REVIEW OF MIDAS GOLD REPORTS ON SITE-WIDE WATER CHEMISTRY AND GEOCHEMICAL MODELING

With reference to associated reports: “Stibnite Gold Project Proposed Action Site-Wide Water Chemistry (SWWC) Modeling” prepared by SRK, dated December 13, 2018

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Summary Comment

In this review I have focused on the water chemistry and geochemical modeling, consistent with my expertise. I noticed that predictions were being made for up to 100 years in the future using the USGS code MODFLOW, but without a 100-yr history of water levels to calibrate with, the latter being the usual approach by groundwater modelers (Anderson et al., 2015; Bredehoeft and Konikow, 2015). I also did not notice any pump test information, but I did not make a thorough search and I might not have those reports. Furthermore, the model results cannot be tested or confirmed because we will never see data from 100 years in the future. Testing hypotheses and theories is a fundamental characteristic of science. Hence, this modeling is not really science and should be called scenario estimation (Greenwood, 1989). Scenario estimations, like models, are not unique, they involve many assumptions (often guesswork), and are much more uncertain than models.

This report is extraordinarily extensive and comprehensive and has utilized substantial amounts of data and technical expertise. It contains a great deal of information that will be important for site planning and execution.

Because this site is complex with limited data, and because sensitivity analyses have not yet been attempted, it is difficult to evaluate the reported future scenarios. Some effects of seasonal variability have been considered, which is very helpful. However, the constraints on the system are few and a future meaningful quantitative scenario is not possible. I understand the challenges of limited data, the complex heterogeneities of the site, the issue of not overly perturbing a site until the environmental consequences are known, etc., and the need for some assurance that the consequences of mining will not create intolerable changes in water quality. A much better idea of the usefulness of the geochemical work in this report will be apparent when the sensitivity and uncertainty analyses have been completed (ongoing work, Section 12).

The basic questions I have tried to address are:

1. Are there serious issues with the “geochemical modeling” that affect the conclusions?
2. Are there other considerations not presented in this report that should be mentioned?
3. Does the report support a qualitatively useful scenario?
4. Does the SWWC report adequately address the original objectives?

Overall Technical Comments

1. The conclusions from this report rest partially on the results of static and kinetic tests. The usefulness of these tests is still under debate and not adequately confirmed against actual field data for a variety of conditions. This fact along with the fact that nearly every mine site has different geologic, hydrologic, and mineral processing conditions and that HCTs are not run similarly to field conditions and should be run for at least 2 years, raises serious concerns in general about their applicability.
2. Kinetic tests such as HCTs may give an estimate within one or two orders of magnitude or not. They (and static tests like ABA) are most reliable when the mineralogy alone clearly shows that the rocks or tailings are dominated by sulfide minerals or by carbonate minerals (Dold, 2017), and then the tests aren't really needed. The serious limitations of these tests have often been pointed out (Parbhakar-Fox and Lottermoser, 2015; Maest and Nordstrom, 2017; Davis et al., 2019) and yet both industry and regulators rely on them. What is seriously lacking are examples of how these tests have compared to actual waste rock drainage after 10-20 years of monitoring. Short-term comparisons of pilot scale leaching of waste rock with static and kinetic tests indicate that they may compare within 1-2 orders of magnitude at best (e.g. the Diavik results from the Waterloo studies headed by David Blowes). They can also be worse than this example, depending on how long and how well the tests were carried out, and how representative the samples were. Part of the problem of defining the applicability to field conditions is defining how to scale up from small-scale leaching to full-scale field conditions. There is no general agreement on this issue (see Nevada Division of Environmental Protection, NDEP, 2018), just as there is no general agreement on a standardized test procedure – which is understandable because waste rock and tailings differ substantially from site to site as well as the hydrology and mineral processing methods. These issues are still being debated in the research literature and make it difficult for a consultant to know how much credence to give to these procedures and for reviewers to know how meaningful they are.
3. There are major assumptions used in the geochemical modeling regarding solubility controls on major and trace elements. Some of these assumptions are completely dubious because (1) they have never been shown to be effective, (2) it is not clear why just choosing a solubility control because it reaches saturation (according to the mineral saturation index calculated from the leachate chemistry) in some samples is strong enough evidence for equilibrium solubility (kinetics are equally important) and (2) no testing of the computer code and database was provided to demonstrate that calculations are meaningful. Some other assumptions are reasonable, but they are applied to HCTs and not actual groundwater analyses, so they may not be meaningful.
4. I did not have access to the groundwater and surface water chemistry reports done by HDR (HDR 2016 and HDR 2017) which made it difficult to comment on the interpretation of how mixing of groundwater with pit water and surface water might affect contaminant movement.
5. Some useful guidelines could have been gained from the NDEP (2018) and Nordstrom and Nicholson (2017) references regarding geochemical modeling.

