Dr. Erik Aspaugh
Associate Editor
Meteoritics & Planetary Science

Friday, 08 February 2002

Dear Dr. Aspaugh:

We appreciate the detailed reviews by the two referees (Aurell and Ernstson) to our manuscript on the Azuara structure. In spite of our disagreement with many of the comments by Ernstson, we have responded to all of them, and tried to consider the corrections and suggestions provided by both reviewers. At the same time, whenever we disagreed with their opinion, we have tried to justify our position, as specified below.

REPLY to COMMENTS by M. AURELL

We thank Aurell for his review and recommendations. Marc Aurell is very much aware of the controversy on the Azuara structure, since he has been working in the region since the early 80's.

1 and 2. OK
3. OK. The objective of the "Future Works" paragraph is to specify those aspects which need further work in order to fully confirm the origin of the structure (although each party in the controversy is convinced that the origin is already fully confirmed!). This further work should be focused on the chronostratigraphy, biostratigraphy and sedimentology of the sedimentary units involved in, and resulting from, the alleged impact cratering process. In addition, geochemistry should be applied in those complex units or geological structures (e.g., dikes, breccias) in the Azuara structure that may have been enriched in siderophyle elements during impact cratering. So far, no clear and unequivocal cosmogenic geochemical anomaly has been identified. All this evidence should aid in the decission about the origin of the structure.
4. OK. Corrections and additions to the text were fully considered.

REPLY to COMMENTS by K. ERNSTSON

A. A few explanatory notes are due beforehand. Our manuscript attempts to summarize the current status about the controversy on the Azuara structure, including at the same time our own interpretations. In the particular case of the Azuara structure, and due to our own experience and observations, we are biased towards a tectonic interpretation, and this is clearly said in our manuscript from the very beginning. As proven by Ernstson's review of our manuscript, **the discussion on the Azuara structure cannot be considered as finished, because the controversy is ongoing, and so will be as far as there are researchers with different opinions**. We say this because Ernstson frequently rejects to even acknowledge that there is a controversy. However, we feel that **the root of this controversy is in the lack of information**, which does not allow impartial researchers who are foreign to the subject to approach it without prejudice and enough background knowledge. On one side, the opinion and interpretation of all those clearly against the impact hypothesis (most of them Spanish geologists, but also including renown impact researchers in Europe) has rarely been published as such in international journals, mostly because they think that it is a waste of time having to prove what is obvious [exceptions are Aurell et al. (1993) and Langenhorst and Deutsch (1996)]. On the other side, the opinion and interpretation of those researchers clearly in favor of the impact hypothesis (mostly just Ernstson and co-authors) was published in international journals which --as it should be-- are the basic source of information for researchers outside Spain. Although we cannot prove it (we do not know who they were), it is our feeling that these latter publications were reviewed by colleagues who were not fully aware of the incongruencies and incompatibilities between the impact hypothesis and the present regional geology, and that they based their review just on the reliability of the content of the article. **We therefore request an opportunity to make the alternative "terrestrial" interpretation widely known to the scientific community in a journal which is appropriate for the subject, such as Meteoritics**. Even if publishing our manuscript does not make Ernstson happy (as it seems from his review), and however inconvenient its publication may be to his reputation, he should at least agree with us in several basic scientific principles: <u>parsimony</u> (Ockham's razor: to choose the simplest explanation using the fewest assumptions), and <u>uncertainty</u> (skeptical and doubting attitude: the truth of all knowledge must always be in question). In the words of Michael Schermer: "Critical feedback is the lifeblood of healthy science, as is the willingness (however begrudglingly) to say 'I was wrong' when faced with persuasive evidence. It does not matter who you are or how important you think your idea is -- if it is contradicted by the evidence, it is wrong. ... Unwillingness to submit to peer review and inability to admit error are the antitheses of good science". We also want to apply this concept to us, as it should be with anybody envolved in a scientific controversy.

Comments (Ernstson):

1 The Cortés et al. paper reminds me of an impact workshop I attended in South Africa in the mid eighties, when the giant Vredefort structure became generally accepted as an impact structure. It's true the meeting was dedicated to impact, but in fact it was used as a stage for most local geoscientists to doggedly defend their models of an endogenetic origin of the Vredefort ring. The reputable Prof. Nicolaysen didn't consider himself too good for presenting a paper on a volcanic-tectonic origin of the Ries and Steinheim impact structures, the nice Vredefort shatter cones suddenly changed into shatter "pyramids" (to suggest a tectonic origin by jointing), and South African reviewers of submitted proceeding papers required that established impact craters like the Ries had to be termed as "proposed" or "possible" impact structures.

2 The Cortés paper reminds me also of an article published at about the same time by the reputable journal "Die Naturwissenschaften". In this article, a geology professor from New Zealand (I forgot his name) claimed an endogenetic origin for most of the impact structures established at that time, the Barringer crater in Arizona included. The iron meteorites found in large quantities around this crater were considered by him as material originating from the deep mantle.

B. The preamble provided by Ernstson to his comments is absolutely out of place. We consider that the first two paragraphs are not appropriate for the purpose of evaluating the rigour and scientific quality of our manuscript. This is because during the history of science, there have been many occassions when hypotheses have been questioned (as it should be). Ernstson provides several examples of verified impact structures also being given an alternative endogenetic interpretation. As he probably knows, there have also been cases in the opposite direction (terrestrial endogenetic structures being given an alternative impact cosmogenetic interpretation). Although the issue here is not a matter of finding examples from the history of science, we have another example of what recently happened at an impact workshop, which Ernstson should know about because it envolves himself:

During the 6th Workshop of the European Science Foundation IMPACT Programme (Granada, May 2001), K. Ernstson and F. Claudín organized and guided a pre-workshop fieldtrip to the Azuara structure in which we helped with the logistics. A total of ten foreign (non-Spanish) impact researchers participated in it, together with the organizers, two of us from the Center for Astrobiology, and five geologists from the nearby University of Zaragoza. A fieldtrip guide favoring the impact hypothesis, written and edited by Ernstson, Claudin and Rampino, was provided to all the participants. During this pre-workshop fieldtrip, discussions took place on the outcrops, with arguments heard from both sides of the controversy. During the workshop, posters relating to the structure were presented from both sides of the controversy, and on the last day of the workshop, the program included a round table about the Azuara structure, with C. Koeberl and one of us (E.D.-M.) as moderators, and with several short participations planned from both sides of the controversy. One day before the round table, Ernstson and Claudin refused to participate in it, and one of their collaborators (Rampino) had to improvise a defense of their hypothesis. At the round table discussion, several participations against the impact hypothesis came from Spanish geologists, as well as from several impact specialists. None of the participants in the pre-workshop fieldtrip showed a fully convinced opinion about the impact hypothesis, or at least they were not willing to speak in its favor at the moment (except for M. Rampino). At the closure of the round table discussion, the final conclusion about the issue was that the evidence for the impact hypothesis (mostly just the shock metamorphism) is not convincing, that more research is still needed before it can be considered a verified impact structure, and that this research should especially be focused in the alleged ejecta, as well as drilling in the structure.

This is not the first time that Ernstson avoids public confrontation with a scientifically knowledgeable audience when given an opportunity to defend his hypothesis. In accord with our own field observations and analytical results, and those of other researchers, **we maintain the point of view that the Azuara structure is not impact-related**. If anything at all, it may be considered as a possibility still to be confirmed, but not as a verified impact structure.

> 3 Similarly, a cosmic origin for the roughly 170 now established impact structures may today be questioned by simply ignoring basic knowledge of impact cratering processes, shock metamorphism and astronomical evidence. And in most cases, geologists may point to the regional geologic setting apparently not compatible with an impact. A cosmic projectile impacting the earth, however, does not show any consideration for regional geology.

C. Concerning this third paragraph of Ernstson's preamble, we totally agree with his first assertion, which is only true when impact metamorphism has been confirmed. However, this is not the case when speaking of the Azuara structure (we shall not deal with the rest of the "roughly 170 impacts"). It is obvious that the location of an impact is independent of the previous regional geology (there is no question about that), but **after the impact occurred, the regional geology should show the results of the impact**. This is the key issue. The basic problem in the case of the Azuara structure is that **there are many features resulting from an impact which should be expected but are not present, and there are many features which should not be expected in the case of an impact, and which are certainly present** (e.g., growth strata and progressive unconformities in sedimentary units related with the deformation allegedly attributed to the impact). These facts cannot and should not be ignored.

> 4 To put this - evidently yet ongoing - discussion on an orderly basis, impact researchers generally make a distinction between established impact structures showing clear shock metamorphism, structures where further investigations are needed, and structures which are obviously better explained by an endogenetic model.

> 5 Correspondingly, since about 15 years, the Azuara structure in Spain is included in maps and lists of established impact structures, due to the occurrence of shock metamorphism.

> 6 Different from this, Cortés et al. believe that Azuara is obviously better explained by an endogenetic origin, and the primary aim of their paper is to eliminate Azuara from these maps and lists.

D. Because we consider it inappropriate, **we will not deal with the permanent disqualification that Ernstson makes of other researchers who disagree with him**, not only throughout his comments to our manuscript, but also very frequently at scientific meetings and in the field (many researchers can testify to this), as well as in his personal messages (M. Aurell can testify about this, and we have a copy of the message that Ernstson sent to him). Only peer review and time will allow to discern who is right. At the moment, **our sole intention is to be able to publish our summary in an appropriate and widely distributed journal related with the subject (and Meteoritics seemed the most appropriate), in order to make the information available to other colleagues**.

E. With regard to his 5th and 6th paragraphs, the Azuara structure was included in world impact maps and lists following the publication of the impact hypothesis in a renown journal (EPSL) in 1985. Ernstson's article was then considered reliable enough by R. Grieve to have the structure be incorporated into his database and maps (Grieve, 1987, 1990). This database has been used by many different authors ever since without much cryticism (almost as a dogma), until Montanari and Koeberl (2000) reviewed the evidence and decided to withdraw it from their list (although not from their map in Fig. 1.3.1!). **Our primary aim is not to withdraw the Azuara structure from these database, which has already been done.** Our primary aim, as we previously stated above, is to summarize the current status about the controversy on the Azuara structure, including at the same time our own interpretations, which support its removal from impact databases. See also our points B, C and D above.

7 This is because in their opinion, the Azuara impact is in competition with many previous investigations on the regional geology in the Azuara zone: always the same old story. Therefore, in their ms on Azuara, they try to "replace" practically all observations compatible with an impact and well known from other impact structures by conventional non-impact explanations (karst fissures and paleosol development instead of impact breccias and breccia dikes, caliche (calcrete) instead of shock decarbonation and globular-breccia formation, dissolution collapse instead of mega-brecciation, volcanic ash instead of impact melt, pressure dissolution instead of spallation by dynamic collision of conglomerate cobbles, mineral and rock defomations by tectonic stress instead of shock metamorphism, and many more) - see my detailed comments on their review.

