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COMMENTARY

What is Relevant to the Unity of Consciousness?

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THERE HAS RECENTLY BEEN an increasing tendency in philosophy of mind to draw on empirical studies in psychology. While attempts at interdisciplinarity are welcome, it is often very difficult for one discipline to treat appropriately what is produced within another discipline. Susan Hurley's paper on the unity of consciousness is extremely interesting. In it she uses empirical data from split-brain patients to raise problems for intuitive conceptions of the unity of consciousness. As a psychologist, I will not address her account of these conceptions nor the philosophical arguments. I will restrict myself to her interpretation of Justine Sergent's data from split-brain patients. The general point I wish to make is that the validity of empirical data and their relevance to particular theoretical issues are highly difficult to establish, especially in clinical cases. The crucial data were of three patients who could make cross-field higher/lower judgements above chance but could not perform cross-field same/different judgements above chance. Dr. Hurley suggests that these data may be interpreted to imply that consciousness is only partially unified. My contention is that the data referred to may have little to do with the unity of consciousness or even with consciousness at all. I shall further suggest that there are more pertinent data, which better fulfil desired criteria and have at least as radical implications. Finally, I shall touch briefly on what can

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be appealed to when considering the notion of 'partial unity of consciousness'.

1. Are Sergent's data relevant to consciousness?

Dr. Hurley's justification for considering that the data in question reflect the contents of consciousness is contained in the following quotation.

> Now the patient is able to indicate that the number on the right is higher than the number on the left with ease and no need to force a guess, as there is in blindsight. This observation may go some way to counter the suggestion that the comparative information must be implicit or unconscious, though it is not decisive. But for the purposes of my thought experiment (as opposed to Sergent's actual experiment) I want to focus not on the issue about conscious vs. unconscious information, but rather on the further interpretative issue about weak or partial unity that arises on the assumption that the relevant information is conscious. So I presume that the patient also consciously perceives that the number on the right is higher than the number on the left. And since there is no indication of dissociation within either the right or the left hemisphere considered separately, presumably the perception of the number on the right as a *7* is co-conscious with a perception of it as higher, and the perception of the number on the left as a *6* is co-conscious with a perception of it as lower. (pp. 69–70)

Before going any further it is crucial to make clear why a reference to 'guessing' in the passage cited is relevant. It is the criterion used by psychologists for nonconscious perception in both normal subjects and blindsight patients. Suppose that under certain conditions a subject reports that they are aware of nothing, but that, when induced to guess, their guesses show above-chance accuracy. Their guessing is then judged to be based on nonconscious information. By contrast, if a subject is willing to report, then they are judged to be reporting (at least partly) what they are aware of. The importance of this will become apparent in what follows.

Let us then turn to the question of whether the split-brain patients' responses in the two tasks reflected contents of their consciousness.

1 First, Sergent reports that neither patients AA nor NG were able to report what appeared in the left visual field. They could neither point to the correct digit nor draw it. LB could only do so after a delay of between 2 and 5 seconds. Since all responses in the experiment were

less than 2 seconds latency, we have little reason to suppose that subjects were conscious of the digit in the left visual field.

2 Second, in her paper Sergent herself suggests that the tasks (of inter-field comparison) were carried out subcortically. This tells us little psychologically, but makes it less likely that conscious information was the basis of responses. That is, when cortical change has an effect on a task it is usually to compromise the conscious basis of that task.

3 Third, and most seriously, Sergent never states in her paper whether or not the subjects had to be induced to guess. However, to clarify this I telephoned her, and she certainly agreed to the following. All three patients had been extensively tested since their commisurotomy operations, i.e. over periods of 26, 25 and 24 years. Now, in the case of blindsight patients, it is true that when patients are first tested, they protest and have to be induced to guess. But when I test GY now (12 years after initial testing) I never have to ask him to guess. I just say what I want him to do, and he does it without protesting. When we carry out certain tests we find that he is indeed guessing. (I will have more to say about such tests below.) In fact it is advisable to give clear instructions as to whether to guess or give confident reports. The point of this is that Sergent's subjects are highly sophisticated. The fact that they do not have to be *induced* to guess, does not mean that they are not guessing. If they were guessing, their responses cannot be taken to reflect conscious content. Sergent herself suggests that the subjects were not conscious of both visual fields or of either field. To quote her verbatim, 'I doubt very much that they were simultaneously conscious of both fields.'

