The goals of a science of first-person experience

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Reports of subjective experience are central in a number of different areas of science, but how those reports are treated and what roles they play differ. In this paper, my aim is to consider the use of such reports in light of the goals of different areas of science. I will focus mainly on psychology and medicine, with a brief consideration (moving from an area of investigation to a set of methods) of qualitative research. In the first of these areas, questions about the scope and limits of introspection have been perennial, and one question I want to explore is the extent to which questions arising in this area can help to shed light on similar questions that are faced by medicine (and vice versa). All of this is very much a work in progress.

I will begin with a discussion of the early days of experimental psychology, contrasting two central figures, Wilhelm Wundt and Edward Titchener, regarding their views on both introspection as a scientific method for psychology and their views on the appropriate goals for experimental psychology. I next compare psychology with medicine, suggesting that there are some parallels in the central, but controversial role that reports of first-person experience have played. In the case of medicine, however, its practical goals provide some way of addressing cases in which subjective reports and objective tests provide inconsistent information. In the second part of the paper, I return to psychology to consider how conflict between first-person reports and objective experimental evidence are addressed, and close by briefly considering the way that qualitative research might be helpful in reframing the conflict – which might also require reconsidering the goals of psychological research.

Introspection and the scope of experimental psychology

When psychology first developed as an empirical, laboratory science in the late 19th century, introspection was a crucial methodological tool. One major debate about the use of this method focused on whether introspection can tell us about the contents of our mind without those contents being altered by the requirement to report on them. Maja Spener identifies three basic modes of introspective access, the relative merits of which were debated by philosophers and psychologists throughout the 19th century. The first mode of introspection is self-observation, in which individuals needed to observe their own experiences as they were occurring. Critics

argued that this observation altered the character of the experiences being observed. The second mode is perhaps more accurately termed “retrospection”; it requires participants to report on an experience or mental activity once it has ended, but while it is still fresh in their mind. The third form of introspection is inner apprehension, “the fairly automatic and passive peripheral awareness one has of one’s ongoing states and episodes, while being attentively engaged with the objects of those states and episodes” (Spener, p. 36).

For Wundt, only the third of these approaches, inner apprehension, was a suitable method for psychology. The other two approaches are bound to change or misrepresent the experiences being investigated. He viewed it as impossible to both experience and report on an experience at the same time; however, it is necessary to do both of these things to investigate mental states scientifically. Wundt believed that this problem could be solved through two methodological strategies. First, the experimental environment had to be carefully designed to ensure that the experimental stimulus would be the focus of introspection. In addition, the experimental participant had to wait for the stimulus in a state of what Wundt called “strained attention in order to be ready to (passively) observe the stimulus when it appeared. He also limited the content of introspective reports to assist in achieving this focus: participants were instructed to report briefly on some particular aspect of the experience, rather than to describe everything they were experiencing.

Perhaps unsurprisingly, this all meant that, for Wundt, the range of psychological phenomena that could be studied was quite narrow. Only relatively simple stimuli could be presented in such a way as to be the focus of inner apprehension. George Mandler notes that, for Wundt, experimental psychology was sensory psychology, focusing on “sensory processes, perception, consciousness, attention, will, affect, and time and space perception” (p. 57). (Wundt also wrote extensively about how higher cognitive processes, including those that are deeply culturally laden, should be investigated. His multi-volume Völkerpsychologie proposed an interpretive method for understanding these aspects of the mind.)

Finally, Wundt viewed the goal of experimental psychology as identifying “psychical” laws that govern mental activity. For Wundt, our experiences are constructed through the process of apperception, and are not just the result of passively taking in experience. This is an important point of contrast with Edward Titchener’s approach. Although both of them accepted the general view that the contents of the mind were elements (something like Locke’s simple ideas), for

Wundt, the mind actively constructed our experience, while Titchener, educated in the British associationist condition, viewed the mind as fundamentally passive.

