

Lampiran Peer Review Korespondensi Proses Submit Publikasi Internasional

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Judul Artikel : **Surgery for Radiologically Normal-Appearing Temporal Lobe Epilepsy in a Centre with Limited Resources**

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Muhamad Thohar Arifin <thohar@gmail.com>

Your partial submission to Scientific Reports

Scientific Reports <srep@nature.com>

Wed, Jan 29, 2020 at 2:00 PM

To: thohar@gmail.com

Ref: Submission ID 6cb72046-43ba-4b04-b2e5-719ca6a60962

Dear Dr Thohar Arifin,

Thank you for your recent submission to Scientific Reports, which you began on 29 January 2020 UTC. Please note that this submission is not yet complete.

To complete your submission, please log into the system using the following link and follow the instructions.

<https://submission.nature.com/submission/a126c3b1-b1a2-4969-8fdf-ad741d9a2b15>

IMPORTANT: before completing your submission please check and ensure that your manuscript is formatted according to the submission guidelines (<https://www.nature.com/srep/publish/guidelines>), and adheres to relevant editorial and publishing policies.

All manuscripts are subject to an Initial Quality Check. Failure to adhere to our submission policies will result in the manuscript being returned to you before being sent to an Editorial Board Member.

Common reasons for a manuscript to fail the Initial Quality Check include:

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- Contributing author details not added to the online submission system
- Papers reporting experiments on live vertebrates and/or higher invertebrates missing statements of approval, accordance and (for human subjects) informed consent
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Please note a recent change to our competing interests policy: specifically, the definition of 'competing interests' has broadened to include financial AND non-financial interests (details and guidelines at <https://www.nature.com/srep/journal-policies/editorial-policies#competing>). When submitting your revised paper, please can you ensure this statement refers to 'competing interests' and, if applicable, also list any non-financial competing interests as outlined in our editorial policies? Please note that the Competing Interests statement on the system must match the Competing Interests statement provided in the article file.

Please contact us if you have any questions or require any assistance.

Kind regards,

Peer Review Advisors
Scientific Reports



Muhamad Thohar Arifin <thohar@gmail.com>

Receipt of Manuscript "Surgery on Radiological..."

Scientific Reports <srep@nature.com>

Wed, Jan 29, 2020 at 2:53 PM

To: thohar@gmail.com

Ref: Submission ID 6cb72046-43ba-4b04-b2e5-719ca6a60962

Dear Thohar arifin Muhamad,

Thank you for submitting your manuscript to Scientific Reports.

Your manuscript is now at our initial Quality Check stage, where we look for adherence to the journal's submission guidelines, including any relevant editorial and publishing policies. If there are any points that need to be addressed prior to progressing we will send you a detailed email. Otherwise, your manuscript will proceed into peer review.

Please note you have submitted to a new peer review system which does not yet offer the ability to track your manuscript status.

Kind regards,

Peer Review Advisors

Scientific Reports

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Muhamad Thohar Arifin <thohar@gmail.com>

Fwd: Decision on your manuscript

Muhamad Thohar Arifin <thohar@gmail.com>

Tue, Mar 24, 2020 at 8:29 AM

To: surya pratama brilliantika <suryapratamabrilliantika@gmail.com>, Zainal Muttaqien <zainalm57@gmail.com>, Yuriz Bakhtiar <yuriz_b@yahoo.co.id>, rofat askoro <rofataskoro@gmail.com>

Sent from my iPhone

Begin forwarded message:

From: Scientific Reports <srep@nature.com>
Date: 23 March 2020 12.16.27 GMT+7
To: thohar@gmail.com
Subject: Decision on your manuscript

Ref: Submission ID 6cb72046-43ba-4b04-b2e5-719ca6a60962

Dear Dr Muhamad,

Your manuscript, "Surgery on Radiological Normal Appearing Temporal Lobe Epilepsy", has now been reviewed and the reviewer comments appended below. You will see that, while the reviewers find your work of interest, they have raised points that need to be addressed.

Editorial Board Member comments

I am looking forward to receiving the revised manuscript.

