

Interactive comment on “Hydrogeological conceptual model of andesitic watersheds revealed by high-resolution geophysics” by Benoit Vittecoq et al.

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Received and published: 15 February 2019

This paper is really interesting, and it was a nice journey for me to read it in details, notably also as I worked for a long time in this island, and with one of the co-authors of the paper. This paper thus comprises a real potential for publication in HESS, particularly as it may present original results/theses on at least 4 different topics: 1. as it addresses the structure and functioning of an andesite-type volcanism (subduction-type) watershed, this kind of geological context being much less studied and documented than basaltic-type volcanism (hot spots, medio-oceanic ridges and transform faults, etc.); 2. as it comprises not only a classical geological approach to characterize the

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structure of the aquifers, but also high-resolution airborne geophysics which is also not so common, particularly in andesitic volcanism; 3. as it tries to compute water budgets at the scale of surface/groundwater watersheds, which is not often performed in such a rather complex hydrogeological context and also notably as it requires data sets long and difficult to acquire; 4. and also and finally as it proposes an original thesis: andesite-type volcanic rocks hydraulic conductivity is assumed as increasing with the age of the rocks, contrary to some case studies of basaltic-type volcanic rocks. This process is interpreted by the authors as a consequence of tectonic fracturing that would develop a “cumulative” permeability, the rocks being not only fractured, but pervious, proportionally to the duration of their exposure to earthquakes, so to their age.

However, some inconsistencies in the data and/or in the interpretations are undermining the current proposed results. It is then necessary, to my opinion, that the authors improve the processing of their data, their interpretations and their argumentation, complete their data with additional ones if possible, to better support all or some of these hypotheses/theses, and thus to strengthen the stronger results of the paper before publication.

POINT N°2 As regards point N°2, the combined geological and airborne geophysical approach: The 1/50'000 scale geological map of Martinique, used and cited by the authors (Westercamp et al., 1989, 1990), is well known since now about 30 years to be a very high quality and highly reliable document. This is particularly the case in this Morne Jacob (MJ) shield volcano - Pitons du Carbet (PdC) area where the later formations (PdC) cut and fill the earlier ones (MJ), and form and fill well identified paleovalleys; these paleovalleys roughly appear on Fig. 2a (orange/brown PdC formations lying on blue MJ formations). I am a bit disappointed as I was expecting that the combination of the new airborne geophysical data, and this high-quality preexisting map would help to produce much higher quality results, valuable for applied geology, and particularly hydrogeology, such as a higher quality geological map, precise geological cross sections and, why not, the mapping of the depth of the main geological

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formations. Then, I feel that the geometry of the different geological formations could have been much better described. Moreover, deepening the interpretation of this data set would surely lead the authors to shift from some vague assertions about “heterogeneity”, “preferential flow circulation”, etc. to the precise geological identification of the structure governing groundwater flows, namely permeable structures on one hand, and impervious ones on the other hand. Globally, and as regards the hydrogeological conceptual modelling of volcanic aquifers (cf. Point N°1), it would help the authors to shift from rather old low resolution conceptual hydrogeological models (highlighted as such by the authors at lines 13, 19, 28-29, etc. of the manuscript) to the nowadays needed high-resolution hydrogeological conceptual modeling (see for instance Lachasagne et al., 2014 where this issue of low to high-resolution conceptual hydrogeological modelling in volcanic aquifers is also discussed in details). I would then suggest the authors to complete their current interpretation with an iterative approach: First: valorize in depth the existing geological map, and particularly draw precise cross-sections or, better draw 3D surfaces, to well delineate the main geological structures (paleovalleys, paleosurfaces, main lithological bodies below or within these paleovalleys, etc.). In that frame, the outputs from the quite recent paper from Germa et al. (2011) may surely be valorized more in depth.

Second: complete this geological model with the airborne geophysics and with the 51 boreholes cited in the text, most of them being not available at the time the geological map was published. For instance, one can see on profiles C1, C2 that a same geological formation (2alpha for instance) is not homogeneous, and comprises structures that can surely be interpreted: paleomorphologies, weathered levels, unweathered higher resistivity areas, etc.. It should also enable to discuss the resistivity range of these geological formations which is finally not so large (less than 5 to about 100 ohm.m). This process should also enable to calibrate much more in details the airborne geophysics with these geological and geophysical data. Then, it would be nice, as soon as this stage of the process, to highlight the outputs of the new airborne geophysics. These data may also be compared/completed with the available existing field geophysical

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measurements (the first author of the paper participated to or directed several geophysical ERT campaigns on that area of Martinique that may be worth to be integrated in this paper.

