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Perspective

The Discovery of the Antiproton between Rome and Berkeley

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Abstract: Solid confirmation for the discovery of the antiproton came shortly after its first detection in September 1955, through the visual evidence offered by the observation of annihilation stars in nuclear emulsions exposed to the Bevatron beam. The emulsion work was a result of a cooperative effort between Emilio Segrè's team in Berkeley and the group of physicists working under the guidance of Edoardo Amaldi in Rome, who had already observed a possible antiproton annihilation star in emulsions exposed to cosmic rays. The origin and development of the Rome-Berkeley collaboration are presented, in the wider context of the changing balance between cosmic ray investigation and accelerator research in the mid-fifties.

Keywords: antiproton; emulsions; cosmic rays

It is fair to state that basically every fundamental discovery in experimental particle physics, up to the early fifties, had been the result of cosmic-ray investigation; each new particle that had enriched the growing zoo of the "elementary" constituents of matter had been found by means of cloud chambers and emulsion plates, the standard tools created to catch the signals coming from outer space. By the mid-fifties, the development of the big particle accelerators gradually dictated a changed hierarchy in the tools of the trade, providing more efficient, more controllable (and much more expensive) instruments to research in particle physics.

I would say that the Sixth Rochester Conference (3–7 April 1956) marked the transition from "little science" to "big science" in particle physics. Until the sixth conference, the decision to give equal treatment to accelerator physics, cosmic ray physics and particle theory had served its purpose. Indeed, during the first half- dozen Rochester conferences, it was a common experience for the cosmic ray experimentalists to describe qualitative features of some new discoveries at high energies, for the theorists to articulate these results into a set of model options and, finally, for the accelerator physicists to present at the same, or the very next conference, the quantitative data that enabled one to select the most likely theoretical model. But, at Rochester VI, it was clear that the stream of results from the Berkeley bevatron and the Brookhaven cosmotron would monopolize strange particle physics and Bob Leighton was led to remark that "next year those people still studying strange particles using cosmic rays had better hold a rump session of the Rochester Conference somewhere else—that the machine work had been pretty hard on cosmic-ray people" . . . It should be noted that 1956 was the year when the production of the antiproton was achieved with the Berkeley bevatron—after years of frustration with cosmic ray experiments [1] (pp. 755–756).

And there is also no doubt that the first half of the fifties was regarded in this respect as a dramatic moment of transition by the protagonists themselves; such was, in particular, the perception of European physicists. Trained in the "poor" research carried out on cosmic rays, they saw themselves pursued ever more closely by the infinitely "richer" research conducted with particle accelerators on the other side of the Atlantic [2]. A significant example is offered by the recollections of Edoardo Amaldi and Charles Peyrou on the International Conference on Particle Physics held in Pisa in July, 1955:

> *A striking fact that emerged in Pisa was that the time for important contributions to subnuclear particle physics from the study of cosmic rays was very close to an end. A few papers presented by physicists from the U.S.A. showed clearly the advantage for the study of these particles presented by the Cosmotron of*

Brookhaven National Laboratory (3 GeV) but even more by the Bevatron of the Lawrence Radiation Laboratory in Berkeley (6.3 GeV) [3] (p. 117).

. . . at the Pisa Conference in July 1955 . . . the cosmic ray physicists could be proud; they had found just in time all possible decays of the heavy mesons, and made it very plausible that there was one and only one K particle. But their triumph was a swan's song. At the same conference the Berkeley physicists brought better proofs of that idea [4] (p. 631).

In the words of Peyrou, "better proofs" could be replaced by "better tools"; the Berkeley physicists were able to bring "better proofs" because they could avail themselves of the "better tool" that was the Bevatron, recently put into operation with a peak energy of 6.3 GeV, the most powerful accelerating machine in existence at that time in the world. Not only the Bevatron was a "better" tool; it was also fundamentally "new", in the sense that it allowed to perform in radically altered conditions the process from the collection of empirical indications on the world of new particles toward their transformation into conclusive evidence: from the observation of the phenomenon, reading the traces left in an unpredictable and uncontrolled way by cosmic radiation, one could move to its artificial production, in copious quantities and under controlled and repeatable conditions. With the construction of the new apparatus, not only the dimensions of science built around it changed, but the cognitive procedures themselves were redefined. In particular, the conditions that allowed what Peter Galison refers to as a "change in the status of evidence" were modified. This change made it possible to transform "evidence . . . from a hint to a demonstration" [5] (p. 1); In other words, it created the necessary framework to move, as Peyrou puts it, from a "very plausible" conjecture to actual "proof" or, to echo Galison once more, to construct convincing arguments about the world around us.

