Jennifer Pursley
103 S. Villa Ave 17
Addison, IL 60101

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APS Editorial Office
1 Research Road
Ridge, NY 11961-2701

Dear PRL editors,

We appreciate the timely return of the refereed comments on our manuscript LF11708, entitled "First Observation of Heavy Baryons Σ_b and Σ_b^*." We have reviewed all recommendations and made changes where appropriate. Our responses are included in this letter and indicated by the bold font text. We hope our responses adequately address all concerns.

Sincerely,

Jennifer Pursley
On behalf of the CDF Collaboration
(630) 840-8619
jpursley@fnal.gov

Responses to Referee A

This paper from the CDF collaboration presents a search for states decaying to $\Lambda_b^0\pi^+$ and $\Lambda_b^0\pi^-$. Any observed states would almost certainly be the charged members of isospin-triplet Σ_b baryons (with bdd or buu quark content). Structure is observed in the distribution of the measured Q value for the candidate Λ_b^0 pi decays, where Q = m(Λ_b^0 pi) - m(Λ_b^0) - m(pi), between Q values of $\sim$ 40 and 100 MeV. In Table I, the authors present the significance of the four states estimated from the observed change in the ratio of likelihoods with and without each state included in the fit, and a Monte Carlo study, where the simulated samples are generated with alternate hypotheses. This is an appropriate method for estimating statistical significances. The authors report parameters for four new states, which are likely the ground states and first excited states of the charged Σ_b baryons.

Actually all four predicted states (both Σ_b and Σ_b^*) are ground (S-wave) states. The first excited states would be those in which the light diquark system has non-zero orbital momentum ($L = 1$), and are expected to be much heavier and wider than the ground states.

The hypothesis that no states exist is rejected at > 5.2 sigma; the hypothesis that each one of the four states does not exist (but the other three do exist) is rejected at 3.4, 2.4, 3.2 and 3.2 sigma. These are the first reported results on Σ_b states. I recommend that the paper be published after the authors have addressed some questions and clarified some descriptions in the text.

Analysis issues:

I have one concern with the analysis and this is the fact that the authors 'optimized' their selection criteria using, as an estimate for the expected background in the signal region, the events in DATA immediately above and below the region in which signals are predicted by theory. This procedure can result in selection criteria that selectively remove events in these sideband regions, and therefore lead to an underestimate of the background in the signal region because these sideband regions are used by the authors in the fit. In this analysis, the potential risk for inadvertent bias due to using the sideband data in choosing the selection criteria is reduced somewhat by the fact that the total sideband region is $\sim$10 times as wide as the signal region. However, it would have been preferable if the authors had used sidebands for choosing selection criteria that were not ultimately used in the fit that estimates the signal yields.

We believe this is a standard approach, and as the Referee acknowledges, the sideband region is much larger than the signal region. Also, our background shapes are determined from a combination of data and Monte Carlo after determining the final selection criteria but before looking in the Σ_b signal region. From these samples, we saw that the backgrounds were smooth in the signal region, with no sign of bias, before we looked at the actual Σ_b signal. Had we seen a bias, we would have re-evaluated selection criteria before continuing.

The authors should give more details on the signal shapes, parameters and parameter uncertainties used in the fits.

1. What kind of Breit-Wigner is used? Perhaps a non-relativistic S-wave BW? Did the authors try a relativistic BW (with a mass-dependent width)? Did they try a P-wave? If the exact form of the

BW makes no difference, than this should be mentioned in the section of systematic uncertainties.

We use a non-relativistic S-wave Breit-Wigner with a fixed width. The width is predicted by the formula from Korner, Kramer, and Pirjol, which takes into account the appropriate P-wave behavior. Thus we did not try fitting with a P-wave BW. Due to the low statistics of the sample and how close the resonances are to the threshold, we also did not try fitting with a relativistic BW and we do not expect it to have any effect.

2. In paragraph 11, the authors state that the "natural width of each BW distribution is computed from the central Q value [8]." The resultant natural width of each state should be given in the text. (The range given in paragraph 3 is very broad: 2 to 20 MeV.) Also, the detector resolution on the Q value is not given in the text. The reader should be informed as to whether the natural width or the detector resolution dominate, or whether they are roughly equal.

