

The author has taken up most of the previous suggestions in an adequate form. Besides, he/she decided to avoid the problems associated with the prior projections in the way that he/she skipped them altogether and replaced them with alternative projections. I (still) regard the topic as very relevant and interesting for the journal's audience, my main concern is, however, that the insights the current empirical analysis provides are not sufficient for warranting an own paper. MY MAIN SUGGESTION is therefore to reframe the paper towards a literature review (maybe illustrate it with some own computations but not as an empirical analysis that stands on its own).

- From the text, it is not clear what the contribution of the author to the projections is. He/she implies to undertake them in the paper: "In this paper, we use cohort component projections..." and "Our projections use..." (p. 19) However, later it appears that they were already published in Goujon, Skirbekk et al. 2007?
- P. 2, line 9: What is Steve Bruce's 2002 formulation?
- P. 2, line -9: "growing importance of values" – please provide citation
- Data and Methods: it is not clear why the author uses ESS Wave 2. Why not the other ESS Waves, why not ISSP? All of them contain church attendance. The choice seems arbitrary.
- P. 8, line 4: Ireland also asks religious affiliation in the census, so obviously Austria, Switzerland and Germany are not the only western European countries; the author should correct this information [remark in first review already]
- P. 8, line -1: please clarify: what does WCD regard as Agnosticism? That respondents don't know if God (or other religious claims) exists (= usual concept)? Or rather that they don't believe? In the latter case (despite WCD using it) this term would be incorrect. If he/she chooses to stick with it please justify.
- Figure 1: How does the author arrive at the projections? What does "data not available for identical time intervals" mean? (In the graph, the intervals appear as identical.) I'm unsure about the added value of Figure 1. The author may just describe the results, saving space.
- P. 9, line -8: Simple descriptive analyses cannot be considered an APC analysis. Analyses on the relative importance of age-period-cohort are conducted nowhere.
- The first part of the analysis resembles the one by Voas (however looking at a longer time-span), nevertheless it remains unclear if the added value is large enough justifying an analysis standing on its own.
- Note of Figure 2: "different methodology" – do you mean survey methodology?
- P. 10, last paragraph: I am still not convinced that one taps "believing without belonging" with a question on self-assessed religiosity – a large share of self-reported believers are in fact belonging. (Look how Voas created his groups.)
- Talking about "believing" implies the belief-dimension of religiosity (belief in God, belief in angels ...), self-assessed religiosity is a broader concept encompassing all dimensions. Please rewrite (e.g. p. 11, line -11: "First, the data on self-assessed religiosity ..." instead of "First, the religious belief data...")
- P. 16, line -7: representativeness and relative size of a group in a survey are different – to talk about representativeness one would need to check the composition of the Muslims
- Literature: Berghammer/Philipov and Sobotka would be:  
[http://hw.oeaw.ac.at/0xc1aa500d\\_0x0017f0e5.pdf](http://hw.oeaw.ac.at/0xc1aa500d_0x0017f0e5.pdf) Régnier-Loilier and Prioux:  
[http://www.ined.fr/en/resources\\_documentation/publications/pop\\_soc/bdd/publication/1366/](http://www.ined.fr/en/resources_documentation/publications/pop_soc/bdd/publication/1366/)
- Endnote 5: already published?
- Figures 2-4: suggestion to show cohorts as lines rather than period as lines