nature portfolio

Peer Review File

Symptomatic lithospheric drips triggering fast topographic rise and crustal deformation in the Central Andes

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

23rd Jun 21

Dear Ms Andersen,

Your manuscript titled "Lithospheric foundering driving crustal tectonics: Are all drips symptomatic?" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of potential interest. However, they have raised a number of substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these concerns.

In particular, it is important that you address the following points:

a) Provide a clear case for the use of symptomatic and asymptomatic terminology with respect to dripping tectonics and compare and contrast your work with the findings of relevant previous studies, such as Pysklywec and Cruden (2004);

b) Improve the methodological section, by including a rigorous scaling analysis and a description of the PIV technique;

c) Improve the presentation of the results, in particular by providing a more quantitative analysis and comparison with the natural examples;

d)Revise the overall structure of the manuscript according to the journal's specifications, making sure that the objectives are clear, that it follows a logical reasoning, and that the reader has all the essential information in the manuscript without needing to refer back to supplementary material. This can be done by including a Methods section at the end of the main body of the manuscript, linked with the text.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript.

However, please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Joao Duarte, PhD
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Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "Lithospheric foundering driving crustal tectonics: Are all drips symptomatic?" submitted to Communications Earth & Environment by A. Julia Andersen and co-authors.

In this study, the authors use analogue models to investigate how mantle lithosphere drip sinking drives sub-lithospheric mantle flow, crustal deformation and surface topography. Three models are performed to study different degrees of coupling between the mantle lithosphere and the crust. Two models have three layers (crust, mantle lithosphere and sub-lithospheric mantle). In one model the authors add a lower crust layer with a relatively high viscosity compared to the mantle lithosphere. The models show that when the coupling between the mantle lithosphere and the crust is high, the dripping process drives a surface depression followed by some uplift (reduction of the depression), as well as shortening with wrinkle-like features in the brittle layer. In contrast, when the coupling is low, the dripping occurs without significant expression at the surface.

1) Significance and novelty of the study:
I find that the investigated question is really interesting and is significant to the community of geoscientists focusing on tectonics and geodynamics in the broad sense. I find the title very exciting. I cannot, however, evaluate whether the scientific issue addressed by the authors is novel since I am not a specialist in drip tectonics.

2) Scientific quality/robustness and methods:
- The methodology section misses the description of details on the PIV technique used (e.g. type of passive tracers, resolution, correlation window size, etc.) and appropriate references that have used the technique in the Earth Sciences, notably to compute the mantle flow velocity field and the surface deformation and topography (e.g. Adam et al., 2005; Funiciello et al., 2006; Boutelier and Cruden, 2013; Strak and Schellart, 2014, 2018; Chen et al., 2016).
- There is virtually no description of the scaling procedure, which makes it impossible to evaluate if the scaling used is robust. A scaling procedure should be included, with appropriate referencing. The authors should moreover explain how their modelled topography scales with natural topography, since it relates to the scaling approach used (scaling densities or density contrasts, see Schellart and Strak, 2016).
- The authors use velocity fields from which they interpret deformation patterns. However, it would have been clearer and more straightforward to use strain maps as they allow to more easily distinguish shortening/extension and to better visualise low vs high strains. I would thus recommend using surface strain maps instead of (or in addition to) the surface velocity fields.
- The deformation pattern at the surface is interpreted from visual inspection of the deformed surface and from the velocity field, but surface strain maps would help the authors to describe more precisely the strain evolution at the surface. More importantly, strain maps would allow the authors to perform a more quantitative description of their models, which is really missing at the moment. A quantitative analysis would allow the authors to strengthen their arguments and would provide more data to compare between models (e.g. it would be interesting to compare the maximum amounts of shortening and extension measured between models and to relate them to the parameters varied). A quantitative analysis would also allow providing a more accurate description of the evolution of surface deformation through time in relation to the different phases of drip sinking.
- I am puzzled by the way the mantle lithosphere drip decouples much more easily in EXP-2. In my view, it cannot be due only to the reduced strength of the crust since this parameter cannot induce a

thinner neck of the drip, nor can it control the flux of material in the drip. This needs to be explained. Wasn't the mantle lithosphere viscosity reduced in EXP-2? Table 1 indicates that it is the same as in EXP-1, but the results seem to indicate otherwise...

3) Writing:
- The paper is generally written concisely but it could be better formulated in a few places. I think it could thus benefit from some minor/moderate re-writing as clarifications are also needed in a few places (see line-by-line comments below).
- The description of the surface strain evolution could be developed/improved. The authors focus a lot on topography but the evolution of deformation and how it relates to the topography and the mantle flow should be described in more detail.
- The description of how the topography relates to the mantle flow (both its horizontal and vertical components) should also be developed.

4) Interpretation of model results:
- The comparison of EXP-2 with natural examples of cratonic lithospheres (e.g. L206-219) does not make sense since EXP-2 has a weaker crust and a mantle lithosphere drip that decouples more easily whereas a cratonic lithosphere should be stronger overall. This part needs re-interpretation.
- The comparison of EXP-2 with the Hangai Dome is not consistent since this model does not seem to produce a dome (none of the three models actually do).
- Fig. 4 does not summarise well the results obtained with the models. The models show that shortening features appear where a topographic depression forms, not where there is a positive topography as indicated in Fig. 4. So the consistency between model results and interpretations needs improvement. I think this issue may partky relate to the lack of clear results on the figures (e.g. Fig. 1-3) compared to what is written in the manuscript (e.g. L194-195). It is indeed, for example, very difficult to see the three stages of topographic evolution as proposed by the authors on these figures. So either these figures should be improved to better render the multi-phase topographic evolution (subsidence, uplift, slow uplift) or the interpretation should be improved. One point on which I would change the text is on the location of the uplift, which should be given clearly and differentiated from the location of the positive topography that forms around the depression, which seems to sit above mantle upwellings, which thus may be a type of dynamic topography. I think then that Fig. 4 misses the inclusion of an essential mechanism of surface topography, which is due to the vertical component of mantle flow.

5) Line-by-line comments and suggestions:
L32-33: "scaled physical analogue laboratory experiments". "scaled analogue models" would be sufficient to describe the type of experiments since by definition analogue models are physical and in a laboratory.
L33: Replace "the" with "a" since the high resolution of the PIV technique used here is probably not universal.
L36-37: It may be clearer to write "and subsequently reverses to regional uplift".
L38: It should be "fold-and-thrust belts"
L39-40: "topographic inversions from ~1 km of tectonic subsidence to surface uplift (recovery) and regional tectonic deformation during the last ~20 Myrs" is unclear. I suggest reformulating. What is meant by "topographic inversions" is unclear. Why "recovery"?

L43: The Puna plateau is rather close to a plate boundary so I suggest changing "beneath continental interiors and cratons" to something more inclusive.
L45: "Active mantle dynamics" is too vague and may mean other processes such as mantle convection etc. The terminology used here should be more specific.
L44-46: This is interesting because it means that the surface might not express that something is actually moving below it. Maybe such a statement could be included to open the abstract.
L65: I suggest writing "new 3D laboratory experiments" or "new 3D analogue models" instead of "a new class of 3D laboratory analogue experiments".
L65: In what sense are the models new compared to refs 21-29? This should be clearly explained in the methods.
L66: Which part of the "descending lithosphere" is actually sinking?
L67: It may be better to write "in space and time" instead of "in temporal and spatial domains".
L77: "The drip tectonics type lithospheric removal model" is poorly written.
L87: "several". Do you mean different types?
L87: What is meant by "others"?
L88-89: "(viz., km-scale of topographic variations)—instances where the crustal tectonic manifestation of lithospheric drip foundering is muted or absent" is poorly written. Please reformulate.
L90: Which "differences"?
L91: I suggest replacing "active mantle (lithosphere)" with "sinking mantle lithosphere".
L96: Write "lithospheric MANTLE drips".
L97: "extension/shortening" are not "associated localized tectonic features" so I suggest reformulating.
L93-102: In this paragraph, it would help clarify the manuscript to write how many experiments were conducted in total and what main parameters were varied.
L106: Displacement vectors are shown but why not computing surface strain? It would be a more straightforward way to visualise shortening/extension.
L106: It would be useful to indicate what "0-40 hours" represent in nature.
L119: Here also, it would be useful to indicate what "1.5 mm of subsidence" represent in nature.
L119-120: "where the topography gradually increases around the ring" is not well formulated.
L120: Why "such"? I suggest replacing with "The".
L123: Delete "The".
L125: Delete "removed".
L130: "EXP-2 explores how the coupling of instabilities to the mantle lithosphere may control...". This is very unclear. What parameter exactly has been changed compared to EXP-1?
L135: "to activate the gravitational instability" is not well formulated since there is a gravitational instability. It is just smaller in volume.
L138: "are highest magnitude lower" is not well formulated.
L141: "developed". Here past tense is used to describe the experiments but it is not consistent with the use of present tense before.
L143-144: "an unstable lithosphere is strongly coupled to the crust". This does not make sense since the crust is part of the lithosphere. Which part of the lithosphere is coupled to the crust?
L147: The neck thins.
L159: The density of the lower crust should also be given.
L199: Replace "muted" with "less prominent".
L486: Why choosing such a high recording frequency while the models last 40 hours?

Figures:
- Fig. 1-3: "40h PIV Time frame of EXP-1" etc. are not good titles. They should be more explicit. The vectors for the mantle flow velocity fields are difficult to visualise. This should be improved. A colour scale is missing for those velocity fields.
- Fig. 4 is not consistent with EXP-1 where a depression forms and shortening occurs in that depression. So this conceptual model is not valid to explain the model results.

References:

Adam, J., Urai, J.L., Wieneke, B., Oncken, O., Pfeiffer, K., Kukowski, N., Lohrmann, J., Hoth, S., van der Zee, W., Schmatz, J., 2005. Shear localisation and strain distribution during tectonic faulting—new insights from granular-flow experiments and high-resolution optical image correlation techniques. J. Struct. Geol.27, 283–301.

Boutelier, D., Cruden, A., 2013. Slab rollback rate and trench curvature controlled by arc deformation. Geology41 (8), 911.

Chen, Z., Schellart, W.P., Strak, V., Duarte, J.C., 2016. Does subduction-induced mantle flow drive backarc extension?. Earth and Planetary Science Letters, 441, 200-210.

Funiciello, F., Moroni, M., Piromallo, C., Faccenna, C., Cenedese, A., Bui, H.A., 2006. Mapping mantle flow during retreating subduction: laboratory models analyzed by feature tracking. J. Geophys. Res.111 (B3), 1–16.

Schellart, W.P., Strak, V., 2016. A review of analogue modelling of geodynamic processes: Approaches, scaling, materials and quantification, with an application to subduction experiments. Journal of Geodynamics, 100, 7-32.

Strak, V., Schellart, W.P., 2014. Evolution of 3-D subduction-induced mantle flow around lateral slab edges in analogue models of free subduction analysed by stereoscopic particle image velocimetry technique. Earth and Planetary Science Letters, 403, 368-379.

Strak, V., Schellart, W.P., 2018. A subduction and mantle plume origin for Samoan volcanism. Scientific reports, 8(1), 1-12.

Kind regards,

Vincent Strak

Reviewer #2 (Remarks to the Author):

In this paper, Anderson et al. report the results of three analogue laboratory experiments that model the response of the crust to a negative gravity instability (RT instability) between a denser mantle lithosphere overlying a less dense mantle asthenosphere. The instability is initiated in the centre of a rectangular tank and the evolution of the resulting mantle "drip" and associated surface deformation is documented. The results are compared to a variety of settings on Earth where similar processes may have driven surface depression and uplift and in some cases upper crustal deformation.

This is an important topic in geodynamics and I think this paper has the potential to make a strong contribution to the field. While it currently provides a useful review of some of the geological and geophysical signatures of drip tectonics on Earth, there is a quite a substantial omission in the experimental work that forms the core basis of the paper. These experiments are characterised as "a new class of 3D laboratory experiments" that provide "new insights" into "modes of tectonic behaviour beneath continental interiors." Unfortunately this is not correct as the experiments presented are essentially replicates of work published by Pysklywec and Cruden (2004) with only minor modifications (e.g., slight changes to the tank size and modelling materials, use of digital photogrammetry and PIV to quantify surface deformation, etc.). However, it is reassuring to see how similar the results are between the 2004 study and this one, including features that the current paper attributes to "symptomatic" versus "asymptomatic" drips. Ideally, a revised version of this manuscript would fully acknowledge this previous work and compare and contrast the findings from both studies to arrive at a better or updated understanding of the geomechanical interactions between mantle drips and surface deformation. Such changes go well beyond the expectations of a typical major revision, meaning that I am recommending that this paper be rejected and possibly reconsidered after resubmission.

In addition to the appropriate attribution of previous work noted above, there are also a substantial number of additional improvements that are required prior to publication. These are detailed in the attached, extensively annotated PDF and summarised briefly below.

1. Symptomatic vs asymptomatic. I was not convinced by the introduction of these terms for drip behaviours. If I understand correctly, when the surface signals of an underlying drip are really obvious (e.g., strong subsidence followed by uplift, development of shortening structures, etc) then a drip is considered to be symptomatic. If the signals are subtle (e.g., more subdued subsidence and uplift, no contractional upper crustal deformation) then the drip is asymptomatic. However, I suspect that the signals of many (most?) drips will be quite subtle, like the Tulare Lake basin adjacent to the Sierra Nevada (Saleeby & Foster 2004) while strong near surface deformation such a faulting and thrusting accompanied by more dramatic subsidence and uplift will be rather exceptional (i.e., asymptomatic).

2. There are a substantial number of improvements and clarifications that are required for the presentation of the experimental work and results (see PDF). Some examples are given below:

(a) How the two types of initial density perturbation that initiated the drips in the centre of the tank need to be explained much more clearly, particularly Method 2. This is important because it appears to influence the drip dynamics.

(b) The behaviours and sizes of the drip in the 3 experiments are described only in qualitative terms. It would be better if the drip descent behaviours could be quantified and plotted against time, as done in Pysklywec & Cruden (2004) who showed that the drip dynamics are strongly modulated by the nature of the overlying crust. Some of the outcomes of Exp. 3 seem to arise from a non-axisymmetric drip geometry and flow, which needs to be fully described/quantified.

(c) The Supplement states that the tank walls were lubricated with petroleum jelly to reduce boundary effects on the ML. Duarte et al. (2014) showed that petroleum jelly (petrolatum) on its own is a rather ineffective lubricant for PDMS. It needs to be mixed with paraffin oil in the right proportion to work as a good lubricant. Were control experiments carried out to assess how effective this lubrication was and its outcome?

(d) PIV matters. PIV doesn't measure surface topography or the drip geometry - it measures surface velocities and fluid velocities in the tank. Surface topography is measured by DaVis using digital photogrammetry. The decent of the drip is recorded by the digital side images. The velocity vector data presented for the fluid in the tank looks quite noisy. I suspect this is because particle velocities are being measured from the whole tank, rather than in a vertical plane containing the drip structure. This could have been resolved by using a light sheet rather than illuminating the entire tank volume. This issue and how the mantle fluid was seeded with particles need to be discussed.

(e) Materials. PDMS was used for the asthenospheric mantle and Table 1 reports its density to be 1010 kg/m^3. One of the physical properties of all molecular weights of PDMS is a density of ~960 km/m^3 so I wonder if this is correct? The reasoning for using the chosen granular upper crustal material is hard to follow. I suspect the main reason it was chosen was to have a density that is scaled to the crust. Contrary to what is stated in the Supplement, most quartz sand using in analogue modelling has a grain size << 0.2 cm (2000 microns) - typically around 400 microns (0.04 cm). Also, why did Exp 2 use a higher density upper crust compared to the other two experiments? This slightly higher density will mean that the UC immediately above the LM will be stronger than in Exp. 1, which may partly explain the lack of surface deformation. It may also contribute to the different drip behaviours (see Pysklywec & Cruden, 2004).

(f) Exp. 1 and 2 have a brittle upper crust that scales to 8 km and no lower crust. Where on Earth or elsewhere in the solar system would there be a lithosphere that looks like this?

(g) Scaling. The scaling section in the Supplement is not very informative. How was it done? Where are the scaling arguments? How has geometric, kinematic and dynamic similarity been achieved/demonstrated? In regards to Fig. 5B, there must also be a time scaling factor (T = 1.8E-10) to equate the experimental times to times in nature and vice versa. This is not mentioned anywhere in the paper but it should be.

I appreciate that this is quite a critical review but I hope that the authors find it useful for preparing a stronger revised version of their manuscript.

A.R. Cruden
Melbourne

Reviewer #3 (Remarks to the Author):

This is an interesting paper which describes observations arising from models of dynamical instability in a fluid system representing the Earth’s continental crust and upper mantle. In general I think the results are worthy of publication, but I found the presentation of this paper rather frustrating. All of the essential information about how the experiments are set up is consigned to supplementary information, and I think there is not even a sentence in the main text that refers the reader to where this essential information can be found. Without this information I could get nothing useful from the paper, so my view is that experimental set-up should be part of the main text and I'd prefer a more conventional presentation in which the experimental set up is described before the results are interpreted. Statements attributing experimental behaviour to e.g. "the addition of a lower crust" (line 254) are uninterpretable without the setup information, but even having the supplementary information the reader is left to wonder if it is the thickness or the viscosity or the density of this additional layer in the experiments that causes the observed effect in the experimental system.

The extent to which these experiments actually represent the nominal lithospheric column is also not clearly explained. The essential data are in the table on the last page. These include a scaling factor for each parameter, without any explanation of how to interpret this scaling factor and what does it mean for the scaling of time that is implicit in Figure 5 and line 260. Other issues, like the non-Newtonian viscosity of mantle lithosphere that is widely accepted, are obscured by statements (lines 510-511) which imply that the experiments use a non-Newtonian viscosity, while the parameters in Table 1 show, no, viscosities are apparently Newtonian. Whatever the physical properties used in the experiments, the results are interesting, but to be useful, the experiments need to be documented accurately and clearly.

I think that some aspects of how the experimental results are presented can be improved: In Fig 1 the lower set of frames (side view) are very hard to read. Better to show fewer frames, or maybe remove the dark backgrounds from the velocity vectors. Do you want to show what these velocity fields look like ? because this presentation is not working very well. The grey scale inset diagram in the upper row of Fig 1 is interesting, but the reader is told only to interpret "wrinkle-like linear features" (lines 126, 172) ; how does this image relate to the surface elevation maps ? If these structures are modifying the surface, why do they not show in the topography map ? or are we looking at an integral view through the box ? Figure 5 is also difficult to interpret. Abbreviations on the Figure should be explained in the caption, so it is unclear to me which part of that 3D system is the focus for the authors and how do the tomographic sections relate to the processes described in the experiments.

It goes beyond the scope of the present paper I think but quantitative information from these experiments might usefully be compared with the quantitative analysis made by Hoogenboom and Houseman (Icarus 180 (2006) 292–307) for the axisymmetric structure and by Neil and Houseman (GJI, 1999) for the linear structure. I make this comment in case it is of use to the authors in further work.

I also list some minor (or not so minor) items that I think should be fixed:

1. Please be aware of what the word "instability" means; using it as a synonym of "drip" is unacceptable I think (e.g., lines 111, 113, 133, 167, 268, 472, 474)

2. line 84: "the enigmatic formation of ~ 100 km wide oval/circular 84 shaped basins scattered across the region". This is an important motivating statement for the study, but you cite reference 32, which I consulted, only to find a study of 2D numerical experiments, and no clear support for the statement, so is there a better reference which actually documents these basins in more detail ?

3. lines 101-102: “..., linear structural patterns, and deep seismic tomography images “ citations needed.

4. line 117: “approximately circular”

5. line 122: radius of the basin: how do you measure this ? Do you use a half-width at half-height type measure, otherwise it seems poorly defined.

6. line 135: “not well-coupled” what does this actually mean in the experimental context ?

7. line 226: “EXP-2 suggest that there may be surface uplift if a “: I don’t see the evidence for surface uplift in Fig 2, or do you just mean that the surface is restored to its pre-instability level ?

8. line 232: “lithospheric drips, detached as an outcome of plate shortening “ this idea could be better expressed.

9. line 251: “Stage 3 of model development is associated with additional surface uplift because the crust above the instability continues shortening, yielding...” I don’t see the surface uplift in your surface elevation maps, so what are you describing here ? Again maybe relates to wrinkles that were not adequately explained at an earlier stage.

Lithospheric foundering driving crustal tectonics: Are all drips symptomatic?

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Abstract

Outside the plate tectonic paradigm, lithospheric foundering events have been identified for driving rapid, ephemeral tectonic evolution of the crust. By using 3D scaled physical analogue laboratory experiments and the high-resolution Particle Image Velocimetry (PIV) imaging technique, we show that lithospheric drip instabilities may be "symptomatic" or "asymptomatic" in terms of their surface and geological manifestations, depending on the degree of crust-mantle coupling. With symptomatic foundering, a lithospheric drip forms a surface basin and subsequent inversion to regional uplift; in conjunction, lateral mantle entrainment induces linear wrinkle-like structures, characteristic of orogenic fold-thrust belts. These drip signatures match with topographic inversions from ~1 km of tectonic subsidence to surface uplift (recovery) and regional tectonic deformation during the last ~20 Myrs that have been interpreted in the Puna plateau of the Central Andes. Asymptomatic lithospheric instabilities yield topographic transients but lack any signal of crustal shortening and extension. This reveals new insights into special modes of tectonic behavior beneath continental interiors and cratons where seismic tomography and intermediate depth earthquakes suggest lithospheric foundering. Overall, the physical models show that active mantle dynamics can progress within a planetary interior, variably being symptom-free or driving a complex style of drip-orogenic tectonics.

Introduction

Rapid and short-lived phases of crustal shortening-extension, topographic perturbations, and exhumation processes are interpreted as the geological manifestations of lithospheric foundering in which gravitationally unstable (thickened) lithosphere sinks/drips into the underlying sub-lithospheric (asthenospheric) mantle[1–4]. In addition to conceptual tectonic models, seismic tomography provides robust examples of such drip-like high velocity anomalies beneath the Colorado Plateau[5,6], Great basin[7], Sierra Nevada region of California[8–10], Altiplano Plateau[11], Tibetan Plateau[12], Carpathians[13], and Anatolia[14].

A suite of geological, geochemical and geophysical studies suggest that “drip tectonics” has been associated with evolution of subduction, collision, volcanic, and arc magmatism processes at Alpine and Cordilleran orogenic belts and beneath continental interiors, such as the Mongolian plateau[15], Siberia[16], the North China[17–19] and Wyoming Cratons[20]. However, the surface tectonic response of the crust to this lithospheric downwelling/drip process is not well resolved. In this work, a new class of 3D laboratory analogue experiments are used to investigate the complex structural/deformational response of the brittle (upper crust) to the descending lithosphere in temporal and spatial domains.

Geodynamical models demonstrate that the tectonic and magmatic responses of lithospheric dripping/removal events are variable and mainly controlled by the size, rheological (viscosity) characteristics of the lithospheric instabilities, and buoyancy of the crust[21–29]. For example, numerical experiments by England and Houseman (1989) show that the loss of the lower part of the lithosphere beneath central Tibet and subsequent mantle upwelling can explain the ~ 2 km surface uplift and (E-W) extensional tectonics across the plateau simultaneous with India-Eurasia plate convergence. The extension/normal faulting is associated with higher elevations of the Tibetan plateau (mostly interior) whereas shortening is more concentrated on the plateau margins, at relatively lower elevations. As such, contrasting deformation types are controlled by variations in gravitational potential energy [30]. The drip tectonics type lithospheric removal model has been invoked to explain the ~ 1 km topographic uplift of the Central Anatolian Plateau that formed during the last 10 Myrs[31]. According to Gögüş et al. (2017), the gravitational/dripping instability beneath the Central Anatolian magmatic arc (interior) caused ascent of hot-buoyant asthenospheric mantle at the plateau margins where mantle derived volcanism, and anomalous surface uplift has been documented. Through a parametric numerical study on testing different lithospheric properties, Wang et al., (2015) show that small scale lithospheric instabilities under the Puna Plateau of the Central Andes can explain the enigmatic formation of ~ 100 km wide oval/circular shaped basins scattered across the region[32].

