

Reply to the commentators **Morten Overgaard**

To begin with, I would like to thank the commentators for taking their time commenting on my target paper. I find it fascinating how diverging reflections it has inspired, and how often commentators express theoretical viewpoints that are compatible or that differ, regardless of the authors' disciplinary backgrounds. I thank Andreas Roepstorff for welcoming the article, and I do feel welcome writing it although Roepstorff is right in pointing out that the paper is in a position to 'please two audiences' (or at least address both of them). In a sense, this captures the primary motivation for writing the paper in the first hand: To introduce and discuss considerations that are more or less typical for so-called 'consciousness studies' to 'Danish brand general psychology'¹. Baars and Willert both say that psychology has means of dealing with the problems I have lined up that go around the kind of sceptic rhetoric, typical for consciousness studies (though they, so to say, come from 'consciousness studies' and 'Danish psychology' respectively). Although they argue so quite differently, the basic point is the same: That 'the problems of consciousness' are not problems of general psychology. Others, such as Roepstorff, refer to 'psychology' and 'consciousness studies' as were they somehow fundamentally different, and not as were the latter a specific subdiscipline of the first. What is interesting here is that even though Roepstorff's understanding of psychology does not stand without opposition, it is not trivially incorrect. For some unknown reason, the specific 'Danish brand' of general psychology to which Roepstorff also refers does *not* have a specific theory to account for the fact that some mental states some of the time are conscious. Hereby, I do not mean to suggest that no activity theorist in the past has ever made use of the word 'consciousness' – only that contemporary Danish general psychology as well as activity theory, ecological psychology and other such main sources of inspiration seems to have no real interest in finding out how to target consciousness for empirical research. Even the issue of how to understand consciousness theoretically seems totally disregarded. Temporary empirical 'consciousness studies' is however on a completely different track. It presents arguments into the scientific arenas of cognitive science and neuroscience, and, generally, it has no interest in the perspectives laid out by 'Danish general psychology'. As I shall return to it at the end of my response, this is not a situation to be conceived of as necessary or desirable.

¹ There is no doubt that general psychology in Denmark, with proponents such as Jens Mammen, Niels Engelsted, Erik Schultz and others has taken on a very special discourse probably not found anywhere else in the world. The discourse is to a large degree inspired by a Russian tradition of activity theory. The reference to a special 'Danish brand' in this reply is supposed to refer to this discourse and otherwise not to be taken too seriously.

Each commentary, with its separate apparatus of methodologies and arguments, points to ways of moving forward in the debate. My response to each one follows.

Commentaries from 'consciousness studies'

Antti Revonsuo's commentary challenges my target paper on a number of fundamental issues. I find all of his reservations and arguments valuable, so I shall try to reply to each one.

Revonsuo believes that my approach is based upon a basic assumption of a so-called act/object structure of experience, arguing that an intentional object is a necessary prerequisite for a conscious state. From here, Revonsuo questions my notion of 'direct awareness', wondering if it is a hollow concept that cannot be distinguished from nothing at all. However, as I write: "The notion of *direct awareness* implies that the subject is better acquainted with the relevant mental state in a fundamentally different way than he would be with the mental states of others". 'Direct awareness', in this context, refers to a specific kind of knowledge by way of which one has information about aspects of the world. I may have knowledge about my own unconscious states in basically the same way that I may have knowledge about other people's conscious or unconscious states. For instance, a psychotherapist may tell me that I have an unconscious desire to kill my brother. Or, the therapist may tell me that he is experiencing a pain in his left foot. In both cases, the kind of knowledge I have about the relevant mental states differs from the kind of knowledge I would have of the two mentioned contents (a desire to kill my brother, and the pain in the foot) if I myself actually experienced them. If that were the case, I would have had a *direct* rather than *indirect* acquaintance with the content. This definition, I find it, is different from the obviously circular statements that 'consciousness is identical to experience' and the like. Of course, given that it could be shown that this notion of direct awareness is reducible to the concepts 'experience' or 'consciousness', it will in that case be as circular as other definitions.

