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Phenomenology and Experimental Design

Toward a Phenomenologically Enlightened Experimental Science

Abstract: *I review three answers to the question: How can phenomenology contribute to the experimental cognitive neurosciences? The first approach, neurophenomenology, employs phenomenological method and training, and uses first-person reports not just as more data for analysis, but to generate descriptive categories that are intersubjectively and scientifically validated, and are then used to interpret results that correlate with objective measurements of behaviour and brain activity. A second approach, indirect phenomenology, is shown to be problematic in a number of ways. Indirect phenomenology is generally put to work after the experiment, in critical or creative interpretations of the scientific evidence. Ultimately, however, proposals for the indirect use of phenomenology lead back to methodological questions about the direct use of phenomenology in experimental design. The third approach, 'front-loaded' phenomenology, suggests that the results of phenomenological investigations can be used in the design of empirical ones. Concepts or clarifications that have been worked out phenomenologically may operate as a partial framework for experimentation.*

How can phenomenology contribute to the cognitive sciences? A number of authors have recently raised this question and have proposed diverse answers (see, for example, the essays in Petitot *et al*., 1999 and Varela and Shear, 1999). I will outline three different responses to this question, with specific reference to the issue of how phenomenology might contribute to experimental design. Although some of these responses will be more positive than others, each one, even the least positive, will be more positive than the fully negative answer proposed by Dennett (2001): 'First-person science of consciousness is a discipline

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Journal of Consciousness Studies, **10**, No. 9–10, 2003, pp. 85–99

with no methods, no data, no results, no future, no promise. It will remain a fantasy'. The differences between the positions I will outline depend not only on differences in how one understands the role of phenomenology in empirical science, but also on differences in conceptions of phenomenology. Furthermore, in so far as these approaches argue for a naturalized phenomenology, they hold to different conceptions of how naturalization is to be accomplished.

Neurophenomenology

One view, neurophenomenology, as espoused by Francisco Varela (1996), follows Husserl in understanding phenomenology to be a methodologically guided reflective examination of experience. This view maintains that both empirical scientists and experimental subjects ought to receive some level of training in phenomenological method (also see Roy, *et al*., 1999). Varela proposes that this training would include learning to practice the phenomenological reduction; that is, the setting aside or 'bracketing' of opinions or theories that a subject may have about experience or consciousness. This approach might at first seem methodologically abstract, but Lutz *et al*. (2002) have shown its practicality with some success.

In many empirical testing situations that target specified cognitive tasks, successive brain responses to repeated and identical stimulations, recorded for example by EEG, are highly variable. The source of this variability is presumed to reside mainly in fluctuations due to a variety of cognitive parameters defined by the subject's attentive state, spontaneous thought process, strategy decisions for carrying out the task, etc. For purposes of this paper, let's call these *subjective parameters* and abbreviate this to SPs — they include distractions, cognitive interference, etc. To control for SPs is difficult. As a result, they are usually classified as unintelligible noise (Engel *et al*., 2001) and ignored or neutralized by a method of averaging results across a series of trials and across subjects. Lutz and his colleagues decided to approach the problem of SPs in a different way. They followed a neurophenomenological approach that combined first-person data and the dynamical analysis of neural processes to study subjects exposed to a 3D perceptual illusion.[1] On the one hand we might think of this study as an attempt to read the subject's experience through a third-person analysis, but Lutz and his colleagues used the first-person data not simply as more data for analysis, but as contributing to an organizing analytic principle.

Specifically, the trials were clustered according to first-person descriptive reports concerning the experience of SPs, and for each cluster separate dynamical analyses of electrical brain activity, recorded by EEG, were conducted. The results were different and significant in comparison to a procedure of averaging across trials.

[1] Random dot patterns with binocular disparities (autostereogram) were presented on a computer screen. By visually manipulating these dots, subjects were able to see a 3D illusory geometric shape emerge (depth illusion). They were instructed to press a button with their right hand as soon as the shape had completely emerged. After the button push, the subjects gave a brief verbal report of their experience.

