Staatstoezicht op de Mijnen
Ministerie van Economische Zaken en Klimaat

Bijlage B

Externe beoordeling
CLG - Aardwarmtewinning en Seismiciteit

c/o
Swiss Federal Office of Energy
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Dear ,

Thank you for your email enquiry of 17 April 2019 where you have asked about my opinion on two reports that have been furnished to you by Californië Lipzig Gielen Geothermie BV.

For the report on the "*Interpretation of the Earthquakes Near the Californië Geothermal Site: August 2015-November 2015*" you have specifically asked my opinion on:

* The relationship between the earthquakes and the operation of the CWG and CLG geothermal systems separately.
* The plausibility of the proposed mechanism causing the earthquakes.
* My high-level expert opinion if the most logical possibilities which could have caused the earthquakes are investigated or if there are other plausible causes which are logical to investigate in this setting too.

For the report "*Seismic Hazard Assessment for the CLG-Geothermal System – Study Update March 2019*" you wanted to know my view on the plausibility of the Seismic Hazard Assessment (SHA), especially in the circumstances that the proposed TLS levels has been activated already once at the highest level.
While you consider my expert opinion to be a comprehensive assessment of the plausibility of the documentation provided, my assessment cannot be viewed as a basis for any of SodM's decisions. Instead, it will be the sole responsibility of SodM to identify the suitability of the SHA for this region. You have ascertained that I cannot be held liable for my view on the plausibility of the report and the SHA. You also accept that this will be my personal view and not that of the Swiss Federal Office of Energy.
In Appendix 1 you will find my opinion on the report titled "*Interpretation of the Earthquakes Near the Californië Geothermal Site: August 2015-November 2015*" and in Appendix 2 my opinion on the report *Seismic Hazard Assessment for the CLG-Geothermal System – Study Update March 2019*".

Thank you for giving me the opportunity to voice my opinion.

Yours,

Appendix 1:

The report “Interpretation of the Earthquakes Near the Californië Geothermal Site: August 2015 – November 2018” summarizes in 5 chapters and 5 appendices an interpretation of earthquakes in relation to geothermal operations furnished by Q-Con GmbH (a service contractor specialized in the domain of induced seismicity). The geothermal field operator is Californië Lipzig Giele Geothermie BV.

Chapter 1 provides a brief, half-page background mentioning a felt earthquake, the presence of a seismic monitoring system, and draws attention to two prior studies addressing the correlation of seismic events with geothermal operations. This report draws on the studies and develops a conceptual dynamic reservoir model to investigate the presumed geomechanical processes that may have operated and resulted in earthquakes. **Chapter 2** provides the “Summary” of the report.

Chapter 3 recounts the observed earthquakes recorded by the operator’s local seismic network since its start of monitoring on 31 August 2014, and the extension of the network as of November 2015. The report demonstrates in a convincing manner that inaccuracies in (and uncertainties associated with) the determination of hypocenter locations are substantial. Nonetheless, the study presents plausible arguments that hypocenters of observed earthquakes are below the geothermal reservoir(s) utilized via CWG and CLG wells. Interpreted depths of observed hypocenters are poorly constrained which poses difficulties in eliminating some of the hypothesis on the geomechanical causes of the earthquakes. The argument that hypocenters cluster on a lineament or plane, which is brought forward using master-event relative location techniques is plausible and is consistent with the presence of a major fault structure.

While some observations and interpretations are consistent with a strike-slip mechanism, it is important to recognize that neither additional kinematic (e.g. information on the fault architecture, tectonic history, etc.) nor dynamic indicators (e.g. GPS or interferometric synthetic aperture radar to monitor ground deformation, historical earthquake data, and so on) or the plausibility of such a focal mechanism is addressed.

Chapter 4 presents strong arguments in terms of a spatio-temporal correlation between geothermal field operations and the observed seismicity. This robust observation and the fact that seismic activity occurred when production and injection was ramped down or altogether stopped, are the basis for any explanation regarding possible causes or operative mechanism.

