

Second Reply to the Referee Reports on “Measurement of the Charged-Pion Polarisability”

December 24, 2014

We take note of the situation that two out of three referees are in favor of publication of our letter in PRL, while *Referee B* keeps criticizing one aspect of our analysis. The editor concludes that this criticism has to be met before considering publication.

We think that this critique is based on misunderstandings. In order to clarify questions related on the extraction procedure, we have contacted one of the theory experts in the field, Barbara Pasquini, for whom the referee has claimed that “not even she would believe our approach”. We are, however, entitled to cite her with the statement that she fully supports our approach and the result of our analysis. Anyone involved is invited to contact her concerning this.

When considering our manuscript for checking our approach, B. Pasquini had first stumbled over the relevant detail, namely whether we make a mere low-energy expansion (LEX), or whether we take higher orders into account. While we describe all the relevant details of our analysis properly in the text, we see that we have to stress the inclusion of higher orders already along Eq. 3. We include in the updated manuscript a clarifying remark, directly following Eq. 3., that we correct for the ChPT loop contributions to first order.

Together with Barbara Pasquini, we have checked that this approach is equivalent, down to the permille level, to the usage of the dispersion theory approach in its generally accepted form [Pasquini, Drechsel, Scherer, Phys. Rev. C 77 (2008) 065211, and Phys. Rev. C 81 (2010) 029802]. We include a statement about this in the discussion of our result.

We are aware that the correct application of dispersion relations for pion Compton scattering has been discussed amongst theorists over the past decade especially between B. Pasquini *et al.* and L. Fil’kov *et al.*, with the so-far final words published in the reference given above. However, to our knowledge L. Filkov has never admitted problems in his approach, such that it is difficult from the experimental point of view to summarize the theory situation in a few sentences. This is the reason why we have obstained in the first version of the letter a statement about the applicability of dispersion relations.

We thank *Referees A* and *C* for their supportive statements, and have taken their suggestions into account in the updated manuscript.

We cite the referee reports in boxed text, and interleave it with our comments and explanations. In blue, the respective changes to the manuscript are highlighted.

We stress that none of the modifications to the manuscript represents a substantial change of its content, and we see the most urge to get it published. Most of the discussion is around the theoretical background for this experimental work. This discussion clearly shows the broad interest and the impact of the new experimental results. We presented a state-of-the-art analysis and interpretation which significantly contributes to the fundamental question of hadron polarisabilities. This justifies a publication of the present result in PRL without further delay.

Report of Referee A – LT14032/Adolph

The authors have responded in detail to the detailed questions and suggestions of the other referees, and they did conclude correctly that my previous report shows me to be the least involved in the field. Still, the ongoing discussions of the measurements, interpretation and the relevance of prior experiments have not passed me by. My suggestion therefore was to add a physics discussion beyond "We find the size of the pion polarizability at significant variance with previous experiments and compatible with the expectation from ChPT.", to which the authors respond with a mere addition of "This result constitutes important progress towards resolving one of the long-standing issues in low-energy QCD." How so, if no discussion and comparison with other experiments is made? In the fringe material they provide, the obvious problem of a long-standing discussion or even controversy becomes evident. I still think that at least a brief reflection on why the values of different measurements might be so different from an experimental perspective or based on theoretical arguments would have enhanced the manuscript.

Still, the manuscript presents a result, which may spur further work and additional discussion, theoretical as well as experimental, in the future. While it does not settle the case, it is indeed an experimental result with better errors. In summary, while I still see the problems stated above, I am inclined to recommend the manuscript in its revised version for publication in PRL.

We are aware that the discussion of the meaning and the impact that our result will have, is scarce in the letter. As the refereeing process shows, it touches unsolved theoretical issues, which can not be adequately covered in the intended form of the letter, which shall primarily communicate the new result.

We would like to point out that we do sketch the experimental situation in the introductory part, and also comment on theoretical aspects. In the latter regard, the result discussion is enlarged in the updated version, in order to comply with the worries of referee B. We assume this to be in tune also with what referee A has in mind as a further discussion. We thank referee A for the supportive statements.

