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EEG to fMRI Synthesis: Is Deep Learning a candidate? (/pdf?id=osDLMKS9H)

Anonymous

06 Mar 2020 (modified: 17 Mar 2020) ECCV 2020 Conference Blind Submission Readers: Paper6659 Authors, Paper6659 Reviewers, Paper6659 Area Chairs, Program Chairs Show Revisions (/revisions?id=osDLMKS9H)

Abstract: Advances on signal, image and video generation underly major breakthroughs on generative medical imaging tasks, including Brain Image Synthesis. Still, the extent to which functional Magnetic Ressonance Imaging (fMRI) data can be mapped from the brain electrophysiology remains largely unexplored. This work provides the first comprehensive view on how to use state-of-the-art principles from Deep Learning to synthesize fMRI data from electroencephalographic (EEG) data. This work focuses on synthesizing multiple haemodynamic signals, hence working with multivariate time series. Given the rich spatiotemporal nature of both modalities, this problem is formulated as the task of learning a mapping function between multivariate time series with highly dissimilar structures. Comparisons of state-of-the-art synthesis approaches are undertaken, including Autoencoders, Generative Adversarial Networks and Pairwise Learning.

Results show the feasibility of EEG to fMRI brain image mappings, pinpointing the role of the current advances in Deep Learning on tackling this complex task. The proposed approaches for EEG to fMRI synthesis further provide a way to enhance and augment brain image data, and unlock the possibility of performing more affordable, portable and long-lasting protocols of brain activity monitoring. The code used in this manuscript is available in Github and the datasets are open source.

TL;DR: EEG to fMRI Synthesis

Subject Areas: Biomedical Image Processing, Computer Vision for General Medical, Biological and Cell Microscopy, Image and Video Synthesis

Author Agreement: All authors agree with the author guidelines of ECCV 2020.

TPMS Agreement: All authors agree that the manuscript can be processed by TPMS for paper matching.

Supplementary Material: zip (/attachment?id=osDLMKS9H&name=supplementary_material)

Revealed to David Calhas, Rui Henriques

28 Feb 2020 (modified: 13 Mar 2020) ECCV 2020 Conference Submission

Authors: *David Calhas (/profile?id=~David_Calhas1), Rui Henriques (/profile?id=~Rui_Henriques1)*

3 Replies Add **Withdraw**

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[–] Official Review of Paper6659 by AnonReviewer2

ECCV 2020 Conference Paper6659 AnonReviewer2

21 May 2020 (modified: 22 May 2020) ECCV 2020 Conference Paper6659 Official Review Readers: Program Chairs, Paper6659 Area Chairs, Paper6659 AnonReviewer2, Paper6659 Authors

Summary Of Contributions:

The paper proposes an approach to synthesize fMRI data from EEG data. This is a compelling problem, and the authors propose an interesting method. However, from my understanding the results do not necessarily demonstrate the feasibility of fMRI reconstruction from EEG. The metrics are not informative for this judgement.

Strengths:

The paper tackles an important problem, and proposes an interesting approach.

It provides an extensive set of quality measures, to evaluate the accuracy of the synthesis.

Weaknesses:
While the aim and approach are relevant and interesting, the main weakness of the paper is that the results don't answer the question if synthesis of fMRI signals from EEG is feasible. The main advantage of fMRI over EEG is spatial resolution. To judge if the synthesis could help neuroscientific analysis, or clinical applications, we would need results that show the spatial resolution of the synthesized fMRI. Currently, the manuscript only mentions one ICA component as the basis for analysis, and I understand that the average signal of this component was used as a training/validation signal.

The paper needs to show the added benefit over approaches the are based on standard models of the hemodynamic response.

It is hard to understand what the numbers presented in tables 1 and 2 mean in terms of usefulness of the reconstructed signal. At a minimum the authors should contrast these similarities with the similarity of e.g., randomly shifted fMRI signals of comparably sized ICA components. That would enable a judging of the specificity of the synthesis.

Suggestion To Authors:
The authors should address the points noted above. In particular expanding on the interpretability of the results with regard to the usefulness of the synthesized signals.

It is unclear (4.2.) how is the region to by synthesized chosen? Was this done before experiments started? Which one is it (e.g., cortical? subcortical?) How big is it spatially?

What do you mean by „The EEG starts“?

