La scoperta dell'antiprotone tra Roma e Berkeley

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Emilio Segrè e Edoardo Amaldi al Cavendish Laboratory, Cambridge, estate 1934 (a sinistra) e circa 50 anni più tardi (a destra)

Emilio Segrè, Edoardo Amaldi, Bruno Rossi

Edoardo Amaldi e Oreste Piccioni, a Berkeley nel 1985 in occasione del convegno per il trentesimo anniversario della scoperta dell’antiprotone

“It is a fact which I have discovered, in my silent, so to say, litigation with Segrè and Chamberlain, that people have two notions in their mind as to why they work in scientific research. One is that when they have something of their own interest in question, they would kill their mother in order to have a little bit more credit. But the other one is that we should all work for the beauty of science or maybe for the benefit of mankind, not asking for credit whatsoever. It is amazing how many of our colleagues live their entire life on this double standard, but they do.”

(O. Piccioni a M. Conversi, 1971)

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“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.”

(P. Galison, “How experiments end”, 1987)

La scoperta dell’antiprotone: una storia all’incrocio di diverse tensioni

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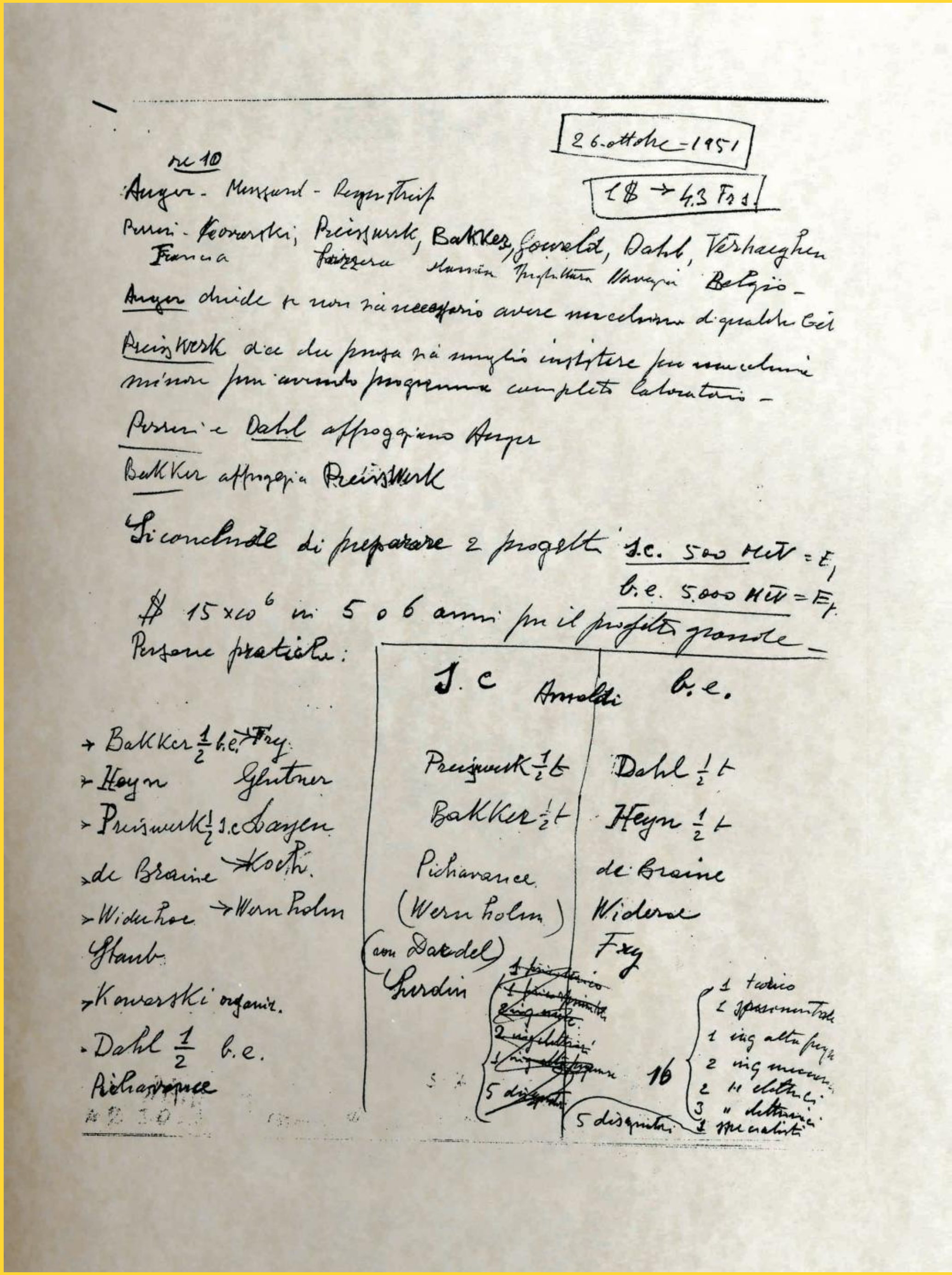
- Europa / Stati Uniti

Alcuni dei padri fondatori del CERN: da sinistra, Pierre Auger, Edoardo Amaldi, nominato Segretario generale del CERN provvisorio alla prima sessione del Consiglio a Parigi nel maggio 1952, e il fisico francese Lew Kowarski, che diventò direttore del gruppo del Laboratorio incaricato della preparazione dei lavori.



rc 10

26-ottobre-1951

1$ → 4.3 Frs.

Auger - Mussard - Regenstreif

Perrin - Kowarski; Preiswerk, Bakker, Goward, Dahl, Verhaeghe
Francia Svizzera Olanda Inghilterra Norvegia Belgio -

Auger chiede se non sia necessario avere macchina di qualche GeV

Preiswerk dice che pensa sia meglio insistere per macchine minori pur avendo programma completo laboratorio -

Perrin e Dahl appoggiano Auger

Bakker appoggia Preiswerk

Si conclude di preparare 2 progetti s.c. 500 MeV = E1
b.e. 5.000 MeV = E2

$ 15×10^6 in 5 o 6 anni per il progetto grande -

Persone pratiche:

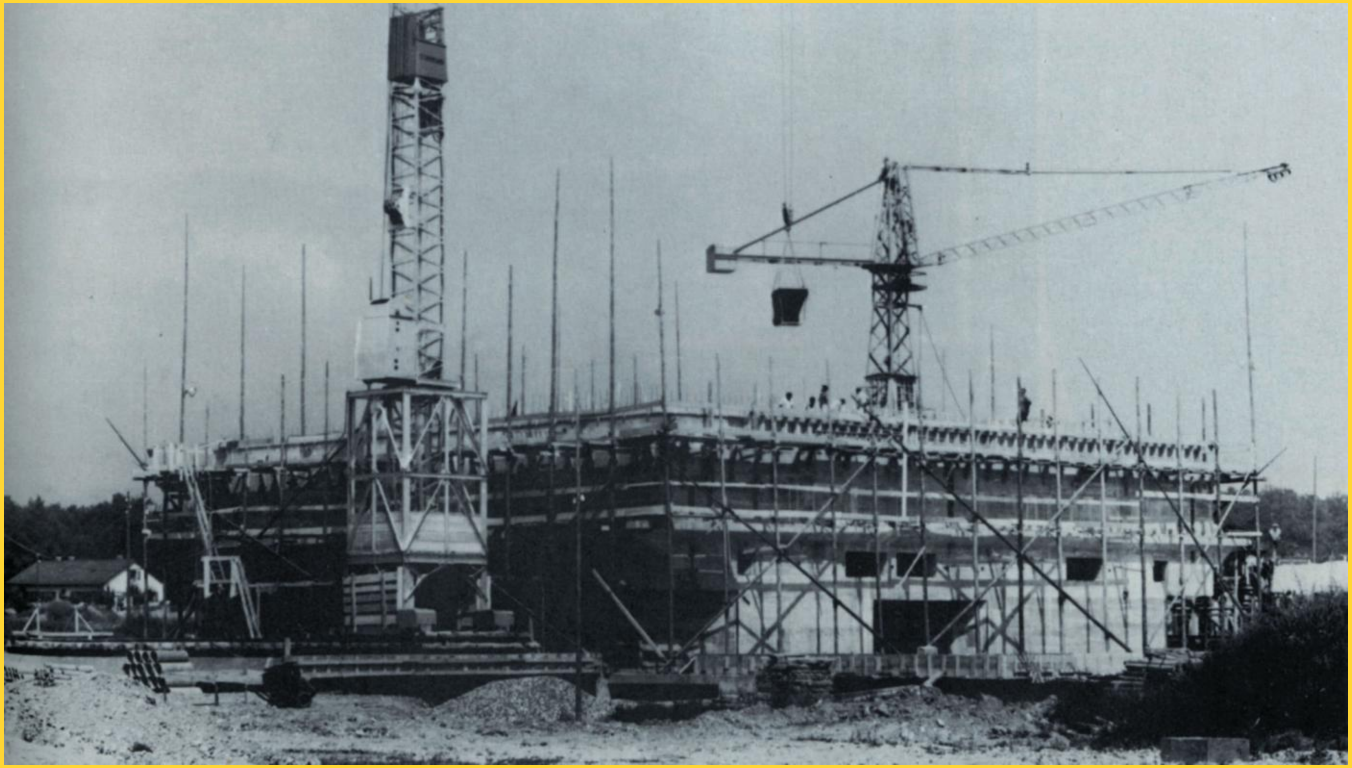
→ Bakker ½ b.e. → Fry
→ Heyn Gentner
→ Preiswerk ½ s.c. Bayen
→ de Braine → Koch
→ Wideröe → Wernholm
Staub
→ Kowarski organiz.
· Dahl ½ b.e.
Pickavance

s.c.	Amaldi	b.e.
Preiswerk ½ t		Dahl ½ t
Bakker ½ t		Heyn ½ t
Pickavance		de Braine
(Wernholm)		Wideröe
(con Dardel)		Fry
Gerdin		

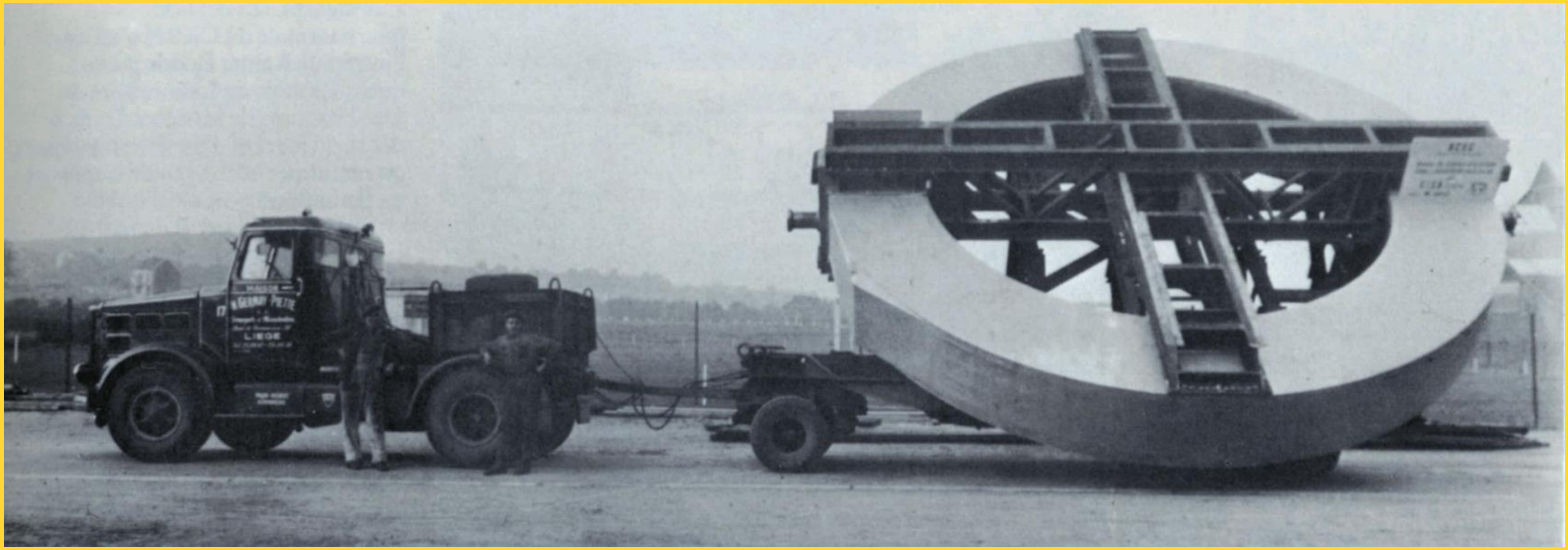
~~1 fisico teorico~~
~~1 fisico sperimentale~~
~~2 ing. mecc.~~
~~2 ing. elettrici~~
~~1 ing. alta frequenza~~
~~5 disegnatori~~

16
1 teorico
1 sperimentale
1 ing. alta freq.
2 ing. meccanici
2 " elettrici
3 " elettronici
1 specialisti
5 disegnatori

Appunti di Edoardo Amaldi relativi alla riunione del gruppo dei consulenti del 26 ottobre 1951; è notata la decisione di preparare due progetti di macchine acceleratrici, un sincrociclotrone da 500 MeV e un sincrotrone per protoni da 5000 MeV. Le energie effettive saliranno rispettivamente fino a 600 e 28000 MeV.



1955. Prende forma
l’edificio che dovrà
ospitare il
sincrociclotrone
da 600 MeV.
In dicembre arriva
sul sito del
Laboratorio
la gigantesca
bobina del magnete.



Raggi cosmici e acceleratori.

Pisa 1955

“... 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.” (Peyrou 1989, p. 631)

“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 the Brookhaven National Laboratory (3 GeV) but even more by the Bevatron of the Lawrence Radiation Laboratory in Berkeley (6.3 GeV)”. (Amaldi 1988, p. 117)

Rochester 1956

“Leighton then suggested 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” (*High Energy Nuclear Physics. Proceedings of the Sixth Annual Rochester Conference*, April 3-7, 1956 , section VI, p. 28)

Il laboratorio della Testa Grigia per lo studio dei raggi cosmici, realizzato nel 1947 presso la stazione superiore della funivia del Plateau Rosa sopra Cervinia, a 3500 metri di quota.

Le collaborazioni internazionali nella ricerca sui raggi cosmici (1952-1954)

- **Sardegna, giugno-luglio 1952 (Bristol, Bruxelles, Glasgow, Gottinga, Londra, Lund, Milano-Genova, Padova, Parigi, Roma-Cagliari, Torino)**
- **Sardegna, maggio-giugno 1953 (Berna, Bristol, Bruxelles, Caen, Catania, Copenhagen, Dublino, Gottinga, Londra, Lund, Milano-Genova, Oslo, Padova, Parigi, Roma, Sydney, Torino, Trondheim, Uppsala, Varsavia)**
- **G-stack, ottobre 1954 (Bristol, Dublino, Copenhagen, Milano-Genova, Padova)**

“Better proofs” grazie a “better tools”

Il Bevatrone di Berkeley. Nel 1955 era l’unica macchina al mondo in grado di accelerare un fascio di protoni all’energia di 6.3 GeV, superiore all’energia di soglia per la produzione di coppie protoni-antiprotoni.

“Why was the machine built? The usual answer from physicists is that the machine came into existence to make antiprotons, or to make particle physicists independent of cosmic rays… With the detection of the negative proton in 1955, it realized its purpose and justified its expense.

To this account, historians would add some or all of the following:

1. The AEC built the Bevatron in order to investigate nuclear forces in the hope that they might be exploited in new sorts of weaponry.
2. The AEC did not build the Bevatron so much to knock the nucleus to pieces as to provide an opportunity to keep the experienced engineering staff at Berkeley together for mobilization in a national emergency.
3. The AEC cared little about particle physics, but much about maintaining good cheer at Berkeley, which was the only one of the Manhattan Engineer District’s installations untouched by the severe decline in morale and staff suffered by the district immediately after the war.
4. The Bevatron, despite its uniqueness in energy, is best understood as only the biggest of the many redundant accelerators commissioned at universities in the immediate postwar years by the Manhattan Engineer District, the Office of Naval Research, and the AEC:

There is good documentary evidence for the several answers I have given to the question, Why the Bevatron? Curiously, the least frequently mentioned before 1955 was the making of antiprotons or the enlargement of the human spirit by the hunt for fundamental particles. The design energy of the Bevatron rose and fell with political processes and financial circumstances, not with calculations of production thresholds.”

J. Heilbron, “An historian’s interest in particle physics” (1989)

Gli "eventi strani" nei raggi cosmici

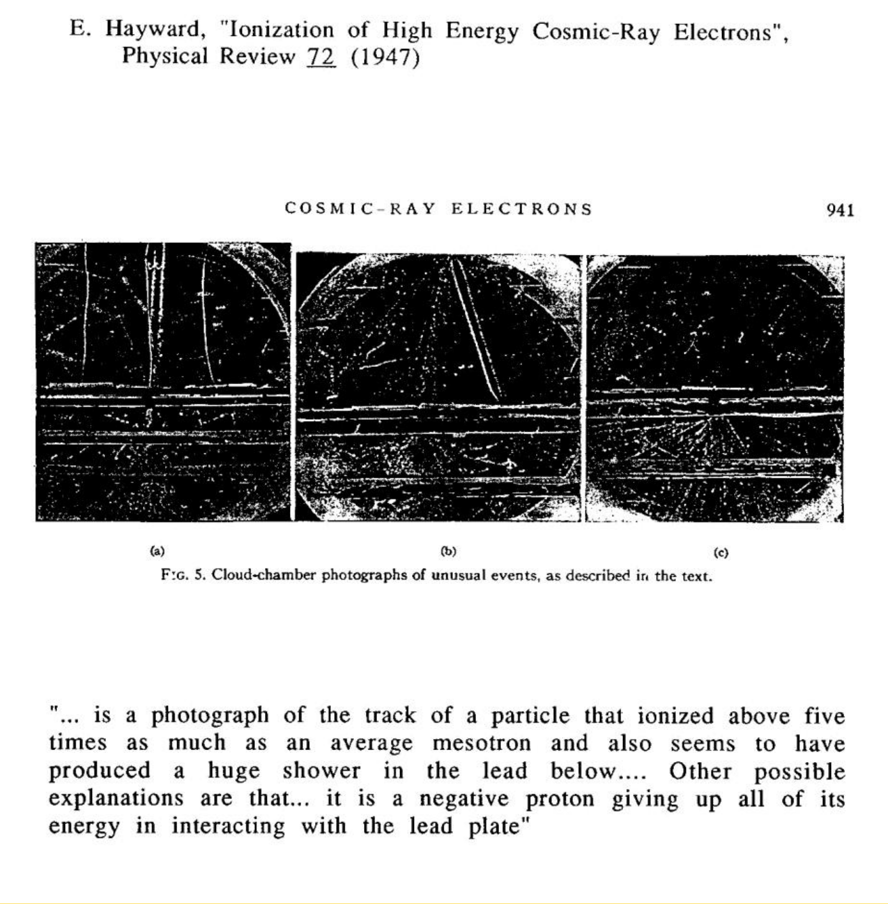
E. Hayward, "Ionization of High Energy Cosmic-Ray Electrons", Physical Review 72 (1947)

E. W. Cowan, "A V-Decay Event with a Heavy Negative Secondary, and Identification of the Secondary V-Decay Event in a Cascade", Physical Review 94 (1954)

M. Schein, D.M. Haskin, and R.G. Glasser, "Narrow Shower of Pure Photons at 100000 Feet", Physical Review 95 (1954)

H.S. Bridge, H. Courant, H. DeStaebler, Jr., and B. Rossi, "Possible Example of the Annihilation of a Heavy Particle", Physical Review 95 (1954)

E. Amaldi, C. Castagnoli, G. Cortini, C. Franzinetti and A. Manfredini, "Unusual Event Produced by Cosmic Rays", Il Nuovo Cimento Vol. I N. 3 (1955)



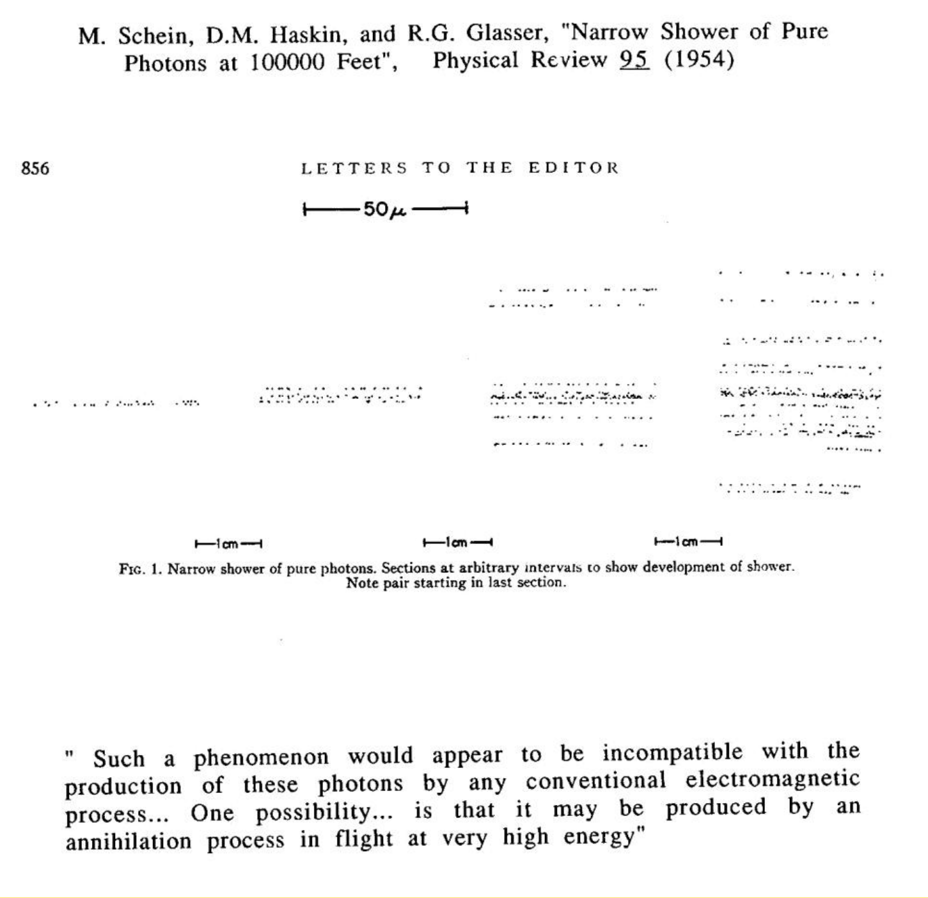
E. Hayward, "Ionization of High Energy Cosmic-Ray Electrons", Physical Review 72 (1947)

COSMIC-RAY ELECTRONS 941

(a) (b) (c)

FIG. 5. Cloud-chamber photographs of unusual events, as described in the text.

