# Review of the thesis of Piotr Jan Strzeleczki: "The origin of deformation bands in the Silesian Nappe (SE Poland)", submitted to the AGH University of Science and Technology, Kraków, Poland

**László Fodor**

First of all, I would like to express my general impression. The PhD work of Piotr Strzeleczki has been executed at a high scientific standard, with careful analyses and resulted in very interesting and well-supported data, conclusions, and reasonable models. My positive view extends to all parts of the manuscript and also to the main publications resulted from this work.

The task of a reviewer however, implies the formulation of questions, criticism, and the remark of some mistakes. Because the work of Piotr is very good and valuable, my comments form and extensive list. However, few (but important) comments question some of the conclusions of the candidate.

In the submitted work itself, I annotated the questioned parts by sticky notes and with yellow colour. The colour, however, also indicates some important segments, that helped my understanding so the yellow do not have a clear connotation. A considerable number of remarks serve just for clarification and spelling corrections. Others are formulated as questions, but I highlights the main features what could be answered by the candidate. Some of the remarks appear only in the text, but most are copied in this detailed review.

## Organisation

The organisation of the thesis is generally very good; the first part of each chapter presents the observation, which is followed by a discussion at the end. One remark here; in chapter 4., maybe a conclusion that fracture sets are symmetrical or asymmetrical to bedding could be given already here. This has not a connotation for the origin (pre- versus post-tilt) but gives just a geometrical statement (which has an obvious conclusion, but it will be given later).

## Terminology

The terminology is an important issue in scientific works. No uniform usage of terms exist in geoscience but we may always try to make closer the definitions. Particularly, because sometimes the term imply a certain interpretation, and another one a different one. None of us is perfect in this field, and some of my comments may also be wrong.

Abstract: Compactant failure: what is this? 'compactant' as adjective is strange for me (and not accepted by my spelling check)

Abstract; the sentence about buckling does not work well. The buckling is itself the folding, so horizontal layer position has been lost. You mean initial shortening?

Abstract; compressional regime? Thrust fault does not seem to be a "regime"

page 1; On page 1 you seem to separate fracturing and faulting; for me the former contains the latter. **How do you mean the relationship of these two processes?**

p. 1; frictional sliding? OK, but more precisely grain boundary sliding

p. 8. **Cenozoic instead of Tertiary**, as far as I know the official/valid terminology.

p. 11. **Thrust and fault are shown as separate features**. In my view the latter contains the former. Maybe faults in general would have been the solution.

**p. 9-11, 39; overthrust. Why not simply thrust? Thursting and overthrusting are separated terms? Fossen uses only 1 time overthrusting. Thrusting would be enough for my 'taste'.**

p. 28. micro-buckling; **the original definition should be cited as reference**. Otherwise, this term is somewhat misleading, while buckling has a slightly (strongly?) different connotation in the case of folding. But later this term seems to be abandoned I favour of kink?

p. 28. I would formulate in different way: more competent grains were pushed into the less competent ones.

p. 30. It is not intergranular porosity?

p. 30; conjugated set; why not simply conjugate set?

p. 35; and fracture-sealing calcite or calcite-sealed fractures

p. 37, 42; joint-sealing veins? Filled joints? **In fact, a joint cannot be sealed**. It becomes vein or veinlet in my terminology. But 'fracture' in general always works. Or extensional fracture

p. 39; (principal) stress axes would be enough and correct for me

p. 39; layer-parallel seems to be the correct term

p. 40. I may be conservative but **hardly use the term "compaction" for a porosity reduction within a sub-vertical zone**. I restrict this word for diagenetic process and sub-horizontal layers. If you use this word differently, you should introduce this in the early part of the MS. I would eventually use shortening or other term.

p. 40; and earlier. You should define the two terms (if they refers to different phenomena).

p. 41. What do you mean with term? (diffusive growth of DB?)

p. 44. How do you mean closed and hydrothermal together? Maybe my knowledge is limited in this topic. You need large fluid flow cells to heat the fluid, so this could hardly be closed. Not speaking about fluid-rock interaction

p. 46; 'Banding': if you refer to DB formation, I dislike this word. In my view, no this type of abbreviation could be possible. Maybe 'shearing within DB'?

p. 48, but earlier, too: sealing fracture veins; correctly: fracture-sealing veins. Or tension gash, if you like it. See hyphenation problems in the 'Grammar' chapter.

p. 72; "minimum horizontal stress ($\sigma h$) becomes higher than the vertical stress ($\sigma v$) and consequently, the stress field permutes to a thrust fault regime". I do not think this is the case. **The $\sigma 3$ becomes the vertical stress**. The minimal stress is the smallest, cannot be larger to any other component.

p. 76, but already much ealier; I may disagree with that term here. Fossen 2010, fig. 11.33., .34 shows that **kink bands make an angle to $\sigma_1$ and not perpendicular to this axis**. They imply shearing along the axial plane. The structure you refer here an earlier is similar to his bending (Fig.11.31c). See also the dictionary of this book about kinks.