Unfortunately, there are no additional data presented and interpreted. Depending on the operator’s needs, conditions of the permit, reporting requirements to assure regulatory compliance, such data sets may include downhole pressures per well, pressure fall-off or build-up data in production or injection wells, temperature and pressure logs at static or dynamic conditions, spinner logs, production chemistry data, and importantly pressure interference data among and between wells. Also, at times data or information regarding reservoir characterization (tracer data, geophysical data etc.) is available. Lack of reservoir monitoring makes any interpretation and speculation on possible or probable causes for induced seismicity highly uncertain.

The overall low number of earthquakes and the calculated and observed low magnitudes do not avail themselves to identify the nature of causal relationships between the geothermal operations and the observed seismicity. Still, the report convincingly suggests that the consistency of the observed seismicity is linked to variations in pore pressure (caused by lowering production rates to shut-in) in production and injection wells. Interestingly, peak rates of production and injection and thus presumably peak differences in pore pressures among wells do not correlate with induced seismicity.

In conclusion, the hypothesis of a causal relationship between the earthquakes and the operation of the CWG and CLG geothermal systems appears likely and thus represents a plausible working hypothesis. The observation of earthquakes 1 to 6 in relation to variations in operational modes of the

CWG doublet, the spatio-temporal correlation of earthquake #8 with CLG operations, as well as the earthquake sequence #9 to #17 are plausibly connected to a re-equilibration of the state of stress within a few days after the shut-in of CWG and CLG operations. However, there is no further analysis (repeating earthquakes etc.) presented and no further inference can be made.

Against the backdrop of very few data, **Chapter 5** provides some interpretation and conceptually some causal relationships. A word of caution: there are very few earthquakes, absolute hypercenter and epicenter locations are inaccurate, and there are large, unquantified uncertainties in the data set relevant for earthquake activity.

The authors quote their earlier studies to suggest that there are only two possible mechanisms that may give rise to induced seismicity: *elevated injection pressures* and *thermal contraction*. The authors do not address or mention other contributing factors that may give rise to induced seismicity.

Elementary rock mechanics already suggests that a fault can be brought to an unstable condition (an earthquake) through an (i) increase of shear stress, (ii) a decrease of the normal stress, (iii) an increase of fluid pressure P, or some combination of the three. The authors focus on increased fluid pressures, and pore pressure diffusion throughout the reservoir and fault structures (Chapter 5.2).

Changes in pore-fluid pressure, in addition to reducing or increasing effective normal stress, also (iv) directly alter both shear and normal stresses through poro-elastic effects. The authors, however, do not mention this possible consequence of loading both, within the reservoir and outside of it.

The fact that earthquakes are (v) triggered in the far-field, where static stress changes are tiny, provides strong evidence that dynamic triggering can also be important. Just to mention in passing studies suggest that such dynamic triggering may account for 15%-60% of events in the near-field. Proposed mechanism(s) to explain dynamic triggering include increases in pore-fluid pressure facilitated by deformation-induced permeability increase and dynamic weakening of fault gouge, leading to a decrease in coefficient of friction.

The authors also briefly address in Appendix C other geomechanical mechanisms such as (vi) tectonic driving forces, (vii) hydraulic overpressures by injection and (viii) reservoir depletion; none of those mechanisms appear to the authors as applicable. Conceptually, the reasoning to disregard those mechanisms appears plausible.

In addition to pressure changes, changes in the tensor stress caused by (ix) expansion or contraction of the reservoir may also be important; changes in the stress tensor may lead to changes in normal and shear stresses on potential rupture planes even in regions where the fluid pressure does not change. The authors investigate this possibility via numerical simulations yet do not describe the details of their approach in this report.

