Lab 7: Fold and thrust belts
Solutions

Fall 2005

1 Definitions, et. al.

See K. R. McClay, *Glossary of thrust tectonics terms*, scanned and posted on the website.

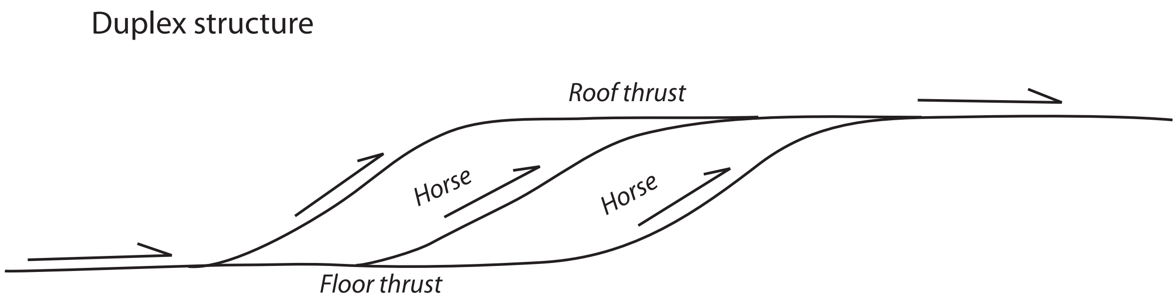
Backthrust . In many thin-skinned fold and thrust belts, most of the fold and thrust structures have a definite, consistent *vergence* to them. That is, the sense of overturn on the folds and the dip and transport direction on the faults suggest consistent transport of material towards the foreland. A *backthrust* is a thrust fault that dips in a direction opposite to that of most of the structures in the belts.

Foreland . Thin skinned fold and thrust belts are often found on the flanks of mountain belts. The area outboard of the mountain belt consisting of undeformed sediments is known as the *foreland*. Since thin-skinned fold and thrust belts typically detach on previously flat lying sediments and propagate deformation towards the foreland, they are often referred to as *foreland fold and thrust belts*.

Hinterland . The core of a mountain belt, often characterized by rocks of high metamorphic grade and ductile deformation histories is the *hinterland* of the range. Thin-skinned fold and thrust belts are generally found between the undeformed foreland and the strongly deformed core, or hinterland of the range. Tectonic transport in foreland thrust belts is generally directed from the hinterland to the foreland.

Thrust nappe . A nappe is a recumbent, often isoclinal fold with definite asymmetry (vergence). Nappes are commonly observed with sheared out lower limbs, or thrust faults. Both the direction of shear or thrust faulting on the lower limb of the fold and the asymmetry of the fold have consistent vergence or direction of tectonic transport. Such a structure is a *thrust nappe*.

Duplex Low angle faults are often characterized by alternating ramps and flats. Ramps are often associated with a series of imbricate (parallel, or "shingled") faults joined by faults above and below them. In a thrust environment, the structural association is: two flat segments (called the *floor and roof thrusts*), connected by several parallel ramp segments. The rock masses bounded by these faults are called *horses*, and the entire structural association is a duplex.



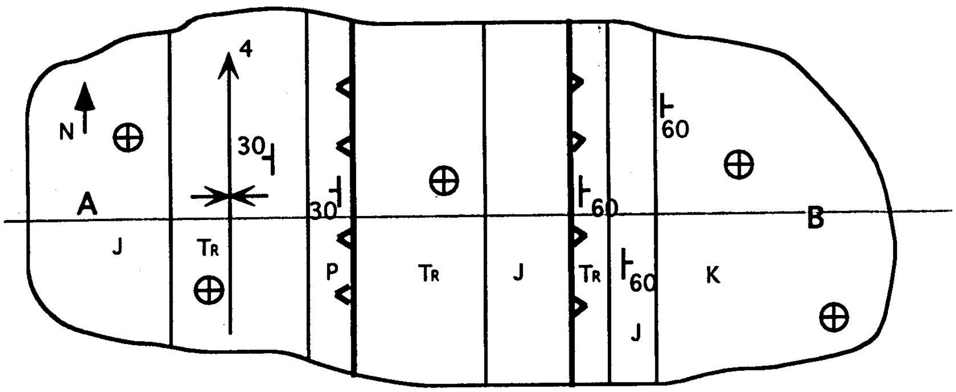
Out-of-sequence thrust . In a fold and thrust belt, deformation commonly propagates towards the foreland. That is, thrusts become progressively younger as you go towards the foreland (in the direction of transport). This is *in sequence* thrusting. A fault that is younger, but is located more hinterland ward than some other fault is, in contrast, *out of sequence*. Note that this has nothing to do with the transport direction or dip of the out of sequence thrust: it need not be a backthrust.

