Authors comments

The authors comments are in italics

Reviewer 1

Comments to manuscript "Iberulite fall and formation mechanism during a Sahara dust event in Switzerland in February 2021" by Grobety et al.

Introduction

Grobety et al. report an observed iberulite fall event in Switzerland and present analyses of the particles' composition, morphology, and size distribution. Based on the meteorological conditions during the event, the authors propose a formation mechanism for these particles. This study represents the first documented iberulite fall beyond Spanish latitudes, i.e., outside regions relatively close to the main dust source areas. The authors further suggest that these specific iberulites may have formed within snowflakes or ice crystals.

Reporting these events and gaining more insights into their formation mechanism is interesting. I am unsure how much additional knowledge is gained regarding their formation mechanism compared with previous literature (e.g., Díaz-Hernández and Párraga, 2008).

Hernández and Párraga gave no meteorological conditions during the IFs' they sampled. They gave no evidence pointing to the formation mechanism active for the formation of their iberulites.

It is undoubtedly valuable to report their sightings in northern latitudes, as this provides evidence of their abundance in the atmosphere. However, it is unclear whether these types of events have an impact beyond the anecdotal, i.e., whether they should be taken into account in models of dust deposition or whether they might have an impact on the radiation budget (Díaz-Hernández and Párraga, 2008).

We found, since the submission of the paper, iberulites in 50% of aerosol samples collected during SDEs'. I do not think that iberulites are anectotal, I think that people, who collect bulk samples, are not aware of the presence of iberulites. An important step to decide, if iberulite are important for models of dust deposition, would be to quantify the amount of dust deposited as iberulites.

I think the paper contributes incremental knowledge about the occurrence and formation mechanisms of iberulites. However, I recommend publication only after the manuscript language, scientific content, and organization are revised as suggested in the comments below.

General comments

The paper presents reliable methodology for iberulite characterization and data for event analysis. However, the structuring of the information is deficient, and the language is sometimes unclear.
The paper has been restructured following the comments of the two rewiers.
Typos and other errors can be found throughout the manuscript. These issues give the reader the impression of an unpolished manuscript (more than just typographical errors) that should have been more thoroughly refined by the authors before submission.

I recommend refining the language and structure so that the information is presented more clearly. Specific examples of where this is particularly important are given in the specific comments.

Figures should be refined and made more homogeneous, paying attention to legibility and their role as adequate supporting material. In their current state, they look like preliminary figures rather than figures that meet the standards of a publishable paper.

All criticized figures have been improved.

Light microscopy, SEM, and BSE images are of good quality. I strongly recommend improving the remaining figures. An additional map illustrating the collection sites and data provenance would also be advisable.

Amap has been added.

Another concern relates to the analysis and discussion of the proposed formation mechanism. The authors go back and forth between different formation mechanisms (snow/ice vs. in-drop formation), and the same conclusions are not consistently reflected throughout the manuscript (for example, in the abstract). This section is quite long, and the discussion should be more concise and focused. It also feels that part of the information presented there as discussion would be more appropriate in the Introduction as part of the state of the art.

The informative part of the original discussion has been moved into the introductiokn.

Specific comments

L24. Is it necessary to use the concept of the *Greenfield gap* in the abstract? Introducing it later in the results/discussion would be adequate.

We eliminated the concept of the Greenfield gap from the abstract

L26. There is no reference to snow as the hydrometeor hypothesized to have formed the observed iberulites.

We added "frozen hydrometeors" as likely scavengers

L31. Is it necessary to refer to the Alps as “European Alps”?

We removed "European", but we point to the fact that there are mountain ranges other than the “European Alps” called "Alps", i.e., the "Japanese" and "New Zealand Alps".

L33. Did the SDE in February-March increase only in occurrence, or also in terms of dust concentration? Please clarify whether this statement refers specifically to the Alps area or to a wider region.

Both the number of events and concentration increased. We added "dust concentration " Euro-Mediterranean region", term used by Cuevas-Agullo et al.*

L35. Please specify the threshold values established by the EU/Switzerland or WHO and how the measured values in the area exceed those.

Threshold value and the actual value during this event have been added.

L37. Please add examples of how dust affects climate and ecosystems.

