FORUM

Comments and Replies on "Structural transect of the western Transverse Ranges, California: Implications for lithospheric kinematics and seismic risk evaluation"

COMMENT

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Namson and Davis (1988) presented a balanced cross section across the western Transverse Ranges and the Big Bend of the San Andreas fault and made a number of inferences about the style and rate of shortening. A good estimate of the deformation across this area would provide needed constraint on regional tectonics and seismic hazard, and as such their work could be very useful. We have problems with three major aspects of their model and feel that these problems, if unresolved, are substantial enough to invalidate many of their conclusions.

The first issue is the "subduction" of the lower crust. The proposed 53 km of shortening is "balanced" by an equivalent amount of convergence and downward motion of the lower crust and mantle lithosphere. Namson and Davis did not discuss the details of this process; however, they cited the work of Bird and Rosenstock (1984), Humphreys et al. (1984), and Sheffels and McNutt (1986) as supportive of their model of balancing upper crustal shortening with lower crustal subduction. Bird and Rosenstock (1984, p. 946, 955, 956) were careful to state that only the upper mantle is going down under the Transverse Ranges. They detached the mantle from the crust at the Moho, not at the ~15 km "brittle-ductile" transition proposed by Namson and Davis, for the obvious reason that they had no evidence for crustal subduction. Bird and Rosenstock (1984) stated that the solution to resolving the issue is mapping out the depth to the Moho boundary, which was done by Hearn (1984). Hearn found that there are only a few kilometres of Moho depression under the Transverse Ranges relative to "typical" continental southern California.

Humphreys et al. (1984) imaged the upper mantle, proposed small-scale convection to explain the velocity anomaly under the Transverse Ranges, and called upon upper mantle flow to support the relief. Sheffels and McNutt (1986) proposed flexural support for the Transverse Ranges, caused by "continental subduction." Whereas Sheffels and McNutt's (1986) paper leads one to infer major crustal subduction, the values they are proposing are trivial; they stated (p. 6428) that the thickening of the crust under the western Transverse Ranges is less than 2 km. Namson and Davis also failed to cite a correction (Sheffels and McNutt, 1987) that brings Sheffels and McNutt's work into agreement with Humphreys et al. (1984) on the density of the velocity anomaly under the Transverse Ranges, further diminishing the need for forces other than those derived from small-scale convection and crustal strength to support the Transverse Ranges.

In summary, Hearn (1984), Humphreys et al. (1984), Bird and Rosenstock (1984), and Sheffels and McNutt (1986, 1987) all agreed that there has been minimal crustal subduction under the Transverse Ranges, especially in the western Transverse Ranges. Although the various models contain disagreements, none of the observations support the magnitude of crustal subduction proposed by Nanson and Davis. One obvious resolution to the problem of how tremendous upper crustal convergence can be accommodated without significant lower crustal subduction is that far less convergence has occurred than proposed. Another is that the convergence occurred over a much longer time, so that surficial erosion and spreading of the lower crust thinned the crust as it was shortened. Although these may be partial solutions, we are compelled by the evidence that major, recent, shallow shortening has occurred in the western Transverse Ranges. Because the crust is only slightly thicker than the surrounding continental crust, one could speculate that it was once anomalously thin. If the 53 km of shortening proposed by Namson and Davis is taken out and no crust is lost from the system, the initial thickness of the crust would have been ≤20 km, essentially the crustal thickness under the continental borderland (e.g., Hearn, 1984). The western Transverse Ranges would therefore have been a part of the borderland and thickened into "typical" continental values by the deformation proposed by Namson and Davis. We do not believe that the geology supports this solution; however, it may be worth considering. Our preferred resolution of major surficial shortening without substantial crustal thickening lies in the kinematic relation between the convergence and regional strike-slip faulting. We have proposed (Weldon and Humphreys, 1986) that the convergence in the western Transverse Ranges is related largely to a left step in a zone of right-lateral strike-slip deformation between faults north and south of this region, and that the convergence in the southern San Joaquin basin is related to the Big Bend in the San Andreas and the western termination of the Garlock fault. Perhaps the lower crust has less unfavorable geometries than the upper crust and can move more smoothly through these regions. The upper crustal convergence proposed by Namson and Davis may be "balanced" by lateral movement in and out of the plane of the section by strike-slip motion and block rotations.