We agree with the Referee's suggestion to add the natural widths, suggested as well by Referee B. The average Σ_b and Σ_b^* widths have been added to the end of paragraph 3. The sentence previously read: "For the range of predicted $\Sigma_b^{(*)}$ masses, the natural widths $\Gamma(\Sigma_b^{(*)})$ calculated from an HQET prediction vary between 2 and $20\ \mathrm{MeV}/c^2$ [8]." The sentence now reads: "Using an HQET prediction [8], the natural widths for the expected $\Sigma_b^{(*)}$ masses are $\Gamma(\Sigma_b^{\pm}) \approx 7\ \mathrm{MeV}/c^2$ and $\Gamma(\Sigma_b^{*\pm}) \approx 13\ \mathrm{MeV}/c^2$."

We agree with the Referee's comment to add the detector resolution, also suggested by Referee B. The detector resolution on the Q value has been added to paragraph 11. The sentence previously read: "Each signal consists of a non-relativistic Breit-Wigner distribution convoluted with two Gaussian distributions describing the detector resolution, with a dominant narrow core and a small broad component for the tails." The sentence now reads: "Each signal consists of a non-relativistic Breit-Wigner distribution convoluted with two Gaussian distributions describing the detector resolution, with a dominant narrow core of an $1.2\ \mathrm{MeV}/c^2$ width and a small broad component of a $3\ \mathrm{MeV}/c^2$ width for the tails."

3. In paragraph 12, the authors mention systematic effects due to "the uncertainty in the natural width prediction from [8]". The reader should be given some quantitative estimate of this uncertainty. Is it $\sim$1%, $\sim$10%, $\sim$50%?

The difficulty with quoting percentages for the systematic effects is that these uncertainties are calculated for each measured value (three mass difference values and four yields). The quantitative estimate of this effect is not the same for all measured values. The systematic due to the natural width prediction ranges from an $\sim$0.02% uncertainty on the mass difference measurements to an $\sim$6% uncertainty on the yield measurements.

The authors should consider giving the mass differences between the Σ_b^+ and Σ_b^- (or equivalently between the Σ_b^{*+} and the Σ_b^{*-}, given the constraint on the mass difference between the Σ_b and Σ_b^*) since it will be difficult for anyone but the authors to extract these results, given the statistical and systematic correlations.

We agree that this is an interesting quantity, and we have calculated it with the correct statistical and systematic correlations. However, we have limited space in this paper for quoting

additional results. Due to the low statistics of our current sample, there are large uncertainties on this quantity as well. We prefer to leave the Σ_b^+ and Σ_b^- mass difference out of this paper. The mass difference should be included in a future paper with a larger data sample.

Editorial feedback and points of clarity:

In the abstract, the authors should consider stating that the mass splittings between the Sigma and Sigma* were fixed to be the same for the positive and negative states. Otherwise, it appears that this is a result of the analysis, rather than a constraint that was imposed.

Based also on comments from Referee B, we have modified the abstract to quote only the Σ_b^+ and Σ_b^- absolute mass values, and the $\Sigma_b^* - \Sigma_b$ mass splitting. We think this makes it clear in the abstract that the $\Sigma_b^* - \Sigma_b$ mass splittings are fixed to be the same.

In paragraph 1, last sentence, is it necessary to say "Unless otherwise noted"? I don't think it is ever otherwise noted in the text.

We agree with the Referee's comment, and the phrase has been removed.

In paragraph 2, it is not obvious which noun the phrase "which couples to the heavy quark spin" refers to.

The phrase was meant to refer both to the isospin and light quark total angular momentum, and the sentence has been modified to reflect this. The sentence previously read: "…the diquark system has strong isospin $I = 1$ and $J^P = 1^+$, which couples to the heavy quark spin and results in a doublet of baryons…" The sentence now reads: "…the diquark system has strong isospin $I = 1$ and $J^P = 1^+$, which couple to the heavy quark spin and result in a doublet of baryons…"

In paragraph 5, it is stated that the cuts on p_{T} are made to "ensure well-understood tracking efficiency". Why is this important for this analysis, given that the efficiencies are not used to extract any results?

We think that efficiency was a poor choice of word in this context. A p_{T} cut of $500\ \mathrm{MeV}/c^2$ is standard in the CDF B physics group for displaced heavy hadron candidate tracks that are not trigger tracks, primarily because the Monte Carlo detector simulation does a better job of reproducing detector behavior for tracks above $500\ \mathrm{MeV}/c^2$. The Λ_b^0 selection criteria were also optimized as part of CDF's Λ_b^0 branching ratio measurement (A. Abulencia et al., The CDF Collaboration, Phys. Rev. Lett. 98, 122002 (2007)), and it was found the tracks below $500\ \mathrm{MeV}/c^2$ contributed mostly combinatorial background.

We feel this information is not very relevant to the analysis, and too detailed for this paper. Thus we have removed the phrase "to ensure well-understood tracking efficiency" to avoid the confusion of such a statement.

