Abstract

We welcome the opportunity to respond to the commentaries on our proposed revised classification of personality disorder. What is refreshingly clear from these is that everybody understands exactly what this classification means, and there is no confusion over its nature (although some expressed concern that it might not be so clear to clinicians –in the field∅). We were especially pleased to note that Livesley (2011) recognized that we are attempting to address the core features of personality disturbance rather than concentrating what he aptly terms –a set of heuristic diagnoses with questionable validity∅ and that it is simply not possible to combine a dimensional classification of personality with the existing categories of disorder, as they are mutually incompatible. The comments cover a range from constructive support to concern that we are being too radical and are throwing away all the gains made in the last 30 years. In the following, we respond to the major themes across the set of comments.

We acknowledge that much needs to be done to improve the definitions of severity. Our identification of interpersonal dysfunction and its definition as –a pattern of general impairment in human relationships that prevents mutual understanding∅ is, we agree, a broad concept that has not previously been used in establishing the diagnosis of personality disorder, and it needs to be defined more carefully with clearer distinctions between the severity levels. The issue of –self pathology∅ is relevant, but we remain unconvinced that it is an essential part of diagnosis, although we accept that it does have a major influence in preventing mutual understanding and could contribute, as Simonsen (2011) suggests, to the more detailed description of the definition of interpersonal functioning. The problem with so many people with personality disturbances, particularly those with treatment-resisting personalities, is not that they have no sense or awareness of self but that they are unable to appreciate the selves of others, and this is a major obstacle to understanding. A further concern is that given the degree of cultural diversity in the concept of the self, it would be difficult to fashion a universally acceptable definition of impairment in self-functioning. Livesley (2011) also brings up the vexed question of whether personality disorder is essentially a relationship disorder or one of an essential disturbance of personality that secondarily leads to relationship problems. This is not easy to answer, and we lack firm evidence that those with personality characteristics that are abnormal in one society are embraced in another. But we suspect they may well be, and if so, this would support the diagnosis of personality disorder as one that is to some extent dependent on social setting.

Where we certainly need more data, and what we are addressing in ongoing field trials, is the reliability and accuracy of the levels of severity we have proposed, and exactly what it is that differentiates among levels of personality disturbance. We are not necessarily arguing that greater severity must be associated with more domain disturbance, and with a clearer definition of interpersonal dysfunction, this apparent association may be shown to be spurious. But at present, it appears that the divisions between domains decrease as severity increases.

We also acknowledge freely that the domain of emotional instability, equivalent to borderline in the DSM classification, is not a domain that naturally springs from empirical research, except insofar as emotional dysregulation is a very strong correlate of individuals who are extreme in the anxious-dependent domain, so here we are influenced by one aspect of clinical utility, the extent to which a diagnosis is supported by practitioners. The evidence that many people in this

group are more likely to seek and complete treatment (Tyrer, Mitchard, Methuen, & Ranger, 2003; McMurran, Huband, & Overton, 2010) is also relevant to practice, no least as most successful treatments are expensive and society is unlikely to tolerate a low completion rate in these (Soeteman et al., 2010). Probably because of treatment seeking, which itself is associated with severity, borderline personality disorder is a diagnosis that is more widely used than any other in the current classification, and Bateman (2011) and Gunderson and Zanarini (2011) are particularly worried about its loss in the revised classification. In making our revision suggestions, it is important to recognize that this category always has been regarded as part of the emotionally unstable group in the ICD, and so, at one level, the loss is minor. In our field trials, we need to find out how practitioners would diagnose those patients they currently classify as borderline in the revised system and, when this is done, whether the adjective 'borderline' remains a useful diagnostic descriptor. If it does not, then the anxious-dependent structure can incorporate this concept at the domain level, and the severity of disturbance will cover other aspects such as risk of self-harm. This would reduce the number of domains to four and be consistent with the now abundant evidence that both normal and pathological groupings of personality are best described in four dimensions (Tyrer & Alexander, 1979; Wigider & Simonsen, 2005; Rossi, Elklit, & Simonsen, 2010). Our main concern at present is that the borderline concept covers a very heterogeneous group that allows the diagnosis to be made very easily (i.e. has high sensitivity) but contains few individuals with the essential core features (i.e. low specificity).

We maintain that the empirical evidence supports our reclassification principles. The present systems of classification, beginning with DSM-III in 1980, were quite untested and had no scientific basis when they were introduced, although they were heuristically useful in testing hypotheses. We must accept now, however reluctantly, that the bulk of the evidence from these studies has not supported the essentials of the ICD and DSM personality disorder classifications. When Gunderson and Zanarini (2011) assert the notion that normal personality offers a valid bridge to the structure of abnormal personality is a 'still unproved idea' they are going against the vast majority of evidence of the last 50 years of research into personality and personality disorder (O'Connor, 2002; Saulsman & Page, 2004; Samuel & Widiger, 2008). Moreover, although they are concerned that both the DSM-5 and ICD-11 revisions are abandoning 'a system that has familiarity and clinical wisdom', it is important to remember that clinical wisdom and familiarity are not tenets of evidence-based medicine and must give way when they yield data inconsistent with the scientific evidence. It also is important to remember the results of the study by Bernstein et al. (2007) that out of 400 experienced clinicians and researchers, 74% felt that the categorical system of personality disorder diagnosis of DSM-IV should be replaced and that 80% felt that personality abnormalities are better conceived of as dimensions. The present system has failed clinical practice and needs much more than mere tinkering to put it right.

Simonsen (2011) claims that the proposal is simplistic (i.e. it oversimplifies) rather than being just simple, yet also suggests that it is far too complicated. He suggests that 'a classification system as proposed by the ICD-11 work group based only on a severity rating will create a myriad of different systems used to describe personality features' but it is difficult to follow his logic here. The main criticisms of a dimensional system, discussed in the scholarly but ambivalent commentary by Paris (2011), is that it makes diagnosis more complicated, but as

Livesley ([2011](#)) and Chanen ([2011](#)) both acknowledge, classification by severity is the simplest form of a dimensional assessment, placing everybody unequivocally to one major diagnostic group and having no loose 'otherwise unspecified' ends. Many of the other comments are basically criticisms of the dimensional construct. Silk ([2011](#)) comes down against it on the grounds that treatment can only be given once clear pathology has been identified, but this does not negate the fundamental evidence that the pathology exists across a continuum. The diagnosis of depressive disorders into mild, moderate and severe also could have been seen as a retrograde step as it abandoned 'endogenous', 'reactive', 'endogenomorphic' and 'psychotic' depressions, but this has helped practice and selection of treatment greatly and has shown that alleged boundaries between these other subtypes do not exist. Paris also points out that at the extreme end of severity, the links to other levels may not apply. This is a reasonable hypothesis— it is currently being debated with melancholia among the depressive disorders— and it needs further testing. But we also have other evidence that when personality disorders become severe, they cover several domains and create major handicap (Yang, Coid, & Tyrer, [2010](#)), and this is in keeping with the severity hypothesis supporting a true dimensional system.

We argue that our revised classification, despite all the limitations imposed by the absence of independent biomarkers or other yardsticks, is a good empirical classification that synthesizes the advances that have been made in the past 30 years. These advances are not mere statistical developments that are esoteric and arcane; they show that there are several personality dimensions that together underpin diagnosis and explain pathology, which can be formulated simply and can be incorporated into a clinically useful nosological system. We agree that this represents only a start in defining a new classification, and we need to bring our proposals and those of the current DSM-5 system closer together. But we hope that our continued field studies and, indeed, the work of our commentators and other researchers will assist this in the next few years.