Eishi Asano, MD, PhD

We therefore invite you to revise your paper, taking into account the points raised. At the same time, we ask you to make sure your manuscript complies with our format requirements detailed here:

<https://www.nature.com/srep/author-instructions/submission-guidelines>

Once you have revised your paper, please use the following link to submit it, making sure you abide by the submission requirements listed below:

<https://submission.nature.com/submit-revision/6cb72046-43ba-4b04-b2e5-719ca6a60962>**SUBMISSION REQUIREMENTS FOR REVISED PAPERS:**

In order to process your paper, we require the following:

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- A point-by-point response to any issues raised by the reviewers. This must be uploaded as 'Related file'. Our guide to writing a response to the reviewers' comments provides advice for maximising its effectiveness.

https://www.nature.com/documents/Effective_Response_To_Reviewers-1.pdf

- Source files for your submission: word.doc or LaTeX

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- The reference .bib file, either included in the zip package, or as separate file under 'Supplementary Information'.

At this stage, please also ensure that you have replaced your initial-submission image files with production quality figures. These should be supplied at 300 dpi resolution for .jpeg and .tiff or as .eps files. Figures should not include Figure number labels in the image (guidelines:

<https://www.nature.com/srep/author-instructions/submission-guidelines#figures-publication>).

Please note that your revised manuscript will be subject to another round of quality checking before it is returned to the Editorial Board Member for assessment.

We hope to receive your revised paper within four weeks. If you cannot send it within this time, please let us know so that we can close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as you haven't submitted similar or related work elsewhere in the meantime.

Kind regards,

Eishi Asano
Editorial Board Member
Scientific Reports

Reviewer Comments:

Reviewer 1

The authors have provided a retrospective review of their series of patients undergoing surgery for medically intractable temporal lobe epilepsy over a 10 year period. Unique aspects of the investigation are the population studied was MRI negative and a long term follow-up of post operative patients.

The Kaplan Meier plot identified 86% seizure freedom initially after surgery with a gradual decline due to recurrent seizures to 59% of patients seizure free at 12 years.

The authors remark that nonlesional epilepsy is a more challenging patient population in the "developing world". Is developing world meant to signify low income countries? A more accurate statement is that nonlesional epilepsy is more challenging in general and may be more so in low income countries due to a lack of resources, advanced imaging tools, and limited expertise to adequately evaluate and treat these patients. However, Indonesia is a middle income country which has medical centers with high level expertise and advanced imaging capabilities. The patients described in this manuscript had access to EEG video monitoring, 1.5T MRI, FDG PET, and intracranial EEG electrode monitoring. Most patients in low income countries have no access or very limited access to these capabilities. The Semarang, Indonesia medical center reporting their epilepsy surgery results does not resemble a "developing country" as far as access to resources for their patients.

The authors should report differences in selective versus anterior temporal resection outcomes.

The histopathology should be reported from the patients operated

The PET scan was hypo-metabolic localizing and lateralizing in most patients who had FDG-PET imaging. This suggests that these patients were likely not truly non-lesional.

The Introduction states patient data from "epilepsy centers in Indonesia" were collected. Where were the epilepsy centers?

In the results section outcomes are reported for 82 patients but table 3 describes surgery in 153 patients. If this report is describing surgical outcomes in 82 patients then the results and tables need to be clear and consistent about patients operated and patients with follow-up results. The authors should report the results of the patient population being studied, which I understand to be the 82 patient cohort. Currently the results are confusing as to the patient population being investigated and reported.

The Conclusion needs improvement. The style is clumsy and not informative.

Reviewer 2

This paper focuses and discusses about an important issue of epilepsy surgery in Indonesia. However, several concerns can be raised for publication. Please consider to fix following issues for improvement of your paper.

Major comments

I believe this paper fails to stress authors' biggest advantage, providing prominent medical treatment (epilepsy surgery) with limited resources. I recommend you to put this prominent outcome to in front of this paper. To do so, 1) first paragraph of discussion part should start with stressing the result, and 2) interpret your data sets with past related articles (in present form, authors tended to just describe results of past papers, did not use them for interpreting authors' data)

P2, line 13 (Abstract) & P4, line 31 (Result): It looks unclear for me how 82 patients were selected from 154 patients? Please add explanation about it.

P3, line 18 (Methods): In current form, "Imaging protocols" seems to include not only protocol, but also criteria of inclusion or exclusion. Authors may describe criteria of inclusion or exclusion in other part, named such as "inclusion/exclusion criteria".

