Exhibit 45

R14-24
Sanitary District of Decatur
April 16, 2018 Response to IEPA and USEPA
Comments Dated January 16 and 19, 2018

Original response submitted on February 8, 2018 (blue text)
With Follow-Up Dialogue in Bubble Comments
Author and Date:
JA = USEPA Combined Comments February 26, 2018
RCS = Robert Santore March 8, 2018
R = Robert Santore March 21, 2018

Introduction

The Sanitary District of Decatur (SDD) appreciates the review and comments of the Illinois Environmental Protection Agency (IEPA) and the United States Environmental Protection Agency (US EPA), received January 16 and 19, 2018, in response to SDD's Amended Petition for Site Specific Rule and Exhibits 14, 28, and 29 (Amended Petition).

What follows are our responses. For your convenience, we set forth your comment in black and then our response in this color immediately following each comment. We note that we were uncertain as to one of the comments and that a conference call would be helpful to talk it through and also address any questions you may have about the responses below. We will contact you promptly to schedule that call.

Illinois EPA Comments:

I'd like to first thank those that participated in incorporating the prior comments of IEPA and US EPA into the amended petition. I acknowledge the difficult task you were given, especially considering the short time frame. The amended petition offers a more practical standard than what was initially filed, as the equation-based site specific standard would allow IEPA to make more accurate assessments of the standard along the specified reach.

That said, there are still two outstanding concerns with the amended petition, both of which are directly tied to the development of the WER value and thus have a direct influence on the resulting standard.

First, it is unclear what DOC concentration is being used in the WER calculation for the Sangamon River, as there are inconsistencies between the petition and the exhibits. For example, the petition states that a WER of 2.33 was developed, which was based on a DOC concentration of 6.5 mg/L derived from the average of 22 water samples collected downstream of the District. However, Exhibit 28 states that a WER of 2.48 is appropriate, which was based

Commented [JA1]: Comment bubbles reflect EPA's understanding of SDD's proposed action items to address Illinois EPA and US EPA's comments based on a February 22, 2018 conference call. The inclusion of action items in this document is only intended to summarize actions agreed to by SDD and does not impose any requirements on SDD. EPA's review of any site-specific criterion that results from this rulemaking will be based on the documents submitted at that time and not whether or not these action items were fulfilled.

on an average DOC concentration of 7.91 mg/L derived from previous characterizations (Santore, 2015). Please clarify.

You are correct in pointing out that there are two calculations. We used both a DOC regression and a BLM calculation to estimate nickel bioavailability. The site-specific criteria and WER are based on the DOC regression, so that is the more relevant of the two results. The differences in how these two calculations were applied were necessary because we could not apply both methods to all of the same samples.

You may recall that our previous average DOC was based on the BLM special study samples. We had originally focused on those samples because they provided complete chemistry information which we needed for the BLM calculation.

For the site-specific calculation, we changed the calculation of average DOC in response to a request in your email of October 17, 2017, in which you pointed out that the DOC and hardness equations we had proposed using for the site-specific standard no longer required full BLM chemistry, and therefore we could include more of the downstream samples in the average DOC calculation. The average we previously reported was a DOC of 6.525 mg/L based on 22 samples as you have noted. However, in reviewing these results for responding to your comments, we realized that this reported average was in error.

In the "Development of a Water Effect Ratio for Nickel in the Sangamon River" document (Exhibit 28), there is a table with the data used to calculate the average DOC. This table was generated using a spreadsheet. The data in the spreadsheet are correct, the correct formula for average was used, and the range for the formula included all of the data in the column. However, the incorrect reported average of 6.525 mg/L was due to the data in the spreadsheet having been filtered down to include only these downstream sites and the incorrect result stems from data filtered out and are not visible but are still in the spreadsheet. By removing this filtering error, the actual average DOC value is 8.33 mg/L. Using this average DOC for the prediction on the Ni WER results in a value of 2.52. We are in the process of updating the WER report (Exhibit 28) and amended petition information to ensure consistency with these corrected values.

