Review 2

A review on "Understanding model spread in sea ice volume by attribution of model differences in seasonal ice growth and melt"

The study evaluates the three UK CMIP models performance in reproducing the Arctic sea ice volume seasonal cycle and discusses the inter-model spread using a simple surface energy balance model as a diagnostic tool. The subject of the study is certainly important as the large spread between the coupled climate models concerning the Arctic sea ice volume and extents results in the uncertainty of future climate projections in the Arctic and lower latitudes.

Understanding such a spread is crucial for the identification and/or development of more adequate models. A simple approach to explain the model spread is presented by the authors. It consists in using an idealized representation of the sea ice bulk and of the surface energy balance to provide a reference or a framework for the analysis of the more complex model results. It is shown that such an approach can provide some useful estimates of the sensitivity of the net surface heat flux on model variables allowing one to draw important conclusions on the relative role of various factors affecting the seasonal cycle of the sea ice cover. In general, the paper is well written and represents a significant input in the research in this area, and the subject is in the scope of The Cryosphere. The paper can be published after minor revision.

We thank the reviewer for their kind comments. We apologise for the time taken to produce this response. The reviewer asked a number of perceptive questions about timescales and the relevance of various physical processes. Constructing a satisfactory reply to these required detailed consideration of sea ice thermodynamics, and of atmospheric boundary layer processes. This in turn involved further examination of model data, of the literature, and of properties of the heat equation under changes in surface forcing, which we have attempted to describe in brief in our response. In addition, three weeks were lost due to COVID in the family.

General comments:

1. The authors propose to use a one-dimensional heat balance model. Obviously, in such a box-type or bulk model the sea ice dynamics is neglected. At the same time, we know that the sea ice volume shows significant geographical variability across the Arctic related not only to the variability of the surface heat budget terms, but also associated with the sea ice drift and deformation. Thus, one can expect that changes in the models physics and resolution can affect the sea ice dynamics and it can affect the sea ice volume and contribute to the spread between the models. The authors do not discuss such issues at all. How well do the considered models reproduce the 2D sea ice dynamics? Is there any spread between the models with respect to the sea ice dynamics? Can we expect that different representation of the sea ice dynamics, e.g. amount of the sea ice transport through Fram Strait, can affect the simulated sea ice volume and its annual cycle? I understand, that to some extent, averaging over the Arctic ocean solves this problem. However, this should be discussed in more detail. For example, it might be important at which step and how the averaging is done. As far as I understood, the simple model is used at each grid node and then the obtained results are averaged over the Arctic ocean. But at each grid node the advective flux of the sea ice volume is not negligible especially in some regions. Thus, the single-column approach has to be better justified.

Our simple model neglects the effects of ice dynamics by construction, because it is designed to diagnose the causes of differences in *surface heat flux*, rather than in ice volume tendency directly.

At the end of section 3.1 (paragraph beginning at line 161) we discuss the link between surface heat flux and ice volume tendency. We will try to make this discussion more prominent, because it is key to the setup of the simple model, and to the issues around ice dynamics and oceanic heat convergence, and because the reviewer asks two other questions about ice volume tendency below to which it is also relevant.

Basically, there are three processes affecting ice volume tendency (or ice growth/melt as we describe it in our study): the surface heat flux, the ice divergence, and the basal ice-ocean heat flux. Our study is principally designed around diagnosing the causes of differences in the first of these processes, the surface heat flux. In line 162 we state the Arctic Ocean average ice divergence in the three models, noting that it is small. However, our study would probably be more complete if we added an explicit 'ice divergence' line to Figure 6. To be clear, this would not be part of the simple model, which estimates the effects of various factors on surface flux differences. Rather, it would complement the induced surface flux differences, representing an additional factor impacting ice volume tendency. We suspect that it would appear quite flat on Figure 6, and appear near zero year-round on the given scale, but it would be useful to see this.

The treatment of the basal ice-ocean heat flux is more complex (see below).

2. The authors obviously neglect the heat flux from the ocean to sea ice which is especially important in the Atlantic sector of the Arctic. The authors should discuss the magnitude of this term in relation with the other terms in their simple model.

This is a good point, though it's more accurate to say that we neglect the oceanic heat convergence rather than the ocean-ice heat flux.

The ocean-ice heat flux has two sources: via oceanic heat convergence, or via exchange of surface heat flux in ice-free areas (e.g. polynyas, the marginal ice zone). Studies show (e.g. Serreze et al. 2007) that the ocean-ice heat flux in much of the Arctic displays a pronounced seasonal cycle, being near-zero in winter (except near the Atlantic ice edge, see below) and significant in size only in summer, particularly late summer. There is plentiful evidence that the ocean heat energy released in late summer derives from direct solar heating (i.e. the surface heat flux) rather than from oceanic heat convergence, both in the real world (McPhee et al., 2003; Perovich et al., 2008) and in models (Steele et al., 2010; Keen and Blockley, 2018).