"... is a photograph of the track of a particle that ionized above five times as much as an average mesotron and also seems to have produced a huge shower in the lead below.... Other possible explanations are that... it is a negative proton giving up all of its energy in interacting with the lead plate"



M. Schein, D.M. Haskin, and R.G. Glasser, "Narrow Shower of Pure Photons at 100000 Feet", Physical Review 95 (1954)

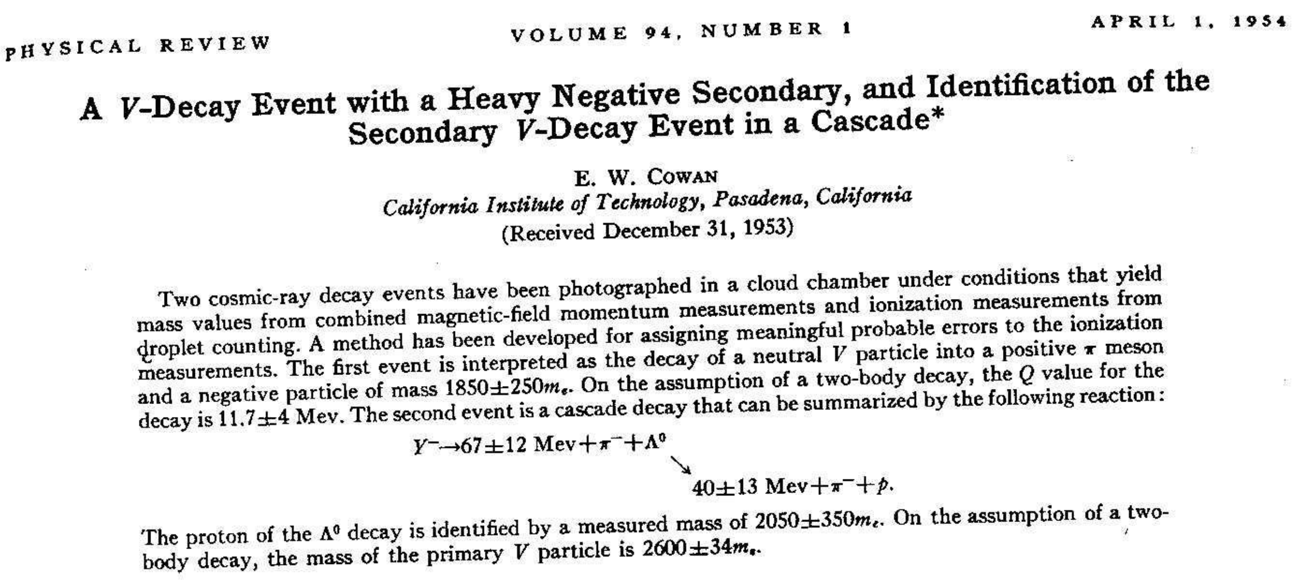
856 LETTERS TO THE EDITOR

FIG. 1. Narrow shower of pure photons. Sections at arbitrary intervals to show development of shower. Note pair starting in last section.

" Such a phenomenon would appear to be incompatible with the production of these photons by any conventional electromagnetic process... One possibility... is that it may be produced by an annihilation process in flight at very high energy"

“Other possible explanations are that… it is a negative proton” (Hayward 1947)

“One possibility… is that it may be produced by an annihilation process” (Schein et al. 1954)



PHYSICAL REVIEW VOLUME 94, NUMBER 1 APRIL 1, 1954

A *V*-Decay Event with a Heavy Negative Secondary, and Identification of the Secondary *V*-Decay Event in a Cascade*

E. W. Cowan
California Institute of Technology, Pasadena, California

(Received December 31, 1953)

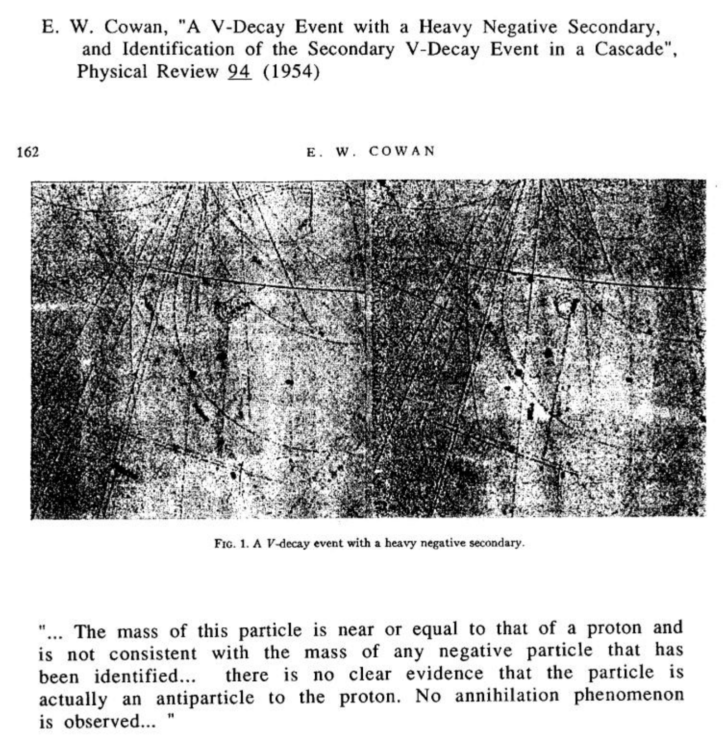
Two cosmic-ray decay events have been photographed in a cloud chamber under conditions that yield mass values from combined magnetic-field momentum measurements and ionization measurements from droplet counting. A method has been developed for assigning meaningful probable errors to the ionization measurements. The first event is interpreted as the decay of a neutral *V* particle into a positive π meson and a negative particle of mass $1850\pm250m_e$. On the assumption of a two-body decay, the *Q* value for the decay is 11.7 ± 4 Mev. The second event is a cascade decay that can be summarized by the following reaction:

$$Y^- \rightarrow 67\pm12\ \text{Mev}+\pi^-+\Lambda^0$$

$$\searrow 40\pm13\ \text{Mev}+\pi^-+p.$$

The proton of the Λ^0 decay is identified by a measured mass of $2050\pm350m_e$. On the assumption of a two-body decay, the mass of the primary *V* particle is $2600\pm34m_e$.

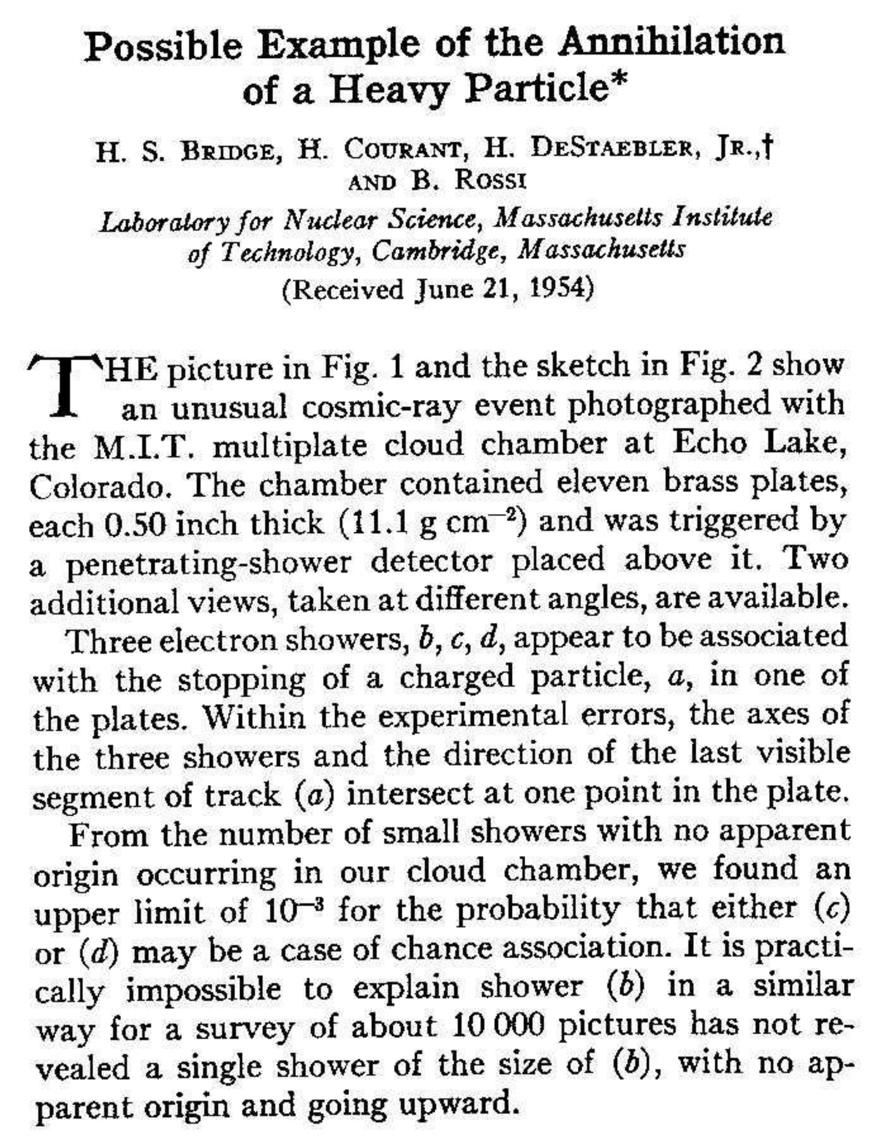
"The mass of this particle is near or equal to that of a proton and is not consistent with the mass of any negative particle that has been identified… there is no clear evidence that the particle is actually an antiparticle to the proton. No annihilation phenomenon is observed…" (Cowan 1953)



E. W. Cowan, "A V-Decay Event with a Heavy Negative Secondary, and Identification of the Secondary V-Decay Event in a Cascade", Physical Review 94 (1954)

FIG. 1. A *V*-decay event with a heavy negative secondary.

"... The mass of this particle is near or equal to that of a proton and is not consistent with the mass of any negative particle that has been identified... there is no clear evidence that the particle is actually an antiparticle to the proton. No annihilation phenomenon is observed... "



Possible Example of the Annihilation of a Heavy Particle*

H. S. Bridge, H. Courant, H. DeStaebler, Jr.,† and B. Rossi

Laboratory for Nuclear Science, Massachusetts Institute of Technology, Cambridge, Massachusetts

(Received June 21, 1954)

THE picture in Fig. 1 and the sketch in Fig. 2 show an unusual cosmic-ray event photographed with the M.I.T. multiplate cloud chamber at Echo Lake, Colorado. The chamber contained eleven brass plates, each 0.50 inch thick (11.1 g cm^{-2}) and was triggered by a penetrating-shower detector placed above it. Two additional views, taken at different angles, are available.

Three electron showers, *b*, *c*, *d*, appear to be associated with the stopping of a charged particle, *a*, in one of the plates. Within the experimental errors, the axes of the three showers and the direction of the last visible segment of track (*a*) intersect at one point in the plate.

From the number of small showers with no apparent origin occurring in our cloud chamber, we found an upper limit of 10^{-3} for the probability that either (*c*) or (*d*) may be a case of chance association. It is practically impossible to explain shower (*b*) in a similar way for a survey of about 10 000 pictures has not revealed a single shower of the size of (*b*), with no apparent origin and going upward.

H.S. Bridge, H. Courant, H. DeStaebler, Jr., and B. Rossi, "Possible Example of the Annihilation of a Heavy Particle", Physical Review <u>95</u> (1954)

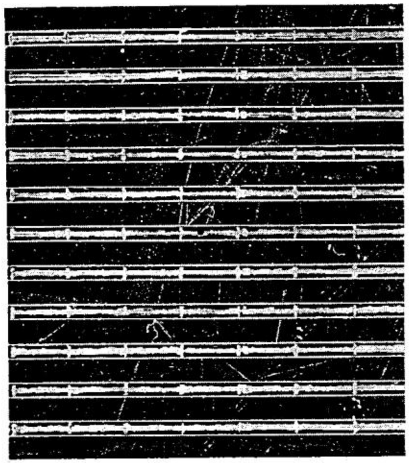


Fig. 1. Cloud-chamber photograph of the cosmic-ray event.

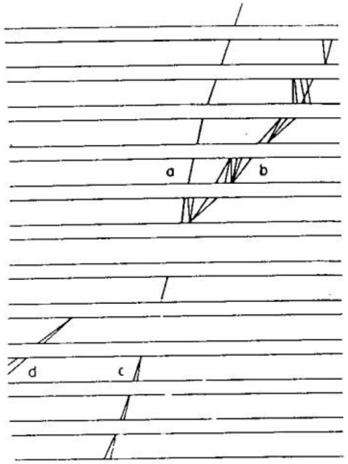


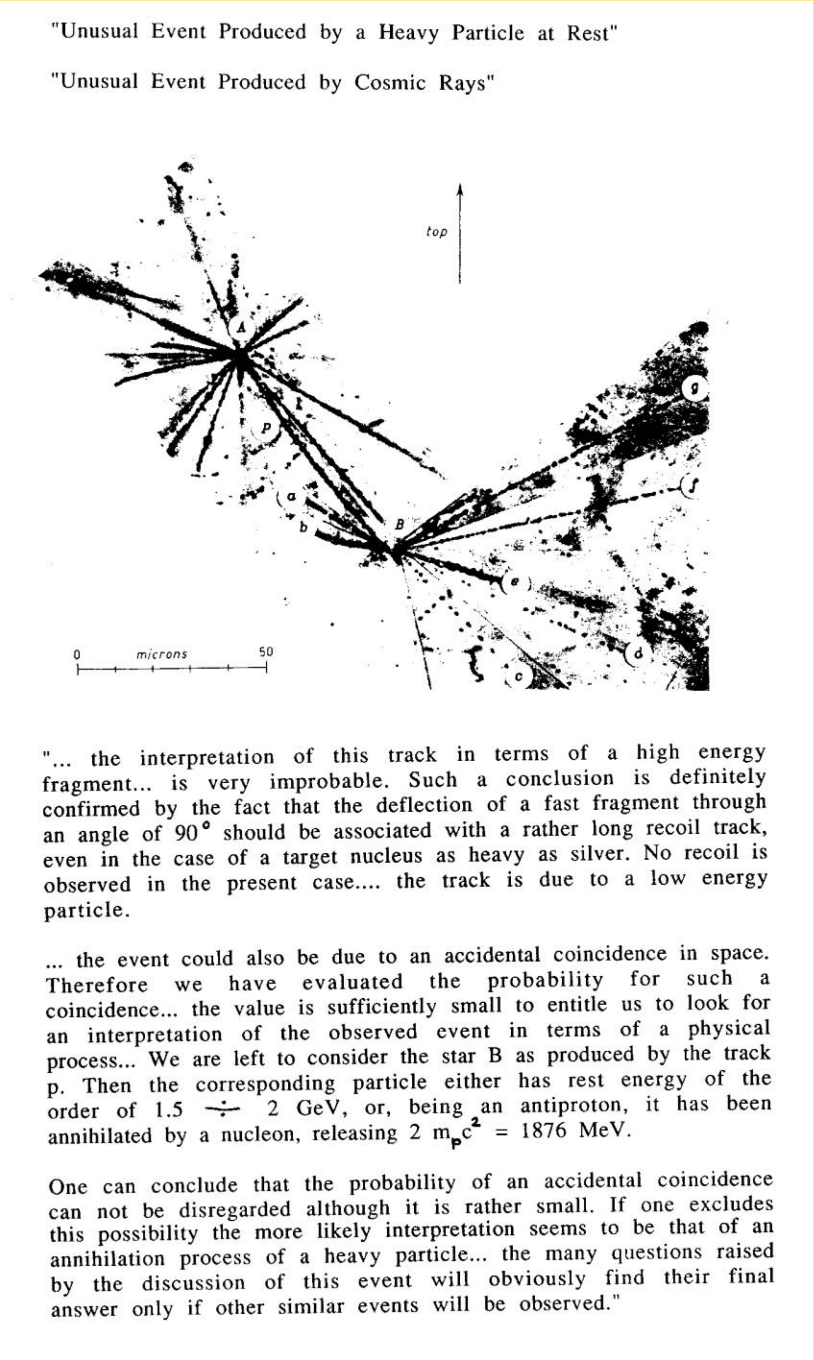
Fig. 2. Sketch of the cosmic-ray event.

"In view of the difficulties of interpreting the event as a decay or an absorption process, one should consider the possibility that the event represents the annihilation process of two heavy fermions. For example, the incident particle might be an antiproton (or an antihyperon) that undergoes annihilation with an ordinary proton. A large fraction of the energy liberated in such a process may well be changed into π^0 mesons and thus ultimately appear in the form of γ rays"

B. Rossi, Rochester Conference, 1956:

"... we used the photometric method to re-analyze the M.I.T. antiproton event, and found a value of 823 ± 155 Mev for the rest energy of this primary particle.... there is thus little doubt that the M.I.T. event was indeed the annihilation of an antiproton"

"… there is thus little doubt that the M.I.T. event was indeed the annihilation of an antiproton" (Rossi 1956)



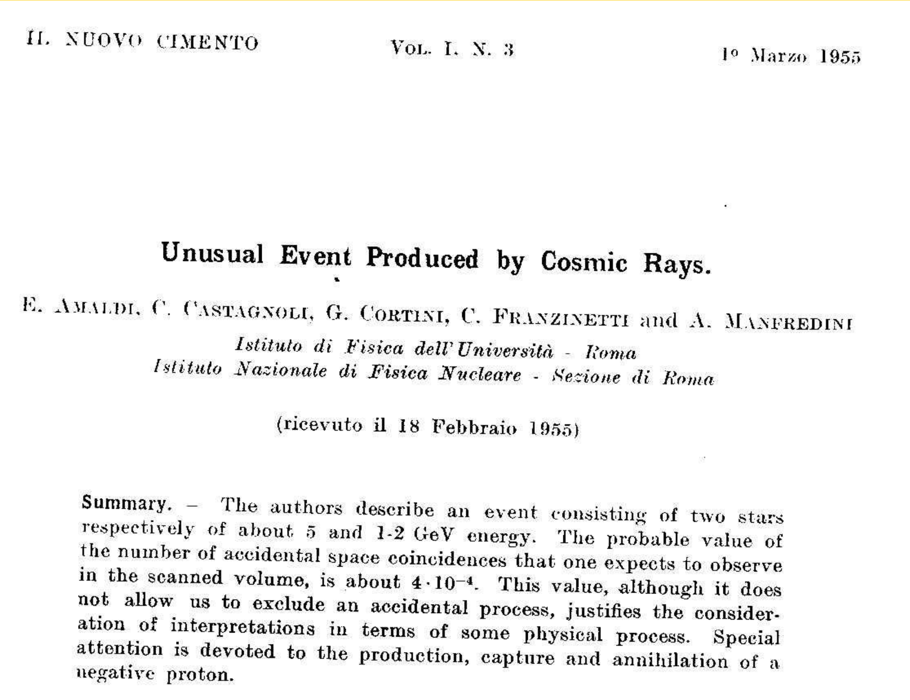
"Unusual Event Produced by a Heavy Particle at Rest"

"Unusual Event Produced by Cosmic Rays"

"... the interpretation of this track in terms of a high energy fragment... is very improbable. Such a conclusion is definitely confirmed by the fact that the deflection of a fast fragment through an angle of 90° should be associated with a rather long recoil track, even in the case of a target nucleus as heavy as silver. No recoil is observed in the present case.... the track is due to a low energy particle.