> *A small excursion on the topic: Without recourse to the 2015 internal studies, a back-of-the-envelope calculation suggests that the CWG system has produced on average over 4 years (34'000 hours) about 250 m^3 per hour resulting in approximately 8'500'000 m^3 of geothermal brine whose thermal energy has been extracted to lower brine temperature from around 75°C to about 35°C for reinjection. Similarly, CLG produced over the period of one year (8'500 hours) about 200 m^3 per hour resulting in about 1'700'000 m^3 of geothermal brine at a temperature of about 85°C, to be subsequently reinjected after heat exchange at about 40°C. The reinjected brine cools the surrounding rock mass. Assuming an average porosity in the reinjection well of about 3%, the rock volume (assuming a cylindrical volume) cooled by the reinjected geothermal brine at 40 °C covers a volume of 0.35 km^3. Further, assuming a heat capacity of 4.2 kJ per kg per °C for water, an average density of the host rock of 2'200 kg/m^3 and a thermal heat capacity of the host rock of 0.8 kJ per kg per °C, the rock is cooled by about 3°C. Assuming a thermal heat expansion coefficient of $3*10^{-5}$ per °C, the rock, if*

uniformly cooled, would contract by about 10^{-4} or 0.01% resulting in very small volume changes relative to the rock mass and volume approximately subjected to cooling stresses (a volume reduction of 35'000 m^3 compared to 350'000'000 m^3 of rock volume). Relaxation of thermal/cooling stresses (on average induced by temperature variations of 3 °C, locally possibly greater, is accommodated by strain at small scales (possibly even earthquakes) – but, the relevance of thermal cracking or fracturing (and thus potential earthquakes) is subject to scientific debates with field observations circumstantial and cause & effect poorly constrained.

Returning to the possible cause of increased pressure (iii), the authors use simple dynamic reservoir models to test scenarios how far pore pressure perturbs the state of stress on the Tegelen fault structure and also invoke thermal/cooling induced stresses (ix). The proposed mechanism causing the earthquakes are to some extent plausible, but whether the mere combination of increased pressures and contraction strains are probable or likely causes cannot be established. The plausibility rests on conceptual reservoir models that are very poorly constrained. The authors do not present any additional data that support their conclusions.

It is interesting to note that the authors do not address poroelastic stress changes in response to production and injection in their scenario. Such stresses are known to induce earthquakes at the boundaries of reservoirs and formations surrounding reservoirs.

Finally, the authors have made no attempt to propose tests or observations that may shed further light on possible causes (see also 2nd paragraph in the discussion of Chapter 4).

Appendix 2: The Seismic Hazard Assessment for the CLG-Geothermal System – Study provides in nine chapters an update as of March 2019.

I have not consulted the 2015 Seismic Hazard Assessment for the Extended Geothermal System Californië.

First, I will provide my view on the deterministic analysis of the stress change. Subsequently I will give my view on the report within a framework of possible hazards, barriers to lowering the probability of a top event from occurring (a damaging or felt earthquake), and associated tasks that may be subject to possible compliance testing/assurance. Finally, I will address the fundamental question of the plausibility, which follows a contextualization of the deterministic analysis.

At the core of the update is a deterministic analysis of the stress changes associated with geothermal production, which the authors lay out in conceptual terms in **Chapter 4** of the report. The basis for this analysis is the existence of a fault capable of hosting a felt or damaging earthquake. The authors focus on two mechanisms that give rise to earthquakes, elevated fluid pressures and accommodation of thermal/cooling stresses by (earthquake) slip. Other mechanisms (mass changes, poroelastic stress changes, chemical processes presumably weakening fault rock and rocks, and stress perturbations of, for example, a dynamic nature (waves from passing earthquakes) are not considered.
The authors offer in the case of the CWG and CLG geothermal systems, neither site-specific discussions nor convincing site-specific observations, nor consistency argument and appropriate models to rule out the mechanisms.

An important argument put forward in Chapter 5 is the treatment of CLG as a “mass-balanced geothermal system”. However, the authors do not explain in a sufficient manner the presence, implication and relevance of such a system at this location, in the absence of documented pressure interference tests (observation, analysis and interpretation), tracer tests etc. at the CLG geothermal system. Note that noticeable pressure interference is a necessary but not sufficient proof for the presence of a “mass-balanced system”. Instead, the authors extrapolate observations from an extremely limited data set of other geothermal projects, to the behavior such as the expected seismicity at the CLG site. The authors do not explain in a cogent manner why beyond a small sub-set, other geothermal systems located in sedimentary basin systems are not included in the analysis.

