SRT Review of the Bird Response of 6 June 2025

Ancillary Report by the Seismic Review Team
Submitted to the Diablo Canyon Independent Safety Committee on August 16, 2025

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Summary. In a series of filings to the Diablo Canyon Independent Safety Committee (DCISC), the U.S. Nuclear Regulatory Commission, and the California Public Utilities Commission , Dr. Peter Bird of the University of California Los Angeles, has argued that the seismic hazard from one or more thrust faults beneath the Diablo Canyon Power Plant (DCPP) has been underestimated by the Pacific Gas and Electric Company (PG&E), the plant's operator. In February 2025, the DCISC commissioned its Seismic Review Team (SRT) to conduct a technical review of the Dr. Bird's critique. The SRT submitted a 98-page *Review of the Bird Critique* to the DCISC on May 30, 2025 ("SRT report"). Dr. Bird filed a 13-page *Technical Response to the Review of the Bird Critique with Recommendations for Further Actions* on June 6, 2025 ("Bird response"). At DCISC's request, the SRT provides in this document a point-by-point analysis of the Bird response ("SRT review"). The SRT's main conclusion is that no evidence or assertions contained in the Bird response require changes to the findings and recommendations of the SRT report. Although the SRT has discounted Dr. Bird's findings, several of his recommendations for further actions are similar to the recommendations of the SRT report, and the overlap suggests a consensus on the geologic and geodetic studies that deserve priority in PG&E's long-term research program. One question raised by Dr. Bird is whether enough new evidence has been gained about DCPP seismic sources to warrant a formal update of 2024 Seismic Source Characterization (SSC24). It remains to be seen whether the implementation of the SRT recommendations will modify the hazard estimates in any meaningful way. Therefore, the SRT suggests postponing that decision until after PG&E has conducted the sensitivity studies recommended in the SRT report.

I. Introduction

On February 19, 2025, the Diablo Canyon Independent Safety Committee (DCISC) appointed a Seismic Review Team (SRT) to conduct a technical review of six filings by Dr. Bird to the DCISC, the California Public Utilities Commission (CPUC), and the U.S. Nuclear Regulatory Commission (NRC) regarding the seismic hazard from thrust faulting beneath the Diablo Canyon Power Plant (DCPP). These filings together constitute the "Bird Critique".

The SRT submitted a 98-page report, *Review of the Bird Critique* ("SRT report"), to the DCISC on May 30, 2025,[1] and summarized the report orally at the DCISC public meeting on June 10-11, 2025. Box 1 reproduces Section 4.4 of the SRT report, which gives an overall assessment of the Bird critique.

Box 1: Section 4.4 of the SRT Report

Overall Assessment of the Bird Critique

The Bird critique comprises three main assertions:

(a) PG&E (2015, 2024) underestimated the hazard to DCPP from thrust faulting under the Irish Hills because it made four false assumptions (§2.2). Removing these assumptions increases the thrust-faulting potency rate by a large factor.

(b) Three independent analytic methods give values for the total slip rate on shallow dipping thrust faults under the Irish Hills in the range of 2.0-2.8 mm/yr (§2.3).

(c) The recurrence interval of Noto24-type earthquakes that involves Irish Hills thrust faulting is 715-1000 yr, which poses an unacceptable risk to DCPP (§2.4).

The SRT analysis discounts the plausibility of all three statements. The SRT concludes that removing the four false assumptions in the development of a revised SSC would have no impact on the hazard estimates (§4.1). The SRT estimates the slip rate on the SWBZ thrust faults under the Irish Hills to be 0.29 ± 0.07 mm/yr, which is an order of magnitude smaller than Bird's estimates (§4.2). The SRT agrees with Madugo & Kottke (2024), LCI (2024), and NRC (2025) that a Noto24-type earthquake is not suitable as a characteristic earthquake for thrust faulting beneath the Irish Hills (§4.3). The SRT notes that, if Noto24 were adopted as the characteristic event for the Irish Hills, the estimated recurrence interval would be ten to twenty times longer than Bird's estimates.

Finding: *The best available science does not support the three main assertions of the Bird critique.*

Dr. Bird submitted a 13-page technical response ("Bird response") to the SRT report on June 6, 2025, which he summarized orally at the June DCISC meeting following the SRT presentations. After that meeting, the DCISC requested that the SRT provide a written review of the Bird

[1] The SRT report can be downloaded from the DCISC website, https://www.dcisc.org/.

response ("SRT review", this document). A point-by-point analysis of the Bird response is contained in Section II. The conclusions of the SRT analysis are given in Section III. The main conclusion is that no evidence or assertions presented in the Bird response require changes in the findings and recommendations of the SRT report.

II. Analysis of the Bird Response

The analysis of the Bird response follows the same format used in Section 2 of the SRT report. The verbatim text of his response is reproduced in **blue font**, and the SRT's point-by-point comments are written below in **black font**. In several instances, Dr. Bird responded to the SRT report by reiterating his original arguments. In those cases, our review points to the sections of the SRT report that scrutinized and evaluated those arguments.

Technical Response to the *Review of the Bird Critique* with Recommendations for Further Actions
Submitted by Peter Bird to the DCISC on June 6, 2025.

1) **Introduction**. The purpose of this Report is to respond in rebuttal to the *Review of the Bird Critique,* a report prepared by a Seismic Review Team (SRT) of three experts retained by the Diablo Canyon Independent Safety Committee (DCISC) for the purpose of assessing my seismic risk evaluation regarding the unacceptable risk of seismic core damage posed by operation of the Diablo Canyon Power Plant (DCPP). As discussed below, the SRT's claims to "problems" raised by my analysis are based on fallacious assumptions and/or faulty technical analytical methods; or debatable propositions are presented as certainties.

   Dr. Bird's assertion that the SRT analysis is based on fallacious assumptions, faulty analytical methods, or debatable propositions presented as certainties is not supported by the evidence contained in his response or elsewhere.

   a) My primary purpose here is to respond to 5 sets of related problems or objections raised by the SRT which I believe to be fallacious, analytically incorrect, or debatable. Afterward, I will include some reactions to the new "fault-related fold model" which the SRT created to explain the formation of the Pismo syncline. I will conclude with both short-term and long-term recommendations (for the SRT, for DCISC, for NRC, and for PG&E).

2) **Background**. In 2023, I was retained by San Luis Obispo Mothers for Peace (SLOMFP) to testify before the California Public Utilities Commission (CPUC) regarding my expert opinion of the seismic risk posed by continued operation of DCPP. Subsequently, on behalf of SLOMFP, Friends of the Earth, and Environmental Working Group, I submitted a total of 6 expert analyses to the U.S. Nuclear Regulatory Commission (NRC) and the DCISC, setting forth the basis for my view that DCPP poses an unacceptable risk of a seismic core damage accident. In making my analysis, I used a set of 3 alternative methods to estimate the total rate of slip on all thrust faults, and showed that each one yielded the same result: Total slip rate of all thrust faults under the Irish Hills is approximately 2 mm/a. I also demonstrated that

PG&E's analysis of seismic risk in the 2015 and 2024 Seismic Source Characterizations (SSCs) was based on incomplete data and faulty analytical methods.

a) In February 2025, the DCISC assembled a Seismic Review Team (SRT) of 3 acknowledged experts, charging it to evaluate all 6 of my expert filings, with particular emphasis on my summary report of October 30, 2024, entitled High Seismic Hazard and Risk at Diablo Canyon Power Plant Due to Thrust Faulting Under the Irish Hills (hereinafter "Summary Report").

b) On May 30, 2025, the SRT submitted their report to the DCISC, entitled *Review of the Bird Critique* (hereinafter "SRT Report"). The SRT Report was very thorough; it quoted almost every paragraph of my October 2024 report, and provided reactions. By my count, 33 of these reactions were either simple agreement, or qualified agreement.

The SRT reactions to the three main statements of the Bird critique are summarized in Box 1.

c) In particular, we agreed that the 2012- 2015 process that Pacific Gas and Electric (PG&E) went through to create their "fault geometry models" and seismic source characterization (SSC) was seriously deficient because it included *neither* (a) computed deformation models of neotectonics that objectively blend geologic, geodetic, and stress constraints, or (b) finite-strain historical models for the development of the Pismo syncline since 5 Ma. If the fault geometry models of PG&E cannot be trusted, then all of PG&E's conclusions in the 2015 and 2024 SSCs are likewise unreliable.

The SRT agrees with Dr. Bird that further work on geodetically constrained short-term deformation models and geologically constrained long-term deformation models could potentially improve the DCPP SSC, and it has recommended that PG&E pursue both lines of research. However, the SRT report did not review the SSC development process *per se* and did not state that this process was "seriously deficient."

It remains to be seen whether the implementation of the SRT recommendations will modify the hazard estimate in any meaningful way. Given the preliminary geodetic analysis in the SRT report (§3.4), as well as the geodetic data comparisons performed by PG&E (2015, 2024), it appears unlikely that further geodetic modeling will radically change the estimation of seismic hazards within the Irish Hills region. The main purpose of the geodetic modeling will be to test whether the SSC fault slip rate models predict regional deformation rates that are consistent with those measured by geodesy.

The SRT has proposed a kinematically consistent alternative fault model for the Southwest Boundary Zone (SWBZ) that accounts for folding and uplift of the Pismo syncline (SRT report §3.3), and we have recommended further development of such alternative models. Kinematically consistent fault modeling may improve upon the existing fault geometry models (FGMs) within the SSC, but the updated fault geometry and slip rate resolved on this fault has not invalidated the existing SSC. The overall architecture of faulting considered beneath the Irish Hills has not changed, and potential hazard impacts may be marginal.

d) However, the SRT also raised a number of asserted "problems" with essential parts of my simple alternative models for seismic hazard and risk at DCPP, concluding in the end that my alternative estimates are "not plausible" and biased high. This Report will address the SRT's 5 chief objections to my analysis.

