

# CONTROVERSY

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## On the Redskins of Scientism and the Aesthetes in the Circled Wagons\*

I have been asked to set out my response to Frank Kelleter's criticism of what he calls neo-naturalist tendencies in the study of literature;<sup>1</sup> it is a pleasure to do so. Rarely is such criticism expressed in an explicit, clearly argued manner. Kelleter makes clear that he is not resorting to the popular catchphrase of reductionism (»for any academic way of knowing a literary text is in this sense ›reductive«, 163). Similarly, the get-out clause that everything is a cultural construct anyway does not, at least in its crude form, intrude into his argument. In actual fact, I share Kelleter's views regarding much of what he criticizes. It is therefore all the more disappointing that his criticism is almost entirely destructive in nature and fails to give neo-naturalism a chance. As a result, for all their stylistic elegance, his »worried reflections« have the overall character of a defamatory tract.

Accordingly, Kelleter's piece contains features characteristic of this text type; I can do no more than consider them in an illustrative fashion here. He assesses the achievements of neo-naturalism on the basis of various bombastic pronouncements it has produced, so that the »proof of the pudding« cannot but end up being meagre; and the way in which he selects his references to the »pudding« and comments on them is deliberately accusatory in nature. Fair enough. I do, however, have certain difficulties with the fact that Kelleter combines so many things in his understanding of neo-naturalism. In a single breath, it is described as containing not just approaches based on neuroscience and Darwinism, but also the empirical study of literature (German *Empirische Literaturwissenschaft*) and the cognitive sciences; the reference to Edward O. Wilson and Steven Weinberg rounds off the mixture with two visionaries in the tradition of Haeckel. Suddenly, I find myself among the people of a close-knit ideological tribe. Kelleter's strategy of creating a common evil in this way is a familiar one in the humanities: the wagons are circled and everything »out there« dashing over the prairie or even just sitting

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\* Deutsche Fassung unter <http://www.jltonline.de>

<sup>1</sup> See Frank Kelleter, A Tale of Two Natures: Worried Reflections on the Study of Literature and Culture in an Age of Neuroscience and Neo-Darwinism, *JLT* 1: 1 (2007), 151–187 of this volume and <http://www.jltonline.de/>.

by the campfire (or in ministerial offices) belongs to the savage redskins of the ›third culture. There is no point here in distinguishing between Apaches, Oglalas, and Winnebago: if you shoot, you're bound to hit someone or other (the relentless ›neo-‹ makes them all look like zombies anyway). On the other hand, the programmes of enquiry tied together here do indeed touch on and complement one another in various ways, and as I myself have taken the liberty of sweepingly describing the people behind the circled wagons as neo-idealists at times, I will take all this sportingly and do my best to come to terms with it. Moreover, I do believe that all fields of enquiry (apart from theology) should take as their guide the heuristic hypothesis that everything in this world is the result of natural causes, and to this extent I probably am a naturalist after all.

Finally, with a view to what I have to say below, it should be noted that my knowledge of the fields Kelleter covers in such wide-ranging fashion is insufficient for me to be able to adopt a secure standpoint regarding each and every one of them. Besides, I cannot stand up for a particular position just because I have been lumped together with it. Even the approach that I favour most and about which I feel able to speak with some competence has two variants, of which I think one to be more promising than the other. The points I select for consideration will therefore be determined not only by their relevance but also by the limits of my knowledge and by my own particular preferences.

## 1.

I begin with *Empirische Literaturwissenschaft*. It has now been in existence for three decades and has (at least) two forms: the radically constructivist variant represented by Siegfried J. Schmidt, and the more specifically empirical variant associated with the name of Norbert Groeben. It seems likely that Kelleter has the second in mind, namely a way of studying literature that draws on the methodological tools of empirical psychology and the empirical social sciences, as well as sharing their concept of scientific endeavour in a more general sense. Ideally, empirical procedures here have the character of *experimenta crucis* for proposed theories that have been set out beforehand; as in the parent disciplines, of course, everything depends on whether such theories are sufficiently refined and how appropriately they are operationalized. There are shortcomings here at times; but then again, one also hears of hermeneutists who speak platitudes – except that when they do, it is less obvious, because they don't need to be as clear as the empiricists. More fundamentally significant is the inclination of many empiricists to confine empiricism to that which can be captured in statistical form. Here, I can accept Kelleter's reservations, for there have been and are empirical approaches in philology and the study of history that perform perfectly well without having to make recourse to statistical procedures.

The question of the relationship between the general and the particular would seem to be somewhat more fundamental. Here, too, I can agree with Kelleter in opposing scholarly activity whose attention is directed only at the general, as he finds to be the case in the work of supporters of *Empirische Literaturwissenschaft*. Such restrictiveness would mean that not just all disciplines concerned with history, but also geography, biology, and astronomy would have to be excluded from the field of scholarly enquiry. This, however, is a false choice, for the terms ›general‹ and ›particular‹ are gradable rather than complementary antonyms (they resemble ›warm‹ and ›cold‹ rather than ›dead‹ and ›alive‹). Even anthropological universals are, seen from a different angle, particular in nature, namely specific to human beings, and as such demand explanation. Conversely, when we, as historians, explain the particular, we always draw it into the scope of more general assumptions; if we did not, we would be unable to employ any concepts at all and would be dealing with nothing but miracles. Even someone who is interested only in individual interpretations (and does not want to confine himself to simple adoration) is continually using hypotheses of a more general nature and should, instead of merely assuming silently that they are valid, make them explicit and test them as precisely as possible.