F. With regard to his 7th paragraph, we do not "try to replace all observations compatible with an impact". What we do is debate those observations which have an alternative terrestrial (non-cosmogenic) interpretation, because **most of the observations and features proposed as evidence for the impact hypothesis had previously been interpreted in a "conventional way"** (i.e., non-impactogenic) in an important number of scientific publications written during the 19th and 20th centuries about many different aspects of the Iberian Chain and Ebro basin, and including the Azuara area (stratigraphy, paleontology, structural geology, geomorphology, etc.). Ernstson and his collaborators present some of the previously known features, and reinterpreted them as related to an impact.

8 Summarizing these roughly 80 comments, the "Review" proves as an accumulation of untruths and half-truths, omissions, misrepresentations, and insinuations, in a concentration I never before read in my about 30 years academic career. This is anything but a fair, concise, exhaustive, professionally sound review of ***both*** models for the origin of the Azuara structure. Instead, the authors try to make the impact advocates out to confuse things, to be ignorant in general and to be ignorant especially of the regional geology. The most intriguing impact evidences are not quoted, or they are brought into a wrong or misleading context. Their text lacks any descriptions, analyses, proofs to show that their endogenetic model of a tectonic structure should seriously be considered. They are writing that they made new investigations (e.g.,of the Pelarda Fm.), but they do not report about the results. They always say that the impact group confuses observations, but this assertion is supported by nothing.

9 Altogether, this is not a scientific process but really very bad scientific style. Therefore, in the present form, the ms of Cortés et al. is not suited for publication in MAPS. A re-submission could be considered, if the authors take to heart the following comments and if they present a completely revised paper

With regard to his 8th and 9th paragraph, which deserve no comments, see points B, C and D above, and the following more detailed explanations.

As a concluding remark about Ernstson's comments, we are grateful for those which attempt to improve our manuscript, but we also believe that most of his other comments simply attempt to justify its withdrawal, and to perpetuate the controversy on the subject. We hope that this is not because the publication of our manuscript may not be convenient for Ernstson's credibility.

Reply to Ernstson's numbered comments:

1 adequate, but not concise summary of the paper

1) We believe that the abstract is adequate, concise, and to the point, summarizing the main conclusions.

> 2 incorrect; Azuara is recorded as a verified impact structure on all international maps and lists, except for the Montanary & Koeberl listing.

2) See our point E above. Montanari & Koeberl (2000) are a good example of what happens to other impact researchers. It is much easier to borrow a previously made map and to incorporate it in our work (of course, with due citation of the reference), than to check each and every bit of information shown on it, in order to make a new one from scratch. Montanari & Koeberl (2000) actually include the Azuara structure in one of their figures (Fig. 1.3.1). However incongruent as it may seem, they also present a reviewed list of impact structures on Earth, younger than 250 Ma and larger than 5 km in diameter, which does not include Azuara. This is simply because the authors do not consider Azuara as a verified impact structure (independent personal communications from A. Montanari and C. Koeberl), and that is what they suggest to potential readers and researchers thereof. It is important to point out that, except for Ernstson and his collaborators, no other group unrelated to them has published research about the alleged ejecta and impact melts of the Azuara structure. We believe that the use of the impact database of R.A.F. Grieve has not been crytical. This is clearly because no impact researcher is willing to verify the evidence presented for every single one of the impacts on the map. So once an impact structure is in the database, it is difficult to be withdrawn from it, in part because the use of Grieve's database and map is already so widespread.

> 3 only by Spanish geologists having worked in the Azuara region, and by Langenhorst & Deutsch; see comment 72.

3) Although opinions against it have rarely been published (see our point A above), the impact hypothesis has been questioned and debated at many international meetings. It is also indirectly questioned by those researchers who, despite knowing about it, ignore it in their studies of the area because they think it is inconsistent and inappropriate.

> 4 inadmissible comparison with the Ries crater; see comment 11

4) We believe that it is perfectly appropriate to compare the Ries crater with the Azuara structure with respect to their size and the features to be expected in case the latter was impact-related. We would never say that they are exactly the same: it is obvious that they are not. The Azuara structure might be considered older by Ernstson, but it is certainly bigger, and one would therefore at least expect some evidence for cratering, development of inner ring, and shock metamorphism, such as what happens at Ries. See also our reply to point 11.

> 5 debate solely among the Ernstson working group and Spanish geologists.

5) This is not true: there are also other European and North American impact researchers involved in the debate (see our points B, C and D above).

> 6 Following the argumentation of Cortés et al., most impact structures recorded on the official lists and maps should be removed; see comment 72.

6) The real problem is the incorporation of alleged impact structures into the widely used "official" lists and maps, when this incorporation is based on poor or controversial evidence. We believe this is the case with the Azuara structure. What happens now is that it is even more difficult to disprove (and few researchers are willing to spend their time doing so), when the appropriate methodology should have been to proceed with a thorough and unbiased verifying before it was included in the list. Most researchers would agree that a structure should only be added to the list when clearcut and inequivocal evidence for shock metamorphism is found. If this ultimately means that most structures must be removed from the impact list, then that is how it should be, and we should not be afraid of it, but instead, proud to comply with basic scientific skeptic procedure. However, we know that this will not happen, because the overall norm is to follow strict independant and unbiased scientific procedure.

> 7 According to the impact model, the origin of these deposits must be revised, because they show typical ejecta features.

7) According to the impact model of Ernstson, the whole Tertiary stratigraphic sequence of the Ebro Basin (which includes the Azuara Basin), and of the adjacent Calayatud-Montalbán Basin, should be completely revised. During the last century, there have been hundreds of detailed stratigraphic and sedimentologic sections studied (both by European and North American geologists) in these deposits. The petrology, sedimentology, stratigraphy, and depositional architecture (amongst other features) of both the Ebro and the Calatayud-Montalbán basins are already well understood and established. The biostratigraphy, magnetostratigraphy, isotope geochemistry and radiometry have demonstrated the accuracy of the established stratigraphic units. All profiles are correlated and stratigraphic units are relatively well dated (by isotopic, paleontological and/or magnetostratigraphic methods). We think that Ernstson's comment is not appropriate. In fact, our believe is that what Ernstson calls "typical ejecta features" are not such things, but instead equivocal features with an alternative interpretation, generally related with tectonism (regional and local stress and pressure changes, deformation, dissolution, etc.).

8 add Ernstson (1994) and Mayer (1990) with the report of diaplectic quartz.

8) OK. Corrected.

9 Table 1 was not performed by Aurell et al.! Also see comment 11.

9) OK. Corrected.

10 The argumentation of the incompatibility of the regional geology with an impact is as old as the discussion about geologic ring structures being possible impact structures. A cosmic projectile impacting the earth does not show any consideration for regional geology.

10) Deserves no further comment. See our points B, C and D above.

11 incorrect statement. nowhere in both articles, Azuara is explicitly compared with the Ries crater. Nowhere in both articles, a similarity between the Azuara and Ries structures is pointed out.

11) We absolutely disagree with Ernstson. Our statement is not incorrect. In fact, Ernstson and colleagues have frequently compared the Azuara structure and the Ries crater in their papers in an explicit way, or cited the Ries crater and its impact features as examples to be compared with in relation to the Azuara structure [e.g., Ernstson et al. (1985: p. 367, 368), Ernstson and Claudín (1990: p. 593, 594), Ernstson and Fiebag (1992: p. 406, 409 (fig. 7), 415]. This is also true of what we have heard from him during field trips, scientific meetings, and even in his current comments about our manuscript, as follows:

- 1st paragraph: "established impact craters like the Ries..."
- comment 17: "…characteristics very similar to Ries and Rochechouart impact suevites…"
- comment 31: "…quite similar deformations in the Ries and Chicxulub impact ejecta…"
- comment 55: "…with similar formations in the Ries crater…"
- comments about Fig. 3: "Very similar deformations are observed in the rim zone of the Ries impact structure…"

When we speak of comparison we are using the word in its two meanings: (1) to consider or describe as similar or analogous, and (2) to examine in order to note similarities or differences. We never attempted to mean that they are equally the same, and this is obvious from our Table 1.

Table1: bad scientific style to compare Azuara with the Ries crater (it's like comparing apples with bananas). The Ries is a morphologically and structurally well preserved impact structure in a mixed crystalline-sedimentary target. The Azuara structure, on the contrary, is a deeply eroded structure in a pure sedimentary, mostly carbonate target. All shock effects in the Ries crater, microscopic and macroscopic (shatter cones), have been reported only for rocks from the crystalline basement. Serious impact researchers, e.g., know that shock metamorphism in sedimentary targets (especially in carbonate ones) is strongly reduced compared with crystalline targets.

Table 1: The sentence "it's like comparing apples with bananas" is not appropriate for a serious and scientific review. In any case, both apples and bananas are fruits from plants, and therefore can be compared in order to note similarities or differences between them. An important concept that Ernstson seems to disregard throughout his research is that things that are similar do not necessarily have the same origin. Different processes and conditions may lead to the same or similar features and morphologies. See also our points B, C and D above, and our reply to his comment 11.

12 Meteor crater in Arizona also has a polygonal shape. Cortés et al. must not forget that the Azuara structure is deeply eroded and may have been overprinted by post-impact Alpidic tectonics.

12) Meteor crater presents a quite well-rounded polygonal shape (roundness, as opposed to angulosity). However, as opposed to other Cenozoic impact structures, a mere observation of a topographic or geologic map of the Azuara structure allows to see that it is far from being rounded. In the text we insisted in this fact by saying that it is polygonal. In fact, the limits of the structure are far from being clear, because of the Neogene infill and Quaternary cover of the basin. In their earlier publications, Ernstson and collaborators said that it "is circular or near circular", and used this as part of the evidence towards their interpretation. Furthermore, if the Azuara structure were really deeply eroded, as Ernstson says, then it would not have such a thick infill of Cenozoic deposits over its inner ring.

13 Here, Cortés et al. withhold from the reader that a central uplift and inner rings may be burried by the post-impact deposits of several 100 m thickness or more. Moreover, the gravity measurements in the Azuara structure suggest the existence of an inner ring which is explicitly discussed by Ernstson & Fiebag (1992, p.407). Why don't Cortés et al. mention this? Very bad scientific style.

13) The interpretation of the gravity data by Ernstson and Fiebag (1992) was discussed by Aurell et al. (1993). Gravity measurements performed by Ernstson and his group may be correct, but the analytical procedure and interpretation of the structure from these data is questionable. The reason why we do not mention their interpretation is because it is not well founded (we provide more details in our reply to Ernstson's comment no. 46).

14 No central uplift in the Ries crater! Age: What is the relation between the age of an impact structure and its central uplift/inner ring?

14) Our sentence refers to verified impact craters in general, and that is why we say "…central uplift and/or inner ring…". The Ries crater does not present central uplift, but does present inner ring. **About the age, we agree with his comment** and proceeded with the suggested correction (it was an error on our side). We agree that there is no direct relation between the age of an impact structure and the presence of a central uplift/inner ring. However, an indirect relation may be found due to the degree of erosion or covering of the crater, which in general will increase with the age, resulting in the progressive obliteration of the central uplift/inner ring. That is what we were referring to.

