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Strain and stress: Reply

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Strain and stress are fundamental concepts in structural geology, so clarity is needed in their discussion. We reiterate the importance of keeping a clear distinction between strain and stress, and again suggest that strain is more useful for analysis of field data, being a more directly measurable quantity than is stress. We re-emphasise our assertion that strain (kinematic) analyses give a *shallower* understanding of structures than do stress (dynamic) analyses, with stress analyses being of great importance in gaining a full understanding of the development of structures.

This discussion about strain and stress highlights some important differences in methodological approach, especially the relative importance and timing of observations in relation to model development. A wide range of approaches have been successfully used in structural geology, and we reject the idea that only one particular approach leads to complete truth, even if that approach uses the laws of mechanics as the starting point. The approach that is now common, and which is expounded by Pollard (also see Fletcher and Pollard, 1999), emphasises initial model development, with the model being supported by use of selected observations. A more old-fashioned approach, which we consider to also be valid and useful, emphasises the importance of initial observations in identifying a problem, and of fieldwork in testing the consequent hypotheses posed to solve the problem. Furthermore, we suggest that it is crucial to be sceptical of models, even when those models are based upon the laws of physics. It is our belief that the departure of data from models is of more interest than a close fit between model and data, because it suggests how models may be improved. First, we will discuss the differences in methodological approach, and then we will deal with the specific criticisms raised by Pollard.

1. Different scientific approaches to structural geology

The discussion by Pollard advocates a valid and important methodology for structural geology research (see also Fletcher and Pollard, 1999), and we restate our belief that dynamics/mechanics provides a deeper understanding of structures than does kinematic analysis. Our original paper (Marrett and Peacock, 1999) did not advocate any specific methodology. We certainly did not claim that purely geometric study is the proper way to conduct all investigation of structures. Our separation of structural analysis into geometric, kinematic and dynamic analyses is a classification rather than a methodology (we did not use the word "phases"). We view structural geology as a broad science encompassing numerous valuable approaches to research. The different approaches have contrasting strengths and weaknesses, so the approach best suited to a specific problem depends on the goals of research. Many questions, particularly in the field of tectonics, are satisfactorily answered by simple geometric or kinematic techniques.

This discussion about the use of stress and strain, and various papers in the "Questions in Structural Geology" issue of Journal of Structural Geology, raise basic questions about approaches to structural geology. The approach to scientific research we personally attempt to follow is based on that described by Chamberlain (1890), Gilbert (1896), Johnson (1933), Anderson (1963) and by Mackin (1963). For example, Johnson (1933) showed that the first stage in an investigation is usually observation, followed by classification, generalisation, invention (hypothesis development), verification and elimination, confirmation and revision, and finally interpretation. Although Johnson states that this scientific method is

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not infallible, he concludes that it does reduce the chances of error. We believe that this approach is well suited to the type of field-based research that we undertake. In the light of Pollard's criticisms of our paper, and our reading of some current trends in structural geology that emphasise the model as the starting point, we have questioned our own approach to science. We have asked ourselves several questions about what we do and how we do it.

1.1. Is the starting point observation or a model?

Pollard twice states that the conservation laws of physics are the starting points of structural geology, while "geometric observations are put in their proper perspective as data, some of which may be useful in testing refutable hypotheses". Much recent science assumes a starting model, with data selected to support the model. This may be intellectually elegant, but is it really possible to start with a model? It is our belief that even the discovery of the laws of physics depends on initial observations. The famous (maybe apocryphal) stories about Archimedes taking an over-flowing bath and Newton having an apple fall on his head illustrate how even the greatest scientists may rely on an initial observation as a starting point for discovering important laws of physics. Some of the more theoretical sciences, such as astrophysics and pure mathematics, do have little direct basis in observations of nature, being based largely on intellectual imagination. But this is rarely true in structural geology.

Geology has traditionally been an observation-based science. There are some cases of a model genuinely preceding observations. For example, Hutton (1795) is reputed to have developed his theory of uniformity before actually seeing an unconformity (see Hallam, 1989, p. 28). It is our belief, however, that almost all structural geology is motivated by some experience of observing structures. Most research in structural geology is carried out by experienced, professional scientists, who have an extensive background in observing the structures they study. For example, Pollard quotes Gilbert (1877) as showing how models can lead field analysis, and states that Gilbert "formulated the conceptual model for laccoliths in the first few days of field work". The model therefore followed closely from the initial observations. Indeed, Gilbert (1896) stated his methodology as "When the investigator, having under consideration a fact or group of facts whose origin or cause is unknown, seeks to discover their origin, his first step is to make a guess", i.e. develop a hypothesis based on observations.

