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► **To cite this version:**

Hannah S. Decker. How Kraepelinian was Kraepelin? How Kraepelinian are the neo-Kraepelinians? - from Emil Kraepelin to DSM-III. *History of Psychiatry*, 2007, 18 (3), pp.337-360. 10.1177/0957154X07078976 . hal-00570896

**HAL Id: hal-00570896**

**<https://hal.science/hal-00570896>**

Submitted on 1 Mar 2011

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## How Kraepelinian was Kraepelin? How Kraepelinian are the neo-Kraepelinians? – from Emil Kraepelin to *DSM-III*

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*The contents of the third edition of the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders (DSM-III) can only be understood by studying aspects of the last one hundred years of psychiatric history. This paper deals with: (1) three aspects of Kraepelinian psychiatry – descriptive psychiatry, Kraepelin's devotion to empirical research and his inability always to carry it through, and his anti-psychoanalytic stance; (2) the optimistic yet troubled state of American psychiatry in the period 1946 to 1974; (3) the work of the so-called 'neo-Kraepelinians', especially that of Eli Robins, Samuel Guze and George Winokur; and (4) Robert Spitzer and the making of DSM-III.*

**Keywords:** *DSM-III; Emil Kraepelin; neo-Kraepelinian; psychoanalytic psychiatry; Robert Spitzer; social psychiatry; USA*

### **Kraepelinian psychiatry**

As all historians of psychiatry know, Emil Kraepelin grouped together, over a century ago, all the functional psychotic disorders into three large groups: dementia praecox, manic-depressive illness, and paranoia. The first two groups, in spite of being sometimes severely criticized at the outset (Kraepelin, 1919: 3–4), became part of psychiatric tradition, although acquiring new names in the twentieth century.

When Kraepelin's new categories were eventually accepted by the majority of western psychiatrists, an unintended consequence was the creation of a common language for them. This had not been Kraepelin's goal when he

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had first hastily (over the 1883 spring vacation) put together a compendium of psychiatry based on his cullings of current thought, diagnoses and classifications in the German-speaking world. But he had been appalled by the wide differences in terminology and conceptions that had confronted him, and he tried to bring some order to his observations, at least enough to organize a small book (Kraepelin, 1883). Like other contemporary psychiatrists and neurologists, Kraepelin was bewildered by the phenomena he saw or were reported by peers. In subsequent editions of his textbook (grown from his compendium), he was able to group certain behaviours in various categories, but the categories developed slowly during the 1880s and 1890s.

To give the reader a sense of what Kraepelin faced, I am presenting descriptions (later seen as falling into certain groups) of what confronted physicians in an era almost totally devoid of psychopharmaceutical aids. We have a fortunate guide in Kraepelin because he excelled at descriptive prose, partly by deliberate attention to his diction.<sup>1</sup> His detailed descriptions became a pedagogical device, creating at least three generations of European and English psychiatrists devoted to phenomenology. It would be hard to find any psychiatrist who described the symptoms of dementia praecox (especially) and manic-depressive illness with the vividness and thoroughness of Kraepelin's pictures.

His second chapter, 'Psychic Symptoms', in the book on dementia praecox is an intense experience (Kraepelin, 1919: 5–73). In rapid fire, tumbling out on one another, are descriptions of 53 symptoms in 68 pages. In agitated dementia praecox, Kraepelin (1919: 123) vividly describes the frequent hallucinations:

The patients ... see mice, ants, the hound of hell, scythes, and axes. They hear cocks crowing, shooting, birds chirping, spirits knockings, bees humming, murmurings, screaming, scolding, voices from the cellar ... The voices say 'filthy things', 'all conceivable confused stuff, just fancy pictures'; they speak about what the patient does ... They say: 'That man must be beheaded, hanged', 'Swine, wicked wretch, you will be done for' ...

About the exalted ideas also in agitated dementia praecox, Kraepelin (1919: 125) notes: 'The patient feels himself destined to great things, works beside royalty, can put any one into prison, speaks many languages, is to be a professor ... is getting an inheritance from the Australian Kaiser, possesses fifty estates, millions ...'.

Moving to manic-depressive psychosis, Kraepelin (1921: 9) points out that, as in dementia praecox, illusions occur frequently and sometimes outright hallucinations are present:

[The patients'] surroundings appear changed to them; faces are double, dark; their own faces look black in the mirror; they see a blaze of light,

white fumes, 'opium-morphia-chloroform vapour'. [People] look like 'phantoms' ... the physician is 'only a sort of image' of the devil. The chairs are moving ... The patient hears a murmuring and a whispering, a roar, the crackling of hell ...

In *severe* manic excitement, Kraepelin (1921: 125) describes how:

Impulses crowd one upon the other and the coherence of activity is gradually lost. The patient is unable to carry out any plan at all involved, because new impulses continually intervene which turn him aside from his original aim. ... The patient sings, chatters, dances, romps about, does gymnastics, beats time, claps his hands, scolds, threatens, and makes a disturbance, throws everything down on the floor, undresses, decorates himself in a wonderful way.

It is widely known that Kraepelin's legacy does not simply emphasize description. The other features are that the course of an illness must be studied to make a diagnosis, that there are two major groupings of functional psychoses, and that scientific knowledge comes only through empirical research. While accurate, this only partly covers his views – it says nothing about his sophisticated classificatory statements and his confessions regarding his alleged accomplishments. So first we will examine his familiar legacy and then turn to the more obscure.

As the years passed and Kraepelin's initial compendium grew into the textbook, *Psychiatrie. Ein Lehrbuch für Studirende und Aerzte*, each edition larger than the previous one, he slowly worked on developing diagnoses. In the fourth edition (1893), he introduced the term 'dementia praecox' as a diagnostic entity. In the fifth edition (1896) he described his work as a:

decisive step from a symptomatic to a clinical view of insanity. ... The importance of external clinical signs has ... been subordinated to consideration of the *conditions of origin*, the *course*, and the *terminus* which result from individual disorders. Thus, all purely symptomatic categories have disappeared from the nosology. (Engstrom, 1995: 294; Kraepelin's italics)

In the sixth edition (1899) there was a clear dichotomy of endogenous psychoses, the separation of dementia praecox from a newly-named entity: manic-depressive insanity.

