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Comment on "Origin and evolution of hydrothermal fluids in epithermal Pb-Zn-Cu ± Au ± Ag deposits at Koru and Tesbihdere mining districts, Canakkale, Biga Peninsula, NW Turkey" by Mustafa Cicek and Tolga Oyman, [Ore Geology Review, 2016, 78, 176-195.]

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Introduction

The paper by Cicek and Oyman attempts to elucidate the fluid processes responsible for mineralization in two spatially close areas, Koru and Tesbihdere in the Biga Peninsula of NW Turkey. This whole area, and that of Western Anatolia in general, contains a number of deposits in production, evaluation and is being extensively explored for further resources. A better understanding of how the mineralization in the region occurs would enable deposits to be better placed within the standard model for porphyry–epithermal deposits. Aqueous fluids and vapours are a key transporting medium for the ore metals. The source of fluids and the processes that have affected them in the epithermal environment is reasonably well known from studies over many decades from around the world and in effect this paper uses the same old methods to collect data and the same ideas on how this should be interpreted. The objectives of the paper being to determine the source of the mineralizing fluids and how they have evolved with time.

However, the ability of the authors to acquire data is not matched by their understanding of how data should be acquired, what the potential limitations of the data are and how the data can be interpreted within a proper scientific framework. There are many mistakes in the methodology used to acquire data from fluid inclusions, the stable isotopes of the inclusion fluids associated quartz and other non-silica phases, the methodology of acquiring data from the alteration, the clay assemblages present and the constraints on the context required for the data to be used in the stated purpose of the paper. We also found significant errors in the interpretation of the data that was collected, which occur for a number of reasons. There is also a significant deficiency in the authors referencing and discussion of previous studies from the region and of studies that are from the same deposits presented in this study. The whole basis of scientific research is to place current research with previous studies to either prove or disprove those or to offer improvements. Simply having some of the previous papers in the list of references, without discussion, is not the way science is done. Papers such as Oyman et al. (2003), Ozen and Arik (2013), Orgun et al. (2004), Yilmaz et al. (2007), Yilmaz et al. (2013), Imer et al. (2013), Yilmaz (2002) and others are all from mineralization of a similar type and in the same approximate area to the deposits in this study. They contain data from fluid inclusions and stable isotopes and a discussion of this study within the context of these published studies would have enabled a more comprehensive appreciation of the mineralization, which is a widespread phenomena in this region. There is also a tendency to use figures from other referenced studies in a “cut and paste” fashion seemingly without understanding that aspects of the figures are simply no relevant to their study.

In our opinion the deficiencies in this paper are so severe to make the data and the conclusions based on it of extremely questionable value.

Mineralization

The general aspects of the mineralization at the different locations are well described but lacks a figure that would clearly show the paragenetic sequence for each. A common theme for all is that the mineralization is located in fault or structural zones. There is also a mention of stockwork and breccia zones. In terms of the fluid flow processes that may have occurred, and that the authors refer to later in the paper, it is of of prime importance to fully describe and understand these main fluid conduits. It would seem that faults or fault related structures were the focus for fluid movement and related to tectonic events rather than fluid pressure events as suggested later. The description later in the paper that the breccias were hydrothermal and caused by boiling is in contrast with a description of being matrix supported milled breccias (Tesbihdere), which is consistent with tectonism. In the next sentence they then describe the veins as composed of massive hydrothermal brecciated ores? There is also descriptions of cataclastic textures in the ores, which are the result of tectonic events. So there is some confusion as to exactly what these are. However, there appears to be a lack of evidence to support their later assertion that breccias were hydrothermal. Clearly a better description and figures would have helped to clarify this key point.

Hydrothermal Alteration (Clay Mineralogy)

There are many unclear points that need to be explained in terms of the alteration assemblages, clay mineralogy and the methodological description used for preparation of the samples.