We added some examples

L38. The statement about the scarcity of dust mineralogical studies is too strong. If you refer to a knowledge gap regarding source-specific mineralogy and its implications for atmospheric processes, please adjust the statement accordingly and include recent literature addressing this gap, since dust modelling efforts increasingly aim to take into account soil mineralogy atlas and their coupling with atmospheric modules.

We removed the sentence about the scarcity of dust mineralogical studies, although the last published of your suggested references (Jeong, 2024) states: " Modeling the effect of mineral dust on the environment requires a consistent data set of mineral composition and properties (Claquin et al. 1999; Scanza et al. 2015), which has not yet been established due to the very low quantity of samples, the lack of systematic analyses and interpretation, and the poor mineralogical information on the soils and sediments in desert sources.

L41. Earlier published studies (e.g., Fiol and Guijarro, 2000) already report the observation of deposited small dust spheres.

The reference has been added

L43. The reported iberulite sightings are confined to Spanish territories at latitudes relatively close to the dust source (Sahara Desert). Would it be more accurate to state that no observations have been reported from regions north of 42°N? The Alps would therefore fall into that category. north of 42°N".
We replaced "Spanish territories" by "regions

L46. Please merge the information about the 2021 SDE event and its monitoring. It seems duplicated in sentences L45–L50. For example, February is mentioned three times and systematic monitoring is mentioned twice.

We merged the information.

L51. Define IF again.

IF was redefined.

L52. Remove the observation coordinates from the Introduction.

The observation coordinates were removed and shifted to subsection 2.1.

L54. Since the in-cloud and below-cloud iberulite formation mechanisms have already been proposed in the literature, the Introduction should include a more detailed summary of them, e.g., connecting the mechanism names to the descriptions in L43–L45.

The description of the two mechanisms have been moved from subsection 4.2 to the introduction.

L59–62. Include here the coordinates of the iberulite collection sites. Adding a map with the collection site locations and other relevant observation sites (Jungfraujoch observatory, measurements in Payerne, weather radars, balloon soundings, etc.) would help provide a clearer overview of where the data and observations were collected. The description of the weather/remote sensing data collection (L72–79) would then be easier to follow.

See L52

L62. It is not clear whether iberulites were collected in Geneva or not. Please specify whether the observation (L59) is different from the collection.

The information has been given.

L71. "Observational data" would be a more appropriate subsection title than "Meteorological data," since more than weather information is presented (e.g., aerosol information). The correct numbering for this subsection is 2.2.

The subsection title and numbering have been changed.

L72–73. Please consider including a map with scale showing where all the observational data were collected.

A map has been added.

L86. The description of HYSPLIT should be placed under a different methods subsection (e.g., 2.3 Dust trajectory analysis).

A new subsection has been introduced.

L99. Can you please clarify what is meant by a *bulk sample*?

L102. What constitutes a sample in this case? "Each sample (area depending on particle load on the sample surface) was analyzed…"

The term "sample" was more detailed.

L108. Please clarify what a "ZAP correction" is.

In the original text, I do not fined "ZAP", "ZAF" has been more detailed.

L112. Why are the ZAP applicability conditions not valid in your case?

The reason for the non- applicability of the ZAF – correction has been given.

L121–122. What are the particles (nature, origin) present on the unexposed substrates? Are they biological contaminants, such as skin debris? Please clarify what you mean by *follicles*.

Hair follicles

L122. The optical microscope is not described elsewhere.

The optical microscope has been removed.

L131. A summary table specifying the physical samples analyzed (loose dust and iberulites) would be beneficial.

A table has been added,

L144. How many HYSPLIT back-trajectory calculations were performed? Were they single calculations or an ensemble? Can you say something about the air-mass height at the dust source area?

All in all 18 back-trajectory calculation have been performed: 3 arrival altitudes x 6 arrival times. The airmass altitudes over the Hauts Plateaux are indicated in new figure2.

L146. Please indicate in Fig. 2 where the "Haut Plateau" is located.

The Hauts Plateaux region has been indicated in new figure 2.

L152. Figure 2. Please include a color legend that helps identify the dust plume. Indicate countries or regions of interest on the map.

Old figure 2 has been moved to the supplement.

L155. Figure 3 should be improved for clarity and interpretation. The arrows are difficult to read, and the overall resolution is poor. X- and y-axis labels and titles are missing.

Figure 3 has been removed.

L159. Please clarify what "source sensitivities" means.