For Sergent's data to bear on unity of consciousness, they have to reflect (equivalent?) consciousness of each visual half-field. The points made above weaken any such assumption.

2. Are Sergent's data relevant to unity (of consciousness)?

Susan Hurley's inference of a departure from unity is based on the dissociation in performance between the two tasks. However, psychologists would certainly not draw such an unqualified inference that a dissociation between same/different and higher/lower judgements has anything to do with the unity of consciousness, especially not in the

specific cases and procedure reported by Sergent. This is for several reasons.

1 Sergent states that same/different decisions are usually based (in default of other instructions) on the physical look, i.e. shape, of stimuli. We do not even know if the subjects were aware of either the lexical identity or shape of the left-field stimulus. Indeed, as noted above, Sergent indicated that the subjects could not draw or point to left visual field stimuli with either their left or right hand. This would suggest that they were not consciously aware of the left-field stimulus.

2 Inferences and comparisons are thought to be difficult, if not impossible, where the to-be-compared entities are referred to under different descriptions. Consider the plausible hypothesis that in the same/different task the digits were treated in terms of their physical or lexical identity, whereas in the higher/lower task they were treated in terms of quantity. If so, then

$$\text{Quantity}_7 > \text{Quantity}_6$$

does not entail

$$\text{Physical/Lexical Identity}_7 \neq \text{Physical/Lexical Identity}_6$$

Note that two of the three subjects started with the same/different task and then, later, performed the higher/lower task. So these subjects would have no reason to treat the digits as quantities at the time that they performed the same/different task. In any case, these were reaction-time experiments and subjects would have had no time to draw off-line inferences.

3 Perhaps the most important point here is the very fact that the two tasks were performed at different times. Thus, we do not know what would have happened if after each higher/lower trial Sergent had asked 'Are they the same or different?' This would at least get us nearer to the issue of *co*-consciousness.

4 A fourth reason psychologists would have for doubting the relevance of the dissociation to the issue of unity is concerned with the effects of brain trauma on 'attitude' or 'access'. In many cases of brain damage, it has been pointed out — by Kurt Goldstein, by Alexander Luria, and by others — that the patient is able to treat entities in some terms but not others. Some patients may be able to treat spoken words in terms of their meaning, but not in terms of their sounds, while other patients show the opposite restriction. The same is true of visual

object perception, where some patients know what something is but cannot attend to its appearance, while the opposite is true of other patients. The distinction is sometimes thought of as attention to wholes vs. to parts, sometimes as attention to meaning vs. to perceptual aspects, sometimes as taking context into account vs. abstracting out from context.

It is therefore plausible that split-brain patients may be restricted to treating digits transferred subcortically between hemispheres in terms of quantity but not in terms of perceptual identity. Indeed in the syndrome called Deep Dyslexia the presence of semantic errors (reading SLEEP as 'dream') has been interpreted by some researchers (e.g. Coltheart, 1980) in exactly this way. Such a restriction may be due to a yet further constraint in split-brain patients. When information is transferred *subcortically* across hemispheres for comparison, that information may be transferred only partially or only nonconsciously. This might permit higher/lower judgements but not same/different judgements.

From the above points, it would appear that even if Sergent's data do reflect aspects of consciousness, they may be interpreted in ways quite other than in terms of unity.

3. Are Sergent's data artefactual?

Naturally, we would only be interested in the behavioural dissociation in question if split-brain subjects' performance of cross-hemisphere higher/lower judgements reflects a genuine ability. Doubt is cast on this by recent experiments performed by Sandra Seymour, Patricia Reuter-Lorenz and Michael Gazzaniga (1994) which failed to replicate Sergent's findings. The paper is in press, but the data have been reported ('The disconnection syndrome: basic findings reaffirmed'; Abstract in The Society of Neuroscience, 1993) and Michael Gazzaniga has been kind enough to send me the manuscript and to allow me to cite it. First, two completely callosally-sectioned patients were unable to perform above chance either Sergent's same/different task or her higher/lower task. The investigators realized that it is possible to achieve scores of almost 80% correct solely by applying a guessing strategy to perception of the digit in just one of the two visual half-fields (i.e. if the perceived digit is *4* or smaller, guess that the other digit is higher; if it is *6* or larger, guess that the other digit is lower; if

it is 5, guess). When three split-brain subjects were instructed to use this strategy, they performed well above chance (around 80%) on higher/lower judgements. When the stimulus set was controlled to eliminate such strategies their performance fell to chance. Sergent's stimulus sets were not controlled to eliminate such strategies. In addition, while Gazzaniga was able to stabilize the image on subjects' retinae, Sergent's subjects were permitted free fixation for 150 millisecond exposures, a duration that is long enough to permit eye movements bringing both stimuli onto one half-field.

