

We thank the reviewer for the time spent in reviewing our paper and making very helpful suggestions. We provided a point-by-point response to the reviewer's comments.

Anonymous Referee #1

Overall I think the paper is very interesting and generally well presented. The topic covered is quite complex (many different components to the modeling study) and therefore it is important that the explanations are as clear as possible. In most cases, I think this is true...

Author's response

We would like to thank the reviewer for his interest in this work.

Anonymous Referee #1

... but there is one example where I think the explanation could be improved and that is in the last paragraph of 4.1.2 where the three models are discussed. I think it needs to be made explicit that models 6, 30 and 54 are linked to the three different snow accumulation schemes.

Author's response

Agreed.

Author's changes in the manuscript

The following statement on page 12159, lines 6–7 of the discussion paper:

“... obtained with three competing model hypotheses (no. 6, 30 and 54) differing only in their snowmelt-accounting options.”

Has been replaced in the updated manuscript with the following statement:

“... obtained with three competing model hypotheses (no. 6, 30 and 54) differing only in their snowmelt-accounting options (respectively B1a, B1b and B1c).”

Anonymous Referee #1

I also found the explanation at the start of 4.2.3 to be quite confusing and could be explained a little better.

Author's response

Agreed. We hope that the following changes in the updated manuscript will help clarify our statement.

Author's changes in the manuscript

The following statement on page 12160, lines 24–26 of the discussion paper:

“The high representation of options F2a and F2b in Cluster 1 suggests that the catchment actually behaves as a serial system and may reveal a better correspondence with its overall physical structure.”

Has been replaced in the updated manuscript with the following statement:

“The frequency of options F2a and F2b in the best-performing cluster suggests that the catchment actually behaves as a ‘serial’ system.”

Please note that this sentence has been removed from Section 4.2 to Section 5.

Anonymous Referee #1

I found the implied definition of equifinality on page 12163 to be very limited. Why is equifinality limited to a single criterion? The concept was borrowed from geomorphology and relates to the same outcome from different causative processes. The definition used in the paper is a very limited ‘mathematical/statistical’ one.

Author's response

As underlined by the referee, the concept of equifinality relates to the same outcome from different causative processes. On Page 12163 line 2–4, we wrote that “two parameter sets are said to be equifinal if they can be regarded as equally acceptable in a statistical sense with respect to one particular criterion”. It seems to us that if one replaces the words “criterion” and “parameter sets” in our sentence by, respectively, “outcome” and “different causative processes”, one gets the original meaning of the concept given by the referee. We slightly modified our sentence to make more explicit that the concept of equifinality is defined here in a statistical context and not in general terms.

Author's changes in the manuscript:

The following statement on page 12159, lines 6–7 of the discussion paper:

“...two parameter sets are said to be equifinal if they can be regarded as equally acceptable in a statistical sense with respect to one particular criterion.”

Has been replaced in the updated manuscript with the following statement:

“...two parameter sets are said to be equifinal in a statistical sense if they can be regarded as equally acceptable with respect to a given model outcome.”

Anonymous Referee #1

It might have been useful to show some time series of flow and rain at the start of the paper to illustrate the hydrological regime (2.3.2). This could help the readers to understand the concepts of greater than 100% runoff coefficients. I assume that these are related to quite slow groundwater release processes where precipitation (or snowmelt) from one year only appears as runoff in the following year. Perhaps this also depends on how you define the hydrological year and this is not adequately explained in the paper.

Author's response

We thank the referee for this relevant suggestion. The hydrological year was defined from May to April so as to capture the snowmelt and peak flow seasons at mid-year. As explained in Sect. 2.3.2, these values of runoff coefficients were most likely due to an underestimation of precipitation at high elevations or to “a greater contribution of groundwater to surface flow”. We realized that this statement was not clear enough and modified it as indicated below.