Hence, in order to crapture the first-order effect of the ocean-ice heat flux, it is sufficient to account for direct solar heating of the ocean – which our model does in fact do. This is because we estimate the gridbox mean surface heat flux – the surface heat flux over all surface types, not just ice. Differences in solar heating of the ocean therefore show up as surface flux differences in our model. This has drawbacks – not all solar heating is going to be converted into ice melt, for example – but it means that a first-order driver of ice volume tendency is captured.

The oceanic heat convergence represents an additional term in driving ice volume tendency, although we note that as above some of this heat can be released directly from the sea surface rather than affecting sea ice. In HadGEM2-ES, HadGEM3-GC3.1-LL and UKESM1.0-LL the Arctic Ocean average oceanic heat convergence is 4.4, 3.8 and 3.9 Wm-2 respectively, but most of this heat is released very close to the Atlantic sea ice edge. We will include a discussion of the role of this term in our revised version.

3. My last comment is related to the applicability of a simple model that the authors use as a diagnostic tool. Obviously, various models can describe the sea ice thermodynamics differently than it is done in such a simple model. The actual sensitivity of the net flux in a particular sea ice model to the model variables can differ from model to model and would also depend on the considered time scale. How large can we expect such differences to be?

The reviewer is correct to point out that the representation of sea ice thermodynamics in our simple model is much more simplistic than is the case in the current generation of sea ice models (although it is quite similar to that of HadGEM2-ES, the CMIP5 model in our study). The most obvious example, to us, is the representation of the ice heat capacity: our simple model treats the ice as having no heat capacity, responding instantly to changes in surface forcing, whereas most (all?) CMIP6 ice models model ice heat capacity, with multiple layers with temperatures that respond on finite timescales to changes in surface forcing.

How would this affect the sensitivity of surface flux to the various variables considered? It's most relevant to the freezing season analysis, where we assume in equation (7) a uniform conductive flux through the ice, with the entire ice column responding instantly to a change in surface forcing. This is what actually happens in HadGEM2-ES, but in the two CMIP6 models the ice column would respond much more slowly. On a short timescale, the response of the surface flux to a large step change in e.g. downwelling LW would be representative of a thinner ice column (as only the top portion of the ice column would react quickly to the change in forcing). In other words, the surface flux would be *more* sensitive (on a short timescale) to a change in downwelling LW than is suggested by our method – because the damping effect of the ice thickness-growth feedback takes time to take effect.

However, there are several reasons why we do not think this is a major problem for our analysis. Firstly, this effect is weakest in the thinnest ice categories, which account for most of the heat loss (and hence *difference* in heat loss). In order to properly quantify the timescales at work here, we solved the heat equation for an ice column of thickness h in the case of a sudden step change in energy flux at the top surface. This approach is described in more detail in an appendix to this response, but the solution is described by an infinite sum of decaying harmonics, the first of which decays the slowest and whose exponent therefore describes the timescale over which the ice column temperature profile approaches a new linear equilibrium. Using this, for each model ice category we can describe the range of e-folding timescales (Table R1):

Category	**Thickness range**	**Range of timescales over which slowest harmonic decays by 1/e**
1	0-0.6m	0-21 hr
2	0.6-1.4m	21 hr – 3.5 days
3	1.4-2.4m	3.5 – 8.9 days
4	2.4-3.6m	8.9 – 18 days
5	3.6m-	18 days -

Table R1. Analytically-derived timescales of response to a sudden change in surface forcing, by ice category

In other words, in the two thinnest categories the zero-layer approximation describes the surface flux response quite accurately after a few days. This is still a sufficiently short timescale for our purposes – and most of the surface heat flux variability comes from these categories.

Secondly, it's useful to view the different variables as acting 'instantaneously' on surface flux in our simple model because this helps to refine causality. In our simple zero-layer model, ice thickness and downwelling LW act instantly on surface flux, and this means that we can define differences in each variable as separately contributing a 'proximate cause' of the surface flux difference. The key here is that it's not necessary that each acts on surface flux instantaneously, just that they act more quickly than they act on each other. In the thickest ice categories, the full effect of a change in ice thickness or downwelling LW is realised slowly, over weeks or even months. But the result of this is that the full effect of a change of ice thickness on downwelling LW, or vice versa, is realised even more slowly. The thermal inertia slows the whole system down, not just the effects on surface flux. Because of this, even in the thickest ice categories we can still resolve the causality to an extent: changes in downwelling LW have to act on the surface flux before acting on the ice thickness. Hence we can still say that we are resolving the proximate cause of the surface flux difference.

