Comments on FCSS-ISG-10, "Justification for Minimum Margin of Subcriticality," Rev. 1

Comment	Agree or Disagree	NRC Response
BWXT		
1. The distinction between the margin of safety residing in the physical system and the margin of subcriticality residing in k_{eff} is very good. The revision provides the necessary ties between experiments, validation, applicable margins, and the facility process analysis methodology.	Agree	No changes needed.
2. In the Technical Review Guidance, the reference to "low-enriched" should be deleted in: "In the past, an MMS of 0.05 has generally been found to be acceptable for most typical low-enriched fuel cycle facilities without a detailed technical justification" and "An MMS of 0.05 should be found acceptable for low-enriched fuel cycle processes and facilities."	Disagree	The removal of "low-enriched" would make this inconsistent with the historical licensing of high-enriched fuel facilities. High-enriched facilities have generally been licensed with MMS values larger than 0.05. This revision of the ISG has already removed reference to a specific 0.1 MMS for high-enriched facilities. It does not take a position on what the MMS should be for high-enriched facilities. We think it is appropriate to neither rule out nor rule in acceptability of the 0.05 for these facilities. There is an example provided of how the increased sensitivity at higher enrichments should be weighed against the reduced uncertainty in the cross sections. A decision needs to be made taking all appropriate factors into account.
3. The language in this version of ISG-10 is consistent with the ANSI standards, and provides the NRC reviewer the appropriate thought process for reviewing an application and understanding what is necessary.	Agree	No changes needed.
NEI		
1. The ISG should be a guide for NCS reviewers, and as such should not place any specific limits on MMS. It should only provide a framework for NCS reviewers.	Agree	No changes needed. The revision to ISG-01 does in fact make it clear that this is guidance for reviewers, and does not endorse any specific limits.

2. ISG-10 places undue emphasis on validation and justifying margins. The risk associated with code errors is very small. Does the NRC validate its computer codes? If it does, NRC validations would be very useful to the industry to see what an acceptable validation looks like.	Agree in Part	We agree that the risk of criticality from mis-validation or insufficient margin is low. However, this has been a problem area in both licensing and inspection, and it is our policy to be both risk-informed and performance-based. The risk is one that has grown over time, because of the increased reliance on computer codes, reduced conservatism from increasingly sophisticated techniques, and the need to reduce margins for operational efficiency. No change needed. The NRC does not validate its own codes. The NRC uses these codes solely for confirmatory purposes; demonstration of safety is the responsibility of licensees. The NRC has, however, issued several NUREGs with detailed guidance on how an acceptable validation may be performed.
3. Use of the restrictive term "benchmark" should be reviewed. Many data that have been included in the International Handbook do not rise to the level of "benchmarks."	Agree in part	Both "benchmark experiments" and "critical experiments" are defined in the Glossary. We agree that it may be appropriate to use critical experiments that are not benchmarks, but then this fact is taken into account in the ISG section entitled "Sufficiency in the Data." **The ISG has been revised to reflect that it is permissible to use critical data that are not benchmarks, but that this would require added margin.**
4. Statements that licensees should employ adequate conservatism are unnecessary, as the SRP already states this. Statements such as "...the conservatism should consist of conservatism beyond the worst-case normal or abnormal conditions..." should never be required by a regulator. There are many instances in which the ISG grossly underestimates the amount of conservatism present.	Disagree	Both the ISG and the SRP discuss conservatism, but the purposes are different. The SRP discusses minimal conservatism needed to ensure that calculations are adequately bounding of operations. However, in order to be credited for a lower margin than would otherwise be found acceptable, the conservatism must go beyond this. This is at the licensee's discretion and is not a requirement. We do not understand the comment that the ISG grossly underestimates the amount of conservatism, because the ISG is not meant to address any specific processes–the processes discussed therein are for example only. No changes needed.
5. ISG-10 draws a clear distinction between the safety and subcriticality of a process...If a criticality analysis demonstrates that a process is subcritical under worst credible conditions, then the system is safe. There is no distinction and to look for one is to add unnecessary margin to systems that are already at their worst credible conditions.	Disagree	This comment contradicts several comments on the original draft of the ISG. In the revised ISG, we have striven to distinguish between margins of safety and margins of subcriticality. When we say something is "subcritical," there is some uncertainty associated with that statement, and the margin of subcriticality deals with the assurance of subcriticality for whatever condition is modeled, rather than whether that condition is truly the worst credible. Clarity would not be improved by removing this distinction, so no change is needed.
6. ISG-10 admonishes against excessive interpolation or extrapolation. NUREG/CR-6698 provides guidance that extrapolation beyond 10% of the range are not recommended. Equivalent guidance should be included for interpolation, similar to that presented in the NUREG for extrapolation.	Agree	The draft ANSI/ANS-8.24 discusses both extrapolation and "wide interpolation," but does not attempt to identify numerical criteria for this. There is no consensus on the bounds of interpolation that we are aware of**. The ISG has been revised to reflect that there is no specific guidance on interpolation, but that the reviewer should rather consider how quickly the physics is changing over the gap**.

7. There continue to be many instances of highly subjective terminology. How does one establish "substantially exceeds" or "substantially falls below." The use of grammatical superlatives is of little use to ISG users.	Disagree	While it would be ideal to have more mathematical precision, unfortunately the setting of safety margins is a somewhat subjective exercise. Many of the criteria discussed in the ISG cannot be readily quantified, and there needs to be room for individual license reviewers' judgement. We believe that the ISG as written cannot be made less subjective without the undertaking of a massive research program. However, it provides guidance that are expected to make determinations of margin substantially less subjective than in the past.
8. The ISG directs the user to ensure that experiments "cover all geometric forms...expected in applications." Why should geometry have any effect of the code's ability to track neutrons? The industry suggests deletion of geometric forms or addition of explanation text.	Disagree	In principle, the geometric arrangement of the system could have an effect on the code's ability to calculate k-effective. While this is theoretically true, the ISG does draw a distinction between different parameters, and even acknowledges that "other parameters, such as...overall geometric shape, are generally considered to be of less importance." The reviewer should exercise reasonable discretion in his or her expectations of how closely benchmarks can match applications. The commenter's example of trapezoidal geometry as something that would not be able to be used is unreasonable. However, there are cases in which geometric shape could have an effect on the code's bias. These include: 1) If a given material is an internal absorber, then its absorption cross section will be important. However, if the same material is a reflector, then its scattering cross section will be important. There could be an error in one and not the other. 2) The scale of material grains (i.e., dimensions relative to mean free path) could have an effect on whether it is acceptable to model a system homogeneously or whether small-scale homogeneity must be taken into account. This is important in the low-enriched regime. 3) The recent KENO errors were specific to certain geometric forms (i.e., they only affected cylindrical holes in one instance and symmetric or asymmetric slab cells in another). 4) It is often necessary to make geometrical approximations. It would be helpful to know how big of an effect they have. 5) Some codes (MCNP) may lose particles if intersecting surfaces are not defined correctly. It should be demonstrated that licensees are sufficiently skilled so that this does not occur. We believe these are adequately well-understood, and no changes are needed to discuss this in the ISG.