The clay minerals were evaluated on the basis of having a hydrothermal origin, but the occurrence of some phyllosilicate minerals (i.e., chlorite) may also develop as secondary neo-formation minerals related to the interactions of volcanic glass and sea water and surfical water. (Bozkaya et al., 2007). There is no definitive proof to exclude other possibilities.

The illite phase may also include primary (volcanogenic) and detrital (inherited) micas, in addition to the products of hydrothermal alteration. Before undertaking O-H isotope studies of clay minerals, the authors should have completed an investigation of the mineralogy, ie. polytype determination (optical and electron microscopy) to prove the alteration was hydrothermal related (Bailey, 1984, 1988: Bozkaya et al., 2016). Failure to do so casts doubt on the veracity of the data and interpretations based upon it.

The distinguishing of kaolinite and dickite polymorphous should be done from their characteristic peaks on the XRD scans of unoriented clay powders. There is no possibility to directly distinguish kaolinite and dickite on XRD patterns of oriented glass slides (Dickite + kaolinite association, Appendix B, it is not clear what the exact methodology was as it has not been fully described). In addition, there should also be proper polymorphic and SEM studies of these supposed alteration products. The evaluation of XRD patterns (Fig 6) shows some inconsistencies in peak identification, there are unnoted peaks near 2θ=27-28, which are probably feldspar.

To properly determine the O-H isotopes of these supposedly alteration products, it is essential that only monomineralic clays should be used. From the scans presented in Figure 6 this seems not to be the case. Additionally, quartz is present in all the XRD scan and this should have been removed from the clay fractions before O-H studies. This should be extracted by fine grain separations (e.g., 1-0.5 μm, 0.5-0.25 μm, <0.25 μm). It would appear that the O-H isotopes are from and mixture of minerals of unknown proportions.

In determining the δD and $\delta^{18}O$ composition of the waters from the clay measurements, the authors used illite - chlorite (Ill? [in Table 2[or [Ilt? in Appendix B] + Chl) associations. However, they do not specify which equation (illite-water or chlorite-water) was used for the calculation of clay-water fractionation? In the text they state illite? without mentioning chlorite?

Stable Isotopes Fluid Inclusions and Quartz

The analysis of the δD and $\delta^{18}O$ composition of the waters in epithermal systems is a key method for determining the involvement of magmatic and meteoric components and the degree of mixing. The δD value is derived from the release of water trapped as fluid inclusions but the accuracy of the determination depends critically on the method the water is released by. In this paper, the authors do not specify the actual method used, merely stating the laboratory which carried out the analysis. However, we assume that the method used was the same as that in a previous study involving Oyman et al. (2010) because the same very low δD values are obtained. In the previous study, the sample was dropped into a furnace at $1200^{O}C$ to vaporise the water. This method has been shown to produce analyses that are significantly in error and give values that are several 10's per mil too negative. Studies by Gleeson et al, (1999); Grant et al. (2003); Faure, (2003) and Baatartsogt et al. (2007) clearly show this effect, which is due to breaking of silane bonds releasing the isotopically light hydrogen. Releasing the fluid mechanically with heating to between temperatures of 300-$500^{O}C$ produces H_2 that is derived from molecular liquid water, ie the fluid inclusions. At temperatures greater than $500^{O}C$ H_2 comes from specific structurally incorporated cation sites, ie –OH defect sites, in the quartz structure. This reservoir of H_2 is fractionated with respect to the molecular water and therefore significantly more negative than the fluid inclusion water. It makes a very significant but variable contribution to the overall δD value. Thus the correct method is to heat the samples to between 300-$500^{O}C$ and mechanically open the inclusions. It appears this method was not followed and therefore the δD values are wrong by large and variable amounts. Values below -100 per mil and as low as -150 illustrate the erroneous nature of the data.