... the event could also be due to an accidental coincidence in space. Therefore we have evaluated the probability for such a coincidence... the value is sufficiently small to entitle us to look for an interpretation of the observed event in terms of a physical process... We are left to consider the star B as produced by the track p. Then the corresponding particle either has rest energy of the order of 1.5 ÷ 2 GeV, or, being an antiproton, it has been annihilated by a nucleon, releasing $2\ m_p c^2 = 1876$ MeV.

One can conclude that the probability of an accidental coincidence can not be disregarded although it is rather small. If one excludes this possibility the more likely interpretation seems to be that of an annihilation process of a heavy particle... the many questions raised by the discussion of this event will obviously find their final answer only if other similar events will be observed."



IL NUOVO CIMENTO Vol. I, N. 3 1° Marzo 1955

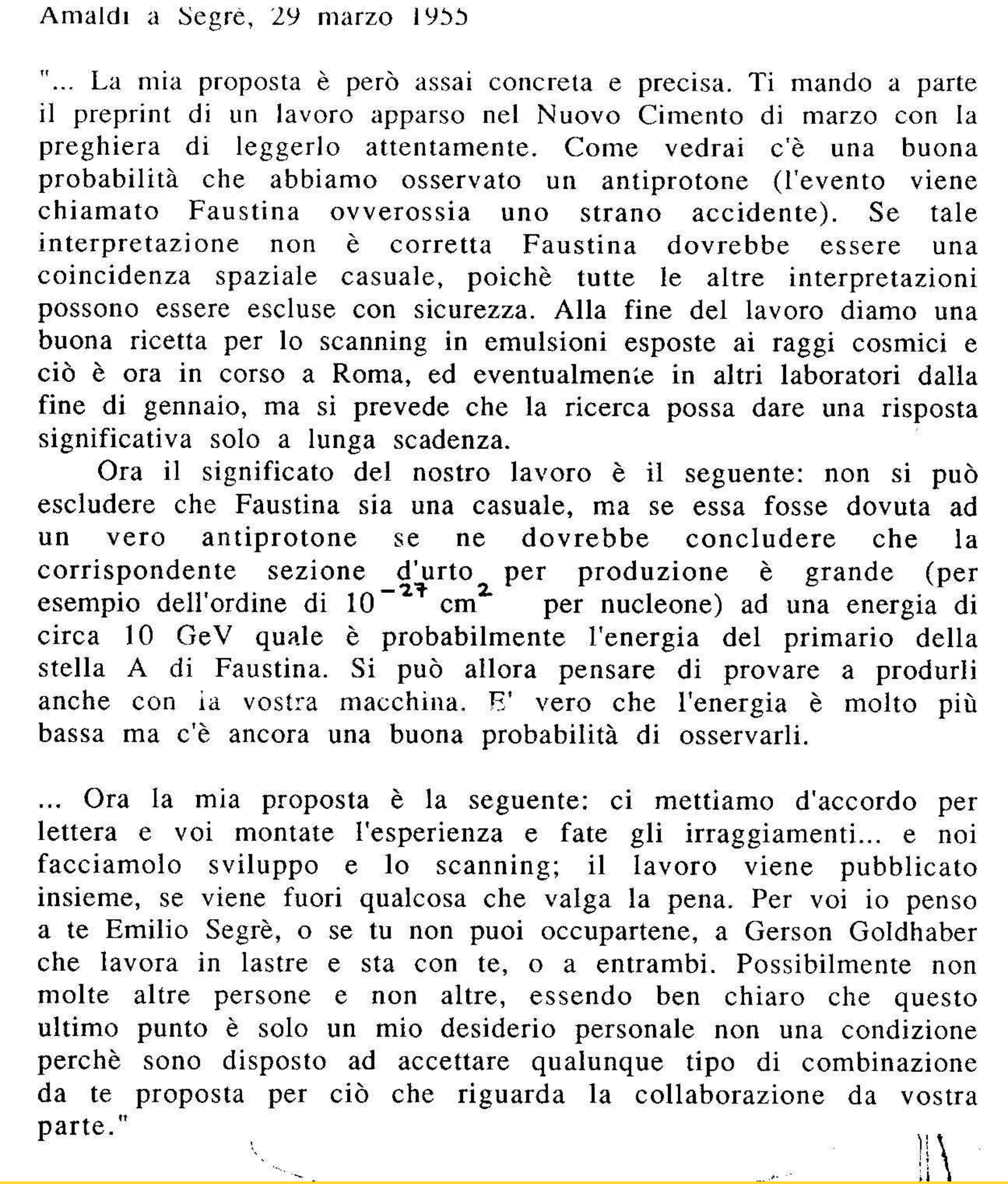
Unusual Event Produced by Cosmic Rays.

E. Amaldi, C. Castagnoli, G. Cortini, C. Franzinetti and A. Manfredini
Istituto di Fisica dell'Università - Roma
Istituto Nazionale di Fisica Nucleare - Sezione di Roma

(ricevuto il 18 Febbraio 1955)

Summary. – The authors describe an event consisting of two stars respectively of about 5 and 1-2 GeV energy. The probable value of the number of accidental space coincidences that one expects to observe in the scanned volume, is about $4\cdot 10^{-4}$. This value, although it does not allow us to exclude an accidental process, justifies the consideration of interpretations in terms of some physical process. Special attention is devoted to the production, capture and annihilation of a negative proton.

“Faustina”, l’evento “strano” rintracciato all’inizio del 1955 dal gruppo di Roma nelle lastre esposte alla radiazione cosmica durante la spedizione di Sardegna del 1953



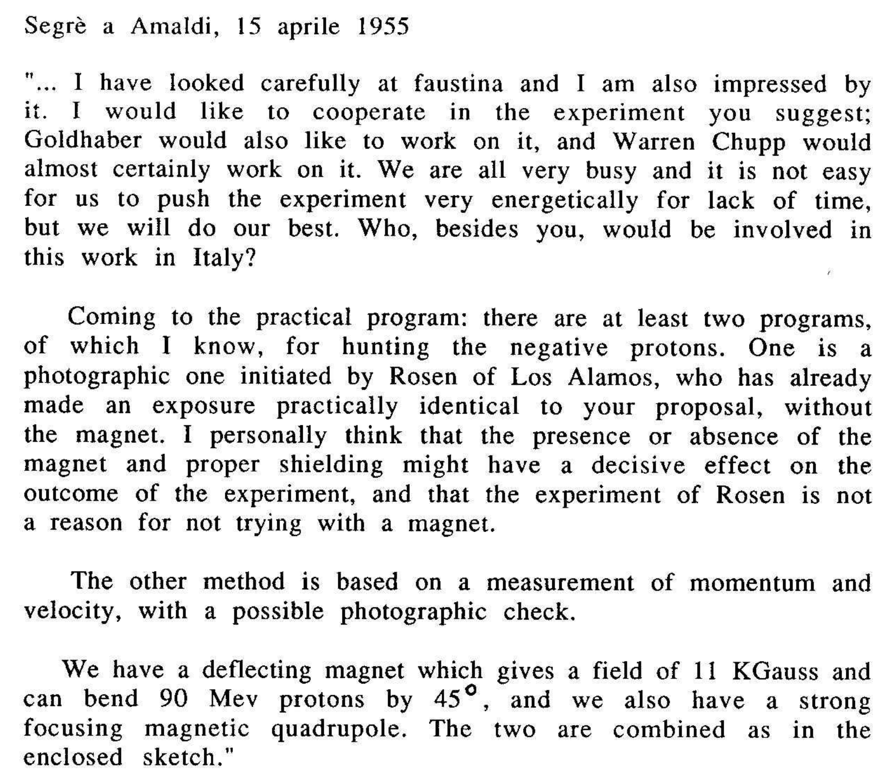
Amaldi a Segrè, 29 marzo 1955

"... La mia proposta è però assai concreta e precisa. Ti mando a parte il preprint di un lavoro apparso nel Nuovo Cimento di marzo con la preghiera di leggerlo attentamente. Come vedrai c'è una buona probabilità che abbiamo osservato un antiprotone (l'evento viene chiamato Faustina ovverossia uno strano accidente). Se tale interpretazione non è corretta Faustina dovrebbe essere una coincidenza spaziale casuale, poichè tutte le altre interpretazioni possono essere escluse con sicurezza. Alla fine del lavoro diamo una buona ricetta per lo scanning in emulsioni esposte ai raggi cosmici e ciò è ora in corso a Roma, ed eventualmente in altri laboratori dalla fine di gennaio, ma si prevede che la ricerca possa dare una risposta significativa solo a lunga scadenza.

Ora il significato del nostro lavoro è il seguente: non si può escludere che Faustina sia una casuale, ma se essa fosse dovuta ad un vero antiprotone se ne dovrebbe concludere che la corrispondente sezione d'urto per produzione è grande (per esempio dell'ordine di 10^{-27} cm^2 per nucleone) ad una energia di circa 10 GeV quale è probabilmente l'energia del primario della stella A di Faustina. Si può allora pensare di provare a produrli anche con la vostra macchina. E' vero che l'energia è molto più bassa ma c'è ancora una buona probabilità di osservarli.

... Ora la mia proposta è la seguente: ci mettiamo d'accordo per lettera e voi montate l'esperienza e fate gli irraggiamenti... e noi facciamolo sviluppo e lo scanning; il lavoro viene pubblicato insieme, se viene fuori qualcosa che valga la pena. Per voi io penso a te Emilio Segrè, o se tu non puoi occupartene, a Gerson Goldhaber che lavora in lastre e sta con te, o a entrambi. Possibilmente non molte altre persone e non altre, essendo ben chiaro che questo ultimo punto è solo un mio desiderio personale non una condizione perchè sono disposto ad accettare qualunque tipo di combinazione da te proposta per ciò che riguarda la collaborazione da vostra parte."

La proposta di Amaldi a Segrè, 29 marzo 1955



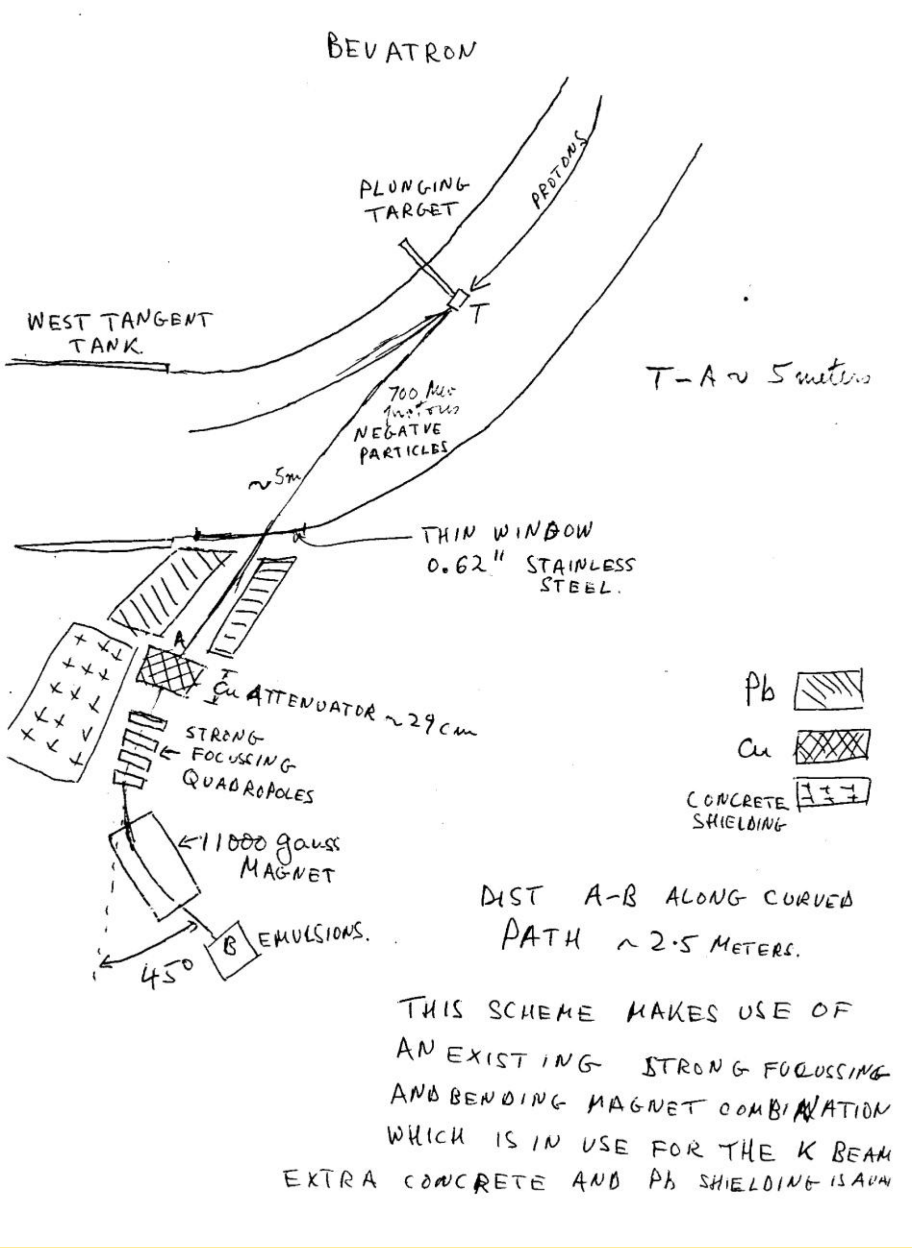
Segrè a Amaldi, 15 aprile 1955

"... I have looked carefully at 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. We are all very busy and it is not easy for us to push the experiment very energetically for lack of time, but we will do our best. Who, besides you, would be involved in this work in Italy?

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. I personally think that the presence or absence of the magnet and proper shielding might have a decisive effect on the outcome of the experiment, and that the experiment of Rosen is not a reason for not trying with a magnet.

The other method is based on a measurement of momentum and velocity, with a possible photographic check.

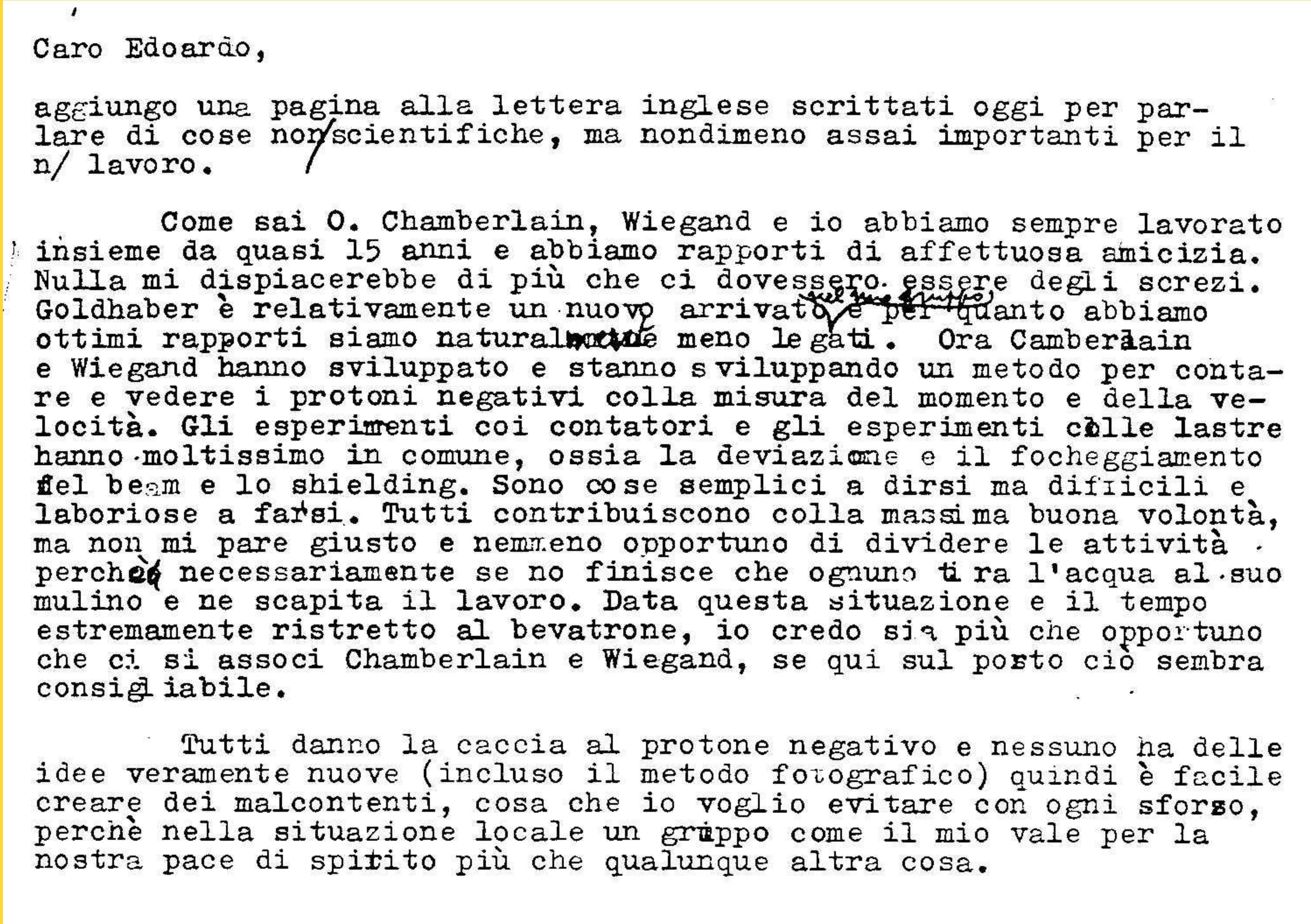
We have a deflecting magnet which gives a field of 11 KGauss and can bend 90 Mev protons by 45°, and we also have a strong focusing magnetic quadrupole. The two are combined as in the enclosed sketch."



DIST A-B ALONG CURVED PATH ~2.5 METERS.

THIS SCHEME MAKES USE OF AN EXISTING STRONG FOCUSSING AND BENDING MAGNET COMBINATION WHICH IS IN USE FOR THE K BEAM
EXTRA CONCRETE AND Pb SHIELDING IS AVAI

Segrè ad Amaldi, 15 aprile 1955



Caro Edoardo,

aggiungo una pagina alla lettera inglese scrittati oggi per parlare di cose non/scientifiche, ma nondimeno assai importanti per il n/ lavoro.

Come sai O. Chamberlain, Wiegand e io abbiamo sempre lavorato insieme da quasi 15 anni e abbiamo rapporti di affettuosa amicizia. Nulla mi dispiacerebbe di più che ci dovessero essere degli screzi. Goldhaber è relativamente un nuovo arrivato nel mio gruppo ~~e per~~ quanto abbiamo ottimi rapporti siamo naturalmente meno legati. Ora Camberlain e Wiegand hanno sviluppato e stanno sviluppando un metodo per contare e vedere i protoni negativi colla misura del momento e della velocità. Gli esperimenti coi contatori e gli esperimenti colle lastre hanno moltissimo in comune, ossia la deviazione e il focheggiamento del beam e lo shielding. Sono cose semplici a dirsi ma difficili e laboriose a farsi. Tutti contribuiscono colla massima buona volontà, ma non mi pare giusto e nemmeno opportuno di dividere le attività perchè necessariamente se no finisce che ognuno tira l'acqua al suo mulino e ne scapita il lavoro. Data questa situazione e il tempo estremamente ristretto al bevatrone, io credo sia più che opportuno che ci si associ Chamberlain e Wiegand, se qui sul posto ciò sembra consigliabile.

Tutti danno la caccia al protone negativo e nessuno ha delle idee veramente nuove (incluso il metodo fotografico) quindi è facile creare dei malcontenti, cosa che io voglio evitare con ogni sforzo, perchè nella situazione locale un gruppo come il mio vale per la nostra pace di spirito più che qualunque altra cosa.

