Consciousness

&

the Flow of Attention

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CONSCIOUSNESS AND THE FLOW OF ATTENTION

BY

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Abstract

This essay is an investigation of the interactions between attention and consciousness. I start by clarifying the nature of the distinction between the easy and hard problem of consciousness made by David Chalmers, and define the debates to be discusses in the rest of the essay. The first focal disagreement lies in the correct interpretation of the paradigm by George Sperling (1960). While Ned Block (1995, 2007, etc.) argues that in addition to the uncontroversial informational persistence, there is also phenomenal persistence involved in the case, most philosophers and psychologists deny that further move. Before going into the details, I clarify the relevant terminologies, such as phenomenal consciousness, sensory memory, access consciousness, accessibility, access, attention, reportability, and so on.

In chapter 1, I examine Block's OVERFLOW thesis in details. I first argue that although the general line Block takes is correct, the actual view is too strong and we should favor a weaker version of OVERFLOW that is compatible with the postdiction interpretation of Sperling proposed by Ian Phillips (2011), which is independently plausible. I then further argue for a view that the degree of attention tracks the degree of consciousness. This COVARIANCE view sits well with both views discussed above, and together they provide a more complete picture of how attention shapes consciousness.

The view is then applied to another debate between Fred Dretske and Michael Tye over the speckled hen kind of cases in chapter 2. A contemporary version of the speckled hen examples is the so-called change 'blindness' cases from psychology. The word 'blindness' is in quotation marks because according to the rich theorists, such as Block and Dretske, change ' blindness' is a misnomer. You might consciously see the difference but without noticing it. This view is contrasted with the sparse theories favored by Tye and Daniel Dennett that what happens is really a case of blindness. Here I argue that Dretske's case for his view is ungrounded because he misconstrues the relations between attention, fixation, and consciousness. I then argue that although Tye's case is better, he overreacts when actually formulating the thesis. After these negative points, I rehearse the COVARIANCE view and argue that it gives us a better picture in the change 'blindness' cases.

Finally in the last chapter, I situate the issues concerning the interactions between attention and consciousness into larger contexts – the contexts of general theories of consciousness. I discuss the higher-order views from David Rosenthal, Hakwan Lau and

Richard Brown, the attention-based view from Jesse Prinz, and the global workspace view from Barnard Baars. Without issuing a verdict of these views here, I evaluate their views on OVERFLOW. I then go on to consider varieties of skepticism raised by Dennett, Eric Schwitzgebel, and Alva Noë. I argue that skepticisms from Dennett and Schwitzgebel are not convincing and the one from Noë is compatible the current project. Finally, I briefly discuss a study of chimpanzee's working memory conducted by Tetsuro Matsuzawa et al. and compare it with the human case. The general finding is that chimpanzees are very good at concrete, perceptual memories while not good at abstract memories, while humans are the other way around. The evolutionary trade-off theory proposed by Matsuzawa is also discussed. There are many remaining questions unresolved by this essay but it serves as a starting point of further researches.

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With Jesse, Fall 2011

For my wife, Jessie Tang

With Jessie, Summer 2011

Acknowledgments

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Preface

In this thesis, I start with a discussion of Ned Block's views on consciousness, access, and related notions. As Ned himself recognizes, his relevant ideas have drawn much more negative responses in both philosophy and psychology. I myself disagree with many of his ideas. However, many people agree that this is an interesting and substantial debate. It gets psychologists to talk to philosophers more, and gives empirically minded philosophers a nice entry point. Ned and I first met in 2008 when he visited Taiwan, and we have come across many times before I started this M.A. in New York City. He never fails me on any occasion. He is the first person I would like to express my extensive gratitude.

This Thesis is not, however, a comprehensive evaluation of Ned's OVERFLOW view. Here in preface I talk only about practical reasons. One is that the whole debate is too big for me to handle here. Ned has been pushing this line for decades. His 1995 paper in *Behavioral and Brain Sciences* is one prominent example. The discussion was so heated that in 1997, we see another session in *BBS* for continuing commentary. A decade later, Ned reopened the debate by providing new empirical data and modifying the main thesis in his 2007 *BBS* paper. Again dozens of comments ensued, both in *BBS* and many other respectable journals. His latest update of the view just appeared in *Trends in Cognitive Sciences* several months ago in 2011 and 2012. As someone who just started to learn empirical stuffs, I confine myself within some specific aspects of the debate.

Another practical concern in related. When I attended Ned's talks, more than once some people expressed their skepticism about this approach. Although with many difference guises, the bottom line is this question: 'what's the take-home message for philosophers? Why should all these worry us as philosophers at all?' I myself do not share this concern. For me, I just want to know how the mind works, and philosophy, psychology, or whatever disciplines are simply resources that help me do so. Of course I do not pretend to be a scientist, but I would like to learn as much as possible, given that I do not lose sight of what I am heading to. Nevertheless, as a philosopher academically speaking, I do need to worry about my colleagues' perceptions, simply because I need to discuss with them, and they will review my stuffs. Therefore after a discussion of Ned's work in Introduction and Chapter 1, I turn to some other works. This second concern is related to the first one because it sits well with the fact that I am more familiar with some more traditional works in philosophy than with empirical works. Therefore in Chapter 2

I turn to relevant views by Fred Dretske and Michael Tye. To be sure, both of them are empirically minded and take sciences seriously, but the way they philosophize is less empirically loaded at least to my eyes, and this fits well with my current comfort zone, and damps some potential worries from my colleagues that Ned's recent works are too close to psychology and therefore too far away from philosophy. Again, I do not share this worry, but I need to take care of it.

I met both Fred and Michael briefly at some events. At Rutgers Epistemology Conference 2011, I got a chance to talk to Fred for almost an hour during a long coffee break. For reasons I don't know of, no one tried to interrupt and open a different topic, so I took my time and asked some questions about his view on change blindness. At one conference at Harvard, I got a chance to talk to Michael, though only briefly on representationalism. I hope to have more chances to discuss OVERFLOW with him in the near future; at the time I met him I haven't started to think about this topic too much.

The next two people I would like to express my extensive gratitude are David Rosenthal and Jesse Prinz. I first met David in Taipei in 2008, and had many email correspondences since then. He was so kind that I was invited to present a paper at the Cognitive Science Symposium in 2009, though in retrospect I realized that my work was utterly unqualified. During my stay at CUNY, I took two classes from him, and it goes without saying that I learned a lot from both. I learned not only materials of those topics, but also (and more importantly) ways of philosophizing. I expect to spend even more time on his works in the foreseeable future. Jesse is my supervisor of this Thesis. We first met in 2010 when I moved to NYC, but we started to come across more since 2011. Jesse is always very supportive and encouraging, and I also learned a lot from his two classes. My personal indebtedness to Jesse is too much to be disclosed here, and some of them are non-academic anyway. In chapter 3 I discuss both David's and Jesse's views on OVERFLOW, and I would like to learn more from both of them whenever it is possible.

In this chapter, I also discuss very different perspectives from Daniel Dennett, Eric Schwitzgebel, and Alva Noë. I have never met Professor Dennett, but I have learned a lot from his writings and hope to meet him in person at some points. Here I have many criticisms to Professor Schwitzgebel, and I hope to have a chance to exchange ideas with him soon; we came across twice but I did not get a chance to have discussions with him. I have a long friendship with Alva. We first met when I visited U. C. Berkeley in 2006. I attended his lecture on philosophy of mind, and we both attended

a seminar by Hannah Ginsborg and Hubert Dreyfus. We had many discussions during his office hours. Recently he moved to CUNY and we come across more. Although so far I did not have many chances to talk to him about this thesis, I try to learn from him from his written works and look forward to discussing more with him in person soon.

Another person I have to thank is David Chalmers. I first met Dave at ANU in 2009. Since then we have come across each other at many events, and we overlapped two semesters in NYC. Dave is very encouraging, but at the same time he also points out some of my problems in my career. In particular, he points out that I 'try to hard' in wrong things, for example make my CV look good. This sounds simple, but being pointed out by a person like Dave is different. I have learned from him so much in the past two years. In this thesis, I begin by discussing some of his ideas about the hard/easy problems. When I asked him about this, he told me that I misunderstood some parts of his ideas. I then made some amendments. Maybe he will still disagree with my remarks about his distinction, but at least I am aware of his worry.

New York City is a great place for philosophy. I have learned so much from this and that event, and it is impossible to record all my gratitude to people around. But I would like to single three people out. They are Richard Brown, Pete Mandik, and Hakwan Lau. Although Richard and I behave very differently (e.g., he plays drums but I do not), we are very similar persons at bottom. I do not know whether he will agree with me, but I think we are both passionate but at the same time too sensitive. Although these characters are not directly relevant to philosophy, they are important since we engage in philosophy in real life. Richard also has focused on the OVERFLOW debate for a while and I have learned a lot from both his written works and conversations. Pete is more different. He is always very confident but in a polite way. His questions at talks and other events always teach me many things. Although He does not focus on OVERFLOW at this point, I have encountered some of his written works occasionally, and I am always surprised by how much he knows. To give it a sketchy description, he knows both brainy stuffs and traditional philosophy. And also very importantly, he is very nice to people. Hakwan is a psychologist academically speaking, but it is well known that he is also a philosopher. He is like that not only because he majored in philosophy in B.A., but also because he actually spends time on philosophy. He hangs out with philosophers, and reads philosophical works carefully (I know this because I actually saw this). I attended both his graduate seminar and undergraduate lecture, and learned a lot from both. As a friend, he is great too. We both grew up in Asia so we share some cultural stuff. When I

I shall end this preface by saying that I love CUNY and I would like to thank everyone who has been parts of my life from fall 2010 to spring 2012, including those who are in different schools, different cities, and different countries. You know you are in the list.

Jan. 22, 2012, the Lunar New Year Eve, and kept revising towards the end

Tony. H. Y. Cheng

INTRODUCTION

Facing up Some 'Easy' Problems of Consciousness

0.1   Chalmers' Reservation

In 'Facing up to the Problem of Consciousness' (1995/2010), David Chalmers poses a challenge to researchers of consciousness with his distinction between the 'easy problems' and the 'hard problem' of consciousness. 'The easy problems of consciousness are those that seem directly susceptible to the standard methods of cognitive science, whereby a phenomenon is explained in terms of computational or neural mechanisms. The hard problems are those that seem to resist those methods' (4). Here are some examples of the *easy* problems he provides:

> The integration of *information* by a cognitive system;
> The *reportability* of mental states;
> The ability of a system to *access* its own internal states;
> The focus of *attention* (ibid., all my emphasis).

Since then, many philosophers devote much more endeavors to the hard problem, as if there is a clear division of labor – easy problems for scientists and the hard one for philosophers. This simplistic picture overlooks Chalmers' reservation ('…*seem* directly susceptible…,' my emphasis). Ideally, well-designed empirical experiments can provide evidence *directly*, though might not conclusively, for a given proposition, but we all know that actually this seldom happens. Albeit careful controls and considerations, there seems

to be an ineliminable gap between evidence and conclusions all the time, since alternative interpretations are almost always around the corner. I suppose this is the right gloss of Chalmers' reservation. Now, while some scientists venture their solutions to the hard problem – the more philosophical battlefield – some philosophers are actively involved in some easy problems. Ned Block is one such example. In 'Consciousness, Accessibility, and the Mesh between Psychology and Neuroscience' (2007) and many other writings, Block attempts to substantiate his views concerning phenomenology and access, among other things, by detailed investigations into cognitive sciences. Now many philosophers join him with this more scientific battlefield.

Some readers might have noticed that my way of distinguishing the easy/hard problems is not exactly the same as some of Chalmers' original characterizations. He says, 'The easy problems are easy precisely because they concern the explanation of cognitive *abilities* and *functions*' (ibid., 6, original emphasis). Later he says, 'When it comes to *conscious* experience, this sort of explanation fails' (ibid., 8, my emphasis). So at first blush, the contrast is between the functional and the phenomenal. Given that Block's concern is whether certain kind of visual memory is *conscious*, as we shall see, doesn't this count as a discussion of the hard problem?[1] No, because the easy problem is about consciousness too: they are easy problems *of consciousness*. As we will see presently, concepts appear in the example of easy problems quoted above – information, reportability, access, and attention – are all key concepts in Block's context. By contrast, the hard problem embodies the 'explanatory gap' (Levine 1983), as Chalmers himself glosses (1995/2010, 8). Here are some characteristic ways of posing the hard problem:

> '*Why* is the performance of these functions *accompanied by experience*?'
> 'I can see that you have explained how information is discriminated, integrated, and reported, but you have *not explained how it is experienced*.'
> '*Why* doesn't all of this information processing *go on "in the dark,"* free of any inner feel?' (ibid., 8, my emphasis)

In next section, we will see that none of these questions concerns us on this occasion. What is at issue between Block and his opponents is *whether* certain visual memory is conscious, not that *given* they are conscious, *why* they are so instead of not so. This is important because before entering all the details, I would like to stress that philosophers

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[1] Chalmers raised this idea in conversation.

should care about the easy problems too. As Chalmers reminds, '"easy" is a relative term' (ibid., 5). Philosophers should care about easy problems too since there are many hard philosophical questions involved as well, contra the simplistic picture that the easy/hard problems distinction maps perfectly onto the science/philosophy distinction.

