Reviewers' comments:

Reviewer #1 (Remarks to the Author):

This manuscript presents experimental results that dwell deep into the non-Newtonian behavior as exhibited by dense suspensions and they mainly touch the granular suspensions. The results are highly interesting, very valuable to the research community, and of interest to physicists in general and especially to Soft Matter Physics. The stability analysis as presented would also be of interest to researchers in the field of Fluid Mechanics.

The manuscript was a pleasure to read, well written, concise, and crisp. The authors have done an excellent job with the experiments and then presenting the model, and eventually tying everything together into a beautiful manuscript. This manuscript should be accepted after the authors address my comments/suggestions.

I have a few questions and suggestions to the authors:

First of all, in this weird time, I hope my comments find the authors to be safe, healthy, and fine. I am sure they are enjoying their research and bits of summer left.

General comments:
1. I start from the conclusion, I find the authors trying to bring together different physics to the common link with the instability they have observed. However, one point the authors miss is to bring some more commonality/connection with the suspension rheology. Especially citing and pointing towards the work for example the experimental work by Boersma et al., the model by Richards et al. (that authors already cite), the recent simulation work by Sedes, Singh, Morris, JOR 2020. As I understand some features observed here should be similar to the fluctuations observed close to DST. Citing such relevant work should be good and relating to them in the conclusion could potentially strengthen the conclusions.

2. Another point is about the system size if the width of the system size is reduced. Can the authors speculate how would that affect the mechanism observed here?

3. How does interparticle friction play a role here? I understand that friction controls the frictional jamming point and hence the non-monotonic flow-curves. However, tuning roughness or surface chemistry can activate additional constrains. How would such constraints affect the overall instability as reported in this manuscript? Can authors touch this point in the conclusion?

Specific comments

1. Lines 45-48: These sentences can be confusing for a general reader. I would strongly suggest the authors mention Stokes number and provide a bit more basics about the relation between Reynolds number Re and Re_K. Is it straight forward to relate the two?

2. Line 72: The Reynolds number defined here could be defined earlier.

3. Line 91 & line 95: Authors mention the Reynolds number and conclude about the inertia. To further help the future reader, it could be beneficial to mention also about the Stokes Number.

4. Line 98 and WC model: Regarding Fig. 2(b) I wonder if the decrease in \tau_C vs \phi as observed by the authors related to the DST onset stress as a function of \phi_C as reported by various authors eg. Wyart & Cates, Singh et al. Guy et al.?

5. Line 145: Authors perform a linear stability analysis and it agrees well with experimental results. Is it possible that authors mention apriori why a linear stability analysis should hold?

6. Figure 4b and line 159: Is the positive feedback as mentioned in Fig. 4b related to change in pore pressure by authors in (PRL 117 (9), 098003 (2016))? Also, line 160 authors mention about anti-diffusion, is this feature related to negative susceptibility as observed by (Sedes, Singh, Morris, JOR 2020). Can the authors comment on these aspects?

7. Line 162: Authors mention Reynolds number Re_c, and critical shear stress \tau_c ⇒ should not the critical volume fraction \phi_c should also appear here?

8. Line 182: Authors mention stabilizing forces like "capillarity": is this sentence in the context of stabilizing the suspensions? Capillary forces could be attractive in nature, how would such attractive forces affect the instability? In terms of flow behavior they induce yield stress, but can authors speculate how such yield stress would affect the aspects touched in this manuscript?

Reviewer #2 (Remarks to the Author):

This paper addresses the flow of a shear thickening suspension down an inclined plane. Using recent theory related to frictional contacts, the authors are able to demonstrate the origin of the surface-wave instability.

Several months ago I read the paper:
Balmforth, N. J., Bush, J. W. M. & Craster, R. V. Roll waves on flowing cornstarch suspensions. Physics
239 Letters A 338, 479–484 (2005)

(Ref 19 in the present article). The authors described the same surface-wave effect 15 years ago but could not offer a theoretical justification. When reading that paper, I thought "surely in light of recent developments, this flow scenario can now be revisited and understood." Metzger et al have achieved precisely this.

