

CHRISTOPHER SCHOLZ

Geller and Jackson have both reproached me for not citing all of the Jackson and Kagan papers in my earlier statement. Space requirements did not allow for a fuller discussion at that time.

The 'seismic gap' hypothesis is nothing more than a restatement of Reid's elastic rebound theory. Is it incorrect? This theory applies only to system-size events, which, rather than being undefined, as suggested by Jackson, is defined in the case of subduction zones as the seismically coupled down-dip width, which can be determined by the areal extent of large earthquakes in the region.

The problem is that this is geographically quite variable, ranging from 50 km (M 7.3) to 200 km (M 8.4). So arbitrarily assuming a constant value of 7.0 (ref. 1) or 7.5 (ref. 2) will always include some events too small to qualify, this being doubly so because the Gutenberg-Richter relation insures that the catalogue will be dominated by events near the lower size cut-off. Hence with that procedure one can expect too many events in 'safe' zones, which was the result of refs 1 and 2, although, as expected, there were less discrepancies when the higher magnitude cut-off was used. This was the flaw I pointed out in my first contribution to these debates. Thus the elastic rebound theory was not properly tested.

In their more recent study³, they found, *in contrast*, less events than predicted by Nishenko⁴. But here the failure was in a different part of the physics: the assumptions of recurrence times made by Nishenko. These recurrence times are based on very little data, no theory, and are unquestionably suspect. But this failure needs to be separated from a failure of the elastic rebound theory, which would lead us to contemplate weird physics. When conducting such statistical tests, it is important to keep aware of what, in the physics, one is testing.

Christopher H. Scholz

Lamont-Doherty Earth Observatory
Columbia University
Palisades, NY 10964

References

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