, in observed cases, mass does increase with speed, and let $H$ be the hypothesis of the existence of unconscious process in the human minds. Finally, let $T$ be the mere \textit{logical conjunction $E$ & $H$}. In this case, $E$ and $H$ are not tautologies and $T$ is logically consistent. Since $T$ entails both $E$ and $H$, and $H$ does not entail $E$, on the grounds of (IC1), $E$ indirectly confirms $H$. But nobody—and no scientist—would ever accept it, as $T$’s confirmation by $E$ appears, in this case, \textit{irrelevant} for the confirmation of $H$.

How should we explain this irrelevance? In my opinion, an insightful answer is the following. It is only by the confirmation of the general principles that unify a set of statements in one theory that evidential support is ‘transmitted’ to the hypotheses that do not entail the confirmatory evidence. Since $T$ includes no general principles able to unify $E$ and $H$, and $H$ does not entail $E$, $T$’s confirmation by $E$ cannot ‘flow’ to $H$. (This view is defended in [5]). This answer appears to match and to provide explanation of the actual scientific practice, where unified theories are typically used to ‘channel’ indirect confirmation.\footnote{Typical examples of it are the indirect support for Kepler’s laws of planetary motion via the independent confirmations of Newton’s mechanics (see [3]) and the indirect support for the Special Relativity Theory via the independent confirmations of the General Relativity Theory (see [6]). It is worth emphasising that Newton’s Mechanics and the General relativity Theory are just \textit{paradigmatic} examples of theory unification.}

In conclusion, a rule for the application of the indirect confirmation method that apparently reflects the actual scientific practice is following:

(\textbf{IC2}) If $E$ is a non-tautological observational statement and $H$ is a non-tautological theoretical statement that does not entail (alone and in conjunction with auxiliaries) $E$, $E$ indirectly confirms $H$ if there is a \textit{unified} theory $T$ that entails $E$ and $H$.

(Notice that, since unification certainly entails logical consistency, the specification that $T$ must be logically consistent is dispensable). (IC2) enables scientists to circumscribe the applicability range of the indirect confirmation method so that to rule out the counterintuitive confirmation cases just considered.

In science and in everyday life, there are cases of indirect confirmation that apply to simple empirical generalisations. They depend on the trivial inductivist belief according to which, if a statement $Pa \supset Qa$ confirms a \textit{nomic} generalisation
\(\forall x(Px \supset Qx)\), then confirmation ‘spills’ over the parts of \(\forall x(Px \supset Qx)\) that extend beyond \(Pa \supset Qa\)—for instance, \(Pb \supset Qb\). (Cf. [2]). In such cases, the relation of indirect confirmation holds not between an observational and a theoretical sentence but between two observational sentences—for instance, \(Pa \supset Qa\) and \(Pb \supset Qb\). In these cases too, the appeal to the unifying power of nomic generalisations allows the inductivist to get rid of superabundant and counterintuitive cases of confirmation.

This consideration makes it plausible that two observational statements confirm each other if they are deducible from a unified theory (which can consist in a mere set of nomic generalisations). We can then generalise (IC2) as follows:

\[(IC3)\] If \(E\) is a non-tautological observational statement and \(C\) is a non-tautological statement that does not entail (alone and in conjunction with auxiliaries) \(E\), \(E\) indirectly confirms \(C\) if there is a unified theory \(T\) that entails \(E\) and \(C\).

This is the condition that apparently governs the main applications of the indirect confirmation method in the actual scientific practice.

### 3 A Bayesian analysis of unification

How should we explain, in general, the fact that scientists assign to unified theories the methodological function described by the rule (IC3)? I put forward the following explanation that strikes me as very intuitive: the belief that grounds rules like (IC3) is—roughly—that any part of a unified theory is epistemically tied to any of the other parts in virtue of the theory’s unifying principles. This means, more precisely, that every non-tautological consequence of a unified theory confirms every other non-tautological consequence of the theory and that—equivalently—every non-tautological consequence of a unified theory is confirmed as the theory is confirmed. This is the ultimate reason why—on my view—theory unification provides the very grounds of the ‘mechanism’ of indirect confirmation used in science.

What I said singles out the following necessary epistemic condition for theory unification:

\[(U1)\] If \(T\) is a unified theory, for every non-tautological consequences \(C\) and \(C^*\) of \(T\), \(C\) confirms \(C^*\).

Notice that (U1) is trivially equivalent to:

\[(U2)\] If \(T\) is a unified theory, for every consequence \(C\) of \(T\) and any non-tautological consequence \(C^*\) of \(T\), if \(C\) confirms \(T\), \(C\) confirms \(C^*\).

Notice finally that (U1) and (U2) entail (IC3).

Let us now ‘translate’ (U1) and (U2) into quantitative conditions. Let \(A\) and \(B\) be two statements and let \(K\) be background knowledge. In accordance with the Bayesian definition of incremental confirmation, \(A\) confirms \(B\) relative to \(K\) if and only if \(P(B|A & K) > P(B|K)\). In what follows, for the sake of simplicity, I will assume that, for every statement \(A\), \(P(A|K) = 1\) if and only if \(A\) is a tautology and \(P(A|K) = 0\) if and only if \(A\) is logically inconsistent.

The Bayesian definition of confirmation and the above assumption allow us to ‘translate’ (U1) into the following probabilistic condition:

\[(BU1)\] If \(T\) is a unified theory, for every logical consequence \(C\) and \(C^*\) of \(T\) and \(K\) such that \(P(C|K) < 1\) and \(P(C^*|K) < 1\), \(P(C^*|C\&K) > P(C^*|K)\).

(BU1) expresses the insight that, if a theory is unified, its non-tautological consequences are probabilistically dependent on one another in a positive sense. This claim is the quantitative counterpart of the qualitative one, made by (U1), according to which, if a theory is unified, its non-tautological consequences confirm each other.

By analogy with the relation existing between (U1) and (U2), (BU1) is logically equivalent to:

\[(BU2)\] If \(T\) is a unified theory, for every logical consequence \(C\) and \(C^*\) of \(T\) and \(K\) such that \(P(C^*|K) < 1\), if \(P(T|C\&K) > P(T|K)\), then \(P(C^*|C\&K) > P(C^*|K)\).

(BU2) expresses in quantitative terms the claim, made by (U2), that if a theory is unified, whenever

\[4\text{Thus } P(T|K) > 0.\]
the theory is confirmed, any of its non-tautological consequences is confirmed too.\(^5\)

4 Conclusion

The notion of theory unification plays a tangible role in scientific methodology, as unified theories are part of the framework by which theoretical and observational statements can receive indirect support by evidence. This suggests a reason why scientists typically prefer unified theories to non-unified rivals: the former, but not necessarily the latter, provide scientists with an additional device to increase a scientist’s reliance on certain scientific statements that he believes in (for instance, a scientist’s confidence in a hypothesis \(H\) will increase as soon as \(H\) is embodied in a confirmed unified theory). This shows that the preference of scientists for unified theories depends, very plausibly, on also epistemic not on just pragmatic reasons.

Indeed, there is another way to argue for the same conclusion that hinges on the analysis of unification given in terms of the conditions (BU1) and (BU2). I will now show that, on certain circumstances, the application of (BU1) enables a unified theory to raise its prior probability over the prior probability of its non-unified rivals, and this makes the theory epistemically preferable to its rivals. Hence, insofar (BU1) reflects with sufficient accuracy the actual conception of unification of scientists, this principle shows that, at least in certain cases, scientists will prefer unified theories on non-unified rivals on genuine epistemic grounds.

Let suppose that a unified theory \(T\), available at a time \(t\), is developed, at a time \(t+n\), into two incompatible rivals \(T_1\) and \(T_2\). \(T_1\) is still unified, as it consists of the conjunction \(T & H_1\), where \(H_1\) is a unified set of hypotheses homogeneous with \(T\). Quite the opposite, \(T_2\) is no longer unified. For it consists of the conjunction of \(T & H_2\), where \(H_2\) is a set of hypotheses including laws and principles very dissimilar (for instance, in terms of their logical structure) from those included in \(T\). Let us moreover suppose that \(H_1\) and \(H_2\) are equally plausible given background knowledge \(K\), so that one can stipulate that (a) \(P(H_1|K) = P(H_2|K)\). Finally, let us assume that (b) \(P(T_1|K), P(T_2|K) > 0\) and \(P(T|K)\), \(P(H_1|K), P(H_2|K) < 1\). I will now show that, given these conditions, if (BU1) is accepted and a very intuitive assumption is made (see below), then \(P(T_1|K) > P(T_2|K)\).

Notice that all conditions to apply (BU1) to \(T_1\), \(T\) and \(H_1\) are fulfilled, as \(T_1\) is unified, \(T\) and \(H_1\) are both entailed by \(T_1 & K\) and, because of (b), \(P(T|K) < 1\) and \(P(H_1|K) < 1\). Since (BU1) applies, it follows that \(P(T|H_1 & K) > P(T|K)\), which trivially entails that

\[(1) \quad P(T & H_1|K) > P(T|K)P(H_1|K).\]

In contrast, since \(T_2\) is not unified, as its parts \(T\) and \(H_2\) are not homogeneous, there is no reason to believe that \(P(T & H_2 & K) > P(T | K)\), while it is quite intuitive that \(P(T | H_2 & K) = P(T | K)\).

This entails that

\[(2) \quad P(T & H_2|K) = P(T|K)P(H_2|K).\]

From (1), (2) and the assumption (a) that \(P(H_1|K) = P(H_2|K)\), it follows that

\[(3) \quad P(T & H_1|K) > P(T & H_2|K).\]

Since \(T\) and \(H_1\) and \(T\) and \(H_2\) are respectively equivalent to \(T_1\) and \(T_2\), (3) is equivalent to

\[P(T_1|K) > P(T_2|K)\]

QED.
References


Family Resemblances
A Thesis about the Change of Meaning over Time
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Abstract
I argue that close examination of Wittgenstein’s remarks on family resemblances (PI 65-67) shows that he is proposing a theory about the development of language over time. According to this theory, a concept is enlarged to a newly discovered object when it is similar to other objects falling under this concept. However, being empirical, theories of language-development cannot be regarded as philosophical positions. I therefore argue that Wittgenstein puts forward this theory only for therapeutical reasons. He thereby wants to bring the metaphysical question “Why do we call all games ‘games’?” back to its everyday use.

