

On Doing Psychological Research from the Standpoint of the Subject¹

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The main point I wish to make here is that there is a way of doing psychological research and of generating psychological theory from the *standpoint of the subject*, a way that can be fully justified by the nature of the subject matter and by ordinary canons of knowledge. This way of doing psychology is distinct from the practices common today which are better described as being carried out from the *standpoint of the researcher*.

All method is, or should be, determined by our understanding of its object and of our motive for studying it. If psychological methods are to be judged as being on the wrong track, it is because those methods are dictated by faulty understandings of the subject matter and motive of the discipline. If you believe that psychology studies behaviour that can be adequately characterized in terms of stimulus and response, then ascertaining correlations of independent and dependent variables makes perfectly good sense. If you believe that the point of psychological knowledge is to better control people – as opposed to understanding them – then statistical treatment of large groups of data, again, makes perfectly good sense. I am asserting, however, that these beliefs are theoretically and morally wrong.

I will want to argue that the proper subject matter of psychology is *human subjectivity*. It will eventually be necessary to specify more closely what I mean by this, but ordinary notions of subjectivity will do as a starter, as long as they refer in some way to the *standpoint of the subject*.

It is useful to remind ourselves that, in its beginning phase as a science, psychology naturally adopted conceptions of its subject matter from the standpoint of the subject, that is, as a form of subjectivity.

The first formal psychology in the modern sense was that of Johann Friedrich Herbart (1776-1841), who succeeded Kant at the university in Königsberg. His was a psychology of

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mental contents as experienced by himself and others. We recognize our minds as full of ideas (which he would have called “Vorstellungen”), not all of which at any given time are present to consciousness. Ideas rising into consciousness tend more likely to be ones related to those already there than to be ones that conflict with what is there. Likewise, it is easier to incorporate into our own thinking new ideas that are compatible with what we already know. Herbart developed an elaborate mathematical theory of the statics and mechanics of Vorstellungen to account for such experiences. The theory itself does not appeal to us today, indeed it is probably dead wrong in its analogy to physics, but it remains important that it was grounded in and intended to explain how subjects experience their mental life.

Another example is found in the work of Gustav Theodor Fechner (1801-1887). We may find his psychophysics to be pretty dull stuff today, but it is worth remembering that the object of his measurements was the individual subjective experience of absolute and relative sensory thresholds.

Other prominent examples are supplied by Wilhelm Wundt, Sigmund Freud, the Gestalt psychologists, Max Wertheimer, Kurt Koffka, and Wolfgang Köhler, and Kurt Lewin. The fact that the standpoint of the subject has been taken seriously by many psychologists right up to our own time tells us that the problems we are about to discuss are not new ones, nor are their solutions. The claims that I will finally make about subjectivity have a long and honorable history.

But this historical persistence of taking the standpoint of the subject seriously raises an important question: Why was it completely overshadowed by the standpoint of the researcher, and why has that standpoint come to dominate the discipline to the enormous extent that it has? The answer is of course an historical one that deserves our present attention.

At the end of the 19th century three models for possible psychological investigative practice were relatively well established. The first was that of Wundt in Leipzig, which was directed at a traditional object of investigation inherited from philosophy by a method borrowed from physiology that was experimental and functional. A second model was the demonstration

experiment that developed in clinical practice and was most clearly exemplified in the work of Jean-Martin Charcot in Paris. A third model was the non-experimental psychometric method developed by Francis Galton in London.

The Leipzig and Paris models are in many respects alike. They both require more than casual relations between experimenter and subject. In the Leipzig model experimenter and subject were fully equal colleagues. This can be seen in, among other things, the fact that the roles of experimenter and *Versuchsperson*, the person providing the data as subject, were routinely exchanged. In the Paris model the relationship was one of doctor and patient, usually well established before any experimentation or demonstration began. The conspicuous difference between the two models is that of the power differential between participants. Despite the obvious power differential in the Paris model, however, the knowledge interests of both experimenter and subject are, as in the Leipzig model, identical. Both experimenter and subject in the Leipzig model want to display the same universal mental process. In the Paris model, both doctor and patient are concerned to display the underlying pathology in hopes that such display will lead to understanding and cure. Neither of these models was concerned with the collection of massive amounts of data for the purpose of inductively discovering universal laws like those that under a naive empiricism were thought to characterize the natural sciences.