Now some readers might not be convinced, or not even care about these rather meta-level discussions. I beg their patience and suggest them to jump to the next section. My discussion below is independent of the verdict here. Again, I start with this topic because I think it is important, but it does not mean that it is required by the following argumentation.

0.2   Block's Puzzle

As many other commentators, in what follows I will focus on Block's use of the Sperling case. However, it should be remembered that the original discussion was situated in a methodological puzzle. Block writes,

> How can we disentangle the neural basis of phenomenal consciousness from the neural machinery of the cognitive access that underlies reports of phenomenal consciousness? …The methodology would seem straightforward… But a puzzle arises: Do we include the machinery underlying reportability within the neural natural kinds of the clear cases? If the answer is 'Yes,' then there can be no phenomenally conscious representations in Fodorian modules. But how can we know if the answer is 'Yes'? (Block 2007: 481)[2]

The problem is this: as theorists, we need to know whether participants are conscious of something through their reports, since many tasks can be performed by unconscious information processing. But if we have to measure participants' reports, then how can we tease apart the neural basis of consciousness and the neural basis of reports? To this, Block offers an indirect solution. He attempts to propose independent reasons for the thesis that phenomenal consciousness overflows cognitive access, and argue that 'if we assume that the neural basis of phenomenal consciousness does *not* include the neural basis of cognitive accessibility' (ibid., 481, my emphasis), then we get a better explanation

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[2] Relevant terminologies, such as 'phenomenal consciousness' and 'cognitive access,' will be explained in 0.3.

of the overflow phenomenon. This is what he means by the 'mesh between psychology and neuroscience.'

So for Block, at least in this particular paper, OVERFLOW is only an *en route* point. However, it is actually the focal disagreement among many theorists, since if we accept it, then the indirect solution Block offers seems to be quite plausible: experiments from psychophysics support OVERFLOW, and the most natural explanation is that the neural substrates of consciousness and of access do not overlap entirely. As a result, most opponents take issue with the *en route* point. In chapter 2, I will discuss some aspects of it and offer my preferred interpretation of the Sperling-style case. But before that, we need to get clear what OVERFLOW exactly is. As we shall see, many theorists have different things in mind when they argue against one another, and we can make better progress if we can get clearer about the thesis itself before joining the battle.

0.3   What Overflows What?

'Overflow' is a relation. In order to get clear what we are arguing about, we need to specify the two relata. I shall start with the earlier discussions in psychology.

The left-hand side of the relation is relatively simple. In the original discussion, Sperling draws the conclusion that '*more is seen than can be remembered*' (Sperling 1960: 1, original emphasis; the experiment will be introduced shortly). Here the overflower seems to be 'information,' conscious or otherwise ('more is seen' seems to be informational, and it does not have to be phenomenal, though it can be). Later Neisser (1967) introduced the term 'iconic memory' to stand for this kind of 'transient visual memory' captured in Sperling's cases. The crucial point is that the very idea of iconic memory does not imply that it is conscious. For an analogy, consider the case of belief. Beliefs can be conscious, but the notion of consciousness is not built into the very idea of belief. In a word, the overflower in the original Sperling discussion is certain kind of information, conscious or not. The term 'iconic memory' becomes controversial these days, since it implies that this sort of visual memory is icon-like, but not every theorist accepts this view. Another term for this in the psychology literature is 'sensory memory' (e.g., S. J. Luck and A. Hollingworth 2008: 5). But again, this term is not entirely neutral since 'sensory' sounds like 'phenomenal' to some ears, and this begs the question against the view that in Sperling's case the overflower is unconscious. Since there is no way to find an entirely neutral term, as in most other cases in philosophy and psychology, we should fix the referent of 'iconic memory' and 'sensory memory' with a description: 'the

overflower in the Sperling-style case.' Whether this kind of memory is conscious or phenomenal is the substantial issue we are going to tackle.

What is overflowed, then? According to Sperling himself, it is another kind of memory ('…than can be remembered'). In the psychology literature, it is called 'visual short-term memory' (VSTM). The standard view has it that '[t]he capacity of VSTM is limited to three or four objects for simple stimuli and one or two objects for more complex stimuli'; they can last 'for several seconds after stimulus offset' (S. J. Luck and A. Hollingworth 2008: 6-7). VSTM is also called 'visual working memory' in psychology. This pair of terminology is relatively uncontroversial. Therefore, one candidate for what's overflowed is VSTM, or working memory for short. However, 'consolidation of information into VSTM is strongly dependent on attention,' so we should also consider whether sensory memory overflows attention.[3] Relatedly, in Sperling's case we collect participants' reports in order to understand the capacity for working memory or VSTM, so reportability is another important notion here.

From now on, we are gradually moving from the territory of psychology to its borderline to philosophy. One common interest of the two areas is *concept*. Concepts are important for humans since they are building blocks for thoughts.[4] It is a huge topic in psychology, where we have definitional theory, prototype theory, and so on. In the case of philosophy, many philosophers talk about concepts without having a worked-out theory of concept. One usual way is to identify concepts as Fregean *Sinn*, or modes of presentation. However, if Fregean modes are abstract objects, then they cannot be building blocks of human thoughts. One way out is instead to talk about conceptual *capacities*. Here is not the place to deal with these metaphysics; I shall now explain why this is relevant to OVERFLOW. One reading of the Sperling case is to hold that it nicely provides empirical substance for the view that perceptual experiences have non-conceptual content. This is the line Jerry Fodor (2007) takes. An elaborated development along this line is offered by Michael Tye (2006). In that paper, Tye distinguishes between the richness argument and the fineness-of-grain argument. Both are reasons to reject conceptualism, but as Tye points out, they are distinct arguments. Roughly, richness is

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[3] Another variety of visual memory is 'visual long-term memory' (VLTM). For example, think about the faces of your parents. Since this kind of visual memory is not relevant to what is at issue here, in what follows I will not further explore its nature.

[4] Some philosophers might think that concepts themselves are abstract objects, so cannot be constituents of thoughts, understood as human mental episodes, not as Fregean proposition. In that case, we can say that tokens or instantiations of concepts appear in thoughts. For brevity, in what follows I will still use the term 'concept' to refer to mental occurrences.

about how much information there is, and fineness is about how specific the relevant information is. Perhaps on further reflections, we cannot really separate them, but at least on the face of it there is indeed a difference between richness and fineness. However, what is common for the two arguments is the conclusion that human concepts cannot adequately capture the information we receive in perception. In other words, information in perception *overflows* conceptual capacities. In that paper Tye carefully discusses the relation between Sperling's case and richness/fineness respectively.

So far, we have not touched on Block's own discussions. In 1995, he first starts the discussion with this famous pair of terminology: phenomenal consciousness and access consciousness. Here are the rough definitions of them:

> P-Consciousness: 'Phenomenal consciousness is experience; what makes a state phenomenally conscious is that there is something "it is like" (Nagel 1974) to be in that state.' (Block 1995/2007: 163)
>
> A-Consciousness: 'A state is A-conscious if it is poised for direct control of thought and action…a representation is A-conscious if it is poised for free use in reasoning and for direct "rational" control of action and speech.' (ibid.: 168)

In that paper, Block only very briefly touched on how the Sperling case is supposed to sustain the distinction between P- and A-Consciousness, but the line is straightforward: the former *overflows* the latter, to use his later way of putting the thesis.

Block renews the discussion in his 2007 paper. There he not only modifies the thesis, but also changes the terminology. 'Phenomenal consciousness' is still there (though Block uses 'phenomenology' and 'P-consciousness' interchangeably), but 'access consciousness' is replaced by 'accessibility' or 'access.' One important reason for this, perhaps, is pointed out by David Rosenthal: 'It is unclear…that Block's notion of access consciousness actually picks out any phenomenon that we intuitively regard as a kind of consciousness' (Rosenthal 2005: 190). Of course people can have different uses of the same term, but still how other people use the term should be accommodated. In daily life, for example, when we say someone loses consciousness, we certainly do not mean anything directly related to rationality. To be sure, this is not to say that Block's use of A-consciousness is illegitimate, but this may partly explain why Block now uses other terms more. I believe that A-consciousness should be understood as accessibility, rather than access, since being 'poised for direct control of thought and action' is a potential, which

does not require the access actually happens. However, as we shall see, the latest debate concerns access more.

Now we can summarize and give some preliminary formulations. The putative overflowers are visual sensory memory and phenomenal consciousness. The putative items to be overflowed are visual working memory, attention, reportability, concept, access-consciousness, accessibility, and access. We shall rule some candidates out here in order to be more focused. Although whether concepts can capture visual information is itself an important topic, it is not directly relevant for our purpose. Reportability plays a crucial role in the relevant context but as Block points out, it has been established that patients who suffer locked-in syndrome have very limited or no capacity to report, but nevertheless still have certain access to their own mental states as shown by EEG studies (Block 2007: 484). So the overflowers are indeed richer than reportability, but this is not very controversial. 'Access-consciousness' can be taken as synonymous to 'accessibility,' so from now on I will only use the latter. So we can have this simple scheme now:

Sensory Memory		Working Memory
P-Consciousness (Phenomenology)	**Overflows**	Attention
		Accessibility
		Access
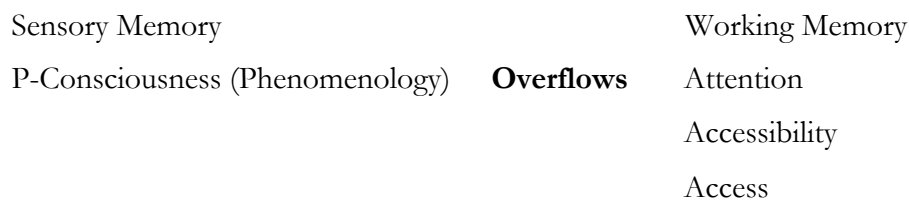
Remember that the very idea of sensory memory does not require it to be conscious. Now, it is safe to say that most people agree that sensory memory overflows working memory, attention, and so on. What is controversial is whether this sensory memory is *conscious*; that is, whether P-consciousness also overflows working memory, etc. This will be the main topic of chapter 1.

CHAPTER 1

Informational Persistence and Phenomenal Persistence?

Sperling Revisited

1.1   The Sperling Paradigm and Its Interpretations

Block's main discussion is structured by a specific interpretation of the 1960 Sperling paradigm. At the beginning of the experiments, subjects look at a blank screen with a fixation point in the middle of the screen. An array of letters constituting a grid then comes in as stimuli for 15-500 milliseconds. After the stimuli disappear, there will be a delay with a blank screen; the period of delay depends on specific experimental settings for different purposes. Then crucially, a cue tone comes in signifying which row the subjects are supposed to report (high tone for the high row, etc.). Since there is a delay between the stimuli and the cue, one might expect that the cue will not have any significant effect, since it comes too late. Surprisingly, however, subjects are actually very good at reporting the given row accurately, even though the cue comes much later than the stimuli (for more details, see Sperling 1960 or Phillips 2011, 382-6; for a demo, see http://www.nyu.edu/gsas/dept/philo/faculty/block/demos/Sperling320msec.mov).

What's crucial here for Block is the distinction between generic and specific phenomenology (Block 2007, 531).[1] According to him, what happens in the Sperling case *before* the cue is this (ibid., original italics):

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[1] Block's relevant argumentation is very detailed, and I do not pretend that my reconstruction here captures every main point he wants to make. Again, this Thesis is not a contribution to Block scholarship; I take the discussion of Block only as a starting point for the entire essay.

> Generic phenomenology: '*that there is* an array of alphanumeric characters.'
>
> Specific phenomenology: 'of specific shapes of all or most items in the array.'

Block then glosses the dialectic situation succinctly:

> There have to be such specific representations given that any location can be cued with high accuracy of response. The locus of controversy is *whether those specific representations are phenomenal.*' (ibid., original italics)

The thought is this. Since subjects can report, i.e., provide *information*, on any row according to the *delayed* cued, there must be enough information before the cue, and those pieces of information have to persist long enough for the cue to act on. Call this *informational* persistence. Virtually everyone in this debate agrees on this. What is crucial for Block, as he himself points out, is whether those representations are conscious, that is, whether we also enjoy *phenomenal* persistence in the Sperling-style case. Different interpretations of this case mainly divide themselves into the positive and negative answers to the question whether there is phenomenal persistence as well.