3) **Isostasy and Isostatic Gravity Anomaly**. The SRT objects to my use of the present negative isostatic gravity anomaly in the Irish Hills to infer that there has been tectonic crustal thickening there since 5 Ma. Specifically, they write that "Airy compensation cannot be applied to loads as small as the Irish Hills."

   a) The conceptual background for this objection is that there is a 10-km-thick "elastic layer" of lithosphere in the region, with great strength that can support local loads non-isostatically. However, this model is untenable. In writing that "elastic plate thickness ***T***e … can be approximated by the thickness of the seismogenic zone," the SRT reveals a core incongruity in the SRT's reasoning: any <u>seismogenic</u> layer has reached the levels of deviatoric stress at which frictional plasticity dominates the strain-rate, so it no longer "elastic." Although the SRT cite simple analytical models for the bending of an elastic layer over a fluid half-space, I am not aware of any solutions for a frictional-plastic layer (especially a shell-like frictional-plastic layer on the surface of a sphere). Therefore, SRT should be less confident in their theoretical expectations of how such a layer should behave; everything depends on the past sequence of loading and strain, which is not known in this case. Perhaps the SRT could revise their objection to something like, "The 10-km seismogenic layer in the Irish Hills region is expected to be <u>strong</u>; therefore, local Airy compensation cannot always be <u>expected</u> in small areas."

There is no incongruity in the gravimetric analysis presented in §3.1 of the SRT report. We agree with Dr. Bird that the seismogenic layer should be modeled as a frictional-plastic layer based on Byerlee's law, but we were surprised to learn that he is unaware that such frictional-plastic models are well developed and routinely used in regional compensation analysis. Watts (2023) describes this modeling in his textbook. A recent example of a western U.S. model derived by Ellis & Wang (2022) is shown in Figure 1.

The elastic plate thickness T_e of a regional compensation model is an effective parameter that characterizes the bending of a rheologically stratified lithosphere. The lithospheric models used in relating T_e to rheological structure incorporate one or more brittle layers with frictional-plastic behavior described by Byerlee's law underlain by ductile layers that yield due to thermally activated creep (Figure 1a). This rheological stratification is described by a yield envelope ("Christmas-tree" diagram) that depicts the maximum stress a material can withstand at different depths within the lithosphere. The effective T_e for the western U.S. computed from Ellis & Wang's (2022) lithospheric model is about the same as the seismogenic thickness (red and blue bars in Figure 1b). This near equality has been observed by Maggi et al. (2000) and Hyndman et al. (2009) and adopted by the SRT (§3.1).

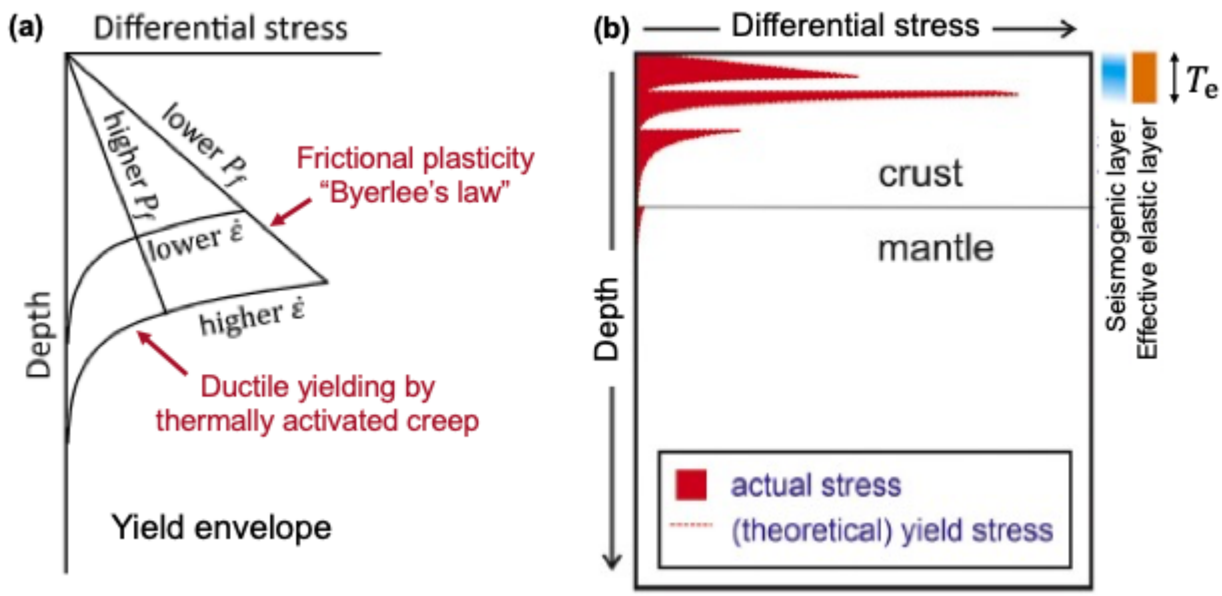


Figure 1. (a) Schematic "Christmas-tree" yield envelope for a uniform lithosphere. The linear segment depicts pressure-sensitive brittle (frictional-plastic) strength, which conforms to Byerlee's law and is insensitive temperature and strain rate $\dot{\varepsilon}$. The curved segment depicts ductile (viscous) strength, which is strongly controlled by temperature and strain rate. **(b)** Yield envelope for Ellis & Wang's (2022) warm, compositionally stratified western U.S. lithosphere deforming at relatively high strain-rates (~300 nanostrain/yr). For this model, the effective elastic thickness T_e (orange bar) is comparable to the seismogenic thickness (blue bar), as assumed in §3.1 of the SRT report. [Source: Ellis & Wang, 2022, figs. 1 and 8.]

b) However, even this non-expectation is contradicted by the actual isostatic gravity anomaly map published by PG&E; since the anomaly is <u>negative</u> (-10 mGal to -23 mGal; Figure 3 in *Langenheim et al.*, 2012; Figure 6-2 in PG&E's 2024 SSC), there is <u>less</u> mass in each vertical column than there would be in the ideal perfectly-compensated case. This means that there has been tectonic crustal thickening during the rise of the Irish Hills, which is at least 6× faster than the present uplift rate of ~0.2 mm/a.

Dr. Bird again assumes that Airy isostasy can be applied to small loads such as the Irish Hills. The evidence for why it cannot is discussed in §3.1 of the SRT report.

c) Incidentally, the SRT cites Figure 10 of *Langenheim et al.* [2012] as evidence that the negative isostatic gravity anomaly can be "explained" by low-density sedimentary rocks in the core of the Pismo syncline. Actually, low-density sediments only explain the more negative values of -23 mGal in the southern Irish Hills, relative to the less negative values of -10 mGal in the northern Irish Hills. However, all values are still negative, indicating overcompensation at a deeper level.

Langenheim et al. (2012) explain the gravity anomalies observed on a profile crossing the Irish Hills using a Bouguer model that involves no compensation below the upper crust. The gravimetric data show no evidence of overcompensation at a deeper level beneath the Irish Hills (SRT report §3.1).

d) There has probably been some semantic confusion over the traditional phrase "crustal roots." Because the elevation of the Irish Hills is modest, and because some isostatic compensation is provided by low-density rocks in the Pismo syncline, it is not necessary for these "roots" to be large or deep. My view is that crustal thickness was reduced below normal by the Miocene phase of extension, and crustal thickening in the Pliocene-Quaternary has been bringing crustal thickness back toward ~normal in parallel with horizontal tectonic shortening and surface uplift.

4) **Offset of Unit Tmo Across the SWBZ**. In my Summary Report, the second of my 3 methods for estimating the rate of thrust-faulting (on faults of shallow dip, underlying the whole Irish Hills) was to use the vertical offset (throw) of map unit Tmo (Tertiary Miocene Obispo Formation) across the SWBZ fault(s), as shown in Figure 7 of that report. This gives a throw rate of (1.6~2.2 km)/(5 Ma) = 0.32~0.44 mm/a. Then, assuming a thrust fault dip of 25°, the fault slip rate on the SWBZ is 0.76~1.04 mm/a.

a) The SRT report objects to this computation on the basis that unit Tmo Is old enough to be Involved In Miocene normal faulting, which could "explain" the vertical offset, without the need for large thrust slip or slip-rate in the Pliocene-Quaternary. However, the present disposition is that Tmo is higher on the NNE side of the SWBZ and lower on the SSW side. If that is to be explained by Miocene normal faulting, then the Miocene normal fault would have to lie within the bracket of these two offset outcrops, and it would have to dip SSW. No such normal fault has been mapped (or even proposed), so this objection is not technically defensible, and it fails.

The elevation of the Obispo formation on the north (hangingwall) side of the SWBZ is about 250 m higher than seafloor outcrops located 4 km away on the south (footwall) side. There is no need to appeal to an additional, unmapped SSW-dipping normal fault to explain this relationship. The present elevation difference is explained by reverse slip on the SWBZ. Prior to the initiation of reverse slip ca. 5 Myr ago, the Obispo formation was at a lower elevation on the north side of the SWBZ because it was situated within the Pismo basin, a graben formed by slip on the San Miguelito fault and Edna fault (Figure 2).

As explained in greater detail in the SRT report (§3.3), we model the south limb of the Pismo Syncline as formed by reactivation of the San Miguelito fault with 1.8 ± 0.2 km of reverse slip. This reverse slip does not exceed the prior normal slip on the San Miguelito fault, thus the net offset on this fault is still normal, with older Mesozoic rocks in its footwall juxtaposed against younger Cenozoic strata of the Pismo basin in its hangingwall. Originally, the footwall region south of the San Miguelito fault (Point San Luis) formed the margin of the Pismo Basin. A very gentle, four degrees of tilting is all that is required to project the seafloor outcrops of the Obispo formation northward to a position adjacent to the San Miguelito fault, above the Mesozoic rocks (Figure 2). Such gentle tilting is consistent with flexural footwall uplift adjacent to the extended Pismo basin. Alternatively, the Obispo formation was not deposited upon the elevated footwall immediately adjacent to the Miguelito fault, in which case no tilting is required to explain the footwall relationship.