In addition samples that have been determined clearly have a mixture of fluids present in them as there is a large range of homogenization temperatures and in some cases salinities. The authors also mention the presence of secondary L-V inclusions and L-only inclusions in the samples. Therefore what is being analysed? any results will only reflect an average of the inclusions present and in the absence of any detailed petrography of the quartz and inclusions the data would have a limited potential for interpretation even if the analyses were only from the fluid inclusions and not from a structural source.

In Appendix B, a summary of the samples and analysis carried out is given. All the samples analysed for δD and $\delta^{18}O$ in quartz do not have measurements of the fluid inclusions present in them and therefore the authors cannot be certain what they were analysing. From Koru, 3 samples have isotope measurements but none of these have any fluid inclusion measurements. From Tesbihdere, 9 samples have isotope measurements but only 3 have fluid inclusion measurements from the same samples, the other 6 samples have no microthermometry data.

The $\delta^{18}O$ value of the quartz is assumed to reflect the same fluid as is present in the inclusions and therefore petrography is crucial. The quartz has to come from the same parts of veins that the inclusions were not only present but were analysed for δD from. Without petrography of the veins, and the location of the inclusions being known relative to the quartz there is uncertainty (and no proof) that the quartz and inclusions were precipitated at the same time. Not having examined the majority of the quartz veins containing the the inclusions is clearly a serious omission. It is in fact necessary that CL-imaging of the veins is done as it has been shown in many studies that veins which appear to be from a single event, when viewed with normal microscopy, are from multiple generations. This is particularly the case in epithermal systems where multiple growth events are recorded (Rusk and Reed, 2002: Moncada and Bodnar, 2012).

Whilst the $\delta^{18}O$ value of the quartz can be measured, it is the $\delta^{18}O$ value of the water in equilibrium with the quartz that is required, calculated from the quartz-water fractionation, which requires the fluid temperature to be known. As the authors have not determined from fluid inclusions what this temperature was in the majority of the samples, the value for the $\delta^{18}O$ of the water they have calculated is a guess. In addition, samples where the homogenization temperature was measured show a very large range of up to $200^{O}C$ and taking an average value again is in fact another guess that will lead to significant error. It is essential to have a good estimate of the temperature and that comes from good petrographic constraint on what was measured. Sadly this is not the case here.

Fluid inclusion studies.

The authors have made 274 microthermometric measurements of fluid inclusions, hosted by quartz in the case of Tesbihdere but by sphalerite and baryte with only one quartz sample from Koru. Measurements were made from what were claimed to be primary inclusions in growth zones and as single inclusions in crystals. They also record the presence of secondary inclusions and liquid only inclusions. In figure 7, which shows a number of different fluid inclusions, there are no photographs that show inclusions in primary growth zones. In Fig 7b+c they claim to show what is evidence of boiling from LV, L-only and V-only inclusions that are co-existing. The quality of the photographs is poor especially for the V-inclusions (maybe there is a bubble there and these are L-V inclusions). However, if these 3 types of inclusion co-exist it cannot be evidence of boiling. Boiling would not trap inclusions that only contain liquid at room temperature. Also in Fig 7G the quality is not good enough and possibly what are claimed to be V-inclusions are L-V.

Their statement that secondary inclusions were not measured is not compatible with understanding the fluid evolution of the hydrothermal fluids in the deposit. They present no information as to the petrography of these secondary inclusions, except that they are L-V smaller than primary inclusions and occur in trails, but in crystals growing in veins it is quite common to see the same fluids as primary inclusions and as secondary inclusions in other parts of the same crystal. It could be that these “secondary” inclusions may represent another pulse of the same fluid that was present in the growing quartz crystals. In any case these secondary inclusions were part of the fluid evolution in the deposit and should have been investigated. Only then could their importance and place in the fluid evolution be determined. There is also nothing presented about the inclusions that only contain liquid and while measurements may be problematic how they exist and their frequency is also important data to understand the full fluid history.

On page 184 it states that there were inclusions which were vapor-rich, yet a few lines later it states that the L/V ratio of inclusions varied from 7% to 50%, this is not compatible with being vapor-rich where there should be more vapor than liquid in the inclusions. Measuring the volume proportions of three dimensional objects by observing them in two dimensions can lead to serious errors as shown by Bodnar (1985, p80).