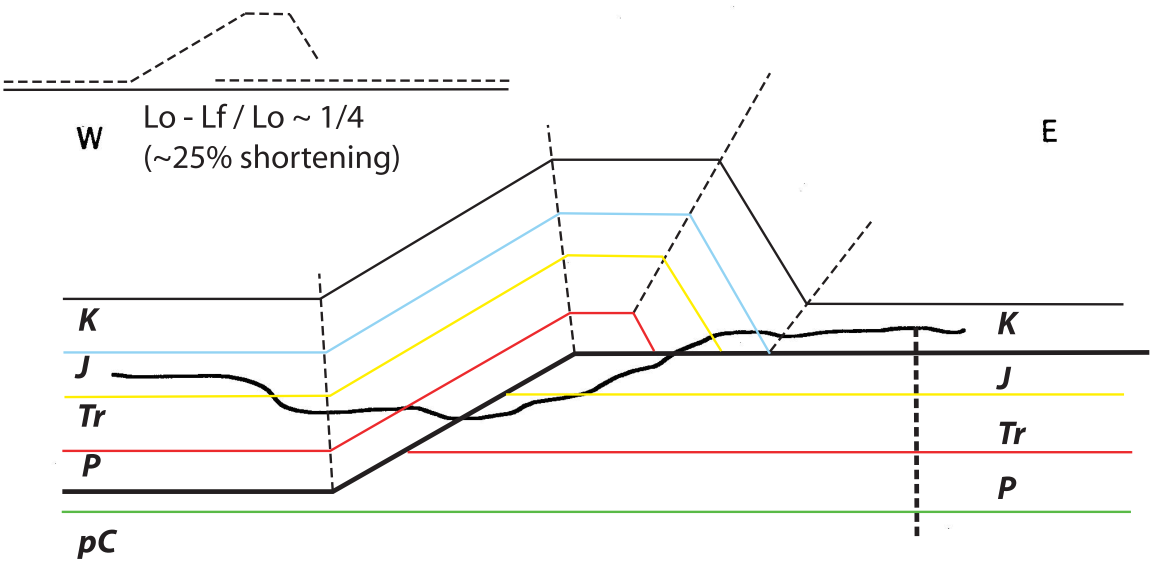
Blind thrust A blind thrust is a thrust fault that does not break the surface. Instead, the tip of the fault is buried in a fold. This fold – a fault-propagation fold – is geometrically required to accommodate slip on a fault past the fault tip.