The second major problem with Namson and Davis's model is strike-slip motion through the cross section. Lateral motion allows one to move material in or out of the plane of the section, which invalidates the "balancing" done simply in the plane of the section. Namson and Davis claimed that the only place one need worry about this is at the San Andreas (discussed more below); we disagree. There has been substantial rotation and lateral shear across the southern half of their section (e.g., Hornafius, 1985; Dibblee, 1982). It is possible that much of the rotation and lateral shear preceded the time period discussed here; however, considerable nonconvergent deformation occurred since the deposition of the Eocene section, so if it does not affect the current kinematics it would have to affect the initial geometry (shown as quite simple in Fig. 2b of Namson and Davis, 1988). In fact, lateral slip and block rotation have not been included at all.

Two special problems arise from the fact that the balancing occurs across the San Andreas fault. First, the two pieces of their cross section, which are across the San Andreas from each other today, were separated by about 100 km when deformation began. Balancing these spacially and kinematically unrelated regions lacks justification until they are across the San Andreas fault from each other. However, it is clear that the low-angle feature that offsets the San Andreas is critical to Namson and Davis's entire model. The presence of their proposed offset of the San Andreas fault by a low-angle structure also causes problems when one considers the well-documented right-lateral sense of the San Andreas fault. North and south of the proposed lateral ramp in the San Andreas the fault is vertical (evidence includes a vertical belt of seismicity down to ~15 km in places where seismicity exists). Because the vertical parts of the San Andreas fault are connected to the proposed low-angle ramp, lateral motion on the fault will produce uplift as the western block rides up the lateral ramp and extension where the San Andreas becomes vertical again and the western block rides down the ramp. This is analogous to a snowplow lifting the snow as its curved blade is pushed forward through the snow and dropping it as the plow passes. In this case the "plow" is the crust on the east side of the proposed lateral ramp in the lower San Andreas fault. The magnitude of this hypothetical deformation would be comparable to the volume displaced by the ramp, which suggests that as the western block moves up the ramp, a dip panel about 20 km wide and more than 8 km high (at the crest by the fault) would form. It would approach 100 km long, the displacement across the San Andreas during the time considered here. One could argue that the proposed ramp grew with time and that the dip panel would be lowered as the backside of the bulge in the San Andreas fault passed by, but it is clear that nothing like this has occurred. Offsetting the San Andreas fault with a low-angle structure solves the contractile balance in the plane of the cross section, but creates unsolvable problems when the much greater lateral motion is considered.

The third problem is whether the western Transverse Ranges ever looked like the initial section proposed in Figure 2b of Nanson and Davis (1988, p. 677). A major justification for balancing a section is that if you could go back in time and examine the region prior to deformation, it would look like the retrodeformed section. The cross section shown in Figure 2b looks like one through the Rocky Mountains, not across an active transform margin. Because there are virtually no data constraining the central part of the section (note how few wells actually penetrate the pre-Oligocene strata, and how much of the deformation is accommodated there), the thickness and original geometry of the Eocene and older rocks at the starting time of 2–3 Ma is unknown. In effect, the units are assumed to have virtually constant thickness, broken only by the relatively few and simple normal and reverse faults shown in Figure 2b. Because there are no data with which to test this hypothesis, it is conjecture. In the absence of data one might suggest that this configuration is the simplest and therefore the most defensible; however, the degree to which this assumption is approached greatly affects the total shortening and the inferred slip rate, something the casual reader is not likely to appreciate. The well-documented rotations, lateral slip, and proximity of the region to the plate and continental boundary almost certainly require that the region was much more complexly deformed by 3 Ma; therefore, retrodeforming to an initially craton-like distribution of thicknesses is unjustified.

Construction of balanced cross sections across a transform boundary necessarily focuses on the convergence or extension associated with the local geometry of the boundary. We suggest that the dip-slip faulting must be understood in the context of the geometry of the more fundamental strike-slip faults in the transform boundary and the kinematics of the overall crustal deformation.

