

Use and Misuse of the PSE

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Summary: The principles underlying the development of the Present State Examination and the instruments and computer programmes associated with it are discussed in the light of comments made in three recent papers by psychiatrists trained in the German tradition of clinical phenomenology. Many of their comments are cogent and highly relevant to the future development of the system. Some common misunderstandings are also considered; in particular, those that lead to results being interpreted outside the limits of the specifications laid down. The potential for improvement in the system is emphasized and the next stage of development briefly described.

It is now 15 years since the publication of the first article on a technique of partially standardizing the Present State Examination (PSE), which is used by psychiatrists to assess the presence or absence of symptoms within the general area of the functional psychoses and neuroses (Wing *et al.*, 1967). Before the article, the technique had been developed and tested over a period of five years and the idea had been germinating even longer—for the five years previous to that. The end of a quarter of a century is a convenient point from which to look back, review progress and consider future developments. Such a review is being made at the moment by many people, working in schools of psychiatry across the world. It is far from complete but already it is possible to look with fresh eyes at some of the underlying problems of constructing instruments of this kind.

It is particularly appropriate to set out some preliminary thoughts on these issues in Vienna because of the important contributions made to clinical phenomenology by psychiatrists of the German-speaking world. This tradition, still alive in Vienna, is the basic material for description and analysis on which standardized instruments must entirely depend.

Several German authors exercised a strong influence during the development of the PSE from the second to the seventh edition. Leonhard's influence on the sections dealing with motor behaviour is obvious. In the sections concerned with the subjective experiences underlying psychotic experiences, the German sources consulted were chiefly the psychopathologies of Karl Jaspers and Kurt Schneider. The textbook by Mayer Gross, Slater and Roth was also an important

source. There were, of course, many other influences and I would not wish to burden any of them with responsibility for our mistakes. However, it was no accident that Jaspers and Kurt Schneider were so influential. The reason was that they were among the German authors who wrote most clearly.

The early stages of development were constrained by the need to provide instruments that could be used to test hypotheses in the research programme of the MRC Social Psychiatry Unit (Wing, 1968). The PSE was not constructed for its own sake but because it was needed in specific studies. It is essentially a clinical instrument, based on a method of cross-examination used by a psychiatrist to discover whether specific symptoms are present. Standardization was achieved by providing a glossary of differential definitions of symptoms, a series of questions, probes and optional cut-off points designed to structure the examination, and a set of instructions for rating, in numerical form, the presence and severity of symptoms. The interviewer is free at all times to depart from the format suggested and to pursue any line of enquiry (including a return to a former line of questioning or a jump to a completely different section) suggested by the patient's responses.

As Professor Berner has recently shrewdly noted (Berner and Küfferle, 1982), we naturally brought to the task an empirical, practical bias which is characteristically British rather than German, but we hoped to marry what was best in the two traditions. In trying to do so, we had to accept certain limitations; these are carefully set out in the instruction manual (Wing, Cooper and Sartorius, 1974). It is only if these limitations are respected that the advantages of standardization can be used fully.

The instruction manual, containing the ninth edition

Based on a paper read at the Psychiatrische Universitätsklinik, Vienna, 8 October 1982.

of the PSE (considerably shorter than the sixth, seventh or eighth), and its glossary of differential definitions, was not published until 1974. We had then had 12 years' experience, including two large-scale international studies; the US-UK Diagnostic Project (Cooper *et al*, 1972) and the International Pilot Study of Schizophrenia (WHO, 1973). By that time, other procedures had been added: a Syndrome Check List for rating previous episodes of disorder: an Aetiology Schedule intended to minimise coding errors when specifying the causes of episodes: and a computer programme (CATEGO) for classification. Subsequently, an Index of Definition was added to CATEGO, which incorporates a set of rules defining the threshold points below which too few key symptoms are present to allow a descriptive classification. The thresholds are set quite low, so that there are likely to be more false positives than false negatives (Wing *et al*, 1977, 1978; Wing and Sturt, 1978). These elements taken together constitute the PSE system. It should be noted that the CATEGO programme was not constructed until 1971, which is sufficient demonstration of the fact (frequently not appreciated) that the PSE was not developed as a 'diagnostic instrument'.