Lastly, in reality the atmospheric forcing will display many changes, upward and downward, over the course of a freezing season. In general, the ISF error due to the thermal inertia effect will be greatest following a rapid change in downwelling LW in one model relative to the other. When averaged over longer timescales (such as monthly means), we would expect the effect of rapid downwards changes to largely cancel with the effect of rapid upward changes, meaning that the overall error is small.

The arguments here are likely too long to include fully in our study, but we will try to condense this into a brief discussion of this issue towards the end, and hope that we have answered the reviewer's question to their satisfaction.

Specific comments

Line 193: To summarise, the weaker summer ice melt of the CMIP6 models relative to HadGEM2-ES is driven by a smaller upwelling SW flux from June – August. It can be the other way round – weaker melt results in a more negative SW flux due to larger sea ice area. How is it possible to identify the cause?

The sentence after the one quoted by the reviewer is relevant here: 'This [the smaller upwelling SW flux] is likely caused by ice area differences in July and August (the surface albedo feedback), but in June other surface albedo drivers are responsible.'

Put simply, upwelling SW differences driving ice melt differences, and ice melt differences driving upwelling SW differences via ice area & surface albedo, are not mutually exclusive. Both processes are almost certainly in action here. Nevertheless the purpose of this section is to trace the ice melt/growth differences back to an immediate, proximate, driver, and the upwelling SW differences are the obvious candidate. We note in the very same paragraph in which the upwelling SW differences are identified (beginning line 169) that ice area differences almost certainly explain the upwelling SW differences in July and August, but that in June they are not sufficiently large. This then motivates the surface albedo discussion in Section 3.3, and subsequently the whole ISF melt season analysis.

In summary, upwelling SW and ice melt differences almost certainly drive each other to a large extent; the question is to what extent do they drive each other, and at what times of the melting season, and this is one of the questions that our analysis addresses.

Section 3.3. Variables influencing surface albedo – I suggest to explicitly write the albedo parameterizations used in the models, so that the reader can clearly see what are the variables influencing albedo.

In analysing surface albedo differences, the first step is to consider differences in the area and albedo of the different surface types present in a grid cell. This is summarised by equation (5) in section 4, but this information may as the reviewer says be better placed here.

Following on from this, in analysing differences in the area and albedo of the different surface types, two parameterisations are relevant. The first is of snow area, for which the parameterisation are already quoted in the text. The second is of meltpond area, which is parameterised in HadGEM2-ES and whose formulation we will describe in greater detail. (In the CMIP6 models as stated, meltpond area is explicitly modelled and no parameterisation can be described). All other relevant variables are either explicitly modelled or are single parameters whose value will be stated.

Equations 1 and 2 – variables have to be explained

Apologies for this omission – variable definitions will be added.

Lines 220-224: "Despite the substantially higher snow thicknesses in HadGEM3-GC3.1-LL and UKESM1.0-LL, the increase in ice area in the newer models is muted…." It is not easy to follow because there is no reference formula for albedo. How does albedo depend on the snow thickness and ice area? It is not clear

This should have read 'increase in snow area' rather than 'increase in ice area', and will be corrected. The formulae for calculating snow area from snow thickness are given in equations (1) and (2) and hence the statement follows from these, and from Figure S1: the statement does not actually depend on the parameterisation of the surface albedo itself.

Lines 254-255: It is assumed that the net heat flux is a function of some model variables which are independent of heat flux. But this is not true on the considered time scales. Obviously, albedo and melt pond fraction would depend on the net surface heat flux already on a weekly and monthly time scales. Does it result in a limit of applicability of this assumption?

'… variables which are quasi-independent in the sense that while they affect surface flux instantaneously, they affect each other on finite timescales.'

This statement was not written very clearly; it is not an assumption, but a definition. We define two variables v1 and v2 to be quasi-independent if a change in v1 does not imply an instantaneous change in v2, and vice-versa. We will try to clarify this.

The example given in the text is of downwelling SW and ice area. If downwelling SW was to suddenly increase from 50Wm-2 to 100Wm-2, ice area would be unaffected on an instantaneous timescale. If, less realistically, ice area was to suddenly increase from 50% to 100%, downwelling SW would be unaffected on an instantaneous timescale. Clearly these variables are not truly independent – over time, each step change would provoke changes in the local modelled weather that would cause changes in the other model variable. But these changes would not take place immediately. This contrasts to e.g. **up**welling SW and ice area: if ice area suddenly changed from 50% to 100%, the upwelling SW would also change, instantaneously, in response to the surface albedo.

