Dear Editor & reviewers,

Please find below our point-by-point response to the reviews we received on our manuscript entitled *Do large earthquakes start with a precursory phase of slow slip?* submitted for possible publication in *Seismica*.

Reviewer B:

This paper is a follow-up to the work presented by the same authors in Science in 2023. In that work they presented evidence that, when stacking the dot-product of 5min GNSS positions with predicted vectors for a point source at the time and location of M7+ earthquakes, an accelerating positive signal exists in the 2 hours before the events. They concluded that this provides evidence for precursory aseismic slip. If true, that would be a very important discovery, and as a result their results were scrutinized in detail, particularly in the non peer-reviewed online blog "Earthquake Insights". This manuscript is an attempt to address some of the concerns, mostly that the observed signal is the result of a common-mode signal without a tectonic origin. The authors find that removing a common-mode signal would make the precursory signal disappear, but argue that that doesn't mean there couldn't be precursory signal, it is simply hard to separate from any other common-mode signal. They present several tests to make their case.

I think this is a strong paper in that it does present the kind of tests the community would like to see to asses whether the precursory signal is real. I have no particular comment about the rationale and specifics of the tests. And I like how the paper is trying to be very even-handed. My main concern is one that was not addressed in this or the original paper; how appropriate is it look for significant displacements in the direction of those predicted by slip of a point source at the location of the hypocenter? In essence you are not testing any other possible precursor. The authors may argue that when they did significantly vary for instance the location or rake of the point-source, the precursory signal went away, and they essentially use that observation to not question the validity of their point source model. More thoughts about this below.

In the calculation of the Green's function the authors assume slip on a 1x1 km fault patch centered on the hypocenter. In the Supplemental Material of the original Science paper they write "We deliberately choose unrealistically small fault surfaces (1 km by 1 km), effectively equivalent to point sources, in order to limit modeling errors due to the nodal plane ambiguity". First of all, nowhere in this or the previous paper is there any discussion on the chosen fault plane. Why is there mention of a nodal plane ambiguity here? These are all large events for which the correct nodal plane is known. Secondly, I think the 1 x 1 km slip patch is the potential source of much uncertainty. If I am not mistaken (and the authors should verify this) the expected displacement field at the Earth's surface looks very different when considering a point source or a realistic finite source model. Considering a tiny patch around the hypocenter has the built-in assumption that there was precursory slip at the location where the earthquake initiated, and fig 10 shows the sensitivity to this assumption when moving the location of the assumed precursory slip. Another approach could be to estimate the coseismic offset at each station and use that as the orientation of the expected precursory displacement. The benefit of that is that it doesn't require any modeling of the source (and the assumptions, including that of the nodal plane), but does assume that the precursory slip signal has the same footprint as the coseismic signal. That may not be correct either, but it offers a second set of predicted displacement vectors with which to do the analysis. As a result, the authors could at least address some of the uncertainty that comes with the made assumptions, if any.

Interestingly the authors did in section 5 what I propose above, and found that the precursory signal went away. They did this to test if the observed signal is contaminated by the coseismic signal. However, the question remains whether having predicted displacement orientations from a point source is better or more realistic than one based on the coseismic orientations.

The rationale for choosing a small fault is that models of earthquake nucleation usually involve a portion of the fault which is much smaller than the subsequent earthquake area. In our test, the reason the signal disappears is most likely because the recorded offsets are dominated by noise for many of the stations. We believe this test is still useful to discuss the possible co-seismic contamination as if the signal resulted from a problem of filtering leakage, even noise would leak, making the signal more apparent when replacing the Green's functions by co-seismic offsets consistently determined from the data set used in the pre-earthquake analysis (see added text in Lines 297-300). Because of the noise in the co-seismic offsets determined from the data, we believe that this test is not relevant to estimate the effect of extended sources rather than point sources.