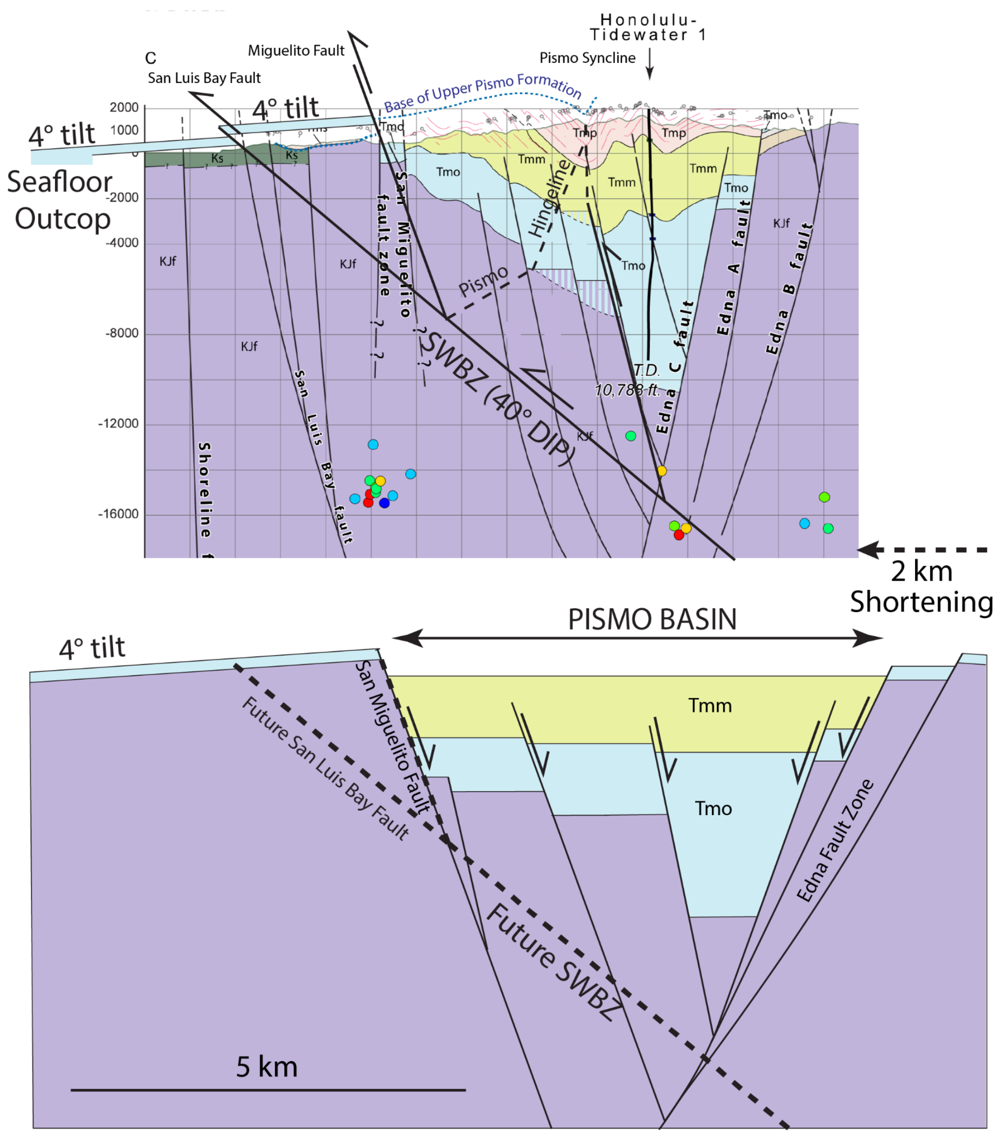


Figure 2. Top panel shows the upper portion of a kinematically controlled fault-related fold model for the SWBZ, annotated with projection of Obispo formation from seafloor outcrops to the San Miguelito fault. Lower panel shows a cross-section of the Pismo basin with reverse fault slip and folding removed. The Pismo basin formed within a graben bounded by the San Miguelito fault and Edna fault. Dashed lines show major faults of the SWBZ prior to initiation of reverse slip.

b) A second objection by the SRT is that the SRT prefers a dip of 38~40° NNE for the SWBZ fault, based on averaging of dips seen in outcrops. However, these outcrop dips range from 25°~56°, showing that most of them cannot be reliable indicators of overall fault dip at depth. There is a well-known phenomenon that faults propagating up from depth will follow a weak bedding-plane, joint, or minor fault in the uppermost kilometer, causing very erratic dips in outcrop.

It is inconsistent to argue that faults must dip 25° based on Mohr-Coulomb mechanics and at the same time argue that deformation may also reactivate a weak bedding plain, joint, or minor faults in the uppermost kilometer, causing very erratic dips. The principles of rock mechanics, including the tendency of faults to reactivate pre-existing planes of weakness, apply throughout the brittle crust. Real faults are complex, undulatory, and consist of multiple closely spaced slip surfaces within a volume of sheared and damaged material (Sibson, 2003). This complexity naturally leads to variation of fault dip in outcrop exposures. To capture this variation, we collated observations of the SWBZ from the literature. This includes outcrop measurements in Franciscan bedrock, faulted contacts between Franciscan bedrock and Cenozoic strata, a sea-cliff exposure in Cenozoic strata, and a well imaged fault in 3-D seismic data. The shallowest dip observation from the SWBZ is 25°; but the mean value is considerably higher, 38 ± 3°. This mean dip is consistent with the dip of the SWBZ necessary to explain folding of the south limb of the Pismo Syncline. As discussed in the report, a dip of 25° would cut up-section in the down-dip direction, which presents a fault configuration inconsistent with the observed dip of the fold limb.

c) Worse, scarps of thrust faults with gentle dips (*i.e.*, 25°) are preferentially modified by collapse of the unsupported hanging-wall, leaving rough scarps composed of rubble rather than a clean outcrop. Such places do not yield field measurements. Thus, I do not put much confidence in such outcrop dips.

Hangingwall collapse tends to produce shallow fault dips as the hangingwall overthrusts the ground surface. The fault dips we report from the SWBZ are measured from intact outcrop exposures, not collapsed scarp material. None of the measured dips from the SWBZ is less than 25°.

d) Later in the SRT Report, the team develops a model in which the NNE-dipping SWBZ fault is a reactivated Miocene normal fault. This is a very interesting idea, with 2 implications:

   i) During Miocene normal faulting, the N-side outcrop of Tmo would have been displaced <u>down</u> relative to the S-side outcrop. This means that the net throw during Pliocene-Quaternary thrust faulting would be <u>larger</u> than I assumed, and the thrust slip-rate also <u>greater</u>, with no upper limit.

The present-day outcrop relationship across the San Miguelito fault confirms that the north-side outcrop of the Obispo formation is displaced down relative to the south-side outcrop. This relationship further shows that reverse slip on the San Miguelito fault is less than the prior normal slip. Contrary to an unlimited amount of slip, this firmly limits the amount of slip on the San Miguelito fault to be less than the total thickness of the adjacent Pismo basin (< 2.5 km including the Obispo, Monterey, and lower Pismo formations). Our estimate of 1.8 ± 0.2 km of reverse slip on the San Miguelito fault is based on the observed length of the south limb of the Pismo syncline. The relationship of limb length to fault slip is well established from fault-bend fold theory (Suppe, 1983). Additional slip on the San Luis Bay fault is limited by the preservation of upper Pismo formation in its hangingwall. The large amount of overthrusting advocated by Dr. Bird would require considerable erosional removal of the hangingwall, contrary to this evidence.

ii) If the Pliocene-Quaternary thrust slip rate on the SWBZ fault is larger than I previously computed, I should retract my proposal that the thrust slip rate from the SWBZ should be doubled in estimation method #2, based on an idea of "symmetry" of the Irish Hills. Instead, I now think that that the SWBZ slip rate should stand alone as the method #2 estimate, but it should be understood as a lower limit.

Thrusting beneath the Irish Hills does appear to be asymmetrical with a higher amount of shortening across the SWBZ than across the Los Osos trend. Dr. Bird's estimation of slip on the SWBZ from overthrusting of the Obispo formation is not supported by structural relationships in the hangingwall of the SWBZ.

5) **Fault dips**. In my previous filings, I pointed out that there is no good data on dips of thrust/reverse faults under the Irish Hills, because the seismic experiments sponsored by CPUC failed to image them. I proposed that the default dip for modeling thrust faults should be 25°, which is based on Mohr-Anderson friction theory for a homogeneous rock with "Byerlee's Law" friction of 0.85. I further stated that the reverse fault dips modeled by PG&E in their 2015 and 2024 fault geometry model (30° to 80°, with most logic-tree options >45°) were mechanically impossible.

a) The SRT has responded by citing *Smith et al.* [2017], a very nice study of friction in fault gouges from the Moonlight fault of New Zealand, showing how low friction in gouges promoted reactivation of a former normal fault as a steeply-dipping reverse fault. Thus, my characterization of "mechanically impossible" was too strong.

Smith et al.'s (2017) study of the high-angle Moonlight fault as well as many other studies of transpressional inverted basins in California and elsewhere demonstrate that the high-angle reverse faults, such as those mapped in the Irish Hills region, are mechanically feasible seismic sources.

b) However, there are 3 cautions that must be observed before endorsing the reactivation model and accepting PG&E's preferred steep dips:

i) <u>What is "Byerlee's Law" friction?</u> Both *Smith et al.* [2017] and the SRT refer casually to Byerlee's Law friction of "0.60," rather than 0.85. Figure 7 of *Byerlee* [1978] shows that friction of 0.85 continues up to effective normal stresses of ~300 MPa, beyond which the tangent friction decreases to 0.6. In continental crust, this transition occurs at a depth of about 18 km. Given that the seismic zone in the Irish Hills is thinner, it is inappropriate to use the lower friction. Changing friction to 0.85 in Figure 3.3(a) of the SRT report would cause the maximum dip for reverse reactivation to decrease below the hydrostatic value of 48° (in homogeneous rock) that is emphasized by the SRT. Thus, a "reactivation" defense of

PG&E's steep dip angles necessarily relies on the model of weak fault gouge in the old normal faults.