Shear thickening theory has advanced in recent years to the point where it now ought to be applied to fluid dynamical scenarios to explain complex engineering flows. The authors of the present paper have demonstrated how this can be done.

I recommend publication in the current form, and would encourage the Editors to consider this important paper for a Cover article/Highlight etc.

Reviewer #3 (Remarks to the Author):

The paper titled 'Surface-wave instability without inertia in shear thickening suspensions' observe an instability in shear thickening dense suspensions which is not usual Kapitza instability observed in free surface flow. This instability arises only at volume fractions where DST is observed in the rheological measurements and shear rate has a negative slope. Further, critical stress, Reynolds number and basal stresses are calculated. These are very interesting and excited results and the authors also give a qualitatively explanation. The data is original and convincing.

The area of shear thickening has been quite active in recent years and this work takes an approach which remains unexplored though widely seen and definitely would be of interest to suspension as well as granular physics community and possibly have wider implications in other soft matter systems.

I highly recommend this paper for publication in nature Comm:phsyics.

I have few minor comments, which are given below.

There are few minor questions, which are as follows:

1) In line 59, the flow rate of suspension q is calculated at the bottom, does it remain constant or changes at particular \phi. I would expect it to vary since in DST, shear rate have a negative slope. If so would it change some feature of the behavior as presented in Figure 2C though it wont change the phenomena.
2) In line 83 the authors have written 'aging of the suspension', and in soft matter community the term 'aging' is used in another context which should be avoided for ST suspensions since it is a reversible phenomena where time of shear or pre shear do not play a role. My suggestion, To avoid effect of air bubbles or water evaporation on measurements, a freshly loaded…
3) In line 99, how the mean flow velocity uo is calculated since uo = q/ho, where q is instantaneous flow rate at the bottom of tilted plane.
4) z which appeared in line 134 is not defined.
5) The instability mechanism qualitatively explain the behavior, however, to me, it seems that it has been rushed. If the authors can give some insight about the \phi dependence. Also, it misses the causality of the behavior that whether increase in flow rate causes stress to increase beyond critical value or stress builds up such that it jams the suspension and shear rate drops. Both flow rate and stress are coupled thus I am not asking for a complete picture but some useful insight will be helpful for understanding and important ingredient to be introduced for simulations and theory.
6) In the movie it seems that the propagation speed varies in starting where the region are merging with the front also at 0.26-0.28 s one of the region disappeared. The starting effect could arise since a steady state is not reached. Does the later effect occurs due to the fact that the stresses in the region ahead and behind carry large stress and one of the regions would dissipate.
7) Do the spacing changes with \phi.
8) In rheological measurements, the slope is negative above 40 and instability is observed at 43 and above where looking at the fitting of W-C model clearly show that at slightly large stresses the suspension can jam thus my question is, does it bear some similarity to jammed and unjammed regions which co-exist and move like a solid phase seen recently observed in JoR 64,299 (2020) and Science advances 6 (16) eaay 5589 (2020). If so then would spacing be lower for higher \phi. Also could the width and height of the channel be determinant of spacing.
9) In corn-starch suspensions, density variations are reported does it play any role in free surface flow.

Reviewer #4 (Remarks to the Author):

This paper reports a novel instability mechanics from shear-thickening rheology of dense suspension. Their experiment identified that this mechanism is unique and different from the known Kapitza instability in the same geometry with Newtonian fluids. The surface wave develops because of the positive feedback due to the unusual stress-flow rate relation, i.e., higher stress makes flow slower. Deeper sections experience higher stress and flow slowly, while shallower sections experience the opposite. The experimental protocol is well considered to characterize this instability. The theoretical model provided in this article is minimum, but it captures the essential part of this instability. Therefore, I strongly recommend publishing this article almost as-is. I write some minor suggestions below.

(1) In Fig 1 (d), Delta h_top and Delta h_bot are plotted as functions of time. However, the flow

condition changes over time in this experimental setup. For a precise determination of the critical Reynolds number, the traveling time needs to be considered. If this is true, it should be noted so.