9. Experimental uncertainties associated with critical experiments are not always available for each experiment. For those experiments, NCS engineers reasonably assume that keff = 1.0000. This approach has been used for decades and should still be acceptable.	Disagree	The practice of setting uncertainties to zero because they are not known is not acceptable, and should not be done. Reasonably conservative values should be assumed to bound whatever uncertainty exists in such cases. No change needed.
10. The often-stated expectation in ISG-10 is that selected critical experiments should provide a continuum of data across the entire validated range. This is not always possible and the expectation should be removed from ISG-10.	Disagree	As with the discussion of conservatism, the context is that this is only important if the licensee is basing acceptance of its subcritical margin on its validation effort. This does not set a new standard for validation, but seeks to provide guidance as to how much margin should be employed. If the critical experiments do not cover the entire range of the AOA, then this merits additional margin. Failure to draw these distinctions means not being risk-informed. No change is needed.
11.Validation documents do not exist in a void. The three codes currently used to calculate reactivity have been in use for decades. When a licensee performs a validation for a specific application, experiments that most directly apply to their use are selected and others are left out. Exclusion of data outside a licensee's area of interest does not mean that data have no relevance.	Disagree	There is some danger that licensees could selectively choose only that data that gives desired results (e.g., a small margin). The expectation is that reviewers will examine which benchmarks the licensee has chosen to include in its validation, and it is appropriate for them to question this. Licensees should, in general, include any available data that is applicable to its operations, and should not choose some experiments from a given data set while ignoring other data without some defensible basis. No change is needed.
12. On page 1, the phrase "actual and expected critical conditions" could be read as "actual critical conditions and expected critical conditions." It should be changed to read "actual conditions and postulated critical conditions."	Agree	**The ISG has been revised to address this issue.**
13. On page 1, change "only applies to margin" to read "applies only to margin."	Agree	We don't see a meaningful difference. **However, the ISG has been revised to reflect this.**
14. On page 1, change "actual or anticipated facility calculations" to read "actual or anticipated facility conditions".	Agree in part	The word "conditions" does not make this more clear. However, we see there could be some difficulty with comparing "experiments" to "calculations." **The ISG has been revised to state "actual or anticipated facility applications."**
15. On page 1, does not the MMS also provide some margin for the uncertainty due to unknown physical variations in the system being analyzed? Recommend insertion of an additional phrase at the beginning of this sentence as to be consistent with the language on page 4: "The MMS is an allowance for any unknown (or difficult to identify or quantify) errors or uncertainties in the method...".	Agree	**For consistency, the ISG has been revised to add the words "(or difficult to identify or quantify)" to this sentence.**

16. On page 2, the ISG correctly asserts that the ability to model a process has improved through use of improved codes and improved versions of codes. However, the overall conservative nature of computer modeling has not changed. The technical justification of the MMS is also not risk significant, as the risk associated with code errors is extremely low. In addition, the statement that more substantial technical justification for reducing the MMS is consistent with the risk-informed approach is focusing risk-informed approaches on things that do not contribute significantly to risk. The entire second paragraph on page 2 seems inappropriate. It attempts to justify the need for ISG-10 again, and the justification is erroneous. Better codes only provide a better picture of the true reactivity, which only minimizes the risk-significance of the MMS. Also, the statement "considerable recent interest in decreasing margin" lack justification, as we are doubtful that more than 50% of fuel cycle licensees have submitted requests to decrease their safety margins. MMS is less risk significant than it has ever been, and truly it has never been that very risk-significant.	Disagree	The purpose of this paragraph is not to justify the need for ISG-10. The fact that the ISG exists means that we believe there is a need for it. The purpose is to put that need in the proper context. It is true that the risk has historically been low, but validation has been an area that has generated many problems in both inspection and licensing. The NRC's regulatory framework is both risk-informed and performance-based. Is it more appropriate to expend resources in high-risk areas in which performance has been excellent, or in areas of lower risk in which performance has been poor? We take both risk and performance into account. The experience of the NRC has been that we have had a number of amendments in which licensees have sought to decrease margins. These have been very difficult reviews, due to complexity of the issues involved. Complexity and difficulty does not always correlate closely to having high risk (fortunately). It is pointless to argue whether "considerable interest" requires a set quorum of our licensees. As computer codes get more sophisticated and easier to use, licensees are using them to model systems more realistically, and that, combined with inappropriate consideration of and basis for the MMS, is the crux of the issue.
17. On page 2, errors in the nuclear data or geometric or material models can indeed cause errors in the neutron flux and energy spectrum. They are not risk-significant; modeling errors by the analyst have a far greater risk-significance.	Agree in part	We agree that modeling errors can result in large errors in the calculated results. However, this does not mean there is no risk from errors in the nuclear data or that criticality code validation is not important. No change needed.
18. On page 2, recommend changing "physical" to "actual," in reference to k-eff values. Use of this adjective more accurately relates the calculated value to the experimentally determined conditions.	Disagree	The context is that we are not talking here about critical experiments, but about other systems for which the k-effective is not known. Addition of 'physical' was made because stakeholders thought that the original word ('true' value of k-eff) was misleading. 'Actual' sounds more like 'true' and is less clear. No change is needed.
19. On page 2, delete the word 'physical' as the word is redundant.	Disagree	See response on Comment 18. No change needed.
20. On page 2, the bias is not just the average calculated keff; an average of the calculated keffs is used to determine the bias. This would be true especially if a trend is evident in the data.	Agree	**The ISG has been revised to read "...the bias is determined <u>from</u> the average calculated keff...."**

21. On page 3, the ISG states that nuclear cross sections are not generally known to better than 1-2%. It then proceeds to use this to justify why the MMS should not be reduced below 0.02. However, the errors in the cross sections are accounted for in the bias and uncertainty. The ISG thus appears to be doubling up on cross section data errors. The entire paragraph should therefore be deleted. The ISG should also have provided a citation for the ~1-2% assertion.	Disagree	This statement would be valid if the benchmarks chosen provided complete coverage of all the cross section and other nuclear data. In reality, there is a finite number of benchmarks analyzed and they do not individually test the nuclear data points, but calculate an overall keff that is dependent on the cumulative effect of a very large number of data uncertainties (e.g., there may be thousands of different nuclide, reaction, and energy group combinations). The population of possible future calculations is extremely large compared to the number of benchmarks, so there is no guarantee that future calculations will be no more dependent on any given data point than any of the benchmarks. Therefore, an additional margin is appropriate. The logical extension of the comment would be that we should not require any margin, since bias and uncertainty accounts for any uncertainties in the method. We do not agree with this statement. There is no specific citation since there is a vast amount of nuclear data in the literature. The ENDF/B-VI cross section database is just one example of the resources readily available to substantiate this statement. No change is needed.
22. On page 3, the sentence "staff should recognize the important distinction between ensuring that processes are safe and ensuring that they are adequately subcritical" is wrong. If NRC determines that a system is subcritical under worst-credible conditions, then the system is safe. How does one determine that a system is "adequately subcritical?"	Disagree	See response to Comment 5. No change needed.
23. On page 3, the final two sentences asserting that MMS depends only on the confidence that applications calculated to be subcritical will be subcritical are erroneous. There are cases in which a licensee's ability to control parameters within certain bounds is appropriately assumed.	Disagree	The licensee should provide margin as needed to ensure that criticality limits are not exceeded. This, however, is not the MMS. The ISG distinguishes between the margin needed to provide confidence of subcriticality (MMS) and the margin needed to ensure compliance with criticality limits (operating margin). No change is needed.
24. On page 3, the "margin of subcriticality" is defined to be the difference between the actual value of keff and the value at which the system is expected to be critical. Does MMS represent the total of the bias, bias uncertainty, and arbitrary margin, or just the arbitrary margin? If it refers to arbitrary margin, the ISG is 'double dipping."	Disagree	The Glossary defines the terms "margin of subcriticality" and "minimum margin of subcriticality." These two terms were used to eliminate the confusion that was present in the first draft of the ISG. In particular, the MMS is defined to be "an allowance for any unknown uncertainties in calculating keff." This then cannot include the bias and bias uncertainty, since they are known quantities. Besides the Glossary, Appendix A contains equations that clearly show that the so-called arbitrary margin is equal to the MMS. We do not understand the reference to 'double dipping.' No change is needed.
25. On page 3, correct punctuation error as follows "...arguments, and, therefore, there will be..."	Disagree	The sentence as written is grammatically correct. No change is needed.