Overall, plateau-type (domal) uplift, initial subsidence, extension, and volcanism may be associated with several lithospheric removal events. However, others may not reveal prominent geological signatures (viz., km-scale of topographic variations)—instances where the crustal tectonic manifestation of lithospheric drip foundering is muted or absent. It may be that these differences occur during the evolving stages of lithospheric removal and 2-D plain strain numerical models so far are unable to properly explain the link between the active mantle (lithosphere) and crustal tectonics.

Experimental Results

Scaled 3-D laboratory (physical analogue) experiments are used to investigate the coupling dynamics between lithospheric drips and an overlying crust, and specifically, their topographic expressions (uplift/subsidence) and associated localized tectonic features (extension/shortening). Using a high-resolution imaging (PIV) technique, we investigate whether all lithospheric drips with different physical properties can yield deformation within the crust. Our reference experiment results are interpreted in the context of the geological evolution of the Arizaro basin in the Puna Plateau where rapid basin inversion (subsidence to uplift), linear structural patterns, and deep seismic tomography images suggest a drip-type lithospheric foundering event.

1.1 EXP-1 (Reference Experiment)

Figure 1A-B shows the plan view evolution of surface topography and the sideview (photograph) evolution of the experiment, including displacement vectors from 0-40 hours. The experiment begins when the initial density perturbation (~19 mm radius) in the centre of the model box develops as a Rayleigh-Taylor/gravitational instability. By 10 hours, a cylindrical shape dripping instability has developed and is actively downwelling (green vectors). In response to the downwelling, upward return flows (red vectors) have grown around the instability akin to a small-scale mantle circulation process (Figure 1B). By 20 hours, the instability undergoes necking (thinning) while its rate of descent has accelerated over the last 10 hours. Note that the downward pointing vectors have greater magnitude around the base of the drip where the instability volume and the viscous tractions are larger than the sides and the neck. This suggests rapid descent at the base (drip head) compared to the neck. After 30 h, the descending process has terminated, and the drip head hits the bottom of the tank.

The variation in surface elevation (measured by the PIV imaging) shows that a circular surface basin develops as a response to the pull of the dripping lithosphere on the overlying crust (Fig 1A). The core of the basin is characterized by up to -1.5 mm of subsidence where the topography gradually increases around the ring. Such downward (pull) force exerted by the drip on the overlying crust is maximum at the start and decreases as the experiment develops owing to progressive down-dip extension/necking. As the experiment progresses, the radius of the circular basin decreases ~75% with progressive shortening of the surface crust. The topographic uplift (> 1 mm) occurs around the periphery of the annular drip as lower density mantle material replaces removed higher density mantle lithosphere material. The crustal deformation driven by the dripping lithosphere is marked as wrinkle-like linear features that develop in the central section of the model with folding and shortening of the upper crust (see Figure 1a 40h frame).

1.2 EXP-2 (Coupling effects of instabilities)

EXP-2 explores how the coupling of instabilities to the mantle lithosphere may control the crust-mantle tectonics and the accompanying surface expressions (while all other model parameters remain the same as EXP-1). The motivation comes from various geological inferences in which changing volumes of the lithospheric instabilities in the Central Andes and Tibet control the formation of hinterland basins with diameter ~100 – 200 km[33–35]. The initial perturbation (~19 mm radius) was not well coupled to activate the gravitational instability compared to EXP-1 (Figure 2a, b), nevertheless, the overall dynamic evolution of the instability in EXP-2 is similar to EXP-1. For instance, by 20 h the drip has travelled approximately half the depth of the box and vectors (green coloured) showing downward displacements are highest magnitude lower in the box (Figure 2b). The drip reaches the bottom of the tank by 30 h but the downwelling process persists until the end of the experiment at 40 h.

Figure 2a shows that a circular depression initially developed in the central part of the box as a topographic expression above the incipient downwelling. Simultaneously, the horizontal displacement vectors at 0 h indicate inward convergence, when initially an unstable lithosphere is strongly coupled to the crust. This convergence stops by 10h and no appreciable surface structure is visible in the crust (Figure 2a 40h frame), indicating that the convergence by the weaker instability is too modest to induce crustal deformation (e.g. linear features) at any time. As the drip moves downward and thins, there is a rebound effect of positive topography (uplift) whereas the localized basin also narrows in the centre (Fig 2a) The PIV measurements show that a small depression remains in the centre of the experiment throughout. Notably, there is no tectonic deformation of the crust, a marked difference from the major fold-thrust structures that developed in EXP-1 in response to a similar type of drip.

For volumetric comparison, the neck (stretched portion) of the drip is approximately one third of the width in EXP-1. Accordingly, there is less lithospheric removal/foundering from beneath the crust, which may be interpreted as smaller scale (attenuated) lithospheric instability—which yields smaller basin formation and no surface crustal deformation.

1.3 EXP-3 (Addition of a Lower Crust)

In this experiment, we kept all model parameters the same as EXP-1 but included a viscous lower crust ($\mu = 2.12 \times 10^4$ Pa s) between the brittle upper crust and mantle lithosphere. The overall dynamic evolution of the instability in EXP-3 is similar to EXP-1, but now it evolves at a slower rate (Figure 3a, b). For instance, by 20 h the drip has only travelled approximately one quarter the depth of the box. At 30 h the drip starts to grow and manifests downwelling more clearly and uprising mantle circulation. The neck (stretched portion) of the drip is wider than that of EXP-1 and EXP-2, approximately two times the width of that in EXP-1. The experiment suggests that the viscous lower crust promotes a more substantial amount of lithospheric foundering as portions of crust are coupled with the lithospheric downwelling; namely, weak lower crust is entrained downward by the descending instability.

Figure 3a shows that the surface response to the voluminous lithospheric instability and its progressive removal is associated with the formation of a large well-defined oval-shape depression. Horizontal displacement vectors indicate inward convergence of surface crust throughout the experiment. The shortening yields significant structural heterogeneities such as wrinkle like/linear features are formed, whereas parts of the crust are stretched on the margins of the basin. This extensional zone is visible at 45 h where the displacement vectors show movement in opposite directions (Figure 3b 45h frame). This dripping lithosphere was well coupled to the crust as evidenced by its slow initial period of subsidence and the surplus of mantle lithosphere material pulled down by the drip.

Symptomatic vs. asymptomatic drips

In all the experiments, the lithospheric drips are associated with vertical and horizontal displacements to a certain degree (e.g., some subsidence and/or uplift), however, not all drips caused appreciable tectonics in the upper crust. This behaviour stems from differing initial conditions and allows for classification of a drip as either "symptomatic" or "asymptomatic" (Figure 4). A symptomatic drip is well coupled to the entire mantle lithosphere (and partially the crust): It sinks through the mantle and pulls the upper part of the mantle lithosphere inward and horizontally, producing shortening and the formation of convergent structures like fold-thrust ridges in the upper crust (Figures 1a, 3a, 4b). These drips tend to have wide necks in one horizontal direction (x) and thin in the other (y), and generally remove more material from beneath the lithosphere. EXP-1 and EXP-3 can be classified as symptomatic drips. There may be other factors controlling the degree of visible symptoms in the crust (such as, thermal and compositional), but we did not explore these in the laboratory experiments.

EXP-1 did not produce as many convergent ridge features in the crust as EXP-3, indicating the addition of a viscous lower crust may more effectively transfer deformation in the mantle lithosphere to the upper crust, thus displaying more geological/structural "symptoms" (Fig. 4B). The symptomatic drips control the evolution of surface topography in three stages with an initial stage of subsidence, subsequent stage of uplift, and a final stage of slower uplift/plateauing.

In contrast, an asymptomatic drip responds with initial subsidence, but is less well-coupled to the entire mantle lithosphere and so does not induce vigorous flow in the upper mantle lithosphere and crustal deformation is subdued (or absent) (Fig. 4A). EXP-2 is an asymptomatic drip where the subsidence stage is shorter and lithospheric removal is muted. This experiment demonstrates that not all drips in nature may deform the upper crust to a large degree and so the symptoms of a drip may manifest less clearly, unless the lithospheric instability in the mantle is observed itself through imaging techniques such as seismic tomography or intermediate depth earthquakes[36].

Geological analogy and setting

Putative locations of lithospheric foundering that could be candidates of asymptomatic type drips include cratons and continental interiors. Unlike orogenic belts, cratons are considered to be highly stable regions associated with thick and strong lithospheres. However, destruction of a craton can occur via lithospheric removal inferred mainly by magmatism (viz. North China Craton)[19]. The Wyoming Craton of the western-central United States is characterized by Archean lithosphere and was exposed to deformation during the Laramide orogeny and interacts with the Yellowstone hotspot[20,38–40]. There are detectable seismic velocity heterogeneities between the north and south of this craton[20] as well as the presence of a high velocity zone could indicate a sinking (possibly dripping) mantle lithospheric root[20,41]. Interpreting the results of EXP-2, in light of seismological and geological observations, we suggest that the dripping process might have developed beneath stable mantle lithosphere, for instance, beneath the North China and Wyoming cratons accompanied by subdued (asymptomatic) deformation in the upper crust[19]. Notably, such lithospheric drips might have been partially mechanically decoupled from the upper/stable part of the mantle lithosphere of the craton during the downwelling process.

The Hangai Dome in Mongolia is another possible example for drip-type lithospheric removal that occurred in the continental interior where the origin of uplift remains conjectural. The main dome is at ~2 km above sea level with observed uplift during the Cenozoic beginning ~25-39 Ma[42–45]. It is estimated from geophysical studies that the lithosphere beneath the Hangai Dome is not thicker than ~80 km [45–47] and the plume model can not satisfy several geological observations including melting/volcanism not originated from deep sources (e.g., lower mantle). Results from EXP-2 suggest that there may be surface uplift if a lithospheric drip is not well coupled to the upper most mantle lithosphere without shortening/thickening in the crust.

The findings from the symptomatic drip experiments (EXP-1 and EXP-3) agree well with the unique crust mantle structure and geological evolution of the last ~ 20 Myrs interpreted for the sedimentary basins of the Puna Plateau of the Central Andes[32,33] (Figure 5A, B). Inferred from a P-wave seismic tomography model[54], high velocity/cold anomalies at ~200 km depth may be interpreted as lithospheric drips, detached as an outcome of plate shortening during the Andean orogenic cycle[3].

The internally drained Arizaro (AZ) is a saucer shaped basin, with ~100 km of diameter and is aged between ~10.8 – 23.8 Ma[33,48,49]. In the subsurface (Fig. 5A), a high velocity (seismic) anomaly labeled A' could be an active lithospheric drip which may help drive the local basin subsidence. The mantle feature is slightly offset from the Arizaro basin, but mantle (corner) flow processes related to the subduction may have entrained and shifted the drip from beneath the basin. The crustal tectonics in the region are consistent with a symptomatic drip: In both (EXP-1 and EXP-3), saucer shaped basins and convergent/ridge-type structures were formed-similar to Sierra de Macon in the central part of the Arizaro Basin and the sedimentary deposits thickening inwards (Figures 1A and 3A).

The Arizaro Basin also has a topographic history reflective of a symptomatic drip. Figure 5b depicts the relative surface elevation of the models as a function of time directly above the instability (roughly the center of the experiment). Based on the negative (subsidence) and positive (uplift) topographic variations and their amplitude, the model evolution was divided into three stages. In EXP-1 and EXP-3, Stage 1 corresponds to ~1 mm (~ 3 km) of surface subsidence that develops as a response to the initial (strong) downwelling effect of the drip. In Stage 2, subsidence has ceased, and surface topography is recovered since there is a release of "drip-pull forces" related to the thinning/stretching of the drip. Stage 3 of model development is associated with additional surface uplift because the crust above the instability continues shortening, yielding surface contraction where linear wrinkle fold-thrust features develop in the crust (Figure 1A & 3A, 40h & 45h respectively). One of the key findings of EXP-3 is that the rate of surface uplift is greater with the addition of a lower crust and the rate of subsidence is slower, in contrast with the formation of the basin in the previous experiments. The Arizaro Basin similarly shows the three stages of topographic evolution (Figure 5B). DeCelles et al. (2015) estimate that the basin underwent an initial ~2 My period of filling while the root grew enough to initiate subsidence (with subsidence stopping at ~10 My). The basin then uplifted, and the root detached. The total length of time from the beginning of basin formation to present was ~18 My with a total uplift of ~1.2 km[33]. In comparing these subsidence/uplift curves (Fig. 5b), EXP-1 ran longer than the age of the Arizaro, but given that the curves are so similar temporally, it implies that the Arizaro Basin may undergo further uplift in the future. EXP-3 exhibits a longer period of subsidence with very rapid rate of uplift. The conditions that control the growth rate of the instability clearly influence the related history of basin subsidence and uplift.

The surface topography in EXP-2 (asymptomatic drip) behaves quite differently from EXP-1, EXP-3, and the Arizaro Basin. Stage 1 in EXP-2 shows gradually increasing surface elevation (0-10h) from initial depression (induced by the perturbation). The rate of uplift increases in stage 2 due to the accelerated descent of the instability (10-24 hours) and then ceases in stage 3 after the drip hits the bottom of the box at ~30 h. Notably, the instability/drip related subsidence was limited and rapid; only present during the very early phase of the experiment, before any downwelling was recorded at 0 h, unlike the basin development and inversion in EXP-1 and EXP-2. Further, the topography progressively elevates in each stage although there is no surface crustal deformation associated with this type of asymptomatic drip.

The findings demonstrate that mantle processes--like these foundering lithospheric drips--may be active in the planetary subsurface but may or may not show symptomatic tectonics at the surface. Basin evolution and tectonics in the Arizaro region, for example, seem to be symptoms of contemporary lithospheric drip removal. The tectonic manifestations of lithospheric instability in continental interiors or cratonic zones may be more elusive. Further, similar mantle dynamics can function on single-plate terrestrial bodies, but also show variable tectonic symptoms.

Author Contributions

JA, OG, and RP conceived the ideas and interpretations. JA and TS carried out analogue experiments and data analysis at the University of Toronto. The depiction of seismic subsurface data was generated by EŞU. The manuscript was written and prepared by JA, OG, and RP, with comments and input included from all authors.

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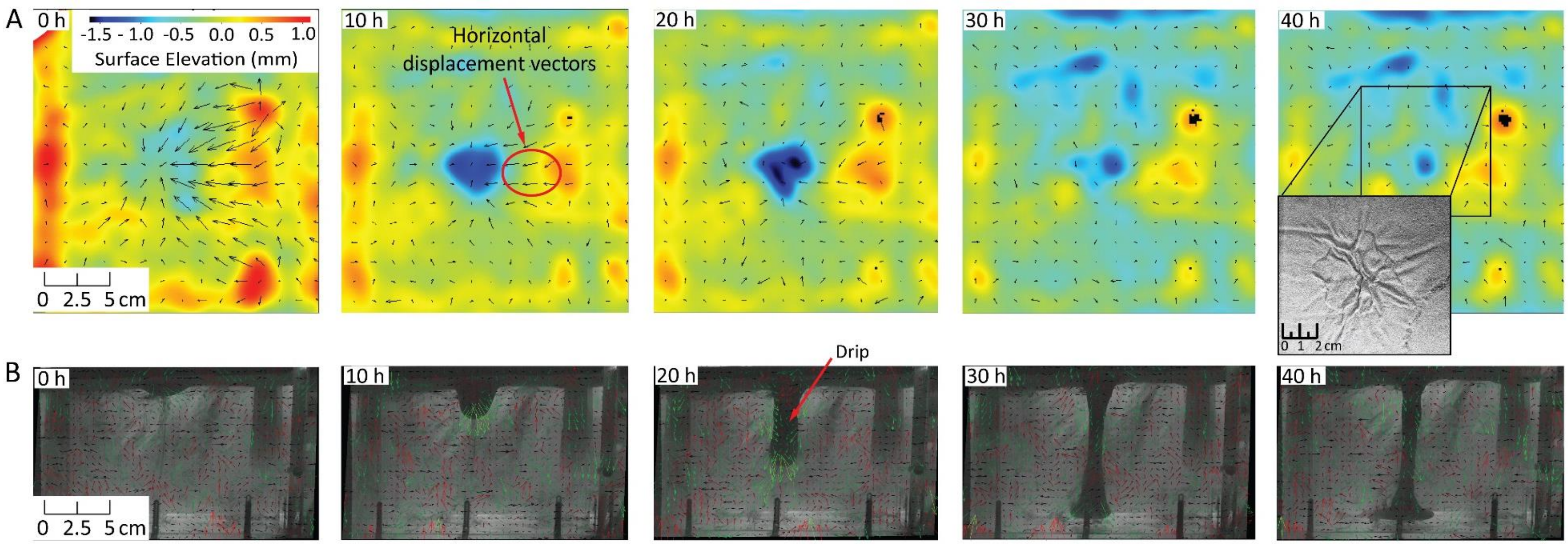

Figure 1| **40h PIV Time frame of EXP-1** a) Plan view evolution of topography over 40 h for EXP-1. Convergence toward the center occurs from 0 h to 20 h. Convergence stuctures are shown at 40h b) Below surface (sideview) evolution of the drip. Drip begins thinning at ~20 h and has touched the bottom by 30 h.

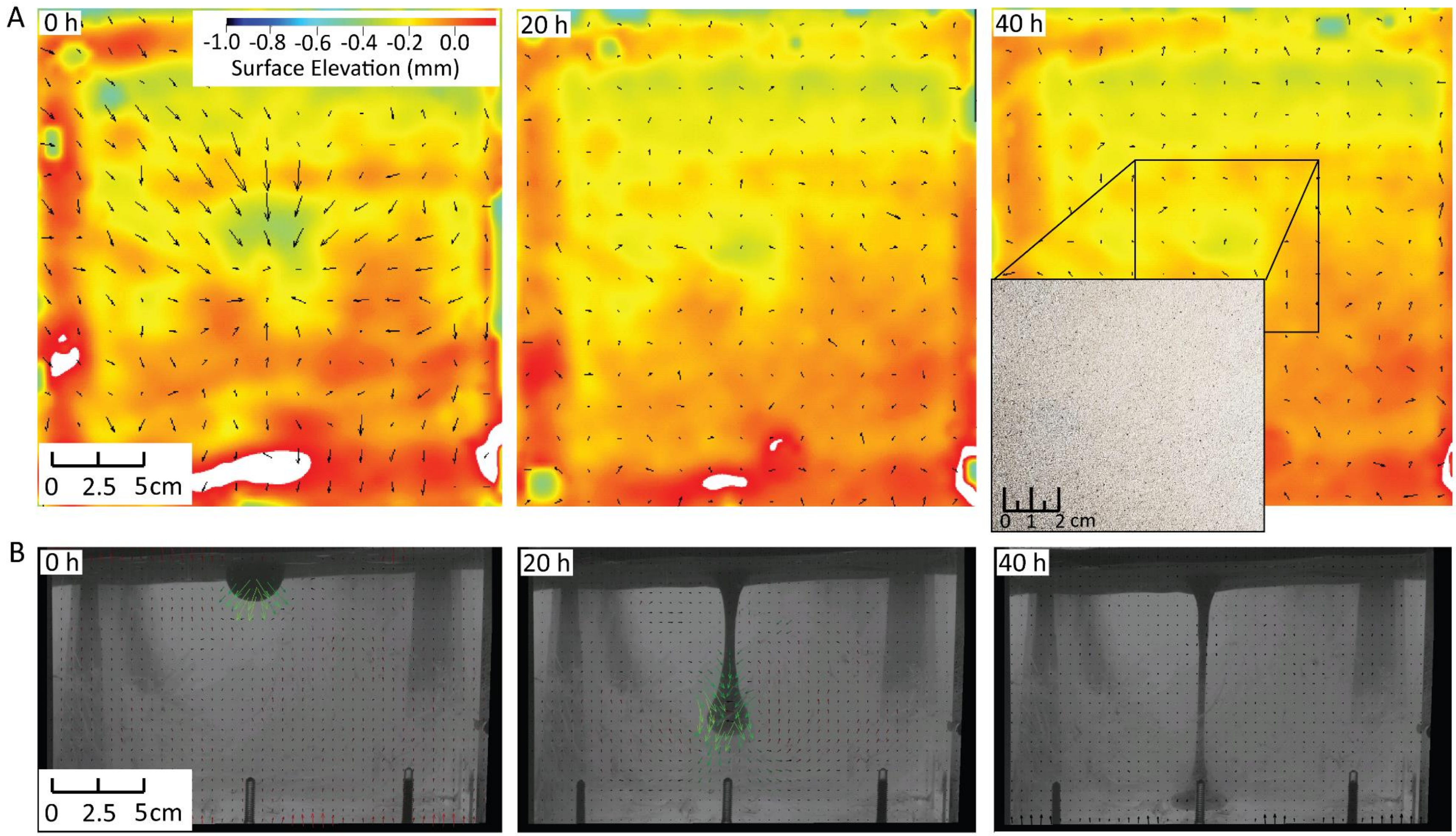

Figure 2 | **40h PIV Time Frame of EXP-2** a) Plan view evolution of topography over 40 h. Horizontal displacement vectors indicate some movement at 0 h. Photograph of the surface at 40h is included. b) Evolution of drip displacement (sideview) beneath the surface with displacement vectors. By 20 h the drip has travelled >1/2 the length of the box. It touches the bottom between 20 h and 40 h.

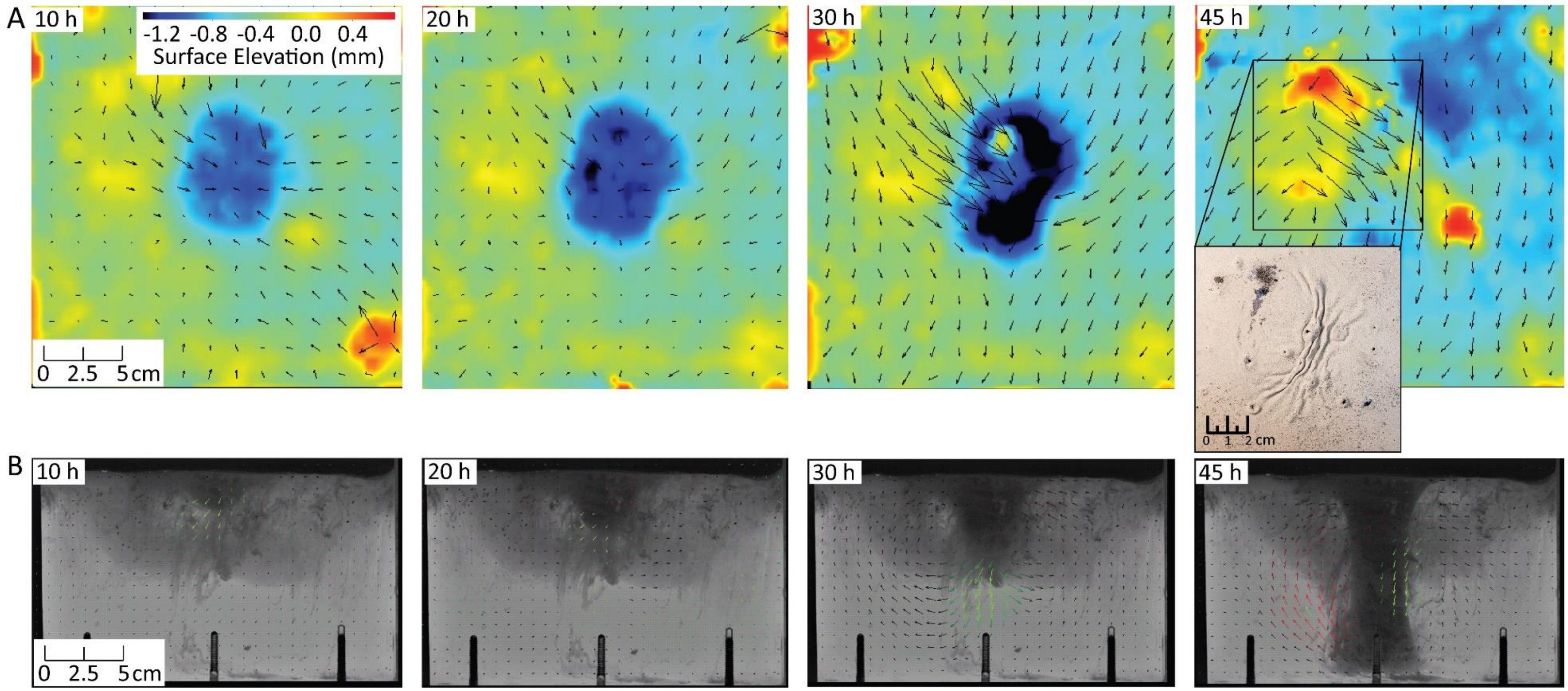

Figure 3 | **40h PIV Time Frame of EXP-3** a) Plan view evolution of topography over 40 h for EXP-3. Convergence toward the center occurs from 0 h to 30 h. Extension was detected around 45h (photograph shown). b) Below surface (sideview) evolution of the drip. Drip begins thinning at ~30 h and has touched the bottom by 45 h. The cloud surrounding the drip is visible discoloration of the (polydimethylsiloxane) PDMS from the clay in the mantle lithosphere.