Kelleter seems to have a generally dim view of such tests; otherwise, he would not sweepingly place neo-naturalist attempts at explanation alongside psychoanalysis, poststructuralism, and Marxism – in other words, alongside supertheories that have either made themselves immune to refutations or have been refuted. »It is no argument against these comparisons that Darwinism is a scientific theory, while psychoanalysis and Marxism are only superstitions, because this is what competing universals always claim about each other« (185). What is the reason for the theoretical relativism we see here (it could easily be extended to cover creationism, the mythology of the Maya, and the like)? Well, the »master narratives« (171) enumerated do have something in common: they can all be used to explain singular states of affairs – and every explanation serves by its very nature to confirm from within the theory used to supply it, even if that theory is entirely wrong. Those inside the circled wagons, though, constantly overlook or underestimate a significant difference which distinguishes scientific theories from other constructs, namely the fact that they are not simply employed to provide explanations but that there are entire university faculties busy with probing, refuting, and modifying them – rather successfully as a matter of fact. It is the principle of critical testing that makes the empirical sciences stand out. There is a piece of work from the circle around Willie van Peer (whom Kelleter singles out for particular attention) that shows how testing even relatively simple hypotheses (prejudices) might be profitable. The study in question (Tsiknaki 2005) is a dissertation that uses statistical methods to analyse the link between emotional intelligence and the reading of literature. The prediction, of course, was that there would be a positive correlation, for readers are, as we all know, better

people. It turns out, though, that there is no such correlation but instead a small correlation of the opposite kind, albeit not a significant one. Another finding of the study, however, is clear and strictly contradicts our intuitions: natural scientists have a clearly higher level of emotional intelligence than scholars in the humanities! It would perhaps be sensible to take note of such findings, if only to refute them properly. A disdainful consensus on its own does not disprove anything. When I once heard Willie van Peer set out the results just described within the circled wagons (during a meeting where you can't just walk away), all that happened was that a mood of distinct irritation made itself felt ...

I turn now to the approaches of *cognitive science*. These redskins, of course, are such a varied bunch that only their opponents find themselves in a position to provide a sweeping appraisal of them. The spectrum ranges from apriorists, who tend to subscribe to analytic philosophy, to empiricists, who tend to work on an evolutionary basis. Personally, I am of the belief that cognitive science will be half-blind if it lacks evolutionary elements. It will always run the risk of becoming a home workshop for self-satisfied model engineers; evolutionary perspectives should be added to give it an empirical grounding and to obtain a second fix for a »cross bearing« (Popper 1972, 43), that is, a second form of observation independent from the first. To recall a distinction made long ago by Christian Wolff: the »nuda notitia facti« should be joined by the »perspicere rationem facti« that can authenticate or correct the »notitia facti.«<sup>2</sup> I have more to say on this in section 2 below.

Here, I restrict myself to some conclusions that might be drawn regarding Kelleter's position. He writes that »a déjà vu can hardly be avoided« when he reads about »foregrounding« and »deviation« (147). And? Why does he want to avoid it? For me at least, it is always a sign of quality when we encounter a new approach – again understood as a »cross bearing« – if it includes familiar tried and trusted elements.<sup>3</sup> Besides, no cognitivist student of literature who wants to be taken seriously will deny that his forerunners include Viktor Shklovsky and Roman Jakobson and Jan Mukařovský. (Though he or she would not, as a rule, want to adulterate them with Heidegger or Derrida.) Kelleter says that »there is little accomplished by cognitive poetics that could not be accomplished with more traditional formalist or narratological tools as well« (156). What »little« there is might perhaps be interesting, of course. But Kelleter's view of the innovative bits is such that they are sweepingly dismissed as a »face-lift« (ibid.), which suggests he is simply not interested in the possible increase in knowledge they could provide. He has every right to hold such views, but here as elsewhere he

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<sup>2</sup> See Wolff 1740, 3.

<sup>3</sup> Put less subjectively: according to Lakatos 1970, a given line of research is a progressive problemshift if it provides further effective explanations *in addition to* the effective explanations already available.

crosses the line between the subjective ›I'm not interested in that‹ and the objective ›that isn't interesting‹.

The same can be said of the way in which Kelleter treats the programme of what Peter Stockwell has called the ›study of literary reading‹ (Stockwell 2002, 165). Kelleter believes this project of reading research to be ›feasible‹ though. ›But how interesting – how relevant – are its results? And for whom?‹ (165). For me they are; for Kelleter, they apparently are not. Why does Kelleter express his disinterest in such an aggressive manner? He asks: ›What do we study when we study ›ordinary‹ readings? Should we, as *students* of literature, aspire to become ordinary readers (again?) – or on the contrary seek to educate ourselves and other ordinary readers to have a better informed understanding of literature, to become more competent readers? Is there something wrong with the fact that academic readings (i.e. readings sensitive to textual structures and historical contexts) differ from the readings of ›the majority?‹ Would we want to make the same democratizing claims for our knowledge of history, economics, nuclear physics?‹ (165; emphasis in original). What a confusing combination of ›is‹ and ›ought!‹ Does Kelleter really believe that the individuality of his stance as a reader will be threatened if attention is given to the reading stances of the rabble? Who on earth is demanding that he reads like any old Tom, Dick, and Harry? (And how does he intend to help Tom, Dick, and Harry become ›more competent readers‹ without knowing how they actually read now? At this point, indeed, I can't help asking: Does he even know how he himself reads?) Kelleter has both feet planted firmly within the circled wagons, standing in the hermeneutic tradition of a ›fusion of horizons‹ (›Horizontverschmelzung‹) in which the distinction between subject and object is levelled – and as far as I'm concerned he can continue to do so, as long as he doesn't interfere with those who want to investigate how the process of reading takes place in reality.

