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**REVIEWER COMMENTS**

Reviewer #1 (Remarks to the Author):

The goal of this study was to understand the large scale neural instantiation of emotional carryover effects – the degree to which brain network activity evoked during emotionally arousing events, and in the minutes after the event, influence brain network activity when performing a cognitively demanding activity. To that end the authors measured ongoing brain activity while participants watched an emotionally evocative vs. a neutral movie, during rest after the movie, and in a subsequent set of cognitive control tasks. A CAPs approach was used to estimate a set of coactivation patterns (CAPs, or spatial patterns captured at a single points in time). The key dependent measure was the frequency of occurrence of each of 5 CAPs, or network patterns, in each emotion condition and phase of the experiment, identified by k-means clustering. Bayesian structural equation modeling (BSEM) was used to examine the degree to which CAP occurrence patterns during and following movies predicted  
those during the demanding task and the degree to which they were mediated by task performance. Reaction times in the cognitive control tasks indicated that preceding negative emotion increased incongruency effects of slowed reaction times in high conflict (incongruent) trials. CAP occurrence findings showed several distinct patterns, by which CAP occurrence during movie or post-movie rest influenced patterns during the task, which were observed in the emotionally arousing but not the neutral movie condition. Some of these were mediated by task performance. These results indicate that emotional perturbations influence fluctuations of ongoing intrinsic network activity later in time and influence network activity associated with performance of a demanding task. This experimental design provides a rich set of data. By leveraging relatively recent approaches to examining dynamic brain network activity the study has the potential make a very important contribution to cognitive neuroscience -- to our understanding of the processes 1) by which fluctuations of distinct brain networks at earlier points in time influence later patterns in general, and 2) by which emotional carryover effects influence performance when cognitive demands are high in particular. However in the current draft there several issues that prevent the paper from realizing this contribution.

As this paper stands it’s impossible to understand or evaluate the fMRI methods as there is far too little detail given. Why did the authors choose this methodological approach to examining dynamic network activity? What methodological steps are involved? Moreover, the Discussion section is mostly focused on reverse inference of cognitive processes evoked by CAP networks – a kind of network reverse-inference blobology, which doesn’t do the study justice. The contribution of this study would be much clearer if the authors discussed what the findings tell us about how or why measuring recurrence of fluctuating brain network patterns over time provides insight into carryover effects of emotional events. And, given that, what are the implications for our understanding of such patterns of activity for real world applications related to mood disorders but also potentially performance of demanding task in emotionally fraught contexts?

Specific comments are below. Methods

What was the justification of the final sample size? Was a power analysis performed?  
  
I just want to note that is a great strength that the study took menstrual phase into account. As hormone levels have been found to influence many cognitive processes as well as emotional responses, more cognitive neuroscience studies should time scans by cycle phase.

Figure 1 is challenging to interpret. In Fig 1 b., the X axis just says subjective ratings, with no explanation of the scales (e.g., that valence ratings are on a bipolar scale and are higher for positive, whereas arousal and presumably emotional experience are unipolar scales where higher levels simply indicate more). The same issue goes for the PANAS scores in Fig 1D. The readers shouldn’t need to have to refer to the supplemental materials to interpret the plots.

Fig 1C is particularly confusing to read/interpret. In the caption for the middle panel please explain the initials for task conditions on the x axis. In the right panel the x congruency axis reads I > C which makes me expect a difference score, but it doesn’t look like a difference score.

Cognitive tasks: I’m curious about why a face /name stroop was used when it’s the colour word stroop that produces the classic interference effect and is easier to interpret in terms of direct conflict between the word and the colour the word is depicting? Was it because it lent itself to binary answers? None of the supporting literature cited to support the choice of stroop task used this version of a stroop task. The Westerhausen paper used a dichotic auditory task. The Verbruggen study used a spatial stroop with a Simon task. And the Rey-Mermet used the classic colour word stroop, which differs in that the conflict is between two forms of identical features rather than two things that are typically associated with each other -- a name and the gender associated with it. While all these versions of the task induce conflict of some sort and require cognitive control, the interpretation of processes evoked can differ and none of the papers support the choice of specific task made here.

