Response to Referee report:

First some general points:

1) The review concentrates very heavily on jet and W/Z boson production, separately or in association. There is very little discussion of other important hard processes, notably heavy quark, photon and Higgs production, except when predictions for some of these appear for the LHC, without any previous discussion of the theory or Tevatron data. The authors should at least make it clear that they are concentrating (for lack of space?) on a subset of hard processes.

The purpose of the review is to serve as a primer, and not as a review for all collider physics. We did not want to go into more detail given the length restrictions of the article. With the editor's permission, however, we did add more discussion of heavy quark physics, Higgs and top production, bringing the length of the article to of the order of 120 pages.

2)In such a long article, a table of contents is essential – presumably this will be provided in the journal, but it should also be included in the preprint version.

We added one.

3) The authors have a habit of saying "As previously discussed...", "As mentioned previously...", etc., without saying where. This leaves the reader to scramble around trying to find the relevant previous discussion -- especially annoying with no table of contents. In every case, such phrases should be replaced by "As discussed in section 5.2.1..." (not just "...section 5..."), etc.

We have added in specific section references where appropriate.

I now turn to specific points, page by page:

p.2: The "free companion website" is referred to as "the benchmark website" throughout the paper. The reader should not have to guess that these are the same thing.

That has been made explicit.

pp.4-6: It could be confusing to non-experts that "y" is used in the DGLAP equations and then to denote rapidity.

The variable has been changed to z.

p.6: tau is not defined and appears unnecessary.

It has been removed.

p.9: s,t,u here become s-hat etc elsewhere (and s later become the full collision energy-squared). The authors should make their notations consistent.

We have.

p.11: The discussion after eq.(16) is confusing: it is not true that the
limit pt --> 0 corresponds to s-hat --> Q2. The authors are referring to the soft and collinear
limit, where both t-hat and u-hat --> 0.

We have modified the discussion.

p.14: In eq.(20) the strange notation (D2+D3)(D1-D3)^\dag should presumably
read Re[(D2+D3)(D1-D3)^*]

It's been changed accordingly.

p.15, fig.9: The direction of colour flow is wrong.

Oops, changed.

p.15: We are told that it is "useful to rewrite" (25) as (26), without any mention that
this exponentiation of logs is highly non-trivial and indeed cannot be done for many jet definitions.

A footnote to that effect has been added.

p.17: I cannot make any sense of the unnumbered equation here. Surely the whole point is that sigma_NLO cannot depend on L.

It has been revised.

p.19 D=4-2\epsilon: The reader should be reminded that epsilon < 0 to regulate the singularities under discussion here (but the convention has changed by eq.(29)!)

We have regulated the definition of epsilon.

p.20 "parton shower programs": is the reader supposed to be familiar with these already?

We added a statement referring to the discussion in a later section.

p.22: First we are told the NLO peak is "...usually near the scale at which the LO and NLO curves cross", then "A different strategy is to pick the scale at which the LO
and NLO coincide", and finally "...these two scales are rather different –around 10 and 200 GeV". So what is the point of saying they are "usually near"?

The discussion has been revised.

p.23 Table 1: is mu_R = mu_F here?

Yes, this is now stated explicitly.

p.27, after eq.(35): "... the p_T distribution vanishes" should read "... the p_T2 distribution vanishes" -- the former is trivially true due to kinematics.

The statement has been revised.

p.28 "..mass of the colour string...": Is the reader supposed to know what this means?

The wording has been changed to "…invariant mass of the colour connected partons."

p.29 "...the method omits the non-singular contributions...": "miscalculates" would be more accurate than "omits".

The wording has been changed.

pp.29-31: What is the resolution/cutoff scale in figs.18-21?

See below.

p.30: "...without emitting a gluon of 10 GeV/c or greater..." suggests the cutoff is 10 GeV (in p_T?) but I cannot believe it is that large. The authors should refer to ref.[82] for the correct value.

Yes, it is. Ref. 82 was written at the suggestion of one of the authors of this review. The numbers for the figures in question came from the author of Ref. 82 and it was confirmed with him that they are correct.

p.33: Why are there no citations for the k_T algorithm?

We've added a reference.

p.38: I think a new paragraph should start at the bottom, at "All global analyses..."

Done.

p.41: I could (just about) follow the procedure in eq.(38), but not the
comment "Note that... added together" underneath. This needs more explanation.

More explanation has been added.

p.42, fig.26: Presumably the x axis is jet p_T?