1) In this paper, I would like to argue for an interpretation of Wittgenstein’s remarks on family resemblances (PI 65-67), according to which he is talking about the development of the extension of concepts over time. I would like to start my paper by reviewing what I think is the most common reading of Wittgenstein’s remarks on family resemblances. This reading is prominently exemplified by Bambrough [4] and by Baker/Hacker [3]. According to it, Wittgenstein proposes a new theory of concepts that states conditions under which it is justified to apply a concept to an object. According to Bambrough, there were traditionally two positions with regard to this question, namely realism and nominalism. Realism holds that there is a property or set of properties that holds of all and only those objects to which a concept can be justifiably applied. Nominalism holds that there is no such property and hence no objective justification for the application of a concept. Both realism and nominalism agree that an objective justification is only possible on the ground of a common and peculiar property. According to Bambrough, Wittgenstein denies this and claims that the application of one and the same concept to different objects can be objectively justified by a network of similarities obtaining among these objects [4, p. 217]. Similarly, Baker and Hacker write:

“What makes the various activities called ‘games’ into games is a complicated network of similarities” [3, p. 326]

and

“The adducing of relevant similarities justifies applications of ‘game’, since it is on account of the relationship among games, [...] that we correctly call games ‘games’ (cf. §65).” [3, p. 327]

Now, this interpretation also has to be integrated into Wittgenstein’s general thesis that linguistic meaning is determined by linguistic rules. Accordingly, the received interpretation has to be formulated thus: Wittgenstein claims that there are linguistic rules stating that an object falls under a concept if it bears family resemblances with other objects falling under this concept. In this way, one can both say that it is because of linguistic rules, and that it is because of similarities that an object falls under a concept. So, the interpretations of Bambrough and Baker/Hacker come down to saying that there are linguistic rules which allow the application of a concept to an object if and only if it exhibits certain family resemblances. In this paper, I am going to refer to this view as the received interpretation.

2) In this section, I would like to argue that the reading outlined in section 1 is unsatisfactory. It is well known that there is a number of problems about family resemblance theories of concepts. I am going to name but a few to illustrate this point:1

∗I would like to thank Eike v. Savigny for some very helpful comments.

1See also the list of problems in [3, p. 332-337].
(1) There are concepts that have explicitly stated definitions. Therefore, it is clear that this theory applies only to a certain subset of concepts, the so-called family resemblance concepts, but it is hard to determine which concepts are family resemblance concepts and which are not.

(2) The notion of similarity, which figures centrally in this theory, is itself not clear, i.e., it is unclear what it means that two objects are similar. It is, for example, certainly not sufficient for two objects to be similar that they share some property. But what is sufficient, and which properties are relevant for similarity and which are not?

(3) This account of the extension of concepts involves a regress. We said that an object falls under a concept if it is similar to other objects falling under this concept. But then the question of membership arises anew with regard to these other objects.

(4) The most pressing of these problems is that resemblances do not seem to determine the extension of concepts in the right way. Any object is in some way similar to any other, so any object would have to fall under any concept. This has been called the problem of underdetermination, the problem of wide open texture, or the problem of coherence. As far as I can see, there are essentially three different ways of solving this problem, either by arguing that only some similarities are relevant for the extension of a concept, or by arguing that similarities are only a necessary, but not a sufficient condition for something to fall under a concept, or by taking the whole conceptual system into consideration.

A great deal of the literature on Wittgenstein’s family resemblance remarks is devoted to these problems. While I think that it is possible to find solutions to them and to construct a theory of concepts around the idea that membership depends on a network of similarities, I do not think that Wittgenstein intended to propose such a theory. This can be seen from the following facts: The above mentioned problems are all very obvious, i.e., the claim that membership depends on similarity is very obviously in need of refinement. On the other hand, there is no trace of any discussion or even awareness of these problems in the text of the PI. Thirdly, the PI is a carefully worked out philosophical text by a very intelligent philosopher. Therefore, it is hard to believe that the PI should contain only the central idea of a theory of concepts, the details of which still would have to be worked out. For these reasons, the received interpretation seems to be at odds with the text.

3) In this situation it might seem worthwhile to go back to the text of the PI to see exactly what Wittgenstein has in mind when he talks about family resemblances. I would like to provide an alternative interpretation of a statement Wittgenstein makes in PI 65, which seems to show quite conclusively that the received interpretation is correct: “And it is because of this relationship, or these relationships, that we call them all ‘language’.” The only possible interpretation of this statement seems to be that the extension of concepts is determined by similarities. However, I think that this statement has to be read quite differently, namely in the light of an explanation Wittgenstein gives in PI 67b. For two reasons I think that PI 67b is to be seen as an explanation of the statement just quoted: First, the sentence following the one just quoted reads: “I will try to explain this.” I think that Wittgenstein is referring us here not only to the immediately following section (PI 66), where he gives his famous account of the similarities that connect the different kinds of game, but also to the section after

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2For example, the activity of going for a walk exhibits the properties of involving exercise of the body, being done for recreation, and not being useful for anything. By virtue of these properties, going for a walk is similar to several activities that fall under the concept ‘game’, for example football, tennis and ring-a-ring-a-roses. But still, going for a walk is not a game. Other examples of activities that exhibit similarities with games but are not called ‘game’ can easily be found.

3This strategy has been applied by Wennerberg [6] and by Hunter [5]. It will also be applied in the interpretation to be proposed in this paper.

4Kuhn proposes to solve this problem along these lines. Cf. Andersen [1].
that, PI 67. PI 66 is only meant to explain the
notion of family resemblance and to show that,
at least in the case of some concepts, there ac-
tually is nothing in common to the things falling
under them. (See Wittgenstein’s statement of the
results so far achieved in PI 66b.) Thus, PI 66
still leaves it unexplained in what sense it is be-
cause of certain similarities that we call something
a game. Consequently, this section cannot be re-
garded as containing the entire explanation an-
nounced in PI 65. The second reason is that, in
PI 67, Wittgenstein still talks about family resem-
blances and even explicitly returns to the question
of PI 65 by asking: “Why do we call something a
‘number’?” (PI 67b) Consequently, the explana-
tion given in the remainder of paragraph 67b is to
be taken as a commentary on the claim in PI 65
that we call all these activities language because
of their similarities.

If this is correct, it will be best to interpret
PI 67b first, and then, in the light of this interpre-
tation, to propose a reading of the crucial passage
from PI 65 quoted above. Wittgenstein writes:

“This why do we call something a ‘number’?
Well, perhaps because it has a – direct –
relationship with several things that have
hitherto been called number; […] And
we extend our concept of number as in
spinning a thread we twist fibre on fibre.”
(PI 67, my emphasis)

This quote indicates that Wittgenstein is con-
sidering the development over time of the set
of objects falling under a concept. It is neces-
sary to admit this temporal aspect, first, be-
cause Wittgenstein distinguishes between things
that have hitherto been called number and things
that are not yet called number, and, secondly, be-
cause he speaks of extending a concept. No inter-
pretation ignoring this temporal aspect can be ade-
quate. Taking these hints at a temporal process
seriously will lead me to attribute to Wittgenstein
a theory of language evolution according to re-
semblances between objects. I do not see how this
conclusion can be avoided without simply treating
these formulations as mistaken or at least radically
misleading. However, such a theory is obviously
empirical and could turn out to be false. There-
fore, I will argue in section 6 of this paper that
Wittgenstein advances this theory only for thera-
peutic reasons.

The situation that Wittgenstein seems to have
in mind here has two important features: First,
we extend our concept of number to an object that
hitherto has not been called number, and second,
it is because of certain similarities that we do this.
I would like to add some flesh to these bones and
construct a more elaborate story which Wittgen-
stein might have had in mind: Imagine that the
mathematical object which we now call the set of
complex numbers has just been discovered.
Before this discovery, only real numbers were known
to mathematicians. We can assume further that
numbers were understood as things that can be
ordered linearly according to greater/lesser rela-
tions. Complex numbers do not satisfy this re-
quirement because they consist of two components
so that they can only be ordered in a plane and not
on a line. Therefore, the newly discovered object
does not fall under the concept number. We can
assume, moreover, that this newly discovered ob-
ject does not clearly fall under any other concept
provided in the language at that time. We can
assume, I suggest, that Wittgenstein is imagining
a situation in which a recently discovered object
defies our classificatory system and thus forces us
to adapt this system. In order to do this, we ex-
tend our concept of number such that it includes
what we now call complex numbers.

It can also be noted that before this change,
one could have applied the concept ‘number’ figu-
ratively to the newly discovered object. This ob-
ject exhibits a number of similarities with other
kinds of number. For example, you can add and
multiply complex numbers and they are defined as
square roots of negative numbers. Therefore, even
before the adaptation of language took place, it
was intelligible and justifiable to apply the concept
of number figuratively. One could say that the

5As a way of speaking, I will say that one object has
been discovered, namely the one we now call the field
of complex numbers. (A field is roughly speaking a set
whose elements admit of addition, subtraction, multiplica-
tion, and division, such that all the usual laws of computa-
tion hold.)
complex numbers were numbers in scare quotes. This parallels the case of the activity of philosophizing and the concept ‘game’. Even though it is intelligible to call philosophy a game, it does not really fall under this concept. In the same way, what we now call complex numbers, was not an instance of the concept ‘number’ before this concept was extended.

Now I would like to provide an interpretation of Wittgenstein’s claim that we call something a number because of certain similarities. I will do this by discussing the question as to what, in the situation described above, can be regarded as the cause of the adaptation of language, i.e., the enlargement of the concept ‘number’. Now, whether an object falls under a concept is in the end determined by certain linguistic rules. Therefore, in order to effect an enlargement of a concept, the linguistic rules governing the use of the concept in question have to be altered. For example, in the story I just told, it was wrong to call complex numbers numbers, because one of the rules for the application of this concept required that numbers can be ordered on a line. This rule, among others, has to be changed so that it will be correct to call complex numbers numbers. It has to become a custom or an institution (cf. PI 199) to subsume the set of complex numbers under the concept ‘number’. We therefore have to consider the question why such a change of linguistic rules might occur.

In the story Wittgenstein seems to have had in mind, the rules of language changed because the newly discovered object defied the classificatory system of the language, i.e., the language, as it was before the adaptation, was unable to deal with this object. So, what caused the adaptation of the language was the discovery of an object that couldn’t be dealt with, i.e., couldn’t be classified. It is important here to note that it was not the presence of similarities between what we now call complex numbers and other kinds of number that caused the change of the linguistic rules. That the presence of such similarities cannot be the cause of such a change, is a lesson which can be drawn from the problem of wide open texture. If the mere presence of similarities was sufficient to effect a change of the rules of language, the fact that philosophy bears certain similarities to games would have to lead to a change in the rules of language. According to the view proposed here, the emergence of objects that cannot be classified causes the adaptation of the rules of language. However, not every discovery of a hitherto unknown object makes it necessary to adapt language. Some of these new objects will be unproblematic with regard to their classification.