## Geology, question related to geological interpretations, etc.

p. 10. Locally it may be true. But not true in the origin of the basin, which is a foreland flysch basin. So a syn-sedimentary thrusting cannot be excluded (neither proved, probably).

p. 18. You mean cooling here?

p. 19. The areal extension and thickness of the upper member can be in contradiction; (small thickness, versus largest areal extension) namely, it can only be true if strong (isoclinal) folding appeared in the member. It is drawn like this, but this section could hardly be balanced in my view.

p. 27. Fig. 4.8D. This is the first case when I doubt this interpretation. Could be just real curvilinear faults

p. 27. How the orientation of micas were calculated? I guess this is shape-preferred orientation, and not crystallographic?

p. 28-29: How fracturing could occur perpendicular to bedding? They are not tensional fractures, perpendicular to $\sigma_3$, but planes perpendicular to $\sigma_1$. No shear is resolved along such planes and no tension could occur. What could be the formation mechanism? You should explain it much better in the text, because it is not obvious.

Related to this, figure 4.9A shows a kink and a microscale buckle fold, along the former a measurable displacement along the DB. For me, it is not a pure PCB, because there was shear along the plane of the DB. Could be transpressional CB?

p.31; If I understand well, on fig. 4.10 the DBs contains large amount of pores, which are interconnected. **Can you comment this?**

p. 30. Why the tilting is not due to shearing?? 4.9A microfold could be due to minor shearing!

p. 30-33: good, exactly this is my experiences (though limited); no pores within DB (thus no pore-filling calcite) and much more pores (and calcite) outside the DB.

p. 39. extension is assumed to have been connected to exhumation. Why? deep burial would not create vertical shortening?

p. 39. Extension and compression are considered to have been coeval. Why? There is no time slice for slightly (or even considerable) time shift between the two events? What about

p. 39. The figure 4.1 does not reflect a simple fold of concentric type, where such situation may exist. The isoclinal folds pose a different tectonic scenario, where non-concentric folds should appear. And also, it depends on the location of the neutral surface of folds, if i guess well.

p. 40. Sub-vertical CDB is not compaction in my terminology. It is a horizontal shortening. The problem here is that the displacement would not be larger in shear band than by kinking (or micro-buckling)! **So the shearing is present in these bands**! And we go back to the problem of fracturing without tension and shearing. **I admit this can be the case, but you should discuss it and the mechanism behind this! The author should precise more in detail the mechanism of fracturing in sub-vertical PCDBs, perpendicular to the S1 stress axis. It is not obvious, a reference would not satisfy me (and potentially other) readers. (The short description on p. 40). If these fractures are vertical, they are difficult to explain. If not, they could be shear microfractures.**

p. 40.41, tomography. I understand this can represent a problem when the observation does not reflect the expectation. Here I am lost a little bit, **please clarify the situation**. The dark colour apparently indicate porosity which could be due to secondary fracturing? Which was not sealed? Or reopened by near-surface dissolution of calcite? **Porosity along but outside of the DB is normal**; this is the buffer, represent contrast for a lot of processes, logically flow, dissolution, could occur there, not?

p. 42. The relative chronology of folding, thrusting and mesoscale thrusting (DBs) are not verified. There is no explanation on p. 42.

p. 42. **Origin of joint; why they would be related to strike-slip regime**? Cross joint for me would reflect along-hinge lengthening (probably due to increase of bending along the arc? Second part of the paragraph: OK for longitudinal joints.

p. 46. You rely very much this temperature estimate. However, the data do not constraint very well the temperature. Graphically at least, the temperature could be changed easily, particularly near the starting point of the curves (where only a short distance change on x axis would result large change in temperature).