26. On page 4, improve the sentence to read "provide the margin between analyzed conditions and postulated critical conditions."	Disagree	The first change ('actual' to 'analyzed') conditions would not be appropriate, as the actual 'margin in keff" includes both the margin between the expected critical conditions and the margin between what is analyzed and what really exists. The change would render this discussion incoherent, as this second margin is what we are talking about in discussing modeling 'conservatism.' The second change ('expected' to 'postulated') decreases clarity. We used the word 'expected' because the concept is that the 'expected critical' value of keff is our best estimate of where some hypothetical system is actually critical. That is, it is 1-bias-uncertainty. No change is needed.
27. On page 4, the reference to ANSI/ANS-8.17-1984 should be updated to the 2004 version.	Agree	The NRC has now endorsed the 2004 version, in the 2005 version of RG-3.71. **The ISG has been revised to reflect this fact.**
28. On page 4, the sentence 'the conservatism should consist of conservatism beyond the worst-case normal or abnormal conditions' should be deleted. This goes beyond what should be required by a regulatory agency.	Disagree	See response to Comment 4. No change is needed.
29. On page 4, change sentence to "...materials, or assuming minimum reflector conditions...."	Agree	**The ISG has been revised to read "assuming minimum reflector conditions."**
30. On page 4, "These technical practices used to perform criticality calculations generally result in conservatism of at least several percent in keff" requires revision. The amount of conservatism seems to be grossly underestimated (generally 10-40%). Compared to technical ability of the NCS analysts and controls in the field, the MMS is insignificant.	Disagree	The NRC's experience has been that specific amounts of conservatism from the modeling practices is on the order of several percent. By using the phrase 'at least,' we include the possibility that in some cases it could significantly exceed this. Once we reach a certain point, however, additional conservatism beyond that is largely irrelevant. The second part of the comment was addressed in the response to Comment 16. No change is needed.
31. On page 5, the concept of 'verifying that this conservatism will be maintained over the facility lifetime' is inappropriate, as plant operations may change, thereby changing the conservatism and operating limits. Those sentences should require that these aspects be maintained as long as the assumptions in the calculation model.	Disagree	If a licensee uses modeling conservatism as part of the basis for its MMS, which is codified as a basic parameter for safety in the license, then there should be some assurance that it will be maintained during the period of time in which the license specified MMS is in effect. While the amount of conservatism may be changed, the licensee should not reduce it below the minimum amount credited in the basis for the MMS. If the licensee wishes more latitude to make future changes, then it should not base acceptability of the MMS on this conservatism. No change is needed.
32. On page 5, in accordance with the foregoing comment, revise the fourth bullet as "are the controls (license commitments) established by the licensee adequate to ensure that the assumptions or factors credited in the modeling practices that support the MMS will be maintained over the required period?"	Disagree	See response to Comment 31. Also, this change introduces the subjective term "required period," which is somewhat ambiguous. No change is needed.

33. On page 5, correct typographical error to read "...specified to account for any additional...."	Agree	**The ISG has been revised to state "for."**
34. On page 5, "...having a high degree of confidence in the bias and bias uncertainty requires...there be a rigorous validation methodololgy..." is unacceptably subjective. Industry suggests changing this to "requires... the validation employs an acceptable validation methodology and include sufficient applicable benchmark-quality experiments...."	Disagree	See response to Comment 4, on conservatism. The validation methodology may be acceptable to meet the regulations because it is in accordance with industry standards, but this does not mean it is acceptable to justify any specific margin. The concept that a more rigorous validation methodology provides a greater degree of confidence in subcriticality is sound and is risk-informed. While this may be subjective, so are the industry's proposed words "sufficient" as applied to benchmark-quality experiments. Starting on page 9 of the ISG, there is a more detailed and concrete discussion of how the degree of "rigor" can be evaluated. Many specific examples are provided and so no change is needed.
35. On page 5, correct spelling error of "results."	Agree	**The ISG has been revised to state "results."**
36. On page 6, rearrange the words for clarity, to read "...experiments having geometric form, material composition, and neutronic behavior similar to specific applications."	Agree	**The ISG has been revised to move the word "similar" to later in the sentence.**
37. On page 6, suggest replacing "supposed" with "expected" so as to read: "...characteristics that can reasonably be expected to affect the bias..."	Agree	**The ISG has been revised to replace "supposed" with "expected."**
38. On page 6, avoid use of the very poor English "It is" structure. Revise this from "It is also important that the..." to "the materials must be present...".	Agree in part	While the intent was to write this for the analyst, and this is a useful construction to highlight what the analyst should find particularly important, this sentence can be changed without affecting the intended meaning. **The ISG has been revised to state that "The materials must be present...."** However, we do not understand the comment that "it is" is poor English.
39. On page 7, replace "this may be justification for reducing the MMS" with "this may be justification for a small MMS." This implies that there is a predetermined MMS, which there is not.	Agree in part	The sentence has to be read in context. The previous sentence contains the phrase "for having a smaller MMS than would otherwise be acceptable." This was also the intended meaning in this sentence. However, it could create the impression that the reduction is relative to a predetermined MMS, which may (in the case of an existing licensee) exist, but may not. **The ISG has been revised to state "this may be justification for a smaller MMS than would otherwise be acceptable."**
40. On page 7, add the clarifying words "changes in process variables such as damage to the fuel or partial flooding."	Agree	The intent was not to provide a comprehensive list of the process variables that could change and thus affect applicability of the critical experiments, but it would be clearer to point out that the two examples provided are not all-inclusive. **The ISG has been revised to add the words "changes in process variables such as" to this sentence.**