In denying the old Reichenbachian division between capricious discovery and rule-governed justification, our task is neither to produce rational rules for discovery - a favorite philosophical pastime - nor to reduce the arguments of physics to surface waves over the ocean of professional interests. The task at hand is to capture the building up of a persuasive argument about the world around us, even in the absence of the logician's certainty [5] (p. 277).

If discovery techniques and justification strategies walk together, the appearance of new tools (new mediators between "what nature tells us" and "what we say about it") helps to redraw the boundary and interaction between the two contexts. The story of the "discovery" of the antiproton, and of the "justification" of its existence, is an excellent example in this regard. Located exactly in the mid-fifties, it represents a decisive moment, perhaps the most significant, of the transition from cosmic ray research to particle accelerator experiments, allowing to grasp some of its most characteristic features. Furthermore, for its developments and its outcomes, this story illustrates some aspects of the way in which the new technology available intervenes in redefining the rules with which "persuasive arguments" are constructed.

An official recognition—the Nobel Prize awarded to Emilio Segrè and Owen Chamberlain in 1959—identifies in a date, a place and an experimental device the discovery of the antiproton: in Berkeley in September 1955 [6], thanks to the identification of particles of negative charge and protonic mass in a sophisticated detection apparatus mounted at the exit of the beam generated by the Bevatron, the only machine able at the time to reach the threshold energy necessary for the production of proton-antiproton pairs (The official version of the Radiation Laboratory has emphatically maintained, starting in 1955, that the energy of the Bevatron had been fixed at the value of 6 GeV from the earliest stages of design, in view of the production of antiprotons. On this aspect of the problem see [7–9]).

We will not dwell on the developments that led to the September experiment of the Segrè group (developments that have already been object of historical research, besides having acquired a certain resonance in public opinion for being at the origin of one of the first legal cases linked to a dispute on scientific priorities [8]) (Reports and impressions of the protagonists are found in [10–13]). In the present paper we are interested in following some threads of a parallel path, which winds throughout 1955 and part of the following year, starting from the probable detection of an antiproton trace in emulsions exposed to cosmic rays by the group of physicists in Rome led by Edoardo Amaldi up to the collaboration between this group and the group of Segrè in Berkeley aimed at searching similar events in emulsions irradiated by the Bevatron beam.

The antiproton did not appear on the scene as an unexpected guest, contrary to the puzzling array of new particles that had enriched the zoology of fundamental physics in previous years. The theoretical doubts about its existence, linked to the difficulty of extending Dirac's theory to objects other than the electron, had been largely overcome at the turn of the fifties; although, in the scientific literature of the time, the terms "negative proton" and "antiproton" still significantly coexisted to distinguish different features of this elusive particle [8]. When it was finally detected, "*it was certainly no surprise*" [11] (p. 283). By the mid-fifties, the hunt for the antiproton was open, even if not with the intensity and determination attributed a posteriori. Still at the end of 1954 E.J. Lofgren declared that at the Bevatron (the machine "built to find the antiproton") "*there are no defined plans for looking for anti-protons*" [8] (p. 185). Long before the new Berkeley accelerator came into play, the hunt was carried out using the cheap tools available to cosmic ray physicists: cloud chambers and nuclear emulsions. In the traps set up to catch the fleeting prey, someone had even believed to have caught something.