Author's changes in manuscript

A new figure representing multi-decadal hyetograph and hydrograph has been added to the manuscript to illustrate the hydrological regime of the catchment studied. The definition of the hydrological year was inserted in the caption of this figure (Figure 2 in the updated manuscript). Moreover, the following statement on page 12146, line 22 of the discussion paper:

“... or a greater contribution of groundwater to surface flow...”

Has been replaced in the updated manuscript with the following statements:

“... or a delayed contribution of groundwater to surface flow from one year to another...”

Anonymous Referee #1

While the authors introduce some ‘real hydrology’ in section 4.2 these discussions are quite limited compared to the much greater detail about the statistics and mathematics of uncertainty. This aspect of the paper could be improved.

Author's response

We do agree that Section 4.2 is quite limited compared to the other parts of the discussion paper. This is mainly because we wish to limit these statements to very basic assumptions requiring much caution, given the lumped conceptual nature of the models involved.

Author's changes in the manuscript

Please note that these comments have been removed from Section 4.2 and put in Section 5 to emphasize their hypothetical nature.

Anonymous Referee #1

I also noted that the issues of data uncertainty associated with the estimation of natural streamflow are only mentioned right at the end, while these could have a very large impact on the modelling results if the naturalization process and the knowledge of abstractions is poor.

Author's response

Agreed. This comment was also made by the other anonymous referee. We admit that this point was not made clear in the paper and this was mainly due to space limitations. As explained in Section 2.1., vineyards and orchards cover most of the valley floors and lower hill slopes, where they benefit from a unique combination of clear skies, high temperatures and overall dry conditions throughout the growing season. Most of the annual precipitation, however, occurs as snow during the winter months, leading to an entire dependence on surface-water resources to satisfy crop water needs during the summer. Irrigation water abstractions occur at multiple locations along the river's course depending on both historical water rights and water availability. Because these abstractions are likely to influence the hydrological behavior of the catchment, especially during low-flow periods, they were added back to the observed streamflow before calibrating the models. This inevitably adds some uncertainty to the modeling of daily stream flows because a significant part of surface-water abstractions actually return to the river system within a few days. In general, ignoring these return flows will lead to overestimating natural stream flows on a daily basis. In this paper, however, the actual water withdrawals were not known with precision but only as percentages of the nominal water rights (these percentages are fixed on a monthly basis by the authorities depending on water availability), so the overall effects of streamflow naturalization on model uncertainty remained unknown.

Author's changes in manuscript

The following statements on page 12143, lines 17–18 of the discussion paper:

“...but account for less than 1% of the total catchment area (INE, 2009; CIREN, 2011). By contrast, natural vegetation outside the valleys is extremely sparse...”

Has been replaced in the updated manuscript with the following statements:

“...but account for less than 1% of the total catchment area (INE, 2009; CIREN, 2011). Most of the annual precipitation, however, occurs as snow during the winter months, leading to an entire dependence on surface-water resources to satisfy crop water needs during the summer. Irrigation water abstractions occur at multiple locations along the river's course depending on both historical water rights and water availability. By contrast, natural vegetation outside the valleys is extremely sparse...”

The following statement on page 12144, lines 22–25 of the discussion paper:

“Naturalized streamflow time series were estimated using information provided by the Chilean *Dirección General de Aguas*, mainly streamflow measurements at the gauging station of Rivadavia and historical surface-water diversion data.”

Has been replaced in the updated manuscript with the following statements:

“Water abstractions for irrigation were estimated using information on historical water allocations provided by the Chilean authorities. Because these abstractions are likely to influence the hydrological

behavior of the catchment during recession and low-flow periods, they were added back to the gauged streamflow in Rivadavia before calibrating the models.”

The following statements on page 12164, lines 21–27 of the discussion paper:

“It was also possible to highlight some errors in the streamflow data. Part of these errors might be associated with uncertainties in the estimation of natural streamflow. Further research is therefore required to better integrate the effect of water abstractions in the hydrological modeling process. From a multiple-hypothesis perspective, the modeling of irrigation water withdrawals should be regarded as a testable model component in its own right.”