So even though I do not rule out the possibility that such a conceptual reduction could be carried through, I will not agree with Revonsuo that his mentioned alternative holds more promise. In fact, I think that it well illustrates the kind of definitional confusion, I wish to avoid. Revonsuo argues that the core element of primary consciousness (identical to phenomenal consciousness) is phenomenality (identical to subjective experience). Those two sets of synonyms are accordingly to be seen as different. A sufficient condition, Revonsuo continues, for the existence of primary

consciousness is the presence of subjective experience. This statement is not very informative – it says that for primary consciousness to be present, its ‘core element’ must be present as well. It does not get us much closer to a definition of any of the concepts.

With regards to my definition of introspection and its relation to classical introspectionists, it is, to begin with, not the main purpose of my approach to agree with certain historical figures. However, I do find numerous examples of understandings of introspection that are more or less identical to the definition I have suggested. For instance, Knight Dunlap writes “‘Introspection’ is usually defined in terms which are equivalent to the expression *consciousness scrutinizing itself*” (Dunlap, 1912, p. 404). Other examples from ‘first generation psychologists’ could be Angell: “It [introspection] consists simply in the direct examination of one’s own mental processes” (1908, p. 5), Stratton: “This direct acquaintance with the state of our minds which all of us to some extent possess” (1914, p. 2) or even William James: “It means of course the looking into our own minds” (James, 1890, p. 185). Finally, I am somewhat confused about Revonsuo’s interpretation of the quotation he mentions. As I see it, I am in perfect agreement with Titchener about the basic, definitional issue. In the selected quotation, Titchener argues that introspection is identical to retrospection – that is, we cannot inspect our own conscious states while they are still ‘running their course’. This is *not* identical to the statement that we by introspection mean something different from a turning of attention towards one’s own mental states. The difference, which undeniably does exist between our theoretical conceptions, is a matter of which *kinds* of mental states that can be the object of introspection. According to Titchener, we can only introspect memories (hence ‘retrospection’). According to me, we can introspect perceptions and thoughts ‘on line’. This is however a matter of *a posteriori* established facts regarding the phenomenon we have defined, given that it is not part of the very definition whether the mental state, we attend to, is a perception or a memory

I agree with Revonsuo that overwhelming theoretical problems occur if one thinks of introspection as ‘internal’ processes, and of contents of consciousness as ‘external’ processes. However, I do not see how this view is related to my definition. In fact, I do not at any time use the terms ‘internal’ or ‘external’ in relation to the definition. As I see it, nor does Titchener – at least not in the mentioned quotations.

Revonsuo finds the notion of ‘conscious state’ problematic. To clear up misunderstandings, I do not consider ‘consciousness as such’ an empty value, but simply a concept referring to whatever all possible conscious states may have in common. I find this no more problematic than talking about ‘perception’ or ‘memory’ as a general concept. According to such very high standards of precision, we cannot talk about such phenomena as a category, but only to speak of them individually, specifying their individual contents. But, of course, if we are not allowed to use the categories ‘memory’ or ‘consciousness’ at a general level, how can we then speak of individual experiences as being ‘a memory’ or ‘a state of consciousness’?

Revonsuo introduces a different interpretation of ‘state of consciousness’. This alternative notion starts with a distinction between normal and altered states of consciousness, arguing that a state of consciousness is altered when the content is somehow distorted or different from ‘everyday content’, whatever that then may be. To me at least, this seems in conflict with his background definition of consciousness that “to have primary consciousness only requires that there are *some* patterns of subjective experience present [...] It is purely about the existence of any sorts of patterns of subjective experience; whether simple or complex, meaningful or meaningless” (my italics). Given this definition of consciousness, ‘normality’ or ‘alteration’ does not seem to play any part.