Dr. Bird is incorrect in asserting that the feasibility of high-angle fault reactivation "necessarily relies on the model of weak fault gouge". The weak-gouge model, documented by Smith et al. (2017), is a plausible mechanism, but the SRT report (§3.2.2) identified two other mechanisms for promoting high-angle faulting: high fluid pressures and oblique slip.

Regarding Bird's comments on Byerlee's (1978) law: Byerlee obtained a high value of internal friction (0.85) from laboratory data at lithostatic pressures less than 200 MPa (not 300 MPa as stated by Dr. Bird). This pressure corresponds to a crustal depth of about 7 km. However, various effects tend to lower the internal friction of faults at shallow depths. In a shear zone, for example, the rotation of the maximum compressive stress reduces the coefficient of friction according to $\mu_a = \sin(\tan^{-1} \mu_i)$, so that $\mu_i = 0.85$ yields an apparent internal friction of $\mu_a = 0.65$ (Lockner & Byerlee, 1993). For these reasons, most investigators (e.g., Smith et al., 2017; Scholz, 2019) use 0.6 as the standard friction coefficient in the seismogenic zone, corresponding to a lock-up angle of about 60º. As described in §3.2 of the SRT report, high fluid pressures, oblique slip, and weak fault gouge can each act to increase the feasible angles of reverse faulting well beyond 60º (see SRT Figure 3.3). Various studies, such as Smith et al. (2017) and Sibson (1989, 2022), have used these mechanisms to explain the observations of high-angle reverse faulting in inverted basins.

ii) Metamorphism. *Smith et al.* [2017] discussed fault gouge that is weakened by the presence of "muscovite" or chlorite. Having some experience in studying phase diagrams and friction of montmorillonite [*Bird*, 1984], I suspect that the weakness of their "muscovite" is due to selective replacement of cations (either Na^+ or Ca^{++} replacing K^+) along selected cleavage planes, creating weak montmorillonite (MM) selvages. My study also showed that MM cannot withstand dehydration by effective normal stress (alone) any deeper than 7 km (Na^+ MM) or 11 km (Ca^{++} MM). Also, it is very sensitive to groundwaters containing K^+ and will quickly revert to strong muscovite, even at depths much smaller than this.

Chlorite is not stable above ~800° C [*Lakey & Hermann*, 2022]. The Miocene extensional event in the future Irish Hills region was initiated, along with strong volcanism (basal unit Tmo) and metamorphism by the passage of a slab window (replacing subducting Farallon plate) at ~28 Ma. Asthenosphere at ~1300°C upwelled in the slab window, strongly heating the crust. That heat pulse has slowly decayed as a new thermal boundary layer gradually formed at the top of the mantle. Would any weak minerals in fault gouge have survived this metamorphism, so that they could still be present for possible fault reactivation at 5 Ma? The answer is unclear.

Thermal constraints from central California do not support the presence of elevated temperatures sufficient to cause regional metamorphism during the Miocene extensional event. Thermal modeling of the passage of the Mendocino triple junction through central California by Heasler

& Surdam (1985) showed that the peak temperatures at 10 km depth did not exceed 300°C. This study specifically modeled the thermal history of the Pismo basin and showed that the base of the Monterey formation may have been heated to as much as 100°C within the deepest part of the basin. This is consistent with preserved high organic content and limited thermal maturity of Monterey formation here. Underwood & Laughland (2001) studied the thermal maturity of Cretaceous marine turbidite strata at Pt. San Luis. These authors concluded that peak temperatures of 135-205°C were reached as these rocks were deformed within the Franciscan accretionary prism. No Late Cenozoic thermal overprint is evident, and the adjacent Pismo Basin strata have not been subject to temperatures exceeding 85°C.

iii) Reactivation of old thrust faults preferred. I have emphasized that the "basement" of the Irish Hills is the Franciscan Complex, a mixture of exotic seafloor lithologies with Cretaceous-Paleogene arkoses derived from the Sierran volcanic arc and deposited in the Franciscan Trench. As this material was accreted to the North America plate by Farallon subduction, numerous low-angle thrust faults were formed within it. Thus, if metamorphism did not preclude survival of weak fault gouges under the Irish Hills, Miocene normal faults would have had to compete with Cretaceous-Paleocene thrust faults for reactivation at 5 Ma. Given equal stresses on both, reactivation of the low- angle thrusts would be preferred and would have happened first, blocking any reactivation of steeply-dipping faults.

Dr. Bird speculates that there are old (Cretaceous-Paleocene) shallow-angle faults within the Franciscan basement that are properly oriented to compete with reactivated Miocene high-angle normal faults in accommodating the compression of the Irish Hills. That slip will preferentially occur on preexisting low-angle faults in the Franciscan basement, rather than on preexisting high-angle faults, is a conjecture without observational support. The SWBZ is an example of thrust faulting in Franciscan basement, but it is not known whether this fault is a relic structure from the Cretaceous-Paleocene subduction regime.

c) Considering all these factors, I now think that my previous characterization of PG&E's steep reverse fault dips should be changed from "mechanically impossible" to "mechanically implausible." However, this semantic change does not affect my conclusion that alternative seismic hazard models are necessary, and it does not affect the content of my models, in which I continue to assume 25° dips.

The SRT agrees that viable alternative models for reverse-fault hazards at DCPP should be explored and has made recommendations to guide that exploration. However, the SRT analysis rejects the specific models advocated by Dr. Bird as viable alternatives.

6) **Global Positioning System (GPS) Data**. GPS data is important to my model because it provides one of the 3 ways to estimate the total slip-rate of all shallow-dipping thrust faults under the Irish Hills. However, interpretation of GPS can quickly become complex.

a) Writing for non-specialists in my Summary Report, I limited discussion to the three most important points:

   i) It is necessary to distinguish between observed interseismic velocities (which are usually continuous across fault traces) from modelled long-term-average velocities (which are usually discontinuous).

   ii) Relative interseismic velocities among the 4 stations bracketing the Irish Hills indicate a shortening rate of about 1.8 mm/a in the NNE-SSW direction.

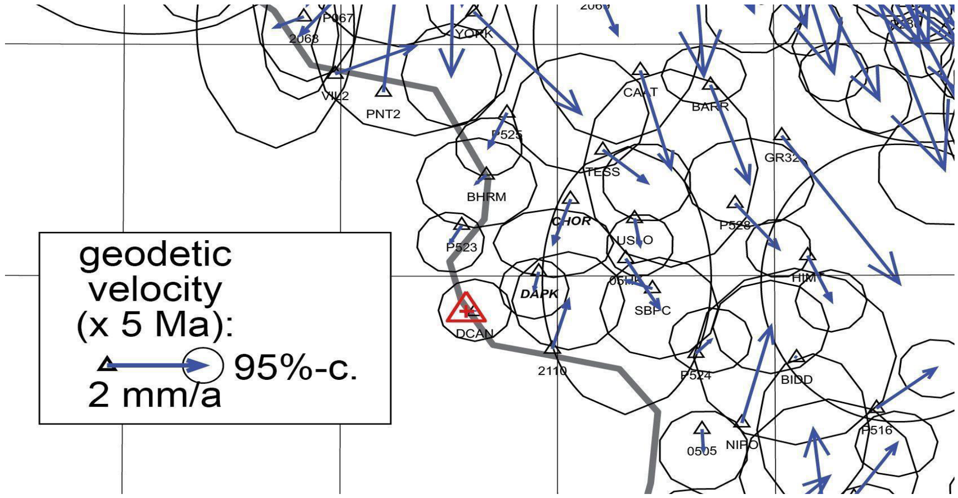
   iii) More sophisticated analysis was provided by my program NeoKinema, used in the USGS-sponsored modeling project reported by *Shen & Bird* [2022]: the long-term shortening rate prediction was similar at ~2 mm/a.

b) The SRT objected that my treatment of GPS was initially opaque, and then simplistic in two different ways. Specifically: (1) *"We were unable to reproduce these shortening estimates"* that I quoted; and (2) when interpreting interseismic strain-rates, it is necessary to estimate how much comes from distant faults; and (3) when interpreting NNE-SSW shortening, it is important to recognize that both strike-slip and thrusting can be involved. I will answer each in turn.

c) Station-to-station velocity differences. I used the *Zeng* [2022] dataset with no modifications. I plotted the velocities (and their confidence ellipses) with my publicly- available program NeoKineMap. And I used the option, in that program, to change the velocity reference frame by adding a simple Eulerian rotation-rate to the entire dataset; I chose an Euler pole that keeps the Irish Hills stationary and non-rotating. Such an operation is extremely common in geodesy, and has absolutely no effect on baseline rates or strain rates, which are contained in velocity differences and derivatives, not in (frame-dependent) absolute velocities. (In fact, the SRT also removed the velocity of DCAN, but did not remove the local rotation.)

The reference frame transformation of the velocity field is common practice, but it requires the selection of a station subset to define a non-rotating Irish Hills block, which Dr. Bird did not specify. In the SRT report, we shifted the velocities from the NAM14 reference frame to one in which the Diablo Canyon station (DCAN) was stationary. We did not rotate the reference frame owing to the sensitivity to fixing the non-rotating station as well as and the non-uniform distortions that the rotations make to the relative velocities of stations along the San Andreas fault. Of course, as Dr. Bird notes, the strain rates and the velocity differences between two nearby stations are insensitive to the choice of the reference frame, so the reference frame issues are minor in the interpretation of the GPS data.

d) The result is shown in Figure 1, which I also provided to the SRT at our 18 March 2025 meeting.