The authors record a wide range of eutectic temperatures from -25 to -54.1^{O}C and present this in Figure 8 as showing the H_2O-NaCl-$CaCl_2$-$MgCl_2$ components of the fluids (caption for Figure 8) from the two mining districts. According to this KCl would seem not to be present in the fluids. The data from LA-ICP-MS of individual fluid inclusions from Tesbihdere, presented by Bozkaya et al (2014), shows the cation composition and that K has the most significant concentration after Na. However, the fact that measured eutectic temperatures overlap the eutectic temperatures of different salt-water mixtures cannot be used as evidence for significant proportions of different salts. Eutectic temperatures are fixed points for different mixtures, there is no definitive first melting temperature shown for any of the samples that would indicate a reliable determination of the composition. The spread of eutectic melting temperatures simply illustrates how difficult it is to visually determine the point of first melting. As presented Figure 8 also gives a false impression of the salt-water components, especially in those inclusions with low eutectic melting temperatures, by implying that many of these fluids lack significant Na which is simply incorrect.

Salinity and homogenisation temperatures vary markedly in what are supposed to be primary inclusions. There is no detail given as to how the quite different fluids occur in relation to each other. Are these in the same primary zones, in the same fluid inclusion arrays or do they occur quite separately? It is important to know how these different fluids co-exist as this has fundamental implications if the authors wish to pursue a mixing origin for many of the data form the different areas of mineralization.

Where there is such a large range of salinity values of c.10% NaCl, it is pointless to present average values as this value solely depends on the proportion of the different inclusion that were measured. Clearly such a range might imply there to be more than one fluid, but the key question is how did inclusions that are supposed to be primary have such a range of values. What is the petrography of such inclusions, are the authors implying that mixing has occurred within the same primary zones or do different zones have different salinities and in their reporting the data these have all been put together? This is a key point and no information is given to show what the facts are, making any interpretation of this data simply a guess.

A similar set of criticisms applies to the homogenization temperatures where there are variations of up to c.200^{O}C. It is pointless to quote average values, how do inclusions with such markedly different temperatures co-exist in the same primary zones or in multiple zones. There is no petrographic information just as with salinities. In addition, the data from bartye must be treated with a good deal of caution as this mineral is extremely susceptible to leakage or stretching leading to homogenization temperatures that are too high (Ulrich and Bodnar, 1988). In this they show how baryte may stretch even prior to the homogenization temperature being reached and also that freezing the inclusions will induce overpressures great enough to stretch the inclusions. Therefore, baryte data always has a very high degree of uncertainty attached to it.

In summary, the authors present data from 274 inclusions from the different locations in the two mining districts. However, because of the absence of critical information on the context of the inclusions the actual value of the data in providing clear answers as to fluid origin and evolution is questionable at best.

Interpretation of fluid inclusion data

In section 9.1 the authors discuss the characteristics of the ore-forming fluids using diagrams of Wilkinson to interpret the data. So they use a "cut and paste" approach with the diagrams, without an understanding of the scientific principles. They show arrows representing the path of a boiling fluid that would increase in salinity while decreasing markedly in temperature. A fluid boils at a set pressure and temperature and as it does the vapor phase is of lesser salinity than the remaining liquid. As boiling proceeds both the vapor and liquid phases increase in salinity as vapor is removed. Therefore a boiling fluid can show a range of salinities depending at what stage of boiling the fluids were trapped. However, the temperature does not decrease as boiling proceeds.