Regarding the BLM, as you may have noticed by the date on the BLM report (Exhibit 14), we did not update it with the new average DOC. In fact, it would not be possible to run the BLM for all of the 22 samples we used for the site-specific criteria because most of those samples do not have chemical parameters needed for the BLM analysis.

The BLM analysis is still relevant, even if it is only applied to a portion of the samples used for the site-specific criteria. As you may know from reviewing our draft BLM paper (Exhibit 29), we have reviewed over 1400 individual nickel toxicity tests as part of a comprehensive review of factors that affect nickel bioavailability. The fact that the BLM could explain and predict the trends in nickel toxicity over that vast literature is an

important demonstration of what the BLM analysis has contributed to our understanding of nickel bioavailability in the Sangamon River. We have tried to use language that discusses the BLM analysis as a supporting line of evidence that produces similar results to the DOC regression and the testing at OSU, even though the BLM analysis was based on a different set of samples with a different average DOC. The main point to take away from both analyses is that DOC is an important toxicity modifying factor for nickel toxicity in the Sangamon River.

Second, it appears that the concerns of IEPA/USEPA regarding use of the ANCOVA DOC slope, as detailed in the October 31, 2017 email from Robie Anson, have not been addressed. Please refer to item numbers 1-5 of the attached email for further information. I will refrain from going into the details contained in the attached email, but the overarching concern with the DOC slope is that it is heavily influenced by the acute slopes derived from the Kozlova et al. 2009 studies, whereas equally valid acute data from other species have been excluded from the slope derivation without justification for doing so. Further, the DOC slope derived from the only available chronic study, which happens to be OSU study conducted in "simulated effluent" from the District, suggests that the mitigating effects of DOC is lesser than that suggested by the Kozlova studies. Please provide sufficient justification for use of the ANCOVA slope, or reweight the dataset so as to provide lesser weight to the acute Kozlova studies.

See response to US EPA Comment (1) below.

US EPA Comments:

Preliminary US EPA technical feedback on the Sanitary District of Decatur site-specific nickel criterion petition January 19, 2018

In general, the approach for developing the water effects ratio (WER) value is reasonable and some of US EPA's comments from this past fall have been addressed (e.g., the materials on the Illinois Pollution Control Board (IPCB) website provide details on the ANCOVA and an explanation for not including all Kozlova et al. data). Although EPA confirmed the results of the ANCOVA as reported, the Agency does not agree with how it was formulated. Moreover, some issues raised this past fall have not been addressed and some issues important to the WER value that had been included in previous presentations given by the Sanitary District of Decatur (SDD) do not appear to be included in the materials available on the IPCB website. Overall, the rationale for the site-specific criterion and explanation of the WER value remains incomplete. Specific technical concerns are provided below. These comments largely repeat issues raised this past fall (specifically, in EPA's October 31, 2016 e-mail):

(1) The selection of the data used to calculate the dissolved organic carbon (DOC) slope should be documented more completely

Commented [JA2]: EPA's understanding is that the DOC values differ in different sections of the petition due to 1) an inadvertent omission of upstream values and 2) differing data availability between the WER and the BLM. To address this:
1)SDD will revise the petition and supporting documentation to update the DOC value used in the WER based on the full dataset. In the petition, SDD will clarify what DOC data was used and why it is appropriate to use that dataset.
2)As discussed below for US EPA Comment (5), SDD will use the WER DOC value in the BLM to provide a side-by-side comparison of the proposed WER with a BLM-derived WER (for support). Based on that evaluation, SDD will revise the original petition documents and supporting documentation. If the petition still includes BLM results using a different DOC dataset, SDD will clarify how that DOC dataset differs from the dataset used in the WER and why it is necessary to use a different DOC dataset for that analysis.

Commented [RCS3R2]: 1) Agreed
2) Agreed

Commented [R4]: Changes made:

BLM report:
- A BLM calculation using average DOC that is consistent with the WER report was added.
- See values added to Tables 1 and 2

WER report:
- The WER report has been changed to reference the BLM result using average chemistry and an average DOC that is consistent with the value used in the DOC-WER equation
- The updated WER is 2.50.
- See the expanded discussion references the average DOC from the modified BLM report in the "Supporting information" section that follows Figure 2.