Bird's Figure 1. Detail from a map of interseismic GPS velocities from the dataset of *Zeng* [2012]. The red triangle by the (very approximate) coastline marks the site of DCPP. Stations framing the Irish Hills include DCAN, 2110, and CHOR; station DAPK is approximately in the center. The velocity reference frame has been adjusted to hold the Irish Hills stationary and non- rotating. Note horizontal shortening, especially between DCAN-CHOR and between 2110-CHOR. Uncertainty ellipses are based on a block-diagonal approximation of the geodetic covariance matrix, as the full matrix was not available.

It is difficult to draw tectonic inferences from the raw GPS velocity data shown in Bird's Figure 1 for several reasons: the density of GPS stations in the vicinity of the Irish Hills is low (only 4 are within the Irish Hills proper), the single-station 95% confidence ellipsoids are large (many comparable in size to the 2 mm/yr rate that Dr. Bird assigns to thrust faulting beneath the Irish Hills), and the two-station velocity differences fluctuate between apparent contraction and apparent extension, reflecting potential large measurement errors and/or non-tectonic motions. For example, the apparent contraction observed on his map between stations DCAN and CHOR is about 1.8 mm/yr, but other stations at similar distances from DCAN show either very little contraction (e.g., BHRM, USLO) or apparent extension (e.g., TESS and 05HK).

The high scatter and low coherence among the two-station relative velocities in the vicinity of the Irish Hills indicate that measurement errors and non-tectonic motions dominate the observed velocities. In Figure 3, we show the dilatational strain rates and velocity vectors predicted for a high slip-rate (2.5 mm/yr), shallow-dipping (25°) elastic dislocation locked to 15-km depth, consistent with the thrust faulting model advocated by Dr. Bird.[2] The model-predicted GPS velocities are smoothly varying, regionally coherent, and small; the largest velocity relative to DCAN (at P528) is ~0.6 mm/yr. For this high-rate model, the CHOR-DCAN relative velocity is only about 0.3 mm/yr. Lowering the slip rate to the SRT preferred value for the SWBZ (0.29 ± 0.07 mm/yr) reduces the relative velocities by nearly an order of magnitude. The scatter observed

[2] See SRT report §3.4.6 and Appendix B for details on the dislocation models.

in Bird's Figure 1 clearly precludes the reliable detection of such a small, localized tectonic signal (see also Figure B8 of the SRT report).

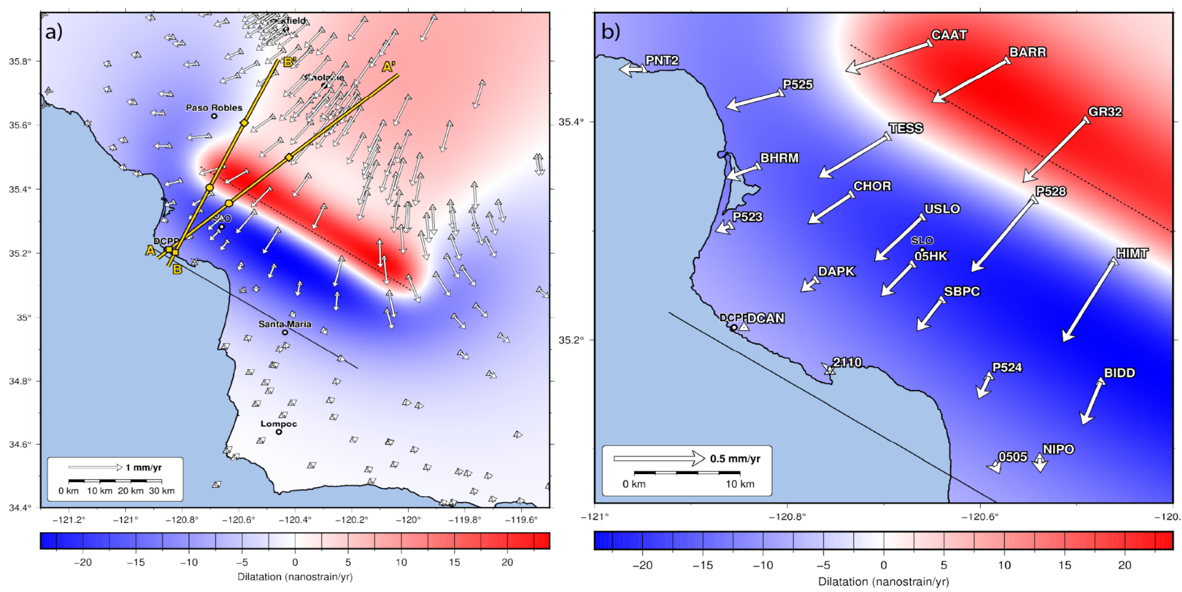


Figure 3. (a) Maps of the dilatational strain rates (extensional in red, contractional in blue) and velocity vectors (white arrows) from a 3D elastic dislocation model similar to the shallow-dipping fault proposed by Dr. Bird. The dislocation model dips at 25° and slips at 2.5 mm/yr below a locking depth of 15 km. Solid black line is the surface trace of the modeled fault; dashed black line is the fault location at the locking depth. Offshore strain rates have been cropped. White arrows show the model-predicted velocities at the GPS stations. **(b)** Zoomed-in view of the Irish Hills region with labeled GPS stations. Area is similar Bird's Figure 1 but the velocity scale is larger. [Source: Figure 3.17a of the SRT report.]

To further investigate the geodetic signals of active faulting, the SRT inverted the GPS data for strain rates under the assumption that the tectonic strain field is smoothly varying (SRT report §3.4.5). Inversion of the raw Zeng (2022) data showed high-amplitude spatial variations in strain rates (± 200 nanostrain/yr, Figure 4a), much of it due to observational noise. This stochastic variability was reduced by an order of magnitude by applying Sandwell & Wessel's (2016) *gpsgridder* to smooth and interpolate GPS velocity vectors in a way that suppresses inelastic motions (Figure 4b). However, in both inversions of the raw data, the greater Irish Hills region is characterized by extension, rather than the contraction expected for a transpressive tectonic regime. It is only when the Zeng (2022) data are corrected for the interseismic deformation due to the San Andreas and other high-rate regional faults (which we here call the "San Andreas correction") that the contractional deformation becomes evident (Figure 4c). The particular feature of the San Andreas system most responsible for the change from extension to contraction in the Irish Hills region is the discontinuity associated with the transition from the creeping to locked sections of the San Andreas fault near Parkfield, as demonstrated in the modeling study by Titus et al. (2011) (see Figure 3.11 of the SRT report).

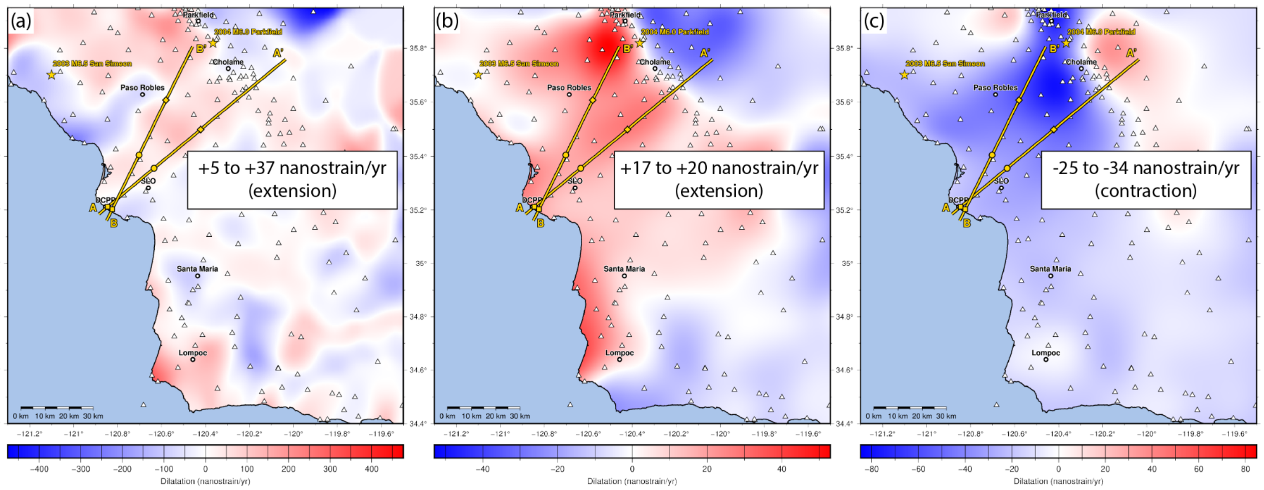


Figure 4. Dilatational rate maps from SRT inversions of the Zeng (2022) GPS data: **(a)** ungridded, raw; **(b)** gridded, raw; and **(c)** gridded, corrected. Red is extensional and blue is contractional; strain-rate scales are different for each plot. Gridded inversions in panels (b) and (c) used *gpsgridder* (Sandwell & Wessel, 2016) to smooth and interpolate GPS velocity vectors in a way that suppresses inelastic motions. In panel (c), the gridded data were corrected by subtracting the interseismic deformation computed from the Loveless & Meade (2011) fault model of the San Andreas, Hosgri, and other high-rate regional faults. The SAF-normal (A-A') and SWBZ-normal (B-B') profiles are shown as gold lines annotated with the locations of station DCAN (square) and 25 km (circle) and 50 km (diamond). Strain rates quoted in white boxes are the ranges of extensional strain rates averaged over 25 km and 50 km baselines on profiles A-A' and B-B' from Table 3.4 of the SRT report. Both the ungridded and gridded inversions of the raw velocities yielded extensional deformation along the profiles. Corrections for the interseismic deformation of the San Andreas system decreased the dilatational strain rate to negative (contractional) values along both profiles. Therefore, the correction scheme employed in the SRT report has the net effect of increasing the estimated contraction rate, not decreasing it as Dr. Bird claims. [Sources: Figures 3.15a, 3.16a, and B.2 of the SRT report.]

e) Correction(s) for effects of distant faults. The SRT is quite right that this correction is appropriate, in principle. For example, *Argus et al.* [1999] removed effects of San Andreas fault-related interseismic strain-rates in order to reveal the rates of thrust faulting along the northern margin of the Los Angeles basin. However, I will argue that the SRT did not perform this step correctly, and improperly reduced the magnitude of the shortening-rate signal by 50~75%.