(2) In the section of “instability mechanism”, I cannot fully get the approximation for the given hydrostatic pressure form. The exact pressure is
P(x, z) = rho*g*(h(x')-z) with x'-x = (h(x')-z)*sin(theta). I think one possible approximation seems to be P(x, z) = rho*g*(h(x)-z)/cos(theta). I may be wrong, but please check the approximation again. I also suggest to add a brief explanation for the approximation.

(3) I suggest that the second axis label in Figure 4 should be z instead of h(x), and the depth of the fluid layer should be shown with h(x) in the figure more explicitly.

Answer Referee 1

"Surface-wave instability without inertia in shear-thickening suspensions"
by Baptiste Darbois Texier, Henri Lhuissier, Yoël Forterre, Bloen Metzger

This manuscript presents experimental results that dwell deep into the non-Newtonian behavior as exhibited by dense suspensions and they mainly touch the granular suspensions. The results are highly interesting, very valuable to the research community, and of interest to physicists in general and especially to Soft Matter Physics. The stability analysis as presented would also be of interest to researchers in the field of Fluid Mechanics.

The manuscript was a pleasure to read, well written, concise, and crisp. The authors have done an excellent job with the experiments and then presenting the model, and eventually tying everything together into a beautiful manuscript. This manuscript should be accepted after the authors address my comments/suggestions.

We warmly thank the referee for his/her very positive feedback.

I have a few questions and suggestions to the authors:

First of all, in this weird time, I hope my comments find the authors to be safe, healthy, and fine. I am sure they are enjoying their research and bits of summer left.

We are all safe, had a fine summer and truly wish the same to the referee.

Before addressing all the referee's comments below, we would like to mention that in the new version of the manuscript we propose to call the new instability 'Oobleck Waves'. Oobleck is the common name for starch in water. For the general public it refers to this fascinating substance which suddenly hardens under large stresses; it is also a reference to the comics *Bartholomew and the Oobleck, 1976.* We think that giving a more specific name will make the manuscript easier to follow and will also make futur reference to the phenomenon more convenient.

General comments:

1. I start from the conclusion, I find the authors trying to bring together different physics to the common link with the instability they have observed. However, one point the authors miss is to bring some more commonality/connection with the suspension rheology. Especially citing and pointing towards the work for example the experimental work by Boersma et al., the model by Richards et al. (that authors already cite), the recent simulation work by Sedes, Singh, Morris, JOR 2020. As I understand some features observed here should be similar to the fluctuations observed close to DST. Citing such relevant work should be good and relating to them in the conclusion could potentially strengthen the conclusions.

Answer: We thank the referee for suggesting these references.

Action: We now include them in the text line 134.

2. Another point is about the system size if the width of the system size is reduced. Can the authors speculate how would that affect the mechanism observed here?

Answer: We have performed preliminary experiments with a narrower channel (5 cm) without observing significant differences. The model is written in the limit of a thin liquid layer, which applies as long as the thickness is small relative to the layer width. For narrower channels, shear stresses at the side walls should affect the stability by screening the coupling between the basal stress and the thickness modulations of the layer. However, we have not addressed this effect, which is negligible here and is beyond the scope of our paper.

3. How does inter-particle friction play a role here? I understand that friction controls the frictional jamming point and hence the non-monotonic flow-curves. However, tuning roughness or surface chemistry can activate additional constrains. How would such constraints affect the overall instability as reported in this manuscript? Can authors touch this point in the conclusion?

Answer: The roughness and/or surface chemistry affect the critical stress τ^* at which frictional contacts are activated. The inter-particle friction coefficient also influences the value of ϕ_1, the critical packing fraction at which the suspension viscosity diverges at large stress. As the instability onset ($\mathrm{d}\dot{\gamma}/\mathrm{d}\tau = 0$) depends on those rheological parameters, one could delay or promote the instability onset by tuning these microscopic features.

Action: We now mention the role of the particle roughness, surface chemistry and inter-particle friction coefficient on the rheological parameters line 122-127.

Specific comments:

1. Lines 45-48: These sentences can be confusing for a general reader. I would strongly suggest the authors mention Stokes number and provide a bit more basics about the relation between Reynolds number Re and Re_K. Is it straight forward to relate the two?