Furthermore, Kraepelin's devotion to empirical research is legendary. Two examples will suffice: very early in his career, he came under the tutelage of Wilhelm Wundt (1832–1920), the father of experimental psychology. While in his first professorship at Dorpat (Tartu) in Estonia, then under Russian rule, Kraepelin set up his own equipment for the measurement of mental reactions as well as the mental effects of drugs, caffeine, tea and fatigue. He wanted to apply Wundt's methods to psychiatry in order to construct separate disease categories (Engstrom, 1995: 294). Over the years,

he carried out this work himself or tried to hire staff who were skilled in experimental psychology. Much later, when he held the chair of psychiatry at Munich, he presided over the opening of the German Institute for Psychiatric Research, an institution which was copied throughout the western world. He sought out the most talented scientists he could find to work on pathological anatomy, histology of the cerebral cortex, brain localization, genetics, serology, metabolism and experimental psychology (Engstrom, 1995: 294).

Kraepelin afforded psychoanalysis no place at the Institute because he found it totally unscientific:

We meet everywhere the characteristic fundamental features of the Freudian trend of investigation, the representation of arbitrary assumptions and conjectures as assured facts, which are used without hesitation for the building up of always new castles in the air ever towering higher, and the tendency to generalization beyond measure from single observations. ... As I am accustomed to walk on the sure foundation of direct experience, my Philistine conscience of natural science stumbles at every step on objections, considerations, and doubts, over which the likely soaring tower of imagination of Freud's disciples carries them without difficulty. (Kraepelin, 1919: 250)

Now we turn to lesser-known aspects of Kraepelin's thought, some of which even contradict the ideas for which he is famous.

Kraepelin taught that psychiatrists should avoid postulating aetiologies to make a diagnosis and should stick to the course of the illness, attend to its final state and do follow-up studies where possible. He insisted that his students should not interpret what they saw, but only describe it (Kraepelin, 1907: 127). But he did not take his own advice. In the fifth edition of *Psychiatrie* quoted above, he wrote about considering 'the conditions of origin'. For the aetiology of dementia praecox, he posited a 'disease process in the brain, involving the cortical neurones [and brought about] by an autointoxication ... as a result of a disorder of metabolism' (Kraepelin, 1907: 221–2; 1919: 244). On the origins of hysteria, Kraepelin (1907: 458–9) wrote about 'morbid' constitution, 'defective heredity' and certain environmental conditions. He considered the possibility of uterine disturbances but said the role played by 'the female sexual organs ... is not clear'. In the eighth edition of *Psychiatrie* (1909–15), he considered at length (70 pages) the origins of paranoia: was it an outgrowth of 'the hard blows life delivers to everyone' or was it owing to innate degeneracy where 'morbid germs ... were already present in the disposition' as in a genetic disease like Huntington's chorea? (Kraepelin, 1921: 258, 264). Kraepelin opted for degeneracy.

The Kraepelinian legacy to modern psychiatry included his distinction between dementia praecox and manic-depressive illness and the separation of the healthy and the ill. However, towards the end of his life he regularly said there was nothing holy about his nosology, which might very well change in

the future (Kraepelin, 1913: 345). He even wondered whether his division of the two psychoses was right. In 1920 he wrote: 'we must, then, accustom ourselves to the idea that the phenomena of illness which we have hitherto used are not sufficient to enable us to distinguish reliably between manic-depressive illness and schizophrenia in all cases.' (Kraepelin, 1920/1974: 29; 1920/1992: 528). In addition, Kraepelin was not being very Kraepelinian when he argued that 'there are no fixed, but only blurred borders between mental health and mental illness' (Hoff, 1998: 350).

Finally, Kraepelin openly confessed to shortcomings. Adolf Meyer (1866–1950) and others critiqued Kraepelin's formulation because he did not publish a monograph with a literature review and comments on others' work.<sup>2</sup> Kraepelin took time to reply to this criticism in his *Memoirs*, in essence admitting Meyer's viewpoint. He acknowledged his failure to compare his work with other related sources. He had presented his ideas as 'the current state of knowledge', and then went on to note that he 'simply could not spare the time to substantiate my opinions' (Kraepelin, 1987: 159). Moreover, although stressing the importance of follow-up studies in making diagnoses, he found it hard to pursue this avenue: 'I was soon forced to admit that this work became increasingly impossible with the continuously growing amount of patients.'

To sum up: by today's research standards, Kraepelin's record-keeping and deductions would raise questions about preconceived notions and observer bias. The scientific shortcomings can be seen in Kraepelin's own description of his methods. For all his brilliance in categorical formulations, his legacy is balanced on shaky empirical foundations.

### **Hope and disappointment in American psychiatry, 1946–1974**

While most European psychiatrists continued to follow Kraepelin, US psychiatrists mainly turned away from his approach. The post-World War II period was a tumultuous one for American psychiatry. On the one hand, there was a movement away from biology to environmental and purely psychological foci. It was a time of great enthusiasm, optimism and extraordinary hopefulness. On the other hand, psychiatry found itself under attack from a variety of sources. All this ferment and complexity set the stage for revolutionary changes that were to occur in the 1970s.

When American psychiatrists returned home after the war, many were determined to become psychoanalysts. They had seen with awe that psychiatrists who were psychoanalysts often successfully treated soldiers sent back from the front line with acute cases of combat neurosis. The soldiers were able to emerge from their debilitating shock and return to the front.<sup>3</sup> This was a therapeutic intervention that was largely unknown in World War I.

Psychoanalytic institutes were thus over-supplied with candidates, who, when they graduated, generally went into private practice and into academia,

often holding voluntary appointments in the psychiatry departments of medical schools, where they taught impressionable residents. Psychoanalysis in the USA was also strengthened by the many European analysts who had fled the Nazi terror and filled posts in local departments of psychiatry. Most department chairs of psychiatry in the 1960s were held by psychoanalysts. The belief was strong that psychoanalysis could alleviate most mental illnesses.<sup>4</sup>

One particular Freudian view became paramount. Contrary to traditional models of disease that postulated a sharp line between the mentally ill and the mentally well, Freud had theorized that the mental life of all people ranged along a continuum with health at one end and illness at the other. The work of Meyer was sympathetic to this outlook. Also, he believed social issues had to be addressed to understand and help the mentally ill. 'Hence early treatment in [the] community ... might prevent the onset of severe mental diseases that required institutionalization.' (Grob, 1987: 413). Many psychiatrists began to see their roles as solving the social problems that made for unhealthy and impoverished environments for their patients. In some quarters, psychiatry lost its unambiguous place as a medical specialty.

