This paper is rather problematic on several fronts and I do not think it should be published.

First of all, the reader is inundated by a number of fits, most of which are ruled out by their p value. Even the best fits have a probability of less than 2%, so the paper rules itself out at the 98% confidence level.

Even then, the choice of a 1 sigma interval for the $\chi^2$ fit explained on the top of p. 6 seems unconventional, and different from the usual $\chi^2$ distribution (explained e.g. in the particle data book). Hence I suspect that the standard definition would lead to even lower p values.

Secondly, the author claim they are inspired by QCD, but then proceed by totally ignoring what we know:

A. At small x, interference terms are very important, and the fact that these terms are totally neglected surely must lead to wrong results. Scattering amplitudes in QCD have no IR divergence if calculated properly, contrarily to what is stated on p.2.
B. There is no reason to believe that QCD obeys the eikonal unitarisation. Only structureless objects can obey the eikonal, hence it is wrong for protons.
C. As the authors explain on p. 7, the extremely smqll x behaviour is what matters. Hence using existing parametrisations, that are admissible above $10^{-5}$, down to $10^{-10}$ has no meaning.
D. The authors use the old Cornwall parametrisation for the effective gluon mass. but it seems to be mainly a way to introduce a cutoff in the minijet model so that it does not diverge. Equations 22-24 have no trace of that mass and it seems to come only in the kinematics.
E. It is hard to understand why Eq. 25 is used instead of Eq. 6. “For simplicity” is not a scientific reason, especially as Eq. 6 is rather simple in itself.

Given all these problems, I do not think this work is publishable.