The fact is, however, that Kelleter does interfere, at least when he provides his readers with a misleading picture of positions about which they are unlikely to have any substantial prior knowledge of their own. It may be of some interest to consider a specific example of how he does this and how inventing a homogeneous neo-naturalist programme serves his purposes in the process. Kelleter complains in his essay that evolutionary aesthetics is unable to cope with non-beautiful works of art (by the way: this is an error). He then introduces Peter Stockwell (a cognitivist who, unfortunately, is not concerned with evolutionary aesthetics at all), remarking: ›The first example that comes to his [Stockwell's] mind when he thinks about a book in which readers have ›to engage with ideas that are not naturally their own‹ (2002, 153) is Hitler's *Mein Kampf*. The term ›naturally‹ is of course deceptive here, because fascist ideology in the 1920s and 1930s was anything but unnatural. Nor was it natural. It was – and still is – cultural and historical‹ (172). What has this got to do with Stockwell's text? The latter, explicating the term ›transportation‹ (the reader's being carried-away), ac-

tually says that »even in extreme cases (such as reading Hitler's *Mein Kampf*), part of the sense of distaste and revulsion for most people comes from the sense of having to engage with ideas that are not naturally their own and feeling too close to them for comfort«. The issue at stake, therefore, is the role of transportation even in texts that we find repulsive; the discomfort experienced when reading Hitler is cited as an extreme example to demonstrate that we do indeed feel this force of transportation. Not an unreasonable idea, I would have thought. Kelleter, however, brutally tears it apart, extracting the word »naturally«, ambiguous without a context, which he »interprets« as though it stood in the context of a neo-Darwinist discussion. As a matter of fact, five lines later, Stockwell himself provides an explicit and perfectly straightforward explication of what he means by »nature« when he writes about »the nature of the text (its architecture of formal patterns and genre characteristics)«. Kelleter himself caused the confusion he laments here – by disregarding the hermeneutic principle of charity.

## 2.

In this section, I turn to work based on *evolutionary theory*. It would seem to be of particular importance not only in and of itself, but also as a complement to other neo-naturalist approaches. It can provide a context for the microscopic investigations of neurophysiology by relating them to functional behavioural modules. For approaches that are empirical in the narrow sense of the word and are restricted to operate with current populations as the only ones available it can serve as a guideline for extrapolating their results to cover other times and cultures. Above all, though, it can, as I suggested above, provide the approaches of cognitive science with an additional, empirically anthropological dimension. Evolutionary Psychology in particular presents cognitive science with the chance to collaborate or even merge with the activities of evolutionary theory. From this perspective, Kelleter would not be entirely wrong in placing cognitive science and neo-Darwinism together in the same class, but unfortunately he doesn't even consider Evolutionary Psychology in the first place – but I am getting ahead of myself here.

Kelleter writes: »What we study are indeed the »many different *cultural* manners« in which humans have made use of their biological dispositions through *history* – and not just evolution« (172; emphasis in original). Oh the perpetual either/or mentality! How are we meant to study the *use* of biological dispositions without studying the dispositions themselves? And how, in turn, are we meant to study biological dispositions without understanding them as a product of evolution? That is all I argue. Accordingly, I am prepared to agree with Kelleter when he says his »point is that a *purely* empirical or naturalist approach to literary works or other cultural artifacts constitutes an inappropriate method« (168; my empha-

sis), at least in the narrow sense in which Kelleter understands »empirical« and »naturalist«. Here, of course, we are faced with one of several enduring misconceptions: the defenders in the circled wagons repeatedly insinuate that the redskins want to explain *everything* with their categories. If informed that the redskins actually just hope to explain *some* things, they turn away in disappointment; they are, after all, used to dealing with totality and nothing less. Thus, Kelleter himself believes he has caught the naturalists capitulating in some way when he finds them remarking that their work should be complemented by perspectives provided by the social sciences. They arrive, Kelleter writes, at »a point where they recognize that in order to do literary analysis they need to confront questions of social and cultural construction in their historical specificity, and *not just* physiological or pragmalinguistic verities« (180, my emphasis). Of course *both* are necessary! From a solely biological perspective, literature does not exist at all (assuming of course that the meaning of the word »biology« has not been extended – in a manner typical in the humanities – to produce the position that »everything is biology«). Nonetheless, there are biological dispositions that make possible the historical fact that literature exists and exists the way it does, and there are biologically grounded functions that can be performed by the historical phenomenon of literature. This should provide a good starting point for further explorations.

In his criticism, Kelleter refers above all to the edited volume *The Literary Animal*, published in 2005 – most recent stuff indeed. I must confess that I am not entirely happy with the volume; it contains a certain amount of material that would have benefited from further reflection, and thus makes life easy for Kelleter but hard for me.<sup>4</sup> Significantly, the foreword is written by Edward O. Wilson, the great champion of sociobiology in the 1970s. Sociobiology was indeed, for a time, the main paradigm for biological approaches to illuminating human behaviour. It was/is concerned primarily with the evolutionary causes of cooperation, and seeks out the relevant analogies or homologies between animals and humans. As Kelleter rightly observes, readers who subscribe to sociobiology tend to comb the works of world literature for content that confirms the insights of sociobiology, just as works used to be combed for points of contact with psychoanalysis or Marxism. Kelleter's quote from a piece by David Sloan Wilson is not atypical in this respect: »if we ask what themes would most interest a non-human primate, those are the themes that are most prominently featured in Shakespeare and indeed all literature«. This is unfortunately phrased, to say the least, and criticism of Wilson's claim might be expected to take it as a springboard for the pursuit of superior insights. Kelleter, however, freely declares that the statement »is probably true« (163), for it is of no interest whatsoever to him: »but what have we understood about Shakespeare, what about Elizabethan culture,

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<sup>4</sup> For more details, see the review that I co-authored (Eibl/Mellmann 2007).

when we see this?» Yet Wilson's aim is not to explain *Shakespeare*, but to explain the *universal success* of Shakespeare's plays; arguing against radical cultural relativism, he traces their popularity back to the fact that they address not only culture-specific but also universal dispositions. In the context of such an argument, Kelleter's complaint that he learns nothing about Elizabethan culture is really somewhat bizarre.

In any case, Kelleter would have been well advised to return to the sources in the manner of good old-fashioned philology and consult the original piece by the psychologist Daniel Nettle to which Wilson, a biologist, is referring when he makes the remark discussed above. Nettle is seeking to define the genre of drama in sociobiological terms as »supernormal conversation« (in the sense of the »supernormal« stimuli of ethology) – sure enough not the most far-fetched of all the many attempts that have been made to define the drama. Had he realized this, Kelleter would have been confronted even more clearly with the fact that his question is inappropriate. Nettle explicitly and repeatedly explains that he sees his work as a contribution to genre theory, and that it should be complemented by the historicist approach: »It is not an alternative to historicist studies; rather it is a set of general principles and parameters within which historicist work should be nested« (Nettle 2005, 61).