The phenomenological part of the experiment involved the development of descriptions (refined verbal reports) of the SPs through a series of preliminary or practice trials, using a well-known depth perception task. In this preliminary training process subjects became knowledgeable about their own experience, defined their own categories descriptive of the SPs, and could report on the presence or absence or degree of distractions, inattentive moments, cognitive strategies, etc. Based on the subject's own trained reports, descriptive categories were defined *a posteriori* and used to divide the trials into phenomenologically based clusters.[2] Subjects were then able to use these categories during the main trials when the experimenters recorded both the electrical brain activity and the subject's own report of each trial. The reports during the main trials revealed subtle changes in the subject's experience due to the presence of specific SPs, reflecting, for instance, the subject's cognitive strategy, attention level or inner speech. The clustered first-person data were correlated with both behavioural measures (reaction times) and dynamic descriptions of the transient patterns of local and long-distance synchrony occurring between oscillating neural populations, specified as a dynamic neural signature (DNS). Lutz *et al*. cite evidence indicating that such coherent temporal patterns occur during ongoing activity related to top–down factors such as attention, vigilance or expectation. They were able to show that distinct SPs, described in the subjects' trained phenomenological reports, translate into distinct DNSs just prior to presentation of the stimulus. For example, characteristic patterns of phase synchrony recorded in the frontal electrodes prior to the stimulus depended on the degree of preparation as reported by subjects. Lutz *et al*. show that these DNSs then differentially condition the behavioural and neural response to the stimulus.

To be clear, phenomenological training in this experiment did not involve teaching subjects about the philosophical work of Husserl or the phenomenological tradition. Rather it consisted in training subjects to deliver consistent and clear reports of their experience. Trained reflective introspection combined with an attempt to firm up descriptive protocols, based on that reflective stance, may indeed be considered phenomenological training, and as Lutz and his colleagues have shown, it is clearly not impractical. But is it a genuine phenomenological method as Varela describes it — that is, as informed by the phenomenological reduction? How does it differ from other attempts to train introspection?

The goal of the phenomenological reduction is to attain intuitions of the descriptive structural invariants of an experience, not to average them out.

[2] For example, with regard to the subject's experienced readiness for the stimulus, the results specified three readiness states: **Steady readiness (SR):** subjects reported that they were 'ready', 'present', 'here', 'well-prepared' when the image appeared on the screen and that they responded 'immediately' and 'decidedly'. **Fragmented readiness (FR):** subjects reported that they had made a voluntary effort to be ready, but were prepared either less 'sharply' (due to a momentary 'tiredness') or less 'focally' (due to small 'distractions', 'inner speech' or 'discursive thoughts'). **Unreadiness (SU):** subjects reported that they were unprepared and that they saw the 3D image only because their eyes were correctly positioned. They were surprised by it and reported that they were 'interrupted' by the image in the middle of an unrelated thought.

Briefly, the reduction involves the bracketing of our ordinary attitudes in order to shift our attention from *what* we experience to *how* we experience it. Varela (1996) identified three steps in phenomenological method, each of which requires training.

(1) suspending beliefs or theories about experience;
(2) gaining intimacy with the domain of investigation;
(3) offering descriptions and using intersubjective validations.

The reduction can be either self-induced by subjects familiar with it or guided by the experimenter through open questions — questions not directed at opinions or theories, but at experience (see Vermersch, 1994 and Petitmengin-Peugot, 1999). Rather than employing pre-defined categories, and asking 'Do you think this experience is like X or Y or Z?' the open question asks simply, 'How would you describe your experience?'

> To train the subjects, open questions were asked to try to redirect their attention towards their own immediate mental processes before the recordings were taken. For example: Experimenter: 'What did you feel before and after the image appeared?' Subject S1: 'I had a growing sense of expectation, but not for a specific object; however when the figure appeared, I had a feeling of confirmation, no surprise at all'; or subject S4: 'it was as if the image appeared in the periphery of my attention, but then my attention was suddenly swallowed up by the shape'. (Lutz *et al*., 2001).