Before considering Block's arguments for the positive answer, I shall pause a bit and try to relate this section to the previous one, where we discussed the 'what overflows what' question. Again the left-hand side is relatively simple; they are either sensory memory or P-consciousness. We can make it even simpler by conceiving the matter like this: the question is in effect 'is the overflower, sensory memory, P-conscious?' And if the answer is positive, then P-consciousness itself is no longer only a candidate for overflower but becomes an actual one. What is more difficult, again, is the right-hand side. Recall that we have visual working memory, attention, accessibility, and access. Here I propose a way to understand their interrelations. First consider this scheme:

Sensory Memory » Attention » Access » Working Memory » Report

At first, there comes the stimuli, and the participants' retina receive the information, and create brief sensory memory. Whether it is conscious is at issue. After the stimuli go offset, we will have a delay and the cue tone. This will draw participants' attention to the relevant row. Now the attention will make relevant information accessible and working memory will store those pieces of information. Finally, participants will be able to report

the cued row by drawing upon the information in working memory. This should be a relatively uncontroversial description of what happens in a standard Sperling case. Now as we have discussed last chapter, reports should not be what at issue, since there are clear limitations on reports as vividly illustrated by extreme cases such as the locked-in syndrome. What concerns us lies in the process from attention to working memory. Here I shall argue that we should primarily focus on attention for our purpose.

On the face of it, it is not too difficult to distinguish between attention and working memory. In daily life, although we do not talk about working memory, we do talk about short-term memory and have some rough ideas about it. Or at least, we have an idea of what memory is. Not that we know its nature, but we know roughly what the concept means, and we know it means something different from attention. However, as a matter of fact it is notoriously difficult to tease them apart empirically. The main reason is that both in real life and in labs they closely follow each other in a very short time period. Attention and working memory take time, to be sure, but they take very short time. As Alan Allport says, at least within the integrated competition framework (ICH),

> [I]t may not be easy to separate clearly the twin concepts of working memory and attention. They differ, if at all, in that working memory may be held to include the *cumulative* persisting products of several iterative cycles of processing, whereas attention – as a transient state of the whole organism – refers more specifically to the *momentary* outcome of a single, convergent 'settling' process. (Allport 2011: 35, original emphasis)

Fortunately, at this preliminary stage we do not need to deal with empirical difficulties immediately. Remember what we are doing here is to determine which should be our primary target of discussion, so conceptual analysis should be suffice for this purpose. I propose to have attention as our primary target. Since attention comes *before* working memory at least in the Sperling, if P-consciousness overflows attention, it will overflow working memory *as well*, because working memory can only store conscious information from what is attended. What working memory retains can only be smaller than what attention catches. If so, we shall focus on attention first. If P-consciousness overflows attention, then P-consciousness definitely overflows working memory as well. If P-consciousness does *not* overflow attention, then there is a further question whether P-consciousness overflows working memory, since it is possible that working memory

cannot retain everything provided by attention and therefore retains less conscious information than P-conscious sensory memory.

So we have reached the preliminary decision to focus on attention rather than other important items. But a further problem arises: attention itself is a huge and difficult topic in psychology, and there are many competing theories in the psychology literature. In what follows I will conduct some elementary discussions of it and try to pin down what we are after in the Sperling case.

Two distinctions are drawn by psychologists when it comes to attention. The first is between overt and covert attention. When one shifts one's attention by moving one's eyes or heads, it is a case of overt attention. By contrast, when one shifts attention without any observable behaviors, then we have a case of covert attention. Since overt attention involves certain bodily movements, it trivially involves a different set of neural substrate. The second distinction – endogenous and exogenous attention – is more complicated. Endogenous attention is also known as top-down attention; it involves subjects' intention to direct her/his attention, while exogenous, bottom-up attention does not. So for example, if you are attending to the lecture and suddenly you are distracted by the siren outside of the building, your endogenous attention to the speech is interrupted by exogenous attention that is drawn by the siren. Of course in real case the distinction is fuzzier, but the idea should be clear. This second distinction involves more controversies, since it is not clear whether endogenous and exogenous attention actually form a natural kind. But since our discussion is not at the neural level, we can set the worry aside for the moment. Now let's take a look of the relations between these two distinctions and the Sperling case.

First, it is not entirely clear whether in the Sperling case participants use only overt or only covert attention. Typically, the experiments start with a blank screen with a fixation point, so presumably participants fixate at the middle throughout the trials. If so, then mostly the experiments involve covert attention, since participants will attend to, say, the low row while they still fixate at the middle. However, it is not clear whether the experimenters ask participants not to move their eyes at all. If in some trials participants move their eyes a bit, it will involve overt attention. Although we cannot settle the matter on the armchairs, this issue does not seem to be too pressing. We should presume that participants fixate at the middle and use covert attention to perform the task, but it is not obvious that there are significant issues if they use overt attention sometimes. We should, at any rate, be open to the possibility that it actually matters, and temporarily proceed

with the assumption that the distinction between overt and covert attention does not matter that much.

I would say similar things about the second distinction. Since the participants know what the task is and how to follow the cue tone, presumably they use endogenous, i.e., top-down attention is performing the task. However, it seems right to say that after several trials, the participants get used to the task and let their attention naturally drawn by the cue. Again, it is not clear what hinges on this, at least for our purpose. So again we will proceed on the assumption that the participants use their endogenous attention in the trials mostly, but be open to the possibility that exogenous attention will have a different effect. With these preliminary, inconclusive reflections, we can go on to consider Block's main argument for his case.

1.2 Block's Case for OVERFLOW and Its Critics

Recall that we have pinned down what the relevant OVERFLOW thesis is:

P-conscious visual sensory memory overflows attention.

Now it is time to consider Block's main arguments for it in details. There are six of them in the 2007 paper:

Argument 1: 'As Burge notices, subjects (including myself [i.e., Block]) in overflow experiments often testify that their responses are based on specific phenomenology that was there all along.' (Block 2007, 531)

Reply: It is true that we should take seriously subjects' reports at least as the starting point, but given the transient nature of the stimuli in the Sperling case, one can reasonably suspect that subjects are not in a good position to have a true description of their experiences. It might be true that they *read off* answers from specific phenomenologies, but it does not mean that those phenomenologies are there *all along*. It is possible that those specific pieces of *information* are there all along, but they become conscious only *after* the cue tone. Reasons for preferring this later view will be offered when I sketch the positive view. The general line is based on attention-induced postdictive phenomena.

<u>Argument 2:</u> 'Subjects are attending to arrays in full view, in good viewing conditions, for half a second in…some versions of the Sperling experiments…more than enough time for specific phenomenology (Burge also makes this point).' (ibid., 532)

<u>Reply:</u> This point is about timing. Suppose, contrary to any Sperling experiment, we show the array of letters for a really long time, say ten seconds. But we ask subjects to fixate at a given point, and try to report all letters by switching their attention. This should be an easy task, since in this setting they have plenty of time (if ten seconds are not enough, let's prolong it to twenty). Now I predict that if subjects fixate at the middle of the screen, and turn their attention to the upper row in order to report letters there, they will not have specific (enough) phenomenologies of the lower row at that moment, no matter how long the stimuli persist. If so, then it is not true that subjects have specific phenomenologies of almost all items all along in the standard Sperling experiments, since they cannot have that even in the version I am envisaging, i.e., the stimuli persist for a really long time. The spatial arrangement of the stimuli prevents us from having specific phenomenologies of most items, given the limits of fovea vision and attention. This is not just a speculation; we can actually do it with the demo above (press the pause bottom to freeze the stimuli) or with the stimuli on a piece of paper.

<u>Argument 3:</u> 'If there is only generic phenomenology before the cue, and if the cue causes the generic phenomenology to be replaced by specific phenomenology, then there is a shift from generic to specific phenomenology. The fact that subjects report no such phenomenological shift might not be strong evidence against this view, but it is some evidence.' (ibid.)

<u>Reply:</u> I agree with Block that it is probably some evidence, but it is not too difficult to explain it away. Consider the famous Carrasco 'attention alters appearance' experiments:
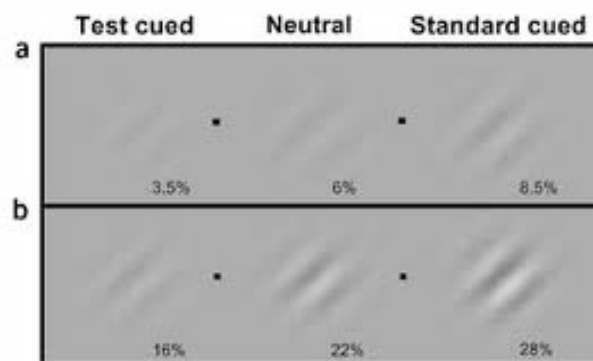
Figure 1, from Carrasco et. al. (2004)

The four dots in the figure are fixation points. Let's try the one between the 22% patch and the 28% one. If you fixate at that dot and covertly switch your attention to the 22% patch, the attention will boost the contrast of that patch so that it looks just like the 28% one (contrasts are defined objectively via mathematics). Now, do subjects feel the transition before and after they shift attention? Arguably not. The reason for this, I submit, is that the effect attention can give us is not very drastic. Comparing the case of eyeglasses. If one has myopic and he uses a pair of glasses for better sight, his visual phenomenologies before and after he puts on the glasses will be extremely different. After all, that is the whole point of having glasses. Therefore, whenever he puts on or takes off glasses, he will feel a shift/transition of phenomenology. However, attention does not work like this. Although it will alter appearances, as Carrasco and her colleagues have shown, the effect is incomparable with glasses. This explains why subjects do not feel the shift of phenomenology, and thereby saves Block's opponent at least in this respect.[2] Moreover, Block's opponents do not have to hold that before the cue there is only generic phenomenology. For example, his opponents can hold that before the cue, there are some specific phenomenologies due to attentional effects: before the cue, subjects' attention will flow randomly so that they will enjoy certain specific phenomenologies. More on this view in my reply to argument 6.

Argument 4: 'There is evidence mentioned in the target article that cortical persistence obtains at all levels of the visual system and therefore at the phenomenal level as well…Hence, there is a neural case for phenomenal persistence.' (ibid.)

    Reply: There is indeed a case to be made here. However, as Coltheart stresses, neural persistence should also be separated from phenomenal persistence (1980, 184). Block is right in pointing out that neural persistence favors his view to some extent, but more need to be said at this point if one wants to bridge neural and phenomenal persistence. Simply pointing out there is neural persistence does not itself constitute a direct support for phenomenal persistence, since whether that particular neural persistence is the neural substrate of phenomenology is what at issue.

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[2] In conversation, Carrasco says that no participants reported the transition of phenomenology, even if in her case there is indeed a shift of phenomenology (e.g., from 22% to 28%).

<u>Argument 5:</u> This one is about 'Di Lollo's paradigm using a 5 by 5 grid in which all but one of the squares is filled with a dot. Subjects see a partial grid with 12 of the dots filled in, then, after a delay, another partial grid with a different 12 dots filled in. The subjects' task is to report which square has a missing dot… [In] the variant by Brockmole et al. (2002)…the appearance of the second partial grid was delayed by as long as 5 seconds and in which subjects were told that a good strategy was to "imagine the dots still being present after they disappear" (317).' (ibid.)

    <u>Reply:</u> In evaluating this argument, readers are encouraged to read Block's relevant passages in his 2007 paper since there are too many details to be quoted here. Block's main point is that the delay of 5 seconds is so long that we need phenomenal persistence to explain the situation. However, I believe the fact that it is so long should be interpreted as a ground to cast doubt on phenomenal persistence, contra Block. As he notices, subjects are instructed to 'imagine' the persistence, and this opens the possibility that subjects are using their conceptual, *cognitive* capacities to do so, as opposed to phenomenal capacities. In psychologists' terms, this becomes a cognitive phenomenon, as opposed to a perceptual one. Again, more needs to be said for Block to hold his view.

<u>Argument 6:</u> 'Kouider et al….hypothesize that Sperling-like paradigms result from "partial awareness: subjects have a transient and degraded access to fragments of all the letters in the grid."…What are Kouider et al. saying about specific phenomenology? No specific phenomenology at all is not compatible with their view, since they say subjects are to some degree conscious of and have access to "fragments of all the letters in the grid".' (ibid.)

    <u>Reply:</u> Block is right in pointing out that Kouider et al. cannot maintain that no specific phenomenology before the cue. But they need not, as I mentioned in my reply to argument 3. Block's opponents can hold that attention gives rise to specific phenomenologies (more on this in section 3). Since before the cue, subjects' attention will flow randomly, they will enjoy some specific phenomenologies. This explains why in the trials without any cue, subjects can still report correctly around four (random) letters. So Block's opponents should not hold that there is absolutely no specific phenomenology before the cue. What they should insist is that those specific phenomenologies are not as rich as Block describes. Consider a similar distinction of gist and object perception in psychology. In relevant experiments, the stimuli persist so short

that subjects can only report accurately about the gist. This does not imply that subjects see absolutely *no* detail. They do see some, but not as much as Block's view predicts.