What is to be gained from isolating ourselves from our experience? Is there still any truth in the old adage that the best geologist is the one who has seen the most rocks? We believe that observations are most usually the starting point to structural geology, and that we should not deny the important contribution of field experience. Furthermore, in our experience, fieldwork commonly leads to the identification of the new problems that need to be solved.

1.2. Are models better than observations?

If, as Pollard states, selected field data are only useful in supporting a mechanical model, then the model must be more important than field observations. Fletcher and Pollard (1999) stated that meaningful fieldwork can only be carried out with a pre-conceived mechanical model, and make a surprising comment about their perception of the failure of fieldwork that is not based on their idea of a "complete mechanics". They stated that important data could easily be ignored when a geometric or kinematic approach is taken, and gave the example of previous workers having missed important information in the linkage of fault segments. We accept that Pollard's approach has led to very important contributions to the understanding of fault linkage (e.g. Segall and Pollard, 1980, 1983). Based on personal experience, however, a thorough geometric approach can give important information about the development of strike-slip (Peacock, 1991) and normal fault zones (Peacock and Sanderson, 1991). It seems to us odd that the results of detailed fieldwork that is not based on a pre-conceived mechanical model should be scorned.

Models are more intellectually pleasing than observations. It is our belief, however, that models are only of use if they explain observations. We certainly do not believe that modelling is wrong and that the laws of physics should be dismissed, but we do believe that some degree of scepticism in the assumptions used and the resultant models is healthy. We accept Pollard's comment (also expressed by Mackin, 1963) that models can and should guide the course of fieldwork; indeed, it is difficult *not* to generate and test working hypotheses during fieldwork. Because observation is so important, however, it would be a shame if it was downplayed or completely ignored in the development of an elegant model. Furthermore, the breakdown of a model in the face of observation commonly is more interesting than a reasonable fit to nature, because it signals a potential direction for improving, or a possible reason for abandoning, the model and it may point to previously undiscovered processes. Let us take the case of the width profiles of dike segments.

Delaney and Pollard (1981) presented a detailed map and mechanical analysis of en échelon dike segments at Ship Rock, New Mexico. Pollard and Segall (1987) used this excellent piece of fieldwork to model the width profiles of dike segments at Ship Rock. Their model is simple, elegant and has a sound theor-