The reviewer's question relates to the relationship between the quasi independent variables vi and the surface heat flux. In the sense described above, a sudden change in any of the vi causes an instantaneous change in surface heat flux. However, a sudden change in surface heat flux does not **cause** a sudden change in the vi (though it might be evidence of such a change having occurred). Rather, it leads to changes to the vi over finite timescales, as the sea ice state, the lowest atmosphere layer, and the top ocean layer respond to the altered surface flux.

In diagnosing surface flux differences due to differences in the considered variables, we are attributing the causes of sea ice growth and melt only in a narrow, proximate sense. We use the quasi-independent framework because it helps disentangle causality: the proximate drivers of surface heat flux differences are those which act at the shortest timescales. As noted above, the ice heat capacity introduces a complicating factor to the 'instantaneous action' view, and we try to explain there why we do not think this invalidates the framework.

Equation 3 – superscripts MODEL1 and MODEL2 are not visible.

Reviewer 1 pointed this out too – this will be amended.

Line 261: I suggest to write explicitly how the ice volume balance is related to the surface heat flux. I wonder why the ice volume tendency is omitted in the simple model.

This is a good idea. The relationship between ice volume tendency (ice growth/melt) and surface flux is briefly discussed at the end of Section 3.1, but it would bear stating with an equation at this point in Section 4. The ice volume tendency is in a sense beyond the scope of the simple model itself, whose purpose is to estimate the causes of differences in the surface flux (which we treat as the principal driver of the ice volume tendency).

Equations 5 and 6 – I suggest different letters for the variable a_melt and the area fractions a_i. Maybe, use capital A for the area fractions, otherwise it is confusing.

This is also a good idea, which Reviewer 1 mentioned. Thank you.

Equation 6 – Fsw-net, t is missing

This will be corrected.

What is the exact definition of a_melt in Section 3 and how is Equation 6 obtained? It is hard to follow.

a_melt is defined as the fraction of time a grid cell is undergoing surface melting. It is possibly a confusing name – perhaps t_melt would be better – and the definition, which is rather obliquely stated in Section 3, will be restated more clearly here in Section 4.

To show how equation 6 is derived, we will explicitly expand equation 5 in terms of the area and albedo values described in the ensuing paragraph. When monthly mean values are considered, a_melt is functionally equivalent to the average area fraction of ice undergoing surface melting during a month (hence its confusing name). Hence it is closely related to a_meltpond, the average area fraction of a grid cell containing meltpond, in a way that depends upon the meltpond parameterisation. In HadGEM2-ES, a_melt and a_meltpond are related by two constants derived from the albedo parameters, 0.17 and 0.22 (representing the proportion of melting surface covered by meltpond over bare ice and snow respectively). In HadGEM3-GC3.1-LL and UKESM1.0-LL, they are related by the variable a_meltpond / a_melt (again, representing proportion of melting surface covered by meltpond), which can be calculated from model diagnostics.

Line 275: We can use this equation – specify which equation

Equation (5) – this will be stated.

Obviously, Equation 7 cannot be used for category zero (open water)

That is true, the derivation is only valid for the ice categories. A different, simpler, equation is used for the open water category, which we will include in our revision.

Line 351: How is it linearized and what is Bup?

At each model grid cell, Fsfc is linearised about $T_{sfc-0}(x,t)$, the monthly mean surface temperature at that grid cell averaged between the two models being compared. Bup then represents $\partial F_{sfc}/\partial T_{sfc}\,|_{T_{sfc-0}}$. In our simple model, we view all components of the surface flux except the upwelling LW flux as being independent of surface temperature – hence $B_{up} = 4\varepsilon\sigma T_{sfc-0}^3$.

The reasoning is described in greater detail in the answer to your next question.

Lines 355-357: First it is stated that Fatmos-ice does not depend on the surface temperature. Next, Fatmos-ice is identified as sum of SW net, LW down and turbulent fluxes. Obviously, turbulent fluxes do depend on surface temperature. It can be argued that LW down also depends on surface temperature on the time scale of the atmospheric boundary layer adjustment (which is not large), because the near-surface air temperature over sea ice is coupled to surface temperature.

Thank you for raising this. The answer to this question goes to the heart of the issue relating to timescales.

The purpose of this derivation is to separate the effects of snow depth and ice thickness from those of 'external' atmospheric thermal forcing on sea ice growth (or lack of) during the ice freezing season. However, there is no way to define the atmospheric thermal forcing such that it is completely, truly, 'external' (independent of ice thickness), as all elements of the climate system are related. Hence the definition above of 'quasi-independence' – we treat variables as independent if they only affect each other on finite timescales.

