REVIEWS and RESPONSES for:

BOUCHON ET AL. 2023, **Observation of a Synchronicity between Shallow and Deep Seismic Activities during the Foreshock Crisis Preceding the Iquique Megathrust Earthquake.** Seismica v2 n.2

Reviewer 1: Pascal Audet

This paper presents new evidence supporting long-range connections between shallow megathrust and deep earthquakes using data from the magnitude M 8.1, 2014 Iquique earthquake in Chile. This evidence is based on the synchronicity of shallow and deep moderate (M>4) earthquakes, their along-dip connections, and the apparent earthquake tracks linking deep and shallow small-magnitude earthquakes in the foreshock sequence. Based on these observations, the authors propose a conceptual model linking fluid-pressure pulses in subducted channels from their source to the shallow seismogenic zone.

This is an interesting set of observations that was also recently reported in other subduction zones and by different means and authors. Such observations yield new insights into far-field deformation and mass changes preceding large fault ruptures. In this paper, the authors describe them at face value; the paper only provides more data and makes conjectures about a potential model that might explain them. It is essentially a repetition of another paper published in EPSL in 2022 by some of the same authors but for a different subduction zone.

My main concern about this paper is the lack of quantitative tests to convince readers that these observations are not observed by chance. For instance, in one part of the text, the authors note (lines 116-117): "(...) one has to realize that deep activity is continuously present in this subduction, regardless of the presence or not of foreshocks." This begs the question of whether the observed synchronicity is due to chance, given that deep earthquakes occur independently of shallow ones. Interestingly, in their 2022 paper, the authors use statistical tests to support their claims. Similar tests should be carried out to address this. Simple visual inspections of the new data are unconvincing.

The Discussion is based on the same discussion as their 2022 study because similar results were found previously. I find it somewhat underwhelming, and with just a bit more work, the new data set could help inform the proposed model. For instance, what is the variability in the time scales predicted in other types of study (e.g., mineralogy and geochemistry), and how does it compare to the results found herein? If there is a discrepancy, how can we use this information to refine the model (i.e., hydrologic properties of the subduction channel)? What other hypothesis exists to explain this type of long-range connection? How could we test it using independent data? Without a broader discussion, this paper is essentially a confirmatory study where the only novelty is the new set of observations.

The figures should be improved:

- All the maps are low resolution with an ugly and saturated colormap for topography. Printed in black and white, these maps are difficult to decipher.
- When not absolutely necessary, colours should not be used (e.g., Figs 2 and 3). Use shades of grey instead.
- In Figs 2 and 3, the width of the bars and the lack of transparency hide many of the finer details.
- The colours used in Fig 4 are not colour-blind friendly, especially given the background colour map (which could be changed to a grey-shaded relief map). Perhaps use different symbols instead, and link each burst with connecting lines? The sense of time between these bursts is also lost in this plot – they are grouped by clusters, but it's not clear which way the connection is going (from deep to shallow or the other way around).
- Fig 5 is interesting but doesn't highlight the "streaks" (or tracks, as referred in the paper) as well as it could. The streaks should be more visible on a "difference" plot, where the difference is between two density distributions (e.g., KDE). I will note that, in addition to the streaks, there is a clear decrease in activity for slab depths of 60-80 km between May and August – is this robust, and how can you explain it? Furthermore, adding earthquake depth information in the figure should be insightful here; it's unclear whether the streak represents all crustal earthquakes in the upper plate or follow the slab dip. This is also not discussed in the text.
- I feel like there should be a summary or conceptual figure that summarizes the model and the observations.

Reviewer 2: Cailey Condit

This paper presents earthquake data preceding the Iquique Megathrust Earthquake in Chile in 2014. The authors show that up-dip and down-dip of the earthquake in several time clusters there are moderate sized earthquakes. They also discuss some smaller seismicity migrating between the up-dip shallow and downdip deep reactions. Given the tight observed temporal clustering of these events, and the spatial correlation (directly down dip from shallow and deep events clustered in time), the workers invoke a fluid-connection between these preceding events clustered in time and partly in space. While these observations are very interesting, I find the mechanism invoked for this connection vague and not grounded enough in the actual geologic processes occurring within the subducting slab, nor are they consistent with our current understanding of the properties of these materials (permeability, porosity etc).