The construction of a simplified kinematic reservoir model to account for the mechanical response of the subsurface in **Chapter 6** (6.1 – 6.4) is a pragmatic zero-order hypothesis to describe the propensity of faults (and/or reservoir) to nucleate an earthquake. The authors make a consistent qualitative argument (but, no details are given regarding the dynamics, i.e. detailed stress and strength distribution in the modeled region) to link the observed induced seismicity to the current, hypothesized state of stress on the fault systems.

To be clear, neither the model nor actual field observations present conclusive arguments that, indeed, the two favored mechanisms (elevated fluid pressures and thermal/cooling stresses) are the culprits for the induced earthquakes. Circumstantial observations lead the authors to the speculation, which, while plausible, is not presented in a persuasive manner to be a firm foundation for a forecast or prediction for the geothermal system’s future behavior.

Instead, the coupling to a simplified dynamic reservoir model (Chapter 8) is a useful first step towards developing a range of scenarios that – in a qualitative manner - suggest the consequences of a sustained longer-term operation of the CLG system. The results, which I would classify as scenario results, are plausible regarding the impact on elevated pore pressures (8.1) and cooling stresses and regarding thermal/cooling stresses and release by contraction (8.2) using a semi-analytical approach. The authors do not explain in sufficient detail (8.3.1.) why the Kaiser effect applies to injection in the fault zone. Progressive injection (or production) from a fault zone may be expected to progressively affect an ever larger fault zone area/section in a spatial sense. The Kaiser effect is expected to be operative in regions where rock or fault zone rock may have been destabilized and have hosted earthquakes during earlier injection phases. However, continued injection (or production) may affect virgin rock or fault zone rock.

While the conceptual incorporation of elevated pore pressures in the injection zone is perfectly plausible, the incorporation of the potential stress rises in the fault rock (and surrounding formation) as a response to a temperature difference is not entirely clear. At a conceptual level, the generation of thermal stresses in the solid rock/fault mass by injecting relatively cool geothermal brine is plausible. However, such stresses are of a local nature and effective relief mechanisms (strain) operate also at a small scale (cracking along grain boundaries – scale). The authors do not make a convincing argument how an averaged, pervasive temperature drop by few degrees Centigrade in the rock mass allows the unrelieved build-up of sufficiently high stresses, which, upon failure of the fault or rock mass, are subsequently relieved in an earthquake.

I find the conclusion that no further earthquake activity is expected to be a somewhat bold statement. Fundamentally, any subsurface activity may result in seismic activity, be it not-felt, felt, associated without damage or in fact, damaging – even small acid jobs may cause a large number of earthquakes that are observable with an appropriate seismic network.

Based on a "deterministic" scenario, the authors then focus on mitigation measures that center on current industry practice, the establishment of site-specific peak ground velocities (PGVs). The PGVs are used to set thresholds for two levels of permissible values. Sensibly and plausibly, they are defined by lower levels of PGV where humans (0.3 mm/s) may feel earthquakes. An empirical benchmark is the recorded PGV of 1.1 mm per second for the M_L = 1.7 earthquake (which occurred on 3 Sep 2018). The authors do not discuss whether a potential "nuisance" earthquake (perceptible) is a suitable threshold instead of Dutch engineering standards, which suggest that damages to ordinary buildings is unlikely at PGVs below 5 mm/s. The authors use the ML = 1.7 magnitude to suggest that a threshold in terms of magnitude ought to be set at M_L=1.4. This approach obviates the need to derive site-specific attenuation equations that enable the interpretation of measured velocities on surface in terms of observed magnitudes as a function of depth. In all, this approach may be described as conservative, maybe overly so, because it remains unclear whether the (stakeholder) issue is "felt" seismicity or "damaging" seismicity. The authors conclude this chapter (Chapter 8) with a qualitative risk assessment where they couple an expected severity of impact in relation to a ML= 2.4 to be a borderline "unlikely/credible" event.

