I already reviewed the first version of the manuscript.

My main concern was on the presentation of the flow law (ESTAR) which is presented as a physically based alternative to describe the anisotropic rheological properties of polar ice. I noticed 2 problems with this presentation: the anisotropic character of the flow law and the applicability of laboratory results on tertiary creep of polycrystalline ice to large portions of polar ice sheets. The same kind of remarks where raised by the second reviewer, however they are contested by the authors in their reply and changes in the manuscript for these points have been minimalist.

The authors recall that the main aim of the paper is to present the implementation of ESTAR in an ice flow model and not to provide a comprehensive review of anisotropic flow models or a justification of ESTAR as it is presented in Budd et al. (2013).
However as we may anticipate that this paper will mostly interest ice flow modellers that are not all specialist of ice rheology, I think that it is particularly important to correctly discuss the hypothesises of the flow law and what it can do or not.

Anisotropic or not?

I think there is a semantic problem with the word "anisotropy" that is used in the paper to describe different things: the "fabric", i.e. the orientation of the ice crystals, and the rheological flow law. "Anisotropy" in a polycrystal comes from the combination between *(i)* anisotropic properties at the crystal scale and *(ii)* a non-random orientation of the ice crystals. "Anisotropic" polycrystalline ice could either mean *(i)* ice with a non random crystal orientation or *(ii)* ice with a mechanical anisotropy. In general there is no need to distinguish between the two meanings as the flow laws that integrate the description of the fabric (referred in the paper as "microstructure approaches") will predict an anisotropic behaviour when the fabric is non-random. However, this is not the case with ESTAR as, because of the hypothesis that the fabric instantaneously adapts to the stress state, the fabric does not enter the constitutive relation so that there is no information about the material structure and orientation in the flow law. So we have to precise the meaning of "anisotropy" and see if it can be applied to describe the flow law ESTAR, i.e. the "A" in the acronym.

I looked for a definition of "anisotropy" in the book "Texture and anisotropy: orientation in polycrystals and their effect on material properties" by Kocks, Tomé and Wenk (1998):
- page 11:
"A material property is isotropic when it does not depend on how the sample is 'turned' [...]; it is anisotropic when it does depend on the orientation of the sample with some external frame. Such an anisotropy is due to the arrangement of the building block of the material: its structure"
- p14, footnote:
"The word anisotropy will be used [Nye 1957] in connection with properties that depend on direction [..], rather than in relation to structural elements [...]. In early works, the word 'anisotropy' was used for what we call 'texture' [...]"
Following these definitions, in the context of ice flow modelling, by "anisotropic polycrystalline ice" we should understand "ice with an anisotropic mechanical behaviour" and not "ice with a non random fabric".

In general, the anisotropic ice viscosity should be a rank-four tensor as it relates two field variables, stress and strain-rate, that are rank-two tensors. This point gave rise to the polemic around the anisotropic character of the CAFFE model.
Faria (2008) gives the following definition for anisotropy:
"Succinctly, in continuum physics (cf. Truesdell and Noll, 1965; Hutter, 1983; Nye, 1985; Liu, 2002, and references therein) a material is said to be isotropic in a given reference configuration if

its response is invariant with respect to any orthogonal transformation (viz. rotation, reflection or inversion) of the body, otherwise it is called anisotropic. (When defining isotropy, some authors consider only rotations (i.e. proper orthogonal transformations), since only these are usually feasible in practice. However, this approach would be unsuitable, for example, for optically active materials (hemitropic media) and is therefore not adopted here. As remarked by Nye (1985): 'if we are to link physics to the mathematical theory of symmetry it is difficult to avoid the use of such unperformable operations'.) The set of transformations that render the material response invariant is called the 'symmetry group' of the material in the given configuration"
We find the same two elements as in Kocks, Tomé and Wenk (1998): *(i)* anisotropy is used to describe a material property, i.e. the "material response", and *(ii)* the property depends on the orientation of the body. Faria (2008) then shows that the scalar-valued enhancement factor in CAFFE is an anisotropic function of the deviatoric stress.

Because ESTAR has no information about the material orientation, it does not enter this definition of anisotropy, as for a given solicitation (e.g. compression, simple shear) the material response will be invariant by any orthogonal transformation as, by definition, ESTAR does not include information about a material orientation. Contrary to what is claimed in response to reviewer 2, the scalar enhancement factor in ESTAR is an **isotropic** function of the deformability which itself is an **isotropic** function of the forcing represented by the deviatoric stress tensor and **n** (the normal to the non-rotating shear plane)

ESTAR is then not an "anisotropic" flow relation, the fact that it predicts a different behaviour between compression and simple shear is not sufficient to comply with the definition of anisotropy.