2 Thrust related folds – cross-sections

2.1

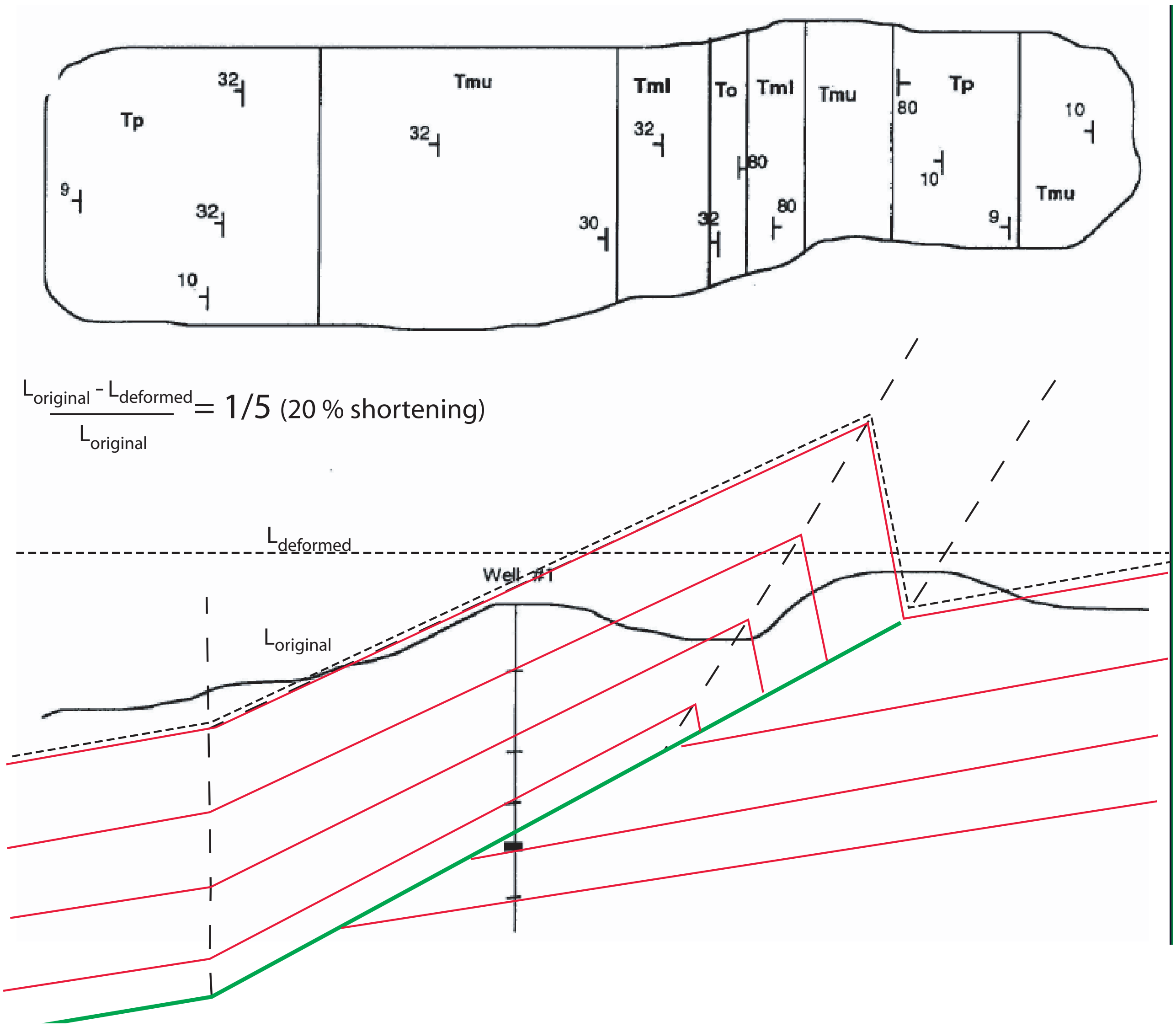
This is a series of fault-bend folds: a syncline above the transition from flat to ramp, and an anticline above the transition from the ramp back to the upper flat. The calculation for shortening is shown schematically as an inset. Note that any measure of shortening will somewhat depend on your choice for the "deformed" line length, since this will scale the results. I made these calculations based on the using the distance from one side of the section to the other as the original length.





2.2

This is a fault propagation fold. To calculate shortening, I measured along the dashed line.



3 Sandboxes and critical taper theory

3.1

The sandbox experiment is an attempt to make a scale analog model of thin-skinned fold and thrust belts. The box was constructed of plexiglass, which is rigid and transparent. In this box, layers of sand and coffee were laid down on a sheet of mylar paper resting on an inclined ramp. The starting thickness of the sand was around 4cm, and the ramp was inclined at 4°. To simulate the transport of material in a thrust belt towards the foreland, the mylar sheet was pulled underneath the sediment, translating the "foreland" towards the back wall ("backstop") of the box. Very quickly, a stable wedge of sand was formed. This wedge was two-sided: towards the foreland, the top of the wedge formed a 6°angle. Towards the backstop, an early formed backthrust and backfold made a steeper (25 °) angle. This geometry was basically stable: even as more material was incorporated into the wedge by continued pulling on the mylar sheet, the wedge grew, but maintained a reasonably constant angle.