Open questions posed immediately after the task help the subject to redirect his/her attention towards the implicit strategy or degree of attention he/she implemented during the task. Subjects can be re-exposed to the stimuli until they find 'their own stable experiential invariants' to describe the specific elements of their experiences, in this case the SPs. These invariants then become the defining elements of the phenomenological clusters that are used as analytic tools in the main trials.

The experimental protocol used in Lutz *et al*. (2001) thus employs a practical phenomenological reduction. The subjects are asked to provide a description of their own experience using an open-question format, and thus without the imposition of pre-determined theoretical categories. They are trained to gain intimacy with their own experience in the domain of investigation. The descriptive categories are intersubjectively and scientifically validated both in setting up the phenomenological clusters and in using those clusters to interpret results that correlated with objective measurements of behaviour and brain activity. Of the three approaches reviewed in this paper, this one is the strictest application of phenomenological method in the experimental context.[3]

The Retrospective and Indirect use of Phenomenology

In regard to usual experimental practice, Overgaard (2001) notes: 'Most of the experimental approaches to consciousness simply ignore these issues and will

[3] For further theoretical and methodological discussion of this experiment, see Lutz (2002).

either just assume certain experienced qualities in the subject or rely on the more unspecific everyday phenomenology. This is not odd at all when considering the immense work on developing a useful phenomenology that is needed to do this properly' (§ 9). The 'immense work' of such training may be only a *perceived* impracticality, however. Training humans in a reflective procedure (as in Lutz *et al.*, 2002) seems clearly easier than training monkeys in an experimental response mode. Nonetheless, a perceived impracticality is likely to motivate a less formal version of this reflective approach. Braddock (2001) argues for this less formal approach and calls it 'indirect phenomenology'.

Braddock, for example, argues that the practice of phenomenology can be naturalized by allowing results from the cognitive sciences, including, especially, the study of pathological cases, to inform phenomenological analysis, and vice versa. He finds the tradition of Jaspers and Merleau-Ponty to be exemplary in this regard. In principle this seems a good strategy. Someone like Merleau-Ponty (1962) did his phenomenology fully informed by the current scientific research, and used phenomenology, retrospectively, to interpret the results of that science. But this does not address the issue of how to incorporate phenomenology into the experimental situation.

Merleau-Ponty, for example, frequently used phenomenological insights to reinterpret experimental results. In such cases, phenomenology takes on a critical function, offering correctives to various theoretical interpretations of the empirical data. This approach can be theoretically productive in that it develops alternative interpretations. But unless these interpretations are subject to further empirical testing they remain unverified. This simply brings us back to the question of how to incorporate such phenomenological insights into experimental studies.

Another problem with this kind of after-the-fact reinterpretation can be seen in regard to pathological case studies. For example, Merleau-Ponty offers a brilliantly conceived reinterpretation of the case of Schneider, a brain-damaged patient of Goldstein (Merleau-Ponty, 1962). Is the reinterpretation correct? As far as I know, there has never been any attempt to take the phenomenologically inspired reinterpretation back to the laboratory — that is, there was no attempt to translate the phenomenological reinterpretation into any kind of follow-up testing,[4] and as a result, Merleau-Ponty's account of the case remains simply one of several possible theoretical accounts. In a very practical way this suggests the inadequacy of this approach if phenomenologists are not working directly with and along side psychologists and neuroscientists in the experimental context. Once again this brings us back to the question of how specifically to incorporate phenomenology into the experimental context.

