

## Commentaries on “First-Rank Symptoms or Rank-and-File Symptoms?”

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There has been much ado over the symptoms which Schneider considered of prime importance in the diagnosis of schizophrenia – too much ado according to Crichton. Crichton's examples are not compelling. I do not think that treatment decisions are often based solely on FRS or that falsifying the pathognomonic hypothesis means that FRS are not useful discriminants in differential diagnosis. The temporal lobe pathology question is neither settled nor central, the semantics of whether FRSs are symptoms is not very relevant, and it is not required that diagnostic indicators have prognostic significance. However, Crichton's conclusions are more compelling than his arguments. So why has the emphasis on FRS not been productive?

Langfeldt (1937, 1939), Eitinger *et al* (1958), and Schneider (1959) gave rise to the hope that a few special psychopathologic manifestations would separate cases of true schizophrenia from schizophrenia-like psychoses. It was thought that thusly defining a putative disease entity would result in greater homogeneity of onset, manifestations, course, and aetiopathophysiology. FRSs came to define schizophrenia in the British Glossary (1968), defined nuclear schizophrenia in CATEGO (Wing *et al*, 1974), and it was even hoped that FRSs would correct the American “all things in excess” diagnosis of schizophrenia. Alas, all this has failed. FRSs and other defining symptoms of nuclear schizophrenia lack prognostic significance (Hawk *et al*, 1975; Carpenter *et al*, 1978; Bland & Orn, 1979; Kendell *et al*, 1979; Stephens *et al*, 1980). While emphasising FRSs, DSM-III introduced prodromal symptoms, duration of illness, residual symptoms, psychosis in the absence of affect disturbance, and social and work dysfunction in order to define a more chronic homogeneous syndrome. Schizophrenia spectrum psychopathology is required to define the more inheritable components of illness in genetic studies. FRSs belong to the reality distortion domain, one of three independent

symptom clusters observed in schizophrenia (Buchanan & Carpenter, 1994; Liddle & Barnes, 1990), and the one which is least relevant to the dissociative pathology of Bleuler (1950) or the avolitional (negative symptom) component of Kraepelin (1919). The heuristic value of FRSs has not been established with any of the myriad of neuropsychological, information processing, neuroimaging, electrophysiology, biochemical, genetic, or neuropathology investigative tools. They appear most relevant to the study of antipsychotic therapeutics, but even here the reality distortion domain must be defined more broadly.

FRSs were at the leading edge of a movement which has given greater attention to reliability and explicit diagnostic criteria. But, as Crichton points out, emphasis on FRSs diminishes attention to signs and symptoms which are more robust for studies of aetiology, pathophysiology, neuroanatomic factors, neuropathology, and onset and course. FRS and associated reality distortion symptoms help assure that the diagnosis of schizophrenia is reserved for patients with a psychotic illness, and have the clinical utility of defining illness attributes most responsive to antipsychotic medication. But it is ironic that neither the manifestation nor treatment responsiveness of FRSs are unique to schizophrenia. It is regrettable that dissociative thought disorder and primary negative symptom pathology was de-emphasised, and now must be resurrected through subsyndrome dissection (Carpenter *et al*, 1993). I agree with Crichton that ideology elevated FRS to worldwide prominence. But FRSs were defined in a manner which facilitated scientific study, and this has led to a more realistic appraisal of their place in the study of schizophrenia. This trend can be discerned in the diminished importance of FRSs in DSM-IV and ICD-10.

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#### Peter McGuffin

This author fails to recognise one of the major merits of first-rank symptoms, that is their high reliability. The good diagnostic agreement between clinicians afforded by 'Schneider-orientated' diagnosis is probably the most important reason why first-rank symptoms have become influential. For example, in a study by McGuffin *et al* (*Archives of*

*General Psychiatry* (1984), 41, 541–545) the diagnosis of schizophrenia based on presence or absence of first-rank symptoms gave a KAPPA efficient of 1 indicating perfect agreement. The same paper also set out to assess validity by estimating heritability and here first-rank symptoms were found wanting. However, in defence of first-rank symptoms it must be said that the initial sample size was small and this was further diminished by the authors' conservative approach in defining first-rank symptoms.