15 "The two lower units (T1 and T4) ..." are in fact what according to the impact model are the suevite-like basal breccia and related impact deposits. Cortés et al. must clearly discuss why the unambiguous breccia deposits (see Ernstson & Fiebag 1992, Fig.14, A-D) are regarded by them as alluvial fans and lacustrine sediments.

15) We believe that **the geometry and syntectonic character** of the Cenozoic sedimentary units within and around the Azuara structure **is unambiguous** and beyond any alternative interpretation: growth strata and progressive unconformities simply cannot develop by a single and almost instantaneous process like an impact (see also our points B, C and F above). **These geometries have been observed and interpreted** without any ambiguity in terms of terrestrial tectonics, and within the geological context of the Cenozoic basins of central and northeast Spain. The age obtained from the study of the paleontological sites is also unambiguous (at least within the present state-of-knowledge in paleontology and biochronology). The Ebro, Calatayud-Montalbán and Teruel basins are very important in the western Mediterranean area for biostratigraphic and magnetostratigraphic studies already published and approved internationally. Most of this information is systematically ignored in the work of Ernstson and collaborators. Why are we supposed to accept Ernstson' s impact interpretation of these deposits if so far we have not seen any unambiguous data supporting his interpretation? We do not argue against the breccias being breccias, which they are. What we argue is against the interpretation of the breccias: there are many types of breccias, and with many different possible origins. In the case of units T1 and T4, we interpret these breccias as part of proximal alluvial deposits related with local nearby reliefs developed during tectonic deformation.

> **16** Perez et al (1995) recognize the difficulties to date the Tertiary deposits in the Azuara zone due to sudden lateral changes without clear index strata. This implies basic difficulties with the dating.

16) Rapid lateral facies changes are a common feature in continental basins. This is why Pérez et al. (1995) had some difficulties dating Miocene deposits within the Azuara Basin. Nevertheless, Usera et al. (1991) found gastropods and foraminiferal tests characteristic of the Early Miocene in lacustrine deposits within the Azuara Basin (Fuendetodos and Moyuela localities): *Potamides* sp., *Rosalina douvillei*, *Bolivina* sp., *Elphidium excavatum*, *Amonia beccari*. In addition, abundant biostratigraphic data from paleontological sites in the adjacent Tertiary basins (where proximal ejecta from Azuara impact should be expected to be found) results in well dated stratigraphical sequences.

Due to this rapid lateral facies changes resulting from Miocene continental paleogeographic environments, it is also somewhat difficult to date the Miocene deposits towards the center of the Ebro Basin. Detrital-carbonatic-evaporitic lateral changes are frequent from the borders towards the center of the basins (as is common in semiarid endorheic basins). As biostratigraphical and paleoecological studies show, most of the paleontological sites in endorheic basins are located in the transition from distal alluvial fan to lake marginal facies, but not in the central deeper lacustrine paleoenvironments. In any case, there is a large number of both well studied paleontological sites, and correlated stratigraphic profiles (frequently bed-to-bed) along the southern border of the Ebro Basin, which allow to characterize the age of the outcropping deposits (see Pérez, 1989; González, 1989; Muñoz-Jiménez, 1991; Villena et al., 1996; etc.).

In any case, there is no doubt at all about the age of the detrital and lacustrine deposits which were folded adjacent to the main anticlines located along the borders of the Azuara basin. At Aguilón and Tosos, the Tertiary basal unit (T1) contains *Microcodium* and *Vidaliella gerundensis* (Pérez, 1989), typical of Paleocene deposits. The base of the Oligocene-Miocene syntectonic unit (T4) was dated by Pérez et al. (1985) at the Las Torcas paleontological site as Arvenian (Chattian = Upper Oligocene). At Villanueva de Huerva, unit T4 is covered by unit T6 (dated as upper Aragonian = Middle Miocene in the Villanueva de Huerva paleontological site; Pérez et al., 1985; Pérez, 1989). These data allow to date the main folding deformational phase in the Azuara area as Late Oligocene-Early Miocene (see Pérez, 1989; Cortés et al., 1999), an age similar to the ones identified for the main deformational phase in nearby zones of the Iberian Range.

> **17** Here, Cortés et al. must discuss the widely exposed basal breccia which has been related to the Azuara impact by Ernstson & Fiebag 1992, Fiebag 1988, Katschorek 1990, Mayer 1991, Müller 1989 - all listed in their ms. These papers describe the basal breccia (which Cortés et al. obviously confuse with conglomerates and fanglomerates) as showing textural and microscopic characteristics very similar to Ries and Rochechouart impact suevites, with the only exception that the basal breccia is predominantly composed of carbonate components.
>
> Also, the "fanglomeratic unit" near Almonacid de la Cuba has been attributed to the Azuara impact by Katschorek (1990). Cortés et al. should refer to this unit because it is interpreted by Katschorek as an impact equivalent to hot, volatile- and melt-rich volcanic mud flows.

17) Textural similarity is not evidence for an impact. We agree that the breccia is widely exposed, but disagree in its interpretation. See also our points B, C and F above. We also disagree with the interpretation of Kaschorek (1990).

18 The paper by Ernstson & Fiebag (1992) has been published as a progress report summarizing the results of the many theses hitherto made in the Azuara structure. In these theses, precise UTM coordinates of all dislocated megablocks are given with a detailed description of the facies, age considerations, and the results of geophysical measurements of the dislocated megablocks. Cortés et al. should know this.

18) Access to the complete information is not easy and free to the public, since it is unpublished material (theses). In any case, no clear location or convincing photographs have ever been provided by Ernstson's group in their international publications. If this information is supposed to be considered so important, clear and convincing, Ernstson and colleagues had many good opportunities to present it in their previously published works (1985, 1990, 1992, 1993, etc.), but never did. We cannot understand the publication (and acceptance) of such an important interpretation as "the Azuara impact" without these data being published, and which still remain resting in unpublished theses. See also our points A, B and C above.

19 The impact model does not exclude block faulting and folding by Alpidic tectonics; not a convincing argumentation by Cortés et al.

19) May be the impact model does not exclude block faulting and folding, but it does not explain the particular style of faulting and folding attributed to the impact, nor does it explain many other features observed in the Azuara area (such as the previously mentioned growth strata and progressive unconformities related with the faulting and folding). In contrast, the present-day structure is more easily explained as the result of Cenozoic tectonic deformation, a model that integrates most observations and unequivocal evidence present in the region. See also our points C and F above.

20 again: A cosmic body does not show any consideration for regional geology!

20) Again, we never said or attempted to imply that a cosmic body will show preference for any particular site. See our points C above.

21 The up to 400 m thick Pelarda Fm. is a diamict deposit which can be subdivided into three parts (lower, middle, and upper). In the middle and upper zones, well rounded quartzite clasts can be observed, however only 30% as a maximum of the total amount. The normal aspect of the components is subrounded or subangular!

21) We believe that our description of the Pelarda Fm. is truly objective according to our own observations and previous descriptions in the literature. In addition to our interpretation of the Pelarda Fm. as proximal alluvial deposits with a southern or southwestern source area, there is another aspect of discussion: its thickness. Ernstson and collaborators have increased the thickness of this unit in more than 200 m during the last months! In previous publications (e.g., Ernstson & Fiebag, 1992)., the Pelarda Fm. was defined as follows (p. 412): "*Extended (c. 12 x 2.5 km) and up to 200 m thick continuous deposits (cf. Oberbeck, 1975; Morrison & Oberbeck, 1978; Hörz et al., 1983) of ejecta could be identified outside the southwestern rim of the Azuara structure and roughly 25 km distant from its center. This Pelarda Fm (see. Fig. 2) was originally described as a fluviatile boulder conglomerate....*". And now, after the Granada Impact Workshop and its fieldtrip to this site, the Pelarda Fm. is "up to 400 m thick" as Ernstson indicates in his comment number 21. The definition of the boundaries of geologic units with a relatively homogeneous composition is fundamental in lithostratigraphy. However, the distribution area and lower boundary of the Pelarda Formation have never been published in detail by Ernstson and collaborators, so **it is difficult to know what they are referring to**. This is simply because we may not be speaking of the same unit! We accept the boundaries and distribution mapped by Carls & Monninger (1974) and the geological map 1:50.000 of the Spanish geological Survey, but we cannot accept this new thickness given to the Pelarda Fm., as it most probably includes other units with different origin and age.

22 This is suggested by Cortés et al. only! Carls & Monninger date the Pelarda Fm. as younger than the Lower Tertiary and older than the Upper Tertiary (their Fig.2, p.7).

22) Ernstson's comment is wrong. Carls & Monninger (1974) actually write: "*After the climax of alpidic movements (late Oligocene-early Miocene), the SE part of the Eastern Iberian Chain was denudated to form the Peña Tajada surface. This was (locally?) covered by more than 200 m of a fluvial boulder conglomerate, the Pelarda Formation*". Therefore, independent of its interpretation, Carls & Monninger (1974) date the Pelarda Fm. as younger than early Miocene. It is only in their figure 2 (p. 7) that the Pelarda Fm. is shown unconformably overlying Early Tertiary conglomerates (*wenig verfestigse Konglomerate des Alt-Tertiärs*, in the German version) and underlying Late Tertiary basin-filling sediments (*jungtertiäre Beckenfüllung*, in the German version), which is what probably induced Ernstson's comment.

23 polemical; delete!

23) It is not polemical or inappropriate to state that the age and interpretation of the clasts is not proved. This is because, as far as we know, Ernstson and collaborators based both the age and the authocthony/allochthony of the clasts solely on visual comparison of lithofacies, and inference of the age thereof. The identification can be considered as "alleged" since no more data have been presented, and methods for dating the clasts have not been specified. The alleged "Buntsandstein megaclast" was visited during the 6th ESF IMPACT Programme pre-workshop fieldtrip (outcrops along the road from Fonfría to Olaya), and it consists of a discontinuous bed of reddish poorly-consolidated (crumbly) muddy sandstone which, in our opinion, is more easily interpreted as a thin interbed within the alluvial deposits of the Pelarda Fm. Marls and other carbonate deposits are found throughout the region within Triassic (Muschelkalk), Jurassic, Cretaceous or Tertiary age units. How was the marl dated as Paleogene?

Ernstson should also take into account that the thickness of the sedimentary pile in the Calatayud-Montalbán basin locally exceeds 2 km towards the central part (Casas et al., 2000), and that clays, silts and marls are the main components there. As many Spanish petrologists and mineralogists can confirm, years of experience in the study of sedimentary units of the Tertiary of the Iberian Peninsula are not enough to visually identify a marly clast as Paleogene in age without any dating method being applied. In the absence of more data, the age and interpretation of these clasts is very poorly based, and the word "alleged" is appropriate. Furthermore, it is important that potential readers be aware of the issue.

24 Cortés et al. confuse the base of the Pelarda Fm. (see Ernstson & Claudin 1990) with alluvial fan deposits from the Calatayud basin. This basal deposit, composed of angular to subangular clasts of Paleozoic pelitic rocks (slates and schists) and quartzites, is basically different from the nearby alluvial fans.