The ANCOVA to derive the DOC slope is based on the Oregon State University (OSU) data and the Kozlova et al. dataset. Other datasets on DOC effects (e.g., citations regarding DOC effects in Exhibit 28 and the draft paper in Exhibit 29) are not included in the analysis, without appropriate documentation on the basis for the exclusion of these data. Previous SDD presentations have indicated that the reason for excluding the Hoang et al. fathead minnow data was the higher effect concentrations for this species (DOC effects being expected to be less at higher nickel concentrations) and Exhibit 28 provides the general statement that "the most relevant data would be for a sensitive organism that exhibits a response to DOC comparable to...the OSU tests." However, if higher effect concentrations are a sound basis for excluding the Hoang et al. data, then the Kozlova et al. data would also need to be excluded because the effect concentrations in both datasets are similar and both are about two orders of magnitude higher than the OSU data. This latter point also raises the question of how the Kozlova et al. data can be considered to be representative of a sensitive organism. Furthermore, if the OSU data are considered insufficient, so that data are to be pooled for an overall assessment of a DOC slope, is requiring other datasets to have comparable effects an appropriate data selection criterion? There might be some chemical speciation relationships/arguments pertinent to this data selection, but such arguments have not been provided. Given these observations, it appears that only the OSU data are clearly relevant here, being for a sensitive species and endpoint and being conducted in test waters similar to the SDD discharge. To the extent that other data are included in the DOC slope estimation, the basis for that decision will need to be fully documented and explained.

We have previously noted that the Kozlova data are more relevant because the Kozlova study was based on *Daphnia pulex*. *D. pulex* is more closely related taxonomically to *Ceriodaphnia dubia* which is the organism used in the chronic OSU study. Both *D. pulex* and *C. dubia* are invertebrates in the order *Cladocera*, and *Cladocerans* consistently rank among the sensitive species to nickel toxicity. The Hoang study, in contrast, was based on fathead minnow and we would not necessarily expect a fish test to be as relevant to the anticipated behavior of *Cladoceran* species.

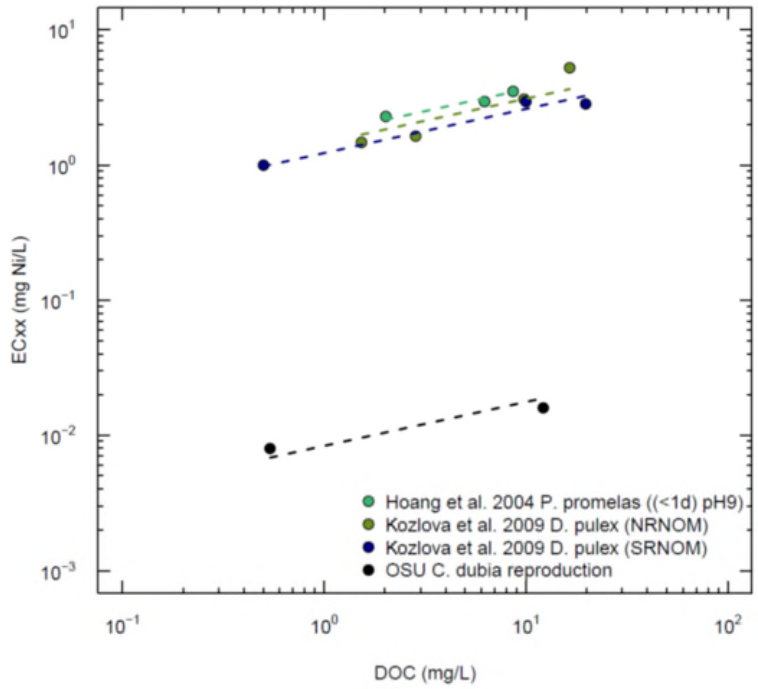
However, there is a good DOC relationship evident in the Hoang study, and we can test to see whether that relationship is similar to that seen in the OSU and Kozlova data. Most of the Hoang data we included in our review are from the Hoang et al 2004 paper. In some of the figures we have presented previously, we also incorporated additional unpublished Hoang data that were supplied to us as a personal communication from Tham Hoang. For the purpose of the ANCOVA, it is appropriate to consider only those data that were included in the 2004 paper, since they were based on tests that were conducted at the same time on the same population of test organisms. If those data are included in the analysis, the ANCOVA produces virtually the exact same result as if we include only the OSU and Kozlova data (with a pooled slope of 0.325). The ANCOVA results indicate that the individual slopes from all three studies are not different. Therefore, we can conclude that the DOC response in each of these three tests are not different from each other.