E. Segrè a E. Amaldi, 28 giugno 1955

Berkeley 1955: da sin., E. Segrè, C. Wiegand, E. Lofgren, O. Chamberlain, T. Ypsilantis

La memoria dei protagonisti. Giulio Cortini (2005)

“L’antiprotone era nell’aria…A Berkeley un gruppo di importanti fisici sperimentali… avevano progettato ed eseguito un esperimento per dimostrarne definitivamente l’esistenza. L’esperimento riuscì e fu premiato con un premio Nobel. Tuttavia quei ricercatori vollero una conferma più sensazionale: provocare nelle loro lastre nucleari fenomeni analoghi al “nostro”… Un nuovo esperimento con questa tecnica avrebbe permesso di studiare - al di là della mera “esistenza” -- le interazioni dell’antiprotone con la materia…
Uno dei problemi che ci si posero era dimostrare che nell’evento si era sviluppata un’energia alta. Io inventai un metodo nuovo di misurare l’energia di una particella in volo la cui traccia passa attraverso più lastre: fu un mio contributo originale alla discussione, di cui vado ancora orgoglioso. Quella mia invenzione avrebbe meritato che pubblicassimo un lavoro tecnico a parte, ma non lo facemmo.
Amaldi era in contatto frequente con il gruppo di Berkeley e grazie al suo prestigio il nostro gruppo venne associato al loro “secondo” esperimento: loro ci mandarono delle lastre che avevano esposto al fascio di antiprotoni prodotti dalla loro macchina, da 6.3 GeV, che era entrata in funzione da poco e noi vi trovammo il “primo” evento del tipo “faustina”: telegramma, congratulazioni. Ma naturalmente il prestigio di questo nuovo risultato, e di quelli che seguirono, rimase in gran parte a loro”…
(G. Cortini in L. Bonolis 2008, pp.84-87)

“E’ stato emozionante scoprire, in un documento conservato presso l’Archivio Amaldi del Dipartimento di Fisica di Roma, che, per il Premio Feltrinelli del 1956, Gilberto Bernardini aveva proposto all’Accademia dei Lincei il conferimento del premio per la Fisica… agli scopritori dell’antiprotone elencandoli, in ordine alfabetico, come segue: Owen Chamberlain, Giulio Cortini, Emilio Segrè”
(F. Guerra, B. Preziosi, “Ricordo di Giulio Cortini”, Il Nuovo Saggiatore vol. 22, 2006, pp. 36-38)

La memoria dei protagonisti. Emilio Segrè (1993)

“For many years, experimental physicists had looked for antiprotons in cosmic rays, with inconclusive results. Among others, Bruno Rossi and his collaborators, using a cloud chamber, and Edoardo Amaldi and his collaborators, using photographic emulsions, had observed particles in cosmic rays that may have been antiprotons. Their observations were not, however, sufficient to establish the particle.

In planning the bevatron, Lawrence and the Rad Lab physicists had consciously chosen as a goal an energy of 6 GeV, slightly above the threshold for the formation of nucleon-antinucleon pairs from a proton colliding with a nucleon at rest. In 1955 the bevatron reached this design energy and thus afforded the opportunity of proving the existence of the antiproton unequivocally, and we wanted to settle the question once and for all.

Several Berkeley groups started the hunt. 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…

I had no doubt that antiproton was the right name for the new particle. Lawrence preferred negative protons, but he did not insist. The mass-spectrograph experiment concluded on October 1, 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.”

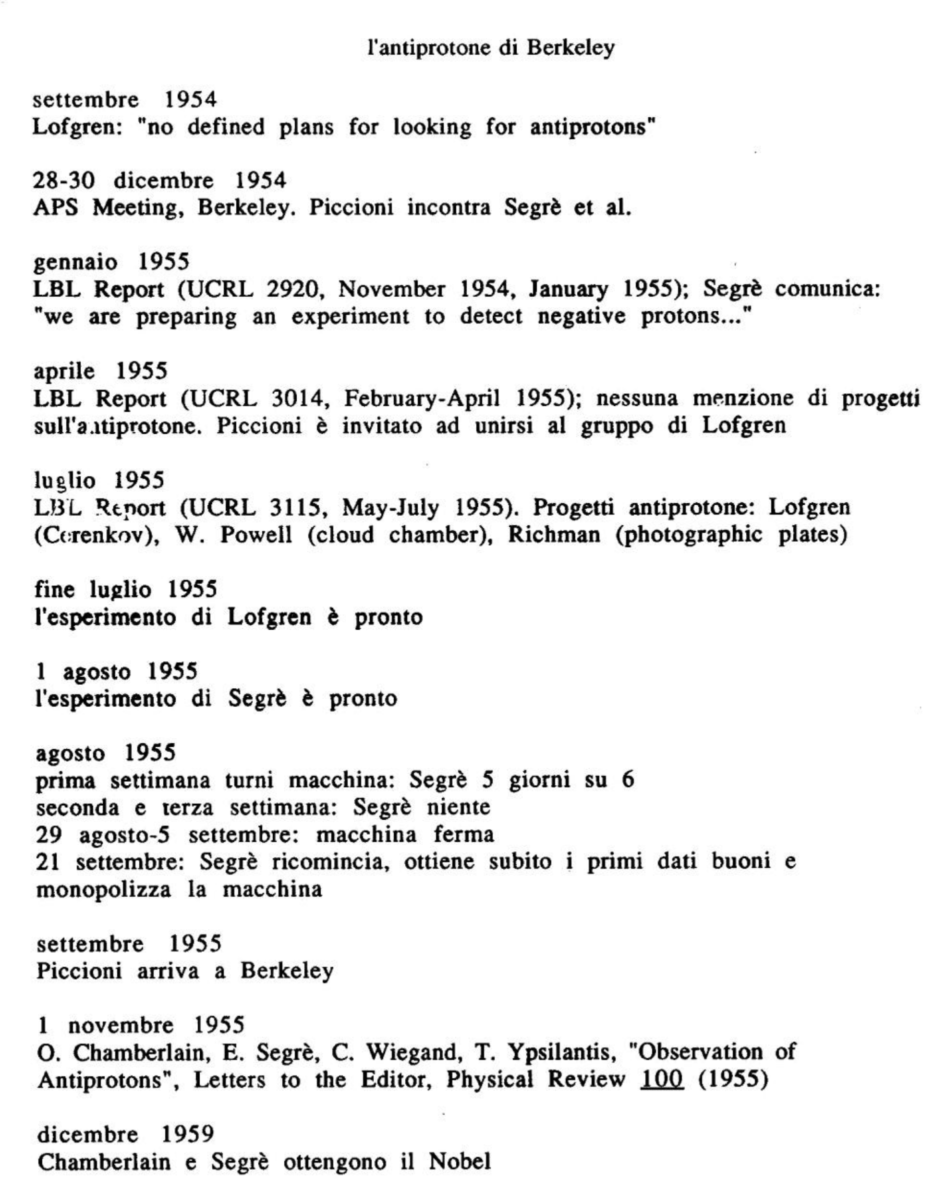
Sulla memoria dei protagonisti. John Heilbron (1989)

“Insofar as historians may be said to have a particular goal, it is to understand the connection of events from a wider perspective than any of the historical actors, however well placed they were, could have attained. This aspiration does not imply a feeling of superiority to the actors, nor any special wisdom. It does imply the obligation and the patience to study a large quantity and broad range of sources from and about the past.

The industrious historian who has looked at private correspondence, government papers, foundation and university reports, newspapers, patent applications, court records, architectural monuments, scientific apparatus, mption pictures, painted neckties, and literary T-shirts - as well as the scientific literature - necessarily sees connections that the people being studied could not have known.

Some of these connections may appear farfetched, and sometimes the lust for originality - from which the historian also suffers - creates grotesque associations. More often, the overly ambitious historian may offer a third-order correction before finding the first approximation. But even in these cases it is not licit to reject the proposed connections merely because they did not leave a trace in the memories of the historical actors or because they do not now appear to make good scientific sense.

From the point of view just sketched, 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.”



l'antiprotone di Berkeley

settembre 1954
Lofgren: "no defined plans for looking for antiprotons"

28-30 dicembre 1954
APS Meeting, Berkeley. Piccioni incontra Segrè et al.

gennaio 1955
LBL Report (UCRL 2920, November 1954, January 1955); Segrè comunica: "we are preparing an experiment to detect negative protons..."

aprile 1955
LBL Report (UCRL 3014, February-April 1955); nessuna menzione di progetti sull'antiprotone. Piccioni è invitato ad unirsi al gruppo di Lofgren

luglio 1955
LBL Report (UCRL 3115, May-July 1955). Progetti antiprotone: Lofgren (Cerenkov), W. Powell (cloud chamber), Richman (photographic plates)

fine luglio 1955
l'esperimento di Lofgren è pronto

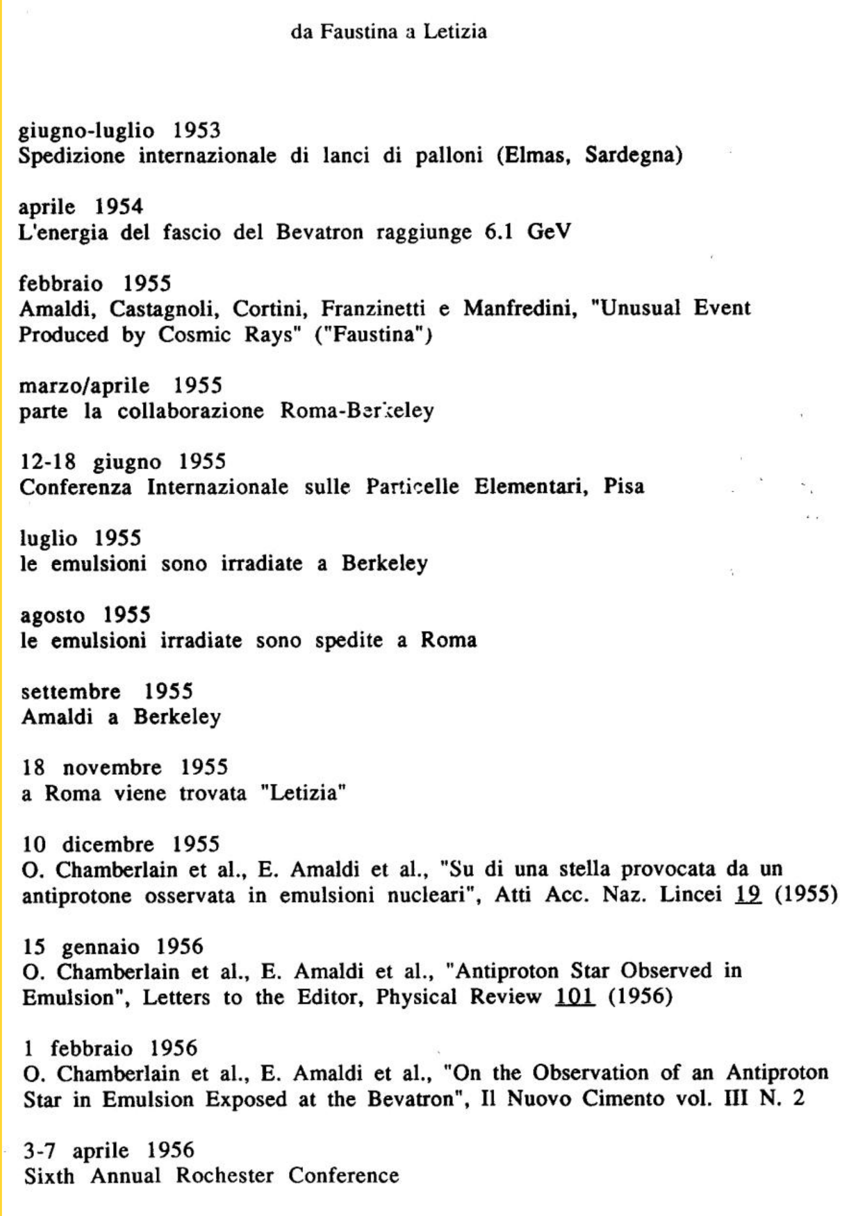
1 agosto 1955
l'esperimento di Segrè è pronto

agosto 1955
prima settimana turni macchina: Segrè 5 giorni su 6
seconda e terza settimana: Segrè niente
29 agosto-5 settembre: macchina ferma
21 settembre: Segrè ricomincia, ottiene subito i primi dati buoni e monopolizza la macchina

settembre 1955
Piccioni arriva a Berkeley

1 novembre 1955
O. Chamberlain, E. Segrè, C. Wiegand, T. Ypsilantis, "Observation of Antiprotons", Letters to the Editor, Physical Review <u>100</u> (1955)

dicembre 1959
Chamberlain e Segrè ottengono il Nobel



da Faustina a Letizia

giugno-luglio 1953
Spedizione internazionale di lanci di palloni (Elmas, Sardegna)

aprile 1954
L'energia del fascio del Bevatron raggiunge 6.1 GeV

febbraio 1955
Amaldi, Castagnoli, Cortini, Franzinetti e Manfredini, "Unusual Event Produced by Cosmic Rays" ("Faustina")

marzo/aprile 1955
parte la collaborazione Roma-Berkeley

12-18 giugno 1955
Conferenza Internazionale sulle Particelle Elementari, Pisa

luglio 1955
le emulsioni sono irradiate a Berkeley

agosto 1955
le emulsioni irradiate sono spedite a Roma

settembre 1955
Amaldi a Berkeley

18 novembre 1955
a Roma viene trovata "Letizia"

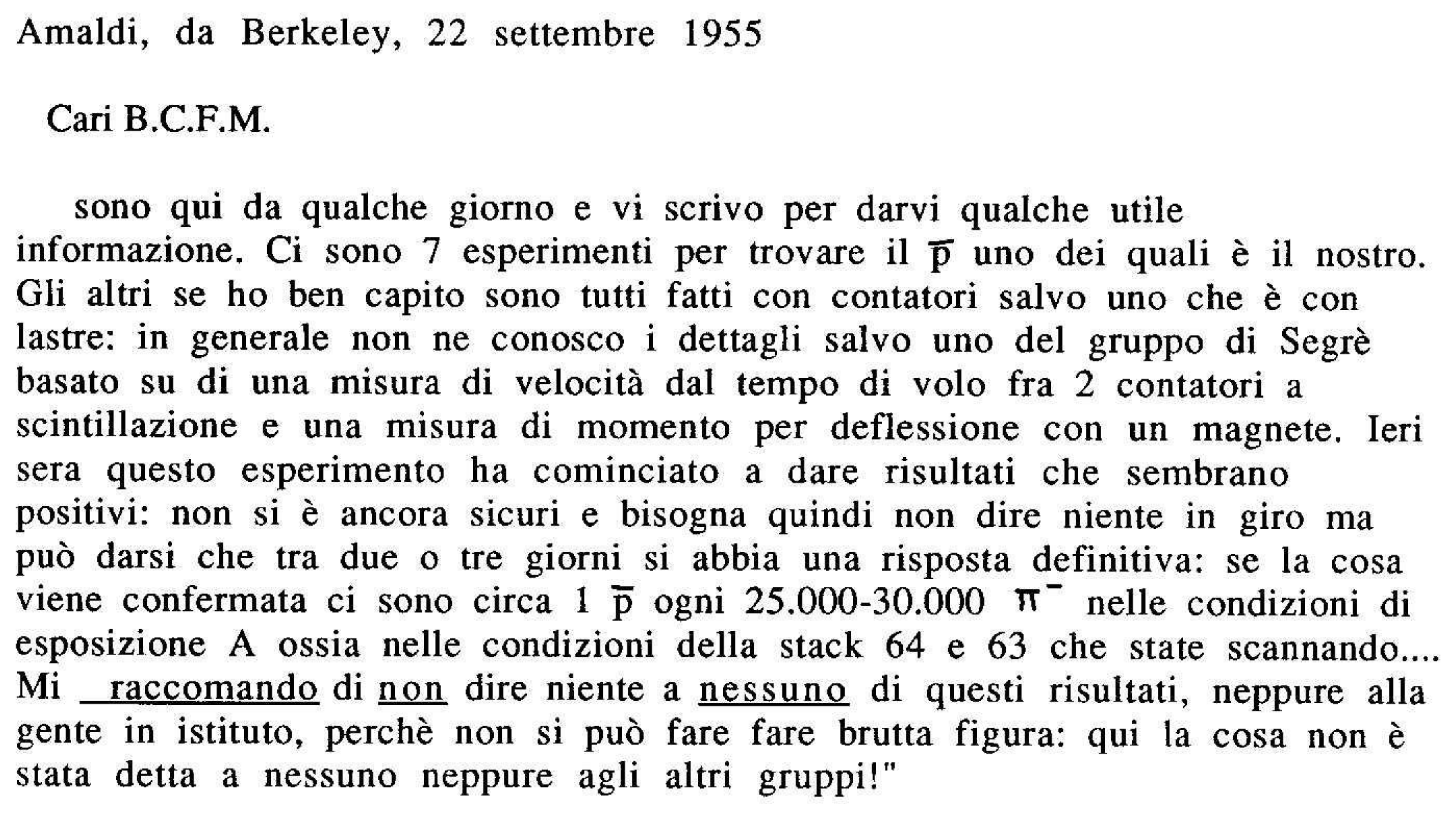
10 dicembre 1955
O. Chamberlain et al., E. Amaldi et al., "Su di una stella provocata da un antiprotone osservata in emulsioni nucleari", Atti Acc. Naz. Lincei <u>19</u> (1955)

15 gennaio 1956
O. Chamberlain et al., E. Amaldi et al., "Antiproton Star Observed in Emulsion", Letters to the Editor, Physical Review <u>101</u> (1956)

1 febbraio 1956
O. Chamberlain et al., E. Amaldi et al., "On the Observation of an Antiproton Star in Emulsion Exposed at the Bevatron", Il Nuovo Cimento vol. III N. 2

3-7 aprile 1956
Sixth Annual Rochester Conference

Cronologia essenziale dell'antiprotone tra Berkeley e Roma

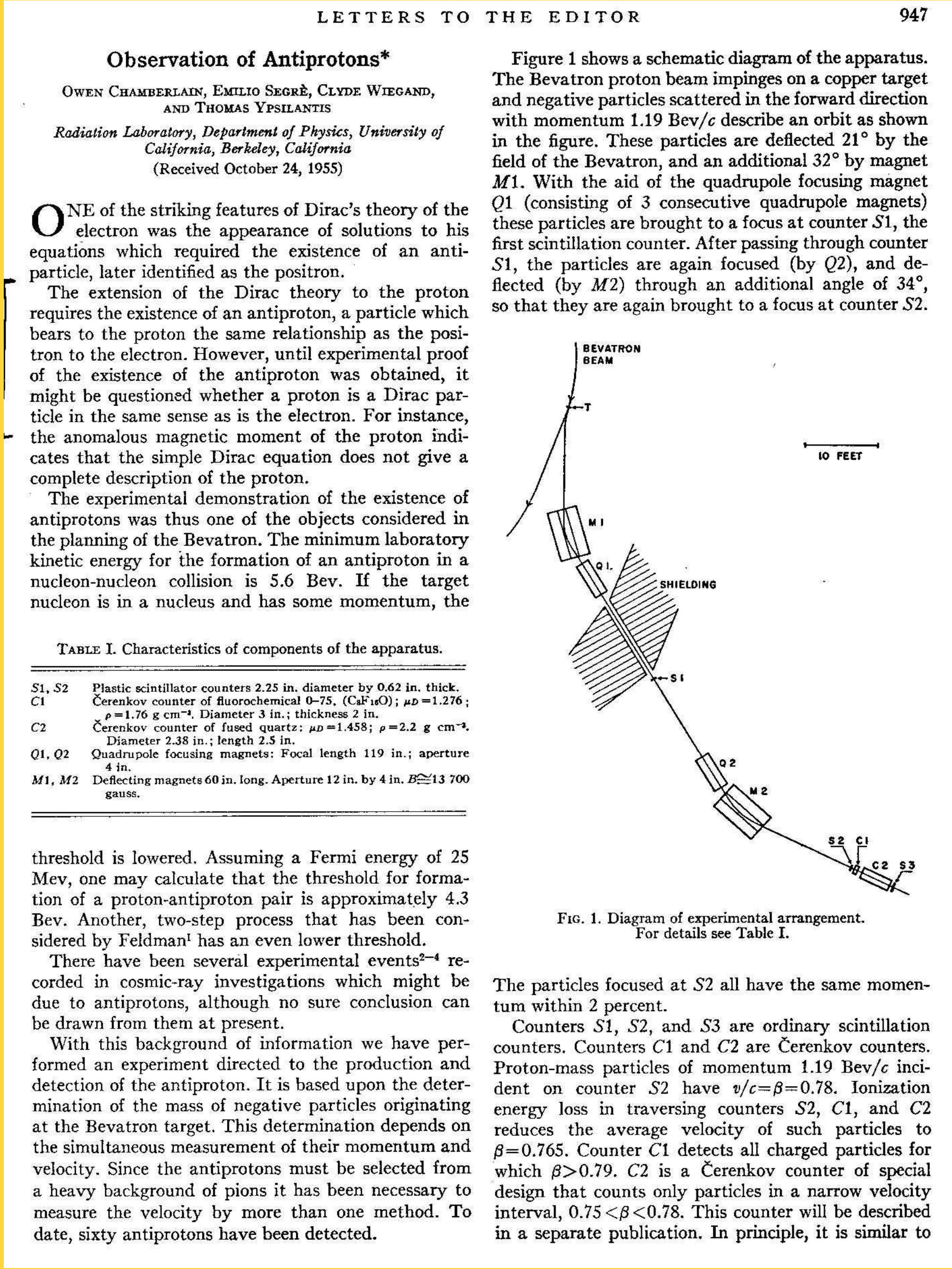


Amaldi, da Berkeley, 22 settembre 1955

Cari B.C.F.M.

sono qui da qualche giorno e vi scrivo per darvi qualche utile informazione. Ci sono 7 esperimenti per trovare il $\bar{p}$ uno dei quali è il nostro. Gli altri se ho ben capito sono tutti fatti con contatori salvo uno che è con lastre: in generale non ne conosco i dettagli salvo uno del gruppo di Segrè basato su di una misura di velocità dal tempo di volo fra 2 contatori a scintillazione e una misura di momento per deflessione con un magnete. Ieri sera questo esperimento ha cominciato a dare risultati che sembrano positivi: non si è ancora sicuri e bisogna quindi non dire niente in giro ma può darsi che tra due o tre giorni si abbia una risposta definitiva: se la cosa viene confermata ci sono circa 1 $\bar{p}$ ogni 25.000-30.000 π^- nelle condizioni di esposizione A ossia nelle condizioni della stack 64 e 63 che state scannando.... Mi raccomando di non dire niente a nessuno di questi risultati, neppure alla gente in istituto, perchè non si può fare fare brutta figura: qui la cosa non è stata detta a nessuno neppure agli altri gruppi!"