Third and finally, I am sure that this systematic process will enable real progress in the knowledge of (i) the geological/hydrogeological structure of this part of Martinique such as: - identification of geological structures such as dipping, paleovalleys, various homogeneous lithological bodies, etc. within each preidentified geological formation, notably within the MJ hyaloclastites, the MJ shield volcano superimposed series of lava flows, and the PdC paleovalleys infilling, - identification and characterization (thickness) of the main paleosurfaces (particularly, if possible, in the MJ formations) and the associated weathering profiles, and (ii) the hydrodynamical properties of these formations.

It may surely lead to improve or even revise the proposed ranges of transmissivity and permeability, and at least to affect them to a larger set of rock types (see my other comment on that topic below).

POINT N°3 As regards point N°3, computation of water budgets at the scale of surface/groundwater watersheds, I have 2 main concerns. The first one deals with the uncertainty of all the measurements and computations required to obtain a surface/groundwater watershed budget: uncertainty on rainfall, particularly with the steep orographic gradients prevailing in this part of Martinique, on real evapotranspiration computation, then on effective rainfall, also on gauging data, etc.. To be convincing, the authors must appropriately compute error propagation, and not only provide numbers in their annual water balances (see for instance Fig. 5) but also uncertainty ranges around these numbers. The authors must also explain more in detail how the groundwater balance computations were performed (maybe there is some information to be extracted from contrasted hydrological years: average, dry, wet, etc.). But also how these data, spanning over several years were averaged, if some exceptional years, or events such as hurricanes and tropical storms were removed or not, etc.. In this area with steep pluvio-climato-hydrological gradients, how did they use the results and

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discretized computations from Vittecoq et al., 2010? How did they use the one from Arnaud et al. (2013)? Additionally, it is not clear how “runoff” and “GwR” were computed, and if this sharing of river discharge provides added value to the paper. If not, it could be removed. Additionally, the river discharge curves should be provided, and discussed at least briefly.

More generally, the paper lacks from a “methods” section (if necessary with adapted references) that would enable to explain to the reader most methodologies used. Moreover, the use of different units to describe the same data (mm/y, m³/y, L/s) adds an unnecessary complexity. Additionally, “perspectives” should be discussed in the discussion section.

I tried to get more in details in the computations and have the following remarks. Alma watershed computations: - how the number of 8 Mm³/y (line 4, p. 6) is computed is hard to follow: it seems that it is only by replacing $R = 5000$ mm by another hypothesis $R = 7000$ mm. Again, this large difference in R suggests large uncertainties on the water budget. Maybe it would be more convincing to present, for each surface watershed, not only uncertainties as I proposed above, but also lowest and highest estimates of the different components of the water budget, and then to demonstrate that, in all cases (in most cases?), some water could be “missing”, and could then recharge aquifers; - I don't understand the about 10% difference between “RivD”= 3515 mm = 323 L/s, and AARivD = 355 L/s.

Fond Lahaye watershed computations: - Line 7, p. 6: “its average specific discharge”.

Case Navire watershed computations: - again, the computations are very hard to follow: $3200 - 1450 = 1750$, not 2300 - for Case Navire RivTD (592 L/s) = AARivD (590 L/s). It wasn't the case for the Alma watershed

The second concern deals with the fact that the authors didn't consider some important known hydrogeological outflows issuing from neighbor areas from the studied area. Particularly, the “Attila” spring is flowing out from the PdC domes (9alpha_bi andesites