Another possible interpretation of indirect phenomenology is that to use phenomenology in experimental testing just means taking introspective reports into consideration. As Braddock himself admits, on this view, what starts out as a conception of formal (e.g., Husserlian) phenomenology, ends up as more or less the kind of informal phenomenology that is currently practiced in the cognitive

[4] Indeed, the case of Schneider was an old one when Merleau-Ponty produced his account. Schneider's brain damage was extremely complex, and was studied by Goldstein between 1915 and 1930.

sciences, or something that is akin to what Dennett (1991) calls heterophenomenology. In heterophenomenology, first-person data are averaged out in statistical summaries. That is, first-person data are treated as third-person facts (e.g., behavioural responses) to be analyzed using mathematical instruments and pre-established categories. As a result, no attention is paid to the phenomenal details of the subject's experience. I have suggested (Gallagher, 1997), however, that this procedure is actually naive, and ultimately unscientific, to the extent that in attempting to say something about consciousness or cognitive experience, it fails to acknowledge that its interpretations of phenomenological reports are ultimately, and at least in part,[5] based on either the scientist's own first-person experience, or upon pre-established (and seemingly objective) categories that are ultimately derived from folk psychology or an obscure, anonymous, and certainly non-methodological phenomenology. The intentional stance required for the scientist's interpretation of the subject's report is not itself something that has been scientifically controlled.

This naiveté could be corrected by basing the interpretive categories on a methodologically informed phenomenological analysis. That is, if one could establish the interpretive categories in a phenomenologically controlled way, then the first-person data would not be washed out of the experiment but would be given their proper significance. One way to establish the scientific credentials of the interpretive categories would take us back to a neurophenomenology of the type outlined above.

Neurophenomenology, as we have seen, employs a phenomenological reduction. Although Dennett (2001) introduces his own version of a heterophenomenological reduction, it serves a very different purpose. After cataloguing subjective reports and other first-person data, Dennett suggests that they all be '*bracketed for neutrality*'. In effect he advises the scientist to treat the verbal reports as if they were fiction. One requires, for this process, a suspension of trust in the subject, and a suspicious interpretational practice. Braddock (2001) thinks this is problematic, but in this particular part of the heterophenomenological approach it seems to me to be nothing other than good scientific practice. It involves testing the experiential reports against all the other non-experiential data, and attempting to draw a coherent third-person picture of the subject's behaviour. This is problematic only if one wants to know what the subject's *experience* is like, and what effect that experience might have on the subject's behaviour. A more complete understanding of experience and behaviour, then, does not eliminate the need for a phenomenological analysis to legitimize the initial cataloguing and interpretation of the original subjective reports.

Of course, Dennett's notion of heterophenomenology is motivated by longstanding suspicions about introspection as a psychological method. With the rising importance of brain-imaging techniques, however, there is a renewed

[5] In Dennett's most recent version of heterophenomenology he explains that it is not just the verbal reports that constitute the data for heterophenomenological analysis, but behavioural and other objective (physiological) data. So some part of the interpretation of the verbal reports would likely be based on the other objective data.

interest in introspective methodology (see, e.g., Jack and Roepstroff, 2002; Schooler, 2002; and follow-up discussions by Frith, 2002 and Gallagher, 2002). Renewed interest in introspection, however, again directs us to the question of precisely how the use of introspection might be made methodologically secure, that is, how it might be more formally controlled using phenomenological techniques in experimental paradigms.

Each of the various proposals for the *indirect* use of phenomenology leads us back to methodological questions about the *direct* use of phenomenology in experimental design. We have seen that Varela's notion of neurophenomenology, as practiced by Lutz *et al*. (2002), offers one model for such direct use. It is a model that provides a clear proposal for making introspection methodologically secure, and it seems quite possible to use such a procedure in many paradigms that test perceptual consciousness. It is not clear, however, that the specifics of this model can be adapted for use in all experimental investigations of consciousness or cognition. In the following, I consider an alternative model for introducing phenomenology directly into experimental design.

Front-loaded Phenomenology

In this section I want to outline and defend a third view of a phenomenologically enlightened experimental science, or what I'll call simply 'front-loaded phenomenology'. Rather than starting with the empirical results (as one would do in various indirect approaches), or with the training of subjects (as one would do on the neurophenomenological approach discussed above) this third approach would start with the experimental design. The idea is to front load phenomenological insights into the design of experiments, that is, to allow the insights developed in phenomenological analyses (modelled on Husserlian description, or the more empirically oriented phenomenological analyses found, for example, in Merleau-Ponty, or in previously completed neurophenomenological experiments) to inform the way experiments are set up. To front load phenomenology, however, does not mean to simply presuppose phenomenological results obtained by others. Rather it involves testing those results and more generally a dialectical movement between previous insights gained in phenomenology and preliminary trials that will specify or extend these insights for purposes of the particular experiment or empirical investigation. I'll discuss several examples of how this can be done.

