Reviewer 2		
Line	**Reviewer comment**	**Response**
	Requires major modifications, including more evidence to underpin some of the parameter assumptions as well as greater clarity about the purpose and key message of the manuscript.	We thank the reviewer for their comments. We emphasise that our paper proposes a way to demonstrate the possibility of risk to life in the absence of any information apart from regional-scale morphometric analysis, such as the Melton ratio. Many of the reviewer's comments regarding our assumptions are correct--but note that we aim to demonstrate the possibility of risk to life, where communities or their decision-makers perceive no risk. To do this easily and cheaply on a regional scale, we need to make "precautionary but realistic" assumptions. These assumptions therefore will err on the side of caution. If our analysis gains the attention of communities and their decision-makers, we are in a position to investigate in more detail. "
	Further sensitivity analysis of the other key risk calculation parameters is needed to back up bullet point 4 of the conclusion. This sensitivity analysis may add to the strength of the argument for identifying the "window of non-recognition	Agreed--this conclusion is not sufficiently supported by the preceding text. It is not a critical part of our argument; the simplest option is to delete it.
	Throughout the manuscript, there needs to be clearer links to the wider debris flow and risk literature. Re-structuring the introduction, discussion and conclusion may more clearly focus on the key message around the methodological framework and end purpose of the information.	Agreed

Line	Reviewer comment	Response
	How does your stated risk threshold (which is based on individual risk) relate to societal/group risk, as these risk thresholds are often developed separately? Can this be explored in more depth? Linked to this is providing justification for considering both individual and societal risk, rather than just one or the other. How may this influence risk managers decision-making?	Here is our understanding. From Strouth and McDougall(2020). "**Individual risk** is the probability that a specific individual will be killed by a landslide. This risk is often assessed for the individual most at risk within a landslide hazard zone or building and is expressed as the probability of death to an individual (PDI). Strouth and McDougall point out **that societal/group risk** is a more complex concept, but "in practice, at least for landslide risk management decisions in Western Canada, societal risk refers more narrowly to the relationship between the probability of, and number of, people killed." We used this definition of societal risk. On a specific fan impacted by a debris flow, the number of deaths will depend on the number of people who occupy that fan, which dwellings are impacted, whether individuals are present, and their vulnerabilities. The reviewers are correct; extending our analysis from individual risk to life to risk of multiple deaths requires knowledge of the variation amongst individuals in terms of these risk variables. We do not have this knowledge--the best we might be able to do is assume uniform values for all individuals (but see comment re PS:H below)
	I was unconvinced by the assumption of the probability of spatial impact calculation. Is it possible to include within the sensitivity analysis an evaluation of this term, as it will likely have a big impact on the risk value. Can prior research such as Zubrycky et al 2021 provide distributions to evaluate within your Bayesian framework? This needs further links to the literature and explored more in the discussion. Is the Zubrycky et al., 2021 approach something that could be adopted in NZ?	To answer the easiest question—the research by Zubrycky et al. looks useful and relevant to NZ and likely many parts of the world. To answer the more difficult question--could the approach by Zubrycky et al be included in our framework? We argue that to do so negates the aim of our approach. We aim to demonstrate the possibility of risk to life **exists** where communities or their decision-makers perceive no risk. To do this easily and cheaply on a regional scale, we need to make "precautionary but realistic" assumptions. These assumptions therefore will err on the side of caution. If our analysis gains the attention of communities and their decision-makers, we are in a position to investigate in more detail. As we note, "The observed frequency of deaths in New Zealand dwellings from debris flow impacts, although admittedly a very small sample, appears to be lower than the assumed value in this study, suggesting that these key parameters (in our model) need further research." We think the research by Zubrycky et al. is a promising approach for more detailed investigations and modelling.

Line	Reviewer comment	Response
35	Reference for "Debris flows as an unrecognised and underappreciated hazard…" Why is that the case?	This statement is from McSaveney et al., 2005. We suggest that this lack of recognition by the public is partly due to confusing terminology, with previous events referred to as "floods", "flash floods", or "slips" (McSaveney et al., 2005). However, awareness has grown in the NZ natural hazard community, and it is not fair or accurate to say that, currently, there is no awareness. NZ natural hazard scientists and practitioners are increasingly aware of the hazards posed by debris flows. The problem of public and political unawareness remains. Apart from the problem re terminology in media reporting, the other main reason is identified in the paper: "the long ARIs for these events create an illusory sense of security so that their risk to life is not recognised" and our paper addresses this.
39	Line 39: Remove the colloquial term "landslips"	Agreed, in NZ, "shallow landslides" is the commonly used terminology and we will use this.
Section 2.1	Section 2.1: Move information about need for methodological framework to introduction.	Agreed.
107	Change from "is necessary" to "may be necessary"	Agreed.
165	Missing references to paragraph	
189	Feels like this belongs in the discussion?	Agreed--will revise.
Table 1	Table 1: Doesn't match earlier description in text. Would be good to provide a separate overview table of published debris flow case-studies and associated ARI in NZ	Apologies—but we have carefully reviewed the variables in Table 1 and they match the variables and their descriptions in Section 2.2. Can the reviewer give us a more specific idea of where Table 1 does not cross-reference to the earlier text? Secondly, we are only aware of estimated ARIs for three case studies in NZ--Matata, Thames and Ligar Bay. Matata and Ligar Bay are cited in the text. We could add the Thames estimate (ARI~500 years). This reinforces the point about a lack of information and awareness, certainly in NZ.
335	Is this because we can't always capture dynamic risk parameters (e.g., exposure and evacuation with heavy rainfall)? It would be good to highlight the need for dynamic risk models.	Agreed, and also the need to collect data to calibrate and test these models.