Regarding his discussion of explanation and correlation, I will agree with Revonsuo in part. Cognitive neuroscience can be seen as a method for explaining mental events as well as just a method for studying the timely correlation between mental events and biological events. This relates to the paragraph in the end of my response regarding the ‘agenda’ of consciousness studies, as Schultz asks about. Revonsuo and I seem to have partly different (though, of course, very much overlapping) agendas, and this may account for at least parts of our disagreements.

In his commentary, Bernard Baars chooses an approach that makes me wonder if I have expressed the intention of the target article clearly. Seemingly, he finds it to be ‘armchair philosophy’, where the intention should be to unravel the foundations for empirical research.

His commentary could be taken as suggesting that we do not need any kind of philosophical or theoretical debates in order to pursue consciousness studies. If this is indeed the claim, I shall strongly disagree. Although it may be the case that we do not need *more* theoretical debates in order to pursue consciousness studies than we do in order to pursue any other line of psychological research, this only underlines how important theoretical debates are in those other fields.

Although I appreciate Baars’ encouragement to ‘young scientists’ to follow in the footsteps of Crick and Koch, I will politely decline. Crick and Koch should definitely be thanked to have played an important role in the introducing of consciousness into the scientific arena of cognitive science and neuroscience, and so, by the way, should Baars who with his book *A Cognitive Theory of Consciousness* (1988) also was among the pioneers. However, much of the approach is captured in the following quotation:

“Everyone has a rough idea of what is meant by being conscious. For now, it is better to avoid a precise definition of consciousness because of the dangers of premature definition.” (1998, p. 97)

The quotation, also, captures the essence of Baars’ commentary. It is argued that we at this point need not have an actual formalised description of what is meant by the term consciousness – all we need is an operationalised definition or a set of observables. An actual definition, it is furthermore claimed, will somehow pop out of the scientific progress so

that one day, as a result of empirical efforts, we will understand what the concept of consciousness is all about.

With regard to the first point that all we need is an operationalisation, one could reply that although there can be operationalised definitions without formal definitions, this is not to be desired in consciousness studies. As previously argued, subjectivity is an irreducible element of consciousness. So, when using the term 'consciousness', we mean this exact phenomenon, we are subjectively acquainted with. If we consider consciousness as an operationalisation only (say, somebody saying 'I am conscious'), we do not study consciousness at all. Rather, we study subjects saying 'I am conscious', which, as such, is a behaviouristic enterprise with a different agenda from consciousness studies. Subjects' reporting about their own consciousness is relevant to consciousness studies only insofar as there is a relation between 'the subjective phenomenon' and the operationalisation hereof. The result of a view, stating that we only need the operationalisation in the case of consciousness studies, is that we are left with a set of implicit definitions that we are not able to discuss or validate. That this is the case in many scientific studies of consciousness is indicated by the confusion about what exactly is meant by the concept of consciousness. It is rarely made explicit when the concept refers to 'being awake', to 'having knowledge about something' or to 'qualia' to list just a few possibilities (Natsolaus, 1978; Block, 1995).

With regards to the second claim that a definition of consciousness will somehow fall out of the scientific process, it seems to rely on an even more faulty idea. It is hard to see how a formal definition of consciousness should 'fall out' of an investigation that is itself completely based upon a definition of consciousness. How should an operationalisation of consciousness, stating that by consciousness we mean some specific entity in the brain, ever give rise to a definition that implies, say, dualism? Scientific investigations are not the sort of thing from which 'some or other' definition will arise. Accordingly, one must take seriously the implicit assumptions present in one's way of asking questions (be they empirical or not). When one has made an initial choice about the framework within which one's hypothesis is to be formed, one has already ruled out certain theoretical conclusions as the result. Therefore, one cannot claim to do atheoretical science (as Revonsuo by the way accuse me of saying). Given this is a fact of conducting scientific investigations, there should be warned against 'jumping right into them' – at least when it comes to drawing theoretical conclusions from them.