The London model is very different from both the Leipzig and Paris models. While the relation between experimenter and subject is like the Paris model unequal, it is also an anonymous one. Moreover, the data provided by the individual subject is of no interest in itself, but only in relation to the information provided by a large number of other individuals. What is now being recorded is no longer a mental process but a performance which indicates an ability. But ability is now conceived as something that is distributed in populations. The individual performance score has meaning only as a difference from other scores, or statistically as a difference from the mean of all performance scores. Unlike the Leipzig and Paris models, the London model requires a multiplication of subjects. The information being sought was necessarily statistical. While the immediate interest of Galton and his colleagues was eugenics,

the scientific pretext was the induction of general natural-scientific-like laws. Needless to say, the knowledge interests of experimenters and subjects here were not identical. While subjects may have been amused by the numerical assessment of their abilities, it was unlikely that such knowledge by itself would change their lives. The researchers, on the other hand, would potentially use performance information as a basis for regulating the reproductive possibilities of the subjects, that is, the researchers' interests were ultimately in social control through biological means.

The shift from inner experience or mental process to performance was an important step in the move toward the model that would become dominant in the 20th century, and it was a relatively easy move given the functional orientation that psychology had taken. An early and instructive example of this shift is found in Ebbinghaus' work on memory. As we know, he constructed lists of nonsense syllables which he then learned and then tested himself by re-learning the lists at varying lengths of time lapsed since the original learning. The particular measure that he used was called "savings." This meant that if the original list of n syllables was learned in, say, 10 trials, and after 2 hours he re-learned the same list in 4 trials, he could say that there was a savings of 60%. Memory was then represented by the decline over time of percent savings thus measured.

What is peculiar about such a performance measure is most clearly seen by contrasting it with studies of memory made in Wundt's laboratory at about the same time. In an experiment by one of his American students, K. H. Wolfe, the subject was presented with a tone. Comparison tones were then presented at varying lengths of time lapsed since presentation of the original standard tone. The subject's task was to say whether the comparison tones matched the standard. Memory was then represented by percentage of correct matches.

Ebbinghaus' and Wolfe's Wundtian experiments are superficially alike. Both involve some sort of reproduction and both measure success in percentages. And both showed the kind of decrease in percentage that intuitively matches our experience of remembering. But the differences are profound. What is reproduced in the Leipzig experiment is a subjective

experience. This is the memory itself, the subject's ability to reproduce a once-heard tone and then compare it to presented tones and judge whether or not they matched. The measurement is a mental measurement in exactly the sense in which Fechner had used the term. It is just this subjective experience and immediate mental process that are missing in the Ebbinghaus experiment. Instead, there is a reproduction of a performance, the overt learning of a list of syllables. And the percentage is calculated by comparing a test performance with a criterion external to the immediate performance, in this case one established by the subject himself in the original learning. At no point is the subject in fact asked consciously to remember anything (though doubtless he does remember some of the syllables — the measure is, however, not of that). What is subjectively experienced in this experiment is a matter of total indifference.

I do not wish to be seen as claiming here that Ebbinghaus was not really studying memory. That cannot be disputed. The point is rather that he is doing it in a very different way than Wolfe. It is a way that emphasizes the objective product of performance over the subjective experience and mental process itself. Its object is an individual capacity that must be inferred from the objective product by comparing it with an external criterion. It is a way, in short, that leads, at least potentially, to different kinds of knowledge about memory than that produced in the Wundtian experiment. This will become more obvious when the experiment is adapted to group performances and statistical analysis.

The shift to performance from experience was important for many reasons. Among these was that performances are more easily measured than experiences. Performances can also be detached from the performers in ways that experiences cannot. And performances can be abstracted, grouped, and averaged in ways that experiences cannot.

