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Reply

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We welcome Wessel and Kroenke's (2001) discussion of our work (Okal and Langenhorst, 2000) in the framework of their model WK97, which involves a proposed change in Pacific plate absolute plate motion (hereafter APM), taking place around 4 Ma, with respect to the model of Gripp and Gordon (1990).

In commenting on the various scenarios presented in Section 8 of OL2000, Wessel and Kroenke (2001) agree with us that Model A is unrealistic, and the crux of their criticism is in their evaluation of Model B, constructed in OL2000 as an end-member theoretical model, in which the PA–AN relative plate motion (RPM) is unchanged during the proposed Neogene reorientation of the APM of the Pacific plate. In other words, Model B assumes the NUVEL-1 RPM between the two plates (DeMets et al., 1990). In this framework, we take issue with Wessel and Kroenke's (2001) sentence:

"This statement is partly correct and partly wrong".

We understand that the statement referred to is the fact that Model B will predict strike-slip solutions on the Eltanin and neighboring systems. The quality of the fit of the strike-slip solutions to NUVEL-1, and hence to Model B, can be assessed by noting that NUVEL-1 used 36 slip vectors in our study area, which are fit with an average residual $\langle r \rangle = 0.35°$, and an average absolute residual $\langle |r| \rangle = 3.39°$. This can be compared with the fit of all 724 slip vectors used in NUVEL-1: $\langle r \rangle = 0.00°$; $\langle |r| \rangle = 6.02°$, and with the average error bar of 18.3° assessed by DeMets et al. (1990) to the individual data points in our study area. These numbers show that NUVEL-1 gives a better than average fit to the azimuths of the Eltanin slip vectors. In other words, there is nothing in the NUVEL-1 sub-dataset of Eltanin slip vectors, representative of present-day plate motions, to suggest that it is misfit in a statistically significant way by the NUVEL-1 model, representative of motions averaged over 3 million years. In this respect, we believe that our statement is "correct". We do not see where it is "partly wrong".

Wessel and Kroenke (2001) correctly point out that Cande et al. (1995) (hereafter C95) have proposed a model of Neogene evolution of the RPM between PA and AN. Géli et al. (1997) also have used C95 to account for rift propagation South of our study area. For the most recent time interval, i.e. Chron 1 corresponding to the past 0.78 million years, C95 propose a finite rotation of 0.68° (or 0.87°/million years) about a pole at 64.25°N, 79.06°W. This pole predicts a relative motion at our reference point at the center of the Eltanin system (54°S, 130°W) of 7.2 cm/year in the azimuth N66°W, while NUVEL-1 predicts 7.6 cm/year at N68°W. The difference in azimuth (2.2°) is negligible when compared with the precision of the slip vector azimuths mentioned above. In other words, C95's

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RPM between PA and AN is not sufficiently different from NUVEL-1's to be resolvable (or infirmed) from the dataset of NUVEL-1 Eltanin slip vectors. This is to say that a model, say "D", of Antarctic APM based on combining WK97 with C95's RPM would be fundamentally indistinguishable from Model B. We note that D consists of a pole at 17°N, 172°E, with a rotation rate of 0.94°/million years.

We reject Model B, not because of misfits in the Eltanin area (it was designed to fit Eltanin seismicity), but because of what it predicts elsewhere in the Antarctic plate. This is where Wessel and Kroenke's (2001) further criticism comes in, namely regarding Erebus, which they claim should not be considered a hotspot, stationary in an absolute reference frame. Because of the youth of the Erebus edifice (at most 1 million years (Kyle et al., 1992)), and in the absence of documented geochemical anomalies, we agree that our argument is weak. We, thus, propose to look at Heard Island, whose origin involves a mantle plume, as well established on the basis of isotopic geochemistry (Barling et al., 1994). The classical APM model of Gripp and Gordon (1990) predicts an eastward motion of Heard at 0.07°/million years, in other words that the plate is for all practical purposes stationary over the mantle at that location, which agrees well with the long record of volcanism at Heard, extending back 10, possibly 30 million years. On the other hand, Model B would predict motion at a hefty 0.89°/million years at an azimuth of 130°, which, over the past 4 million years would have built a 350 km long chain of seamounts, or a ridge, extending southeast from Heard. The Kerguelen Plateau cannot be a candidate for such a structure (as suggested by Wessel and Kroenke (2001)), since it extends in approximately the *opposite* direction. Therefore, Model B cannot be reconciled with the properties of hotspots on the Antarctic plate. Similarly, Model "D" derived by using the most recent RPM from C95, predicts 0.89°/million years in the azimuth 129°, as the absolute motion of Heard, a vector essentially equivalent to that for Model B.

Thus, we stand by our conclusion in Section 8: the Eltanin seismicity remains overwhelmingly strike-slip and therefore, the RPM in the area must be very close to that predicted by NUVEL-1 (in particular, it is compatible with C95, which predicts very similar vectors of relative motion at the Eltanin reference point). Any such RPM, when combined with WK97, results in an APM for Antarctica which is irreconcilable with the spatially stationary character of volcanism at Heard. This is the case of Models B and D. Model C, which was built to accommodate the tensional stress released across the Eltanin system in the anomalous normal-faulting events, was indistinguishable from B. Thus, we reaffirm that "the composite seismicity of the Eltanin system does not support the idea of a drastic change of absolute motion of the Pacific plate having taken place 4 million years ago". As for the origin of the normal faulting events, they can be explained by the presence of discrete asperities along the transform, which can also explain the deficiency in moment release (Beutel, 2000).

Finally, we wish to comment on Wessel and Kroenke's (2001) remark that "the APM change [in WK97] is most clearly displayed in the geometry of the Hawaiian Islands". This is of some concern since the combination of the two APM poles for the Pacific plate (WK97 and Gripp and Gordon's (1990)) predicts a difference of azimuths of only 20° in Hawaii, as opposed to 40° at Pitcairn and Tahiti, and more than 60° at the center of the Eltanin system. The choice locations for testing WK97 are thus, the more southerly ones. The model fits the Hawaiian chain well, but becomes progressively more mediocre as one travels South: in the society and Pitcairn–Gambier chains, the predicted azimuths of APM (342 and 341°, respectively, at Tahiti and Pitcairn) do not match the direction of progression of volcanism along the major island chains (293° along Pitcairn–Gambier and 290° along Mehetia–Bora Bora); finally, as shown here, it encounters very serious problems along the Eltanin system.

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