Deviations from a perfect wedge resulted from the top surface being deformed about folds verging towards the foreland. Viewed from the top, at the end of deformation, five or six major structures dominated the top surface. Most of the shortening structures were folds, although these were presumably cored by faults. In one instance, material from the middle layer broke the surface.

The presence of a plexiglass sidewall created some edge effects, in that frictional drag along the wall resulted in less shortening. Another edge effect was the abrupt thinning of the original package of sediment. Numerous tear faults formed were the sediment package thinned laterally.

Most of the deformation was localized in the toe of the wedge: once folds and faults had formed in the back of the wedge, there was little or no continued deformation. Thus, most of the structures were developed "in sequence", with the youngest structures closest to the foreland and vice versa.

We tested the idea that significant erosion can affect the wedge deformation dynamics by removing a large portion of the wedge top material. Upon continued shortening, the original back thrust and back fold was reactivated, presumably in an attempt to restore the original stable wedge geometry.

3.2

The geometry of a wedge is set by the strength of the material deforming within it, and the frictional resistance of the decollement upon which the wedge forms. In particular, the weaker the decollement, the lower the wedge angle; strong wedge material has the same effect. In a material like sand, these parameters can be captured by the internal friction angles of loose sand and sand on mylar. In thin-skinned fold and thrust belts, rocks presumably deform according to the Mohr-Coulomb criterion so sand is not a horrible choice as an analog material.

3.3

This section asks you to take the concept of self-similar wedge growth to an absurd level. If the wedge angle remains at 5°, and the wedge tip remains at sea level, self-similar growth to a 180km long wedge suggests that the top of the wedge be at elevations in excess of 15.5 kilometers. This is three times higher than the highest regions of the Earth today (individual peaks in the Himalaya reach 8km, but the average elevation at the crest of the range is a bit over 5km).

This analysis neglects several important parameters. First, isostatic compensation is neglected. We know that for every 1km of topography, there is a corresponding 6 or 7 kilometers of crust present as a "root", much like most of the volume of an iceberg is below the ocean. So we might expect that isostatic subsidence would take care of most of our 15km high wedge. Second, critical wedge theory assumes constant strength, but we know that the strength of rocks varies considerably with depth. While the increase in strength with depth due to increasing pressure is accounted for by appealing to Mohr-Coulomb rheology, above certain temperatures, rocks deform ductilely and according to viscous or viscous-plastic flow laws. Finally, since we expect that erosion to scale – at least to a first order – with average slope and therefore elevation, the higher we make mountains, we expect erosion rates to increase as well. It could be that geomorphology, and not crustal strength is the real limit for the height of mountains on Earth.

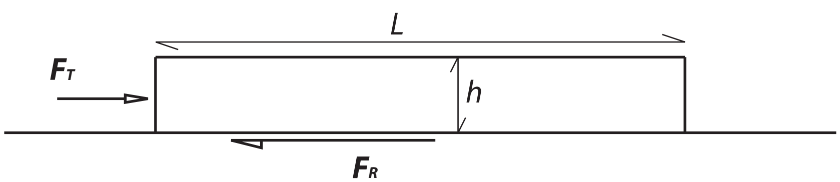
3.4

The backstop in the sandbox experiment is probably the most unsatisfying part of the whole set-up. What, in nature, corresponds to a vertical, unyielding wall? Early papers on critically tapered wedges had cartoons showing bulldozers pushing wedges in front of them, but this is surely just trading one suspect metaphor for another.

One thing to realize is that the critical taper models and sandbox experiments are meant to simulate or describe fold and thrust belts or accretionary prisms. That is, they are models of a small part of the anatomy of an entire mountain range, in particular, the exterior parts. The backstop then, is just the interior (hinterland) of the mountain range, and all the model requires is that this part of the mountain range consists of thicker crust and higher elevations. How that part of the range became thickened and whether sandbox experiments shed any light into this is beside the point.