REPLY

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1. Weldon and Humphreys take issue with our model of subducting the lower crust and upper mantle because subduction of the lower crust is not supported by geophysical studies. Weldon and Humphreys suggest crustal thickening as an alternative, but they question whether the geology supports this solution. They favor "balancing" shallow crustal convergence in the lower crust by lateral movement or rotation on strike-slip faults out of the plane of the section. Subduction of lower crust is required in many areas of convergence (Laubscher, 1988). The ability of geophysical techniques to detect lower crustal subduction may be hampered by phase changes that cause the lower crust to be absorbed into the upper mantle (Laubscher, 1988). Our lithospheric subduction model is only one possible solution to balancing shallow crustal shortening; we have also suggested lithospheric thickening (Namson and Davis, 1988). Lower crustal thickening also quantitatively fits our model. The lower crust thickens from 23 to 34 km from the borderland across the southern margin of the western Transverse Ranges (Keller and Prothero, 1987; Hearn, 1984). The lengths of our cross section and restoration are 123 km and 176 km, respectively. Our proposed 53 km of shortening would take an unshortened 23-km-thick crust to a shortened crustal thickness of 33 km, which is in close agreement with the geophysical measurements in the borderland and western Transverse Ranges, respectively. Weldon and Humphreys's doubt that the geology of the modern-day borderland resembles the preconvergence geology of the western Transverse Ranges is unfounded. Nearly 50 years of study of marine geology, paleoenvironments, and stratigraphy have drawn comparisons between the Neogene geologic histories and settings of the two areas (e.g., Shepard and Emery, 1941; Teng and Gorsline, 1989).

The solution favored by Weldon and Humphreys cannot work. It is not possible to balance shallow crustal convergence in the lower crust by lateral motion because lateral motion does not yield shortening. Volume conservation requires that shallow crustal shortening be balanced in the lower crust and upper mantle by tectonic thickening or subduction. Balanced cross sections such as ours tell how much thickening or subduction must occur.

2. Weldon and Humphreys disagree with our balanced cross section approach because they say there have been strike-slip motions on faults that are not accounted for in our cross section. This issue was discussed in Namson and Davis (1988). Weldon and Humphreys present no evidence of significant late Pliocene and Quaternary lateral motion on faults in our cross section, nor do the references they cite. Evidence for coeval rotations is weak; if they have occurred, they are minor.

3. Weldon and Humphreys suggest two problems with balancing cross sections across the San Andreas fault: (1) it is unjustified because the regions across the San Andreas are kinematically and spatially unrelated, and (2) offset of the San Andreas fault by a ramp is incompatible with the geology. Balancing the blocks north and south of the San Andreas fault separately is justified because we assume the San Andreas fault was originally vertical and therefore use it as a reference line in restoration; it is irrelevant how much strike slip has taken place. Weldon and Humphreys assume that the ramp cutting the San Andreas fault must go from vertical to low angle and back to vertical. They state that strike-slip motion along such a ramp geometry would produce large amounts of uplift and a particular deformational geometry that is not observed. Weldon and Humphreys's suggestion is only one of several kinematic and geometric possibilities for ramp/San Andreas fault interaction. For example, one model that requires neither uplift nor the deformational geometry proposed by Weldon and Humphreys is material moving in the strike direction of the ramp accompanied by near-horizontal simple shear. This model, which is only one of several possibilities, is consistent with both the geology and our cross-section interpretation. Constraining the three-dimensional geometry requires additional balanced cross sections and geophysics.

4. Weldon and Humphreys question our restoration because (1) it looks like the Rocky Mountains, (2) it does not fit their vision of an active transform margin, and (3) there are no data to constrain the original thickness and geometry of Eocene units. Our restoration does not look like the Rocky Mountains, and we refer Weldon and Humphreys to the classic works of Bally et al. (1966), Price and Mountjoy (1970), and Royse et al. (1975). One purpose of our analyses is to provide a quantitative model of the southern California margin just prior to late Pliocene and Quaternary convergence, not to accommodate a preconceived notion of transform margin geometry. Our cross section integrates an amount of data substantially greater than any preexisting structural interpretation across the western Transverse Ranges and explains the geology at a 1:24000 map scale. The thickness and geometry of the Eocene and other units are extremely well constrained by detailed maps, measured sections, and biostratigraphy (references cited in Namson and Davis, 1988). We have projected the surface data to depth by techniques commonly used by structural geologists in mountain belts since the early part of this century. The restored geometries of Eocene-age and other units are determined by unfolding angular unconformities and successively restoring beds to the horizontal, following Steno's law of original horizontality of sedimentary beds.

