Reply to "Comment on 'A Historical Review of Induced Earthquakes in Texas' by Cliff Frohlich, Heather DeShon, Brian Stump, Chris Hayward, Matt Hornbach, and Jacob I. Walter" by Steve Everley

by Cliff Frohlich, Heather DeShon, Brian Stump, Chris Hayward, Matt Hornbach, and Jacob I. Walter

BACKGROUND

Our paper (Frohlich *et al.*, 2016) reviewed the literature concerning Texas earthquakes to reach four conclusions: (1) induced earthquakes have occurred in several geographic locations, including the east, northeast, west, and Gulf Coast of Texas; (2) induced earthquakes are not exclusively a recent phenomenon—there exist plausible, documented examples as early as 1925; (3) several different industry practices have caused induced earthquakes: these include not only injection disposal of fluid wastes but also production from shallow reservoirs and secondary recovery activities such as waterflooding; and (4) beginning in 2008 rates of Texas earthquakes having magnitudes of 3 and greater increased from about 2 per year to 12 per year, and this increase is attributable to induced earthquakes.

The week following the online publication of our review in *SRL*, Everley (2016b) published his critique of our five-question test in *Energy In Depth*, a blogsite "launched by the Independent Petroleum Association of America in 2009 [as] a research, education and public outreach campaign focused on getting the facts out about the promise and potential of responsibly developing America's onshore energy resource base" (Energy In Depth, 2016). It is productive to the assessment of these important issues that critics frame their critiques of the science in the peer-reviewed literature. We thus applaud Everley's decision to publish a Comment on our paper in *SRL* (Everley, 2016a), as it is a step in this direction.

EVERLEY'S FIRST OBJECTION

Everley's comment does not challenge the conclusions of our paper directly; rather, he criticizes two features of the question-based test we apply to individual earthquakes to assess evidence suggesting an induced cause. His first and principal objection is that, unlike a previous test (Davis and Frohlich, 1993), our test no longer includes questions about fluid pressures at the well bottom and their mechanical effects at the hypocenter. This criticism misses the point of removing these questions for historic events that do not have such complementary data; many only have intensity-derived epicenters and no information on earthquake depth. Our paper frames a set of questions that can be used to assess events for which pressure measurements are not available, and thus provides a mechanism for exploring the nature of historic earthquakes and human activities across Texas and elsewhere.

We are in complete agreement with Everley that it would be desirable to include information about downhole pressure and its effect at the hypocenter. Unfortunately, as we note in our review, the needed data are generally unavailable, both in Texas and elsewhere. In the original Davis and Frohlich (1993) question-based test, the authors were motivated to include questions about pressure by the Hsieh and Bredehoeft (1981) modeling study of the Denver earthquakes. However, subsequently there have been only a few studies that include geomechanical pressure modeling concerning earthquakes near injection wells or petroleum fields (e.g., in all of Texas, four published studies: Pennington *et al.*, 1986; Davis and Pennington, 1989; Hornbach *et al.*, 2015; Fan *et al.*, 2016).

To inform modeling efforts, what would be especially useful are repeated shut-in tests performed at higher-volume injection wells—tests where pressures are increased to a predetermined value until the well is then shut in and the pressure fall-off is monitored over time. Analyses of these pressure histories can help constrain formation permeability, effective reservoir volume, and—if performed repeatedly—time variation in formation pressure, all essential parameters for modeling. Data from such tests are only rarely available. For example, of 290 verified disposal wells injecting into the Ellenberger of the Fort Worth basin, we have been able to obtain shut-in pressure history records at only six wells—five collected by order of the Texas Railroad Commission (TRC) following a 7 May 2015 M_w 4 earthquake near Venus, Texas (Hornbach *et al.*, 2016), and one proprietary measurement provided by XTO Energy in

doi: 10.1785/0220160148

support of our efforts to analyze earthquakes near Azle, Texas (Hornbach *et al.*, 2015).

These one-time measurements (not repeated) were thus available at only 2% of the Ellenberger injection wells. If, as Everley indicates, pressure measurements are deemed essential for assessing whether or not earthquakes are induced, then regulatory agencies such as the TRC should consider expanding routine reporting requirements to include pressure testing and downhole monitoring, at least in critical situations. The TRC does collect once-per-month wellhead pressure measurements as reported on publicly available H-10 forms. However, these data are of limited value for geodynamic modeling; reliable average daily measurements were available for only 167 of the 290 injection wells we analyzed in the Ellenberger of the Fort Worth basin (Hornbach *et al.*, 2016).