Enough. I shall not pursue this micrological criticism of Kelleter's argumentation any further. I find it more important to point out the fact that sociobiology is indeed not an entirely appropriate paradigm when it comes to matters of literature. Evolutionary Psychology, which emerged out of sociobiology in the 1990s, seems likely to be more profitable than sociobiology for the study of literature. (I can't hold back from saying that Kelleter could have learnt this from my book [Eibl 2004], if not before.) The attention of Evolutionary Psychology is directed more strongly at the biological foundations of *species-specific* human behaviour, and the species-specific psychological apparatus behind it. Then what becomes significant are, for example, the human-specific uses of emotions and the realm of cognitive dispositions; likewise the difference between the environmental circumstances that gave rise to our adaptations and the worlds they have to cope with now;<sup>5</sup> and the whole nexus of language (as a mode of representation) and its role in constituting the world. There is much still to be done here.

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<sup>5</sup> The following assertion is indicative of Kelleter's view: »Eibl [...] concedes that human dispositions developed in the Pleistocene partly stand in completely different contexts today and have a completely different function« (Eibl 2004, 327). Pronouncements like these typically occur in the final chapters or pages of neo-naturalist books and articles« (178). There is nothing to »concede« here; instead, this is one of the basic tenets of Evolutionary Psychology; it was established in the early 1990s by, among others, Tooby/Cosmides (1990) and Symons (1992) in opposition to the human biology found in offshoots of sociobiology at that time. It therefore stands not at the end but at the beginning of work on Evolutionary Psychology; in my book it appears for the first



The tapestry of material and ideas involved is rich indeed and I cannot, of course, unfold it in its entirety again here.<sup>6</sup> Instead, I wish to highlight just two points that, in my view, have been neglected or even ignored not only by those who criticize the biological position but also by some of those who support it. Specifically, I have in mind the distinction between the functional mode and the organizational mode, and the phenomenon of decoupling. Both are discussed in a number of essays by Leda Cosmides and John Tooby, the most important theoretical figures in Evolutionary Psychology (they are not mentioned by Kelleter).<sup>7</sup>

Observers (and critics) of the biological approach repeatedly and correctly stress the fact that individual ontogenetic development is of truly special importance for human beings. The multiplicity and heterogeneity of the adaptations evolution has given us is such that we are able to act successfully in hugely different environments, but it also leads to huge problems of internal coordination. Even in animals, the use of adaptations in an exercising mode of employment disconnected from the drive for success serves, so to speak, to exercise and round off those adaptations and with them the organism itself. This has, of course, long been known by the name of play. Cosmides and Tooby speak of an employment of adaptations in the organizational mode. In the case of humans, the need of our cognitive abilities for such training is particularly prominent. Their interaction apparently is so precarious that suitable exercises are required to calibrate and maintain them, even as we grow older. True, such activities in the organizational mode do not have an intended purpose. But they are perfectly useful nonetheless: their use is what biologists call an ultimate cause, and this ultimate cause was the crucial factor in evolutionary selection. As a rule, though, the people involved know nothing of this. Their (main) motivation is the fact that what they are doing is simply ›fun‹, or, in more sophisticated terms, the fact that it is intrinsically satisfying. This is one of those cases in which we should be grateful to neurophysiologists for providing us with specialist knowledge: they have shown that this kind of intrinsic reward is grounded in endocrine activity. And there lies the basis of aesthetic pleasure. Aesthetic enjoyment without

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time on page 62, and then on several occasions after that. And then, when it appears on page 327, as cited by Kelleter, it is accompanied by the somewhat apologetic confession »it cannot be repeated often enough« (»man kann es nicht oft genug wiederholen«). This is quite obviously the case – enough so, I think, to give some ground for exasperation. – See also online publication [1].

<sup>6</sup> See Eibl 2004. I have also written a short introduction 2007.

<sup>7</sup> There is only one essay in which they deal with literature (Tooby/Cosmides 2001), but this one essay was considered important enough to merit translation into German (as Tooby/Cosmides 2006). In addition, I recommend reading Tooby/Cosmides 2000 for a rather more detailed discussion of some fundamental aspects of decoupling, and Cosmides/Tooby 2002 for a convincing attempt to link the concept of decoupling to the disputed question of modularity. For a brief introduction, see online publication [2].

(intended) purpose, ›disinterested pleasure‹ in Kant's term (›interesseloses Wohlgefallen‹), is therefore not an apparition of the philosophers: it can be shown to be a product of biological evolution.

The possibility of decoupling (facilitated by the descriptive and argumentative functions of language) is closely related to this; it is probably the crucial species-specific ability of *Homo sapiens* and the underlying capability behind everything we know as culture.<sup>8</sup> In order to avoid misplaced idealistic expectations, it should be noted that there are, of course, hints of this even in animals, and that the possibility of decoupling in no way means that the power of reflection opens the way to freedom and emancipation from the demands of nature. It just makes possible a far more flexible use of our adaptations, which itself requires cultural regulation.

Then what does decoupling mean? The central and distinctive feature of human evolution identified by Cosmides and Tooby is a dramatic increase in the use of contingently applicable information in any particular situation. They refer to this with the term ›cognitive niche‹ (Tooby/DeVore 1987). This cognitive niche is marked by the fact that information can be treated in a highly flexible manner by having meta-information, or taggings, attached to it. Play, one imagines, was the evolutionary platform on which the use of meta-information became established. A dog's or a parrot's invitation to play and the expression of a chimpanzee at play (an early form of our smile) are well-known examples of how a form of behaviour can be augmented with meta-information: ›this is a game‹ (so don't take it as a serious threat if I growl). In humans, this use of meta-information in information processing has, on the basis of the representational function of language, developed into a comprehensive apparatus for dealing with possible and actual worlds. ›These are the new worlds of the might-be-true, the true-over-there, the once-was-true, the what-others-believe-is-true, the true-only-if-I-did-that, the not-true-here, the what-they-want-me-to-believe-is-true, the will-someday-be-true, the certainly-is-not-true, the what-he-told-me, the seems-to-be-true-on-the-basis-of-these claims, and on and on‹ (Tooby/Cosmides 2001, 20). It is possible, then, for propositions to be marked in such a way that