"Source sensitivities" has been explained.

L160. Define JFJ here or elsewhere when measurements at Jungfraujoch are described.

JFJ has been defined in the introduction.

L164. Please define what an emagram is or use instead a T/RH profile.

Emagram has been replaced by T/RH profile.

L165. Can you clarify what the *zero-degree level* means?
Zero-degree level has been replaced by 0°C level.

L169. Figure 4. Please improve the figure quality. The panels could be merged into a single plot with a double x-axis for temperature and relative humidity.

Original figure 4a and b (new figure 3) have been merged and improved.

L175. Can the below-cloud reflections located between 2500–4000 m with a streaky appearance be attributed to dust here, or could they correspond to ice particles? In L146 the strong reflection wall from 3500 m to the ground is attributed to dust. What are the differences in attribution between that feature and the reflections observed earlier than 9:00 UTC?

The main feature is the temperature, although the thickness of the melting layer plays also a role (explained in the text.

L176. What is the reason for the 9:00–11:00 ceilometer recording gap?

The gap is due to ground fog blocking the laser.

L180. Figure 5. What do the black dots represent? Please add this information and their location to the figure caption.

The black dots are the lower limit of the meteorological cloud. The information has been added

L183. When were the lidar measurements taken? Is this a single measurement or an average of multiple measurements? If averaged, over what time span? To which time in Figure 5 does it correspond? Please make this connection explicit in the text and figure.

Lidar measurement period has been given in the text and figure caption.

L189. Figure 6. Improve the resolution and add a legend. Include location, time, and other relevant information in the figure caption.

Old figure 6, new figure 5 has been reworked and the relevant infotmation has been added.

L191. I suggest changing the section 3.3 title to something like *Size and morphology of iberulites* or *Morphology and microstructure of iberulites*. Were the St. Légier samples the only iberulites analyzed?

The Iberulite from St. Légier and Fribourg were analyzed. Fribourg was added.

L213. Figure 9b. What do the colors indicate? Please add a legend or clarification in the caption or text.

The different colors are used to better distinguish individual particles. The indication has been added.

L219. Could you clarify what is meant by spherical surface depressions being rare? Are they rare among the collected iberulites, or simply less frequent than those reported by Díaz-Hernández and Párraga (2008)?

The precision has been given.

L221. Please clarify what "long-term particle size distribution measurements at Jungfraujoch" means in this context. The number of acronyms in the first two sentences of this subsection is quite high.

The sentence has been skipped.

L225. I suggest adding a particle size distribution representative of non-SDE conditions for clearer comparison with the PSD shown in Figure 10 and supporting the statements made in L226.

A PSD when JFJ was SDE-free, meteorological clouds-free and above the PBL has been given in new figure 10 (green

bars).

L228. Do you mean dust plumes? If so, please modify.

"Cloud" has been replaced by "plume".

L235 onwards. from the iberulites, and the conversion between d_m and d_{ve} (steps described in equations 1–3), should be better described in the methods or in a methodological appendix to simplify this section.

The equivalent volume diameter and shape factor calculation have been moved to subsection 2.5

L242. What does it imply that the PSD of the iberulite components shows a minimum at the same size range where the aerosol PSD measured at JFJ shows a maximum?

That there is a Greenfield gap as expected for BSC!!

L243. The aspect ratio is not defined, nor is it explained why it is an interesting parameter to analyze.

AR has been defined. It is interesting because the value supports long distance transport for particles.

L255–265. This passage is poorly written and appears unfinished (e.g., "the ratio given by Peng et al. is the reverse of the latter one!" or "has been measured only(?)"). Please clarify how this conversion factor is derived, what value was used in the d_{ve} calculations, and whether the ratio $C_c(d_m)/C_c(d_{ve})$ corresponding to salt particles was used.

This passage has been rewritten and moved to subsection 2.5

L269. The title of this section suggests that the composition of the iberulite components and the dust (at JFJ) will be presented. However, in sections 3.5.1 and 3.5.2 only results on the mineralogy and elemental composition of the iberulite components are shown.

Sections 3.5.1 and 3.5.2 have been reunited into one section.

L270. The title of section 3.5.1 is misleading, since single-particle analysis results are not presented here.

The analysis is for single particles. The sentence has been corrected.

L275. Please improve the legibility of Figure 11. Axis labels, titles, and legend are difficult to read.