Early intervention might prevent acute mental illness from moving on to an incurable psychotic state. There thus appeared new roles for psychiatrists in community and private practice. Robert H. Felix (1904–90), the first director of the National Institute of Mental Health from the end of World War II until 1964, urged psychiatrists to 'go out and find the people who need help and – that means, in their local communities' (Grob, 1987: 417). The charge was to find incipient schizophrenics and begin treating them before their disease progressed and required institutionalization.

The American Psychiatric Association (APA) shifted its focus towards the resolution of significant social problems. The federal government also became active in this pursuit. In 1949 the National Institute of Mental Health (NIMH) was created. Its initial goal was to support research into the social bases of mental disorders. Biological research took a back seat. Felix was very active in attempts to convince Congress and philanthropic agencies that mental illness could be prevented.

Meanwhile, American psychiatry was being affected by other trends, 'By the 1960s the legitimacy of institutional [mainly state hospitals] care and treatment had become problematic' (Grob, 1987: 411). Activists in part stirred on by the successes of the civil rights movements sought to move hospitalized mental patients into local communities and treat them there. The activists overlooked the important fact that these chronic patients had often remained institutionalized because they had become elderly and physically sick and had nowhere to go. 'The overwhelmingly chronic population ... contributed to the creation of a depressing' and discouraging atmosphere (Grob, 1987: 412), were not supported adequately by state legislatures, and did not attract a professional and auxiliary staff who were

motivated to provide a therapeutic environment. Most American physicians tended to look down on the psychiatrists who worked in state hospitals, condemning them for the backward and unscientific medicine they practised. Under such circumstances, it was understandable that public hospital psychiatrists began to seek other venues in which to practise and were also drawn to new therapeutic models.

Criticism of state hospitals – and, by connection, state policies – was frequent. Where the states had failed, it was thought, the federal government could succeed. People who were highly placed – the Secretary of Health, Education, and Welfare and the director and deputy director of the NIMH – worked towards developing community mental health centres funded by the federal government and anticipated the demise of the state hospital within a generation. President John F. Kennedy in his address to Congress in 1963 proposed a radically optimistic mental health programme: ‘The new knowledge and new drugs acquired and developed in recent years ... make it possible for most of the mentally ill to be successfully and quickly treated in their own communities and returned to a useful place in society.’ Such ‘breakthroughs,’ he added ‘have rendered obsolete ... a prolonged or permanent confinement in huge, unhappy mental hospitals.’<sup>5</sup>

While this idealized plan never came to fruition, partly for lack of funding, the state hospital systems did see a remarkable decline in their population for reasons unconnected with the goals of the NIMH. New effective antipsychotic drugs had been developed. The state legislatures were seeking ways to cut the high cost of maintaining state hospitals. The hospitalized mentally ill were either released to communities, often with little care, or sent to nursing homes paid for by the new federal programs of Medicare and Medicaid.

The image of American psychiatry took a downward turn. The psychoanalysts and the social activists had promised more than they could deliver, and an inevitable disappointment occurred both within the profession and among the public at large. Moreover, the profession began to suffer many serious challenges. An ‘anti-psychiatry’ movement began to form in the 1960s. It became so broad that when the APA met in Miami for its 1969 annual meeting, a small plane flew back and forth pulling a banner, ‘Psychiatry Kills’ (Stone, 1976: 17). One critic came from within its own ranks. The psychiatrist Thomas Szasz – today, still strong in his original opinion – published *The Myth of Mental Illness* (1961), contending that psychiatry declared as illness the non-conforming behaviour it found threatening; therefore, mental illness was a myth (Dain, 1989: 8; Mayes and Horwitz, 2005: 252).

Szasz was not alone in his criticism of psychiatry. The French social philosopher, Michel Foucault (1926–84), although not calling mental illness a ‘myth’, agreed with Szasz in finding psychiatry an authoritarian extension of the attempt by the state to control non-conformists. In his book *Madness and Civilization* (1961), Foucault argued that the so-called ‘humanitarian’ treatment of madness in the eighteenth century was really punishment of the

afflicted person until he or she learned to act 'reasonably' (Dain, 1989: 8; Mayes and Horwitz, 2005: 252).

Within twentieth-century American psychiatry there were also the so-called 'radical therapists' who argued for the social, cultural, economic and political determinants of mental disorders. They 'saw the goal of traditional psychiatry as the maintenance of personal and professional power and prestige, economic well-being, and control over others' (Talbot, 1974: 122).

Some sociologists added their voices to the attack – for example, Thomas Scheff (b. 1929) who is known for his 'labeling' theory: mental disorder is a label behind which psychiatrists hide because they do not know the real causes of unconventional behaviour (Mayes and Horwitz, 2005: 252). The sociologist Erving Goffman (1922–82) saw in mental hospitals a system that infantilized and oppressed the patient population.

The first wave of feminists in the 1960s and 1970s joined the chorus. They were angry about Freud's views on women and attacked psychiatry because it was so heavily psychoanalytic. They saw Freud's declaration that 'anatomy is destiny' linked to his views of women as being morally deficient to and less altruistic than men. Furthermore, his conclusion that 'equality of the sexes is impossible because of their different roles in the process of reproduction'<sup>6</sup> bore out negatively the notion that 'anatomy is destiny'.

The year 1973 was an especially bad one for American psychiatry. In January the prestigious journal *Science* carried a sensational article by the Stanford psychologist and lawyer D. L. Rosenhan (1973). The article purported to show that American psychiatrists had no scientific standards for making a diagnosis and also that a patient's incarceration in a mental hospital was an irrational, even bizarre, experience. Rosenhan orchestrated the secretly planned admission of people with no psychiatric illness to a variety of psychiatric hospitals. They were all given the unvarying diagnosis of schizophrenia when they appeared in admission wards complaining of hearing the words 'empty', 'hollow' and 'thud'. The day after admission they stopped talking about hearing voices and acted normally; nevertheless they remained confined – one pseudo-patient for as long as 52 days. Rosenhan then presented evidence to charge that American psychiatrists could not 'distinguish the sane from the insane' (Rosenhan, 1973: 257).

The year ended as problematically as it had begun. In December, the APA voted to delete the diagnosis 'homosexuality' from its *Diagnostic and Statistical Manual, DSM-II* (APA, 1968) and replace it with 'sexual orientation disturbance'. The APA had been under heavy pressure from activists in the Gay Liberation movement which had been demonstrating at psychiatric meetings for several years. The public response to the APA decision was predictable. The entire process 'seemed to violate the most basic expectations about how questions of science should be resolved' (Bayer, 1981: 3). American psychiatry, already seen by many professional and lay critics as not a part of medicine, slipped even lower in the estimation of their detractors. It is

important to realize that the anti-psychiatry movement was not marginalized. It was accepted by many college students and intellectuals, and was part of the anti-authoritarian stance of the late 1960s and early 1970s.