41. On page 7, multiple comments on the second bullet: (41a) What is the basis for a ck>0.95 calculated by TSUNAMI and 0.90 not being acceptable? The TSUNAMI code has not been sufficiently tested over time. In addition, SCALE 5 has some errors that are now becoming known. How many undetected errors does TSUNAMI have?	Agree in part	We acknowledge that TSUNAMI has not been extensively vetted over time. This is why the ISG states that we "currently consider" a correlation coefficient >0.95 to be indicative of a very high degree of similarity. We do not say that 0.90 is not acceptable, but only that "a correlation coefficient less than 0.90 should not be used as a demonstration of a high degree of benchmark similarity." Acceptability for demonstrating inclusion in the validation is not the same as acceptability for demonstrating a "high degree of benchmark similarity". Our conclusions are based on NRC experience comparing TSUNAMI to other methods of comparing benchmarks in several licensing reviews, not all of which are publically available. We know there are errors in SCALE 5 (as well as earlier versions of SCALE), which is part of the reason we need MMS. We do not know how many errors TSUNAMI has. No change is needed, because we clearly show the tentative nature of the status of TSUNAMI, by stating it should not be used as a "black box" and stating that the licensees should not base acceptability solely on its use.
(41b) Guidance should be added for the region between 0.90 and 0.95.	Agree	This range of correlation coefficients was not discussed because it is an area where there is some uncertainty. However, this is a logical gap in the ISG. **The ISG has been revised to state that the staff currently considers a ck between 0.90 and 0.95 to be indicative of a high degree of similarity (as opposed to a "very high" degree), but that it should be supplemented with other methods owing to the tentative nature of TSUNAMI.**
(41c) The criterion that the correlation coefficient should be >0.95 is too stringent. There would be very few cases that can meet this.	Disagree	The NRC's experience is that there are many cases of benchmarks that have more than 0.95 correlation with postulated calculations. This is generally the case if they have the same material composition, roughly the same size, and very similar neutron spectra. We do not understand this comment, as it appears to contradict Comments 41a and 41b. We believe that, given the tentative nature of TSUNAMI, a conservative value for the correlation should be initially chosen. It is very possible that we will be able to accept a lower number once more experience and confidence is gained with the method. Reducing the threshold would mean we have greater confidence in the code.

(41d) There is also no substitute for a qualified NCS engineer's judgement. As industry noted in its February 2005 comments, the TSUNAMI code remains in development, and the NRC should not specify specific codes or tools and any whose use may be suggested must first be well tested and their validity established. Revise this bullet accordingly.	Disagree	Judgement is based on experience, and as fuel cycle operations move into areas in which there are few benchmarks (e.g., mixed oxide fuels, >5% enrichment), there will not be the same experiential base for professional judgement. It is the staff's experience that some of the findings of TSUNAMI are counter-intuitive, causing us to learn new insights. Also, there is a substitute for judgement in a vacuum, and that is informed judgement. The use of quantitative or analytical tools should be viewed as an aide to gaining better judgements through insight, not as a substitute to professional judgement. The ISG was revised to clarify that TSUNAMI is only one possible method that may be used. Is the industry suggesting that TSUNAMI not be used by licensees? There are two cases in our experience in which licensees have used TSUNAMI in drawing licensing conclusions. How will the industry gain experience with the use of TSUNAMI if its use is proscribed? It is appropriate to encourage licensees to make use of this code, with a high degree of caution, and in conjunction with other methods, until greater experience is gained. The NRC is here giving some preliminary guidance in its use, which will likely be revised over time.
42. On page 8, multiple comments on bullets 1 & 3: (42a) The bullets "Do the benchmarks cover all geometric forms, material compositions, and neutron energy spectra expected in applications?" and "Do the licensee's criteria...consider all nuclear reactions and nuclides that could affect the bias." contradict the ISG's acknowledgment that "...the experiments analyzed cannot cover all possible combinations of conditions or sources of error that may be present...."	Agree	This does appear to be inconsistent. The intent of the two bullets on page 8 was to make sure that the benchmarks provide adequate coverage of conditions that must be modeled. The intent of the other quotation was to indicate that, while total and complete coverage is the goal, it cannot be achieved perfectly, and thus there is a need for some additional margin (the MMS). **The ISG has been revised to state that "the benchmarks adequately span the range of geometric forms, material compositions, and neutron energy spectra" and "consider all nuclear reactions and nuclides that can have a statistically significant effect on the bias."**
(42b) The ISG directs the user to ensure that the experiments "cover all geometric forms...expected in applications." Why should the shape of the fuel have any effect on the code's ability to track neutrons?	Disagree	See response to Comment 8. No change is needed.
43. On page 8, the phrase "benchmark quality" should be hyphenated so as to be consistent with other uses in ISG-10.	Agree	**The ISG has been revised to read "benchmark-quality."**

44. On page 8, the reviewer should realize that most statistical methods will increase the associated bias uncertainty as the number of benchmarks decreases. Thus, a decrease in the USL may be due to less data as well as a narrowing of the AOA. The reviewer should also consider that "most applicable" and the reduced AOA may not cover possible abnormal conditions. Recommend addition of some explanatory text.	Agree	We agree that most statistical methods increase the uncertainty as the number of benchmarks decreases. However, there may be a minimum number below which the method cannot be used with any degree of reliability (e.g., because we cannot confirm normality). All the statements in the comment are correct. However, the intent of this section is to point out that just having a lot of data is not sufficient; it is very important that the data be applicable to the systems to be evaluated. The section entitled "Similarity of Benchmark Experiments" states that "the full spectrum of normal and abnormal conditions" must be considered. Therefore, no change is needed.
45. On page 8, reference to the IHECSBE handbook is missing from the list of references on page 18.	Agree	**The ISG has been revised to add the IHECSBE to the list of references.**
46. On page 9, experimental uncertainties are not always available for every documented critical experiment. For those experiments, NCS engineers reasonably assume keff = 1.0000.	Disagree	See response to Comment 9. No change is needed.
47. On page 9, item (1) seems to mandate that only a "cookbook" approach may be used and that validation analysts should not demonstrate and understanding of the various statistical tests or how choices of data grouping affect test results. It further suggests that validation analysts should not interpret statistical test results in a reasoned and logical manner. Correct these serious misunderstandings.	Disagree	While we do not want licensees to use statistical methods as a "black box," or to not take physical and neutronic considerations into account, it is necessary that statistical techniques be properly employed. This includes not disregarding what the statistics is telling you because 'everybody knows' the code is ok over some range. Preconceived notions can distort judgement. We do not understand the reference to "Item (1)," but the discussion that is on this page appears reasonable and we do not believe it implies a cookbook approach should be used. No change is needed.
48. On page 9, correct the verb to match the plural "data."	Disagree	"Data" may be either plural or singular. Both are grammatically correct. In this particular instance, the focus is on data not as an aggregate of distinct items but as an integrated set. No change is needed.
49. On page 9, change the verb to acknowledge that "data" is a plural word.	Disagree	See response to Comment 48. No change is needed.
50. On page 9, the explanation that "the critical experiments chosen should provide a continuum of data across the entire validated range" is impossible to meet unless licensees are expected to perform their own critical experiments. This expectation should be removed from ISG-10.	Disagree	The intent of this is that there should not be very large gaps in the data, so that it is possible to determine trends with high reliability. However, **the ISG has been revised to state that "the critical experiments chosen should ideally provide a continuum of data across the entire validated range."** This, in conjunction with the remainder of the sentence about observation of trends, should sufficiently address the concern.
51. On page 9, rearrange the following words for clarity: "...discrete clusters of experiments having a calculated keff lower than the set of critical experiments..."	Agree in part	The suggested wording does not change the intent of the ISG. **The ISG has been revised to move the word "lower" to later in this sentence.**