Figure 11 has been removed.

L280. Can you clarify what the 1–2 μm resolution in the EDS analysis implies for the interpretation of the results?

It implies, that many of the compositions are mixed particle analyses as stated in the text

L291–299. It is not clear to me what dust the authors are referring to in section 4.1 (dust sampled at JFJ, loose dust collected along with the iberulites, or individual dust particles inside iberulites). Since composition is used to trace the origin of the dust, I recommend rewriting this section and connecting it more clearly with the back-trajectory analysis results.

The subsection has been adjusted accordingly.

L294. It is stated that "The general trends are increases in I/K from S to N and from E to W." What region are the authors referring to here? Northern Africa? Please clarify.

The clarification has been added.

L301. The discussion in section 4.2 presents several problems that should be addressed. First, the actual discussion of the results is obscured by extensive background information that would belong either in the Introduction (e.g., in-cloud and below-cloud scavenging mechanisms) or in methods/supplementary material (e.g., collection efficiency calculations, ice crystal aerodynamics).
The formation mechanism is not presented as a clear step-by-step scenario, and there is no clear logical flow, making the section difficult to follow. Additionally, the main hypothesis (frozen hydrometeor scavenging) emerges only after a long sequence of background explanations.
The discussion would benefit from a clearer structure focusing on the interpretation of the observations and presenting the proposed formation mechanism more concisely. The discussion could be structured around the following topics: observational constraints (meteorology and PSD comparison), evaluation of possible scavenging mechanisms, evidence for frozen hydrometeor scavenging, formation of the core–shell structure during droplet evaporation, and the proposed formation sequence. Organizing this discussion into subsections would be advisable.
Background information and presentation of the scavenging mechanisms have been rewritten and moved to the introduction.

Technical comments

L19. Below scavenging → below-cloud scavenging
Done

L24. I recommend using the term "iberulite constituents" to refer to the particles within iberulites. This may make clearer the comparison between the particle size distribution of atmospheric dust and that of iberulite constituents.

done

L60. Remove the extra closing bracket after ASL.

done

L83. (Ashpole and Washington, 2013) → Ashpole and Washington (2013)

done

L95. “The source meteorological data” → “The dust source area meteorological data”

Done

L97. e.g. → i.e.

rephrased

L242. JFJ → JFJ-measured aerosol
done

L250. d_m is introduced here for the first time. Please define it and clarify that it refers to the mobility diameter measured by the SMPS.

Has been clarified.

L263. Do you mean aspect ratio instead of axis ratio?

Has been corrected.

Reviewer 2

The submitted manuscript “Iberulite fall and formation mechanism during a Sahara dust event in Switzerland in February 2021,” reports on field measurements of iberulite dust particles, discusses formation/transport mechanisms, and suggest the breadth of the underlying phenomenon. Broadly, the manuscript text is well organized, and the data reported is novel. Given the growing relevance of atmospheric dust for human health applications, as well as climatology, I believe the manuscript is within the scope and impact the journal seeks. However, the current organization of evidence in the manuscript, particularly the figures, requires substantive revision. Therefore, I recommend it for publication only after major revision.

Major Comments

1. Some of the claims in the introduction are not detailed enough and seem more like bullet points or sentence fragments. For example, in Line 37, you simply write “Dust also affects climate and ecosystems.” There is no interpretation of what this means physically/chemically/biologically, what the cost is (monetary and/or health), and most importantly why the community should care. Please restructure the first paragraph to give more concrete examples of why desert dust transport is important. It is not necessary to make it much longer, but rather more centered on meaningful claims backed by the literature.

Details and references regarding health, climate and ecology, as well as monetary consequences of dust intrusions have been added.

2. The second paragraph of the introduction is poorly organized. Please split into three paragraphs: (1) paragraph explaining what Iberulite is, and its relevance, (2) SDEs in Switzerland, monitoring infrastructure etc., (3) introducing the hypothesis, study design, and research goals.

The second paragraph has been reorganized.

3. In Methods section 2.1, some mention/citation/discussion of standards in dust collection is needed. Are the methods used considered “state-of-the-science”? Why are they different What implications does this have for study credibility?

The sampling method is not standard, but loss of particles in the transfer from the foil onto a filter is negligible. The statement and other domains (space science), which use sampling of particles on foils, have been added.