The delay in publishing the PSE was deliberate. The authors were conscious both of the limitations of the procedures, always strictly observed in their own work, and of the fact that appropriate restrictions on interpretation would often not be observed when used in studies outside the authors' control. However, it was already available in several editions and their variants and it had been translated into many languages—(now 40 or more)—and pirated versions were beginning to circulate.

Aims of the PSE system

The first and most important use of the material collected by using the PSE system is to describe clinical phenomena clearly, precisely and reliably. If this is achieved, symptom and syndrome profiles and various kinds of scores can be used in clinical research to measure change and for matching and selection. Since the CATEGO rules are invariant, their application to a symptom or syndrome profile produced in such a way should produce a classification which is also useful for reference purposes, for matching and comparison.

This first, descriptive aim is the most important but it can only be achieved within technical limits. One does not criticise a telescope because it cannot be used to look at the back of the moon; it is not intended to look round corners: every instrument must only be used within its specifications.

If we have achieved our first aim, we have created an instrument which, properly used, should facilitate

studies of many different kinds—epidemiological, biological, therapeutic, prognostic and psychosocial.

A second and subsidiary aim is to facilitate the investigation of diagnostic rules and practices. Comparison between the CATEGO classification and, for example, a set of clinical diagnoses, can throw light on aspects of nosology and suggest how they might be improved. At its best, new light might be thrown on the nature of the conditions themselves. This is very far from regarding a CATEGO class (even when data from previous episodes and the Aetiology Schedule are added) as equivalent to a clinical diagnosis, or the computerized rules as equivalent to a nosology. An example will be given later.

The third aim is dependent on, and subordinate to, the others. Since the system is simply a standardization of an ordinary clinical approach, it may have educational as well as clinical uses. Many of those using the PSE have found that their style of interviewing, coverage and ability to describe psychopathology have increased. However, this is still using the PSE as a clinical aid, not as a substitute for clinical decisions.

Limitations

Many limitations are built into the PSE system, and it cannot be used to full advantage outside these limits. Firstly, and most obviously, there are large gaps in coverage; it does not deal in any detail with organic psychoses, with hysterical, subcultural or psychosomatic conditions, or with disorders that require good historical data such as mental retardation, personality variants, alcohol or drug abuse, and chronic disabilities.

The second kind of limitation concerns training. Reliability depends not only on the instrument but on its user. Short training courses lasting about one week are available in several reputable centres, including our own, but they are not sufficient to establish reliability. A trainee must complete at least 20 interviews under proper supervision before it is clear whether he or she can use the techniques. Some (already well-trained clinically within a similar tradition) can use them almost immediately; others, particularly those who find cross-examination distasteful, can never acquire them. Most come somewhere in between. The fact that the PSE, and/or its associated procedures, has been used does *not*, in itself, guarantee the authenticity or reliability of the results.

A number of other limitations arise from the technical restraints imposed in translating the procedures into practice. For example, a time period must be specified. We chose one month. The manual points out that a user can vary this if necessary but, whatever the period chosen, the results must not be interpreted as though they referred to a longer or a shorter time;

experience suggests that users are often not meticulous about this. Similarly, it is necessary to decide how many degrees of severity of symptoms will be rated and how each will be defined; how free the examiner should be to determine the order and form of questions; whether cut-off points should be used; whether or not case-records may be consulted before the interview; what procedure should be followed if a patient is too excited or slow or incoherent or unwilling to take part in a full interview, and so on. A standard instrument and its manual must adopt conventions in such matters; interviewers must accept them, and the results must be interpreted accordingly.

More important perhaps, and certainly more controversial, is what may be called the dictionary problem. No selection of symptoms, and no set of definitions, will satisfy everybody. Nor should it, since few symptoms, at present, can be checked against external criteria. As Karl Koehler has observed, the only interesting question in such circumstances is whether divergent views about particular symptoms can be specified or not (Koehler, 1979). One may be led into interminable discussions about how many angels can dance on the point of a pin, a scholastic indulgence that Jaspers and Kurt Schneider were careful to avoid. I do not think that some other German authors, more interested in interpretative than descriptive phenomenology, have been as successful.