The trouble is that this is of course a simplification. Some timescales, while finite, are sufficiently short that it does make more sense for the purposes of what we are trying to do to treat them as being instantaneous. We argue that for a meaningful characterisation of induced surface flux differences, it makes sense to treat both the sensible heat flux, and also the downwelling LW flux, as being independent of surface temperature, and illustrate why we think this is the case with an example.

Consider an ice floe of thickness 1m in typical inverted (cold, clear) Arctic winter conditions. Assume the following conditions, which are fairly realistic: there is negligible SW radiation; the surface skin temperature is -20C; the air temperature at 2m is -18C; ambient wind conditions are such that there is a substantial sensible heat flux into the surface of 10 Wm-2; the downwelling LW is a fairly typical 190 Wm-2; specific humidity is sufficiently low that latent heat flux can be neglected for the purposes of this example.

From these conditions we can also derive the following: the upwelling LW flux is 228 Wm-2; there is net surface heat flux of -28 Wm-2, indicating cooling and growth of the ice column.

The central question which the ISF analysis is addressing, for each point of model space and time, is: how sensitive is this surface heat flux to differences in ice thickness, and how sensitive to differences in atmospheric thermal forcing? And the question we are trying to address in this example is: how does the treatment of the sensible heat flux affect the answer to the first question?

To judge this, imagine a second ice column of different thickness – let's say 1.5m – to be placed in conditions of 'identical atmospheric forcing'. The key question becomes then how that identical atmospheric forcing is defined, because not all possible definitions give useful characterisations of the 'dependence of surface flux on ice thickness, irrespective of atmospheric forcing'.

Under any meaningful definition of identical atmospheric forcing, a 1.5m ice column is going to transmit heat from the ocean to the atmosphere less efficiently than our original, 1m column, and that because of this the surface of the thicker column is going to converge to a colder surface temperature. Suppose it converges to -24C – this is realistic, as the temperature gradient of the thicker column is then still shallower than that of the thinner column, supporting a weaker surface flux. The upwelling LW flux is a weaker 214 Wm-2; what is the sensible heat flux?

Again, under a meaningful definition of identical atmospheric forcing, we can assume that the ambient wind conditions are identical over our two ice columns; the processes by which ice thickness affects wind speed are too complex, and act over too long a timescale, to be considered as an immediate response to ice thickness. Hence we can assume that the relationship between sensible heat flux, and surface-to-2m air temperature gradient, is the same between our two ice columns. And the 2m air temperature above the 1m ice column was -18C. Does this then mean that we should assume the sensible heat flux to the 1.5m ice column is three times larger than that to the 1m ice column, -30 Wm-2?

No, because the 2m air temperature would in reality respond very quickly to the change in surface temperature, and cannot therefore be considered to be independent of the ice thickness. In other words, the 2m air temperature is not a useful, meaningful diagnostic of atmospheric thermal forcing for these purposes, because it is affected at least as closely by the ice thickness as by the prevailing weather conditions. In a sense, it is almost as much a part of the sea ice system as is the surface temperature. Because of this, we have to view the sensible heat flux as varying extremely weakly with the surface temperature: small perturbations in surface temperature will produce similar perturbations in near-surface air temperature on a very short timescale.

To support this we have examined daily timeseries of surface temperature and sensible heat flux (amongst other atmospheric variables) in our evaluated models, for a few grid cells of the Arctic in winter 1990-91, chosen at random. Figure R1 below, a plot of the trajectory of surface temperature and (upwards positive) sensible heat flux for four grid cells in the Arctic Ocean in Dec 1990-Jan1991, bears out the argument above well: sudden drops in surface temperature are not systematically associated with sudden drops in sensible heat flux (Figure R1). Over the two months, there is no systematic relationship on any timescale; the variability in sensible heat flux is dominated by atmospheric conditions.