24) As pointed out in previous paragraphs, the description (composition, extension, limits) of the Pelarda Fm. provided by Ernstson and collaborators remains unclear. This is very important, because the interpretation depends on what we are talking about, which depends on its description: we should all call the same things with the same name, so we know that we are all referring to the same thing. If the Pelarda Fm. now suddenly consists of "up to 400 m" of clastic deposits, instead of the initial "up to 200 m", then it is normal that we seem confused, because Ernstson and collaborators keep changing their definition.

In addition, in the Azuara area (alleged "target region"), Palaeozoic rocks did not dominantly crop out until the Miocene, as can be inferred from the composition of clasts in Paleogene and Neogene units of the Ebro Basin. In a conventional interpretation, the Mesozoic sedimentary cover was eroded during the Paleogene tectonic movements (Mesozoic clasts are dominant in Paleogene units), then clasts from the underlying Paleozoic units started to be incorporated into the Tertiary units in the Miocene (Pérez, 1989; Villena et al., 1996). The tectono-sedimentary evolution of the Calatayud-Montalbán Basin is different because Paleozoic pebbles first appear in late Eocene conglomerates, although calcareous pebbles still remain dominant (Casas et al., 2000). The Azuara area was practically undeformed before the alleged impact, so that Paleozoic (mostly siliciclastic) rocks were covered by 1-2 km of Mesozoic (mostly calcareous) rocks. Ernstson and collaborators adduce complex explanations for the predominance of Paleozoic clasts and practical absence of Mesozoic clasts in the Pelarda area, whereas the opposite is found at Puerto Mínguez. All these features are easily and more simply explained by the tectonic interpretation.

25 also refer to Claudin et al. (2001)! - see References -

25) OK

26 incorrect: Accrding to Adrover et al. (1982), the Pelarda Fm. is a fluvial deposit overlying Fonfria Paleogene material and underlying the Olalla Miocene material. Thus, their results contradict the Pliocene-Quaternary dating as well as the "raña"- deposit interpretation.

In the Spanish literature, there is no unambiguously dated Miocene-Pliocene overlain by the Pelarda Fm.! If Cortés et al. insist on a Pliocene-Quaternary age for the Pelarda Fm., they must give support to this by a sound dating.

26) OK. We deleted the reference of Adrover et al. (1982) from the text. The precise age of the Pelarda Formation still remains to be determined.

27 What information? - Explain! Confusion - what confusion? - Explain!

27) OK. We replaced “confuse information” with “no clear data”. As can be deduced from the context, we are referring to the interpretation of the Pelarda Fm., and we also provide an example in the parenthesis within the text.

28 ... our own field observations ... Give results of your own observations; otherwise: omit.

also see (26): The geologic term "raña" is used to describe a glacis deposit of quarzitic clasts immersed in a muddy matrix and developed over Paleozoic terrain under periglacial or arid climate conditions. This definition seems difficult to be ascribed to the Pelarda Fm. deposits which are located in the highest topographical levels of the zone and overlie different lithological units (not only Paleozoic rocks). Also, the enormous thickness of the Pelarda Fm., 300 - 400 m, is hardly to be explained by arid or periglacial climate conditions. In the same way, it is very difficult to explain the three different units (see 21) of the Pelarda Fm. and its clear diamict character by the "raña" model.

Cortés et al. must discuss these evident inconsistencies in a concise manner!

28) The details of our field observations and sampling on the sedimentology of the Pelarda Fm. remain to be published, but that does not imply that they are not valid, nor that we are lying. Furthermore, Ernstson seems to not understand the sedimentological and tectonic significance of the so-called "raña" facies present in the Spanish Neogene and Quaternary deposits. "Raña" deposits are not always diamictites (they frequently include conglomerates), they are not always glacis deposits (they frequently include alluvial fan deposits), and do not always overlie Paleozoic units. Furthermore, their thickness is independant of the sedimentary environment and climate conditions, because they are mostly related with local reliefs, and depend more on the local tectonics. In these continental environments, the main cause of deposition and the development of extreme thicknesses are diferential tectonic uplifts, as can be clearly seen in any orogenic system. In addition, to find a Pliocene-Pleistocene deposit located on a high topography is not rare: recent tectonic movements have been described within the Calatayud-Montalbán Basin by different authors. In fact, Carls & Monninger (1974) described post-Pontian (Upper Miocene-Pliocene) faulting affecting the Calatayud-Montalbán Basin. Several descriptions and bibliographic references to recent tectonics and neotectonics (with vertical movements) of the area can be found in Moissenet (1985), Colomer (1986), Simón (1986, 1989), Ferreiro et al. (1991), Gutiérrez (1995, 1999), and Cortés (1999), amongst others. In addition, some of the authors of our paper (Cortés & Sanz-Rubio) have considerable experience with the stratigraphy and tectonics of the Calatayud-Montalbán and Teruel basins. Indeed, we are currently involved in projects financed by the Spanish government and local institutions about stratigraphy, biostratigraphy, magnetostratigraphy, geochemistry, etc., of the area. Another coauthor (Díaz-Martínez) is well experienced with the sedimentology of diamictites of different ages and environments. Hence, our assertions and opinions about the Pelarda Fm. are soundly based, and are our best interpretation with the available information. We do not say that we have the ultimate word, but that our alternative interpretation is valid.

About the maximum thickness of the Pelarda Fm., we do not agree with the new observations of Dr. Ernstson (see our reply to comment 21 above).

√29 not in the reference list

29) OK Corrected.

√30 Middle-Miocene conglomerates; dated by what? Give clear answer or omit.

30) Dating is provided by Pérez (1989) and Casas et al. (2000), which are already cited in the nearby text.

√31 This is the controversy! Cortés et al. claim a tectonic origin in contrast to the impact origin.

The difference: Cortés et al. only insist on a relation of the plastic deformations to the tectonic thrusting. The impact advocates (see, e.g., Claudin et al. 2001), however, explain why these deformations within a soft, uncemented matrix can result from dynamic, short-time processes only.

Likewise, Cortés et al. only mention striae, polish and dissolution pits, and they "forget" (?) the abundant rotated fractures (for the first time described by Ernstson & Claudin 1990) and bread cut to slices features (see Claudin et al. 2001). There is no reasonable fracture-mechanical explanation for an origin of these unusual features by quasi-static tectonic stress.

Cortés et al. must honestly refer to these and to quite similar deformations in the Ries and Chicxulub impact ejecta; they must refer to the work done by Chao, Rampino et al., Ocampo et al. and others, on this subject. This is not a concise review but bad scientific style.

31) There is always controversy when there are different colliding opinions about an issue (this is precisely how the word controversy is defined), and so is the case with the interpretation of the origin of the Azuara structure. Ernstson and collaborators ignore the existence of one of the main thrusts in the Central Iberian Chain: the Utrillas Thrust, included in the Portalrubio-Vandellós thrust line [see also Viallard (1983), Guimerà (1988), González and Guimerà (1993), Casas et al. (2000), etc.]. This thrust is a very important compressional structure located only several hundred meters from the Puerto Mínguez outcrop, and had a marked influence in the development of micro-, meso-, and macro-scale tectonic features, which indicate changes in stress and strain in time and space during the Neogene and Quaternary.

How do Claudín et al. (2001) know that the deformation could only occur in a short time? Their model is just but one of the possible explanations, but we suggest another possibility that also leads to the same results. The type of fracturing that Ernstson mentions is not exclusive of meteoritic impact deformation. Compressional tectonics can also cause important fracturing of rocks and clasts, even more than those shown by Ernstson and collaborators. Local and differential tectonic stress linked to thrusting processes can cause fracturing, cleavage, intense brecciation and/or mylonitization of rocks in the proximity of (but not limited to) the main faults.

We do accept that similar features may be found related with impacts, but we do not agree with the assertion that they are inequivocal evidence for an impact. This is simply because there is also an alternative explanation for these same features unrelated with impacts. Some of the works cited by Ernstson in his comment also fail to consider this alternative explanation. It is not a question of finding lots of very good examples of deformation. It is a question of considering (and not ignoring) that different processes may lead to very similar features, and therefore similar feaatures may have different origins. This is aldo the case for the Ries and Chicxuub impact ejecta, and as M. Rampino put it at the 2001 Granada Workshop, "this may open a whole can of worms".

Ernstson believes that we have a "bad scientific style". However, we believe that our assertion in the manuscript is well founded, and that it is precisely Ernstson who does not use the scientific methodology properly (see also our points B, C, D and F above).

32 In their 1990 paper, Ernstson & Claudin say that the Pelarda Fm. ejecta were deposited by ballistic erosion and sedimentation (p. 596), and not by aerial deposition. In the same article, Ernstson & Claudin argue that the non-random orientation of the striae results from their origin by a roll-glide mode of ejection and deposition under high confining pressure <u>in the last stage of the ejecta emplacement process.</u>

Either, Cortés et al. never read this article, or they forgot the text ....

32) OK, we substituted “ballistic” for “aerial”.

Ernstson's argument about instantaneous striation in the last stage of the ejecta emplacement process is not convincing. The preferential orientation of striae in clasts of matrix-supported deposits is not compatible with roll-glide deposition, because rolling of clasts within a soft matrix would imply a random orientation of marks.

We have certainly read their article, although we did not memorize the text. Ernstson and Claudín (1990) provide one interpretation, but we believe there are other alternatives (see our points B, D and F above).

33 Curious: On the one hand, the Pelarda Fm. is said by Cortés et al. (see comments 26, 28) to be a post-tectonic Plio-Pleistocene raña deposit, and on the other hand, the Tertiary tectonic stress regime is made responsible for the raña-deposit striations - very curious.

33) The age of the "raña" deposits in Spain is typically Pliocene and/or Pleistocene, and they are normally interpreted as being syn- to post-tectonic. Furthermore, the Pliocene is included in the Tertiary, and the tectonic stress regime during the late Tertiary (Neogene, i.e., including Pliocene) was basically the same during the Quaternary, and is similar to the present-day stress regime (Simón, 1986, 1989; Cortés et al., 1996; Herraiz et al., 1999; etc.). Ernstson's ironic comment about our assertion is out of place. Our assertion may only seem "very curious" for those who do not know the regional geology. See also our points C, D and F above.

34 poor argumentation. A serious review would point to the striking observations made and published by Ernstson et al. (2001). These are the complete absence of any dissolution features (in thin sections!), the abundant tensile - not compressive - signature below the miniature craters, the PDF's related with the spallation features, and the shock experiments made in the EMI institute where the complete observations of the Buntsandstein deformations were reproduced - Bad scientific style to simply disregard these basic facts.

34) This comment is not appropriate. We consider that our arguments are enough to disprove the relation with an impact, because these are dissolution features, related with compression, with no proven PDFs originating from them, and common in many sedimentary units with similar characteristics (strained siliciclastic conglomerates). Their experiments only prove that it is possible to obtain similar (though not exactly the same) features under laboratory conditions, which certainly does not imply that it is the only way to generate them. Our Comment to Ernstson's article in Geology (Ernstson et al., 2001) was published in the January 2002 issue of this journal. See also our points B and F above.