American psychiatrists were also besieged from another direction. The third-party payers of psychotherapeutic treatment mounted a campaign to pay only for 'real diseases', not for upsetting life situations. They wanted accountability for outcomes of psychiatric treatment; psychotherapy was to them a bottomless pit. Increasingly, the insurance companies and the Federal Government were sceptical about the legitimacy of psychiatry (Mayes and Horwitz, 2005: 253). Moreover, in their eyes, psychiatry's competitors – psychologists, the clergy and lay therapists – could do the same work more cheaply (Mayes and Horwitz, 2005: 257).

Arising from these events came a challenge, ultimately successful, to return mental health care to medically-minded psychiatrists, to shut the psychoanalysts out of psychiatric dominance, and to refocus American psychiatry on studies that rejected environmentalism and the mental illness–mental health continuum (Klerman, 1977; Wilson, 1993). These physicians called for what they deemed to be a scientific psychiatry – they were the so-called 'neo-Kraepelinians'.

### **The neo-Kraepelinians**

In the 1960s and early 1970s a small band of psychiatrists at Washington University in St Louis were dissatisfied with and critical of the state of American psychiatry. In their view, here was a psychiatry that dealt in non-psychiatric pursuits, had largely eschewed the medical model, did not value diagnosis and classification, rejected sharp distinctions between mental illness and mental health, and seemed unbothered by the abysmally low scores of inter-rater reliability – two or more psychiatrists coming to the same conclusion about the diagnosis of a patient. The Washington University psychiatrists and their few sympathizers believed that only empirical psychiatric research with a strong focus on biology held any hope for the treatment and improvement of the mentally ill. The domination of American psychiatry by psychoanalytic and psychodynamic thinking, they felt, was responsible for its unscientific character. What was needed, then, was a psychiatry that limited itself to the description of the mentally ill and avoided speculation about aetiology because it was unknown for almost all psychiatric diseases.<sup>7</sup> In addition to description, the course of illness should be observed, and case follow-up and family histories should play significant roles in diagnosis. Descriptive psychiatry would lead to better communication among all psychiatrists, which would be the first step towards accurate research, the only path to progress.

The leaders of the Washington University group were Eli Robins (1921–94), Samuel Guze (1924–2000) and George Winokur (1925–96). These men

were all of the same generation, and they shared science and socializing. For years they ate lunch together every day, brainstorming, sounding out ideas, each drawing from the others the emotional conviction that they were on the right track. At night there were legendary parties, usually given by Robins and his wife. The isolation of these men from the psychiatric establishment drew them ever closer.

In their residency programme, the 'Wash. U.' psychiatrists trained embryonic psychiatrists in descriptive psychiatry and imbued them with their own fervour. John Feighner, a resident who arrived in the training programme in 1966, later wrote:

It became painfully clear to me that the state of the art of psychiatric diagnoses was frankly in a mess. Trying to draw conclusions from the scientific literature with regards to virtually any area of the major psychiatric disorders was extremely difficult. Patients that were described in one article as having acute schizophrenia, showing a very positive response to electroconvulsive therapy (ECT), seemed quite different from patients described in other articles as having a similar disorder and responding poorly to ECT but positively to neuroleptics. Also, with the progressive use of lithium and other more specific pharmacological treatments at that time, it seemed imperative to me that we refine our diagnostic criteria to assist us in selecting specific treatments for specific patients and to improve communication between research centers. (Feighner, 1989: 14)

Feighner found that in his contacts with his teachers, particularly Robins with his 'no-nonsense data-oriented approach', it was apparent that something should – and even better – could be done. So in his third year as a resident, Feighner began to develop diagnostic criteria for the affective disorders and discussed with Robins, Guze and Winokur the possibility of expanding the criteria to include other psychiatric disorders.<sup>8</sup> During Feighner's fourth year he began to do this and, with the 'Wash. U.' triumvirate, set up a committee. Then he reviewed almost 1000 articles and from the data obtained he proposed criteria for a variety of disorders. The criteria were discussed by the committee, and Feighner and others published a paper in the *Archives of General Psychiatry*. Although the co-authors of the article included Robins, Guze and Winokur, the diagnostic criteria become immortalized as the 'Feighner criteria'. It also turned out that this paper became the most cited paper ever published in a psychiatric journal (Feighner, 1989: 14). Robins said it was one of the two most important papers he had ever written.

This paper had been foreshadowed by one published two years earlier (Robins and Guze, 1970) which did not spell out diagnostic criteria for specific psychiatric disorders, but did set out five steps thought necessary to develop a valid classification: (1) clinical description; (2) laboratory studies (which they admitted did not exist for 'the more common psychiatric disorders');

(3) exclusion criteria to weed out patients with other illnesses; (4) follow-up studies; (5) family studies (Robins and Guze, 1970). The paper included the memorable phrase, 'classification is diagnosis' (p. 983).<sup>9</sup> Feighner *et al.* (1972: 57) repeated the five steps and announced that their

communication is meant to provide common ground for different research groups so that diagnostic definitions can be emended constructively ... The use of formal diagnostic criteria by a number of groups should expedite psychiatric investigation.

Then the authors took issue with the APA's current *Diagnostic and Statistical Manual (DSM-II)*, which was heavily influenced by psychoanalytic thought. They said that in contrast to *DSM-II*, 'in which the diagnostic classification is based upon the "best clinical judgement and experience" of a committee and its consultants, [their] communication will present a diagnostic classification validated primarily by follow-up and family studies' (p. 57).

Feighner and his colleagues had prepared specific diagnostic criteria for sixteen psychiatric illnesses and for secondary depression.<sup>10</sup> As an example of their overall proposal, this is their entry for schizophrenia (p. 59).

**Schizophrenia.**—For a diagnosis of schizophrenia A through C are required.

A. Both of the following are necessary: (1) A chronic illness with at least six months of symptoms prior to the index evaluation without return to the premorbid level of psychosocial adjustment. (2) Absence of a period of depressive or manic symptoms sufficient to qualify for affective disorder or probable affective disorder.