**The interpretation of the observations is always the most interesting – but most difficult - part any works; this is the case in the work of Piotr as well. The Summary, mainly from p.46 to 49, give a cohesive view of the author. However, I may challenge some of the interpretations and technical**

**parts of these pages. Some of the comments may be wrong, others may point to steps after the thesis work.**

In the model almost all the deformation happened at the early stage of burial. Nothing occurred during the thrusting, when Dukla nappe thrust over the Silesian unit. This would mean thrusting without footwall deformation; this could be the case, but would be very unusual for me, just near the thrust contact. The same is interestingly true for the late phase (exhumation) when only re-opening of existing fractures appeared but no newly formed fractures formed. **Can you comment these comments?**

You suggest that extension and contraction (by pure compaction DB) were almost coeval, and represent a sort of strain partitioning between layers occupying different position within the folded sequence. However, your figure 4.18 is imprecise in the sense **that lengthening of the outer part of the fold in not in the hinge of a syncline,** as you put on the point 2 of Fig. 4.18. This is even more marked on sub-figure 3, within the hinge zone of a very closed fold, the hinge would experience contraction – you would see thrusts accommodation additional shortening in the core. Here you missed to mention what type of folding you suggest; concentric, similar? the closed interlimb angle would not vote for the former….**So I think that you may turn to alternative scenario to explain almost coeval shortening and extension.** However, **in a simple tapered wedge model of thrust belt the extension is logical step during the generally contractional regime**; when the critical angle of the orogen becomes to large (too high orogen) than an extension would lower this angle toward the stable value. The reason behind is probably found in the geometry of the subducting slab beneath this basin (with incipient deformation); see Balázs et al. (2022) for this issue. This may provide you an alternative scenario. If you keep the folding model, you may assume the migration of the site Q within the fold system; from an external hinge position (anticline) to the core of the syncline, this is not excluded (for fault-bend folding this is a must, not an option)

(Balázs et al. 2022: The Dynamics of Forearc – Back-Arc Basin Subsidence: Numerical Models and Observations From Mediterranean Subduction Zones. Tectonics, 10.1029/2021TC007078

If you put a high (~100°C) temperature for calcite 1 this would be higher than the rock temperature. OK, it is really a hydrothermal system (what you should have declared more clearly). **However, it has an implication for regional fluid flow; in a foreland basin,** I would expect low temperature and not high. Only if fluids from the orogen descended deeply and brought deep and warmed fluids from below the orogen to the foreland basin, than you can have high temperature. It is just for future thinking of fluid flow modelling. **However, it is again the importance of showing the possible regime of calcite 1 on upper fig. 4.17!**

Why I insist on calcite 1? Because for me the relationship of thrust-type shear DB and calcite 1 is the key to say if the folding happened in shallow depth, as your fig. 4.17 upper curve, point 2 would suggest. If calcite 1 appeared almost in point 3, then thrust-type DB or thrust fault(?) would have formed in deeper position.

In this issue a lot of things depend on the terminology, definitions, and some preservation/observation questions. You seem to shift from DB interpretation toward thrust faulting, even within the thesis. You start with DB and change to thursting, if I see well. It is logical/natural during research, but it is better to finish with a solid conclusion in this issue. **So what is your final word in this question; thrust-type DB, fault or both?**

Related to the previous; separation of faults from DB is difficult. But for me the striae is important; they indicate strain localisation along a plane, which is a character of a fault – and not for DBs.

If one accept early tilting (at least in R) could it be a simple gravitational submarine sliding?

**The question of reactivation is supported by the figure 4.14C**. The DB (originally PCB?) seems to have a clear shearing character. Or, at least, the calcite has been formed due to shearing along the DB. **But here a formerly pure CDB has been reactivated as shear DB, due to rotation of the DB itself.**

**It might have an application to your model**, (shear-type reactivation after rotation!) and this I do not see in the text.

You should clarify how the thickness values were obtained for fig. 4.17? I may miss the explanation…

The problem of late-stage exhumation. **Do you mean exhumation by denudation or by faulting**, if the latter where are these faults? A pervasive and ubiquitous normal faulting would also leave at least few traces on the map, **where are these elements**? Despite this small questions, I agree that the opening of fractures could really related to a young deformation event. I would even agree with that gravity induced re-opening of some fractures. OK, but in this case, any connection does not exist with stress field, but with the topography, the strain/stress axes could be even oblique to the surface. If one assume a true tectonic force for extension; then **the driving force should be found**. In the Late Miocene I do not see such plate-tectonic force, but gravity-related deformation could be OK. Again, why there are not new fractures in the game and why fractures oblique to the surface and bedding were reactivated? In tectonic scenario, opening could/should be highly oblique for a lot of fractures not oriented favourably to (vertical/horizontal) stress axes.

In idea; why the "normal faults" are not reverse faults in near-vertical layer position?? **Could it give a solution for relative chronology** of this enigmatic extension?