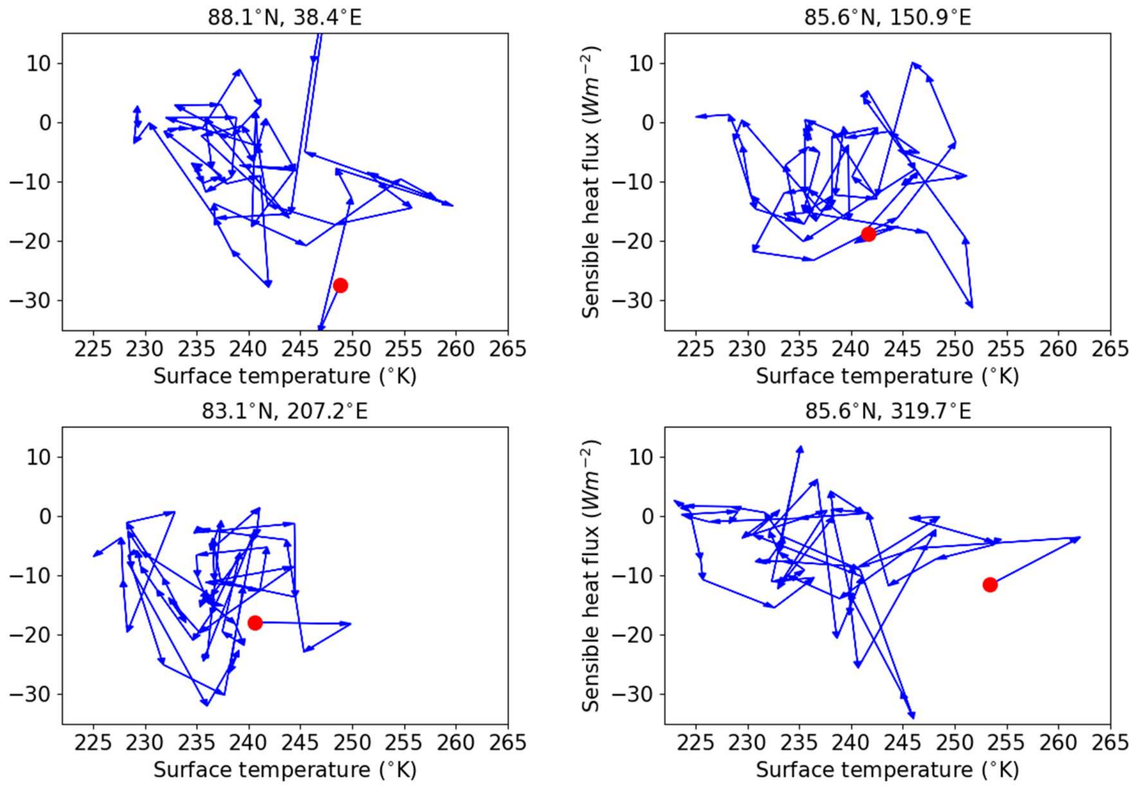

Figure R1. The evolution of surface temperature and sensible heat flux in four grid cells of the Arctic Ocean, Dec 1990-Jan 1991, in UKESM1.0-LL.

For the purposes of our simple model we feel it is therefore not unreasonable to make the approximation that the sensible heat flux is independent of surface temperature.

Regarding the downwelling LW, the answer is related to the previous one – indeed the adjustment of near-surface air temperature to a change in surface temperature will form part of a large-scale adjustment of the atmospheric boundary layer. The effect of this adjustment on downwelling LW will depend on multiple factors: how deep is the boundary layer, how high is the cloud base, what is the phase mixture in the lowest cloud layer. This includes many processes far too complex to include in our simple model.

However, some general remarks are possible. The LW response to boundary layer temperature changes alone is likely of second-order importance; changes in absolute humidity are likely small on short timescales. It is likely the cloud response that is most important. In a review of mixed-phase clouds in the Arctic boundary layer, Morrison et al. (2012) again present evidence of persistence of two distinct states (mixed-phase clouds and radiatively clear) with occasional rapid transitions between the two; this picture does not support substantial changes in cloud properties in response to short timescale surface temperature changes (in the real world, that is).

In models, the cloud liquid fraction is systematically, usually dramatically, underestimated over the Arctic, and mixed-phase clouds are very rare except in a few models (Pithan et al., 2014). Notably this is the case for all of our own evaluated models – we have evaluated liquid water fraction with respect to MODIS and all of our models have most cloud liquid water fraction in the 0-10% range which is not the case for MODIS (Figure R2, reproduced from West (2021)). We note that this is likely to mean the response of downwelling LW to surface temperature changes is actually lower in our evaluated models than in reality, due to the lower emissivity of ice particles relative to water droplets. Effectively, it is not obvious to us from the existing evidence that downwelling LW adjustment due to short-timescale response to surface temperature is of a significant magnitude (compared to the upwelling LW response, which is much easier to parameterise). Given this, and the complexity of satisfactorily accounting for this effect in our simple model, we think that our original decision to neglect this effect was in this case justified.