Executive Summary section: Comments

p. ii-iii, **Development Rock Storage Facilities**

Large assumptions are made about the proportion of infiltrating water that goes to groundwater compared to the amount the appears as seepage. Because it is so difficult to know these amounts in advance, it becomes necessary to consider the consequences if these amounts differ by a large margin like 50%. One example of where a sensitivity analysis would be helpful.

According to this report, the larger proportion of infiltrating water from the DRSF will go to groundwater. The problem that arises is that in a mountainous terrain, the contaminated groundwater will discharge to the streams and rivers downgradient. Depending on the contaminant concentrations and attenuating factors, this transport could gradually degrade water quality downstream. Monitoring with ability to capture and treat would be required to protect surface water systems down drainage.

It is stated that solute concentrations are inversely related to flow. There is an important exception to this generalization. During the early part of snowmelt or a major rainstorm after a prolonged dry period, a flushout of soluble minerals and evaporated pore waters containing high concentrations of contaminants occurs. This transient event often causes fish kills in mining areas. To prepare for this event a larger capacity for transitory high flows will have to be constructed. A similar scenario is likely with the tailings storage facility seepage. Berms may not be satisfactory to hold these transient events and spillover could contaminate receiving streams.

Are the secondary standards referred to, the EPA secondary drinking water standards?

p. iv, **Operational Pit Dewatering**

Using static and kinetic tests to estimate pit water chemistry is not a very reliable method because lab tests have little in common with field conditions and no one has shown the degree to which this approach actually works. Natural analogues might be helpful here if they can be found. It seems very strange that aluminum concentrations are considered to be high in the baseline groundwater. Perhaps there was an issue with filtering colloids in the HCT sample collection.

p. iv-v, **Post-Closure Pit Lakes**

There are a considerable number of uncertain assumptions that are summarized in this section. For example, the dewatering water is anticipated to be elevated in arsenic, iron, manganese, and antimony relative to groundwater quality standards. Then, the report states that “This [what?] is a function of the presence of these constituents in baseline groundwater surrounding the pits.” One problem here is that I have not seen the water chemistry data on the groundwater (other than a few averages) surrounding the pits and its interpretation. The degree to which the pits fill up with surface water relative to groundwater flow is very important. If largely surface water (and it could be engineered this way) then it dilutes potential contaminants and fills more rapidly than by groundwater that would add more contaminants from both baseline and pit wall reactions.

p. vi, **Site-Wide Surface Water Chemistry (SWWC)**

The main concern here is that so much of the contaminant transport is dependent upon highly uncertain kinetic leach tests (HCTs) and there has been no sensitivity analysis to determine what the SWWC is most sensitive to. Several sources of uncertainty without some kind of sensitivity or uncertainty analysis makes it difficult to put an error bar on the final conclusions. Otherwise, the statements here are mostly reasonable and highly qualitative because of the paucity of data.

p. vii, **Potential for Mercury Methylation**

While it is generally true that there is not a quantitative relation between the amount of total mercury or even total dissolved mercury and methylmercury, it is also true that the concentration of methylmercury is what is important for toxicity, not the fraction of the total mercury. By saying that the amount of methylmercury will "probably fall within a similar range to that observed in Sugar and Cinnabar Creeks (i.e. less than 2% of total mercury)," not only contradicts the author's own statement that it is not the total mercury concentration that controls the amount of methylmercury, but it is not supported by wide-ranging relevant research on the subject. Hence, although I am not convinced that there may be a mercury problem, this report does not convince me of the contrary. Furthermore, the USGS study on mercury only covers Sugar and Cinnabar Creeks. There is no information on possible mercury contamination elsewhere on the site. There may be residual mercury from early gold mining as well as other possible mercury mineralization.

DRSF Modeling: Comments – 4.5.1 DSRF Solution Inputs/Development Rock Chemistry

One of the problems with the use of 'steady-state' sulfate release rates as a basis for defining the source term is that they may not be representative of what actually occurs in the field situation. The definition of ±5 mg/kg/wk for sulfate as a value to use is rather arbitrary and if other constituents of concern such as arsenic and antimony are still changing then a steady state has not been reached. Furthermore, although plots were made to show the "strong correlation" between arsenic and sulfate and antimony and sulfate release rates, the plots are logarithmic and actually not well correlated except for a few specific sample types. There are also examples in these plots of arsenic and antimony release rates changing with no change in sulfate release rate and vice versa. Considering the complications of temperature gradients and variability, rather different hydrologic conditions between lab and field, and different microbial communities that effect sulfide mineral oxidation and dissolution, it would be a more realistic scenario to take the highest and lowest release rates that occurred during the entire HCT and assume that this may represent the range of conditions that could occur in the field. This approach should include the very first sample taken which might represent conditions of a rainstorm after a prolonged dry period (Maest and Nordstrom, 2017).