52. On page 9, delete the comma following "closely."	Agree in part	This is a matter of style, but does not change the intended meaning. **The ISG has been revised accordingly.**
53. On page 10, provide guidance for limits of interpolation.	Agree	See response to Comment 6. This has already been addressed, so no change is needed.
54. On page 10, remove the word "still" from "derived basis is still valid in the extrapolated range."	Agree in part	The suggested wording does not change the intent of the ISG. **The ISG has been revised to remove the word "still."**
55. On page 10, correct the punctuation as follows: "...data is still larger than, or comparable to, the MMS...."	Agree	**The ISG will be revised to add the commas as suggested.**
56. On page10, revise to acknowledge the plural form of "data."	Disagree	See response to comment 48. No change is needed. Here, the singular form of "data" is used because it is distribution of the data as a whole that must be normal, not the individual data points.
57. On page 11, what agency defines "acceptable standards for validation?" Some reference to a "standard" is required here.	Agree in part	Use of the word "standards" could be confusing. The Agency that approves what constitute acceptable standards is ultimately the NRC, which has the purview to endorse industry consensus standards. That is not, however, what is meant here. **The ISG has been revised to replace the word "standards" with "practices."**
58. On page 11, delete the "s" on "exceeds," since "criteria" is plural.	Agree	**The ISG has been revised to state that the licensee's "confidence criteria exceed the minimum accepted criteria."**
59. In paragraph 6 of the section entitled "System Sensitivity and Uncertainty," there are two comments: (59a) System sensitivity has nothing to do with MMS. A system may be very sensitive to one parameter, but that does not change the MMS and only changes how one controls that parameter. MMS is just the value with which one is confident to ensure subcriticality.	Disagree	We strongly disagree with this comment, which also contradicts many other previous comments. System sensitivity has been used in past licensing actions to justify margins. The basic principle is that if a quantity of interest (in this case, keff) is very sensitive to a given parameter, then errors in that parameter that you may not know about could have a large effect on the calculated quantity. But if the quantity is shown to be very insensitive to a given parameter, then even large errors will not significantly affect the results. This is the connection, and it has a sound regulatory and technical basis. No change is needed.
(59b) Further reorganization of the ISG is required. Suggest that this section be moved to the section on Validation Methodology and Results. The current organization is unnecessarily confusing.	Disagree	This comment contradicts previous comments that we were putting too much emphasis on validation. This topic is distinct and separate from validation, as is acknowledged later in the comment, and so it is unclear why there would be any benefit from combining them. No change is needed.
60. On page 12, the uncertainty expression erroneously assumes that all individual terms are independent. Generally, however, the terms are not independent. Correct this misunderstanding.	Disagree	Immediately before the equation, it states that the equation follows "if all the individual terms are independent." **However, the ISG has been revised to state that additional terms exist if the factors are not independent.**

61. On page 12, the ISG continues to characterize HEU and Pu as having "higher sensitivity to changes in parameters" than LEU systems. While the following discussion acknowledgs that such systems may benefit from smaller uncertainties in the U-235 cross sections, it does not go far enough in avoiding sweeping generalizations.	Disagree	The context is that this is mentioned as an example, with the qualifier "typically" employed. This is sufficient to avoid interpretation as a categorical statement. No change is needed.
62. On page 14, correct the expression "outside of specifications" to read "outside specifications." No need for two prepositions.	Agree	**The ISG has been revised to remove "of."**
63. On page 14, change "rigid and unchanging" to "fixed and unlikely to change," when talking about fixed geometry.	Agree in part	The proposed words do not change the intended meaning, except that the form should be more than "unlikely to change." **The ISG has been revised to state: 'Is the geometric form and material composition of the system fixed and very unlikely to change?"**
64. The purpose of the section entitled "Likelihood of the Abnormal Condition" is unclear. Why does the likelihood of an upset condition affect the MMS? This actually contradicts statements made in the fourth paragraph of the discussion section.	Disagree	This concept is as discussed in NUREG-1718. If the condition is very remotely unlikely, then we are willing to accept a somewhat higher risk that systems that are calculated to be subcritical will be critical. The total risk is: (1) the risk that the worst-case condition evaluated will be realized; and (2) that this condition will be critical. We do not believe there is any contradiction between this and the discussion of subcriticality and safety on page 3 of the ISG (which is what we think you are alluding to). That is meant to distinguish between different kinds of margin, while the abnormal condition section is balancing the risk of miscalculation with the risk of having the condition arise in the first place. No change is needed.
65. On page 14, insert the word "have" in "some facilities have been licensed."	Agree	**The ISG has been revised to reflect the addition of the word "have."**
66. On page 14, insert a comma after "permissible."	Agree	**The ISG will be revised to add the comma.**
67. Two comments on page 14: (67a) Correct punctuation to add commas, as in "...subcritical will, in fact, be critical...."	Agree	**The ISG has been revised to add the commas.**
(67b) The statement "There is some likelihood that processes calculated to be subcritical will in fact be critical; and, this likelihood increases as the MMS is reduced..." may be true, but its likelihood is extremely low. Recommend some acknowledgment of this observation in the ISG.	Agree	**The ISG will be revised to state that "There is some low likelihood that processes calculated to be subcritical will in fact be critical."**
68.On page 14, add commas as follows: "...subcritical is, in fact, critical."	Agree	**The ISG has been revised to add the commas.**
69. On page 14, replace the "It is" structure, so that "It is also true that..." reads as "There is generally more..."	Agree	**The ISG has been revised to begin this sentence with the words "There is generally more..."**

70. On page 14, there are two comments on the increased risk. (70a) Correct the punctuation to read "commensurate with, and offset by, the low likelihood..."	Agree	**The ISG has been revised to add the commas.**
(70b) The increased risk associated with having a lower MMS is very small compared to the likelihood of achieving the abnormal condition. Recommend some acknowledgment in the ISG.	Agree	See response to Comment 67b. No further changes needed.
71. On page 15, The second sentence about being risk-informed requires correction. If this activity were truly risk-informed, then the level of effort spent on validation and MMS would be small compared to other activities that contribute to risk.	Disagree	See response to Comment 16. No change is needed.
72. On page 16, spell out SER.	Agree	**The ISG has been revised to spell out "Safety Evaluation Report."**
73. On page 16, by definition (10 CFR 70.61), criticality accidents are high consequence events and their likelihood must be highly unlikely to satisfy the performance requirements. Comparatively speaking, the risk associated with code errors is very small.	Agree in part	Criticality accidents are only high-consequence events if they can exceed a dose of 100 rem. It is possible that in a shielded facility they would only rise to the standard of being intermediate-consequence events. However, notwithstanding the likelihood of a criticality accident, they must still be shown to meet 70.61(d), which is the regulatory basis of FCSS-ISG-10. No change is needed.
74. On page 16, it is recommended that "0.05 should be found acceptable for low-enriched fuel cycle processes" under certain conditions. There is no similar recommendation for HEU or Pu systems. Recommend that this sentence be reworded as "an MMS of 0.05 should be found acceptable for design applications with limited sensitivity to changes in process conditions if..."	Agree in part	The reference to HEU systems was removed in the current revision because of the numerous comments on the original draft about having a larger MMS (0.1) for HEU and Pu. Since there is no historical guidance on the acceptable margins for HEU or Pu systems, it is inappropriate to provide such guidance here. Rather, these must be evaluated on a case-by-case basis. **The ISG has been revised to clarify that no general guidance is provided for HEU and Pu facilities–to provide a "starting point" k-effective value–but that rather all of the factors and considerations in the ISG should be applied on a case-by-case basis.**
75. On page 17, why should the validation methodology "substantially" exceed industry guidelines. The term "substantially" is unnecessarily subjective.	Disagree	See response to Comment 34. No change is needed.
76. On page 17, the title for this subsection (Recommendation) is totally inappropriate. An ISG does not offer recommendations.	Disagree	This section offers recommended changes to the SRPs based on the position in the ISG. This follows the standard format for an ISG. No change is needed.
77. Revise the citation to the 2004 version of ANSI/ANS-8.17.	Agree	**The ISG has been revised to reflect the updated reference to ANSI/ANS-8.17-2004.**
78. Add a citation to the IHECSBE handbook to the list of references.	Agree	See response to Comment 45. No further changes needed.