Our approach to understanding the overall crustal deformation is to develop quantitative, restorable, and integrated models that resolve the map-scale geology. One result is the amount of crustal shortening recorded in folding, which has been ignored or only partially considered in previous tectonic models. With the exception of the San Andreas fault, the fundamental late Pliocene and Quaternary structures of the western Transverse Ranges are folds and thrust faults, not strike-slip faults.

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COMMENT

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Namson and Davis (1988) used the difference between present-day and restored length of their cross section as evidence for contraction of 53 km, contraction rates of 17.6–26.5 mm/yr, and recurrence intervals of 50–100 yr for earthquakes on some part of their section. Their technique leads to useful insights about subsurface structure. However, balancing the cross section does not make it unique, and different cross sections using the same observations produce different values for total contraction. Because Namson and Davis (1988) did not consider the uncertainties in their model, their calculations of contraction rates and recurrence intervals are of little value in estimating seismic risk. Furthermore, we disagree with the interpretation of the southern 40 km of their cross section, which is mainly drawn from Yeats (1979). We focus our comments on this part of the section, where most of the contraction has occurred.

For a cross section to be balanced, it must be drawn in the direction of tectonic transport so that the displacement of elements in the section may be measured directly in the plane of the section as tectonic shortening between undeformed and deformed states. Namson and Davis's cross section is north-south, whereas tectonic transport is north-northeast to northeast, on the basis of earthquake focal mechanisms and borehole breakouts (Zoback et al., 1987; Yeats et al., 1988). The direction of maximum principal compressive stress (σ_1) is nearly normal to the San Andreas fault, not normal to the trend of the Transverse Ranges (Zoback et al., 1987). Accordingly, the estimates of shortening by Namson and Davis (1988) are at least 10% too low, on this basis alone.

Namson and Davis (1988) interpreted the Oak Ridge fault as a late Miocene to Pliocene north-dipping normal fault rotated by Quaternary anticlinal folding and reactivated as a reverse fault. This interpretation ignores the possibility that these strata underwent flexural-slip folding, as documented for folded strata of the same age and facies on the north side of the Ventura basin (Yeats et al., 1981). Direct evidence of shortening in the Pliocene is provided in cross section E-F in Figure 7 of Yeats et al. (1981), in which the Oak Ridge fault tip does not extend upward into the Pliocene. In this cross section, the length of a 3 Ma horizon is about 1.5 km greater than that of a 1 Ma horizon, indicating that shortening occurred in the time between deposition of the two horizons, rather than extension, as claimed by Namson and Davis (1988). Shortening is also documented between these same horizons in an adjacent area (cross sections C-D of Fig. 7), where the fault tip extends upward to the 1 Ma horizon, and the cut-off angles between bedding and fault are similar to those shown by Namson and Davis (1988).

Namson and Davis (1988) showed the Ventura Avenue anticline as a rootless structure above a decollement in the early Miocene, following Yeats (1983). But their decollement goes the wrong way; the fold is interpreted as related to a south-vergent blind thrust related to the San Cayetano fault (their SCT1). The Ventura Avenue anticline and the San Miguelito and Rincon anticlines farther west are fault-propagation folds updip from north-verging, south-dipping reverse faults (Barnard, Padre Juan, and C-3 faults, respectively). Yeats et al. (1988) showed that these folds are related to a buried frontal thrust of the Oak Ridge fault to the south, not a blind thrust to the north.

The Namson and Davis (1988) estimate of total shortening depends on their highly speculative interpretation of the Santa Ynez and San Cayetano faults at depth. We regard their positioning of ramps and flats within the Mesozoic at 10–15 km depth beneath the mountains north of the Ventura basin as arbitrary, unsupported by geophysical or geological evidence. Their two ramps have a height of 9 km, leading to a shortening of 14 km. By varying the height of these ramps from 7 to 12 km, the shortening figure varies from 19 to 12 km. Furthermore, their depiction of the San Cayetano fault as a low-angle thrust at depth does not consider constraints on the position of the fault at depth, based on earthquakes located by Yerkes and Lee (1979) and Simila et al. (1987; G. Simila, 1988, personal commun.).