4. Section 2.3 (“Scanning Electron (SEM) and Optical Polarization Microscopy (OPM) analyses”) of the methods is poorly organized, and difficult to understand the relevance to the study. I recommend 1) briefly describing the method with citations, 2) if any section is different from typical literature methods describe why and how it impacts the findings, 3) move all other technical details to a supplement/appendix (since they are not novel nor directly relevant). In this way, you can combine with section 2.4, for a single concise and readable analytical methods section.

I do not think that CCSEM is a standard method. Therefore I would prefer to maintain the secion. Particle

analyses is special, because of the deviation from common sample geometries (flat surface). All the papers I looked up in AS describe the SEM and TEM methods within the paper, not in an appendix. Our description does not differ much from the latter.

5. Figure 1 is not appropriate for scientific journal articles. Figure 1 is a diagnostic plot automatically generated from HYSPLIT, it is both missing key information (e.g., axis labels on the line plot in the middle panel), and contains extraneous information (e.g., "Job ID"). Either extract the relevant data to combine with the suggested map of measurement locations (see Minor Comment #3), eliminate, or move to supplement (with a note that it is an automatically generated diagnostic plot).

The old figure 1, new figure 2, has been improved accordingly.

6. Figure 2 is not appropriate for scientific journal articles. There is no legend presented, so it is impossible to interpret the plot. Readers cannot tell what units are used (if any), the geographic scale, or many other necessary components. Please remove. If you want to show the data, I recommend a simple time series of the dust product in the supplement.

Figure 2 has been updated with a legend, geographic indications and a scale.

Figure 3 is not appropriate for scientific journal articles. The resolution is bad – it is blurry. Although it at least has an appended legend, the figure is difficult to interpret and does not directly support the arguments of the paper as well as the trajectory analysis or analytical methods applied to samples do. I recommend it to be removed entirely.

Original figure 3 has been removed

7. Figure 4 requires significant revision. The figure resolution is bad, too blurry. Why are there breaks in the horizontal lines from y-axis in panel a? If the figures are sharing a y-axis, generally, the y-axis labels should be on the left-most figure, not the right figure. The green line in panel a should also be removed.

Original figure 4a and b, new figure have been combined and redrawn.

8. Figure 5 requires significant revision. I strongly encourage the authors to review the figure and illustration guidelines from the publisher. The colormap used is unscientific (i.e., biased in color intensity), and therefore misleading (see http://blogs.egu.eu/divisions/gd/2017/08/23/the-rainbow-colour-map/). The figure labels are not thoughtfully placed, and are distracting (e.g., "cloud base" has a dotted line running though it; "lower thick SD level" looks like it may be covering important data). I recommend extracting the underlying data, plotting anew, and carefully placing labels to ensure no overlaps.
The figure has been turned to grey scale and most of the information in the figure have been moved to the caption.

9. Figure 9 only referenced once, and it is not essential to the arguments being made. Please remove.

I do not agree, the BSE image and the AVIZO segmentation of it are important to extract PSD of the particles inside an iberulite, the key argument to prefer BCS over ICS as formation mechanism in the presented IF.

10. Section 3.4 ("PSDs at JFJ and inside the iberulites") does not flow well either internally or with respect to the rest of the paper. I recommend citing any routine calculation (e.g., equivalent volume diameter), moving any non-routine calculations to their own section of the methods section, and simply give the results with a concise description here.

Calculations and citations have been moved to section 2.5.

11. Figure 11 is not appropriate for scientific journal articles . It looks like another diagnostic plot with overlapping figure captions. The text on the figure is also too small to easily read and grasp the meaning of the figure. I recommend moving to a supplement or appendix after fixing the text size and captions.

The figure has been moved to the supplement.

12. Figure 12 seems like the most important figure but requires significant revision. The it suffers from the same problems as the other figures: the text is unreadable, bad resolution, unclear units, seems

automatically generated rather than extracted data. Please follow the feedback from the other figures and apply here.

I have a problem with the figure numbering. Probably you mean original figure 10, looking at the placement of the comment. New Figure 9 has been enlarged with larger labels.

13. Section 4.1 ("Dust Composition") is just literature review, not needed in the Discussion. Remove it, or fold it into the introduction section.

The literature review has been moved to the introduction.