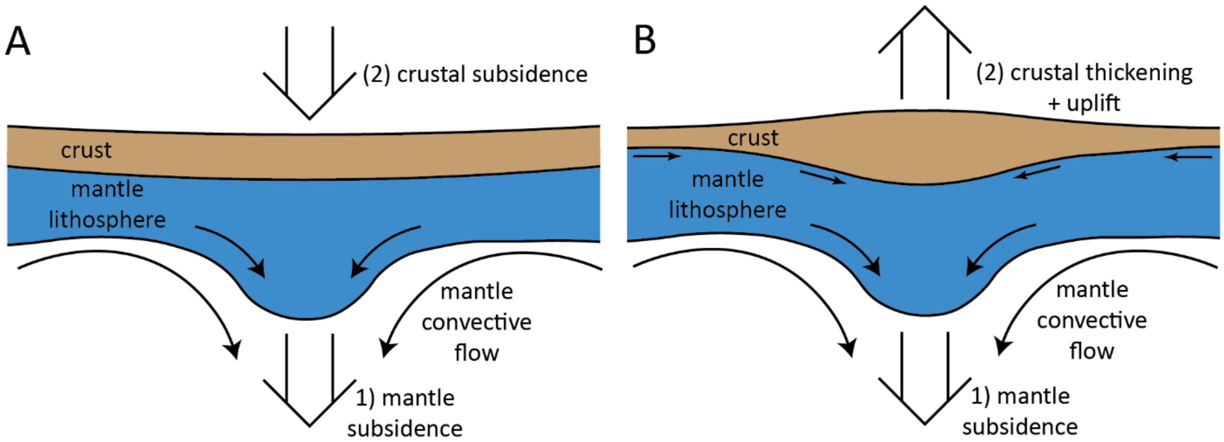

Figure 4 | **Illustration of Asymptomatic vs. symptomatic type drips** A) Example of an asymptomatic drip showing some crustal subsidence but no thickening/convergence. B) Example of a symptomatic drip showing crustal thickening and uplift due to significant horizontal flow in the upper mantle lithosphere. Modified from [37].

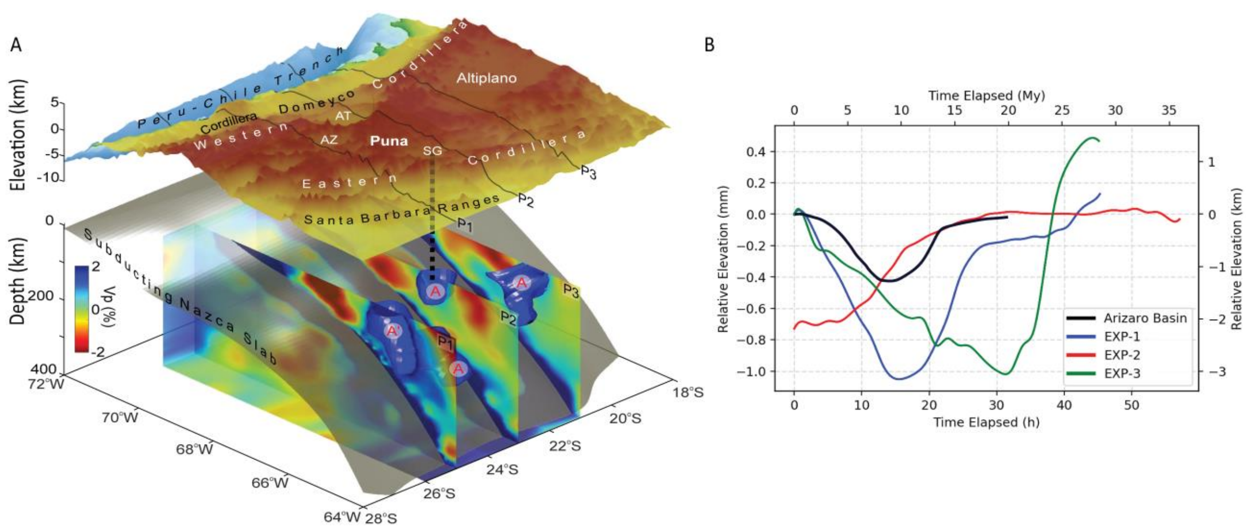

Figure 5 | **Tomography and Elevation Data in comparison to Arizaro Basin** A) P-wave tomography images beneath Arizaro Basin, Puna, and Altiplano Plateaus. Diagram modified from [50]. B) Relative surface elevation of EXP-1, EXP-2, and EXP-3 with Arizaro Basin from [33].

Supplementary

Experimental Setup

A plexiglass box with dimensions 25 cm × 25 cm × 21.5 cm was filled with Polydimethylsiloxane (PDMS) and left to rest until the air bubbles rose to the surface and the PDMS was free of any air (approximately 14 days). An ~2 cm thick layer of modelling clay and PDMS was mixed homogenously to represent the mantle lithosphere (ML). EXP-3 contained a lower crust made of a different ratio of PDMS and plasticine. The LC thickness was ~0.5 cm and it was layered on top of the ML ensuring little air was trapped between the layers to avoid air bubbles.

Drips will form naturally during an experiment due to the density contrast between the mantle lithosphere and mantle. However, to control the location and size of a drip, one is initiated in the center of the box. We introduce an instability into the ML to simulate a density perturbation (e.g., thickened crust, eclogitization, etc.). The drips were initiated using two different methods (Method 1 & 2). In Method 1, a drip is initiated by attaching an instability to the bottom of the ML (top of the mantle). In Method 2, a drip is initiated from the top of the ML (bottom of the crust). The ML is placed on top of the mantle ensuring the least amount of air is trapped between the layers. These two different methods of drip initiation became important to how a drip evolves over time in each experiment. Petroleum Jelly was used to lubricate the edges of the Plexiglass box to reduce the effects of the boundary on the ML in the experiment[51]. The UC material was sieved on the ML to a thickness of ~2 mm in all experiments.

The experimental setup is depicted in Figure 6. The monitoring equipment consists of three digital cameras (CCD, 11 MPx, 16 bit), two from above (*Image ProX* by *LaVision GMbH*, Göttingen, Germany) and one from the side (*Imager Pro* by *LaVision GMbH*, Göttingen, Germany). The recording of all cameras is synchronized. The cameras are connected to a computer where the imaging software *DaVis* by *LaVision GMbH* records the evolution of the model at a frequency of 0.017 Hz or one image every 58 s. The model is illuminated from above and from the side. The technique used in the experiments is Particle Image Velocimetry (PIV)[52]. Using this technique, we can observe changes in surface topography and descent of the drip through the mantle in time.

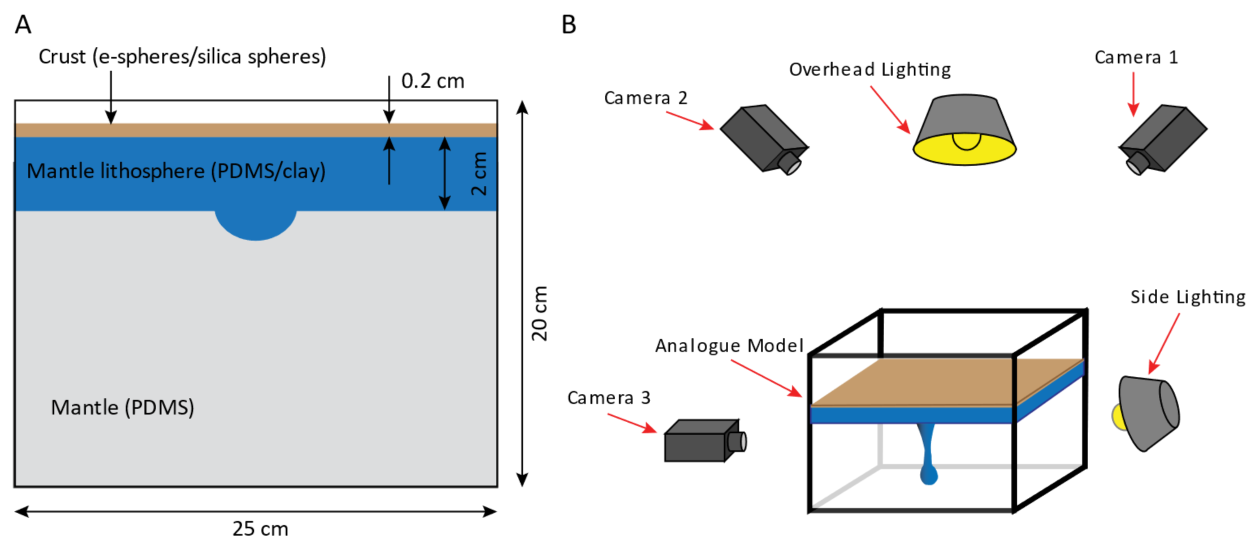

Figure 6 | **Schematic illustration of experimental setup** A) Analogue model dimensions and materials. Modified from[53]. B) Experimental setup and PIV camera positioning.

Materials and Rheology

The analogue model is a simplified, physical, 3D model of the crust, ML, and mantle using materials that have been scaled for viscosity, and density from their natural Earth values. Previous studies have discussed jelly sandwich vs. crème brûlée profiles[54]. However, we use a jelly sandwich approach with and without the presence of a LC. The crust material is a mixture of e-spheres (hollow spheres made with recycled plastic) and silica spheres, and behaves with a brittle Coulomb failure law. These materials were chosen for the UC instead of sand because they are spherical and have a small grain size (0.2 cm). Rocks in the UC have a cohesion of σ_0 ~50 MPa. Suitable materials for modelling brittle behaviour should have little cohesion with internal friction values similar to rock therefore using a granular material as an analogue to the crust is suitable [55,56]. The UC material was modelled using fine grained silica spheres that have no cohesion (σ_0 = 0 MPa) and angle of internal friction (φ) was ~40°.

The mantle is polydimethylsiloxane (PDMS), which is an organic silicone polymer and the ML and LC are mixtures of PDMS and plasticine. PDMS and plasticine are non-linear viscous (non-Newtonian) and their rheology is defined by the flow law:

$$\sigma^n = \eta\dot{\epsilon} \qquad (1)$$

where σ is stress, $\dot{\epsilon}$ is strain rate and viscosity (η) and n are material constants. The viscosity (η) and n values for each viscous material were measured using a strain-controlled (Discovery HR-3 Hybrid) rheometer.

In nature, temperature and metamorphic processes can play an important role in mantle convective processes, but are not easily incorporated into analogue models. Viscosity is temperature dependent and so we vary viscosity with discrete layers of differing material as an analogue for temperature variations. Clay is used to increase density of PDMS to mimic processes such as eclogitization.

Scaling

The density and dynamic viscosity of the analogue materials were scaled to corresponding natural values. The models were scaled for time, length, and gravity. Table 1 outlines the rheologic properties of the materials used in three experiments and the scaling factor (SF). The n value of all viscous materials was ~1.00.

EXP-1	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times10^{-7}$)	Model	Nature	SF	Model ($\times10^{4}$)	Nature ($\times10^{21}$)	SF ($\times10^{-17}$)	
Upper crust	2	8	2.5	820	2485	0.33				40
Mantle lithosphere	20	80	2.5	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.5	1010	3060	0.33	1.50	1.00	1.50	
EXP-2	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times10^{-7}$)	Model	Nature	SF	Model ($\times10^{4}$)	Nature ($\times10^{21}$)	SF ($\times10^{-17}$)	
Upper crust	2	8	2.5	1100	3333	0.33				35
Mantle lithosphere	20	80	2.5	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.5	1010	3060	0.33	1.50	1.00	1.50	
EXP-3	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times10^{-7}$)	Model	Nature	SF	Model ($\times10^{4}$)	Nature ($\times10^{21}$)	SF ($\times10^{-17}$)	
Upper crust	2	8	2.5	820	2485	0.33				40
Lower crust	5	30	2.5	1120	3394	0.33	2.12	1.41	1.50	
Mantle lithosphere	20	80	2.5	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.5	1010	3060	0.33	1.50	1.00	1.50	

Table 1 | Experimental setup Rheologic properties of materials used in the analogue models with a comparison to nature. The scaling factor (SF) is included.

Response to reviewer comments

Line numbers correspond to the original document that was submitted for review, not the newly revised manuscript.

Response to Reviewer 1

1) Significance and novelty of the study:

I find that the investigated question is really interesting and is significant to the community of geoscientists focusing on tectonics and geodynamics in the broad sense. I find the title very exciting. I cannot, however, evaluate whether the scientific issue addressed by the authors is novel since I am not a specialist in drip tectonics.

Response: Thank you for your comments. The title to the paper has been modified to "3D Geodynamics of lithospheric dripping beneath the Central Andes" to clarify the new enhanced focus/implications of the work on the geological case example, the Arizaro Basin and other related basins in the Puna Plateau of South America. We believe that connecting the modelling results more closely to a geologic example strengthens the manuscript considerably.

2) The deformation pattern at the surface is interpreted from visual inspection of the deformed surface and from the velocity field, but surface strain maps would help the authors to describe more precisely the strain evolution at the surface. More importantly, strain maps would allow the authors to perform a more quantitative description of their models, which is really missing at the moment. A quantitative analysis would allow the authors to strengthen their arguments and would provide more data to compare between models (e.g. it would be interesting to compare the maximum amounts of shortening and extension measured between models and to relate them to the parameters varied). A quantitative analysis would also allow providing a more accurate description of the evolution of surface deformation through time in relation to the different phases of drip sinking.

   Response: A more quantitative analysis has been performed to present and compare cumulative strain in each model. Specifically, we re-processed the data for all the models and derived the cumulative strain and have plotted these as vectors on each of the figure frames in Figures 2-4. These tracked strain results are then interpreted in the explanation of the model results. We appreciate the reviewer's suggestion and agree that this expanded quantitative analysis helps strengthen the explanation and interpretation of the drip process and induced surface deformation.

3) Writing:
   - The paper is generally written concisely but it could be better formulated in a few places. I think it could thus benefit from some minor/moderate re-writing as clarifications are also needed in a few places (see line-by-line comments below).
   - The description of the surface strain evolution could be developed/improved. The authors focus a lot on topography but the evolution of deformation and how it relates to the topography and the mantle flow should be described in more detail.
   - The description of how the topography relates to the mantle flow (both its horizontal and vertical components) should also be developed.

Response: We have edited the paper with a number of major and moderate changes (as documented through this response). All of these changes involve addition of results/data, new figures, and the appropriate textual changes.

This includes a much more detailed exploration and explanation of the surface strain evolution, as mentioned in the response to the previous point. We agree with the reviewer that there is a lot of important information in the evolution of surface deformation (horizontal tectonics) in addition to the topography (vertical motions). The substantial changes to the manuscript—including detailed comparison to the Arizaro Basin as a geologic example—involve the surface deformation of the models. We have expanded Figure 5 to detail these points and added relevant text. Similarly, we have modified the manuscript to enhance the description of how the topography relates to the mantle flow. This is particularly aided by new results and a new figure frame (Fig. 5b).

4) Interpretation of model results:

- The comparison of EXP-2 with natural examples of cratonic lithospheres (e.g. L206-219) does not make sense since EXP-2 has a weaker crust and a mantle lithosphere drip that decouples more easily whereas a cratonic lithosphere should be stronger overall. This part needs re-interpretation.

- The comparison of EXP-2 with the Hangai Dome is not consistent since this model does not seem to produce a dome (none of the three models actually do).

- Fig. 4 does not summarise well the results obtained with the models. The models show that shortening features appear where a topographic depression forms, not where there is a positive topography as indicated in Fig. 4. So the consistency between model results and interpretations needs improvement. I think this issue may partky relate to the lack of clear results on the figures (e.g. Fig. 1-3) compared to what is written in the manuscript (e.g. L194-195). It is indeed, for example, very difficult to see the three stages of topographic evolution as proposed by the authors on these figures. So either these figures should be improved to better render the multi-phase topographic evolution (subsidence, uplift, slow uplift) or the interpretation should be improved. One point on which I would change the text is on the location of the uplift, which should be given clearly and differentiated from the location of the positive topography that forms around the depression, which seems to sit above mantle upwellings, which thus may be a type of dynamic topography. I think then that Fig. 4 misses the inclusion of an essential mechanism of surface topography, which is due to the vertical component of mantle flow.

Response: We considered the reviewer's comments regarding comparison with cratonic lithosphere and the Hangai Dome and agreed that these were fair points. Accordingly, we have deleted these discussions. This revision fits better with the enhanced focus of the manuscript on the South American drip tectonics.

Regarding Figure 4 (now Figure 3), this (as well as the other model figures) has been completely redone. As mentioned above, we have added strain data, but we have greatly cleaned up the presentation of the side view and topography plots. We have also revised Figure 5 which depicts the topographic evolution. Together, based on the reviewer's comments, we believe there is a much improved and clearer explanation of topography in the models.

5) I am puzzled by the way the mantle lithosphere drip decouples much more easily in EXP-2. In my view, it cannot be due only to the reduced strength of the crust since this parameter cannot induce a thinner neck of the drip, nor can it control the flux of material in the drip. This needs to be explained. Wasn't the mantle lithosphere viscosity reduced in EXP-2? Table 1 indicates that it is the same as in EXP-1, but the results seem to indicate otherwise...

   Response: A detailed description of the method of drip initiation has been included in the Methods section. We explain in EXP-2 results section the effects of this in further detail by describing how initiation at the bottom of mantle lithosphere meant there was little influence of the instability on the upper part of the system and therefore no convergence.

6) Line 32: "scaled physical analogue laboratory experiments" Scaled analogue models would be sufficient to describe the type of experiments since by definition analogue models are physical and in a laboratory.

   Response: We removed the word "physical" and "laboratory", so it now reads "scaled analogue experiments".

7) Line 33: Replace "the" with "a" since the high resolution of the PIV technique used here is probably not universal.

   Response: Replaced

8) Line 36-37: It may be clearer to write "and subsequently reverses to regional uplift"

   Response: Changed "surface basin and subsequent inversion" to read as "subsequently reverses to regional uplift".

9) Line 38: It should be fold-and-thrust belts.

   Response: Changed fold and thrust to fold-and-thrust.

10) Line 39-40 "topographic inversions from ~1 km of tectonic subsidence to surface uplift (recovery) and regional tectonic deformation during the last ~20 Myrs" is unclear. I suggest reformulating. What is meant by "topographic inversions" is unclear. Why "recovery"?
   Response: With inversion, we mean the switch from subsidence to uplift. We have revised the sentence to be clearer in our explanation (including deleting "recovery").
11) Line 43: The Puna Plateau is rather close to a plate boundary so I suggest changing "beneath continental interiors and cratons" to something more inclusive
   Response: This statement was removed from the abstract.
12) Line 44: This is interesting because it means that the surface might not express that something is actually moving below it. Maybe such a statement could be included to open the abstract.
   Response: Thank you for this suggestion. We have changed the focus of the paper to comparisons of the model results to observations in the Arizaro basin and Central Andes

in general so the abstract now reflects those changes. We do discuss this finding in the discussion section with less emphasis.

13) Line 45: "Active mantle dynamics" is too vague and may mean other processes such as mantle convection etc. The terminology used here should be more specific.
Response: Thank you for this comment. We have removed this statement from the abstract.
14) Line 65: I suggest writing "new 3d Laboratory experiments" or "new 3D analogue models" in stead of a "new class of 3D laboratory analogue experiments"
Response: Changed "new class of 3D laboratory analogue experiments" to read as "3D scaled analogue experiments".
15) Line 66: Which part of the "descending lithosphere" is actually sinking?
Response: We have now specified **mantle** lithosphere
16) Line 67: It may be better to write "in space and time" instead of temporal and spatial domains.
Response: We have removed this statement from the introduction.
17) Line 77: This is poorly written
Response: This statement was removed from the introduction. We have heavily revised this section of the paper.
18) Line 87: "several" do you meant different types.
Response: This statement has been removed from the introduction.
19) Line 87: what is meant by others?
Response: This statement was removed from the introduction.
20) Line 88-90: "geological signatures (viz., km-scale of topographic variations)—instances where the crustal tectonic manifestation of lithospheric drip foundering is muted or absent" This is poorly written. Please reformulate.
Response: This statement was removed from the introduction.
21) Line 90: Which differences?
Response: This statement is no longer in the revised Introduction.
22) The methodology section misses the description of details on the PIV technique used (e.g. type of passive tracers, resolution, correlation window size, etc.) and appropriate references that have used the technique in the Earth Sciences, notably to compute the mantle flow velocity field and the surface deformation and topography (e.g. Adam et al., 2005; Funiciello et al., 2006; Boutelier and Cruden, 2013; Strak and Schellart, 2014, 2018; Chen et al., 2016).
Response: Thank you for the references. We have included the citations for Adam et al., 2005, Funiciello et al., 2006, Boutelier & Cruden, 2013, and Schellert & Strak, 2016. We have also expanded the description of details on the PIV technique. On line 344 we have included the resolution of the cameras. On line 341 & line 351 we have included materials that were used as tracers.
23) Line 140: Why choose such a high recording frequency for the software?
Response: When the images are processed, the software compute changes between the current image and the previous. For this reason, the first images are never as good as the

images at later time steps. We used a high frequency to ensure the data were high quality when we looked at changes in topography and strain.

24) Line 186: There is virtually no description of the scaling procedure, which makes it impossible to evaluate if the scaling used is robust. A scaling procedure should be included, with appropriate referencing. The authors should moreover explain how their modelled topography scales with natural topography, since it relates to the scaling approach used (scaling densities or density contrasts, see Schellart and Strak, 2016).
Response: We have added a detailed description of the scaling procedure in this section. This is on lines 166-194 of the revised manuscript and follows conventions from other analogue modelling studies in tectonics (as cited).
25) Line 97-98: "extension/shortening" are not "localized tectonic features" so I suggest reformulating.
Response: The word "localized" was removed from the results section so it now reads as "tectonic deformation".
26) Displacement vectors were shown but why not computing surface strain? It would be a more straightforward way to visualise shortening/extension
Response: Thank you for this suggestion. As noted above, we have reprocessed the data to compute cumulative surface strain as a percentage from start to end of experiment. The strain data are overlain on the elevation data in Figures 2-4. We can see that strain has accumulated in EXP-1 and EXP-3 but not EXP-2 despite changes in elevation.
27) Line 107: It would be useful to indicate what "0-40 hours" indicate in nature.