Logan Trujillo speculates on the notion of a 'qualiascope' as a possible alternative to subjective reports in a scientific approach to consciousness. Personally, I find this notion rather abstract, which of course is due to my own cognitive limitations. Furthermore, and more interesting than what I find abstract, are the reasons why the 'qualiascope' does not bring about any progression for a scientific approach to consciousness.

The interesting issue at hand is not why qualiascopes are contingently impossible, but why they are principally impossible. Trujillo is right when saying that I insist on the fundamental subjective nature of experiences. This is,

basically, why his suggested empirical test of a qualiascope falls short. The philosopher Sidney Shoemaker has famously argued for the possibility of 'the inverted spectrum' (1997): A case in which two subjects with identical brain processes have different experiential properties. They may both have the same activations in visual cortex, they both point to a coloured object and say 'red' (as they always have learned is the correct name for their particular experience), though, in experience, the referent of the word does not look the same. We do not have any scientific tool to address or solve this theoretical possibility of 'inverted qualia', in that all scientific tools are created to collect 'third person data' (the brain activations and the reports). The experiences, however, are subjective and therefore not observable to the scientist. We do not know of any law of nature that allows us to *rule out* the theoretical possibility of there being variations in experience that do not co-vary with variations in the body, although we normally do not assume that such phenomena can occur.

A philosophically simpler reason to deny the 'qualiascope test' is this: The empirical problem, my target article attempts to address is the problem of *transferring* subjective 'phenomena' into objective data by way of a report. If the qualiascope should be a success – due to some technological effort so far unbeknownst to man kind – it would *not* bridge this gap between the first and third person. It would (at the very best) create two tokens of one type of subjective phenomena.

Regarding Roepstorff's elaboration of my 'NCC model', I most certainly agree with what it says except for one aspect of it. However, I must also admit that this one aspect is the only aspect that really differs from my original model. The 'black boxes' have replaced the arrows in my model, and the 'pragmatic' and 'principal correlations' are identical to the 'desired' and 'actual correlation' in my model. I am, however, somewhat sceptical to Roepstorff's attributions of the terms 'ontological' and 'epistemological', and his view on the model as consisting of 'levels'. Obviously, 'measures of brain activity' and 'behavioural indications' are as 'ontological' as are brains and conscious states, and, vice versa, one can speak of consciousness and brains in epistemic terms. If one wishes to speak of the elements of the model as 'levels', I believe that what Roepstorff refers to as 'epistemological' should correctly be referred to as 'methodological' or 'operationalisations' (in that they are taken as methods for acquiring information about the 'lower level').

To some degree, I am sympathetic to Roepstorff's attitude of 'jumping into it with both feet'. It is absolutely correct that creativity and experimentation is as important to a scientific field as are 'rules', and, of course, the two should be seen as mutually interacting. However, I think it is dangerously naïve to argue that one can indulge head on into consciousness studies (or any other kind of study) without any discussion about fundamental issues. In my response to Baars, I mentioned reasons why one cannot assume that by 'going ahead' and practicing experiments, one day a theoretical framework will fall from the sky.

Commentaries outside of 'consciousness studies'

Erik Schultz' contribution is a very fascinating example of the above mentioned differences in thinking between 'Danish general psychology' and consciousness studies. Schultz lists a series of authors (Leontjev, Bergson etc.) to show important representatives of the theoretical standpoint that the 'genesis' of mind in evolutionary history is no 'blind accident', but the result of actions performed by organisms and their interplay with environmental matters of fact. Although the example 'we have lungs because we breathe' intuitively has a rather odd ring to it, it does contain an undeniably important message: Lungs illustrate, so to say, a certain kind of functionality. One could then of course, easily continue the story by pointing out that 'breathing' has its own evolutionary background – e.g. that we are material beings that work by way of oxygen. Thus, breathing has been, in a Darwinistic kind of reasoning, a 'functional solution'.