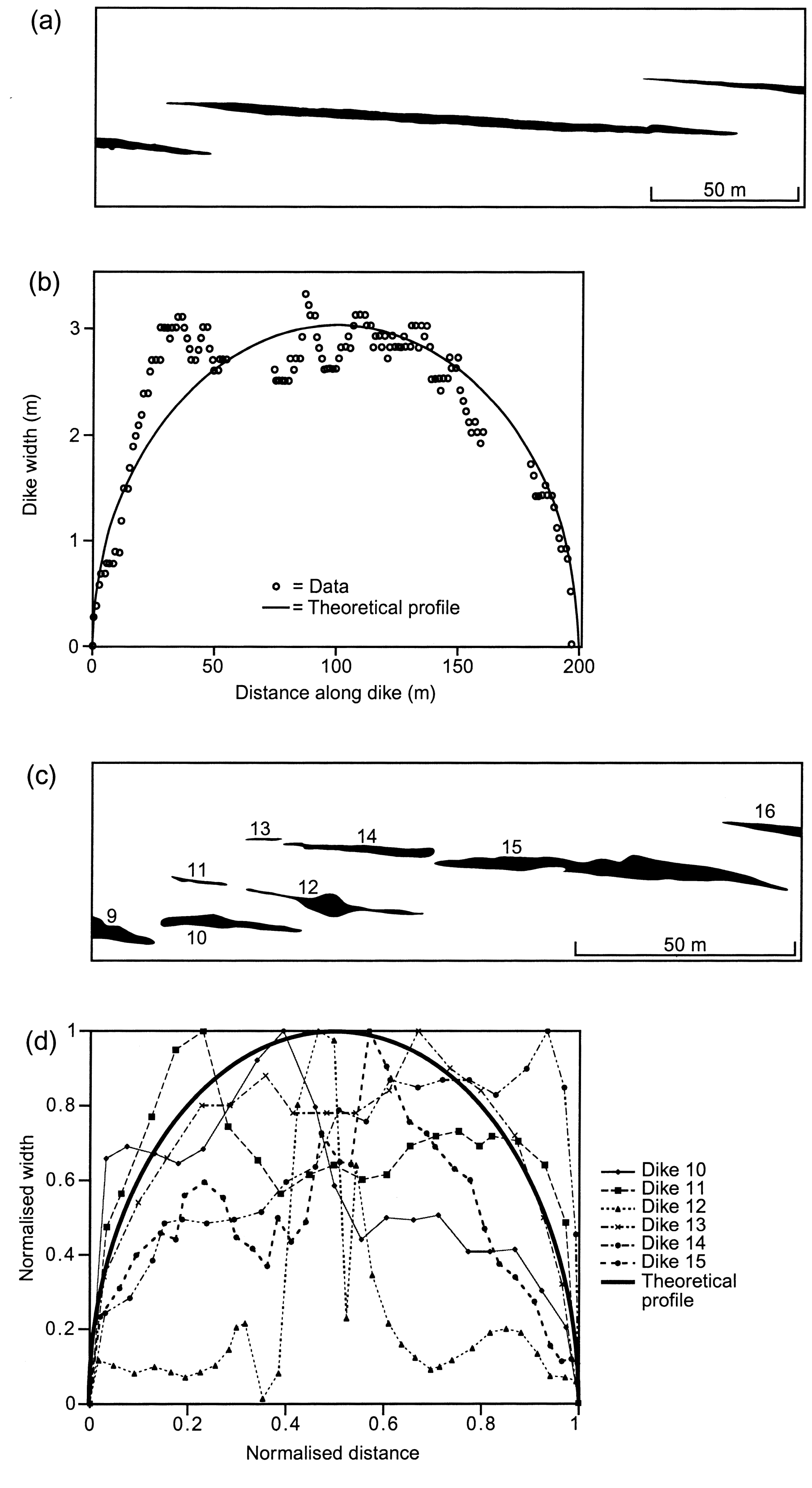

Fig. 1. (a) Example of a dike segment at Ship Rock (Pollard and Segall, 1987, 8.20). (b) Thickness against distance plot for the dike segments shown in (a), along with the theoretical profile of Pollard and Segall (1987, fig. 8.21b). (c) Other dike segments at Ship Rock, that do not correspond to the theoretical profile. (d) Normalised thickness against distance plots for the dike segments shown in (c), along with Pollard and Segall's theoretical profile. There is a considerable scatter of data-points.

etical basis. But is it supported by the field data and is it correct? Pollard and Segall (1987) used one dike segment from Ship Rock (Fig. 1a, b) that has a similarity to their elliptical displacement model, but they did not show that many of the other dike segments have markedly different shapes (Fig. 1c, d). The data suggest that the Pollard and Segall model is, at best, only partially correct, and does not account for one or more other important processes. Delaney and Pollard (1981) suggest that non-elliptical shapes of the Ship Rock dike segments are caused by mechanical interaction, inelastic processes at dike tips, thermal fracture of the wall-rocks, and by erosion of the wall-rocks by the flowing magma. The research should now be directed towards determining how such factors cause the data depart from the model, thereby improving the model. In that context, the model is of use. This breakdown in the model is important because it indicates that the dike segments at Ship Rock did not form as simple mode I cracks in an ideal elastic material. This example also illustrates that there must be a balance between models and field data. This is especially true in structural geology, where structures and mechanics are commonly more complex than is the case in mechanical engineering of human-built structures.

We agree with Pollard that structural geology cannot be restricted to purely geometric studies. We did not advocate purely geometric studies, and recognise that dynamic analysis and model-development are crucial to gain full understanding in structural geology. We do suggest, however, that geometric and kinematic analyses provide a more appropriate framework for analysis of *field measurements* from specific natural structures. We offer the opposite opinion that if structural geology is just concerned with numerical or mechanical models without proper use of field data, it will become completely worthless. Kinematic and geometric analysis of field data and mechanical analysis of underlying principles are both necessary for deep understanding of structural geology. A proper balance is needed.

Another important characteristic of observation is that the resulting data should be true forever. If well-collected data are flawed, it is by incompleteness rather than by incorrectness. In contrast, history suggests that our current models are likely to be supplanted in the future by better understanding. As naïve and quaint as some historic concepts now appear to us, the models of our generation may one day be viewed as equally dated and even misguided.