While I am not an earthquake seismologist, and I cannot comment on the catalog and time-space patterns shown here in detail, I am sure another reviewer has the expertise to review this portion of the contribution. My expertise is more in the petrologic, geologic, and physical properties/processes the authors invoke to explain the correlation in time between these up-dip and down-dip events.

Below I detail some of the challenges I find with the mechanism discussed and suggest a path froward for the authors: Can you be clear about if you are invoking actual up-dip movement of fluids from ~90 km depths all the way up to ~20-30 km depths where the shallow events occur? This would require fast timescales (the workers mention migration rate could be similar to that of tremor), but simple calculations for the flux rate of fluids through these very low permeability rocks (how low is hard to say, but quite low, even if these fluids can move through migrating fracture-vein networks), which would preclude these very fast timescales needed for fluid transfer.

To this end, the geologic studies on fluid production at depth in the subduction system that are cited here are all strong contributions, but really only focus on the time-scales of dehydration reactions (the production not movement of these fluids). While it still remains challenging to constrain these reaction timescales, the work you cite does suggest these dehydration events may be quite fast, perhaps even on the timescales needed for these fluids to cause the deepest events (e.g., Plümper et al., 2017). However, these studies are not able to robustly comment on the fluid migration *rates* up-dip along the plate interface. The Taetz et al., 2018 discusses this as an option, but again does not have a robust handling of the permeabilities or porosities needed to move these fluids. Thus, I was left wondering, do you mean pore-pressure waves caused by increased fluid rather than up-dip fluid movement (Cruz-Atienza et al., 2018) rather than actual literal fluxes of fluids from these deep depths so far? I suggest making this much clearer in the discussion. The the differences between these two end member models is quite large from a process perspective. A simple set of calculations for the porosoity and permeabilities needed to move fluids up-dip that far that quickly would likely suggest this is not possible.

Additional constraints: It is also apparent from the isotopic record of exhumed terranes that fluids from below the subduction seismogenic zone (as deep as the antigorite-out reactions you invoke in the hydrated portions of the subducting lithospheric mantle do not appear to be making it up to such shallow depths (e.g., 20 km depth). There has been some up-dip movement observed from these studies, but the timescales of those fluid movements are likely quite long (Jaeckel et al., 2018; Epstein et al., 2020).

Minor Comments:

Fig. 4 colors for temporal labels are challenging to read or see. I suggest reworking these colors (black text or different colored backgrounds) to be able to property see the time-space patterns in question.

EDITOR DECISION LETTER

Dear Michel Bouchon, Stephane Guillot, David Marsan, Anne Socquet, Jorge Jara, Francois Renard:

Thank you for your patience while awaiting reviews on your manuscript "Observation of a Synchronicity between shallow and deep seismic activities during the foreshock crisis preceding the Iquique megathrust earthquake. " I have received two thorough and thoughtful reviews.

Both reviewers found the observations interesting, but found the manuscript's core interpretation on the mechanism of connecting shallow and deep activities insufficiently defined and supported. One reviewer pointed out that statistical tests performed by yourselves and colleagues in a similar recent manuscript had not been reported for this Iquique case. The other mentioned that reasonable tests of permeability and timescales for long-distance fluid transport or pressure pulse migration have been published in similar settings.

I agree with Reviewer 1's comments that the figures need improvement - specifically, more annotation, attention to visualization quality issues caused by color choice, lack of legends, and to graphically demonstrate correlations directly instead of requesting readers to pick them out of the data.

I invite you to revise the manuscript to address these questions and the rest of the reviewer comments. As these are fairly substantive revisions, I will send the revised manuscript back to review.

Kind regards,

Christie Rowe

christie.rowe@mcgill.ca

AUTHORS' RESPONSE AND STATEMENT OF REVISIONS:

RESPONSE TO EDITOR AND REVIEWERS
Responses are in bold characters. Added text in the revised manuscript is in red Dear Christie Rowe,

We thank you for your work on our manuscript and for your comments. We appreciate very much the time you spent on it and your helpful comments. Below is a rapid summary of the changes we have made in the revision before addressing in details the comments of the two reviewers:

- One reviewer noted that statistical tests presented in your recent EPSL paper on seismic bursts before the Tohoku paper are absent in this manuscript. **We have now added these tests and they show that the probability that the synchronisations observed in Figs. 2b, 2c and 3 are produced by chance are in each case less than 10^{-5}.**