Yes, this has been made explicit in the figure caption.

p.46 "...is guided by the ratio...": a reference to eq.(36) would be helpful.

Added.

p.50 "Pythia and ResBos": a reference to sec.3.4 would be helpful.

Added.

p.51, fig.38: Some of the curves and legend are almost invisible in black-and-white. There is no legend at all for the Pythia curves.

Not available in another form. Wording has been added to the legend.

p.52, fig.40: What is a ttbar plot doing here? If this process is to be discussed at all, it should be done properly.

We weren't discussing the process per se but rather the use of DY data to determine the ISR uncertainty. In any case, we have re-arranged the discussion in the expanded version.

p.52 "...logarithmically...as predicted by (18)...": As it stands, this statement appears to me to be mathematical nonsense.

Wording has been changed.

pp.53-70: Do the authors really intend 5.2.1 and 5.2.2 to be subsubsections of 5.2 "Underlying event"?
Sec. 5.2.1 is in fact extremely long and rambling and would benefit from radical pruning. In my opinion it
is too detailed for a review of this type. Shortening it would make room for the other important hard processes mentioned above.

No, that was a TeX error. It has been corrected.

We felt the discussion important for a primer as many of the readers may not be aware of these issues. These issues involve all analyses involving jets. We did shorten this section somewhat, however.

p.57, last line: "Section 2" should read "Section 3.2.2"

Changed.

p.61: The word "spatial" should be replaced by "angular" or "(y,phi)". The spatial distribution of the partons is certainly irrelevant!

p.62: "...energy distributions..." should presumably be replaced by "angular" or "(y,phi)" again. Or is energy smearing really intended here?

The wording has been clarified.

p.63 "R_sep": a reference back to sec.3.5 would be helpful.

Added.

p.66 "JetClu": how is the reader supposed to know what this is?

We have changed the wording to "…the CDF Run 1 cone algorithm."

p.68 "An estimate...Section 3.": This is not a sentence -- there must be words missing.

The wording was changed.

p.69, fig.60: The histograms are indistinguishable in black-and-white. Also, why are there no error bars on the data? How is one supposed to assess the success of the predictions?

Only a preliminary version of this plot was available at the time of the review. The final plot should answer the criticisms.

p.70-71, figs. 61 and 62: Histograms are indistinguishable in black-and-white.

These have been changed.

p.74: "a particular value of" should presumably read "a particular cut on".

The wording has been changed.

p.75, fig.68: it would be helpful to label the different families of

curves, or at least to add "(right to left)" before "y=..." in the caption.

Wording has been added.

p.77: Here again it is surprising that top quark production is mentioned so peripherally.

More discussion of top production has been added.

p.79, heading of section 6.3: "Sudakovs" --> "Sudakov form factors"."...gluon of 10 GeV/c...": see the comment on p.30 above.

The heading has been changed.

p.83 "MHV rules": a reference back to sec.3.1.3 would be helpful.

Added.

p.86 "...two figures below...": in my copy they are above, not below.

We have removed the reference.

p.87 (and elsewhere) "...producing a Higgs of this mass...": I may be
fighting a losing battle, but I find the use of "Higgs" as a noun objectionable. It has no precedent as far as I am
aware. The use of "Higgs" as an adjective: "Higgs mass", "Higgs cross section", "Higgs
production" etc. is fine, by analogy with "Drell-Yan process" etc. However, "producing a Higgs" is surely the prerogative of Mrs Higgs. Is "Higgs boson" considered too cumbersome?

We have changed the usage to Higgs boson where possible.

pp.89,90, figs. 85 & 86 are too small, with too many curves, many indistinguishable in black-and-white, legends illegible.

Figure 85 has been replaced. Figure 86 is not available in any other form.

p.90: "1200 GeV/c" --> "12 GeV/c"

It has been corrected.

p.91: The terms "q_T" and "p_T" are used for the same quantity, sometimes in successive sentences.

We have tried to standardize the usage.

p.91 "... (like BFKL)...": Is the reader of this "primer" supposed to know what this means? Likewise "...Collins-Soper-Sterman formalism...".

We have added references and additional discussion.

p.96: In the Abstract the authors stated they will include "rules of thumb" and "official recommendations" and "dispel some myths". There are indeed many such contributions scattered throughout the text, and I think it would be very useful to list them in the summary. Certainly at present the summary is not helpful.

We have explicitly summarized some of the recommendations and rules of thumb.

p.97: I cannot believe that the UK authors approved the frivolous final acknowledgement.

Removed.