What, then, does Wittgenstein mean when he says that it is because of certain similarities that we call an object ‘number’? Even though it is not the role of the similarities to cause an adaptation of the language, they determine how, or in what way, the language is adapted, if it is adapted at all. We extend the concept of number so as to include the complex numbers, and not some other concept, because the complex numbers were similar to other kinds of number. So, Wittgenstein’s claim here is that the language is adapted by enlarging one of those concepts that have family resemblance ties to the defiant new object. When a recently discovered object makes it necessary to adapt our classificatory system, then similarities determine which concept we extend in order to be able to classify the new object. In this sense it is because of certain resemblances that we call a newly discovered object ‘number’.

To summarize my interpretation of PI 67b: Wittgenstein says in PI 67 that we call something a number because of certain similarities. This means that we give the name ‘number’ to a newly discovered object that defies our classificatory system if it exhibits similarities with other objects falling under the concept ‘number’. We give this object the name ‘number’, and not some other name, because of these similarities. Thus, according to my interpretation, the verb to call in PI 67 has to be understood in the sense of giving a name to something, while the received interpretation understands it in the sense of calling something by a name it already has. Both interpretations of the...
verb ‘to call’ are of course possible, and this is also true for the German original ‘nennen’.

After this interpretation of PI 67, I would like to explain my interpretation of the statement “And it is because of this relationship, or these relationships, that we call them all ‘language’.” (PI 65), which turned out to be the main pillar for the received interpretation. As I argued above, PI 67 has to be regarded as an explanation of this statement. However, we can hardly read this statement in the same way as PI 67, i.e., as talking about ‘baptizing’ a new object. This is, on the one hand, because Wittgenstein uses the verb ‘to call’ in the present tense, and, on the other hand, because he is talking about “them all” (i.e., all activities called language) and not about a single object as in PI 67. Therefore, if we read ‘to call’ here in the sense of ‘to give a name to something’, this statement would claim that we now give the name ‘language’ to a whole set of activities. This is plainly false. Rather, Wittgenstein seems to be claiming here that the members of a certain set of activities today bear the name ‘language’, and that this is because of certain resemblances. Therefore, we have to read ‘to call’ here as ‘to call something by a name it already has’. However, PI 67 can also serve to explain the claim that all these activities bear the name ‘language’ because of their family resemblances: All these activities are called ‘language’ (today) because, when they first were discovered, or invented, or evolved, we gave this name to them. And we gave the name ‘language’ to them, rather than some other name, because of certain similarities. So, when Wittgenstein claims in PI 65 that there is nothing in common to all the phenomena called language, and that we call them all language because of similarities, he wants to say that this concept, as many other concepts like ‘game’ or ‘number’, has undergone a historical development in the course of which it was extended to new phenomena that were similar to some others falling under these concepts. The resemblances are the reason that the concept of language developed the way it did, such that we now call all those phenomena language we do.

It might be argued against my reading that it puts too much weight on a few allusions to a temporal process in PI 67 while it ignores the fact that most of Wittgenstein’s assertions regarding family resemblances have nothing to do with such a process. My answer to this objection is that Wittgenstein is talking about a temporal process only in his explanation of the (because-of-) relation between similarities and extensions of concepts, which he had claimed in PI 65b. And this explanation is indeed very short, it takes up only PI 67b. All the other paragraphs from PI 65b on do not aim at explaining this relation. I would like to give a list of what I think these other paragraphs are about: PI 65b formulates the question why we today call many different things by a common name and indicates Wittgenstein’s answer that this is because of certain similarities. So, this paragraph discusses states of affairs that obtains today, and therefore does not refer to a temporal process. Sections 66 and 67a explain the idea of a network of similarities and motivate the term ‘family resemblances’. Consequently, they do not refer to a temporal process either. PI 67b then explains, by claiming that a certain historical process took place, why we today use language the way we do. PI 67c, again, does not mention a temporal process. This is because this paragraph is meant to refute the idea that a disjunction of properties might count as something common and peculiar to the things falling under a concept. Finally, the text from PI 68 on is concerned with the usability of concepts with vague boundaries. In short, the fact that Wittgenstein most of the time does not talk about temporal processes does not cast doubt on my interpretation. This textual fact can be accounted for by seeing that the explanation of the relation between similarities and extensions of concepts is indeed very short, and that elsewhere in these sections, Wittgenstein is aiming at things other than explaining this relation.

The rest of this paper is organized as follows: Section 4 explains how my reading allows for a solution for the problem of wide open texture, and section 5 comments on interpretations proposed by Wennerberg and Hunter, which are similar to mine. Section 6 then deals with the problem that the theory of language development attributed here to Wittgenstein is empirical. There I will argue that he advanced it only for therapeutic
reasons.

4) Apart from the textual evidence for the historical reading found in PI 67, there is another argument in favor of it, namely that the problem of wide open texture can be solved in a straightforward fashion. As I mentioned before, the received interpretation, saying that an object falls under a concept if and only if it is similar to other things falling under that concept, leads to the problem that the extensions of concepts would have to be much wider than they actually are. This problem arises because the received interpretation takes the presence of similarities to be a sufficient condition for an object’s falling under a concept. At first sight, it might seem that a corresponding problem arises also for my interpretation. One might argue that, according to my interpretation, a concept is extended to every object that has family resemblances with other objects already falling under the concept. However, this argument ignores the role which similarities play according to my interpretation. Their role is not to cause enlargements of concepts but only to determine which concept is enlarged. The similarities determine only how language is changed, if it is changed at all. Consequently, similarities are only necessary but not sufficient for extending a concept to a new object. When an activity exhibits resemblances with games, it does not follow that the concept ‘game’ will be extended to this activity. In this way, the historical interpretation avoids the problem of wide open texture.

5) Interpretations similar to mine have already been proposed by Hunter [5] and by Wennerberg [6]. Like me, Hunter thinks that Wittgenstein’s remarks are to be taken in a sociological sense:

“Wittgenstein could have used the ‘because of’, not in the criterial sense, as it is natural to suppose, but in this kind of sociological sense.”

According to Hunter, Wittgenstein thinks

“that in the evolution of language the extension of a concept may have been gradually enlarged, here to include this, because of such an such a similarity, and there to include that, because of a quite different similarity.” [5, p. 54]

However, Hunter fails to distinguish between the use of ‘because of certain similarities’ in PI 65 and in PI 67. Only in the latter place does Wittgenstein argue that our concepts are enlarged according to (because of) similarities. In PI 65, Wittgenstein says that the concept of game, for example, has the extension it has today, because it has been gradually enlarged according to (and in this sense because of) similarities. Thus it can also be said that the concept ‘game’ has the extension it has today because of certain similarities. So, my view is that Hunter’s interpretation is a correct interpretation of PI 67 but not of PI 65. Hunter himself regards his sociological interpretation as an interpretation of PI 65.

Wennerberg is another scholar who proposed to read the remarks on family resemblances as a thesis about the evolution of language. However, he does not base his interpretation on textual evidence. One gets the impression that Wennerberg adopts his reading only because it allows for a plausible solution of the problem of wide open texture. Wennerberg argues that, faced with this problem, Wittgenstein could either give a much more exact definition of similarity or

“he could admit that family resemblances between a set of objects is not a sufficient but only a necessary condition for the existence of a general term which denotes these objects. I think he took the latter view.” [6, p. 117]

Wennerberg goes on to explain:

“When a new object emerges that has not yet been subsumed under any term it will be subsumed under some term A because it is similar to some of the objects already subsumed under A.” [6, p. 117]

Wennerberg argues that such similarities cannot be sufficient for the enlargement of a concept by considering the case that a new object has family resemblances to several concepts. In such a case, why is this object subsumed under one concept rather than another?
“People make a decision to subsume the new object under A and not under B. Such a decision is similar to a convention but this does not mean that it is completely arbitrary.” [6, p. 118]

The word ‘decision’ should be read in scare quotes here. What Wennerberg is getting at, seems to be that it somehow has to become a custom or an institution to apply the concept to a new object. Without such a ‘decision’ there would be no change of the rules of language. And the mere presence of similarities does not force the linguistic community to enlarge a concept to an object.

Interestingly, Wennerberg notes the following problem for his interpretation:

“This theory is partly of an empirical nature: it tries to explain why we classify objects as we do.” [6, p. 119]

His reading attributes an empirical claim about the development of language to Wittgenstein, a claim that does not seem to belong in a philosophical investigation. The empirical nature is clear from the following consideration: Even if we accept that the emergence of a new object that defies our classificatory system causes an adaptation of this system, it is not necessary that this adaptation should occur by enlarging a concept. We could also adapt language by introducing a new concept. Of course, this problem also exists for my interpretation, and I will propose a solution to it in section 6 of this paper.

6) Even though there is considerable textual evidence for the historical reading, and it allows for a solution of the problem of wide open texture, it has received relatively little attention. This is probably due to the fact that, according to this reading, Wittgenstein does not give a philosophically interesting answer to the question of why we call all games games. The answer my reading attributes to Wittgenstein – we call these activities games because of the historical development of language according to similarities – seems to be an empirical hypothesis rather than a philosophical theory, a hypothesis that doesn’t even belong in a philosophical investigation of language.

I think that this is correct, i.e., I have to admit that I am attributing a philosophically uninteresting claim to Wittgenstein here. However, I would argue that Wittgenstein did not mean to give a philosophical answer to the question posed by the interlocutor. Rather, his answer is to be seen in the context of his strategy of bringing “words back from their metaphysical to their everyday use.” (PI 116) In order to explain the difference between these two uses of language, I would like to turn to PI 189, which, I think, provides an example of this difference. In the sections before PI 189, Wittgenstein has shown that, in the end, there is nothing one can say when a pupil insists on his way of understanding a formula (e.g., for the series ‘+ 2’). Impressed by this argument, the interlocutor asks: “But are the steps then not determined by the algebraic formula?” After remarking that this question contains a mistake, Wittgenstein gives two examples of ways in which the words “the steps are determined by the formula” are actually used in our everyday discourse.