Let me begin, however, with two notes. First, and on the one hand, according to this approach, one can incorporate the insights of phenomenology into experimental protocols without training subjects in the method. On the other hand I think that phenomenological insights developed on the basis of such training and in neurophenomenological experimentation can contribute to experimental design by contributing to the phenomenology that can be front loaded into further experiments. That is, the phenomenology that is front loaded might be developed in pure phenomenological analysis (as in Husserl), or in neurophenomenological experiments. Second, it is a natural consequence of front-loading phenomenology that, as in the neurophenomenological model, the phenomenology becomes part of the analytic framework for interpreting the results, and not just part of the data to be analyzed.

The experiments that I will focus on here all involve brain imaging. Furthermore, I have deliberately chosen experiments that do not involve introspective reports in order to eliminate any confusion about where precisely the phenomenological contribution lies on this approach.[6] Specifically, the phenomenological analysis is done prior to the experiments and the results of that analysis are used to work out the experimental design.

For the first two experiments, the phenomenology concerns a distinction between self-agency and self-ownership. In the normal experience of intentional action these two aspects of self-awareness are close to indistinguishable. But consider the phenomenology of involuntary action. If, for example, someone pushes me from behind, I sense that it is my body that is moving — it is *my* movement and I experience ownership for the movement — but I do not experience agency for the movement (I have no sense that I intended or caused the movement). To get the phenomenology right, however, we need to distinguish between the first-order phenomenal level of experience and higher-order cognition. It is possible to make the distinction between 'attributions of subjectivity' (or ownership) and 'attributions of agency' on the level of higher-order, reflective or introspective report (e.g., Graham and Stephens, 1994; Stephens and Graham, 2000). It is also possible to make the distinction at the level of first-order phenomenal consciousness (Gallagher, 2000; 2003a). That is, in the case of involuntary movement, I directly experience the movement as happening to me (sense of ownership), but not as caused by me (no sense of agency). Ownership and agency are seemingly (and in the case of phenomenal experience, 'seemingly' means 'really') built into experience. They are part of a pre-reflective (non-conceptual) self-awareness implicit to the experience of action. Indeed, this is usually the basis for attributions of subjectivity and agency at the higher introspective level.[7]

If neuroscience accepts this phenomenological distinction, then one task is to determine what neurological processes generate these first-order phenomenal experiences. Furthermore, if this distinction is in fact implicit in first-order phenomenal experience rather than the product of second-order introspective attribution, then this suggests that neuroscientists should look for a more basic set of primary processes that are activated in motor control mechanisms rather than in areas that may be responsible for higher-order cognitive processes.

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[6] There certainly are experiments that rely on front-loaded phenomenology that employ introspective reports. Brøsted (in press), for example, designed an alien-hand experiment to test visual *versus* proprioceptive awareness of the body in bulimic patients, as evidenced by first-person reports. For the experimental design he relies on phenomenological distinctions between body image and body schema (see Gallagher, 1995; Gallagher and Cole, 1995; Paillard, 1997; 1999), and between sense of agency and sense of ownership (Gallagher, 2000a; 2000b).

[7] Graham and Stephens suggest that these distinctions are actually generated at the higher cognitive level on the basis of an intentional or narrative stance that I take toward myself. I've argued that the distinction originally belongs to the first-order level of phenomenal experience (Gallagher, 2003a).

This phenomenologically based supposition has informed the design of several recent experiments. Experimenters have relied on the phenomenological conception of the sense of agency, as distinct from the sense of ownership, as I have defined it, in experiments that attempt to distinguish the neural correlates of the sense of agency for one's own actions (self-agency) in contrast to the sense that the action belongs to someone else (other-agency).