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- 9iAqbi) less than 1 km west from the upper part of the Case Navire watershed. The discharge of this spring is about 1 million m³/year (30 L/s) (Lachassagne et al. 2003) which is far from negligible. This well-known spring from Martinique should at least be cited and integrated in the interpretations. In fact, this is, to my opinion, an important outflow from this aquifer, considered by the authors “as an important perched aquifer” (line 27). The authors should surely: 1. consider the boundaries of this lithological unit in their study, and then its inflows (recharge) and outflows, and also its relationships with the neighbor aquifers and surface watersheds; 2. hierarchize the springs, in order to distinguish high discharge springs, that constitute the real outflows from this aquifer (“regional outflows”), and low discharge springs that may be very local, and even that may be only representative of superficial formations in this context of very high rainfall. Additionally, presenting and discussing the geological context of the main springs (why they outflow where they outflow) may be very helpful. From this hierarchization of springs, the authors may revise their conceptual model (Fig. 9), and for instance not “plot” on it the very local springs. The same could be done for all springs (see again section 2.3.1.) emerging from the other geological formations. As far I remember, and as it is acknowledged by the authors in several parts of the paper, most of these springs have a rather low discharge. The co-ordinates of this Attila spring are the following: $X = 701,550$ (km); $Y = 1627,240$ (km) ; $Z = 495$ (m NGM)

Then, on Fig. 1 (or on a table to be provided that would support section 2.3.1.), the average discharge of each springs should be provided, as some springs, like Attila, are large discharge springs, but most others, as highlighted by the authors (line 23), are very local ones, with a very low discharge (a few L/s). In such a study that would like to get from the local to the watershed scale, not the same importance must be given to all springs.

Additionally to these concerns, to my opinion, an important and rather simple to acquire data set is missing in the paper. A survey of the streams during the dry season would surely have highlighted on one hand areas with outflows from the aquifers that feed

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the rivers, on the other hand, areas with water leaks from the rivers to the aquifers; and also of course areas without interactions between aquifers (or lack of aquifer) and rivers. These inflows/outflows/no flow could (i) have served as real proofs of river water leaks, to strengthen the argumentation, and (ii) have been correlated to the lithology. It would also have fulfilled the need for a high-resolution approach in such a complex geological environment.

POINT N°4 As regards point N°4, I feel that the “correlation” between transmissivity and age is biased. First of all, I consider that, in that frame, resistivity should not be compared with transmissivity, but with hydraulic conductivity. The thickness of the “aquifer”, which is included into transmissivity, has nothing to play with the rock’s resistivity which, to my opinion, should be independent from its thickness, if the geophysical data are well inverted. Then, comparison should only be made with hydraulic conductivity (K, permeability, on Fig. 8B).

Second, as described below in additional remarks, the way to compute the hydraulic conductivity should be revised, and also better explained. Then, depending on the permeability type (generalized to the lithological body, or localized to some fissures for instance), the relationships between K and the resistivity may be challenged.

Third, as no borehole from the PdC domes is included in the data set, these formations are not used for the “correlation” between K and age. I “fear” that, including such data would completely modify the conclusions here as, to my opinion (but also as the authors suggest from the watershed budget data), such domes exhibit a rather high permeability. The authors could use the data from wells drilled in more recent domes such as the ones from the Champflore area (a few kilometers North from the study area) as proxies of the hydraulic conductivity of the PdC domes. I however “fear” that it would completely modify the relationships between formation age and hydraulic conductivity and then deny the proposed correlation.

Fourth, the impact of the lithology on hydraulic conductivity is not considered. In the

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literature, and on the field in Martinique (boreholes data), the muddy debris flows (lahars, etc.) are intrinsically considered of rather low permeability, not as a result of their age, but due to their lithology.

And also, a correlation with only 3 couples of (resistivity, age) data, additionally with large uncertainty on resistivity data, is maybe a clue but, to my opinion, not a proof.

Then, the proposed explanation in the “Discussion” section (p. 12, lines 15 and followings) cannot be considered, to my opinion, as a demonstration.

Additionally, hydrochemical (major elements), and particularly a synthesis of isotopic data, would surely be of a high added value to understand groundwater flows in the studied area. Such an approach would in fact be independent from the quantitative methodologies currently developed in the paper.

CONCLUSION Then, as a conclusion and to my opinion, the proposed conceptual model of these watersheds (as well as the discussion and conclusion) will have to be revised according to the outputs of the revision of the basics of the paper. It would surely enable to much less use the conditional wording in this part of the paper, that deserves its credibility. Moreover, the watershed conceptual model will have to better consider consistency between flow lines and inferred piezometry (for instance, (i) vertical flow lines in the 9alphaBi domes, that seem to go deeper than the hyaloclastites &H are hardly understandable as hyaloclastites are considered by the authors to be of low permeability; (ii) as said above, location of all reported springs is not very consistent, low discharge springs should surely not be represented on this figure; (iii) subhorizontal arrows – flow lines – in the 2alpha formation are not consistent with the existence of strong hydraulic conductivity variations in this formation).