The difference between compression and simple shear comes (mainly) from the fact that, in tertiary creep, the polycrystal has different fabrics depending on the stress state, however that does not make of ESTAR an anisotropic relation.
Again I refer to Kocks, Tomé and Wenk (1998), p421:
"Texture itself evolves with straining [...]. This fact contributes, for example, to differences in stress/strain curves between tension and compression, since texture evolves differently in the two cases. It also leads to a difference between tension and torsion stress/strain curves; merely on the basis of the differently evolving textures [...]. These topics are not properly labeled as consequence of 'anisotropy'; but they are macroscopic effects that can be explained on the basis of texture and the single crystal yield surface."

To summarise, I am not contesting that laboratory results shows that in tertiary creep the fabric depends only on the stress configuration and that the mechanical response depends on the fabric and thus on the stress configuration. ESTAR captures these properties and, integrated in an ice flow model, will give a spatially varying mechanical behaviour depending on the flow configuration. However, this is not the definition of anisotropy.
In consequence the manuscript must be revised to use the term "anisotropy" where appropriate, especially it can not be included in the acronym to describe the flow law.

Tertiary creep:

In their reply, the authors suggest that we may remain agnostic about the processes that control the material response. The idea is that we can use empirical laboratory results in different situations without a proper understanding of the detailed physical processes; **this is only true as far as the "physical processes" remain the same between the empirical results and the situation where it is applied.**

Tertiary creep is usually defined from the creep curves obtained in laboratory experiments that are performed at high stresses and temperature. According to the review by Faria et al. (2014) at low temperatures and stresses (that would results in strain-rates below 10^-10 s^-1, that are not unusual in the central parts of polar ice sheets), observations are inconclusive about tertiary creep as this would require to run experiments for centuries (or more). There is then a circular logic in defining tertiary creep only from the cumulative strain that is observed in the creep curves: i.e. lab experiments shows a tertiary creep stage after 10-20% deformation, so if ice has been deformed by more than 10-20% it must be in tertiary creep and the physical processes must be the same.

The microstructure evolution in laboratory creep experiments is described by Budd et al. (2013): *"These results show negligible change from the initial isotropic structure up to about 1–2% strain. From 2% to 10% strain there is relatively rapid recrystallization giving clear well-established fabric patterns by 10%. Extending to 20% strain strengthens the fabrics, which tend to a steady state in orientation and crystal size with the continuing tertiary flow. These changes have been well studied in the laboratory to temperatures below –15C."*
This is also summarised, e.g., in the review by Faria et al. (2014), *"The accelerating part of tertiary creep is accompanied by the development of lattice preferred orientations (LPOs) and an increase in the mean grain size [...]. It should be noticed that the rapid LPO formation in such* **"***fast***"** *experiments is not caused by slip-driven lattice rotation, since strains of only a few percent are not sufficient to produce noticeable LPOs by lattice rotation alone (Azuma and Higashi, 1985; Jacka and Li, 2000). Rather, this early LPO formation must be related to the nucleation of new grains."*

From this I draw two conclusions:

- migration recrystallisation is an important process in the laboratory experiments against which ESTAR is calibrated. Using ESTAR in configurations where migration recrystallisation is not important is an extrapolation of the laboratory experiments.
- If tertiary creep is defined as a stage where the microstructure is in steady state, then if nucleation is not important and fabric evolution if mainly driven by slip-driven lattice rotation and e.g. rotation recrystallisation, then it could take much more than 10-20% deformation to reach a steady state and this steady state may be different from the steady state where migration recrystallisation is important.

For the applicability of ESTAR, the question is then not too much ***"is tertiary creep occurring?"*** **but** ***"is tertiary creep as seen in laboratory experiments where migration recrystallisation is important occurring?*****". The answer is clearly related to the activity of migration recrystallisation in-situ.**

The authors mention in their reply that there is evidence that migration recrystallisation is occurring at low temperature (below -15C). However, according to Faria et al. (2014), *"From the microstructural analyses of ice cores, we conclude that the formation of many and diverse subgrain boundaries and the splitting of grains by rotation recrystallization are the most fundamental mechanisms of dynamic recovery and strain accommodation in polar ice. [...] Evidence of nucleation of new grains is also observed at various depths, provided that the local concentration of strain energy is high enough (which is not seldom the case). [...] Nucleation is not predominant in polar ice, but newly nucleated grains can be found regularly in ice-core samples from any depth, and are specially frequent in samples from the lower firn."* and Diprinzio et al. (2005) note that their observations of recrystallised microstructures at shallow depth and low temperature is "unusual" compared to observations in other ice cores.