What do all these leave to us? I am certainly not asserting that Block's six arguments are all defeated. What I attempt to show is that all of them raise substantial issues that need to be considered further, but none of them is unanswerable by his opponents. I spend much space for replying those arguments because I find them important, and to my knowledge all papers against Block 2007 do not try to answer these six arguments directly.

1.3   COVARIANCE: A Hypothesis Introduced and Defended

The hypothesis developed here is not original. It has recently been introduced by Ian Phillips (2011). In a sentence, the hypothesis is that in the Sperling case what happens is *cross-modal postdiction.* It is postdiction, because according to the view the cue in effect *retrospectively* modifies what the participants consciously see. It is cross-modal because the cue tone is *auditory*, while the original array of stimuli are *visual.* This second part might not be essential to the discussion, since if in the trials we instead use visual cues such as arrows, we can still get the same effect. However, since normally the cue is auditory, it will be important for the proponents of the postdiction interpretation to also argue that cross-modal postdiction actually happens in standard Sperling cases.

Before proceeding, I shall explain why I am developing the view here, given that it has been well argued by Ian Phillips. Here are some reasons. First of all, I do not agree with certain details of the view, in particular I believe it is compatible with a weaker version of OVERFLOW. Secondly, I will strengthen the view by considering different measurements for consciousness and cognitive access. Thirdly, I will apply the view to other philosophical as well as empirical issues, as we shall see in the following chapters. These should be sufficed to justify my project here. The view is also similar to the one developed independently by James Stazicker (2011), and I will conduct relevant discussions in due course.

Now let's start with postdiction. A relatively uncontroversial case is 'backward visual masking.' Consider this often-cited figure below:

Figure 2. Target Stimulus Masking Stimulus

If the timing and the shapes of the stimuli are suitably designed, the participants will *not* be aware of the first stimulus. If the second target comes in between 50 to 100 ms, the effect will be the strongest (Alpern 1953; see Bachmann 1994 for a review). One thing we need to acknowledge immediately is that in the Sperling case, the delay can be much longer than 100 ms, so what we observe in this simple case cannot be applied directly to the Sperling case. Phillips then cites a study by Weisstein and Wong (1986) for the possibility of longer delays (Phillips 2011: 387-8). The second important thing to be recognized is that we need *cross-modal* postdiction, as mentioned above. For this, consider 'sound-induced visual bounce.' See this following figure:

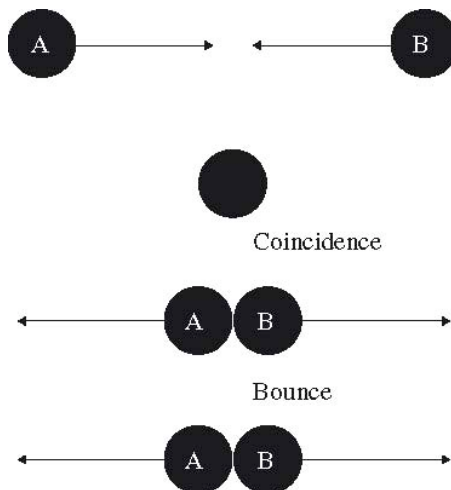**Figure 3**. Ambiguous display in Sekuler, Sekuler, and Lau (1997)

A and B move towards each other, briefly coincide, and finally move away from each other. Participants either see them as bouncing and reversing directions, or see them as pass through each other without changing the original directions. If a sound is played at the point of coincidence, or close enough, participants more often see them as bouncing.

Even when the sound is played with a 150 ms *delay*, the effect is still there (Sekuler, Sekuler, and Lau: 1997: 308). Again, the temporal window here is not big enough to apply to the Sperling case. However, in a later study Watanabe and Shimojo (2001) shows that 'though a sound presented 300 ms after visual coincidence does not induce bounce, the "bounce-inducing effect was attenuated when other identical sounds (auditory flankers) were presented 300 ms before and after the simultaneous sound" (109)' (Phillips 2011: 390).

The descriptions provided in this section so far is certainly not detailed enough to support the cross-modal postdiction interpretation for the Sperling case. But since the view presented here is not original, as I acknowledged at the beginning of this section, I do not wish to repeat what the original author says too much. Readers are encouraged to read Phillips' 2011 paper themselves. Here what I provide is only minimal to motivate the view and give it some initial plausibility. What is more important here is to say more about the three things I promised also at the beginning of this section, namely the three reasons why I am still developing the view given that I endorse the main line from Phillips.

First, I believe under suitable interpretation, the cross-modal postdiction view is compatible with a weak version of OVERFLOW. Recall Block's strong version of the view: before the cue, we have specific phenomenology for all or almost all letters. As we have seen, this view is not well supported by his arguments. Some of Block's opponents go for the other extreme and argue that there is only generic phenomenology before the cue, for example Kouider et al. 2010. However, this view does not seem to do justice to the fact that in the trials in which there is no cue at all, the participants can still report about 4 letters accurately. If without the cue we enjoy only generic phenomenology, it is not clear how we can accommodate the full report condition. Now, even if the cross-modal postdiction interpretation is correct, we can and should still hold that before the cue, the participants will have specific phenomenology, though not for almost all letters, but for perhaps 4 letters or so. The reason is that before the cue, given the fixation point and the pre-cue attention, some parts of the visual field will still be more salient than other parts. But overall, the pre-cue phenomenology still overflows what attention can later capture, since before the cue there is also generic phenomenology. In a sentence, something between Kouider and Block is right (i.e., there are specific phenomenologies before the cue, but not as much as Block supposes), and this in-between view sits well with the cross-modal postdiction interpretation.

Secondly, I would like to strengthen the cross-modal postdiction view with some other empirical studies. In order to do so, I will need to digress a bit first. The conceptual resources for this view are mainly from works by Morten Overgaard and his colleagues. In what follows I first introduce the view, then explain its plausibility, and finally connect it to the current debate.

The leading question for Overgaard et al. is this: 'Is conscious perception gradual or dichotomous' (Overgaard 2006, 700)? According to Sergent and Dehaene (2004), it is dichotomous. The idea is that there is a clear threshold for being conscious. In showing this, Sergent and Dehaene design a scale consisting of 21 nodes, with the two extremes defined as seen or unseen. They then use this scale in experiments of attentional blink, 'the phenomenon that the identification of a stimulus hinders an explicit report of a second stimulus if the two are temporally separated by between 200 and 500 ms' (Overgaard et al. 2006, 701). What Sergent and Dehaene found is that subjects report in an all-or-none fashion: they are either conscious of the second stimulus, or not conscious of it at all.

As Overgaard et al. point out, however, this result from Sergent and Dehaene is flawed. For one thing, as Sergent and Dehaene themselves notice, subjects' reports show a more continuous pattern when the experiments are purely about masking, i.e., are not combined with attention blink. For another, and more importantly, the Sergent-Dehaene scale is biased: only two extremes are explicitly defined, and there are too many nodes (i.e., 19) in between (Overgaard et al. 2006, 702). To see the problem, recall whatever demos with short stimuli you have seen before, and ask yourself: is it possible to decide in a given case, the degree of conscious awareness is, say 6, as opposed to 8? If subjects have a feeling that they clearly see the stimuli, does it make sense to choose, say 18, as opposed to 20? It is predictable that given the design of the scale, subjects will tend to give all-or-none verdicts.

In order to make plausible the view that consciousness is a gradual phenomenon, Overgaard et al. propose another scale of measurement, Perceptual Awareness Scale (PAS). It is 'a four-point scale categorized as "not seen," "weak glimpse" (meaning "something was there but I had no idea what it was"), "almost clear image" (meaning "I think I know what was shown") and "clear image"' (Overgaard et al. 2006, 702). The improvements are, first, all nodes are clearly defined, and secondly, there are only two options between the two extremes. With this scale, subjects report in a 'continuous

manner' (ibid.). With these two improvements, PAS is better than the Sergent-Dehaene scale, and PAS shows that consciousness is gradual, not dichotomous.

The results from Overgaard et al. can be used to sustain the view I shall call COVARIANCE. On this view, the degree of cognitive access tracks the degree of phenomenology. This view requires that both access and phenomenology come in degrees, and that they are covariant. As we have seen, it is plausible to think that phenomenology is a gradual phenomenon. How about cognitive access? Interestingly, when Block discusses this issue, he also mentioned the Sergent-Dehaene view discussed above (Block 2007, 533), and conclude that '[c]ognitive access appears to be more of a binary phenomenon…' But we have seen that the Sergent-Dehaene view is untenable. However, Block is right in gesturing that although Sergent and Dehaene attempt to measure consciousness, the scale itself also reveals reportability and access. Since reportability requires access, scales with right design, such as PAS also reveal degrees of access. With good scales, we are able to find out subjects' degree of access, and its corresponding degree of phenomenology. The very idea of COVARIANCE underlies this kind of measurement.

Now let's go back to the original discussion. Before the digression, we have reached the interlude conclusion that in the Sperling case, the cross-modal postdiction interpretation is plausible, and it is compatible with another plausible view, namely weak OVERFLOW. The view COVARIANCE chimes with those two views as well, since it embodies the plausible view that consciousness is a gradual phenomenon. Generic and specific phenomenology do not form a dichotomy; they instead constitute a continuous spectrum. This sits well with weak OVERFLOW because the latter has it that there is a transition between generic and specific phenomenology. If those two kinds of phenomenology are strictly speaking different in kind, then it is harder to see how that transition can happen; as Block emphasizes, the participants never report that there is a transition of phenomenology. So the three views argued in this section cohere with one another. In addition to the independent reasons for each of them, as we have seen above, the coherence between them is itself a further reason to believe in all of them. They form an inter-supporting triad.

This completes my discussion of Block's OVERFLOW debate. In next chapter I apply the overall outlook developed here to a different but related debate between Fred Dretske and Michael Tye over the so-called 'change blindness' phenomenon.

CHAPTER 2

Change Blindness or Change Inaccessibility?

Speckled Hen Revisited

2.1   Change 'Blindness' and Its Interpretations

'Change blindness' is a relatively new set of phenomenon discovered in recent years. The phenomena are so controversial that theorists cannot even agree on the name of it, as we shall see presently. It is important to compare it to the Sperling-style case since it is also closely related to attention, working memory, and so on. In this section I will briefly introduce the set of phenomena. However, I will not dwell on the huge psychology literature too much since it is beyond the scope of this essay. Just as in chapter 1 I did not set myself to conduct a comprehensive discussion of Block's case, in chapter 2 I do not wish to cover everything concerning change blindness. The primary aim of this essay is to understand some interactions between consciousness and attention, and every topic I touch on along the way is built up for that aim.

As hinted just now, it is a set of phenomena, rather than a single phenomenon, that is under the name of 'change blindness.' We have at least those focusing on saccades (Grimes 1996), on attention (Simons and Chabris 1999), and on masking (Rensink, O'Regan, and Clark 2000). Sine this essay is primarily on attention, I shall discuss the phenomenon identified by Simons and Chabris 1999. But again, we should acknowledge that the boundaries between those cases are always fuzzy.

In one study, the experimenters asked the participants to count how many times the basketball is passed by those who are in white t-shirts. There is another team in black

t-shirts that serves as distracters. During the task, there is an experimenter either in a gorilla suit or in normal outfit with an opened black umbrella crossing the scene. Since the participants concentrate on the basketball if they abide by the instruction, about half of them do not detect the experimenter with the gorilla suit or the umbrella.

Figure 1. For a demo, see http://www.youtube.com/watch?v=vJG698U2Mvo.

What has happened in this kind of scenario? There are at least two hypotheses here. The first has it that the participants literally did not see the gorilla suit or the umbrella; the second has it that the participants did see the target, but for some reasons the phenomenology is not accessed. If one holds the former hypothesis, one is willing to call the case change *blindness*. If, on the contrary, one holds the latter hypothesis, one wants to call the case change *inaccessibility* or *amnesia*. Inaccessibility and amnesia are not identical: pieces of information can be inaccessible or not accessed *because of* amnesia, but they do not have to be. We can say that change amnesia is a version of change inaccessibility. Since in this essay we concern about access and accessibility, in what follows I focus on the contrast between change blindness and inaccessibility. This distinction roughly corresponds to Daniel Dennett's distinction between 'Starlinesque'

and 'Orwellian' interpretation (1991) and Michael Tye's distinction between 'representational' and 'comparison' failure (2010). In order to keep the discussion focused, in what follows I do not use those other terminologies, since after all they are not exactly the same given different theoretical presuppositions of individual philosophers. In section 2 I evaluate a recent debate between Tye and Dretske, since both of them have something to say about the Sperling case and it is helpful to think about their views in a different context. In section 3 I apply the set of view developed in chapter 1 to the Dretske-Tye debate.