DSRF Solution Inputs/4.5.2 Groundwater Chemistry

Many of the analytical tables from the HCT tests were provided with charge balance percentages. These should be included on all tables that report complete analyses including the average groundwater chemistry as shown in Table 4-8.

DSRF Solution Inputs/4.6 Seepage+Runoff Source Terms

Equation 3 (and equation 5) is an intriguing equation that has some very reasonable components. It has some empirical variables and some deterministic ones. Combined with equation 4 (and equation 6, respectively), it looks to be quite practical. I am wondering: what is the source for these equations? No citation is given. More importantly, how well does they work? Has anyone used this method to predict source terms from mine wastes and been able to compare it with actual release rates in the field? It is relatively easy to formulate an equation like this, but it is substantially more difficult to test it and show that it gives a quantitatively comparable value to actual field values. Without that information there is no way to evaluate its usefulness. Furthermore, if the comparison were made and it was found the comparison was not acceptable, one would not know which terms in the equation were chiefly responsible. There are enough probable errors in both the hydrologic and geochemical terms that it would take a major effort to sort these out. For example, fines are estimate at 20%, but could vary between 10 and 50%. Average release rates are based on HCT steady-state sulfate concentrations, which can vary by more than an order of magnitude over the course of a year or more. Arsenic release rates are based on a rough correlation with sulfate release rates which has a substantial variance. Only 4% of the total rock mass in the DRSF is thought to come in contact with infiltrating water (on average), but this is also a rough estimate with a substantial potential variance. There is also the average infiltration rate (for seepage) and average runoff rate (for runoff) that could have a large variance, but the cumulative effect of all these variabilities will not be known until the uncertainty and sensitivity analyses are completed. Hence, we are left with the problem that we don't really know how reliable these source terms are for contaminants released from these sources.

DSRF Solution Inputs/4.7.1 Groundwater Chemistry Data

Although different approaches have been used to deal with non-detect concentrations, the best general rule of thumb is to take ½ the detection limit, not the detection limit. Taking the detection limit is definitely too high.

DSRF Solution Inputs/4.8 Solubility Controls and Trace Element Adsorption

This section is rather important because, by using PHREEQC to force equilibrium solubilities when saturation is reached, it puts a limit on estimated soluble concentrations. It is a challenging task requiring knowledge of what minerals are likely to reach equilibrium and which ones won't because of kinetic constraints and because of competitive mineral solubilities that have common components. Table 4-10 lists the mineral phases that were assumed to reach equilibrium in the DRSF models. Below I have listed these minerals and problems I can see with their application in this context:

1. $Ba_3(AsO_4)_2$ – does not occur as a known mineral; the observed mineral dussertite, $BaFe_3(AsO_4)(AsO_3OH)(OH)_6$, is not the same compound; neither compound is known to have an important control on barium or arsenic solubilities
2. Calcite – very likely to control calcium concentrations
3. $CdMoO_4$ – does not occur as a known mineral; unlikely to be important in controlling cadmium or molybdenum concentrations; has not been found at the site

4. Ferrihydrite – important control on iron concentrations, but depends on what solubility product constant is chosen and can vary by orders of magnitude; the solubility product constant was not provided explicitly
5. Fluorite – likely to control fluoride concentrations
6. Gibbsite – likely an important solubility control on aluminum concentrations but solubility product can vary by orders of magnitude; the solubility product constant was not provided explicitly
7. Gypsum – an effective solubility control primarily for calcium, but can also affect sulfate concentrations in circumneutral pH water; concentrations have to be fairly high to reach saturation
8. Hgmetal(l), preferably written as Hg_l^o – could conceivably control Hg concentrations, but also highly volatile; analytical speciation for dissolved Hg species would be required to determine if solubility control and this measurement has not been done
9. HgSe – this mineral, tiemannite, has not been found at the site and is highly unlikely to be a solubility control because of uncertainty of thermodynamic data, lack of selenium at the site and redox chemistry would not support it
10. Malachite – a likely solubility control in areas of weathering copper deposits, but not at this site; insufficient copper mineral sources
11. $MnHPO_4$ – not a known mineral and phosphate concentrations are not high enough for this compound to form
12. Pyromorphite – highly unlikely to be a solubility control because phosphate and chloride concentrations are not high enough
13. SbO_2 – not a known mineral and highly unlikely to form
14. Smithsonite – zinc concentrations are not nearly high enough for this mineral to form