This brings us to another point which is that, in the absence of validating criteria, any operational definition of schizophrenia, is, to a degree, arbitrary. Therefore a statement such as 'FRS have subsequently been found in several conditions' has no real meaning unless we can specify what conditions and by what criteria they are defined. For example, under the CATEGO system (Wing *et al*, 1974) the approach is explicitly hierarchical so that FRS 'trump' any other symptoms. Therefore when one or more FRS is present the diagnosis is automatically schizophrenia. Other sets of diagnostic criteria are claimed by their authors to avoid hierarchies but this is only partly true. For example, if one attempts to interpret DSM criteria explicitly in the form of algorithms which can be written into a computer program such as OPCRIT, it is rare to find patients who are both positive for FRS and who fall into some straightforward diagnostic category such as bipolar affective disorder. Patients with FRS are nearly always categorised either as schizophrenic or in the DSM rag-bag category of atypical psychosis.

Finally, the proposition that Schneider put forward FRS purely on the basis of clinical observation, is not in dispute. Nevertheless it is worth pointing out that all the first-rank symptoms, and not just the ones to do with loss of 'ego boundaries' can be regarded as quintessentially the type of phenomena that Jaspers regarded as 'non-understandable'. The notion of non-understandability is not nearly as abstruse and difficult as Dr Crichton suggests. Indeed it has much in common with the idea of mood incongruent delusions and hallucinations as included in DSM-III and IV. Interestingly such genetic evidence as is available points to affective disorder with mood incongruent psychotic symptoms being more closely related to schizophrenia than to straightforward affective illness.

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**Clive S. Mellor**

This review is a polemic against the value of first-rank symptoms of schizophrenia (FRS) in diagnosing schizophrenia. The author has four main points of attack: the findings of subsequent investigators; the postulated relationship between FRS and the temporal lobes; the apparent random choice of these symptoms; Schneider's personal prestige obscuring inadequate methodology.

Reports are cited that FRS have a high frequency in the mood disorders, but there is some contrary evidence. Tandon & Greden (1987) estimated the diagnostic specificity of FRS for schizophrenia to be 95% and O'Grady (1990), as noted by the author, found narrowly defined FRS occurred only in subjects with schizophrenia and depressed schizoaffective disorder. Akiskal & Pasantian (1979) presented evidence that delirium was a frequent complication of bipolar disorders and could be responsible for the appearance of FRS.

The proposition that FRS have no diagnostic specificity because they are found in temporal lobe disorders, and there is no firm evidence of temporal lobe abnormalities in schizophrenia, is a straw man.

The apparently random, that is non-systematic, identification of FRS is consistent with Schneider's atheoretical approach to clinical psychiatry. When Schneider states that FRS are not symptoms in the same sense as medical symptoms, he is saying that schizophrenia is not a disease with an established physiopathology, but is a psychopathological concept. The symptoms, given his state of knowledge, are the disease.

There is no doubt that Schneider's writings on FRS, by present standards of scientific publication, appear to be ex cathedra (from the professorial chair) pronouncements. FRS are disorders of experience and lack the objective qualities of disorders of behaviour, such as negative symptoms. There are methodological difficulties in their identification (Mellor, 1991). Nevertheless, clinicians continue to find them diagnostically useful. It would be a betrayal of Schneider's pragmatism, however, if we failed to continue questioning their value.

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This paper is another laudable attempt to divide schizophrenia using clinical symptoms. It is especially interesting since it disputes the psychiatric shibboleth of the importance of first-rank symptoms. However except for our desire to have a diagnostic Rosetta stone, it is unclear to me why we should expect schizophrenia to be divisible using clinical symptoms. I am also puzzled why we should expect correlations between clinical symptoms and neuroanatomical changes measured by MRI. Although MRI provides superb pictures of the brain compared to what was available in the past, in reality its measurements are still quite gross. I believe that the ultimate aetiological divisions of schizophrenia will come from the laboratory and that they may not correlate at all with clinical differences or MRI measurements as are presently available.

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**John O'Grady**

There are interesting ideas in this paper but the author tends to present a one-sided argument. His first major point is that first-rank symptoms arose from Schneider's clinical experience and became accepted based on his authority. This is typical of much pioneering work of that time and in essence what Schneider did was to propose a hypothesis that a certain set of symptoms when present were of special diagnostic importance for schizophrenia. This hypothesis is capable of being tested through empirical study and is therefore in the best traditions of a scientific hypothesis.