79. On page 19, revise the reference to the 2004 version of ANSI/ANS-8.17.	Agree	**The ISG has been revised to reflect the updated reference to ANSI/ANS-8.17-2004**.
80. Inclusion of the material in Appendix A may infringe upon ANS copyrights. NRC's Office of General Counsel should consider obtaining ANS approval to duplicate this information.	Disagree	NRC regulatory guidance and technical documents routinely cite ANSI standards. There is no legal difficulty in doing so. No change is needed.
81. Appendix A contains several conflicting definitions and errors in the equations, including the following: (81a) The calculated value of keff is defined in two different ways. Early in the second section of this appendix, the calculated value of keff is said to exceed the actual physical keff value due to conservative assumptions in the modeling. The application keff would be defined as ks + delta_ks + delta_ksa, where ks = calculated keff, delta_ks = its uncertainty, and delta_ksa = contribution due to conservatism. Later, the keff for the application is listed as ks + delta_ks. The correct representation for the calculated keff for an application is ks + delta_ks + delta_ksa.	Disagree	ks is defined as the calculated value of keff, and delta_ks is the uncertainty in the calculated value of keff. These terms are defined in the second paragraph on the first page of Appendix A. The next section (the one referenced in the comment), entitled "Relation to the Minimum Subcritical Margin," introduces the term delta_ksa. We do not know what the commenter meant by "application keff," but if the commenter meant the actual (physical) keff, then the expression is misquoted. The correct expression is "ks - delta_ksa," not ks + delta_ks + delta_ksa." The expression is correct as written, and no change is needed.
(81b) The term in parentheses (ks + delta_ksa) is not the physical value of keff for the modeled system, since delta_ksa is added conservatism.	Disagree	See response to Comment 81. The term in parentheses is not "(ks + delta_ksa)," but "(ks - delta_ksa)." No change is needed.
(81c) When the terms for the USL are substituted into the middle equation as written, the MMS cancels out incorrectly.	Disagree	The USL is defined on the previous page as USL = kc - delta_kc - delta_km. If this is substituted into the "middle equation" on the following page, then the result is the top equation. It could be that the commenter has misread the equation, as in the previous two comments. No change is needed.
(81d) The minimum allowed value of the MoS is reached when the actual (physical) keff for a system (ks + delta_ksa) is equal to the expected value of keff for the system (kc - delta_kc). The statement in Appendix A is incorrect as the MMS is included in the USL.	Disagree	We do not understand the comment. The actual (physical) keff for a system is not "(ks + delta_ksa)," but is "(ks - delta_ksa)." No change is needed
(81e) Although the conclusion that the minimum MoS $ delta_km + delta_ksa is correct, the method used to obtain it is incorrect. The correct method would include MoS = ke - kp +delta_km + delta_ksa, where ke = (kc - delta_kc) and kp = (ks + delta_ks).	Disagree	The proposed equation is equivalent to the first equation in this section, with the exception that the commenter's proposed equation double-counts the MMS (delta_km). The expression as written is correct, because the MMS is part of the total MoS, not added to it. No change is needed.
82. In the Glossary, for consistency, NEI recommends that the acronym for this term be included in the glossary.	Agree	Whereas there are acronyms for MoS, MMS, and USL included in the glossary, this would be consistent. **The ISG has been revised to add the acronym AOA to the glossary.**

83. In the Glossary, for "Subcritical Limit," use of the phrase "bounding value" implies that this is the greatest possible value, rather than the greatest allowed value. Suggest replacing "bounding" with "limit."	Agree in part	While the context is that this is the bounding value "under normal case conditions," which should limit the potential for misunderstanding, we agree clarity could be improved in this definition. The suggested wording is still somewhat ambiguous. **The ISG has been revised to change "bounding value" to "maximum allowed value."**
Peter Vescovi		
1. In the Introduction, there are some inconsistencies between ISG-10 and NUREG-1520 and -1718. ISG-10 does not include the uncertainty in the bias in the definition of MMS. However, NUREG-1520 states that the margin must include, among other uncertainties, "adequate allowance for uncertainty in the methodology, data, and bias to assure subcriticality."	Agree in part	ISG-10 defines the MMS carefully, so that it is clear that this is an extra margin that is applied after the bias and bias uncertainty is taken into account (e.g., through use of equations). However, the margin referred to in this section of NUREG-1520 is not the MMS, but includes known uncertainties, including uncertainty in the bias. The ISG goes beyond the NUREG in separating out the various types of margins involved. However, we agree that the NUREG is not put in the proper context in this section, which could lead to confusion. **The ISG has been revised to reflect that NUREG-1520 is not talking about solely about the MMS, but that the MMS is just part of the "margin" discussed in NUREG-1520.**
2. ISG-10 introduces the terms "physical (experimental)" and "physical (actual)." The terms "experimental" and "actual" are not used consistently throughout ISG-10.	Disagree	By "physical" value of keff, we mean the actual neutron multiplication factor for a real system. This system may be either a critical experiment or some system that exists in the plant (or is hypothesized to exist). In general, unless one is modeling a critical experiment, one does not know the keff. In general, no attempt is made to infer it from an in-situ neutron multiplication measurement. ISG-10 was revised to clarify the difference between the known bias for a critical experiment and the estimated bias (based on similar known critical configurations) for an arbitrary system whose keff is not known. No further change is needed.
(3a) Revise "the bias is determined as the average calculated keff for a set of experiments" to read "The bias is determined using the actual calculated keff for a set of experiments." The bias is not just the mean keff.	Agree	See NEI Comment 20.
(3b) Revise "The expected (best estimate) critical value of keff is the mean keff value of all critical experiments analyzed (bias), including consideration of the uncertainty in the bias (i.e., kc - delta_kc)." to "The expected (best estimate) critical value of keff is the mean value of all critical experiments analyzed (i.e., kc), including consideration of the uncertainty in the bias (i.e., delta_kc)."	Agree	**The ISG has been revised to move the "kc" term to where it talks about the "bias," and replace "bias" with "kc," since bias is not necessarily just the mean keff.**