Commented [JA5]: EPA's understanding is that SDD will re-calculate the pooled slope after including the Hoang fathead minnow data (excluding the low DOC value that was generated in a different study) and revise the petition and supporting documentation accordingly. SDD will also revise the petition to explain why it would be inadvisable to rely solely on the OSU data, the decisionmaking process by which outside studies were evaluated for inclusion, and why the Kozlova and Hoang data are relevant despite the fact that they are for less sensitive endpoints.

Commented [RCS6R5]: 1) The pooled slope with FHM has been recalculated.
2) We will continue to pursue the inclusion of multiple species in the DOC slope including D. pulex and FHM, and will expand the text to discuss why multiple organisms are relevant. In that regard, the demonstration that the bioavailability relationships are consistent for multiple species will be discussed as an ideal result for establishment of a guideline that needs to address how bioavailability affects all aquatic life. Demonstrating this generality will provide evidence to indicate that the guideline will be protective for all organisms even with bioavailability adjustments.

Commented [R7]: Changes made

WER report
- Expanded discussion on why multiple species are relevant, and why Kozlova and Houng data are relevant
- Added FHM to the ANCOVA results
- Added a new discussion of uncertainty in the OSU slope
- Conducted a Monte Carlo analysis to quantify confidence intervals around the DOC slope
- Expanded the discussion to show that the ANCOVA derived slope is consistent with the response seen in the OSU data and is within the confidence interval on the slope of the response in the OSU data.



(2) Low weight given to OSU data in the ANCOVA

The OSU data are much more relevant to the SDD WER than the Kozlova et al. data. The OSU effect concentrations are for the sensitive species and endpoint important to the Illinois Ni standard and were determined in test waters similar to that of the SDD discharge, whereas the Kozlova et al. data are for an endpoint 100-fold less sensitive in a much different test water. However, because there are seven data points for the Kozlova et al. data, versus two for the OSU data, the DOC slope is dominated by the less relevant data (i.e., the slope for just the Kozlova et al. data would be 0.37 and for the OSU data 0.22, with the pooled slope being 0.33, much nearer the Kozlova et al. slope). The analysis needs to consider the issue of data relevance, if data other than the OSU data are to be used. Again, there might be speciation information that is relevant to this issue (i.e., what is the expected degree of Ni binding in the different test waters?) but this does not appear to be presented in the supporting documentation for the proposed site-specific criterion.

The purpose of conducting an ANCOVA was to determine if the DOC slopes from individual tests reflect different responses, or if the differences were small enough to be due to chance. This test does not depend on the number of points in each of the individual

datasets. If the DOC response in the OSU data were different than the DOC response in the Kozlova or Hoang data, then the ANCOVA would still indicate that the slopes were different, even though there are fewer points in the OSU dataset. Since the results of the ANCOVA suggest that the slopes are not significantly different, we conclude that the small apparent differences are due to chance and the overall slope is therefore appropriate for characterizing the DOC response for all of the data.

This also means that if the slopes were dissimilar we would not combine these datasets into an overall response. Hence, the "weight" given to the OSU data should not be an issue since we are not combining data from datasets with different responses. Considering more data in this analysis produces a more robust analysis and reduces the degree to which random noise is expected to be influencing the result.

From the standpoint of developing a site-specific criterion, the demonstration that the DOC response is consistent for different species in different tests also provides a much stronger argument for the development of the nickel criterion. The generality of the result suggests that different species are following similar responses to factors that affect nickel bioavailability.

The additional suggestion in the comment that speciation could be used to further support the conclusion that DOC is expected to have an effect on nickel bioavailability is a good one. Since this suggestion was also made in comment (5) below, we will include a comparison there.