The other main argument in the article is that symptom clusters divorced from fundamental aetiological, biological or course of illness factors are not unique or fundamental to that disorder. This of course lies at the heart of any symptom-based cross-sectional diagnostic system. The author

should place his criticism of first-rank symptoms in the context of criticisms of symptom-based cross-sectional diagnostic systems (he quotes Boyle, 1990 but others could be similarly quoted). Both DSM-III-R and ICD-10 use essentially Schneider's approach, namely to look at symptom clusters through consensus by clinical experts independent of aetiology or other validating criteria. The author may wish to look at alternative theories of diagnosis (see for example Charlton B. G.: A 'PC' model of the mind (*BJP*, 1995, 167, 149–153)). The author needs to deal with the point made by Wing & Nixon, 1975 (quoted by the author), that such cross-sectional diagnostic symptom clusters stand or fall to the extent that they are "useful". Those authors conclude that first-rank symptoms are indeed useful if defined rigorously and narrowly because they cluster together and can be reliably identified, but point out that no set of symptoms by themselves can ever be 100% accurate or exclusive to that condition. The author needs to deal with further elaboration of Schneider's system as in diagnostic systems such as DSM-III-R.

The author may also wish to deal more fully with Koehler's (1979, quoted by the author) recommendations that first rank symptoms should be viewed as a continuum with narrow and wide definitions employed for practical purposes. The question then arises as to whether the findings by authors such as Taylor & Abrams (1973, quoted by the author), Carpenter (1973, quoted by the author) etc., would be different had those authors used a narrower and more rigorous definition of first-rank symptoms, leading to greater specificity. Lastly, there are studies using factor analytic and cluster analytic studies (Kendall, Brockington, IPSS etc.) which have a bearing on whether Schneider's particular cluster of symptoms are supported through such empirical studies.

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In common with previous commentators, Crichton highlights some of the inadequacies of Schneider's first-rank symptoms (FRS) as a diagnostic marker for schizophrenia. To what extent is his conclusion that clinicians should give them little special diagnostic weight justified? Despite its deficiencies, the diagnosis of schizophrenia remains clinically useful. The use of diagnostic criteria (e.g. DSM-IV,

RDC, ICD-10) both improves reliability and reduces the reliance on any single symptom. The utility of FRS, as with any diagnostic aid, depends on how well they discriminate between patients meeting criteria for schizophrenia and those who do not. To evaluate their performance, we need evidence from a cross-sectional study of patients suspected of meeting criteria for schizophrenia who are assessed both for the presence of clearly defined FRS and independently diagnosed according to accepted criteria (Jaeschke *et al*, 1994a,b). This would allow the calculation of clinically useful indices such as the likelihood ratio which expresses the odds that a given level of a diagnostic test result would be expected in a patient with (as opposed to one without) the target disorder. Using a nomogram, the likelihood ratio can easily be used to convert the pre-test probability of the patient suffering from schizophrenia (which would depend on the presence of other symptoms and the prevalence in the clinical situation) to the post-test probability that the patient will meet criteria for schizophrenia (Jaeschke *et al*, 1994b).

Using a computerised Medline search, we have been unable to find a study of the diagnostic properties of FRS which meets recommended quality criteria (Jaeschke *et al*, 1994a). However, using the data of O'Grady (1990) which was referred to by Crichton, we have estimated that the likelihood ratio of narrowly defined FRS for RDC schizophrenia is around 30 (Geddes & Christofi, 1996). A likelihood ratio of this size is likely to generate large changes from pre-test to post-test probabilities and so it would be premature to relegate FRS to the rank and file. Applying this likelihood ratio, when the pre-test probability of the patient suffering from schizophrenia is around 30–50% (e.g. there is other corroborative evidence from history or examination, or on a ward where the prevalence of schizophrenia is high), the presence of a FRS would increase the probability of meeting RDC criteria to 85–95%. On the other hand, when the pre-test probability of schizophrenia is lower, say 5–10% (e.g. in a community mental health centre or a prison – and when little other history is available), the presence of a FRS would only increase the post-test probability of RDC schizophrenia to around 65–75%. Although these calculations are rough and are only applicable to RDC schizophrenia, they show the power of combining the likelihood ratio of a diagnostic marker with other information, including clinical experience. They also demonstrate why Crichton is right to urge caution in the interpretation of the presence of FRS in many clinical situations where

other information is limited – and why it is so hard to be certain of diagnoses when working in the community!