Answer: For a Newtonian fluid in the laminar regime, Kapitza waves emerge if the flow Reynolds number $Re = \rho h_0 u_0/\eta$ is larger than a critical Reynolds number given by $Re_K = 5/(6 \tan\theta)$. Predicting this threshold is not trivial and results from a full linear stability analysis as given in refs 29-31. Here, the Stokes number is roughly equal to the particle Reynolds number since the density of the particle is close to the suspending fluid density: $St \sim (d/h_0)^2 Re$, where $d \sim 10\mu$m is the particle size. The Stokes number is thus $\sim 10^5$ times smaller than Re, showing that inertia is also negligible at the particle scale.

Action: To help the reader, we now provide the definition of Re earlier in the manuscript line 47-48. We also estimate the Stokes number and show that inertia is negligible at the particle scale line 104-106.

2. Line 72: The Reynolds number defined here could be defined earlier.

Action: We agree, we now provide in line 47-48 a definition of the Reynolds number based on standard quantities: $Re = \rho u_0 h_0/\eta$, where ρ is the fluid density and u_0 the mean flow velocity.

3. Line 91 & line 95: Authors mention the Reynolds number and conclude about the inertia. To further help the future reader, it could be beneficial to mention also about the Stokes Number.

Answer: This is now done, please refer to answer to question 1.

4. Line 98 and WC model: Regarding Fig. 2(b) I wonder if the decrease in τ_c vs ϕ as observed by the authors related to the DST onset stress as a function of ϕ_c as reported by various authors eg. Wyart & Cates, Singh et al. Guy et al.?

Answer: Yes. Our stability analysis in the vanishing inertia limit shows that the instability onset stress corresponds to $A \propto \mathrm{d}\dot{\gamma}/\mathrm{d}\tau = 0$, which is precisely the DST onset. The decay of τ_c with ϕ we observe is thus consistent with the decay of the DST onset with ϕ reported by those authors.

5. Line 145: Authors perform a linear stability analysis and it agrees well with experimental results. Is it possible that authors mention apriori why a linear stability analysis should hold?

Answer: Provided the flow is linearly unstable (not subcritically unstable), using a linear formalism is relevant for seeking for the instability onset. The mechanism we propose is found to lead to a linear instability, which legitimates, a posteriori, the use of the linear formalism.

6. Figure 4b and line 159: Is the positive feedback as mentioned in Fig. 4b related to change in pore pressure by authors in (PRL 117 (9), 098003 (2016))?

Answer: No, the feedback we refer to does not involve biphasic effects and transient dilatancy effects as discussed in PRL 117 (9), 098003 (2016). We consider the suspension as an effective homogeneous fluid having an S-shape flow curve. As sketched in Fig. 4, this single feature, together with its coupling to mass conservation and free surface deformation, is enough to explain the instability onset.

6b. Also, line 160 authors mention about anti-diffusion, is this feature related to negative susceptibility as observed by (Sedes, Singh, Morris, JOR 2020). Can the authors comment on these aspects?

Answer: Yes it is directly related. The dynamics becomes anti-diffusive when $\mathrm{d}\dot{\gamma}/\mathrm{d}\tau$ becomes negative at the base of the flow, i.e., when the 'stress susceptibility', as defined in this article, $\mathrm{d}\tau/\mathrm{d}\dot{\gamma}$ becomes negative.

7. Line 162: Authors mention Reynolds number Re_c, and critical shear stress τ_c ? should not the critical volume fraction ϕ_c should also appear here?

Answer: If by ϕ_c the referee means ϕ_J, indeed, the critical Reynolds Number and the critical stress depend on ϕ_J, see Equ. 7 and 8 of the SuppMat.

8. Line 182: Authors mention stabilizing forces like “capillarity”: is this sentence in the context of stabilizing the suspensions? Capillary forces could be attractive in nature, how would such attractive forces affect the instability? In terms of flow behavior they induce yield stress, but can authors speculate how such yield stress would affect the aspects touched in this manuscript?

Answer: Our use of the word ‘capillarity’ was somewhat ambiguous. We did not mean adhesive interparticle forces but the capillary stresses (Laplace pressure) arising when the free surface of a flow is deformed. Like gravity, such capillary forces work against the deformation of the layer interface. For a Newtonian fluid they have, like gravity, a stabilizing effect on the layer flow. Conversely, for a shear-thickening S-shape rheology, we expect that they have a destabilizing effect, for the same reason why gravity has a destabilizing effect in this case.