(3) Better analysis should be provided regarding the difference of the pooled and separate slopes (Kozlova et al. vs OSU)

Using the interaction term to conclude that slopes do not differ significantly is not definitive, in that the interaction term does not directly address slope differences and can reflect other differences. This issue should be addressed by directly testing significant differences between alternative models – one with separate slopes and one with pooled slopes.

We are uncertain as to the suggestion you are making about testing the differences between slopes. We will schedule a call with you to discuss the specific statistical tests you are looking for in this comment.

Commented [JA8]: EPA's understanding is that SDD will consult with a staff statistician to discuss methods to weight the datasets differently to prevent dilution of the more relevant OSU data. If the pooled slope is re-calculated, SDD will revise the petition and supporting documentation. Regardless of whether SDD re-calculates the pooled slope, SDD will revise the petition and supporting documentation to explain why the weighting used is appropriate.

Commented [RCS9R8]: We do plan to use the pooled slope and we agree with the suggest changes that will result.

Commented [R10]: Changes made:

WER report:

- New analyses were added to quantify the uncertainty in the slope of the DOC response in the OSU data.
- A new pooled ANCOVA slope was calculated that includes the Hoang FHM data.
- The ANCOVA slope is discussed in the context of the uncertainty in DOC slope showing that the ANCOVA slope is within the confidence limits of the response in the OSU data.

Commented [JA11]: After further consideration and discussion with SDD's contractor, EPA agrees that SDD's ANCOVA analysis is an appropriate means to test significant differences of the slopes for a linear model. Therefore, EPA believes that SDD has addressed this comment.

However, SDD's ANCOVA analysis only addresses the issue of whether to reject an assumption of equal slopes. For these data, it does not sufficiently address whether the resulting pooled slope is appropriate, given that the slopes of the separate experiments are substantially different (0.22-0.52) and this data set has low power for detecting differences. As already stated in EPA Comments (1) and (2) above (pages 4 and 5), SDD should provide additional documentation for using a slope greater than that calculated in the OSU study.

Commented [RCS12R11]: We appreciate that this technical issue has been resolved. We have completed an additional analysis that provides additional documentation for why the pooled slope is appropriate for C. dubia.

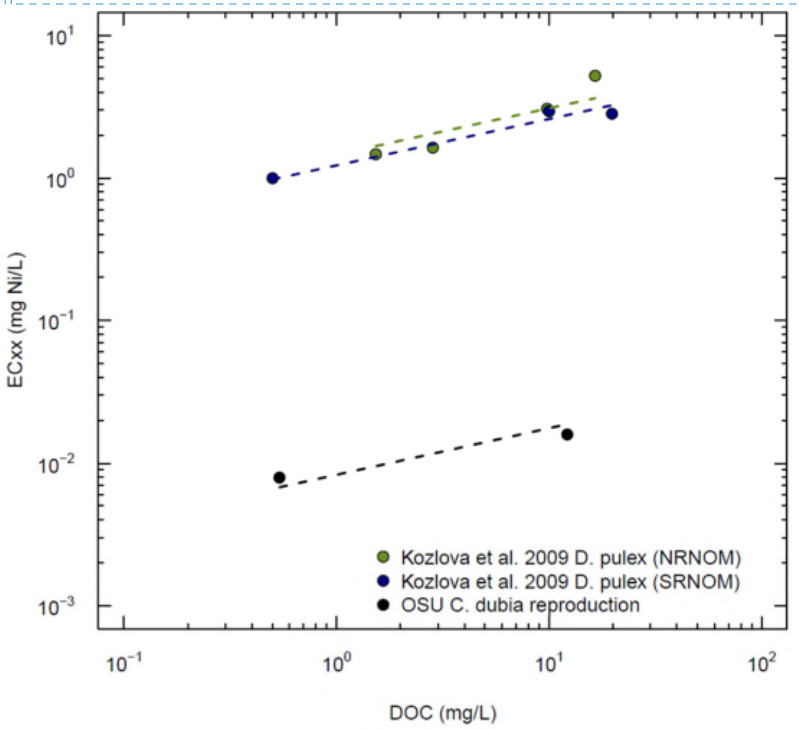
Commented [R13]: Changes made:

WER report:

- A Monte Carlo analysis was used to estimate uncertainty in the DOC slope and we demonstrated that the ANCOVA pooled slope is within the confidence limits on the OSU slope
- This uncertainty is shown as a histogram (Figure 1) and as a shaded region on the Ni toxicity vs DOC figure (Figure A2-1).
- These additional analyses demonstrate that the pooled slope is appropriate for the response seen in the OSU data.