The definition of L_{xy} could be made clearer. (For example, avoid using "transverse plane vector".)

We agree that the definition could be more clear, and have modified it as follows. The sentence previously read: "$L_{\mathrm{xy}}(\Lambda_b^0)$ is defined as the length of the projection, onto the two-track mo-

mentum vector, of the transverse plane vector from the primary vertex to the Λ_b^0 vertex." The sentence now reads: "$L_{\rm xy}(\Lambda_b^0)$ is defined as the projection, onto $p_{\rm T}(\Lambda_b^0)$, of the vector connecting the primary vertex to the Λ_b^0 vertex in the transverse plane." The statement regarding $L_{\rm xy}(\Lambda_c^+)$, which used to read the same as the previous statement, now reads: "$L_{\rm xy}(\Lambda_c^+)$ is defined analogously to $L_{\rm xy}(\Lambda_b^0)$ but computed with respect to the Λ_b^0 vertex."

The definition of d_0 ("impact parameter of the Λ_b^0 candidiate") is incomplete. Perhaps "impact parameter of the momentum vector of the Λ_b^0 candidate with respect to the primary vertex" is accurate?

We agree with the Referee's comment, and the definition has been expanded as suggested.

The second sentence of the caption for Fig. 1 is very confusing. I think the authors mean that there is no *curve* corresponding to the indicated decay modes. The Λ_b^0 signal region should be defined in the text (in addition to the caption) since it is used in the text.

The authors did intend for the caption to explain that curves for fully reconstructed decays are not indicated separately. The sentence previously read: "Fully reconstructed Λ_b^0 decays such as $\Lambda_b^0 \to \Lambda_c^+\pi^-$ and $\Lambda_b^0 \to \Lambda_c^+K^-$ are not indicated on the figure." The sentence now reads: "Curves for fully reconstructed Λ_b^0 decays such as $\Lambda_b^0 \to \Lambda_c^+\pi^-$ and $\Lambda_b^0 \to \Lambda_c^+K^-$ are not indicated on the figure."

We agree with the Referee's comment that the Λ_b^0 signal region should also be defined in the text. The Λ_b^0 signal region is now defined again the first time it is used in the text.

In paragraph 7, make is clear whether m(Λ_b^0) is the measured Λ_c^+ pi mass or the mean Λ_b^0 mass. I assume it is the former? If so, the authors might consider changing the notation to m(Λ_c^+ pi).

That is correct, we subtract the measured Λ_c^+ pi mass. We have added the phrase " where $m(\Lambda_b^0)$ is the reconstructed $\Lambda_c^+\pi^-$ mass" to the end of this sentence.

In paragraph 8, the definition of cos theta* is ambiguous because it involves two reference frames and the orientation of the frames is not uniquely defined. The angle should be defined in a single reference frame.

This definition of the angle is common at both CDF and D0. See, for example, the recent CDF publication on J/ψ polarization (arXiv:0704.0638[hep-ex]). We also see a similar definition appearing in a CLEO publication (G. Brandenburg et al., the CLEO Collaboration, Phys. Rev. Lett. 78, 2304 (1997)). The definition is unique because θ^* is the dot product of two well-defined momentum vectors, and taking the cosine ensures that the sign is correct.

In paragraph 9, it is not clear which background parameters (yields? shape parameters? both?) are being determined by the various samples. Phrases like "determines the combinatorial background", "gives the B hadronization background", and "…background is obtained from…a simulation" are ambiguous as to exactly what is being 'determined', 'given' or 'obtained': shapes only? Or absolute normalization? It is not clear whether the total background shape and normalization shown in the inset of Fig. 2 is from the (fixed) yields and shapes of the various backgrounds determined from the samples described earlier in this paragraph, or whether this is from a fit in which the yields are free parameters. Please clarify this in the text. Please clarify whether "The total background shape ... is a fixed component in the Σ_b^* fit" means that only the shape parameters are fixed, or whether the shapes and normalizations are all fixed in the fit that includes the signal region.

Individual samples are used to determine the background shape, and we have added the word "shape" after "background" in each of these sentences to clarify this.

The normalization of each sample is determined from the Λ_b^0 mass fit, and both the shape and normalization are fixed in the fit to data. To clarify this, we have rewritten the last two sentences of this paragraph. The sentences previously read: "After establishing the shape and normalization of each background Q distribution, the background shapes are parameterized by a power law multiplied by an exponential. The total background shape shown in Figure 2 (inset) is compatible with the Q sidebands and is a fixed component in the $\Sigma_b^{(*)}$ fit." The sentences now read: "The background shapes are parameterized by a power law multiplied by an exponential, and the normalizations are fixed from the percentage of that background component in the Λ_b^0 signal region. The total background shown in Figure 2 (inset) is compatible with the Q sidebands and the background shape and normalization are fixed components of the $\Sigma_b^{(*)}$ fit."