Procedural criteria demand that vague or contradictory definitions must be excluded. *Praecox gefühl*, for example, is a symptom much beloved by several clinicians for whom I have great respect. Not one of them, however, can teach me how to recognise it; therefore it is omitted. Berner and Küfferle (1982) may be right to suppose that British psychiatrists 'rely too heavily on symptoms which are easy to define', but British empiricism suggests that symptoms which cannot easily be defined cannot easily be discussed, or recognised by anyone other than the originators. Their significance, therefore, cannot easily be tested.

Other symptoms are left out because they are both very rare and nearly always associated with a host of commoner symptoms. 'Voices experienced as coming from a part of the body' is such an item, present in the seventh edition but omitted in the eighth and ninth. We may have cut too enthusiastically when reducing the seventh, and then the eighth edition to the size of the ninth, but I do not think that any publication by the original authors, and based on those editions, makes claims which go beyond the limitations of the procedures themselves.

There is another serious limitation in the rules adopted to conflate or classify the data. Symptoms are grouped into 'syndromes'—a word used purely for descriptive purposes and with a precise operational

connotation. The reason for this is to separate out elements that might be useful for subsequent classification, while reducing the amount of information to a size that can be visually appreciated on one profile. Even the 140 items of the ninth edition, though too few for some purposes, cannot be presented in this way. However, it is, of course, true, that the PSE syndromes must be taken or left as they stand, and interpretation of any results based on them must not go beyond these built-in limits.

The same rule applies, *a fortiori*, to CATEGO classes based on PSE symptom-profiles. This is why the authors of the Instruction Manual specifically warned against using CATEGO (even when a Syndrome Check List and Aetiology Schedule have also been completed for key episodes throughout the clinical history) as a prescriptive nosology in the sense that DSM III (APA, 1980) is intended to be used. The term 'CATEGO diagnosis' is deplorable, because a diagnosis can only be made by a clinician utilising all the information at his disposal including, perhaps, his appreciation of 'difficult to define' phenomena. The PSE system can be used by a clinician as an aid to the diagnostic process, but this is a very subordinate aim.

Finally, the limitations on the use of the Index of Definition should be mentioned. Any set of threshold points is bound to be, in some degree, arbitrary; this is true, for example, of a global clinical definition. Specifying the rules lays them open to public inspection and criticism. (This is an advantage, not a disadvantage). However, the threshold level (level 5) of the ID is set below the level at which many British psychiatrists would make a diagnosis (Urwin and Gibbons, 1979; Wing, 1980); this was deliberate because it was thought preferable, in most field surveys, to accept false positives rather than false negatives. Three more definite levels are also provided.

Another, perhaps more important, problem is that sample population surveys rarely catch psychiatric disorders near their peak, whereas a survey of admissions to in-patient, and even out-patient, care would usually do so. The former, therefore, provide a prevalence type of estimate while the latter (if properly based, for example on a case register sample) provides an attack rate. This is not, of course, a limitation of the instrument but of the design of the project in which it is used. It should, therefore, always be remembered when choosing the project design.

Advantages

When the PSE system is used without regard to its specifications, or the results interpreted without consideration of its limitations, we must speak of *misuse*. On the other hand, proper use brings some

definite advantages. The first aim of the system has been reasonably well achieved; the system is useful within the limits laid down. It is impossible to review here the substantial volume of research that has been conducted with its aid, but work on genetics, epidemiology, treatment trials, social causes, rehabilitation, course and services, has been included (for example, Bebbington *et al.*, 1982; Newson-Smith and Hirsch, 1979; Orley and Wing, 1979; Urwin and Gibbons, 1979; Hirsch and Leff, 1975; Leff and Wing, 1971; Leff *et al.*, 1982; Vaughn and Leff, 1976). The second aim was tested in two large international studies and, again, the system appeared to be useful (Cooper *et al.*, 1972; WHO, 1973).