4. What is the normal case concerning unity of consciousness?

Quite apart from pathological cases, we must be careful in what we assume to be the normal case. A question that has occupied psychologists over the last century is what empirical criteria to use to assess the limits of co-consciousness of experiences in normal people. We may think, when intro- or retro-specting, that we are normally co-conscious of different aspects of the content of a percept (e.g. quantity and identity relations of digits). But this may not be the case. In fact, there are various findings in psychology which put pressure on our assumptions about the limits of (simultaneous) co-consciousness in normal people. Experimental psychologists try to use careful criteria of co-consciousness. Thus brief exposures followed by an interfering stimulus (e.g. visual or auditory mask) are used in order to limit the temporal availability of the percept. Otherwise a subject may attend *successively* to colour and shape. Speeded responses are required, to pre-empt off-line inferences. Cued partial report vs. whole report of a display are compared in order to estimate what is available with selective attention vs. non-selective perception. When such techniques are used, we find limitations in co-consciousness. For example when a written word is briefly exposed, it turns out that we may be conscious of its meaning, or its particular lexical identity, or whether it is printed in lower or upper case letters, but not conscious of more than one of these at a time. Of course if we are able to perceive a visual display over unlimited time, we can be aware of many different aspects. But that hardly qualifies for *co*-consciousness.

5. Are there more relevant psychological data?

I have expressed doubts as to the relevance of Sergent's data either to the unity of consciousness or to consciousness at all. Given the complications in assessing the relevance of Sergent's data, we may ask if there are data that are more straightforwardly applicable and that can be used to challenge our intuitive notions of the unity of consciousness. In doing so, there are two points to consider. First, at the beginning of this commentary I drew attention to criteria for consciousness, specifically to the criterion of claimed reporting in contrast to claimed guessing. Second, one of Susan Hurley's criteria for co-consciousness is inferential integration. The data I offer seem to me to meet the first criterion and to violate the second better than Sergent's data do.

These data form part of a longer series of studies and a brief report is available (Marcel, 1993). In the relevant experiments all the subject has to do on each trial is judge whether a threshold luminance light with a known location has come on or not in the 1-second interval between two auditory signals. There are two crucial variables.

1 In some blocks of trials the subject is asked to *report* whether s/he is aware of the light coming on. In other blocks of trials the subject is asked to *guess* whether the light came on or not, irrespective of whether they are aware of it.

2 There were three ways of indicating 'Yes' or 'No', in both the Report conditions and the Guess conditions: by a wink, by a button-press with the forefinger, by saying 'Yes'. In some experiments the subject has to use all three responses on each trial; in other experiments the subject uses each of the three types of response on separate blocks of trials.

In Table 1 are presented the results of a condition where all response modes were to be used on each trial. Although these are

Table 1. Normal subjects' report and guessing performance with simultaneous speeded use of three response types

	Hits: False Positives (%)		
	Report		Guessing
	Blocks 1 + 2	Blocks 7 + 8	Blocks 9 + 10
Blink	65 : 37.5	77.5 : 30	87.5 : 20
Finger	62.5 : 42.5	67.5 : 32.5	82.5 : 20
Verbal	55 : 42.5	57.5 : 40	80 : 25

Source: Table 4 from Marcel (1993).

group data from 10 subjects, individuals' data all show the same pattern. Importantly, the same pattern of performance is also shown when each response type is tested separately. These results are in terms of percent 'Hits' vs. 'False Positives', i.e. correct 'Yes' responses vs. incorrect 'Yes' responses.

There are two main points to note. First, in all cases subjects show greater discriminability (i.e. more correct Yes responses and less incorrect Yes responses) in the Guessing condition than in the Report condition. This is not an effect of a criterion shift; if it were, both correct and incorrect detections should increase together. The accepted inference from this difference is that the Report condition is a genuine reflection of report of conscious contents. (Of course this is not to deny that nonconscious information contributes to report of what is conscious, nor that many trials in the Guessing condition were influenced by conscious information.) If this difference in accuracy is not interpreted in this way, then by the same token we cannot interpret data on blindsight or subliminal perception as having anything to do with consciousness vs. nonconscious information.