- The other reviewer pointed out that the mechanism of updip-downdip communication is insufficiently well defined, but recent work in the field has offered tests for timescales of either pore pressure pulse translation or pore fluid transport based on estimates of in situ permeability and hydraulic diffusivity. **We have modified and largely expanded the discussion section to address these issues. The aim of the paper, as its title says, is to report the observations of synchronicity which we show are statistically robust and, according to the reviewers' comments, interesting. We think this alone would warrant publication of the paper. We also recognize that this unexpected synchronicity may challenge some of our present understanding of slab dynamics. In the new discussion we try to clarify what the observations tell us about the updip-downdip communication mechanism involved. What our observations show is that such a communication exists, at least intermittently, during the foreshock crisis. This communication is hard to conceive without the presence of transient fluid connections in which pressure pulses or pressure gradients can propagate. We discuss now that this condition could be met by the presence of either fluid channels or localized zones of high permeability along the plate interface in which pore-pressure waves would propagate. The theoretical work of Atienza et al. cited by the reviewer is quite pertinent in this respect as it shows that pore-pressure waves could propagate along the plate interface at the fast velocities that we observe. It is not possible to define more precisely the mechanism involved because there is no direct information in the observations on the fluid flow itself (how big how rapid it is).**

- . I also urge you to follow carefully reviewer 1's comments regarding the figures - the data is presented with some challenging color combinations that make the figures hard to read, and the patterns invoked in the interpretation should be explicitly visualized instead of asking readers to pick them out indirectly from the data. **We have tried to answer these requests by redrawing most of the figures, replacing red and blue colors in Figs 2 and 3 by more color-blind friendly colors, removing topographic colors in Figs 4 and 5 and using symbols to identify bursts in Fig. 4.**

Reviewer B:

This paper presents new evidence supporting long-range connections between shallow megathrust and deep earthquakes using data from the magnitude M 8.1, 2014 Iquique earthquake in Chile. This evidence is based on the synchronicity of shallow and deep moderate (M>4) earthquakes, their along-dip connections, and the apparent earthquake tracks linking deep and shallow small-magnitude earthquakes in the foreshock sequence. Based on these observations, the authors propose a conceptual model linking fluid-pressure pulses in subducted channels from their source to the shallow seismogenic zone.

This is an interesting set of observations that was also recently reported in other subduction zones and by different means and authors. Such observations yield new insights into far-field deformation and mass changes preceding large fault ruptures. In this paper, the authors describe them at face value; the paper only provides more data and makes conjectures about a potential model that might explain them. It is essentially a repetition of another paper published in EPSL in 2022 by some of the same authors but for a different subduction zone.

My main concern about this paper is the lack of quantitative tests to convince readers that these observations are not observed by chance. For instance, in one part of the text, the authors note (lines 116-117): "(...) one has to realize that deep activity is continuously present in this subduction, regardless of the presence or not of foreshocks." This begs the question of whether the observed synchronicity is due to chance, given that deep earthquakes occur independently of shallow ones. Interestingly, in their 2022 paper, the authors use statistical tests to support their claims. Similar tests should be carried out to address this. Simple visual inspections of the new data are unconvincing.

Thank you for this comment. We have now added the statistical tests requested and have added the following text which presents them in the discussion of Fig.2: "In statistics the two time series displayed in Fig. 2, are termed temporal point processes. To estimate the probability that one temporal point process (A) is dependent on the other one (B), a distribution of interevent times is constructed by fixing the events from series (B) and measuring the time from each event in (A) to the closest event in (B). This method is described in Galbraith et al. (2020). Probability is calculated by fixing the timings of the deep events, drawing randomly the timings of the shallow events, and comparing their mean interevent time with the one observed. In doing so we do not make any hypothesis on any of the properties of the two time series. We simply look if the interevent time observed is due to random chance or if it is an intrinsic property of the data. The application of the method to seismic sequences is straightforward and described in Bouchon et al. (2022). In Fig. 2b (first crisis) the chance probability that shallow events (i.e. foreshocks) are as closely synchronized with the occurrence of deep events is < 10^{-5} (more than 100,000 random draws of the 9 M>4 shallow events are required to reach an interevent time with the 7 deep events present as small as the one observed). A similarly small chance probability that shallow events occurring during the second crisis (Fig. 2c) would be as closely synchronized with deep events located within 200km of epicentral distance is obtained. The combined probability that shallow events would be as closely synchronized with deep events below during the two foreshock crises is thus infinitesimal. The smallness of the values may seem surprising but it likely reflects the burst-like characteristic of the seismicity: As shown in Fig. 3, a burst is not simply made up of one shallow and one deep events but usually of a multiplicity of them interweaved together within a short time, a characteristic difficult to be reproduced by a random process."