Titchener also viewed the role of introspection as fundamental in psychology in the following sense: it could provide a complete catalogue of the elements of experience. Only once this descriptive phase was complete could the second phase of research truly begin – this would identify the statistical regularities in the relationships among the different elements. As he put it, "the data of introspection are never themselves explanatory; they tell us nothing of mental causation, or of physiological dependence, or of genetic derivation. The ideal introspective report is an accurate description, made in the interests of psychology, of some conscious process. Causation, dependence, development are then matters of inference." (Titchener, 1912). Here, too his approach differs from Wundt's, as the latter believed that experimental psychology aimed both to describe and to explain by establishing laws of psychic causality.

Titchener and Wundt also disagreed about the appropriate use of introspection and of experimental psychology. Titchener believed that the use of introspection should not be constrained to a small area of our mental life; he believed that trained introspectors could hold a complex experience unchanged in their memory while they delivered a long report about the content of their experiences, and he believed that, although training in introspective techniques did not allow experimenters to actually *experience* the elements of the mind, they could learn to isolate them analytically and report on them accurately.

In addition to Wundt and Titchener, other groups of psychologists developed their own introspective methods, so that, by the early 20th century, there was widespread disagreement about the nature of the contents of the mind, how to investigate them, and the relationships among them. In his behaviorist "manifesto," James Watson cited this disagreement as evidence that introspective psychology was not a viable approach and should be replaced by behaviorism. I'll return to psychology later in the paper and suggest that reconsidering the scope and purpose of experimental psychology, drawing on Wundt, in particular, might help to address contemporary questions about the validity of first-person reports.

First person reports in medicine

Similar problems occur in medicine, where patients' reports of their own experiences are both necessary for diagnosis/evaluation of treatment effectiveness and deeply suspect as

indicators of pathophysiology. Richard Cytowic has described this problem as “the clinician’s paradox” of “believing those you must not trust” (Cytowic, 2003). In this section, I suggest that this paradox has a similar history to the problems with introspection in psychology: as competing theories proliferated and new methods became available for investigating the physiological processing underlying patients’ experiences, the focus of medicine shifted from patients symptoms to their physiological state – and conflicts between the two are generally settled in favor of understanding the latter.

Notoriously, prior to the development of medical science in the late 17th and early 18th centuries, medicine was characterized by what Wulff et al. (1990) have called “speculative realism.” Medical knowledge was highly theoretical, but the theories in question were not supported by empirical investigation. To be sure, physicians did listen to patients’ descriptions of their symptoms, and generally observed their clinical state (though usually without any physical examination). On this basis, physicians used their preferred theory to diagnose the patient and offer treatment suggestions. Most famous of these theories was the Greek theory of humors: disease was held to be an imbalance among the four humors, and treatments were designed to restore balance. Yet there were other theoretical systems that viewed disease as arising from problems with specific parts of the body, such as the nerves or the blood vessels. Shared by all of the theories, however, was the view that the problem was an imbalance, and also the view that, contrary to contemporary medicine, the symptoms simply *were* the disease, rather than an indicator of an underlying disease process.

Developments in medical science shifted attention away from subjective symptoms and toward objectively observable manifestations of disease. In the early 19th century, medical research consisted in the development of techniques of observation. The elements of physical examination were developed at this time, including auscultation, palpitation, and percussion to indirectly observe the inside of the body. In addition, the increasing frequency of autopsies allowed correlation of symptoms with gross anatomic changes, and developments in microscopy linked symptoms with histological changes.

Later in the century, progress in laboratory research led to a greater knowledge of physiological processes. The historian Merrilee Borrell points to the development of recording techniques that “allowed investigators to separate and measure transient physiological events… such as muscle contraction and nerve conduction” (1987, p. 293). Although this research was

only able, at first, to characterize physiological processes and not to link them with diseases, researchers had faith that continued research would allow the discovery of how physiological processes were altered by disease, which would provide a new scientific framework for medicine. Even in the absence of pathophysiological knowledge, this new research began to shift conceptions of disease; the symptoms that used to be seen as the disease were now increasingly thought to be mere indicators of underlying disease processes. Steven Stowe documents a telling shift in published case reports during this time. Previously, case reports had been highly narrative in form, seeking to link a patient's biography and their bodily state. They now became more like contemporary case reports, on which "specialized, calibrated, and often highly quantified language drains the patient's suffering from the case" (Stowe, 1996, p. 165).