Second, whereas in the Guessing condition discriminability hardly differs for the three response modes, in the Report condition discriminability is greater for some response modes than for others. That is, on some trials subjects say, 'I saw a light' with their finger, but 'I did not see a light' with their mouth. Since the same pattern of data is obtained when only a single response is required on each trial, this disjunction of responses cannot be due merely to requiring more than one response at a time. It should be emphasized that even though subjects are making simultaneously contradictory responses, they do not spontaneously detect any contradiction in their behaviour (unless they are allowed to make responses entirely at their leisure).

Now it is important to note that in these data we are not comparing awareness of different stimuli at different times, as in Sergent's experiments. Either there is or there is not an awareness of a luminance increment at a certain time. The inference (or contradiction) is of the most transparent kind. (It is a simple case of affirmation or contradiction, as opposed to an entailment, as in Sergent's experiments.) Quite apart from whether there is co-consciousness of two perceptual stimuli or co-consciousness of two aspects of a single stimulus, there may not even be a single state of awareness of *one* aspect of *one* stimulus at a single time, in a single person — which Dr. Hurley seems to take for

granted. My own speculation, therefore, is to split consciousness in two ways.

To start with, we may distinguish first-order phenomenal experience from nonconscious information states. We may then first distinguish the former from second-order reflexive consciousness. Reflexive consciousness can be thought of as attending to our own phenomenal states and has its contents (partially) expressed in reports. However, second, we may also divide this latter aspect of consciousness such that it is non-unitary, whereby different 'reporters' have differential access to first-order phenomenal states. Thus, in a single person reflexive consciousness controlling a report by a button-press may have greater access to a visual experience than a separate reflexive consciousness controlling a report by an oral response. In a recent paper (Marcel, 1993) I have indicated how these functional divisions not only apply to the dissociations in the psychophysical data mentioned here, but also how they apply to various clinical conditions.

It should be noted that I am positing dis-unity in reflexive consciousness, as opposed to 'weak' or 'partial' unity, which is what Dr. Hurley proposes. My reason is that I cannot imagine a phenomenological reality captured by such partial unity. If 'partial unity' is supposed to allow that I can be conscious of a light being on and of it being off, and yet fail to notice the contradiction, then this is something I find hard to imagine. (In the Visual Movement Aftereffect, the waterfall illusion, a person has a sensation of visual movement yet experiences nothing moving. But here they always notice the contradiction.) On the other hand, if we are dealing with more complicated chains of inference rather than perceptual experience, then failure to notice a contradiction seems no threat to complete unity of consciousness. The failure to notice a contradiction in simultaneous awareness, on my account, implies a split in consciousness rather than partial unity.

There are, however, two ways to view this inferred fractionation, which relate to two ways of viewing the co-consciousness relation. If one considers only reflexive consciousness, then there is a complete split in consciousness. However one can consider that reflexive consciousness A is co-conscious with phenomenal state P, and that reflexive consciousness B is also co-conscious with phenomenal state P, but that reflexive consciousness A and B are not co-conscious. This conception is one indeed of partial unity. But it is not quite the same as the partial unity postulated by Susan Hurley, since the co-consciousness of reflexive consciousness with a phenomenal state is one of access, not

of synchronous co-reference. Second, the main point of the explanation of the above-mentioned experiment is that one reflexive consciousness does not have access to the phenomenal state while the other does, in which case there is not even partial unity between the contents of the different reflexive consciousnesses. In addition, consider exactly the same kind of split in reflexive consciousness with regard to memory, i.e. fugue states and multiple personality. In such cases there is no simultaneous first-order phenomenal state with which the two (or more) reflexive consciousnesses can be co-conscious. There would then be only split consciousness. Thus, characterizing a state of consciousness as having partial unity or disunity seems to depend on the nature of the co-consciousness relation and on whether another type of state (e.g. phenomenal) is co-synchronous or not.

I have tried to indicate as briefly as possible in this discussion how difficult it is to come up with acceptable empirical data that speak to issues that arise in both philosophy of mind and psychology, in particular the possible states of unity of consciousness. I have shied away almost completely from the philosophical aspects of Dr. Hurley's paper. This is a poor reflection on my own inter-disciplinarity. However to the extent that my comments are of any relevance, I would like to acknowledge the help of Justine Sergent, Mike Gazzaniga, Naomi Eilan, and especially Susan Hurley and Christopher Peacocke.

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