And for the second figure presenting the synchronization (Fig. 3) we have added; "Calculating, as in Fig. 2, the chance probability that the interevent time between the shallow (26 events) and the deep (22 events) occurrences is as small as the one observed yields a value < 10^{-5}."

The Discussion is based on the same discussion as their 2022 study because similar results were found previously. I find it somewhat underwhelming, and with just a bit more work, the new data set could help inform the proposed model. For instance, what is the variability in the time scales predicted in other types of study (e.g., mineralogy and geochemistry), and how does it compare to the results found herein? If there is a discrepancy, how can we use this information to refine the model (i.e., hydrologic properties of the subduction channel)? What other hypothesis exists to explain this type of long-range connection? How could we test it using independent data? Without a broader discussion, this paper is essentially a confirmatory study where the only novelty is the new set of observations.

We have changed and expanded the discussion section to address these important questions. In doing so we try to build the discussion of what the new observations tell us about the mechanism involved. We also discuss a possible alternative model to the presence of fluid channels. The additional text is "The present observations suggest a mechanism involving the circulation of pressure pulses in fluid-filled channels. The burst- like nature of the seismic activity would indicate that these channels are very transient in time, opening during overpressure passage and closing as soon as fluid pressure locally in the channel drops below local confining pressure. This characteristic of the seismicity supports that pressure propagation and fluid flow, like the opening of the channel, are very intermittent. The along-dip organization of the bursts denotes an along-dip orientation of the channels, which probably reflects the strong down-slip corrugation of the Nazca slab interface (Soto et al., 2019). Such corrugations have been recently proposed to act as fluid conducts (Edwards et al., 2018). The occurrence of the events in packets of short duration including both shallow and deep events, often interweaved together, suggests that they are associated with the updip and downdip propagation of pressure pulses. While surges of overpressured fluids in the seismogenic zone are probably producing the foreshocks, they are accompanied by decompression pulses propagating downdip.

Another clear characteristic of the seismic activity is its long remarkable extension along the strike of the subduction (Figs. 1 and 2). This long extension of the activity does not evolve in a continuous fashion but occurs in jumps. For instance, after ~4 months of quiescence, the second crisis begins suddenly in early January ~150km away from where the first crisis had started and 50km beyond the zone where foreshocks had previously occurred. The activity was strong there for a few days, then completely disappeared and

by the end of January, foreshock activity had jumped back to a zone close to where it initiated.

The major characteristics that are observed, the rapidity of the up-dip/down-dip interactions, the jumps of the activities along subduction strike, the broad width of the subduction zone involved are not characteristics unseen before. These same characteristics have long been reported for tremors. What is novel here and before the Tohoku earthquake are the very long range and the depth reach of these phenomena as well as the relatively large magnitude of the seismic events produced.

One may question the existence of physical fluid channels at the depths considered. Their presence in the dehydration zone itself, however, is observed in exhumed rocks originating from this zone and is now well documented but direct observation on how these fluids migrate afterwards is lacking. Fig. 4 shows the presence of near continuous seismic paths connecting the foreshock zones to the locations of the largest intermediate depth events. The significance of these paths may at first be doubted on the ground that they are complex and multiple, but their convergence towards the foreshocks and epicenter locations is clear and supports the existence of a physical connection between the shallow and the deep activities. The significance of the snapshot image of Fig. 5 might be also doubted because its statistical significance is difficult to assess, but it shows two clear seismic paths between the shallow and deep activities during one of the most active month of the foreshock crisis. The propagation of pore-pressure waves or porosity waves along or near the plate interface may be an

alternative to the strong spatial localization of fluid flow of a channel model. Cruz-Atienza et al. (2018) have shown theoretically that tremor migration and speed can be explained by the propagation along the plate interface of non-linear pore-pressure waves under conditions that the interface is treated as a damage shear zone with strong permeability anisotropy. The seismic paths observed here could then be following the zones of highest permeability/highest shear deformation at or near the plate interface.

