Response to Comments of Referee #2
"Behavioral Measures and their Correlation with IPM Iteration Counts on Semi-Definite Programming Problems" by R.M. Freund, F. Ordóñez, and K-C Toh

1. *Use of SDPLIB suite versus Gaussian random problem instances for test set.* The SDPLIB has the advantage that it is more likely representative of the types of SDP problems that are solved in practice, but as we point out, this suite (and probably the universe of practical problems as well) has some systematic bias. Testing via randomly generated problems can be done so as not to introduce bias, but has the obvious disadvantage of being quite distinct from the problems one solves in practice. Furthermore, randomly generated problems are very likely to be well-conditioned, have good aggregate geometry measure, and exhibit strict complementarity with very high likelihood, as a number of theoretical papers have shown. Thus, to account for the wide variation in the four behavioral measures, we would have to develop and use methods to generate "badly behaved" problems which would then lead to other biases. For these reasons we feel that the disadvantages of the SDPLIB suite are much less than the advantages we might gain from using a suite of randomly generated problems.

2. *the* $\| \cdot \|_{E1}$ *norm.* This norm is called the "Ky Fan norm" in Bhatia's text *Matrix Analysis*, which we cite as reference [3] in the revision. We discuss this on page 4 at the end of Section 1 and give the reference to [3].

3. *dual norms.* Technically, we were consistent because in the opening line of Section 2.3 we state "Given a norm $\|z\|$ on the space of dual variables...." But it would be more consistent with the tracking of primal and dual norms and variables to instead call this $\|z\|_*$. Of course, the only norm that leads to a computationally tractable way to compute D_d^ε is the norm defined in (7), which we had already called $\| \cdot \|$ so we stuck with that notation.

4. *a priori versus a postiori estimator.* Thank you for pointing out that some of our statements might be misleading. We have amended the text to this effect in the abstract, the introduction (third paragraph), and in the discussion on the bottom of page 10.

5. *possible inaccuracy of estimate of* $C(d)$. For the 48 problems with finite $C(d)$, the ratios between the upper and lower bounds on $C(d)$ are all less than 20.4, see Table 6 in the paper. Therefore the estimate of $\log(C(d))$ used in the paper can differ from

the true value of $\log(C(d))$ by no more $0.65 = \log_{10}(\sqrt{20.4})$. We actually think that this is pretty accurate. Looking at Figure 3, moving the dots horizontally somewhat randomly to the left and right by at most 0.65 should not appreciably change the essence of the graph nor the resulting sample correlation value. This is mentioned briefly in the revision on page 15.

6. *local nature of non-strict complementarity and degeneracy.* Thank you for highlighting this fact. We mention this in the revision at the end of Section 4 on pp. 19-20, at the end of Section 5 on page 23, and in the last paragraph of the paper on pp. 25-26. Based on your comment, we did some preliminary testing that indicates some modest correlation between κ and the local rate of convergence, but no such correlation between γ and the local rate of convergence. This is now discussed, albeit briefly, on pp. 19-20 and page 23.