**Panel Summary #1**

The panel and the external referees agree that the PIs have outlined a strong project that should constrain two problems in the application of laboratory rheology data and the interpretation of seismic data: (1) the influence of pyroxene on the rheology of peridotite and (2) the influence of strain and pyroxene content on the development of LPO. The techniques to be employed are appropriate and the descriptions of the field areas indicate that there are sufficient structural markers to quantify relationships between strain and LPO (although the uniqueness of the field areas was perhaps overstated). By the nature of the problem, there will be some uncertainties in constraining the conditions of deformation. For example, how can temperature of deformation (versus cooling temperature) be constrained? One mail reviewer also emphasized that the PIs have not incorporated recent observations on the role of water on fabric development into their hypotheses. The panel urges the PIs to take account of the possible influence of water in their analyses, but did not view this omission as a "fatal flaw". The project takes advantage of a strong international collaboration and provides an excellent opportunity for a graduate education on a project which bridges structural geology, rheology and geophysics.

**Review #1:** Excellent

This excellent proposal is for research that should solve fundamental questions and advance knowledge and understanding within the field of structural geology and mantle/lithosphere dynamics. This is not a statement I make lightly, and I can only remember making a similar statement once before, years ago. It was a pleasure to read such a well crafted proposal.

The big picture question is the relative strength of the different layers in the Earth's lithosphere, which is critical to understanding lithospheric deformation and plate movement. They propose to investigate the rheology of various mantle compositions through a combination of field-based research and comparison with experimental results. This approach is important because many variables in the mantle (e.g. the effects of large scale structures) cannot be tested in the laboratory, plus some of the recent experimental results by Ji et al. (2001) are opposite what is observed in natural samples.

They have carefully selected two locations that are well suited for their investigation. One will allow them to test the differences in behavior of a pure olivine unit, a pure orthopyroxene unit, and a bimodal mixture. The other shows large scale folding and shear localization. From their descriptions, these two locations are ideal and will allow a multi-pronged approach to investigate this problem. They propose to do detailed field mapping, calculate finite strain from dikes, determine lattice preferred orientations, geothermobarometry, and apply experimentally-derived flow laws to determine the rheology of the different units. They very carefully describe how each part of the investigation will be carried out, how the different parts are related and build on each other, and what specific fundamental questions each part will help answer.

Both investigators are extremely well qualified to do the work. Tikoff is an excellent structural geologist with the appropriate expertise for the strain studies, kinematic analyses and field work He has previously made fundamental contributions to structural geology. Newman is also and excellent scientists with a strong background in deformation mechanisms and processes, including working with mantle rocks, and experimental deformation. The combined talents are well suited to this investigation. Little is also an excellent structural geologist and will provide the needed local expertise in New Zealand. The universities have the appropriate facilities.

Broader impacts

They will be involving one graduate student and several undergraduates, plus will be setting up a graduate student exchange with a New Zealand student. Based on Tikoff's and Newman's past record, they will disseminate the results in appropriate scientific journals.

Summary

This is an excellent proposal to address a timely problem using a creative combination of approaches to answer fundamental questions. Both are excellent scientists. It should be supported.

**Review #2:** Good

What is the intellectual merit of the proposed activity?

This is a well-written, well-thought proposal on field-based study of rheology of the upper mantle. The PIs combine expertise in structural geology of meso- and macro-scale together with micro-scale deformation to propose to better characterize the rheology of the upper mantle. The goals of the project are well defined, although none seems unique and this research seems to provide some 'incremental' progress in this area. My main concerns are (1) the inappropriate setting of the problem, and (2) the lack of knowledge of recent progress in experimental studies on upper mantle materials (mostly olivine), which has led to a fatal omission of one key issue, namely water. As to (1) I disagree with the PIs that the main issue in establishing the rheological structure of the lithosphere is the upper mantle rheology. The upper mantle rheology has been very well characterized by the experimental studies on olivine (and to a lesser extent those on opx) including recent studies on the role of water on olivine rheology by two Minnesota groups (Kohlstedt, and Karato group). I notice that none of these critical papers is cited (the point (2)). Also, there has been a major modification to the grain-size paleopiezometer, which is again ignored by the PIs. At least for olivine (in dislocation creep regime), a (nearly) complete rheological equation has been established including the parameterization of effects of water and pressure. In contrast, quantitative rheological equation is still missing for any of the crustal rocks.