Report of Referee B – LT14032/Adolph

This is the second time I’ve read and reviewed this paper. I’ve reviewed many papers for PR pertaining to Compton scattering and the measurement of electromagnetic polarizabilities, and I know this field very well.

We do not appreciate statements about expertise in the field in the asymmetric situation in which the referee knows who we are, but we do not know his/her identity. Many of us have been deeply involved in, and have co-authored most relevant experimental works on the nucleon polarisabilities (R. Beck, N. D’Hose, J. M. Friedrich, M. Ostrick, H. Schmieden,...), and expertise over the past 20 years can be safely assumed.

The authors still seem to have the mistaken idea that this particular reaction mechanism, radiative π^+ Primakoff production, does not require an analysis that goes beyond the low energy expansion (LEX), their Eqn. 1 in the paper.

A few misunderstandings appear to us in this introductory sentence. First, the reaction we investigate is not “radiative π^- Primakoff production”¹, but “radiative π^- Primakoff *scattering*”. The difference is relevant, as in our reaction pions do not have to be produced but are only (softly) scattered. This mechanism allows for the extraction of the subtle polarisability effect with high precision.

Second, it seems to be a misunderstanding by referee B that we do not go beyond the mere low-energy expansion. In fact, we do not include quadrupole or even higher multipole polarisabilities in our fit, and neither we allow for a non-vanishing sum of the dipole polarisabilities. This is justified, as for the kinematical region of the current analysis, the existing predictions for their contributions are sufficiently small that their omission is permissible. We do, however, include the knowledge about additional terms beyond the polarisabilities. We do this by correcting with the ChPT chiral-loop contributions. This is equivalent to the predicted effect from dispersion relations, as discussed in the appendix of this reply. We refer to this only as one of the corrections employed to the quantity σ^0 introduced in Eq. 3, to which we compare the measured cross-section. As this could be overlooked when skimming through the letter, we have placed a comment at a more prominent place following Eq. 3:

where $\sigma_{\pi\gamma} = N/L$ refers to the measured cross section, $d\sigma_{\pi\gamma}^0$ to the simulated cross section expected for $\alpha_\pi = 0$ (including corrections to the pure Born cross section as those from chiral loops, as specified below), N is the number of events, and L is the integrated luminosity.

This is analogous to trying to understand Compton scattering on the nucleon in the pion threshold region through the LEX; it simply doesn’t work.

Certainly there are similarities between the polarisability effects for pions

¹we have corrected the sign of the pion charge, which is however not relevant for the discussion

and nucleons, but there are also important differences. Most prominently, the first (i.e. Delta) resonance is only $0.3 m_N$ away in the nucleon case, while the rho resonance is $4.5 m_\pi$ away from the pion. Therefore, although it may seem too much to take into account $\pi\gamma$ -CM-masses up to $3.5 m_\pi$, it is equivalent to study the near-threshold region in the nucleon case. Clearly, $\pi\pi$ rescattering has to be taken into account already at smaller masses. We do this by accounting for the chiral-loop corrections, as described in detail in the Appendix.

Their comment in the response, that Primakov scattering "does NOT require additional theoretical assumptions, as needed for the other two proposed/realized experimental approaches, namely the reactions $\gamma p \rightarrow \pi^+ \gamma n$ and $\gamma \gamma \rightarrow \pi^+ \pi^-$ " is false. They haven't supplied a single reference that can backup this claim for the validity of the LEX at energies up to $M_{\pi\gamma} = 500 \text{ MeV}$. I'm not surprised that they have no supporting references, because no one believes this, including B. Pasquini who's mentioned prominently several times in the authors response.

Here, the referee mixes different aspects that we will sort out in the following. The validity range for our ansatz, which must obviously extend over the used range as discussed below, has nothing to do with the principle constraints of the other mentioned experimental approaches. We insist that our approach is not faulty, but rather features the least systematic uncertainties realized so far.