Fine settembre 1955. Amaldi da Berkeley informa i suoi collaboratori a Roma che l'esperimento coi contatori comincia a dare i primi risultati



Observation of Antiprotons*

Owen Chamberlain, Emilio Segrè, Clyde Wiegand,
and Thomas Ypsilantis

Radiation Laboratory, Department of Physics, University of California, Berkeley, California

(Received October 24, 1955)

ONE of the striking features of Dirac's theory of the electron was the appearance of solutions to his equations which required the existence of an antiparticle, later identified as the positron.

The extension of the Dirac theory to the proton requires the existence of an antiproton, a particle which bears to the proton the same relationship as the positron to the electron. However, until experimental proof of the existence of the antiproton was obtained, it might be questioned whether a proton is a Dirac particle in the same sense as is the electron. For instance, the anomalous magnetic moment of the proton indicates that the simple Dirac equation does not give a complete description of the proton.

The experimental demonstration of the existence of antiprotons was thus one of the objects considered in the planning of the Bevatron. The minimum laboratory kinetic energy for the formation of an antiproton in a nucleon-nucleon collision is 5.6 Bev. If the target nucleon is in a nucleus and has some momentum, the threshold is lowered. Assuming a Fermi energy of 25 Mev, one may calculate that the threshold for formation of a proton-antiproton pair is approximately 4.3 Bev. Another, two-step process that has been considered by Feldman[1] has an even lower threshold.

There have been several experimental events[2–4] recorded in cosmic-ray investigations which might be due to antiprotons, although no sure conclusion can be drawn from them at present.

With this background of information we have performed an experiment directed to the production and detection of the antiproton. It is based upon the determination of the mass of negative particles originating at the Bevatron target. This determination depends on the simultaneous measurement of their momentum and velocity. Since the antiprotons must be selected from a heavy background of pions it has been necessary to measure the velocity by more than one method. To date, sixty antiprotons have been detected.

Table I. Characteristics of components of the apparatus.

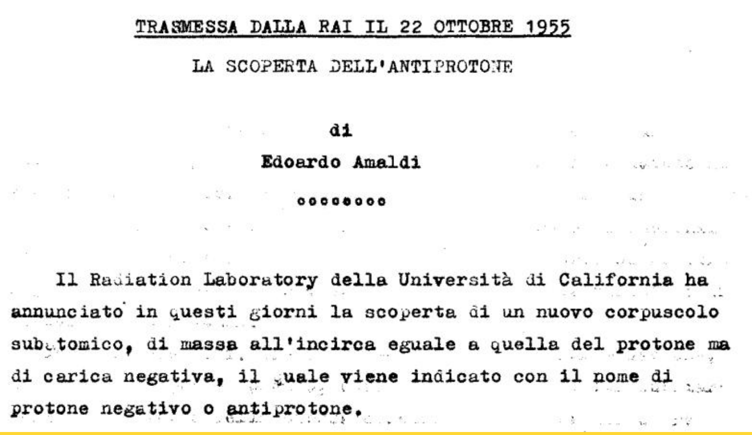
$S1$, $S2$	Plastic scintillator counters 2.25 in. diameter by 0.62 in. thick.
$C1$	Čerenkov counter of fluorochemical 0–75, ($C_8F_{16}O$); $\mu_D=1.276$; $\rho=1.76$ g cm^{-3}. Diameter 3 in.; thickness 2 in.
$C2$	Čerenkov counter of fused quartz: $\mu_D=1.458$; $\rho=2.2$ g cm^{-3}. Diameter 2.38 in.; length 2.5 in.
$Q1$, $Q2$	Quadrupole focusing magnets: Focal length 119 in.; aperture 4 in.
$M1$, $M2$	Deflecting magnets 60 in. long. Aperture 12 in. by 4 in. $B\cong 13\,700$ gauss.

Figure 1 shows a schematic diagram of the apparatus. The Bevatron proton beam impinges on a copper target and negative particles scattered in the forward direction with momentum 1.19 Bev/c describe an orbit as shown in the figure. These particles are deflected 21° by the field of the Bevatron, and an additional 32° by magnet $M1$. With the aid of the quadrupole focusing magnet $Q1$ (consisting of 3 consecutive quadrupole magnets) these particles are brought to a focus at counter $S1$, the first scintillation counter. After passing through counter $S1$, the particles are again focused (by $Q2$), and deflected (by $M2$) through an additional angle of 34°, so that they are again brought to a focus at counter $S2$. The particles focused at $S2$ all have the same momentum within 2 percent.

Fig. 1. Diagram of experimental arrangement. For details see Table I.

Counters $S1$, $S2$, and $S3$ are ordinary scintillation counters. Counters $C1$ and $C2$ are Čerenkov counters. Proton-mass particles of momentum 1.19 Bev/c incident on counter $S2$ have $v/c=\beta=0.78$. Ionization energy loss in traversing counters $S2$, $C1$, and $C2$ reduces the average velocity of such particles to $\beta=0.765$. Counter $C1$ detects all charged particles for which $\beta>0.79$. $C2$ is a Čerenkov counter of special design that counts only particles in a narrow velocity interval, $0.75<\beta<0.78$. This counter will be described in a separate publication. In principle, it is similar to

La lettera alla Physical Review che annuncia la scoperta dell'antiprotone (ottobre 1955)



TRASMESSA DALLA RAI IL 22 OTTOBRE 1955

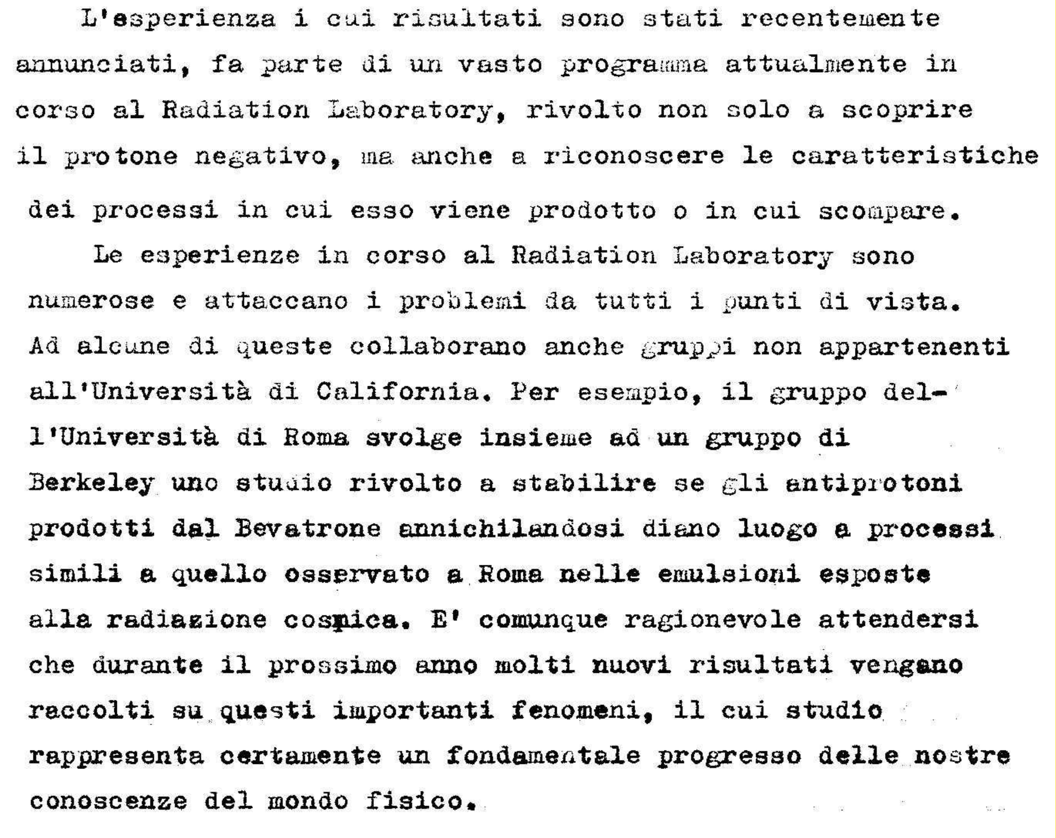
LA SCOPERTA DELL'ANTIPROTONE

di

Edoardo Amaldi

oooooooo

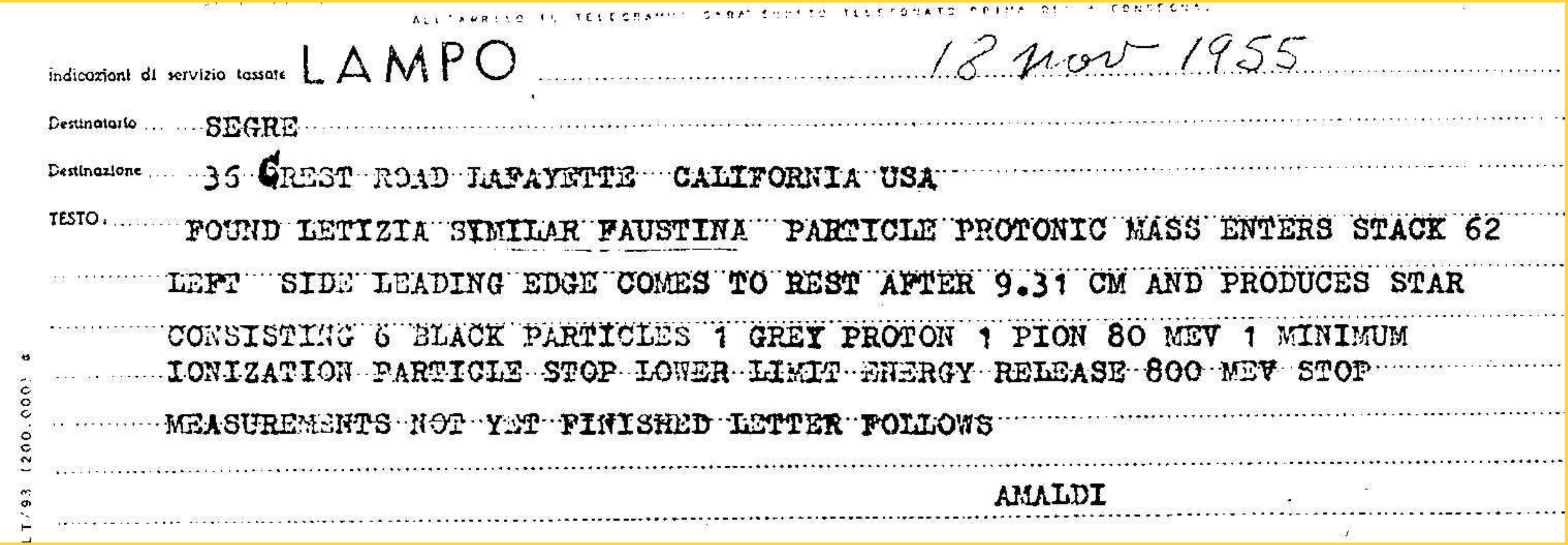
Il Radiation Laboratory della Università di California ha annunciato in questi giorni la scoperta di un nuovo corpuscolo subatomico, di massa all'incirca eguale a quella del protone ma di carica negativa, il quale viene indicato con il nome di protone negativo o antiprotone.



L'esperienza i cui risultati sono stati recentemente annunciati, fa parte di un vasto programma attualmente in corso al Radiation Laboratory, rivolto non solo a scoprire il protone negativo, ma anche a riconoscere le caratteristiche dei processi in cui esso viene prodotto o in cui scompare.

Le esperienze in corso al Radiation Laboratory sono numerose e attaccano i problemi da tutti i punti di vista. Ad alcune di queste collaborano anche gruppi non appartenenti all'Università di California. Per esempio, il gruppo dell'Università di Roma svolge insieme ad un gruppo di Berkeley uno studio rivolto a stabilire se gli antiprotoni prodotti dal Bevatrone annichilandosi diano luogo a processi simili a quello osservato a Roma nelle emulsioni esposte alla radiazione cosmica. E' comunque ragionevole attendersi che durante il prossimo anno molti nuovi risultati vengano raccolti su questi importanti fenomeni, il cui studio rappresenta certamente un fondamentale progresso delle nostre conoscenze del mondo fisico.

Comunicato di E. Amaldi sulla scoperta dell'antiprotone trasmesso dalla RAI il 22 ottobre 1955



ALL'ARRIVO IL TELEGRAMMA SARÀ SUBITO TELEFONATO PRIMA DI [illegible] CONSEGNA

indicazioni di servizio tassate LAMPO 18 nov 1955

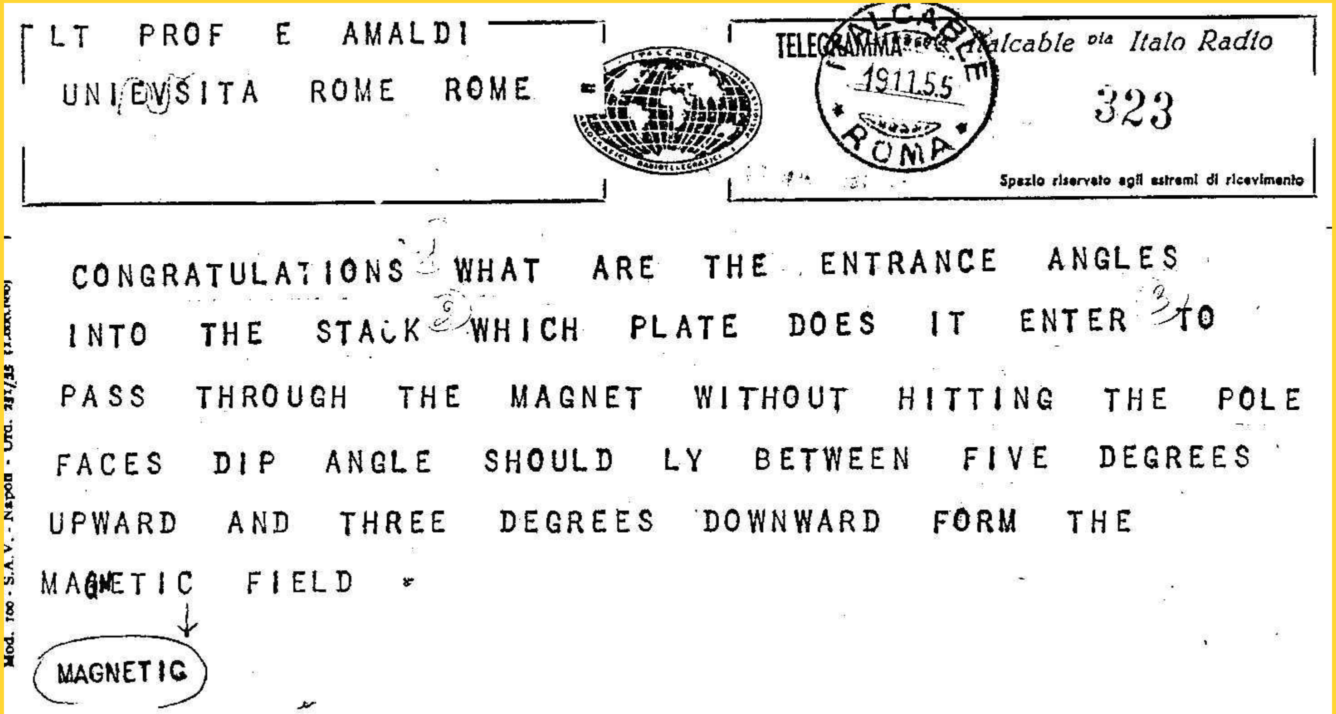
Destinatario SEGRE

Destinazione 36 CREST ROAD LAFAYETTE CALIFORNIA USA

TESTO 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

LT/93 (200.000) a



LT PROF E AMALDI
UNIEVSITA ROME ROME =

TELEGRAMMA via Italcable via Italo Radio

ITALCABLE 19.11.55 ROMA

323

Spazio riservato agli estremi di ricevimento

CONGRATULATIONS WHAT ARE THE ENTRANCE ANGLES INTO THE STACK WHICH PLATE DOES IT ENTER TO PASS THROUGH THE MAGNET WITHOUT HITTING THE POLE FACES DIP ANGLE SHOULD LY BETWEEN FIVE DEGREES UPWARD AND THREE DEGREES DOWNWARD FORM THE MAGMETIC FIELD =

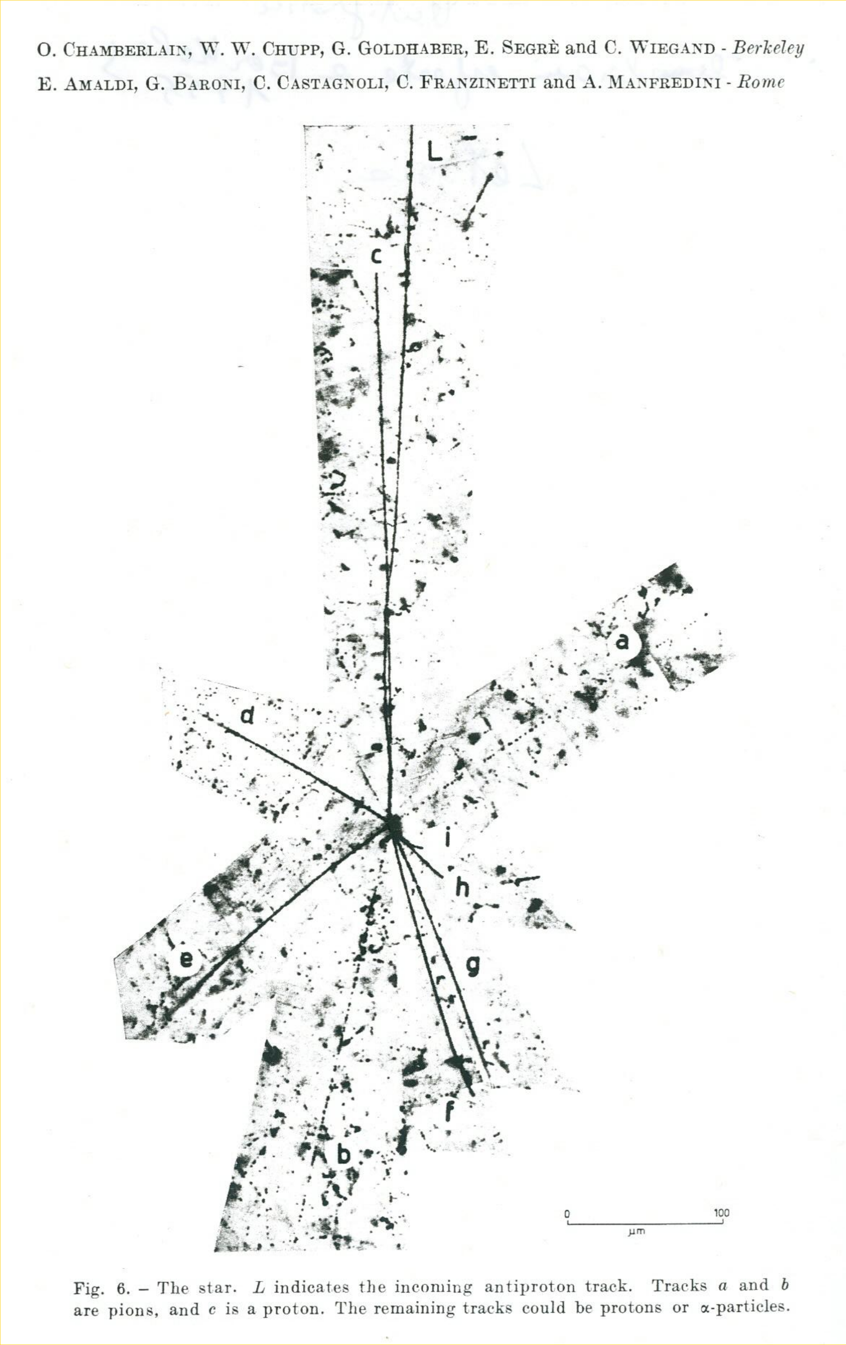
MAGNETIC

Mod. 100 - S.A.V. - Napoli - Ord. 231/55 (1.000.000)

“Letizia”:

il primo evento di annichilazione trovato a Roma nelle lastre esposte a Berkeley

(18 novembre 1955)



