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Commentary

No Psychology In - No Psychology Out

A Remark on the *"Visions"* of a Field
Commentary on
"Biological Psychology 2010 – Visions of the Future of the Field in Psychology"

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In the last issue of this journal, leading experts of Biological Psychology shared their visions of the development of their field. In this, they described the path which shall prevent "the entire field of Psychology as a science to soon lose its significance and degenerate to a course of studies for questionable professionalism of psychotherapists and vocational advisors" (Birbaumer). Not surprising, it is Biological Psychology which will save us, especially with respect to General Psychology and cognitive science, from this descent: a merger of Biopsychology and General Psychology, and an integration of Experimental Psychology with scientific-medical disciplines. Such visions, borne by a refreshing enthusiasm "for one of the most elegant and most important of all sciences" (Birbaumer), naturally should not be confronted with some nitpicking remarks. Still, they provoke a response just there, where they pretend that it only was a matter of course, "a normal scientific development" (Güntürkün), which by virtue of its own dynamic virtually inevitably will cause fundamental research in psychology to become identical with Biological Psychology. A bold and venturous vision, if only its assumptions were right. But first things first.

A look into the field's history of science is, as always, very instructive if one looks for an understanding of its differentiation into sub-disciplines. As short as the history of academic psychology may be, as long is the history of questions and investigations which form the basis of it. In an attempt to rewrite the field's history in favour of Biological Psychology, we learn that "the roots of all Experimental Psychology rest in Biological Psychology (Wundt, James)" (Birbaumer). Sure enough, anyone who has glanced at the history of the field knows that neither Wundt nor James lend themselves as witnesses for such a reduction of Experimental Psychology to Biological Psychology. Even worse: With such a thesis the history of science of experimental and scientific Psychology is truncated by at least 200 years. The 17th century yielded a series of profound conceptual clarifications, which could only unfold its effectiveness – after the respective development of mathematical logics – during the so-called

cognitive revolution. However, let us refrain from this, since conceptual clarifications, as they have been and still are an essential part in the theoretical development of other natural sciences, are not at a high rate in nowadays' psychology. Rather, let's keep at the experimental: In his *History of Psychology,* 1808, Carus listed more than 120 authors of the 18th century, who had engaged in quantitative psychological studies in terms of theoretical systematisation of observation. And after all, we should not forget that Helmholtz, the most forceful proponent of a psychology as a natural science, emphatically regarded psychology as an autonomous science which could not be reduced to physiology. The history of psychology, therefore, does not lend itself to deduce the primacy of Biological Psychology. If anything, the opposite is the case. However, history aids in better understanding the structural institutional problems of psychology, which Rösler rightly points out: Academic psychology has been from its beginnings on, and still is, a pretty odd mélange of very heterogeneous research intents and perspectives. At present, the dominating research perspectives pertain to the social sciences and social-technological fields, as well as to the natural sciences, whereof the social-technological disciplines make up a continuously growing part. The resultant conflict between these perspectives brings forth serious and well-known problems in our curricula, so that there are good reasons to consider whether "the field should be broken down from its traditional form" (Rösler). However, this is an entirely different issue and would call for a separate discussion.

In the proclaimed visions, the applications and the need to secure the field's due social acceptance play a more important part than do questions regarding a cumulative advancement of adequate theories of our understanding of mental processes. While the agenda as regards issues of theoretical substance stays rather meagre in these visions, the scientific-political aims are blatantly obvious: According to Güntürkün, in the year 2010 one will find Biological Psychology having absorbed General Psychology; furthermore, Biological Psychology published "in the most rigorous journals, accumulated the most citations and collected most third-party funds, as compared to the other divisions of Psychology." One can see: success is just success, and it can only be enviers who still mumble of criteria of success as regards the contents (anyway, the scientific ideal of *veritas*, which is no more quite the zeitgeist, is not as readily capable of evaluation as its successor, the *visibility*). Presumably, if one may add to this sound of fanfares a somewhat more pessimistic tone, in the year 2010 indices such as fundraised third-party finances, publications or citations, which originally were posterior indicators of scientific achievements, will become indices that will be optimised quasi directly with the planning of career. As indicators of research activity, they will then become increasingly useless. We then will again have to engage in discussions as regards the theoretical substance of what has been achieved –, obviously, 'visions' can become nightmares this fast.