Schultz uses this argument to point out that no (part of the) brain constitutes a necessary and sufficient criterion for consciousness. This is, however, also where he gets it wrong. Let me illustrate through exemplification. Using a bow is a sufficient criterion for shooting off an arrow. It is not a necessary criterion, in that an arrow could be shot off using some other instrument. This is true *even though* there is a historical background of technological development and warfare, as well as there is an evolutionary component (given humans are created with a specific arm length that constrains how the bow is built) for the existence of 'legs'. There are no logical reasons at all that make it necessary to consider the two claims, bows being sufficient for shooting arrows and bows having a social/evolutionary history, as somehow mutually exclusive.

It is important to stress that my speculating that there are necessary and sufficient conditions in the brain for the existence of conscious states in humans is *not* identical to the statement that the relevant brain processes are causes to those states. It is certainly not identical to the statement that the conscious states are then *reducible* to the brain states, as indicated in the example of water being 'in' hydrogen (Schultz, this issue). One might even say that this debate about reductionism is of little interest to the project of approaching consciousness scientifically, in that the reducing or not reducing of consciousness is a philosophical matter. The necessity and sufficiency do not regard matters of causality, but of correlation. This should be expressed in the very term 'neural correlates of consciousness'. Given that we cannot assume a 1-1 relationship between conscious states and brain states, we cannot argue a priori that those conscious states are necessary and sufficient conditions for the existence/activation of the brain areas. If, say, two brain states correlate with the same conscious state, both of these would be sufficient for the conscious state, but the conscious state would not be sufficient for one of the two brain states. However, one may argue that the conscious state is necessary and sufficient for both of them. As in the previous case, this also is no claim of

causality, and it says nothing more than 'if one were to take away consciousness, one would take away the brain activations as well'. If one would take the argument any further than this, one would get into deep problems regarding how to speak of intentionality, qualia, and subjectivity in a physicalistic framework (see Chalmers, 1996; Jackson, 1986; Overgaard, 2003). This is, also, why Schultz' argument for the phylogenetic cause of conscious states misses the point. *Of course*, I would dare to say, there is more to mention about why we are conscious than what goes on inside the brain.

Put differently, you might say that there are certain constitutive phenomena that give rise to the very possibility of an individual to be conscious. This is the kind of issue that Schultz is addressing. Consequently, there are certain constitutive phenomena in a given individual that relate to its being conscious. This is the issue, I address. The connection between the two 'levels' (the level of species and of individuals) seems so obvious that it looks like an absolute mystery why one should consider oneself in paradigmatic opposition to researchers focusing at the other level.

Willert, alongside with Schultz, questions the paradigm, on which the approach is based. He argues that he at large, however, agrees with me, but he does point out a difficulty with my phrase that consciousness is "directly observable in itself". He goes on, then, to point out that we are not conscious of conscious states, as such, but of tables, chairs, computer screens etc. To this extent, I completely agree with Willert. What I meant to say, and what I should have explicated, is that by way of introspection, we can distinguish whether we are in a given conscious state or not. This relates to the concept of direct acquaintance, as discussed above. In this way, 'conscious states' are not theoretical inferences as are our concepts about objects, the existence of which we cannot consciously perceive. Only in this sense, one can speak of conscious states as being 'observable'.

Willert argues with reference to a fable about seven blind men and an elephant that no single discipline 'owns' consciousness. To this length, I agree with him. As should be obvious based on the discussion in this response, at least, the intention is nowhere near to figure out which discipline is 'best' at carrying out consciousness studies. One should however be very much aware that one's conception of the term 'consciousness' is not atheoretical, as Willert almost suggests by questioning whether we really need to define consciousness in one specific way. That is, the attempt to define consciousness as *not* 'one particular something' is as theoretical as the attempt to define it as such. There is, in other words, the inherent flaw in a theoretical viewpoint stating that we do not need to have one particular understanding of something that the statement in itself is one particular understanding of epistemology with certain theoretical consequences. In the end, this leads to the same kind of thinking as expressed by Baars and Crick & Koch in spite of the paradigmatic differences. I see no way around the necessity of defining one's topic of research.