- In the first experiment (Farrer and Frith, 2001), subjects manipulated a joystick to move an image on a computer screen while fMRI brain images were taken. Sometimes the subject caused this movement and sometimes the experimenter caused it. In each case, however, the subject moved the joystick appropriate to the movement on the screen. This allowed for a separation between the sense of agency and the sense of ownership. The effect related to the sense of ownership was present in all conditions and was thus cancelled in the imaging contrasts. The experiments show contrasting activation in the right inferior parietal cortex for perception of action caused by others, and in the anterior insula bilaterally when action is experienced as caused by oneself.[8] The experimenters suggest that the role of the anterior insula in providing a sense of self-agency involves the integration of three kinds of signals generated in self-movement: somatosensory signals (sensory feedback from bodily movement, e.g., proprioception), visual and auditory signals that could generate an ecologically self-specifying sense of movement, and corollary discharge associated with motor commands that control movement. 'A close correspondence between all these signals helps to give us a sense of agency' (p. 602).[9]
- The second study, Chaminade and Decety (2002), involves individual subjects controlling a computer image using a mouse. They are instructed to

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[8] Decety *et al*. (2002), further explore the function of the inferior parietal cortex, and show there is more activation in the left inferior parietal lobule when a subject imitates another person compared to more activation in the right inferior parietal lobule when the other person imitates the subject.

[9] Discussion of these experiments in Farrer and Frith (2001) is not always as clear as it should be in regard to precisely what they were testing. First, in some respects the experimental paradigm, while clearly distinguishing between senses of agency and ownership, fails to distinguish between the first-order phenomenal level and the level of higher-order attribution. I think there are good reasons for interpreting the results in terms of the first-order phenomenal level of experience. It seems reasonable to think that the kinds of information integrated by the anterior insula — proprioceptive feedback, ecological sensory self-specifications involved in movement, and corollary discharge associated with motor commands — constitute implicit, first-order aspects of motor experience rather than the neural correlates of higher-order cognitive attributions or judgments. Second, the fact that a subject moved under both conditions (when she was moving the computer image, and when the computer image was not being moved by her), supposedly, in this context, to distinguish the sense of agency from the sense of ownership, actually confuses the issue with respect to the sense of agency. In one respect it rightly shifts the focus to the issue of the intentional goal of the action — the question is whether I am moving the image on the computer or not. In another respect, however, since at the level of motor behaviour exactly the same movement is made in both cases, it is not clear why that same movement would not generate the same self-specifying information that would tell the subject that she is the agent of that movement. Similar problems are to be found in Farrer *et al*. (2003). This study, however, nicely shows that conflicts between visual and proprioceptive feedback about one's own movement may cause problems with respect to the sense of agency, and confirms that these problems are correlated with activity in both the posterior insula and the right inferior parietal lobe.

> lead or to follow or simply to observe another image on the screen. No reports are required of the subjects; a PET scan images areas of their brains as they perform their movements. The scans show bilateral activity in the inferior parietal cortex in conditions that involved confusion about the origin of an action. When subjects are required to lead (taking the lead in moving an image around on a computer screen), and so when the participant in the scanner sees the other image following his/her actions, more activity is apparent in the right inferior parietal cortex than in the contrasting situation where the subject is asked to follow. In the latter case — involving less of a sense of agency, and more a sense of passive control, or being acted upon — more activation occurs in the left inferior parietal cortex than in the right. The experimenters conclude, 'the lateralization of the inferior parietal lobule activity may be critical for distinguishing consequences of actions generated by the self from those initiated by others, especially when confusion may occur' (p. 1978).

In these experiments subjects can be perfectly naive about the phenomenological details of their own experience. They are not even required to give a report of their experience. Yet it is clear that the phenomenological description of the sense of agency both informs the experimental design (the experiment is set up to find the neural correlates of precisely this experience) and part of the analytic framework for interpreting the results. Moreover, the experiments do not simply presuppose the phenomenological description. Rather, they test and verify that description and extend its application to issues that involve social cognition, at least to the extent that in some of the tasks self-agency is contrasted with the sense that the action is caused by someone else. Since issues pertaining to social cognition and intersubjectivity are also of concern to phenomenology, this opens further opportunities for the interaction between phenomenology and experimental neuroscience.[10] Indeed, in this regard, and as I will suggest in the following example, it is possible to outline a phenomenological proposal for further work in this area.

