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Hydrology and Earth System Sciences Discussions

Interactive comment on "Identification of glacial melt water runoff in a karstic environment and its implication for present and future water availability" *by* D. Finger et al.

Anonymous Referee #4

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"IdentiïňĄcation of glacial melt water runoff in a karstic environment and its implication for present and future water availability" by D. Finger et al. The problem of glacier-karst hydrogeology has largely been piecemeal studies, largely by the empirical investigation of field sites. Despite use of quantitative measurement and analytical tools, our understanding remains essentially qualitative. This paper therefore has an ambitious objective in attempting to further our understanding at a generalisable quantitative level, by incorporating field observations and predictive models for underground flow routing and future mass balance scenarios. Unfortunately, these ambitious aims are only met superficially. The models used are poorly developed and are not integrated into the

overall study. The field work components also appear to have little overall integration. The study therefore is a useful, but essentially parochial (local) investigation of glacier-karst hydrogeology. The Glacier de la Plaine Morte is perhaps the definitive example of a glacier-karst system as the glacier largely occupies a massive closed depression. However, it is far from typical and the ice is largely stagnant. (This probably means it hosts a more stable internal drainage system than more dynamic glaciers.) I suggest that extrapolating from this unusual site (and the limited data set) to global scale is over ambitious. The generalisation that loss of alpine glacier ice will reduce late summer flow is widely recognised, and poorly supported by the analyses presented here. The "karst model" utilised in designing the tracer experiments appears to be an excellent tool, particularly valuable in the complex tectonics of the Swiss Alps. It does appear to have some limitations, however. First, the relationship between lithology, structure and hydrogeology has to be explicit. (Here it is not apparent which units and discontinuities (faults) are considered susceptible to karst permeability.) Similarly, there is no obvious inclusion of glacier ice (or talus), so the predictions are tenuous (and indeed appear to have been misleading in missing recharge from the major outlet stream). Finally, the tool's effectiveness relies on parallax-based three dimensional rendering and fails when printed on a page. It would have been much more effective to provide a clear map showing the inferred flow routes. Overall, the flow visualisation model seems a bit disappointing. The "predictions" (in text as figure 3 is unreadable) seemed to indicate various underground drainage routes. In contrast, the results seemed to show (again figure 4 is not readable) that the subglacial karst has surprisingly limited and conditional permeability (which we already know) and the tracer delivery more or less travels to the nearest spring. The dominant results of surface routing and subsequent capture on the Bernese side were not explictly predicted by the model (as far as I can see. Although the likely capture of the surface stream to springs is fairly evident using Google Earth;to which I would add the possibility of a talus aquifer linking the Reitzliberg and Siebenbrunnen Springs). The routing model might be more effective if were used to make specific, well-illustrated and tested hypotheses. The hydroclimatic data are quite

substantial, but their discussion does not seem to relate to the main purpose of the paper. They are only used explicitly in attempting to characterise glacier melt, though presumably used in various ways in analysis and modelling. The artificial tracing work is interesting, must have taken considerable work and is worth publishing. However, I would recommend some revision to reach a reasonable professional standard. The description and analysis lack the substance I would expect in a work of this scale. The tracers and traces are not well described. I could not find data on the Duasyne, but as one of the "optical brightener" tracers, it will show quite different optical and tracer performance to the other two closely related compounds (eosine and uranine). Unfortunately, the use of a synchronous scanning strategy implied in the terse section on analysis is likely to fail quite badly on typical optical brightener tracers. "Blue" (and to some extent "green) tracers are also expected to have a high natural background which is not apparent from the figures. The masses of tracer injected are exceptionally large and could result in prosecution in some jurisdictions. The injection descriptions imply random release on the ice surface. Tracer injection usually requires prior dissolution (typically to ppt level) and instantaneous injection without perturbation of steady flow. This is virtually impossible with the masses used, even if a suitably large supraglacial stream were present. (I could see none larger than perhaps 1m3/s in Google Earth.) Did the stream then sink into a moulin, or run over the surface? I was also confused by the descriptions of the 2012 injection sites; were they close to or near the 2012 I2 site? Why was the glacier outflow not monitored? It is surely the most critical monitoring point! The tracer analysis remains fairly rudimentary (which is appropriate in a novel setting), but some clarification is required. To compensate for contrasts in fluorescence intensity, the tracer concentration can be readily normalised to concentration per mass (typically 100g for some reason). Tracer velocities require a characteristic travel time (first arrival, mean or peak?) and path length (straight line, sinuous or true path?). It is not clear what is being used here. Tracer recoveries are useful in general, but not meaningful when the breakthrough curve is poorly defined as it is for the main river traces in 2011. The conditions of melt and routing through the glacier are fundamental