B. The patient must have at least one of the following: (1) Delusions or hallucinations without significant perplexity or disorientation associated with them. (2) Verbal production that makes communication difficult because of a lack of logical or understandable organization. (In the presence of muteness the diagnostic decision must be deferred.)

C. At least three of the following manifestations must be present for a diagnosis of 'definite' schizophrenia, and two for a diagnosis of 'probable' schizophrenia. (1) Single. (2) Poor premorbid social adjustment or work history. (3) Family history of schizophrenia. (4) Absence of alcoholism or drug abuse within one year of onset of psychosis. (5) Onset of illness prior to age 40.

The paper concluded with the message that 'what we now present is our synthesis of existing information, a synthesis based on data rather than opinion or tradition' (Feighner *et al.*, 1972: 62).<sup>11</sup>

The 'Wash U.' physicians and others who shared their interest were given the name 'neo-Kraepelinians' in 1978 by a Harvard psychiatrist Gerald Klerman (1978: 104 ff.). He reported that there was a Kraepelinian revival

among researchers and academicians while there was a Meyerian and Freudian decline. Klerman (1978: 104–5) synthesized a nine-point ‘credo’ of the neo-Kraepelinians.

1. Psychiatry is a branch of medicine.
2. Psychiatry should utilize modern scientific methodologies and base its practice on scientific knowledge.
3. Psychiatry treats people who are sick and who require treatment for mental illnesses.
4. There is a boundary between the normal and the sick.<sup>12</sup>
5. There are discrete mental illnesses. Mental illnesses are not myths. There is not one but many mental illnesses. It is the task of scientific psychiatry, as a medical specialty, to investigate the causes, diagnosis, and treatment of these mental illnesses.
6. The focus of psychiatric physicians should be particularly on the biological aspects of mental illness.
7. There should be an explicit and intentional concern with diagnosis and classification.
8. Diagnostic criteria should be codified, and a legitimate and valued area of research should be to validate such criteria by various techniques. Further, departments of psychiatry in medical schools should teach these criteria and not deprecate them, as has been the case for many years.
9. In research efforts directed at improving the reliability and validity of diagnosis and classification, statistical techniques should be utilized.

#### *The careers of Robins, Guze and Winokur*

Eli Robins was an intellectual and a voracious reader. He had enormous energy, running two laboratories simultaneously while attending to his clinical responsibilities. As catalyst, mentor and a man of great charisma, he fulfilled the role of an exemplary department chair. He published over 175 works and in almost all of them was the consummate biological psychiatrist. He achieved all this in spite of the fact that for a large part of his life he suffered from increasingly debilitating multiple sclerosis.

After residencies in both psychiatry and neurology, Robins arrived at Washington University in 1949. Beginning in the 1950s, he published throughout his life in the fields of brain neurochemistry and histology, concentrating in the areas of suicide, depression, schizophrenia and alcoholism. In the 1960s he developed an interest in the problem of vague descriptions of psychiatric disorders. This culminated in the 1970 paper that he wrote with Guze on the establishment of diagnostic validity in psychiatric illness (discussed above). A good summary of Robins’ interests and beliefs is contained in a short article he wrote for family doctors and internists (Robins, 1977). In 1974, with Robert Spitzer, Jean Endicott and others,

Robins presented a paper on research diagnostic criteria at the First CNS Symposium of The Squibb Institute For Medical Research (Spitzer, Endicott, Robins, Kuriansky and Gurland, 1975*b*), officially bringing together Robins with Spitzer and Endicott who were (and still are) at the New York State Psychiatric Institute (Garfield, 1989).<sup>13</sup>

Samuel Guze went to medical school and served his internship at Washington University. Even when a student, Guze thought psychoanalysis was 'baloney' (Guze, 1994). He began a residency in medicine at the university but did not enjoy his work. Nevertheless, he served the third year of his medical residency at the Veterans Administration Hospital affiliated with Yale University. He returned to Washington University in 1950 to work in a division of psychosomatic medicine. Guze remembered this as a 'wonderful' year. It was then also that he met Robins and George Winokur.

Guze next took a residency in psychiatry and ended up with a joint appointment in medicine and psychiatry. He recalled that in the 1950s there were many psychiatrists who were psychoanalytically oriented, 'and you had to know the language. Even at [his] most critical, [he] always told the residents you had to know the language in order to be critical [of psychoanalysis]' (Guze, 1994).

By 1955 Guze was thinking of going into an internal medicine practice, but he was offered a leading position in the psychiatry department. In the late 1950s,

Winokur, Robins and I suddenly realized we were now in a position to try to shape the department in the direction we thought it should go. We didn't want a psychoanalytic department, we wanted a broad research effort, and we wanted to put tremendous emphasis on improving the diagnostic system in psychiatry. (Guze, 1994)

The department chair backed the three men. Another helpful factor was that most of the psychoanalysts in St Louis went into private practice after World War II: 'So we didn't have to cope with full-time and therefore influential psychoanalysts.' As Guze, Robins and Winokur discussed things over lunch, they thought that they 'could really make a dent in American psychiatry' (Guze, 1994).<sup>14</sup>

Change began to come from more than one direction. The psychoanalytic hegemony was on the wane. The introduction of psychopharmacological agents offered a new area for research. And there was a growing interest in genetics. This pleased Robins and Guze in particular, because this was an area they wanted to work in. Guze became one of the first Americans to use studies of twins as a means of identifying the role of heredity in psychiatric illness. Also they began to realize that

there were people around the country who felt that they wanted something different and were looking for some place to take the lead. For many years

that was a big advantage to us when it came to recruitment. Residents who were looking for something other than psychoanalytic training were always told to go out to St. Louis. We got a lot of interesting residents. (Guze, 1994)

Guze served as Vice-Chancellor for Medical Affairs and President of the Washington University Medical Center from 1971 to 1989. He was head of the department of psychiatry from 1975 to 1989 and again from 1993 to 1997. These administrative positions placed him ideally to support the research and biological work of the psychiatry department (Harris, 2001).