35 How to date radiometrically, when there are no silicate melt rocks exposed??

35) Ernstson should clarify his ideas: in this comment he says there are no silicate melt rocks exposed that can be dated radiometrically, whereas in his comment 62 he says there is "clear occurrence of silicate melt rocks". In order to date an impact structure, the most precise evidence is radiochronology of the new mineral phases formed during or immediately after the impact, and which form part of the impact melt or suevite. This method has never been applied in the Azuara structure because of the absence of silicate melts. At the Granada ESF Impact Workshop (May 2001), Ernstson and collaborators presented for the first time evidence for a silicate melt (in their abstract, poster, and fieldtrip guide). We hope that these rocks can be dated and thoroughly studied, as they will provide useful information on the origin of the structure. However, our preliminary interpretation after field observation and petrographic study of the rocks they proposed is that they are very similar to a Miocene volcaniclastic tuff which is found in the region. See also our points B, D and F above.

36 see 13 - the central uplift missed by Cortés et al.!

36) See our reply to his comment 13. We believe that this information needs to be provided to the readers.

37 What is the logical relation between the demonstration of the existence of impact-derived minerals (Coesite, Stishovite ... ?) and a dating of the impact?

37) OK. We substituted the word “demonstrated” with “shown”.

38 not only ejecta, but also deformations of rocks within the crater, breccia dikes, post-impact sedimentary cover of impact features.

38) Previously commented. See also our points B, C and F above.

39 incorrect: Ernstson & Fiebag point to the work of Gwosdek 1988 and Mayer 1991 and their paleontological gastropode dating giving an upper bound to the impact age.

39) What kind of gastropod? What is its name (genus and species)? Is there only one sample? We have not had access to this information (both works are unpublished theses). In any case, Tertiary continental sedimentary units in the Iberian Chain present very few reliable gastropod taxa that allow precise biostratigraphic dating (one of the few is *Vidaliella gerundensis*, from the Paleocene). If the name of this gastropod is not specified by Ernstson & Fiebag (1992), so that its validity can be ascertained, then our sentence "the methodology used to date these Eocene sediments is not described in their work" is correct.

Furthermore, within the impact hypothesis, this always would be a re-worked fossil. All fossils found within an ejecta deposit are to be considered reworked (from a taphonomic point of view), since organisms cannot survive the impact, and non-fossilized calcareous shells are broken due to the high pressures. In any case, the presence of Eocene clasts within the Pelarda Fm. does not imply an Eocene age for these rocks by itself. It only indicates that it is late Eocene or younger.

At the same time, Ernstson and collaborators continuously ignore the existence of other paleontological sites (mostly mammals and other vertebrates) which disprove their interpretation of certain beds as impact breccias. These vertebrate paleontological sites have been thoroughly studied, and the results are already published (Adrover et al., 1982; López, 1986; Alcalá et al., 2000).

Ernstson and collaborators have been changing the age of the Azuara structure, from Late Cretaceous to Eocene-Oligocene. Their latest version [Ernstson et al. (2001) and Hradil et al. (2001)] provides a Lower-Middle Tertiary age for the impact. The Lower-Middle Tertiary spans a period of about 40 or 50 My. If they consider the Middle Tertiary to include the whole Miocene, which is not clear, then it would be a span of about 60 My. Eocene-Oligocene-Miocene means a period of about 45 My. So then, what is the age of the impact? If it is not clear, then our sentence is correct.

40 No paleontological vertebrate dating has ever been described for sediments unambiguously deposited below the Pelarda Fm. (also see comment 26)

40) This is not correct. The Olalla paleontological site (Peláez-Campomanes, 1993) is located immediately below the Pelarda deposits and corresponds to the Early Oligocene Rupelian stage (MP21). See also our reply to comment 26.

41 see comments 28, 33 on the thickness of the Pelarda Fm., its exposition in the highest mountains, and the needed strong Quaternary tectonic overprint to produce the heavy plastic deformations of the Pelarda Fm. clasts!

41) See our reply to Ernstson's comments 28 and 33, and our points A, B, C and F above.

42 omit! Cortés et al. try to suggest a confusion of the Azuara shatter cones with diagenetic features; has all been discussed in the papers by Müller, Katschorek and Mayer, where there are nice photographs shown, partly as stereo images of the shatter cones.

42) We shall not omit this sentence because it is important information that should be made known. According to the few pictures that we have been able to see, the alleged shatter cones could be tectonic or diagenteic features. No truly good and unambiguous examples of shatter cones from the Azuara structure have been published yet. See also our points A, B, C and F above.

43 Do not write about what has not been done!

43) We shall keep this sentence, because readers should be aware of what studies have and have not been done so far within the structure. This was an important issue brought up during the round table discussion at Granada (ESF-IMPACT Workshop in May 2001), and is one of the key issues in proving or disproving the impact hypothesis. See also our points A, B, C and F above.

44 incorrect statement: Large impact structures are as a rule not bowl-shaped (small, simple craters, yes), and their negative gravity anomaly is in most cases attributed to low-density impact breccias and fracturing of the target rocks, but not to sedimentary post-impact infill.

44) OK. We substituted "bowl shape" with "bowl or dish shape", and "sedimentary fill" with "infill materials".

45 Nowhere in the article, a basement (what kind of basement?) has been located 4 km in depth at the center of the Azuara structure. Evidently, Cortés et al are not familiar with gravity modeling.

45) Once again, Ernstson attempts to disqualify us (see our point D above), which is certainly not appropriate in this discussions. We are familiar with gravity modeling, although it is not our field of expertise, as it is for Ernstson (he is a geophysicist). We also are familiar with his publications: figure 6 of Ernstson & Fiebag (1992) shows that the lower boundary of the mass deficient volume in the mean radial profile (density model of circular symmetry) is located at 4.5 km depth at the center of the structure. Normally, the term basement refers to the foundation of a structure, and in geology it refers to a complex of undifferentiated igneous and/or metamorphic rocks underlying sedimentary strata. We agree that "basement" may not be the most appropriate term, so we substituted it for "base of the fractured target rock"

46 What does this mean - "not referred to any standard system"? Gravity anomalies are basically and always related to relative data, they do not change by relating the data to any standard system.

Cortés et al. obviously try to question the quality of the measurements - bad scientific style!

46) This is not correct. We are not trying to question the quality of the measurements. We think that the measurements are correct (Bouguer anomaly map), but we disagree with the way of handling the measurements, and the interpretations obtained thereof.

In gravity modeling, the **map of residual anomalies** is more interesting (provides more information) than the **map of Bouguer anomalies**. The **map of residual anomalies** is calculated after the removal of the **regional anomalies** from the map of Bouguer anomalies. Depending on what regional map has been selected, the possible interpretations can be quite different.

As an example (see enclosed maps), we show the gravity modeling by Ernstson et al. (**maps 1, 2 and 3**). Using the same data (map of Bouguer anomalies), we show other alternative regional maps. The first one (**map 4**) is directly deduced from the regional map by Salas & Casas (1993) for the Iberian Chain and Ebro Basin. The second one (**map 5**) can be deduced from the regional patterns of the Bouguer map by Ernstson & Fiebag (1992). After removing **map 5** from **map 1**, a new map of residual anomalies can be drawn (**map 6**), which may allow for different interpretations.

Why do Ernstson and Fiebag (1992) use **map 2**? Probably because it fits their hypothesis. How was the regional pattern of **map 2** deduced? We would like to know. We think that publishing these maps implies certain manipulation of the data. We therefore prefer to write in the text that "it is not referred to any standard system". No further comments about this, unless Ernstson asks for a discussion about this matter.

47 Ernstson & Fiebag in their original paper and not only Aurell et al (1993) argue that the magnetic map shows the local magnetic gradient of the undisturbed field. Here, Cortés et al. suggest criticism of the geophysical measurments by other authors - bad scientific style.

47) OK. However, it is true that other authors have critiziced the geophysical measurements. We not only suggest it, but want to make it known to the reader.

48 The magnetic map shown by Ernstson & Fiebag is not concurring with this statement which does not take any reference to the short-wavelength anomalies in the peripheral zone of the crater. There, abundant anomalies with amplitudes exceeding 100 nT (which is a lot in sedimentary deposits!) have been measured, and according to susceptibility measurements of samples, these anomalies are suggested to be correlated with the basal breccia. A serious review should have pointed to this - bad scientific style again.

48) These anomalies may also be related with minor folds involving Mesozoic rocks covered by Tertiary sediments. These folds can be laterally observed in geological maps. At the outcrop scale, these folds correspond to (a) anticlines with a Triassic evaporitic core and calcareous Jurassic limbs, and (b) synclines involving Mesozoic (Jurassic and Lower Cretaceous) and Tertiary rocks. According to theoretical and practical studies on magnetism (see, for example, Keary and Brooks, 1991), the magnetic anomalies could also be related with these folds. Ernstson keeps accusing us of bad scientific style, but what is truly a bad scientific style is to ignore other possible interpretations (see our points B, D and F above).

49 What is the definition of "true" here? Explain!

49) OK. We deleted the word "true".

50 By whom? Give references! - Don't write what has not been shown to exist, but what has been shown, for example the basal breccia as a suevite equivalent!

50) Calling a rock with the impact terminology implies that it is proven to be related with an impact, and therefore may be interpreted as such. The terms melt and suevite are part of an interpretive terminology, and not merely descriptive. We believe that the impact hypothesis for the Azuara structure is still not proven (see our points A, B, C and F above), and we therefore prefer to leave the sentence as is. We agree with Ernstson about the conveniency of using terms which are descriptive (breccia, conglomerate, diamictite, etc.), but this is not the case.

51 also by Katschorek (1990) and Fiebag (1988) with very detailed and sound investigations of the megabreccia. Both authors and their work are referred to in the ms of Cortés et al. Why do they not consider the basic work on the megabreccia, the precise mapping, the petrographical studies made by Katschorek and Fiebag? Why do they not mention the cataclastic flow texture observed in thin sections of the megabreccia? In a serious review, I would expect an unbiassed consideration.

51) See our reply to Ernstson's comment 18, and our comments A, B, C and F above.

52 Cortés et al. simply say that the megabreccia in the Azuara structure is the result of collapse in the Hettangian without any reference to investigations made by themselves or by other authors.

Different from this, the impact advocates present own studies on the megabreccia, they compare the well-known collapse breccia with the Azuara megabreccia and show basic differences not allowing to confuse the megabreccia with the Hettangian collapse breccia. Never once, the "impact authors" have claimed an impact origin for the collapse breccia as Cortés et al. and other authors (Aurell, e.g.) pretend.

52) The breccias near the Triassic-Jurassic boundary are due to secondary dissolution collapse, both during early diagenesis, and during epigenesis (also influenced by tectonism). We therefore consider them as secondary sedimentary breccias, as opposed to primary sedimentary breccias (which are most frequently of alluvial or colluvial origin). The breccias near the Triassic-Jurassic boundary are not exclusive from the Azuara region. They are typical throughout Spain, and specially in its northeastern quadrant, from the Pyrenees to the southern Iberian Chain (see references). We believe that this paragraph is clear enough.