***Technical remarks,*** p.46-48, including fig.4.7, 4.18.

- The reader is somewhat lost in the chronology of the fracturing, cementations events, in overall, the story. The connection between different figure parts are missing for me. For the presentation, you may improve the fig. 4.17 and also on Fig. 4.18. The steps shown on lower row, fig. 4.18 is only vaguely shown on fig. 4.17 and these numbers does not appear in the main part of fig. 18. You may easily put numbers on the left side, making connections between lower row and main part. You can separate shear band formation (as an event of 1a, or 2c?) from the faulting events, both for normal and thrust type features. They might have different timing, and seems to be a later(?) event in your chronology. I would agree if the normal shear bands and also the thrust type were reactivated later as faults – but a separate visualisation would help to understand them (fig. 17 mainly).
- You may help understanding if on the subsidence curve, some of the features are indicated. Calcite 1 seems to me the most important, and also thrust-type shear DB; I would like to see them directly on the subsidence curve.
- There is however, one problem; the stereograms suggest pre-tilt origin even for the striae themselves. (If I understand well that the points and arrows on fig. 4? indicate calcite fibres of faults).
- Folding should also be part of the figure 4.17, make a separate line on lower sub-figure. On fig. 4.18 in the site Q there was folding between step 2 and 3, (ca. 120° rotation), and it is not seen between the boxes of the right side 3D models (otherwise they are nice!).
- Finally, I understood that the temperature curve for the host rocks were different from the fluid temperatures in different calcite generation. It would be really helpful to see this on a diagram similar to fig. 4.17 upper part. Maybe it is too dense to put all data there (and you would need a new axis on the right side) but a separate figure would also help. In this way, the conclusion could be visualised; the fluids were always former than the rock themselves.
- 4.18. Quarry section. The rotation arrows showing rotation should be just opposite. It is not clear how the block diagram reflects the difference in step 2 and, when additional 120° rotation occurred.
- So make modifications on this figure; using enough numbers and make them a unique equivalence between figure parts, and also between figure 4.17 and 4.18.

p. 50. points 2) and 4). What's the truth about calcite "within" and "along" and "outside" the DB? For me, point 2 indicate zero calcite within, point 4 a lot of late calcite within the DB. ("See the statement in point 2). The two seems to contradict to each other. **You may precise here, what you mean "within" the DB** or just ", versus "along their boundaries". If there is no calcite, no reactivation "within" the DB")

p. 51-55: There is no **clear definition of first-order and second-order folds**.

The stereos reflect a certain scatter of fracture planes. E.g., 7 is almost a conjugate shear set. **What is your explanation for this?**

**I have (a little) concern about the classification of all bands as pure compaction.** There is evidence of shear, at least in the so-called kink/microfold type bands. The stereographic picture may indicate shear or hybrid factures as well. On the fig. 5.3. there is clear displacement on both the lower and upper bedding surface. So how do you comment these remarks? **The problem with the potential existence of shear bands instead of compaction bands, that they would have normal shear sense, as fa as I see (on photos and stereos). So this would change the interpretation considerably. Your comment?**

p. 55. Zatwarnica anticline has different strike on the map as deduced from the dip data. Site 1 has even more different dip. Explanation?

p. 53, Fig. 5.1A; **How this section was constructed? How the thickness was estimated? What could be the origin of these variations?**

p. 56, Fig. 5.4C, D; I may suspect striae on surfaces, near PCB for example. On D, there are nice small-scale faults, or DBS?

p. 57, Fig. 5.4B; for me the lower sample would show shear bands. There is a clear displacement, in the order of what would be expected of such bands. What is your reaction?

p. 59; **for me the pore collapse is not a mechanism**. It is a phenomenon whose result is the decrease of porosity. The mechanism of this process is the grain reorganisation, by rotation, sliding, or fracturing. If I look to the classification of textbooks, like Fossen, Structural Geology, this is not listed as mechanism. If you have taken this term from publications, you may refer to this work.

p. 61; what is the difference between fig. 5.8 CR and CP sub-types? For me they are very similar.

p. 73; The core of fold, you do not see, so you have no control if anything changes there or not. Am I right?

On pages 69–76 the author tries to quantify the circumstances for the DB formation. These calculations depend on several factors, all of them are not very well constrained for the study area; so I consider them as preliminary approaches. Few questions related to this part. Thicknesses and burial. It is not exactly clear how you have got the thickness values. How could you take into account of internal (within unit) repetition by small-scale thrust faults? Invisible folding? As I am not so familiar with the theory behind, I may ask non-relevant question; **are the pressure estimates „calculate" fracturing**? The yield word would point to this. However, what about rotation, and grain-boundary sliding? Is there a yield in these mechanisms? Are these mechanisms involved in such calculation? They are important mechanisms in PCB formation, as you demonstrated well.