Between 1947 and 1955, a series of "strange" events recorded by the detectors of different experiments carried out on cosmic radiation had led to advance, with different levels of conviction and determination, the hypothesis of having observed a phenomenon interpretable as evidence for an antiproton annihilation process [14–18]. Everything was however still at the level of "hints" that did not reach the status of "demonstration", as it is exemplarily proved by the precautions with which the results were presented. "*Such an event does not seem unlikely*", "*other possible explanations are that it is a negative proton*" [17] (pp. 937–941), "*one should consider the possibility that the event represents the annihilation process . . . for example, the incident particle might be an antiproton*" [15] (p. 1103), "*one possibility is that it may be produced by an annihilation process*" [18] (p. 857). Claims were thus in the domain of "reasonable possibilities", but no one had yet "persuasive arguments". It is worth pointing out that the arguments which might appear persuasive to some physicists (as in the case of Bruno Rossi, leader of the MIT group, which fully defended the validity of his interpretation of the data) (For example, at the 1956 Rochester Conference Rossi claimed that ". . . *there is thus little doubt that the M.I.T. event was indeed the annihilation of an antiproton*" [19]; see [20] (pp. 95–96)), were not- or were no longer- persuasive arguments for all.

Fairly persuasive appeared the evidence shown by the last of the "strange events" mentioned above. In one of the emulsions exposed to cosmic rays during the campaign in Sardinia in 1953 (On the European collaborations of the early fifties, see [2, 21]), and examined by the group of physicists under the guidance of Edoardo Amaldi in Rome, a double star was found in February 1955, which could be interpreted in terms of the process of "*production, capture and annihilation of a negative proton*". The conclusions reached did not allow to exclude with certainty that the event (known as "Faustina" ("Fausta" is a female proper name, which corresponds to the adjective "fausto", meaning "auspicious"; "Faustina" is the diminutive of "Fausta")) might be an accidental coincidence, but the connected probability was so low that the Roman group felt entitled "*to look for an interpretation of the observed event in terms of physical process and not of an accidental coincidence*" [14] (p. 497).

> *This value* (the expected number of similar events due to casual spatial coincidences in the volume explored) *is sufficiently small to entitle us to look for an interpretation of the observed event in terms of a physical process and not of an accidental coincidence. We are left to consider the star B as produced by the track p. Then the corresponding particle either has a rest energy of the order of 1.5–2 GeV, or, being an antiproton, it has been annihilated by a nucleon, releasing* $2\,m_p c^2 = 1876\,\mathrm{MeV}$. *We do not have any argument in favour of one or the other of these two possibilities apart from the fact that unstable particles of rest energy of the order of 1.5–2 Gev have never been observed; nor has the antiproton,but this, at least, is expected to exist as a consequence of very general arguments based on symmetry with respect to the sign of the electric charge . . .*
>
> *We are glad to express our thanks to Prof. B. Ferretti, Dr. B. Touschek, Dr. G. Morpurgo and dr. R. Gatto for various criticisms, and enlightening discussions.*

Caution, however, was essential: the title originally envisaged for the paper to be published in *Il Nuovo Cimento* was "Unusual Event Produced by Heavy Particle at Rest", but was soon changed in the more cautious "Unusual Event Produced by Cosmic Rays". In the final remarks it was stated that "the many questions raised by the discussion of this event will obviously find their final answer only if other similar events will be observed" [14] (p. 499).

To "observe similar events" and obtain the "final answer" the physicists of Rome decided to use the same observation technique (i.e., the exposure of nuclear emulsions), but in order to avoid the whims of cosmic radiation they aimed at employing a more reliable and controllable source. A few days after the publication of the note on Faustina, Amaldi wrote to Segrè at Berkeley proposing a collaboration to search for a definitive proof of the annihilation of antiprotons, exposing the nuclear emulsion plates to the proton beam of the Bevatron.

> *Now the meaning of our work is the following: we cannot rule out the possibility that Faustina be a casual coincidence, but in case it is due to a real antiproton one should conclude that the corresponding production cross section is large at an energy of about 10 GeV, which is likely the energy of the primary of Faustina's A star. One can then think of trying to produce them also with your machine. True, the energy is much lower, but there is still a good probability to observe them . . .*

> *Now my proposal is as follows: we make an agreement that you set up the experience and make the irradiations, and we take care of development and scanning; if anything worth comes out of the work, we publish together. When I say "you", I mean you Emilio Segrè, or Gerson Goldhaber who works on emulsions and is with you, or both . . .*
>
> *Here all the matter has been discussed extensively with our theoreticians (Ferretti and Touschek) and with the emulsions group* (Amaldi to Segrè, 29 March 1955 [22]).