Guze's main contribution to descriptive psychiatry was his goal of re-medicalizing the psychiatry of the 1950s and 1960s. In a number of papers and finally in a book he insisted on the primacy of the medical model in psychiatry. This provides one of the strongest links between Kraepelin and Guze, making the label 'neo-Kraepelinian' valid in his case.<sup>15</sup>

The third of the 'Wash. U.' triumvirate was George Winokur who came to St Louis in 1951 to serve on the faculty of the psychiatry department. He quickly developed a close personal and working relationship with Robins and Guze. Winokur remained at the university until 1971 when he accepted a chair and the chairmanship of the psychiatry department at the University of Iowa College of Medicine (Tsuang, 1999). In this way, the 'Wash. U. approach', as Guze termed it, was carried to another medical school. Winokur focused on affective disorders and schizophrenia, writing a valuable monograph on manic depressive illness with two younger authors at 'Wash. U.' This book provides one of the clearest pieces of evidence of the connection of the 'Wash. U.' psychiatrists with Kraepelin (Winokur, Clayton and Reich, 1969). Winokur, using family history data, distinguished between unipolar depression and bipolar disorder.

Carrying further his dual interests in genetics and affective disorders, Winokur investigated genetic linkage of known genetic markers with supposed genes for affective disorders. From his clinical work, he noticed a dearth of cases in which both father and son had bipolar disorder, although other types of parent-child transmissions were relatively common. This idea stimulated a great deal of interest, and work on it has continued into the twenty-first century. Continuing the 'Wash. U.' stress on follow-up studies, Winokur took part in the Iowa 500 Research Project which involved the long-term (30–40 years) follow-up of patients diagnosed with schizophrenia, depression and bipolar illness (Winokur and Tsuang, 1996). Winokur's great interest in genetics provides another link to Kraepelin.

### **Robert Spitzer and the making of *DSM-III***

The drastically revised *Diagnostic and Statistical Manual of Mental Disorders* was produced under the direction of Robert Spitzer. *DSM-III* truly wrought a revolution in American psychiatry and, to some extent, in psychiatry

worldwide. But the preceding sections of this paper show that the revolution was not a *deus ex machina*: the production of *DSM-III* cannot be understood without considering Kraepelin's own revolution, the history of American psychiatry before Robins, Guze and Winokur met, and the work done at Washington University in the 1950s, 1960s and 1970s.

Spitzer is one of the most influential persons in twentieth-century American psychiatry. Therefore, it is of no small significance that he has formally stated he is not a 'neo-Kraepelinian' (Spitzer, 1982: 592).

Spitzer's early medical career followed the usual channels of the 1950s and 1960s, and when he finished his residency, he went into psychoanalytic training and practised psychoanalysis for a while. But Spitzer also stood out: he published three papers in a highly ranked journal while still in medical school; while he was in psychoanalytic training, he was a research fellow in biometrics, co-principal investigator on 'Anamnesis and social adaptation of mental patients' with a grant from the NIMH; and he took a course at IBM on data processing, computer programming and using a computer language, FORTRAN. He had other positions and grants that were not related to psychoanalysis, and he eventually stopped doing analysis because he did not find it satisfying.<sup>16</sup>

Spitzer's route to become the head of the Task Force to produce *DSM-III* was complex.<sup>17</sup> He became involved in a crisis on the issue of whether homosexuality was a psychiatric disorder and belonged in the *DSM*. Feelings ran very high, but Spitzer skilfully defused the crisis by suggesting 'homosexuality' should be dropped as a psychiatric disorder and replaced with 'sexual orientation disturbance'. The APA leadership was impressed, and aware of his prior experience with *DSM-II*, appointed him to the recently vacated chair of the Task Force on *DSM-III*. As Spitzer and others tell the story, he probably got the job because it was not considered very significant. No one paid much attention to *DSM-II* and especially the psychoanalysts who did not believe diagnosis was very important.

But the APA leadership underestimated Spitzer. He is an unusually gifted and talented man, a convincing writer and debater and a resourceful pragmatist. He was ambitious, had great energy and was a very hard worker. The revolutionary way in which Spitzer was preparing the *DSM-III* put him in the hot seat. Analysing himself in 2003, Spitzer said 'there is something in me that is always looking for trouble or something to challenge the orthodoxy' (Drescher, 2003). But at the same time, he was determined to succeed. Discussing how he responded to criticism of the Task Force's activity, Spitzer (2001) explained:

[W]e attempted, whenever possible, to respond to criticism by coming up with some solution that might at least partially satisfy our critics, provided we did not give away the store (so to speak). A good example ... is the origin of the famous *DSM-III* multiaxial system. The actual impetus for

this was to meet the mounting criticism that by developing such a large and seemingly authoritative diagnostic manual, American psychiatry was giving the impression that the only important part a psychiatric evaluation was making ... a psychiatric diagnosis. ... Providing a multi-axial system that included physical disorders (axis III), psychosocial stressors (axis IV), and level of functioning (axis V) enabled DSM-III to be presented as within a broad biopsychosocial model – rather than the narrow diagnostic model that its critics feared. We are proud of the DSM multi-axial system, but its innovation was in response to criticism.

Spitzer, the pragmatist, concludes: ‘It is better to win (by offering your critics something) than to lose (offer them nothing and have the entire project stop – as several times seemed possible).’ (Spitzer, 2001: 358).

Spitzer threw himself into his job as head of the Task Force. He worked 12–16 hours a day and at weekends. (His marriage broke up, partly owing to this enormous work schedule.) The Feighner criteria had a great influence on Spitzer’s thought, and by the early 1970s he had met Robins when they were both involved in a project on the psychobiology of the depressive disorders sponsored by the Clinical Research Branch of the NIMH. With Robins, Spitzer compiled a list of 25 research diagnostic criteria (RDC) for the field of psychiatric research,<sup>18</sup> that is, nine more than in the 1972 paper by Feighner *et al.* The 25 RDC were first presented at a conference in June 1974 and published the next year (Spitzer *et al.*, 1975*b*). Thereafter, there were several revisions and some fine-tuning.<sup>19</sup> Immediately Spitzer, Endicott, and Robins (1975*a*) proposed that the RDC be included in the upcoming edition of the APA’s *Manual, DSM-III*. They argued that this should be done to improve the training of psychiatric residents and other mental health professionals and improve communication among them. Interestingly enough, Spitzer *et al.* (1975*a*: 1191) wrote: ‘the criteria that may be listed in DSM-III would be “suggested” only, and any clinician would be free to use them or ignore them as he thought fit.’ This, of course, never happened.

Spitzer also hoped the very specific diagnostic criteria in *DSM-III* would also improve diagnostic reliability. He declared that the RDC had already shown this was possible. Many studies had demonstrated that diagnostic reliability among psychiatric examiners was highly unsatisfactory. Diagnostic reliability was acceptable for just three categories: mental deficiency, organic brain syndrome and alcoholism. The level of reliability was only fair for psychosis and schizophrenia. For every other category, it was extremely poor. If inter-rater reliability improved, argued Spitzer, then the psychiatric profession would get improved validity, which he defined as the ‘utility of the [classification] system for its various purposes’ (Spitzer and Fleiss, 1974) – clinical, research and administrative. Of course, Spitzer concluded, the categories would undergo extensive review and revision before there could be a recommendation to include them in *DSM-III*.