4. The ~1-2% uncertainty in cross sections does not necessarily translate into a 0.02 uncertainty in keff. Errors in the nuclear data would be incorporated into the bias assuming the critical experiments are properly selected. Also, it is not accurate to conclude that errors in the code will introduce 1-2% differences in keff based on recent errors in KENO-Va. There are examples of errors in computer codes that result in errors larger than 2%. Uncertainty in cross sections or computer codes should not be justification for the required MMS of 0.02. The physical relationship between keff and subcritical multiplication would be a better justification for the MMS of 0.02. The change in subcritical multiplication is greatest when keff is above 0.98.	Agree in part	We agree with both of these comments, except for the point raised about subcritical multiplication (See Comment 12 for a discussion of this). It is true that errors in the cross section of ~1-2% may lead to larger or smaller errors in keff. It is also true that differences in keff due to code errors may be larger than ~2%. **The ISG has been revised to provide a more defensible basis for the 0.02 minimum value of MMS.**
5. The examples provided on systems which have a large safety margin but a small subcritical margin (and vice versa) are misleading. Dry LEU powder systems may have a large margin of safety if robust process controls are in place to exclude water from the system. The examples should be revised to include likelihood. Also, the second parenthetical "(i.e., with keff << 1)" should read "(i.e., with keff ~ 1)."	Disagree	Margin of Safety is defined in the Glossary in terms of margin in system parameters. What the commenter is talking about is margin in terms of likelihood, which is a totally different concept. Two systems having the same physical parametric values would have the same margin of safety, even if one is controlled by passive means and the other by administrative means. No change is needed to address this. In the second parenthetical, the ISG does contain the expression "keff ~ 1." The commenter must have misread this.
6. The statement "The appropriate MMS depends only on the confidence that applications calculated to be subcritical will be subcritical" contradicts Section (3) ("The increased risk associated with having a smaller MMS for abnormal conditions should be commensurate with and offset by the low likelihood of achieving the abnormal condition.") It is also inconsistent with NUREG/CR-6698. The ISG is inconsistent with itself and with other regulatory guidance in distinguishing between the margin of subcriticality and the margin of safety.	Agree in part	We do not entirely understand this comment. The quote in the comment is not from NUREG/CR-6698, but is actually from FCSS-ISG-10. The intent of this sentence was to show that the MMS does not depend on margin needed to ensure that parameters are controlled within certain limits. This is thus distinct from the so-called "operational margin." The sentence as worded, however, could be misleading. Clearly the margin does depend on a large number of factors, which are discussed at length in the ISG. By stating that it "depends only on" the confidence of subcriticality, the ISG inadvertently excluded other considerations (such as risk of even attaining an upset state). **The ISG has been revised to state that "The MMS is intended to account for the degree of confidence that applications calculated to be subcritical will be subcritical. It is not intended to account for other aspects of the process (e.g., safety of the process or the ability to control parameters within certain bounds) that may need to be reviewed as part of an overall licensing review."**

7. The term "Upper Subcritical Limit" is not consistent with certain other regulatory guidance. The use of USL should be consistent.	Disagree	As the commenter pointed out, USL is defined as the "Upper Subcritical Limit" in both ISG-10 and NUREG/CR-6361, and is defined as "Upper Safety Limit" in NUREG/CR-6698. Since some guidance uses one term and some guidance the other, it is not possible to make ISG-10 consistent with all the regulatory guidance. Moreover, different licensees also use slightly different terminology. The term "Upper Subcritical Limit" was adopted to avoid confusion between the margin of subcriticality (related to the USL) and the margin of safety (which is a different concept). No change is needed.
8. With regard to statistical justification, it is not the intent of NUREG/CR-6371 to show that an adequate MMS has been used, but to demonstrate that the arbitrary margin is large compared to uncertainty in the bias. Recommend that the discussion section be revised to correctly state the intent of the USLSTATS methodology.	Agree in part	While we agree with the commenter's explanation of the intent of the USLSTATS approach, several licensees and applicants have misunderstood it. The purpose of this discussion is to address the method as it has been misapplied. We believe that the methodology is not being dismissed, as the commenter states, because the ISG acknowledges that the condition (i.e., that USL-1 < USL-2) is "necessary, but not sufficient, to show that an adequate MMS has been used." No change is needed.
9. The rigor of review for the MMS should not be dependent on the absolute value of the MMS, but rather on the MMS compared to the conservatism in modeling practices and the uncertainty in the bias. A more appopriate statement would relate the rigor of the review to the value of the MMS compared to the uncertainty in the bias and conservatism. In addition, evaluation should take into consideration how well the physical behavior, both normal and process upset conditions, of the system, is understood. An evaluation of a complex solvent extraction process may require a larger MMS than a simple storage tank.	Disagree	On page 10, the ISG reiterates that NUREG-1520 states that the MoS should be large compared to the uncertainty in the bias. A whole section addresses how to take conservatism in modeling into account. In the Summary Section, we are merely taking the position that smaller margins merit more regulatory scrutiny. We believe that this is a generally true principle. Some guidance needs to be given as to what an acceptable value of the MMS would be. Otherwise, reviewers have no common yardstick to enable shared understanding of what is appropriate. (This is similar to the SRP's use of quantitative likelihood guidelines for "highly unlikely" and "unlikely," so different reviewers will be in the same ballpark range of acceptability.) We believe the use of 0.05 as a starting point, followed by application of all the criteria discussed in the ISG, as applicable, is an appropriate approach to determining whether the MMS is acceptable. No change is needed. With regard to the second part of the comment, this concept of how well known the system is, is discussed at length in the Section entitled "Knowledge of the Neutron Physics" (though there the focus is on knowledge of neutronics rather than system behavior). If the concern is that the analyzed condition may not be bounding, then this could be treated by applying additional operating margin, rather than being part of the subcritical margin.

10. In the Appendix, the discussion of the relation of the USL to the MoS is confusing. In addition, refering to the term delta_km as MMS is also confusing, because of the reference to the minimum MoS as being delta_km + delta_ksa. ISG-10 recommends that "MMS may be assured either by conservatism in modeling practices or in the explicit specification of delta_km." Therefore, it is implied that MMS refers to a combination of delta_km and delta_ksa. Revising the last sentence to remove the "(MMS)" after the term delta_km would eliminate the confusion.	Disagree	The intent in the Appendix is to clearly state how the various terms relate to each other. In particular, the ISG states that "delta_km is the MMS." This is the intent. Also, "delta_km = an allowance for any additional uncertainties (MMS)." The commenter is mistaken on what the Appendix says. ISG-10 does not say that "MMS may be assured either by conservatism in modeling practices or in the explicit specification of delta_km." It states that "adequate margin (MoS) may be assured either by conservatism in modeling practices or in the explicit specification of delta_km (MMS)." We believe this is very clear and do not understand why the commenter stated this as above. No change is needed.
11. In the Introduction, rephrase "These two factors–the increasing interest in reducing the MMS and the reduction in modeling conservatism–make technical justification of the MMS more risk significant than it has been in the past." Also, rephrase "The geometric form and material composition of the system determine–together with the underlying nuclear data...–the spatial and energy distribution of neutrons in the system..." as "The geometric form and material composition of the system determine the spatial and energy distribution of neutrons in the system...together with the underlying nuclear data...."	Agree	**The ISG has been revised to state that "The increasing interest in reducing the MMS and the reduction in modeling conservatism make technical justification of the MMS more risk-significant than it has been in the past."** **The ISG has been revised to move the word "determine" after the aside.**
12. In general, ISG-10 infers a linear relationship between the keff and margin of subcriticality. The relationship between subcriticality and keff is nonlinear. The physical relationship is actually asymptotic, for example, M=1/(1-k), as shown in the attachment. An MMS of 0.02 provides a relatively large decrease in subcritical multiplication. Increasing the MoS beyond the MMS of 0.02 does not provide nearly the same decrease in subcritical multiplication.	Disagree	We do not understand what the commenter means by "the relationship between subcriticality and keff." It appears that the commenter may mean "subcritical multiplication," a phrase also used in the comment. However, "subcritical multiplication" is not the same as "subcriticality," by which we mean the state of being subcritical, or the extent to which the system is subcritical (either in terms of keff or other measurable quantities). We acknowledge the asymptotic relationship between the subcritical multiplication M and keff, but do not find this particularly relevant to the topic being discussed. The ISG does not assume there is a linear relationship between keff and either the subcritical multiplication (which it does not discuss) or the system parameters. The nature of the relationship between these quantities is not particularly important to determining whether the applicant's choice of MMS is appropriate. No change is needed.
Dennis Mennerdahl		