2.2   Dretske for RICHNESS versus Tye for SPARSENESS

Recall Block's position for the moment. He holds that phenomenal visual sensory memory *overflows* what attention can capture. Another way to state the thesis is to say that he thinks phenomenology is *richer* than access. Call this the rich view. His opponents generally hold that phenomenology is *not* richer than access. Call this the sparse view. Below I will consider the Dretske-Tye debate with this pair of terminology. As we shall see, Dretske holds a version of rich view, but according to his view our phenomenology is even richer than Block supposes. I shall label Dretske's position the *hyper-rich* view.

Dretske has been arguing for the hyper-rich view for many years and with many examples; here I focus on his example of collections of balls (2010: 66, 67; figure 2 and 3 below). Take a look of the following two figures first:
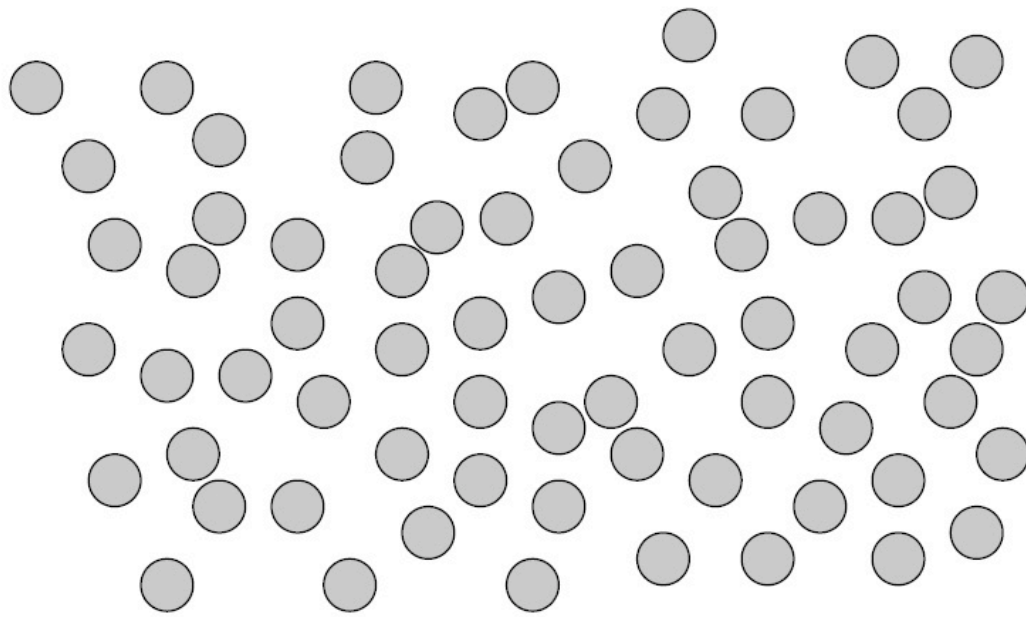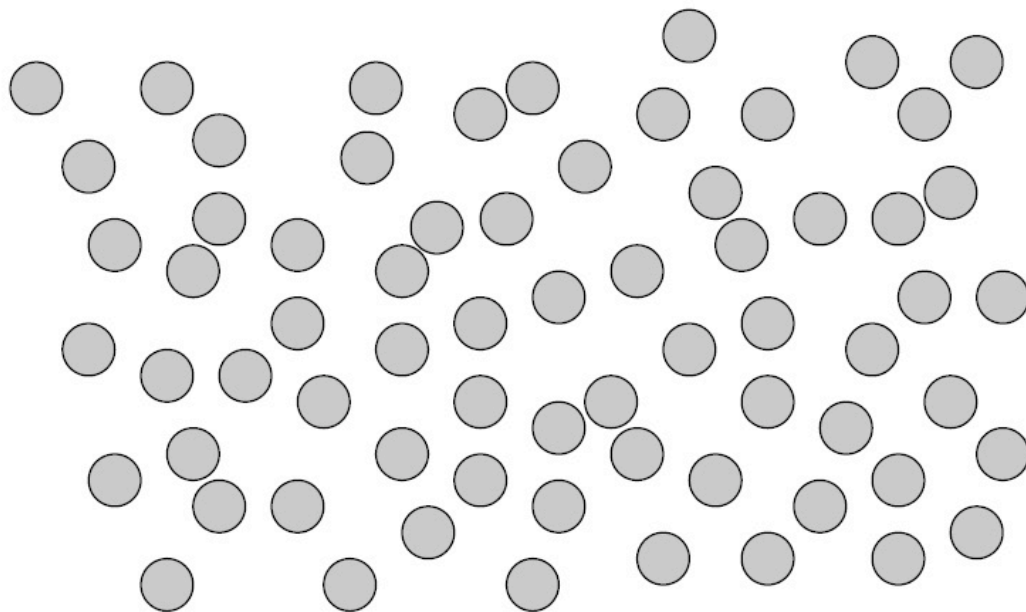
Figure 2

Figure 3

Although in conducting this discussion, Dretske is explicitly arguing against the change 'blindness' interpretation (as opposed to the inaccessibility or amnesia one), his examples are always static, which is very different from what we see in the psychology literature. I

will stick to these static examples because I think they provide a nice contrast to the Sperling case, in which timing is crucial. In the examples here, what's more important is the relevant spatial arrangement. Another way to think about the debate here is to connect it to the old speckled hen debate in philosophy. The speckled hen problem was suggested to A. J. Ayer by Gilbert Ryle, and made popular by Roderick Chisholm (1942). It was originally tied to discussions about foundational knowledge and the sense-datum theory, but nowadays it provides a useful way to think about visual phenomenology.

Dretske believes that we consciously see 'a lot of the detail' (ibid.: 54), and he substantiates this claim by referring to our experiences to figure 2 and 3. The first point to be stressed is that in this kind of folk experiment (i.e., readers see the figures by themselves and reflect upon their own experiences), as opposed to real empirical experiments done in labs, we need to be extremely careful about how we describe the situation. As Bence Nanay complains, however, in many cases the folk experiment are simply 'under-described' (Nanay 2009: 501). This is what I find in Dretske's way of describing the cases. He asks readers to 'look, for just a moment,' at the figures he offers, and claims that 'a quick glance (one or two seconds) is enough to see *all* the balls…' (Dretske 2010: 59, my emphasis). This is highly problematic. In two seconds, we will have six to eight saccades, and we can do many voluntary eye movements if we want. Presumably, the reason why Dretske is so flexible about his descriptions is that he thinks even if we do it very quickly, we will still see much of the detail, but this causes a problem: Dretske provides absolutely no constraint on fixation points and where to direct one's attention, and in two seconds we can see very much by moving our eyes and directing our attentions. Given this flexibility, no wonder we see so much detail even when there are more than forty balls in front of us. What's more, how far should we view the figures? As Nanay (2009: 501-2) points out, distance does matter; if you view these figures from, say, two feet away, the texture of your experience will be very different from the experience you get when viewing it from five inches away. Given Dretske's presumptions that we read the book from normal reading distance for two seconds, it might be fair to say that we see much of the detail, probably every ball. This is not controversial, but at the same time *not substantial*: given that you have two seconds with no required fixation point, no wonder you can see all the details. Even a sparse theorist can agree with this.

Although Dretske's discussion is based on insufficient constraints, the figures he uses do provide a good way to conduct further discussion. Let's make the folk

experiment more precise first by the following stipulations: fixate at the middle of figure 2 or 3, with both eyes, from ten inches away, for whatever seconds you like. The reason for *not* constraining the time is this: given where you fixate and how the array is arranged, the visual phenomenology is fixed, no matter how long you look at it (let's assume for the moment that your attention goes with your fixation in the present case). I criticized Dretske that he does not make clear how long we should look at the figures *because* he also does not require the readers to fixate at a given point, and the readers can thereby move their eyes a lot during the time (e.g., two seconds), and thereby see almost all the details. In my stipulations, given that we fixate at a point, the temporal dimension becomes unimportant since simply prolonging the time we look will not enhance peripheral vision. Again, this provides a nice contrast to the Sperling case, which depends on both spatial and temporal dimensions

Given the stipulations above, some readers might consciously see the difference: figure 2 has an additional ball on the right hand side. But how about fixating at the left edge of the figures? If we do so, then even if we now know where the difference lies, we are not able to see it consciously. We cannot even consciously see the middle part of the balls clearly: if we can, then we can count them, but we cannot count them, therefore we do not consciously see them individually. It should be clear that if we have more satisfying descriptions of how we conduct the folk experiments, Dretske's case for rich phenomenology could not be made.

Apart from the considerations of figure 2 and 3, there are other reasons to doubt Dretske's proposal. For example, it has become a piece of educated commonsense that when we read texts we can consciously take in only a small part of the texts; the impression to the otherwise is simply a confabulation (this has been shown by the 'moving window' experiment by McConkie and colleagues 1975 and 1979). Dretske's view seems to commit an overly detailed visual phenomenology: a clear, detailed, high-resolution snapshot (Tye 2010: 432; Noë 2004: 35). This picture is outright incompatible with what we now know about vision, and it seems to be unmotivated even from Dretske's own point of view: as a representationalist (e.g., 1995), it is better to deflate phenomenology to facilitate the explanation of it through representations, but what Dretske's did is the opposite.

Maybe what Dretske has in mind is a thin notion of conscious seeing. Consider an object in the periphery of your visual field. Perhaps you could not report its shapes, colors, and other properties accurately, but you could still report its rough location, e.g.,

upper or lower. If the notion of conscious seeing is that tenuous, maybe we can grant Dretske a more detailed visual phenomenology. However, this still cannot give Dretske what he wants, that we 'see *all* the balls' (quoted above), since in the periphery many balls simply collapse together in visual phenomenology, and we cannot even report a *specific* ball's location, let alone its more fine-grained properties. Of course, if a given ball is in a very different color, say red, then we will be able to consciously see it. Nevertheless, though we will definitely see that pop-out ball, we still cannot see other balls separately. If you turn all the balls red, then no ball is popping out; as a result, you will not see any of them individually again. Even with this thin notion of seeing, Dretske's proposal of excessive phenomenology should still be rejected[1].

In addition to put forward his own positive proposal, Dretske also argues against positions like mine. Let me consider the major one. Dretske thinks that positions like mine commit a failure to see the distinction between seeing *facts* and seeing *objects*. Here is the alleged fallacy:

1. S does not see (detect, notice) the difference between A and B.
2. Therefore, S does not see (the fact) that they differ.
3. The difference between A and B is x (some object) or P (some property).
4. *Therefore*, S does not see x (or P) -- the object or property that makes A and B different. (Dretske 2010: 58; my italics)

Dretske argues, correctly I believe, that 4 does not follow from previous steps, given the distinction between seeing facts and seeing objects. However, my case for the view that we do not consciously see many things in our visual fields does not rely on this inference. I agree with Tye that given certain fixation point, even if we direct our attention to the periphery, still many details do not enter our phenomenology (more on this from the next paragraph). In other words, there are some independent reasons for my position; the change blindness fallacy is not invoked in the above argumentation.

This of course does not mean that we have rejected Dretske's view. At most, we have found faults in his major argument for the view. But this is enough for our purpose, since again the main aim of this essay is to understand some interactions between

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[1] This line of possible response was suggested by Dretske in conversation; it seems to be more concessive than the materials in print, though I still think it does not work for reasons just stated.

consciousness and attention. It is enough to note that a certain suggestion from Dretske does not work. Now I turn to Tye's relevant view.

Here we face some exegetical issues. In arguing against Dretske, Tye is clearly a sparse theorist (2010). Also, when he comments on Block's view on the Sperling case, he writes:

> [W]ith respect to each letter in the array, the subject's experiences enable them at least to wonder 'what is that letter?' Their experiences thus put them is a position with respect to each letter, to bring the letter under the demonstrative concept *that*. (Tye 2007: 527-8).

He then concludes from this that 'cognitive accessibility…*outstrips* phenomenology, not the other way around' (ibid.: 528, my italics). However, in his 2006 paper Tye explicitly use Sperling's case for a version of the *richness* argument. After 2006, he never indicates that it is a change of mind. How should we make of this?

The situation is not as complicated as it might initially appear. Recall that in the section on 'what overflows what?', we identified many items as candidates of what's overflowed. Given different items, together with different overflowers, one philosopher might accept or deny overflow. In both Tye 2006 and 2007/2010, the overflower is phenomenology. However, in 2006 the item being overflowed is concept (the main aim of that paper is to argue against conceptualism), while in 2007/2010 the items are demonstrative concepts and attention respectively. Although demonstratives are a kind of concept, as many theorists hold, they are arguably less demanding than full-fledged concepts. As Block (2007b: 539) and many others point out, demonstration requires attention. Therefore, Tye's 2007/2010 view is more relevant to our discussion. His 2006 view has not been abandoned; it is on a related but distinct topic.