P3, line 23 (Methods): Authors should clearly describe how they decided MRI as non-lesional. For example, “two out of three neuroradiologists agreed to brain as non-lesional” or “Consensus of professional team consisting of experienced neuroradiologists, epileptologists, and neurosurgeons agreed to brain as non-lesional”

P3, line 25 (Methods): It looks vague what “FDG-PET for patients without lateralization” means. Authors should clearly how “lateralization” can be decided, based on ictal EEG?, interictal EEG?, or both. When it can be judged as “without lateralization”.

P3, line 26 (Methods): In surgical consideration part, authors raised judgment criteria of introducing subdural electrodes (SDEs). To clarify possible epileptogenic zone in the raised criteria, extensive placement of SDEs would be needed. Your readers may have questions; “Was it possible to introduce bitemporal SDGs in non-lesional TLE in this study?” In result part, if needed, authors may present representative cases’ history and its detailed results.

P4, line 8 (Methods): Does “baseline 12 months” mean “patients followed over 1 year were included”? I am sorry in case my confusion. Please give additional explanation.

P4, line 8 (Result): It would be good for authors to evaluate relationship between seizure type (FAS, FIAS, FIAS to GTCS), interictal EEG, ictal EEG, PET findings way of surgery, and prognosis to deepen current study. It may also be ideal to add detailed information about aura type to current seizure classification (FAS, FIAS, FIAS to GTCS), because aura reflects symptomatogenic (or possible epileptogenic) zone and it may associate with seizure prognosis after surgery.

P5, line 16 (Discussion): I believe “shorter” duration of epilepsy will be good prognosis factor. Please double check.

P5, line 17 (Discussion): It will be good for readers’ understanding that “localized EEG focus in ipsilateral the temporal lobe” is replaced with original description by Wang et al. “ictal or interictal electroencephalographic anomalies precisely localized in the ipsilateral temporal lobe”.

At tables, several patients look not included, as represented by summation of patients number in some characteristics did not reach to total number (n=154). For example, it looks 2 patients’ information missing at seizure frequency at table 1. At table 2, 2 patients’ information looks missing in PET scan, one patient’ missing in scalp EEG, six patients’ missing in EEG focus. At table 3, 11 are missing in surgical side, and 9 are missing in surgery type.

It is confusing for me that “subdural EEG was implanted in 21 patients (P4, line 24)” and “7 patients received ECoG (table 3)”. With my understanding, the both are same device. Please give explanation.

Many references (#6-8, 10, 12-14, 16, 17) looks to be described with incomplete form, like missing journal name. Please fix them.

Minor comments

P2, line 7 (Abstract): “Centers” may be replaced with “center”, as I understood this study was performed in the Kariadi hospital.

P2, line 11 (Abstract): Similar terms may be unified ideally. (e.g. “Anterior temporal lobe resection” may be unified to “Anterior temporal lobectomy”)

Authors used “too much” conjunction (e.g. Furthermore, meanwhile, moreover...). Sometimes, they looks used in inappropriate manner. I recommend not to use too much conjunction, as text (or collection of sentences) can be complete without conjunction.

Inappropriate use of capital letter was seen at several parts (e.g. P2, line 11 (Normal)), Please double-check them.

At table 1, Male/Female may be shown in the same row, like Male/Female 91/63..

At table 3 (and P4, line 27), “selective hippocampal-amygdalectomy (SHA)” may be unified to “selective amygdala-hippocampectomy (SAH)”

At figure 1, Kaplan Meier plot starts form 6 years after surgery. Does it mean all include patients had no seizure 6 years after surgery?! Or patients without seizure 6 years after surgery were subjects of your study?

Surgery for Radiologically Normal-Appearing Temporal Lobe Epilepsy: Experience in a Centre with Limited Resources

Responses to Reviewers' Comments

Reviewer 1

The authors have provided a retrospective review of their series of patients undergoing surgery for medically intractable temporal lobe epilepsy over a 10 year period. Unique aspects of the investigation are the population studied was MRI negative and a long term follow-up of post operative patients.

The Kaplan Meier plot identified 86% seizure freedom initially after surgery with a gradual decline due to recurrent seizures to 59% of patients seizure free at 12 years.

The authors remark that nonlesional epilepsy is a more challenging patient population in the "developing world".

Is developing world meant to signify low income countries?