The complicated interpretation of structure beneath the mountains north of the Ventura basin includes major Oligocene thrust structures that remain unrestored on their balanced cross section. There was major tectonism in the Transverse Ranges during the Oligocene; however, this was not *contractile*, but *extensional* tectonism during deposition of nonmarine sedimentary and volcanic rocks (Simmler and Vasquez Formations) into grabens and fault-angle depressions (Muehlberger, 1958; Bohannon, 1975).

Namson and Davis (1988) based their convergence rates on the initiation of convergence at 2–3 Ma, assuming equal rates of convergence from then until now. However, the north dip of Miocene and older strata south of West Montalvo predates the deposition of the Sisquoc Formation, which onlaps this north-dipping homocline; this means that their "South

Mountain thrust," which presumably controls this homocline, is 4–5 Ma. In contrast, the Ventura Avenue anticline began to form after 0.4 Ma and perhaps after 0.2 Ma. Displacement on the Oak Ridge fault accelerated in the past 2 m.y. (Yeats, 1988), and the slip rate for the past few hundred thousand years locally is as much as an order of magnitude faster than the rate from 4 to 3 Ma. Because the slip rate for the past few hundred thousand years is the only rate important to slip-rate and recurrence-interval calculations, convergence rates based on a restored cross section at 3 Ma have little bearing on seismic risk evaluation.

On the other hand, we agree with Namson and Davis (1988) that balanced cross sections can be of value in calculation of convergence rates across the Transverse Ranges (Yeats et al., 1988), but our use is much more conservative than theirs. If a decollement horizon can be located independently, and the thickness of strata above the decollement is known, then the amount of convergence can be determined by area-balancing the section, even if the internal details of the cross section are not known. A section is area-balanced by dividing the cross-sectional area of the deformed sequence by the thickness of the undeformed sequence to give the original bed length of the undeformed sequence. If it is assumed that no rock moves in or out of the section during deformation, the difference between the original bed length and the present length of the deformed sequence is the amount of shortening (Dahlstrom, 1969; Woodward et al., 1985). This can be done for the decollement at the base of the Miocene shale section (Yeats et al., 1988) and, to a lesser extent, for the base of the seismogenic zone, established by flat-thrust earthquakes (Webb and Kanamori, 1985). In an area of extensive subsurface control, several cross sections can be drawn along strike by using independent data sets. The total convergence represents the approach of blocks north and south of the Transverse Ranges, and convergence should be the same for all sections or should vary smoothly. Convergence values that pass this consistency test are more likely to be correct.

Balanced cross sections through the upper crust can provide valuable insights, such as the depiction of the Lion Mountain anticline by Namson and Davis (1988) as a fault-bend fold above a blind frontal thrust related to the San Cayetano fault and the interpretation of the Lion fault as a passive backthrust related to the deeper blind thrust (cf. Huftile, 1988). The cross section is *admissible*, as Dave Elliott put it, but it is not *unique*. Therefore, values for contraction are not useful for seismic risk evaluations unless they are constrained by admissible cross sections giving maximum and minimum values. In addition, the Namson and Davis (1988) cross section is less in accord with available data than other solutions.

REPLY

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Yeats and Huftile raise several issues about the usefulness, originality, and accuracy of our recent structural interpretation. We address the issues as follows.

1. Yeats and Huftile disagree with our interpretation of the 40-km-long segment of the Ventura area, yet state it is mainly drawn from Yeats's earlier work. Our cross section is 115 km long; well-log correlations of only a 15-km-long segment are after Yeats (1979) and are cited in the original detailed work (Namson, 1987). Our structural interpretation bears little resemblance to that of Yeats other than location and showing late Cenozoic convergence.

2. Yeats and Huftile state that our cross section is not in the direction of tectonic transport which they believe is defined by focal mechanisms and borehole elongations. Although this approach is novel, we believe Yeats and Huftile have confused stress and strain. Borehole elongations provide some information on the instantaneous state of stress, and focal mechanisms provide some data on the instantaneous stress and instantaneous fault slip. These data do not provide the tectonic transport direction (strain) over the past 2–3 m.y. Structural cross sections (balanced or otherwise) should be drawn perpendicular to the strike of regional fold axes and thrusts (in compressional terranes), because these define the direction of tectonic transport and insure that the beds will appear in their true thickness and that geologic surfaces will show their true dip geometry (Suppe, 1985, p. 60; Woodward et al., 1985, p. 47; Ramsay and Huber, 1987, p. 365). Our section is drawn perpendicular to the average strike of the regional folds and thrusts.