If one accepts that fluid/pressure circulation is the motor of the slab seismic activity observed during the foreshock crisis, one intriguing question is why in such a short time (a few months) overpressured pulses/fluids would ascent from different distant places spanning such a long segment of the subduction. One likely mechanism would be the existence of connections between the deep rock reservoirs where water from dehydration is thought to be stored, so that pressure changes in one would affect others. Another possible mechanism could be a rapid deformation or slip of the slab, too small or too deep to be detected geodetically, but of broad spatial extent, which could disturb the slab interface or the fluid reservoirs present at depth."

The figures should be improved:

• All the maps are low resolution with an ugly and saturated colormap for topography. Printed in black and white, these maps are difficult to decipher.

We have tried to comply with this comment by redrawing four of the five figures. In the maps of Figs. 4 and 5 we no longer use color for the topography and have replaced it by white and grey shade. We have left some color in the map of Fig. 1 which we think is useful to mark the trench location, the shallow subduction under sea and the deep subduction under land. The important information carried by this map (the foreshock locations) is itself in black and white.

- • When not absolutely necessary, colours should not be used (e.g., Figs 2 and 3). Use shades of grey instead.
- • In Figs 2 and 3, the width of the bars and the lack of transparency hide many of the finer details.

As asked by the reviewer, we have reduced the width of the bars in Figs. 2 and 3, so the individual events can be better seen and do not mask each other. In these two figures we have changed the red and blue colors to a light pink color and to a dark blue color respectively which should separate better when printing in black and white and be more color-blind friendly.

• The colours used in Fig 4 are not colour-blind friendly, especially given the background colour map (which could be changed to a grey-shaded relief map). Perhaps use different symbols instead, and link each burst with connecting lines? The sense of time between these bursts is also lost in this plot – they are grouped by clusters, but it's not clear which way the connection is going (from deep to shallow or the other way around).

To comply with this comment we no longer use background color for the topography, but white and grey. As also suggested we now use different symbols to identify the various burst periods. We have refrained from "physically" linking each burst with connecting lines because we think that this would complicate the figures and be necessarily subjective. We think it is up to the reader to decide if these links are present or not for him.

As we explain in the text "multiple deep and shallow events are often interweaved together within a burst. This complexity prevents the reading of a simplistic chronology between deep and shallow events." For this reason and because an updip propagating overpressure is necessarily accompanied by a downdip propagating depression there is no expected sense of time between deep and shallow events (This is also true in tremors which can reverse migrating direction).

• Fig 5 is interesting but doesn't highlight the "streaks" (or tracks, as referred in the paper) as well as it could. The streaks should be more visible on a "difference" plot, where the difference is between two density distributions (e.g., KDE). I will note that, in addition to the streaks, there is a clear decrease in activity for slab depths of 60-80 km between May and August – is this robust, and how can you explain it? Furthermore, adding earthquake depth information in the figure should be insightful here; it's unclear whether the streak represents all crustal earthquakes in the upper plate or follow the slab dip. This is also not discussed in the text.

We think that to achieve the type of treatment (KDE) suggested above one would need to perform some statistical analysis of the seismicity pattern over different time windows. This is not an easy task and we do not see how to perform it with the limited dataset we have. The decrease of activity for slab depths of 60-80 km between May and August is interesting but it is difficult to estimate if it is robust so we do not mention it. The

seismicity streaks of Fig. 5 do not contain crustal earthquakes. Sorry for not specifying it before. We have now added this information in the figure legend: "Only the events occurring near the slab interface and below are shown"

• I feel like there should be a summary or conceptual figure that summarizes the model and the observations.

We have built the new discussion section as a presentation of what the observations tell us and seem to imply about the model. We feel it is too early to draw a conceptual figure. The various characteristics of the observations (rapidity of shallow/deep interactions, presence of seismicity paths, along-dip interactions, lateral jumps of the activities, large width of the subduction segment involved) give us some insight into some of the characteristics of the model, but we do not feel that we know enough to draw a conceptual model at this stage.

--- Reviewer C:

Review

This paper presents earthquake data preceding the Iquique Megathrust Earthquake in Chile in 2014. The authors show that up-dip and down-dip of the earthquake in several time clusters there are moderate sized earthquakes. They also discuss some smaller seismicity migrating between the up-dip shallow and downdip deep reactions. Given the tight observed temporal clustering of these events, and the spatial correlation (directly down dip from shallow and deep events clustered in time), the workers invoke a fluid-connection between these preceding events clustered in time and partly in space. While these observations are very interesting, I find the mechanism invoked for this connection vague and not grounded enough in the actual geologic processes occurring within the subducting slab, nor are they consistent with our current understanding of the properties of these materials (permeability, porosity etc).