On the 'agenda' of consciousness studies

Now, having addressed each commentator individually, I shall turn to the issues, mentioned by Roepstorff and Schultz: What is really the agenda in consciousness research, and what is its relation to Danish general psychology?

If one looks at the application of the phrase 'the problem of consciousness', it seems to have several different meanings. I shall describe four separate interpretations – an epistemological, an ontological, a definitional, and a methodological - which I believe cover more or less all usages of the term.

The epistemological interpretation. Our knowledge of objects in the world is 'third person knowledge'. We experience objects from the outside, and our knowledge about them is not identical to the observed objects themselves. Our knowledge of our own experiences, however, is 'first person knowledge'. The subject having the experience has a direct acquaintance with it, whereas other subjects only can be informed about it by way of reports issued by the experiencing subject. When we, say, look at the brain, we see no experiences in there – only greyish-white tissue. On the other hand, when we introspectively examine our own experiences, we have no feeling of brain processes being their cause. All in all, knowledge about and the 'perceiving of' our own conscious experiences is a different matter compared to knowledge about and the perceiving of external objects. This debate is seen in e.g. McGinn (1989, 1991), Jackson (1986), and Nagel (1974, 1984).

The ontological interpretation. What is the 'nature' of consciousness? Is consciousness essentially different from physical matter or is it nothing but physical matter? We can observe close correlations between psychological, behavioural, cognitive and neurobiological phenomena, but such correlations do not imply specific causal relations between those phenomena. The problem of consciousness, then, is how to understand consciousness in relation (e.g. causal relation) to other things. This interpretation is possibly the most widespread one, and it is debated in e.g. P.S. Churchland (1986), Chalmers (1996), Searle (1992), Kripke (1980), and Rosenthal (1990).

The definitional interpretation. The very concept of consciousness has several different meanings and it is often uncritically applied to mean different things. It seems almost unavoidable that a definition of consciousness will be circular (saying, e.g. that consciousness is 'phenomenality' or 'experience', which then in turn can be defined as consciousness). Are we to arrive at one single definition of consciousness or rather a whole set of different phenomena? This discussion is seen in e.g. Block (1995), Allport (1988), and Nelkin (1993).

The methodological interpretation. How can we scientifically study something that is subjectively defined? Obviously, we cannot apply standard scientific methods to subjective data. Do we need a new 'science of consciousness' or can we with few adjustments rely on contemporary cognitive psychology or neuroscience? This is discussed in e.g. Chrisley (2001; 2002), Nagel (1986), and Petitot *et al.* (1999).

Each of the above represents different interpretations of the alleged 'problem of consciousness'. It is of course not a matter of choosing which one that is the correct interpretation – rather, 'the problem of consciousness' is a problem in every one of the afore mentioned ways by most theorists' account.

We cannot *a priori* assume necessary relations between the four interpretations. For instance, one could claim that the ontological problem of consciousness can be solved with *or* without claiming that the epistemological problem can be solved. Furthermore, one can in principle argue for a certain definition of consciousness without being tied to one specific methodology as the consequence hereof. One might point to an important relation between the epistemological and methodological problem: We need a specific methodology because our knowledge about consciousness is constrained in this specific way. Similarly, there might be a relation between definitional and ontological questions – for instance the philosopher David Chalmers or the psychologist Max Velmans could be criticised for defining consciousness in such a way that it cannot be reconciled with physical matter (Velmans, 1991). However, although such relations seem plausible, they cannot be assumed simplistically.

There are several 'agendas' in consciousness studies that in different ways are tied to one's interpretation of 'the problem of consciousness'. The primary agenda in the ontological debate is whether or not a theory of consciousness is reducible to a theory of the brain or some other physical entity.