Sometimes **it seems that the author estimates the formation time of PCBs as a short period; my estimate would be opposite**, they were formed gradually, the lower ones earlier and the higher ones later.

p. 74; "present-day stratigraphic overburden". For me it is zero, because they are on the surface. You wanted to say the equals more or less with the thickness of the still preserved (largest), non-eroded cover sequences (of the Lower Krosno Fm.).

p. 80-81. **The faulting developed because the sequence was progressively buried and the rheology has changed**. You do not need to have had overpressure.

The problem with the otherwise nice figure of 5.18 (and related text) that **the main deformation phase, the thrusting itself is missing**. OK, after your opinion, this phase did not play a role in DB formation, but still, this is a major gap in this figure. In this way one can hardly know which unit covered the whole formation? **How the back-thrusting was related to fore-thrusting, what I consider as the primary phase?** The backthrusts appeared in the front of forethrusts? **Is it mean a triangle zone?** Did the folding occurred in a sort of triangle zone? How this folding was related to backthrusting (in time)? All these questions are not very clear from the discussion. OK, I may also agree this would represent the target of further study, a real quantitative reconstruction – **but you may challenge an answer.**

## Grammar, mistyping

The English of the thesis is very good, very fluent and contains only minor mistakes – as far as a non-native English speaker can judge this.

p. 4; shear-enhanced, as correctly shown on fig.1.2.

p. 6; Bieszczady Mts. 2x

p. 8. **Thrust, thrust thrust**, irregular verb

p. 8. **Convergence is not advancing**, it is a process, or deformation style. Advancing deformation front could be correct in the context.

p. 12. sericitic phyllite

p. 13; calculated...? Something is missing here

p. 20; carbonate slickenfibres not, calcite or carbonate mineral slickenfibres

p. 46; thrust-type shear bands might remain open and enhanced fluid flow? This is what you wanted to say?

p.48, fig.4.18; "pure compaction bands reactivation" for me is not correct. '*Reactivation of pure compaction bands*' is definitely OK.

p.52. „On the perpendicular to the bedding plane cross-sections" it is a typical expression, trying to shorten the text. Sometimes it does work, more often does not. '*On the cross-sections perpendicular to the bedding plane*' would certainly be correct. if you want to use a combination of words as adjective, hyphen is needed; '*On the perpendicular-to-the-bedding-plane cross-sections*'.

**The hyphenation remains a problem through the text. Fault-related folds, etc.** Take care on correct spelling in these composite words.

## References

They are always relevant. The list reflects the large and broad view of the candidate I his field.

p.9. but already Jiricek 1979, Linzer 1995 Geology, so there are several works that showed this

p.9. Rauch could be mentioned, she showed two shortening directions in the OWC, if I remember well.

## Figures

**The figures are generally very nice and informative**. The Legend often could be more extensive, explaining some of the symbols used, or just giving more explanation. Consider that readers can consult only the figures and not reading the whole text.

2.2; cross section: legend is not identical with the section itself, probably the middle member is stippled on the latter.

4.1; see comment in Geology chapter

4.3; and others: pole of bedding is missing for me!

4.4., p. 22. The indication for bedding is missing for me

4.9D, p.28: Displacement direction is worth showing

4.10., legend: Instead: ...along one of the DBs. But which one? An arrow would help.

The location of the figure on 4.7 is not indicated or simply I do not see this. In the legend, the reference for figure is missing/incomplete.

I do not understand clearly the orientation of the stereograms. What is the equatorial plane of the net with respect to the Figures 4.10A, B?

It seems to me that the Fig 4.10D is viewed from upside down with respect to part C, isn't it?

4.12. What is on the Y axis of the area fraction sub-figures?

p. 53, Fig. 5.2; I understand that the author wanted to reduce the size of the figure but the changes of the orientation of the map is not helpful for the readers

p. 56, Fig. 5.5; bedding planes or their intersection lines could be figured.

p. 71. 5.14, and related text; ...indicated by grey and black letters in Fig. 5.14. This addition would be necessary in the legend as well!

p. 78, Gig. 5.16; Here you speak about a ridge (SE from the Silesian basin). However, it is not shown on cross section.

**Altogether, (and despite the comments, question raised in he work and in this review) I consider that the PhD work of Piotr Strzeleczki is above the general standard of a PhD thesis and largely merit the doctoral degree.**

In a traditional classification (applied in Hungary but probably elsewhere) the evaluation would have been "summa cum laude".

**I congratulate to this work!**

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