For that purpose, we believe that recalculating the Green's function for extended sources (see added section 8.2.2 and added Figure 18) is preferable. To follow reviewer #1 suggestion, we added Figure 18, which shows the stack calculated considering extended sources of different sizes. Before doing this, we selected the correct focal planes for every 90 events using published results when available or the most likely according to the known regional tectonics. Ambiguity remains for a few (poorly-instrumented) earthquakes, but they do not influence the resulting stack. Compared to the point source originally used in Bletery and Nocquet (2023), we found very small variations in the obtained stacks. Nevertheless, using the metric (r) used all along the study, the signal appears to be slightly more significant when considering larger sources: $r = 2.02$ for sources corresponding to the entire rupture areas that we estimated using the scaling laws proposed by Wells and Coppersmith (1994), compared to $r = 1.82$ for point sources. We believe this change is too small to conclude that potential pre-slip occurs on large fault portions – possibly corresponding to the entire co-seismic slip areas. Nonetheless, the observation is definitely interesting as the signal appears to be robust to the small changes of the pre-slip pattern.

We added a section (section 8.2.2) discussing those findings.

Minor comments:

Line 89 – What is the "the exponential-like signal in the stack"? This has not been noted before in the text. This wording is also used later on (e.g., line 130 and title of section 3.2), at which point I think the authors use it to describe the earlier noted "growing consistency between the recorded and the expected displacements (line 60)" towards the end of the time-series of "the stack". This "exponential" terminology seems to be a left over from the original Science paper. Why do you use the term "exponential"? The time-series simply shows a positive increase of sorts towards the end. I understand you want to impose a function, to mimic slip acceleration, when creating synthetic results, and in the original Science paper the authors talk about fitting a simple function, but I think that should be repeated here.

We removed the "exponential-like" formulation, except for the synthetic tests in which we did imposed an exponential signal (section 3) and for the description of our previous work on the Tohoku case, for which we did model the obtained stack by superimposed sinusoidal and exponential functions (section 6). In the rest of the manuscript, we reformulated "the exponential-like signal" by "the signal" as defined at the end of the introduction (Line 69) or by "positive increase" as suggested.

Line 92-93 – Remove commas before and after "in the far field"

Done.

Line 110 – with the exception of Gobron et al., the other papers mentioned here did not look into the noise characteristics but in the spatial correlation of the residual time-series, which partly reflects the spatial correlation in the noise. I think the other papers are worth mentioning as they present different analyses of common-mode errors, important to this paper, but this sentence should be rewritten.

We reformulated the first sentences of the paragraph to be more specific about previous studies of regional common mode and noise properties of GNSS time series (Lines 108-117).

Section 8.3. - Independent data are mentioned. It might be worth pointing out that while this paper is review another paper got published using tiltmeter data in Japan:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL109384
They found no anomaly 2 hours prior to the Tohoku earthquake.

Reference added (and discussed) in section 6.

Recommendation: Revisions Required

Reviewer G:

(Jeff Freymueller)

This paper is a bit unusual, but I think useful to publish. It is unusual in that it responds to some commentary on the Bletery and Nocquet (2023) paper that has been made in online forums. Chances are, those discussions will be more ephemeral than Seismica, so having this paper in the peer-reviewed literature is helpful. The authors do a fair job, I think, of analyzing the criticisms of their earlier paper and in examining which aspects are debatable and which appear to be robust.

I did have a discussion with the authors about this paper about a year ago, so some elements of their arguments are familiar. I don't recall whether I made any useful suggestions at that time…