Segrè accepted the proposal, saying he was "impressed" by Faustina:

> *I have looked carefully to Faustina and I am also impressed by it. I would like to cooperate in the experiment you suggest; Goldhaber would also like to work on it, and Warren Chupp would almost certainly work on it . . . Coming to the practical program: there are at least two programs, of which I know, for hunting the negative protons. One is a photographic one initiated by Rosen of Los Alamos, who has already made an exposure practically identical to your proposal, without the magnet . . . The other method is based on a measurement of momentum and velocity, with a possible photographic check* (Segrè to Amaldi, 15 April 1955 [22]).

Segrè had his reasons for being "impressed". Faustina was a further indication that cosmic ray physicists had good chances of ending the hunt for the antiproton even before his experiment at Bevatron started the operational phase (the experience plan had just been approved by the management of the laboratory (Lawrence Berkeley Laboratory Report (UCRL 2920, November 1954, January 1955))). The question, of course, did not only involve the personal projects and scientific ambitions of Segrè and his group, but invested the entire Radiation Laboratory: a successful conclusion on the existence of the antiproton by the cosmic ray physicists would have nullified most of the scientific arguments advanced by the leaders of the laboratory, Lawrence in the first place, to obtain by the Atomic Energy Commission the provision of the amount of money necessary for the construction of the most expensive experimental apparatus ever made in a physics laboratory.

Starting in March, therefore, the timing of the experiment of the group of Segrè (which we will call the "counter experiment", and is the one that will provide in September the results that will lead to the recognition of the Nobel Prize) intertwined with the timing of the experiment carried out by the collaboration between Rome and Berkeley, which used the same machine through a different technique ("emulsion experiment"). Meanwhile, the composition of the American group in the collaboration was defined. While Amaldi had advanced, besides of course the name of Segrè, the only name of Gerson Goldhaber—the Berkeley expert in emulsions—Segrè extended the participation to Owen Chamberlain, Clyde Wiegand and Warren Chupp (Segrè to Amaldi, 28 june 1955 [22]). Thus, with the sole exception of Tom Ypsilantis, the whole group of the counter experiment was also present in the work with emulsions.

Towards the end of July the emulsions were exposed to the Bevatron beam, and at the beginning of August some of them were sent to Rome to be studied by the Italian team led by Amaldi, which consisted of Giustina Baroni, Carlo Castagnoli, Carlo Franzinetti and Augusta Manfredini. The scanning of the emulsions began in August, while in Berkeley the experiment with the counters started to run. Towards the end of September, the latter provided the first positive data. Amaldi was visiting Segrè in Berkeley at that time, and hastened to inform his team in Rome:

> *There are 7 experiments to find the antiproton . . .* (among them) *one of the Segrè group based on a measurement of velocity from the time of flight between two scintillation counters and a measurement of momentum by deflection through a magnet. Yesterday this experiment started giving results that look positive: nothing is for sure yet, and therefore nothing should be circulated, but possibly a definitive answer will arrive in two or three days: should the thing be confirmed, there must be about one antiproton in 25.000-30-000 negative pions in the conditions of exposition A, that is in the conditions of the stacks 63 and 64 you are scanning . . . Therefore, keep your eyes open and go ahead full force . . .* (Amaldi to Baroni et al., Berkeley, 22 September 1955 [22]).

Only on November 18 the slow work of scanning the emulsions provided the desired result, producing the event called "Letizia" ("Letizia" is another female proper name in Italian language. The word stands for "joy"): a clear annihilation star with a visible energy release that left little doubt about its interpretation.

> *Found Letizia similar Faustina particle protonic mass enters stack 62 left side leading edge comes to rest after* 9.31 cm *and produces star consisting 6 black particles 1 grey proton 1 pion 80 MeV 1 minimum ionization particle stop lower limit energy release 800 MeV stop measurements not yet finished letter follows Amaldi* (Telegramme, Amaldi to Segrè, 18 November 1955 [22]).