In terms of conclusions (Chapter 9), the authors emphasize the use of a deterministic model that underlies the suggestion that continued production at rates less than 200 m^3/hour (some 60 l/s) will avoid earthquakes on known faults. Consequently, the authors suggest some specific thresholds for traffic light systems. These are all plausible suggestions if one were to follow the logic of the authors.

However, I suggest that the study does not exhaustively treat the seismic hazard within a framework of possible threats and hazards. Also, the study does not expand on all options to identify barriers which may lower the probability of a top event (that is, a felt or even damaging earthquake), and finally the study stops short of suggesting associated safety critical tasks that may be subject to possible tests for regulatory compliance.

I identify a number of potential threats, some of which the authors have suggested or have alluded to.

(1) First and foremost is the issue of "potentially reactivating a major fault". The fault may be known or blind. The field operator and consultants have identified known faults appropriately and have presented arguments why the faults are stable in case of production, and, under what circumstances the major fault zone may remain stable in case of injection. What is less satisfactorily explained, is why invoking a Kaiser effect may be appropriate for previously undisturbed fault zones or patches, which will be accessed as injection (or production) continues.

In addition, when considering earthquake triggering, the time delay between a triggering event and the occurrence of an earthquake may also be viewed as a time-dependent failure property of earthquake-prone faults. Consequently, rheological properties of a fault are often described in terms of rate-and-state-dependent friction laws that effectively represent the time-dependent characteristics of fault slip and occurrence of earthquakes. Already very small perturbations in the stress field (from geothermal operations or operations-induced seismicity) may in turn trigger earthquakes. While local seismicity records do not suggest otherwise, there is still considerable uncertainty about the nature of e.g. Tegelen fault zone. This would call for monitoring of the fault.

It is unclear why the authors do not expand on additional barriers (beyond a simplified dynamic reservoir model coupled with a geomechanical module and an “elementary” approach to a traffic light system). Of course, subject to careful attention to the value-of-information and a cost-benefit analysis, the authors do not describe or expand on additional potential barriers for reactivating the Tegelen fault zone. The barriers listed below are by no means exhaustive or considered to be effective. The selection serves to think of additional barriers beyond simple traffic light systems:

- One barrier, which appears to be under-explored, relates to further characterization of the Tegelen fault zone from a geological/kinematic and rheological/dynamic point of view. Stress measurements as well as direct and indirect characterization of the strength of the fault may provide valuable insights, as might detailed investigations into the transport properties of the fault zone (in-situ and possibly in the laboratory). A more detailed understanding of the topology, rheology and transport characteristics will enhance confidence in dynamic reservoir models of the reservoir and thus support production scenario development, as well as history matching and expected production performance.

- A second barrier may be developed via an improved understanding of the fault zone (and surrounding rock mass) in response to production by higher accuracy and higher precision (e.g. seismic) monitoring of the fault zone. One of the goals is, for example, to detect and confirm the slip tendency (or proximity to failure) of the zone. By doing so, and in particular in conjunction with comparatively low-cost options (low-cost when compared to potential permanent shut-in of wells) such as specific types of vertical seismic profiling or walk-away seismic profiles, one obtains potentially valuable sets of data and information. Additional information may also help resolve absolute (and by extension) location uncertainties and eliminate some of the hypothesis on possible causes for earthquakes.

- A third barrier may be improving the resolution of the seismic survey and applying advanced techniques to analyze signals to extract more information from seismic records (e.g. template matching). More data may also lend themselves for statistical analysis of induced, triggered or natural earthquake sequences, and further the development of and extensions to traffic light systems. Let it be understood, that applying such techniques to geothermal field operations is at the leading edge of research and innovation. Hence, these techniques may not yet be suitable for robust and routine “industry-standard” approaches to field operations. Hence, such novel techniques may not be suitable for assurance and regulatory oversight. However, the application of novel methods may demonstrate the willingness of the operator to go to certain lengths to minimize the risk of inducing damaging (if not, felt) earthquakes.