Boiling may increase the salinity of the residual liquid through the loss of a high percentage of vapor, but it will not decrease the temperature of the fluid. What Wilkinson states is the liquid will cool if it undergoes a volume increase through adiabatic expansion. So for this to happen the pressure has to decrease during the boiling process. This cannot happen when the pressure is hydrostatic, (only if the pressure the fluid of a set temperature intersects the liquid-vapour curve is less than the confining pressure) but can occur if the fluid pressure changes from lithostatic, or close to it, to hydrostatic during vein opening. This was clearly shown in the papers by Bozkaya et al (2014) and Bozkaya and Banks (2015), which the authors have chosen to ignore in their discussions and interpretations.

They use salinity – homogenization temperature diagrams for different zones from the two deposits and combine these with direction of correlations as suggested by Wilkinson (2001) to infer mixing or boiling (Figure 9). The lack of a detailed petrography of inclusions with different salinities and temperatures, as previously pointed out, suggests the authors have no idea of the relative timing of inclusions with different salinities and homogenization temperatures and how the variable temperature and salinity of the inclusions relates to any aspect of evolution due to boiling or mixing. By suggesting mixing or boiling, and that these are all primary inclusions, the authors are implying that the range of temperatures and salinities occurs in fluid inclusions that are contemporaneous. This is highly unlikely and there has to be some spatial and temporal differences within the ore zones sampled.

In our opinion the fluid inclusion data in Fig 9 shows the following and does not support isothermal mixing or boiling in the majority of cases.

Fig A. Seems to be a scatter of points and it is hard to see how a temperature range of 200^{O}C is consistent with either boiling or isothermal mixing?

Fig B. The temperature range of baryte is ~150^{O}C but at a reasonably consistent salinity while sphalerite has a lower temperature range but higher and variable salinities. There is no evidence of boiling.

Fig C. Just scattered data points and if there is a correlation with salinity it is positive, ie going from bottom left to top right.

Fig D. Tightly grouped sphalerite salinities but with a temperature range of ~100^{O}C. Quartz has a greater salinity range and equally large temperature range. Any correlation is a positive one as in Fig C, but again difficult to equate the large temperature range with isothermal mixing.

Fig E. Two groups of inclusions with small temperature ranges but variable salinities. Clearly no negative correlation that would be consistent with the author’s interpretation of boiling.

Fig F. This has the same two horizontal groupings as is seen in Fig E. No negative correlation is present.

They also cite evidence for boiling from the co-existence of liquid and vapor-rich inclusions, but in fact they have no conclusive evidence of vapour-rich inclusions (even from photographs in Figure 8) and in addition they state that the maximum vapor phase percentage is only 50%. All their inclusions homogenize to the liquid phase and to prove boiling it is required that inclusions homogenize to both liquid and vapor at the same temperature. It is also necessary to have trapped both liquid and vapour rich inclusions at the same time and in the same growth zones, which the authors show no evidence for.

Their statement that the wide range of homogenization temperatures and necking down of fluid inclusions is further evidence of boiling is wrong. Boiling occurs at a single temperature (or at temperatures that are tightly grouped, if certain P-T conditions are met as previously described) and their data is more likely to represent different generations of fluids trapped at different times and at different temperatures. They use Roeder and Bodnar et al. 1985 to support their statement that “coexistence of liquid and vapour rich primary inclusions have wide ranges of homogenization temperatures with necking down phenomena observed at all mineralization in the study area, indicating progressive boiling and/or mixing of the hydrothermal fluids through space and time.” This is not what Bodnar specifies as criteria for proving boiling and in addition to the required criteria outlined above cautions against the measuring of inclusions where necking down process may have occurred. The following quote from Roedder (1967) gives some key criteria when inferring boiling.

> The trapping of gas or vapor bubbles with the liquid from an originally heterogeneous, two-phase system (that is, primary gas) invalidates most attempts to use such inclusions for geologic thermometry by the homogenization method, as this method is based on the trapping of a homogeneous single fluid phase. On the other hand, the very presence of simultaneously formed inclusions of both liquid and vapor gives the best evidence available that the fluids were actually on the boiling curve, ... ”. .