At the end of the 19th century those who were concerned to call themselves psychologists and thus to establish for themselves a professional identity were facing problems of legitimation. The problem amounted to more than just organizing themselves into a consensual community, which was already being handled effectively through the founding of journals and professional societies. As important, or even more so, Psychologists had to convince the public of their value,

not least those segments of the public who were in a position to bestow significant recognition through the approval of psychology departments and the granting of funds for the pursuit of psychological research.

To this end, psychologists at that time, particularly American psychologists, appear to have recognized the need to fulfill two requirements. The first was to assure the public that psychology was a science. The science that the public understood — or at least acknowledged — and regarded with justifiably high esteem was physical or natural science. There was nothing to be gained through identification with the struggling social sciences, and certainly not with philosophy.

But, as already indicated, the section of the public that was most crucial were those who controlled the purse-strings, and those people would want to be assured of something in return. In addition to being scientific, psychology would have to find ways of being socially useful.

So psychology need to assert itself simultaneously as a pure natural science and as an applied science. To this end, the emphasis on experiment and performance on the one hand, wedded to the Galtonian style of measurement and statistical analysis on the other, promised the right combination.

Application was a consideration for psychology from its earliest beginnings, but the application that most significantly shaped the investigative practice of psychology came about between the turn of the century and World War I. This was signaled by the alliance between psychologists and the newly emerging professional school administrators in the United States. The administration of schools by persons other than teachers was a new development that was associated with the rapid expansion of the public education system, which, in turn, accompanied the expansion of industrial capitalism in the U.S., though there were comparable developments elsewhere. It is reported, for instance, that in the U.S. between 1890 and 1920 there was at least one public high school built for each day of the calendar year, and the enrollment increase for the same period was of the order of 1,000 percent. Between 1902 and 1913 public expenditure on education more than doubled; between 1913 and 1922 it tripled.

Those administering this expanding school system were largely men with what one historian (A. G. Powell, 1971) described as “the deeply felt need of careerist male educators to separate themselves as much as possible from classroom teaching.” “Teaching, by and large,” he wrote, “was viewed as transient, unrewarding, unprofessional, and female. Separate organizations, journals, and university courses were demanded by former teachers seeking a better and more respectable career identity through specialized administrative positions.”

To these potential clients of psychology, wrote another historian (Danziger, 1990, p. 103), “research meant something that was rather different from the latter’s traditional laboratory practice. Research, to the administrators, was an activity whose results had to be relevant to managerial concerns. It had to provide data that were useful in making immediate decisions in restricted administrative contexts. This meant research that yielded comparable quantitative data on the performance of large numbers of individuals under restricted conditions. Excluded was research that went beyond the given human and social parameters within which the administrators had to make their decisions. It was, in other words, technical research that would help in dealing with circumscribed problems defined by currently unquestioned social conditions. Not infrequently, administrators simply needed research for public relations purposes, to justify practices and decisions they judged to be expedient.”

The immediate and pressing concern here was not the experience of the individual pupil or even the process of individual learning as such; it was rather the management of pupils in the classroom so as to accomplish most efficiently the educational goals of the school, which, whatever else they were, involved social control. As an administrator of the time is quoted, “education is a shaping process as much as the manufacture of steel rail” (Bobbitt, 1913). The literature spoke frequently of the extension of Taylor’s principles of “scientific management” from industry to the classroom. It was not uncommon to see parents referred to as “stockholders” and superintendents as “general managers.” This was linked to a widespread equation of the goals of science with the goals of engineering. John B. Watson was not alone in thinking that the aim of psychology was to “establish laws or principles for the control of human action so that it

can aid organized society in its endeavor to prevent failures in such adjustments” (1917). About the same time (1918) that Watson uttered these words, the Laura Spelman Rockefeller Memorial established an endowment in the amount of \$74 million for social research which would yield “knowledge which in the hands of competent technicians may be expected in time to result in substantial social control” (Samelson, 1985). And this is why the shift to performance from experience was significant. Performances are more readily controlled than experiences.