The inference of T-P, stress and water content at the time of deformation is very critical, but this part is very weak. I do not see how the PIs infer the T and P at the time of deformation rather than obtaining T and P of last chemical equilibrium. Nothing is mentioned about water. Rheological contrasts (between the lower crust and the upper mantle, also between olivine and opx) are likely affected by the water fugacity (or water content). The lack of information about water or lack of any attempt to determine water content significantly reduces the scientific value of this whole project. The description of application of paleo-pieozometer is very weak too. In this type of analysis, we usually focus on recrystallized grains in areas that are made of single phase (in regions of multi-phase rocks) and hence the presence of a secondary phase particles is only a minor issue.

Study of influence of two-phase mixture is rather disappointing. If as PIs say the peridotites there show 'interconnected weak layers', then the influence of a secondary phase (i.e., opx) will only be minor. Why then do they worry about this minor issue?

Despite these shortcomings, which are in my opinion, serious, there are some plus in this proposal. A proposed study of relative rheology (viscosity) from the structural analysis of folds is an interesting attempt.

In short, this proposal is very strong as far as the classical structural geology approach is concerned (mostly 'kinematic' approach, and to some degree 'dynamic' approach), but weak in the area of applying modern (latest) experimental studies that are highly relevant to the proposed work. Considering all aspects, I would recommend support for this proposal, if the fund is available (G or VG).

What are the broader impacts of the proposed activity?

Involvment of a graduate student and undergraduate students is a plus.

Summary Statement

Well written proposal from classical structural geology point of view. A weak proposal from the view point of lithosphere rheology mostly due to the ignorance of key factor, namely water. The PIS are unaware of important recent development in laboratory studies.