Tye provides a test of phenomenology: 'if I am conscious of [a thing] then it must be marked out or differentiated in the phenomenology of my experience' (Tye 2010: 413). This is congenial to my point that in the case of figure 2 and 3, since we cannot mark out the crucial difference, we are not conscious of individual balls in the periphery. This seems to be perfectly intuitive. In Tye's terms, we can consciously see the things in our peripheral visual field *collectively* but not *distributively* (ibid.: 415-6): we cannot consciously see the individual balls there, since we cannot consciously mark them out. Again, Dretske might reply that we see that all the balls are grey, since if one ball were

red, we will surely mark it out. In addition to the explanation offered at the end of last section, this might also be explained by inferential knowledge: *if* there were a red ball, the overall phenomenology will be drastically different; *since* this does not happen, we naturally infer that every ball in the periphery is grey. Dretske needs to provide independent reasons to rule out this natural explanation. Now consider this figure:
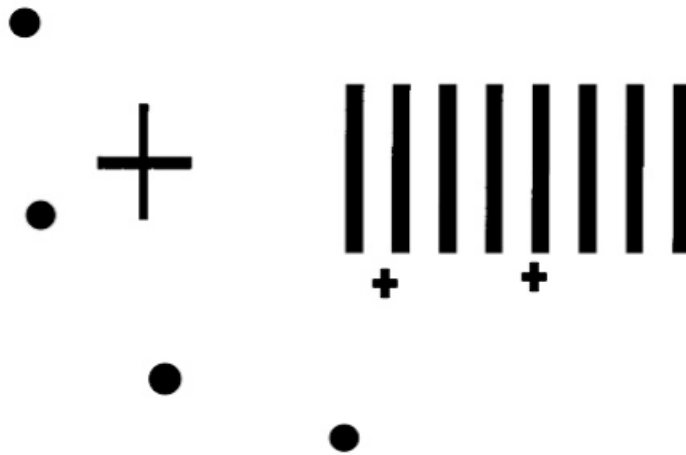
Figure 4. From Tye 2010

Tye argues, correctly I believe, that what you consciously see depends crucially on where you fixate and where you attend. If 'your eyes track down from the top black spots to the lower black spots' (ibid.: 417), what you consciously see in relation to the bars on the right hand side will be very different from the case in which your eyes track the plus signs from left to right. Fixation and attention matter, and if we bear this in mind, Dretske's excessive phenomenology picture can be firmly resisted.

What, then, is my view different from Tye's? To see this, we need to consider his response to the Sperling paradigm when criticizing Block, as quoted two pages above. Let's apply this view to figure 4. If you fixate at any spot, as opposed the pluses, you will find your peripheral experiences of bars indeterminate. But with respect to certain bars, say the most remote one, you could still ask yourself the question, 'what is that?' That is, you could differentiate it in your peripheral phenomenology. According to Tye, given that you could ask that question with respect to at least certain bars, you have access to those bars. However, your phenomenology with respect to those bars is arguably very sparse, so it follows, Tye urges, that accessibility overflows phenomenology.

But it does not follow. Accessibility, like phenomenology, comes in degrees. We can agree with Tye that when we are able to ask the 'what is that?' question, we have certain amounts of access with respect to those objects. However, this does not mean that our access in the periphery is the same as the access within the fovea region: we can also ask the 'what is that?' question with respect to objects falling within the center of our visual fields, but that does not mean that our access there is the same as our access to the periphery. Arguably, access to the periphery is much lower than access to the center, but if that is so, Tye's contention that the access overflows the phenomenological is ungrounded.

Worse still, the very idea of 'cognitive accessibility overflows phenomenology' is problematic conceptually, no matter how we understand accessibility. To access something is to access something out there; in the present case, what is out there is phenomenology (our there in our minds, not in the world; there is a distinction between mental appearance and mental reality). Now what does it mean to say one can access more than what is actually out there? It does not make clear sense if we agree that 'access' is factive: one can access O only if O is actually out there in the relevant sense. To insist cognitive accessibility overflows phenomenology is like saying that epistemology overflows metaphysics, e.g., one can see more than what is out there in the world. In this kind of case, we say that the subject is hallucinating, not genuine seeing. If the subject reports more than his phenomenology, then what we should say is that he misreports, not that his cognitive accessibility overflows phenomenology.

This completes my discussion of the Dretske-Tye debate. In section 3 I shall apply the view developed in last chapter to the current topic.

2.3   COVARIANCE: Extending the Analysis

Let me restate the set of view I arrived in chapter 1:

a) Cross-Modal Postdiction: In the Sperling-style case, the auditory cue retrospectively modulates visual sensory memory so that relevant generic phenomenology becomes more specific.

b) Weak OVERFLOW: Before the cue, we do have specific phenomenology for some letters (typically around 4), contra Block's most opponents, but Block is

wrong to think that we have specific phenomenology for almost every item. Phenomenology does overflow attention, but not as much as Block thinks.

c) COVARIANCE: After the postdiction effect, the degree of phenomenology does not overflow the degree of attention anymore, since after the effect much phenomenology has been dropped out.

Since examples in this chapter are all static, as indicated above, postdiction does not apply. I shall then explain how Weak OVERFLOW and COVARIANCE apply in this current debate. But first I shall say why other views are unsatisfactory. Consider again the figures containing collections of ball above. Dretske's hyper-rich view has it that the participants see almost every ball. As we have argued, he reaches that conclusion only by falsely assumes that fixation an attention do not matter. Block's rich view predicts a similar result, though what exactly he wants to say on this case is unclear, since he only discusses cases such as Sperling 1960 that involves transient timing. However, it should be clear that he would hold a view similar to Dretske's, given his commitment to OVERFLOW. Since we have casted doubt on both Dretske's and Block's argumentations respectively, we can tentatively reject the rich views. How about Tye's sparse view? It is closer to my overall outlook, but given that I do not find the thesis that 'accessibility outstrips phenomenology' intelligible, his view should also be rejected.

Here are some positive remarks. According to weak OVERFLOW, if we fixate at a given point in figure 2 or 3, we will have an overall generic phenomenology of the picture, with some parts more specific, including the fovea region and some attended parts. After we are asked to focus on a given region without shifting the fixation point, the original phenomenology will be changed by the attentional shifts, and COVARIANCE has it that in performing this task, what you can access tracks your phenomenology. Exactly how this might work is not a question answerable from the armchair, but I have provided independent reasons for both weak OVERFLOW and COVARIANCE in chapter 1. In this section I simply apply the view to a new case. However, the details of the process need to be figured out empirically, and although the author is a philosopher, the intellectual duty should not be thereby avoided. I end this chapter with this acknowledgement. In chapter 3, I will situate the discussion into some larger contexts in order to further reveal the philosophical as well as empirical significance of the present study.

CHAPTER 3

Larger Contexts

Theories of Consciousness

3.1   Points of View from other Theories: HOT (Rosenthal), AIR (Prinz), and GWT (Baars)

In this section, I will discuss some points of view from those who also hold systematic theories of consciousness. The reason is this. In the forgoing discussion, it might look as if the interpretations of the Sperling case and the change 'blindness' case we discussed are side remarks from Block, Dretske, and so on. But in fact, though most philosophers try not to beg the question against their opponents in case studies, it is hard to make sure that they do not, simply because people are generally blind to their own assumptions. Here I discuss views from David Rosenthal, Jesse Prinz, and Bernard Baars in order to make those background assumptions salient. Rosenthal holds that 'mental states are conscious only if one is in some way conscious of them' (Rosenthal 2005: 3). Prinz holds that 'visual awareness derives from Attended Intermediate-level Representations (AIRs) (Prinz 2000: 249). Baars instead holds that 'global work space model [that explains consciousness]…is a distributed society of specialists that is equipped with a working memory, called a *global workspace*, whose content can be broadcast to the system as a whole' (Baars 1988, original emphasis).[1] All of them have something to say about the

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[1] In the past decade, Prinz keeps refining the view and the latest version can be found in his forthcoming book *The Conscious Brain*. Since the book has not come out, here I use his classical statement in the 2000 paper. Similar considerations apply to Baars.

Sperling case. In what follows I will evaluate their comments on Block and relate those comments to their own theories respectively. In due course, I will also consider other philosophers' views when needed.

I shall start with Rosenthal. Before considering his theory of consciousness, let's see how he thinks about Block's interpretation of the Sperling case:

> [S]ubjects identify all the items as alphanumeric independent of any cuing…So all the items are at least *partially conceptualized* independent of cuing, presumably before the stimulus ceases…Identification of specific letters might then rely *solely* on that conceptual information, rather than on persistent phenomenology. (Rosenthal 2007: 523, my emphasis)

He then quickly acknowledges that it is '*one* way,' not *the* way to explain it (ibid., my emphasis). Indeed, this is a *prima facie* possible interpretation that should be taken into further considerations. However, since on that occasion Rosenthal's piece is a brief comment in *BBS*, the relation between the Sperling case and HOT theory is not fully clear. Therefore the following discussion will be based on two other pieces: one is Lau and Rosenthal (2011) and the other is Brown (2011).

In Lau and Rosenthal (2011), they first articulate the empirical predictions of their view and of their opponent views respectively, and then argue that HO theories' predictions are better supported.[2] The most relevant part for our purpose is their view of peripheral vision. It is most relevant because in both the Sperling cases and the speckled hen cases, peripheral vision is crucial. Both in daily life and in experimental settings, we tend to inflate how much we actually see in the periphery of the visual field. The main study here is Rahnev et al. (2009). The phenomenon is called 'inattentional inflation.' We know that peripheral vision has much lower spatial resolution and color sensitivity. Given this, it should be acknowledged that our peripheral vision is fuzzy, blurry, and relatively colorless. Now consider the following two pictures:

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[2] Lau holds a different HO theory called 'higher-order statistical inference view' (2008). It is mainly based on the distinction between d' and perceptual certainty and related considerations. I do not discuss this specific view here.
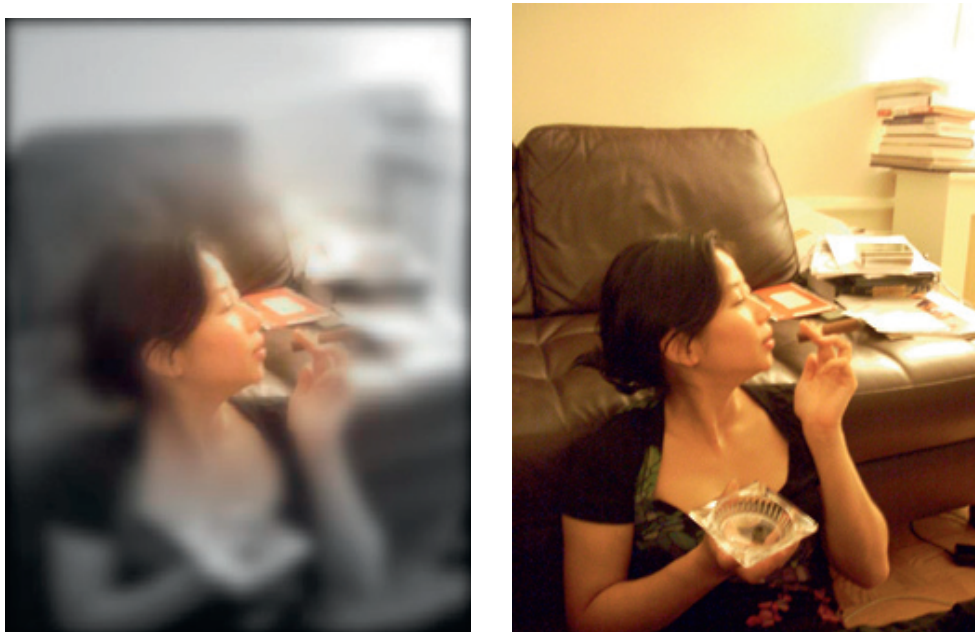
Figure 1. From Lau and Rosenthal (2011)

We tend to *think* or even *feel* that our visual field is like the right hand side picture, but actually it should be more like the left hand side one, given what we know from physiological studies and from the psychophysical study from Rahnev et al. (2009). Now let's see how Lau and Rosenthal use this finding to support the HO views:

> On the higher-order view, one possibility is that under the lack of attention the higher-order system might mistake noisy early input as reliable signal: it might produce more false alarms and give higher confidence even when the task performance for unattended objects is not higher than for attended objects. (Lau and Rosenthal 2011: 369)

And in the Sperling case, they argue that

> On our view, subjects' sense that they consciously see the identities of all 12 letters is unwarranted, and is probably due to the phenomenon of 'inattentional inflation of subjective perception' described above. (ibid.: 369)

I think the main empirical case for 'inattentional inflation' is solid. The question is whether it supports the HO theories. On the face of it, I think it does, and we need more

follow-up studies and discussions for both sides. Here I want to cast doubt on a side issue, that how much we actually see in the periphery. The relevant physiological result is not questionable: we indeed have much less rods and cons in the periphery of our retina. However, if we ask ourselves to fixate at a given point and reflect on our conscious experiences carefully, though we do find out that the only high-resolution region is foveal, which is only as big as a quarter dollar roughly speaking, we do not see the periphery as blurry or colorless. It is true that we have very bad discriminatory abilities concerning colors and other properties there, but it does not look blurry and colorless. Lack of information is not identical to information of lack. To be sure, this might not affect the main point made by Lau and Rosenthal, but I believe it is a significant point for further discussion. Now I turn to another defense of HOT by Richard Brown (2011).