(1) We could use these words in a psychological context: Given that some people have been taught to use the formula \( x = y^2 \) in a certain way, whereas others do not know how to use it, one can say that for some people the steps are determined by the formula, but not for others.

(2) We could also use these words in a mathematical context: There are algebraic formulae that uniquely determine a number \( x \) for a given value of \( y \) (e.g., \( x = y^2 \)), and others that do not uniquely determine a value (e.g., \( x \neq y \)). In this context, one can say that some formulae determine the steps to be taken, while others do not.

According to Wittgenstein, these are two ways in which we actually use the words ‘Are the steps determined by the formula?’, i.e., they constitute everyday uses. However, the interlocutor is not likely to be satisfied with these answers. When he inquired into how formulae determine the steps to be taken, he did not use the word ‘determine’ in the psychological sense that some people know how to use a certain sign, nor in the mathematical sense that in a psychological context...
sense of unique determination. Nor does he seem to use the word ‘determine’ in any other sense in which it is ordinarily used by us. This dissatisfaction with such practical answers shows that the interlocutor does not understand the question in an everyday way, but in a metaphysical or philosophical way. So, PI 189 shows that there are several ways of understanding one and the same question and that some of these ways are everyday ones, which Wittgenstein deems legitimate, and that others are metaphysical or philosophical ones, which contain a mistake. Something similar can be found in PI 85. Here, Wittgenstein considers the question as to whether a sign-post leaves room for doubt or not. Wittgenstein first gives a philosophical answer, namely that the sign-post does not leave room for doubt, even though it could be interpreted in several ways. However, at the end of this section, he corrects himself and writes:

“Or rather: it sometimes leaves room for doubt and sometimes not. And now this is no longer a philosophical proposition, but an empirical one.”

I hope that it is intuitively clear that there is a difference between understanding a question in a philosophical way and understanding it in an everyday way. However, it is hard to state in general terms what this difference consists in. One clue is given by the fact that using words metaphysically is something illegitimate in Wittgenstein’s eyes. For Wittgenstein, speaking is part of an activity (cf. PI 23), i.e., it is interwoven with non-linguistic actions (cf. PI 7). This activity, in the course of which we use linguistic utterances, can be regarded as the context of this utterance. The meaning of an expression depends on the context in which it is uttered, and, divorced from its context, an utterance has no meaning at all. Thus, it can be assumed that using words metaphysically generally consists in using them outside any such context and interwovenness with other activities.

Gordon Baker has argued that the metaphysical use of language is characterized by a concern for properties that hold generally and without exception [2]. I think that I can agree with this conclusion, which is also exemplified in the two cases just considered. In the case of PI 189, the interlocutor is looking for a property that explains in general how formulae determine the steps to be taken. Wittgenstein’s everyday kind of answers, on the other hand, do not say anything about formulae in general. They say that some formulae uniquely determine a value and others do not. In the case of PI 85, there is the philosophical question of whether sign-posts in general leave room for doubt. Wittgenstein’s everyday kind of answer, again, is not general: He says that some sign-posts leave room for doubt. In this sense, I can agree with Baker’s conclusion that the metaphysical use of language is concerned with the essences of things (cf. [2, p. 298f]).

Next, I would like to consider the question as to what Wittgenstein means by bringing words back to their everyday use. The phrase “bringing the metaphysical use back to an everyday use” could simply be understood as “stopping to use words metaphysically”. However, this interpretation cannot account for the fact that Wittgenstein talks of ‘bringing back’. As an alternative, I would like to submit the interpretation that bringing back a metaphysical question to an everyday use involves assuming that it was uttered in a certain everyday kind of context, a context in which that question is interwoven with other activities, and in which it has an ordinary and humble sense (cf. PI 97). This is what Wittgenstein does in PI 189. As we have seen, the interlocutor poses a metaphysical question and his words do not seem to stand in any ordinary kind of context. Wittgenstein reacts by saying that it could be seen as standing, for example, in a psychological context or in a mathematical context. Then the question would acquire a certain meaning and could be answered in one way or the other. So, I think that bringing words back to an everyday use consists in relocating them into an everyday kind of context.

Now, Wittgenstein seems to be applying the

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7 Another example of a proposition mentioned by Wittgenstein that can both be understood in an everyday way and a metaphysical way is “One cannot step into the same river twice” (cf. TS 220, §111).

8 Baker does not address this question in his article (cf. [2, p. 290]).
same strategy in the family resemblance remarks. When the interlocutor comes up with the question ‘Why do we call all games games?’, he certainly expects to get a metaphysical answer, something on par with his own view that there is an essence common to all games. He wants to know why we call many different objects by a common name, i.e., he inquires into the essence of the relationship between a concept and its instances. So, the interlocutor is using this question metaphysically in PI 65. Wittgenstein, however, reacts by making an empirical claim about the development of language, i.e., he reacts as if the interlocutor had uttered his question in the context of a discussion about the historical development of language. In such a context, this question, which amounts to an illegitimate metaphysical question in the mouth of the interlocutor, has an ordinary and humble sense: ‘Why do we call all games games?’ can be taken to express the question for the laws that govern the historical development of the set of objects falling under a concept. According to my interpretation, Wittgenstein takes the interlocutor’s question in this ordinary and humble way and answers it by making an empirical claim about the evolution of language.

If it is true that Wittgenstein presents this empirical family resemblance theory of language development in order to bring the interlocutor’s metaphysical question back to an ordinary context, then we can assume that he does not really want to defend this particular hypothesis. He puts it forward only in order to indicate what kind of answer could sensibly be given to the interlocutor. This explains why Wittgenstein introduces his answer with the words “well, perhaps”. He writes:

“Why do we call something a number?
Well, perhaps because it has a direct relationship with several things that have hitherto been called number;” (PI 67, my emphasis, in German “Nun etwa”)

To sum up: It could be objected that the historical interpretation ascribes to Wittgenstein a philosophically uninteresting answer to the interlocutor’s question. I have to admit that this is true. In my view, however, the account of family resemblances is not meant to be a philosophically interesting theory, just as the remark that formulae of the kind \( x = y^2 \) determine a value for \( x \) and formulae of the kind \( x \neq y \) do not is not meant to be philosophically interesting. Rather, the philosophically interesting thing in both cases is the way in which Wittgenstein reacts to metaphysical questions.

7) Finally, it might be asked how the historical interpretation fits into the wider context of PI 65. For reasons of space, I cannot answer this question adequately here, but still I would like to indicate what I think about this matter. As is well known, the interlocutor reproaches Wittgenstein in PI 65 with talking about language games all the time while not saying what a language game is. The interlocutor wants to know what the essential properties of language games are, i.e., he is asking for something that would correspond to the general form of a proposition. It might be objected against my interpretation that, in order to defend his decision not to look for the universal form of language, Wittgenstein has to show that there is no essential property of the instances of a concept. To do this, the objection goes, he has to propose a new theory of concepts that has to prove more adequate than the traditional one. On my reading, Wittgenstein does nothing of this sort, he does not even engage in a philosophical (metaphysical) discussion with the interlocutor.

However, I do not see why the burden of proof is on Wittgenstein to show that there is no essence of language. In PI 1-64, he has introduced his method of language games, and he has shown that it is a powerful tool to get rid of metaphysical problems. So, Wittgenstein can be quite satisfied with what he has achieved, and this achievement is independent of whether or not there exists a general form of language. Therefore Wittgenstein can simply let himself off the hook with regard to this search – er schenkt sich die Suche (cf. PI 65a). This last phrase indicates that Wittgenstein does not bother to look for an essence of language. He thinks it unlikely that it exists and he can do very well without it. If, on the other hand, the interlocutor was able to show that there has to be an essence to each concept, it seems that then also Wittgenstein would be obliged to look for it, be-
cause in this case it seems possible to achieve much greater clarity by finding this essence than by using Wittgenstein’s method of language games. Therefore, Wittgenstein only has to refute arguments proposed by the interlocutor for the thesis that each concept must have an essence. Assuming this as true, the argumentative structure of PI 65-77 can be seen as follows: The interlocutor offers three different arguments for the claim that for each concept there has to be an essence. The first argument is discussed in PI 65-67, the second in PI 68-71, the third in PI 75-77. The sections PI 72-74 form an appendix to the second argument.

In PI 65, the interlocutor implicitly puts forward the argument that there must be something in common to the instances of a concept because we use one and the same name for them all. He brings up the question ‘Why do we call all of these different things by one and the same name?’ and assumes that the only possible answer is ‘There is something common and peculiar to all of them’. According to my interpretation, Wittgenstein refutes this argument in a very peculiar (Wittgensteinian) way: By bringing the interlocutor’s question back from the metaphysical to an everyday use. Wittgenstein thus intimates that the interlocutor’s argument involves a misuse of language. In PI 68-71 Wittgenstein discusses the question whether concepts without fixed boundaries, such as concepts without essence would be, would be useful at all. Therefore, these sections can be read as the refutation of another argument for the claim that there has to be an essence: The argument that without essence, concepts would be useless. Finally, PI 75-77 discuss what it means to know the meaning of a concept. Here, the interlocutor argues implicitly that our knowledge of meanings can only consist in subconscious knowledge of an essence. Wittgenstein refutes this by drawing attention to the way in which we explain concepts to others and by saying that such explanations already contain our entire knowledge of the meaning of concepts.

References


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9He brings up this question only implicitly. In the text of PI 65 we only find Wittgenstein’s reaction to such a question.
Must Values Have Subjective Existence?
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Abstract
In this paper, I begin by outlining two assumptions that are routinely taken for granted in ethical discourse, one of which entails that values have subjective existence. I then consider a causal account of valuational activity offered by Bruce Morito which serves to question the truth of that assumption, and the extent to which that account falls short of overturning it. Finally, in light of that short-coming, I sketch what I characterize as a “quasi-objective theory of value” the truth of which depends upon a non-regularist and non-actualist conception of laws of nature.