We have tried to clarify these issues in the revised manuscript and for this we have modified and expanded the discussion. We focus the new discussion on what the different characteristics of the observations (the shallow/deep synchronicity, the along-dip pattern of these interactions, the jumps of activities along subduction strike, the presence of near- continuous seismic paths between foreshock locations and intermediate-depth activity, the broad width of the subduction segment where these seismic activities take place) tell us about the mechanisms involved. Among these characteristics, the synchronicity of the foreshocks with activity below is the most surprising. It is also the one which is best established because it can be shown statistically that it cannot be due to chance (its chance probability, regardless of the catalog used, is now calculated and is less than 1 in 100,000). This observation is new because of the long range over which this synchronicity occurs and because of the magnitude of the events concerned, but it is otherwise similar to what is observed in tremors. And like for tremors, some type of fluid connections seems necessary to explain this observation. The data we present do not provide direct information on the nature of these fluid connections. However they show the presence of seismic paths between the foreshock locations and the deep activity which suggest the presence of preferred paths for fluid circulation.

While I am not an earthquake seismologist, and I cannot comment on the catalog and time- space patterns shown here in detail, I am sure another reviewer has the expertise to review this portion of the contribution. My expertise is more in the petrologic, geologic, and physical properties/processes the authors invoke to explain the correlation in time between these up-dip and down-dip events.

Below I detail some of the challenges I find with the mechanism discussed and suggest a path froward for the authors: Can you be clear about if you are invoking actual up-dip movement of fluids from ~90 km depths all the way up to ~20-30 km depths where the shallow events occur? This would require fast timescales (the workers mention migration rate could be similar to that of tremor), but simple calculations for the flux rate of fluids through these very low permeability rocks (how low is hard to say, but quite low, even if these fluids can move through migrating fracture-vein networks), which would preclude these very fast timescales needed for fluid transfer.

To this end, the geologic studies on fluid production at depth in the subduction system that are cited here are all strong contributions, but really only focus on the time-scales of dehydration reactions (the production not movement of these fluids). While it still remains challenging to constrain these reaction timescales, the work you cite does suggest these dehydration events may be quite fast, perhaps even on the timescales needed for these fluids to cause the deepest events (e.g., Plümper et al., 2017). However, these studies are not able to robustly comment on the fluid migration *rates* up-dip along the plate interface. The Taetz et al., 2018 discusses this as an option, but again does not have a robust handling of the permeabilities or porosities needed to move these fluids. Thus, I was left wondering, do you mean pore-pressure waves caused by increased fluid rather than up-dip fluid movement (Cruz-

Atienza et al., 2018) rather than actual literal fluxes of fluids from these deep depths so far? I suggest making this much clearer in the discussion. The the differences between these two end member models is quite large from a process perspective. A simple set of calculations for the porosity and permeabilities needed to move fluids up-dip that far that quickly would likely suggest this is not possible.

We have extended the discussion originally focused on the existence of fluid channels to other possible fluid connections. We thank you in this respect for bringing to our attention the study of Cruz-Atienza et al. In both cases (transient channels or pore-pressure waves) the propagation of pressure pulses or pressure gradients may provide the necessary mechanism for the observations. What type and magnitude of fluid flow or mass transfer is associated with them is not possible to determine from the observations. The involvement of the deep slab shows that pressure pulses or pressure gradients must be travelling between ~90 km depths and ~20-30km depths and it seems logical that some fluids are ascending during these periods but how far and how much is not resolvable by the present observations.

Below is the text we have added in the discussion section of the revised manuscript to try to clarify these issues: "The present observations suggest a mechanism involving the circulation of pressure pulses in fluid-filled channels. The burst-like nature of the seismic activity would indicate that these channels are very transient in time, opening during overpressure passage and closing as soon as fluid pressure locally in the channel drops below local confining pressure. This characteristic of the seismicity supports that pressure propagation and fluid flow, like the opening of the channel, are very intermittent. The along-dip organization of the bursts denotes an along-dip orientation of the channels, which probably reflects the strong down-slip corrugation of the Nazca slab (Soto et al., 2019). Such corrugations have been recently proposed to act as fluid conducts (Edwards et al., 2018). The occurrence of the events in packets of short duration including both shallow and deep events, often interweaved together, suggests that they are associated with the updip and downdip propagation of pressure pulses. While surges of overpressured fluids in the seismogenic zone are probably producing the foreshocks, they are accompanied by decompression pulses propagating downdip.