The applications, which are mentioned, be that neural prostheses, the biology of violence, treatment of speech or speaking disorders, or of chronic pain, psychopathology or psychosomatic medicine, foremost belong to psychological-medical applied research. It may be helpful to recall that a psychological approach does not turn into a natural science approach

solely by using cortisol values, EEG-recordings, or illustrations of the local metabolic activity of the brain, instead of values on a rating scale. As long as the body of substantial psychological theory on which such research is based, does not transcend classifications and correlations, one can speak as little of scientific fundamental research, as one will in the case of a food chemist concerned with applied questions, who consults a gas chromatograph for his work. The usage of complex instruments, whose design is based on rigorous and comprehensive scientific theory, is obviously not enough to turn applied investigations into fundamental research. Biological Psychology in many of its questions shares the trend with academic psychology, to reduce psychological-theoretical aspirations to a minimum in favour of a direct applicability and a manipulative or predictive mastery of complex functional interrelationships. As much as Biological Psychology, by means of a biological-medical mastery of highly complex psychological phenomena, may make significant contributions to socially relevant problems, as little does it compete by this with fundamental research pursued by General Psychology and cognitive science – let alone the somewhat abstruse idea of the capability to replace it. However, as in fact it is a matter of just this vision, we are bound to scrutinize some of the premises which rather silently underlie this brainchild.

As a general psychologist, first, one is stunned how it should be possible to largely understand the biological bases of the most complex phenomena as depression and anxiety, and at the same time to know virtually nothing about the neural principles which underlie comparably simple performances. Should the *architectus divinus* have meant well with humans and created the world, and with this us, so that theoretical comprehension of biological systems is even the more easy, the more complex they are? Admittedly, such a view may manifest a certain Panglossian simple-mindedness. However, since as scientists we do not want to preclude anything *a priori*, this remains a possible hypothesis. Let us have a look how it fares in face of some relevant facts.

Let us set out with a well studied protozoan, the flagellate Euglena, and its phototactic behaviour. This behaviour is in fact, as in line with our hypothesis, too complex to be predictable merely on the basis of the structural design of the organism. Therefore, in hope of a more simple theoretical understanding, let us look at a neural system, which is clearly more complex (although it exhibits an almost negligible small complexity in comparison to the human brain): the neural system of Nematodes. The nervous system of the approximately 1 mm in length species *C. elegans* consists of exactly 302 nerve cells and its 'wiring diagram' has been completely decoded (White et al., 1986); moreover, its 19,099 genes have been completely sequenced since 1998. Approximately 5000 chemical synapses and 600 gap junctions connect these neurons. Also, its neurotransmitters and neuromodulators have been identified. This small-sized nervous system regulates a wide repertoire of behaviour, including chemotaxis, thermotaxis, mechanosensory reactions (on the basis of only six mechanoreceptor cells), a complex foraging behaviour, and a set of other functions (i.a., a thermo memory). The complete knowledge of the components of its biological hardware is in fact a favourable condition *sui generis* for 'Biological Nematodal Psychology' to absorb 'General Nematodal Psychology' and making it dispensable. Why then is there no one to be