Response: We have included the scaled time in My.

28) Line 120: It would be useful to indicate what "1.5 mm of subsidence" represent in nature.
Response: We have included the scaled subsidence in km for amount in nature.
29) Line 121-122: This sentence is not well formulated
Response: This sentence was revised and the statement "where the topography gradually increases around the ring was removed".
30) Line 121: Why such? I suggest replacing with "The"
Response: "such" was replaced with "The".
31) Line 125: Delete "The".
Response: Deleted
32) Line 126: Delete "removed"
Response: Deleted
33) Line 132-134"EXP-2 explores how the coupling of instabilities to the mantle lithosphere may control...". This is very unclear. What parameter exactly has been changed compared to EXP-1?
Response: We have indicated the coupling has changed via the method used to initiate a drip in the mantle lithosphere. The two different methods of drip initiation are described earlier in the Results and again in the Methods section.
34) Line 137: "to activate the gravitational instability" is not well formulated since there is a gravitational instability. It is just smaller in volume.

Response: This sentence was changed to “The lack of horizontal crustal convergence and accumulation of strain in the crust indicates that the lithospheric drip was not well coupled to the upper mantle lithosphere compared to EXP-1 (Figure 3a). Figure 3b shows an overall dynamic evolution of the drip in EXP-2 that is like EXP-1, although the volume of mantle lithosphere involved in the drip is significantly reduced.”

35) Line 140: “ downward displacements are highest magnitude lower in the box” This is not well formulated.
Response: This sentence was removed from the results of EXP-2.
36) Line 143: “developed” here past tense is used to describe the experiments but it is not consistent with the use of present tense before.
Response: It now says “develops”.
37) Line 145-146“when initially an unstable lithosphere is strongly coupled to the crust” This does not make sense since the crust is part of the lithosphere. Which part of the lithosphere is couple to the crust?
Response: That statement was removed.
38) Line 161: The density of the lower crust should also be given
Response: Density has been given.
39) On Figure titles “40h PIV Time Frame of EXP-#” etc. are not good titles. They should be more explicit. The vectors for the mantle flow velocity fields are difficult to visualize. They should be improved. A colour scale is missing for those velocity fields.
Response: Title is now “Temporal Evolution of EXP-1”. Vector fields are colour coded according to strain value.
40) Fig 4 is not consistent with EXP-1 where a depression forms and shortening occurs in that depression. So this conceptual model is not valid to explain the results
Response: This figure has been removed in the revised manuscript.

Response to Reviewer 2

1) This is an important topic in geodynamics and I think this paper has the potential to make a strong contribution to the field. While it currently provides a useful review of some of the geological and geophysical signatures of drip tectonics on Earth, there is a quite a substantial omission in the experimental work that forms the core basis of the paper. These experiments are characterised as "a new class of 3D laboratory experiments" that provide "new insights" into "modes of tectonic behaviour beneath continental interiors." Unfortunately this is not correct as the experiments presented are essentially replicates of work published by Pysklywec and Cruden (2004) with only minor modifications (e.g., slight changes to the tank size and modelling materials, use of digital photogrammetry and PIV to quantify surface deformation, etc.). However, it is reassuring to see how similar the results are between the 2004 study and this one, including features that the current paper attributes to "symptomatic" versus "asymptomatic" drips. Ideally, a revised version of this manuscript would fully acknowledge this previous work and compare and contrast the findings from both studies to arrive at a better or updated understanding of the geomechanical interactions between mantle drips and surface deformation. Such changes go well beyond the

expectations of a typical major revision, meaning that I am recommending that this paper be rejected and possibly reconsidered after resubmission.

Response: We appreciate the reviewer's comments here. In the revised manuscript we have made many modifications to properly put our work into context and to cite relevant previous work. This includes a revised description in the introduction and especially in the Methods section of the paper. With consideration of the reviewer's comments, we have included with these revisions a number of passages that improve the explanation of the differences between the studies and the implications. In this study we were studying the effects of differing rheology of the mantle lithosphere on the crustal expression. We have emphasized that not all drips will cause appreciable thickening in the crust and that these models, particularly EXP-1 is consistent with interpretations from DeCelles et al. (2015) tracking the topographic evolution of the Arizaro Basin in the Altiplano-Puna Plateau.

2) Symptomatic vs asymptomatic. I was not convinced by the introduction of these terms for drip behaviours. If I understand correctly, when the surface signals of an underlying drip are really obvious (e.g., strong subsidence followed by uplift, development of shortening structures, etc) then a drip is considered to be symptomatic. If the signals are subtle (e.g., more subdued subsidence and uplift, no contractional upper crustal deformation) then the drip is asymptomatic. However, I suspect that the signals of many (most?) drips will be quite subtle, like the Tulare Lake basin adjacent to the Sierra Nevada (Saleeby & Foster 2004) while strong near surface deformation such a faulting and thrusting accompanied by more dramatic subsidence and uplift will be rather exceptional (i.e., asymptomatic).

   Response: The reviewer is correct in their understanding of what we meant by symptomatic vs. asymptomatic drips. This was one of the primary points in the original manuscript (including the title), but in this revised version, we have reduced the emphasis, and have re-written the relevant passages to try to clarify things here. We focus on the evolution (e.g., topography and surface deformation) of a symptomatic drip—around the Arizaro Basin. This makes for an interesting comparison with what the reviewer cites as the Sierra Nevada case, as perhaps a less symptomatic case (at least for surface deformation). Accordingly, we have left discussion of asymptomatic vs. symptomatic in the manuscript as an interesting illustrative result from the models, and hopefully helpful in informing the readers of such behaviour.

3) How the two types of initial density perturbation that initiated the drips in the centre of the tank need to be explained much more clearly, particularly Method 2. This is important because it appears to influence the drip dynamics.

   Response: We agree that the method for drip initiation is important. As such, we have moved this from a supplementary section to the Methods section in the manuscript and described more clearly how each perturbation was initiated as Method 1 & 2. E.g., lines 327-332.

4) The behaviours and sizes of the drip in the 3 experiments are described only in qualitative terms. It would be better if the drip descent behaviours could be quantified and plotted against time, as done in Pysklywec & Cruden (2004) who showed that the drip dynamics are strongly modulated by the nature of the overlying crust. Some of the outcomes of Exp. 3

seem to arise from a non-axisymmetric drip geometry and flow, which needs to be fully described/quantified.

Response: As recommended by the reviewer, we have quantified and plotted the drip descent for all three experiments. These data have been added as a separate figure frame (frame C) at the bottom of each model figure (Fig. 2-4). We have also added the related description/analysis of these in the revised text. All the drips were initiated using a point instability (a hemisphere) in the center of the experimental box. Non-axisymmetric flow is a consequence of the model. In the cases with convergence of the crust, there was always a preferred direction of shortening rather than equal shortening on all sides of the drip.

5) The Supplement states that the tank walls were lubricated with petroleum jelly to reduce boundary effects on the ML. Duarte et al. (2014) showed that petroleum jelly (petrolatum) on its own is a rather ineffective lubricant for PDMS. It needs to be mixed with paraffin oil in the right proportion to work as a good lubricant. Were control experiments carried out to assess how effective this lubrication was and its outcome?

   Response: The petroleum jelly was used to grease the side of the tank when inserting the mantle lithosphere into the tank. It wasn't intended to create a free surface or to allow the ML to flow. We did not conduct control experiments to test the effectiveness of this lubricant.

6) PIV matters. PIV doesn't measure surface topography or the drip geometry - it measures surface velocities and fluid velocities in the tank. Surface topography is measured by DaVis using digital photogrammetry. The decent of the drip is recorded by the digital side images. The velocity vector data presented for the fluid in the tank looks quite noisy. I suspect this is because particle velocities are being measured from the whole tank, rather than in a vertical plane containing the drip structure. This could have been resolved by using a light sheet rather than illuminating the entire tank volume. This issue and how the mantle fluid was seeded with particles need to be discussed.

   Response: We have revised our description of the PIV technique and included into the new Methods section. Agreed on some of the challenges with measuring velocity vectors on the side view. Some of the velocity vector data are better than others—e.g., models 1 and 2 are better than 3. We still think there's value in plotting these data, but we're more careful with how we interpret the velocity data for model 3.

7) PDMS was used for the asthenospheric mantle and Table 1 reports its density to be 1010 kg/m^3. One of the physical properties of all molecular weights of PDMS is a density of ~960 km/m^3 so I wonder if this is correct? The reasoning for using the chosen granular upper crustal material is hard to follow. I suspect the main reason it was chosen was to have a density that is scaled to the crust. Contrary to what is stated in the Supplement, most quartz sand using in analogue modelling has a grain size << 0.2 cm (2000 microns) - typically around 400 microns (0.04 cm). Also, why did Exp 2 use a higher density upper crust compared to the other two experiments? This slightly higher density will mean that the UC immediately above the LM will be stronger than in Exp. 1, which may partly explain the lack of surface deformation. It may also contribute to the different drip behaviours (see Pysklywec & Cruden, 2004).

   Response: The density of the PDMS was measured correctly. We tested to make sure it would float in water, and it sank. The PDMS likely has impurities in it because it has been recycled from previous experiments. A series of 10 experiments were completed in total. The crust was changed due to lack of crustal convergence. We suspected it was too heavy and the pull force from the drip was not strong enough to pull in such a heavy crust to observe shortening. We determined that the ratio of viscosity of the mantle lithosphere to the mantle in conjunction with the method of drip initiation (Method 1 & Method 2) were the deciding factors, not the crust material.

8) Exp. 1 and 2 have a brittle upper crust that scales to 8 km and no lower crust. Where on Earth or elsewhere in the solar system would there be a lithosphere that looks like this?
Response: For simplification purposes in the model, we chose a thin upper crust to ensure that the dynamics occurring in the mantle lithosphere would propagate to the crust and we could clearly observe the structures formed in the crust. This thin sand upper crust is the purely brittle uppermost part of the crust—i.e., not including mid/lower crust—so we didn't want to make it excessively thick. Roughly, we justify the brittle/sand crustal thickness based on estimates of the rather thin elastic thickness for the Central Andes of <10 km (Garcia et al., 2017) The viscous portions of the lower crust are included as a simplification within the viscous mantle lithosphere, or as a separate lower crust layer (EXP-3). We have added discussion of this in the revised manuscript (including reference).

García, H., Gianni, G., Lupari, M., Sánchez, M.A., Soler, S., Ruiz, F., Lince Klinger, F., Effective elastic thickness in the Central Andes. Correlation to orogenic deformation styles and lower crust high-gravity anomaly, Journal of South American Earth Sciences, 2017. 87. 10.1016/j.jsames.2017.11.021.

9) The scaling section in the Supplement is not very informative. How was it done? Where are the scaling arguments? How has geometric, kinematic and dynamic similarity been achieved/demonstrated? In regards to Fig. 5B, there must also be a time scaling factor (T = 1.8E-10) to equate the experimental times to times in nature and vice versa. This is not mentioned anywhere in the paper but it should be.
Response: As described with the response to the previous reviewer, we have included in the Methods section a detailed and expanded explanation of the scaling. The experiments have been scaled for length, time, viscosity, density, and gravity. We include the time scaling factor (1.82×10^{-10}) for the figure.

10) On the title: Symptomatic of what? This sounds nice and trendy but its meaning is unclear
Response: We have changed the title. The focus of the paper has shifted to modeling the dynamics of the Arizaro Basin in the Altiplano Puna plateau rather than asymptomatic vs symptomatic drips, although we still have some discussion on this interpretation/observation.

11) What is new beyond what Pysklywec & Cruden (2004) reported based on almost identical experiments? The only thing new is the use of symptomatic versus asymptomatic.

Again, this is pretty much the same conclusion reached by Pysklywec & Cruden (2004).

With only very minor modifications these experiments are essentially replicates (i.e. copied from) of those reported in Pysklywec & Cruden (2004).

Response: We have built on this previous work with some enhanced investigation of the surface deformation and topography. This now includes an analysis of the strain in the crust during the experiment. We concede that we didn't do a proper job of putting the present work into context with Pysklywec and Cruden (2004) and have improved this in the revised manuscript. Further, we have modified the scope and direction of the paper to include a comparison of the model results to a geological example of the Arizaro Basin.

12) Not clear why Göğüş and Pysklywec (2008) makes this list and not Pysklywec & Cruden (2004).
Response: This sentence was removed, and the introduction was rewritten. We have included a reference to Pysklywec & Cruden (2004).
13) I.e., as explained much more clearly in Pysklywec & Cruden (2004) and illustrated in Fig. 6A. Is the "instability" made of the same material as the rest of the LM? Or is it different in some way?
Response: All instabilities are the same material as the mantle lithosphere and the same size in all experiments. We have included a more thorough explanation of the methods that were used to initiate a drip in the Methods section. Method 2 used in EXP-2 is the same method used in Pysklywec & Cruden (2004) and Method 1 was our method where a drip is initiation at the top of the mantle lithosphere between the ML and crust.
14) Yes but Duarte et al. showed that petroleum jelly (petrolatum) on its own is an ineffective lubricant for PDMS. It needs to be mixed with paraffin oil in the right proportion to work as a good lubricant.
Response: The petroleum jelly was sufficient lubrication to insert the mantle lithosphere into the tank on top of the PDMS so that it wouldn't drag on the side of the tank. Once in place it was stuck to the sides for the duration of the experiment. It was not used to create a free slip surface on the sides.
15) Line 529 Which version?
Response: This was corrected in the updated Methods section. We used DaVis 7.2 for the top cameras and Davis 7.1 for the side camera
16) Is the side illumination uniform or a vertical light sheet? Was the mantle PDMS seeded with PIV particles to aid with velocity vector calculation? Also the flow in the tank is axisymmentric - how did avoid measuring velocities that are between the camera and the drip?
Response: The side illumination is uniform. The PDMS has clay impurities in it that aid with vector calculation. Velocities between the drip and the camera are measured. These velocities would be mantle flow velocity. We were interested in the response of the mantle to the downward motion of the drip and the removal of the mantle lithosphere.
17) Line 566: How do you know this? Did you actually measure it? If so, how?

Response: We did not measure this. We assumed this based off the definition given by the Encyclopedia of Engineering Geology for noncohesive soils. This is referenced on line 373.

Keaton J.R. (2018) Noncohesive Soils. In: Bobrowsky P.T., Marker B. (eds) Encyclopedia of Engineering Geology. Encyclopedia of Earth Sciences Series. Springer, Cham. https://doi.org/10.1007/978-3-319-73568-9_212

18) Line 567: Again, how do you know this?

Response: We have included an explanation on how the angle of internal friction was calculated in the Materials and Rheology section of the Methods section.

19) There is a problem with the value of the density of 1100 g/m^3 for this material in Table 1. All molecular weights of PDMS types have a density of ~ 960 kg/m^3 so I wonder if this can be correct?

Response: We measured the density of our PDMS. We tested the value by putting the PDMS in a cup of water. If it had density of 960 kg/m^3 it would have floated, which it did not. The PDMS has been recycled from other experiments, so it is highly likely there are impurities in it.

20) Line 573: The parameter is NOT the viscosity. It is often called the “consistency”

Response: We have defined this parameter as viscosity rather than dynamic viscosity. This is the same equation used in other analogue modeling studies by Pysklywec & Cruden (2004) and Molnar et al. (2017).

21) This is a wholly inadequate account of the scaling. You could of course refer to Pysklywec & Cruden (2004) and the equations therein from which this scaling has clearly been adopted.

Response: As indicated, we have now included the calculations that were used to calculate the scaling factors for our experiments. Reference to Pysklywec & Cruden (2004) has been included.

22) Line 587: How was this done? Where are the scaling arguments? How has geometric, kinematic and dynamic similarity been achieved/demonstrated?

Response: We have included the details of the scaling calculations in the revised version.

23) PIV: you need to spell out what this acronym means

Response: Thank you for pointing this out. We have specified it stands for Particle Image Velocimetry.

24) Why is this considered to be a reference experiment? That is, what are its properties and parameters that make it a reference case to which other experiments are compared?

Response: We no longer refer to EXP-1 as a reference experiment in the revised manuscript.

25) Surely you need to direct the reader to the Supplementary to learn something about the set up etc. of the experiments?

Response: The experimental setup is now in the main body of the paper no longer the supplementary materials. The Methods section must be at the end of the paper as per the journal’s formatting guidelines. However, we direct the reader to this section to read in detail about the experimental setup.

26) These vectors are very hard to see in the poor quality images provided. Also the velocity data are very noisy suggesting that particle velocities have been computed throughout the volume of the tank, rather than what should have been done, which is to measure velocities in a vertical illuminated sheet centred on the drip structure.
Response: We have re-drafted all of the results figures to improve their clarity. As we describe in our response above, we have tried to improve the visual representation of the velocity vectors, but inherently the measurements for Models 1 and 2 are superior to Model 3 (whereas the review notes, some of the data are noisy).

27) Line 113: Is there a plot that shows this?
Response: In the revised manuscript, we have included in Figures 2-4 frame C plots of the progression of the drip beneath the surface of the experiment. The plot does demonstrate the rate of descent increased.

28) Line 118: 1. Surface elevations are measured in the software used (DaVis) by digital photogrammetry and not by PIV.

2. PIV is a technique applied to digital images. It is not in itself an imaging tool.
Response: We have corrected this, and the phrase "measured by PIV imaging" has been removed. A better explanation of these techniques has been included in the Methods section.

29) Line 129: You should also mention explain the very obvious air bubble feature in the centre right of the frame that seems to have been active from early stages.
Response: We have included mention of this in the caption of Figure 2.

30) Line 132: Rewrite – hard to make sense of this. How are differing degrees of coupling varied and how is it measured?
Response: We have included a better explanation of what we mean by coupling in the discussion section where we discuss symptomatic vs asymptomatic drips. Poorly coupled drips don't cause convergence in the crust.

31) Line 133: OK, so what parameter has been changed? How are we supposed to know?
Response: The method used to initiate a drip was changed compared to the other experiments. We have clarified this in the revised manuscript.

32) Line 137: What does this mean? Please explain. From the Supplementary I cannot tell how this experiment is different to Exp. 1, except for having a slightly denser upper crust with a slightly lower friction angle (the reason for this is never explained). This slightly higher density will mean that the UC immediately above the LM will be stronger than in Exp. 1, which may explain the lack of deformation. It may also contribute to the different drip behaviour (see Pysklywec & Cruden 2004, who explore in some detail how the nature of the crust influences drip descent velocities).
Response: We have made clearer how this experiment is different from the others via the method used to initiate a drip in the model. A description of the two methods used is included in the Methods section and in the Results section under EXP-1 and EXP-2. In our study, the crust we used didn't change the outcome. It was the method of drip initiation and the viscosity of the mantle lithosphere that determined whether the crust deformed appreciably or not.

33) Line 138: How do we know this? Has it been measured/plotted?
Response: We have included new plots in Figures 2-4 showing the progression of the drip beneath the surface (as recommended by the reviewer).

34) How do you know this? There are no velocity vectors shown in the tank at 40 h?
Response: Even after the drip has touched the bottom of the box, we can see in the image that material is being removed from the mantle lithosphere down the neck of the drip.

35) Line 145: This is out of place/does not follow the first part of the sentence
Response: The phrase "when initially an unstable lithosphere is strongly couple to the crust" was removed from the sentence.

36) Line 147: It is still not at all clear how/why this instability is weaker.
Response: We have made it clearer by describing the method used to initiate the instability in the Methods section.

37) Line 153: But earlier you say that this instability is weaker without explaining why.
Response: We have removed this statement from this section. We do describe how the overall dynamic evolution of the drip in EXP-2 is similar to EXP-1 but not the same.

38) Line 156: What does this mean and how do you know it?
Response: This statement was removed.

39) Line 161: This parameter has not been defined anywhere in the paper. I assume it the effective dynamic viscosity.
Response: Thank you for pointing this out. The value μ was a typo. It has been changed to η which is the dynamic viscosity and was defined earlier in the paper.

40) Line 162: Has this been quantified/plotted
Response: We have included plots for the side data in Figures 2-4 frame C

41) Line 165: is this true in all direction? We are only shown an image from side.
Response: No. It is true in the direction shown in the image. It was quite thin in the other direction.

42) Line 166: This could be explained more clearly. One also wonders whether the initial instability might have been large than in Exp 1? This is supported by the fact that the surface depression is elongated northwards from the centre of the tank.
Response: We have specified that the same size of instability was used in all three experiments.

43) Line 170: Can this be quantified so that we have a meaningful comparison to the other experiments?
Response: the instability was the same in all three experiments. We have removed this statement.

44) Line 171: See previous comment- oval because the initial instability was larger than in EXP 1 and possibly also slightly elongated?
Response: The term "oval shape" was removed. The same size and shape of instability was used in all experiments.

45) Line 173: Rewrite – clumsy grammar and unclear
Response: Rewritten. The sentence now reads "The buildup of strain in the crust as it converges yields similar wrinkle like/linear features to EXP-1 (Fig. 5 45h frame), with the highest cumulative strain reaching 20%. Zones of the crust are extended (beginning at

~10 h) on the margins of the basin as material is pulled inwards in the finite volume model box.”.

46) Line 175: Not at 45h – all vectors are divergent by then
Response: This sentence was rewritten to correct and discuss the strain vectors rather than displacement.

47) Line 188: This has not been documented at all in the paper but it does come back to one of my previous questions.
Response: The discussion on symptomatic vs asymptomatic drips has been given less emphasis and we have shifted the focus to compare the models to the Arizario Basin and the Central Andes in general. This sentence was removed in this section of the revised manuscript.

48) One major issue is that EXP-1 has no ductile lower crust and only a brittle crust that scales to 8 km thick. Where on Earth or any other planet could this be expected in a continental setting?
Response: As described above, this is a simplification to the model. We’re assuming it is a location like the Central Andes with a thin elastic thickness of the lithosphere and not a tectonic regime where a hot thick lower crust dominates the tectonics.

49) Line 197: These latter stages are not well documented/analyzed here but they are measured very carefully in Pysklywec & Cruden (2004).
Response: As stated previously, the focus of the paper is now less on symptomatic vs. asymptomatic drips and more on a geologic application of the models to the Altiplano-Puna Plateau.

50) Line 201: Shorter in time or shorter in wavelength, or both?
Response: Shorter in time. This statement was removed in the revised manuscript.

51) Line 203: What about the formation of a sedimentary basin? Surely this is a significant “manifestation” of a drip??
Response: Agreed. In the revised manuscript we have changed this description, and overall have downplayed the symptomatic/asymptomatic discussion.

52) Line 206: analogues?
Response: The title is now “Discussion”

53) Line 206: Lots of rather poor writing/grammar in this section
Response: We have re-written this entire section and improved the writing/grammar.

54) Line 219: Not clear
Response: We include a more thorough explanation of coupling and decoupling in the results section of EXP-2. The discussion section where this was mentioned has also been heavily revised.

55) This should have been presented earlier. Why does the surface in Exp. 2 start off a low elevation. This needs to be explained.
Response: We present Figure 5b (now Figure 5A) in the discussion section because we are comparing topographic changes of the models to those in the Arizaro Basin. Figure 5A was modified and now demonstrates that subsidence happened quickly in EXP-2 before the camera was turned on and data collection began. The curve reflects that the

surface was subsided and lifted over time (which is also reflected in Figure 3A). This is explained more thoroughly in the discussion.

56) Pretty much exactly the same as Pysklywec & Cruden (2004) but less precise and without error bars.......
Response: We have added error bars to the plots. The discussion focuses more on a geologic application of these models. The drip models are similar to those in Pysklywec & Cruden (2004) but we are comparing these models to case study observations from DeCelles et al. (2015).

57) Line 253: This is probably more likely due to isostasy
Line 255: Again – isostasy
Response: The discussion section of the manuscript has been heavily revised, and we have mentioned isostasy when discussing the findings in EXP-3 particularly as it relates to the rate of uplift and subsidence.