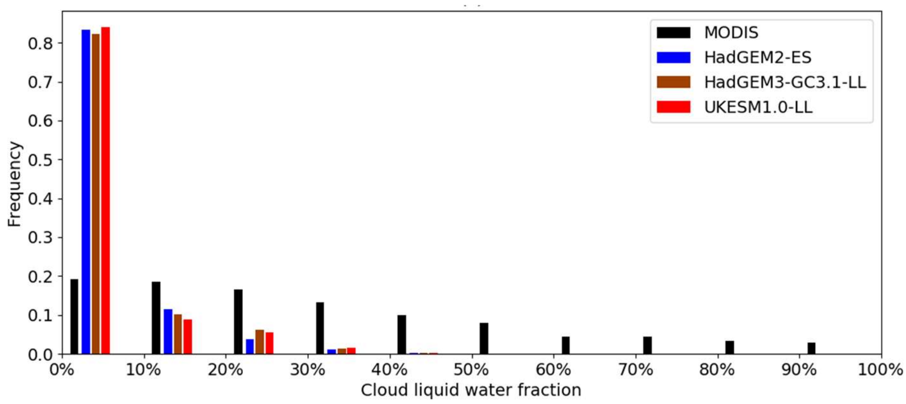

Figure R2. Histogram of cloud liquid water fraction over the Arctic Ocean modelled by HadGEM2-ES, HadGEM3-GC3.1-LL and UKESM1.0-LL, and measured by MODIS

Figure 7 and lines 445-450: It should be better explained how the curves in Figure 7 are obtained. Ice melt and ice growth are not described by the model in Section 4. Such terms are simply missing. So it is not clear at all how Figure 7 is obtained.

But the whole premise of the study is that the surface flux is the principal driver of ice melt and growth – hence we are diagnosing causes of surface flux difference, which we treat as synonymous with ice melt and growth difference. We will try to make this clearer.

Line 479: modelled sea ice and growth (??)

'modelled sea ice melt and growth' – this will be amended

References

Keen, A. and Blockley, E.: Investigating future changes in the volume budget of the Arctic sea ice in a coupled climate model, The Cryosphere, 12, 2855–2868, https://doi.org/10.5194/tc-12-2855-2018, 2018

McPhee, M. G., Kikuchi, T., Morison, J. H., and Stanton, T. P.: Ocean-to-ice heat flux at the North Pole environmental observatory, Geophys. Res. Lett., 30, 2274, https://doi.org/10.1029/2003GL018580, 2003

Morrison, H., de Boer, G., Feingold, G. et al. Resilience of persistent Arctic mixed-phase clouds. Nature Geosci 5, 11–17 (2012). https://doi.org/10.1038/ngeo1332

Perovich, D. K., Richter-Menge, J. A., Jones, K. F., and Light,B.: Sunlight, water, and ice: Extreme Arctic sea ice melt during the summer of 2007, Geophys. Res. Lett., 35, L11501,https://doi.org/10.1029/2008GL034007, 2008

Pithan, F., Medeiros, B., and Mauritsen, T.: Mixed-phase clouds cause climate model biases in Arctic wintertime temperature inversions, Clim. Dynam., 43, 289–303, https://doi.org/10.1007/s00382-013-1964-9, 2014

Serreze, M. C., Barrett, A. P., Slater, A. G., Steele, M., Zhang, J., and Trenberth, K. E. (2007), The large-scale energy budget of the Arctic, J. Geophys. Res., 112, D11122, doi:10.1029/2006JD008230

Steele, M., Zhang, J., and Ermold, W.: Mechanisms of summer Arctic Ocean warming, J. Geophys. Res.-Oceans, 115, C11004, https://doi.org/10.1029/2009JC005849, 2010

West, A., New methods for evaluating sea ice in climate models based on energy budgets, PhD thesis, https://ore.exeter.ac.uk/repository/handle/10871/127762 2021

Appendix: how to characterise timescales of ice temperature response to changes in surface forcing

Statement of the problem

Consider an ice column of thickness *h,* with base temperature $T(t,-h) = T_{bot}\ \forall t$. We ignore turbulent and shortwave fluxes, hence the principal forcing on the ice is the downwelling LW forcing $F_{LW\downarrow}(t)$, and the surface flux

$$F_{sfc} = F_{LW\downarrow} - \varepsilon\sigma T_{sfc}^4 \qquad (1)$$

where ϵ, σ and T_{sfc} denote emissivity, the Stefan-Boltzmann constant and surface temperature respectively.

By flux continuity also, $F_{sfc} = k_I \frac{\partial T}{\partial z}\Big|_{z=0}$ (we assume uniform, constant, ice conductivity k_I throughout the ice column.

We assume that for all time t<0, the ice column is in thermodynamic equilibrium, forced by a constant downwelling LW flux $F_{LW\downarrow}^1$, with a linear temperature profile $T(z,t) = T_{sfc} + \frac{z(T_{bot}-T_{sfc})}{h}$, with T_{sfc} chosen to satisfy flux continuity.