found in the field of biology who seriously holds this view? Rather, different and to a large extend autonomous levels of analysis have for all times been distinguished in biology, which provide building blocks of a theoretical understanding of a biological system (i.e., abstract analysis of functions, research into biological structure, research into physical foundations which realize structure and functions, as well as ontogenetic and phylogenetic development of the system). However, the associated performances of this 302-nerve cell system are not apprehensible on the exclusive level of analysis of biological-physical structures, albeit the complete knowledge of its biological hardware. "Relatively little is known about how interneurons integrate sensory information" (Thomas & Lockery, p. 157). Even with such a simple organism, the explanatory chasm between a biological description of its components and its performances is over-obvious: "C. elegans responds behaviorally to the presence or absence of food in a plethora of ways. …Surprisingly little progress has been made in understanding these responses" (Thomas & Lockery, 2000, p. 156). Hence, the abstract-functional level obtains special significance as a matter of course (e.g., Ferrée & Lockery, 1999). General Psychology has ever since followed biology in the recognition of equal autonomous levels of analysis and by nature, has ascribed its research approach to a psychological and abstract-functional level. Why then should it now adopt an altogether non-biological conception, i.e., that there is only one level, for the investigation of psychological phenomena, namely that of Biological Psychology. It may be related to the fact that according to our Panglossian hypothesis, things become the more simple, the more complex the investigated neural systems are. This would explain why one does not get a good grip on the investigation of comparable simple neural systems of insects, and for this reason is pressed to draw on an abstract-functional level of theory building in order to understand, for example, its performances in navigation (e.g., Hartmann & Wehner, 1995; Dyer & Dickinson, 1996). The averseness of Biological Psychology to purely psychological and abstract-functional levels of theory building (especially if those are mathematical, as is the case for computational theories) may be related to a certain preference for the concrete, according to which the *actual* explanation of these performances is to be found on the level of such realia as neurons, neuropeptides, NMDA- or AMOA-receptors. Compared to theses 'really existent' things, the explanatory value of psychological entities is put down as highly dubious. Resultant from such a stance, the needlessness of an independent psychological level of analysis should be clear in evidence, as we hope for in relying upon our hypothesis, when we deal with the performance of the human brain, i.e., instead of with 302 with a couple of billion neurons (psychologists like to proudly point to the fact that their object of research, the human brain, is supposedly the most complex structure of the universe). Consequently and in accordance with our Panglossian hypothesis, a theoretical understanding of its performances should be much more easy than in the case of Nematodes: Its psychological performances should be readily deduced from the present-day knowledge of biological-physical components, i.e., on a level of abstraction that does not greatly exceed the one of a Ramón y Cajal. However, that even in biology it is stressed (e.g., Braitenberg, 1992; Kauffman, 1995) that a theoretical understanding of biological systems of such a complexity can only be achieved on a

profoundly abstract level[1] (and that mathematical concepts, as spawn by today's physics, are not sufficient), can obviously be only due to a blatant misunderstanding of that which is truly rigorous science. Thank God that we are more enlightened in Biological Psychology. However, before we join the chorus of self-congratulation, let us have a look at our state of knowledge of relevant biological-physical components of psychological performances of the brain.

Just a few arbitrarily selected examples: As is generally known, so far we do not understand how the motor system coordinates the respective components of speech production – from the tongue to the lips –, we do not understand on the basis of which conceptual endowment we ascribe mental states to others (cf. Premack & Premack, 2003), or segment the sensory input in terms of perceptual categories. We do not understand due to which principles the brain of an newborn is capable of mimic imitation and how it translates a visual input into an adequate motor output. On our theoretical map of psychological achievements of the brain, up to now there are no more than a few vague contours. What is it then supposed to mean that we understood the biological-neural foundations of psychological achievements, if we do not even yet have a sufficiently well developed understanding of how to formulate the appropriate theoretical questions? It is here as in other disciplines: Someone, who wants to analyse a technical system, does not know what an electromagnetic resonant circuit is, does not really know either what at all he should be looking for. It does even get worse: So far, we do not even have a satisfactory neurophysiological account of seemingly elementary phenomena as the simultaneous colour contrast or trichromacy of colour vision (I hope, however, to spare the objection that there are three types of cones). And even in neurophysiological fundamental research we do not understand, for example, which information is coded by which aspect in the sequence of action potentials (Riecke, Bialek & Warland, 1997). It is also unclear, just to mention another fundamental example, whether neurons represent at all the computationally relevant units of neural information processing; rather, some evidence suggests that sub-cellular structures (i.e., certain systems of interacting proteins which so far have been only accredited a part in cellular metabolism) present functionally relevant units (e.g., Bray, 1995). Especially with respect to the brain, there is no satisfactory theory of electro-physical activity. Consequently, we cannot explain why fMRI findings look the way they do and not differently. We can only relate them to the body of psychological theory which has guided the investigation. In the rare cases in which we dispose of a psychological theory of sufficient explanatory width and depth, these kinds of findings can, of course, aid to the sophistication of theory building in times; usually, however, the indices provided in such cases are much too imprecise. This explains why fMRI-studies play virtually no part in the context of theoretical fundamental questions in the field of visual perception in which we dispose of a comparatively rich body of theory (cf. Hoffman, 2000).