O. Chamberlain, W. W. Chupp, G. Goldhaber, E. Segrè and C. Wiegand - *Berkeley*

E. Amaldi, G. Baroni, C. Castagnoli, C. Franzinetti and A. Manfredini - *Rome*

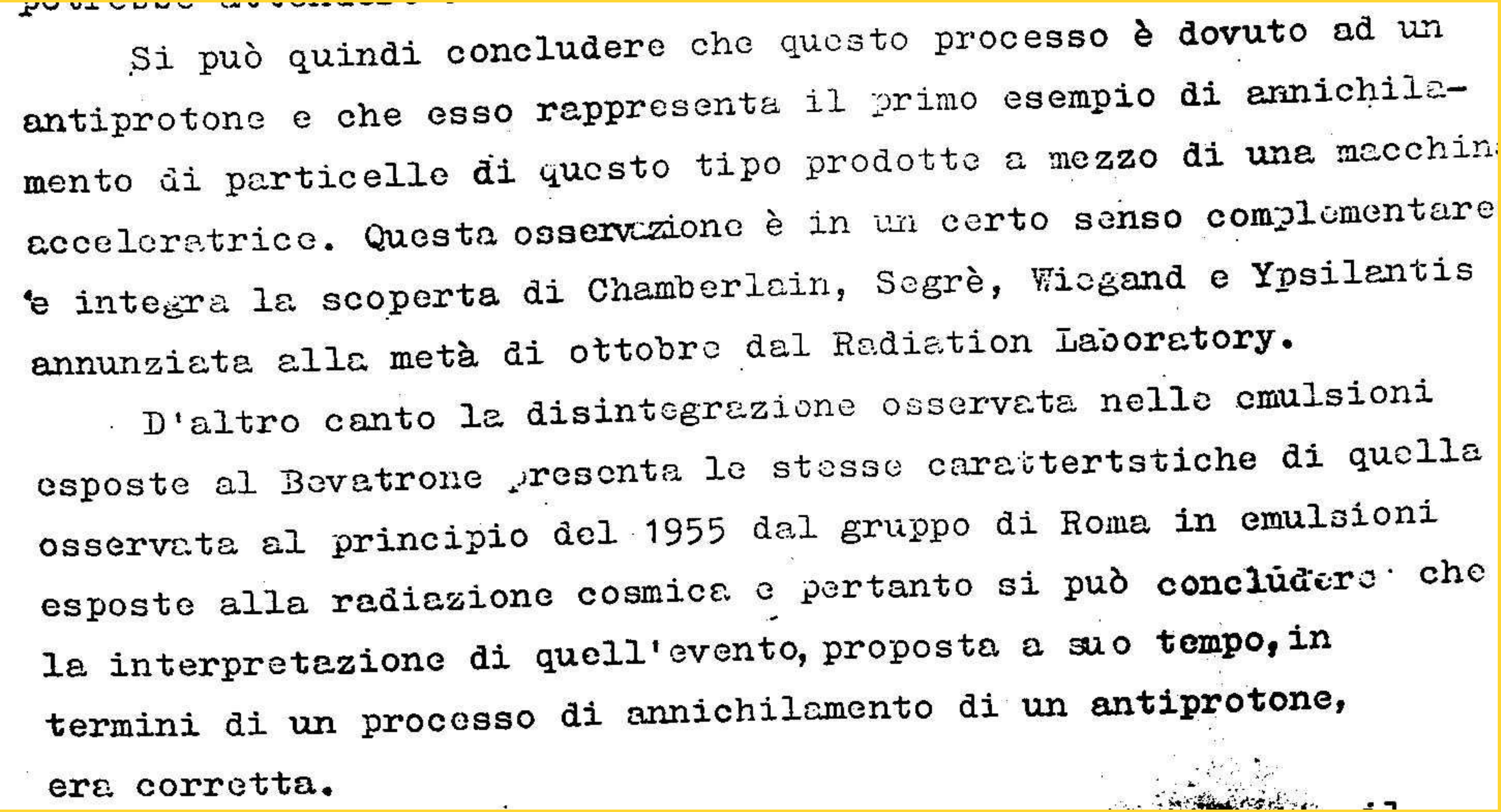
Fig. 6. – The star. *L* indicates the incoming antiproton track. Tracks *a* and *b* are pions, and *c* is a proton. The remaining tracks could be protons or α-particles.

"A quest'ora avrai saputo del ritrovamento del primo evento di annichilazione di un antiprotone nelle lastre esposte a Berkeley ed esplorate qui a Roma. La doppietta di Amaldi, a cavallo del bell'esperimento americano, è stato un buon colpo, di cui siamo tutti molto contenti: ha risollevato un po' gli spiriti depressi per la solita mancanza di quattrini e per le difficoltà che si stanno incontrando per ottenere una ragionevole legge sulla Energia Nucleare in Italia."

(L. Mezzetti a O. Piccioni, 22 novembre 1955)

Roma, sabato 10 Dicembre 1955.

In occasione della seduta mensile della Accademia Nazionale dei Lincei il Dr. E. Amaldi dell’Università di Roma ha comunicato alcuni risultati preliminari di una ricerca sull’annichilamento dei protoni negativi fatta in collaborazione da un gruppo della Università di California, Berkeley, ed un gruppo della Università di Roma



potrebbe attendere.

Si può quindi concludere che questo processo è dovuto ad un antiprotone e che esso rappresenta il primo esempio di annichilamento di particelle di questo tipo prodotte a mezzo di una macchina acceleratrice. Questa osservazione è in un certo senso complementare e integra la scoperta di Chamberlain, Segrè, Wiegand e Ypsilantis annunziata alla metà di ottobre dal Radiation Laboratory.

D'altro canto la disintegrazione osservata nelle emulsioni esposte al Bevatrone presenta le stesse carattertstiche di quella osservata al principio del 1955 dal gruppo di Roma in emulsioni esposte alla radiazione cosmica e pertanto si può concludere che la interpretazione di quell'evento, proposta a suo tempo, in termini di un processo di annichilamento di un antiprotone, era corretta.

E. Segrè a E. Amaldi, 22 novembre 1955:

“Intanto ho avuto il benestare di McMillan per fare il paper lungo sul Nuovo Cimento e una lettera alla Phys. Rev. contemporaneamente. Se vuoi preparare la lettera telegrafami e noi aggiusteremo l’inglese…”

E. Segrè a E. Amaldi, 28 novembre 1955:

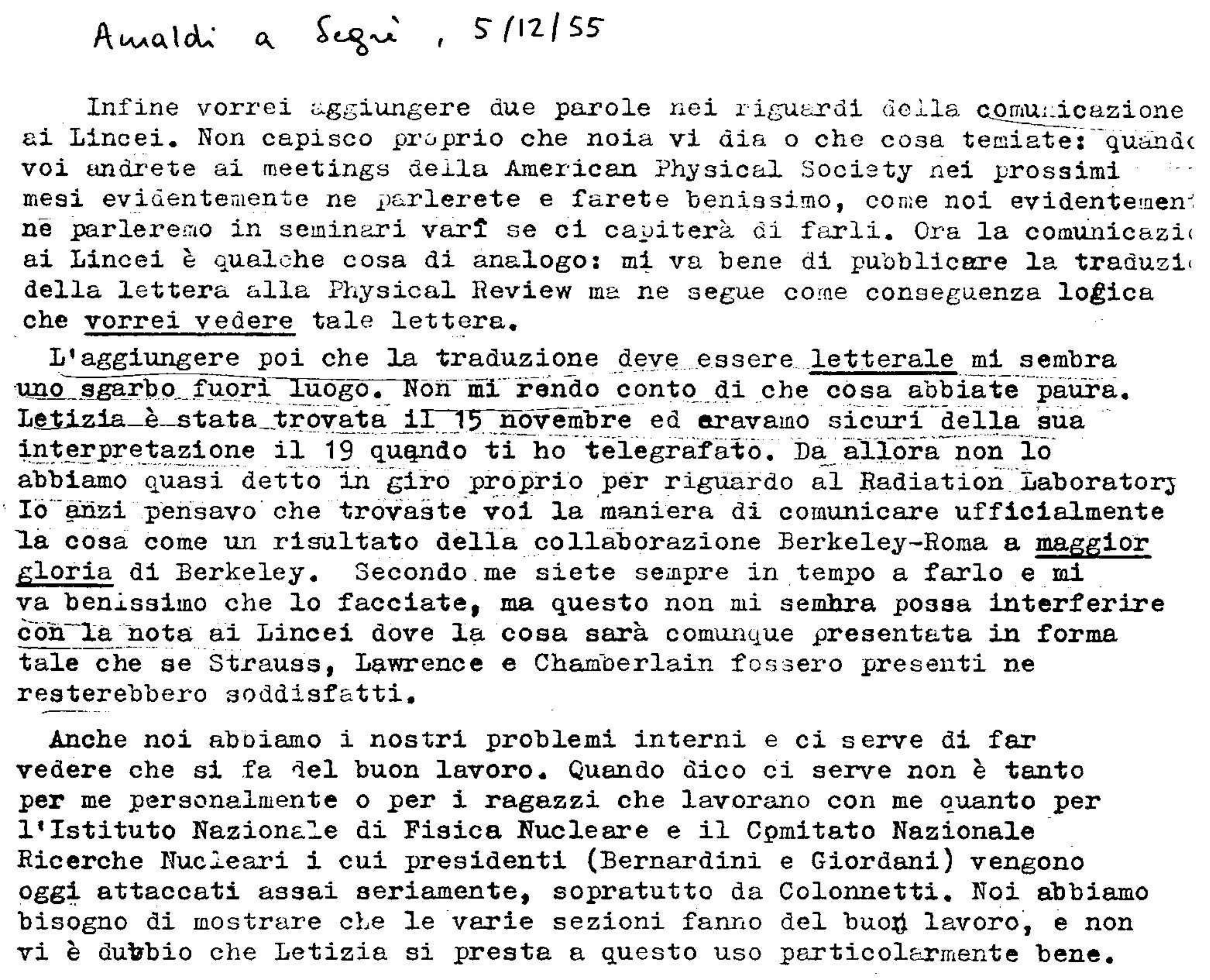
“Complete article Cimento must be here before releasing for declassification”

E. Amaldi a E. Segrè, 29 novembre 1955:

“It is a bit disappointing to have to wait the declassification because that means a delay of one month in the publication in Nuovo Cimento”

E. Segrè a E. Amaldi, 29 novembre 1955:

“As far as the Lincei is concerned it seems superfluous to us and we do not want it. If you feel very strongly about it you may submit a literal translation of the letter to the Phys. Rev.”



Amaldi a Segrè, 5/12/55

Infine vorrei aggiungere due parole nei riguardi della comunicazione ai Lincei. Non capisco proprio che noia vi dia o che cosa temiate: quand voi andrete ai meetings della American Physical Society nei prossimi mesi evidentemente ne parlerete e farete benissimo, come noi evidentemen ne parleremo in seminari varî se ci capiterà di farli. Ora la comunicazi ai Lincei è qualche cosa di analogo: mi va bene di pubblicare la traduzi della lettera alla Physical Review ma ne segue come conseguenza logica che vorrei vedere tale lettera.

L'aggiungere poi che la traduzione deve essere letterale mi sembra uno sgarbo fuori luogo. Non mi rendo conto di che cosa abbiate paura. Letizia è stata trovata il 15 novembre ed eravamo sicuri della sua interpretazione il 19 quando ti ho telegrafato. Da allora non lo abbiamo quasi detto in giro proprio per riguardo al Radiation Laborator Io anzi pensavo che trovaste voi la maniera di comunicare ufficialmente la cosa come un risultato della collaborazione Berkeley-Roma a maggior gloria di Berkeley. Secondo me siete sempre in tempo a farlo e mi va benissimo che lo facciate, ma questo non mi sembra possa interferire con la nota ai Lincei dove la cosa sarà comunque presentata in forma tale che se Strauss, Lawrence e Chamberlain fossero presenti ne resterebbero soddisfatti.

Anche noi abbiamo i nostri problemi interni e ci serve di far vedere che si fa del buon lavoro. Quando dico ci serve non è tanto per me personalmente o per i ragazzi che lavorano con me quanto per l'Istituto Nazionale di Fisica Nucleare e il Comitato Nazionale Ricerche Nucleari i cui presidenti (Bernardini e Giordani) vengono oggi attaccati assai seriamente, sopratutto da Colonnetti. Noi abbiamo bisogno di mostrare che le varie sezioni fanno del buon lavoro, e non vi è dubbio che Letizia si presta a questo uso particolarmente bene.

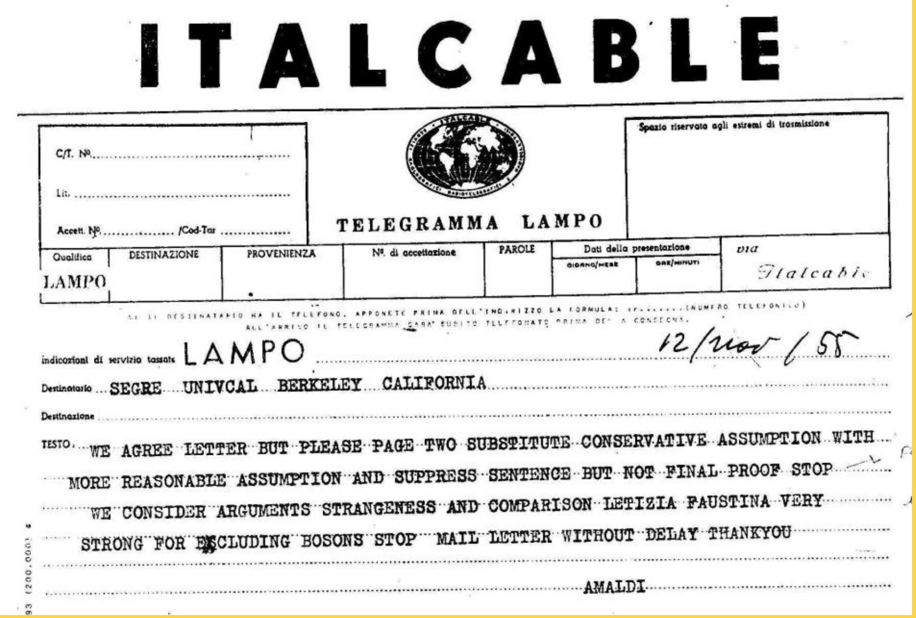
Traduzioni (non del tutto) letterali

Atti Accademia Lincei

“Questo evento conferma, anche se non in maniera definitiva, l'interpretazione... che le nuove particelle osservate al Bevatrone siano antiprotoni. Esso conferma anche l'ipotesi che la stella descritta in (5) (cioè Faustina, n.d.a.) fosse effettivamente dovuta ad un antiprotone.”

Physical Review Letters

"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."



ITALCABLE

C/T. N°.........

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Accett. N°.......... /Cod-Tar ....

TELEGRAMMA LAMPO

Spazio riservato agli estremi di trasmissione

Qualifica	DESTINAZIONE	PROVENIENZA	N°. di accettazione	PAROLE	Dati della presentazione GIORNO/MESE	 ORE/MINUTI	via
LAMPO							Italcable

SE IL DESTINATARIO HA IL TELEFONO, APPONETE PRIMA DELL'INDIRIZZO LA FORMULA: TF.......(NUMERO TELEFONICO) ALL'ARRIVO IL TELEGRAMMA SARA' SUBITO TELEFONATO PRIMA DELLA CONSEGNA.

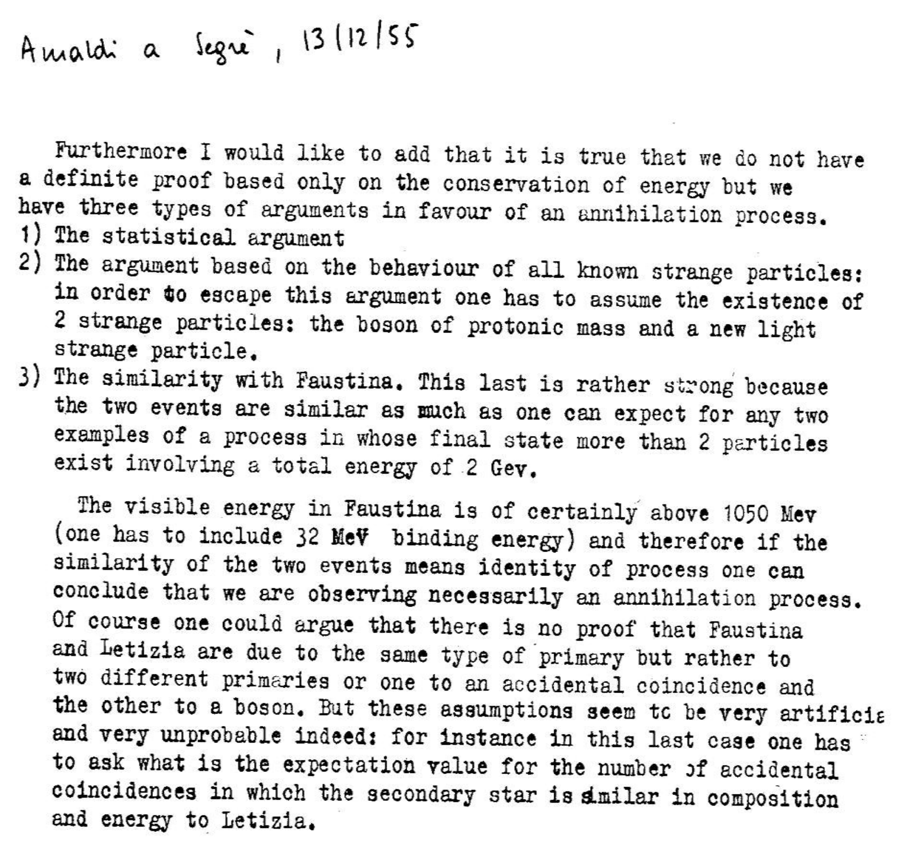
indicazioni di servizio tassate LAMPO 12/nov/55

Destinatario SEGRE UNIVCAL BERKELEY CALIFORNIA

Destinazione

TESTO WE AGREE LETTER BUT PLEASE PAGE TWO SUBSTITUTE CONSERVATIVE ASSUMPTION WITH MORE REASONABLE ASSUMPTION AND SUPPRESS SENTENCE BUT NOT FINAL PROOF STOP WE CONSIDER ARGUMENTS STRANGENESS AND COMPARISON LETIZIA FAUSTINA VERY STRONG FOR EXCLUDING BOSONS STOP MAIL LETTER WITHOUT DELAY THANKYOU

AMALDI



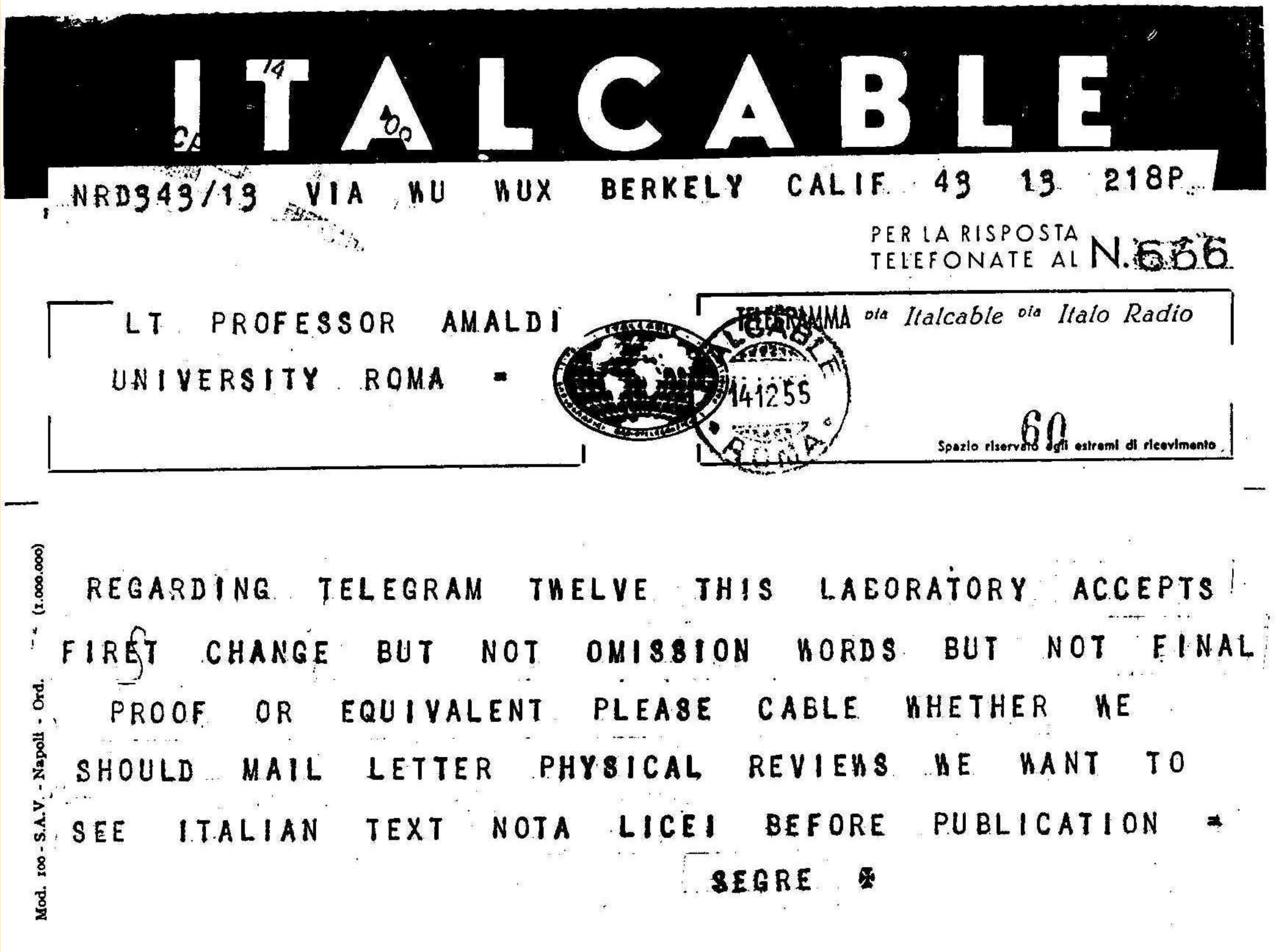
Amaldi a Segrè, 13/12/55

Furthermore I would like to add that it is true that we do not have a definite proof based only on the conservation of energy but we have three types of arguments in favour of an annihilation process.