Ernstson tries to discern between impact and collapse processes referring to the breccia near the Triassic-Jurassic boundary within the Azuara area. Of course, there are many inhomogeneities and local differences within the breccia unit, both due to the inherited inhomogenities and local differences in tectonic stress, groundwater flow, etc. (there are many studies about this unit in the Spanish literature). The crucial issue here is that he calls this same unit, with all its inhomogeneities, sometimes as impact breccia, and other times as collapse breccia, depending if it suits his interpretive model or not. No further comment. See also our points C and F above.

53 incorrect: The basal breccia rock is not interpreted as carbonate melt. The basal breccia is compared with suevite impact breccias, and the carbonate melt in the basal breccia is related to the silicate melt in the suevites.

53) OK. Ernstson does not consider the whole breccia as a carbonate melt, but only parts of it. We made a correction accordingly.

54 incorrectly quoted: In this paper, Aurell et all. pretend that the basal breccia might have been confused with conglomerates of the Middle Miocene - there is no "probably", no "breccias" in the text of Aurell et al.

Now, in the ms of Cortés et al., the basal breccia is " preliminarily interpreted" as a fanglomerate. "fanglomerate" or "conglomerate" or "breccia" is not a matter of interpretation but of description!

In fact, the basal breccia is a breccia and nothing else, as is clearly shown in all photographs so far published. If Cortés et al. are not able to make a difference between breccias, conglomerates and fanglomerates, they should not write a review article about or against impacts.

54) Conglomerate and brecia <u>are</u> descriptive terms. Fanglomerate <u>is</u> an interpretive term, and with such meaning we use it: heterogeneous material deposited in an alluvial fan and cemented into solid rock (USGS Bull. 730, 1923, p. 88). The location of deposition of a fanglomerate (the alluvial fan) implies an interpretation of past sedimentary process typical of this particular sedimentary environment. It is as interpretive as the terms turbidite or tillite may be.

Ernstson's last comment is not appropriate. If <u>he</u> cannot make a difference between descriptive and interpretive terms, may be <u>he</u> should not review articles about or in favor of impacts.

55 In the Ernstson & Fiebag paper, I count more than 200 words about the globular breccia, and I read a very detailed description including a comparison with similar formations in the Ries crater (Graup 1981) and the Sevetin structure.

Again, in contrast with the impact advocates which report the results of analyses they made, Cortés et al. only say that the globular breccia is not impact-related and probably caliche (calcrete). They lengthily refer to well-known features of caliches, but this doesn't say anything about the globular breccia.

55) What is important, for our purposes, is not the amount of words in the text, but the conceptual content of the text. The information provided by Ernstson and Fiebag (1992) about the location where the globular breccia was found ("…in an isolate block…", p.418), and about its features (not about their interpretation) is certainly very poor, and does not allow for resampling or reinterpretation. That is why we refer to it as "poor basic information ... about its description and location". The few features that they mention with descriptive (and not interpretive) terms, may also be interpreted as resulting from epigenesis and paleosol development processes, and in particular with carbonate pedogenesis. See also our points A, C and F above.

56 too vague, be more precise: Where and how do Ernstson & Fiebag describe features like pedorelics, ooids, peloids, Microcodium, root tubules? Cortés et al. assume this kind of explanation without any analyses, any proof, only believing that Ernstson & Fiebag confuse the rocks. This is bad scientific style.

56) We did some rephrasing of our text. We have analyses and proof of what we say, but because it is not yet published, but coincides with what Armenteros (1989) found, we use his work as a reference and example. Apart from the globular breccia interpretation, many of the features described by Ernstson and Fiebag (1992) can also be interpreted as resulting from paleosol and karst development. Furthermore, finding of *Microcodium* rules out the possibility of the material being impact-related, as there is no other alternative interpretation. See also our points C and D above.

57 Cortés et al. probably mean "defenders" and not "defendants"? A Freudian slip?

57) Yes, we meant to say defenders. It is certainly a slip, but just related with our insufficient knowledge of English as a second language.

58 What is the contrast? There are impact dike breccias and also collapse breccias, all right.

Altogether: Very poor review of the Azuara dike breccias which have been described in great detail by the impact advocates: crossing breccia dikes, dikes within dikes, breccia-dike generations, pseudotachylites, dikes cutting chert nodules, dikes with ribbon textures, breccia pipes, and much more.

These dikes give strong evidence for an impact origin, and it appears impossible to confuse them with any karstification phenomena and paleosol developments. Cortés et al. are demonstrating nothing, they only believe that the breccia dikes could have formed by other processes.

58) We deleted the words "In contrast…". As previously stated, we agree with the purely descriptive part, but disagree with the interpretive part, mostly because there are other possible alternatives to explain the same features. The presence of dikes does not provide strong evidence for an impact origin. One of the reasons is that there are other processes which can result in their development. This is not our own opinion or believe, but a fact. See also our points B, C, D and F above.

59 Impact melts are not true igneous rocks. Igneous rocks have crystallized from a magma, and an impact melting from shock is a completely different process.

59) OK. That certainly was an important slip on our side.

60 bad scientific style: Ernstson & Fiebag do not report on supposed melt; they describe melt and recrystallization products.

..... have only been described ... What does "only" mean? Explain!

The find of melt in the basal breccia and in the breccia dikes seems very convincing of an impact origin to me.

60) OK. Part of this is just a matter of interpretation of processes envolved, as deduced from the observation of certain features. We just disagree with the interpretation, and think that the descriptions are frequently biased. We deleted the word "only". See also our points B and F above.

61 add Fiebag (1988) to the references: detailed description of the melt products there.

61) OK

62 What does "insist" mean? Ernstson et al. and Hradil et al. do not insist on the presence of melt rocks. In their papers, they report for the first time the clear occurrence of silicate melt rocks and of melts of carbonate/phosphate composition.

62) We substituted "insist" for "report".

63 I don't know whether Cortés et al. have really read the Hradil et al. paper. Hradil et al. give a very detailed petrographic and geochemical analysis of the melt rocks and show that the educt rocks for the melts can reasonably be deduced from the local target.

poor location of the sampling sites: Along the road from Barrachina to Navarrete, the megabreccia containing the melt rocks is more or less continuously exposed. And evidently, Cortés et al. were able to locate the melt rocks!

64 Again, Cortés et al. don't give the faintest support of their claim. They simply say that the silicate melt is a volcanic tuff. They should know and, respectively, should have read in the Hradil et al. paper:

> In Spain, the young volcanism - except for Cabo de Gata volcanism related with subduction - is basically alkaline and related with distension stress pattern. In most cases, the first eruptions are hydromagmatic. In this kind of volcanism, the tuffs are basically rich in lithics (see Mallarach et al. 1985, Marti, Ortiz, Claudin & Mallarach 1987, Araña & Ortiz 1984).

The melt described by Hradil et al. (2001) contains more than 90% pure glass spheroids with a second interstitial glass phase without any shards. The bulk composition of this melt is completely different from any tuff composition (basically alkali basalt).

63 and 64) Again, this is all just a matter of opinion and interpretation of the features observed. We visited and sampled this outcrop following photographs and data shown by Ernstson in his web page (http://www.impact-structures.com). It was also visited during the pre-workshop fieldtrip last May 2001, which included a stop at this particular site. At this stop, the relationship between this alleged melt rock and a probable impact was questioned and refuted by most of the participants. Our opinion is that the "breccia" is a deposit (probably Pliocene-Quaternary, since it contains clasts of probable Upper Miocene lacustrine limestones) which was deformed due to karstic collapse by dissolution of the underlying Middle-Upper Miocene gypsum rocks of the area (Sanz Rubio et al., 1997; Alcalá et al., 2000). This process is very frequent within the Calatayud Basin (Gutiérrez, 1999, etc. and references therein). Within this context, the alleged melt materials correspond to a thin volcanic tuff interbedded between the Miocene marls and gypsum. See also our comments B, C, D and F above.

65 What does this contribute to the question of impact melts?

65) OK. We moved it to the next section "Microscopic deformation and melting features"

66 Not very convincing; the group of the impact advocates has studied about 1,000 thin sections of rocks from the Azuara region. I do not know of thin sections of Azuara rocks studied and described by Cortés et al.

66) This comment by Ernstson is very disrespectful towards an important number of geologists (both Spanish and not Spanish) who have worked in the Azuara region during the last century and/or are currently working there. Only during the last 15-20 years, thousands of thin sections have been studied by a great number of authors for theses, scientific papers, scientific official reports, etc. (for example: Tejero, 1986; Pérez, 1989; Aurell, 1990; Soriano, 1990; Soria, 1997; Gutiérrez, 1994, 1999; Sanz Rubio, 1999). Up to date, and after their respective detailed studies and revisions, none of these authors has found any evidence for impact in their thin sections. It should be needless to mention that the study of thin sections of rocks and sediments is a basic procedure in any geological study, although the quality of a study does not depend on, nor is it proportional to, the number of thin sections studied. See also our comment D above.

67 As early as in their 1985 EPSL paper, Ernstson et al. show photomicrographs of multiple sets of PDFs in quartz from polymict dike breccias in the Azuara structure. In this paper, they present the result of an optical analysis (by universal-stage measurements) of these PDFs clearly showing omega and pi accumulations for the crystallographic orientations of the PDFs, typical of shock. In the same paper, Ernstson et al. present also universal-stage measurements of multiple sets of PFs (planar fractures) in Azuara breccias strongly pointing to shock origin. Here, Cortés et al. only point to the biotite kink bands, also shown in the 1985 EPSL paper. - Scientifically dishonest.

> 67, p.15: "The presence of kink bands in micas can also be the result of normal tectonic deformation ..." The same has been noted by Ernstson et al. in their 1985 EPSL paper (p.368) upon showing a photomicrograph of a heavily kinked biotite. In contrast to Cortés et al., Ernstson et al. point to the high percentage (70 - 80%) of kinked micas, to the high frequency of the Azuara kink bands per grain, their narrow width, and the high kink-angle asymmetry, supporting their formation by dynamic shock pressure (see Hörz 1970). Again, Cortés et al. keep quiet about important facts.
>
> Instead, they are speaking of "... two or three cleavage preferent [?preferential] orientations, which are very common in Palaeozoic rocks throughout the Iberian Range ..." Cleavage or kink banding ?? Perhaps, Cortés et al. are confusing both? Explain, and take more honest reference to the Ernstson et al. and Hörz papers!

67) Ernstson is partly right. Ernstson et al. (1985) described and showed photographs of probable PDFs in quartz taken from the "Nogueras polymict breccia", and only from there. They did not provide clear indications about the exact location, although in page 365 they indicated: "The third polymict breccia forms a <u>small, very poorly exposed lens</u> within Devonian quartzites near Nogueras, off the road between Santa Cruz and Nogueras".