[1] "We are convinced that ultimately a satisfactory explanation of thought and behavior will be given in a language akin to that of physics, i.e., mathematics" (Braitenberg, 1992, p. 473).

Even though, undisputedly, there is no part of the brain, about which neurophysiologically there is as much known as about the visual system, I have never heard from any neurophysiologist a comparable vision as that Biological Psychology of the visual system would make indispensable an independent psychological analysis of its performances. Far from it! Regarding the alleged primacy of neurophysiological studies of visual perception, Barlow commented (1983, p. 11): "Nothing could be more misleading, for all the important properties of the visual system were first established by psychophysical and psychological observations made on the system working as a whole. …physiologists need to be told what the visual system does before they can set about the difficult task of finding out how it does it." See the resonant circuit above. Apparently, circumstances seem to be downright contrary to the premises which underlie the visions of Biological Psychology. The more we know about psychological performances of the brain on neurophysiological and psychological level, the more explicit is the explanatory gap between the two, and with this the more important it is to reach a better theoretical understanding on an abstract-functional level, meaning in this context: on a psychological level.

Thus, there is not much evidence for the appropriateness of our Panglossian hypothesis that the theoretical understanding of biological systems is the more easy, the more complex they are. With this, there is no reason why General Psychology should diverge from standard scientific procedures – i.e., to pursue different independent and autonomous levels of analysis. If in the investigation of psychologically relevant performances of the brain, Biological Psychology nevertheless pronounces as dispensable an independent psychological level of analysis, as is the focus of General Psychology, then Biological Psychology evidently deviates from the conventional procedure of biology. With its pragmatic interest of manipulative mastery of complex functional relationships it seems to be rather proximate to medicine.

To regard a manipulative mastery already as theoretical understanding is a widespread misunderstanding. Without doubt, it is possible, as respective successes of Biological Psychology demonstrate, to modify and manipulate complex performances of the brain by biological means. Still, we cannot infer a theoretical understanding from the manipulative and predictive mastery of a complex functional relationship (Mendelian genetics and Babylonian astronomy may suffice as examples). Only on the basis of such a misunderstanding can the quite idiosyncratic inference be made from the correct premise that "human behaviour and experience is generated on the basis of brain processes" to the asserted conclusion that because of this "only by including knowledge of these brain processes behaviour and experience can be optimally modified" (Güntürkün). On the face of it, this may sound plausible, actually however, it is a fundamental fallacy. Indeed, those who want to "optimally modify experience and behaviour" have to presuppose the invalidity of this conclusion, because our theoretical understanding of "these brain processes" which generate human behavior and experience, is – according to the conventional explanatory standards of natural science – fairly close-by zero. The premise itself only reiterates what Carl Vogt phrased this way over 150 years ago: "that all those functions which we subsume under the term functions

of the soul are only functions of the brain, or, to express myself rather roughly, that thoughts are in the same relation to the brain as the bile to the liver or urine to the kidneys" (Vogt, 1847, p. 206). That mental processes are a function of the brain, has been a scientific truism since La Mettrie and Joseph Priestley, and as such is no more of particular interest. In the all together mundane meaning that psychological performances are performances of a biological organ, the brain, scientific psychology by nature is a part of biology. Sure enough, nothing follows from this that would justify a privileged position of a neurophysiological level of analysis – be that with a theoretical understanding or an optimal modification of experience and behavior in mind – or even the elimination of a psychological level of analysis.