**Review #3:** Excellent

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| What is the intellectual merit of the proposed activity?  Scientific excellence of the researchers  Dr. Basil Tikoff is a highly respected structural geologist who is internationally  well-known for his work on finite strain and petrofabric analyses. He has made  important contributions to our understanding of deformation processes in the  crust and upper mantle. His papers on transpressional orogens (1993, 1994, 1997)  are particularly important contributions. Dr. Julie Newman is also a very high  quality researcher with an established reputation. Both applicants' published  results are of very high quality and at the cutting edge of the science.  Intellectual merit of the proposal  The proposal aims to provide new constraints on rheological properties of the  upper mantle from naturally deformed, serpentization-free, monomineralic  dunite and pyroxenite and polyphase peridotites from the Twin Sisters massif  in Washington State (USA) and the Red Hills area of New Zealand. That sort  of information is crucial because it represents the fundamental input parameters  for any and all rheological models of lithospheric deformation and geodynamics.  Those data can only be obtained through careful observations of experimentally  and naturally deformed rocks using cutting edge methods and techniques. The  applicants certainly have the expertise to carry out the proposed research, and  have proven their ability to do so. The proposed work will keep them at the  international forefront in the discipline. The proposal is generally well thought out,  planned and structured, and almost all elements, technological and personnel are  in place. This is also a good project for the training of graduate and undergraduate  students. There is no overlap between the proposed research in the Twin Sisters  massif and those conducted previously by N. Christensen.  The research should be funded.  The following points need to be taken into consideration in the proposed research:  (1) Deformation mechanism maps can only be constructed for monomineralic  aggregates, but become meaningless for polyphase rocks in which different  minerals may deform by different mechanisms.  (2) The applicants propose to use the relationship between wavelength and  thickness of folded pyroxenite layers in the matrix of dunite to estimate the  relative viscosity between olivine and pyroxene. However, folds can be formed  by various geological processes such as compression, shear, and back-rotation  between extensional shear zones (e.g., Harris et al., 2002, Earth-Science  Reviews, 59, 163-210). Thus, the relative rheology of the folded layer and the  matrix as a function of the fold wavelength and layer thickness is certainly not  unique for the folds formed by different processes.  (3) The applicants used their results shown in Fig. 5 to support the numerical  model of Tommasi et al. (1998) and against the interpretation of Zhang and  Karato (1995). However, they appeared to take for granted that the movement  direction was parallel to the shear zone boundary and the resulting deformation  in the zone was simple shear. This had to be proven to be true. Virtually  movement along many natural shear zones is commonly oblique to the shear  zone boundary and contains a boundary-parallel (simple shear) component and  a boundary-normal (pure shear) component. In other words, deformation in the  shear zones is commonly transpressional or transtensional.  (4) The applicants propose to use the "interconnected weak layers" model of  Handy (1994) to compute the rheology of two-phase mantle rocks. In fact, this  model yields the same results as the Reuss average that was based on an  unrealistic assumption that the stresses are equally distributed among  different mechanical phases.  **Review #4:** Excellent  This proposal, meant to study two peridotites on which relatively little  structural work has been done, is excellent. I know the work by the utrecht  group as a successfull combination of field and microstructural work, and  the involvement of Julie Newman from that group, and the expertise of  Tikoff in structure is a strong combination. The idea to use dykes for the  analysis is very good. The proposal is well-written and referencing is good  and up to date. The publication record of the participants is very good,  and the proposal sound like a promising project that will definitely  benefit us all in understanding more of mantle rocks trough more field and  microstructural observations; the more peridotites investigated in  structural detail, the better. The financial proposal is sound. I strongly  support this project !!  **Review #5:** No Rating Provided  Determining mantle rheology from field and microstructural observations of  naturally-deformed peridotites  I should begin my report with the caveat that I have not been able to provide my  normal level of review due to considerable other commitments. However, I do believe  that my comments are nevertheless justified and useful in the assessment of this  proposal.  This is a project close to my own heart and I firmly believe that it should be supported.  As the proposal states, in spite of considerable efforts there remains much to learn  about the 'structural geology' of the Earth's mantle. Personally, I subscribe to the  view that there is serious 'regional' structural geology in the mantle, probably  similar in styles to that found in the crust but on larger scales. I see no reason  therefore why we should not expect to find, for example, both shortening and  extensional systems, such as 'thrust' zones and 'normal fault' arrays, throughout  the mantle. However, this view does demand that the mantle contains lithological  variations that promote structural identities such as folds, shear zones and faults.  It is only by proposals such as the one presented here that we can advance our  understanding of this crucial region of the Earth, which ultimately impacts on most  if not all Earth system processes.  For me, the main problem with lithospheric models to date has been an over-reliance  on the 'Christmas Tree or Bird's Beak' constructions, which in their simplest  formulations have a 'strong-weak-strong' sandwich-like appearance. This over  reliance is understandable given the fact that the models have essentially been  utilised by geophysicists who need to make basic assumptions in their interpretation  of geophysical data. Unfortunately, this has led to a degree of circularity in the  arguments presented to understand, for example, lithospheric rheology. However,  superposition of known crustal lithologies, structures and flow laws results in  essentially any rheological variation required in the profiles. Thus, whilst the aims of  this proposal have been known for many years, they have not been exploited. It is  the intention therefore to exploit our current knowledge and database constructively  that makes this proposal exciting.  