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<sup>8</sup> The concept of decoupling has been known, as ›hiatus‹, to German anthropologists since Arnold Gehlen's major work, *Der Mensch* (first published in 1940; translated into English as *Man*, Gehlen 1988). Gehlen's anthropology enjoyed a certain amount of popularity among social scientists and scholars in the humanities because it defined humans as deficient biological beings with reduced instincts, making it safe to turn away from the biological dimension for apparently biological reasons. The trouble is that Gehlen's anti-Darwinist biology was not a very good one. More on this can be found in, for example, Promp 1990, who writes that philosophical anthropology (and with it the social sciences) have ›practically preserved the state of biological knowledge in the 1940s – and curiously attempted to bring it to bear against that of today on more than a few occasions‹ (›praktisch den biologischen Kenntnisstand der 40er Jahre konserviert – und kurioserweise nicht selten gegen den heutigen ins Feld zu führen versucht‹, 15).

they remain intact without being blindly used as action-related data. More than any other factor, the capacity of propositions to be decoupled (at times) from utilitarian purposes has allowed *Homo sapiens* to outdo all its competitors in the ability to deal with changing (even self-created) environments and thereby become *the* evolutionary success story. Here too, of course, lies the technical foundation for such interesting things as reconstructing the problem-solving behaviour of others (ranging from the theory of mind, with its enormous enhancement of our cooperative skills, to the study of history), the technical foundation for the counterfactual and the hypothetical – and the technical foundation for poetic fictions.

Together with the utilization of the organizational mode, the possibility of decoupling is the precondition for the ability of phylogenetically old cognitive and emotional structures to be used with a new function in art, specifically in literature. Authors of sociobiological interpretations refer us to the dense concentration of practical information in the narratives of tribal cultures, where narration is indeed the most eminently suitable medium for preserving such information on a lasting basis. The discovery of biologically old aspects of feeling and knowledge in more recent literary works, though, has not been exploited to the full until it is considered together with the possibility of decoupling: we see then that we are dealing not (only) with direct information but with archaic triggers of attention, on the basis of which various kinds of refinement and symbolic processing can take place. A child in danger sets alarm bells ringing inside us, whether on the stage, in a novel, or in reality. A relative in danger (and the hero of a novel is something like an adopted relative) calls on our readiness to help and fight. The unknown other fills us with trepidation (and perhaps curiosity too), the thunder of Jehovah or Jupiter fills us with dread. Violations of biologically grounded taboos such as incest, fratricide, and patricide, or infidelity and treachery, place all righteous people in a state of appropriate disgust. In each case, the emotions involved stem, directly or indirectly, from the pool of primal emotions. It is little different on the cognitive front, where there are the schemata or expectations of ›Gestalt‹ that lead us through a text – stories of departure and homecoming, of courtship and marriage, of war and victory or defeat, and so on. But, thanks to our ability to decouple, we do not react as we would to real events as these stories unfold; instead, we follow cognitions that develop out of them or intrude into them and can lead into very different contexts. The old dispositions are particularly liable to being filled or re-filled with semantic elements: the fear of natural forces can be reinterpreted as awe of the sublime, and the schema of departure and homecoming, which guides the search for food, can be filled with the search for the Holy Grail or even with an entire philosophy of history such as the departure and return of God. When this happens, the same mechanisms of emotional arousal and cognitive expectations are triggered irrespective of whether the events involved are real or fictional.

The cognitions and behaviour that develop out of them, however, can be fundamentally different.

These remarks will have to suffice here. We cannot deny Kelleter all understanding simply because he fails to be particularly convinced by the available studies in literary *history* that are based on evolutionary theory. In many cases, they still have the nature of exploratory excursions. But now, the first thorough study in literary history to reap the benefits of the findings of Evolutionary Psychology has been published (Mellmann 2006). The proof of the pudding should turn out differently here.

### 3.

The philosophers have long been telling us how science works. But recently they have only really been telling us how (or even merely that) it does *not* work, and that it should in no uncertain terms keep its hands off the field of philosophy. M. R. Bennett and P. M. S. Hacker have written a lengthy polemic (2003) against various popular *neuroscientists* that is currently the representative example of this tendency. It was not initially clear to me what their book has to do with the issues at stake here, for the study of literature is mentioned neither by the criticized nor by their critics. Generally speaking, in fact, the neurosciences have to date been no more than a footnote to the study of literature. One valuable insight lies in the fact that, as mentioned above, the body has its own internal reward system; this was demonstrated as long ago as the 1950s and makes it seem probable that aesthetic pleasure occurs in conjunction with physical involvement.<sup>9</sup> The study of memory is likely to provide an increased understanding of the foundations of narrative.<sup>10</sup> Even the function of mirror neurons is beginning to be considered on occasion (Lauer 2007). But Kelleter does not even take the time to consider these modest efforts. He declares that the neurosciences are the »model-field« of neo-naturalism, which allows him to go for the heart of the matter with the help of powerful backing and give the old scientific dualism, well, a »face-lift« (156). Consequently, it would seem sensible to test, at the very least, the principle and appropriateness of Bennett and Hacker's argumentation. The very title of their book is enough to attract attention: *Philosophical Foundations of Neuroscience*. Is there such a thing? I do believe that philosophy can interpret and criticize the insights and methods of the empirical sciences. It cannot, however, serve as a foundation for them – unless it makes dogma of its own underlying assumptions

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<sup>9</sup> For a more detailed study, which is however concerned only with the pleasure evoked by geometric forms and thus stands in the tradition of Gustav Theodor Fechner, see Jacobsen et al. 2006.