This tendency to reduce moral properties to subjective properties seems due to two assumptions often presupposed by ethical theory that are conceived as nearly tautological. First, much ethical theory proceeds on the assumption that

(1) actions possess moral properties only if actions possess value.\(^1\)

While this assumption has been routinely taken for granted as a point of departure in ethical discourse, technical as well as ordinary, it is not an assumption that ethicists need to presuppose. Why the assumption is regularly made is by no means clear, but the writings and discourse of some theorists vaguely attach its plausibility to the widely accepted ontological distinction between fact and value, primary and secondary qualities, which is assumed to originate to some degree with Descartes and Locke, and carried forward in the work of Hume, and finally Kant in some sense.\(^2\) Roughly speaking, according to this distinction there are two domains of objects of inquiry: (i) a domain of quantifiable, empirical, and hence “factual” entities (ultimately reducible to

According to much ethical theory, recent or otherwise, the prevailing tendency is to suppose that moral properties of actions have subjective existence [7, 2, 4, 21, 8, 13]. Indeed, that such a tendency has persisted throughout the history of ethical theory is further underscored by considering Thomas Hobbes’ reduction of moral properties to objects of aversion and desire, as spelled out in Leviathan [10], Ch. VI and Ch. XI, David Hume’s reduction of moral properties to feelings of approbation and disapprobation in his Treatise of Human Nature [11], Book III, Section II, and Jeremy Bentham’s and John Stuart Mill’s justifications for Utilitarianism, as articulated in An Introduction to the Principles of Morals and Legislation [1], Ch. I, and Utilitarianism [15], Ch IV, respectively. In fact, on some reading, there is also reason to interpret Kantian Ethics, and various species of deontological ethics, as subjectively grounding right and wrong, moral accountability and obligation, in the sense of subjective existence intended herein.

\(^1\)Indeed, that grounding moral properties in value is prevalent throughout recent ethical theory explains why so much analysis of the nature of value appears in current ethical discussion.

\(^2\)Many ethicists have remarked on this ontological legacy embedded in current ethical discussion. Sometimes referring to it as the view of “scientific naturalism” or the “classical scientific world view”, Baird Callicott characterizes it in the following way: “A fundamental doctrine of modern science remains a formidable obstacle, however, to all the heroic attempts of philosophers to establish the existence, and adequately explain the nature of intrinsic value, the value of something in and of itself. The objective physical world is sharply distinguished from subjective consciousness in the metaphysical posture of modern science as originally formulated by Descartes. Thought, feeling, sensation, and value have ever since been, from the point of view of scientific naturalism, regarded as confined to the subjective realm of consciousness. The objective, physical world is therefore value-free from a scientific point of view” [4, p. 132]. And again, according to Callicott, “From the scientific point of view, nature throughout, from atoms to galaxies, is an orderly, objective, axiologically neutral domain. Value is, as it were, projected onto natural objects or events by the subjective feelings of observers. If all consciousness were annihilated at a stroke, there would be no good and evil, no beauty and ugliness, nor right and wrong; only impassive phenomena would remain” [4, p. 147]; cf. also [20, pp. 127-8; p. 132]; [17, p. 50].
combinations of matter and motion) the existence of which is not dependent on subjective acts of awareness, and (ii) a domain of irreducible, non-quantifiable, nonempirical, and hence nonfactual entities, the existence of which depends upon subjective acts of awareness. It has been taken for granted that whatever values are, they are properly members of the latter domain.3

The second assumption that ethical theory typically presupposes is that

(2) something has value only if some being performs a particular act of consciousness,4

where the beings capable of performing such specific acts are typically, but not always, restricted to the class of human beings.

Alternative formulations include further temporal specificity, such as

(2a) something has value at $t$ only if some being performs a particular act of consciousness at $t$, or

(2b) something has value at $t$ only if some being performs a particular act of consciousness at some time.

Various considerations render (2a) and (2b) more or less plausible, and various consequences follow from each. For example, according to (2a), if no one is performing the relevant act of consciousness at $t$ with regard to a particular object, call it “$O$”, then $O$ has no value at $t$. And this seems to be, at first glance, intuitively plausible. For if there is no one around to value $O$, it would seem that $O$ has no value—for example, if all Homo sapiens capable of the relevant, value-constituting acts of consciousness were annihilated in a single stroke (cf. Note 2 referenced above).

On the other hand, according to (2b), $O$ may be said to continue to possess value even when no one is valuing $O$. And this also may make some sense if one imagines that, for example, money may be said to retain value even if everyone capable of conferring value on money momentarily suspended the appropriate act of consciousness by means of which money is purported to have value.

Rather than address the plausibility of (1), it is the purpose of this paper to consider how (2) may be falsified, and thus, how theories that presuppose (2) may be mistaken. In the following, I intend to explain how.

One way that serves to question (2) has been proposed by Bruce Morito, who argues that the anthropocentric version of (2) is “superficial and leads to false conclusions regarding the relationship of human interest to nature” [17, p. 51]. Mirroring remarks made in an earlier work by Holmes Rolston III, (cf. [20, p. 136; p. 138]), Morito asserts:

“Values do not originate in the human subject, but in interaction between evolutionary processes and the human organism throughout history. As human beings evolved, their constitutions changed, which in turn changed the values they held […] So, at minimum, our values originate independent of any preferences we consciously hold because they are causally dependent on the relationship between our particular constitutions and the environment” [18, p. 36] (cf. also [17, pp. 56-78]).

It is novel to suggest the importance of the evolutionary processes in the shaping of values, and not to exaggerate the contributions of human consciousness as singularly, and in isolation, causally responsible for values. But to say that environmental processes and circumstances causally contribute to the shaping of the values conferred on nature does not entail that values would be conferred on nature, and thus would exist, in the absence of human acts of valuing. So, at least in this respect, the anthropocentric version of (2) is not clearly undercut by emphasizing the importance of

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3 Consider Callicott’s account and attribution of this position to Hume in [3, pp. 160-161]

4 As Holmes Rolston, III, suggests, “[…] under prevailing theories, it is widely held that the phrase “unexperienced value” is a contradiction in terms, with “experienced value” a tautology” [20, p. 138; p. 144]. Furthermore, the implications of (2) are nicely summed up by Callicott’s remark that “[i]f all consciousness were annihilated at a stroke, there would be no good and evil, no beauty and ugliness, no right and wrong; only impassive phenomena would remain” [4, p. 14].
these factors. That is, even from Morito’s evolutionary axiology, it seems as if human beings and their acts of consciousness may remain the sole avenue by means of which the universe comes to possess value.

A step on the way to establishing the falsity of (2a) or (2b) might begin with the assertion that the possession of value by something, and hence something being “valuable” in some respect, may not depend upon actual performances of certain acts of consciousness, be those acts ones which humans or nonhumans are capable of performing. For if something can possess value, even without the actual performance of the relevant acts of consciousness performed by human or nonhuman beings, then it would appear that the existence of value is not dependent upon instantiated acts of human consciousness. In this respect, the existence of such value may be said to be both “nonanthropocentric” and “objective”, or at least “quasi-objective.”

It may help, at this point, to cursorily explain what I have in mind by the expression “actual” performances. *Prima facie*, a particular act of consciousness at a particular moment, call it “t”, is an “actual” act of consciousness at t only if it is instantiated at t, that is, only if it actually occurs or is actually performed at t. In contradistinction, a particular act of consciousness at t is a “potential” act of consciousness at t even if it does not occur at t, but, in some meaningful sense, “could” occur at t; its existence as a potential act of consciousness at t does not depend upon its occurrence at t. And by extension, one may specify a “purely” potential act of consciousness as an act of consciousness that never occurs at any point in time, but, in some sense, “could” occur at some time.

Using this distinction, one may begin to sketch a quasi-objective theory of value. One may first assert that a given something may possess a value, say the characteristic of being beautiful, even though no one ever actually finds it beautiful; that is, its possession of the property “beauty” does not depend upon the actual performance of some relevant act of awareness or consciousness. For example, one may propose that it possesses “beauty” at t if, subjunctively speaking, some person would find it beautiful were some person to perform the relevant acts of consciousness, which, as a matter of fact, no person will.

What might give such a proposal plausibility? I suggest that the proposal may be rendered plausible by appeal to a coupling of Morito’s causal reasoning about the relationship between evolutionary processes and the emergence of certain interests with a particular view of what laws of nature are. The latter view is a non-Humean, “non-regularist” or “non-actualist” one which treats laws of nature as nomological relationships the existence of which are not dependent on the existence of actual phenomena, but the existence of which would govern the actual behavior of actual phenomena, if such phenomena were ever to become actual (cf. [23, 5, 6, 9, 16, 19]). According to this view, a statement such as “All Fs are G” may qualify as a statement of law even when (i) “All Fs are G” is uninstantiated, and (ii) “All Fs are G” is not entailed, even in part, by any instantiated universal statement, where any universal statement, “All X are Y” is said to be instantiated when there exists at least one member of the class represented by the subject term (X).

Conceiving laws of nature as construed in the account above, consider also how Morito defines the relation between the having of interests, the predication of value and the environment:

“[...] our having interests and our ability to confer value is dependent upon environmental processes. They are de
ependent on the development history of the organism both as a member of a species and as an individual. Insofar as this development is a process of interaction between organism and environment, causally significant influences are involved in the dependency relation. Environmental conditions and processes, then, are preconditions for the existence of interest and value conferring activity in a causally relevant sense” [17, p. 52].

Assume also that Morito imagines that the process of interaction is nomologically governed. Accordingly, one may suppose the existence of laws of nature that govern the shaping of human and nonhuman interests, nonhuman valuing activity, and hence values.

Given our brief account of laws of nature, one may then suppose that there are laws of nature that govern the emergence of interests even if at no moment the actual environmental conditions and the existing state of human nature are favorable to the development of such interests. That is, one may suppose the existence of laws of nature that govern the emergence of interests which the actual environmental conditions and state of human nature preclude from existing. Indeed, Morito assumes the existence of such uninstantiated possibilities, the instantiations of which are nomologically governed, when he counterfactually asserts that “[...] if the balance of environmental facts had been qualitatively and significantly different, our present interests would be different” [17, p. 52f]; [22, p. 239].