1) The statistical argument
2) The argument based on the behaviour of all known strange particles: in order to escape this argument one has to assume the existence of 2 strange particles: the boson of protonic mass and a new light strange particle.
3) The similarity with Faustina. This last is rather strong because the two events are similar as much as one can expect for any two examples of a process in whose final state more than 2 particles exist involving a total energy of 2 Gev.

The visible energy in Faustina is of certainly above 1050 Mev (one has to include 32 MeV binding energy) and therefore if the similarity of the two events means identity of process one can conclude that we are observing necessarily an annihilation process. Of course one could argue that there is no proof that Faustina and Letizia are due to the same type of primary but rather to two different primaries or one to an accidental coincidence and the other to a boson. But these assumptions seem to be very artificia and very unprobable indeed: for instance in this last case one has to ask what is the expectation value for the number of accidental coincidences in which the secondary star is similar in composition and energy to Letizia.

E. Amaldi a E. Segrè, 12 e 13 dicembre 1955



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PER LA RISPOSTA
TELEFONATE AL N.666

LT PROFESSOR AMALDI
UNIVERSITY ROMA =

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REGARDING TELEGRAM TWELVE 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 BEFORE PUBLICATION =

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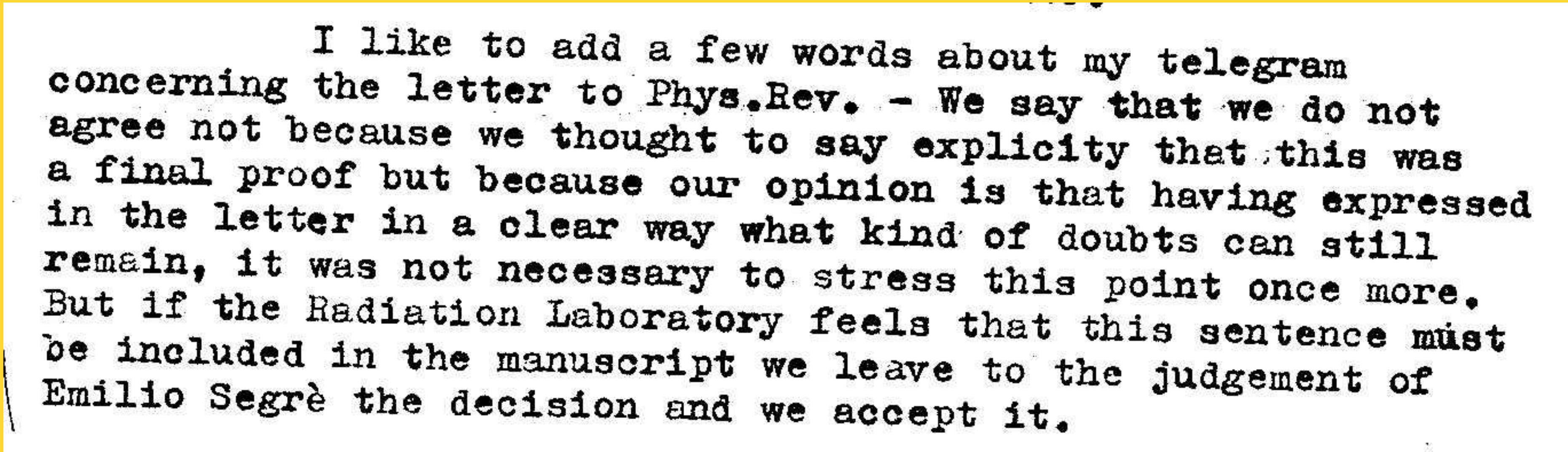
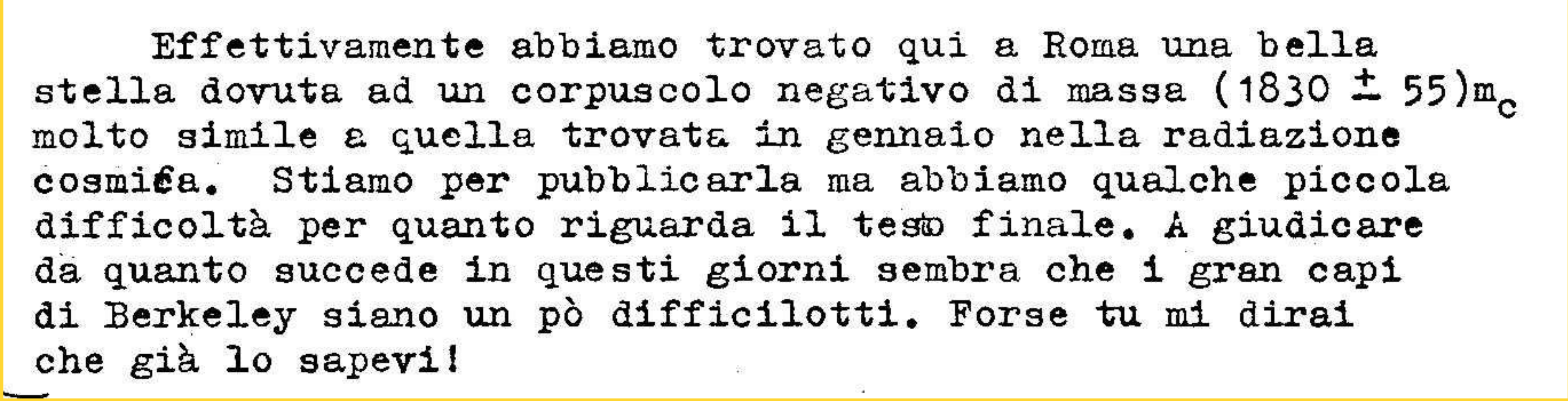
E. Segrè a E. Amaldi, 14 dicembre 1955



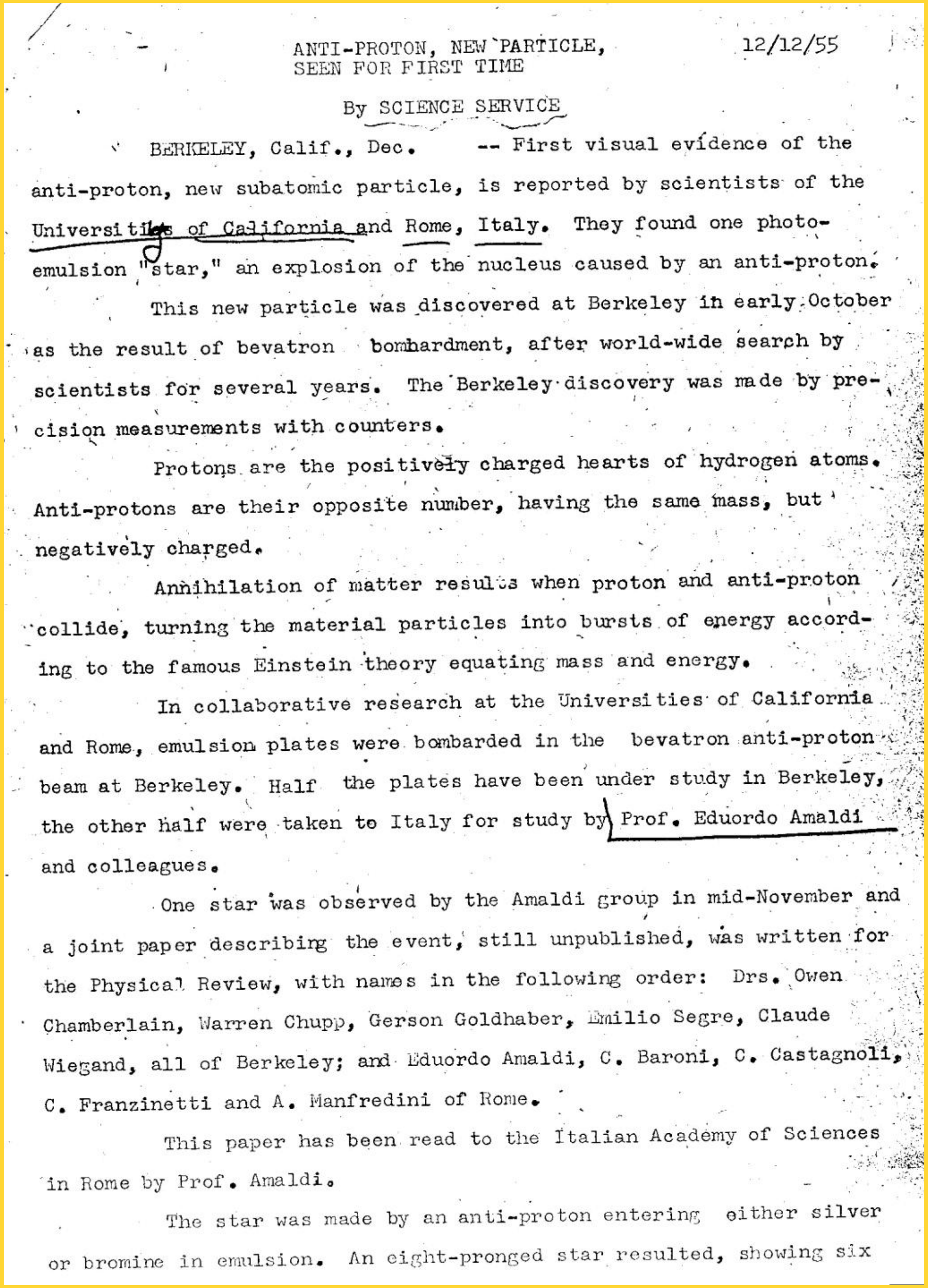
I like to add a few words about my telegram concerning the letter to Phys.Rev. - We say that we do not agree not because we thought to say explicity that this was a final proof but because our opinion is that having expressed in the letter in a clear way what kind of doubts can still remain, it was not necessary to stress this point once more. But if the Radiation Laboratory feels that this sentence must be included in the manuscript we leave to the judgement of Emilio Segrè the decision and we accept it.



Effettivamente abbiamo trovato qui a Roma una bella stella dovuta ad un corpuscolo negativo di massa $(1830 \pm 55)m_c$ molto simile a quella trovata in gennaio nella radiazione cosmica. Stiamo per pubblicarla ma abbiamo qualche piccola difficoltà per quanto riguarda il testo finale. A giudicare da quanto succede in questi giorni sembra che i gran capi di Berkeley siano un pò difficilotti. Forse tu mi dirai che già lo sapevi!

E. Amaldi a E. Segrè, 15 dicembre 1955

E. Amaldi a G.C. Wick, 15 dicembre 1955



ANTI-PROTON, NEW PARTICLE,
SEEN FOR FIRST TIME

12/12/55

By SCIENCE SERVICE

BERKELEY, Calif., Dec. -- First visual evidence of the anti-proton, new subatomic particle, is reported by scientists of the Universities of California and Rome, Italy. They found one photo-emulsion "star," an explosion of the nucleus caused by an anti-proton.

This new particle was discovered at Berkeley in early October as the result of bevatron bombardment, after world-wide search by scientists for several years. The Berkeley discovery was made by precision measurements with counters.

Protons are the positively charged hearts of hydrogen atoms. Anti-protons are their opposite number, having the same mass, but negatively charged.

Annihilation of matter results when proton and anti-proton collide, turning the material particles into bursts of energy according to the famous Einstein theory equating mass and energy.

In collaborative research at the Universities of California and Rome, emulsion plates were bombarded in the bevatron anti-proton beam at Berkeley. Half the plates have been under study in Berkeley, the other half were taken to Italy for study by Prof. Eduordo Amaldi and colleagues.

One star was observed by the Amaldi group in mid-November and a joint paper describing the event, still unpublished, was written for the Physical Review, with names in the following order: Drs. Owen Chamberlain, Warren Chupp, Gerson Goldhaber, Emilio Segre, Claude Wiegand, all of Berkeley; and Eduordo Amaldi, C. Baroni, C. Castagnoli, C. Franzinetti and A. Manfredini of Rome.

This paper has been read to the Italian Academy of Sciences in Rome by Prof. Amaldi.

The star was made by an anti-proton entering either silver or bromine in emulsion. An eight-pronged star resulted, showing six

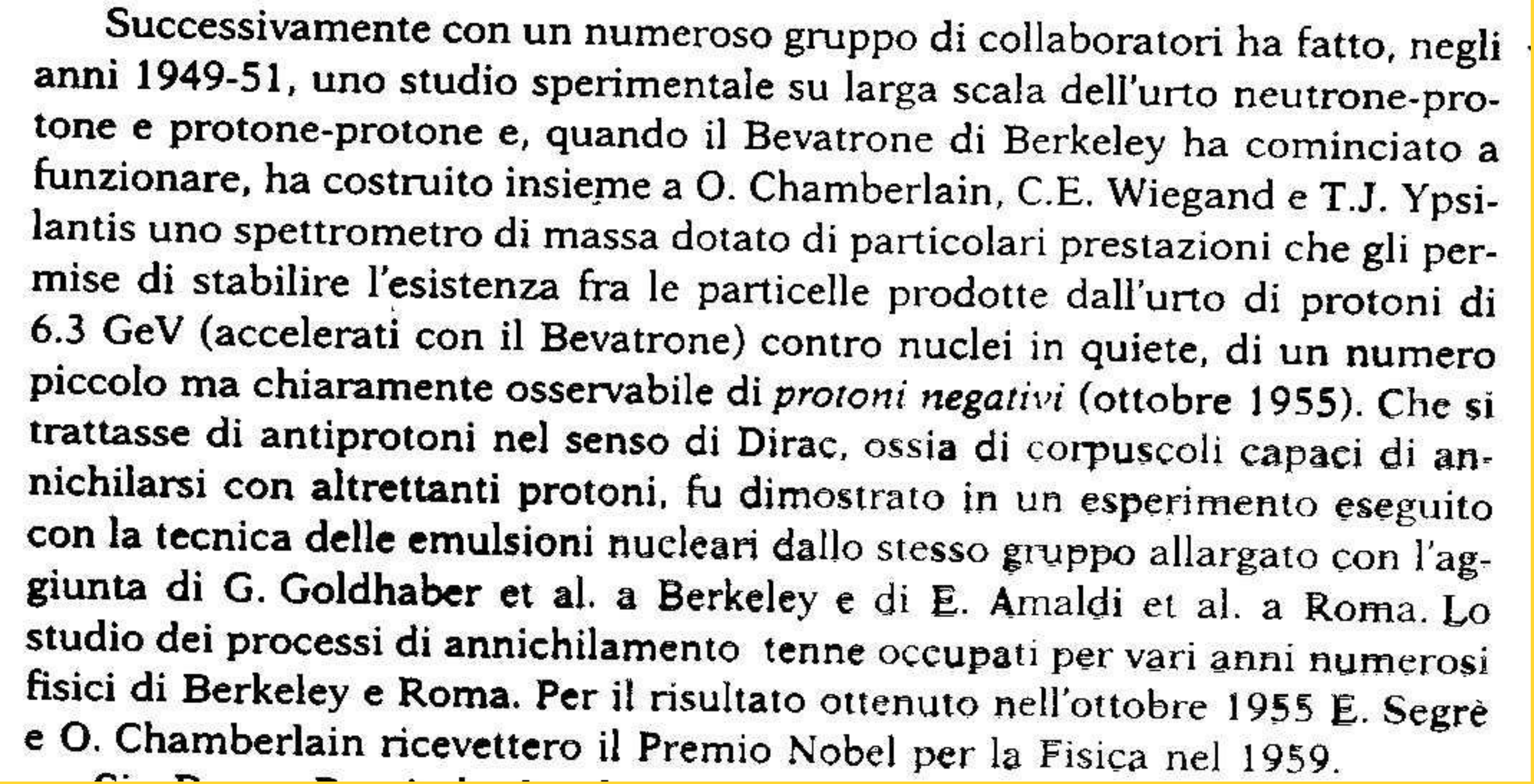
Comunicato stampa dell’agenzia americana Science Service del 12 dicembre 1955:
“Antiproton seen for first time”

La memoria dei protagonisti. G. Goldhaber (1989):

“By October 1955, the counter experiment had clearly demonstrated the following:
1. There were negative particles of protonic mass within an accuracy of 5 percent.
2. 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, despite the handicaps mentioned earlier, 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 necessary condition for antiprotons…

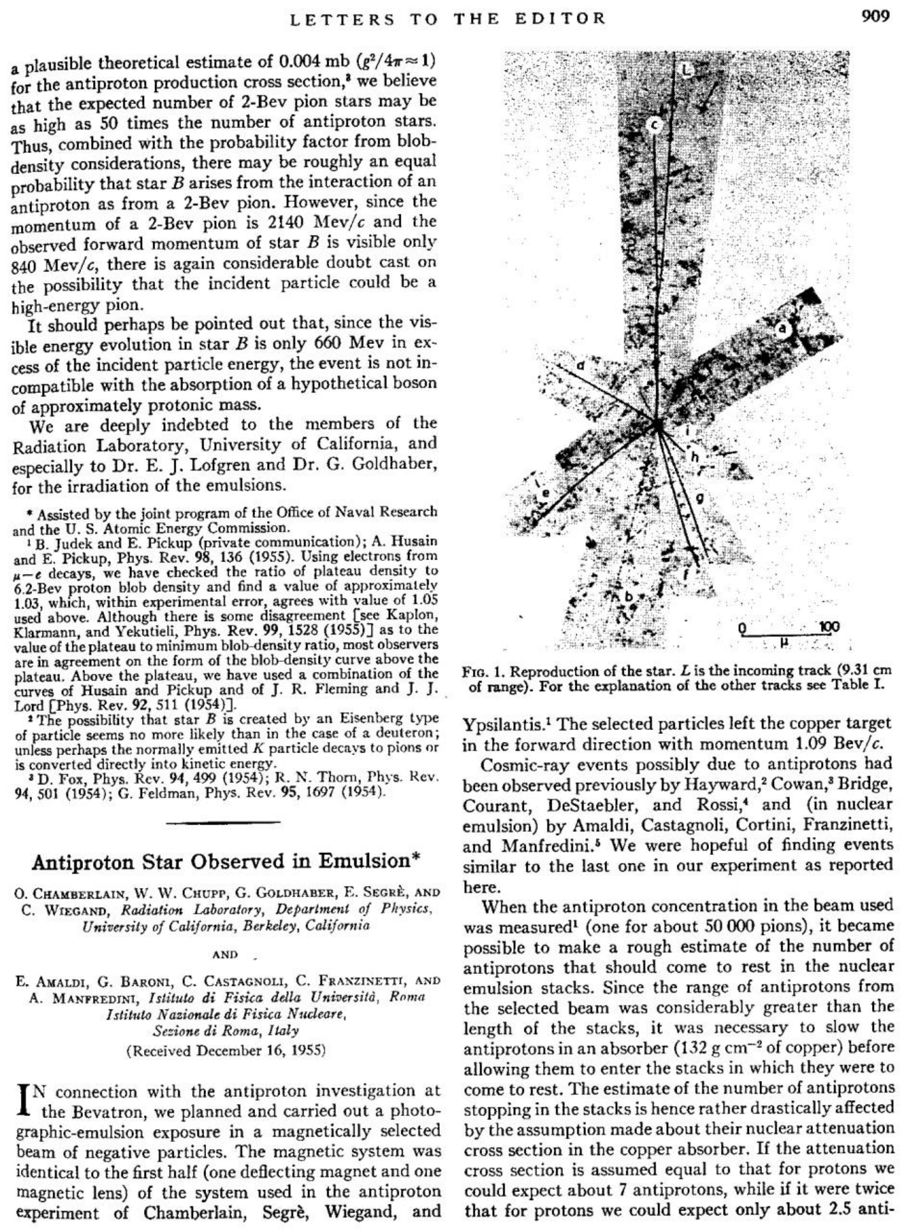
In December 1955 we decided to try another emulsion exposure - this time at 700 MeV/c, so that the antiprotons could enter the emulsion stack and come to rest in it… In the morning of 11 January 1956, he (G. Ekspong) followed a track to the end of its range, where it came to rest and formed a large star! 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.”