1.3. Is it better to have faith or scepticism in the laws of mechanics?

Pollard twice states that the laws of mechanics are the starting point of structural geology (also see Fletcher and Pollard, 1999). How safe is it to have a rigid faith in these laws? A famous example of geological observations being at odds with the established laws of physics is the argument during the Nineteenth Century about the age of the Earth. Thorough reviews of this debate were given by Geike (1899), Geike (1905), Hubbert (1967) and by Hallam (1989, chapter 5). Hutton (1785) suggested the Earth formed over "an indefinite space of time". Lyell (1830 to 1833) used geological evidence to suggest that the age of the Earth is almost unlimited. Darwin (1859) attempted to measure the minimum age of the Earth by using erosion rates to give a rough estimate of the age of the Weald, SE England, at about 300 million years. Lord Kelvin, one of the most respected physicists of the day and still very highly regarded, ridiculed such estimates for the great age of the Earth. Kelvin used sound physics, including estimates of the cooling rate of an initially molten Earth, and stated that the Earth must be no more than about 24 million years old (Kelvin, 1899). It is perhaps unfair to criticise the science of Kelvin, who could not have known about the importance of radioactive decay. This example does illustrate, however, the dangers of starting with the laws of physics and of ignoring geological evidence in preference for a model.

Geike (1899) made it clear that models are important to explain field observations, but made a scathing attack on the approach by Lord Kelvin and his followers. Writing in support of geologists' view of the great age of the Earth, Geike stated:

> "So cogent do these geological and palaeontological arguments appear, to those at least who have taken the trouble to master them, that they are worthy of being employed, not in defence merely, but in attack. It seems to me that they may be used with effect in assailing the stronghold of speculation and assumption in which our physical friends have ensconced themselves and from which, with their feet, as they believe, planted well within the interior of the globe and their heads in the heart of the sun, they view with complete unconcern the efforts made by those who endeavour to gather the truth from the surface and crust of the earth.... We know infinitely more of the history of this earth than we do of the history of the sun. Are we then to be told that this knowledge, so patiently accumulated from innumerable observations and so laboriously co-ordinated and classified, is to be held of none account in comparison with the conclusions of physical science in regard to the history of the central luminary of our system? These conclusions are

founded on assumptions which may or may not correspond with the truth."

Take another example, this time in structural geology. An important issue around the middle part of the Twentieth Century was the "thrust paradox", the problems in explaining the movement of very large thrust sheets (e.g. Davis and Reynolds, 1996, p. 336). The forces needed to move a large thrust sheet were modelled to be much greater than the strength of the strongest rock. Observations were apparently at odds with the laws of mechanics. A satisfactory mechanical understanding was not developed until Hubbert and Rubey (1959) showed the importance of pressurised fluids along basal detachments. In this case, the field observations highlighted a problem in the existing knowledge of rock mechanics. Fortunately, these field observations were not dismissed because they did not fit earlier understanding of rock mechanics.

Even the laws of Newton (1687) have been shown to break down (Einstein, 1953). That is not to say that we are presumptuous enough to ignore or dismiss the laws of physics, only that it can be dangerous to take them as the only valid starting point for research. As stated so eloquently by Gilbert (1896), "However grand, however widely accepted, however useful its conclusion, none is so sure that it can not be called in question by a newly discovered fact. In the domain of the world's knowledge there is no infallibility".

1.4. Are numbers necessary for good science?

Quantification, the attempt to explain mathematically a set of observations or a particular phenomenon, is the aim of much modern science. Such quantification appears to be implicit in the arguments of Pollard (Fletcher and Pollard, 1999). But is this approach becoming dogma? Is Lord Kelvin's statement true that what cannot be stated in numbers is not science (Mackin, 1963)? Is quantification in structural geology at least in part a response to an inferiority complex with regard to apparently more rigorous sciences, such as mathematics and physics?

Let us consider the two theories that perhaps have been most important in geology over the last 150 years: Darwin's theory of evolution and plate tectonics. Both are based on painstaking observations of nature, and both still defy a complete numerical explanation. Taking this argument further, the causal processes of both theories are still not known with certainty. Evolution of species may be driven by such processes as competition and genetic mutation, while plate tectonics is probably driven by mantle convection, ridge push and slab pull (e.g. Moores and Twiss, 1995), but the exact roles of each potential cause is still debated. Does this mean they are not valid, important theories? Indeed, these two theories could be used to argue that a direct mechanical cause is not necessary for a working understanding of a process.

We believe, therefore, that quantification is a very important aspect of science, but that there are other valid approaches. Mackin (1963) emphasised the importance of developing a qualitative understanding of the complex systems that occur in geology before developing a quantitative understanding. Although Mackin made it clear that he was not opposed to mechanical methods, he stated that "When mechanical processes *replace* reasoning processes, and when number *replaces* understanding as the objective, danger enters".

2. Specific criticisms by Pollard

2.1. The history of structural analysis

Pollard's history of the development of structural analysis during the Twentieth Century is very interesting, and we find no fault with his account. We would, however, like to make several points about our paper (Marrett and Peacock, 1999). First, our paper was not intended as a discussion of the history of research on strain and stress; indeed, only four or five sentences in our paper are about the history of research. We believed this to be in line with the editorial policy that "We hope that Questions in Structural Geology gives a true flavour of where structural geology is, today..." (Evans and Treagus, 1999). We certainly did not intend to cause offence by failing to mention many individuals who have made monumental contributions to structural analysis.