The alliance with school administrators had produced a significant burst of life in psychology that some referred to as a “renaissance.” Joseph Jastrow, an influential psychologist of the time, recalled in 1930 that: “To speak of the renaissance of psychology, especially in the American setting, without explicit recognition of the practical motive would be a glaring omission; for that renaissance found its momentum in the appeal to psychology for the regulation of human affairs. Applied psychology is in many a quarter the pay-vein that supports the mine. The educational application is the oldest and most comprehensive.”

In 1912, Edmund Sanford published an article in the then recently founded *Journal of Educational Psychology* called “Methods of research in education.” In it he listed three basic methods of psychology. The first was what he called “standard laboratory work.” His example was Meumann’s pedagogical applications of Ebbinghaus’ “economic methods of memorizing.” The second was the study of individual differences by means of mental testing, an obvious reference to psychology’s Galtonian legacy. The third was the classroom experiment, about which I will have more to say in a moment. The first and third were useful for picking efficient classroom practices. The second was useful for selecting individuals for particular treatment or programmes.

The second method, mental testing, surely was, to borrow Jastrow’s term, a “pay-vein” for psychology. It was the widespread use of this method in education by psychologists that attracted attention from the U.S. War Department at the time of that country’s entry into World War I and created the special unit of psychologists charged with developing efficient means for selecting officer trainees. Although there is no good evidence that they actually succeeded in this

task, they did attract much attention and were able to use the opportunity to develop new mass-testing procedures. Whatever they did or did not accomplish, it is now generally agreed that it was this professional experience that “put psychology on the map” (Samelson) and created demands for psychological services beyond the boundaries of the educational establishment.

The first and third methods listed by Sanford would soon become a single method, known for most of the 20th century simply as the experimental method. The classroom experiment was first devised in 1908 by a British school administrator, W. H. Winch. His interest at the time was in classroom conditions that contributed to “transfer of improvement of memory.” He selected two classes of comparable ability and subjected one to experimental treatment, leaving the other class to serve as a control comparison. Initially, he simply compared the average performances of the two groups. The defect in this treatment of the data was spotted almost immediately by none other than Charles Spearman who informed Winch about available techniques for evaluating mean differences in terms of probable error. By 1913 these techniques were incorporated into a classroom experiment on conditions affecting the teaching of spelling. The method was so obviously efficient and effective that it attracted wide attention and was soon being adopted on a broad scale in applied settings. Eventually, it was imported into the laboratory where it replaced the “standard laboratory experiment” that Sanford had referred to. By 1923 the method was fully described and prescribed in a textbook by Thorndike’s Columbia University colleague W. A. McCall, *How to Experiment in Education*. Just two years later, this and other similar experimental designs based on the statistical analysis of group differences were codified in R. A. Fisher’s very influential book, *Statistical Methods for Research Workers*. This book became the model and authority for almost all textbooks on statistical analysis and experimental design in psychology and education from that date until the 1970s and beyond.

The total shift from the experimental study of individual experience and processes to the study of the statistical properties of groups of data served the legitimation needs of psychology in two important ways. First, it helped to establish the image of psychology as a natural science. As

we have seen, under naive empiricism, natural science was expected to discover general laws. But individual psychological processes were so variable, so apparently indeterminate, that the eventual discovery of such laws seemed hopeless. But the combining of individual data into groups did produce regularities that had all the appearance of being law-like. Curves of learning and forgetting soon took on the smoothness of expansion curves in physics or growth curves in biology. The fact that individuals seldom behaved in such fashion was easily accounted for by error variance and the inherent complexity of psychological phenomena.

Second, it helped make psychology useful. Given that the most common use expected of psychology was social control and the concern with control was largely administrative and managerial, group data were ideally suited to the task. After all, the effectiveness of administration or management was itself measured in terms of reliable average increases in efficiency and decreases in cost.