<sup>10</sup> For example, Young/Saver 2001.

and thus comes to resemble a religious undertaking. This, it seems to me, is the path taken by Bennett and Hacker.<sup>11</sup>

The core of their criticism lies in the accusation that a category mistake has been committed. The term ›category mistake‹ was coined by Gilbert Ryle, who originally used it to undermine the body/soul dualism (the ›ghost in the machine‹); it has since come to enjoy considerable popularity whenever there is a need to declare certain views or formulations illegitimate. Its meaning is roughly the same as what is known in linguistics as semantic anomaly. If I say, for example, ›the sun is smiling!, then my utterance contains a ›mistake‹ because something is being said about a member of the category of heavenly bodies that can only be said about members of the category of humans. But it would clearly be foolish to proceed to instigate a witch-hunt against metaphor in general, for ›is smiling‹ means different things in each case. Metaphors are tolerated even in scientific language, whether in the form of black holes, red giants, and white dwarves, or in the form of the *gyrus insularis*, the insular cortex. There are, though, well thought-out and ill-conceived metaphors, good and bad metaphors, and above all enlightening and misleading ones. Personally, I think that biologists should treat metaphors, and above all anthropomorphisms, with greater care.<sup>12</sup> The coinages ›selfish gene‹ and ›honest signals‹ can act as momentary sources of illumination, but if they establish themselves as terminology, their implications can lead us astray. In the case of popular writers on neurophilosophy, the situation is exacerbated by the fact that we are largely at their mercy because we lack the extensive specialist knowledge we would need in order to be able to form a critical judgement of our own. Again and again, we find ourselves confronted with the question of how literally the statements involved are to be understood, of how much is a summary of solid empirical findings and how much is the work of the imagination soaring on the wings of metaphor. Careful criticism of the use of metaphor in scientific language would therefore be most desirable. A mighty tome such as the book by Bennett and Hacker with its 460 pages is the kind of thing that might be able to help here, and sometimes indeed it does.

Unfortunately, though, Bennett and Hacker construct their argument on the basis of a highly dubious – in my view false – assumption. True, they identify

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<sup>11</sup> It is a fine coincidence that a volume edited by Gerhard Roth and Klaus-Jürgen Grün has now appeared with the title *Das Gehirn und seine Freiheit* (*The Brain and its Freedom*, 2006), promising in its subtitle *Beiträge zur neurowissenschaftlichen Grundlegung der Philosophie* (*Contributions to the Neuroscientific Foundation of Philosophy*). Who is now giving (or denying) whom a foundation? The best thing would probably be to abandon this scuffle for the right to lay foundations, and to concentrate instead on procedures of critical testing and corroboration. On the problems involved in the pursuit of certain knowledge, which leads to the Munchhausen trilemma of infinite regression, circular argument, or, as here, dogmatism, see Albert 1991.

<sup>12</sup> Dawkins 1998 attempts to provide such criticism; but the creator of such delicacies as the ›selfish gene‹ and the ›meme‹ is of course not entirely neutral in this respect.

themselves with analytic philosophy, but they give every impression of belonging to its idealist branch, whose members criticize only the language used by other people while trusting in their own as a reliable source of truth. Thus, Bennett and Hacker immunize their position against criticism by making themselves guardians of correct terminological usage while keeping their activity in this role completely separate from material issues. They distinguish between »philosophical problems about the a priori nature of things« and »scientific problems about the empirical characteristics of things and their explanation« (Bennett/Hacker 2003, 399). As Bennett and Hacker themselves stress, they want nothing to do with empirical hypotheses; they are dealing instead with a priori knowledge. This means excluding any research programme in which such knowledge is subjected to examination by the empirical sciences. Even philosophers can err, Bennett and Hacker admit (they do, after all, have colleagues), »but the error, like an error in pure mathematics, is an a priori one, identifiable independently of experience and experiment« (ibid., 404). This can be accepted so long as we are dealing with Kant's classical kinds of a priori knowledge, with mathematical axioms, Euclidean geometry, or the rules of formal logic, and Bennett and Hacker do indeed refer to what they call »our logico-grammatical investigations« (ibid., 400). In actual fact, however, they are concerned very much with *semantic* standardization based on »what competent speakers, using words correctly, do and do not say«, in other words on the linguistic competence of the authorities Bennett and Hacker, a competence placed beyond the reach of all criticism by being raised to a priori status. What a fine dogmatic circle. Kelleter, Bennett, and Hacker must forgive me when I say that I do not intend to be fooled like this at my age. One of the central questions is precisely that of where a priori semantic givens really come from. Do they owe their existence to divine inspiration? In the naturalist view, at any rate, they are products of socialization and individual experience (based on inherited dispositions), and thus rest on a wide variety of preconditions. It is only their intuitive use that allows them to acquire that naively self-evident state whose certainty is guaranteed a priori – as it goes with prejudices. Then there are those cases where the simple chronological sequence of before and after is converted in familiar fashion into non-negotiable (or *unhintergebbare*, as it is popular to say in German at the moment) conditions of possibility:<sup>13</sup>

Philosophy is *concept elucidation* by means of the description of the rule-governed use of words. Such descriptions antecede experience, and are presupposed by the use of relevant words in making *any* true or false empirical claim. Clarifications of the concepts of *perception*, or *memory*, or *imaging* and *the imagination* antecede any empirical theories about the neural underpinnings of these capacities. For the concepts are already presupposed in the formulation of the theories. (Bennett/Hacker 2003, 402; italics in original)

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<sup>13</sup> The meaning of German *unhintergebbbar* is something akin to that of English »irreducible« (translator's note).

But of course – were it not for the fact that chronological priority is no basis for factual impregnability. In an approach that follows the model of the empirical sciences, such concepts would belong to the initial assumptions that are confirmed, modified, or refuted by subsequent research. The meaning of words like »perception, or memory, or imaging and the imagination« after such investigations is different from their meaning before it.