I do not know the two field areas proposed from personal experience. However,  my knowledge of other exposures of (upper) mantle suggests that whilst some  lithologies are indeed relatively simple, others are much more complex. For example,  similar metre-scale, and larger, isoclinal folds of orthopyroxene layers within dunite  and peridotite occur in the Lers massif, France, whilst mantle nodules (e.g. from  my own collection from southern Africa) may contain complex and varied lithologies  and microstructures, such as olivine-pyroxene banding and grain size variations, and  porphyroblastic clinopyroxene and garnet. One reason why the 'strong upper mantle'  hypothesis exists could be due to a failure to recognise, or to oversimplify, such  lithological complexity. For example, even the simplest rheological constructions,  based on quartz-feldspar crust and olivine upper mantle flow laws, results in only  a very thin 'carapace' which is 'strong' (i.e. brittle), as my final year students soon  discover in one of their rheology practicals!  Unfortunately, whilst our knowledge of the deformation behaviour of monomineralic  lithologies (e.g. quartzites, pure marbles, dunites, etc.) has progressed significantly,  particularly in recent years due to the evolution of new techniques (e.g. such as  SEM/EBSD, which will be exploited in this proposal), the same cannot be said of  polymineralic lithologies that are volumetrically the most important constituents  of the Earth. This is true particularly for the development of lattice preferred  orientation (LPO), which appears to be quite different in polymineralic rocks  compared to monomineralic varieties. The problem appears to resolve essentially  in to deformation partitioning between load-bearing and non-load bearing phases  and the maintenance of strain compatibility in the aggregate between different  neighbouring phases. Thus, the intention of this proposal to investigate the rheology  and behaviour of polymineralic rocks is in itself justification for supporting the proposal.  Even biminerallic rocks are likely to represent a simplification of the behaviour of  most rock types. For example, I have been amazed by the complexity and/or simplicity  of the LPO I have measured recently, using EBSD, for the various phases in amphibolite  and granulite facies gneisses from NW Scotland, the Himalaya and southern India. It  is possible therefore that LPO investigation will reveal only a part of the story. For  example, the advent of new analytical techniques has shown that there are crucial  aspects of rock microstructure that have not previously been taken in to consideration  (e.g. grain boundary networks, misorientation, etc.), which might impact significantly  on rheological behaviour. Although the proposal does not mention specifically these  aspects, the opportunity exists within the proposal to pursue them simply because  the basic data (i.e. LPO measurements) will have been collected. Furthermore, using  the LPO measurements it is a relatively simple matter to predict the elastic and  hence seismic properties of the samples, which might ultimately be used to predict  and/or constrain existing geophysical models of the field (and similar) areas (e.g.  the complex LPO for individual phases that I have measured in gneisses typically  combine to yield rather simple predictions of the three-dimensional seismic  properties of the whole rock).  The fundamental questions posed by this proposal are indeed fundamental and  important. I am not sure that this proposal alone will answer all, or even most,  of them. However, this is not a criticism; it is rather an example of the appropriate  level of ambition and understanding contained with the proposal that will add  significantly to our knowledge. My responses to these questions are as follows,  which I include as a means of both reviewing and stating my overall support for  this proposal.  Question 1. Is it possible to calibrate LPO development as a strain gauge?  In my opinion, not without a lot of work, if at all (i.e. see previous comments  on LPO development in polymineralic rocks). Polyphase materials may respond  in a variety of ways that may either enhance, impeded or reduce LPO development  (e.g. cyclical dynamic recrystallisation, grain-size reduction and associated  transfer to diffusional creep, strain-hardening exacerbated brittle deformation,  etc.). Thus, even the presence of LPO may not be indicative of the total strain  sustained. However, this view should not prevent an attempt to calibrate LPO  development as a strain gauge, as even failure will lead nevertheless to  significant further understanding of the behaviour of complex but typical rock  types.  Question 2. Does the presence of more than one phase affect slip system activation?  Almost certainly, but so will water content, temperature and pressure differences,  amongst other factors. However, as mentioned previously, understanding the  behaviour of polymineralic rocks is now achievable using modern techniques.  One of my few concerns about this proposal is the emphasis on dislocation creep  as the main deformation mechanism. This needs to be established and may  well apply only to some of the minerals phases and/or lithologies present  (e.g. my own observations on polymineralic crustal gneisses suggests that  dislocation creep is active in some minerals whilst diffusional processes, both  creep and reaction related, occurs in others, and the occurrence of boudinage  attests to both brittle, i.e. layer fracture, and diffusional, i.e. infill of gaps,  processes). I would caution also in using etching procedures to image dislocations.  In my experience, there are too many other variables that might impact on the  etching characteristics of minerals and I would always recommend using TEM,  which provides much more quantitative information, particularly now in  collaboration with SEM techniques.  Question 3. What is the effect of a second phase for palaeo-stress estimation?  I would suggest that this will be significant and that considerable information and  understanding is available already via the literature on composite materials. The  measurement (e.g. via ultra-sonic, neutron diffraction, etc.) and/or prediction  (e.g. via theoretical modelling) of the (e.g. elastic) properties of composite  materials is a major aim of modern materials technology research. Polymineralic  rocks and the Earth's mantle are merely examples of extreme 'technological'  materials and their 'operational' conditions. However, the problems mentioned  previously concerning the establishment of an LPO derived strain gauge are  also likely to apply to any palaeo-stress estimator.  