Second, we did not wish to downplay the importance of continuum mechanics. Indeed, the first sentence of our introduction clearly states its importance.

Third, we did not wish to overplay the importance of Sander (1970) and his followers. Our only motivation for discussing the work of Sander was his explicit discussion of strain and stress as descriptive and genetic concepts. In our literature review, we could find no other precedent for this conceptual framework. We wrote "the modern *conceptualization* of structural analysis was initiated early in the Twentieth Century by Bruno Sander (1970)..." and did not mean to imply that Sander developed the modern *theoretical* or *analytical* framework of structural analysis. Furthermore, a full sentence partly quoted by Pollard is: "*While we do not advocate a return to the limited techniques of that era*, the conceptual underpinnings of modern structural analysis can be traced to that time." This means that we are not in agreement with the theoretical or analytical approach to structural analysis of Sander. Pollard's lengthy criticism of Sander and the implicit criticism of our paper therefore surprise us. Pollard's argument against symmetry is particularly misleading, as we neither advocated nor even mentioned symmetry. We certainly did not provide "invocations that symmetry is the fundamental property of naturally deformed rock....". Pollard (end of section 1.2) apparently directly quotes us as stating that Sander provided a "most original and significant contribution to structural geology"; this quotation actually comes from Turner and Weiss (1963).

In balance to Pollard's lengthy criticism of Sander, it should be pointed out that Jiang and Williams (1999) stated that "The two most important concepts in Sander's theory and methodology that remain relevant today are the concepts of the movement picture and the symmetry principle".

2.2. Is there a simple cause and effect relationship between stress and strain?

Pollard emphasises his belief that stress is always the cause and strain is always the effect during deformation (also see Fletcher and Pollard, 1999). Stress, being the cause, is therefore of more importance and greater interest than the effect, strain. Although we agree that stress can cause strain, we would complement such a statement with another that strain can cause stress. Our argument was based not on mathematical manipulations, but rather on conceptual reasoning. We maintain that the two concepts represent inseparable phenomena within real bodies in nature, and that the distinction is a mental construct.

Interestingly, four papers in the "Questions in Structural Geology" issue of Journal of Structural Geology argued for the value of strain analysis or even consider stress as an effect rather than a cause of deformation (Marrett and Peacock, 1999; Nieto-Samaniego, 1999; Tikoff and Wojtal, 1999; Watterson, 1999). One common theme among these papers is that they focussed on the analysis of specific natural structures. Tikoff and Wojtal (1999) gave a very clear and well-argued alternative to the assumption that applied stresses are the controlling parameters. They suggested that material velocity, or incremental or total displacement, is imposed on the system, with stresses being a response to the imposed boundary conditions. They stated that strain is a useful parameter to study because it can be measured in ancient rocks, and current strains can be measured using geodetic data, while stress cannot be directly measured. Stress measurement requires measurement of strain; even stress "measurements" from borehole breakouts in fact are calculated from measurements of the change of borehole shape and size. Tikoff and Wojtal (1999) questioned the meaning of regional stress measurements because stresses vary in both time and space.

Nieto-Samaniego (1999) argued that strain analysis is more appropriate than stress analysis for regions containing multiple simultaneously active fault sets. Watterson (1999) suggested that use of strain to study shear structures should be re-evaluated, largely because "strains in rocks can be observed but ancient stresses can only be inferred". Watterson pointed out that engineers commonly express problems in terms of stress because small strains are usually involved before failure and because stresses can be calculated or measured. He states "engineering practice provides no basis for geologists either to view stress as a 'cause' of deformation (Edelman, 1989) or for a conjectural stress configuration to be the structural geologist's apotheosis". Although Jiang and Williams (1999) showed that there are limitations with kinematic analyses, they concluded that such analyses do have useful applications.

We are not so presumptuous as to claim that Newton's Laws are incorrect. What we question is whether there is only one correct way to think about them. Let us take one simple example. If a strike-slip fault has along-strike irregularities, there will be restraining and releasing bends and oversteps (Fig. 2). It would traditionally be argued that stress builds up around the fault until slip occurs, i.e. stress causes strain. But what happens at the bends and oversteps? The displacement and strain associated with the fault will have a significant modifying affect on the stresses. This indicates that there is *not* a simple cause and effect relationship between stress and strain. Tikoff and Wojtal (1999) went further and argued that the movement of tectonic plates controls deformation, with stresses acting to accommodate the displacements.