Anyway, in the sciences discussions on which approach grants the fastest theoretical understanding are otiose, as history teaches us; ultimately, what counts is the success of the construction of explanatorily successful theories, and this may come about on labyrinthine and unexpected pathways. Biological Psychology, as other fields of psychology, has accomplished important and impressing contributions in its field. And it will, just as General Psychology, continue to do so in future. But why then does it want to impose regulations over General Psychology as to which is the most successful approach to a theoretical understanding of the mental; why does it even express the wish to absorb General Psychology? As always, different hypotheses lend themselves to this. First, let us act upon the idealistic assumption that it is all about the welfare of General Psychology itself; that General Psychology has to be freed from the ties of the profound misunderstanding which has led it on the wrong track in the scientific investigation of mental phenomena. This would be the impetus of enlightenment and as such would merit our enthusiastic affirmation. However, prior to indulging into ecstasy, let us, as before, examine a few premises which underlie such an endeavor of enlightenment.

One of the premises is quite easily identified: It is the (mostly tacitly posited) assumption that the *real* level of explanation of mental processes was to be located on the level of neural processes, and that psychological theories were preliminary makeshift constructions at best, until the 'real' explanation of the dealt-with psychological phenomenon has been found on the neural level. After all, since the mental was nothing more than a complex way of appearance of the qualities of those neural processes, the understanding of them would be settled with the increasing understanding of neural foundations of the brain. In short, a neuro-reductionist perspective. Even if it is rarely explicitly stated, the neuro-reductionism seems to be a sort of house philosophy of psychologists with a neurophysiological affinity.

On first sight, its plausibility, to put it mildly, is somewhat diminished by the fact that up to now – notwithstanding the abundance of neurophysiological knowledge – we do not know the physical principles upon which the psychological achievements of the brain are based. As long as it stays this way, the neuroreductionist thesis made remains no more than a dogmatic declarative statement that the answer to the nature of the mind is to be found on the level of neurons instead of – for instance, on the level of quantum theoretical processes. As such, it is of little theoretical interest. It is worthwhile to clarify that taken as a hypothesis, it depicts a

hypothesis about present neurophysiology, not however, a hypothesis about psychology. A however privileged status of neurophysiological data cannot be inferred from this, whatsoever. Psychology as a naturalistic inquiry will draw on *all* kinds of data which it considers interesting and relevant for the construction of explanatory theories of the structure and functioning of the mind: neurophysiological data just as phenomenological observations, observations on the development of perception and thought in infants, observations of lesions of the brain, introspective reports of experimental subjects, et cetera. There is, however, no justification to ascribe neurophysiological data an epistemic superiority or to even regard it as the only relevant data: The firing of neurons, the localization of metabolic or electrical brain activity, or the behaviour of subjects are some of many possible *indicators*. They are, however, by no means a substitute of inner processes. In the construction of explanatory adequate theories about internal states and mental processes, scientific psychology will have to go beyond observable measures, as other natural sciences have done so previously, and needs to be prepared for introducing every theoretical entity which will increase the explanatory width and depth of its theories. It is an entirely absurd limitation (as would be nowhere accepted in any other natural science), to restrain theory building in psychology by means of forcing it to refer to those levels of analysis which in neurophysiology are deemed relevant at the particular historical point in time. With this, the first premise, viz. the neuro-reductionistic credo, presents itself as a profound misunderstanding of the application of scientific principles to the investigation of the mind. Therefore, little speaks in favour of ascribing to it as a guideline of our research.

There remains a second premise which could show a certain intuitive attractiveness, again part of the neuro-reductionist credo as was the first premise. It states that in the sciences it was the aim of theory building to reduce phenomena of 'higher' levels of analysis to phenomena of more basic levels of analysis. Consequently, it would precisely be the aim of a scientific psychology to eliminate the mental as an object of psychology, by means of tracing it back to its 'real' foundations. Now, it is not that easy with reality. Be that as it may, we can notice beforehand, in order to prevent a popular misunderstanding, that the classification of phenomena as mental implies as little a metaphysical mind-body dualism, as would the classification of phenomena in chemical and biological imply a physics-chemistry or physics-biology dualism.