This final example actually involves a set of important experiments that have already been completed and reported in the literature. They will serve as an example here, but only in the sense that they are missing something that phenomenology could have provided. And what they are missing clearly qualifies their results.

Experiments conducted by Jeannerod, Decety, and their colleagues (Blakemore and Decety, 2001; Decety and Grèzes, 1999; Decety, *et al*., 2002; Jeannerod, 1997; Ruby and Decety, 2001; and other studies reviewed by Grèzes and Decety, 2001) using various brain-imaging techniques show that there are a

[10] Intersubjectivity is an issue in at least two ways in these experiments. First, as mentioned, the experiments target the distinction between self-agency and other-agency, and this can have application to questions of social cognition. Second, the experiments rely upon the interaction between experimenter and subject, and on the instructions or scripts that are presented to the subjects by the experimenters. For an excellent discussion of this issue in regard to top–down versus bottom–up explanations, see Roepstorff and Frith, in press.

number of brain areas (including the SMA, the dorsal premotor cortex, the supramarginal gyrus, and the superior parietal lobe) that are activated in common when a subject

- Engages in intentional action
- Observes others engaging in such action
- Consciously simulates (or imagines) performing such action
- Or prepares to imitate such action

The experimenters who have conducted experiments on these intriguing shared neuronal representations consider the overlapping activation of brain areas to be an important part of the explanation of how we come to understand others. That is, we activate parts of our own motor and cognitive systems in a simulative way, and this neural reverberation gives us insight into what the other person's experience must be like. Those areas that are non-overlapping across these different conditions are also of importance. Jeannerod (2001) has proposed that the non-overlapping areas may account for our ability to distinguish our own activities from those of others, and may contribute to a sense of self-agency (also see Jeannerod *et al*., 2003; Ruby and Decety, 2001). The experimental paradigm for these experiments is based on an important distinction between first-person perspective and third-person perspective worked out in an influential paper by Barresi and Moore (1996). I've suggested elsewhere, however, that the way in which this distinction is put to use in some experiments suffers from a certain phenomenological impoverishment (Gallagher, 2003b). The two perspectives are defined operationally in the following way in regard to conscious simulation:

First-person perspective: Subjects are asked to imagine themselves performing a given action, for example, reaching to grasp a glass.

Third-person perspective: Subjects are asked to imagine the experimenter performing the same action.

Within the first-person perspective as defined, however, and using a phenomenological technique called 'imaginative variation', I could imagine myself performing an action from within an egocentric spatial framework.

First-person-egocentric perspective: I am located here, and I imagine moving this very hand to grasp the glass in front of me.

Alternatively, however, I could imagine this action using an allocentric spatial framework — taking an external perspective toward myself.

First-person-allocentric perspective: I imagine myself sitting over there, and I can visually imagine how that person, who happens to be me, would reach to pick up a glass that is nearby.

Likewise, for the third-person perspective, it is possible that I could imagine the other person performing the action from an external, allocentric perspective.

Third-person-allocentric perspective: I imagine her over there reaching for the glass.

Or I could imagine taking the other person's place and working out how it must be for her as she reaches for the glass.

Third-person egocentric perspective: I imagine being over there in her place doing the action 'from the inside'.[11]

Table 1: Complex perspectives

Perspectives	First-person	Third-person
Egocentric	'I imagine doing X here'	'I imagine occupying the other's perspective as the other does X'
Allocentric	'I imagine seeing myself doing X over there'	'I imagine seeing the other person doing X'

The issue of perspectives, then, is a complex one, and it leads to the following question: When subjects are asked to simulate (or imagine) performing an action from the first-person perspective (or third-person perspective) do we know whether they are taking an allocentric or egocentric perspective, and is neural activation the same or different across these different perspectives? Employing these phenomenological distinctions and answering this question may help to make the concept of neuronal simulation and the differentiation between self-agency and other-agency more precise.