The observation that several minerals are indicating equilibrium saturation indices in these waters when the mineral has not been recorded at the site and is not likely to be at saturation in these waters suggests that either the thermodynamic data in the database used was incorrect, or that the analytical data is incorrectly entered. With only 5 out of 14 minerals/compounds as having a good probability of providing a solubility control and the other 9 not, this exercise is only partially relevant. The other 9 are the only ones involving potentially contaminating trace elements and some of these are not known to occur as minerals or at the site and for the rest (5) we have no demonstration that they have ever provided a solubility control in a mining environment. Even calcite, which reaches equilibrium saturation in some limestone aquifers, is inhibited from reaching equilibrium solubility in many aquifer systems and in seawater and goes supersaturated. Other minerals may show the same behavior, but we don’t have as good examples of those.

The database used in these and other computations in this study was minteq4.dat in PHREEQC. Although this database is rather extensive, it also has some unreliable values and it does not, for example, consider the wide range of solubility by different forms of $Al(OH)_3$ or ferrihydrite, schwertmannite, and goethite. Changes in the solubility product constants of these minerals can be orders of magnitude and will have substantial effects on the mass of precipitate and the solubility.

DSRF Solution Inputs/4.9 Nitrate leaching

Although ammonium and nitrate are potential pollutants, nitrate can also oxidize sulfides in the absence of oxygen and should increase the rate of sulfide mineral dissolution. Hence, contaminant release rates could be higher than estimated.

DSRF Solution Inputs/4.10.1-2 Runoff Water Quality-Toe Seepage Water Quality

These predicted water quality scenarios appear to mostly reflect the inverse relation described between solute concentration and flow. Unfortunately, this relationship is never quite so straight-forward. Many surface waters show a hysteresis effect, which is related to complicated factors including length of preceding dry period, geochemical processes taking place within waste materials, and intensity of precipitation events. Even in the Appendix D there are plots of conductivity and discharge in Graph 1 that show that there is a lag effect, a delay, between the peaks in discharge and the lowest point in conductivity and vice versa. Also, the time series plots shown in Attachment B of Appendix D show that the changes in arsenic concentration (YP-SR-2) are not at the same rate as the discharge. Even for changes in chloride concentration, the chloride change is substantial for very little change in discharge. Most telling is the rainstorm event near the end of 2012 for which the chloride concentration increases from before to after the rainstorm.

The figures shown, 4-17 to 4-22 for runoff appear to follow a direct inverse correlation between concentration and flow with notable exceptions that are not really discussed. These exceptions are more notable sometimes after year 4 and usually at high dilution levels. These trends are not intuitive and require explanation. Is it a detection limit an issue?

The figures and 4-23 to 4-26 for toe seepage are more confusing because they are said to follow the same inverse correlation and yet there is almost no correlation. This disconnect between text and figures requires an explanation. Is there a solubility control here?

5 TSF – 5.4.2 Groundwater Chemistry

The interpretation of the groundwater chemistry receiving discharge from the TSF suffers from the same problems that other water chemistry interpretations suffer from – phases assumed to play a role in controlling solute concentrations through solubility equilibrium that likely do not. There does not seem to be much consideration of geochemical reactions occurring within the tailings facility that may change the pore solutions that eventually infiltrate the groundwater system. The mostly flat-lying lines in Figures 5-5 and 5-6 seem entirely unrealistic. Even natural variability in the groundwater without the TSF will show some fluctuations more than these lines do. The report should refer to the historical groundwater monitoring to gain an appreciation of the natural variability.

Concluding remarks

With respect to the questions that were to be addressed at the beginning of this review, I suggest the following responses.

Are there serious issues with the "geochemical modeling" that affect the conclusions?

Yes, but the degree to which they affect the conclusions is difficult to determine because so many assumptions and estimates enter into the calculations. In addition to the yet-to-done uncertainty and sensitivity analyses, an error propagation is needed to effectively answer this question.

Are there other considerations not presented in this report that should be mentioned?

Yes. Even with all the surface water measurements for quantity and quality, no one thought to do a tracer injection study to quantify unknown inflows, presumably base flows and their chemical loads. Also, the results of groundwater pump tests have not been mentioned. This would help substantially in understanding the groundwater conditions and how they fit into the overall SWWC. They may be in another report not available. If they were not done, they should have been.

Does the report support qualitatively useful scenarios?

Yes, but with the qualification that the uncertainties that are inherent in the calculations make it difficult to say just how useful.

Does the SWWC report adequately address the original objectives?

In general, it does, but several aspects of the geochemical modeling can be substantially improved. The overall concept and goal of the SWWC is excellent but a challenging one to accomplish.

References

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