Then, as highlighted above in this review, below, and in the additional remarks, several of the affirmations of this section are highly conjectural to my opinion.

1. Andesitic domes To my opinion, the data from the Attila spring, and also the wa-

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ter budget data (if appropriately recomputed/explained) may sustain the fact that this formation can be considered as a high permeability aquifer. Then, it would be nice to use geological and geophysical data to map this aquifer, identify its surface (and underground if any) outputs, explain the hydraulic conductivity contrasts (notably with the neighbor other geological formations) that explain the location of the main spring(s), etc.. - p. 9, line 5: as said above, the way this 85% value is computed is not explained in the paper - as said above, the explanations about springs will have to be revised considering low discharge springs on one hand, and the high discharge ones such as the Attila spring. - p. 9, line 14: in hydrogeology, "source" has no meaning for a river; specific discharge must be preferred, and then the origin of this specific discharge (either high rainfall, or aquifer outflow, or both) must be discussed.

2. Andesitic lavas It is dangerous to consider this huge complex as an unique "aquifer" (see for instance p. 10, lines 17 and following: lithological continuity doesn't mean aquifer continuity; a higher resolution is needed). Again, as said above, the interpretation of the whole data set should help get into high-resolution in this aquifer, considering its lithological structure (more or less listed p. 9, lines 23 to 25), the fracturing (that could indirectly be supported by the results from the paper of Lachassagne, Léonardi, Vittecoq et al. (2011) that would be worth to be cited), and the weathering. It would help not only to deal with vague concepts such as "heterogeneity" (p. 9, line 23 for instance), but to explain the origin of this spatial and vertical variability of hydrodynamic parameters. Then, a more detailed analysis of the data set would enable to less use the conditional wording in this part of the paper.

3. Regional aquitard If this formation is really an aquitard, and if the overlying andesitic lavas are a regional aquifer largely supplied with recharge, then significant springs or outflows to the rivers should be observed at their interface. This must be better described, and conclusions issued from these observations.

4. Geothermal insight I fear that this part of the paper is highly conjectural, out of the scope of the data, and then deserves the other results of the paper. - p. 10, line 25 and

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following. Be careful, the hyaloclastites are known to be of low resistivity in most areas of Martinique. To explain this low resistivity, the hypothesis of their lithology, as well as their weathering must also be considered, and not only hydrothermal weathering. Moreover, there are also similar thermal springs elsewhere in Martinique.

As a conclusion, as regards the conceptual model of the watershed, to my opinion the authors should consider the following arguments: - the lavas from the PdC domes may be a rather high permeability perched aquifer. I feel that, with a few additional arguments and explanations, this point could rather easily be demonstrated, and that their functioning as an "unique" aquifer may be supported or, at least, strongly hypothesized; - the fact that the MJ hyaloclastites constitute a regional aquitard may also surely be supported; - I fear that the other formations (PdC paleovalleys and MJ lavas) cannot be considered individually as a whole as aquifers. There is a need to get to a higher resolution to understand and describe the pervious structures and the impervious ones that they contain. Then, to my opinion, these formations are a juxtaposition of small sized aquifers and semi-pervious to aquitard formations; however, at this stage, this is an opinion, not a demonstration. With their data set, the authors may demonstrate that, or demonstrate other concepts. If I compare the results proposed in this paper with the one obtained in Mayotte (Lachassagne et al., 2014), which support the presence of a few rather high hydraulic conductivity, rather low extension (a few hundred of meters) aquifers within low to very low hydraulic conductivity formations, I would consider that the MJ aquifers may be smaller (and of lower hydraulic conductivity) than those of Mayotte. In Mayotte, the determinisms of the hydraulic conductivity was described and demonstrated (for instance in lavas = discrete clinker layers and thicker cooling fractures layers), the present paper should help to provide similar observations in the Martinique MJ formations; - then, the "correlation" between permeability and age, and, beyond that, the interpretation that tectonic fracturing mostly governs the hydraulic conductivity of these rocks is, to my opinion, with the available arguments and data, highly conjectural. Moreover, the authors should not only consider the benefits of earthquakes on hydraulic conductivity (cf. Lachassagne, Léonardi et al. (2011)), but

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also the impacts of the clogging processes that fast follow the permeability increase due to the earthquake (see for instance, the paper from Lachassagne, Wyns et al. (2011) that summarizes all these issues).