After having updated Barbara Pasquini on the status of our publication and having discussed with her the details, she turns out to be fully supportive of our approach and our analysis result, as summarized in the Appendix. The central message is that our approach is fully validated up to 500 MeV, and even beyond. We include a respective statement in the discussion of our result:

No significant effect was found when varying this limit between $0.40 \text{ GeV}/c^2$ and $0.57 \text{ GeV}/c^2$. Furthermore, the functional behavior of our model, including the chiral-loop corrections, was compared to the approach using dispersion relations [Pasquini2008,2010 and priv. comm.], and very good agreement was found in the mass range up to $4m_\pi$. The respective cross sections do not differ by more than 2 permille, which corresponds to less than 15% of our given systematic uncertainty estimate for the polarisability value.

The best way to test the validity of the LEX is to plot Compton cross sections at fixed CM angle as a function s, and look for the expected deviation from the LEX. This is standard technique in Compton scattering analyses, and I'm surprised that the authors have not investigated an analysis along these lines. For me this demonstrates a limitation of using a 200 GeV muon beam to initiate the Compton scattering process, then having to integrate over the entire Compton scattering phase space.

We are aware of this kind of analysis technique, used e.g. in the Appendix. We would produce this kind of plots for our data, if we had sufficient statistical precision for it. Given the present data, we need to integrate the covered CM angle and energy spectrum, in order to achieve a meaningful result.

This is neither a limitation of our experimental approach in general, nor of publishing now the present results. With more data indeed the analysis as the referee proposes is feasible and even foreseen – it is the background for the data of the COMPASS acquisition campaign in 2012, the analysis of which is however still ongoing.

In order to measure the pion polarisability, we initiate Compton scattering using a pion beam. With muon beam, we do an independent control measurement under the same experimental conditions for controlling systematic uncertainties.

The analysis is highly leveraged on the use of simulation for comparison with experimental yields.

It is a strength of modern particle physics experiments to be fully modelled by Monte Carlo simulations, and a vast number of effects can elegantly be corrected for this way.

The authors have varied the energy cut used in the analysis (see Fig. 1d in paper) over a relatively small interval, from 400 to 570 MeV and they don't see a change in α within statistical error. This doesn't surprise me very much, since the relative change in energy is modest. The problem with the data set is that the statistics are peaked at low energy, (see Fig 1d) where the sensitivity to the polarizability is the weakest. This is the opposite from the situation in nucleon Compton scattering, where the statistics are peaked at energies where the sensitivity is the greatest. This underlines the necessity for an analysis that goes beyond the LEX.

Having in mind the comments of the referee in the first round, we want to mention our surprise that on the one hand the referee assumes that we do not go beyond LEX, but does not expect a change of the result with varying the cut on $m_{\pi\gamma}$ on the other. Doing a low-energy expansion with the polarisability parameter alone would significantly depend on this cut, as can be understood from the material in the Appendix.

I do understand that the authors have treated the incoherent $\rho^- \rightarrow \pi^- \pi^0$ background, and the supplemental note makes clear that there was an analysis based on an empirical nuclear coherent background, plus the interference term. However, there are other sources of nuclear incoherent background besides ρ^- when using a nuclear target. See Phys. Rev. Lett. 106, 162303 (2011) and references therein. Has there been any effort to model the nuclear incoherent backgrounds and fit them to the experimental Q2 distribution?

In our first reply we had answered to the referee's specific question about background originating from ρ decays. However, as indicated in the letter, our method allows for the subtraction of all the background, both coherent and incoherent, involving π^0 decays. We consider it a big advantage that these background data were taken within the same experiment, and under the same

conditions they are to be used in.

