

Summary of the main issues in our blog:

1. Misunderstandings (where we sort of probably agree)

TY critiques us for saying the dACC is *selective* (as opposed to preferential) for pain, but then assumes we mean something by selective that we don't. We use selective the same way it is used in the MVPA literature where selective refers to preferential responses to one class of stimuli or processes over another. This comparative, rather than absolute, nature of our claim is clear in our abstract: "Results clearly indicated that the best psychological description of dACC function was related to pain processing—not executive, conflict, or salience processing."

TY also criticizes us for not comparing pain to fear, emotion, and autonomic processes. Although we do include a figure (in the supplementary materials) showing the reverse inference maps for fear and various types of negative emotions and discuss these findings in the text, this was not the main point of the paper. We explain how we were driven by past work on cognitive and salience that suggest dACC activations to pain are "nothing but" cognitive or salience processes. We have written many times about how we think the dACC is involved in distress-related processes including pain, fear, and negative affect, and thus we see these accounts as complimentary – all in the same family – and in contrast to the cognitive and salience accounts.

2. L&E's unobjectionable claims are hardly novel.

TY concludes that dACC has preferential responses for pain processes and that dACC plays a greater role in pain than in many other cognitive processes that have been attributed to dACC, but this is "hardly novel".

Except for the "hardly novel" bit, we think its great that TY reaches mostly the same conclusions we have. That said, we think the "hardly novel" claim is off base and we discuss this.

3. L&E cherry picked the data they showed

TY doesn't like the terms we chose to compare to pain (executive, conflict, and salience related terms) or that we only show midsagittal images. TY shows his own maps at a more liberal uncorrected threshold than what we showed so the comparison is of limited value (we used the standard thresholds that TY built into Neurosynth). We now show more of the dACC and the story stays the same.

4. Surprising lack of appreciation for what the reverse inference maps show in a pretty straightforward manner.

Visual inspection of the maps alone should be enough to make folks wonder where the reverse inference evidence is for the dominant cognitive/salience accounts of dACC. No mention from TY of how strikingly barren the reverse inference map is for,

say, salience and whether there should be a moratorium on folks assuming dACC activity implies salience processes.

5. L&E did the wrong analyses

TY believes we should have compared posterior probabilities (i.e. effect sizes) for different terms rather than comparing the Z-scores (i.e. accumulation of evidence for reverse inference) as we did. But our question was really about whether there is more accumulated evidence for one account of dACC function than another and so comparing Z-scores makes sense.

6. About those effect size comparison maps

TY goes on to compute effect size comparisons for several terms. First, these analyses do not address our question of interest. Second, there are multiple aspects of how these analyses were performed that diminishes their meaningfulness.

7. Biases all around

TY doesn't think such a large region of the brain would be devoted to pain. We suspect lots of others might agree. But given the utterly central role of pain in survival, we think this is entirely plausible. Being blind or deaf are not death sentences, but the inability to feel pain (i.e. congenital insensitivity to pain) is.