ADDITIONAL REMARKS Some additional remarks or points to be addressed: - Fig1: a few French words are remaining (“élevée”, “faible”)

- Fig 1 & 2: the 2 maps contain probably too much data and are then hard to read. The spring symbol and/or color should be changed as blue over blue is not visible. The AEM data should be presented with dots (location of data) and not lines.

- p. 3, line 27: this is not annual temperatures that vary from 18°C to 32°C in Fort-de-France. Please rephrase

- p. 3, line 31 and following, p. 6, etc.: maybe the wording of “surface water intake” or “SW catchwork” should be preferred to “dam” in the present case

- p. 4, line 24: domes?

- p. 4, line 26: flow out?

- p. 5, line 13 and following: Hydraulic conductivity The authors highlight that the hydraulic conductivity was computed by dividing transmissivity by the height of the saturated screen interval. This is a rather rough approach. They could describe to which kind of lithology it refers, as well as to which kind of permeability (interstices, thermal or flow cracking, clinker layers, tectonics, etc.) and, where necessary, weathering. In fact, in such geological formations, most of the discharge/hydraulic conductivity of a given borehole can come from very narrow intervals. Then, dividing the transmissivity of the well by all the screened interval may not be relevant. Then, a detailed description of the geological logs, and their correlation with water inflows should be performed and some examples proposed as well as statistics. Such an approach was for instance performed by Lachassagne et al. (2014) (see Figs 4 and 5 of their paper). Instead, these values of hydraulic conductivity should be considered only as orders of magnitude, and

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I would recommend to not cite them in the paper, and keep transmissivity values as orders of magnitude of the relative productivity of the different geological formations.

- p. 5, line 17 and following: piezometry As highlighted by the authors, these data show that the piezometric level are shallow, and that there is no identified “basal aquifer”. Nevertheless, a higher resolution interpretation of these piezometric data could surely provide additional information, notably on the extension of most aquifers. For instance, by zooming on local data from Fig. 3B, the authors could also surely discuss about piezometric gradients at the local scale of a given aquifer; this local gradient is surely much less than the apparent mean 4% “gradient” visible on Fig. 3B; this latest surely reflects the topographic slope. From these data, I suspect that most aquifers have a very small lateral extension (a few hundreds of meters at the max. This clue resulting from the piezometric data interpretation should be mentioned in the paper. It may help to understand groundwater flows at the watershed scale, and refine the proposed conceptual model. Again, to complete my previous remark about springs, the discharge of the springs should be indicated on Fig. 3, for instance with an appropriate symbol/legend.

- p5, line 24: confined

- Fig. 4 : effective rainfall is not monitored; prefer computed? Piezometric level in m (and not mamsl as a shift was made to enable their drawing on the same graph). There is surely a mistake in the legend as the Case Pilote borehole is cited in the caption, but not in the legend. No influence of pumping on some data? Notably Case Navire 165 in 12/2006, 06/2009?

- p. 5, line 27: mean annual specific discharge?

- Fig. 5: liter should be abbreviated “L”, and not “l”

- Line 16, p. 6: “. . .2012. It allows. . .”

- Arnaud et al. (2013) is not listed in the references

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- p. 8, line 28: characterizes
- p. 8, line 29: highlights
- p. 10, line 10: it's not really "underlying" aquifers but rather neighbor ones
- p. 10, line 11: "a significant"
- Fig. 7: there is no legend for the vertical dotted lines. Also lithological contact like the subhorizontal ones?
- Fig. 6 & 7: springs must be located on the profiles: C1 = 1 spring, C4 at least 4 springs, C5 1 only (and not 2). Moreover, springs should be plotted at their real elevation to show the correlation with the geophysical structures. Topographic artefact are also visible ; they could be discussed.
- p. 10, line 20: what means computation "of high flowrates"? (i) computation (moreover not presented in the paper) is not a proof of sustainable discharge, (ii) high flowrate at a given well doesn't mean high natural flow in the aquifer, (iii) pumping may completely change the groundwater age, etc.. Then, as it is written, the demonstration in this section is not convincing.

Additional references cited in the review:

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