We visited this outcrop during the last ESF-IMPACT Program fieldtrip (May 2001). In the fieldtrip book (p. 7), Ernstson et al. (2001) indicate that "It has a <u>maximum width of about 70 cm</u> and can be traced in the field over a distance of <u>more than 10 m</u>" and "The stratigraphic position of the components <u>is unknown</u>, but could be <u>Paleozoic and/or Mesozoic-Cenozoic</u>" (underlining is ours). So it seems that there is no clue with regard to the age of the grains said to present PDFs. During the fieldtrip we sampled the breccia, and later made thin sections, analyzed the scarce and low quality quartz samples obtained from that outcrop, but did not find any PDFs. We do not think there is any scientific dishonesty on being skeptical.

The controversy between Ernstson on one side, and Langenhorst & Deutsch on the other, about samples with or without PDFs is partly based on samples from this outcrop (see below). As these latter researchers can testify, Ernstson sent them samples which he described as containing abundant quartz grains with PDFs. After studying them with TEM, they found no PDFs, but just many free dislocations, intragranular boundaries, and planar fractures (PFs), which they interpreted as tectonic.

Ernstson et al. (1985) cited about 70-80% of the biotites to be deformed (kinked micas). Ernstson insists about this in his comments ("..high frequency of the Azuara kink bands per grain…"). However, this percentage (70-80%) only corresponds to the biotites in the Nogueras breccia, a small and very poorly exposed lens which we alternatively interpret as a fault breccia. Extrapolation of these data to the whole studied area is not appropriate. See also our comments B, C, D and F above.

> **68** "... are said to be frequently found ...". The isotropic quartz grains are <u>not said to be found</u>, they are precisely described and discussed by Mayer with a very nice large photomicrograph of a diaplectic quartz on the front cover of his thesis! I expect Cortés et al. to have read this thesis (and seen the diaplectic quartz on the front cover!), because they frequently refer to the thesis in their ms.

> **69** "... at the same time ...": This is correct, but Cortés et al. hide the later paper by Ernstson (1994) where a detailed description of a completely isotropic (diaplectic) sandstone clast from a polymict dike breccia in the Azuara structure is given.
>
> Why is this paper missing in the *References* of the Cortés et al. ms? They should know this article because it was published in their own house, the Zaragoza university! - scientifically dishonest.

68) 69) See our reply to Ernstson's comment 18, and our comments A, B and C above.

Ernstson (1994) was not included in our reference list simply because we did not cite it in the main text. Ernstson must not consider his work as important either since, in spite of the "detailed description" that he mentions, he never cited it in his subsequent papers, and it is only shown in his web page (http://www.impact-structures.com/).

70 In the 1985 Ernstson et al. paper, the Pelarda Fm. is not mentioned at all.

70) OK, we are sorry for the mistake.

71 Cortés et al. should be precise: Here, in connection with the ejecta deposits, the PDFs and PFs from the 1985 EPSL paper suddenly appear (see 67). Erroneously, or on full purpose? The multiple sets of PDFs and PFs have been described for polymict breccias within the Azuara structure. In the ms of Cortés et al., they are, however, mixed into the Pelarda Fm. - their Quaternary "raña" deposits outside the crater ... honi soit qui mal y pense.

71) We do not understand Ernstson's point here. We admit to have made a mistake when mentioning Ernstson et al. (1985) with regard to PDFs in the Pelarda Fm. However, Ernstson and Fiebag (1992) mention that "the Pelarda Formation ejecta exhibit moderate shock effects in the form of planar elements, planar fractures (cleavage), mosaicism, and deformation lamellae in quartz (Ernstson & Claudín, 1990)". How can we conciliate Ernstson's comment about the lack of PDFs in the Pelarda Formation, with the descriptions in Ernstson & Claudín (1990) and Ernstson et al. (2001, field trip book, p. 10) indicating the presence of PDFs in clasts of the Pelarda Formation?

Apart from this, Ernstson's last phrase in French is not proper for a scientific discussion. We could consider it as an insult (literally: "evil be to him who evil thinks"), but, in order to avoid further controversy, we shall just ignore it, because we are not thinking evil. The purpose of our manuscript is clearly stated in our points A through F above.

72 Here, Cortés et al. refer to an old story, to an abstract paper by Langenhorst & Deutsch about a TEM analysis of planar features in quartz. The result of this analysis led L & D to negate shock for the Azuara structure, although - due to a

basic misunderstanding - they had investigated a sample from authochthonous rocks exposed outside the Azuara structure, whereas the original shock-metamorphic effects had been reported for polymict dike breccias only from within the Azuara structure (see comments 67, 71) . Everyone is in the know of the mistake of L & D, Cortés et al. included. Nevertheless, they again bring up this old story, referring to all kinds of personal communications but not to the original published work on Azuara PDFs in the EPSL article.

Meanwhile, there are Azuara PDF analyses (all known to Cortés et al.) made by three independent teams: by Ernstson et al. (1985, EPSL), by Marques et al. (1995) from the Madrid university (Departamento de Petrologia y Geoquímica), and by Ann Thierault from the R.A.F. Grieve group at the Canadian Geological Survey. They all show more or less the same results of multiple sets of PDFs in preferentially pi and omega crystallographic orientations.

The most comprehensive study by Ann Thierault shows up to five sets of PDFs in a quartz grain, very high PDF density, mostly 100% coverage of the grains, spacings generally below one micrometer, and strongly reduced birefringence related with the PDFs. Never before, such deformations have been attributed to tectonic stress, and all serious impact researchers consider these PDF features as in proof of shock.

Why don't Cortés et al. refer to this study which is well known to them? Instead, they claim TEM analyses for final proof. It is true that according to current knowledge, TEM unambiguously allows to distinguish between shock-produced PDFs and tectonic features. There are cases of indistinct, widely spaced planar features in the form of one or two sets, where this method may be necessary. However, a general demand for a final TEM proof would for the present "kill" most established impact structures (without this "TEM proof") on the official maps and lists.

In any case, the PDFs are not the only conclusive shock-metamorphic effect in the Azuara structure. G.Mayer (1991) and K. Ernstson (1994) report on the frequent occurrence of diaplectic glass in Azuara samples (see comments 68, 69). Diaplectic glass requires even higher shock pressures than does PDF formation, and all impact researchers are convinced that diaplectic quartz cannot form in endogenic processes. With respect to this, the old TEM story about the Azuara PDFs and the shock seems to be simply superfluous!

72) All the controversy is centered on three thin-sections from samples PDFK-55B, PDFK-1A and IDN-314. In first place, in the letter that Ernstson sent to Alex Deutsch, he explicitly indicated that these samples contained many good examples of PDFs (Alex has kept this letter, which he read at the round table on the Azuara issue during the 6th IMPACT Workshop at Granada). After their thorough study with TEM, Langenhorst & Deutsch (1997) concluded that the samples did not contain PDFs. It was only after knowing these results, that Ernstson made it known that he had made a mistake and erroneously sent the wrong samples. However, Ernstson never sent the right samples to Falko Langenhorst or Alex Deutsch after they concluded their study. In so doing, Ernstson contributed to his loss of credibility.

Ernstson indicates that the samples were studied by three independent teams, and this is not true. Ernstson has been working with the team at Madrid University for many years, so they cannot be considered independent, since members from both groups have been working and publishing together (see references).

With regard to the analyses performed by Ann Thierault, see our reply to Ernstson's comment 73 below.

> 73 Considering the sound analysis made by Ann Thierault from the Canadian Geologic Survey (R.A.F. Grieve working group), it should be considered unambiguous and unequivocal.
>
> See 72: Never once, multiple (up to five!) sets of PDFs in a quartz grain with extremely high density , a 100 % coverage, spacings generally below 1 micrometer, and connected with strongly reduced birefringence have been reported for a tectonically deformed quartz.

73) Under a petrographic microscope, there are few totally unambiguous and unequivocal interpretations, mostly because no one is infallible, and an interpretation is just a personal point of view based on available data and conditioned by a theoretical context. Quite appropriately, in a message sent by Ann Thierault to one of us with regard to these samples and the discussion to take place at the round table about Azuara (Granada IMPACT Worshop), Ann Therriault wrote: "I feel strongly about the fact that what I looked at was pretty good. However, it would be a very good idea to cross-check my data with TEM. I do not believed that I possess the truth or that I am infallible". We keep copies of all messages, and Ann can also be contacted if it is so required.

As with PDFs, diaplectic glass should also be confirmed by other means besides the optical microscope. Furthermore, the possibility that the quartz with PDFs is inherited from older units should also be considered by Ernstson's group. See also our points A through F above.

> 74 This turns scientific argumentation upside down!

74) Our statement does not turn scientific argumentation upside down. In fact, what we are emphasizing is precisely the need for skeptical testing of each and every piece of evidence, in order to comply with basic scientific argumentation. If the crucial issue, which is impact metamorphism, does not pass the test, then the whole impact hypothesis fails. **If some of the best specialists in the world on impact metamorphism dedicated time to study the samples with TEM with an unbiased approach, and concluded that there is nothing in them worth noting with regard to impact metamorphism, then Ernstson should acknowledge the possibility of being wrong, and be capable to back up with all the other interpretations of different pieces of evidence for which there are alternative interpretations unrelated to a meteorite impact.** See also our points A through F above.

> 75 See my comments on the breccia dikes and on the globular breccia: Cortés et al. pretend confusion and do not provide any evidence for that.

75) We do not pretend confusion. We are just indicating that similar features in breccias can also be formed by other processes unrelated to impacts.

> 76 see comment 18: Exact descriptions, locations, and analyses can be found in all eleven theses performed in the Azuara structure and referred to in the progress report (Ernstson & Fiebag 1992). As many of these theses are referred to in the ms of Cortés et al., they must not talk their way out of it.

76) May be we are not choosing the right approach. Aurell et al. (1993) also mentioned this issue of the unpublished theses. Despite the 11 unpublished works, it is impossible to check and discuss these features, since it is not always clear where the data and samples were taken. As demonstrated during the fieldtrip last May 2001, when the exact location of the evidence is finally shown to the scientific community, and an alternative interpretation is proposed, Ernstson argues that the dating and stratigraphic correlation is not reliable, or simply attempts to disqualify whoever he considers an oponent.

77 The impact advocates have never intended to verify the impact nature of the Azuara structure by geochemical analyses.

As Cortés et al. note themselves, a clear verification of impact is provided by shock evidence only. Therefore, paragraph 5) Geochemistry is superfluous; omit!

77) Geochemical analyses can provide very useful information in verifying a probable impact. Much of the evidence proposed for one or the other hypothesis in the Azuara controversy should be checked for alternative interpretations with the help of geochemistry. The two paragraphs that we included on geochemistry are important in order to help the reader understand what has and has not been done with respect to this important subject in the Azuara region. Ernstson suggests to omit this part, but we think that it is truly important to include it, and that it may help other researchers become interested in pursuing this type of study. In fact, we suspect that it may provide the clue for the resolution of the controversy, together with the study of the alleged shock metamorphism.

Table and Table caption: As already noted in comment 11, the comparison of the Azuara structure with the Ries crater is like comparing apples with bananas. No good science - take out completely.