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#### John Wing

Kurt Schneider, like Alzheimer, Asperger and Kanner, described a clinical syndrome. His claims for it were modest. He did not say that first-rank symptoms (FRS) were essential for a particular diagnosis, nor that they could never occur in the presence of affective symptoms. He thought that most clinicians who recognised the syndrome in their patients' descriptions of internal experiences would, in the absence of evident brain disease, make a diagnosis of schizophrenia. He was right. The US–UK Diagnostic Project and the International Pilot Study of Schizophrenia demonstrated the fact empirically on a large scale. Data analysis (Wing *et al*, 1974, chapter 7) showed that five operationally-defined FRS were highly specific for a diagnosis of schizophrenia, compared with depressive psychosis or mania (probabilities 0.72–0.97, 0.02–0.17 and 0.02–0.05 respectively). In some cases of discrepancy there was good evidence that FRS were not rated according to the operational definitions. FRS were often associated with affective symptoms. The relevant part of the CATEGO computer program used later with PSE–9 was based on such empirical analyses. Category S

can be reached in the absence of FRS. The full printout for a single patient, however, is mainly concerned with clinical description, *not* diagnosis (Wing, 1983).

The overlap and salience, in the clinical profile of diagnosed schizophrenia, of the clinical poverty syndrome ('negative symptoms') with two other types of phenomena (incoherence of speech and therefore of thought, and coherently expressed delusions and hallucinations) were already obvious in the middle to late 1950s and had, of course, been described long before that (Wing, 1961, 1995). The relationships between them are worth study whether or not we use the term 'schizophrenia' to embrace them all. To clinicians (if there really are any) who have only recently become interested in 'negative' and overtly motor problems in schizophrenia, may I suggest a further widening of their horizons? These phenomena (including catatonia) are to be found at high frequency in people with disorders within the autistic spectrum (Wing, L. 1996).

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#### Heinz Häfner

Dr Crichton is right in coming to the conclusion "that no scientific evidence exists in Schneider's writings to establish the special importance of FRS for the diagnosis of schizophrenia". Schneider's only information with regard to quantity, i.e. the small number of unclear diagnoses among 2789 patients between 1932–1936, was based on the diagnoses at discharge carried out by the doctors under Schneider's direction and not on a detailed assessment and analysis of symptoms. In fact the admissions to Schneider's unit were, similar to those in Kraepelin's Psychiatric University Hospital, biased through selection because there

was the Mental Hospital in Haar-Egling with more than 3000 beds that offered its services to the same catchment area. Schneider was criticised early but without success because the definitions of FRS are imprecise and some appear frequently in affective disorders as well (Häfner, 1953; Matussek, 1958). The normative application of FRS in criminal law and in other areas of jurisdiction which for a long time was the prevailing view in forensic psychiatry in Germany was not really justified.

However, some of Crichton's statements evoke slight reservations. The statement that "the heritability of FRS is zero" (McGuffin *et al*, 1984) is based on a single study of the Maudsley twin sample. McGuffin *et al* themselves expressed concerns about a generalisation of their findings. After all, the first findings on the genetic basis of schizophrenia from twin and family studies referred to the identification of the phenotype mainly through positive symptoms.

The statement that there is "no association with antecedents" is not a good argument against the diagnostic validity of FRS. If onset of the disease is defined as the first positive symptom only unspecific and negative symptoms or indicators of neurodevelopmental disorder precede the first psychotic episode. It is in the nature of such antecedents that they do not possess a high predictive power for the appearance of FRS and psychotic episodes in general (Parnas & Mednick, 1991).

This is also true for the statement that FRS do not predict the course of the illness. Generally speaking the predictive power of the positive syndrome – whatever its definition – on the course of other symptom dimensions and on the social development is low or zero (Biehl *et al*, 1986; Andreasen *et al*, 1991; Copolov, 1996). Does the predictive power of single symptoms and particularly of FRS necessarily have to be higher? Most predictor studies do not take into account the fact that negative symptoms often persist from the prodromal phase, whereas positive symptoms on the other hand appear episodically. In any cross-section only 15–20% of the patients actually show psychotic symptoms whereas 60% or more show negative symptoms (Harrison *et al*, 1994; an der Heiden *et al*, 1995). Thus, it is far more probable for a cross-sectional assessment that negative symptoms predict other negative symptoms than it is for positive symptoms in the psychotic episode to predict positive symptoms at late cross-sections. Therefore, the predictor validity of positive symptoms and FRS is not yet explored completely.