2.3. Geometric, kinematic and dynamic analyses

We point out that our paper (Marrett and Peacock, 1999) addressed "the analysis of specific natural structures for which only the final state is known, as opposed to general mathematical models or laboratory experiments that focus on processes". We therefore stand by our statement that geometric observations are the foundation of analysis rather than the laws of mechanics. For example, mechanical analyses of the faults portrayed in figure 5 of Maerten et al. (1999) cannot have been done without any observations of those faults. The laws of mechanics are of little use for modelling specific natural structures if we have nothing to model. In contrast, admittedly shallow geometric and kinematic analyses of the faults can be made in the absence of the laws of mechanics. For acquiring observations, we advocated neither a purely geometric nor a hypothesis-free approach to fieldwork. We agree that impartial collection of data is not undercut by having hypotheses, and we are at a loss to explain why

Pollard accuses us of using "genetic" as a code word for "prejudicial."

Our statements comparing and contrasting kinematic and dynamic analyses were focused on problems posed by specific natural structures. Considering again the faults in figure 5 of Maerten et al. (1999), techniques for calculating the average longitudinal strain in a certain direction due to slip on such an array of faults are straightforward. Given the same set of observations, different analysts will determine the same unique estimate because it is a forward problem. Using data to determine the stresses that were active during slip of the same specific faults is an inverse problem (note that this was not the research goal of Maerten et al., 1999). If a state of stress is assumed then calculation of the resulting fault displacements is, however, a forward problem, but such calculations must be done iteratively (one form of inversion) in order to determine an acceptable fit to the data. Moreover, there probably are numerous assumed states of stress that would result in comparably satisfactory fits. This is the limitation of uniqueness to which we alluded, not to a problem of uniqueness in the equations of linear elasticity. The kinematic analysis is based on assumptions, some of which might be poor, but they do not include constitutive behavior. Of course this results in ignoring deformation mechanisms other than fault slip, such as elastic compressibility,