Another clear characteristic of the seismic activity is its long remarkable extension along the strike of the subduction (Figs. 1 and 2). This long extension of the activity does not evolve in a continuous fashion but occurs in jumps. For instance, after ~4 months of quiescence, the second crisis begins suddenly in early January ~150km away from where the first crisis had started and 50km beyond the zone where foreshocks had previously occurred. The activity was strong there for a few days, then completely disappeared and by the end of January, foreshock activity had jumped back to a zone close to where it initiated.

The major characteristics that are observed, the rapidity of the up-dip/down-dip interactions, the jumps of the activities along subduction strike, the broad width of the subduction zone involved are not characteristics unseen before. These same characteristics have long been reported for tremors. What is novel here and before the Tohoku earthquake are the very long range and the depth reach of these phenomena as well as the relatively large magnitude of the seismic events produced.

One may question the existence of physical fluid channels at the depths considered. Their presence in the dehydration zone itself, however, is observed in exhumed rocks originating from this zone and is now well documented but direct observation on how these fluids migrate afterwards is lacking. Fig. 4 shows the presence of near continuous seismic paths connecting the foreshock zones to the locations of the largest intermediate depth events. The significance of these paths may at first be doubted on the ground that they are complex and multiple, but their convergence towards the foreshocks and epicenter locations is clear and supports the existence of a physical connection between the shallow and the deep activities. The significance of the snapshot image of Fig. 5 might be also doubted because its statistical significance is difficult to assess, but it shows two clear seismic paths between the shallow and deep activities during one of the most active month of the foreshock crisis. The propagation of pore-pressure waves or porosity waves along or near the plate interface might be an alternative to the strong spatial localization of fluid flow of a channel model. Cruz-Atienza et al. (2018) have shown theoretically that tremor migration and speed can be explained by the propagation along the plate interface of non-linear pore-pressure waves under conditions that the interface is treated as a damage shear zone with strong permeability anisotropy. The seismic paths observed here could then be following the zones of highest permeability/highest shear deformation at or near the plate interface.

If one accepts that fluid/pressure circulation is the motor of the slab seismic activity observed during the foreshock crisis, one intriguing question is why in such a short time (a few months) overpressured pulses/fluids would ascent from different distant places spanning such a long segment of the subduction. One likely mechanism would be the existence of connections between the deep rock reservoirs where water from dehydration is thought to be stored, so that pressure changes in one would affect others. Another possible mechanism could be a rapid deformation or slip of the slab, too small or too deep to be detected geodetically, but of broad spatial extent, which could disturb the slab interface or the fluid reservoirs present at depth."

Additional constraints: It is also apparent from the isotopic record of exhumed terranes that fluids from below the subduction seismogenic zone (as deep as the antigorite-out reactions you invoke in the hydrated portions of the subducting lithospheric mantle do not appear to be making it up to such shallow depths (e.g., 20 km depth). There has been some up-dip movement observed from these studies, but the timescales of those fluid movements are likely quite long (Jaeckel et al., 2018; Epstein et al., 2020).

Thank you for mentioning these studies. As we say in our answers above, the presence of fluid connections seems imposed by the synchronicity which is observed. The circulation of fluid pressure pulses or pore-pressure waves in these connections is difficult to imagine without some fluid ascent. How large is the fluid flow and how rapid it is however cannot be assessed by the present observations. For the first eight months of the foreshock crisis the largest events have magnitude 4 or 5, that is a rupture size of the order of one kilometer square or less. What volume of overpressured water is needed to unstick or open enough this area of a fault or the plate interface to let it slip ? Maybe not very much if its surface is smooth.

Minor Comments:

Fig. 4 colors for temporal labels are challenging to read or see. I suggest reworking these colors (black text or different colored backgrounds) to be able to property see the time- space patterns in question.

Thank you and sorry for the difficulty to read the labels. We have reworked this using black text as suggested.