Now, with all of this background in mind, let us return to the question of subject matter. Up to this point, subjectivity has been specified only as individual experience. I think that if psychology had retained its original commitment to take individual experience seriously it would have found it eventually necessary to expand the concept to something more embracing. The main problem is that individual experience neither takes place in a social vacuum nor does it take place solely for its own sake. The concept of subjectivity will need, in short, to be contextualized and made more concrete.

But let us begin with the ordinary textbook designation of psychology's subject matter. The last I looked, the textbooks were claiming this to be "behaviour and cognition."

The definition in terms of behaviour, as performance came to be conceptualized, has been dominant in the last 100 years and for good reason. Aside from the historical reasons already mentioned, we note that it is seductively obvious that we do behave and nearly all questions that count as psychological can be translated into questions of behaviour. We recognize neurotic persons, intelligent persons, or extraverted persons by their behaviour. And we can tell when people are learning, when they are perceiving something, or when they are motivated by their

behaviour.

The problem is that behaviour is conceived objectively only, and as such is reduced to responses evoked by or emitted in the presence of stimuli and other environmental conditions. This conception of behaviour is most clearly and forcefully manifest in the methods we use to study it. We spend much time and energy teaching our students analytic statistical techniques that, no matter how simple or complex, all derive from the notion of correlation, the relationship between independent and dependent variables. We are interested in establishing correlations between behavior, responses, or dependent variables and stimuli, conditions, or independent variables because we are committed to an understanding of our subject matter in terms of Humes' notion of causation. Though we often express this commitment in terms of a technical desire to predict, the underlying idea is that of causal explanation. In short, we make no methodological distinction between psychological, biological, or physical events.

But the complaint has been raised many times in the last century that, though this approach to the psychological subject matter seems often to work, it is seldom representative of the ways in which we experience ourselves. When we as psychologists ask our "Subjects" to respond to stimuli, they often do it for us, providing the data we need to test our hypotheses. But all thoughtful subjects, including "Subjects," know very well that it is not the stimulus that finally determines what they do. Among other things, what account for our behaviour are considerations of politeness and decency toward the experimenter and a consequent decision to be coöperative and to follow the experimenter's instructions. These considerations and decisions are usually ignored by psychologists in accounting for their results. Impolite or uncoöperative "Subjects" are simply eliminated as bad apples. But our decision to coöperate or not to coöperate is absolutely essential to how we act and understand ourselves as human beings. Behaviour, understood as a function of experimentally manipulated conditions, is a very poor specification of the subject matter of psychology.

How about cognition? Again, we do not deny that we think or otherwise cognize. But, again as in the case of behaviour, there are few (if any) psychological theories of cognition that

do justice to our experience of ourselves cognizing. The problem here is not hard to locate. Given all the fanfare around the “cognitive revolution” in the 1960s and 70s, it is striking how the methods of investigation failed to change. All that happened was that behavioural dependent variables were abandoned for more cognitive sounding ones. The resulting theoretical models have remained just as remote and mechanical as any account of behaviour. In short, methods are still correlational and explanations are still causal. This was no revolution!

From the standpoint of the subject, I do not respond to the world so much as I act in and upon it. And my actions are always thoughtful in one degree or another. I can think without acting, but I cannot act without thinking, at least in principle. Moreover, my thinking is always for the sake of acting. It is not the mere cogitation portrayed by most psychologies of cognition. The case can be made – convincingly, I think – that thought has its origin in action and remains an essential component of action. Though it has become possible and supremely advantageous for us that we can abstract our thinking from our action so as to think all sorts of possible and impossible things, in short, to be creative, action will always include thought.

What is the nature of the thought that is included in action? It is surely not simply an internal narration corresponding to what one is doing: “Now I am standing, now I am walking, now I am picking up the suitcase, etc.” The important role that thought plays in relation to action is in its motive and intention: “I must catch the train, therefore I will pick up my suitcase and go to the gate.” It is in fact the intention that defines the action, making it the kind of action it is. I cannot do anything except that I do *something*, and that something is determined by my intention. I am not just walking; I am going somewhere. I am not just moving my arms; I am waving to a friend. Etc. It will thus be clear that *it is the presence of intention that defines action* and distinguishes it from reflexes and other bodily reactions to stimuli.