In their review, Faria et al. (2014) mainly discuss observations of the grain shape and size, and their main critics are about previous views on the occurrence of normal grain growth. I don't see any consideration about the fact that previous views on fabric evolution should be reconsider. On the contrary we can read in their part I: *"For the upper 1500 m of EDC, Wang et al. (2003) could show that the gradual clustering of* ***c****-axes towards the vertical (which is expected for an ice dome undergoing uniaxial compression) agrees well with equivalent datasets from GRIP and Dome F (cf. Sects. 4.1 and 6.2), when plotted together with respect to a common normalized depth (i.e. depth/total ice thickness). Furthermore, a simple model of strain-induced* ***c****-axis rotation based on the assumption that basal dislocation glide is the dominant deformation mechanism (Azuma, 1994) satisfactorily reproduces the anisotropy evolution with depth in all these cores."* Note that according to the definition of "anisotropy" given by Kocks, Tomé and Wenk (1998), in the previous sentence, "anisotropy" should be replaced by "fabric". This could be extended to the upper parts of NGRIP and NEEM as Montagnat et al. (2014) show that the fabric evolution is similar down to a depth where shear becomes dominant. Note that observed fabrics in shear dominated areas and their evolution with depth are also consistent with a fabric evolution mainly driven by slip-induced lattice rotation.

In their reply, the authors seem to suggest that the development of fabrics is a proof of the existence of tertiary flow. I don't see the causality, as the plastic deformation by slip induce an evolution of the fabric, so there is no requirement to reach the tertiary creep to have a fabric. I am also surprised by this sentence : *"The observation within polar firn of microdeformation processes that are necessary for the development of fabric throughout ice sheets (e.g. Kipfstuhl et al., 2009; Faria et al., 2014)".* Kipfstuhl et al. (2009) and Faria et al. (2014) discuss the occurrence of dynamic recrystallisation in polar firn including nucleation, but these processes are not "necessary" to explain the development of a fabric; again plastic deformation by intra-crystalline slip also induce a fabric development.

In a creep test, a steady state for the fabric (i.e. what I take as definition for tertiary creep according to the authors reply) will then only be achieved if all crystals reach a steady state orientation compared to the stress configuration or if a recrystallisation process can balance the slip-induced fabric evolution. That may well require much more than the 10-20% strain threshold used to define the tertiary creep. Jacka an Jun (Physics of Ice Core Records, 2000) present the results of laboratory experiments at low temperature and stresses; they show that migration recrystallisation, which is the dominant mechanism at high temperature and stresses, degenerates to the point where it becomes insignificant. For these cases, the experiments show negligible fabric evolution between 1 and 10% deformation so that strain-rates are nearly constant after the secondary creep minimum at 1%.

I then maintain that the occurrence of migration recrystallisation and its importance in controlling the microstructure evolution, is an important observation to assess areas where in-situ conditions could be compared to the laboratory tests that have been used to calibrate ESTAR.

Looking at microstructure observations in all deep ice cores where temperature and stresses are very low, there is no evidence that migration recrystallisation is dominant, so there is no clear evidence to which extent results of laboratory experiments on tertiary creep can be used to asses the in-situ ice mechanical properties. Most of the previous works on ice polycrystalline mechanical anisotropy have used these observations for calibration and or validation and they had success in explaining fabric evolution with depth. Tertiary creep observations, as used to calibrate ESTAR, can not explain such observations. **So I think it is better not to present ESTAR as an alternative to previous approaches but as complementary as they have been calibrated against observations where different conditions and thus different mico-deformation processes prevail**.

This do not prevent to implement ESTAR in a large-scale ice-sheet model and test its performances. But the different hypotheses and the character of the flow law must be

described more carefully so ice flow modellers can discuss the choice of the flow law depending on the targeted applications. The authors should clarify and revise their use of both “anisotropy” and “tertiary creep” all along the manuscript.

There was only few comments about the implementation and application parts. The authors have expanded the discussion on the ISMIP test. I have no specific comment except that again the authors should be careful in their use of the words “anisotropy” and “tertiary creep” used for the justification of the assumptions of ESTAR (the compatibility of the fabric with the flow).