Other similar events showed up in the following months, as the Roman and Berkeley groups proceeded in the work of examining the emulsion plates, while in the meantime other Berkeley physicists became involved (The final results of the collaboration are in [23, 24]). On 11 January 1956, an annihilation star was found in the plates scanned by the Berkeley team clearly showing an energy release that dispelled any trace of remaining doubts:

> *This event turned out to be particularly important because it gave the conclusive proof ("sufficient condition" for those who were still in doubt) of the annihilation process. The visible energy release in this star was* 1300 ± 50 *MeV. Clearly greater than the mass of the incident negative particle! ... Chamberlain gave an invited talk at the 1956 New York meeting of the American Physical Society. There he reported on both the counter experiment and our annihilation event. He told me afterward that the proof supplied by the annihilation event was an important ingredient in the minds of the audience* [25].

Goldhaber's remarks point to two key issues: the kind of evidence deemed necessary to claim the discovery, and the effectiveness of how the evidence was presented in creating a consensus about what actually had been observed. The second point is related to the wider issue of the complementary interplay of visual and logic means to provide information on the piece of nature under investigation. In the case here considered, "seeing" the annihilation, beside the intrinsic convincing power given by visualisation, had the advantage of showing the physical process that allowed to properly label the particles as antiprotons and not just negative protons, something that from the "logic" inference offered by the counter experiment could not be derived (The standard reference on the general issue is [26]. For the specific case of the antiproton discovery, a strong case is found in [27]).

When was the antiproton "discovered"? Excessive attention to priority disputes has produced historiographical practices of dubious reputation, but scientific priority is not the main historical issue to be addressed here. Indeed, behind the above question stands precisely the problem of the building of persuasive arguments, able to finally transform "hints" into "demonstrations" and to allow the formation of consensus around a new stabilized piece of knowledge. Is the identification of particles having protonic mass and negative charge (the evidence provided by the experiment with the counters) a "demonstration" of the existence of the antiproton? It is worthwhile to listen to some of the protagonists:

> (E. Segrè was able) *to establish the existence of a small but clearly observable number of negative protons, among the particles produced by the collision of protons of 6.3 GeV, accelerated with the Bevatron, against quiet nuclei ... That they were antiprotons in Dirac's sense, i.e. corpuscles capable of annihilating themselves with as many protons, was demonstrated in an experiment carried out with the technique of nuclear emulsions by the same group extended with the addition of G. Goldhaber et al. in Berkeley and by E. Amaldi et al. in Rome* [28] (p. 121, original text in Italian).

By October 1955, the counter experiment had clearly demonstrated the following:

- *There were negative particles of protonic mass within an accuracy of 5 percent.*
- *There was a threshold for the production of these particles at about 4 GeV of incident-proton-beam kinetic energy.*

These were necessary conditions for the identification of antiprotons.

> *Then, in November 1955, our efforts in the emulsion experiment ... yielded one event, found in Rome, that came to rest and produced a star with a visible energy release of about 826 MeV. Again a* <u>*necessary condition*</u> (Here and in the following quotations, underscores are made by the author of the present paper) *for antiprotons* [25] (p. 267).

To evaluate the margins of uncertainty within which the discussion was still moving at the end of 1955, it is useful to compare some crucial steps of the conclusions of the preliminary work in which Letizia was presented, in the different versions we have: the transcription of the report made by Amaldi at the monthly session of the Accademia dei Lincei in December, the Note published in the *Rendiconti* of the Accademia (both in Italian) and the translation of the latter, which appeared soon after in the *Physical Review*. In the Amaldi report we read:

> *It can therefore be* <u>*concluded*</u> *that this process is due to an antiproton ... This observation is in a sense complementary and integrates the discovery of Chamberlain, Segrè, Wiegand and Ypsilantis announced in mid-October by the Radiation Laboratory.*
>
> *On the other hand the disintegration observed in the emulsions exposed to the Bevatron has the same characteristics as that observed at the beginning of 1955 by the group of Rome in emulsions exposed to*

> *cosmic radiation and therefore it can be concluded that the interpretation of that event, proposed at the time, in terms of an antiproton annihilation process, was correct. (Report by Amaldi at the meeting of the Accademia dei Lincei, December 10, 1955, p. 3; Amaldi Archive (section Archivio Amaldi Eredi, box 21) Original document in Italian*).