Currently, evidence from laboratory studies, imaging studies, and other objective means of measuring the body's state are generally viewed as telling us the truth of a patient's clinical state, regardless of whether they agree with a patient's own reports of their experiences. Consider, as an example, the phenomenon of poorly perceived asthma. Some patients are characterized as "poor perceivers" because their experience of breathlessness is less than would be expected given the degree of constriction of their airways. (Others are "overperceivers" – their subjective experience of breathlessness is greater than their airway constriction would seem to predict). Note that I am not saying here that bronchoconstriction is irrelevant: if it is a better predictor of severe asthma exacerbations than subjective reports, there is good reason to take it very seriously. My point is simply that the objective indicators of disease are considered to reflect a patient's true clinical state, so that when reports of their experience differ from these measurements, it is the subjective assessments that are too strong or too weak, compared with the "just right" objective tests.

Yet even if objective measures are a better predictor of asthma exacerbations, at least in a significant number of patients, than subjective reports of breathlessness, we cannot conclude that the objective reports are always better or more useful. If the purpose of collecting both kinds of information is to benefit the patient, then either might be more important in different circumstances. There are medical conditions, such as gallstones, which can be asymptomatic and do not pose a significant threat in the absence of symptoms. There are also conditions, including asthma, where the control of symptoms is in practice the usual clinical goal. So there are some situations in which first-person reports of symptoms are more important for meeting the goals of

medicine than are objective tests. Qualitative research is often used to understand patients' needs and preferences, including how symptoms affect their daily lives and experience of their own health; this kind of research takes an interpretive approach to investigating first-person experience.

In summary, both in early psychology and in contemporary medicine, first person reports play a complicated role. The practical goals of medicine can often determine, at least to some extent, the role patient reports should play versus the role of objective evidence, in cases where the two disagree. A similar problem of disagreement arises in contemporary psychology; however here there is no practical goal that guides researchers in adjudicating between them when introspective reports disagree with other experimental evidence. I turn to this point next.

Cognitive psychology and first-person reports

With the beginning of the cognitive revolution in the late 1950s, it became respectable again for psychologists to study the mind. Yet this era differs significantly from pre-behaviorist psychology in that the goal has shifted from the study of the *contents* of the mind to the study of cognitive *processes*. This point is well-illustrated in a 2005 chapter by Gary Hatfield on the role of introspective evidence in understanding the mind. He surveys the various problems that have been raised about the use of introspection to "discover or infer the character of mental or psychological processes" (p. 263). His conclusions are rather similar to Wundt's: introspection, defined as "deliberate and immediate attention to certain aspects of phenomenal experience" can indeed provide evidence about the nature of perceptual and cognitive processes, not because we have independent access to these processes, but because evidence obtained from reports of our phenomenal experience, in conjunction with other kinds of evidence (e.g., performance on cognitive tasks, knowledge of the experimental setup) can help researchers draw inferences about these processes.

This raises the question of whether introspection can also tell us about the cognitive processes themselves. Currently, the consensus seems to be that it cannot, but I think that may be too quick a conclusion. Consider two examples of classic experiments that Owen Flanagan (1994) discusses in his explanation of the methods used in cognitive psychology. The first is Saul Sternberg's experiment on short term visual memory search. In this experiment, participants were asked to memorize a vertical string of digits. After a pause, a single test digit was presented

and the participant's task was to report whether the test digit was in the original string of digits. The aim was to characterize the strategy used to perform this search. On one potential strategy "Mind's Eye Sees all," the entire list is recalled to mind and the participant "looks" at the list to see whether the test digit was on the list. On a second strategy, "Self-Terminating Serial Search," the participant recalls the digits on the original list, one at a time (from left to right) and, if they reach the test digit, they stop the search and report that the digit was on the original list. Only if the test digit was not on the list (or was last on the list) do they run through the entire list in their mind. The third strategy, "Exhaustive Serial Search," is similar to the second strategy, except that the search *always* runs to the end of the list of digits, regardless of whether, or where, the test digit was on the list.

