TECHNICAL RESPONSE TO THE *REVIEW OF THE BIRD CRITIQUE* WITH RECOMMENDATIONS FOR FURTHER ACTIONS

Submitted to the Diablo Canyon Independent Safety Committee
on behalf of San Luis Obispo Mothers for Peace, Friends of the Earth,
and Environmental Working Group
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I. 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.

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).

II. 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.

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").

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. 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.

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.

III. DISCUSSION

1. 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."

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 $\boldsymbol{T}_{e}$ … can be approximated by the thickness of the seismogenic zone," the SRT reveals a core incongruity in the SRT's reasoning: any seismogenic 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 strong; therefore, local Airy compensation cannot always be expected in small areas."

However, even this non-expectation is contradicted by the actual isostatic gravity anomaly map published by PG&E; since the anomaly is negative (-10 mGal to -23 mGal; Figure 3 in *Langenheim et al.*, 2012; Figure 6-2 in PG&E's 2024 SSC), there is less 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.

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.

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.

2. 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.

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.

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. 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.

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:

(1) During Miocene normal faulting, the N-side outcrop of Tmo would have been displaced down relative to the S-side outcrop. This means that the net throw during Pliocene-

Quaternary thrust faulting would be larger than I assumed, and the thrust slip-rate also greater, with no upper limit.

(2) 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.

3. 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.

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

(1) What is "Byerlee's Law" friction? 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.

(2) 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.

(3) 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.

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.

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

(1) 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).
(2) 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.
(3) 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.

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.

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

2. 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%.

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($\boldsymbol{x}/\boldsymbol{d}$), where $\boldsymbol{x}$ is distance from the fault and $\boldsymbol{d}$ 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.

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.

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.

<u>Second</u>, 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.

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.

<u>3. Partitioning shortening between thrusting and strike-slip.</u> 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.

<u>5. NOTO EARTHQUAKE as ANALOG</u>

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.

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

(1) 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?
(2) 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.
(3) The Noto earthquake was excellently recorded on multiple 3-component digital seismograms installed on firm foundations.

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.

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.

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.

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.

IV. 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.)

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.)

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

(1) 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.
(2) 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.
(3) 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.
(4) 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.
(5) 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.

V. CONCLUSION 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, SRG 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.

1. 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.

2. 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:

(a) 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°).
(b) Consideration of whether the "fault-related fold model" by the SRT should be supplemented by any alternative models of finite strain since 5 Ma?
(c) 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.
(d) 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.

VI. FIGURE

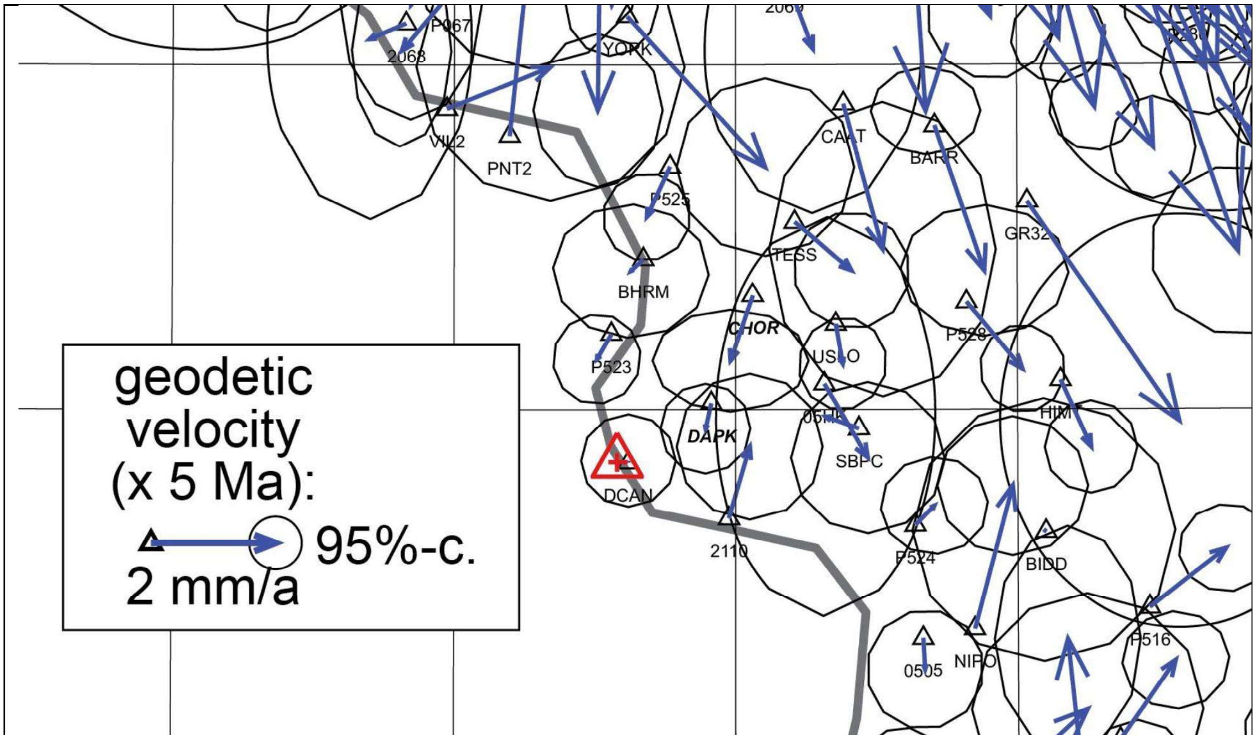

Figure 1. Detail from a map of interseismic GPS velocities from the dataset of *Zeng* [2012]. The red triangle by the (very approximate) coastline marks the site of DCPP. Stations framing the Irish Hills include DCAN, 2110, and CHOR; station DAPK is approximately in the center. The velocity reference 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.

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