A more accurate statement is that nonlesional epilepsy is more challenging in general and may be more so in low income countries due to a lack of resources, advanced imaging tools, and limited expertise to adequately evaluate and treat these patients. However, Indonesia is a middle income country which has medical centers with high level expertise and advanced imaging capabilities. The patients described in this manuscript had access to EEG video monitoring, 1.5T MRI, FDG PET, and intracranial EEG electrode monitoring. Most patients in low income countries have no access or very limited access to these capabilities. The Semarang, Indonesia medical center reporting their epilepsy surgery results does not resemble a "developing country" as far as access to resources for their patients.

Author response

Thank you for this observation. As you point out, it is difficult to strictly generalise on a setting, especially for an entire country. Semarang is the only epilepsy centre in Indonesia and it therefore has to serve a population of ¼ billion. The budget allocation for healthcare in Indonesia is only 5% of the national budget. We have had only one FDG PET (in Jakarta) since 2010.

Even with lower technology, equipment may be scarce. In 2010 we used a separate video camera and synchronized the time manually with the EEG. We began using EEG video in 2011. At present there are only 3 EEG video centres in Indonesia. Our EEG electrode monitoring is part of a gift from Japanese doctors who visited Indonesia to teach us. Subdural EEG electrode was donated by a Japanese partner center.

We should state that our health sector is typical of that of a developing country. There are only a few advanced technical resources that serve a vast population. However, your point is very valid (and in fact our second reviewer also noted this) because it appears that we have quite a list of high-technology equipment at our disposal.

It was recommended that the paper could use this as a positive aspect of our operations. We have therefore referred to our situation as being a centre with limited resources. We have indicated this in a number of places in the manuscript (Page3-line 27, Page4-line 7-9).

Reviewer Comment

The authors should report differences in selective versus anterior temporal resection outcomes.

Author response

Thank you for this suggestion. We have included this on page 9, line 18.

Reviewer Comment

The histopathology should be reported from the patients operated

Authors' response:

This is a valid observation which we appreciate. Unfortunately, the quality of our histology registry was not good over the period of the study. At times, there would be no anatomic pathologist who properly understood the pathology of epilepsy and therefore we did not have a report for it. The data is unfortunately incomplete and we have not been able to include it in this study.

Reviewer Comment

The PET scan was hypo-metabolic localizing and lateralizing in most patients who had FDG-PET imaging. This suggests that these patients were likely not truly non-lesional.

Authors' response:

FDG PET is performed if temporal semiology scalp EEG does not meet lateralisation or when lateralisation is suspected.

Method (Page 6, Lines 9-13): Additional functional neuroimaging studies with FDG-PET were obtained for patients when a consensus regarding the lateralisation of the semiology was not achieved.

Reviewer Comment

The Introduction states patient data from "epilepsy centers in Indonesia" were collected. Where were the epilepsy centers?

Authors' response:

Thank you for identifying this mistake. We have corrected it (Page 4, Line 25).

Reviewer Comment

In the results section outcomes are reported for 82 patients but table 3 describes surgery in 153 patients. If this report is describing surgical outcomes in 82 patients then the results and tables need to be clear and consistent about patients operated and patients with follow-up results. The authors should report the results of the patient population being studied, which I understand to be the 82 patient cohort. Currently the results are confusing as to the patient population being investigated and reported.

Authors' response:

We agree that we have not been clear enough about this and we have made some corrections. The difference in figures was caused by patients who dropped out because of geographical difficulties and could not be contacted by phone (page 8, lines 9–12).

Reviewer Comment

The Conclusion needs improvement. The style is clumsy and not informative.

Authors' response:

We agree that the conclusion did not do justice to the study and have therefore rewritten it.

Reviewer 2

This paper focuses and discusses about an important issue of epilepsy surgery in Indonesia. However, several concerns can be raised for publication. Please consider fixing following issues for improvement of your paper.

Major comments

I believe this paper fails to stress authors' biggest advantage, providing prominent medical treatment (epilepsy surgery) with limited resources. I recommend you to put this prominent outcome to in front of this paper.

Authors' response

Thank you for the observation and useful suggestion. We have taken your advice and stressed this more at appropriate places in the manuscript.