58) Line 261: No explanation has been provided for how the time comparison has been carried out. One assumes that a time scaling factor has been used but this is not provided anywhere.
Response: Scaling has been included in the Methods section along with the time scaling factor.

59) Line 265: Unfortunately, these have never been properly explained
Response: This statement was removed from the revised manuscript.

60) Line 269: Not explained anywhere
Response: The statement "induced by the perturbation" was removed from the discussion.

Line 269: This doesn't make any sense.

Line 271: This doesn't make any sense
Response: We have heavily revised the discussion section of the manuscript to improve clarity. These passages have been removed/edited.

Response to Reviewer 3

1) This is an interesting paper which describes observations arising from models of dynamical instability in a fluid system representing the Earth's continental crust and upper mantle. In general I think the results are worthy of publication, but I found the presentation of this paper rather frustrating. All of the essential information about how the experiments are set up is consigned to supplementary information, and I think there is not even a sentence in the main text that refers the reader to where this essential information can be found. Without this information I could get nothing useful from the paper, so my view is that experimental set-up should be part of the main text and I'd prefer a more conventional presentation in which the experimental set up is described before the results are interpreted. Statements attributing experimental behaviour to e.g. "the addition of a lower crust" (line 254) are uninterpretable without the setup information, but even having the supplementary information the reader is left to wonder

if it is the thickness or the viscosity or the density of this additional layer in the experiments that causes the observed effect in the experimental system.
Response: Thank you for your positive and constructive comments. The manuscript has been significantly revised to include the Methods section in the main body of the paper. As per the journal's formatting guidelines, this section must go at the end of the paper. These changes address comments from the other reviewers as well, regarding the description of the model set up and approach. We believe with this information moved into the body of the paper, and expanded on, that the revised work is a much-improved presentation of our work.

2) The extent to which these experiments actually represent the nominal lithospheric column is also not clearly explained. The essential data are in the table on the last page. These include a scaling factor for each parameter, without any explanation of how to interpret this scaling factor and what does it mean for the scaling of time that is implicit in Figure 5 and line 260. Other issues, like the non-Newtonian viscosity of mantle lithosphere that is widely accepted, are obscured by statements (lines 510-511) which imply that the experiments use a non-Newtonian viscosity, while the parameters in Table 1 show, no, viscosities are apparently Newtonian. Whatever the physical properties used in the experiments, the results are interesting, but to be useful, the experiments need to be documented accurately and clearly.
Response: We have included a scaling table (Table 1) in the Methods section of the revised manuscript with the model values and their corresponding value in nature. We have also included a detailed description of how these scaling factors were derived from their natural values. The values of the viscous materials used in the models are non-Newtonian as they have an n value of 1.01 which was measured using a viscous rheometer. This discrepancy was corrected in the manuscript.

3) I think that some aspects of how the experimental results are presented can be improved: In Fig 1 the lower set of frames (side view) are very hard to read. Better to show fewer frames, or maybe remove the dark backgrounds from the velocity vectors. Do you want to show what these velocity fields look like ? because this presentation is not working very well. The grey scale inset diagram in the upper row of Fig 1 is interesting, but the reader is told only to interpret "wrinkle-like linear features" (lines 126, 172) ; how does this image relate to the surface elevation maps ? If these structures are modifying the surface, why do they not show in the topography map ? or are we looking at an integral view through the box ? Figure 5 is also difficult to interpret. Abbreviations on the Figure should be explained in the caption, so it is unclear to me which part of that 3D system is the focus for the authors and how do the tomographic sections relate to the processes described in the experiments.
Response: We have redrafted all the figures in the manuscript, especially focussing on an improved representation of our modelling results (Figures 2-4). In doing so, we took the reviewer's comments (and those of the other two reviewers) into consideration. We also add new information and focus on the surface deformation in the models (wrinkle-features), with a direct comparison to the Arizaro Basin.

4) It goes beyond the scope of the present paper I think but quantitative information from these experiments might usefully be compared with the quantitative analysis made by Hoogenboom and Houseman (Icarus 180 (2006) 292–307) for the axisymmetric structure and by Neil and Houseman (GJI, 1999) for the linear structure. I make this comment in case it is of use to the authors in further work.
   Response: Thank you for the suggestions. We agree that this would make for an interesting study in the future.
5) Please be aware of what the word “instability” means; using it as a synonym of “drip” is unacceptable I think (e.g., lines 111, 113, 133, 167, 268, 472, 474)
   Response: We have corrected these to say drip rather than instability.
6) “the enigmatic formation of ~ 100 km wide oval/circular 84 shaped basins scattered across the region”. This is an important motivating statement for the study, but you cite reference 32, which I consulted, only to find a study of 2D numerical experiments, and no clear support for the statement, so is there a better reference which actually documents these basins in more detail ?
   Response: This paragraph was removed from the introduction. The motivation of the study comes from work by DeCelles et al. (2015) that suggests the Arizaro basin underwent uplift due to lithospheric drip in the Miocene era. Schoenbohm & Carrapa (2014) also suggest the formation of basins (50-100 km wide) beneath the Puna Plateau have also been formed by lithospheric drip removal.
7) My view is that experimental setup should be part of the main text and I’d prefer a more conventional presentation in which the experimental setup is is described before the results.
   Response: Thank you for the suggestion. As mentioned, we have moved the experimental setup to the main body of the paper instead of in the supplementary section. This has improved the flow of the paper and understanding of the analogue model setup. As per the journal’s preferred format, Methods must be at the end of the paper, but we make reference to where the reader can look to obtain information regarding experimental setup.
8) Other issues, like the non-Newtonian viscosity of mantle lithosphere that is widely accepted, are obscured by statements (lines 510-511) which imply that the experiments use a non-Newtonian viscosity, while the parameters in Table 1 show, no, viscosities are apparently Newtonian. Whatever the physical properties used in the experiments, the results are interesting, but to be useful, the experiments need to be documented accurately and clearly.
   Response: The materials used in the experiments are slightly non-Newtonian. The n value was 1.01 meaning the materials are somewhat strain weakening. This n value was measured using a strain-controlled rheometer.
9) The authors use velocity fields from which they interpret deformation patterns. However, it would have been clearer and more straightforward to use strain maps as they allow to more easily distinguish shortening/extension and to better visualise low vs high strains. I would thus recommend using surface strain maps instead of (or in addition to) the surface velocity fields.

The deformation pattern at the surface is interpreted from visual inspection of the deformed surface and from the velocity field, but surface strain maps would help the authors to describe more precisely the strain evolution at the surface. More importantly, strain maps would allow the authors to perform a more quantitative description of their models, which is really missing at the moment. A quantitative analysis would allow the authors to strengthen their arguments and would provide more data to compare between models (e.g. it would be interesting to compare the maximum amounts of shortening and extension measured between models and to relate them to the parameters varied). A quantitative analysis would also allow providing a more accurate description of the evolution of surface deformation through time in relation to the different phases of drip sinking.

Response: We have taken these suggestions and revised the figures to include an analysis of cumulative surface strain (Figures 2-4A). Results show that in EXP-2 there was no accumulation of strain over 60h of the experiment in comparison to EXP-1 and EXP-3 which show higher levels of strain as the crust converged due to the drip.

10) Line 102: "…linear structural patterns, and deep seismic tomography images" citations needed.

Response: This statement was removed. There is a detailed explanation on the tectonic setting of the Puna Plateau in the introduction (including abundant citations).

11) Line 119: "approximately circular"

Response: Now reads as "approximately circular"

12) radius of the basin, how do you measure this? Do you use a half-width at half-type measure, otherwise it seems poorly defined.

Response: We have approximated the radius by measuring the width at the widest points of the basin. It is not perfectly circular.

13) I am puzzled by the way the mantle lithosphere drip decouples much more easily in EXP-2. In my view, it cannot be due only to the reduced strength of the crust since this parameter cannot induce a thinner neck of the drip, nor can it control the flux of material in the drip. This needs to be explained. Wasn't the mantle lithosphere viscosity reduced in EXP-2? Table 1 indicates that it is the same as in EXP-1, but the results seem to indicate otherwise...

Response: We have included in the Methods section a more detailed description of how a drip was initiated in this experiment in comparison to EXP-1 and EXP-3.

14) Line 137: what does this actually mean in the experimental context? From the Supplementary I cannot tell how this experiment is different to Exp. 1, except for having a slightly denser upper crust with a slightly lower friction angle (the reason for this is never explained). This slightly higher density will mean that the UC immediately above the LM will be stronger than in Exp. 1, which may explain the lack of deformation. It may also contribute to the different drip behaviour (see Pysklywec & Cruden 2004, who explore in some detail how the nature of the crust influences drip descent velocities).

Response: The statement "The initial perturbation (~19 mm radius) was not well coupled to activate the gravitational instability compared to EXP-1" was removed from the results section of EXP-2. We have also included a more detailed explanation of how this experiment is different from the other two via the method used to initiate a drip.

15) The comparison of EXP-2 with natural examples of cratonic lithospheres (e.g. L206-219) does not make sense since EXP-2 has a weaker crust and a mantle lithosphere drip that decouples more easily whereas a cratonic lithosphere should be stronger overall. This part needs re-interpretation.
Response: We also agree this interpretation wasn't clear and we have removed this from the discussion section of the revised manuscript.
16) The comparison of EXP-2 with the Hangai Dome is not consistent since this model does not seem to produce a dome (none of the three models actually do).
Response: This comparison has been removed in the revised manuscript.
17) I don't see evidence for surface uplift in Fig 3, or do you just mean that the surface is restored to its pre-instability level?
Response: Yes. We mean relative uplift of the surface with reference to its rapid initial subsidence. We clarify this in the revised manuscript.
18) Line 228: I don't see the surface uplift in your surface elevation maps, so what are you describing here? Again, maybe relates to wrinkles that were not adequately explained at an earlier stage.
Response: When we say uplift, we are referring to the recovery of the surface topography that was undergoing subsidence as the drip pulled it down. In the revised manuscript we attribute the wrinkles to strain accumulation.
19) Statements attributing experimental behaviour to e.g. "the addition of a lower crust" are uninterpretable without the setup information. Is it the thickness, viscosity or density of this additional layer that causes the observed effect in the experimental system?
Response: The experimental setup—including the description of the lower crust--has now been added to the main body of the paper. We believe the buoyancy of the weak lower crust helps inhibit the descent of the mantle lithosphere instability. i.e., it is likely the combination of density and viscosity that modify the behaviour of the model.
20) The grey scale inset diagram in the upper row of Fig 1 is interesting, but the reader is told only to interpret "wrinkle like features" (lines 127, 172) How does this image relate to the surface elevation maps? If these structures are modifying the surface, why do they not show in the topography map? Or are we looking at an integral view through the box?
Response: These images are from a top-view photograph of the crust at the end of the experiment. The features are the structures that formed in the crust due to the dripping mantle lithosphere pulling the crust inwards. We have clarified these points in the revised manuscript.
21) The lower set of frames (side view) are very hard to read. Better to show fewer frames or maybe remove the dark background from the velocity vectors. Do you want to show what these velocity fields look like? Because this presentation is not working well.

Response: We have brightened the figures, so they are easier to view. As mentioned for the previous reviewers, we have redrafted all the figures, including the model figures 2-4, to improve their clarity and presentation.

22) Figure 5 is difficult to interpret. Abbreviations on the figure should be explained in the caption, so it is unclear to me which part of that 3D system is the focus for the authors and how do to the tomographic sections relate to the processes described in the experiments.

Response: Figure 5 has been completely revised. The content was split into two separate figures: 5A is now Figure 1 and a thorough explanation of the tomography images and how it relates to dripping beneath the Altiplano Puna Plateau has been added to the introduction. Figure 5B is now Figure 5A. We have tomographic evidence that there are drips beneath the Altiplano Puna plateau where the Arizaro Basin is located. DeCelles et al. (2015) have also tracked the changes in the elevation of the Arizaro basin over the last ~20 My and have attributed these changes to the removal of the lithosphere beneath the basin via a drip. The changes in topography correspond to the topographical changes in our experiments (EXP-1 and EXP-3)

22nd Feb 22

Dear Ms Andersen,

Please allow me to apologise for the delay in sending a decision on your manuscript titled "3-D geodynamics of lithospheric drips beneath the Central Andes (Southern Puna)". It has now been seen by the 3 original reviewers, and I include their comments at the end of this message. As you will see, they all agree that this is a much-improved version, but they still raise several issues that must be addressed. In particular, please ensure that you provide a clear description of the modelling rationale and the used apparatus and discuss their errors, uncertainties and limitations.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joao Duarte, PhD
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Joe Aslin
Senior Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "3-D geodynamics of lithospheric drips beneath the Central Andes" submitted to Communications Earth & Environment by A. Julia Andersen and co-authors.

In this revised version of their original manuscript submitted to Communications Earth & Environment, the authors addressed most comments raised by the reviewers and improved the paper consistency considerably, notably on their model result interpretation and the applicability to a natural example, by focusing on the Arizaro basin in the central Andes, which can be better compared with their models. However, the manuscript needs improvement as indicated below.

1) Main comments:
Many points remain to clarify, notably on:
- the objective regarding the model parameter that is tested. It is still not clear which parameter has been varied.
- the description of the dripping process both in nature and in the models in the introduction and results sections, as its effect on deformation and topography is strongly dependent on time and space, and it is not described as such in the current manuscript (see line-by-line comments below).
- the clarity of the comparison with the natural case to make the conclusions of this study robust (see comment on Fig 1 and L219-224).
- (and for other points see line-by-line comments below).

2) Line-by-line comments and suggestions:

L27: Write "gravitationally unstable (thickened) MANTLE lithosphere" since the crust does not sink.
L43: "has not been uniform in morphology" is unclear. Do you mean "has not been constructed uniformly through time"?
L44-46: "geological and paleoelevation estimates indicate that the timing and mechanism of

topographic uplift and the tectonic deformation between the Puna and Altiplano plateaus are different". Do you mean that the cause for producing topography is different than the cause for producing deformation? This needs to be clarified.
L49-51: "various studies invoke lithospheric removal (and the associated mantle dynamics) to account conceptually for the widespread (non subduction related) magmatism, surface deformation, and relatively rapid uplift of the plateaus". Lithospheric mantle sinking would actually tend to drag down the lithospheric layers that sit above it during a first phase, creating a depression at the surface. So the dynamics is very time-dependent and it should be presented as such in the introduction. Then, a rebound can occur above the drip at a later stage. Also, mantle upwellings due to return flow can occur laterally away so the question is do we see a dynamic topography as a result of these mantle upwellings and how does it evolve through time. Thus, there is a strong dependance of time and space on the process, which should be considered when introducing and discussing the drip process. I note that this polyphase evolution is better described in the discussion with the help of Fig 6, but it is absent in the introduction and results sections.
L84: I suggest to write "SUB-LITHOSPHERIC mantle" to facilitate the differentiation with the mantle lithosphere.
L91 states "The main objective of the study was to understand the effect of varying rheology of the mantle lithosphere" but L94 states "We have chosen three representative experiments with the same rheology of the mantle lithosphere". This does not make sense obviously. Please make sure that the statements are consistent. From reading this paragraph, I still struggle to understand what parameter was varied to test its effect on lithosphere deformation and surface topography. This is a crucial point to clarify.
L105: What is the "2.5 cm diameter hemisphere" made of?
L106-107: What happens in between the time when the density anomaly is placed and the crust is placed (and the recording starts)? Does the anomaly start to sink? This should be described.
L113: Perhaps indicate that the mantle circulation process is a return flow.
L143: "PDMS" is unclear. It would help to write which layer it simulates. Also, the description of method 2 is very unclear. Please clarify.
L166-167: "there is relative uplift as the surface rebounds back from its initial very rapid subsidence". In such a study making use of the PIV technique, it should be possible to investigate the evolution of uplift through time, which would help to distinguish when the phase of rebound starts. Also, is this rebound an elastic or viscous rebound? I think it is important to precise this point, along with the timing of this process.
L179-181: You should actually write what the causal link is here (between the entrainment of lower crust material and slower drip descent).
L182-187: More details should be given on how the results (strain, topography) of Exp 3 compare with those of Exp 1 since these two models share a lot of setup similarities.
L219-224: "While the current positions of the inferred 219 anomalies may have been offset due to the mantle flow generated by the subduction of the Nazca plate beneath the South American plate, the surface projection of them suggests that they are approximately located beneath the basins situated above, and as such they may be interpreted as lithospheric drips that have controlled the tectonic evolution of the Arizaro basin, the Salar de Atacama (basin) and the Salinas Grandes.". This statement currently represents a weak point in the current version of the paper. Indeed, there is no certainty that the tomographic positive anomalies are located right below the topographic depressions. Perhaps this is why the authors write "suggests that they are approximately located beneath the basins situated above" because they are not convinced of the correlation in space. This point should be improved by making cross-sections that could be added to figure 1 to better compare the position of the tomographic anomalies in relation to the position of the depressions. If

there is an offset, then the issue must be discussed more extensively. Note that mantle flow due to subduction will actually drag the drip towards the trench (and slab), not away from it, due to trench-ward mantle flow in the upper mantle (e.g. Strak and Schellart, 2021, JGR).
L314-324: It is nice to see a description of the setup and scaling. The effect of the sidewalls on the mantle flow should be discussed.
L363-364: As commented by reviewer 2, the software makes use of the stereo photogrammetry technique to compute topography.
L372: What is the grain size of the silica particles and e-spheres? What is an e-sphere???
L373: The cohesion is negligible but it is certainly not 0 MPa. Please verify this statement.

3) Figures

Fig 1: This is a nice figure. But the goal here is also to try to help a quantitative comparison with the models. I suggest to produce cross-sections to see where the tomographic anomalies are located in relation to the topographic depressions and highs.
Fig 2-4:
Panels A) The colour scale used to show the cumulative strain is not ideal since it is not linearly increasing with increasing cumulative strain. It would greatly help to choose a different colour scale that does not have colours that look similar to the colour scale representing elevation. The type of strain should be given (shortening?). The elevation map uses a discrete colour scale but the colour scale indicated on the right uses a continuous colour scale. This should be corrected to ensure consistency.
Panels B) The velocity field looks very noisy for this experiment so it becomes hard to distinguish between noise and true velocities. Is it possible to reduce this noise?
Panel C) The y-axis indicates "Position", but it is unclear which object it refers to.
Fig 5: It would help to indicate the phase of subsidence and the phase of uplift due to viscous rebound directly on the graph in panel A).

Note that Strak and Schellart (2014, EPSL) would be a better reference using the PIV technique than Schellart and Strak (2016).

Kind regards,

Vincent Strak

Reviewer #2 (Remarks to the Author):

A previous version of this manuscript was assessed by three reviewers, including me, who provided extensive feedback. Based on these reviews the authors have made extensive changes, which has resulted in a refocusing of the paper from a rather general presentation of drip tectonics to a specific application of their experimental findings to the Arizaro basin in the Puna Plateau, which, based on stratigraphic, structural and geophysical evidence, previous authors have suggested formed by a mantle downwelling. Consequently the title has been changed and overall the manuscript is greatly improved.

Unfortunately, I think this paper still has some way to go before it is ready to be published. I have provided an annotated PDF with extensive detailed comments and only summarise major points for consideration below.

1. The quality of the writing still requires improvement. Many sentences and or phrases are unclear - these are highlighted in the annotated PDF. The authors also have a habit of separating two words with a forward slash (e.g., geological/surface, known/obvious, shortening/thickening). I'm not certain what style guide they are using, but it seems that they are asking the reader to choose between possible options. I suggest these should be remove or written out in full.

2. While I like the new focus on the Puna Plateau, the Introduction could set this up more convincingly. Lines 37-39 are very important in this regard but their meaning is currently unclear. It would better to change this section so that the aim of the paper is to test a hypothesis (using analogue experiemnts). Namely, that the Arizaro and other basins on the Puna Plateau formed and then were subsequently inverted due action of mantle drips. In this regard, other competing hypotheses for these basins should also be presented and discussed.

3. As requested in my original review, the authors now acknowledge better the previous experimental work of Pysklywec & Cruden (2004) (P&C). However, I also suggested that they compare and contrast their results with this previous work. Unfortunately, this has not really been done apart from tangential references to the way the drips were initiated in R&C versus their experiments.

4. One aspect that does come across more clearly in the revised version is that the mode of drip initiation has a major impact on the size and vigour of the resulting mantle drip. It is clear how Method 2 of drip initiation was carried out as it follows the technique detailed in R&C. Unfortunately, how Method 1 was implemented in practice remains very unclear. Was the hemisphere placed on top of the mantle lithosphere and then left to settle back to a flat surface; or was the hemisphere pushed into mantle lithosphere material; or was a hole excavated in the mantle lithosphere into which the hemisphere was inserted? Clarifying this is important because Method 1 produced more vigorous drips and surface responses similar to that of the Arizaro basin. It is also puzzling because some of R&C's experiments behaved in a similar fashion to the new Method 1 experiments and others were more like Method 2, despite the fact that R&C only used the Method 2 approach.

5. Following on from #4, it would be useful to learn about the geological basis or motivation for the two modes of drip initiation, and how this in turn might apply to the Puna Plateau setting. There is a brief attempt to do this in the Methods section but these needs to expanded and included in the main text.

6. The authors contend that by using Particle Imaging Velocimetry (PIV) and Stereo Digital Image Correlation (S-DIC) their results provide a "more detailed investigation of topography and strain" to previous work (e.g., R&C). I am not convinced that the measurements presented of surface strain or topography is any more "detailed" (i.e. precise) than in P&C. But this is actually impossible to assess because no information about the accuracy of the PIV and DIC analysis is provided. Nor are errors discussed.

7. The description of the PIV and SDIC techniques is still not very clear and should provide more detail (e.g., #6 above). Note that the terms PIV and DIC are often used interchangeably and their underpinning algorithms are the same. However, common practice is to use PIV for measuring velocity vectors in fluid flow and DIC for measuring velocity vectors and surface strain in deforming solids and granular media. Note that the Methods section makes no mention of how PIV was applied to the side view images to measure flow in the model asthenosphere.

8. In my previous review I questioned the significance of having a model lithosphere comprising a mantle lithosphere overlain by a thin brittle upper crust with no intervening ductile lower crust. I didn't find the new Methods section particularly helpful in clarifying this matter and, furthermore, this kind of important experimental detail should be discussed in the main text. There is large body of published geophysical and petrological work that suggest that the Central Andes contains a substantial thickness of very hot, in places partially molten, middle to lower crust - indeed this is one of the origins proposed for the Puna and Altiplano Plateaus, in a similar manner to the Tibetan Plateau. Disregarding such observations may be valid but the reasons for do so need to be more convincing.

9. Overall I liked the comparisons made between the experiments and the Aziro basin (e.g., Fig. 5) and the general similarity of the subsidence curves for the basin and Exp. 1 is encouraging. However, it is important to also compare the WIDTH of the Arizaro basin to the surface depression formed in the experiments. Looking at Fig. 5c, the basin in Exp. 1 appears to be ~5 cm across, which scales to ~200 km in nature. This is very much larger than the Arizaro basin, which is ~50 km across and ~75 km long. This discrepancy and the lower amount of subsidence in the Arizaro basin need to be fully acknowledged and discussed.

10. Following #5,6,7, 8 and 9 the authors make little attempt to identify and discuss the limitations of their experiments and their application to the Puna Plateau. I think adding such discussion in the appropriate sections would help to make the paper stronger and more convincing.

I am sorry to be providing yet another critical review. However, I hope these comments help the authors to make continued improvements to their interesting work.

A.R. Cruden,
Melbourne

Reviewer #3 (Remarks to the Author):

I have reviewed an earlier version of this manuscript last June, so I think I can keep my comments on this paper fairly brief. I do think that the paper describes some interesting analog experiments and I think that the experiments are relevant to the evolution of the Central Andes, and provide insights into the formation of several significant sedimentary basins in the region. I therefore recommend that you accept the paper for publication.