We found that our original formulation in the letter (“...background from π^0 mesons produced in electromagnetic and strong interactions, $\pi^- \text{Ni} \rightarrow \pi^- \pi^0 \text{Ni}$ ”) is a little sloppy in that regard, and have now reformulated it as follows:

...background from π^0 mesons produced in electromagnetic and strong interactions, $\pi^- \text{Ni} \rightarrow \pi^- \pi^0 X$, where in the considered low Q^2 region X is predominantly a Ni nucleus in its ground-state, but in principle nuclear excitation or breakup is also included. The probability to misidentify such $\pi^- \pi^0$ events as $\pi^- \gamma$ events due to missing or overlapping photons is estimated from a pure sample of beam kaon decays, $K^- \rightarrow \pi^- \pi^0$, and the observation of corresponding (in this case unphysical) $\pi^- \gamma$ final states. The same probability is assumed for misidentifying $\pi^- \pi^0$ as $\pi^- \gamma$ for the studied $\pi^- \text{Ni}$ reactions in each x_γ bin, and the fraction f_{π^0} of background caused by π^0 events is presented in the bottom panel of Fig. 2. The simulated...

Furthermore, incoherent background that involves the breakup or excitation of the target nucleus, is at most a tiny contribution at small Q^2 , suppressed by the respective transition form factors. It does not exhibit any structure in the Primakoff peak region. This becomes apparent also when looking at the PrimEx reference the referee is pointing to.

Lastly, the supplemental note includes an old, and outdated quote from Mike Pennington that the only way to measure the pion polarizability is in the Compton scattering process near threshold and not in $\gamma\gamma \rightarrow \pi^+ \pi^-$. I’m not sure why they’re quoting Pennington here, to tell the referees that this analysis is as good as it’s ever going to get?

The polarisabilities are defined in the $s \rightarrow m_\pi^2$ limit for the usual Mandelstam variable in $\pi\gamma \rightarrow \pi\gamma$ scattering. While the same amplitude is accessible in $\gamma\gamma \rightarrow \pi\pi$, the measurement covers a different region in the (s, t) -plane, and the mentioned extrapolation must be made to the point where the polarisabilities are defined. It is indeed not important who states this.

This is clearly not the case, and if you examine modern analyses of $\gamma\gamma \rightarrow \pi^+ \pi^-$ (see Pasquini Phys. Rev. C 77, 65211, 2008) there is reasonably good sensitivity to the polarizabilities in the cross sections, rather comparable to what you see in Compton scattering on the nucleon. Furthermore, I must add that the JLab PAC, a committee that’s overseen and directed by Pennington, recently approved a JLab experiment to measure the charged pion polarizability in $\gamma\gamma \rightarrow \pi^+ \pi^-$

We definitely do not intend to discourage or ignore the experimental effort mentioned by the referee. We acknowledge that this planned JLab experiment²

²To our knowledge the PAC was chaired by Naomi Makins when this experiment was approved (July 2013)

has the potential to bring interesting physics results, complementary to ours, and we are looking forward to its realization.

In summary, as a bare minimum for publication in PRL, I recommend that the authors do an analysis that goes beyond the LEX expression of Eqn. 1. There are dispersion approaches available for this, see ref. Eur. Phys. J. A23, 113 (2005), and newer approaches under development (see the Pasquini reference). The authors also need to investigate the sensitivity of their analysis to the nuclear incoherent backgrounds which are not included in their Q2 fits. See Rodrigues et al. Phys. Rev. C 82:024608 (2010).

We underline again that we do go beyond LEX, as discussed above and explained in the Appendix. It is in full agreement with the state-of-the-art “dispersion approach”.

We have discussed the various background subtractions in the previous reply and above. In fact, our analysis does account for nuclear incoherent background.

The quoted paper by Rodrigues et al. refers to nuclear incoherent meson production by incident photons. Therefore, this paper does not apply to the present analysis. Potentially this bases on the same misunderstanding as the referee’s comments about “radiative π^+ Primakoff production” above.