**fMRI Methods**

As I read through Methods and results, I’m struggling to find information about what the rationale for using this particular approach to measuring brain dynamics? What theoretically does number of times CAPs occur each condition tell you about what’s going on in each condition? Given the research questions, why did the authors choose an approach where you look at recurring snapshots of BOLD spatial patterns rather than time series between pairs of voxels or seeds? This needs to be spelled out explicitly from the beginning – not offloaded onto a reference to another paper.

Also I don’t get an understanding of from the methods, as currently written, is HOW the CAP analysis captures the temporal dynamics of identified networks as they fluctuate over time. After reading the three additional papers referenced for the methods I now understand that CAPs are identified by using clustering to identify spatial patterns across the brain at single time points and identifying how often these spatial patterns occur. But I could not extract even this basic outline of information from the paper.  
  
The authors write (p. 12): “Further description of our methodological procedure is provided elsewhere55,80” This is not adequate. Please include a description of how CAPs were identified in this manuscript so the reader doesn’t need to dig up and read two other papers to understand what was done! Even having read the referenced papers I am still missing the rationale for a number of specific choices made in this paper: TbCAP toolbox choices

1. Choice of Frames

• Given the use of the TbCAPseed free analysis option, exactly how are coactivation patterns calculated at a particular point in time? Going to the referenced Bolton TbCAPs paper, I read that CAPS approaches typically identify frames/timepoints at which a seed region reaches a sufficiently high level of activation (or sometimes de-activation). But there is a seed-free option which the authors of the current paper selected. So:

• How were the relevant frames for CAP identification chosen here? It’s clear from Fig 2 that motion-corrupted frames were rejected but what frames were retained? Was it every TR? If not what were the criteria for frame selection prior to clustering?

2. Clustering.

• Is the number of 5 clusters used for comparison of different numbers of clustering solutions based on the default from the “cluster” function of the TbCAPS toolbox

3. Computation of metrics.

• I’m making the inference that the choice made at this decision point was number of entries. Or was it raw counts? What was the rationale behind deciding on this metric specifically?

BSEM (from the supplementary methods).

• If it’s not possible to use informed priors then what is the advantage of a Bayesian over a frequentist approach?  
  
Results  
The authors write (p. 15). “These (RT) results ensure a reliable comparison of cognitive control performance across conditions with no task confound, and no habituation or learning effects. All behavioral and imaging data were therefore pooled across the two tasks.”  
• Then why use two different conflict tasks in the first place? Also just because RT and accuracy results didn’t differ between tasks, it doesn’t necessarily follow that they are evoking exactly the same underlying brain processes….

Dynamic functional connectivity results.

• More information and explanation are required. This paragraph jumps into a description of finding optimal k-means solutions without setting up a frame of reference that makes the description meaningful. The methods described in the caption to Figure 2 are also very hard to follow without a frame of reference. This is also true in the description of the SEM results -- there are a number of references to procedures or terms that are not described or explained anywhere in the manuscript.

BSEM results.

• It looks as though the RTs from each task condition were entered into the mediation model separately. Is this true? When RT is found to be a mediator is this RT across all the task conditions? So similar results to what would be observed if a mean RT across all task conditions were used? Or does the difference in RT between task conditions come into play here? If so how does that work?

• What is the direction of the relationship between RT and the effect of one CAP on another? For example, in Aim 2, is more frequent occurrence of the SN-SMN CAP during the movie on occurrence of the FPN CAP during the task associated with faster or slower RTs? Or more of an effect of the negative movie induction on RTs?

• I didn’t see where in the text the comparison of the magnitude effects between neutral and negative contexts in the BSEM models with RT mediators, illustrated in Fig 5c, was reported. If I understand the results illustrated in Figure 5C correctly, in the direct comparison of magnitude effects between neutral and negative contexts in the models with mediation, what really differed for both hypothesis one and two was the degree to which the SN\_SFN in either movie or rest predicted FPN CAP occurrence during the task as a direct effect. Is this correct? If so is it not worth emphasizing?

• Also not mentioned is the finding (Tables S14, S16) is the direct and total effect of FPN CAP occurrence during movie watching on FPN occurrence during task. So it looks as though, if I’m interpreting the table correctly, that in the absence of strong emotional context, it is simply control network activity in the earlier activity that predicts control network activity during the conflict task. If so that may be worth mentioning in the main text as well.