It is worth noting that Brown makes a similar prediction about peripheral vision: 'it is similar to what it is like when things actually are fuzzy' (Brown 2011: 4). To repeat, I do not agree with this but it seems that this does not affect their main points. What, then, is Brown's positive view on the matter? He writes that the 'higher-order camp' has it that:

> What it is like [in seeing the Sperling stimuli] is like having the experience of a bunch of indeterminate letters arranged in a grid. There is no (conscious) specific content in these cases, on the Rosenthal model…the specific content is the content of the first-order states but we are only conscious of them in some respect but not others. (ibid.: 3-4).

HO theorists then also provide theories of first-order states, for example Rosenthal's 'homomorphism theory' (Rosenthal 1991, 1999, 2005b). Obviously I cannot go into this here, but it is sufficed to say that HO theories have a coherent and to some extent plausible story to tell when it comes to the Sperling case. It is therefore important for us to bear them in mind in further evaluate the whole debate. Now I turn to Prinz's view.

Prinz's comments in *BBS* is very condensed, so I will just focus on this piece as opposed to considering his more positive statement of his own theory. He starts by drawing a three-way distinction between inaccessible, accessible, and accessed. Prinz's own theory holds that '[only] attended representations are accessible' (Prinz 2007: 522). Given this, unattended representations are inaccessible. And he and Block agree that a representation is accessed 'if it has been encoded un working memory (or encoded in a

"global workspace" in frontal cortex)' (ibid.: 522).[3] The kernel of Prinz's AIR is that consciousness requires attention (and *vise versa*), so he must hold that consciousness requires accessibility. This means that he thinks that phenomenology does *not* overflow accessibility. How about access then? He says that 'Block correctly concludes that phenomenology outstrips working memory encoding [i.e. access]' (ibid.: 522). So in a word, Prinz thinks that phenomenal consciousness overflows access but not accessibility. Now recall the scheme I sketched in chapter 1:

Sensory Memory » Attention » Access » Working Memory » Report

The term 'accessibility' does not figure in it explicitly, but it should be clear that it goes with attention: the stimuli come in and participants' endogenous or exogenous attention kicks in, make certain information *accessible*, and then working memory retains those that has been actually *accessed*. Those pieces of accessed information then sustain reports. This picture agrees with Prinz's identification of attention and accessibility. However, it is not clear that Block agrees with this usage. In his reply, he writes that 'Lycan asks me to explain how there could be a form of awareness that is intrinsic to consciousness but does not involve *attention or cognitive access*' (Block 2007b: 540). In this context, Block is sensitive to the distinction between accessibility and access, since this is where he replies to Prinz too (i.e., in the same sub-section). Therefore, we can safely conclude that he identifies attention with access, rather than accessibility. As indicated in chapter 1, it is notoriously difficult to pin down what different theorists mean by attention, and here although we are relatively clear about what they mean, i.e., Prinz means accessibility, which I agree with, and Block means access, and so on, it is not easy to provide reasons for one usage and against the other one. Here I bracket this controversy and go on to consider Prinz's other remarks and Block's reply to them, but readers should bear in mind that the relations between attention, accessibility and access have not been settled.

Prinz's next important point is that 'Sperling cases involve accessibility without (complete) encoding [i.e., access], and inattention cases render stimuli inaccessible' (Prinz 2007: 522). For Prinz, the Sperling case and cases such as inattentional blindness, attentional blink, and extinction are radically different. 'There is no reason to postulate phenomenology under total inattention, [but] overwhelming reason to postulate

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[3] Here Block's terminology is 'narrowly accessible.' As Prinz points out, this term is misleading and it is more accurate to call it 'accessed' (Prinz 2007: 522).

phenomenology in Sperling cases (and cases of change blindness)…' (ibid. 522). Let's see what Block has to say about this. First, Block thinks it is not the case that 'attention is necessary for accessibility, that is, potential broadcast':

> Dehaene and his colleagues (Dehaene 2006; Kouider et al. 2007) have convincingly shown that in conditions in which attention is maximally drained away by other tasks, representations that are as unattended as can be ensured by such conditions are nonetheless very strongly activated. (Block 2007b: 540)

The point is that some representations are unattended but still potentially broadcast. Block soon acknowledges that 'it is difficult to make absolutely sure that there is no attention devoted to a certain stimulus,' but correctly insists that 'this evidence points away from both of Prinz's claims' (ibid.: 541; the second claim will be assessed presently). Here Block's point is fair: in doing sciences, there is basically no decisive evidence, since alternative interpretations are always possible, as discussed in the introduction. This is partly why easy problems are not easy. However, Prinz can still say that his position has not been ruled out, and given that he offers other positive reasons for the position (e.g., Prinz 2010), his view is plausible to some extent. I leave this point here and turn to the second disagreement.

The second point is Block's insistence on the dissociation of attention and phenomenology:

> On attention without phenomenology, subjects' attention is drawn by nudes of the opposite sex (for heterosexuals) in conditions in which the nudes are invisible according to experimental standards (Jiang et al. 2006)…Kentridge et al. (1999) have shown the effects of attention in blindsight. On phenomenology without attention, again in a 'dual task' paradigm, subjects are able to see and report certain kinds of scene gist. (e.g., the presence of a face in the periphery). (Block 2007b: 540-1)

Prinz has something to say in response to the study of Jiang et al. 2006. He provides an alternative explanation that 'the nude attracts saccades and not attention' (Prinz 2010: 326). He then offers two reasons for this alternative. First, Jiang et al. uses a technique 'interocular suppression' in which 'the high-contrast stimulus is able to attract attention

away from the low-contrast stimulus' (ibid.: 327). Since in the study the nude pictures are low-contrast stimulus, there are reasons to believe that they do not attract attention. And secondly, 'functional MRI studies of interocular suppression suggest that suppressed stimuli are not in fact associated with increased ventral processing [where object representations are processed] (Fang and He 2005). The increases are observed in the dorsal stream, which plays a role in saccadic eye movements and spatial perception' (ibid.: 327). It seems to me that this pair reply is cogent, though of course non-decisive.

What about blindsight? Blindsight patients can certainly pay attention to her or his blind field but has no consciousness there. This is a hard case. My tentative answer is that it is not entirely clear that patients can really pay attention to those given locations. Close one eye and try to pay attention to the black field, and you might feel that it is not clear that we can really do it. Of course this is far from a sufficient response, but in any case, Block has not shown that in the case of blindsight we really have attention without phenomenology. On the study of phenomenology without attention, as discussed above there is no way to ensure that there is absolutely no attention, but Block makes the fair point that we can still say that evidence points away from Prinz's view.

I shall end this section by briefly consider the global workspace theory. On the face of it, it looks a lot like the AIR theory. Here is the crucial difference: 'On the global workspace theory, consciousness requires working memory *encoding*' (Prinz 2010: 322, my emphasis). Remember that Prinz thinks that phenomenal consciousness overflows access but not accessibility. Crucially, GWT holds that phenomenology does *not* overflow actual access. In this respect, it makes a stronger claim than AIR.

Baars himself replies to Block as well (with Murray Shanahan), so let's take a look. Baars has two main points: first, Block 'relies on an outmoded and imprecise concept of access,' and secondly, he 'perpetuates a common misunderstanding of GWT that conflates the global workspace with working memory' (Shanahan and Baars 2007: 524-5). In the response, Baars describes his preferred way of using the notion of 'access,' which goes something like this:

> The overall pattern of information flow alternates episodes of broadcast with bursts of competition for workspace *access*, and the typical duration of discrete episodes of broadcast is on the *100 msec* scale. (Shanahan and Baars 2007: 525, my emphasis)

The key point is the temporal window. Later Baars and Shanahan write that given 'the more precise and temporally fine-grained technical term found in the contemporary GWT literature,' experiments cited by Block do not support Block's conclusion.

Admittedly, there are many substantial issues behind the scene, but the direct point seems to be verbal. GWT is a very complicated empirical theory, and it is not clear that how much we can learn from that brief discussion of their preferred notion of access. Similar situation occurs in the second point. Baars and Shanahan write that 'presence in the global workspace *enables entry to working memory*' (Baars and Franklin 2003, original emphasis). That is, 'items can then be held in working memory unconsciously' (Shanahan and Baars 2007: 525). This point is indeed worth making, since we do need correct understandings of who means what when we communicate. However, as Block points out, this involves a verbal issue as well (2007: 539). Block uses the term 'working memory' to mean Baars' 'active working memory' (Baars and Franklin 2003). Block then goes on to explain the more standard model of memory described by Cowen (2005). To give this controversy a fair discussion will take us too far. But in any case, we need to bear in mind that in future discussions, to get clear a common set of terminology is the first and foremost thing to do.

This completes my brief treatments of competing theories with respect to the Sperling case. In the next section I will continue to evaluate other different perspectives. The main different of the targets there is that they all marshal their challenges in more skeptical ways.

3.2   A Grand Illusion (Dennett), an Unsolvable Puzzle (Schwitzgebel), or Let's Go out of Our Heads (Noë)?

If Dretske is holding the hyper-rich view of conscious experiences, then Dennett can be said to hold the hyper-sparse view.[4] Alva Noë attributes the 'new skepticism' to Dennett, which 'questions whether we even have the perceptual experience we think we have' (Noë 2002: 1). In another word, we suffer the 'grand illusion' that our consciousness is very rich and detailed. Gerald Edelman famously writes that '[O]ne of the most striking things about consciousness is its continuity' (1989: 119). Many people agree with him, but this is famously denied by Dennett:

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[4] In conversation, Dretske makes it clear that his position is mainly a reaction to Dennett. According to me, of course, it is an overreaction.

One of the most striking things about consciousness is its *discontinuity* – as revealed in the blind spot, and saccadic gaps, to take the simplest examples – The discontinuity of consciousness is striking because of the *apparent continuity* of consciousness. (Dennett 1991: 356, emphases rearranged)

After two decades or so, most theorists of consciousness have recognized what Dennett has emphasized. What is at stake then? Two things at least: first, there is a debate between Dennett and O'Regan & Noë over what laypeople really believe; O'Regan & Noë ask: 'is it true that normal perceivers think of their visual fields [as in sharp detail and uniform focus from the centre out to the periphery]' (O'Regan & Noë 2001)? And Dennett gives an affirmative answer by citing the fact that participants are very often surprised by the experimental results. This debate is not our main concern here; we do not here debate about subjects' beliefs, but their conscious experiences *as such*. So secondly, we need to see how Dennett conceives of our conscious experiences.

There is one place where Dennett speaks to this issue and these experiments directly. It is in his co-authored paper with Michael Cohen (2011). One main aim of the paper is to argue that 'it is the products of cognitive functions (i.e. verbal reports, bottom pressing etc.) that allow consciousness to be empirically studied at all' (Cohen & Dennett 2011: 358). On this ground they draw the conclusion that 'consciousness cannot be separated from function' (the title of that paper). Before entering the details of the paper, I want to point out that this motivation seems to be badly *ad hoc*: of course we want a science of consciousness and we want that science to explain everything about consciousness, but we cannot on this ground argue that consciousness is *actually* not separable from functions and access, given that only this view make empirical studies possible. To hold this view is to commit a too strong operationalism, and it begs the question against any version of OVERFLOW. But apart from this in-principle point, let's go into the details of the paper and evaluate their finer points.

What do Cohen and Dennett want to say about Sperling directly? Here it is:

Participants can identify cued items because their identities are stored *unconsciously* until the cue brings them to the focus of *attention*. Before the cue, participants are conscious only of the few letters they attend to and the impression that there are other items on the display whose identities they do not know. Once the cue is presented, they are able to access an unconscious representation before it decays

and successfully recall the letters presented. (Cohen & Dennett 2011: 359, my emphasis)

This is a common view among Block's opponents, and it is not without its plausibility. Indeed, I find this description very close to my own view, which says that before the cue we have a generic phenomenology and some specific phenomenology corresponding to our distributed attention. However, Cohen & Dennett only propose this as an alternative explanation without arguing for it directly. Given that alternative interpretations are always possible, mere pointing to the possibility is not very impressive.

The basic view argued in Cohen & Dennett (2011) is that there is no conscious experience outside the scope of attention. They rightly point out that in addition to focal attention, which is aptly captured by the metaphor of 'spotlight,' there are also many other versions of attention, including 'distributed, featured, spatial, internal, and so on' (ibid.: 360). Again, I find this view plausible and it is almost indistinguishable with my own view, but they do not offer direct arguments for it. They state them as if they are obvious facts and Block's view is ruled out by those facts. Although my view might be closer to theirs, I cannot agree with the methodology. Later in the paper, Cohen & Dennett reinforce the point that any version of OVERFLOW is ruled out because they make scientific theory of consciousness impossible. I would say that if that were the case, so much the worse for the science of consciousness. OVERFLOW, true or false, is a possible theory. To use the possibility of scientific study to rule it out is bad scientism. In this recent paper Dennett seems to retreat from the hyper-sparse view to a more plausible version of the view, but the methodology is not very convincing.