Dr. Bird misreads the SRT report. He claims that, by making the San Andreas correction, we improperly reduced the shortening-rate signal by 50-75%. This assertion is clearly false: the correction increases the contractional strain, as clearly stated in the SRT report and shown by the maps in Figures 3b and 3c. Without this correction, the average strain rates in the Irish Hills are extensional.

f) The SRT's initial step was to plot GPS velocities on profiles parallel and perpendicular to the San Andreas fault. This did not show any influence of the San Andreas on GPS in the Irish Hills. As every geophysicist knows, interseismic velocities around a locked strike-slip fault in an elastic lithosphere vary as arctan(x/d), where x is distance from the fault and d is the locking depth. The SRT's Figure 3.7 shows that such a pattern only appears at distances of more than 40~50 km from DCPP. Closer, velocity variations are dominated by other faults, and the profile is quasi-linear, but with important jumps. I argue that it was not helpful that the SRT fit a linear trend to this nearer data; it effectively averaged the higher shortening rates in the Irish Hills with lower rates outside. This is part of the explanation for the SRT's downward revision of shortening rates in the Irish Hills.

Shear deformation along a locked strike-slip fault does indeed follow an arctangent function with interseismic strain rates most pronounced close to the SAF (SRT Figure 3.7b). The San Andreas correction removes this shear-strain signal from the profile-projected data, leaving a well-resolved linear trend in both the profile-normal velocities (SRT Figure 3.12b,c), which we attributed to regional strike-slip faulting not included in the San Andreas correction.

Dr. Bird characterizes the corrected velocity profile data as "quasi-linear, but with important jumps," saying that our procedure "effectively averaged the higher shortening rates in the Irish Hills with lower rates outside." The profile data in Figure 3.12 show substantial scatter, but a visual examination reveals no significant jumps. This conclusion is supported by the strain-rate estimates in SRT Table 3.3. In all cases, the strain rates averaged over the 25-km baselines across the Irish Hills are statistically consistent with the strain rates averaged over the 50-km baselines that extend northeast beyond Irish Hills. There is no evidence in the GPS data that the shortening rates across the Irish Hills are higher than the shortening rates closer to the San Andreas fault.

g) The SRT's second attempt to remove effects of distant faults used the block/dislocation model of Loveless and Meade [2011]. Quoting the SRT: "The block model, shown in Figure 3.9, includes the Hosgri, Garlock, and San Jacinto faults, although their contributions have a negligible effect on the results. We subtract the predicted velocities from the raw Zeng GPS data and refer to the residual as the 'corrected velocities'." This method also resulted in removal of a large fraction of the shortening rate in the Irish Hills; however, there are two important problems with this approach.

The last sentence repeats Dr. Bird's misreading of the SRT report. A correct sentence would read, "This method also resulted in a substantial <u>increase</u> in the shortening rate across the Irish Hills…"

i) First, the Loveless and Meade [2011] algorithm includes a very questionable step: during each iteration of the model velocity field, residuals with respect to GPS velocities are computed, and all GPS sites with large residuals are deleted as "outliers." Perhaps for this reason, their model was not included among the deformation models informing the 2013 Update of the USGS National Seismic Hazard Map [Field et al., 2013], even though it was produced in the same time-window.

These comments about the details of how Loveless & Mead (2011) fit the GPS data are irrelevant. The only components of their model that we used are the fault geometry, slip rates, and locking depths of the high-rate faults (SRT Figure 3.9). Their values for the on-shore faults are geologically reasonable and supported by other studies. The Loveless-Meade slip rate of the off-shore Hosgri fault was too high (5-6 mm/yr), so we lowered it to 1.7 mm/yr, consistent with SSC24. Sensitivity tests performed by the SRT showed that the plausible errors in these parameters would produce no significant variations in the San Andreas correction of the Irish Hills data. As noted above and in the SRT report, the most important feature in the model used for the San Andreas correction is the well-constrained transition from the creeping to locked sections of the SAF near Parkfield, which produces extension in the greater Irish Hills region (Titus et al., 2011).

ii) Second, their block model includes only a fraction of the faults studied by other modelers. It is well-known that when some sources of strain-rate in a GPS dataset are not modelled, a weighted least-squares solution will attempt to 'absorb' that strain energy by assigning it to modelled faults. (This probably caused the San Andreas and Hosgri slip-rates to rise inappropriately, and their inferred locking depths to deepen.) Then, *"correcting"* the GPS data in the Irish Hills by using such a defective model as a reference is inappropriate.

Again, this criticism is irrelevant to the issues at hand. The Hosgri slip rate in the Loveless-Meade model was indeed too high, and we lowered it. The Loveless-Meade slip rates along San Andreas segments in the region of interest are 31-32 mm/yr, which is within the range of geologic estimates obtained by Sieh & Jahns (1984, 31.0-36.8 mm/yr), Grant Ludwig et al., (2019, 35-37 mm/yr), and Toke et al. (2011, 21.9-32.6 mm/yr).

h) Conspicuously missing from the SRT's discussion of this subject is the point that the NeoKinema program of *Shen & Bird* [2022] has already solved the problem much more completely and accurately. In every iteration of every solution, it solved very large linear systems that attempted to explain all ~5,000 GPS velocities in the western U.S. by solving for the slip-rates of all the active faults in the western U.S. (Geologic slip-rate constraints and stress-direction constraints were also included in this inversion.) This provided all necessary corrections for influences of distant faults. Furthermore, NeoKinema serves the additional function of converting observed interseismic GPS velocities to modelled long-term-average velocities. (This step uses dislocation-modeling methods similar to those in section 3.4.6 of the SRT report, except that we use more accurate dislocation-modeling methods that take account of the curvature of many thrust fault traces [*Carafa et al.*, 2018].) The result, as I stated in October 2024 and elsewhere, is a long-term-average NNE-SSW shortening rate in the Irish Hills of about 2 mm/a. This near agreement of the interseismic and the long-term- average rates shows that effects of distant faults are not important in this particular case.

We acknowledged in the SRT report that Bird's NeoKinema is a potentially useful methodology for modeling the interseismic deformation measured by GPS, and we recommended that, "in the long term, PG&E should consider implementing the NSHM methodology that uses an ensemble of deformation models (including NeoKinema), driven by a common set of inputs, to validate the slip rate and fault geometry inputs to the hazard models" (SRT report, p.3).

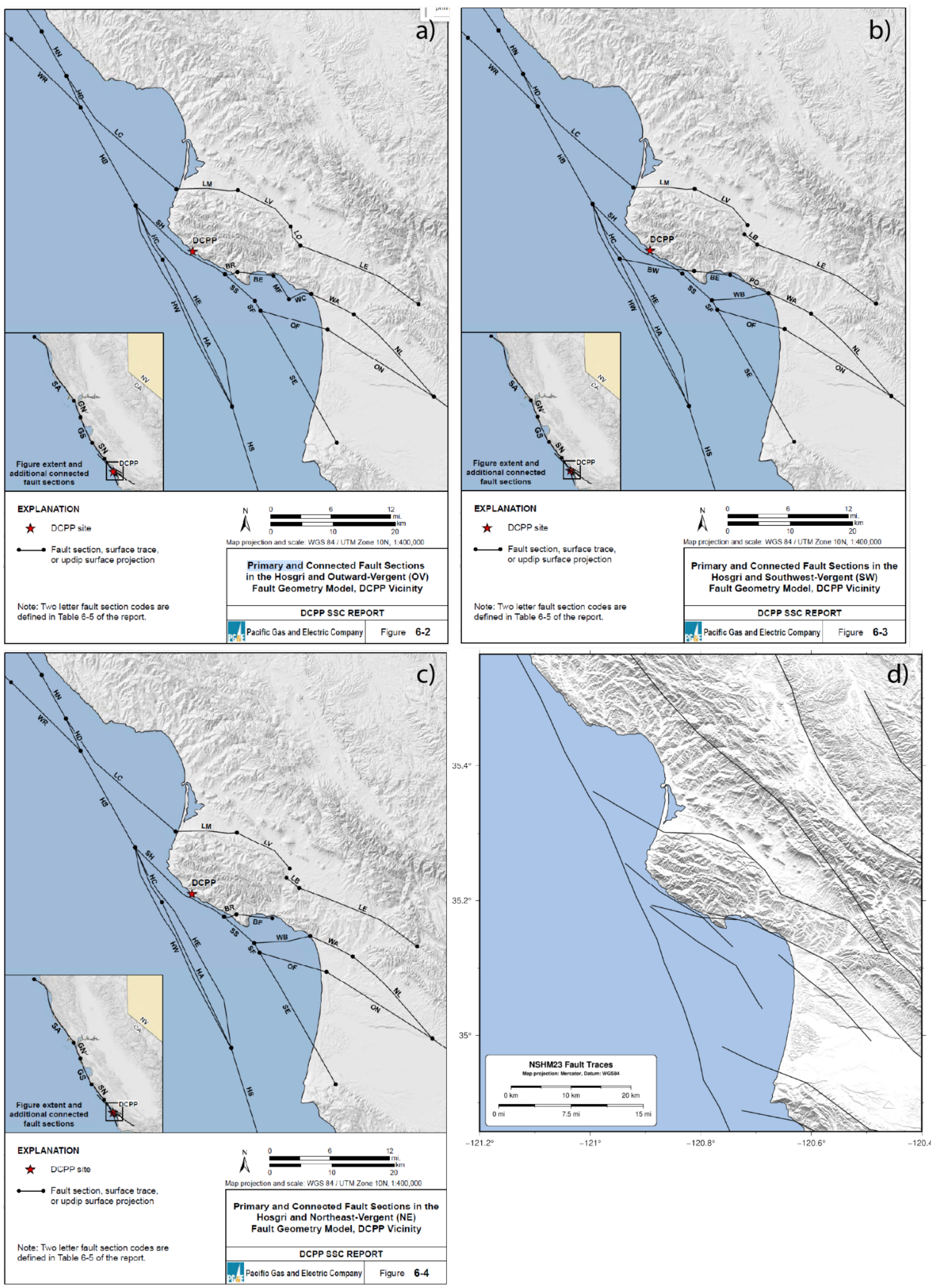


Figure 5. Fault-trace maps comparing the fault geometry models from the PG&E SSC (**panels a-c**) with the NSHM fault geometry model (**panel d**) used by Shen & Bird (2022). Significant differences are evident in the fault geometry south of DCPP, especially regarding the SWBZ, the Hosgri fault system, and the fault-to-fault connectivities.