DISCUSSION  
• The authors write (P. 27): “We show here that enhanced DMN at rest after negative events predicts enhanced FPN recruitment by a subsequent cognitive task .” Is “enhanced” the appropriate description for an increase in the occurrence of DMN CAPs at rest? Given its conventional use, readers are likely to associate “enhanced” with “more” activation. And since readers are less used to thinking in terms of numbers of occurrence of network co-activation than in terms of magnitude of BOLD activation, it would help if the language consistently emphasizes that that is the measure being interpreted.  
  
• There are a lot of interpretations that could be made about DMN activity carryover from rest to task in the negative condition. One important feature of DMN activity is it is consistently linked to internally-directed attention. An alternative interpretation to emotion regulation is the role of the DMN in episodic/autobiographical memory. It seems that an equally plausible interpretation of DMN carryover could be the more frequent and persistent re-occurrence of memories evoked by or associated with the negative movie clips during rest and task. This arguably could also have an impact on cognitive control capacity. But this multiplicity of interpretations also speaks directly to the problems and limiations of reverse inference of activation patterns.

• In general there is a LOT of reverse inference and speculation the Discussion section that could be scaled back substantially (maybe making room for more concrete explanation of the methods earlier on). Moreover, the reverse inference approach in this section basically treats the CAP patterns as network level blobology. It would be much more informative if there were instead discussion of the importance and role that the frequency of fluctuating network configurations play, and of the importance of measuring them to understand emotional carryover effects -- rather than trying to reverse infer of the function of the key networks identified by the CAP analysis.

Reviewer #2 (Remarks to the Author):This is a well-written paper describing a thoughtful study examining the neural basis of how negative emotion influence cognitive control processes. They also use an interesting approach to examining dynamic patterns of functional connectivity (co-activation patterns; CAPs) in functionally defined neural networks that map reasonably well onto canonical resting state networks. They show that their emotion induction has the expected cognitive/behavioral effects during cognitive control, which lends confidence to the concomitant neural results.

That said, I am not sure the study is of the level of impact associated with a journal like Nature Communications. My largest concern here is that the sample size is quite small in relation to current high-impact fMRI studies (N=24). They use sophisticated Bayesian analyses to help address this, but it nonetheless leads to relatively low confidence in the replicability or generalizability of the results.  
  
Beyond this, I will list a number of major and minor issues throughout in the order they came up as I read the manuscript:  
  
1. Starting in the abstract, but also in several other places, they use acronyms without first defining them. Two examples are FPN and IFNs. They also call the FPN ( which I assume stands for frontoparietal network) by other names instead despite using the acronym, such as cognitive control network or executive control network. I’d keep network naming consistent and matching the acronyms.

2. The introduction sometimes refers to effects without stating their directions or other details that would help with interpreting their reasoning. For example, they state: “Several studies reported behavioral changes in attentional processing induced by emotional stimuli, including effects on spatial orienting and resistance to distractor interference in conflict tasks”. I this case, it would be helpful to indicate which emotions (or what types of emotional cues) and specifically how they affect orienting and distractor resistance.  
  
3. They underline words several times in the text. This seemed atypical, but is up to journal formatting rules.  
  
4. Typo – 2nd to last line on page 5, “tracking” should be “track”. On the next line, ”voxel” should maybe be “single-voxel” or “voxel-wise”?

5. How well was the content of the videos matched other than the affect-induction aspects. One might be concerned that relevant network effects involved priming from other content differences as opposed to the emotional differences. It would have been nice to see correlations between neural/behavior effects and individual differences in the magnitude of change in affective state (e.g., did those who felt worse after the negative movie also show stronger network or RT differences?). If so, it would help support the idea that it is the affect change and not other content differences that matter.

6. I would have liked to see how emotions from the first movie affected subsequent resting state neural network activity. It would also help if they explained more what they found in their previous study analyzing the 2nd rest period, as it could be relevant to interpreting their findings here.  
  
7. I found it a bit strange (and not theoretically motivated) that RTs were treated as mediators. Presumably, the RTs are a RESULT of neural activity during the cognitive control tasks, and their hypothesis is that the emotion induction affects this task-related neural activity. So the natural causal model to me would be that the emotion induction effects during rest/movie would have an influence on RTs as mediated by network activity during the cognitive control task.