In addition, why are the dataset-specific relationships for the Kozlova et al. data in Exhibit 28, Appendix 2 Figure A2-1 nonlinear? Although these nonlinear relationships are a minor issue, they are contrary to the discussion.

You may be referring to the second line on the figure (the solid, dataset-specific line). We agree that this additional line is distracting and should be removed. Here is a cleaner version without the second set of lines.



Commented [JA14]: EPA's understanding is that SDD will revise this figure in Exhibit 28.

Commented [RCS15R14]: Agreed.

Commented [R16]: A revised figure is included in the latest draft (Figure A2-1)

(4) Documenting DOC for the reference water in the WER equation

A WER addresses the relative toxicity in site water compared to laboratory water, with the laboratory water needing to be representative of the test waters used for deriving the criterion to which the WER will be applied. Although the material presented here notes that the OSU laboratory water had a DOC concentration of about 0.5 mg/L, it does not address the appropriate basis for setting the DOC for the reference water – the laboratory tests upon which the chronic Illinois nickel criterion was based. The reference effect concentration in the WER equation is thus not adequately defined. If the DOC concentration of the reference water (i.e., the low-DOC

laboratory water used in the OSU tests) is similar to that of the test waters used for deriving the chronic Illinois nickel criterion, the material presented in support of the Petition should state this and provide evidence supporting this statement.

We discussed during our conference call on October 16, 2017 that the other data used to derive the Illinois chronic nickel criterion were either from tests conducted in synthetic waters with no added DOC, or in a natural water for which we documented low TOC and DOC concentrations. From the discussion in that conference call, we believe we were all in agreement that the DOC of 0.5 mg/L in the OSU laboratory water was representative of the test conditions for other studies with sensitive organisms used to derive the Illinois chronic nickel criterion.

Would you like us to include these supporting data and discussion in the derivation of the site-specific criterion?

Commented [JA17]: EPA's understanding is that SDD will revise the petition and/or supporting documentation to provide a discussion as to why 0.5 mg/L is an appropriate value for the "low DOC" reference value.

Commented [RCS18R17]: Agreed. We will include the discussion that was previously presented as to how the sensitive organisms used in the derivation of the Illinois state standard were all tested in low DOC waters.

Commented [R19]: Details of the water sources and DOC concentrations for the most sensitive tests in the Illinois guideline have been added (see discussion top of page 8).

(5) Discussion is needed regarding the biotic ligand model (BLM) as supporting the WER

The discussion of the BLM is rather vague, cursory, and, in some places, appears to be inconsistent. The draft BLM paper is included as Exhibit 29, but does not directly address this WER and its application to the WER is not described. While a January 16, 2014 report that applied the BLM to the receiving water is presented as Exhibit 14, it is unclear whether the model used in this Exhibit is up-to-date (i.e., has the nickel BLM changed, is the WER based on the same reference and site DOC, etc.?). It appears, based upon Tables 1 and 3 in Exhibit 14, that the DOC value used to derive the WER in this document (DOC = 9.99 mg/L, WER value = 2.62) is approximately 3.5 mg/L higher than the DOC value used to derive the WER presented in the Petition (DOC = 6.525 mg/L, WER value = 2.33, as presented on pp. 50-51 of the Petition). This seems to call into question the degree to which Exhibit 14 is useful as a point of comparison for the proposed WQS revision. Additionally, p. 51 of the Petition indicates that "[t]he nickel BLM was used to predict nickel toxicity in site water and reference water. From this analysis, a WER of 2.48 was determined. See BLM Adjustment Report, Exhibit 14." Exhibit 14 does not appear to indicate that the BLM predicted a WER value of 2.48; as stated above, Exhibit 14 indicates that the BLM predicted a mean WER value of 2.62. Why did SDD not update Exhibit 14 by running an up-to-date Ni BLM with the DOC value used to derive the Petition-proposed WER value (i.e., DOC = 6.525 mg/L) to demonstrate that the BLM predicts a similar WER value to that derived using the OSU toxicity tests?