Shen & Bird's (2022) model is based on the NSHM fault geometry model (FGM) as input, which is significantly different from the SSC15 FGMs. The differences are illustrated by fault-trace comparisons in Figure 5. If an FGM does not contain all active faults or has an incorrect geometry, the predicted deformation can be inaccurate, as Dr. Bird points out in paragraph (6.g.ii). The FGM differences disqualify the Shen & Bird (2022) model as appropriate input to the SSC. A bias in the NeoKinema rate is indicated by the Zeng (2022) GPS data, which give corrected rates that are two to five times smaller than the Shen-Bird shortening rate of 2 mm/yr.

i) Partitioning shortening between thrusting and strike-slip. Again, the SRT is right in principle; this partitioning is just not important in the Irish Hills. Strike-slip faulting has only been mapped outside the Irish Hills, on the adjacent Shoreline fault. However, PG&E's 2015 SSC estimated its slip rate as 0.06 mm/a. The only inference of strike-slip within the Irish Hills was by *Hardebeck* [2010, 2011, 2014], who studied microseismicity under the Local Area Source; however, her estimate that strike-slip is slightly faster than thrusting was superseded by two later moderate-magnitude thrusting earthquakes {2023.12.27 m3.1 at 6.2 km depth under the Irish Hills, and 2024.01.01 m5.4 slightly offshore from the NW end of the Irish Hills [D. J. Weisman, pers. comm., 2024.01.02]} that change the net moment budget back toward thrusting. Thus, partitioning is a minor problem that should be studied in a future revised SSC, but it has little effect on my first-order estimates of the rate of crustal shortening by thrust-faulting.

We agree with Dr. Bird that the strain-rate partitioning between strike-slip and reverse faulting is an important problem for future research on DCPP hazard, and two of our recommendations are directed at this issue. The regional geology indicates that the present-day tectonic regime of the greater Irish Hills region is transpressional, but better constraints on the fractions of strike-slip and reverse-slip motion would be useful in the hazard modeling. Seismicity studies show both strike-slip and reverse faulting beneath the Irish Hill, but the relative moments of these two types of faulting are uncertain owing to the shortness and sparsity of the seismicity catalog.

Although the available GPS results have large uncertainties, including potentially large modeling bias, they are consistent with a transpressional regime in which the rate of NW-SE right-lateral shear is several times larger than the rate of NE-SW contraction (SRT Table 3.3). This partitioning is in general agreement with the 70% strike-slip and 30% reverse faulting assumed in the Local Areal Source Zone of the SSC. To our knowledge, however, the net strain partitioning of the complete SSC model into strike-slip and reverse-slip components has not been closely examined. In its Recommendation (7), the SRT advises PG&E to do so.

According to Dr. Bird, "Strike-slip faulting has only been mapped outside the Irish Hills, on the adjacent Shoreline fault." Mapping of low-rate strike-slip faults is difficult, and the research is incomplete. For example, there is some evidence, not definitive, that the Los Osos fault has a significant strike-slip component. The SRT recommended further investigation of potential strike-slip faulting along the Los Osos trend and its impacts on the pattern of marine-terrace uplift (SRT Recommendation 4).

7) **Noto Earthquake as Analog**. The primary reason that the SRT objects to the use of the 1 January 2024 Noto Peninsula earthquake as a model source for the Irish Hills is that its rupture was too long to “fit” between the Hosgri fault and the southeastern end of the San Luis Range in California. This would be a problem for old-fashioned “cookbook” probabilistic seismic hazard analysis (PSHA) in which each seismic source is reduced to a magnitude and a rupture rectangle, and then these are plugged into multiple ground motion prediction equations (GMPEs) to get alternate estimate of peak ground acceleration (PGA) at the reactor site. However, it is a solvable problem. The Noto earthquake ruptured bilaterally, and the part of the earthquake responsible for PGA observations on the inhabited Noto Peninsula was the southwestern part. Therefore, a “reduced Noto earthquake” with shorter rupture, less moment, and lower magnitude could be modeled.

Here Dr. Bird agrees with the SRT conclusion that the 2024 Noto Peninsula earthquake (Noto24) fails as an analog for thrust faulting beneath the Irish Hills because the great length of the rupture (~150 km) cannot be accommodated by the SWBZ or any other reverse fault system involving the San Luis-Pismo block.

He then proposes to retain a doctored version of the Noto24 rupture as an analog event using an ad hoc procedure that is seismologically unacceptable. He would chop off the northeastern half of the bilateral rupture while preserving the high slip on the southwestern half and call it a “reduced” Noto earthquake. Such a concoction would in no way be an observed earthquake, thus defeating the purpose of the analog-event exercise. Moreover, fault slip is known to scale with the rupture length (e.g., Shaw, 2013), and his fix would violate those scaling relations, leading to an implausibly high stress drop.

a) Despite this issue, I advocate keeping the Noto earthquake as an analog for DCPP for 3 reasons:

   i) As shown by the PGA-versus-distance graph of *Todo and Stein* [2024], this earthquake produced PGA values much higher than the USGS ShakeMap model predicts. This dangerous anomaly deserves more study with respect to the following questions: Did Noto PGAs exceed those of all GMPEs, or only some? Do certain GMPEs require revision? Should the weights on different GMPEs be changed?

   ii) My goal of developing a quick and inexpensive estimate of seismic core damage frequency (SCDF) at DCPP implies that I must focus on an earthquake source (however infrequent) with a serious chance of causing seismic core damage.

   iii) The Noto earthquake was excellently recorded on multiple 3-component digital seismograms installed on firm foundations.

Dr. Bird argues for keeping Noto24 as an analog event because it had well-recorded ground motions that were anomalously high and because it provides a quick and inexpensive way to argue for high seismic risk at DCPP. These reasons beg the question of whether Noto24 is an appropriate analog for a SWBZ event. The seismological evidence indicates that it is not.

b) Actually, given the importance of SCDF at DCPP, and the importance of this Noto earthquake analog, I advocate that traditional PSHA should be supplemented by a new kind of seismic probabilistic risk assessment (SPRA) in which actual seismograms are used as models of ground motion expected at DCPP. In this approach, the earthquake moment, magnitude, rupture length, and other such statistics would not appear. The seismograms could be truncated to remove the diffuse, late-arriving waves from the northeastern part of the rupture, which may not be comparable to the California fault system. It is very likely that the part of the rupture that produced the high PGAs on the Noto Peninsula was the southwestern segment, directly under the seismograms.

As we understand this paragraph, Dr. Bird would replace (or supplement) the validated methodology of probabilistic seismic hazard assessment (PSHA) with an ad hoc deterministic analysis based on doctored versions of Noto24 waveforms. His proposal involves chopping off the contributions of the northeastern half of the rupture to the Noto24 seismograms. This procedure suffers from the same problems as the adoption of a "reduced" Noto earthquake.

c) A second part of the SRT's discussion of the Noto earthquake analog concerns different ways of scaling the recurrence interval from Japan to California. These calculations are contaminated by SRT's use of their own "corrected" rates of NNE-SSW shortening in the Irish Hills, which are 50~75% lower than the actual rate of 2 mm/a. If the correct rate is used, there is a natural scaling. For instance, the crustal shortening rate at Noto is between 10 mm/a [*Bird*, 2003] and 14~16.5 mm/a [*Ito et al.*, 2019]; thus it is 5~8× faster than the California rate. The uplift rate of the northwestern side of the Noto Peninsula is from 0.7~1.5 mm/a [*SRT Report*, 2025], which is 3.5~7.5× faster than the Irish Hills uplift rate of 0.2 mm/a. These factors are roughly equal, supporting the use of the Noto earthquake as an analog in the Irish Hills. The only thing that would not scale with horizontal shortening rate is wave erosion and coastal sediment transport, which are probably about equal in Japan and California. This helps to explain the greater prominence of fault scarps in Noto.

We stand behind the scaling and recurrence-rate analysis given in §3.5 of the SRT report, which reviews these observations and translates them into earthquake recurrence rates. The three main findings regarding Noto24 are summarized in §4.3 of the SRT report:

- <u>Finding 7</u>: Based on the Noto Peninsula uplift data, the best estimate of the recurrence interval for a Noto24-type event on the Noto Peninsula fault system is 4000-5700 yr.
- <u>Finding 8</u>: Applying the same model to the Irish Hills yields a Noto24 recurrence interval of 22,000 ± 3,800 yr, about 20 times longer than Dr. Bird's estimates.
- <u>Finding 9</u>: Owing to tectonic differences, including the geometric incompatibility of Noto24-type rupture with the faults available in the SWBZ, the SRT concludes that Noto24 is an implausible analog for reverse faulting beneath the Irish Hills.

d) Third, the SRT points out that the proper term for the analog (as I have used it) is "maximal earthquake recurrence model" rather than "characteristic earthquake." I accept this correction.

e) The SRT's final point is that, if Noto is accepted as an analog, its mean slip should be 4 m [*Xu et al.*, 2024], not the 2 m that I obtained from the USGS finite-fault solution displayed in *Toda and Stein* [2024]. However, before accepting their result it will be important to understand why the *Xu et al.* seismic moment is twice as large as the USGS moment. If their result is generally accepted, it will mean that my SCDF estimates for DCPP will be halved. However, they would still be 17~20× higher than SCDF estimates that PG&E filed with NRC in 2014 and 2024; this would still be a serious problem requiring responses from DCPP, CPUC, and NRC.