14. Section 4.2 ("Iberulite formation and internal structure") needs to be reorganized.

    1. Line 302-303: what does it mean that the IF was "observed in person" – does this mean that there is some qualitative flag recorded? Please elaborate.

       We elaborated what "in person " means.

    2. Lines 304-307 restate what has already been mentioned twice before, please make more concise.

    The description of the two iberulite formation mechanisms has been moved to the introduction.

    3. Lines 315-324 muddle the arguments which follow by introducing yet another equation that is not explored, plotted, or constitutes a novel result.

       The equation has been removed.

    4. Lines 326-337 should be their own paragraph, please try to refrain from introducing too many new concepts

       See Lines 304 - 307.

    5. Lines 338-340 is built on anecdotal evidence ("some iberulites striking car windshields were still wet"). Please use some of the abundant meteorological data in the paper to support this finding. Anecdotal evidence is notoriously unreliable; there could be multiple other explanations for wet windshields.

       The windshields were dry; it was the iberulites that were wet, and when they hit the windshield, they were squished. The same information has been given in the same wors by Diaz.Hernandez and Parraga. We can add a picture when necessary. We added data (supersaturated layer between 5000 and 5100 m ASL, which indicates the probable origin of the frozen hydrometeors.

    6. Much of this section reads like an outline or informal note rather than a scientific journal submission. The parenthetical notes on lines 353, 357, and 359 as well as the general sentence structure is excessively informal or fragmented.

    7. Pages 19-20 introduce many new concepts and literature review. Please condense considerably to only the most essential points regarding iberulite formation.

       6. +7. Background information and presentation of the scavenging mechanisms have been rewritten and moved to the introduction.

    8. The last paragraph of the section (lines 397-403) is well-written and neatly summarizes many of the concepts necessary to understand the field observations. Please eliminate the extraneous concepts in the section unnecessary to understanding this paragraph.

    The extraneous concepts have been eliminated

    The acknowledgement section seems to suggest that this manuscript was a rough draft or outline. Why are there question marks for some contributors in lines 426-427? Please give a very close

reading before re-submission to ensure unnecessary figures are eliminated, sentence fragments are rectified, extraneous information removed, and data is presented in the correct section.

Minor Comments

1. At lines 34-37, you claim aerosol concentrations during these events often exceed air quality standards, it would be useful to the reader to first state what the standards are (Annual? Daily? Hourly?), what an example concentration looks like (and at what scale – hourly? Weekly sustained?)

   This information has been added.

2. You conclude your first paragraph with "dust mineralogical studies are scarce," a reasonable statement, but support with a paper from 2010. Has there been any growth or decline in atmospheric mineralogical studies in Europe or elsewhere? A more recent review is warranted.

   A more recent reference has been given. The last published reference (Jeong, 2024) states, what Engelbrecht et al. already said 14 years before: " Modeling the effect of mineral dust on the environment requires a consistent data set of mineral composition and properties (Claquin et al. 1999; Scanza et al. 2015), which has not yet been established due to the very low quantity of samples, the lack of systematic analyses and interpretation, and the poor mineralogical information on the soils and sediments in desert sources. "

3. Line 65: "see below" – what does this refer to? Use figure numbers

   "see below" has been skipped.

4. There are many collection locations to keep track of in section 2.1. A map with these locations and their terrain either here or in a supplement would be useful.

   A map has been added (new Figure 1).

5. Line 71: looks like two different sections called "2.1," please make second "2.1" into 2.2

   The section numbering has been corrected

6. In Line 84, please cite the relevant peer-reviewed article on the satellite rather than sharing a link, and briefly describe its relevance.

   The link has been replaced by a reference and the dust detection mechanism described.

7. The paragraphs in Section "Meteorological Data" gives too many abbreviations for unnecessary terms, e.g., "NOAA" or "ARL." Simply cite the papers, and use the full form for terms used only a few times.

   The abbreviations have been removed.

8. Figure 6 requires some revision. Again, the resolution is bad – too blurry. The figure title is irrelevant, please remove or make it more easily readable. Also consider removing the blue line, and simply keeping the uncertainty band, as it more convincingly tells the story.

   The original figure 6 has been reworked. The new figure has no title and no blue line

9. Either remove Figure 7, or move to supplement/appendix.

   It is hard for me to remove figure 7. The latter is the best way for the reader to get, what iberulites are.