Schneider's stress on positive symptoms within the range of diagnostic criteria can be regarded as a

conceptual weakness as long as the concept of schizophrenia, as Schneider himself admitted, is defined as a unity of syndrome and course (Kraepelin). Given this concept – and this goes for all criteria sets in the definition of schizophrenia in ICD-10 and DSM-IV – it is necessary to take into account the more uncharacteristic negative symptoms as well which are more closely linked with the course of the illness than the positive symptoms.

Our concerns with regard to precise boundaries have meanwhile become less grave (Copolov, 1996), particularly because twin studies and epidemiological family studies (Maier, 1995) like Kendell's (1991) multiple-threshold modelling show a continuous psychopathological dimension which ranges from schizophrenia to schizoaffective disorder and depressive disorder. Statistical analysis on the symptom level like Taylor *et al*'s discriminating functional analysis confirms these findings. But Kurt Schneider himself had few doubts that FRS were an appropriate means for differentiation "because we do not find this overlap in our clinical practice".

#### Historical background

Schneider had already emigrated as a professor in Heidelberg when I moved there from Munich. He used to invite me occasionally to afternoon tea in order to report on my work. Although he hardly ever shared my opinions he was always interested to know them. When I started to study the continuity model of schizophrenia he made it clear that I would have had no problems in clearly differentiating between schizophrenia and non-schizophrenic disorders if I only had experienced a more thorough professional training in psychiatry than I had received under Ernst Kretschmer. However, he admitted the rare existence of 'interforms' ("Zwischenfälle") which would today be classified as schizoaffective disorders.

After his habilitation Kurt Schneider gave up carrying out empirical studies and thus in his position as director of Germany's most important research institute in psychiatry of the time did not set an ideal example. At Schneider's time there was already empirical research at the Deutsche Forschungsanstalt für Psychiatrie in Munich using simple statistical methods, e.g. Luxenburger's and Schulz's epidemiological twin and family studies. Moreover, empirical methods were applied to a vast extent by Ernst Kretschmer in Tübingen who tried to confirm his hypothesis on constitution types. The fact that much of this work has been forgotten

today is due to the short memory of science and the disappearance of constitutional theories by taking into account the age variable in the comparison of constitutional types with the risk of schizophrenia and affective psychosis (Zerssen, 1980; Häfner, 1990).

Kurt Schneider's method becomes clearer in the context of philosophical traditions. Karl Jaspers had written his comprehensive book entitled "General Psychopathology" without having much experience with patients – he had only stayed a few months at a ward of the Psychiatric University Hospital in Heidelberg – merely on the basis of publications and patient records before he began philosophy. Schneider's teacher in philosophy was Max Scheler. Schneider used Scheler's layer model of emotions for the conceptualisation of affective disorders. Like Heidegger and Jaspers, Scheler was a student of the founder of phenomenology, Edmund Husserl. The philosophical concept of the term phenomenology has little to do with the meaning of phenomenology in psychiatry. Husserl's explanation of how man gains knowledge consisted in what he called "Wesensschau". The observed phenomena have to be analysed in the form of "Epoché" through ideatoric variation and abstraction, thus revealing their common "eidos" by "evidence". This does not encourage the application of empirical methods. Schneider's concepts follow those of Jaspers and Scheler. However, this strongly simplified phenomenology does not achieve Edmund Husserl's methodological standards.

Schneider's psychopathology tried to combine Kraepelin's classification, which was increasingly felt to be inexact, with Karl Jaspers's extremely detailed psychopathology and his theory of understanding, which was rather doubtful. His book and the small number of other publications from his pen are an extremely clear and radical simplification of the sources with regard to methods and contents. The easy applicability, the clarity and apparent safety of psychiatric diagnoses which once were so difficult to assess, so confused and uncertain, constituted the incredible success of his "New Testament" and of the FRS. However, it should be kept in mind that any science begins with careful observation and description. Even if FRS cannot be rated among first-rank symptoms they are well-observed indicators – though not the only ones – of a reality distortion syndrome which frequently occurs in the presence and more rarely in the absence of negative symptoms. The diagnosis of schizophrenia is a conventional one.

Therefore, criticism of FRS on the basis of current diagnostic conventions would only be of fundamental importance in connection with an external validation. So far, such a validation is still missing.