After two years of work by Spitzer and the Task Force, objections were being raised from several sources. So Spitzer had to answer his critics formally; his replies were skilfully phrased (Spitzer and Sheehy, 1976). (1) *DSM-III* was said to be anti-humanistic, 'failing to do justice to the complexity of the human mind and condition'. Spitzer argued that, on the contrary, clarity is not incompatible with humanism. The opportunities were great. 'One use of operational criteria improves the reliability and validity of the diagnostic categories', and this would result in better treatment of patients – medical humanism at its highest. (2) Another challenge came from the psychoanalysts. Spitzer replied: *DSM-III* supposedly 'abandons the legacy of Freud', because the 'neurotic disorders' have disappeared from the nomenclature, but this was not so; they were just grouped under 'affective disorders', 'anxiety disorders' and 'hysterical disorders.' 'The abandonment of neurosis as a basis for classification is in no way viewed by us as a rejection of Freud's psychologic [*sic*] insights. Unfortunately, the term "neurosis" seems to have such symbolic meaning attached to it that for some it is a shibboleth that distracts from an informed discussion of the issues.' (3) Finally, some thought that *DSM-III* was too radical – good for researchers but not for ordinary clinicians. Spitzer said that the Task Force had anticipated the criticism, and *DSM-III* was having extensive trials in community mental health centres, offices of private practitioners, residency programmes and private psychiatric hospitals. The results of the field trials would be to refine and simplify the classification.

The psychoanalysts, however, were persistent. Due to the APA's concern with this Task Force of neo-Kraepelinians, which at that point did not represent the majority of American psychiatry, the APA Board of Trustees asked Spitzer to put an analyst on his Task Force. Spitzer complied, appointing two: John Frosch and his nephew, William Frosch (Spitzer, 2006*b*). The APA also had its own formal committee working as a liaison with Spitzer. He remembers that the chair of the committee, looking at the descriptive nosology without psychoanalytic theories of aetiology, said 'but we know so much more'. Whereupon Spitzer asked, 'Why don't you take one category and write it up the way you think we should have it?' So one analyst chose obsessive-compulsive neurosis and prepared a definition. Spitzer later said, 'and really, it would be embarrassing to read it to you – I mean it was about anal conflicts. So we said, "this is interesting but it's theoretical, and that's just not the approach we are going to take"' (Spitzer, 2006*a*, 14–15). Spitzer offered the analysts a sixth axis in the multiaxial system, but that came to naught.

At the APA annual meeting in 1980, the new *DSM* was to be voted on. Spitzer (2006*b*) was unsure what would happen. First, an analyst gave reasons why 'neurotic depression' should remain. Then Spitzer spoke in favour of using the term 'dysthymia', and also suggested a compromise: 'Dysthymia (neurotic depression)'. The Assembly voted to support Spitzer,

and when he came to the podium to express his gratitude, he was given a standing ovation (Spitzer, 2006a, 15).

## Conclusions

### 1. *The question of being neo-Kraepelinian*

When Robert Spitzer bridled at being called a neo-Kraepelinian, he readily admitted that his Task Force was filled with neo-Kraepelinians. Moreover, both *DSM-III* and *DSM-III-R* are, to some extent, neo-Kraepelinian documents. In addition, Spitzer's Research Diagnostic Criteria (RDC) were inspired by the criteria of Feighner and colleagues who were openly neo-Kraepelinian. *DSM-III* and *DSM-III-R* were neo-Kraepelinian classifications by being descriptive, eschewing psychoanalytical aetiologies, stressing that psychiatry was decidedly a part of medicine, and emphasizing the importance of follow-up studies and family histories. The *DSMs*, however, did not follow Kraepelin in one important way: they refused to speculate about aetiology, something Kraepelin did freely.<sup>20</sup>

Part of Spitzer's rejection of neo-Kraepelinian labels was motivated by his desire that the Task Force should be seen to be doing something new and different, not 'neo'-anything. Looking back on the 'values and assumptions' in the development of *DSM-III* and *DSM-III-R*, Spitzer (2001: 353; emphasis added) wrote, in a single paragraph:

The first decision the DSM committee had to make was whether to follow the DSM-II approach of adopting the ICD system ... or whether to develop an *innovative* classification for American psychiatric use. ... We were relatively unconcerned by frequently having a different definition of a DSM category than of a corresponding ICD-9-CM category. We believe this was a small price to pay for our ability to be *innovative*.

He concluded the paragraph by referring to 'our decision to break new ground in psychiatric nosology' and started the next paragraph: 'Having decided to be *innovative*, we had to decide the following *substantive* questions.' (original italics). Being in the footsteps of another – even the admired footsteps of Kraepelin – was something that Spitzer did not value in itself.

### 2. *A 'dent' in American psychiatry?*

Robins, Guze and Winokur wanted to make a 'dent' in American psychiatry. By Robins and Spitzer coming together, the Washington University group made a revolution – a meteor crater rather than a dent.

### 3. *Curing the world*

The psychoanalysts of the 1950s and 1960s and the community mental health professionals of the 1960s and 1970s felt they could go even further and 'cure the world'. Spitzer and the neo-Kraepelinians were more reality-bound, as

Kraepelin had been. He said we can diagnose, follow the course and usually give a prognosis, but admitted that, for the time being, this is all we can reliably do.

Kraepelin's goal of making a composite picture of an illness has been criticized for its lack of humanity – losing the patient as an individual. But the 'composite picture' was very like the 'diagnostic criteria' a century later. They both contained message: 'that is all one can do for the time being'. Admitting that, for the present, there were some limitations should not keep the psychiatric researcher from learning all that he or she can. Progress is built on knowledge.

Spitzer and the neo-Kraepelinians had great goals but, at the same time, they took the position that if they were going to make progress they had to be sure they knew what they were talking about. Aetiology and pathogenesis could be discovered only with the help of biological research. If their plans had a flaw, it was this: they were also going to have to find out how the psychological melded with the biological. In many of their papers, they gave lip service to this fusion, but most were far from taking seriously that nurture must be studied alongside nature.