A further phenomenological, and possibly neurological, complication involves a more precise definition of the mechanisms that allow for the distinction between one's own action, along with the sense of agency for that action, and the actions of someone else. Georgieff and Jeannerod (1998), for example, have proposed a 'Who system' based on the shared (overlapping) and unshared (non-overlapping) neural representations for action. The complication involves what we might call the primary first-person framework that structures all of a subject's experience. That is, in all cases, even in the third-person allocentric framework, I am the one doing the imaginative enactment — third person perspectives are still accomplished within the first-person framework of my own experience. One might say that there is something it is like to be imaginatively enacting an action from a third-person perspective. How is this primary first-person framework accounted for in the 'Who system' so that even when through a process of simulation (personal or subpersonal) I put myself into the place of the other, I never lose track of who is simulating and who is simulated?

These phenomenological distinctions may present difficulties for neuroscience, but it is not beyond the realm of possibilities that they could be front-loaded into the experimental design — specifically by providing instructions to the experimental subjects about how exactly to perform the imaginative enactment. It would also be possible to go further, along the lines of the

[11] Farrer and Frith (2001) claim that this is not possible: 'it is not possible to represent the actions of others in the egocentric coordinates used for generating our own actions' (p. 601). It is not clear to me why not.

neurophenomenological procedure of Varela and Lutz, and to train the subjects in a way that would further refine the distinctions. In any case, to the extent that such distinctions are not taken into consideration in experimental design one needs to consider important qualifications on the experimental results and their analysis. The phenomenology of imaginative enactment suggests that the issues concerning overlapping and non-overlapping brain activation is more complex than experimenters may think.

Conclusion

The experiments reviewed in the previous section do not involve any direct introspective reports. I indicated that my choice of experiments was meant to clarify where I think a front-loaded phenomenological approach does its primary work, i.e., in the distinctions and insights that contribute to experimental design and interpretation. The idea is not, I think, that every experiment has to privilege the first-person data internal to the experiment, as long as the significance of first-person experience gets taken into account at some point in the process. In the case of front-loaded phenomenology, the first-person data is taken seriously in the phenomenological analysis that serves to set up the design.[12] In this respect I hope it is obvious that I do not mean to rule out the use of introspective reports in experimentation. To the extent that one may require subjects to report their experiences in the experiment, then the approach of front-loaded phenomenology would be to follow neurophenomenological rather than heterophenomenogical procedures. In all cases, it is good scientific practice to understand and to introduce controls on the various experiential categories that may be involved, both in experiential reports and in the interpretation of those reports. It is not a matter of blindly trusting the subject, or blindly distrusting the subject. Rather, it is a matter of giving both subjects and experimenters methodologically controlled, phenomenologically enlightened ways of understanding the importance of first-person experience and how it can affect the experimental results. Such phenomenologically enlightened approaches are in clear contrast to heterophenomenological procedures that would average out or wash out all first-person data using anonymously formed categories that are considered to be scientific only because they are third-person categories.

Not all scientific concepts are third-person concepts. Consider psychophysical concepts that rely on experience, for example, the felt intensity scales central to Steven's power law (on which the decibel scale for measuring loudness is based). The fact that one cannot eliminate such phenomenal data in well-established areas of scientific psychology has motivated Steven Horst (in press) to remark:

> You simply cannot banish the qualitative aspect of such effects from your description of the psychophysical data: eliminate the qualitative phenomenological property of percept brightness and you have not sanitized the portion of psychophysics concerned with brightness, but eliminated it entirely. No phenomenology, no psychophysics.

[12] I thank an anonymous referee for motivating a clarification of this issue.

It is something of a *fantasy*, to use Dennett's term, to suggest that neuroscience or psychology are best done by averaging out, reducing, and re-engineering first-person data so that it looks like third-person data. There is no scientific promise in failing to consider experimental designs that leave the complexity of first-person perspectives out of the equation.

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