If we now consider Bennett and Hacker in more detail, we see that they are attacking a particular kind of category mistake, specifically, what they refer to as the mereological fallacy. Mereology concerns the theory of parts and the whole. Unlike straightforward category mistakes, which involve the mixing of different categorial fields, a mereological fallacy occurs when different levels of a single field are not properly distinguished from one another. The presence of a mereological fallacy can be identified when the whole is treated as if it were a part, or when a part is treated as if it were the whole. Ryle wanted to use this to argue against psychophysical dualism and show that it is absurd to suppose there is, in addition to and separate from the parts of the body, a mind controlling the body. Bennett and Hacker begin with the second variant of the fallacy, which is known to philologists as synecdoche. I commit a mereological error in this sense if, for example, I say that my heart rejoices. Only my whole person can rejoice; my heart, on the other hand, is incapable of such arousal. It is similarly »forbidden« to say that the brain thinks, for thinking is likewise a privilege of the whole person. Such is the mantra of Bennett and Hacker: »It is not the eye (let alone the brain) that sees, but *we* see *with* our eyes [...]. So too, it is not the ear that hears, but the animal whose ear it is« (Bennett/Hacker 2003, 72–73; emphasis in original). »Human beings, but not their brains [...]; animals, but not their brains [...]; people, but not their brains [...]« (ibid., 73). They make it quite clear that even the »mind is just a part: »it is not the mind, that feels pain, perceives, thinks and desires, makes decisions and forms intentions, but the person« (ibid., 106).<sup>14</sup>

Bennett and Hacker also know, of course, that the kind of language under attack can be treated as figurative, which could make the overall classification »category mistake« rather less clear-cut. They attempt to differentiate: »It makes no sense to ascribe psychological predicates (or their negations) to the brain, save metaphorically or metonymically« (ibid., 72). And they suspect that »whether neuroscientists' ascriptions to the brain of attributes that can be applied literally only to an animal as a whole is actually merely metaphorical (metonymical or

<sup>14</sup> Kelleter himself stumbles here and introduces his quotation from this very sentence with »it is not the brain« rather than with »it is not the mind«. The entry on the German word *Person* in the *Historisches Wörterbuch der Philosophie* (Historical Dictionary of Philosophy) contains around seventy columns, and even if we confine ourselves to that part stemming from analytic philosophy, a good two columns, the term turns out to be so disputed that it certainly should not be used without accompanying elucidation. Nonetheless, the English word »person« does not appear in the index to Bennett and Hacker's book.

synecdochical) is very doubtful« (ibid., 79). The problem lies in the »merely« that Bennett and Hacker repeatedly use when referring to metaphor. It would appear that, for them, words can be understood only in one of two ways: in terms of literal meaning, such that a sentence like »the brain thinks« is false, or in terms of a figurative sense treated as mere rhetoric that, as »poetic licence«, does not need to be taken seriously in further consideration.

This binary schema of harmful literal and harmless figurative nonsense fails to reflect the diversity of the purposes (and pitfalls) of figurative speech in both everyday and scientific language. I shall highlight just one of those purposes here, chosen because it concerns fundamental aspects of both sides of the argument. It seems likely that most of the neurophysiologists under attack are working under the belief that the mind-body problem can be resolved with the help of identity theory.<sup>15</sup> Ernst Mach captured this in his phrase »two ways of considering the same thing« (»zwei Beobachtungsweisen desselben Vorgangs«; 1922, 305).<sup>16</sup> If the perspective of everyday language (refined by philosophers) and the perspective of scientific language have the same object, then it seems reasonable to interpret terms belonging to the semantics of everyday language in neurophysiological contexts, and to interpret findings of neurophysiology in terms of everyday language, in both cases with the objective of exchanging knowledge and making that knowledge more balanced. The metaphors or »category mistakes« that appear in the process mean, among other things, that *both* category systems are marked as contingent. There is no denying that this leads to dilettantish hotchpotch every now and then that presents itself as a form of synthesis. But it is precisely because of this that such contacts should be subject to informed criticism from those professionally concerned with interpreting the world with the help of everyday language. The position of Bennett and Hacker, though, amounts to a ban on any contact whatsoever and thus to a new dogmatic dualism in which Descartes's *cogito* has been replaced by the *apriori* of Bennett and Hacker as the central guiding force. This is not acceptable.

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<sup>15</sup> For a description of identity theory, see for example Pauen 2003, who characterizes it as the »view that every mental process is identical with a neural process« (»Auffassung, dass jeder mentale Prozess mit einem neuronalen Prozess identisch ist; 107.). This formulation displays one of the problems philosophers and empirical scientists have in communicating with each other: philosophers have a tendency to raise things to the level of an entire world-view, in this case the »view that every [...]«. Empirical scientists, on the other hand, permit such »views« to do no more than act as signposts for the next stage in their enquiries. It would then be better to talk of a supposition that the mental processes to be studied in any particular case are identical with neural processes.

<sup>16</sup> The version of Mach 1922 translated into English (Mach 1996) does not include this particular passage (translator's note).



## 4.

Prove all things: hold fast that which is good.  
(1 Thessalonians 5.21)

Kelleter finds nothing »good«. After all, he takes the position of eliminative idealism. The things he thinks neo-naturalist work has to offer have either been known for a long time inside the circled wagons, or are trivial or uninteresting. Everyday life at a university demonstrates the existence of an aesthetic consensus whose immune system is highly sensitive and sure to react to any serious threat by rejecting it (less serious ones like poststructuralism are hermeneutically assimilated with relatively little difficulty).<sup>17</sup> The relevant strategies and the motives at work in them would be worth a detailed study of their own. But Kelleter presents us with arguments – reasons – so it would not be entirely fair to engage in speculation about causes (although Kelleter himself does just that in his closing assessment, if not before – a performative contradiction?).