Lines 274-284. I think the argument that the impact of the earthquake on the kinematic solution is negligible is a sound argument. The one thing I still wonder about, bit which is hard to test or get at, is whether there is some subtle bias to the orbits (or more likely the clocks) in the global solution that generated the orbit and clock products. My guess is that there is not, although I would not be entirely surprised if there are clock anomalies at some point AFTER the earthquake (while surface waves are passing through some stations used in that solution). The list of stations used in the JPL solution can be extracted from their orbit product files (stations used appear in the .wlpb file, while only the stations used to align the solution to ITRF are listed in the .x file). I note that on the day of the earthquake they did use the station DAEJ (Daejeon, Korea), which had a ~40 mm displacement, although JPL did NOT use this station as part of the alignment of their solution with the ITRF. No other station used had more than a ~1 mm level coseismic displacement. It is possible that by estimating a single coordinate for this station for the entire day, they might have induced some bias into other parameters. It is also possible that JPL cut out either the pre- post-earthquake observations from DAEJ, meaning no bias would be present. Whether or not this could explain the apparent pre-earthquake signal is not clear to me, although I think that potential impacts of coseismic displacements (both static and dynamic) are a topic that needs more investigation. Doing so would require replicating the global solution that generated the products, and then testing alternatives.

This possible issue has also been pointed out by P. Rebischung at (IGN/IPGP), as spurious offsets over an unrealistic wide area have been reported in JPL time series after an earthquake in California. As far as we know, preliminary efforts to recompute orbits/clocks accounting for change of position at ground stations during the day of a significant earthquake have been performed by A. Santamaria (GET) using the GINS software for the Tohoku earthquake, the earthquake for which this effect is expected to be the largest. No significant change in the determined orbit was detected (Santamaria, pers. communication, March 2024). As an additional check, we use the JPL wlpb files for the day of all earthquakes used in Bletery & Nocquet (2023) to evaluate coseismic offsets at GNSS sites used by JPL analysis. We find significant (>~1 cm) co-seismic offsets for 7 sites for 7 different earthquakes:

EQ	SITE	D(km)	DN	DE	DU	sDN	sDE	sDU
20060503_MW_8.0	TONG	132.7	24.1	55.7	-9.1	1.6	1.9	8.1
20090929_MW_8.1	ASPA	249.6	68.5	75.7	-18.2	2.8	1.7	7.3
20100227_MW_8.8	LPGS	1411.7	-0.1	-17.7	0.3	2.4	1.4	12.2
20110311_MW_9.0	CHAN	1545.0	-4.8	17.9	-0.1	1.2	1.0	3.7
20161208_MW_7.9	SOLO	278.0	1.5	-14.1	-0.1	3.1	1.8	10.8
20170717_MW_8.0	AC60	395.9	4.8	7.9	-1.3	1.2	2.0	4.3
20201019_MW_7.6	AB07	99.5	-0.0	-13.1	-2.4	1.7	2.3	2.6

It looks like DAEJ has been removed from the JPL analysis on day 2011/03/11 (perhaps removed after re-analysis?), but is present on 2011/03/09 for the Mw 7.3 foreshock. Aside from the case of Tohoku, none of the 6 earthquakes has a significant contribution to the stack.

This check makes it unlikely that the positive signal in the stack arises from orbits/clocks mismodelling induced by ground station coseismic offsets. Nonetheless, we agree that the impact of both static and dynamic motion of ground stations used to determine orbits/clocks need further investigations in future (see added text in Lines 305-307 and 309-310).

Minor points

Line 29. Change decennial to decadal

Done.

Line 122. Change “common modes” to “common mode errors”

Done.

Line 170. Change “than in” to “as in”

Done.

Line 252. Add “When” before “Perturbing”

Done.

Line 265. Add “to” before “every 5 minutes”

Done.

Line 268. “is completely negligible”. I am not so sure this is true. The effect is small, but I am not sure it is negligible (if Geoff Blewitt argued that it is negligible, then I would be satisfied with personal communication on that topic).

Geoff Blewitt wrote that pseudorange contribution, once phase ambiguities are resolved, is completely negligible. Geoff kindly checked our paragraph on GipsyX/NGL processing before submission of the paper.

Line 288. Add “this test” after behind

Done.

Line 324. They comment on an option (3), but only (1) and (2) are presented. What is (3)?

Option (3) is our original interpretation of precursory slip (which we comment throughout the paper). We reformulated to clarify option (3).

Line 379. Change than to as

Done.

Line 467. Change Freymuller to Freymueller

Apologies.
Done.

Recommendation: Revisions Required