Even in those rare instances when subsurface pressure data are available to inform geodynamic modeling, there is no well-established consensus concerning just how detailed our knowledge about subsurface porosity, diffusivity, and stress must be, or even what physics must be included in these models. The fundamental question remains: what promotes earthquake activity under these conditions? Is calculation of frictional coulomb stress sufficient? Or must models also include poroelastic and thermal stresses? At present the science of stress modeling is less advanced than the science of earthquake location; even when regional velocity models are poorly constrained, the community recognizes that it is possible to determine hypocenters quite accurately if impulsive P and S arrival picks are available at stations situated such that the largest azimuthal gap is small (~90°), and station spacing for near-epicentral stations is comparable to the focal depth. For example, in our paper concluding that the Azle, Texas, earthquakes were most likely induced (Hornbach *et al.*, 2015), no one has questioned the focal depths we reported, nor our observation that some hypocenters occurred along a mapped fault within a kilometer of production wells and two kilometers of an injection disposal well. But Everley (2015) and others have criticized various features of the modeling and the conclusion that the earthquakes were induced.

EVERLEY'S SECOND OBJECTION

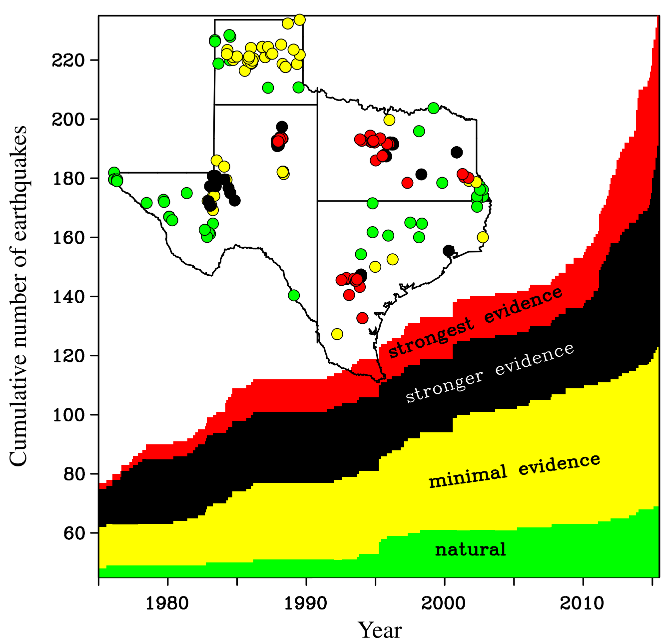
Everley's second objection to our question-based test is that it incorporates double-scoring of proximity. This is because one question (question 2, on spatial correlation) asks whether epicenters lie 5–15 km from injection or production operations, whereas another question (question 4, on faulting) asks whether there are faults within 5–15 km.

Some might say that the existence of any earthquake is evidence for the presence of a fault, and therefore questions 2 and 4 are redundant. This is not how we interpreted the questions in constructing table S2 in electronic supplement of our paper, however. Table S2 scores numerous earthquakes with epicenters in or near petroleum fields but where we have no information about faulting (e.g., 1925 and 1936 sequences in the Texas Panhandle), and in those cases question 2 can be answered but question 4 cannot. In a few cases (e.g., Cleburne and Snyder) a group of well-located epicenters forming a linear feature does provide our only evidence for faulting. However, more often the primary evidence to independently address question 4 comes from faults as mapped in the publicly available literature (e.g., Dallas-Fort Worth, Azle, Irving, and Timpson).

When assessing evidence that an earthquake is or is not induced, proximity is fundamentally important. Although Tinker (2016) correctly notes that association is not causation, spatial association is not without value. It is important evidence that is consistent with causation. That this evidence significantly contributes to the scoring is not a flaw in the test. As Tufte (2006) points out, "Correlation is not causation but it sure is a hint."

Finally, as we noted in our paper, our question-based test is subjective, and thus others will score earthquakes differently than we did. Also, we acknowledged our choices were arbitrary for the cutoff scores used to categorize earthquakes as "possibly induced," "probably induced," or "almost certainly induced." Although we stand by our scoring and all four conclusions of our paper, it is reasonable for others to prefer different cutoffs or propose different criteria for assessing whether an earthquake is induced.

Some may believe that one can never categorize any earthquake as "probably" or "almost certainly" induced in the absence of far more evidence (including subsurface pressure information and geomechanical modeling) than is currently available. For these individuals, we present Figure 1, where we have relabeled figure 5 in our paper to emphasize that our question-based test evaluates the strength of available evidence supporting an induced cause. That is, in Figure 1 the categories are labeled "minimal evidence" instead of "possibly induced," "stronger evidence" instead of "probably induced," and "strong-



▲ **Figure 1.** Strength of available evidence supporting the conclusion that earthquakes in Texas have an induced cause. This figure is relabeled from figure 5 in Frohlich *et al.* (2016). The color version of this figure is available only in the electronic edition.

est evidence" instead of "almost certainly induced." There is considerable evidence supporting the conclusion that numerous Texas earthquakes are induced, and it is unreasonable to dismiss it.

DATA AND RESOURCES

All data used here came from published sources listed in the references.

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Published Online 5 October 2016