Reviewer 1, Second Review:

The paper has been significantly improved following the first round of review, and it is now in good shape. I have a few more minor comments that the authors should address.

- The statistical tests are a welcome addition. These strengthen the discussion and increase the impact of the paper.
- The figures have mostly all been improved, although Figure 1 still needs some work. The resolution is still very low (pixels are visible), and I recommend using a different colour palette. The authors appear to use GMT for the maps with the "relief" colour palette. Simply switching to something like "terra" (and maybe playing with the range of values) would improve the look of the figure. Using a higher-resolution topography data set is also easily done; see, e.g., https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019EA000658. Finally, the figure should include an inset to situate the region geographically and perhaps add some political boundaries and tectonic features like plate boundaries.
- The discussion has been expanded and includes some alternative models. However, it lacks references to support their arguments (only 3 references are cited in ~40 lines of new text).
- When rebutting my comment about connecting lines between deep and shallow events, I disagree with the following remark: "**We think it is up to the reader to decide if these links are present or not for him.**" I believe it is the job of the authors to convince the reader about their argument, which is oftentimes done using simple visuals/graphics. I agree that connecting the lines may make the figure too busy; I still believe the authors should think of a way to render these connections visually.
- Figure 5 (and the new sentence in the caption) still begs the question of their depth (i.e., along-dip) connection. The remark: "**Only the events occurring near the slab interface and below are shown**" is certainly helpful, but it would have been nice to see a streak-orthogonal profile showing the earthquake depths (maybe as subplots), perhaps colour-coded by their temporal occurrence. I realize that hypocentral depths may be uncertain, but such a plot would help convince the reader of this connection.

EDITOR'S DECISION MESSAGE:

From: "Christie Rowe" <christie.rowe@mcgill.ca>

To: "Michel Bouchon" <Michel.Bouchon@univ-grenoble-alpes.fr>, "Stephane Guillot" <Stephane.Guillot@univ-grenoble-alpes.fr>, "David Marsan" <david.marsan@univ-smb.fr>, "Anne Socquet" <anne.socquet@univ-grenoble-alpes.fr>, "Jorge Jara" <jorge@gfz-potsdam.de>, "Francois Renard" <francois.renard@mn.uio.no>

Subject: [Seismica] Editor Decision

Dear Michel Bouchon, Stephane Guillot, David Marsan, Anne Socquet, Jorge Jara, Francois Renard:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Observation of a Synchronicity between Shallow and Deep Seismic Activities during the Foreshock Crisis Preceding the Iquique Megathrust Earthquake". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some minor revisions. I attach some review comments from one of the reviewers who commented on your initial submission. I also attach an annotated pdf with my comments on your edited manuscript.

I found your improved discussion and figures compelling and look forward to helping polish this manuscript for publication in Seismica. I included some minor grammatical suggestions where I thought a change in word choice or sentence structure might make your arguments more clear to readers. I also note that the changes in citations in the revisions were not propagated to the Reference list. Please update this with your revised copy.

When you are ready to resubmit the revised version of your manuscript, please put the text into one of Seismica's templates (docx, odt or tex are available here: https://seismica.library.mcgill.ca/templates/). Please stay compliant with the template, to save time of our scientist-volunteers on the Standards & CopyEd team. If you have any trouble with the templates please message me and I would be happy to answer questions or help. For this revision you do not need to show tracked-changes, please just upload the final manuscript and I will confirm the changes prior to acceptance. If you wish to submit a response note you are welcome to do so. If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Christie Rowe

AUTHORS’ RESPONSE AND STATEMENT OF REVISIONS:

Dear Christie,
We thank you very much for the time you spent on editing so carefully our paper. The quality and pertinence of your comments are greatly appreciated and have been very helpful in clarifying issues raised by the paper and in improving the text, the presentation, and the discussion. We are both thankful and impressed by your editing work.
We have tried to follow your recommendations and the ones of the reviewer as best as we could. We have redone Fig. 1 according to the reviewer’s comments and we have tried to expand the discussion to alternate possible mechanisms which could produce the synchronicity observed. What we are unable to do is proving the existence and geometry of the water paths which are necessarily multiple and complex. Even in “controlled” experiments conducted for exploration or geothermal purposes, these paths are always complex and challenging to infer. We nevertheless think that the seismicity images we provide support the existence and the general along-dip geometry of these paths.
With many thanks and kind regards,
Michel