Alternatively, smaller ranges might be described as two Coulomb wedges back to back. Along these lines, our experiment yielded a clue as to what the backstop was all about. Recall that the crest of the wedge did not occur at the backstop. Instead, one of the earliest structures was a back thrust / back fold. The wedge we created was a two-sided wedge, one with a gentle foreland dipping angle of about 5°, the other with a hinterland / backstop dipping angle of about 20°. In essence, there were two wedges, backing up against one another. Each wedge forms the backstop to the other. In some experiments, researchers have pulled the underlying mylar sheet through a slit in the middle of the original pile of sediment. What happens is very similar to what happened in our experiment: two wedges form, each making the backstop to the other. A often-cited example of a double-sided mountain belt is the island of Taiwan, which has been described as two thin-skinned wedges verging in opposite directions on either flank of the mountain range.

4 The Mechanical Paradox of large overthrusts



$$\sigma_{xx} = \frac{F_T}{h} \quad \Rightarrow \quad F_T = \sigma_{xx} h$$
$$F_R = \sigma_{yx} L$$
$$\sigma_{yx} = \mu \sigma_{yy} = \mu \rho g h \quad (= \tan\phi \rho g h)$$
$$\text{if} \quad F_R = F_L$$
$$\sigma_{xx} = \frac{\sigma_{yx} L}{h} = \mu \rho g h$$

Supposing a horizontal tectonic stress of 100MPa, $\mu = 0.038$. In terms of the angle of internal friction, $\phi \sim 2°$. Price (1988) cites a value for μ of 0.577 and $\phi = 30°$for typical values of rock strength known from rock deformation experiments. Twiss and Moores (page 171, eg.) describe results from the deformation of sandstone samples that yield $\phi = 28.7 \pm 7.4$. In other words, our analysis seems to predict much, much weaker faults than we expect from experimental results.

Supposing we assume a far more reasonable value for $\mu = 0.6$. Then, to initiate sliding along the base of the rigid block, we require $\sigma_{xx} =\sim$ 1.6 GPa. Twiss and Moores (p. 207) cite 250 MPa as being a maximum value of stress based on the stress required to fracture rock. The actual value will depend on the confining pressure (and hence the height of the block), but 250 MPa is a very permissive number. (TM discuss this problem in terms of the maximum length of block that you can push from behind, using 250MPa as a maximum stress. They get 17km.)

Hubbert and Rubey get around the apparent paradox by appealing to a mechanism that will greatly reduce the effective frictional resistance at the base. In particular, the expression for frictional resistance, modified for pore fluid pressure, becomes:

$$\sigma_{yx} = \mu \sigma^*_{yy} = \mu(1-\lambda)\rho g h$$

where λ is the pore fluid factor, the ratio between the pore fluid pressure p and the lithostatic pressure $\rho g h$. Even hydrostatic pore fluid pressure (i.e. $p = \rho_w g h$, where ρ_w is the density of water) greatly reduces the frictional resistance along the base of the fault ($\lambda \sim 0.4$). If pore fluid pressures approach lithostatic pressures, then $\lambda \sim 1$ and the frictional resistance approaches zero.

The question then becomes: do we have evidence of such high pore fluid pressures in nature. Certainly, in some environments, very high pore fluid pressures exist. On the other hand, field observations of many faults suggest that this cannot be a general mechanism. In particular, Clark showed a few slides of the Keystone Thrust in Nevada where field evidence clearly indicated that the thrust sheet was emplaced over a subaerially exposed erosion surface. The Keystone thrust sheet rode over deposits of gravel streams and unconsolidated alluvial deposits, which are not the sorts of rocks that could sustain near-lithostatic fluid pressures.

Price (1988) suggests that the main problem to the so-called "mechanical paradox of large overthrusts" is that the model description is at fault. That is, its only a paradox to the extent that we buy into a specific mechanical description (a model) of how large thrust sheets are emplaced. Price argues that if we go out and look at real thrust faults, both ancient (such as faults in the Canadian Rockis) and active (such as the great Alaska earthquake of 1964), we would realize that this mechanical description was entirely inappropriate. Toss out the model and you also get rid of the paradox. (At some level, the existence of the mechanical paradox of thrust faults should have alerted us to the possibility that the model was deeply flawed).

