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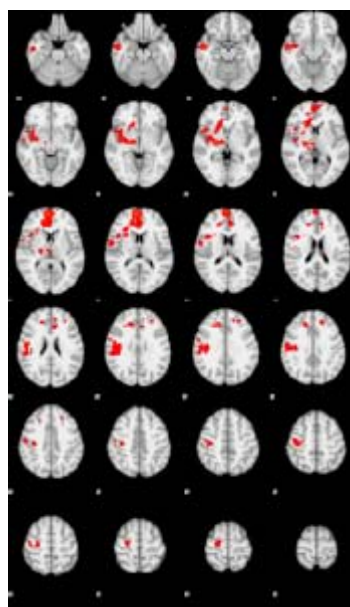
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New Meditation Study in Neuroimage: “Meditation training increases brain efficiency in an attention task”

July 11th, 2011 [§ 7 Comments](#)

Just a quick post to give my review of the [latest addition to imaging and mindfulness research](#). A new article by **Kozasa et al**, slated to appear in Neuroimage, investigates the neural correlates of attention processing in a standard color-word stroop task. A quick overview of the article reveals it is all quite standard; two groups matched for age, gender, and years of education are administered a standard RT-based (i.e. speeded) fMRI paradigm. One group has an average of 9 years “meditation experience” which is described as “a variety of OM (open monitoring) or FA (focused attention) practices such as “zazen”, mantra meditation, mindfulness of breathing, among others”. We’ll delve into why this description should give us pause for thought in a moment, for now let’s look at the results.



Results from incon > con,
non-meditators vs
meditators

In a nutshell, the authors find that meditation-practitioners show faster reaction times with reduced BOLD-signal for the incongruent (compared to congruent and neutral) condition only. The regions found to be more active for non-meditators compared to meditators are the (right) “lentiform nucleus, medial frontal gyrus, and pre-central gyrus”. As this is not accompanied by any difference in accuracy, the authors interpret the finding as demonstrating that “meditators may have maintained the focus in naming

the colour with less interference of reading the word and consequently have to exert less effort to monitor the conflict and less adjustment in the motor control of the impulses to choose the correct colour button." The authors in the conclusion review related findings and mention that differences in age could have contributed to the effect.

So, what are we to make of these findings? As is my usual style, I'll give a bulleted review of the problems that immediately stand out, and then some explanation afterwards. I'll preface my critique by thanking the authors for their hard work; my comments are intended only for the good of our research community.

The good:

- Sensible findings; increases in reaction time and decreases in bold are demonstrated in areas previously implicated in meditation research
- Solid, easy to understand behavioral paradigm
- Relatively strong main findings ($P < .0001$)
- A simple replication. We like replications!

The bad:

- Appears to report uncorrected p-values
- Study claims to "match samples for age" yet no statistical test demonstrating no difference is shown. Qualitatively, the ages seem different enough to be cause for worry (77.8% vs 65% college graduates). Always be suspicious when a test is not given!
- Extremely sparse description of style of practice, no estimate of daily practice hours given.
- Reaction-time based task with no active control

I'll preface my conclusion with something Sara Lazar, a meditation researcher and neuroimaging expert at the Harvard MGH told me last summer; we need to stop going for the "low hanging fruit of meditation research". There are now over 20 published cross-sectional reaction-time based fMRI studies of "meditators" and "non-meditators". Compare that to the incredibly sparse number of longitudinal, active controlled studies, and it is clear that we need to stop replicating these findings and start determining what they actually tell us. Why do we need to active control our meditation studies? For one thing, we know that reaction-time based tests are heavily biased by the amount of effort one expends on the task. Effort is in turn influenced by task-demands (e.g. how you treat your participants, expectations surrounding the experiment). To give one in-press example, my colleagues Christian Gaden Jensen at the Copenhagen Neurobiology Research recently conducted a study demonstrating just how strong this confounding effect can be.

To briefly summarize, Christian recruited over 150 people for randomization to four experimental groups: mindfulness-based stress reduction (MBSR), non-mindfulness stress reduction (NMSR), wait-listed controls, and financially-motivated wait-listed controls. This last group is the truly interesting one; they were told that if they had top performance on the experimental tasks (a battery of classical reaction-time based and unspeeded perceptual threshold tasks) they'd receive a reward of approximately 100\$. When Christian analyzed the data, he found that the financial incentive eliminated all reaction-time based differences between the MBSR, NMSR, and financially motivated groups! It's important to note that this study, fully randomized and longitudinal, showed something not reflected in the bulk of published studies: that meditation may actually train more basic perceptual sensitivities rather than top-down control. This is exactly why we need to stop pursuing the low-hanging fruit of uncontrolled experimental design; it's not telling us anything new! Meditation research is no longer exploratory.