Action: We now clarify this point in line 202, where we write ”Our result could thus be extended to other forces that are usually stabilizing surface flows, such as capillarity forces arising from the free surface deformation”.

Answer Referee 3

"Surface-wave instability without inertia in shear-thickening suspensions"
by Baptiste Darbois Texier, Henri Lhuissier, Yoël Forterre, Bloen Metzger

The paper titled 'Surface-wave instability without inertia in shear thickening suspensions' observe an instability in shear thickening dense suspensions which is not usual Kapitza instability observed in free surface flow. This instability arises only at volume fractions where DST is observed in the rheological measurements and shear rate has a negative slope. Further, critical stress, Reynolds number and basal stresses are calculated. These are very interesting and excited results and the authors also give a qualitatively explanation. The data is original and convincing.

The area of shear thickening has been quite active in recent years and this work takes an approach which remains unexplored though widely seen and definitely would be of interest to suspension as well as granular physics community and possibly have wider implications in other soft matter systems.

I highly recommend this paper for publication in nature Comms-phsyics.

We thank the referee for his/her very positive review.

Before addressing all the referee's comments below, we would like to mention that in the new version of the manuscript we propose to call the new instability 'Oobleck Waves'. Oobleck is the common name for starch in water. For the general public it refers to this fascinating substance which suddenly hardens under large stresses; it is also a reference to the comics *Bartholomew and the Oobleck, 1976.* We think that giving a more specific name will make the manuscript easier to follow and will also make futur reference to the phenomenon more convenient.

I have few minor comments, which are given below.

1) In line 59, the flow rate of suspension q is calculated at the bottom, does it remain constant or changes at particular ϕ. I would expect it to vary since in DST, shear rate have a negative slope. If so would it change some feature of the behavior as presented in Figure 2C though it wont change the phenomena.

Answer: The flow rate is indeed measured at the bottom of the incline but it is also constant in time (or varying much more slowly than the relaxation rate of the flow), hence, at each time the flow rate is constant along the incline.

2) In line 83 the authors have written 'aging of the suspension', and in soft matter community the term 'aging' is used in another context which should be avoided for ST suspensions since it is a reversible phenomena where time of shear or pre shear do not play a role. My suggestion, To avoid effect of air bubbles or water evaporation on measurements, a freshly loaded?

Answer: By 'aging', we mean that the rheological properties of an acqueous cornstarch suspension changes over long times (typically more than one day). Starch particles may swell in water and/or bacterial growth may occur, which eventually affects the suspension rheology. To avoid these biases, a freshly prepared suspension of cornstarch is used for each measurement.

Action: We removed this part of the sentence now simply stating: "A new freshly prepared suspension of cornstarch is used for each measurement."

3) In line 99, how the mean flow velocity u_o is calculated since $u_o = q/h_o$, where q is instantaneous flow rate at the bottom of tilted plane.

Answer: As specified in our answer to 2) the flow rate is uniform along the incline plane. The mean flow velocity u_0 can thus be safely obtained from $u_0 = q/h_0$, with q measured at the bottom of the incline.

Action: We have added this sentence to the experimental protocol line 78-79: "Note that the flow rate is varied sufficiently slowly that at each instant the flow rate is uniform along the incline."

4) z which appeared in line 134 is not defined.

Answer: We thank the referee for pointing this out.

Action: x and z are now defined in fig.1 and fig.4, and also in line 151.

5) The instability mechanism qualitatively explain the behavior, however, to me, it seems that it has been rushed. If the authors can give some insight about the ϕ dependence. Also, it misses the causality of the behavior that whether increase in flow rate causes stress to increase beyond critical value or stress builds up such that it jams the suspension and shear rate drops. Both flow rate and stress are coupled thus I am not asking for a complete picture but some useful insight will be helpful for understanding and important ingredient to be introduced for simulations and theory.