Let us now combine the above account of laws with Morito’s counterfactual remarks about uninstantiated, but possible interests. Imagine that there are laws of nature that govern the emergence of dispositions to find an interest in certain phenomena, as well as laws of nature that govern the actualization or manifestation of those dispositions. Could it plausibly be said that had a given agent, at a certain moment, call it “t”, possessed a disposition to experience something, call it “S”, as beautiful, then the agent would have experienced S as beautiful at t? But wait. The mere possession of a disposition to experience something as beautiful does not seem to be a sufficient condition to experience something as beautiful. For if it were such a sufficient condition, then any agent who possessed such a disposition would always experience beauty as long as it possessed that disposition. Assuming this is counter-intuitive, perhaps it should be admitted that for there to be an experience of something as beautiful, other conditions must be satisfied in addition to the instantiation of the disposition to experience something as beautiful. Now, one might suppose that these additional conditions are additional properties of the agent who experiences beauty. And one might add that whenever the agent possesses these additional properties in conjunction with the disposition to experience something as beautiful, the agent experiences something as beautiful. But if one conceives all the properties necessary for the experience of beauty as agent-dependent, then it is not obvious to what one would appeal to explain why any given agent experiences one thing as beautiful and not another. Indeed, to explain why an agent experiences one thing as beautiful at a given moment, t, and not another thing as beautiful at t, it would seem that one needs to postulate that the thing experienced as beautiful possesses certain features that serve to pick it out from a range of possible objects to be experienced as beautiful at t. Going back to our case above, one might then say that not only is it necessary to suppose that the agent who experiences S as beautiful at t needs to possess the relevant disposition to experience S as beautiful, but it is also necessary to suppose the fulfillment of certain additional conditions, including apparently objective features of S that serve to explain why S as opposed to something else is experienced as beautiful at t. Appealing back to our account of laws of nature, one could then imagine that not only must an agent possess a disposition to experience something as beautiful at a given moment in order for an experience of beauty to exist, but the existence of such an experience also requires that the thing experienced as beautiful by an agent must have certain properties that constitute a disposition to be experienced as beautiful by an agent. Furthermore, one might add that the relationship between the disposition to experience and the disposition
to be experienced stand in a nomologically governed relationship that exists independent of the instantiation of “being experienced” or “experiencing,” and even independent of the instantiation of the “disposition to experience”.  

It should be noted that some suggestion of the foregoing has already been made. For example, Homes Rolston’s general view of the modern scientific distinction between what qualifies as objective and what qualifies as subjective foreshadows what is implied here about the ontological seat of value. Consider Rolston’s remarks:

“All natural science is built on the experience of nature, but that does not entail that its descriptions, its “facts”, just are those experiences. All valuing of nature is built on experience too, but that does not entail that its descriptions, its “values”, are just those experiences. Valuing could be a further, nonneutral way of knowing about the world. We might suppose that value is not empirical, since we have no organs and can make no instruments for it. But it could just as well be an advanced kind of experience where a more sophisticated, living instrument is required to register natural properties” [20, p. 139].

Indeed, to further the plausibility of the foregoing ontological account of value, consider an analogy. One might argue that an experience of a thing as red requires that the being having the experience of redness possess the appropriate disposition to experience the thing as red. That is, an agent without the appropriate disposition to experience a thing as red will not experience a thing as red. But presumably, supposing the existence of a disposition to experience something as red is not sufficient to explain the experience of something as red. What is needed in addition to the presence of the disposition is that the thing so experienced as red must possess some properties or characteristics that serve in part to trigger the experience of redness, to actualize the disposition to experience redness, and these properties or characteristics reside in the things so experienced as red whether or not any being has the corresponding disposition to experience the thing as red. By analogy, in order for something \( S \) to be experienced as beautiful by some living being, there must not only be a disposition in that living being to experience \( S \) as beautiful, but also, \( S \) must possess some properties which in part trigger that living being’s disposition to experience \( S \) as beautiful. And just as the experience of an object as beautiful cannot occur if the agent who has that experience does not have the appropriate disposition to experience the thing as beautiful, so the object cannot be experienced as beautiful if the object does not possess certain features that, in part, serve to trigger and make manifest the agent’s disposition to experience that object as beautiful. And, just as the ontological possession of the disposition by the agent is not dependent on the object possessing the appropriate features to be experienced as beautiful, the ontological possession of those features by the object so experienced is not dependent upon the existence of the disposition to experience the object as beautiful.

To cause trouble with the foregoing, one might insist that the characteristics of \( S \) that trigger the experience of \( S \) as beautiful are not the “beauty” of \( S \). Rather, one might point out, those characteristics of \( S \) are only partial conditions of the experience of beauty, where the other necessary conditions of the experience of beauty reside in the being who experiences \( S \) as beautiful.

As Rolston suggests, “[s]ubjective experience emerges to appreciate what was before unappreciated. But such valuing is a partnership and the free-standing objective partner cannot enjoin value upon the subjective partner if it has nothing to offer” [20, p. 142].

Rolston presents something like this objection in the following: “To say that wood is combustible means that wood will burn if ignited, although it never nears fire. But this is a predicate of objective potential; wood may ignite
one may then add that these other conditions constitute the “real”, ontological, seat of beauty. But if we consider our ordinary discourse about beauty and redness, we might just as easily say that when a thing is called “beautiful” it is so-called because it is imagined to possess some characteristic which triggers the experience of it as beautiful, and thus, that the referent of the term “beautiful” is some characteristic of the thing so-called, and not some feature of the disposition to experience the something as beautiful, nor, for that matter, any other property which resides exclusively in the being who experiences that something as beautiful. Likewise, one may argue that while the experience of redness resides in the observer of redness, the term “red” is meant to refer to the characteristic in the thing experienced as “red” which triggers the experience of redness in the observer, and this is why we call the thing experienced as red “red”, and not some feature of the agent who has the experience of red.

By way of a fictional case, let us now further elaborate our ontological account of value by connecting it more explicitly to the non-Humean account of laws of nature. Suppose a world, call it “W”, which contains a nonhuman species, call it “N”, no member of which is capable, at a certain time, t, of experiencing any given flower of that world as beautiful because no member of N possesses the appropriate disposition to experience any given flower as beautiful. Suppose also that evolutionary processes, call them, “P”, are such in W that were P to be interfered with by a certain occurrence, call it “O”, at t, members of N existing posterior to O would develop the perceptive capacities necessary to experience some given flower as beautiful. On the basis of these suppositions, could one say that a given flower at t possesses a disposition to be experienced as beautiful and that this disposition is constituted by certain, objective, actual properties of that flower at t? And could one then say that these objective properties exist as characteristics of the flower, whether or not the necessary corresponding perceptive capacities ever develop, and thus, whether or not experiences of beauty ever become manifest in W?

On the basis of this approach, we may broaden our claims. We may suppose that there exists a myriad of laws of nature linking numerous objective properties of entities with numerous capacities humans and nonhumans could develop were circumstances to become right, but which actually do not develop because circumstances, as a matter of fact, do not become right. And could we say that these laws entail nomological relationships between dispositions to experience certain objective features of things and the presence of those objective features in things, dispositions which will never be actualized in any actual observer, because as a matter of fact, environmental processes have not been and will not be suitable for their development; that is, there will be no appropriate Os which would trigger the appropriate environmental processes that would lead to the development of those dispositions. We may then imagine a world with a multitude of objective seats of value, e.g. seats of beauty, etc., which are latent, unmanifested, ready to trigger experiences of beauty, of value, etc., or objectively speaking, to be “noticed” once or if circumstances become right. And by extension we might argue the same about a myriad of laws of nature which cannot be empirically noticed, because, while operative, were circumstances to become right and they became instantiated, as a matter of fact, circumstances will not become right and the laws of nature will not become instantiated.

The above is merely a preliminary sketch of how an objectivist account of value may be formulated. Naturally, such an account presupposes a certain ontological view of laws of nature. If this view and its application to the nature and relationship of values to acts of valuing have theoretical merit, then there may be a way of providing a plausible alternative to the view that values have only subjective existence.

in the spontaneous course of nature. But to say that wood is valuable is a predicate of subjective potential. If a human subject appears in relation to wood, wood can be valued. This sort of dispositional predicate can be realized only in human experience. Some exception can here be made for subhuman experience” [20, p. 141].
References


Über die Voraussetzungen der Erfahrungserkenntnis aus der Sicht des Operationalismus
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Abstract
It is shown that—at least in contemporary physics—the single measuring results are completely irrelevant w.r.t. testing a metric law. As a consequence, laws are neither gained by induction nor rejected by falsification. Even statistical generalizations of measuring values, when—in the sense of some philosophers—falsifying laws are not always regarded as falsifying instances by physicists.

Mit dieser Schrift möchte ich meine Sicht des wissenschaftstheoretischen Arbeitens kurz und prägnant darstellen. Ich will also nicht irgendwelche großen und fertigen Teilbereiche aus der Wissenschaftstheorie vorstellen, etwa die Definitionslehre, oder die Theorie der Metrisierung, oder die Theorie der epistemologischen Wahrscheinlichkeit; vielmehr möchte ich hier das operationale wissenschaftstheoretische Vorgehen in der rationalen Rekonstruktion konkreter methodologischer Probleme darstellen. Dieses wissenschaftstheoretische Arbeiten am konkreten Fall verhilft dazu, das methodische Vorgehen in den Erfahrungswissenschaften bei der Ermittlung allgemeiner Zusammenhänge in den Einzelheiten und vor allem auch im Hinblick auf die dabei gemachten Voraussetzungen und deren jeweiliger Berechtigung versteht zu können.

Diese Rekonstruktion wird zeigen, dass die herkömmliche Unterscheidung der empirischen Sätze in Theorien bzw. Gesetze und in Erfahrungen bzw. Beobachtungen zu grob ist und daher verfeinert werden muss, damit sie zu einem geeigneten intellektuellen Instrument der wissenschaftstheoretischen Erfassung dieses Prozesses der Ermittlung und Prüfung von Gesetzen anhand von Messungen werden kann. Ich werde sechs Typen oder Klassen von erfahrungswissenschaftlichen Aussagen unterscheiden, die inhaltlich und methodisch oft eng miteinander verwoben sind und die nur in der idealen methodologischen Situation der rationalen Rekonstruktion so einfach und schön voneinander abgrenzbar sind:

(1) die Wahrnehmungssätze (im speziellen Fall die Messergebnisse),
(2) die Beobachtungssätze (die gesicherten Wahrnehmungen bzw. Messwerte),
(3) die Erfahrungssätze (die auf Bezugsklassen verallgemeinerten Beobachtungssätze),
(4) die metrischen Gesetze (die mathematischen Systematisierungen geeignet zusammenhängender Erfahrungssätze),
(5) die Theorien (die systematischen Darstellungen metrischer Gesetze wie auch der ihnen zugrunde liegenden Messtheorien), und
(6) die Rahmenbedingungen (die etwa festlegen, im welcher Geometrie bzw. Chronometrie Theorien und Gesetze zu formulieren sind, ob und welche Homogenitätsforderungen für Raum und Zeit zu gelten haben, welche Erhaltungssätze zu verlangen sind usw.).