Brecciation is also claimed to be evidence of boiling, which is not strictly true. It is evidence that the fluid pressure exceeded the confining pressure and the strength of the rock. It is not specified if the authors are claiming that boiling produced the brecciation or if local boiling was a consequence of brecciation. If the space generated was large and sudden, and the time for it to be filled with fluid was great enough certainly there could be adiabatic expansion of the fluid with resultant cooling due to the temperatures drop but the fluid might not boil.

δD and $\delta^{18}O$ interpretation of the mineralizing fluids

The interpretation of the mineralizing fluids is based on waters from fluid inclusions and the $\delta^{18}O$ calculated to have been in equilibrium with the host quartz. There are also δD and $\delta^{18}O$ values calculated from the fluids in equilibrium with clay minerals. We have previously detailed significant discrepancies in the methodology used to acquire this data, for both the fluids and clays, which make any interpretation irrelevant. However, we have comments on what has been presented.

Data is presented on the conventional δD vs δ^{18}O diagram with some additional fields added. These additional fields add little to the deposits in this study, being so large as the main constraint on the fluids is the latitude and altitude of the epithermal systems controlling the local meteoric δD value. This also influences the size of the field for the more magmatic dominated mineralization. Why there are fields for hydrothermal alunite from Peru and an oxygen isotope shift from deposits in the Great Basin of Nevada is unclear as their geological setting is not pertinent to this study and their relevance is not mentioned in the text. We note the actual kaolinite line is missing from the diagram.

What is surprising (and somewhat worrying) is that the authors have chosen to ignore data in the literature that has a direct bearing on this study as it comes from the same deposits and for one of the papers Oyman was the second author . These papers (Yilmaz et al. 2010, Bozkaya et al. 2014) and that of Bozkaya et al 2016, which was published prior to the revised version of this paper being returned, contain δD and δ^{18}O data of fluid inclusions and clays from the same deposits as this study yet it is not presented nor is it discussed in light of this study. The δD and δ^{18}O data from fluid inclusions in the Bozkaya papers is quite different to that presented here primarily because it has been obtained using the correct analytical techniques and not from the incorrect method of heating the sample to 1200^{O}C.

The authors choose to reject 2 analyses from 5th Viraj on the basis that these have moss textures indicating deposition from supersaturated solutions and indicate colloidal amorphous silica deposition and that the data may have been affected by post depositional processes (citing Moncada 2012). However, in this paper Moncada indicates that these textures may come about from boiling solutions. There is no data presented from fluid inclusions and the δ^{18}O value of the water is calculated from fluid inclusions in baryte (which we assume they believe precipitated at the same time as the baryte) which gives values considerably heavier than from other samples. In addition, a δD value of -153 per mil is by far the lowest. So on this basis the data is rejected. However, something was measured? We suggest that in the absence of fluid inclusions the very negative δD actually reflects the structurally bound H_2, which heating samples to 1200^{O}C releases as well as the molecular water released when fluid inclusions are present. Hence the values of δD from actual fluid inclusions, which are very variable and too low. We could also suggest that the δ^{18}O of the quartz is too heavy because too high a temperature from baryte was assumed as the homogenization temperatures from baryte were artificially high.

Any interpretation made by the authors using the data of δD and $\delta^{18}O$ of the fluid inclusions is questionable at best, but in reality is just wrong. We have previously discussed how the δD values are wrong because of the incorrect analytical method in this and in their previous paper. The $\delta^{18}O$ values of the water in equilibrium with quartz are also questionable because of several errors detailed previously in doing the calculation without knowing the correct temperature. In samples from Koru, 3 had determinations of δD and $\delta^{18}O$, none of these have any fluid inclusion determinations, so the authors have no idea what they are measuring or what temperature to use for determining the $\delta^{18}O$ of water in equilibrium with quartz. In Tesbihdere samples, 9 have δD and $\delta^{18}O$ determinations, only in 3 of these have the fluid inclusions been measured and one other has measurements in sphalerite. The authors state on p188 that the fractionation between quartz and water was calculated from the fluid inclusion analysis of each sample. This is not correct as can be seen in Appendix B which shows the analysis carried out on each sample and that each sample analysed for stable isotopes did not have the homogenization temperature measured. So again for the majority of the samples the authors do not know what they are measuring despite their claim that they do. So are the inclusions dominated by a single generation that reflects the conditions of mineralization or are there many secondary L-V, or L-only inclusions also present?

