to 'limitations' in both MRI and meta-analysis. The authors are right to highlight the problem of anatomical definition of the amygdala *in vivo* and how other imaging parameters may obscure (or reveal) laterality effects and differences between subject groups. However, they are wrong to blame meta-analysis. Systematic review and meta-analysis of MRI data is a powerful means of quantifying the precise effects that are the subject of speculation by Chance and colleagues.

We have recently carried out just such a review of the normal human amygdala (Brierley *et al*, 2002). Some 39 studies and 51 data-sets met our inclusion criteria, allowing comparison of 1491 amygdala pairs. The weighted mean volumes (95% CI) for the left and right amygdala were 1726.7 mm^3 (35.1) and 1691.7 mm^3 (37.2), respectively. The range was from 1050 to 3880 mm^3. This variance is clearly worrying. We were able to examine systematically some of the causes of this and found that most imaging parameters, such as slice thickness and plane of orientation, were not particularly influential. Study size had a weak but significant effect, with larger studies tending to give smaller volumes. Anatomical definition was the most important variable. Studies which employed the Watson criteria (Watson *et al*, 1992) gave significantly larger volumes than the remainder. Gender differences persisted (male greater than female) even in studies which attempted to control for intracranial volume. Laterality effects were not significant.

The ease of obtaining high-resolution anatomical brain images afforded by modern MRI on large samples of individuals across the life span means that MRI should be taken as the gold standard on regional volumetrics rather than post-mortem samples with all their attendant deficiencies. However, in order to exploit the advantages of MRI, researchers must pay particular attention to reliability of anatomical definitions. We have proposed that Watson's criteria be adopted generally and have recommended some minor improvements (Brierley *et al*, 2002).

Brierley, B., Shaw, P. & David, A. S. (2002) The human amygdala: a systematic review and meta-analysis of volumetric MRI. *Brain Research Reviews*, in press.

Chance, S. A., Esiri, M. M. & Crow, T. J. (2002) Amygdala volume in schizophrenia: post-mortem study and review of magnetic resonance imaging findings. *British Journal of Psychiatry*, **180**, 331–338.

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Authors' reply: We agree with David *et al* that the key problem, which we have highlighted, in *in vivo* MRI studies of the amygdala, is anatomical definition. The ability to define anatomical boundaries at the cellular level means that post-mortem samples set the gold standard for anatomical delineation. Indeed, the generally smaller volumes of the amygdala (uncorrected for tissue shrinkage in Chance *et al*, 2002) reported in post-mortem studies are indicative of more conservative estimates when the precise boundaries can be seen. This is consistent with Brierley *et al*'s (2002) conclusion in their meta-analysis that anatomical definition is the most prominent contributor to variance in MRI volume estimates of the amygdala.

Our criticism is not of meta-analysis *per se*, but of the inclusion of some studies, which owing to low scan resolution use only very approximate anatomical definitions. Particularly problematic in schizophrenia is the use of landmarks, which may be systematically shifted with respect to the boundary of the amygdala, owing to other changes in the temporal lobe. While MRI studies have the obvious advantages of an *in vivo* assessment and larger sample size, post-mortem studies are also important as a check on the possibility of systematic bias which may enter the MRI literature (Walker *et al*, 2002).

We agree with the importance of consensus criteria for anatomical definitions which take full advantage of the improvements in up-to-date MRI visualisation. Our paper concludes with some references to studies attempting to tackle this issue for the amygdala, to which the paper of Brierley *et al* (2002) should be added.

Brierley, B., Shaw, P. & David, A. S. (2002) The human amygdala: a systematic review and meta-analysis of volumetric MRI. *Brain Research Reviews*, in press.

Chance, S. A., Esiri, M. M. & Crow, T. J. (2002) Amygdala volume in schizophrenia: post-mortem study and review of magnetic resonance imaging findings. *British Journal of Psychiatry*, **180**, 331–338.

Walker, M. A., Highley, J. R., Esiri, M. M., *et al* (2002) Estimated neuronal populations and volumes of the hippocampus and its subfields in schizophrenia. *American Journal of Psychiatry*, **159**, 821–828.

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Phenomenology of acute confusional states

I read with great interest the paper by Dr Fleminger (2002) on delirium, and the relevant controversy raised by Dr Philpott regarding to whom should be attributed the first description of hypoactive delirious states (Philpott, 2002). May I suggest that this initial description was made around one century earlier than mentioned by both authors. In fact, as early as 1892 the French alienist Philippe Chaslin borrowed the term of '*confusion mentale primitive*' from a previous description proposed by Delasiauve during the 1850s. He was probably one of the first authors who gathered under a unified entity what was previously described under separate clinical features as psychosis post-influenza, post-acute diseases, post-fever and epilepsy (Chaslin, 1892). He also clearly noticed its similarity with what Lasegue had described earlier as *delirium tremens*, in which perceptual disturbances were considered as a dream-like experience (Lasegue, 1881). In his later monograph, Chaslin describes the acute confusional state as 'an acute brain disorder, consecutive to an organic significant disease, with cognitive impairment associated with delusions, hallucinations, psychomotor agitation, or reciprocally, with psychomotor retardation and inertia' (Chaslin, 1895). Despite this very early description of what has since been called hyperactive and hypoactive subtypes of delirium, there have been very few attempts to test the validity and the relevance of these subtypes. To our knowledge, at this time only one empirical exploration of what are the constitutive symptoms of each dimension has been proposed (Camus *et al*, 2000). We would like to add, concerning what Fleminger cites as possible psychological consequences of confusional experience, that another French alienist described 'permanent ideations' and 'chronic delusional states' following the post-dream-like confusional experience (Regis, 1911). We agree with

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