Suggestions for improvement

The question arises, however, whether some of the limitations can now, after nearly 10 years' experience of the ninth edition, be overcome. It would not be worth achieving this at the expense of reliability and comparability of measurement and classification. We should not pretend, for example, that it is easy to quantify or classify reliably many of the most important factors in the clinical history or current concepts of personality disorders.

However, I should like to consider some of the constructive ideas that have been put forward in three recent papers by authors steeped in the traditions of German-speaking psychiatrists (Berner and Küfferle, 1982; Koehler, 1979; Schmid, Bronisch and von Zerssen, 1982). All are in English, addressed to Anglo-American clinicians, and use the PSE as one of the focal points for their analysis. I could have selected many similar papers from British or American sources, and perhaps they may be the subject of another article, but the three I have chosen are fairly representative in that they illuminate several of the basic problems involved in trying to increase the advantages of a set of procedures, such as the PSE system, without, at the same time, increasing the disadvantages as well.

Berner and Küfferle (1982), for example, suggest that it would be useful to regard the PSE symptom of 'hypochondriasis' as a fear of illness, to be included in the section on phobias. This is a valuable phenomenological suggestion and, in a draft for an eventual tenth edition, fear of illness *will* be included. Phenomenologically, however, I suggest that there exist hypochondriacal states that do not show themselves as anxiety, dread or repulsion, nor in the form of depressive or other delusions. There is something to be said, therefore, for retaining the original item as well.

The reason for collecting suggestions of this kind is that it is possible to test the underlying theories, for example that hypochondriasis is a kind of phobia in the sense that its epidemiological characteristics, treat-

ment and course are the same as for similar phobias. This is also true of the idea put forward by Berner and Küfferle that symptoms rated as partial delusions of persecution because they are intermittent are, at least in an unspecified number of cases, more usefully regarded as phobias of persecution. It is not clear whether there is empirical evidence for this but the implications are obvious. Another interesting suggestion is that there should be an item for diurnal elation as well as for diurnal depression. It would be useful to know how frequently the symptom occurs but it could certainly be included in a future edition.

Other suggestions in their paper illustrate different kinds of problem, notably that of definitions usable in practice. This is particularly true of the kinds of thought disorder and affective abnormality that underpin the authors' 'endogenomorphous schizophrenic axial syndrome'. It is reassuring to be told that the component symptoms are definable and that algorithms for combining them have been constructed. This suggests that Berner and Küfferle, too, have found a way to translate complex concepts into simpler ('easy') formulations. When these are made public, it will be possible for trained investigators elsewhere to test their reliability. A demonstration of their utility would be of great value when constructing a more effective set of instruments for clinical use. The point I wish to make is that imprecision cannot be accepted as a virtue. Certainly various kinds of thought and affective disorder are difficult to rate reliably during the course of a clinical interview. Other types of measurement, that are compatible with the interview, may be needed.

Finally, I hope it is now clear that the following assumptions made by Berner and Küfferle are incorrect; that, in the PSE, "delusions, even partial ones, rapidly point to schizophrenia", and that a first rank symptom rated in the PSE inevitably leads to a 'diagnosis' of schizophrenia. Partial delusions of persecution, for example, if the only psychotic symptoms present, are given a very low position in the final level of classification, a level which need not be reached at all if the investigator wishes to use a profile of possible categories rather than one overall class. First rank symptoms are given prominence in this final classification, a fact that, as Kurt Schneider predicted, is empirically in accord with the practice of many clinical psychiatrists throughout the world (Wing, Cooper and Sartorius, 1974, chapter 7); however, this is its only diagnostic significance.

The second paper I should like to discuss is entirely concerned with the question of first rank symptoms. Its great merit is that the author, Karl Koehler, tries to provide a systematic and comprehensive account of the symptoms, with clear definitions, and suggestions as to

how they relate to each other (Koehler, 1979). In doing so, he provides a hypothesis that would explain why, in the International Pilot Study of Schizophrenia, the only important discrepancy, compared to Kurt Schneider's hypothesis, occurred in patients given a diagnosis of mania. On examination of the written examples of first rank symptoms that had been rated in patients clinically diagnosed as manic, most appeared to be rated incorrectly (WHO, 1973, chapter 11). The definitions of first rank symptoms were clarified in the ninth edition but there is still room for improvement.