Reviewer Comment

- 1) first paragraph of discussion part should start with stressing the result, and sdah..
- 2) interpret your data sets with past related articles (in present form, authors tended to just describe results of past papers, did not use them for interpreting authors' data)

Authors' response

We appreciate this important observation. We have taken another look at the literature and how it can be used to interpret our data. We have highlighted the passages which are as follows:

1. Compare my result to another result and analyses the difference (in term of sizure free result)
2. Evaluation for using advance radiological evaluation

Reviewer Comment

P2, line 13 (Abstract) & P4, line 31 (Result): It looks unclear for me how 82 patients were selected from 154 patients? Please add explanation about it.

Authors' response

We agree and have added the explanation about patients lost to follow-up as explained above.

Reviewer Comment

P3, line 18 (Methods): In current form, "Imaging protocols" seems to include not only protocol, but also criteria of inclusion or exclusion. Authors may describe criteria of inclusion or exclusion in other part, named such as "inclusion/exclusion criteria".

Authors' response

We agree with your observation and have moved the criteria to the relevant section on page 6, lines 8–13.

Reviewer Comment

P3, line 23 (Methods): Authors should clearly describe how they decided MRI as non-lesional. For example, “two out of three neuroradiologists agreed to brain as non-lesional” or “Consensus of professional team consisting of experienced neuroradiologists, epileptologists, and neurosurgeons agreed to brain as non-lesional”

Authors' response

We agree that this should be included as we did indeed have an effective system. We have added the relevant text on page 6, lines 8–11.

Reviewer Comment

P3, line 25 (Methods): It looks vague what “FDG-PET for patients without lateralization” means. Authors should clearly how “lateralization” can be decided, based on ictal EEG?, interictal EEG?, or both. When it can be judged as “without lateralization”.

Authors' response

Thank you for this suggestion. We have accordingly added the following information: Additional functional neuroimaging studies with FDG-PET were obtained for patients when a consensus regarding the lateralisation of the semiology was not achieved.

Reviewer comment

P3, line 26 (Methods): In surgical consideration part, authors raised judgment criteria of introducing subdural electrodes (SDEs). To clarify possible epileptogenic zone in the raised criteria, extensive placement of SDEs would be needed. Your readers may have questions; “Was it possible to introduce bitemporal SDGs in non-lesional TLE in this study? “ In result part, if needed, authors may present representative cases' history and its detailed results.

Authors' response:

Thank you for this suggestion. We will include the additional data in a table as you have suggested.

Reviewer comment

P4, line 8 (Methods): Does “baseline 12 months” mean “patients followed over 1 year were included”? I am sorry in case my confusion. Please give additional explanation.

Author respond:

Authors' response

Your observation is noted. We have therefore added a better explanation to ensure clarity.

Reviewer comment

P4, line 8 (Result): It would be good for authors to evaluate relationship between seizure type (FAS, FIAS, FIAS to GTCS), interictal EEG, ictal EEG, PET findings way of surgery, and prognosis to deepen current study. It may also be ideal to add detailed information about aura type to current seizure classification (FAS, FIAS, FIAS to GTCS), because aura reflects symptomatogenic (or possible epileptogenic) zone and it may associate with seizure prognosis after surgery.

Authors response:

We agree that it would be a useful addition and have added the following to the manuscript: (page 9, line 22).

Reviewer comment

P5, line 16 (Discussion): I believe “shorter” duration of epilepsy will be good prognosis factor. Please double check.

Authors’ response

We appreciate the observation and have checked this. Although our data is not significant, the total number of seizure-free patients was more than the number of seizure-persistent patients.

Reviewer comment

P5, line 17 (Discussion): It will be good for readers’ understanding that “localized EEG focus in ipsilateral the temporal lobe” is replaced with original description by Wang et al. “ictal or interictal electroencephalographic anomalies precisely localized in the ipsilateral temporal lobe”.

Authors’ response:

We agree that it would be less confusing if we quoted the author directly and we have therefore inserted the quote on page 11, line 10.

Reviewer Comment

At tables, several patients look not included, as represented by summation of patients number in some characteristics did not reach to total number (n=154). For example, it looks 2 patients’ information missing at seizure frequency at table 1. At table 2, 2 patients’ information looks missing in PET scan, one patient’ missing in scalp EEG, six patients’ missing in EEG focus. At table 3, 11 are missing in surgical side, and 9 are missing in surgery type.

Authors’ response:

A review of the excel data revealed some errors in cell formatting. We have made revisions in the data shown in **Tables 1, 2, and 3.**

Reviewer Comment

It is confusing for me that “subdural EEG was implanted in 21 patients (P4, line 24)” and “7 patients received ECoG (table 3)”. With my understanding, the both are same device. Please give explanation.