Another issue is what temperature to use for the fractionation factor between quartz and water. If you don’t measure the inclusions, it is simply a guess. Even in those samples where there have been measurements of the homogenization temperature the issue is not straightforward. There is in some cases, as much as $200^{O}C$ variations in the homogenization temperature and their approach seems to be to simply take the average. This is incorrect as it leads to huge potential errors in the calculated $\delta^{18}O$ value for the water by not knowing the correct temperature of the fluid when the quartz was precipitated. As previously mentioned this results from the lack of a detailed petrography of the quartz and the fluid inclusions. It is now obvious that even petrography using normal optical microscopy is insufficient to ensure that the vein quartz being measured is of a single homogeneous generation and CL imagery is required. However, despite this there appears not to have been sufficient control of the quartz that was measured using conventional microscopy.

Again from Appendix B we can see that there is no direct measurement of the temperature of formation of the clay minerals despite there being minerals in the same vein samples, which may have provided this information. The authors instead choose to use generalised temperatures from other studies of mineralization in different parts of the world. Again this is poor science and will lead to unquantified errors in the calculated data. In addition as previously discussed the analysis was not carried out on pure clay minerals therefore the actual data has errors.

The data form this study plots in the same area of this figure as does data from the earlier paper of Yilmaz et al. for the Tesbihdere deposit, but is not presented by the authors. The points are not close to the magmatic water box and are as much as δD 70 per mil more negative (average probably around 40 per mil, ignoring the value at -150 per mil) than the local meteoric water or local geothermal waters at the present time. It is not clear what reasoning is used to state there was a “progressive dilution of ore forming fluids” by meteoric fluid along the flow path between the Koru and Tesbihdere deposits?? The data the present is not consistent with either a magmatic or a meteoric origin. Certainly equilibration of clays at different temperatures will move the δD and δ^{18}O values towards the meteoric water line, however the starting point for both fluid inclusion waters are waters in equilibrium with clays is nowhere near what would be expected for magmatic systems, which is because the data are wrong.

The δD and δ^{18}O results presented from fluid inclusions, quartz and clays from Koru and Tesbihdere by Bozkaya et al. are consistent with each other. The high temperature fluid inclusion data plot close to the magmatic water box, whilst slightly lower temperature fluids plot in the direction of the local meteoric water at the time of mineralization. However, the clay data indicate that where the data plots on this type of diagram can be due to equilibration with the alteration products and not fluid mixing.

Characteristics and origins of the fluids.

The authors sum up the study by making claims about the fluids and mineralization. These are not supported by even by the data they present (which is dubious at best). When you see homogenization temperature from baryte, that they claim is an early and high temperature mineral, with a range from 160 to 407^{O}C it is obvious it is just wrong. There is no petrography on any of the fluid inclusion data to allow assertions of mixing and or cooling with time to be supported. They claim to have mixing of magmatic and meteoric water, but do not state which fluid is magmatic or meteoric? The δD and δ^{18}O values do not support this (even if the data were correct). Further assertions or suggestions related to the fluid origins, processes or evolution along flow paths between the different deposits are incorrected and have no basis in fact.

In conclusion, the paper has so many errors in the data and it's interpretation that the authors suggestions and conclusions are extremely dubious. The model they present is speculative and based on incorrect data. The paper provides a false impression of the fluid processes, evolution and source at these two areas. The lack of any discussion of the data in previous studies of these deposits and of similar deposits in this region neglects a basic rule of science in that new data should be discussed in relation to previous studies.

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