At time t=0, the downwelling longwave flux abruptly changes from $F^1_{LW\downarrow}$ to $F^2_{LW\downarrow}$. As $t \to \infty$ the temperature profile will approach a new straight line. The problem then is to determine the timescale in which the temperature profile decays to the new equilibrium, and how this timescale depends on *h.*

Solution

To simplify the maths we carry out a change of vertical coordinate: $Z = z + h$. Hence the bottom boundary condition becomes $T(t, 0) = T_{bot}$.

The evolution of the ice temperature is described by the standard heat equation

$$\rho c_p \frac{\partial T}{\partial t} = k_I \frac{\partial^2 T}{\partial Z^2} \qquad (2)$$

solutions to which are sums of terms of the form

$$Ae^{-\lambda\alpha t} sin\big(Z\sqrt{\lambda}\big) \qquad (3)$$

for $\lambda > 0$, where $\alpha = k_I/\rho c_p$.

This much is standard theory; the unique contribution of the ice column problem is the top boundary condition which restricts λ to a particular discrete set:

$$k_I \frac{\partial T}{\partial Z}\bigg|_{Z=h} = F^2_{LW\downarrow} - \varepsilon\sigma T^4(t, h) \qquad (4)$$

We linearise this and substitute in $T = Ae^{-\lambda\alpha t} sin\big(Z\sqrt{\lambda}\big)$ to find

$$-k_I\sqrt{\lambda}\, cos\big(h\sqrt{\lambda}\big) = B\ sin\big(h\sqrt{\lambda}\big) \qquad (5)$$

where *B* is the rate of dependence of outgoing longwave radiation on surface temperature.

Roots to this equation can be found numerically, and indeed tend to $(n + 1/2)\pi$ as $n \to \infty$. However we are interested in the long timescale response of T, and are hence interested in the first, smallest root of equation (5) as this will produce the slowest-decaying harmonic. Solving numerically for the root in the interval $(\pi/2\,, \pi)$ we find the following relationship between *h* and the decay timescale (Figure R3):

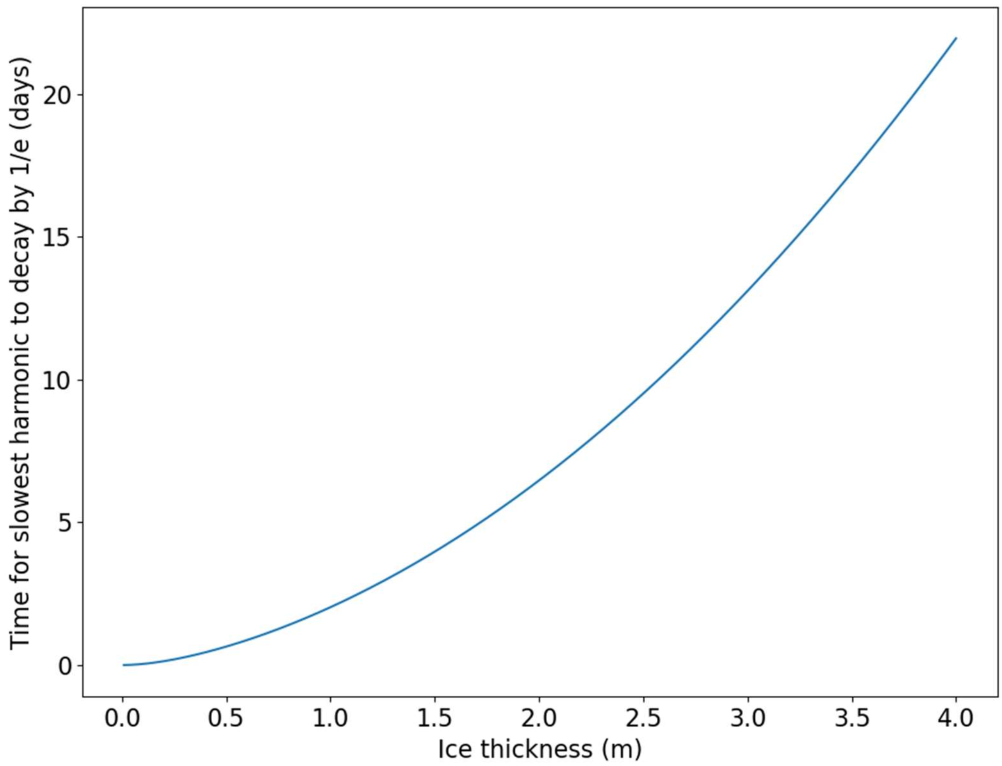

Figure R3. The relationship between ice thickness and time taken to relax towards a new temperature profile

These results also form the basis of Table R1.