Answer: Actually, our model quantitatively predicts the dependence of the instability onset on all control parameters of the problem, including the dependence on ϕ (see eq. 7 and 8 in the Supp Mat whose solutions are plotted in Fig. 2, solid red lines). The predicted decrease of τ_c (and therefore of Re_c) with ϕ is a direct consequence of Wyart and Cates' rheological law, in which the DST onset stress also decreases with ϕ (see Fig. 2 in their PRL 2014 paper). Physically, this decrease comes from the fact that less and less frictional contacts are needed to jam the suspension when approaching the maximal possible packing fraction ϕ_0.

Concerning the causality between the flow rate and stress, we are not sure to understand the question. In the current simplified approach, which uses a steady (instantaneous) rheology and neglects inertia, there is no anteriority between stresses and deformations and the link between the shear stress and flow rate is simply set by univoque relation $\tau_b(\dot{\gamma}) = \tau_b(q, h)$. However, considering a finite inertia or/and a small but finite deformation needed before solid contacts between the particles are established, might slightly alter the picture. These subtile points are clearly out of the scope of the present paper, but they will be addressed in a future and more technical work.

Action: We now provide a more detailed discussion about the ϕ dependence of the instability onset line 182-189.

6) In the movie it seems that the propagation speed varies in starting where the region are merging with the front also at 0.26-0.28 s one of the region disappeared. The starting effect could arise since a steady state is not reached. Does the later effect occurs due to the fact that the stresses in the region ahead and behind carry large stress and one of the regions would dissipate.

Answer: In the initial transient, when the leading front of the flow wets the incline, waves are found to merge with the front, which fully agrees with the theory since waves are expected to travel at about twice the mean flow velocity. However, we have not studied this point in details. All our measurements and modeling concern the perturbation of the steady flow, after the whole of the incline has been wetted.

7) Do the spacing changes with ϕ.

Answer: The wavelength of the instability does vary with ϕ. However, our model does not contain any large wave-number cut-off mechanism, so we cannot predict which (most unstable) wavelength should be observed. This constitutes an interesting question for future studies.

8) In rheological measurements, the slope is negative above 40 and instability is observed at 43 and above where looking at the fitting of W-C model clearly show that at slightly large stresses the suspension can jam thus my question is, does it bear some similarity to jammed and unjammed regions which co-exist and move like a solid phase seen recently observed in JoR 64,299 (2020) and Science advances 6 (16) eaay 5589 (2020). If so then would spacing be lower for higher ϕ. Also could the width and height of the channel be determinant of spacing.

Answer: It might be that the suspension locally jams at very high volume fractions. However, our measurements show that the deviation from the Kapitza trend occurs around $\phi_{DST} \approx 0.41$. This is fully consistant with the mechanism we provide, which does not rely on jamming, but applies to a suspension layer that flows entirely (from the top surface to the bottom).

9) In corn-starch suspensions, density variations are reported does it play any role in free surface flow.

Answer: Corn-starch grains are indeed denser than water. However, in the present case of rather large volume fraction ($\phi > 0.30$) and short duration of the experiments (< 1 min) sedimentation effects can be safely neglected.

Answer Referee 4

"Surface-wave instability without inertia in shear-thickening suspensions"
by Baptiste Darbois Texier, Henri Lhuissier, Yoël Forterre, Bloen Metzger

This paper reports a novel instability mechanics from shear-thickening rheology of dense suspension. Their experiment identified that this mechanism is unique and different from the known Kapitza instability in the same geometry with Newtonian fluids. The surface wave develops because of the positive feedback due to the unusual stress-flow rate relation, i.e., higher stress makes flow slower. Deeper sections experience higher stress and flow slowly, while shallower sections experience the opposite. The experimental protocol is well considered to characterize this instability. The theoretical model provided in this article is minimum, but it captures the essential part of this instability. Therefore, I strongly recommend publishing this article almost as-is.

We thank the referee for his/her very positive feedback.

Before addressing all the referee's comments below, we would like to mention that in the new version of the manuscript we propose to call the new instability 'Oobleck Waves'. Oobleck is the common name for starch in water. For the general public it refers to this fascinating substance which suddenly hardens under large stresses; it is also a reference to the comics *Bartholomew and the Oobleck, 1976*. We think that giving a more specific name will make the manuscript easier to follow and will also make futur reference to the phenomenon more convenient.