A science of consciousness, however, is first and foremost a matter of the definitional and methodological issues. A science of consciousness is *not* a science of the brain. At least, one would only think otherwise if one agrees with a strong version of reductionism, equating consciousness and brain. Likewise, it is not the study of concepts². The overall agenda is to empirically study a phenomenon that is subjective in nature. Because of the objective criteria for science, which makes the goal look very self-contradictory, 'consciousness research' has gone through many more methodological and definitional fights than has most other empirical disciplines. Some would say that this debate has been hopelessly stuck since Plato, but this is somewhat superficial. Although philosophers have been debating consciousness for centuries, some would even say millennia, there have been specific controversies over consciousness during the last 100 years, and the theoretical positions have varied tremendously. Consciousness has been conceived of as 'the starting point of psychology' and as 'the only true reality',

² Although, of course, it rests upon a basis of a certain conception of consciousness as well as on some set of fundamental theories about its nature.

but also as 'uninfluential' and even 'non-existing'. In the beginning of the 1990's, especially, the new interest in a 'science of consciousness' appeared. This development was at least in part inspired by a paper by Francis Crick and Christof Koch entitled *Toward a neurobiological theory of consciousness* (1990) and held alive by the annual conference series *Toward a Science of Consciousness*, arranged by Center for Consciousness Studies at the University of Arizona, and biannually held in Tucson, Arizona.

Still, however, we have no 'science of consciousness' as an established discipline. One compelling notion is that the idea was wrong from the beginning: That a whole new scientific discipline devoted to consciousness is not what we need. Furthermore, when one looks at the contents of the Tucson conferences, no clear agenda is revealed. All sorts of subdisciplines within psychology, neuroscience, physics, just to mention some of the sciences involved, have claimed to address each other in a common 'consciousness framework'. However, there has been no common theoretical ground or a common framework to guide the empirical research.

In spite of this continuing lack of success for the establishment of a common ground, much empirical and theoretical work of undeniable value and interest has been done. One could argue that even though a 'science of consciousness' may be futile, scientific approaches to consciousness are not. This difference may sound artificial, but we find notable similarities in other sciences. For instance, it could be argued that we do not need a 'science of horses', separate from biology and zoology, but this does not exclude that horses can be studied scientifically. The overwhelming amount of confusion in consciousness studies about how to conceptualise the phenomena in question, how to collect data, and how to interpret them makes it necessary for each discipline to have a very explicit and thoroughly developed methodology. Thus, an argument against a whole scientific discipline about consciousness does not imply that we do not need a reflected framework for empirical consciousness research.

My own, personal agenda is this: Consciousness studies can – if the right methodologically oriented debates and empirical studies are carried through before the explosive interest in consciousness is worn out – bring conscious experience back to psychology. Empirical psychology, especially, lost completely track of conscious experience after Titchener's death, and in theoretical psychology, the situation has not been much better. The mistake has been the attempt to carry through experiments about all kinds of psychological phenomena in methodological frameworks designed in such a way that excluded the fact that mental states can be conscious. So we should use this perhaps relatively short-lived change in scientific fashion to solve this age-old problem.

As should be obvious, I – for one – do not believe that we should 'start all over'. However, we should expect that the opening up for consciousness in empirical science will make it necessary to some degree to change the kinds of methods (for, say, reporting and for data analysis) that are used by experimental psychologists.

On the relation between consciousness studies and 'Danish brand general psychology'

At the rare occasions where researchers representing the so-called special 'Danish brand' of general psychology interact with researchers representing 'consciousness studies', they are often laid out as two different theoretical frameworks to understand similar problems. However, given the lack of clarity of concepts about consciousness and other aspects of mind, I believe it to be the case that 'Danish general psychology' largely has evolved around the attempt to answer a 'mind-world problem' rather than a 'mind-brain problem'. Where the 'mind-brain problem' concerns the relation between brain states and mental states, the 'mind-world problem' concerns how our mental states refer or otherwise relate to objects and living creatures in our surroundings. My discussion with Schultz above hopefully reveals that what intuitively looks like two alternative approaches are in fact not opposing theories, but in stead theories that address different (though related) questions.