Most of the things that irritated me in the previous version of the paper have been fixed, but I would wish that the authors would also change the following, mostly minor, details:

1. line 48, pointless words: "Regardless of the timing and magnitude of tectonics.."
2. line 64, hard to interpret words: "plate boundary scale deformation. "
3. line 88, introduced a perturbation or anomaly (not instability)
4. line 127, explain how you measure radius in your method section - what do you choose as the points on the margins of the basin ?
5. line 129, "occurs around the periphery of the annular drip " its not really around the periphery is it? it is elsewhere in the box, off to one side typically.
6. line 131, 134, 356, Figs 2,3,4: strain is a tensor quantity, what aspect of the tensor do you refer to on line 131, 356 ? What are strain vectors ? These words give the appearance of the authors not understanding what strain is.
7. line 165 "this style of drip does not induce crustal deformation " It is not the style of drip that changed, but the way in which the initial perturbation is applied (as correctly stated on line 193). Hoogenboom and Houseman (Icarus, 2006) showed that if the low density crust is actually coupled to the deforming mantle, the question of whether the crust is deformed or just flexed depends almost entirely on the relative viscosity of lithospheric mantle and crustal layers. The authors do not seem aware that this has been understood for some time. Decoupling of the initial anomaly or decoupling of mantle and crust by a low-viscosity lower crustal layer would also inhibit crustal deformation.
8. line 210, The "our experiments agree well with observed geology" statement is always a bad way to start a discussion. Can you let the reader decide that ?
9. line 218, 223 (this is important). I could not identify on Fig 1 what the A' is associated with. It does not seem to be placed on a high velocity anomaly. Is the label in the right location ? The authors say that these anomalies are associated with the 3 basins indicated in Fig 1. A seems to be under AZ, B seems to be under AT, none of the 3 high velocity features appear to be under SG, and C seems to be underneath the Altiplano. There is just something of a disconnect between the words and the diagram which I hope can be cleared up.
10. line 227, In relation to a dense body detected by gravity, depth is a critical parameter, how deep is it thought to be ? I'd question whether the length and width of the body referred to here are consistent with the process described in this paper.
11. line 229: "This" is a bit unclear - I guess you mean removal of lower crust ?
12 . line 256: "the formation/reactivation of the Sierra de Macon ridge and basin inversion " When did that happen + citation.
13. lines 293-295: this is a very speculative statement. If something extends over the whole fore-arc of the subduction system, isn't it more likely it is a consequence of the subduction rather than the cumulative effect of a number of smaller scale processes ?
14. line 354: The "results" I guess you mean the images captured through the box from the sides; "integrated' is not the right word either if I actually understand the method. How about "superposed?
15. line 380: Last time I questioned why you introduce an apparently linear viscosity with the non-linear equation 1. You go on to say that viscosity and n are measured at a fixed strain rate (line 383). How can you measure n at a constant strain rate? Where does the n = 1.01 come from ? The authors appear not to understand what equation 1 implies - just remove n from the text, it makes no sense here based on the other information provided.
16. Figs 2,3,4. Caption refers to "red strain vectors" Do you mean displacements ? Else see point 6: strain is not a vector. This needs to be explained properly. Also, while the colour maps on the left use a discretized colour scale - maybe 6 or 7 different colours, the adjacent colour bar uses

continuous colours so it is quite hard to interpret numerical values off this mismatched colour bar.
Fix the colour bar.
Greg Houseman

3-D geodynamics of lithospheric drips beneath the Central Andes

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Abstract

The basin and plateau regions of the Central Andes have undergone rapid phases of subsidence and uplift during the last ~ 20 Myr in addition to internal tectonic deformation. Paleoelevation data and the presence of high velocity seismic anomalies beneath the Puna plateau suggest that these tectonic events may be related to lithospheric foundering. By using ~~3D~~ scaled analogue models and a high-resolution Particle Image Velocimetry (PIV) ~~technique,~~ we show how a gravitational instability of the mantle lithosphere developing into a lithospheric drip may form a circular sedimentary basin in the crust that undergoes subsidence and subsequently reverses to uplift, as well as internal crustal shortening. Model results reveal some drips may be "symptomatic" or "asymptomatic" in terms of their surface and geological manifestations, depending on the degree of coupling of the downwelling to the upper mantle lithosphere. Overall, the physical models suggest that the formation of the Arizaro basin and nearby Central Andean basins are caused by symptomatic lithospheric dripping events and emphasize the important role of non-subduction geodynamic mechanisms~~,~~ driving surface tectonics.

Introduction

Rapid and short-lived phases of crustal shortening and extension, topographic perturbations, and exhumation processes are interpreted as the geological manifestations of lithospheric foundering in which gravitationally unstable (thickened) lithosphere sinks/drips into the underlying sub-lithospheric (asthenospheric) mantle[1–4]. In addition to conceptual tectonic models, seismic tomography provides robust examples of drip-like high velocity anomalies beneath the Colorado Plateau[5,6], Great basin[7], Sierra Nevada region of California[8–10], Altiplano Plateau[11], Tibetan Plateau[12], Carpathians[13], and Anatolia[14]. This type of "drip tectonics" has been associated with magmatism beneath continental interiors, such as the Mongolian plateau[15], Siberia[16], ~~the~~ North China[17–19] and Wyoming Cratons[20]. Among these many examples, however, the surface tectonic response of the lithospheric foundering process is not well resolved since, apart from a few studies, geodynamic model predictions have not been tested in the context of direct regional geological or geophysical observations. Here we link quantitative analogue (laboratory) models with geological and geophysical constraints for the topographic-tectonic evolution of hinterland basins of the Central Andes to study the potential for an active lithospheric removal process.

The Central Andean plateau is defined by the Puna and Altiplano high plateaus (> 3 km elevation; Figure 1) and was formed by subduction of the Nazca plate beneath the South American plate[21–24]. Paleoaltimetric studies suggest that the Central Andean topographic rise has not been uniform in morphology, but rather was built during the Cenozoic through pulses of uplift[25–27]. Further, geological and paleoelevation estimates indicate that the timing and mechanism of topographic uplift and the tectonic deformation between the Puna and Altiplano plateaus are different[28,29]. The Puna is characterized by higher average elevation/higher relief topography including local isolated hinterland basins (e.g., Salar de Arizaro, Salar de Atacama) and distinct volcanic centres (Fig. 1). Regardless of the timing and magnitude of tectonics, various studies invoke lithospheric removal (and the associated mantle dynamics) to account ~~conceptually~~ for the widespread (non subduction related) magmatism, surface deformation, and relatively rapid uplift of the plateaus[3,30–33].

Figure 1 shows a P-wave seismic tomography model beneath the Puna Plateau surface. The locations of basins within the plateau have been marked (viz., Atacama (AT), Arizaro (AZ) and Salinas Grandes (SG)). Higher velocity/colder anomalies (A, A', B, & C) at ~200 km depth (within the mantle domain/lower velocity anomalies) have been interpreted as foundered lithosphere, formed following plate shortening/thickening during Andean orogenesis along three transects (P1-P3)[3]. Based on stratigraphic and structural work, DeCelles et al. (2015) suggests that the Miocene Arizaro (Fig. 1), an internally drained, 3.5 km deep oval shaped hinterland basin represents a geological/surface manifestation of a dripping lithosphere from beneath the overlying crust[34]. ~~The authors~~ contend that an event of rapid subsidence changing to uplift is documented in the basin's sedimentary record, but these motions are not consistent with characteristics of tectonic flexural response and local stretching. Further, crustal shortening in the Arizaro basin interior is well documented by folding and local thrust faults, but the basin is not bounded by known/obvious fault systems indicative of plate boundary scale deformation. Overall, this suggests ~~the role of~~ more regional geodynamic processes ~~that accounts~~ for the geological evolution of the Arizaro basin since the Miocene. Furthermore, local basin structural features (50-100 km wide) beneath other parts of the Puna Plateau, for instance the southern part of the Arizaro and the Salar de Atacama related to lithospheric dripping events have also been suggested[35,36]. Such studies advance evidence for the concept of lithospheric foundering/drips in the region, but the dynamical process(es) of lithospheric dripping and their role in driving local tectonics in these purported geological cases are uncertain.

~~In this work, 3D~~ scaled analogue experiments ~~are used~~ to investigate the complex structural/deformational response of the brittle (upper crust) to descending mantle lithosphere. The experiments build on a suite of analogue models from Pysklywec and Cruden (2004) that explored lithospheric instabilities and surface tectonics. Here we use techniques that permit a more detailed investigation of the evolution of topography and strain in the models so that the model findings can be interpreted in the context of active foundering in the Central Andes region (Fig. 1). As such, ~~the~~ work aims to clarify the link between mantle processes and crustal tectonics, and how such geodynamics may be interpreted with observed or inferred episodes of lithospheric removal.

Results

~~In this study, we~~ conducted a series of 10 scaled experiments in a plexiglass box. These models contained ~~a~~ mantle made of Polydimethylsiloxane (PDMS), a mantle lithosphere made of modelling clay and PDMS with varied ~~rheology~~ (density ~~&~~ viscosity), two different brittle upper crust materials (e-spheres/silica spheres or sand), and ~~a~~ case with a viscous lower crust made of modelling clay and PDMS. To control the size and location of a drip in the center of the experiment we introduced an instability in the mantle lithosphere using two different methods (Method 1 and Method 2) that evolved into distinct styles of removal. Details regarding materials, scaling, and experimental setup are given in the Methods section below. The main objective of the study was to understand the effect of varying rheology of the mantle lithosphere on ~~the~~ deformation in the brittle upper crust to build upon previous analyses in which variable rheological properties of the upper and lower crust were investigated[37].

We have chosen three representative experiments with the same rheology of the mantle lithosphere (material properties are outlined in Table 1) where we reconcile experimental findings with the geological and geophysical observations of the southern Puna region. Using ~~the~~ Particle Image Velocimetry (PIV) and Stereo Digital Image Correlation (SDIC) techniques, we ~~have~~ tracked crustal strain and evolution of topography and drip descent throughout the experiments.

1.1 EXP-1

In this experiment, the initial density perturbation (12.5 mm radius) in the centre of the model box develops as a Rayleigh-Taylor/gravitational instability; attached using Method 1 of drip initiation. In Method 1 (EXP-1 and EXP-3), the mantle lithosphere was placed onto the mantle (PDMS) and then a 2.5 cm diameter hemisphere was inserted into the mantle lithosphere from the top of the layer. The experiment begins when the brittle upper crust is placed on top of the mantle lithosphere, and the cameras start recording.

Figure 2a-b shows the plan view evolution of surface topography ~~with~~ ~~overlying~~ cumulative surface strain and the sideview (photograph) evolution of the experiment, including displacement vectors from 0-40 hours, which scales to 25.2 Myr total time in nature. By 10 hours, a cylindrical shape drip has developed and is actively descending (green vectors). In response to the downwelling, upward return flow (red vectors) have evolved around the drip akin to a small-scale mantle circulation process (Fig. 2b). By 20 hours (h), the drip undergoes necking (thinning) while its rate of descent has accelerated (Fig. 2c). Note that the downward pointing green vectors have greater magnitude around the base of the drip where the drip volume and the viscous tractions are larger than the sides and the neck. This suggests rapid descent at the base (drip head) compared to the neck. The red vectors in the mantle (PDMS) surrounding the drip demonstrate upward motion as the drip displaces adjacent material. After 30 h, the descent has terminated, and the drip head hits the bottom of the tank. Figure 2c demonstrates that the descent rate is marked by two distinct stages increasing between 7-15 hours, and then a phase of consistent decrease in descent rate.

The map of surface elevation shows that an approximately circular surface basin develops as a response to the pull of the dripping lithosphere on the overlying crust (Fig. 2a). The core of the basin shows ~ -1.5 mm of subsidence (approximately 6 km in nature). The downward force exerted by the drip on the overlying crust is maximum at the start and decreases as the experiment continues owing to progressive down-dip extension/necking. As the experiment progresses, the radius of the circular basin is ~6 cm at its widest point and decreases to ~1.5 cm (~75%) with progressive shortening of the surface crust and loss of dynamic support. Topographic uplift (> 1 mm) occurs around the periphery of the annular drip as lower density mantle material replaces higher density mantle lithosphere.

Crustal strain is 0% at 0h (Fig. 2a). By 10h the strain is increasing as the surface is pulled down and inwards by the drip as indicated by the vector magnitude and directions. By 30 h, we observe the highest accumulation of strain (approaching 15%) right above the drip with little change in magnitude and direction of strain vectors between 30h and 40h despite further increase in surface elevation. The high crustal strain manifests as wrinkle-like linear features that developed in the central section of the model with shortening and folding of the upper crust (see Fig. 2; 40h frame).

1.2 EXP-2

In EXP-2 an alternative method (Method 2) was used to initiate a drip to explore the effective coupling of the unstable mantle lithosphere (while all other model parameters remain the same as EXP-1). In Method 2, the same size hemisphere as EXP-1 was inserted into the PDMS rather than the mantle lithosphere and then the mantle lithosphere was placed/attached on top akin to the method described in Pysklywec and Cruden (2004).

The lack of horizontal crustal convergence and accumulation of strain in the crust indicates that the lithospheric drip was not well coupled to the upper mantle lithosphere compared to EXP-1 (Fig. 3a). Figure 3b shows an overall dynamic evolution of the drip in EXP-2 that is like EXP-1, although the volume of mantle lithosphere involved in the drip is significantly reduced. By 20 h the drip has travelled approximately half the depth of the box and we see strong downward motion (green vectors) with the greatest velocity occurring at the bottom of the drip. The red vectors show outward and upward motion of the PDMS away from the drip as it displaces surrounding mantle material. The drip initially descends slowly before the rate increases at ~7 h - ~15 h where it begins to slow again as the drip approaches the bottom of the box (Fig. 3c). It reaches the bottom of the tank by 30 h (Fig. 3c) but the downwelling of mantle lithosphere persists down the neck of the drip until the end of the experiment. The neck (stretched portion) of the drip is approximately one third of the width in EXP-1. There is less lithospheric removal/foundering from beneath the crust in comparison to EXP-1, which further supports the drip was not well coupled/attached to the mantle lithosphere.

Figure 3a shows that a circular depression initially develops in the central part of the box as a topographic expression above the incipient downwelling. This depression formed very rapidly--over a few minutes in the time between model construction and before (unfortunately) the ~~start of the~~ imaging system. The horizontal strain vectors at 0 h indicate low (<2%) strain inward, and this does not change over the span of the 40h experiment. There is no appreciable surface strain measured in the crust (40h frame) despite the active mantle dynamics below, showing that this style of drip does not induce crustal deformation (e.g., horizontal tectonics). As the drip moves downward and the neck thins, there is relative uplift as the surface rebounds back from its initial very rapid subsidence, and the planform of the basin narrows (Fig. 3a) The topography measurements show that a small depression remains in the centre of the experiment throughout (0.5 mm deep).

EXP-3 (Addition of a Lower Crust)

In this experiment, we kept all model parameters the same as EXP-1 but included a viscous lower crust ($\eta = 2.12 \times 10^4$ Pa s & ρ=1120 kg/m^3) between the brittle upper crust and mantle lithosphere. A drip was initiated using Method 1 with the same size instability as EXP-1 and EXP-2. The overall dynamic evolution of the drip in EXP-3 is like EXP-1, which is evident in Figure 4c where the drip is shown to descend in two stages. However, it evolves at a slower rate than EXP-1 (Figure 4c). For instance, by 20 h the drip has only travelled ~85 mm compared to EXP-1 where the drip had descended ~125 mm. At 30 h (Fig. 4b) the drip starts to grow, and the rate of descent increases rapidly (Fig. 4c). The neck of the drip is wider in the side view than that of EXP-1 and EXP-2, approximately two times the width of that in EXP-1. The weak lower crust that is included in EXP-3 is entrained downward by the descending drip. This is not visible in the sideview images but was discovered during the post-experiment dissection of the model.

Figure 4a shows that a large well-defined depression develops in response to the lithospheric instability. Strain vectors indicate inward convergence of surface crust throughout the experiment. The buildup of strain in the crust as it converges yields similar wrinkle like/linear features to EXP-1 (Fig. 4; 45h frame), with the highest cumulative strain reaching 20%. Zones of the crust are extended (beginning at ~10 h) on the margins of the basin as material is pulled inwards ~~in the finite volume model box~~.

Symptomatic vs. asymptomatic drips

In all the experiments, the lithospheric drips are associated with vertical surface displacements (e.g., some subsidence and/or uplift), however, not all drips caused appreciable shortening and accumulation of strain in the upper crust. This behaviour stems from differing initial conditions and allows for classification of a drip as either “symptomatic” or “asymptomatic”. A symptomatic drip is well coupled to the entire mantle lithosphere (and partially the crust): It sinks through the mantle and pulls the upper part of the mantle lithosphere inward and horizontally, producing an accumulation of strain (shortening) and the formation of structures like fold-and-thrust ridges in the upper crust. These drips tend to generally remove more material from the lithosphere. EXP-1 and EXP-3 can be classified as symptomatic drips (Figs. 2a and 4a). There may be other factors controlling these symptoms in the crust (such as thermal and compositional), but we did not explore these variables in this study.

In contrast, an asymptomatic drip responds with initial subsidence, but is less developed throughout the entire mantle lithosphere and consequently not as efficiently coupled to the crust. Accordingly, crustal strain is subdued and there may be little to no ~~tectonic damage to~~ the crust despite the active mantle dynamics below. EXP-2 is an asymptomatic drip where lithospheric removal produces ~~the~~ transient subsidence and topographic recovery/uplift~~,~~ but ~~there is~~ no tectonic deformation ~~to~~ the crust.

Discussion

Our findings from the analogue experiments agree well with the unique lithospheric/mantle structure and key features of the tectonic evolution of hinterland basins in the Southern Puna Plateau over the last 20 Myrs[32,33] (Figure 5). Below, we describe how our primary experimental results may explain several geological and geophysical ~~anomalies~~ pertaining to the geodynamic evolution of the region.

Our experiments find that lithospheric downwellings are characterized by a sinking bulbous "drip head" attached to a thin neck, formed by the stretching of a viscous lithospheric instability. A seismic tomography model (Figure 1) shows that, there are four higher velocity (seismic) anomalies (~25°S and 65.5°W), labeled A, A', B and C surrounded by a lower velocity upper mantle domain ~~in~~ the southern Puna region. While the current positions of the inferred anomalies may have been offset due to the mantle flow generated by the subduction of the Nazca plate beneath the South American plate, ~~the~~ surface projection ~~of them~~ suggests that they are approximately located beneath the basins ~~situated above~~, and as such they may be interpreted as lithospheric drips that have controlled ~~the~~ tectonic evolution ~~of the Arizaro basin, the Salar de Atacama (basin) and the Salinas Grandes~~. Note that these basins and drips/colder velocity anomalies are similar in size and geometry (viz., oval shaped). The presence of these drips is supported by Götze and Krause (2002) who ~~also~~ suggest that ~~there is a~~ Central Andean gravity high, associated with a dense lithospheric body (400 km long and 100-140 km wide) ~~that~~ stretches across the Arizaro and Atacama basins[38]. Our experiments captured the continuous removal of the lithosphere, including portions of the (lower) crust. This is suggested for the Arizaro basin where the Moho depth is much shallower (42 km), inferred from receiver function and wide-angle seismic studies[39].

The diagram in Figure 5a shows a comparison between the deposition history of the Arizaro basin and the experiment results (EXP-1, 2 and 3)[29]. For the experiments, the plot represents the relative surface elevation change of a point (approximately the center of the experiment) above the dripping lithosphere as a function of time. The black curve for the Arizaro basin is from DeCelles et al. (2015), based on sedimentological work[40]. The results of EXP-1 and EXP-3 agree with the episodic (three stage) evolution of the relative elevation of the Arizaro basin. The stages are characterized by deflection of surface topography from negative (subsidence) to positive (uplift). In EXP-1 and EXP-3, Stage 1 corresponds to ~1 mm (~ 4 km) of accelerated surface subsidence that develops as a response to the initial (strong) downwelling effect of the drip (0 h-15 h in EXP-1 and 0 h-32 h in EXP-2) in which substantial lithospheric material is removed. This stage also includes the very initial growth of the lithospheric instability. In stage 2, the surface topography is recovered since there is a release of "drip-pull forces" related to the thinning/stretching of the drip (15 h-42 h in EXP-1 and 32 h-38 h in EXP-3). Stage 3 of topographic development is associated with additional isostatic surface uplift likely due to further removal of mantle lithosphere along the neck of the drip. During these stages we observe surface contraction where linear wrinkle fold-and-thrust belt like features develop (Figure 5c). In EXP-3, with the addition of a lower crust, the rate of surface uplift is greater, and the rate of subsidence is slower, compared to basin development in the previous experiments. The Arizaro Basin similarly shows the three stages of evolution (Figure 5a). DeCelles et al. (2015) estimates that the basin underwent an initial, ~2-3 Myr period of subsidence followed by an accelerated phase with a higher rate of sedimentation inferred from deposition deep water lacustrine facies (6-9 Myr). Subsequently, there has been deposition of evaporates and mudflats which leads to the interpretation that the basin subsidence rate decreased (e.g., topographic recovery of the surface basin) where deepening in the centre stops. The final stage where the experiment predicts crustal shortening corresponds to the formation/reactivation of the Sierra de Macon ridge and basin inversion (uplift).

The total length of time from the beginning of basin formation to present was ~18 Myr with a total uplift of ~1.2 km[29]. In comparing these subsidence/uplift curves (Figure 5a), EXP-1 ran longer than the current age of the Arizaro and the size of the instability scales larger, but given that the curves are so similar temporally, it implies that the Arizaro Basin may undergo further uplift in the future. EXP-3 exhibits a longer period of subsidence with a rapid rate of uplift. The conditions that control the growth rate of the instability clearly influence the related history of basin subsidence and uplift. The surface topography in EXP-2 behaves quite differently from EXP-1, EXP-3, and the Arizaro Basin. Stage 1 in EXP-2 shows gradually increasing surface elevation (0-10h) from initial depression (induced by the perturbation). The rate of uplift increases in stage 2 due to the accelerated descent of the instability (10-24 hours) and then ceases in stage 3 after the drip hits the bottom of the box at ~30 h. Notably, the instability/drip related subsidence was limited and rapid; only present during the very early phase of the experiment, before any downwelling was recorded at 0h, unlike the basin development and inversion in EXP-1 and EXP-3. Further, the topography progressively elevates in each stage although there is no shortening or accumulation of strain in the upper crust. This experiment suggests mantle lithospheric drips can create a basin without internal shortening and that topography may fully recover over the lifetime of the drip. The important implication then is that major geodynamic processes like these may have or will occur beneath the surface of the Earth (or other planetary bodies) without leaving indicating structures in the crust.

Our experimental findings of EXP-1 and EXP-3 where surface deformation is localized both by convergence/shortening above the drip and extension on the periphery of this area is consistent with the observed crustal tectonics in the Puna region. This is especially true for the Arizaro Basin and the internal shortening inferred from thrust faulting that develops in conjunction with the basin subsidence (see Figs. 5b and 5c for comparison). Based on our classification, the tectonic manifestation of the underlying mantle lithospheric dynamics suggests the drip beneath the Arizaro Basin is a symptomatic one. In both EXP-1 and EXP-3, saucer shaped basins and convergent/ridge-type structures were formed with the sedimentary deposits thickening inwards -similar to the Sierra de Macon in the central Arizaro Basin (Figs. 2a and 4a, 5b). Previous studies also show geological evidence for normal faulting/extension in the Pasto Ventura region (in the southern part of the Arizaro basin) that occurred by lithospheric removal. However, the authors suggest another/alternative dripping event linking this through the Arizaro drip in the north. According to the authors, such extension followed the phase of shortening[35]. Although we have not tested the effects of relatively smaller scale drips and since our models do not yield extension above the drip (rather the periphery), we speculate that a single drip- well imaged by tomographic anomalies- can account for recorded syn-convergent extension in the area. Further, even the tectonic effect of a single drip may extend over the fore-arc of the Andean subduction system in which normal faulting has been documented by the computed earthquake focal mechanism (e.g. Salar de Atacama)[36].