We may now consider what speaks for the appropriateness of this second premise. In doing so, it is helpful to take a look at history of science. It shows that not reduction, but rather a striving for explanatory unification has driven the sciences at all times. No one would propose the idea to eliminate the chemical from chemistry and the biological from biology, and to reduce everything to a more fundamental basis – in this case physics. Also, at most times it was the would-be fundamental science – especially physics –, which had to change to allow a theoretical unification between the fields. It is thus a misunderstanding of the history of science to regard reductionist efforts as a paramount concern. The relationship of chemistry and physics suffices to back this: In the 19th century, no one would have proposed the idea to regard the laws of chemistry as less valid and reasonable because they are not reducible to

physical laws. The descriptions and laws of chemistry are valid up to the present day, and it had been physics, which had to fundamentally change in order to allow for an explanatory unification. The same is true for other fields: Was the concept of genes of a dubious status as long as it could not be described by DNS-molecules? Or was the principle of natural selection regarded as unscientific because it was not deducible from Newtonian mechanics? Additionally, the fact of reducibility or rather irreducibility as such is relatively uninteresting: Electromagnetic characteristics are not reducible to mechanical ones, characteristics of water are not predictable on grounds of the quantum states of the involved hydrogen and oxygen atoms, and the concept of cells in molecular biology is not completely reducible to molecular chemistry. That, which has driven the natural sciences for all times, is by no means the reduction to a more basic science, but rather the development of adequate theories and the explanatory unification of principles upon which different classes of theories are based.

Consequently, it is an outright witless remark that neurons are the actual foundation of behaviour or mental processes, as one can remark with the same right that atoms are the actual foundation of neurons, quarks the foundation of atoms, and superstrings the foundation of quarks. If reduction to a more basic science would be a central characteristic of the sciences – which it is not –, how then does neuro-reductionism account for being only a pretty half-hearted reductionism which stops at a physically arbitrary level in-between, instead of reducing mental processes to physics (and explaining behaviour, for example, in terms of quarks)? It is interesting to observe that such a neuro-reductionist credo flourishes right there where theoretical substance is the poorest, while neither in biology nor in chemistry analogous proposals can be encountered. To reiterate it once more in explicit terms: In the natural sciences, explanations of 'higher' levels are explanatorily autonomous. Thus, also the second premise is of no avail to lead psychology onto the right track.

Psychology cannot aspire to the status of a natural science by trying to get rid of the mental and by reducing everything to a 'more basic' materialistic foundation. With no doubt, we can formulate our theories in appropriate *psychological* terms – notwithstanding that fact that in most cases we still have to develop appropriate theoretical frameworks – and nevertheless develop successful explanatory theories in line with the principles of natural science. Psychology can offer enough examples. Every general psychologist will have his favourites (mine are the investigation of conceptual semantics, e.g., Jackendoff, 2002, and computational analyses in the field of visual perception, e.g., Knill & Richards, 1996).

Hence, the premises on neuro-reductionism is based are hard to reconcile with the conventional principles of natural science. Therefore, they are not appropriate as guidelines for psychology. What then remains as justification? In the real existent business of science, it is not consciousness that determines being but social being that determines consciousness. As the fight for the continuously diminishing resources is a zero-sum-game, the idea suggests itself that what is at stake is not how we reach a deeper theoretical understanding of mental phenomena but rather quite profane and tangible matters. The reference to expensive instruments, to allocation of funds of the DFG (German Research Foundation), to successes in

the solution of urging social problems, and to the fact that "the German Biological Psychology could again attain international competitive power if it would gain the required funds" (Birbaumer) clearly demonstrate why it is strived at absorbing General Psychology. In a similar context, Jerry Fodor put it this way: "If you're in the research business, you will recognize at once the rhetoric of technohype. It is the hidden idiom of grant proposals and of interviews in the Tuesday *New York Times: The breakthrough is at hand; this time we've got it right: theory and practice will be forever altered: we have really made fantastic progress; and there is now general agreement on the basics; further funding is required*" (Fodor, 1998, p. 84).

Even if pretentious and pompous promises of success are part of scientific rhetoric, they are of no good as guidelines for the construction of successful theories. The problems, with which we are confronted in our strivings for a scientific understanding of mental phenomena, are gigantic, and there is little reason to be overenthusiastic about the present state of the field. But one certitude remains: The mental can be driven out of psychology as little as the biological can be driven out of biology or the chemical out of chemistry. Where no psychology is put in, no psychology is put out.

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