1. In "Validation Methodology and Results," the ISG states that "There are four factors that the reviewer should consider in evaluating the validation...(4) conservatism in the calculation of the bias and its uncertainty." This is an unfortunate choice of terms. It is better to refer to conservatism in the "bias correction" and its associated "uncertainty allowance." A bias is neither conservative nor non-conservative.	Disagree	The terms "bias" and "uncertainty" are standard industry terms, whereas "bias correction" and "uncertainty allowance" are not. The change would not improve the clarity of the guidance. It is not conservatism in the bias, but conservatism in the "calculation of the bias" that is being discussed–that is, conservatism in the method used to calculate the bias. No change is needed.
2. There is often a very clear understanding of the reason for positive bias. For instance, an older library may have a net positive bias for plutonium systems, while a newer library, with improved cross-section data, will not. This does not mean that the newer library is better, since it has not been as well validated and verified through constant use in many applications. The current proposal discourages better understanding of biases and uncertainties, including funding of new critical experiments.	Disagree	The prohibition on the use of positive bias is a historical NRC position, although we understand that the DOE position allows the use of positive bias. NRC has never allowed use of positive bias, because it is allowing credit for an error in the method that is not, in general, well-understood, and may not be present in every case. We do not see a need to change the historical NRC position at this time. No change is needed.
3. In the phrase "Are there discrete clusters of experiments for which the bias appears to be non-conservative?", it is the bias correction and not the bias that determines if the information is used conservatively or not.	Disagree	We do not entirely understand this comment, though it seems that the commenter wants the NRC to use to term "bias correction" instead of "bias." See response to Comment 1 above. No change is needed.
4. The equation for the k-effective sensitivity is wrong. This requires that k-effective is a sum of independent terms involving the various variables. K-effective is not a simple sum or product of independent local variables. The only way to write k-effective as a sum of terms is to define each term as a function of the neutron flux in addition to other variables. None of the terms are independent. Evaluation of individual k-effective uncertainties and using them to determine combined uncertainties could be very misleading.	Disagree	The equation referred to does not have any particular physical insights. It is merely the standard error propagation equation, which applies to any continuously smooth function of several variables. Rather than assuming that k-effective is a product of independent terms, we are merely assuming it is a smoothly varying function of a very large number of variables. Experience shows that, in general, k-effective is a smoothly varying function of the continuous system parameters and is a function of a very large number of variables. The assumption that the uncertainties in these variables are independent is made for the sake of simplicity. The purpose of presenting this equation is to show that each variable's contribution to the overall k-effective uncertainty depends on the product of the uncertainty and the sensitivity. This formula is widely used throughout the entire scope of mathematics, science, and engineering, and is considered valid when the uncertainties are small and are independent. The formula is valid, and no change is needed.

Summary of Actual Changes

Changes below are identified by the comment number and summarized to show how the comment was addressed.

NEI-3: The term "benchmark experiment" was used rather indiscriminately, excluding other types of critical data. To clarify, this has been replaced by "critical experiment" almost everywhere in the ISG. The ISG now states that benchmark-quality experiments are preferable, but that there may be some cases in where there are not sufficient benchmarks. In such cases, additional margin may need to be employed.

NEI-6: Added a statement on page 10 that there is no specific guidance on the limits on interpolation, so that the reviewer should evaluate how rapidly the physics is changing over the area of interest.

NEI-12: Added the word "conditions" on page 1.

NEI-13: On page 1, changed "only applies" to "applies only."

NEI-14: On page 1, changed "calculations" to "applications."

NEI-15: On page 1, added the words "(or difficult to identify or quantify)."

NEI-20: On page 2 (now 3), changed "as" to "from."

NEI-27: References to ANSI/ANS-8.17-1984 (R1997) have been changed to ANSI/ANS-8.17-2004. The new version has now been endorsed in the 2005 version of RG-3.71.

NEI-29: On page 4, changed "requiring" to "assuming."

NEI-33: On page 5, changed "or" to "for."

NEI-35: On page 5, changed "resutls" to "results."

NEI-36: On page 6, moved “similar” to later in the sentence.

NEI-37: On page 6, changed “supposed” to “expected.”

NEI-38: On page 6, removed the beginning of the sentence beginning: “It is also important that....”

NEI-39: On page 7, replaced “reducing the MMS” with “a smaller MMS than would otherwise be acceptable.”

NEI-40: On page 7, added the words “changes in process conditions, such as....”

NEI-41b: A statement addressing correlation coefficients between 0.90 and 0.95 has been added to page 7, along with a few other editorial changes to make this section flow better.

NEI-42a: Changed the questions on page 8 to acknowledge that it is not always possible to provide complete experimental coverage. Revised the wording in the first and third bullets.

NEI-43: On page 8, hyphenated “benchmark-quality.”

NEI-45: Added the IHECSBE to the list of references.

NEI-50: Added the word “ideally” to the sentence about “a continuum of data” on page 9.

NEI-51: On page 10, moved the word “lower” to later in the sentence.

NEI-52: On page 10, the comma after “closely” has been deleted.

NEI-54: On page 10, removed the word “still.”

NEI-55: On page 11, commas added.

NEI-57: On page 11, replaced “standards” with “practices.”

NEI-58: On page 12, changed “exceeds” to “exceed.”

NEI-60: On page 12, added a parenthetical to further clarify that the variables must be independent, or else the equation will have additional terms.

NEI-62: On page 14, replaced “outside of” with “outside.”

NEI-63: On page 14, changed “rigid and unchanging” with “fixed and very unlikely to change.”

NEI-65: On page 15, added the word “have.”

NEI-66: On page 15, added a comma after “permissible.”

NEI-67a: On page 15, added commas after “will” and “in fact.”

NEI-67b: On page 15, added the word “low” before “likelihood.”

NEI-68: On page 15, added commas after “is” and “in fact.”

NEI-69: On page 15, replace “It is also true that” with “also.”

NEI-70: On page 15, added commas after “commensurate with” and “offset by.”

NEI-72: On page 16, spelled out “Safety Evaluation Report.”

NEI-74: Added a parenthetical expression on page 16 to address the MMS for high-enriched and plutonium facilities.

NEI-77: This has already been addressed in NEI-27.

NEI-79: This has already been addressed in NEI-27.

NEI-80: *No change has been made to date–need to discuss with OGC.*

NEI-82: The acronym “AOA” has been added to the Glossary.

NEI-83: In the Glossary, “bounding” has been changed to “maximum allowed.”

Vescovi-1: On page 2, added a clarification that the “margin” discussed in NUREG-1520 is not just the MMS, but the MMS is part of this margin.

Vescovi-3b: On page 21, rearranged the terms k_c and Δk_c for clarity.

Vescovi-4: The discussion of the basis for the minimum acceptable MMS of 0.02 has been entirely rewritten, on page 3.

Vescovi-6: On page 3, change the sentence about what the MMS is intended to account for so as to not exclude consideration of other factors.

Vescovi-11: Removed the beginning of the sentence beginning “These two factors...” and moved the word “determine” on page 3.