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#### The meaning of first-rank symptoms

Crichton's critique of the status of first-rank symptoms in diagnosis reveals two questionable assumptions:

- (a) that there is an entity called schizophrenia that can be defined and, with adequate psychiatric expertise, diagnosed
- (b) that the presence or absence of temporal lobe dysfunction can be readily determined by measuring temporal lobe size or blood flow.

Assumption (b) is clearly erroneous. Anyone who believes that existing techniques for assessing brain structure or function have revealed the nature of psychosis, or can be used for diagnosis, is unaware of the limitations of current knowledge.

If Crichton's assumption (a) is correct then he has succeeded where Kraepelin and Bleuler failed. Unfortunately if Crichton does know how "schizophrenia" is to be diagnosed he has failed to communicate this information. Apart from a brief reference to the "until... recently neglected... negative symptoms which may significantly handicap the patient" and adverse comments on the extent to which existing systems (RDC, DSM-III, ICD-10) depend upon the inclusion of first-rank symptoms he offers no advice as to how it should be diagnosed in terms of second or any other rank symptoms.

#### Who decided that "schizophrenia" was an entity?

Kraepelin's late (1920) doubts about the reality of the binary system (the dichotomy of manic-depressive insanity and dementia praecox) that he had created and that has dominated psychiatric classification for a century deserve to be much better known. Schneider's (1957) devastating critique of Bleuler's (1950) claim to have defined a disease entity on the basis of "fundamental" or "basic" or "primary" symptoms is equally neglected (Crow, 1995a). Neither paper has made any significant impression upon prevailing psychiatric opinion that "schizophrenia" is a real entity, distinct from other categories of psychiatric illness, lacking only a rigorous clinical description or a diagnostic test.

The size of the problem was made clear by Endicott *et al* (1982) who applied 11 different sets of operational criteria to a consecutive series of 46 patients (who each met at least one set of criteria for a diagnosis of schizophrenia) admitted to the Psychiatric Institute in New York. By the most

liberal diagnostic criteria (Astrachan *et al*, 1972) over seven times as many patients (44) were diagnosed as suffering from schizophrenia as by the most restrictive (Taylor & Abrams, 1975) criteria (six patients). By the modal set (Research Diagnostic Criteria) it is apparent that as the concept of schizophrenia expands it does so at the expense of patients who could be labelled as suffering from schizo-affective or affective psychoses. The findings are much more consistent with the concept that there is a continuum of psychotic illness (Crow, 1990; Crow, 1995b) that includes affective and schizophrenic realms than that there exist discrete categorical entities. No set of criteria for diagnosis has a claim for greater validity than any other.

Crichton tells us that "between 6 and 23% of manic or manic-depressive patients... had FRS (first-rank symptoms)" but this leaves unanswered by what criteria the diagnosis was established. One of the authorities (Wing & Nixon, 1975) that he (surprisingly) quotes in support of this statement is making the same point as Schneider, that what is at issue is how we are defining psychiatric syndromes. Unless there is an explicit rule for deciding what is or is not "manic or manic-depressive" illness the above statement has no meaning. What Crichton is distressed about is that first-rank symptoms have become, as Schneider proposed, a part of the rules for defining classes. The implication of his complaint is that there are real Kraepelinian categories and there is an independent method of defining them. In this respect I believe he is mistaken. All we have are the symptoms, and our problem is to make sense of them.

#### The epidemiology of first-rank symptoms

Crichton has overlooked that first-rank symptoms do seem to have certain interesting properties that other symptoms lack. He states that Schneider "presented no data to suggest that FRS are anything more than a chance cluster". In the World Health Organization Ten Country study Jablensky *et al* (1992) found that these symptoms could be used to define a core syndrome (referred to as S+ in the CATEGO system), and that when a diagnosis was made in this way incidence rates showed less variation than when broader criteria were used, and did not differ significantly, across centres. In addition to the fact that these symptoms can be relatively reliably assessed by different workers (Wing *et al*, 1974) this may be the critical contribution of Schneider to our understanding of psychosis – that he identified a set of symptoms



that, when they are experienced for the first time, make it highly likely that the patient or his relatives will seek help. Through these symptoms we are able to see (from the WHO study) that nuclear schizophrenia, and perhaps by extrapolation the major psychotic presentations in general, are a "disorder of humanity" (Crow, 1995c).