I shall now turn to another version of skepticism championed by Eric Schwitzgebel. In his recent published collected paper, *Perplexities of Consciousness*, one of his main aims is to argue that 'people in general know very little about what might seem to be obvious features of their stream of conscious experience' (Schwitzgebel 2011: ix). He asks questions such as 'Do you have constant tactile experience of your feet in your shoes?' And in general, he gives pessimistic answers. This question about tactile experience is especially relevant to our present concern: if you believe in OVERFLOW, you might think that the answer is yes, you do have conscious tactile experiences even when you do not pay attention to the given region, while the sparse theorists will incline to give the negative answer. Instead of talking about the rich versus the sparse views, Schwitzgebel uses 'abundant' versus sparse, but what he means by it is essentially the

same as what Block means by 'rich,' as the context shows.

What is the reason for this pessimistic attitude? Schwitzgebel first points out that people's intuitions diverse a lot in this field (ibid.: 92), and he also points out that simply dwelling on definitions of consciousness will not take us too far (ibid.: 93-4). I agree that these two points should be generally granted, and they should be obvious to those who have some familiarity with the consciousness literature in philosophy. What should we do then? This is where I find Schwitzgebel's approach not very helpful. He points out, again correctly, that '[w]e might look for empirical arguments favoring one view over the other – arguments that go beyond mere appeal to our intuitive sense of our own experience' (ibid.: 96). However what he immediately discusses are Simons and Chabris 1999 and Mack and Rock 1998. These are well known starting points, but it has long been recognized that those early methodologies are too crude (e.g., Stazicker 2011). This flies in the face of the fact that the whole discussion has been transformed after Block's series of paper starting from 2007. Notice that the paper from Schwitzgebel is also from 2007, so maybe an excuse is that he produces this independent of Block's relevant works and that is why this piece is not responsive to Block's recent works. This excuse cannot be the full story. The paper was later collected in Schwitzgebel's 2011 book, and he adds some new materials when doing so. For example he does refers to Block 2007 in page 98, but only in passing when he talks about the refrigerator-light illusion – a topic that can be discussed well independent of any reference to Block. Schwitzgebel also refers to Kouider et al. (2010) but again only in passing without any substantial discussion. What about the details in Block 2007? What about various critics of Block and Block's replies from 2007 to 2010? They are simply absent in Schwitzgebel's discussion. To be sure, good philosophy does not rely on references to certain works. However, if a detailed discussion of the topic is already available and the author knows that (i.e., he refers to it though without discussing it), and the author still tries to conduct his own discussion all by himself, and as a result the substance is entirely incomparable, it is not clear how serious his readers should be when evaluating the work. The three correct points made by Schwitzgebel – about intuitions, definitions, and empirical works – can all been made in year 2000, when works by Simons and Chabris 1999 and Mack and Rock 1998 just appeared. Therefore I conclude that Schwitzgebel's skepticism here is ungrounded, and the diagnosis is that he relies too much on outdated materials and overlooks new developments made by Block and many others.

A final line of skepticism to be considered is from Alva Noë, who has been

pushing the 'enactive approach' for more than a decade (e.g., 2002, 2004, 2010). The most representative work is *Action in Perception* (2004). In that work Noë targets what he calls the 'input-out picture' (chapter 1, *passim*). The experimental paradigm we focus in this present investigation – the Sperling paradigm – looks pretty much like one of the examples: the participants simply sit there without any movement, and report what they experience to the experimenters. If the enactive approach is correct, this input-output approach should be deemed obsolete. This is a huge topic that cannot be fully dealt with on this occasion. I shall focus on Noë's emphasis that what he calls *experiential blindness* can lend support to the enactive view.

'Experiential blindness' is supposed to be contrasted with the standard sense of blindness, which 'due to damage or disruption of the sensitive apparatus' (Noë 2004: 4). Experiential blindness, however, is 'due not to the absence of sensation or sensitivity, but rather to the person's (or animal's) inability to integrate sensory stimulation with patterns of movement and thought' (ibi,: 4). For a supposed real life example, Noë invokes the case of *Ganzfeld* (means 'entire field' in German; see Metzger 1930, Gibson 1979: 150-1). An easy version of it used half a Ping-Pong ball to cover each eye. Ping-Pong balls are translucent, so it induces special visual experiences. This is supposed to show that 'stimulation of the retina by light is not sufficient for vision' (ibid.: 4). In Noë's term, you have visual sensations, 'but they are bleached of content' (ibid.: 4).

I believe we should agree with Noë that stimulations stimulation of the retina by light is not sufficient for full-fledged vision. In other cases he discusses, for example patients recovered from cataract surgeries, human subjects do have visual sensations but hardly any meaningful visual perceptions (ibid.: 5). The question is whether this shows that the present approach – the Sperling paradigm which instantiates the input-output picture – is problematic. I submit that the answer is 'no.' Noë's project here is to show what is constitutive of or sufficient for full-blown vision, though he would not like this way of putting this. Assuming that he is right about the general line – that experiential blindness does exist and it supports the enactive approach – it does not follow that we should give up standard psychophysical experiments, like Sperling's one, for those experiments do not suppose that patients just recovered from cataract surgeries can perform those input-output tasks. Instead, the participants in those experiments already enjoy full-fledged vision. The whole psychophysical approach is compatible with Noë's enactive approach, at least in broad outline. But to be fair to Noë, in my reading he never tries to cast doubt on the standard psychophysical paradigm. What he attempts to show

is that if we are too used to psychophysics, we will lapse into certain pitfall, for example we might commit the wrong idea that sensorimotor skills are irrelevant to real life vision. If this is the correct interpretation, then the current project is compatible with, though not entail, the broadly enactive outlook.

3.3 Do Chimps Beat Humans in Memory Test?

I shall end this chapter by briefly consider a comparative study. No matter how one interprets the Sperling-style cases, an uncontroversial point is that people are not very good in the full report condition – where the participants need to report everything they see without being cued. As mentioned before, in this circumstance people can only report accurately about four letters. This is itself surprising without going into details about the partial report condition. However, our close relatives chimpanzees seem to do much better than us. In what follows I first describe a recent experiment and try to draw some implications from it.

The works to be discussed are Matsuzawa et al. on chimpanzees' working memory (2007, 2009). Their basic finding is that although chimpanzees might have worse memories in some respects, they nevertheless 'have an extraordinary working memory capacity for numeral recollection – better even than that of human adults tested in the same apparatus following the same procedure' (2007: 1004). I would say their description of chimpanzees' relevant capacities is too moderate; in fact, they are *much* better than both adult and young humans. Take a look of the demonstration then you will see (http://www.youtube.com/watch?v=zJAH4ZJBiN8). The subjects are six chimpanzees. They are all trained to use Arabic numerals, though five of them had no experience of using numerals in any task. The initial task is to touch the numerals with the 1-2-3-4-5-6-7-8-9 sequence. Once being touched, the given numeral will disappear, but this task is not very challenging since all the rest numerals will *not* disappear until being touched. Although not difficult, one interesting fact is that chimpanzees can do it very quickly, much quicker than average human beings – see the clip from 0:35 on. It seems that they have different ways of retaining information. But what is really crucial is that the later task in which all other eight numerals (2-9) will be *masked* after the first one (1) is touched. It is highly challenging since subjects need to retain all the required information for several seconds, and when subjects start to perform the task they need to touch the screen by hands, an action that will distract them. But surprisingly enough, the chimps

perform the task very well. This is very different from human participants' performance in the Sperling case. See the following figure for a further illustration
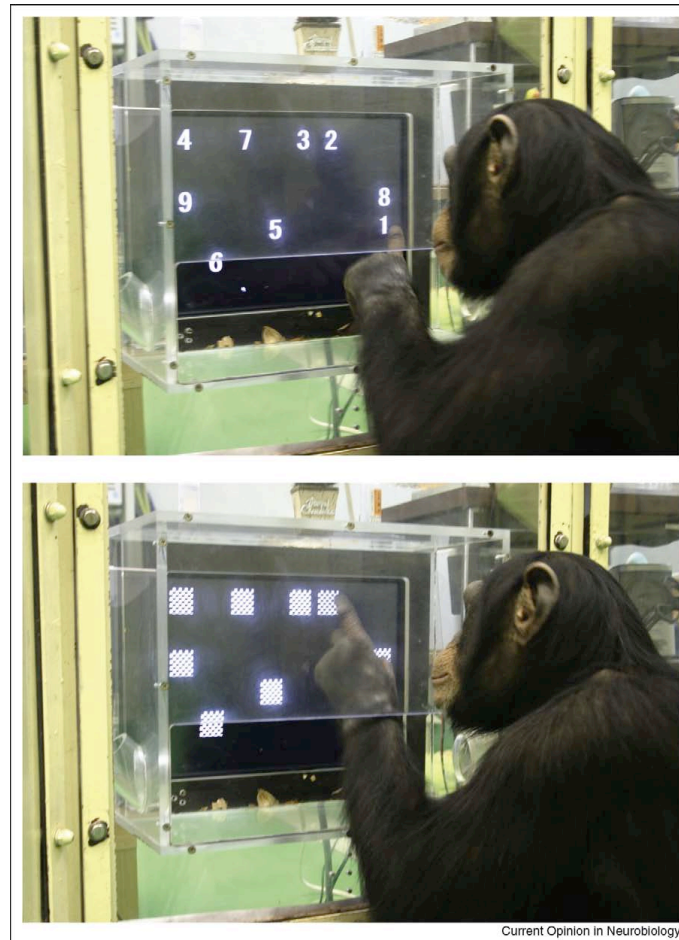

Figure 2. From Matsuzawa et al.

On the other hand, there are many other cognitive tasks that are easy for humans but difficult for chimpanzees. Chimpanzees are, for example, not good at 'generalized imitation' (Myowa-Yamakoshi et al. 2004), 'cross-modal matching' (Hashiya and Kojima 2001), 'number concepts' (Wood et al. 2008), and so on. The point about number concepts is especially relevant here. Given that chimpanzees are so good at the task designed by Matsuzawa et al. (2007), one might expect that they and their relative primates might be able to manage number concepts to some extent. However, as shown by Woods et al., rhesus monkeys are able to compare 1 and 2, 2 and 3, 3 and 4, but profoundly unable to do 4 and 5 / 3 and 8. This suggests that their grasp of numbers is not very good. Perhaps their working memory for abstract things is incomparable with their working memory for perceptual salient items. This is the main contention of the 'trade-off theory' proposed by Matsuzawa:

At a certain point in evolution, because of limitations on brain capacity, the human brain may have acquired new functions in parallel with losing others – such acquiring language while losing visuo-spatial temporal storage ability. (Matsuzawa 2009: 97).

And not surprisingly, number concepts go with language. How to work out the details of this trade-off story is of course difficult and controversial, but the general line seems to be promising.

To be sure, we cannot draw any direct conclusion from this comparison, for the task designed by Matsuzawa and the Sperling case are not entirely the same. There might be some other concerns too, for example how to tease working memory apart from iconic memory in the Chimp case is not clear.[5] Nevertheless, given that human's performances are extremely poor and chimps' performances are extremely well, the comparison might point to something real. Here is not the place to conduct further speculations, but I submit that this is a fine point for future research.

This ends my discussion of the relations between attention and consciousness, among other things. To be sure, everything I write here is preliminary and premature, and it is only an initial attempt to say something about some complex issues. However, I believe this project is workable and I would like to keep pursuing it in the following years. For now, this is the best I can offer and I am aware of how unsatisfactory it is. This is a field where philosophy meets sciences in a serious way, so we should expect more cooperative works, as opposed to many isolated armchairs.

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[5] Block makes this point in conversation, but since it is not in print, I do not know how exactly the objection would go.

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Have you noticed the difference? It is striking to discover the extent to which attention modulates consciousness. However, exactly how attention does it is in great dispute among philosophers and psychologists. In this essay, Tony H. Y. Cheng attempts to offer an account of the complex relations between consciousness and attention. The discussion starts with a clarification of relevant terminologies, such as attention, phenomenal consciousness, accessibility, attention, and so on. He then discusses Ned Block's recent contention that phenomenology overflows access based on the famous Sperling paradigm, and offers his own preferred view of it. The view is then applied to the debate between Fred Dretske and Michael Tye over the speckled hen style examples. Finally, the discussion is situated in larger contexts of general theories of consciousness offered by David Rosenthal, Jesse Prinz, and Bernard Baars, among others. Varieties of skepticism concerning the study of consciousness are also discussed. A general direction for future research thus emerges in the course of argumentation.

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