Koehler simplifies various elaborations of the distinction made by Jaspers between true delusions and delusion-like notions. He suggests that a delusional perception, narrowly defined, occurs almost exclusively in schizophrenia, while delusion-like notions linked to, or provoked by, a perception, can also occur in affective disorders. He gives an example. Does the fact that someone crossed his legs set off a chain of associations that made the patient believe that other people thought he was homosexual? Or did this observation, *in itself*, contain the meaning that people thought this? A positive answer to the first question does not necessarily suggest schizophrenia. Koehler then goes on to analyse all the first rank symptoms in even greater detail.

There are two points here. the first is descriptive. In the section on 'delusions of reference', the PSE does not at the moment attempt to make Koehler's distinction because it seemed unlikely that it could be reliably made in the general conditions in which the PSE is used. The second point is on the significance for classification. In the CATEGO programme, delusions of reference are not a sufficient criterion for Class S. A symptom profile that includes delusions of reference may be classified as Class S, P, M or D, depending on what other phenomena are present. There is another symptom ('primary delusions', No. 82) where the specific experience of delusional perception may be rated; however, it was not found reliable and was therefore moved from its original place in the nuclear syndrome (NS) to a non-specific syndrome (Wing and Sturt, 1978).

These illustrate both the limitations and the advantages of the system. One cannot achieve as exact a description as Koehler would wish, undoubtedly a loss from the point of view of a few very experienced clinicians. Moreover, some cases with delusional perception as the only first rank phenomenon may not be included in the nuclear syndrome (NS). The advantages are greater reliability for research purposes and greater assurance that false positives are excluded from Class S.

The elaborate scheme for defining first rank symptoms proposed by Koehler is of great interest and

raises the question as to whether a further edition of the PSE could incorporate some of his suggestions without sacrificing its present advantages. I will discuss this in the next section.

The third paper is particularly interesting because it is based on the AMDP (*Arbeitsgemeinschaft für Methodik und Dokumentation in der Psychiatrie*), a checklist of symptoms with provision for rating based on a glossary of definitions (AMDP, 1980). A detailed comparison of AMDP and PSE items, leaving aside sections that are deliberately omitted from the PSE, reveals a very substantial overlap. Almost all symptoms are covered by both. The concept of 'Antrieb' (drive) is insufficiently covered in the PSE, and other items, such as 'middle insomnia', excessive appetite and self-mutilation, might well be included. Similarly, the AMDP list could in some ways be extended, and there is no equivalent to the standardised PSE interview nor yet a very extensive literature.

In parenthesis, it should be noted that 'hypochondriasis' is defined in the AMDP much as in the PSE glossary, and separately from the single symptom of 'phobia', which includes all the three PSE symptoms ('situational', 'social' and 'specific'). No provision is made for a phobia of persecution. Professor Berner's comments, therefore, should not be confined exclusively to British practice.

The paper by Schmid, Bronish and von Zerssen (1982) describes results obtained using a checklist of 'present state' items, which has not so far been published, but is based on the AMDP plus 59 unspecified history items. No standardised interview is mentioned. A computer programme, DiaSiKa, classifies this material into 45 'ICD diagnoses'. This is a major development and could perhaps lead to technical improvements in other systems as well.

This project also used 107 (unspecified) items from the 140 in the PSE but not the Syndrome Check List for the present and previous episodes. The Aetiology Schedule was used, in an unspecified way, presumably to rate the present episode. The Aetiology Schedule is intended to avoid coding errors, but apparently did not serve this minimal function in the hands of the local raters, who often diagnosed an organic condition without recording it in the schedule. Any comparisons between classes requiring an examination of the sensorium, or rating of the present or previous episodes, or accurate coding of the Aetiology Schedule, or use of PSE symptoms omitted from the interview, are thereby rendered suspect.