Authors’ response

The terminology we used was as follows. The subdural EEG was implanted, and long-term video EEG from these electrodes was recorded. ECoG was the intra-operative EEG monitoring.

Reviewer Comment

Many references (#6-8, 10, 12-14, 16, 17) looks to be described with incomplete form, like missing journal name. Please fix them.

Authors’ response

Thank you. The references have been checked and amended where necessary.

Reviewer Comment

At figure 1, Kaplan Meier plot starts form 6 years after surgery. Does it mean all include patients had no seizure 6 years after surgery?! Or patients without seizure 6 years after surgery were subjects of your study?

Authors' response:

Yes. Your initial observation is correct. All patients remained seizure-free for up to 5 years.

Minor comments

Author responds: Thank you for these observations below, they have been addressed.

P2, line 7 (Abstract): “Centers” may be replaced with “center”, as I understood this study was performed in the Kariadi hospital.

Author respond:

P2, line 11 (Abstract): Similar terms may be unified ideally. (e.g. “Anterior temporal lobe resection” may be unified to “Anterior temporal lobectomy”)

Authors used “too much” conjunction (e.g. Furthermore, meanwhile, moreover...). Sometimes, they looks used in inappropriate manner. I recommend not to use too much conjunction, as text (or collection of sentences) can be complete without conjunction.

Inappropriate use of capital letter was seen at several parts (e.g. P2, line 11 (Normal)), Please double-check them.

At table 1, Male/Female may be shown in the same row, like Male/Female 91/63.

Author respond: Table 1

At table 3 (and P4, line 27), “selective hippocampal-amygdalectomy (SHA)” may be unified to “selective amygdala-hippocampectomy (SAH)”

Author respond: Table 5



Muhamad Thohar Arifin <thohar@gmail.com>

Scientific Reports: "Surgery for Radiologically Normal-Appearing Temporal Lobe Epilepsy: Experience in a Centre with Limited Resources "

Vaishnav Khade <vaishnav.khade@springernature.com>
To: "thohar@gmail.com" <thohar@gmail.com>

Mon, Apr 27, 2020 at 11:08 AM

Ref: Submission ID 6cb72046-43ba-4b04-b2e5-719ca6a60962

Dear Prof. Muhamad Thohar Arifin,

I hope this email finds you well.

Congratulations for the acceptance of your manuscript for publication in Scientific Reports. Before we proceed with accepting your manuscript, we request you to kindly rewrite the title.

In-house Editorial Feedback:

To aid our readers, and to maximise the accessibility of your manuscript, the title should have a clear, precise scientific meaning and should not contain a colon. Where possible, the title should be read as one concise sentence. Please could you re-write the title ensuring that it is informative and appropriate?

The link to make necessary change is given here: <https://submission.nature.com/submission/15e9d871-2fe6-40d5-940a-a72840109f7c>

Please note that only changes in the title of the manuscript and not in any of the other contents of the manuscript. Please ensure the new title reflects on the system, in the manuscript file and the cover letter as well.

Please feel free to contact me in case of any queries.

Kind Regards,

Vaishnav Khade

Peer Review Advisor

Scientific Reports

Nature Research

email: vaishnav.khade@springernature.com

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Muhamad Thohar Arifin <thohar@gmail.com>

Decision on your manuscript

Scientific Reports <srep@nature.com>
To: thohar@gmail.com

Mon, Apr 27, 2020 at 2:16 PM

Ref: Submission ID 6cb72046-43ba-4b04-b2e5-719ca6a60962

****COVID 19 and impact on peer review****

As a result of the significant disruption that is being caused by the COVID-19 pandemic we are very aware that many researchers will have difficulty in meeting the timelines associated with our peer review process during normal times. Please do let us know if you need additional time. Our systems will continue to remind you of the original timelines but we intend to be highly flexible at this time.

Dear Dr Thohar Arifin,

We're delighted to accept your manuscript, "Surgery for Radiologically Normal-Appearing Temporal Lobe Epilepsy: Experience in a Centre with Limited Resources", for publication in Scientific Reports. Thank you for choosing to publish with us.

Editorial Board Member comments

I would like to congratulate the effort by the authors. I hope all of the authors, friends, colleagues, and families will stay safe. Eishi Asano, MD, PhD

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