One of the neo-Kraepelinians did develop doubts about the knowledge that could be achieved through diagnostic reliability. Shortly after *DSM-III-R* appeared, George Winokur, one of the original Washington University trio, prepared a paper (with two others) on the reliability and validity of diagnostic criteria. He urged 'a healthy dose of cynicism' when reading research reports because their methodology might have resulted in errors. He gave examples from his own experience. Once, he had discovered that a researcher made a mistake by misreading criteria. Another time he learned that investigators at two different centres applied the same diagnostic criteria differently:

Just because the criteria give us an increase in diagnostic reliability does not mean that they are good enough for a final answer. Moreover, just because investigators report they were reliable does not mean that they correctly interpreted the criteria.

The Bible may tell us so, but the criteria don't. They are better than what we had, but they are still a long way from being perfect. (Winokur, Zimmerman and Cadoret, 1988)

Spitzer recently concurred with Winokur's conclusion.<sup>21</sup>

Kraepelin, too, had developed a scepticism in the years following his delineation of the functional psychoses into two groups.

At present we are at every step met by obscurity and doubt in forming a practical judgment on the material of clinical experience. We are still so far removed from a real knowledge of the causes, phenomena, course, and termination of the individual clinical forms that we cannot yet dream of a surely established edifice of knowledge at all. What we have formulated

here is only a first sketch, which the advance of our science will often have occasion to change and to enlarge in its details, and perhaps even in its principal lines. (Kraepelin, 1913: 345)

Kraepelin and a neo-Kraepelinian – separated by a century – came together to remind eager researchers that there is no magic formula. Winokur showed that mistakes could arise even from the use of supposedly infallible diagnostic criteria. Kraepelin warned that researchers had to keep an open mind about change. In both cases the message is loud and clear: there is no perfect method.

### Acknowledgements

I want to thank Norman Decker M.D. for the part he played in the structure of this paper. My thanks also to Richard Meisch M.D. for his careful reading of this paper. The advice and comments of John Burnham Ph.D. and Michael Sokal Ph.D. are much appreciated

### Notes

1. In his 'self-assessment' (*Persönliches*) written in his sixties, he noted: 'The finer nuances of verbal expression have always been of great importance to me, and over the years, I have taken increasing pains to exploit the tools of language to the full' (Kraepelin, quoted in Engstrom, Burgmair and Weber, 2002: 101).
2. Kraepelin's book 'does not enter upon a critical review of contradictory views of other writers; thus he would do his work and his readers a great favor if he should give his material the benefit of monographic publication. In the meantime the conscientious critic must refrain from comparisons unless he have as many or more records of patients collected with the principles in view which Kraepelin has brought forth for the first time.'; Meyer, 1896: 299–300; 1994: 142.
3. Although it appears as if doctors returning soldiers to the front were not following the Hippocratic Oath, the battle-shocked troops were actually better off than comparable soldiers in World War I, who often returned home psychic wrecks, unable to live normal lives.
4. Two notable works in this regard are by Leo Stone, 1954, 1961.
5. President John F. Kennedy, quoted by Grob, 1987: 444.
6. Sigmund Freud quoted by Boyer, 1978.
7. Psychoanalytic aetiologies such as 'unconscious conflict', they regarded as being unproved.
8. Paula Clayton, another resident who went on to become the first female chair of a department of psychiatry in the country, recalls that all residents had to do original research, either on their own or with a faculty member (personal conversation with author, 29 Jan. 2007).
9. And three years before this paper, Robins had published an early attempt to establish operational criteria in a personality disorder; see Robins, 1967. For the idea that Kraepelin himself was primarily a diagnostic clinician rather than a classifier (nosologist), see the introduction by Engstrom and Weber to Kraepelin, 2005[1887].
10. The illnesses covered by Feighner *et al.* (1972: 58–62) were depression, mania, schizophrenia, anxiety neurosis, obsessive compulsive neurosis, phobic neurosis, hysteria, antisocial personality disorder, alcoholism, drug dependence (excluding alcoholism),

- mental retardation, organic brain syndrome, homosexuality, transsexualism, anorexia nervosa, and undiagnosed psychiatric illness.
11. On his own, Feighner (1979: 1173–4) discussed a six-stage diagnostic model that now included treatment outcome studies.
  12. I have found no explicit statement of this point, but I think Klerman is correct to infer this. I would welcome hearing from readers who have found (4) stated explicitly.
  13. There are varying accounts of the circumstances under which Robins and Spitzer first met.
  14. Yet they had a long way to go. Guze (1994) recalled ‘a senior person at the NIMH came to tell [the department chair] what a bad impression his department was getting because of the way Winokur, Robins and Guze were turning things.’ He also said: ‘For maybe seven or eight years, we had a lot of trouble getting grants from NIMH if they weren’t for laboratory research.’
  15. I will discuss Guze’s major publications in my book on the making of *DSM-III*. But for now I will just cite one paper and a book: Guze, 1978; Guze, 1992.
  16. Moreover, his training analysis had not gone well; Spitzer, 2006b.
  17. He had been a consultant in the preparation of *DSM-II*, and he asked Melvin Sabshin, Medical Director of the APA, if he could head the Task Force to prepare *DSM-III*. But Henry Brill was chair of the Committee on Nomenclature and Statistics, and could not be asked to step down. Fortunately, at that moment, Brill resigned and the chairmanship was now vacant. On Spitzer and the Task Force, see Spitzer, 2003.
  18. The first draft had 24; there came to be 25: schizophrenia; schizo-affective disorder, manic type; schizo-affective disorder, depressed type; manic disorder; hypomanic disorder; bipolar depression with mania; bipolar depression with hypomania; major depressive disorder; minor depressive disorder; panic disorder; generalized anxiety disorder; cyclothymic personality; depressive personality; Briquet’s disorder; antisocial personality; alcoholism; drug abuse; phobic disorder; unspecified psychosis; other psychiatric disorder; borderline features; not currently mentally ill; never mentally ill.
  19. ‘The use of operational criteria for psychiatric diagnosis is an idea whose time has come!’ (Spitzer, Endicott and Robins, 1978: 781; Spitzer and Forman, 1979).
  20. In writing this paper, I frequently came across authors who said that what was new about the Kraepelinian system was the emphasis he put on possible aetiologies.
  21. Author’s telephone conversation with Robert Spitzer, 27 Sept. 2006. Spitzer said: Winokur is ‘absolutely right [and he (Spitzer) has] no problem with what he says.’ But Spitzer added: ‘The alternative to diagnostic criteria is problematic.’

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