In the middle of paragraph 12, it is not clear how uncertainties on mass differences are determined from the uncertainties on the absolute mass scale.

We calculate the mass differences, or Q values, of the mentioned decays (D^*, Σ_c, and Λ_c^*) and find the discrepancy between the CDF measured Q value and the PDG Q value. We then parameterize this discrepancy as a linear function of Q and extrapolate to the Σ_b Q values. To clarify this in the text, we have replaced "mass" with "Q" in the second sentence. The sentence previously read: "The systematic uncertainty on the mass scale is determined by the discrepancies of the CDF II measured masses of the D^*, Σ_c, and Λ_c^* hadrons from the world average mass values [13]." The sentence now reads: "The systematic uncertainty on the mass scale is determined by the discrepancies of the CDF II measured Q values of the D^*, Σ_c, and Λ_c^* hadrons from the world average Q values [13]."

I assume that at a number of editorial problems in the text will be corrected; (e.g., Table captions appearing below the tables rather than above; the variables "c", "N_c" and others not in 'math mode'; Tab. $\rightarrow$ Table; more appropriate punctuation of the results in the abstract; tiny fonts in the labels on the vertical axes in the insets in Fig. 2).

We have modified the text as follows:

- **The table captions are above the tables.**
- **The variables "c" and "N_c" are in math mode.**
- **We changed "Fig." to "Figure" and "Tab." to "Table" where they appeared.**
- **We made Fig. 2 slightly larger so that the axis labels will be easier to read.**
- **Results in the abstract now appear on separate lines.**

Responses to Referee B

This is a review of article LF11708 entitled "First Observation of Heavy Baryons Σ_b and Σ_b^*" by T. Aaltonen, A. Abulencia, J. Adelman, T. Affolder, et al.

This article will be of high interest to the particle physics heavy flavor community and it should be of general interest to the physics community that the 3-quark mass spectra containing a b-quark follows the expected pattern seen in lower mass 3-quark spectra. The Σ_b states and Σ_b^* states were predicted several years ago, but these states have not yet been observed. The interesting numbers to determine are the masses and the natural widths of the states. This paper reports the masses for the charged states; the neutral states are not studied. From the mass values, one can determine the hyperfine and isospin mass splittings. The Σ_b states and Σ_b^* states decay strongly and as reported the natural widths vary from 2 to 20 MeV/c^2in the paper by Korner, Kramer, and Pirjol.

The crux of the paper is that two independent, charge-signed invariant mass distributions are simultaneously fit using an unbinned maximum likelihood fit for four mass peaks with a constraint on the mass differences. The samples are acquired without particle identification by using Λ_b^0 events selected via the decay chain $\Lambda_b^0 \to \Lambda_c^+\pi^-$ with $\Lambda_c^+ \to pK^-\pi^+$. Kinematics and decay length fits are used to obtain a clean Λ_b^0 sample.

In the abstract four distinct mass values are reported for the four $\Lambda_b^0\pi^\pm$ resonances, yet the values are correlated via their fitting constraint that $m(\Sigma_b^{*+}) - m(\Sigma_b^+) = m(\Sigma_b^{*-}) - m(\Sigma_b^-) = \Delta_{\Sigma_b^*}$. Also there is no mention of the significance of the observation. It would be fairer to report the values that they directly measure, name $\Delta_{\Sigma_b^*}$, ($m(\Sigma_b^+)$ or $Q_{\Sigma_b^+}$), and ($m(\Sigma_b^-)$ or $Q_{\Sigma_b^-}$) and interpret the states within the paper. I also think the paper should report a significance of observation of 5.2 sigma for observing all four states.

We agree with the Referee's comments on the values quoted in the abstract. The abstract now quotes only the three values $\Delta_{\Sigma_b^*}$, $m(\Sigma_b^+)$, and $m(\Sigma_b^-)$.

We do not wish to include the 5.2σ significance in the abstract. We feel there would be confusion as to what exactly the 5.2σ referred to (whether it was for all four peaks or each peak individually), and it is better to explain this in the text than introduce it in the abstract.

Further, I have a mild concern about the title. It would be more accurate to say "First Evidence of Heavy Baryons…". Most of individual states have a significance around 3 standard deviations. Usually the term discovery or observation is reserved for the 5 standard deviation level, unless one knows the mass and width of the state. In this case it is the pattern of all four states that yields a 5 sigma effect.