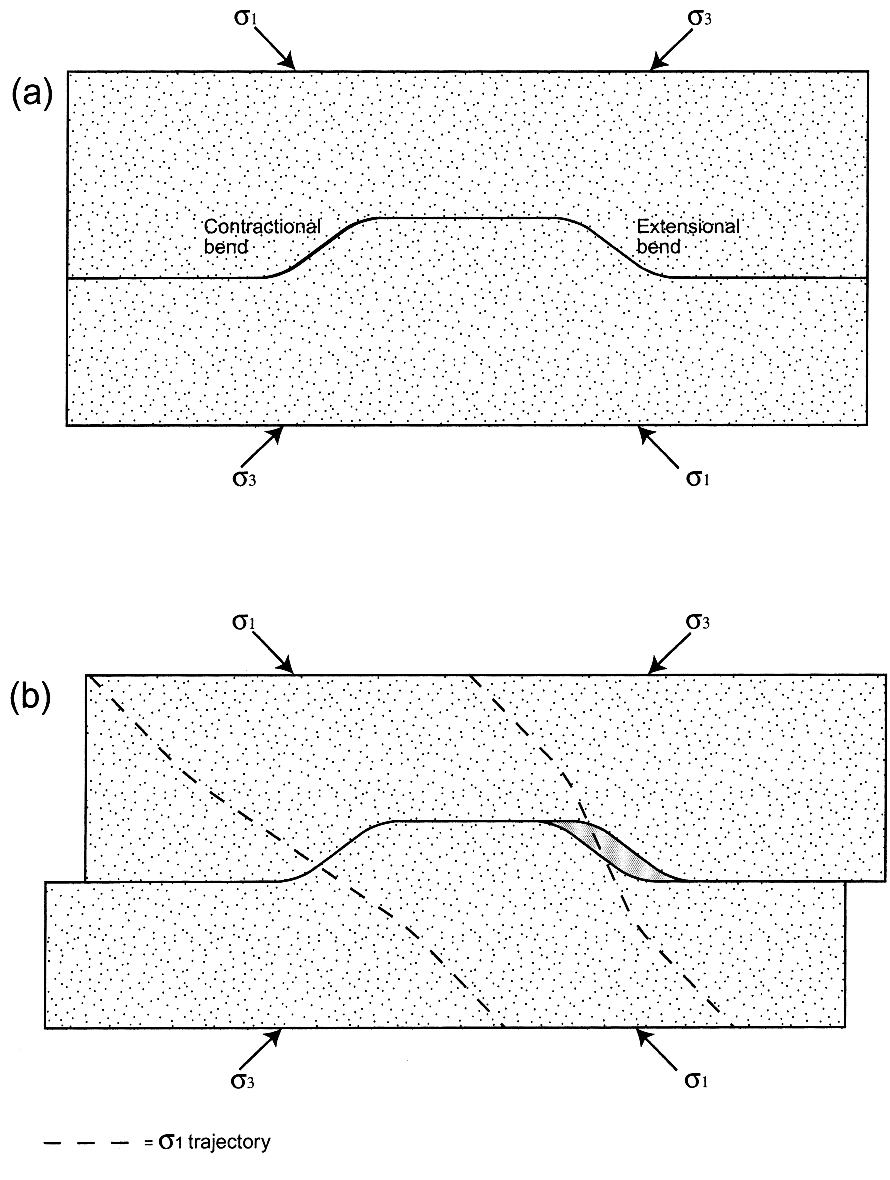

Fig. 2. (a) A fault trace with an extensional bend and a contractional bend. Stresses build up around the fault in response to the movement of the plates. (b) The fault moves, with the displacement causing variations in the stress field, especially at the bends. There is, therefore, not a simple cause and effect relationship between stress and strain. See Tikoff and Wojtal (1999) for a fuller discussion of the relationship between stress and strain.

but this does not affect the estimate of strain for the fault-slip mechanism. In contrast, a dynamic analysis of data from the faults cannot be done without considering the constitutive behavior of rocks at the time of deformation (which may have been millions of years ago, when the rheology of the rocks in question was significantly different than at present). The choice of rheology will affect the results calculated.

The description by Ramsay (1980) of shear zone geometries is an example of successful and important research that is based on geometries and strains. Ramsay gave little discussion of the mechanics or stresses involved in shear zone development. This illustrates that there is an important role for geometric and kinematic analyses in structural geology.

2.4. The use of strain and geometric descriptions in field analysis and field classification

Pollard (end of section 2.1) implies that we oppose dynamic and mechanical interpretations. This is not the case. For example, we clearly stated that kinematic analysis provides a *shallower* understanding than does dynamic analysis. We do suggest, however, that strain is more directly inferred from field observations of specific natural structures and that a clear distinction is required between stress and strain, with this distinction being reflected in the terminology. Such odd mixtures of terminology as "compressional and extensional tectonics", that commonly appear in the literature, illustrate problems in thinking (Marrett and Peacock, 1999).

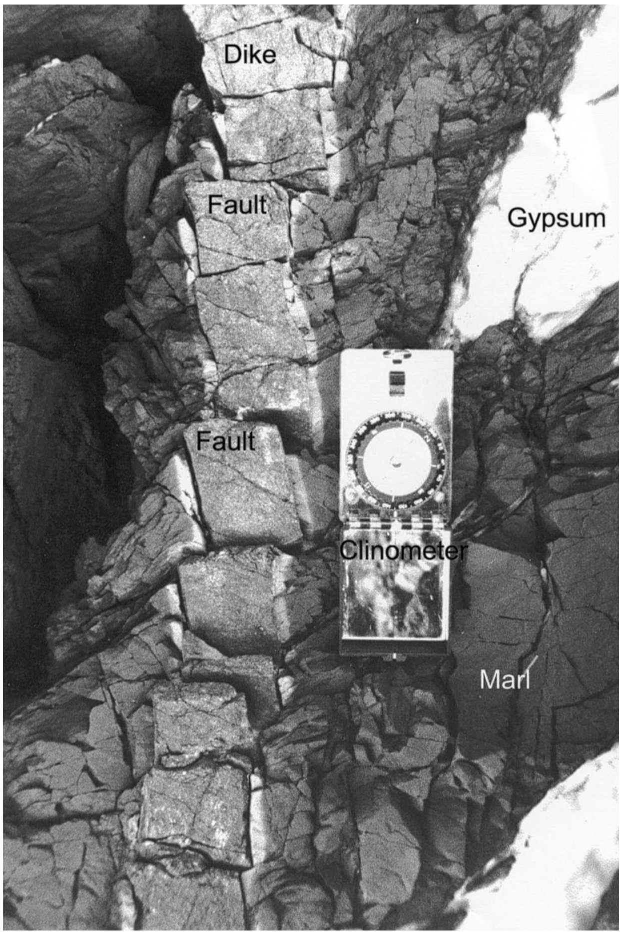

Fig. 3. Photograph of a sub-vertical sandstone dike in Triassic marls, Watchet, Somerset, England. The dike has been cut by a set of faults. Is stress or strain the most appropriate for descriptive and classification purposes? Geometric features would usually be measured at such an exposure, including fault displacement, fault orientations, fault spacing, and extension of layering.