These analogue models support geological and geophysical findings in the Puna region of South America, indicating basin formation and tectonic evolution of these basins are driven by lithospheric dripping in the form of vertical tectonics. The results also suggest that asymptomatic lithospheric dripping may occur in some cases (not in the Central Andes) without inducing crustal shortening ~~and~~ the resulting basin ~~produced by the drip~~ may be shallow or fully recover its topography leaving little to no trace in the crust that this mantle lithosphere process occurred.

Methods

Experimental Setup

A plexiglass box with dimensions 25 cm × 25 cm × 21.5 cm was filled with polydimethylsiloxane (PDMS), an analogue for the mantle, and was left to rest until it was free of ~~any~~ air bubbles (approximately 14 days). A ~~composition~~ of modelling clay and PDMS ~~was mixed homogenously to~~ represent a 2 cm thick layer of mantle lithosphere. Plasticine was used to increase the density of the PDMS to create a higher density mantle lithosphere than the underlying mantle. Overlying the mantle lithosphere in EXP-3 was a lower crust ~~of thickness of 0.5 cm,~~ made of a different ratio of PDMS and modelling clay ~~that was~~ lower viscosity than the mantle lithosphere.

Lithospheric downwellings in the form of viscous drips develop naturally during experiments owing to the density contrast between the mantle lithosphere and underlying sublithospheric mantle (asthenosphere) and minor undulations of the thickness of the mantle lithosphere. However, to control the location and size of a drip and ~~accelerate~~ the sinking process, a localized perturbation ~~is initiated~~ in the center of the box. We introduce a 2.5 cm diameter hemisphere with the same density ~~of~~ mantle lithosphere to trigger ~~the~~ lithospheric instability (Figure 6a). We hypothesized that in nature the instability may correspond to localized high density lower crustal eclogites within or resting above the mantle lithosphere, formed by the gabbro-eclogite phase transformation. ~~The~~ lower crustal eclogitization ~~process~~ has been previously interpreted for the cause of lithospheric removal in the Central Andes while adding to the net negative buoyancy of the lithosphere[41].

Specifically, two different methods were used to initiate a point instability at the center of the mantle lithosphere (Method 1 & 2):

1. In Method 1 (EXP-1 and EXP-3), the mantle lithosphere was placed onto the mantle (PDMS) and then a 2.5 cm diameter hemisphere was inserted into the mantle lithosphere from the top of the layer.
2. In Method 2 (EXP-2), the same size hemisphere was instead inserted into the PDMS and then the mantle lithosphere was placed/attached on top akin to the method described in Pysklywec and Cruden (2004).

In both methods of model setup, the addition of the crust (upper and lower) was the last step. Further, we have discussed how these two methods of drip initiation became important to show how a drip evolved over time in the model in conjunction with the rheological properties of the mantle lithosphere and model design.

Petroleum Jelly was used to lubricate the edges of the Plexiglass box and reduce the drag forces on the mantle lithosphere when inserting this layer on top of the PDMS during model setup. These edges were fixed during the experiment thus an additional lubricant such as a mixture of petroleum jelly and paraffin oil was not neccessary[42]. The upper crust material was sieved on the mantle lithosphere to a thickness of ~2 mm in all experiments. Black sand particles are used as tracers to track movement in the crust. The plexiglass box is open, so the model has a free surface.

The monitoring equipment consists of three digital cameras (CCD, 11 MPx, 16 bit): two from above (*Image ProX* by *LaVision GMbH*, Göttingen, Germany) and one from the side (*Imager Pro* by *LaVision GMbH*, Göttingen, Germany) (Figure 6b). The recording of all cameras is synchronized and connected to a computer where the imaging software *DaVis 7.2* for the top cameras and *DaVis 7.1* for the side camera by *LaVision GMbH* records the evolution of the model at a frequency of 0.017 Hz (or one image every 58 s). The model is illuminated from above and from the side against a black background so the camera only records motion from the model. The PDMS is recycled and thus has small particles from the clay dispersed throughout that act as tracers whose displacement is detected by the camera and velocity calculated by the software. The nature of using external imaging to track the instability/drip at the center of the box, however, means that the results show flow velocities integrated from the front face through the box.

Particle Image Velocimetry (PIV) is used in the experiments to calculate cumulative strain at the surface. This technique uses optical image correlation techniques to visualize non-linear flow and deformation and has been shown in previous sandbox experiments to be a useful analytical tool[43–45]. Other studies have also used this technique or similar imaging techniques to compute mantle flow and crustal velocity fields[46–48]. In these experiments we used 2D PIV where the displacement field is created by cross-correlation of successive images at the given frequency and interpreted from marker particles that have been translated between time steps[45]. To interpret surface topography, the *DaVis* software employs the Stereo Digital Image Correlation technique.

Materials and Rheology

The dimensions and materials used in the experiments have been scaled for length (L), viscosity (η), and density (ρ) compared to natural estimated Earth values. Following other analogue modelling studies, we use a granular material as an analogue to the brittle upper crust[49,50]. This material is a composition of 37% fine grained silica spheres and 63% e-spheres that has no cohesion due to their granular nature (σ_0 = 0 MPa)[51]. The angle of internal friction (φ) was measured to be ~40° based on the angle of repose of the material in a conical pile and equating it to the angle of internal friction[52]. This assumption works for granular materials that are uniform in size, density and moisture content[53].

The mantle is a silicon polymer polydimethylsiloxane (PDMS), and the mantle lithosphere and lower crust are mixtures of PDMS and modelling clay. These materials are non-linear viscous (non-Newtonian), and their viscous rheology is defined by the power law:

$$\sigma^n = \eta\dot{\epsilon} \quad (1)$$

where σ is stress, $\dot{\epsilon}$ is strain rate, η is viscosity and n a material constant[37,54]. The η and n values for each viscous material were measured using a strain-controlled rheometer (Discovery HR-3 Hybrid) at a strain rate of 10^{-5} s^{-1}.

In nature, both thermal and metamorphic processes may change the composition of the mantle lithosphere by changing its density. This can play an important role in mantle dynamic processes, however, thermal effects are not easily incorporated into analogue models, therefore as an approximation to the Earth like rheological structure, we use discrete layers of differing materials as an analogue to temperature.

Scaling

For each rheological and physical property, scaling relationships were derived for: time, length, gravity, density, and dynamic viscosity. The length scale (L) was defined based on a model mantle lithosphere thickness l_m=20 mm. A teleseismic study by Heit et al. (2014) suggests a thin lithosphere beneath the Southern Puna Plateau with the lithosphere asthenosphere boundary (LAB) at 70 km – 90 km depth[55] (Figure 1). The upper crust in these models is thin to model the purely brittle part of the upper crust and geophysical studies have determined an elastic thickness of <10 km for the Central Andes[56]. In two of three experiments we assumed that the viscous lower crust was part of the mantle lithosphere for simplicity. Based on a mantle lithosphere thickness of l_n=80 km in nature, the length scale ratio is $L=l_m/l_n$ =2.50×10^{-7}. The density of the PDMS used for the mantle in the experiments was ρ_m= 1010 kg/m^3. Assuming an average density of the mantle in nature of ρ_n=3060 kg/m^3, this yields a density scaling factor of $P=\rho_m/\rho_p$=0.33. The experiments are gravity driven and the scaling ratio for gravity is $G=g_m/g_n$=1. The measured effective dynamic viscosity of the PDMS (η_m) is 1.9×10^4 Pa s, and scaling with an approximate viscosity of the asthenosphere (η_p) of 10^{21} Pa s gives a viscosity scale ratio $M=\eta_m/\eta_n$=1.50 ×10^{-17}. Using M, P, L and G; the time scaling factor (T) can be defined as $T=M/PLG=t_m/t_n$=1.82×10^{-10}. Using the scaling factor, 1.59 hours in the model is 1 Myr in nature. Table 1 lists the material properties for materials used in each experiment and the scaling factors. The n value for all viscous materials was ~1.01. The upper crust material in EXP-2 had a higher density (ρ) and a lower angle of internal friction (φ) than EXP-1 and EXP-3. This change was made to a few experiments from the full model set of ten experiments due to a lack of crustal deformation in some models. Further experiments revealed that the primary controlling parameter of deformation of the crust in the model was not the density of the crust, but rather the method used to initiate a drip (Method 1 vs. Method 2) and the viscosity of the mantle lithosphere. Owing to limitations in terms of suitable materials, some scaling in these types of experimental tectonic models (especially when the layers are thin) is exaggerated compared to actual geological expressions.

EXP-1	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times 10^{-7}$)	Model	Nature	SF	Model ($\times 10^{4}$)	Nature ($\times 10^{21}$)	SF ($\times 10^{-17}$)	
Upper crust	2	8	2.50	820	2485	0.33				40
Mantle lithosphere	20	80	2.50	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.50	1010	3060	0.33	1.50	1.00	1.50	
EXP-2	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times 10^{-7}$)	Model	Nature	SF	Model ($\times 10^{4}$)	Nature ($\times 10^{21}$)	SF ($\times 10^{-17}$)	
Upper crust	2	8	2.50	1100	3333	0.33				35
Mantle lithosphere	20	80	2.50	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.50	1010	3060	0.33	1.50	1.00	1.50	
EXP-3	Thickness			Density (kg/m^3)			Dynamic Viscosity (Pa s)			φ (°)
	Model (mm)	Nature (km)	SF ($\times 10^{-7}$)	Model	Nature	SF	Model ($\times 10^{4}$)	Nature ($\times 10^{21}$)	SF ($\times 10^{-17}$)	
Upper crust	2	8	2.50	820	2485	0.33				40
Lower crust	5	20	2.50	1120	3394	0.33	2.12	1.41	1.50	
Mantle lithosphere	20	80	2.50	1128	3418	0.33	2.92	1.95	1.50	
Mantle	173	692	2.50	1010	3060	0.33	1.50	1.00	1.50	

Table 1 | Experimental setup Rheologic properties of materials used in the analogue models with a comparison to nature. The scaling factor (SF) is included.

Author Contributions

JA, OG, and RP conceived the ideas and interpretations. JA and TS carried out analogue experiments and data analysis at the University of Toronto. The depiction of seismic subsurface data was generated by EŞU. The manuscript was written and prepared by JA, OG, and RP, with comments and input included from all authors.

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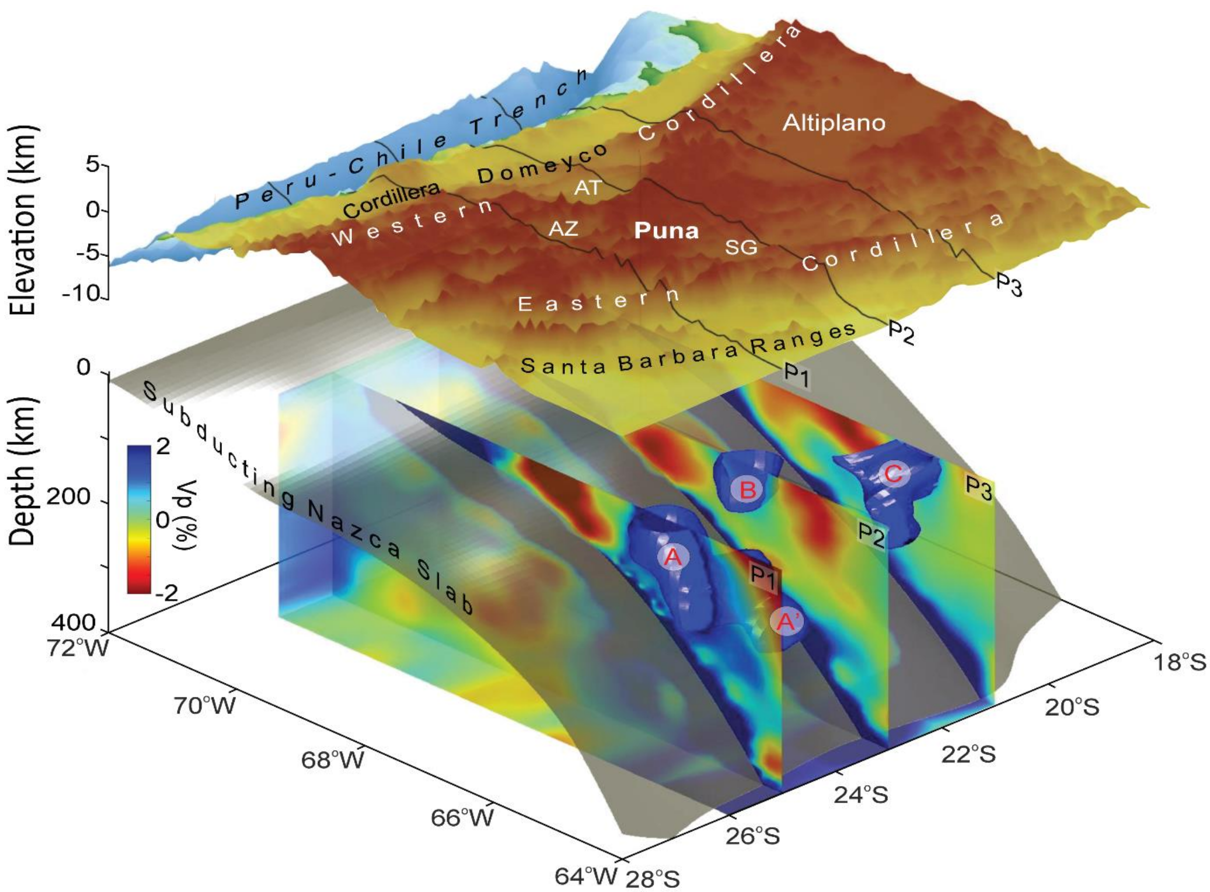

Figure 1 | Seismic Tomography of the Altiplano-Puna Plateau. The locations of the Atacama (AT), Arizaro (AZ), and Salinas Grandes (SG) basins are marked. High velocity P-wave anomalies are visible along three transects P1, P2, & P3. A & A' correspond to P1, B corresponds to P2, and C corresponds to P3. Diagram modified from [57].

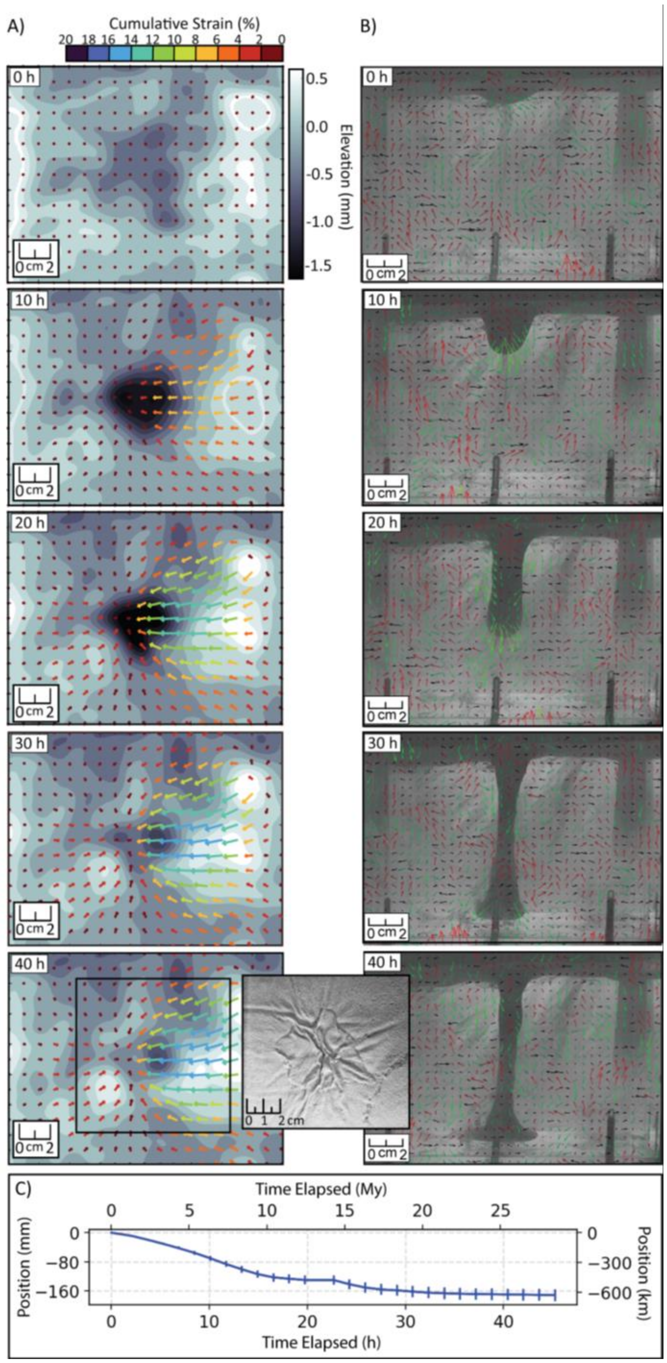

Figure 2 | Temporal Evolution of EXP-1 a) Plan view showing the evolution of topography from 0 to 40 h for EXP-1 with vectors depicting cumulative strain. Convergence toward the center occurs between 0 h to 40 h. Convergence stuctures are shown in the photograph at 40h with a possible extensional feature in the bottom right corner. Note the presence of an air bubble in the top right corner of the images. b) Below surface (sideview) evolution of the drip from 0 to 40 h. Green vectors track downward motion of the drip and red vectors the upward motion of the PDMS. c) Progression of the drip through the mantle starting at the base of the mantle lithosphere with corresponding scaled Earth values. The drip touched the bottom of the box at ~35h. Drip descent appears to cease at ~23h before sinking again.

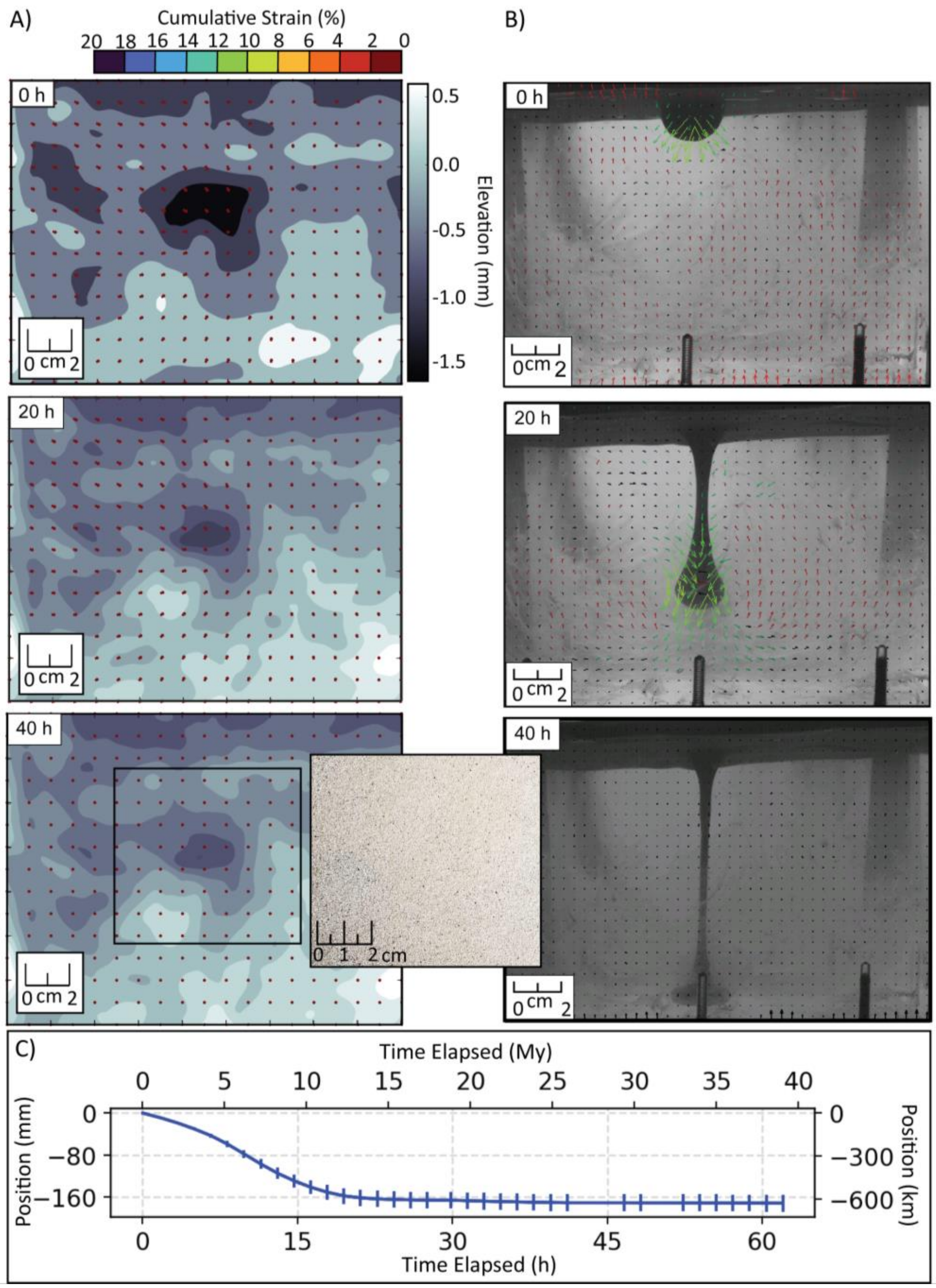

Figure 3 | Temporal Evolution of EXP-2 a) Plan view evolution of topography and cumulative strain over 40 h. Red strain vectors indicate some accumulation at 0 h. Photograph of the surface at 40h depicts no convergence in the crust. A basin 0.5 mm deep is illustrated at 40h. b) Evolution of drip (sideview) beneath the surface with downward green displacement vectors. Red vectors indicate upward displacement of the PDMS. c) Progression of drip beneath the base of the mantle lithosphere. The drip sank in a continuous path and touched the bottom of the box at ~30h.