3. Yeats and Huftile disagree with our interpretation that the Oak Ridge fault is a late Miocene to Pliocene normal fault rotated by Quaternary compression, and state that we ignored flexural-slip folding. Our construction technique assumes that flexural-slip folding is the main deformation mechanism, but this is irrelevant with reference to the origin of the Oak Ridge fault. Yeats and Huftile's observation of increasing shortening between 1 and 3 Ma horizons is consistent with our interpretation of the crossover from normal faulting to compression between 2 and 3 Ma. Because Yeats and Huftile do not address our principal evidence in support of a late Miocene to Pliocene Oak Ridge normal fault, we still consider this the most reasonable geological interpretation.

4. Yeats and Huftile state that our interpretation of the Ventura Avenue anticline follows Yeats's (1983) interpretation of a rootless detachment fold above a decollement. However, his most recent interpretation (Yeats et al., 1988) changes it to a fault-propagation fold. They further state that our decollement transport direction is going the wrong way because it is not in agreement with their recent interpretation. Nagle and Parker (1971, Fig. 14) first showed the Ventura Avenue anticline as a rootless fold. Our interpretation of the anticline is that it is an imbricate wedge of fault-bend folds above a decollement in the lower Miocene. The symmetric geometries of the Ventura Avenue, San Miguelito, and Rincon anticlines do not resemble the asymmetric fault-propagation fold geometry (see Suppe, 1985) as proposed by Yeats et al. (1988).

We have drawn a south-vergent thrust system under the western Transverse Ranges (south of the San Andreas fault) because that is the dominant direction of vergence of the large folds and thrust faults. In addition, the structural relief progressively increases northward from the Ventura basin. The cross sections of Yeats et al. (1988) are restricted to a narrow area of the Ventura basin and do not consider the bulk of the Transverse Ranges, most of which lie north of the basin. The sections of Yeats et al. (1988) are neither balanced nor provide a restoration. Specific problems consist of a mismatch of hanging-wall and footwall cutoffs, no pinning lines, unrealistic relations between fault and fold shape, and increasing fault slip upsection. Therefore, Yeats et al. (1988) did not present an integrated, restorable, or complete solution of deformation of the western Transverse Ranges. Northward and southward extension and restoration of their cross sections would show their proposed north-vergent Sisar detachment and its linkage to the Oak Ridge fault as untenable.

5. Yeats and Huftile state that the geometry of thrust ramps north of the Ventura basin shown in our cross section is arbitrary and could be easily modified, affecting convergence calculations. The position and ramp height in our interpretation are based on solving for the observed fold geometry and structural relief of the Lion Mountain anticline, Santa Ynez Range, San Rafael Mountains, and Pine Mountain and are not arbitrary.

6. Yeats and Huftile state that the only major tectonic event during the Oligocene in the western Transverse Ranges was extensional. There is abundant evidence throughout the western Transverse Ranges and southern Coast Ranges for late Eocene to early Oligocene convergence such as the folds and thrust faults of the San Rafael high (Reed and Hollister, 1936) and Ynezian orogeny (Dibblee, 1982; see Namson, 1987). Late Oligocene and early Miocene extension is also a widespread tectonic event; however, it should not be confused with the earlier compressive event (Davis and Lagoe, 1988).

7. Yeats and Huftile question the usefulness of deformation rates averaged over 2–3 m.y. to earthquake hazard evaluation. This is an open

question; however, the first order of business in analyzing compression is to develop a restorable geometric and kinematic solution. Determining variable deformation rates requires high-quality subsurface data that include seismic reflection data, a unique set of geologic conditions, and application of compressive growth-structure analyses (Medwedeff, 1989). We feel that this type of analysis is extremely useful. However, Yeats (1988) did not use this approach.

In conclusion, our interpretation is not unique, but it is balanced and *admissible*. It is a minimum-slip solution for which there are only a few possibilities. On the other hand, Yeats et al.'s (1988) and Yeats's (1988) interpretations are not balanced and are therefore *not admissible*. In addition, their interpretations are not minimum slip solutions. Therefore, we believe that our interpretation, estimated slip rates, and recurrence intervals are substantially more defendable.

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