We prefer to keep the title as "Observation" rather than changing to "Evidence." The effect is greater than 5.2σ from a null result. Based on the theoretical predictions for the ground state Σ_b baryons, we would not interpret these results as the Σ_b baryons if we did not observe a pattern of four states similar to what we see.

The fitting description is a bit terse and it would aid the reader to add more information. The natural widths used in the fit for the Σ_b^+ and Σ_b^{*+} states should be reported rather than referencing a long theory article. It would also be nice to report what the detector resolution for the dominant narrow core is for the Σ_b^+ and Σ_b^{*+} states. Providing this information would allow the reader to gain insight on whether the difference in widths shown in figure 2 is a result of a difference in natural width or the detector resolution.

We agree with the Referee's comment on the natural widths, and have added this information to the theory section as indicated in the response to Referee A on this same topic.

We also added the detector resolution widths, as requested by both Referees. We use the same detector resolution model for the Σ_b^+ and Σ_b^{*+} states; the Q values differ by only $\sim$20 MeV$/c^2$, and we do not expect the resolution to change appreciably over such a small range.

The background parameterization looks pretty good, but I don't see a mention of possible higher mass Λ_b^0 and Σ_b states. The background under the $\Sigma_b^{(*)}$ is determined from a fit of the Q distribution up to 500 MeV$/c^2$. The undiscovered, but expected Λ_b^0 and Σ_b states at higher mass could distort the "combinatoric" background at high Q value and lead to a flatter background. As the background is a fixed component in the $\Sigma_b^{(*)}$ fit, the systematic effects from varying Q background range - say 200 MeV$/c^2$, 300 MeV$/c^2$, 400 MeV$/c^2$and the default at 500 MeV$/c^2$should be considered. The background fit determination also looks to be tricky in terms of how many low mass bins (below 40 MeV$/c^2$) are used.

Actually, the background is not determined from a fit to the Q distribution up to 500 MeV$/c^2$. The background determination is explained in paragraph 9. We isolate each of the three dominant background sources (prompt Λ_b^0 baryons, misidentified B mesons, and combinatorial background) and, using data for the combinatorial and B mesons and PYTHIA Monte Carlo for the prompt Λ_b^0 baryons, form a Σ_b Q distribution for each background source. We then fit these individual background sources. The Λ_b^0 baryon and B meson samples we are fitting have much higher statistics than our data sample, so the number of events in low mass bins has no effect on these fits. The combinatorial sample has very low statistics, but it also contributes very little to the overall Σ_b background (about 3%), so any uncertainties due to the low statistics are adequately accounted for by our systematic variations of the background shapes. Because the background contribution is fit from individual background samples, rather than from a fit to the Σ_b sidebands, changing the range of the background in the fit from 500 MeV$/c^2$will have no effect on the measurement.

After evaluating the shape and normalization of each background component, we compare our estimated background to the Σ_b Q distribution in data, and see very good agreement in the Σ_b sidebands. This indicates that any contribution from higher mass Λ_b^0 and Σ_b states is minimal. We would also expect the contributions from these states to be negligible from theoretical predictions. The P-wave Σ_b states should have much higher masses and a much lower production rate than the S-wave states, so we do not expect them to contribute to the background in the current fit range. The Λ_b^* baryons must also have a much lower production rate than the S-wave Σ_b states because they again require an $L = 1$ orbital momentum. So we expect Λ_b^* to be suppressed even with respect to the individual Σ_b states (e.g. Σ_b^+, which has only 30 events). Given the small number of individual Σ_b candidates in this sample, there is expected to be a negligible effect on the background shape from Λ_b^* states. We may refer to the situation in the charm sector, where Λ_c^* never contributed much to the $\Sigma_c^{++,-}$ spectrum

until there were 1300 candidates of each charm state.

The summary paragraph on page 13 is carefully crafted to say "we observe" a signal of four states. It does say that the widths of the four states are consistent with expectations, but we were not provided sufficient information to assess the sensitivity of the signal to the natural widths of the states. This is partly a question of detector resolution in the Q range where the signals are studied. I presume that there is insufficient statistics to let the width float in the fit. (One could have just two widths - one for the Σ_b and one for the Σ_b^*.)

We now provide both the detector resolution values and the width estimates for our average Σ_b and Σ_b^* masses, so the reader should be able to judge the sensitivity of the signal. There is still insufficient statistics to let the width float, even if we use only one width for both Σ_b states and one width for both Σ_b^* states. With this data sample, we can only say that the widths are consistent with theoretical predictions.