Question 4. Can we calculate rheology for naturally deformed two-phase mantle  rocks? Most certainly and this aim is certainly readily achievable given the  design of this proposal. However, I would caution against assuming a single  power law exponent value (i.e. n = 3). This parameter ranges considerably in  rocks (and other crystalline materials) and indicates different rheological  responses. For example, in superplastic materials, 1 < n < 3 and it is interesting  to note that superplastic behaviour is typical of two-phase materials, particularly  where one of the phases is modally dominant. In addition, superplastic behaviour  and strain localisation may be intimately related.  Question 5. Does compositionally-controlled metre-scale partitioning of strain  occur in the mantle and if so what is the rheology of a banded material? Of  course (e.g. folds of orthopyroxene in a matrix of olivine) and can be explained  and investigated partly by the methodology presented in this proposal and also  by my responses to previous questions. In addition, the presence of boudinaged  layers can be exploited both as a strain gauge (i.e. the well-known and variously  termed fibre-loading/stress-transfer/shear-lag/strain-reversal model) and also  as a means of estimating the far-field rheology (e.g. see 1980's papers of  C.C. Ferguson).  Question 6. Does the mantle deform on structures that are larger than 1km?  My own prejudice is that it most certainly does. As it becomes possible to fine  tune geophysical profiling of the mantle to improve spatial resolution it is becoming  obvious that the mantle contains 'structural provinces' similar, but on larger scales,  to those observed in the crust. The crucial role played by proposals such as this  is to provide understanding of the rheology of mantle rocks, which in turn can be  used to interpret and constrain geophysical profiles.  Question 7. What controls LPO in mantle deformation? Very simply, precisely the  factors that control LPO development in crustal rocks, only the ambient conditions  and mineralogies are different. Solutions to LPO development in mantle rocks,  particularly polymineralic varieties, will have significance therefore for the  behaviour of more familiar crustal lithologies and environments. There is a  significant synergistic aspect to this proposal that will provide multiple feedbacks  and links to a controlled whole-Earth system.  Question 8. Why does the mantle localise deformation? Once again, for precisely  the same reasons that deformation localises in the crust. There is nothing special  about mantle rocks in a continuum mechanics sense. They are merely the stable  materials for the (range of) conditions extant throughout the mantle. Thus, the  results of this proposal will be significant not only for the specific questions posed  concerning mantle behaviour but also for our understanding of the mechanics of  geological deformation in general.  In summary, this is an exciting project the aims of which can be achieved by  the participants who have demonstrated previously the knowledge and abilities  required. There is an impressive list of collaborators who will no doubt impose  their own experience, expertise and enthusiasm on to the project. I particularly  welcome the intention to incorporate into the project training of undergraduate  and graduate students, especially concerning field mapping training and integration  of field, laboratory and other techniques. The international collaboration planned  with New Zealand staff and students is also justified and appropriate. Although  I am unfamiliar with the costing procedures of NSF grants, the amount requested  seems justified given my experience of UK NERC grant applications. I fully endorse  therefore this proposal and wish the investigators well in its implementation if  their application is successful.  **Review #6:** Excellent  What is the intellectual merit of the proposed activity?  Addresses one of the most important and topical questions in Tectonics and  Geodynamics, i.e. the strength profile of the lithosphere. As pointed out by the  applicants, a generally accepted model for lithospheric strength profiles has recently  been challenged, leaving us with many questions regarding the dynamic evolution  of the crust and upper mantle. As a result, we need new, quantitative studies of the  relative strengths of the crust and the mantle components of the lithosphere. The  proposed research will focus on the mantle component of the lithosphere, including  field work in two well chosen areas, combined with multidisciplinary lab studies that  include observational and modeling invetigations.  The research team is highly qualified to pursue the proposed research.  The strength of the lithosphere and its dynamic responses to tectonic stresses is  one of the most important questions in Tectonics and Geodynamics. Combining a  strong field component with the lab components of the study is essential, and it  makes the proposal paricularly attractive. The proposers have wisely chosen to  focus their energies on mantle peridotites, which make up the bulk of the  lithosphere. The research should go some way to answering whether the mantle  lithosphere is relatively strong or weak, compared to the crust, and it should also  further our understanding of the dynamics of the mantle lithosphere.  The proposed activities are clearly described, well organized and achievable. They  are also highly focused on a very important question.  All the necessary analytical facilities are available.  What are the broader impacts of the proposed activity?  Results of the proposed work, and hopefully other, complementary studies, may  led to a revised dynamic framework for understanding continental tectonics. The  results will also be important for testing various geodynamic models.  A highly qualified PhD will result from the project.  Based on the proposers publication records, we can be quite sure that the results  will be efficiently disseminated.  The resulting availability of better constraints on lithospheric dynamics will of  course benefit society as the new knowledge will be useful for understanding  natural seismicity and continental margin dynamics.  Summary Statement  Excellent ideas, well presented proposal, good focus, all the tools are in place,  should be considered as a top priority proposal for funding. |