In fact, we have three obviously related scientific problems: The relation between brain and world, the relation between consciousness and world and the relation between consciousness and brain. The overlap of these scientific problems is so obvious that they are easily confused or considered parts of one more general question concerning all the mentioned relations. This is however at present a somewhat dangerous enterprise, given the fact that the questions have in the past been dealt with in different paradigmatic frameworks. To let the three research questions continue as separate is however far from desirable as well, given that researchers interested in each question would cut themselves off from potentially crucial information.

It is, however, possible to be more ambitious than just to recognise that researchers of the 'mind-world problem' and the 'mind-brain problem' do not contradict each other by necessity. Exactly how a fruitful exchange of knowledge between Danish brand psychologists and consciousness researchers should take place is beyond the scope of this reply. It is however a consideration that needs to be made.

References

- Allport, A. (1988): What concept of consciousness?, in A. Marcel & E. Bisiach (eds.): *Consciousness in Contemporary Science*, Oxford University Press
- Angell, J.R. (1908): *Psychology*, 4th ed., 2nd chapter, Henry Holt & Co.
- Baars, B.J. (1988): *A Cognitive Theory of Consciousness*, Cambridge University Press
- Block, N. (1995a): On a confusion about a function of consciousness, *Behavioral & Brain Sciences*, 18, 2, 227-287
- Chalmers, D.J. (1996): *The Conscious Mind*, Oxford University Press
- Churchland, P.S. (1986): *Neurophilosophy*, MIT Press

- Chrisley, R. (2001): A view from anywhere: prospects for an objective understanding of consciousness, in P. Pykkänen & T. Vadén (eds.): *Dimensions of Conscious Experience*, John Benjamin Publishers
- Chrisley, R. (2002): Is there a seemings-is distinction for seemings? Paper presented at the *First Meeting of the Nordic Network for Consciousness Studies*, May 3-4, Skövde, Sweden
- Crick, F. & Koch, C. (1990): Toward a neurobiological theory of consciousness, *Seminars in the Neurosciences*, 2, 263-275
- Crick, F. & Koch, C. (1998): Consciousness and neuroscience, *Cerebral Cortex*, 8, 97-107
- Dunlap, K. (1912): The case against introspection, *Psychological Review*, 19, 404-413
- Jackson, F. (1986): What Mary Didn't Know, *The Journal of Philosophy*, 83, 291-295
- James, W. (1890): *Principles of Psychology*, Dover
- Kripke, S. (1980): *Naming and Necessity*, Harvard University Press
- McGinn, C. (1989) Can we solve the mind-body problem? *Mind*, 98, 349-366
- McGinn, C. (1991) *The Problem of Consciousness: Essays Toward a Resolution*, Blackwell
- Nagel, T. (1974): What is it like to be a bat? *Philosophical Review*, 83, 435-51
- Nagel, T. (1986): *The View from Nowhere*, Oxford University Press
- Natsolaus, T. (1978): Consciousness, *American Psychologist*, 33, 906-914
- Nelkin, N. (1993): What is consciousness? *Philosophy of Science*, 60, 419-434
- Overgaard, M. (2003): *Theoretical and Empirical Studies of Consciousness*, Ph.D. thesis, University of Aarhus
- Petitot, J., Varela, F., Pachoud, B. & Roy, J.M. (1999) (eds.): *Naturalizing Phenomenology*, Stanford University Press
- Rosenthal, D. (1990): A theory of consciousness, Report no. 40, Research Group on Mind and Brain, Zentrum für Interdisziplinäre Forschung, Bielefeld, Germany
- Searle, J. (1992): *The Rediscovery of the Mind*, MIT Press
- Shoemaker, S. (1997): The inverted spectrum, in N. Block, O. Flanagan & G. Güzeldere (eds.): *The Nature of Consciousness: Philosophical Debates*, MIT
- Stratton, G.M. (1914): *Experimental Psychology and its Bearing upon Culture*, Macmillan
- Velmans, M. (1991): Is human information processing conscious? *Behavioral and Brain Sciences*, 14, 4, 651-726