For a paragraph, in fact, it might even seem that Kelleter is granting the efforts of neo-naturalism a certain significance. He writes that they provide »a necessary antidote to the obscurantism of much humanist scholarship« and act »as a control on hasty brands of cultural relativism« (181). But he then goes on to say that »a mutually enhancing dialogue must begin with disciplinary self-awareness: with recognizing and respecting the real contentions that exist between categorically distinct types of knowledge« (182). Now, what is the difference between the »categorically distinct types of knowledge« that Kelleter says must be recognized in order for a dialogue to take place? Kelleter's piece contains a whole series of dualisms. They are probably meant to firm up one another in some way; precisely how, though, is not all that obvious. At any rate, it is clear that although the choice of words calls the distinction made by Bennett and Hacker to mind, this cannot be meant, for the Bennett/Hacker distinction is conceived of as a one-way critical street, making it hard to imagine a »mutually enhancing dialogue«. Right at the beginning, Kelleter speaks of »a defining feature of humanist knowledge: its concern not with facts but with meaning« (154): again, we find the anankasm to think in either-or terms. Contrary to what Kelleter himself asserts, his dualistic view of science does indeed seem to have ontic roots. He marvels at »the perplexing fact that human beings, alone among

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<sup>17</sup> Sociophobia was a notable forerunner of the biophobia we see today. In it, similar strategies were used to lump together the redskins of Marxism and social history before the approach of social history was stripped of its claws and assimilated into aesthetic cultural studies. Menninghaus 2003 is an interesting recent attempt to assimilate the biological perspective into the aesthetic one. See my response in *KulturPoetik: Zeitschrift für kulturgeschichtliche Literaturwissenschaft* 4 (2004), 278–287; a slightly modified version of the review can be found online at <[http://www.literaturkritik.de/public/rezension.php?rez\\_id=8698&ausgabe=200512](http://www.literaturkritik.de/public/rezension.php?rez_id=8698&ausgabe=200512)>.

species, have developed and refined means and possibilities of transcending their natural limitations« (169). How is this to be understood? People who go beyond their natural limitations come to grief, just like ants and amphibians who try to go beyond theirs. Anyway, let's take it as an edifying figure of speech from the same stock as ›liberation from the demands of nature‹ and ›walking erect‹ and such like, none of which one is allowed to examine in any particular detail. But what really, then, is the propositional content behind it? »The ›literary animal‹, in other words, is not *just* an animal« (169; emphasis in original). Kelleter claims that the crucial factor in our interest in human beings is »not their biological animal nature, but the self-made, post-animal part of their existence that is grafted onto biological givens« (169). What does »self-made« mean here? What does »grafted« mean? (And are they not mutually exclusive?) Behind the lack of clarity, clearly, there lies a tendency to lift humans out of nature in some way and provide them with culture as a home instead of it. That, though, would, if seriously pursued, be a – »mereological category mistake« (Volland 2007). The human capacity for culture (or ›second nature‹ if one wants to call it that) is part of human nature, a product of evolution, not something that was added (be it »self-made« or »grafted«) to nature as a mysterious other, let alone something that liberated humans from their ›first‹ nature and brought them to pure intellect or the like. Yet it is, nonetheless, a very special part of nature, of which only hints can be found in other animals; its specific manifestations pose special problems, and the study of it requires special tools.<sup>18</sup> Consequently, neither general biology nor even primatology is sufficient: we also need sociology and, as a hinge between them, a developmental psychology that is open to biology.

According to Kelleter's own position, the »real contentions that exist between categorically distinct types of knowledge« (180) cannot even exist in the first place. He says that »physiological or biological discussions of literature are not in conflict with historical or interpretive scholarship; nor are they in competition with it (cf. Bennett/Hacker 2003, 366). Both forms of knowledge are categorically distinct, not at variance or incompatible« (177). This stands in the long tradition of the German *Geisteswissenschaften*, and in the even older tradition of the double truth theory. I readily agree with Kelleter that we are concerned with two different kinds of knowledge or ways of dealing with the ›mind‹ (German *Geist*, *Gemüt*, soul, psyche, brain, person, ...) – specifically with two *perspectives* on the same thing, an internal and external perspective. ›Mind‹ can be the mental *sphere of movement*, the conceptual tradition in which thought moves and is to be found in reflection, needing no empirical knowledge beyond the conceptually coded and transmitted wealth of human experience, self-referential and a priori. But ›mind‹ can also be the *object* investigated by the humanities in the sense of *Geisteswissenschaften*, ›sciences of the mind‹, understood as empirical sciences. We could

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<sup>18</sup> See [1] for some basic remarks on this.

also speak here of scholarly approaches of reflection and distanced observation (aesthetic and scientific) respectively.

Just how we picture the relationship between these two approaches may well be crucially important. Again and again, one encounters dreams and nightmares about an all-encompassingly scientific way of life; and the process of rationalization identified by Max Weber is indeed bound up with profound crises of demystification. Nonetheless, the primary control system for how we communicate will always be everyday language, from »every curse of the oarsmen in the galleys« (»jedem Fluch der Ruderer in den Galeeren«) to the finest flowerings of »cultivated semantics« (»gepflegter Semantik«; Luhmann 1980, 19). It will always tend to encompass the totality of the world in which we live and provide the source of orientation for our everyday actions. The imprecise and unreliable utterance »I love you« cannot be replaced by a precise and reliable account of the speaker's overall neurophysiological status, although both relate to one and the same event. The ability of a scientific *Geistes-Wissenschaft* to influence our lives will always be confined to correcting the view of the world constructed by everyday language. This follows, in fact, from evolutionary theory: our cognitive apparatus developed under pressure from the need to prove itself in everyday situations. We can improve it, but cannot replace it. Intersubjectivity comes into being in everyday language, which structures our environment, provides our medium for communal decision-making, and offers solutions to the problems of our lives, or at least tells us that we are not alone in having them. Furthermore, it tells us which enemies to kill, which ones to enslave, how we can or should punish troublemakers, whom to hate how, and who the brutes and other dark forces are that steal our women, defile our children, and poison our wells. It is, in short, a colourful, organic mixture of truth and error, good and evil, that is corrected only spontaneously and by chance by the catastrophes, large and small, of practical life – as long as there is no source of critical evaluation to intervene.

*Geistes-Wissenschaft* in the sense of a distanced science of the mind (cognitive science in a broad sense) presents us with just such a source of evaluation in a form with stringent methods. If the idea of »transcending their natural limitations« (167) is to be more than edifying nonsense, it can mean only this: the ability of the mind to make itself the object of study, *con-scientia*, consciousness, knowledge of knowledge; not, that is, a simple crossing over into a sphere of freedom but something like »transcending through reflexion«. Kelleter says that »humankind is the only species on earth that has proven able to actively influence its own evolution by creating a »second« nature« (ibid.). Such a claim should not be made without studying the first one.

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