I write some minor suggestions below.

(1) In Fig 1 (d), Δh_{top} and Δh_{bot} are plotted as functions of time. However, the flow condition changes over time in this experimental setup. For a precise determination of the critical Reynolds number, the traveling time needs to be considered. If this is true, it should be noted so.

Answer: Here the flow rate is varied sufficiently slowly that, at a given time, it can be considered as uniform along the incline plane. We can therefore safely use the flow rate measured at the bottom of the incline and h_0 as measured mid-way to define the critical Reynolds number of the instability.

Action: We have added this sentence to the experimental protocol line 78-79: "Note that the flow rate is varied sufficiently slowly that at each instant the flow rate is uniform along the incline."

(2) In the section of "instability mechanism", I cannot fully get the approximation for the given hydrostatic pressure form. The exact pressure is $P(x, z) = \rho g(h(x') - z)$ with $x' - x = (h(x') - z) * \sin(\theta)$. I think one possible approximation seems to be $P(x, z) = \rho g(h(x) - z)/\cos(\theta)$. I may be wrong, but please check the approximation again. I also suggest to add a brief explanation for the approximation.

Answer: The expression of the pressure is valid in the slender slope approximation (longwavelength or lubrication limit), which gives the exact pressure field for the case of a steady uniform flow (invariant with x) and neglects second order terms when the thickness of the flow depend on x. In this limit, the vertical momentum balance in the direction perpendicular to the plane (z direction) reduces to an hydrostatic-like equation, $-\rho g \cos\theta - (\partial P/\partial z) = 0$, with the boundary condition: $P = 0$ for $z = h(x, t)$, yielding the expression $P(x, z, t) = \rho g \cos\theta(h(x, t) - z)$. This relation is classic for thin film flows and derived, for e.g., in ref 21 (eq. 30 at the lowest order in ϵ). Note that for parallel film flows, the iso-pressures are parallel to the free surface, which might explain the confusion.

Action: We have added line 152 a reference to the review of Craster and Matar on liquid film flows (ref 21), where the derivation of the hydrostatic approximation in the lubrication limit can be found. We also specify that the pressure profile is hydrostatic in the direction perpendicular to the plane (line 150).

(3) I suggest that the second axis label in Figure 4 should be z instead of $h(x)$, and the depth of the fluid layer should be shown with $h(x)$ in the figure more explicitly.

Answer: We have modified Fig. 4 accordingly.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have successfully answered all the questions and points raised by me. The manuscript is a great piece of work and should be accepted and eventually published as it is. Having said that one of the responses from authors has puzzled/confused me. This question though does not change my initial suggestion for publication:

Answer to question 7: I intended to ask the exact location of onset of instability is Re_c, \tau_c, \phi_c.
When I mentioned \phi_c, I meant the critical volume fraction for DST onset which is the relevant parameter here. Since the instability is related to \phi_c beyond which non-monotonic flow-curves are observed. Could the authors clarify their response? Though \phi_c is intrinsically related to \phi_J, but is not \phi_c more relevant here? I am just curious.

Reviewer #3 (Remarks to the Author):

The authors have answered all of my comments and queries. The word 'oobleck wave' seems appropriate.
I highly recommend it for publications and as one of the referees has already mentioned, the publication deserves to be on the cover.

Reviewer #4 (Remarks to the Author):

I recommend publishing this Oobleck Waves paper as this form.

Answer Referee 1

"Surface-wave instability without inertia in shear-thickening suspensions"
by Baptiste Darbois Texier, Henri Lhuissier, Yoël Forterre, Bloen Metzger

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Answer: We completely agree with the referee. The instability onset is reached at Re_c, or equivalently at τ_c, when the volume fraction $\phi \geq \phi_{\mathrm{DST}}$, the critical volume fraction for DST.

Action: We made this point more clear by writing: "We find that the volume fraction at which the new instability appears ($\phi \approx 0.41$), corresponds precisely to ϕ_{DST}, the volume fraction at which the shear-thickening transition becomes discontinuous"