As noted above, we have used more samples for calculating the average downstream DOC than can be used with the BLM, since most of those samples do not have the corresponding chemical parameters needed to run the model. As a result, the BLM calculation and the DOC-equation have been applied to different samples.

We intentionally kept the discussion of the BLM in the WER document brief because we understood from previous discussions that US EPA preferred that we use the BLM for supporting evidence, but not for direct determination of the site-specific criterion. The

takeaway message from the BLM report is that toxicity modifying factors (and DOC in particular) affect the bioavailability and toxicity of nickel in the Sangamon River. This finding is consistent with the OSU toxicity tests which also demonstrate that DOC is an important factor for modifying nickel bioavailability. In addition, we derive a similar WER using the BLM, even though it is applied to a different set of samples with different average DOC.

Additionally, as already noted, one thing that might be done to provide support from a BLM perspective is to report the expected shift in speciation due to the DOC in the different test waters, using the speciation models in the BLM. After all, the BLM is premised on free nickel activity driving response, so the BLM predictions would simply reflect such speciation changes, and providing these changes would be more simple, direct, and convincing.

We could include a figure and discussion to further reinforce the primary message in the BLM report that points to DOC as among the most important toxicity modifying factors for nickel in the Sangamon River. For example, in the figure below the distribution of nickel between organic and inorganic forms is shown over a range of DOC concentrations. At low DOC most of the dissolved nickel will be in the form of inorganic complexes shown as a dark blue line (for example, Ni-sulfate, Ni-bicarbonate, and Ni-hydroxide). As the DOC concentration increases, the amount of nickel in organic complexes increases (green line) with a corresponding decrease in inorganic complexes. The amount of the free nickel ion (light blue line) also decreases with increasing DOC. Since nickel bioavailability is more closely related to free nickel, the BLM predicts that increasing DOC concentrations will reduce nickel bioavailability. This result indicates that natural waters with a lot of natural organic matter (e.g., high DOC) will have reduced nickel bioavailability. The elevated DOC concentrations in the Sangamon River, therefore, are expected to reduce nickel toxicity. Hence, information about nickel speciation supports the derivation of a site-specific criterion for nickel in the Sangamon River.

Commented [JA20]: As discussed for IEPA Comment (1) above, EPA's understanding is that the differing DOC values result from the use of different datasets. To provide a clearer side-by-side comparison of a BLM-derived WER with the proposed WER, SDD will evaluate the BLM results using the same DOC value used to calculate the proposed WER. SDD will also revise the petition and supporting documentation based on that evaluation and add text to clarify the role of the BLM in the petition.

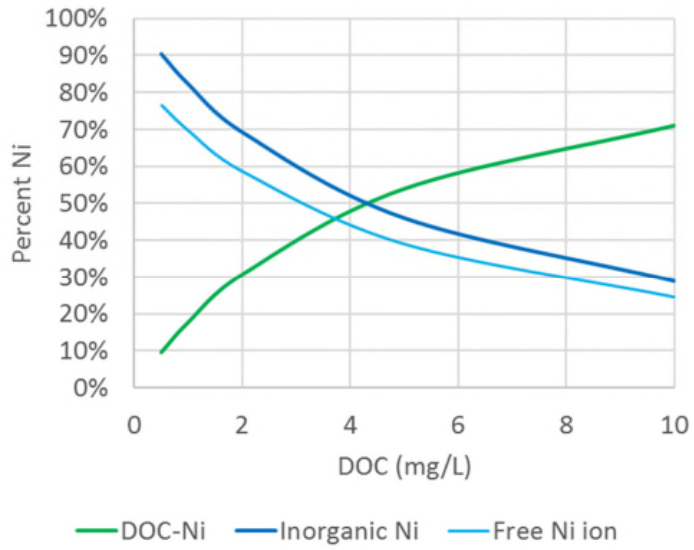
Commented [RCS21R20]: Agreed as noted in response to the first comment.