According to this view nuclear symptoms do not represent the characteristics that isolate one categorical entity from other categories, but rather a boundary that demarcates illnesses of a certain level of severity or non-understandability from those that lack these features. Crichton objects that nuclear symptoms do not have prognostic implications and have "zero heritability", but these claims are limited by the context of the original studies. Whereas some investigators have found that *within* the spectrum of schizophrenic illness (defined by various criteria broader than the presence of nuclear symptoms) nuclear symptoms are neutral with respect to outcome, it is surely self-evident that *within the population as a whole* individuals who experience an episode of first-rank symptoms are more likely than those who have never experienced such an episode to have further psychiatric problems. Likewise such individuals are more likely than the rest of the population to have a first-degree relative with a psychiatric problem, and there is as yet no evidence (although it should be further sought after) that there is a symptom of psychosis that more reliably predicts the presence of such illness than first-rank symptoms. The fact is no one knows what is inherited. It may be a quantity that contributes to an individual's location on a continuum of variation in the general population.

#### First rank symptoms as cerebral dominance failure

With respect to the meaning of first-rank symptoms Crichton omits a crucial element in Trimble's (1990) theory – that the symptoms are signifiers of temporal lobe pathology, *particularly in the dominant hemisphere*. This theory relates these symptoms to what may be the central element in the pathogenesis of the psychoses, the process of establishing dominance in one or other hemisphere. I suggest that it is not that psychotic illness is associated with one or other hemisphere but that there is a failure to establish dominance unequivocally in one hemisphere (Crow, 1995b). This concept is consistent with another hypothesis of the mechanism of first rank symptoms which is that some (e.g. thought insertion, delusions of passivity) arise as alien intrusions into dominant hemispheric consciousness of activity from an independent

"consciousness" in the non-dominant hemisphere (Nasrallah, 1985). If this is so then there may be a sense in which some psychotic patients do suffer from a split brain. In this case nuclear symptoms rather than being rank and file tell us directly about the disease process.

#### Summary

The quest to define categorical disease entities such as are envisaged in Kraepelin's concept of dementia praecox and manic-depressive insanity and Bleuler's group of schizophrenias has not succeeded. Such attempts are arbitrary because the psychoses represent not categories but ranges in dimensions of variation that include the population as a whole. First-rank symptoms are interesting because they identify a boundary that defines a level of severity or non-understandability at which it is highly likely that an individual who develops these symptoms for the first time (or his relatives) will seek help. Psychotic illnesses thus defined are relatively stable across different cultures, a stability that indicates that these illnesses in some sense are disorders of *Homo sapiens*. A hypothesis that relates first-rank symptoms to pathogenesis is that these symptoms arise from a failure to establish dominance unequivocally in one hemisphere and from the intrusion of dysregulated activity from the non-dominant hemisphere into dominant hemispheric consciousness.

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#### Brief response from P. Crichton to some of the commentaries

I am very pleased that Häfner, who knew Kurt Schneider personally, has confirmed my belief that Schneider was dogmatic in his pronouncements on the status of FRS and made no attempt to collect empirical data to substantiate his views on this matter. Thus it is misleading to suggest, as O'Grady does, that Schneider wished to formulate a hypothesis, which (in the sense of Karl Popper) could then be subjected to testing and possible

falsification. The “New Testament” is not a hypothesis. Crow states that my critique of the status of FRS reveals the questionable assumption “that the presence or absence of temporal lobe dysfunction can be readily determined by measuring temporal lobe size or blood flow”. What I have argued, however, is merely that brain-imaging and neuropathological studies to date have not provided conclusive evidence for Trimble's theory. McGuffin believes that I have overlooked “one of the major merits” of FRS, namely their “high reliability”. I would add here: “perceived high reliability”. Schneider describes the FRS somewhat vaguely and gives us only a few brief examples to illustrate what he means by some of these symptoms. Wing *et al* (1974) found it necessary to clarify some of the FRS to improve reliability. But McGuffin seems to have missed the important point made in the paper by Koehler (1979) which I cited: while reliability *within* a study is usually high (as it was in McGuffin's study), the reliability *between* studies by *different* investigators is variable because FRS can be interpreted narrowly or widely along a continuum.

Several commentators have suggested that the debate about the validity of FRS is only part of a larger debate about the validity of the diagnosis of schizophrenia. I agree. For too long, in my view, has the dubious notion of schizophrenia as a single disease entity been propped up by the supposed validity of FRS. But the very important question of the validity of the diagnosis of schizophrenia itself would have to be the subject of a separate paper.

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