A number of other things can be said about action as we experience it in ourselves. I shall mention four. First, action always involves a *choice*. Whatever we do in any particular situation we could have done something else. However conscious of the fact we may be, we always act in the way that we choose to act. A corollary of this is that we can always have refused to do what

we did.

Second, action implies *agency*. This is most evident when we have urges, desires, or motives that conflict. It is sometimes claimed that it is simply a matter of the stronger desire prevailing. But what determines which is the stronger motive? When Jeremy Bentham confronted this problem, he was forced to say that what was stronger depended on the particular individual. But that only means that the *subject* determines the strength and therefore effectively decides which desire to act upon. The choice, in short, is the subject's. And that means that the subject is also an agent.

Third, choosing to act in some particular way is always directed at the anticipated consequence. Action is *teleological*. This is already implied in the labels we give to actions: sitting down, eating breakfast, taking a shower, making a gadget. And it is the goal of the action that governs judgements of technical appropriateness, but also of the moral and aesthetic appropriateness of the action.

Fourth, actions are *not caused* by external circumstances. Of course they are governed by external circumstances. A major concern of the subject is appropriateness to these circumstances. But the circumstances do not cause the action. It is the subject/agent him- or herself who determines what action shall be performed, and that determination is made on the basis of the reason implicit in the action's motive and intention. It is only this reason that serves as a sufficient condition for action.

Now, a sufficient condition is just what one needs for an adequate causal account. But the reason, which is the sufficient condition, cannot be a cause in the strict sense. Causes, rightly considered, are necessarily independent of their effects. But the reason is not independent of the action. It is part of the action. Without the reason — that is, apart from the thought accompaniment — the action would not be an action but a reflex. The action is the realization of the reason, an intention carried into its objective form. Another way of saying this is that the action is the deductive consequence of its reason, that is, of its motive and intention. The relationship is not causal; it is *logical*.

I suggest that these characteristics of action — choice, agency, teleology, non-causality/ logicality — all imply each other, forming an integral set of mutual interconnections. To exercise choice, we must have agency, which, again, would make no sense if our actions were caused by external conditions. And all of this is necessarily teleological: we choose in terms of anticipated consequences. The entire set of mutual implications is summarized in the word “intentional,” which also carries with it the entailment of thought and meaningfulness.

From the standpoint of the subject, then, intentional action is a justifiable subject matter of psychology. Aside from being intrinsically characterized by choice, agency, teleology, non-causality/ logicality, and meaning, intentional action is also *essentially social*. In a sense, this already follows from what we have said. The choice, agency, teleology, and non-causality/ logicality are all made possible because action is thoughtful and therefore meaningful. But the principle carrier of meaning in our lives is language and there is a good case to be made, as Wittgenstein and others have argued, that language is essentially social. There is no private language because there can be no private meanings. Meanings exist necessarily and only in a basically three-way relation between the subject, the object, and other subjects. If meanings are essentially social and action is essentially meaningful, then action is essentially social.

But it would be too restrictive to claim that intentional action is *the* subject matter of psychology. *Subjectivity* is the broader term that includes both action and all the subjective states, like thinking and feeling, that can go on exclusive of overt action. Moreover, as a designation of psychology’s proper subject matter, subjectivity can legitimately include all the physiological processes, like the sensory and reflexive, that provide the usual supports for psychological activity. With the emphasis on intentional action, subjectivity rises above mere experience to become experience for the sake of acting the world. To be truly subjective is to be effective as an agent; it is to be the subject of one’s own actions.

Finally, what are the methodological implications of a psychology from the standpoint of the subject?

1. **Subjects’ interests.** It should be clear that research from this point of view would have

as a criterion the interests of the subjects.