Commented [R22]: See previous response.

Commented [JA23]: EPA's understanding is that SDD will add the figure below to its petition and/or supporting documentation along with a description of the input parameters and total Ni concentration used to generate the figure. The input parameters and total Ni concentration used in the BLM should correspond with those for SDD's effluent and downstream water in the Sangamon River. SDD will also add text describing how this figure supports the proposed WER.

Commented [RCS24R23]: We will include this figure and associated discussion.

Commented [R25]: Figure is now included in the WER report as Figure 2.



(6) Conflicting WER and DOC values in Petition and supporting documentation

pp. 2-3 of Exhibit 28 indicate that a WER value of 2.48 is appropriate. This is apparently based upon a DOC concentration of 7.91 mg/L in the Sangamon River. p. 2 of the Petition requests that Illinois's State-wide chronic nickel criterion be multiplied by a WER value of 2.33 for the site in question, and pp. 50-51 of the Petition indicate that a WER of 2.33 is appropriate. This is apparently based upon a DOC concentration of 6.525 mg/L in the Sangamon River. Why are two different DOC values presented for the Sangamon River and why is the WER value presented in Exhibit 28 different from that presented in the Petition (and Exhibit 14)? Please ensure that the Petition and the supporting documentation presented in the Exhibits are consistent with one another.

As we noted above in response to the IEPA's first comment, the consistency issue has been addressed. Also, regarding the different values in the BLM analysis, recall that the BLM analysis is limited to a subset of samples used for the DOC equation. As a result, the samples used for the two analyses have different average DOC values. Both analyses, however, support the conclusion that natural organic matter (quantified as DOC) is reducing nickel bioavailability in the Sangamon River, and that a site-specific criterion that considers nickel bioavailability is warranted. Although the application of these two methods are necessarily applied to different sets of samples, we believe it is still appropriate to discuss both results and to point to the BLM analysis as a supporting line of evidence.

We can, however, add to the discussion of the BLM to make it clear that different samples were used.

Commented [JA26]: As noted above, EPA's understanding is that SDD will revise the petition and exhibits to clarify the use of different DOC values and how the BLM-derived WER is used in the petition.

Commented [RCS27R26]: Agreed.

Commented [R28]: The BLM and WER reports were modified as discussed.

Exhibit 46

Exhibit 46

Nickel Calculator for Illinois, Indiana, Iowa, and USEPA

= guideline parameters (do not edit)
= calculated output values (do not edit)
= input cells for editing

State and Federal Dissolved Nickel Standards								
	Illinois[1]		Indiana[2]		Iowa[3]		US EPA[4]	
	Acute	Chronic	Acute	Chronic	Acute	Chronic	Acute	Chronic
Slope	0.846	0.846	0.846	0.846	0.846	0.846	0.846	0.846
Intercept	0.5173	-2.286	3.3612	1.1645	2.255	0.0584	2.255	0.0584
Hardness mg CaCO3/L	Acute WQG µg/L	Chronic WQG µg/L	Acute WQG µg/L	Chronic WQG µg/L	Acute WQG µg/L	Chronic WQG µg/L	Acute WQG µg/L	Chronic WQG µg/L
359	243.37	14.75	4181.77	464.89	1383.39	153.81	1383.39	153.81

Proposed Decatur Site Specific Dissolved Ni Standard				
DOC WER Eq				
Slope	0.3260			
Intercept	0.9215			
	DOC mg C/L	Ni EC20 µg/L	WER	SS Chronic µg/L
Reference	0.5	6.66		
Site	8.33	16.66	2.50	36.90

Total Ni Standard	
Translator:	0.966
	SS Chronic µg/L
	38.20

[1] 35 Ill. Adm. Code 302.208(e).

[2] 327 Indiana Adm. Code 2-1-16.

[3] 567 Iowa Adm. Code 61.3(3).

[4] USEPA National Recommended Water Quality Criteria - Aquatic Life Criteria Table, available at: https://www.epa.gov/wqc/national-recommended-water-quality-criteria-aquatic-life-criteria-table.