Even if the technical specifications of the PSE system had been respected, however, the purpose of the comparative exercise is questionable. The purpose of DiaSiKa is not clearly explained but it appears to be a prescriptive 'diagnostic system'. One would expect

that clinicians trained in its use, and expected to use its criteria when making their clinical diagnoses, would provide data showing a fair degree of concordance between clinical and computer diagnosis. This is not very illuminating result.

The PSE system, however, although also described in this article as a 'diagnostic system', is nothing of the kind. Clinicians using it are *not* trained in the CATEGO programme and need not agree with or even know its rules. They are free to make their own clinical diagnoses. Whatever diagnostic significance the broad final CATEGO classes possess (assuming they are properly derived and interpreted within the limits laid down) is shown by the fact that they have been found to be highly concordant with clinical diagnoses in many studies, including two large scale and international ones. The DiaSiKa system would presumably not be so concordant, if as seems possible, examination of the algorithms shows it to be different from the PSE system. This would be an interesting result if only because it would indicate that epidemiological, biological, therapeutic or other scientific research projects based on the diagnostic system represented by DiaSiKa, were not comparable with those carried out in other specified parts of the world.

What such an exercise would *not* demonstrate is which diagnostic system was better. It is recommended that the PSE system should not be used in such a way and that research projects should continue to be based on local clinical diagnoses. There is, however, some purpose in making sure that enough patients in the series can be classified in a standard way to ensure comparability with studies carried out elsewhere. Data should, of course, also be given for the clinical diagnoses if these are discrepant from the standard classes, and if it is considered necessary.

Future developments

This brief review suggests that technical improvements might be made to the PSE system that would reduce its limitations without reducing its advantages. The system is flexible enough to allow modifications and additions while retaining the elements of earlier versions. Thus it would be necessary for a tenth edition of the PSE to incorporate the ninth so that comparability with earlier research could be maintained.

This would make it possible to re-incorporate items that were lost when the seventh edition was reduced in size, to add well-tried items from other systems, to try out new items suggested by clinicians from a wide range of psychiatric schools and cultures, to sharpen the glossary of differential definitions and to provide more precise instructions for conducting the interview. Gaps in coverage could be filled by adding optional modules (for example, on the sensorium, alcohol

abuse, long-term clinical impairments, etc.). Such an increased 'item-pool' would make possible construction of algorithms designed to mimic the rules of various diagnostic systems, thus allowing comparison between them. (It would perhaps then be clearer to users why a set of standard instruments should not prescribe any particular diagnostic system).

Prototypes of many of the modules required have been tested and could probably be made compatible with a more extensive system. Providing a reliable account of personality disorders and of the clinical history would be the most difficult task.

A complex documentation of this kind would not look very much like any current clinical instrument, which may be an advantage. On the other hand, it is likely that only a few highly-trained clinical research workers would need to use it. One very practical function, however, might be to help the process of development of the next (tenth) revision of the psychiatric section of the International Classification of Diseases. This revision has already moved towards a greater degree of specification through the publication of DSM III which, although not fully developed for standard application does try to lay down prescriptive criteria for clinical diagnosis.

Since most research psychiatrists will continue to require a more modest and practical set of instruments, it would be necessary to include a simpler version, which could be abstracted from the full documentation and would include the items of the ninth edition of the PSE.

Further development would be required to provide an instrument suitable for surveys of population samples. The Diagnostic Interview Schedule (DIS), designed for this and extensively used in epidemiological studies in the USA, provides a model from which to start (Eaton *et al*, 1981; Robins *et al*, 1981). A combination of PSE 9 and the DIS (called the Composite International Diagnostic Interview) has been produced and is now being tested.

These developments would need the collaboration of psychiatrists throughout the world, and the process of producing and testing a new set of clinical instruments would take several years. The World Health Organisation and ADAMHA recently sponsored a large-scale international review of diagnostic concepts, within the framework of which a Task Force concerned with instruments for standardised assessment and classification was established. A most welcome outcome of this would be if a new drive towards matching technical expertise to clinical experience were to result.

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(Received 6 December, 1982)