to the design of the tracer tests, the analysis of results and the future modelling. It is therefore surprising that the glacier hydrology in the paper is fairly superficial. It is implied that peak melt develops with full ice cover. This is not normally the case, peak local runoff occurs in the late stage of snowpack loss. It is not clear what route the melt water was taking. Was it supraglacial, marginal or through a perennial or seasonal conduit system? What was the likely state of this system at the time of tracing? How is the routing likely to change through the year and how will this influence recharge? Is the absence of recharge really due to "silt" and "loess" at the bed, or is the melt water routed away from recharge windows? I have never seen basal silts beneath a glacier and loess (a wind-blown silt) seems improbable. Most deglacierised karst surfaces exhibit extensive solutional permeability and sediments are generally coarse (permable) and dominantly very late or post glacial in timing. So either some substantive evidence has to be advanced, or the absence of recharge maybe attributed to routing of melt through the ice. The concluding diagram (Figure 11) proposes a dual porosity model for the glacier. This might seem a reasonable proposition for testing, but seems poorly supported by the work. A potentiometric surface is often postulated in glacier hydrology despite lack of empirical evidence. Such a model is very difficult to sustain for impermeable glacier ice. Instead, a seasonally and spatially variable sheet-cavity-conduit is more widely supported. It is most unlikely that a coherent water table exists in a glacier such as Plaine Morte. Some direct or indirect observations are needed to construct an appropriate glacier hydrology in a setting like this. In the light of the results provided, the only substantial evidence appear to be the high season tracer results (five traces in all) reported cessation of surface outflow (no data are given). No tracing has been undertaken under non-overflow conditions, so the Bernese subglacial routing remains hypothetical. The lesson learned from the substantial body of work on glacier tracing is that results vary dramatically with injection conditions, location, season and runoff (diurnal and storm driven). Glacier karst tracing is expected to be even more idiosyncratic because of the likely variation in recharge opportunity at the bed. So the only relevant tracer test of the key focus of this research is those injections (three in all)

observed at Loquesse spring. The stable isotopic data are too sparse to provide much insight. Two sites have not been proven connected to glacier. The key heavy signal at Tieche is presumably a transient rain event captured at that site. I would omit this section. A glacier melt model is used to predict the future geometry of the glacier. It is not clear how a daily model is calibrated using two digital elevation models fifty years apart and validated using three years of limited accumulation and ablation data. Although it provides a crude linear trend, extrapolating this trend into the untested geometry of a closed basin seems very risky, especially when rather dire implications are drawn. It is not clear that the hydrology has been fully implemented. If the leaky closed depression model (figure 11) is used, then it seems that an increasingly large fraction of the melt would be retained in the depression and subsequently recharged. The data in this paper would seem to suggest that relatively little melt would travel north (Bern) and a greater fraction would be routed south (Valais). Overall, there are substantial weaknesses in all the components of the paper and in their integration. It is not clear that the conclusions are based on substantive analysis. This is unfortunate as the various components of the research are interesting and challenging, but lack adequate treatment in attempting to compose an integrated report. The results provided suggest that a test of underground flow predictions using dye tracing would be worth reporting. Similarly, the forward modelling looks promising. It is not clear that the results allow much advance in our understanding of glacier karst recharge. Therefore, the primary purpose of the paper is not adequately addressed. A final comment on the language: the English is good, but in places the technical usage is incorrect and possibly misleading. A editorial proof reading is advisable.

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