Successivamente con un numeroso gruppo di collaboratori ha fatto, negli anni 1949-51, uno studio sperimentale su larga scala dell’urto neutrone-protone e protone-protone e, quando il Bevatrone di Berkeley ha cominciato a funzionare, ha costruito insieme a O. Chamberlain, C.E. Wiegand e T.J. Ypsilantis uno spettrometro di massa dotato di particolari prestazioni che gli permise di stabilire l’esistenza fra le particelle prodotte dall’urto di protoni di 6.3 GeV (accelerati con il Bevatrone) contro nuclei in quiete, di un numero piccolo ma chiaramente osservabile di *protoni negativi* (ottobre 1955). Che si trattasse di antiprotoni nel senso di Dirac, ossia di corpuscoli capaci di annichilarsi con altrettanti protoni, fu dimostrato in un esperimento eseguito con la tecnica delle emulsioni nucleari dallo stesso gruppo allargato con l’aggiunta di G. Goldhaber et al. a Berkeley e di E. Amaldi et al. a Roma. Lo studio dei processi di annichilamento tenne occupati per vari anni numerosi fisici di Berkeley e Roma. Per il risultato ottenuto nell’ottobre 1955 E. Segrè e O. Chamberlain ricevettero il Premio Nobel per la Fisica nel 1959.

E. Amaldi su Segrè e l’antiprotone (1991)



a plausible theoretical estimate of 0.004 mb ($g^2/4\pi \approx 1$) for the antiproton production cross section,[3] we believe that the expected number of 2-Bev pion stars may be as high as 50 times the number of antiproton stars. Thus, combined with the probability factor from blob-density considerations, there may be roughly an equal probability that star *B* arises from the interaction of an antiproton as from a 2-Bev pion. However, since the momentum of a 2-Bev pion is 2140 Mev/*c* and the observed forward momentum of star *B* is visible only 840 Mev/*c*, there is again considerable doubt cast on the possibility that the incident particle could be a high-energy pion.

It should perhaps be pointed out that, since the visible energy evolution in star *B* is only 660 Mev in excess of the incident particle energy, the event is not incompatible with the absorption of a hypothetical boson of approximately protonic mass.

We are deeply indebted to the members of the Radiation Laboratory, University of California, and especially to Dr. E. J. Lofgren and Dr. G. Goldhaber, for the irradiation of the emulsions.

* Assisted by the joint program of the Office of Naval Research and the U. S. Atomic Energy Commission.

[1] B. Judek and E. Pickup (private communication); A. Husain and E. Pickup, Phys. Rev. **98**, 136 (1955). Using electrons from $\mu-e$ decays, we have checked the ratio of plateau density to 6.2-Bev proton blob density and find a value of approximately 1.03, which, within experimental error, agrees with value of 1.05 used above. Although there is some disagreement [see Kaplon, Klarmann, and Yekutieli, Phys. Rev. **99**, 1528 (1955)] as to the value of the plateau to minimum blob-density ratio, most observers are in agreement on the form of the blob-density curve above the plateau. Above the plateau, we have used a combination of the curves of Husain and Pickup and of J. R. Fleming and J. J. Lord [Phys. Rev. **92**, 511 (1954)].

[2] The possibility that star *B* is created by an Eisenberg type of particle seems no more likely than in the case of a deuteron; unless perhaps the normally emitted *K* particle decays to pions or is converted directly into kinetic energy.

[3] D. Fox, Phys. Rev. **94**, 499 (1954); R. N. Thorn, Phys. Rev. **94**, 501 (1954); G. Feldman, Phys. Rev. **95**, 1697 (1954).

Antiproton Star Observed in Emulsion*

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and

E. Amaldi, G. Baroni, C. Castagnoli, C. Franzinetti, and A. Manfredini, *Istituto di Fisica della Università, Roma Istituto Nazionale di Fisica Nucleare, Sezione di Roma, Italy*

(Received December 16, 1955)

Fig. 1. Reproduction of the star. *L* is the incoming track (9.31 cm of range). For the explanation of the other tracks see Table I.

In connection with the antiproton investigation at the Bevatron, we planned and carried out a photographic-emulsion exposure in a magnetically selected beam of negative particles. The magnetic system was identical to the first half (one deflecting magnet and one magnetic lens) of the system used in the antiproton experiment of Chamberlain, Segrè, Wiegand, and Ypsilantis.[1] The selected particles left the copper target in the forward direction with momentum 1.09 Bev/*c*.

Cosmic-ray events possibly due to antiprotons had been observed previously by Hayward,[2] Cowan,[3] Bridge, Courant, DeStaebler, and Rossi,[4] and (in nuclear emulsion) by Amaldi, Castagnoli, Cortini, Franzinetti, and Manfredini.[5] We were hopeful of finding events similar to the last one in our experiment as reported here.

When the antiproton concentration in the beam used was measured[1] (one for about 50 000 pions), it became possible to make a rough estimate of the number of antiprotons that should come to rest in the nuclear emulsion stacks. Since the range of antiprotons from the selected beam was considerably greater than the length of the stacks, it was necessary to slow the antiprotons in an absorber (132 g cm^{-2} of copper) before allowing them to enter the stacks in which they were to come to rest. The estimate of the number of antiprotons stopping in the stacks is hence rather drastically affected by the assumption made about their nuclear attenuation cross section in the copper absorber. If the attenuation cross section is assumed equal to that for protons we could expect about 7 antiprotons, while if it were twice that for protons we could expect only about 2.5 anti-

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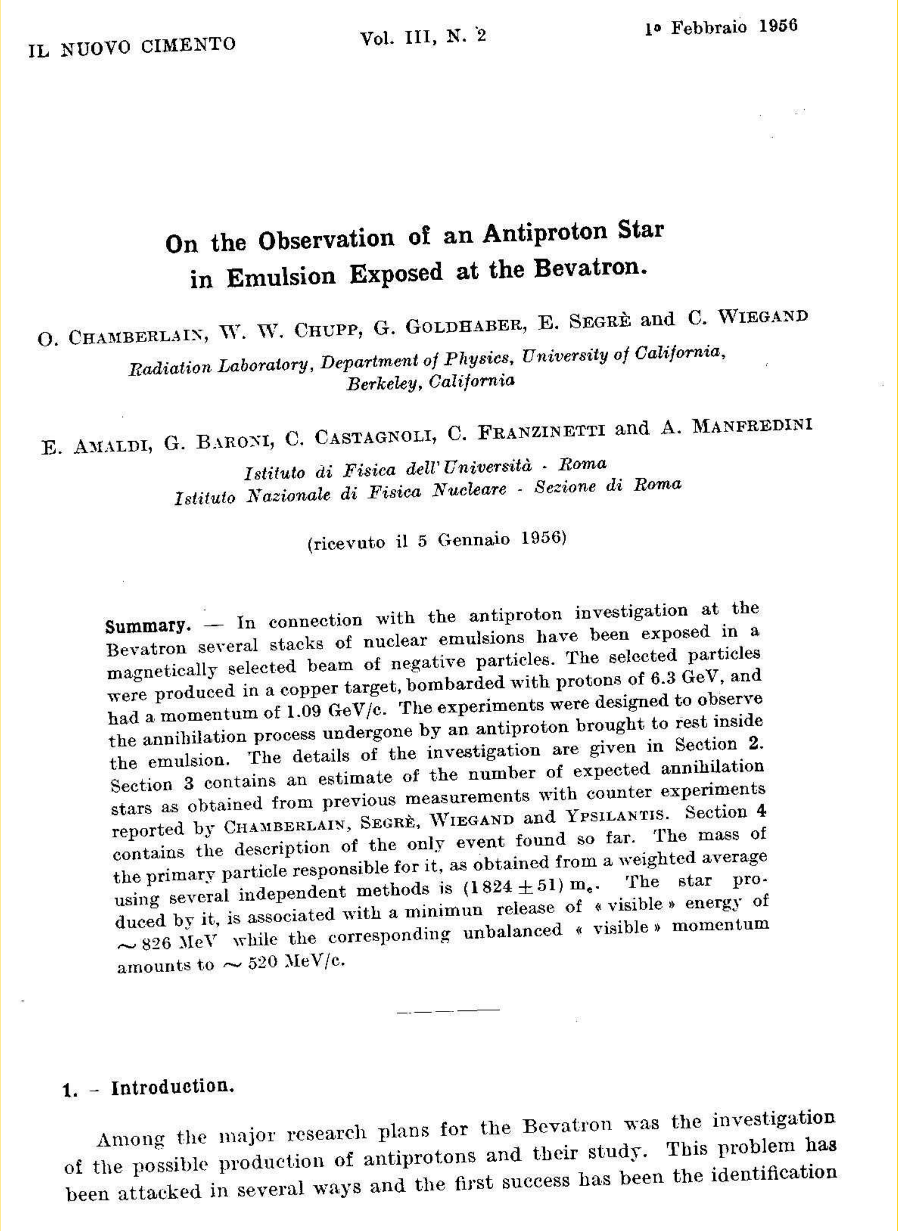

IL NUOVO CIMENTO Vol. III, N. 2 1° Febbraio 1956

On the Observation of an Antiproton Star in Emulsion Exposed at the Bevatron.

O. Chamberlain, W. W. Chupp, G. Goldhaber, E. Segrè and C. Wiegand
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E. Amaldi, G. Baroni, C. Castagnoli, C. Franzinetti and A. Manfredini
Istituto di Fisica dell'Università - Roma
Istituto Nazionale di Fisica Nucleare - Sezione di Roma

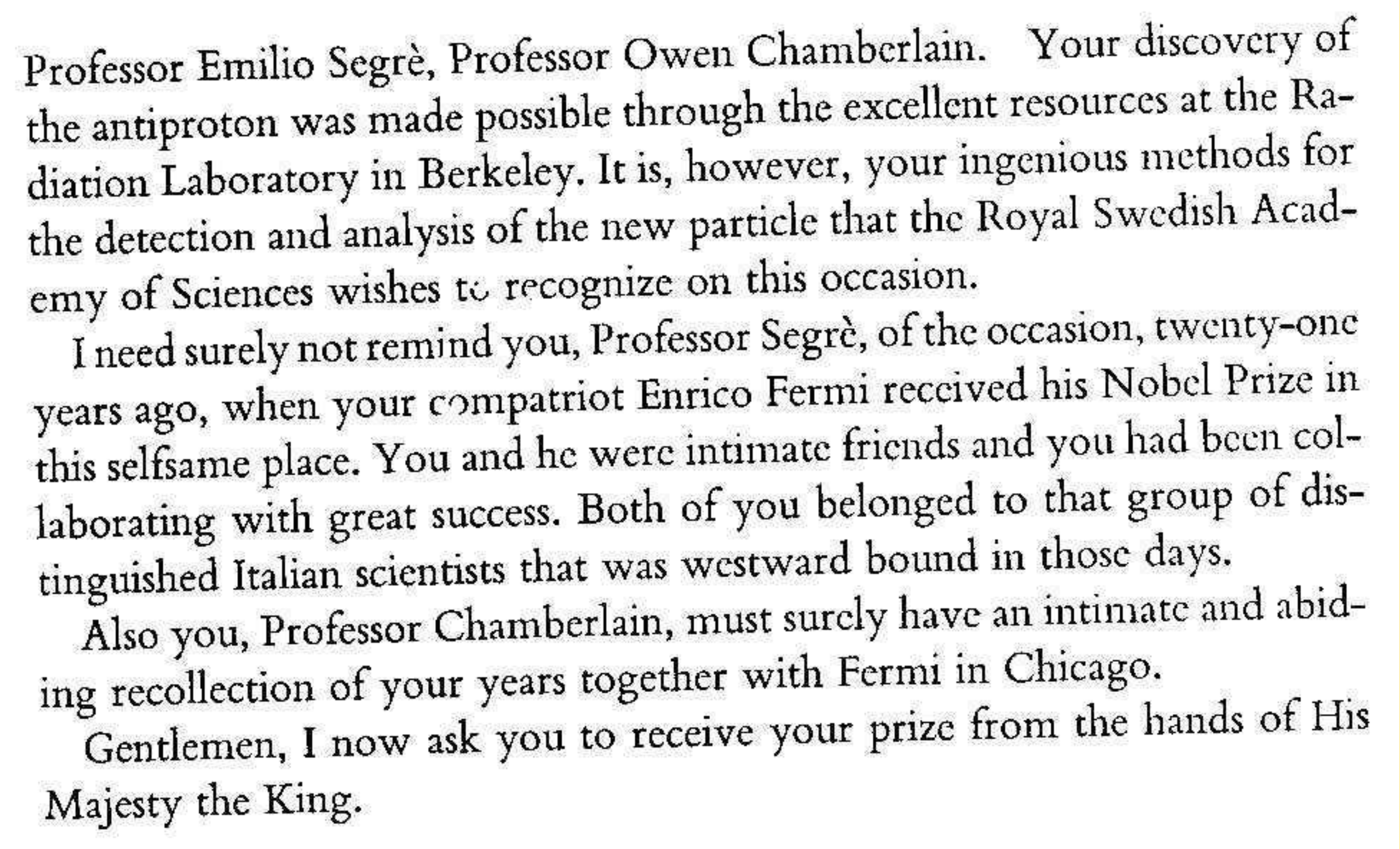
(ricevuto il 5 Gennaio 1956)

Summary. — In connection with the antiproton investigation at the Bevatron several stacks of nuclear emulsions have been exposed in a magnetically selected beam of negative particles. The selected particles were produced in a copper target, bombarded with protons of 6.3 GeV, and had a momentum of 1.09 GeV/c. The experiments were designed to observe the annihilation process undergone by an antiproton brought to rest inside the emulsion. The details of the investigation are given in Section 2. Section 3 contains an estimate of the number of expected annihilation stars as obtained from previous measurements with counter experiments reported by Chamberlain, Segrè, Wiegand and Ypsilantis. Section 4 contains the description of the only event found so far. The mass of the primary particle responsible for it, as obtained from a weighted average using several independent methods is $(1824 \pm 51)\, m_e$. The star produced by it, is associated with a minimun release of « visible » energy of ~ 826 MeV while the corresponding unbalanced « visible » momentum amounts to ~ 520 MeV/c.

1. – Introduction.

Among the major research plans for the Bevatron was the investigation of the possible production of antiprotons and their study. This problem has been attacked in several ways and the first success has been the identification

I risultati della collaborazione pubblicati sulla Physical Review e sul Nuovo Cimento



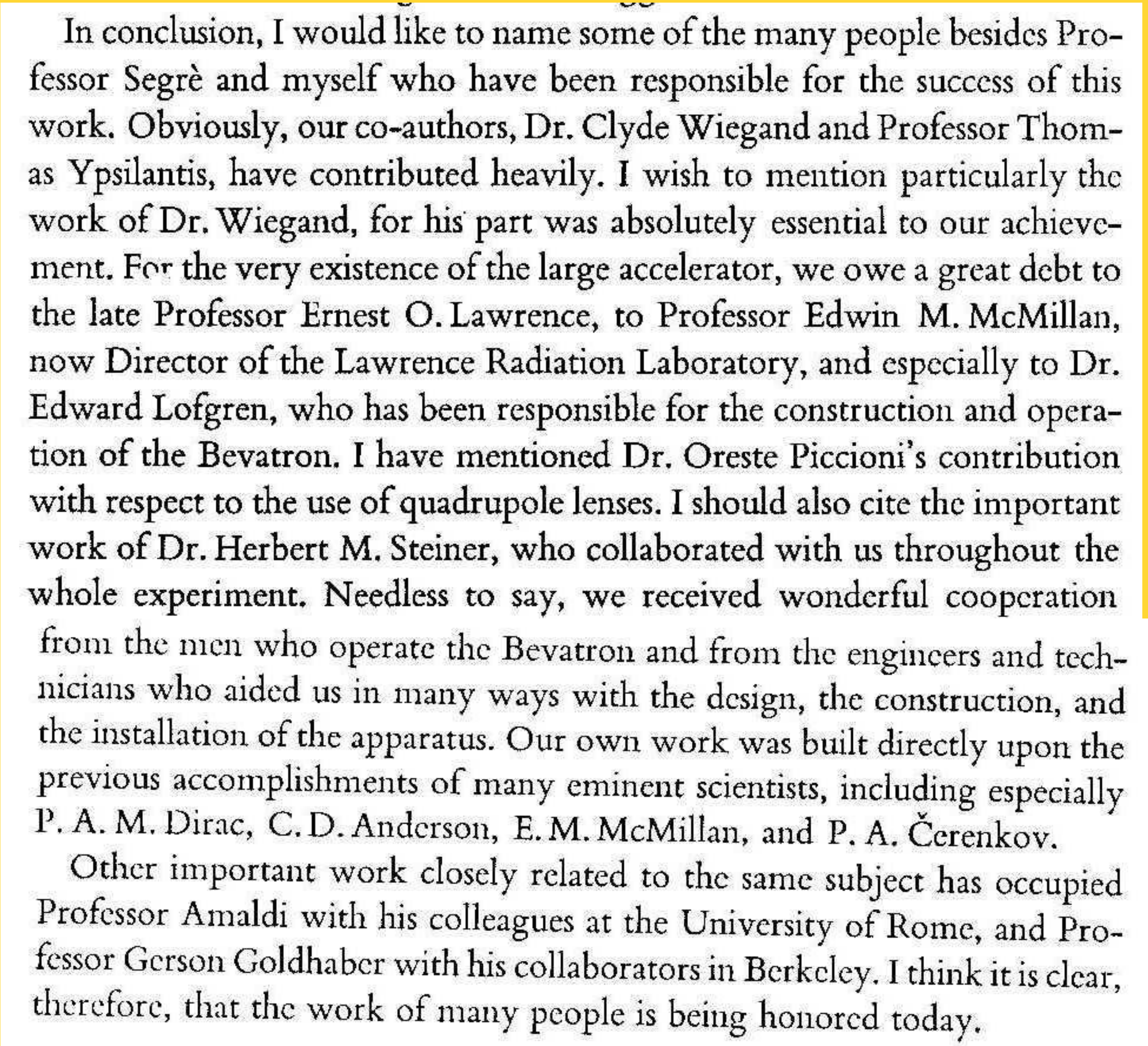
Professor Emilio Segrè, Professor Owen Chamberlain. Your discovery of the antiproton was made possible through the excellent resources at the Radiation Laboratory in Berkeley. It is, however, your ingenious methods for the detection and analysis of the new particle that the Royal Swedish Academy of Sciences wishes to recognize on this occasion.

I need surely not remind you, Professor Segrè, of the occasion, twenty-one years ago, when your compatriot Enrico Fermi received his Nobel Prize in this selfsame place. You and he were intimate friends and you had been collaborating with great success. Both of you belonged to that group of distinguished Italian scientists that was westward bound in those days.

Also you, Professor Chamberlain, must surely have an intimate and abiding recollection of your years together with Fermi in Chicago.

Gentlemen, I now ask you to receive your prize from the hands of His Majesty the King.

E. Hulthén (Presidente del Comitato Nobel per la Fisica), discorso di presentazione alla cerimonia di conferimento del premio Nobel, Stoccolma 1959



In conclusion, I would like to name some of the many people besides Professor Segrè and myself who have been responsible for the success of this work. Obviously, our co-authors, Dr. Clyde Wiegand and Professor Thomas Ypsilantis, have contributed heavily. I wish to mention particularly the work of Dr. Wiegand, for his part was absolutely essential to our achievement. For the very existence of the large accelerator, we owe a great debt to the late Professor Ernest O. Lawrence, to Professor Edwin M. McMillan, now Director of the Lawrence Radiation Laboratory, and especially to Dr. Edward Lofgren, who has been responsible for the construction and operation of the Bevatron. I have mentioned Dr. Oreste Piccioni's contribution with respect to the use of quadrupole lenses. I should also cite the important work of Dr. Herbert M. Steiner, who collaborated with us throughout the whole experiment. Needless to say, we received wonderful cooperation from the men who operate the Bevatron and from the engineers and technicians who aided us in many ways with the design, the construction, and the installation of the apparatus. Our own work was built directly upon the previous accomplishments of many eminent scientists, including especially P. A. M. Dirac, C. D. Anderson, E. M. McMillan, and P. A. Čerenkov.

Other important work closely related to the same subject has occupied Professor Amaldi with his colleagues at the University of Rome, and Professor Gerson Goldhaber with his collaborators in Berkeley. I think it is clear, therefore, that the work of many people is being honored today.

O. Chamberlain, “The early antiproton work”, Nobel Lecture, 11 dicembre 1959

“In parallel with their investigation of the social role of big machines, historians would wish to study the socialization of the men and women who worked with and around them. By “socialization” I mean the effects of training, working conditions, the award system, and so forth on the attitudes of physicists toward their discipline and its goals. These attitudes include expectations about level of support and also political tone, which differed markedly between, say, Brookhaven (or Rome, n.d.a.) and Berkeley.
The award system offers a useful probe into this socialization… We often find the study of priority disputes, and the system of credit and reward, particularly instructive about the general circumstances of the science of any period…

The Nobel Prize does not seem well adapted to experimental particle physics. The one is set up for the individual, and serves the cult of personality; the other requires teamwork and group allegiance…

The award of the Nobel Prize for the antiproton almost opened the practices of the community of particle physics to public scrutiny. Historians might regret that it did not.”

J. Heilbron, “An historian’s interest in particle physics” (1989)

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