Report of Referee C – LT14032/Adolph
 I’m satisfied with the reply to my initial questions and recommend publication in the PRL. I have looked over the comments of the other referees and I think that the replies are reasonable – of course I cannot speak for them.
 The pion polarizability is a fundamental quantity in confinement scale QCD and testing the theoretical predictions is very important and of general interest in physics. I consider the experimental work reported here to be of high quality and to be the best determination of this fundamental quantity. The fact that the experiment agrees with theory is not the reason for my judgment which is based primarily on the experimental method and the quality of the work and the paper to the best of my ability to judge. This does not rule out the fact that further experimental tests with different techniques and errors will also be welcome in the future.
 In response to my comment that the Z^2 dependence of the Primakoff effect was not verified the authors have responded
 “We adopted the policy to not quote on further work that could be done in the future. However, as additional information for the referee, we state that the size of the Coulomb peak was checked for different targets on smaller statistics (tungsten, silicon, carbon) data, showing consistency with the Z^2 expectation.”
 I strongly suggest that before publication that following Eq. 2, the authors include the sentence “the size of the Primakoff peak was checked for different targets on smaller statistics (tungsten, silicon, carbon) data, showing consistency with the Z^2 expectation.”
 The editors can check this point without sending it out again for another review.

We take over the referee’s proposal and add this information below Eq. 2,

when discussing the Primakoff peak Fig.1(c),

Events corresponding to photon exchange are selected by requiring $Q^2 < 0.0015 \text{ (GeV/c)}^2$. The size of the Primakoff peak was checked for different targets on smaller-statistics data (tungsten, silicon, carbon), showing consistency with the approximate $\sim Z^2$ expectation.

We thank the referee for the supportive statements, stressing the general interest for a timely publication of this letter.

Appendix: Higher-order terms in \sqrt{s} : Chiral Loops and Dispersion Relations

Barbara Pasquini has kindly provided us with the cross section behaviour according to the dispersion theory approach [Pasquini, Drechsel, Scherer, Phys. Rev. C 77 (2008) 065211, and Phys. Rev. C 81 (2010) 029802] for two values $\alpha_\pi = 2.00$ and $\alpha_\pi = 2.85$. We compare it in Fig. 1 with the model that we use in the letter, i.e. polarisability plus chiral loops. As evident from the plot, the two approaches are fully equivalent on the permille level, and their difference is irrelevant given the current experimental resolution, in the full range up to about $4m_\pi$ used in the analysis. The experimental resolution is displayed approximately by the difference between the two cases $\alpha_\pi = 2.00$ and 2.85 , which differ by one standard deviation of our full experimental uncertainty.

Furthermore, the effect of chiral-loops alone is shown (as green dotted line). For our analysis, we include this effect in the quantity σ^0 (Eq. 3 in the letter), to which we relate the measured cross section. Thus chiral loops and polarisability are factorized, while in reality they enter on the amplitude level and there is in principle an interference term. This, however, is a small effect as it is seen from the dashed green line, which demonstrates how the polarisability shows up when compared to “Born plus chiral loops” as done in our analysis.

We have also compared the angular dependence of the two approaches, see the lower panel of Fig. 2. As already seen for the s -dependence, there is no evidence that the usage of dispersion relations features anything different from the chosen approach within ChPT.