8. They treated two cognitive control tasks as engaging identical processes and combined their data. It’d be helpful to know if this has ever been done in previously published studies and to include more consideration of limitations or potential concerns about doing this.

9. It could be clearer in the main text whether the CAPS were estimated by combining data across all periods (rest/movie/task) or only from rest.

10. They often talk about differences in network relationships across tasks that were only present in negative or neutral contexts. But were those formally detected as interaction effects? This isn’t explicit in the main text. In general, what their statements mean in terms of main effects and interactions in the models could be clearer, since it’s stated somewhat informally in the text, and associated results tables are only in supplemental materials.

11. Typo – one of their listed “ROPE” effects says “0.” (presumably missing a 0).

12. Their descriptions of network relationships were almost always stated non-directionally. It would be helpful for interpretation if they were clearer in their descriptions whether relationships between networks were positive or negative.

13. Since MOVIE2 comes between REST1 and the task, it seems reasonable to expect that MOVIE2 responses would moderate or mediate the effects of REST1 on the task neural/behavioral measures. Was this examined at all?

14. In their discussion, they refer to constructs such as self-regulation and cognitive control (which is often associated with voluntary emotion regulation), which sound overlapping. They also refer to salience vs. “affective meaning”. It would be helpful if they better clarified what they mean by these abstract constructs and how their distinguished.

15. They state: “We surmise that, in post-emotional resting state, the SN-SMN system might encode interoceptive signals and bodily feelings through an interaction with self-referential representations elaborated in DMN areas that together contribute to subjective emotional experience.” This suggests and interaction between networks during their 1st resting state scan that they could very easily test in their data.  
  
16. They hypothesize mechanisms associated with sustained arousal. Do they have any peripheral physiological data during scanning to test this suggestion?

I hope these suggestions are helpful.

Reviewer #3 (Remarks to the Author):

This study investigated neural carry-over from an emotion induction task to a cognitive control task. It used innovative dynamic connectivity methods to identify relationships between neural patterns elicited by negative films which were related to FPN engagement during conflict resolution. These relationships were not observed during/after a neutral film. The strengths of the investigation include a well-piloted task, a commitment to making the data available after publication, and the consideration of dynamic networks (vs a traditional voxelwise linear model). However, there were several limitations as well, which diminished the potential impact of the paper on the field. Ultimately, I found the behavioral effects of the task to be better specified than the purported neural mechanisms, and the dynamic connectivity measure did not allow for precise investigation of specific behavioral effects. Furthermore, the mediation analysis using behavior as a mediator were puzzling to me.

The behavioral effects reported are quite interesting – the fact that the emotion manipulation exaggerated both the main effect of conflict and the conflict sequence effect is a great foundation. However, the neural investigation is not able to identify which regions or dynamic functional networks represent the impact of negative emotion on conflict or conflict adaptation, merely performance of the entire tasks (for example, the main effect of RT for the negative vs neutral conditions). Furthermore, I was unclear on how the authors were conceptualizing behavioral performance as it relates to the model. At several points in the paper, they indicate that they are interested in how negative emotion impacts cognitive control. However, when they model behavior, they include it as a mediator, indicating that they see negative emotion as causing changes in behavioral performance, which then recruit the FPN network changes. Furthermore, the discussion does not directly address the FPN is engaged more during the negative condition, but that’s associated with LOWER performance, not better control.

No power analysis was provided, which is unusual. It is unclear whether the authors were well powered enough with a final sample of 24 to conduct the analyses they report. I was curious whether the magnitude of self-reported negative affect in response to the film moderated the behavioral performance on the tasks. This would be a behavioral carry-over effect beyond the group-level manipulation. The interpretation of the DMN at rest is that it is reflecting more intense subjective experience, but this is not tested with the subjective data available, nor is the subjective data used rather than the brain data to test the same hypothesis.

Smaller comments:

The authors should include that their sample was limited to only those identifying as women in their limitations section. Furthermore, the authors should clarify whether they measured sex or gender and be consistent throughout.

The authors indicate that the CSE was driven by the iC condition- usually I think of it as more driven by cI vs iI. Is this a limitation? I recommend moving the regions identified in the CAPs to the main manuscript.  
In the supplemental material, please specify which table reports congruency effects on page 6.  
  
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