2. **No deception.** The infamous Milgram experiments as well as many seemingly innocuous uses of experimental deception would find no place in our methods. The usual rationale for deception is that if subjects knew what was going on they would not cooperate or they would influence the results in nongeneralizable ways. But subjectivity can hardly be an “extraneous variable” if it is the focus of the research. And if the research is being carried out in the interests of the subjects, then the subjects will have no motivation for distorting the outcome.

3. **Results for subjects.** Rather than producing experimental results and knowledge about subjects, research from this standpoint will necessarily produce results and knowledge for subjects. The end result should be something that illuminates the experience of subjects and gives them ways of expanding their effective agency.

4. **Sharing expertise.** There will obviously be a difference, including that of power, initially between researcher and subjects. One point of the exercise would be to reduce or eliminate this difference. If special techniques are required, then the subjects should be instructed about them and why they are necessary. This may require that the researcher share specialized theoretical knowledge with subjects so as to raise their level of expertise in the matter.

5. **Whose problem?** From this standpoint, it makes no sense to study problems that are not in fact problems for the subjects. Only problems that touch their lives in significant ways deserve the time and effort of study.

6. **Subjects as co-researchers.** These points can be summarized by saying that the subjects should be co-researchers in any project aimed at studying them. This does not mean that there would be no division of labour. The professional researcher still bears the greatest burden of knowledge and responsibility. But to whatever extent is practicable, the subjects ought to be drawn into the project on an equal footing.

If all of this sounds impractical or outlandish, it will be useful to recall that it has already been done on a fairly broad scale. I am speaking of psychoanalysis. Freud’s theories were generated precisely to illuminate the experiences of his patients. Patients were obliged to learn

these theories so as to participate in their own therapy. In a sense, every patient of Freud's became a fellow psychoanalyst. And their interests were at the centre of the project. One might argue – correctly, I think – that Freud's project was highly imperfect in this regard, but that should not detract us from what, in principle, he was exemplifying and what might be perfected.

There are many who will object that a psychology from the standpoint of the subject as I have described it would hardly count as a science. They might go on to argue that the results of such research are not generalizable.

As for whether it is science, I would argue that the objection is based on a false conception of science. If science is, as its German equivalent “Wissenschaft” implies, the business of gaining real knowledge about our subject matter, then it is precisely a move to lived experience, subjectivity, agency, etc. that is needed, because those are real for us. How can theories of behaviour and cognition that tell us nothing about ourselves, that do not illuminate our experience of the world, that do not yield more effective agency, count as the true knowledge expected of science? A real science must be more responsive to its subject matter. Its methods should be determined by its subject matter, not its subject matter determined by its methods.

As for generalizability of knowledge produced under the methodological considerations I have outlined, one might simply ask how general have the experimentally produced “laws” of psychology been up to now? They have managed to create consensus from time to time, but consensus hardly constitutes the kind of generalizability that science seeks.

But is it not true, one might ask, for instance, that fixed-interval reinforcement universally produces a scalloped cumulative record of responding? Hasn't this been demonstrated reliably among a wide range of species, including humans? Yes, all this is true, but it reveals a serious misunderstanding about generalizability. The “general” here referred to has been called the “abstract general.” It is achieved by pairing away concrete particulars to leave only that which is common among all instances. The only way to reproduce the “law” of the fixed-interval is to eliminate all the complexity that makes up our daily lives. This might be done (providing we cooperate) by putting us in a Skinner Box with absolutely nothing else to do but

press a lever. But these results do not generalize to the complex real world of everyday human experience. That kind of generalization is concrete, not abstract. Freud's theories, once again and however successfully, were aimed just at this kind of generalization. The theory of the Oedipus Complex was intended to illuminate the experience of boys growing up in a male-dominated society, and to the extent that it did this it can be said to have been generalizable. It would be a mistake to expect such a "law" to be true in all cases and at all times. Concrete generals are not like that.

To conclude, a psychology from the standpoint of the subject is manifestly possible. Properly understood, it counts as "more scientific" than most existing psychologies. And it would certainly satisfy the need for application, but its application would be to further the interests of individual human subjects, to help them make sense of their experience, and to expand the effective range of their inherent agency. It would be a psychology for people not just about them.

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