Polarisability and Loop Contributions $z=-1.0$

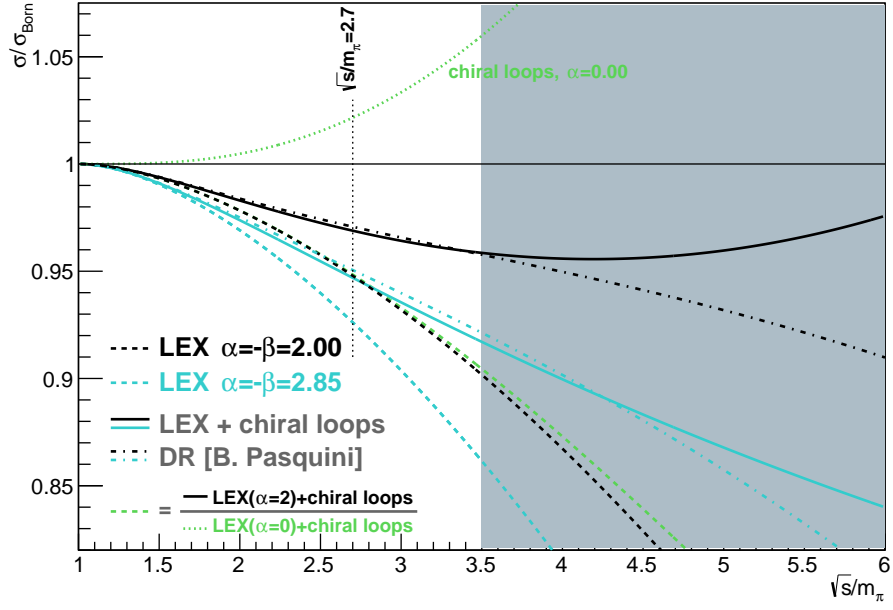


Figure 1: Relative deviation from the Born cross sections by various effects for backward scattering, $z = \cos \theta_{CM} = -1$. Dashed lines: effect of only the LEX dipole polarisability term $\alpha_\pi - \beta_\pi$ with the two given values ($\alpha_\pi = -\beta_\pi$ assumed). Continuous lines: The same as the dashed lines, but including the chiral-loops correction. Dash-dotted lines: calculation using dispersion relations (DR) for the same two polarisability values. The dotted vertical line indicates the \sqrt{s} -value for which the angular spectrum is shown in Fig. 2. The green lines are explained in the text.

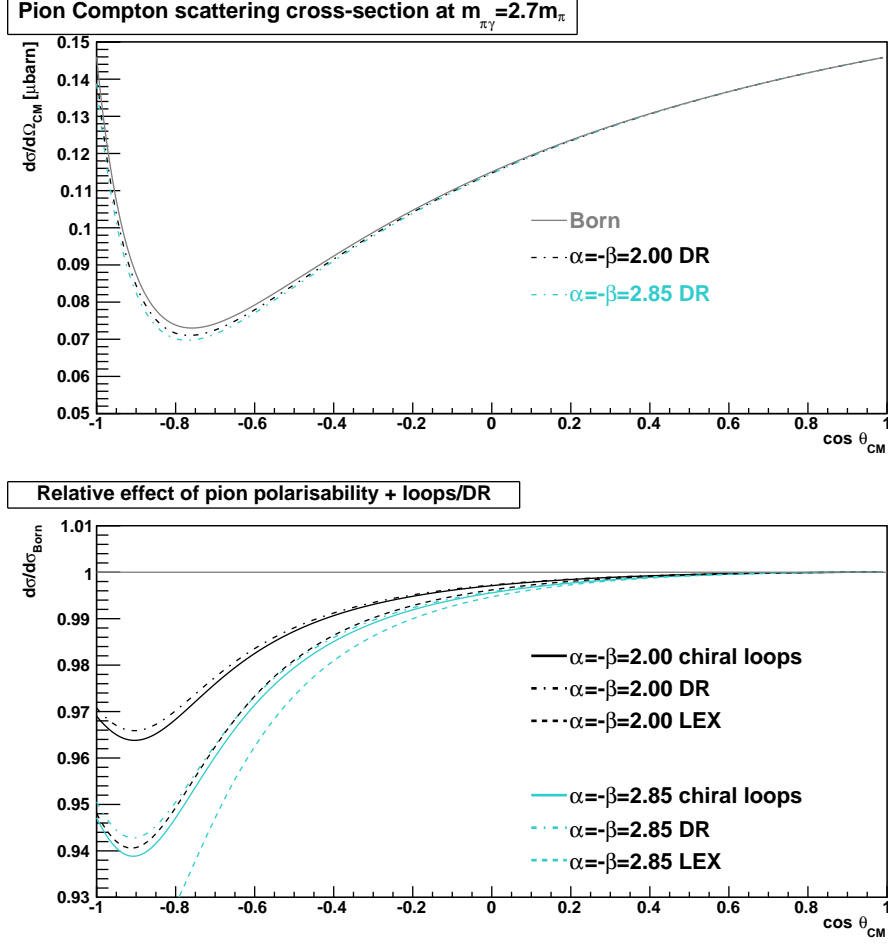


Figure 2: Cross section dependence on $\cos \theta_{CM}$ for $\sqrt{s}/m_\pi = 2.7$, where the deviation between dispersion relations and chiral loops is largest (see Fig. 1). The angular dependence for the two approaches is extremely similar over the whole range.