In particular, the mechanical model assumes that thrust sheets move (1) entirely rigidly; (2) are pushed from behind; (3) slip along the base of the thrust sheet occurs simultaneously over the entire fault surface. Price points out that all three assumptions are ruled out by observations of real faults in nature. Thrust sheets are not rigid: deformation – folding and fracturing – occurs throughout the entire thrust sheet and the amount of slip along the fault is variable both along strike and in the direction of motion. More to the point, slip along thrust faults takes place by the addition of many small slip events that affect only a small amount of the fault at any one time. Even in one slip event, rupture does not take place simultaneously, but instead propagates at rates that scale with shear wave velocity. He quotes Oldow: "thrusts did not move simultaneously over the whole of their extent, but partially, first in one part then in another ... the movement would not be like that of a sledge, pushed bodily forward over the ground, but more akin to the crawl of a caterpillar which advances one part of its body at a time, and all parts in succession".

Washington's reply is actually fairly subtle. He doesn't want to rescue the Hubbert and Rubey model, but doesn't like Price's explanation either. In particular, he dismisses Price's explanation that the fact that fault motion occurs non-simultaneously over the whole surface resolves the paradox. This is a subtle point: he doesn't dispute – for example – the observations that Price summarizes from the 1964 Alaska earthquake. He just argues that the fact that slip occurs non-simultaneously makes no difference to the paradox. His claim is that fault slip and earthquakes are simply the release of elastic strain built up along a fault; that at any given time, the built-up elastic strains are such that the pre-failure shear stresses along an active fault are generally at or near the stresses required for failure. He argues, therefore, that the need to explain how the entire fault surface comes to this point of critical balance is essentially the same thing as the Hubbert and Rubey problem of balancing the basal resistance with the tectonic driving stress at the back of the thrust sheet. His solution to the paradox also involves tossing out a basic part of the model, but what he tosses out is the conceptualization that thrust sheets move as tabular bodies being pushed from behind.

Washington appeals to the general wedge geometry of thrust belts. Thrust belts can be translated along the basal decollement because the area of surface across which the driving stresses are applied increase towards the back of the wedge. Individual thrust sheets move along with the entire wedge, so a large part of the motion of any given thrust sheet might be due to drag along the upper surface of the thrust sheet. What Washington seems to be saying, in effect, is that part of the problem is considering a thrust sheet in isolation. Thrust belts consist of series of faults, stacked shingle-like. Thrust sheets move along a fault at their base, but typically also have another thrust bounding the top of the sheet, whose motion may contribute importantly to transmitting the appropriate stresses down to the base of the sheet. (*Note: when I first read this paper, I thought that Washington was simply off-base. Upon re-reading it a few times, I now think that there is a lot more to his argument than I first gave him credit for. I do think that his argument could be re-stated much more clearly.*

Price's response is two-fold. First, he disputes Washington's assertion that active thrust faults are everywhere near failure (a claim that Washington provided without much in the way of evidence). The point stands: if thrusts do not slip simultaneously along their entire surface, then there is no need to balance a resisting force that is in large part a function of the surface area. It is true, however, that having demolished this model of a thrust sheet, Price fails to explain how stresses are transmitted across thrust sheets, or what the origin of those stresses are. Price resolves the paradox by eliminating the model, but provides no alternative model.

Second, Price takes Washington to task for his appeal to critical wedges as a model that can explain fault motion. Critical wedge models (sandbox models) are idealized as a penetratively deforming mass of material that are slip along their base. Price is correct that, apart from the basal decollement, there are no faults in these models. Washington's figure 1 certainly appears a little *ad hoc*, and its easy to see why Price, a geologist who had spent over 20 years looking at thrust faults in the field, would have nothing but disdain for this totally unrealistic cartoon of a thrust sheet. But what Washington is actually trying to do is show that there is another source of stress driving individual thrust sheets that has to do with their being located in a larger deforming mass (something like a critically tapered wedge). At least he provides some hand-waving in the direction of a model (whose details are, at a minimum, a bit unclear).