Diese Unterscheidung von empirischen Sätzen kann bei Bedarf weiter verfeinert werden. So können insbesondere die Erfahrungssätze, die hier ja bereits als Verallgemeinerungen und somit als nicht-singuläre Sätze verstanden werden, in strikte und statistische Aussagen untergliedert werden; und in konkreten methodologischen Fällen werden noch weitere Feinunterschiede zu berücksichtigen sein.

Auf ein weiteres, hier nicht näher ausgeführtes Resultat dieser Analyse möchte ich ebenfalls verweisen: Auch die herkömmlichen Methoden der enumerativen wie der eliminativen Induktion sowie der Falsifikation sind Verfahren, die bereits in vielen Fällen der klassischen Physik nicht mehr ohne Einschränkung anwendbar sind: Die herkömmliche Induktion führt demnach zu keiner der gegenwärtig akzeptierten Theorien, die mit
den akzeptierten Beobachtungen verträglich sind; und die Methode der Falsifikation verwirft sie alle, da stets akzeptierte Messungen zu Werten führen werden, die weitab von dem liegen, was die Theorie bzw. die Gesetze vorher gesagt haben.

Über die Rahmenbedingungen möchte ich in diesem Aufsatz wenig sagen, sondern eigentlich nur darauf verweisen, dass sie zum Allerheiligsten der erfahrungswissenschaftlichen Theorien gehören; an ihnen zu rütteln heißt, eine erfahrungswissenschaftliche Revolution einleiten zu wollen. Sie entscheiden nicht nur, in welcher Sprache die empirischen Theorien zu formulieren sind, sondern auch, in welcher die den Messungen erkenntnistheoretisch vorgängigen Messtheorien darzustellen ist, die ja in entwickelten Theorien ein Teil von diesen sind. Wenn die Rahmenbedingungen dann unterschiedliche Rahmen bereitstellen, etwa die euklidische Raumstruktur für die Messtheorie und die nichteuklidische für die Gesamttheorie, so muss sie diese Diskrepanz klären können.

Im Allgemeinen werden dann praktische Gründe der folgenden Art angegeben: Die Messtheorie handelt nur von einem sehr spezifischen Teil des Universums, nämlich von Messapparaten, d. h. von Objekten der Größenordnung, für die approximativ die Euklidizität vorausgesetzt werden könne; und mit euklidischen Sätzen lasse sich ein leichter rechne als mit nichteuklidischen. Wie immer dem sei: Das Vorhandensein solcher Diskrepanzen und die Notwendigkeit ihrer Erklärung zeigt, dass die jeweils gemachten Voraussetzungen nicht selbstverständlich oder gar a priori beweisbar sein können. Dennoch entscheiden oft sie und nicht irgendwelche Beobachtungen, ob Gesetze oder gar ganze Theorien akzeptiert oder verworfen werden: Empirische Gesetze, die etwa nicht den Erhaltungsgesetzen genügen, werden von den Wissenschaftlern im Normalfall nicht einmal diskutiert, geschweige denn akzeptiert.

In den meisten empirischen Wissenschaften setzen Messtheorien frühere Formen der Theorien, die sich auf Teilbereiche bewährt haben, voraus; in einigen höher entwickelten Disziplinen, in denen die Theorie die empirischen Gesetze und die diesen zugrunde liegenden Messtheorien systematisch vereint, muss zur Anwendung der Messtheorie die bereits vollzogene Anwendung eines anderen Teils der Theorie vorausgesetzt werden. Dass hier kein echter Zirkel vorliegt, darf angenommen werden, desgleichen aber auch, dass die nicht-alexandrische Lösung dieses Knotens viel Geduld und methodologische Feinarbeit erfordert und so mit nicht allzu schnell zu bewältigen sein wird. Im alltäglichen Messen allerdings, d. h. im Wahrnehmen, ist nirgendwo eine Zirkularität zu erkennen; denn die betreffenden Messverfahren sind uns hier in der Form des Funktionierens unserer Sinnesorgane angeboren. Sie sind uns, gerade weil sie uns angeboren sind, dann so selbstverständlich, dass wir uns ihrer Voraussetzungshaftigkeit erst dann bewusst werden, wenn wir mit Menschen zusammentreffen, deren Sinnesorgane von den unseren abweichen, die etwa farbblind oder zumindest rotgrünlind sind.

An den Messtheorien einer Disziplin wird selten gerüttelt; denn sie sind ja das Arsenal an Kriterien, anhand derer die Mitglieder der jeweiligen Forschergemeinschaft ihre Theorien überprüfen. Weichen daher Messapparate, die im Gegensatz zu den Sinnesorganen nicht natürliche, sondern künstliche Realisierungen von Messtheorien sind, von diesen erheblich ab, so widerlegen diese nicht die Messtheorie; vielmehr wird dann umgekehrt gesagt, sie seien wenig geeignete Realisierungen von ihnen. Das Messen der Länge eines Objekts nach Augenmaß ist im Normalfall keine so gute Realisierung der Messtheorie der Längenmessung wie das Messen mit dem Pariser Urmeter oder mit geeigneten Kopien von ihm; und auch diese künstlich geschaffenen Instrumente sind in manchen Situationen des Messens immer noch als wenig gute Realisierungen der Messtheorie anzusehen, insbesondere dann, wenn sie die messtheoretische Bedingung der Symmetrie oder auch der Transitivität der Gleichheitsbestimmung von Messgrößen verletzen.

Eine Änderung der Messtheorie erschüttert eine Disziplin tiefer als die Änderung von empirischen Gesetzen: Eine solche Änderung verändert ja die Prüfungskriterien für einzelne Messungen und damit letztlich auch für empirische Gesetze. Sie wird deshalb von der Forschergemeinschaft nur zögernd vollzogen, und jedenfalls nur dann, wenn die Vor-
teile einer dann schon detailliert entwickelten neuen Messtheorie nicht mehr zu übersehen sind.

Messtheorien können gelegentlich auch die Gesetze anderer Disziplinen voraussetzen. Will man etwa Galilei-Experimente durchführen, so kann man die Länge des Weges von rollenden Kugeln mit einer Kamera bestimmen, die nach den Gesetzen der Optik gebaut ist. Mechanisch aufwendiger, wenngleich theoretisch weniger aufwendig ist gemäß Abb. 1 und 2 das folgende Messverfahren:

Die Sperre wird zur Zeit $t_0 = 0$ hochgezogen; und alsbald beginnen die Eisenkugel – oder vielleicht eine in die dritte Dimension reichende Reihe von Eisenkugeln – zu rollen. Nach $t$ Sekunden öffnen sich automatisch alle Platten, die die schiefe Ebene ausmachen:

Die Kugel fällt in eine der Spalten; bzw., falls es sich um eine größere Serie von Kugeln handelt: Jede Kugel aus dieser Reihe fällt in einen (aber nicht notwendigerweise in ein und denselben) Spalt.

Wir sehen an diesem Beispiel, dass ein Messinstrument als ein Makroobjekt des physischen Universums nie ideal sein kann: Nie wird es an-gaben, dass der Messwert eine bestimmte reelle Zahl ist, sondern vielmehr stets registrieren, dass dieser in einem bestimmten, im Prinzip durch ein Paar von rationalen Zahlen zu determinierendem, Intervall liegt; die empirischen Gesetze hingegen liefern in aller Regel durch reelle Zahlen markierte Messwerte. Eine solche reelle Zahl kann nun als eine unendliche Folge von rationalen Zahlen dargestellt werden; versteht man sie als Messzahl, so heißt dies: als eine unendliche Folge von immer genauer, praktisch wie auch aus theoretischen Gründen stoßen wir an endliche Grenzen dieser Verkleinerbarkeit der Messintervalle.

Wenn dennoch die reellen Zahlen und somit die Konzeption der beliebig scharfen Messbarkeit in den die Begriffe der Mathematik benützenden empirischen Gesetzen verwendet werden, so hat diese – empirisch falsche, daher nur als Idealisierung zu verstehende – Voraussetzung meist gute praktische Gründe: Auch hier sagt uns der Wissenschaftler, der dabei gemachte Fehler in seinen Auswirkungen sei bescheiden, viel kleiner als der durch mangelnde Abschirmung des Experiments entstandene Fehler, und die praktischen Vorzüge der Analysis, insbesondere die Einfachheit ihrer Handhabung, würden dieses Manko der Ungenauigkeit mehr als aufwiegen. Und die Erfahrung gibt ihnen in der Regel recht, wohl deshalb, weil sie darauf achten, dass sich solche Fehler im Verlauf der Berechnungen nicht kumulieren und dann la-winenartige Konsequenzen nach sich ziehen.

Das genannte Galilei-Experiment sei an einer einzigen Kugel durchgeführt worden, oder zwar an einer größeren Serie von Kugeln, wobei wir uns aber für den Moment mit einer – willkürlich aus dieser Serie ausgewählten – befassen. Diese Kugel sei zur Zeit $t$ durch mein Auge an einer bestimmten Wegstelle wahrgenommen worden. Wenn man davon ausgehen kann, dass in diesem Moment nichts meine Wahrnehmungsfähigkeit getrübt hat, so wird man aus meiner Wahrnehmung induktiv erschließen, dass jeder andere geeignete Mensch sie ebenfalls an jener Stelle wahrgenommen hätte, dass sie also an dieser Stelle zu beobachten gewesen ist. Ein Beobachtungssatz oder ein Messwert enthält somit viel mehr an nicht beobachteten Ele-
menten als der Wahrnehmungssatz oder das Messergebnis; denn aus ihm lassen sich Urteile über Menschen deduktiv ableiten, die an diesem Wahrnehmungsvorgang gar nicht beteiligt gewesen sind.

Ist die besagte Kugel an jener Stelle in den dortigen Schacht gefallen, so werden wir nicht zögern, diese Messung sofort als eine gesicherte Messung und daher das Messergebnis den Messwert anzu- zuschreiben, d. h. zu behaupten, jede andere analog konstruierte schiefe Ebene mit den analogen Klappen und Spalten hätte das gleiche Ergebnis er- bracht. Und für dieses Experiment gilt dies wohl auch, weil man bei ihm die gesamte experimentelle Situation leicht so abschirmen kann, dass störende Faktoren wie Magnetismus nicht nennenswert wirksam werden, dass von der schiefen Ebene kei- ne Kugeln unbemerkt entfernt oder angesetzt wer- den können, usw. Im Bereich der Mikrophysik hat man mit der Frage der Abschirmung gelegentlich größere Schwierigkeiten: Ist die Voraussetzung der Abgeschirmtheit dann nur in grobem Umfang er- folgt, so muss der Wissenschaftler damit rechnen, dass die Fehlerquelle hier umfangreicher ist und dass sich in der Auswertung von Messdaten solche Fehler schneller kumulieren können.