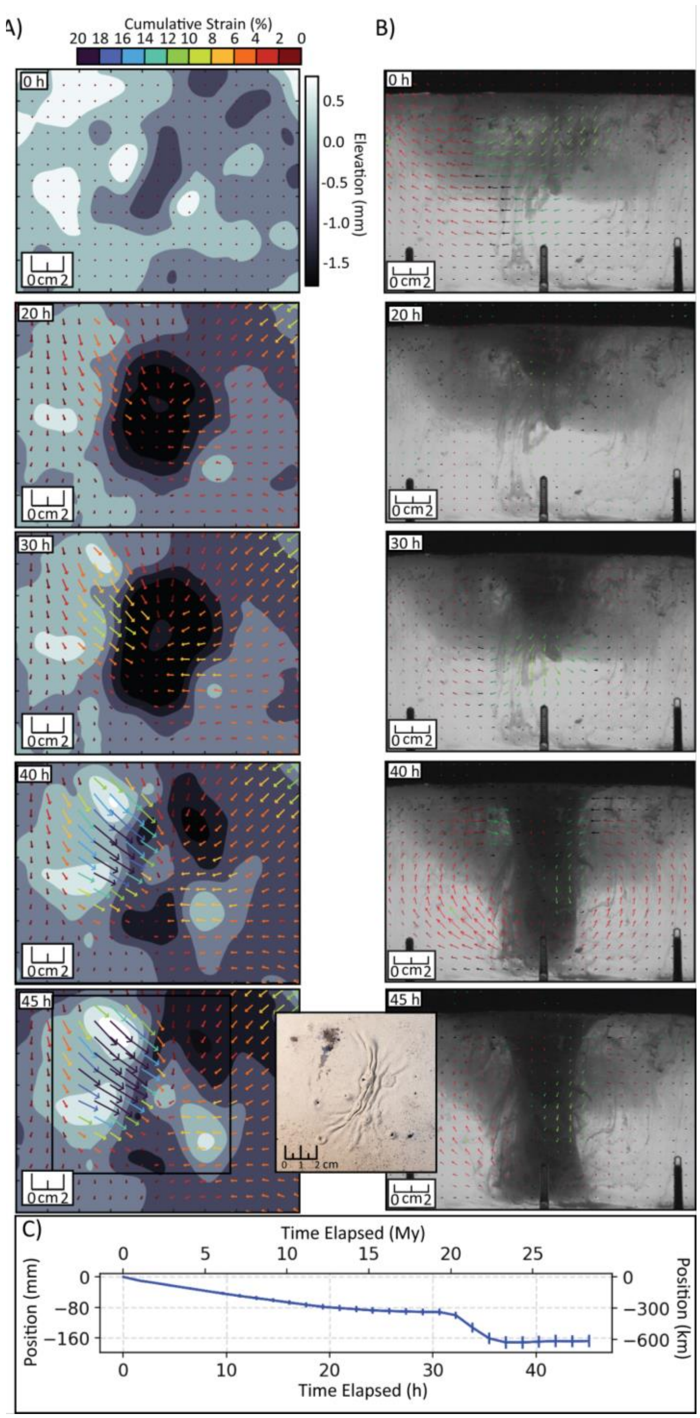

Figure 4 | Temporal Evolution of EXP-3 a) Plan view evolution of topography over 40 h with cumulative surface strain. Hour 10 has been removed. Extension was detected around 45h (top left of photograph b) Below surface (sideview) evolution of the drip. The cloud surrounding the drip is visible discoloration of the (polydimethylsiloxane) PDMS from the clay in the mantle lithosphere. Green vectors show downwward motion of drip while red vectors illustrate out and upward movement of the PDMS (mantle). c) Progression of the drip beneath the base of the mantle lithosphere. Drip progression temporarily ceases at ~32h before sinking again. The drip touched the bottom of the box at ~37h.

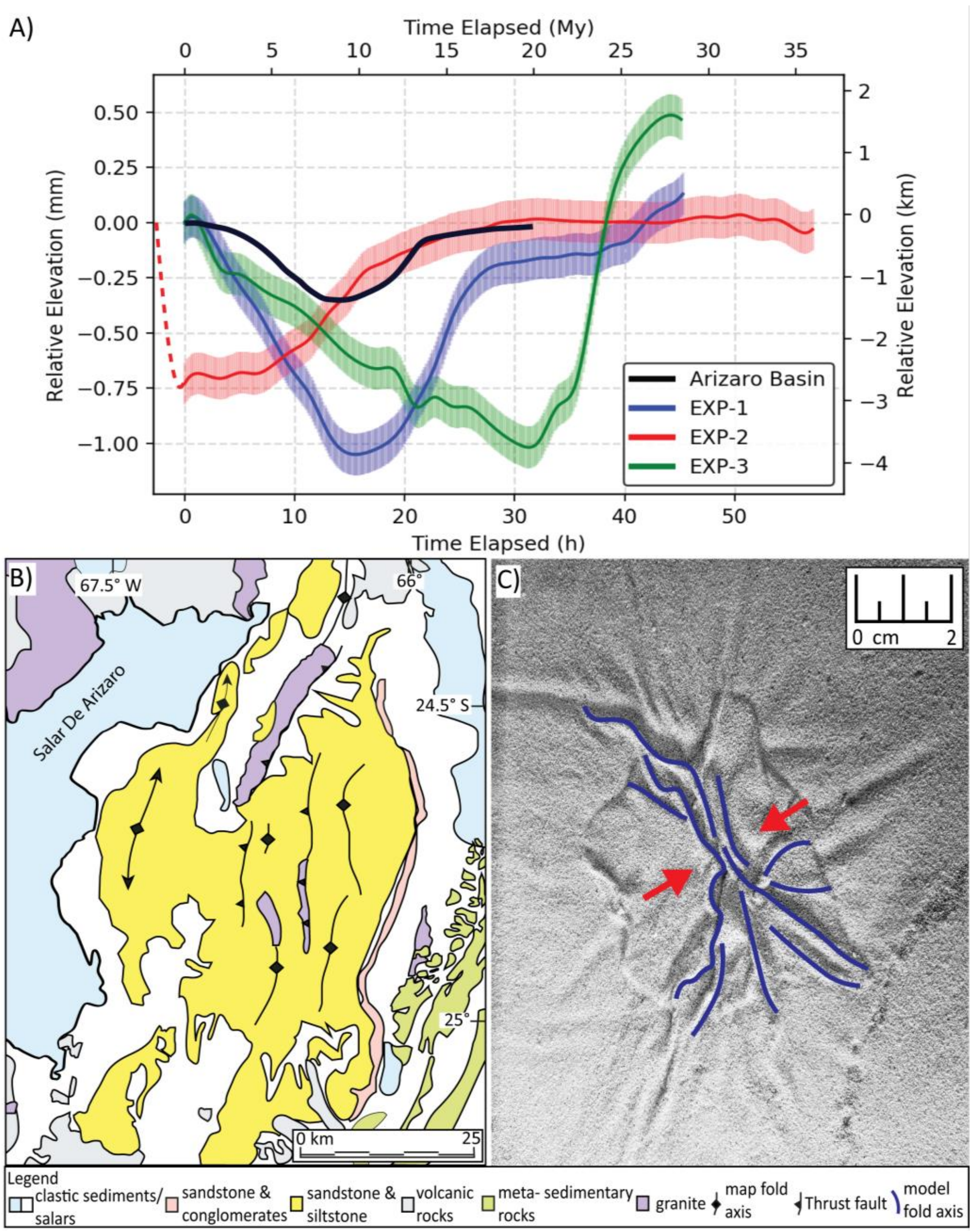

Figure 5 | Comparison of EXP to Arizaro Basin a) Relative surface elevation of EXP-1, EXP-2, and EXP-3 with elevation of Arizaro Basin from [29]. The dotted red line in EXP-2 depicts rapid subsidence before the recording began. This occurred in a short span of time between setting up the experiment on top of the PDMS and starting the camera recording (minutes). b) Geologic map of the Arizaro Basin demonstrating folding and thrust faults within the basin. Modified from[29]. c) Surface view of EXP-1 above the drip demonstrating folding and direction of shortening depicted with red arrows.

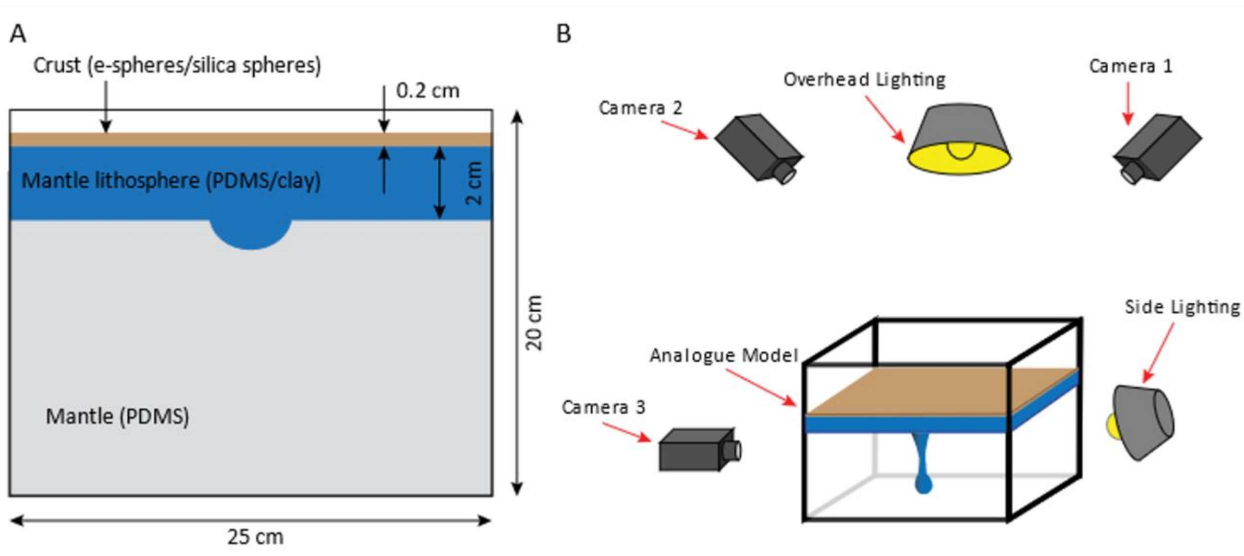

Figure 6 | Schematic illustration of experimental setup a) Analogue model dimensions and materials. Modified from[37]. b) Experimental setup and camera positioning.

Response to Reviewer comments

Line numbers correspond to the original document that was submitted for review (and correspond to the Reviewer comments) not the newly revised manuscript.

Response to Reviewer 1

Main comments:

As described in detail below, we have made a number of modifications for the revised manuscript to take into account and address these main comments by the Reviewer—i.e., objective/model parameter considered; description of dripping process in model and nature; comparison of the models with the natural prototype. The Reviewer's comments here overlapped with comments from the other two Reviewers as well. By addressing these constructive and helpful comments, we believe that the revised manuscript represents an improved presentation of our research.

2. Line-by-line comments and suggestions:

Line 27: We clarify indeed that a portion of the crust (eclogite) can also sink. As such, we leave as "gravitationally unstable lithosphere".

Line 43: Agreed that this is unclear. We have revised to "has not been uniform in time".

Lines 44-46: In the revision we clarified this to state: "the timing and mechanism of topographic uplift and tectonic deformation are different between the Puna and Altiplano plateaus".

Lines 49-51: As the reviewer notes, we describe this multi-phase behaviour in detail in the results and discussion (indeed, this is one of the main points of the work, and our work over the years). This is a lot of detail to get into in the Introduction, so we can't be too expansive there. However, based on the reviewer's suggestion, we have modified the passage to: "and time-dependent topographic evolution of the plateaus".

Line 84: As suggested, we specified to "sub-lithospheric mantle".

Line 91: Good point made by the reviewer. We have fixed this to properly explain: "to understand different styles of instability of the mantle lithosphere". We have also modified the paragraph to clarify the main parameter: "We investigate two different methods of initiating the lithospheric perturbation…The main objective of this study is to understand the different styles of instability."

Line 105: We clarified to "hemisphere of clay+PDMS".

Lines 106-107: As we already explained in the results (lines 161-162 of previous version), there is a delay in assembling the model and start of recording that can show in the results.

Line 113: We already explicitly state in the same sentence that this is a "return flow".

Line 143: We already defined explicitly what PDMS is (at the onset of the Results section) and explained that this is the sub-lithospheric mantle. In the revised manuscript, we have added much improved description of both Method 1 and Method 2, including an inset figure for Figure 6 to illustrate.

Lines 166-167: Yes—Figure 5 and our description of the results show the evolution of topography in time. We explained the rheology of the model: brittle crust, viscous (slightly non-linear) mantle lithosphere and sub-lithosphere.

Lines 179-181: The causal link is that buoyant (compared to mantle lithosphere) crust is being pulled down into the downwelling. We have revised the sentence to state "The weak and buoyant lower crust…is entrained downward" to clarify.

Lines 182-187: In the manuscript we directly compare EXP-3 to EXP-1 (in sentence starting "The buildup of strain …yields similarly features to EXP-1…"). The comparison of topography between EXP-1 and 3 is made in the Discussion section—several sentences make this direct comparison; these discuss the results and comparison from Figure 5.

Lines 219-224: This is a fair comment by the reviewer. In the revised manuscript, Figure 1 has been revised to clarify the spatial link between the mantle anomalies and surface features, with re-labelling, a map view tomography slice at 55 km to help locate features, and a line track to connect the subsurface feature C with the surface. This helps to clarify the explanation. In the discussion of this issue, we prefer to leave the interpretation of mantle flow in this complex subduction wedge region open. i.e., generally suggesting the motion of mantle flow, rather than a definitive direction of motion. We have also added the reference (Strak and Schellart, JGR, 2021) suggested by the reviewer in this discussion.

Lines 314-324: As recommended by the reviewer, we have added a passage to describe the effect of the sidewalls on the mantle flow. In the revised manuscript in the methods section, we state "These edges were fixed during the experiment and will produce drag…".

Lines 363-364: We have made this clarification, as noted by both Reviewers 1 and 2.

Line 372: In the revised manuscript (Materials and Rheology section), we have specified that the grain size of the silica particles is 500 micrometres. We have also explained that an e-sphere means Envirosphere (the brand name of the material).

Line 373: We have deleted the statement that the cohesion is exactly zero.

3. Figures

Figure 1: The Reviewer recommends modifying Figure 1 to clarify interpretation of the tomography features. We have redrafted the figure so that now it uses cross-sections to show where the subsurface tomographic mantle anomalies are. We have also modified Figure 1 to indicate clearly where the mantle features are relative to the surface features. For example, re-relabelled high-velocity anomalies; a dashed line to link anomaly C with the Salinas Grandes; and a map-view tomography slice at 55 km depth. The Reviewer's comment helps clarify the interpretation of the rather complex subsurface features.

Figure 2-4:

Panel A: To address these concerns on the colour scale of cumulative strain, we have tested many colour scales and the colour scale chosen was optimal to view the strain overlaying topography. For topography, we have modified the scale to a discrete colour scale (also commented by Reviewer 3).

Panel B: As mentioned in the response to Reviewer 2's points, the side velocities are a challenge in these experiments

Panel C: We have clarified what Position is "Progression of the drip through the mantle starting at the base of the mantle lithosphere (Position=0) with corresponding scaled Earth time values."
Figure 5: It would clutter the diagram to add a bunch (4) series of indicators for phases of subsidence and uplift. We think it's best to leave the figure frame as is, showing the subsidence and uplift in the data itself.

As recommended by the Reviewer, we have replaced the Strak and Schellart (EPSL, 2014) reference with Schellart and Strak (2016).

Response to Reviewer 2

Main Comments

1. We have gone through the annotated pdf to fix the numerous comments the Reviewer made for improving the quality of the writing. On the specific point about using multiple words separated by a forward slash (we admit there were a lot of these…), we combed through the manuscript and deleted these instances. We don't list every instance, but 18 of these were found and removed.

2. As recommended by the Reviewer we have re-written the passages of the Introduction that set up the purpose of the paper. The revised manuscript now states: "Here, using analogue (laboratory) models with geological and geophysical constraints, we test a hypothesis that the topographic and tectonic evolution of hinterland basins of the Central Andes are caused by lithospheric drip processes." Also, in paragraph three of the Introduction, we cite DeCelles et el. (2015) and state the alternate hypotheses they discuss for the anomalous topography and tectonics of the Central Andean basins. i.e., flexural tectonics and region extension.

3. Following the Reviewer's suggestion, in the revised manuscript, we have added some key direct comparisons of our findings to previous 3-D analogue modelling results of lithospheric drips (e.g., Pysklywec and Cruden, 2004; Pysklywec et al., 2010):

On a comparison on the topography from EXP-1: "These results show a more distinct circular basin formation than the point instability (P) experiments of Pysklywec and Cruden (2004)".

On the features of surface deformation: "This deformation is akin to the results of other 3-D analogue drip experiments (Pysklywec and Cruden, 2004; Pysklywec et al., 2010)."

On asymptomatic models: "These results with essentially no surface deformation are consistent with findings from Pysklywec and Cruden (2004) describing a non-published experiment (P4)."

On crustal entrainment: "Compared to Pysklywec and Cruden (2004; Figure 5), a smaller amount of crustal material is entrained with sinking mantle lithosphere."

4. The Reviewer asks for clarification on Method 1 vs. Method 2. We agree that this explanation in this version of the manuscript was not well written. In the revision we have entirely revised the description (in the Methods section):

1. In Method 1 (EXP-1 and EXP-3), the mantle lithosphere layer was placed onto the sub-lithospheric mantle (PDMS). Then, a 2.5 cm diameter hemisphere was pushed into the top of the mantle lithosphere layer and left to settle until a flat surface reformed. This created a perturbation at the base of the mantle lithosphere into the PDMS sub-lithospheric mantle.

1. In Method 2 (EXP-2), the same size hemisphere was inserted directly into the sub-lithospheric mantle (PDMS) and left until a flat surface reformed. Then the mantle lithosphere was placed/attached on top of the instability, akin to the method described in Pysklywec and Cruden (2004).

5. The motivation for the two methods of drip initiation was more to explore the results of two models with varying effective coupling between the perturbation and mantle lithosphere layer (i.e., Method 1 is well-coupled; Method 2 less coupled). There really wasn't a motivation specifically linked to observables with the Central Andes. We believe this approach is justified since geodynamic models tend to be significantly influenced by initial conditions and configurations (as emphasized by Reviewer 3, and our responses to them).

6. The Reviewer makes a fair point on a comparison of the relative detail or quality of the techniques used. In the revised manuscript we have re-written the passage so that it doesn't suggest that this new approach is necessarily more detailed:

"Here we use techniques that permit a detailed investigation of the evolution of topography and strain in the models so that the model findings can be interpreted in the context of active foundering in the Central Andes region (Figure 1)."

7. The Reviewer provided helpful information describing the difference between PIV and DIC. We have improved the methods (Lines 348-365) to provide accurate descriptions of these methods. We have clarified that DIC was used to compute crustal strain in the sand specifically in the top view and that PIV was used to compute velocity vectors in the mantle lithosphere and mantle visible from the side. We did not pay particular attention to the magnitude of these vectors, rather we used it as a tool to visualize flow in the mantle in response to the removal of the mantle lithosphere.

8. The Reviewer questions about the absence of the lower crust in two of the experiments. Based on the interpretations of Beck and Zandt (2002), we assume that lower crust participates in removal of the mantle lithosphere. As such, we approximate for our two model configurations that it is part of the dense mantle lithosphere that will be removed. We have expanded on this discussion in the revised manuscript (Methods section):

"In two of three experiments we assumed that the viscous lower crust was part of the mantle lithosphere. Seismic studies interpret that there is no evidence for the presence of mafic (high velocity) lower crust beneath the Central Andes, suggesting that this layer has been removed (Beck and Zandt, 2002). This interpretation is in accord with our model design in which the dense (possibly eclogitized) lower crustal material is part of the underlying mantle lithosphere that will be removed."

9. As suggested by the Reviewer, we have added a sentence to compare the width of the basin (model vs. observed for the Arizaro Basin):

"In terms of the width of the basin, our results scale to 60-240 km, within the range of the 75-100 km diameter of the Arizaro Basin (DeCelles et al., 2015)."

We note that this can be a tricky comparison since the present-day Arizaro Basin represents only whatever remnant of deposition remains of full deposition at the time (i.e., the basin margins have decreased with subsequent erosion). Also, the subsidence produced by the model is not necessarily completely filled to produce a replicate-sized basin. These issues are a bit convoluted to get into in the manuscript, but the range offered in the response above signals some bounds and uncertainty.

10. The Reviewer raised helpful points for improving the limitations of the models and application to the Central Andes. With our revisions described above, these points have been strengthened in this version of the manuscript for a more compelling presentation of our research.

Response to PDF annotations

Reviewer 2 kindly provided an annotated manuscript with a large number of suggestions and comments for improving the manuscript. To facilitate the response to all of these (and the Editor's review of our responses), we have included these as tracked changes in the revised manuscript document, rather than re-writing them all in this document. Please refer to the document to follow our changes. We very much appreciate the Reviewer's careful and detailed review of the document (recognizing the substantial time invested here in reviewing the work). The suggestions were very helpful for improving the description of the research, as well as clarifying some key points.

Response to Reviewer 3

1. Line 48: As suggested by the Reviewer, we have deleted these extraneous words.

2. Line 64: We have modified this to say: "but the basin is not bounded by known plate boundary scale fault systems".

3. Line 88: As suggested by the Reviewer, we have modified to "perturbation".

4. Line 127: The radius (diameter) was approximated on the surface topography figure. The margins of this measurement were the widest points of the circular feature in the image.

5. Line 129: We have clarified what we mean by the uplift: "uplift occurs around the periphery of the circular basin and right side of the box".

6. Line 131: We agree with the reviewer that our description of strain is a bit loose (with the incorrect use of "strain vectors"). In the revised manuscript we have corrected this to: "Crustal strain on the top surface…strain magnitude and directions".

7. Line 165: We have clarified the explanation, as recommended by the Reviewer: "that this perturbation and resulting drip does not induce crustal deformation (e.g., Hoogenboom and Houseman, 2006)".

8. Line 210: As recommended by the Reviewer, in the revised manuscript we have rephased the sentence where we previously suggested "…agrees well…" "Our findings from the analogue experienced are reconciled with the unique…".

9. Line 218: As we indicated above (in response to Reviewer 1), Figure 1 has been revised to clear up some things with the figure. We have renamed and relabeled on the plot what the mantle feature are (features A, B, C, D)—this fixes the uncertain location and label of A' from the previous version. Also, we have added dotted lines and a map view frame to clarify the spatial link between the mantle tomography anomalies and surface features.

10. Line 227: We agree with the context of the gravity results could be clarified. In the revised manuscript, we now write: "Götze and Krause (2002) suggest a Central Andean gravity high, associated with dense lithosphere (400 km long and 100-140 km wide; 10-38 km depth) that stretches…".

11. Line 229: As the Reviewer suggests, this sentence is a bit unclear. We have fixed the narrative: "Our experiments show continuous removal of the lithosphere including portions of the (lower) crust. This crustal removal is consistent with the Arizaro region where the Moho depth…".

12. Line 256: As recommended by the Reviewer, we have added the time of the event (~10 Ma) and citation (DeCelles et al., 2015).

13. Lines 293-295: We agree that this is speculative (and have stated as such). However, this specific discussion is part of ongoing debate on the potential role of lithospheric removal in the region—in particular, in the Metcalf and Kapp (2015) work. We believe our results may help shed new light on the discussion and respectfully we prefer to leave this passage in the revised manuscript.

14. Line 354: We have made both suggested changes from the Reviewer (and the text changes a bit with changes recommended by Reviewer 2).

15. Line 380: The original manuscript was incorrect to state that the rheometer was used at a fixed strain rate. We have deleted that phrase in the revised manuscript. The correct version of the equation (with n=1.01; very mildly non-linear) is still included.

16. Figs. 2,3,4: The author requests clarification related to descriptions of "strain vectors" (as in the point above). We have fixed these descriptions in the captions to: "arrows depicting cumulative surface strain", "Red strain arrows". The colour scales have been fixed in the revised figures so that they are consistent.

28th Apr 22

Dear Ms Andersen,

Your manuscript titled "Three dimensional geodynamics of lithospheric drips beneath the Central Andes (Puna Plateau)" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

João Duarte
Editorial Board Member
Communications Earth & Environment

Joe Aslin
Senior Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily modified the manuscript according to my comments.

I have just an additional minor comment: Ref 29 and 34 are identical.

Best regards,

Vincent Strak

Reviewer #2 (Remarks to the Author):

This is the second revision of this manuscript. I appreciate the effort the authors have put into addressing my suggestions on the first revised version. I am satisfied that all requested changes have been made and the manuscript is now of a sufficiently high standard for publication.

Other than a few typos and grammatical glitches, which should be picked up in a final editorial reading, I have no further comments to make.

A.R. Cruden
Melbourne

Reviewer #3 (Remarks to the Author):

I have previously reviewed this manuscript twice in its earlier forms. Last time I recommended that you accept it. I listed 16 details that I thought the authors should amend. They have accepted most of those comments and changed the paper accordingly. The most important of these was the connection between text and Figure 1, and that now is reasonably explained. The one or two comments that the authors disputed are not significant enough to hold up the paper (e.g. whether these materials should be described as non-linear or not) so we can agree to disagree.
I again recommend that you accept the paper.