Stress cannot be estimated for specific natural structures in ancient rocks without its representation by strain (e.g. Tikoff and Wojtal, 1999; Watterson, 1999). It seems logical that field descriptions and classifications should be based on observed, measurable quantities, which in structural geology is usually strain and allied concepts (Fig. 3). It also seems sensible to us to keep a division between what is measured (usually strain) and the interpretation (commonly stress). We believe that our use of strain for field classification of structures is compatible with, for example, Sibson (1977) classification of fault rocks, which is useful for field analysis because it is based on texture and is without genetic connotations.

We accept Pollard's comments that there are problems in kinematic analyses (also see Jiang and Williams, 1999). One argument used by Pollard is that many kinematic analyses do not take full account of heterogeneous deformation, but the same may also be said of many dynamic analyses (see Tikoff and Wojtal, 1999). Another argument used by Pollard (section 2.1) against the use of kinematic analyses is that it assumes rocks are incompressible. Faulting typically produces strain of the wall-rocks (e.g. Barnett et al., 1987), and this strain is commonly ignored in, for example, many balanced cross-section techniques (e.g. Hossack, 1979). Furthermore, the power-law distribution of fault displacements indicates that significant strain can be missed by a survey at any particular scale (e.g. Marrett and Allmendinger, 1991). We accept that these are potential sources of error, but believe that detailed field measurements of strains can overcome these problems. It is disingenuous of Pollard to imply that as many assumptions are needed for kinematic analyses as for dynamic analyses. Tikoff and Wojtal (1999) discussed the assumptions about forces and rheology, and suggest that these are generally poorly understood for large and complex rock masses, which are necessary for dynamic analyses. Indeed, Fletcher and Pollard (1999) stated that their mechanical analyses require an explicit choice of constitutive relations, boundary conditions and initial conditions. Ramsay (1967) (introduction to chapter 3) clearly stated the importance of strain analysis and the problems in attempting to determine stresses from strain in naturally deformed rocks.

Pollard (section 2.1) criticises much of the work carried out on joints before Pollard and Aydin (1988), who improved understanding in part by making measurements of a series of *geometric* features. These geometric measurements were therefore crucial in advancing understanding of joints, allowing inferences to be made about mechanics. We are puzzled, therefore, that Pollard seems to argue against the validity or importance of geometric measurement.

2.5. The role of fluid pressure and the use of fracture propagation modes

We accept Pollard's criticism of our use of the term "effective stress", although the common current usage has changed from the definition given by Terzaghi (1943). For example, the definition of effective stress given by Cosgrove (1997) is the same as given by Marrett and Peacock (1999). We still argue, however, that most dikes and veins propagate and increase in width because of high magma or fluid pressures within the crack, not because the rock is in true tension, which is thought to be uncommon in the crust (e.g. Gross and Engelder, 1995; Cosgrove, 1997).

We also accept Pollard's criticism of our reasons for recommending against the use of fracture propagation modes. We would still, however, recommend caution in the use of these terms for the field classification of structures. For example, faults appear to be mode II or mode III cracks, but there is good evidence that faults commonly propagate by the linkage of mode I cracks (e.g. Martel et al., 1988, Petit, 1988). Also, usage of fracture propagation mode terminology must serve a purpose; in our opinion, the terms "vein", "dike" and "joint" should not be lost in preference to "mode I crack".

3. Conclusions

In spite of Pollard's criticisms, we still believe that strain and stress are fundamentally different quantities, which do not necessarily share a simple cause-and-effect relationship. As in our earlier paper (Marrett and Peacock, 1999), we advocate the use of strain (kinematic) terms and analyses for field and descriptive purposes, and reiterate that strain analysis would tend to give a shallower understanding than would stress (dynamic) analyses for genetic purposes. Indeed, we do not completely dismiss stress (dynamic) analyses, but suggest they are most appropriate when inferences are being made about the genesis of natural structures, when mathematical or mechanical models are considered, or when forces are directly measurable, as in laboratory experiments.

We emphasise that we have great respect for the work of Professor Pollard, and admire much of his vision for the future of structural geology, as expressed by Fletcher and Pollard (1999). There is certainly a need for more rigorous application of the laws of mechanics to structural geology. It is our belief, however, that there are other valid approaches to problems in structural geology. Geometric, kinematic and dynamic analyses are all necessary for the understanding of processes in structural geology. In particular, we feel that the importance of proper field analysis, both in identifying problems for research and in testing models, should not be undermined, and observations that are incompatible with a model should not be simply discarded. The final sentence of Tikoff and Wojtal (1999) is particularly apt: "We need, as a community, to document how rocks actually deform, rather than analyzing how we think rocks might deform".

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