Angenommen, das Galilei-Experiment mit der einen Kugel sei $k$ mal wiederholt worden, bzw., um es einfacher zu formulieren: mit dem Hochziehen jener Sperre seien $k$ Kugeln (mit $k \geq 1$) zum Rollen gebracht worden. Nach $t$ Sekunden seien die Schächte geöffnet worden. Wie zu erwarten ist, sei die Mehrzahl der Kugeln in einen bestimmten dieser Schächte gefallen, und die anderen in die Schächte um diesen herum. Will der Statistiker mit seinen Methoden daraus Schlüsse ziehen, so muss er vom Experimentator mindestens zweierlei erfahren:

1. ob Störfaktoren wie etwa magnetische Ein- flüsse oder Luftbewegungen oder Unebenhei- ten der schiefen Ebene ins Gewicht fallen, so dass systematische Verzerrungen der eigent- lich zu erwartenden Resultate möglich sind, oder ob dies nicht der Fall ist und die Kugeln dann für beliebige andere Serien von solchen Experimenten repräsentativ sind; und

2. ob die Kugeln gleichartig (oder gleichförmig) konstruiert sind, so dass auch von dieser Sei- te aus eine Streuung der Ergebnisse zu erwarten ist, oder ob wegen der gleichartigen Konstruktion mit einem uniformen Ergebnis zu rechnen ist (sei es der $k$ unabhängig wie- derholten Einzelexperimente mit einer Kugel oder des einmaligen Experiments mit einer Gesamtheit von $k$ Kugeln von dieser Art).

Hat er die jeweils positive Information erhalten, so kann er daraus – und auch hier mit Idealisierungs- annahmen – den Erwartungswert der Weglänge und die Streuung um diesen Mittelwert errechnen. Nimmt man als Irrtumswahrscheinlichkeit einen Wert von Ausnahmen, die man als Risiko zu tragen bereit ist, etwa 0,05, so bedeckt das Konfi- denzintervall die Gesamtheit jener – anders als in Abb. 3 dargestellt, vielmehr hinreichend schmal zu wählenden und mit Kugeln bis zum Querstrich aufgefüllten – Messintervalle um den Erwartungs- wert (d. h. um den Mittelwert), deren Wahrscheinlichkeiten insgesamt 0,95 sind; der Fehlerbalken ist dabei die halbe Länge des Konfidenzintervalls um den Mittelwert. Im Hinblick auf die gemessenen Werte ist wegen der bekannten und mit der statistischen Induktion verträglichen Umstände der experimentellen Situation somit im Grad 0,95 wahr- scheinlich, dass künftige Messungen dieser Art ih- re Werte in diesem Konfidenzintervall haben wer- den. Das Konfidenzintervall zeigt somit an, in welchem Bereich künftige Messungen nahezu mit Si- cherheit erwartet werden können.

Ein solches Konfidenzintervall ist eine Erfah- rung, d. h. eine für prognostische Zwecke statis- tisch verarbeitete Bewertung von gemachten Be- obachtungen; sie ist eine statistische Erfahrung, wenn man in diesem Sinn mehrere Messwerte in Erwägung ziehen muss, um jenen Grad von 0,95 an Sicherheit zu erhalten, und sie ist eine strikte Erfahrung, wenn nur ein einziges Messintervall in jenem Konfidenzintervall liegt.

Man ist nun im Alltag wie in den Wissenschaf- ten bestrebt, nicht nur in einer Hinsicht Erfah- rungen zu machen. Man wird also im gegebenen Galilei-Experiment die Kugeln nicht nur $t = 1$ Sekunden rollen lassen, um daraus auf statisti- sche Weise Erfahrungen abzuleiten, sondern da- nach $t = 2$ Sekunden lang, sodann $t = 3$ Sekunden lang, usw., gemäß Abb. 4, 5 und 6.

Trägt man den Zeitfaktor auf die waagerechte Achse auf und den Weg samt der Konfidenzintervalle auf die senkrechte, so erhält man folgende Darstellungen der Abb. 7a und 8a.

Die durch die mathematischen Erfahrungen durchgezogenen Linien markieren die empirischen Gesetze, im vorgegebenen Fall Galilei-Gesetze. Man sieht an den Zeichnungen sofort, dass eine noch so große Anzahl von Erfahrungen dieser Art mit unendlich vielen, in mathematischer Sprache
formulierten, empirischen Gesetzen dieser Art verträchtlich sind. Der Wissenschaftler wird versuchen, eine von ihnen daraus auszuwählen, und er wird sich dabei von mathematischen Einfachheitsüberlegungen und nicht mehr von Wahrscheinlichkeitsbetrachtungen leiten lassen.

Selten wird es der Wissenschaftler allerdings so schön haben wie in diesem konstruierten Fall. Dann und wann wird nämlich eine Erfahrung nicht vom Gesetz getroffen werden, wie mit \( t = 2,5 \) in den Abb. 7b und 8b.

Er wird, insbesondere wenn solche Ausnahmen von statistischen Verarbeitungen systematisch erzielter Beobachtungen durch eine einfache Gesetzmäßigkeit nicht erkannt werden, sondern nachträglich den Umständen der gemachten Beobachtung eine ge ringe Vertrauen schenken, d. h. er wird nicht mit Hilfe der Erfahrungen das Gesetz falsifizieren, sondern aufgrund des Gesetzes und der übrigen Erfahrungen jenen Ausreißer ignorieren.

Er wird dies allerdings gelegentlich auch dann tun, wenn solche Ausreißer überhand nehmen, insbesondere dann, wenn das Gesetz hinreichend mit anderen Gesetzen übereinstimmt, die er als dem vorgegebenen Fall analog betrachtet. Das folgende – nunmehr nicht konstruierte – Beispiel der Abb. 9 möge dies verdeutlichen.

Die kleinen senkrechten Striche markieren hier nicht die Konfidenzintervalle, sondern deren halbe Längen, d. h. die Fehlerbalken. Das rein empirisch gefundene Gesetz müsste dann eigentlich die durch die Mittel- oder Erwartungswerte gezogene – gestufte Linie markieren; aber ein solches Gesetz akzeptiert kein Erfahrungswissenschaftler. Er nimmt hierfür vielmehr jeweils die durchgezogene Linie, die er sich aus verwandten Gesetzen und Analogieüberlegungen erschlossen hat. Dass dabei diese durchgezogene Linie oft den Fehler- balken – und manchmal auch dessen Verdopplung – verpasst, dass sie also durch diese abweichenden Messwerte eigentlich falsifiziert ist, stört keinen Naturwissenschaftler; denn sonst müsste er ja alle – oder zumindest fast alle – seine Gesetze als falsifiziert erachten. Im „Uni-Report“ (Frankfurt am Main), steht als Kurzmitteilung:

„Mit Hilfe des UNILAC-Beschleunigers sind Uranionen auf Palladium-, Blei- und Uranatome (v. o. n. u.) geschossen worden. Die dabei entstehenden Positronen sind bei festgelegter Stoßenergie und in einem bestimmten Winkelbereich von Backe et al. gemessen worden (gestufte Linien mit Fehlerbalken). Die durchgezo-
genen Kurven zeigen die Berechnungen von Dr. J. Reinhardt in sehr guter Übereinstimmung mit dem Experiment.”

Und in der Tat ist mir von verschiedenen Physikern bestätigt worden, dass sie froh wären, wenn sie immer so gute Übereinstimmungen zwischen den statistisch ermittelten Erfahrungen und den metrischen Gesetzen hätten.

Die Messungen sind in dieser Abbildung überhaupt nicht mehr aufgezeichnet: Sie stehen nicht in Korrelation zum Gesetz; und selbst die Beobachtungen (als die gesicherten Messungen) sind hier nicht verzeichnet (wollte man sie einzeichnen, so müsste man sie als Punkte darstellen). Einzig die Erfahrungen werden mit dem Gesetz verglichen. Und weil man dieses Gesetz mit anderen Gesetzen mathematisch gut in Übereinstimmung bringen kann, akzeptiert man das Gesetz und erklärt, warum die Erfahrungen verzerrt sind, oder besser: Man gibt Hinweise, warum diese dem Gesetz widersprechenden Erfahrungen zu Ausreißern deklariert werden dürfen, so dass man sie vorerst nicht zu beachten braucht:

(1) weil einzelne der Messungen, entgegen der Annahme, keine Beobachtungen sind, so dass sie der Statistik nicht hätten zugrunde gelegt werden dürfen und diese dann auch in ihrem Ergebnis nicht verzerrt hätten (der Wissenschaftler nimmt die betreffenden Messdaten somit von seinem Schreibtisch; er sollte sie aber in die Schublade stecken und nicht in den Papierkorb werfen); oder

(2) weil Störfaktoren im Spiel waren, weil vielleicht nicht-diagnostizierte lokale Magnetfelder gewirkt haben oder weil Luftwirbel nicht ermittelt und somit nicht ausgeschaltet worden sind, oder weil sonst irgendwie störend in die Situation eingegriffen worden ist (er muss dann aber, wenn er an diese Hypothese glaubt, die Serie von Experimenten wiederholen); oder

(3) weil die Kugeln nicht gleichförmig konstruiert und somit eine größere Streuung vorzusetzen gewesen ist: das Konfidenzintervall hätte somit, unter dieser Voraussetzung, größer ausfallen müssen, oder man hätte eine größere Serie von Kugeln als die der vorgegebenen Anzahl $k$ wählen müssen (man ergänzt dann entsprechend das Experiment, oder man zeichnet der Einfachheit halber ein größeres Konfidenzintervall ein, das dann die Linie überdeckt).

Man nimmt also die Voraussetzungen des statistischen Schließens teilweise zurück, mit deren Hilfe man unter Rückgriff auf Einfachheitsüberlegungen die Gesetze ermittelt hat. Dies hat jedoch mit Fingerspitzengefühl zu geschehen, das sich möglicherweise nicht in Regeln fassen lässt und das andererseits auch nicht in eine Immunisierungsstrategie münden darf; denn sonst stellt sich der betreffende Wissenschaftler ins Abseits. Er muss also zuvor bereit sein, sein metrisches Gesetz zu verändern, auch wenn dies eine Stütze seiner Theorie gewesen ist.