- While alluded to, the authors do not exhaustively address a forth barrier that may prevent unstable slip and felt/damaging earthquakes. Elevated pore pressures are arguably and plausibly a prime driver for induced seismicity. Hence, setting high and low limits to absolute (relative to the initial) pore pressure differences, and also setting limits on peak-to-trough pore pressure gradients across the reservoir’s injection and production zones, may be a useful barrier that prevents the top event from occurring. With increased understanding of the reservoir and its behavior, one may gradually relax limits. The authors do not expand on this point in sufficient manner, and also do not discuss the merit (or not) of setting limits on injection and production rates. In my opinion and from a conceptual point of view, setting limits on production and injection rates may prevent the operator from seeking ways to improve inflow performance of production and injection wells. It has been suggested (Fryer et al., 2018 and 2019), albeit only conceptually, that lowering the draw-down related to production by well stimulation lowers the poroelastic stress build-up in and around the reservoir and thus lowers the likelihood of induced seismicity.

- Another barrier may be developed by designing a number of production- and injection ramp-down scenarios and modeling their expected impact on the mechanical response of the reservoir and its surroundings.
- There are additional possibilities to improve barrier performance such as independent scrutiny of production and injection profiles.
- Potential escalation factors such as unstable growth of slip patches to unexpected dimensions are, of course present and need to be considered in any barrier development (e.g. quality of detection of a seismic monitoring network).

(2) A second threat derives from reactivating unstable areas around the CWG site in response to CLG continued operations. One can imagine a number of potential barriers such as precise knowledge of the location and the geological context (high-resolution static reservoir model) and dynamic communication among all wells as derived from pressure interference tests. One may envisage as a barrier, pressure monitoring in wells of the CWG site to improve the dynamic understanding of the entire CWG and CLG system, and thus the entire reservoir.

(3) A third threat, identified by the authors, relates to extensive cooling of the reservoir, associated stress relief and possible unstable fault slip on the Tegelen fault zone. Beyond developing a conceptual understanding by numerical modeling, the authors may explore ways to develop and test additional barriers. If the CLG system is not a mass-balanced system, what is the effect of extensive cooling – do divergent flow paths render other segments on known/unknown faults and fault segments unstable?

(4) While in recent history not observed, another threat derives from naturally occurring earthquakes and a multitude of scenarios of interactions with geothermal field operations, particularly with respect to the Tegelen fault stability. Barriers may include continued comprehensive baseline monitoring possibly refined and in conjunction with the Netherlands Seismic and Acoustic Network of KMNI; and continued characterization to help distinguish natural from induced earthquakes, and to identify any possible triggering of natural earthquakes by induced ones.

To summarize, the authors have executed the update of the seismic hazard analysis in a satisfactory manner. By incorporating, albeit briefly and in a mostly qualitative manner, aspects related to risk management of induced seismicity, the authors have also provided useful input in identifying and managing some aspects of the risks associated with induced seismicity. However, while Chapters 8 and 9 address a few key barriers, the authors do not argue in a convincing manner why a number of other barriers have only been alluded to or not mentioned. Such barriers allow both, the operator and the regulatory oversight authority to monitor the risk and impose additional precautionary measures to avoid the risk of a damage or felt earthquake.

The lack of subsurface knowledge, analysis and interpretation suggest that strong statements regarding the likelihood of additional earthquakes (Section 8.3.1), however cushioned with caveats, are premature. In my experience, virtually all geothermal operations are associated with earthquakes, certainly below the threshold of human perception and increasingly less likely “felt” or “damaging”. Careful analysis, understanding of the value-of-information and cost-benefits analysis of barriers to prevent top events from occurring, and recovery measures to minimize consequences, will enable operators and authorities to arrive at an appropriate level of risk management. The authors have presented a slim selection and have not convincingly argued that “within the framework of [their] subsurface model (“expected case”), [they] therefore conclude that no induced seismicity is to be expected in the future production scenario.” In the absence of a convincing argument brought forward by the authors, my working hypothesis therefore is that TLS thresholds will be triggered, that triggers need to be handled appropriately with the ultimate purpose of avoiding permanent shut-in because TLS threshold have been triggered. The certainty in forecasting to what extent will increase with years of monitoring and experience.

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