The seismic moment of the Xu et al. (2024) model for Noto24 matches the USGS estimate of 2.2×10^{20} Nm, which corresponds to a moment-magnitude of 7.5.

8) **Thoughts About SRT's Fault Model**. In one section of their report (3.3), the SRT created a "fault-related fold model" to partially explain the formation of the Pismo syncline. (Specifically, they explained the present dip of the South limb. A more complex model, involving additional slip on the Los Osos thrust fault, may be necessary to explain the present dip of the North limb.)

a) They began from an observation that the present compression in the Irish Hills area was preceded by a Miocene phase of crustal extension by normal faulting. (PG&E agreed in their 2015 SSC; so do I.) They added the hypothesis that one of these Miocene normal faults was reactivated, in reverse slip, to form the Southwest Boundary Zone thrust system. (PG&E raised this possibility earlier; I potentially agree, with the reservation that the reactivated fault dip should be no stepper than ~40° so that reactivation is mechanically plausible.)

As summarized in the SRT report, reverse fault dips steeper than 40° are both mechanically plausible and commonly observed in coastal California and elsewhere.

b) Importantly, their development of this model led to 5 implications that support my own modeling of tectonics in the Irish Hills region:

i) The SRT inferred the starting age of fault reactivation (from geologic map relations) to be 6~4.2 Ma, with preferred median age of 5.1 Ma. This agrees with my own estimate of 5 Ma for the beginning of formation of the Pismo syncline.

ii) This new SRT model includes a SWBZ thrust fault dipping 40° that connects to a "flat" section with 0° dip under the northern Irish Hills and adjacent valley. The combined mean dip of these 2 segments of the fault (leaving out deeper segments under the Santa Lucia Range) is 17°, which is even gentler than the 25° dips that I have advocated, and much gentler than any thrust dip that PG&E ever considered in any of their fault geometry models.

The SRT fault model includes a flat section inferred from the transition from the uplifting Irish Hills to the Los Osos Valley, which exhibits little subsidence except locally at Morro Bay.

Assuming the ramp participates in the rupture, the combined area of the ramp-flat model would modestly increase the rupture area and potential seismic moment relative to the PG&E SSC. It is important, however, to point out that this model geometry does not increase the slip rate of the SWBZ, which is based on the uplift rate of marine terraces in the hangingwall of the upper ramp section of fault, and corroborated by folding of the Pismo Syncline.

iii) Their model implies that, prior to 5 Ma, map unit Tmo was lower on the NNE side of the SWBZ (due to Miocene normal slip) than on the SSW side of the SWBZ. This means that my calculated thrust slip rate based on the apparent vertical offset (throw) of unit Tmo is only a lower limit for the throw in the compressional phase, with the upper limit now unconstrained.

Rather than an unlimited amount of fault throw, the throw on the SWBZ is limited by the thickness of sediments contained within the Pismo basin and preserved in the fault hangingwall. This thickness, approximately 2.5 km (Figure 2), is constrained from the continuous, conformable section of strata preserved within the south limb of the Pismo syncline. A larger amount of uplift requires a comparable amount of erosion of the hangingwall that is unsupported from the mapped geologic relationships. Further evidence for limited fault throw comes from contact relationships at the base of Upper Pismo Formation. This unit, which has been deposited since the onset of contraction, exhibits a conformable contact with the Lower Pismo Formation in the syncline axis, but spans an angular and buttress unconformity across the south limb of the Pismo Syncline and the SWBZ. The large amount of uplift and erosion of the hangingwall of the SWBZ proposed by Dr. Bird would have removed this evidence from the geology of the Irish Hills. We note that the stratigraphic thickness constraint is a crude upper limit on fault throw that is considerably greater than the amount predicted from a kinematically constrained fault model. The SRT recommended further development of kinematically constrained fault models to improve the PG&E seismic source model.

iv) They computed a mean slip rate of 0.5±0.1 mm/a for this model thrust since 5.1 Ma, with a lower rate of 0.29±0.07 mm/a for the Holocene. Thus, their model says that the present slip rate is 58% of the mean rate during formation of the Pismo syncline. This directly contradicts PG&E's assumption (in their 2015 and 2024 SSCs) that the Pismo syncline has stopped forming, and that the crust between the SWBZ and the Los Osos thrust fault trace is now behaving as a rigid block.

There is no contradiction in the SRT fault model between the marine terrace evidence, which indicates that the Pismo Syncline is uplifting without undergoing folding, and the total deformation across the SWBZ, which includes slip on a reactivated San Miguelito fault to produce the south limb of the Pismo syncline. In the SRT model, a shift from slip on the San Miguelito fault to slip on the San Luis Bay fault led to a transition from folding to block-like uplift of the SWBZ hangingwall. Note that the 0.29 ± 0.07 mm/a rate spans the Late Pleistocene since the formation of the Q2 terrace at 124 ± 5 ka. This includes the Holocene epoch since 11.7 ka.

v) Because the SZBZ thrust fault caused the Pismo syncline, this thrust must continue West as far as the syncline does, and it must connect to the Hosgri fault. This means that an active thrust fault passes directly under DCPP.

The continuity of the syncline does imply continuity of the underlying fault system that produced the syncline. This is consistent with the SW-vergent model of the PG&E SSC.

9) **Conclusions and Recommendations**. In conclusion, some of the SRT's criticisms of PG&E's work align with my own. In particular, PG&E should have used neotectonic deformation modeling and finite-strain historical modeling to develop the 2015 and 2024 SSCs. However, SRT raises "problems" in my analysis that are based on fallacious assumptions and/or faulty technical analytical methods; or debatable propositions are presented as certainties.

In my expert opinion, significant additional analyses must be conducted before any regulatory agency can responsibly approve the continued operation of DCPP beyond its initial operating license terms of 2024 (Unit 1) and 2025 (Unit 2). My recommendations for additional study are set forth below.

a) Immediate: to the SRT. Since I have now dispelled all (or most of) the primary objections to my own rough estimates of seismic hazard and risk at DCPP, I ask the SRT to seriously consider changing the primary conclusion of their draft report from "Not Plausible" to "Plausible." Such a characterization would not be an actual endorsement of my views, but it would be a step back from their present unreasonable position of outright rejection.

Dr. Bird's response to the SRT report has not dispelled our objections to his estimates of the DCPP thrust-faulting hazard. We have carefully reviewed his response but found no new observations or theoretical considerations that would justify changing our overall assessment (Box 1). The main assertions of the Bird critique are not supported by the scientific evidence and should be discounted as arguments for high thrust-fault hazard at DCPP.

b) Longer-Term: to DCISC, CPUC, NRC, and PG&E. The criticisms of PG&E's 2015 and 2024 SSCs raised by my previous filings (and, in some respects, echoed by the SRT) are serious and create uncertainty about whether continued operation of DCPP is acceptably safe? Therefore, it seems appropriate to advocate updated (or completely new) SSC and SPRA studies that would include, at minimum:

i) Alternative fault geometry models, including my own (i.e., 2 conjugate thrust faults dipping ~25°) and also the new "fault-related fold model" by the SRT (one master thrust fault with dip varying locally between 40° and 0°).

ii) Consideration of whether the "fault-related fold model" by the SRT should be supplemented by any alternative models of finite strain since 5 Ma?

iii) Deformation modeling of neotectonics for each fault geometry model, using the constraints of GPS velocities, stress directions, and geologic limits (if any) on fault offfset rates (such as throw rate), to determine best-fitting neotectonic slip rates for each fault, and also permanent (non-elastic) strain-rates for the crust between modeled faults, which may also generate earthquakes.

Dr. Bird's recommendations (i)-(iii) overlap with the SRT recommendations regarding fault-system (§4.6.1) and geodetic modeling (§4.6.2), and they are reasonable suggestions for general research topics that should be pursued under PG&E's long-term research program. Whether PG&E should launch a formal update to SSC24 is a question that should be addressed after the sensitivity studies recommended by the SRT (§4.6.3) have been completed.

iv) A supplemental (or completely new) Seismic Probabilistic Risk Assessment for DCPP including actual 3-component digital seismograms from the 1 January 2024 Noto Peninsula earthquake (with masking to eliminate late-arriving waves from the northeastern half of the rupture), to determine the probability of seismic core damage (SCD) when a similar event (with a shorter thrust trace) occurs under the Irish Hills.

The SRT does not recommend using the Noto24 waveforms to directly drive the DCPP SPRA for two salient reasons: (1) Noto24 is not suitable as a characteristic earthquake for thrust faulting beneath the Irish Hills, and (2) we are aware of no validated methodology for directly assimilating event-specific (deterministic) waveforms into probabilistic risk assessment. Proper handling of the large uncertainties in hazard estimation dictates that any new empirical hazard information, such as the Noto24 seismograms, should be integrated with existing information at the hazard-modeling stage (PSHA), not the risk-modeling stage (SPRA). Of course, deriving insights about DCPP hazards from well-recorded earthquakes around the world, including Noto24, remains an important research goal.

III. Conclusions

The SRT has reviewed the documents constituting the Bird critique, including Dr. Bird's response to the SRT report submitted to the DCISC on June 6, 2025. We conclude that no evidence or assertions presented in the Bird response require changes to the fifteen major findings and nine major recommendations listed in §4 of the SRT report.

Although we have discounted the main assertions of the Bird critique, we agree with Dr. Bird that the thrust-fault hazard to DCPP deserves special emphasis in future SSC model development, and we note that several of Dr. Bird's recommendations regarding fault-system and geodetic modeling are consistent with the SRT recommendations. This suggests a consensus on which research topics should receive priority in PG&E's long-term research program.

One question raised by Dr. Bird is whether enough new evidence has been gained about DCPP seismic sources to warrant a formal update of 2024 Seismic Source Characterization (SSC24). It remains to be seen whether the implementation of the SRT recommendations will modify the hazard estimates in any meaningful way. Therefore, the SRT suggests postponing that decision until after PG&E has conducted the sensitivity studies recommended in the SRT report.

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