Have been replaced in the updated manuscript with the following statements:

“It was also possible to highlight some errors in the streamflow data. The observed streamflow was ‘naturalized’ by simply adding back the estimated historical water abstractions. When applied on a daily basis, this process inevitably adds some uncertainty because a significant part of surface-water abstractions actually return to the river system within a few days due to conveyance and field losses. In general, ignoring these return flows leads to overestimating daily natural flows. In this paper, however, the actual water withdrawals were not known with precision but only as percentages of the nominal water rights (these percentages being fixed on a monthly basis by the authorities to account for water availability), so the overall impact of streamflow naturalization on model uncertainty remained unknown. Further research is underway to integrate the effect of water abstractions and crop water-use in the hydrological modeling process (Hublart et al., 2015; see also Kiptala et al., 2014). From a multiple-hypothesis perspective, the modeling of irrigation water water-use should be regarded as a testable model component in its own right.”

Anonymous Referee #1

I think the paper contains too many references - it is not a review paper and many of them are somewhat superfluous. There are also several that are included in the reference list that are not used in the text (Clark et al, 2009; Fenicia et al., 2007; Fowler and Kilsby, 2007; Freer et al., 2013; Hrachowitz et al., 2013; Krueger et al., 2010; Lang and Braun, 1990; Leavellesley et al., 2002; Loukas et al., 2002; Montecinos and Patricio, 2003; Olssen and Andersson, 2007; Staudinger et al., 2011; Strauch et al, 2006 and Zhang et al., 2010). Some of these could be related to wrong dates as the following included in the text could not be found in the list: Clark et al., 2005; Fenicia et al, 2006; Freer et al, 2003; Montecinos and Aceituno, 2003). Shaefli et al, 2011 is also spelt wrong and Souvignet et al. has the wrong date?

Author's response

We apologize for all these typos which were corrected in the updated manuscript. Moreover, only the most relevant references have been kept in the revised paper..

Author's changes in manuscript

Agreed. We apologize for these typos which have been corrected in the updated manuscript.

Anonymous Referee #1

Figures 4 to 8 could all be improved in clarity with larger font sizes and other improvements. There is space to do this.

Author's response

Agreed.

Author's changes in manuscript

These figures have been modified with larger front sizes and minor modifications in the updated manuscript.

Some minor points:

Anonymous Referee #1

Are the 12 and 8 (precip & temp) stations supposed to be shown on Figure 1?

Author's response

No, the weather stations could not be shown on Figure 1 because many of them are actually located outside the catchment.

Author's changes in manuscript

In the updated manuscript, Figure 1 has been modified to include those precipitation and temperature stations which belong to the catchment.

Anonymous Referee #1

Page 12149 line 14 – where is Eq 1 referred to?

Author's response

We apologize for this typo. This reference to “Eq . 1” is completely undue and was removed from the paper in the updated manuscript.

Anonymous Referee #1

Page 12157 line 8 – Is ‘emblematic’ the right word here’?

Author's response

We used “emblematic” in the sense of “illustrative” or “representative” but, as non-native English speakers, we cannot be totally sure of this choice. In the updated manuscript, this adjective was simply removed without impacting the overall meaning of the sentence.

Anonymous Referee #1

Page 12159 line 6 – ‘... internal state variable obtained...’

Author's response

Agreed and modified.

Anonymous Referee #1

Page 12160 line 5 – ‘... absence of sublimation...’

Author's response

Agreed and modified.

Anonymous Referee #1

Page 12161 – The reference of Figure 7 at the start of 4.3 should be Figure 8 I presume.

Author's response

Agreed and modified.

Anonymous Referee #1

Page 12162 line 14 – ‘...filling of a moisture...’

Author's response:

Agreed. Please note that this sentence is no longer used in the updated manuscript.