These statements sound less conclusive in the text published in the *Rendiconti*:

> *This event confirms, even if not definitively, the interpretation ... that the new particles observed at the Bevatron are antiprotons. It also confirms the hypothesis that the star described in (5)* (i.e., Faustina, authors' note) *was actually due to an antiproton* [29] (p. 386, original document in Italian).

Finally, the statements appearing in the *Physical Review* are clearly weaker:

> *This event is corroborating evidence, but not final proof, for the interpretation ... that the new particles observed at the Bevatron are antiprotons. It also gives support to the hypothesis that the star described in ref. 5 was indeed due to an antiproton* [30] (p. 910).

There is a certain difference between drawing a "conclusion" and having "corroborating evidence". A conclusion is definitive; it does not require further "final proof". Even leaving out the transcription, understandably stronger, of the report to the Italian Academy and concentrating on the two published works, it is not possible to ignore the subtle linguistic discrepancies (and this despite Segrè had explicitly insisted, arousing Amaldi's irritation, that the note for the *Rendiconti* should be the "literal translation" of the one prepared for the *Physical Review*) (Segrè to Amaldi, 29 November 1955; Amaldi to Segrè, 5 December 1955 [22]). The two "confirmations" become "corroborating evidence" and "support"; one may argue that "confirmation" applies to what is already firmly established, while "support" is needed for what cannot stand on its own.

The point is that it was not simply a question of subtle linguistic discrepancies. The apparent inconsistencies can be explained if one takes into account the fact that what was said emerged as the result of a mediation in which not everyone wanted to say the same things. Different expectations were at stake, which triggered different priorities and led the two groups to place different emphasis on the various aspects of the results achieved. For the physicists in Rome, in particular, it was central to highlight the "similarity" of Letizia with Faustina; precisely what the Berkeley physicists were not willing to concede. Physicists in Rome were looking for annihilation stars in order to confirm their interpretation of the dubious result they already had—Faustina, the uncertain annihilation star observed in the cosmic radiation—and regarded Letizia as sufficient final proof for their hunt; physicists in Berkeley were looking for annihilation stars in order to prove that the solid result they already had—the detection with counters of negative protons—was indeed the discovery of the antiproton, and judged Letizia not yet "final proof" for that purpose.

These tensions may easily be documented in more detail by resorting to the extensive exchange of correspondence that took place since November 1955 between Amaldi and Segrè, concerning the forms of publication of the scientific results of the collaboration (The exchange of letters is in [22, 31]). What we wish to emphasise here is that these difficulties arise naturally from the inherent tension between the desire to make definitive claims about nature and the inevitable ambiguity with which nature reveals itself; in Rome and Berkeley, attempts were being made to construct "persuasive arguments" in ways that do not coincide.

Be it as it may, in the end a consensus was built, or at least a mediation was reached, given that a text was published, in which all the authors shared with equal weight credits and responsibilities, and the disagreements on the interpretations of the results were canceled in the unanimous version that was exposed to the judgment of the scientific community. If for the group in Rome Letizia represented necessary and sufficient proof, while this was not the case for the team in Berkeley, then it remains to be understood which was the shared "persuasive argument", on which the groups based the agreement reached in the final version of the published work. Indeed they agreed to insert the controversial statement that the observed event was to be considered "not final proof". The authority of the facts was, by itself, insufficient to impose a decision; and then the intervention of some other kind of authority was needed to release the tension and resolve the issue. It is an authority that doesn't come out of the lines of the published paper; however, we can identify it with reasonable certainty from other sources:

> *This laboratory accepts first change but not omission words but not final proof or equivalent please cable whether we should mail letter Physical Reviews we want to see Italian text nota Licei (sic) before publication Segrè* (Telegramme, Segrè to Amaldi, 14 December 1955 [22]).

The final agreement was no longer (not only, anymore) the result of a comparison between the arguments of Amaldi and those of Segrè. It was "this laboratory", the authority of the prestigious- and rich, and powerful—Lawrence Radiation Laboratory, which was putting all its weight on the plate of the discussion to close it; thus intervening, not as a spurious element introducing irrationality into a process regulated by pure reason, but as a concrete part of a framework in which the game was played according to rules that did not have the clear "certainty of logic".