In addition to these issues, there is another issue a bit more specific to meditation research. That is the totally sparse description of the practice- less than one sentence total, with no quantitative data! In this study we are not even told what the daily practice actually consists of, or its quality or length. These practitioners report an average of 8 years practice, yet that could be 1 hour per week of mantra meditation or 12 hours a week of non-dual zazen! These are not identical processes and our lack of knowledge for this sample severely limits our ability to assess the meaning of these findings. For the past two years (and

probably longer) of the Mind & Life Summer Research Institute, Richard Davidson and others have repeatedly stated that we must move beyond studying meditation as “a loose practice of FA and OM practices including x, y, z, & and other things”. Willoughby Britton suggested at a panel discussion that all meditation papers need to have at least one contemplative scholar on them or risk rejection. It’s clear that this study was most likely not reviewed by anyone with any serious academic background in meditation research.

My supervisor Antoine Lutz and his colleague John Dunne, authors of [the paper that launched the “FA/OM” distinction](#), have since stated emphatically that we must go beyond these general labels and start investigating effects of specific meditation practices. To quote John, we need to stop treating meditation like a “black box” if we ever want to understand the actual mechanisms behind it. While I thank the authors of this paper for their earnest contribution, we need to take this moment to be seriously skeptical. We can only start to understand processes like meditation from a scientific point of view if we are willing to hold them to the highest of scientific standards. It’s time for us to start opening the black box and looking inside.

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§ 7 Responses to *New Meditation Study in Neuroimage: “Meditation training increases brain efficiency in an attention task”*



[cathy_kerr](#) says:

[July 11, 2011 at 12:58 pm](#)

Micah... great post. I can’t find the Jensen paper — can you point me to it. Also, does your call for longitudinal active-controlled studies speak to your earlier point (in your immediate MLSRI reaction post) — that maybe monks don’t have that much to teach us. Can you elaborate?

[Reply](#)



○ [Micah](#) says:

[July 11, 2011 at 1:20 pm](#)

Hi Cathy, thanks sincerely for your comment! Just to clarify a few things- I definitely believe that monks can teach us loads about the mind! What I meant to convey was really more specific to debates over (for example) representational vs enactive cognitive science, or regarding the mind-body problem. So what I wanted to argue is that, if we start these debates by granting priority, authority, or reliability to certain kinds of practitioners, we’re muddling up what we want to study. One way to think about this is the often repeated and inevitable responses we get from Christian practitioners and theologians who get a bit defensive when we don’t rush to study their brains. I think enactive cognitive science has to view the mind as a culturally embedded phenomenon, and so we should be careful about the way we view any one particular practice. So we should definitely be studying the minds of monks, but when we do so we have to give both phenomenological and anthropological perspectives room to talk.

That being said, there is definitely a relationship to my call for more tightly controlled studies. I really enjoyed that article from the Atlantic you shared, which discussed the repeated failure of both modern and alternative medicine to pass the placebo controlled trial. I even go so far as to completely agree with the conclusion; from a pragmatic perspective, if these things work to reduce stress, we should be funding them and teaching our doctors about them. But as a cognitive scientist, I want to begin to understand the nature of these practices. So it's very important to start to study for example, differences between specific types of meditation practice (what's the difference between mantra, body, and focus-based practices?). In the end, the question of "what is specific to meditation" isn't just about confounds. To quote Olaf Sporns, it's only by identifying and categorizing the variance of human phenomenon, that we can begin to identify the invariant! He may have said that in reference to neurological systems, but I think it applies to the scope of cognitive science.

Edit: Christian's paper is currently in press. I will post/blog it the moment it is released 😊

[Reply](#)



- *David* says:

[July 11, 2011 at 4:55 pm](#)

I think that you bring up a very valid point regarding the styles of meditation. I don't think we can say that one style is better than another but simply because the outward appearance of stillness looks the same does not mean that the internal synaptogenesis will be the same. The easiest example I can think of is how surprised people are when I tell them that when one sits in zazen the eyes remain open whereas most other traditions like vipasana will have the eyes remaining closed. The difference here being that in zazen the hope is to achieve a unity of internal and external through experiencing reality as it is where vipasana turns inward and focus on the experience of the body with a deeper focus on the breath. These are subtle yet significant differences if we are talking on the scale of even just 3 hours a week for 9 years that is over 1400 hours.

[Reply](#)



- *betterlivingthroughscience* says:

[July 12, 2011 at 12:12 pm](#)

I find it hard to believe that people are STILL publishing results with uncorrected p values – particularly in NeuroImage, where I'd expect the reviewers to be quite keyed in to those issues. Odd.

[Reply](#)



- *Micah* says:

[July 12, 2011 at 12:21 pm](#)

No kidding. I guess they thought it was "significant enough" but if so, why not just report the FDR corrected values? It should remain significant.

[Reply](#)



- *larrywelkowitz* says:

[July 12, 2011 at 4:21 pm](#)

Agreed and appreciate your thoroughness here. I would go one step further and say that reporting P values should include confidence intervals as well. We have a romantic notion about what works and what doesn't...but people need to understand that science is hard work.

Cheers

[Reply](#)



- [Michael Lifshitz](#) says:
[July 13, 2011 at 7:59 pm](#)

Thanks for the insightful analysis Micah. I was wondering, has Jensen published or planned to publish that study you mentioned?

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