Flanagan notes that, with regard to the third hypothesis "[i]t is hard to think of examples of situations where we consciously operate this way" (p. 186). He offers as a plausible example of exhaustive serial search a situation in which an eyewitness to a crime looks at criminals in a police lineup. They might look carefully at each suspect in turn, even if they think they have identified the perpetrator prior to reaching the end of the line "just to make sure that they are not mistaken" in their identification (p. 186). Moreover, I think most people have the intuition that we use Mind's Eye Sees All or Self-Terminating Serial Search; at least this has been my experience in talking with undergraduates about this experiment. Yet, as Flanagan explains, the reaction time data support the third hypothesis, Exhaustive Serial Search, showing that we are wrong about how we perform the task.[1]

This seems like a clear victory for objective data over introspection; as Flanagan puts it, "our intuitions are not to be trusted very far in generating knowledge of our mental mechanisms" (p. 188). But things are not that simple, for at least two reasons. First, consider the second case Flanagan analyzes, Shepherd and Metzler's experiment on mental rotation. Here, participants are shown pairs of geometric objects (3D objects presented visually in two dimensions) and are asked whether the two objects in each pair are the same. Within each pair, the objects are oriented differently, either in a 2-D plane or in three dimensions. Participants report mentally rotating one of the objects until it matched (or was in the same orientation and failed to match) the second, and in this case, their introspective reports turned out to be at least partially correct.

[1] Specifically, reaction times vary with the length of the list, ruling out Mind's Eye Sees All, and are the same for trials where the test digit was presented in the original list and trials where it was absent, ruling out a self-terminating serial search.

The reaction time data varied linearly, for both 2-D and 3-D rotations, with the degree of rotation required to determine whether the objects matched.

Unlike Sternberg's experiment, this experiment seems to be a victory for introspection, though Flanagan cautions that, because similar results have been observed when people who have been blind from birth complete a tactile version of the task, the underlying cognitive process is best understood as spatial rotation rather than strictly visual rotation. Still, the point remains that we seem to know what we are actually doing.

But there's a second lesson, I think, that we should draw from the fact that our experiences of solving the Sternberg memory search task don't track the actual search strategy we are using. Recall that Flanagan suggests that it's hard to think of situations in which we knowingly engage in exhaustive serial search, or even exhaustive search in other contexts. (Why are our keys always in the last place we look? Because we stop looking for them once we find them.) Also, imagine a version of the task focused on visual search, without the memory component: if asked whether a digit is present in a short row of digits we're currently viewing, the actual eye (unlike the mind's eye) *would* see all. For longer strings of digits, or cases where we want to be extra careful to avoid mistakes, we might engage in a serial search and stop looking once we found the test digit.

Because of this, I want to suggest that our experience of performing the memory version of the task may be shaped not just by what we are actually doing (similar to the way that our experience of the Shepherd and Metzler task is shaped by the spatial rotation we are actually performing), but also by what we have experienced doing in relevantly similar situations, and by what we think it makes sense for us to be doing. (Flanagan also makes that latter point with regard to the Sternberg task: exhaustive search is an inefficient way of performing the task and not the way an ideal cognitive system would be designed.)

In closing, I will pull together some of the threads from my discussion of first-person reports in early experimental psychology and in medicine, to consider a different way of thinking about the role first-person reports might play in cognitive psychology. The disagreements between Wundt and Titchener, and the weighing of first-person reports and evidence from medical examinations and tests in medicine, suggest that much depends on the goal of investigation. Contemporary cognitive psychology seems to take its goal to be the elucidation of

cognitive processes, so that when first-person reports do not track the cognitive process identified through objective measures, the reports are viewed as simply being wrong. But if we understand the goal of psychology to be broader, so that it also aims to investigate the way in which we think about our own mental lives, it may well be worth separating more clearly the question of how we perform a certain cognitive task from the questions of what we experience when we do so, and of what we understand ourselves to be doing. Addressing these latter two questions may be better served by using interpretive methods, instead of, or in addition to, experimental ones.