That the "big bosses" in Berkeley were playing the game, and not just his old friend Segrè, Amaldi made clear in a letter sent to Gian Carlo Wick:

> *We have actually found here in Rome a nice star due to a negative corpuscle with mass (1830 ± 55)* m_e *very similar to the one we found in January in the cosmic radiation. We are in the publication process but we have some small difficulties as to the final text. Judging from what is happening these days it seems that the big bosses in Berkeley are rather difficult to deal with. You might possibly tell me that you were already well aware of that!* (Amaldi to Wick, 15 December 1955; Amaldi Archive (section Archivio Dipartimento Fisica, box 5). Following his emigration to the States after the war, in 1948 Wick had replaced Robert Oppenheimer in Berkeley as professor of theoretical physics, a post from which he was dismissed in 1951 for refusing to swear the anticommunist loyalty oath required by the State of California).

"When" was the antiproton discovered? Everything suggests that, abandoning the easy punctual attributions possible *a posteriori*, it makes sense to answer by transforming the discovery from an event into a long-term process: and to argue that the "change in the status of evidence" that led to accumulating arguments persuasive for everyone, changing a series of "hints" into a definitive "demonstration"—and into a new cognitive acquisition—was the terminal stage of a journey that began somewhere in 1954 and ended at some point in the first half of 1956. Through this path two novelties were established in parallel: the existence of a new element of nature, and the emergence of a new way of questioning nature and formulating convincing assertions about it. Perhaps involuntarily, but certainly in a strongly symbolic way, this passage was well represented by the titles of the papers which announced Letizia to the world. The preliminary version of the work, published in English and in Italian respectively on the *Physical Review* and on the *Rendiconti* of the Accademia dei Lincei, was entitled "Antiproton Star Observed in Emulsion"; the accent was on "emulsion", the revelation technique that in the previous years had established itself as the workhorse of cosmic ray physics. But the title of the most complete work that appeared in *Il Nuovo Cimento* [32] sounded "On the Observation of an Antiproton Star in Emulsion Exposed at the Bevatron"; the chief protagonist was no longer the emulsion, the technique that allowed to see the antiproton, but the Bevatron, the machine that made it possible to make it.

Awarding of the Nobel prize to Segrè and Chamberlain "for their discovery of the antiproton", and the ensuing focus on the "ingenious methods" employed in their "logic" counter experiment, has left in the shade the contribution given to the whole process of discovery by the "visual" work with the emulsions (In his Nobel lecture [10], entirely devoted to an accurate description of the techniques used in the counter experiment, Chamberlain devotes only a few closing lines to the emulsion work, highlighting the "final proof" provided by the annihilation star found in Berkeley in January 1956, and mentioning that "other important work closely related to the same subject has occupied Professor Amaldi with his colleagues at the University of Rome"); as a consequence, in turn, any reference to the previous evidence gained by the work done on cosmic rays has been wiped off in the collective memory of the community. In the process, the role played by the emulsion scanners in Rome, and their previous pioneering work on cosmic rays, has been, if not neglected at all, largely underestimated. A good example of this is given by Segrè in his autobiography:

> *My group had for some time studied the problem and prepared for it. I decided to attack the problem in two ways. One was based on the determination of the charge and mass of the particle. The other concentrated on the observation of the phenomena attendant on the annihilation of a stopping antiproton . . .*
>
> *For the first attack, Chamberlain, Wiegand, Ypsilantis and I designed and built a mass spectrograph with several technically new features. For the second attack, Gerson Goldhaber, who was then in my group, exposed photographic emulsions in a beam enriched in antiprotons by our apparatus. Many other people were involved in the enterprise, and we had agreements on how to publish the results and give appropriate credit to everyone . . .*
>
> *The mass-spectrograph experiment concluded on 1 October 1955, having proved the existence of the antiproton, and soon thereafter the emulsion work confirmed it . . . At the time of the antiproton*

> *experiment, Amaldi and his wife Ginestra were at our home in Lafayette as our guests. He and I established a collaboration for the study of photographic emulsions exposed at Berkeley, taking advantage of the numerous well-trained scanners available in Rome* [13] (pp. 256–258).