Preliminary Rating: 1: Strong reject

Preliminary Rating Justification: The paper explores an interesting avenue, but the results don't support the claim, and need substantial work to become helpful

Confidence: 3: Medium, published weakly related work

Add **Rebuttal**

[-] ## Official Review of Paper6659 by AnonReviewer1

ECCV 2020 Conference Paper6659 AnonReviewer1

14 May 2020 (modified: 22 May 2020) ECCV 2020 Conference Paper6659 Official Review Readers: Program Chairs, Paper6659 Area Chairs, Paper6659 AnonReviewer1, Paper6659 Authors

Summary Of Contributions: This paper studies whether it is feasible to synthesize fMRI data from electroencephalographic (EEG) data based on deep learning. The key idea is to learn a mapping function between multivariate time series with highly dissimilar structures. The experiment results showed the feasibility of EEG to fMRI brain image mappings.

Strengths:
-EEG to fMRI synthesis is an interesting problem to investigate.
-A set of deep learning methods are evaluated for the task.

Weaknesses:
-This paper is poorly written with many grammar errors.
-The technical novelty is limited.
-Many details are not clear in the work.
-The experiment results are not thoroughly discussed.

The following are my main concerns:

(1) I did not see the unique technical challenge of learning a mapping between electroencephalography (EEG) and fMRI data by comparing it to establishing mappings between CT, fMRI, and PET modalities. This also explains why existing methods, e.g, AE, GAN, WGAN, can be used to handle this problem.

(2) Since this work mainly assesses the effectiveness of existing works for EEG to fMRI synthesis, its technical contribution is limited.

(3) Only one dataset is used for evaluation. Therefore, it is difficult to assess whether this work can generalize to other datasets.

Suggestion To Authors: (1) Many details are not clear in this work, e.g, what's the size of the input, eeg, and synth_bold?

(2) It is not clear how to capture the spatial and temporal information in EEG data and synthesize it to the fMRI images.

(3) What's the meaning of each evaluation criterion? Why each of them is used to assess the effectiveness?

(4) The experiment results are not thoroughly discussed. If LCOMB achieves the best results, why it works better than other approaches?

(5) Visual examples should be provided to justify the quality of the generated fMRI images.

Preliminary Rating: 1: Strong reject

Preliminary Rating Justification: The authors investigate an interesting problem. However, I did not see the unique challenge in this problem and the technical novelty of this work is limited.

Confidence: 3: Medium, published weakly related work

Add **Rebuttal**

[–] **Official Review of Paper6659 by AnonReviewer3**

ECCV 2020 Conference Paper6659 AnonReviewer3

25 Apr 2020 (modified: 22 May 2020) ECCV 2020 Conference Paper6659 Official Review Readers: Program Chairs, Paper6659 Area Chairs, Paper6659 AnonReviewer3, Paper6659 Authors

Summary Of Contributions: This paper proposed a framework for synthesis of fMRI signals conditioned on EEG signals. Several commonly used generative models including AE, GAN, Wasserstein GAN are experimented and compared for the synthesis task.

Strengths: 1. The paper addressed a relatively new application domain which is synthesizing brain signals across different modalities.
2. The paper discussed several different ways of training the proposed model as well as different loss terms associated with each type of training.
3. The model is evaluated on a variety of quantitative metrics.

Weaknesses: 1. Despite the strength mentioned, the overall technical novelty is limited. The generative model used are existing commonly used model. The loss function is also not novel.
2. The experimental evaluation can be strengthened. The purpose of each quantitative metrics needs to be better explained to provide insight on the results obtained. Qualitative results should be provided to illustrate the capability of the model. For example, synthesized signals corresponding to a good or bad quantitative metric can be shown. Based on current quantitative results, it is difficult to evaluate whether the model can generate meaningful fMRI signals.
3. The related work section can be trimmed by skipping the technical details of existing works and focus on difference from the proposed method.

Suggestion To Authors: Please address the concerns mentioned in the previous question. I have following additional comments.
1. The Top-K training part, encoder and decoder seem trained separately. If so, have the authors considered end-to-end to training in order to improve the performance?
2. The paper claimed fMRI synthesis but actually synthesize BOLD signals. It is unclear to me whether they are equivalent modality. Please clarify.
3. Based on the discussion in Section 4.2, the pairing between EEG and BOLD may not be precise. How does that affect the final performance of synthesis?

Preliminary Rating: 2: Weak reject

Preliminary Rating Justification: This paper seems to be a primitive work along an interesting direction. The main technical contribution is at the generative model part, which has limited novelty. I'm not convinced on the evaluation due to lack of qualitative results and lack of explanation of the intuition behind all the quantitative metrics. Overall, I feel the interest of this work to ECCV audience is limited.

Confidence: 3: Medium, published weakly related work

Add **Rebuttal**