Reply to comment to Table and Table caption

By no means. The comparison of the Azuara structure and the Ries crater is perfectly appropriate, in spite of the differences, which are also many and obvious. Our response to Ernstson's statement is more clear in our reply to his comment 11, and in points B, C and D above.

Reply to comments on Figures and Figure captions.

Figures and Figure captions

Fig.1: What is the meaning of the question marks following R (crater radius) and 2R? Explain or omit.

Fig. 1) We shall explain, because we do not want to omit the question marks. The R indicates the radius of the Azuara crater suggested by Ernstson and colleagues, who indicate that ejecta and other evidence were found within a 2R distance from the center of the structure. It is therefore of interest to potential readers to know the areal distribution of this circles with R and 2R radius. The question marks refer to our questioning of the use of these circles, resulting from our questioning of a crater. We are just trying to be coherent.

Fig.2: Does not give any contribution to the text - take out.

Fig. 2) In order to facilitate a better understanding of our text, it is important to include a regional stratigraphic column, so that names and ages of units can be discerned during its reading. We therefore believe that this figure truly contributes to the manuscript.

What is certainly intriguing is why did Ernstson and colleagues never include a general stratigraphic sequence in their most important international publications. A simple glance to the regional stratigraphy allows to identify several irregularities with regard to the impact hypothesis. We just hope that this was not purposely done to avoid skeptical points of view!

Fig.3: What is the reason to show this Figure? Folds and thrusts like those shown in Fig.3 are typically observed also in impact structures. Very similar deformations are observed in the rim zone of the Ries impact structure (see, e.g., Chao et al. 1978, Fig.24.8.

Fig. 3) These are geological cross-sections across the main anticlines along the northern border of the Azuara structure. Ernstson and colleagues have never published a similar intermediate depth geologic cross-section, as interpreted from basic surface geology data, which, as with a general stratigraphic sequence (our figure 2), would have made things much clear. The sections show details from a general balanced and restored cross section that was published in Geodinamica Acta (1999; vol. 12, p. 113-132). They are based on detailed stratigraphic, sedimentological, paleontological and structural studies (see references therein). They are important because they show the relationship between folds and strata in the area, and allow to identify clastic wedges of continental alluvial, fluvial and lacustrine deposits which include progressive unconformities. These latter originate by the progressive deformation of proximal beds taking place during time spans of millions of years, a process which is impossible in relation with an impact crater, where most of the deformation occurs in seconds.

Fig.4: What is the meaning of this Figure? I don't see any contribution to the text. Take out.

Fig. 4) With this photograph, we attempt to show that the northern border of the structure does not show overturned beds, inverted stratigraphy or chaotic structuration, but moderate folding and faulting which affect none-metamorphosed sedimentary or brecciated rocks.

Overturned beds are restricted to Palaeozoic beds close to the Datos Thrust, with late Palaeozoic (Hercynian) movements at the scale of the Iberian Chain.

Inverted stratigraphy is restricted to the frontal limbs of the main anticlines (Aguilón, Belchite,…), and to the multiple-phase tectonic deformation within the Palaeozoic massif.

Brecciated rocks are restricted to the Cortes de Tajuña Fm. (Hettangian), paleosols developed over Mesozoic carbonates, and to areas near large faults and thrusts.

Fig.5: Not very conclusive with respect to the impact controversy and the aim of Cortés et al. to eliminate Azuara from the lists and maps of verified impacts.

Chattian-Agenian age of Unit T4: How were the conglomerates dated? Explain! - especially in view of the abundant reproaches addressed to the impact advocates. - Take out?

Fig. 5) This picture speaks by itself with regard to the presence of progressive unconformities and the progressive deformation along the northern border of the Azuara basin (see our reply to Ernstson's comment about Fig. 3 above). We believe it is certainly constructive to provide this information to any potential reader (we would be cheating if we do not show it, together with the other figures). If this photo is not conclusive about the deformational processes, we don't know what else can be more obvious. The photograph is clear and unambiguous with regard to the evidence for progressive unconformities, <u>and</u> we are providing the exact location for anyone to go and check for themselves. As we mentioned before, reference for the dating is provided in the paper mentioned above. See also our reply to Ernstson's comment 16.

With regard to our alleged objective "to eliminate Azuara from lists and maps of verified impacts", see our point E above.

Fig.6: Again, not conclusive with respect to the controversy. What is the meaning of this Figure other than to demonstrate the abundant occurrence of this geologic layering? - all right. Take out?

Fig. 6) See our reply to the previous point for Fig. 5. The subject deserves no more comments.

Fig7: Nice cross-section, but based on what informations? Cortés et al. themselves point to the lacking boreholes and seismic profiles. Figure caption should read as <u>*"Suggested geological cross section .... No informations about the deeper structure of the Azuara basin are available."*</u>

Fig. 7) In geology, any cross-section is interpretive, no matter what (unless it is just a representation of uninterpreted physical data, as in a seismic section, or any other geophysical profile). Our cross-section is just one more possible interpretation, based on the same published and field data that Ernstson and colleagues had available about the deep structure of the Azuara basin. Furthermore, our interpretation is more simple, and leaves less features unexplained than the impact crater hypothesis.

With regard to the figure caption that Ernstson proposes, it is obvious that this (or any) assertion is just a suggestion made according to available informations. For example, we were not able to find in any of Ernstson's papers any figure caption such as "suggested PDFs", "suggested crater shape", "suggested globular breccia", etc., nor any statement such as "no information about age is available", "no information about depth is available", etc. Evidently, any interpretation is just a suggested most probable hypothesis from the authors. This concept is obvious and needs not be specified every time an interpretation is presented to the public, as far as it is clearly stated as such, and not as a dogma.

> Fig.8. gives no new evidence. As long as Cortés et al. do not present results of new investigations of clear pressure dissolution features (shown in thin sections) in cratered cobbles from outcrops investigated by Ernstson et al., all references to other outcrops and authors and deformed Tertiary and Quaternary rocks are waste of time.
>
> Here or in the text, Cortés et al. should refer also to their comment article on the Ernstson et al. (2001c) paper submitted to Geology and to the Reply by Ernstson, Rampino and Hiltl. - Eliminate Figure and refer to these Comment and Reply articles.

Fig. 8) Once again, Ernstson wants us to eliminate any figure which may show information or data supporting other alternatives to the impact hypothesis. This figure is similar to the one published in Geology (vol. 30, no. 1, p. 91), although it is not exactly the same, since it includes more data. We therefore think it is worth including it within the context of the review article.

Ernstson's comment about other people's work being a waste of time is not appropriate. We believe that, if he is so interested, he should have investigated a wider selection of sections where the cratered cobbles are present, and not only those that are convenient for his hypothesis. Disregarding the rest of the evidence says little about the methodology he is using in testing his hypothesis.

> Fig.9: What is the meaning of this Figure other than to show the existence of Liassic breccias (according to the caption) at many places in northeastern Spain? In the text, this Figure is referred to as to show reported outcrops of the Cortes de Tajuña Fm., but this formation covers not only the Liassic but also the Upper Triassic. Explain this inconsistency!
>
> As to the controversy about the megabreccia - see comments 51, 52 -, this Figure is without any support of the Cortés et al. model. The impact advocates have never doubted the existence of the Cortes de Tajuña Fm. collapse breccia throughout Spain. Eliminate Figure!

Fig. 9) Similarly to his previous comment, Ernstson wants us to eliminate any figure which does not suit his model.

About the “Liassic breccias”, we agree with Ernstson. These breccias are present in most of the Upper Triassic-Lower Jurassic transitional layers. They are mostly Liassic, which explains this common "mistake" (actually, oversimplification) of calling them Liassic. But it is not an important inconsistency. The real inconsistency is with Ernstson’s model and the presence of these same breccias, in the same stratigraphic level, 200-300 km away from the target area.

His second comment is not exactly true. Ernstson and colleagues cited “carniolas” and the Cortes de Tajuña Formation but, up to day, they did not acknowledge the existence of this brecciated unit out of the Azuara area.

Algunas referencias que se pueden utilizar.

L. Alcalá, A.M. Alonso-Zarza, M.A. Alvarez Sierra, B. Azanza, J. P. Calvo, J.C. Cañaveras, J.A. van Dam, M. Garcés, W. Krijgsman, A.J. van der Meulen, J. Morales, P. Peláez-Campomanes, A. Pérez González, S. Sánchez Moral, R. Sancho y E. Sanz Rubio (2000): El registro sedimentario y faunístico de las cuencas de Calatayud-Daroca y Teruel. Evolución paleoambiental y paleoclimática durante el Neógeno. 323-343. RSGE 13 (2)

Ancochea, E.; Muñoz, M. y Sagredo, J.: Las rocas volcánicas neógenas de Nuévalos (provincia de Zaragoza); 7 Geogaceta, 3 1987

J. P. CALVO y otros (1993). Up-to-date Spanish continental Neogene synthesis andpaleoclimatic interpretation. *Rev. Soc. Geol. España.* Vol.6 (3-4); 29-41.

Colomer i Busquets, M. y Santanach i Prat, P.: Estructura y evolución del borde sur-occidental de la Fosa de Calatayud-Daroca; 29 Geogaceta 4 1988

Ernstson, K. (1994): Looking for geological catastrophes: the Azuara impact case. In: Extinción y registro fósil /E. Molina, ed.). Cuadernos Interdisciplinares, Universidad de Zaragoza, 5: 31-57.

GONZÁLEZ & J. GUIMERA (1993). Sedimentación sintectónica en una cuenca transportada sobre una lámina de cabalgamiento: la cubeta terciaria de Aliaga. *Rev. Soc. Geol. España.* Vol.6 (1-2); 151-167.

Meléndez, G.; Sequeiros, L.; Brochwicz-Lewínski, W.; Gasiewicz, A.; Suffzynsky, S.; Szatkowski, K.; Zbik, M., y Tarkowski, R.: El límite Dogger-Malm en la Cordillera Ibérica: anomalías geoquímicas y fenómenos asociados; 5 GEOGACETA 2. Junio de 1987

F. Ortí (2000): Unidades glauberíticas del Terciario ibérico: Nuevas aportaciones. *Rev. Soc. Geol. España*, Vol. 13 (2), 227-249.

Pocoví Juan, A.; Besteiro Ráfales, J.; Osácar Soriano, M. C.; González Martínez y Lago San José, M.: Análisis estructural de las mineralizaciones de baritina de la Unidad de Herrera (Cordillera Ibérica Oriental); 25 Geogaceta 8 1990

Torres, J. A.; Lago, J. y Pocovi, A.: Modalidades del emplazamiento de intrusiones calco-alcalinas, Stephaniense-Pérmicas, en el sector norte del Anticlinal de Montalbán (provincia de Teruel); 43 Geogaceta 6 1989

Viallard, P. (1983). Le décollement de couverture dans la Chaîne Iberique Méridionale (Espagne): effet des raccourcissements différentiels entre substratum et couverture. Bull. Soc. Géol. France.