There is nothing wrong in these lines, but, in my opinion, the overall picture, although not incorrect, can be misleading. It is true that Segrè "decided to attack the problem in two ways", but the second decision (hunting the antiproton with the emulsions) was taken as a result of a suggestion by Amaldi, as their correspondence clearly indicates. In Segrè's reconstruction of the events, Amaldi only appears at the end, and it sounds like their collaboration started after his visit to Berkeley in September, while it had origin with Amaldi's proposal in March. It is well known that perceptions of the past become altered in the course of time, even in the memory of the historical actors. As historian of physics John Heilbron has effectively put it,

> *one can understand that most historians do not consider the unsupported recollections of former participants very good evidence about events in the distant past. The problem of partial observation is in this case compounded by failing and selective memory* [7].

And so it also happens that "partial observation" and "failing and selective memory" sometimes cooperate to lead former protagonists to present reconstructions of past events giving credit to narrations diminishing their own personal contributions, as is the case with Giulio Cortini, one of the members of the Roman team that found Faustina:

> *The antiproton was in the air... A group of leading experimental physicists in Berkeley designed and performed an experience aimed at the final demonstration of its existence. The experiment was successful and was rewarded with a Nobel prize. Nonetheless, they wanted a more sensational confirmation: producing in their nuclear plates phenomena analogous to "our" . . . Amaldi was in touch with the Berkeley group, and thanks to his prestige our group was associated to their "second" experiment: they sent us plates that had been exposed to the beam of antiprotons produced by their 6.3 GeV machine, and we found there the "first" event similar to "Faustina": telegram, congratulations. But naturally the prestige of this new result, and of those who followed, fell largely on them . . .* [33] (pp. 84–87).

Again, nothing wrong, but a picture that, in my opinion, could be misleading. The Berkeley physicists wanted indeed a more sensational confirmation, but the nuclear plates were not "their" plates. Cortini should have said "our" plates; it was Amaldi who ordered the emulsions from the Ilford Company in Britain, and had them shipped to Berkeley, and he did so not because he had been "associated to their second experiment", but because of that second experiment he had been the prime mover.

Yes, as Cortini puts it, "*the prestige of this new result, and of those who followed, fell largely on them*". So much so that, in the Feature article "Fifty years of antiprotons" published in the November 2005 issue of the CERN Courier, one finds a beautiful picture of "the first annihilation star imaged in the photographic-emulsion stack experiments, led by Gerson Goldhaber of the Segrè group, which confirmed the discovery of the antiproton"; it is a reproduction of Letizia, found in Rome in the photographic-emulsion stack experiments led by Edoardo Amaldi, the annihilation star which at the time was considered by the Berkeley physicists as being "not final proof" for the confirmation of the discovery of the antiproton.

A last remark is probably in order. Regardless of the scarce mark left in posthumous memories, the work on emulsions performed in the fifties at the physics institute in Rome was an exciting time for all the experimentalists involved. Most likely, not only for them. From the closing lines of the Faustina paper, and from the recollections of some of the protagonists, it is clear that their results, and their meaning and possible interpretations, were subject of discussions with the small group of theoretical physicists active in the institute and at the new born laboratory of INFN in Frascati: Bruno Ferretti, Bruno Touschek, Giuseppe Morpurgo and Raoul Gatto. Cortini in particular remembers the "very important contribution" to these "heated discussions" given by Touschek, who "took the thing very seriously". With hindsight, knowing the fundamental contributions given by the Roman school of theoretical physics in the following years, it is tempting to assume that the involvement of theoreticians in Rome in the discussion about the antiproton findings of their experimentalist colleagues most likely contributed to strengthen their confidence in symmetry arguments. And the actual making of the antiproton turned antimatter from a theoretical speculation into a manageable tool. It is possible to suggest that in this respect the discovery of the antiproton contributed to pave the way in Rome for theoretical and experimental developments that followed, from the consequences of CPT theorem to matter-antimatter physics.

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Conflicts of Interest

The author declares no conflict of interest.

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