EARLY LINES OF RESEARCH IN ITALY:1900-1940

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Solid state physics, as a definite field of research, developed in Italy only after the second world war. However, if we look back at earlier times, we find a great deal of works on the physics of solid matter: many of them belong to rather well defined lines of research; others appear instead as more or less isolated contributions. They can be divided into seven groups:

a) elastic properties
b) thermal properties
c) electrical properties
d) magnetic properties
e) galvanomagnetic and thermomagnetic effects
f) Volta and photoelectric effects
g) optical properties of ions in solid solutions or crystals

In this communication it is impossible to report on all these topics or even to fairly substantiate the conclusions we have arrived at. Therefore, we prefer to commit ourselves to an exposition primarily based on our conclusions (that we will try to support in coming publications) and present some exemplars taken from the above topics as sketchy illustrations of the conclusions.

In a paper to be published ([1]) we have studied the institutional development of physics in Italy between 1871 and 1940 with particular emphasis on the first four decades of our century. Quoting from the conclusions of that paper: "*In the period taken into consideration (1900-1940) the Italian physics community appears as a small group of scientists facing formidable challenges with inadequate means (both cultural and material). The smallness of the group limited the possibility of a collective and prompt assimilation of the new developments within the discipline. Its slow rate of increase made this task even more difficult.*

The more general cultural, economic, social and political context was not a fertile field for the growth of scientific disciplines . The distribution of academic power and of students between the various faculties, the scarce funds devoted to research, the technological underdevelopment of the industry, are at the same time causes and effects of a looped process typical of a country in which the overall demand for science is far behind in the list of priorities.

Therefore, the necessary conditions were lacking for either bringing Italian research up to competitive levels or favouring the diffuse rooting of new fields of research. Isolated and temporarily limited cases such that of Fermi's group are interesting not for their exceptionality but for how they can be fitted into the general frame we have described."

We can implement this picture on the basis of our investigation of the physics of solid matter. Therefore, the description given below relies on the assumption that the features of Italian physics which depended primarily on the institutional context and/or on the general traits of the Italian physics community, were reflected fairly well in the subfield of the physics of solids. This does not seem a strong assumption, because of the quantitative relevance of the research on the properties of solids ([2]) and of the fact that this type of research has not been, during the entire period considered, a specialized field of activity.

In the period considered, the main part of physical research in Italy has lived on the borders, when not far apart, of the main streams of advancement of physics. As partially argued in our paper on the institutional context (1), exceptions like that of Fermi's group should be viewed as hints of a possible new trend which however never acquired enough strength to modify the general situation, not only because of the outbreak of the war.

We can speak of an apartness of the Italian physics community with respect to the international one. This apartness had many features of the centre-periphery interaction. The hard core of this apartness endured through the four decades

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[1] S. Galdabini, G. Giuliani, "Physics in Italy between 1900 and 1940: the universities, physicists, funds and research". To be published in "Historical Studies in Physical and Biological Sciences".

[2] See below.

we have studied. Some traits instead changed over the years. The international community followed and used results of Italian research, at least when they were published in foreign reviews. Of course Italian physicists too used results obtained abroad; however they were allowed to ignore foreign contributions even on fundamental issues. The term ignore had three applications: not being able to follow and/or master recent theoretical developments; neglecting theoretical approaches diffused abroad and different from their own; not quoting foreign contributions to which their papers were more or less tightly connected. Of these three applications only the first one may be attributed to the entire period considered. The other two can be found in the habits of the important Italian group which worked on galvanomagnetic effects in the tens; they were no more in use in the thirties and it is to be verified if widespread before. Today's reader may rightly wonder how that was possible. The only reasonable answer one can find is that the evaluation of scientific achievements was based on an average background and/or specialized knowledge mainly derived from Italian publications. This restraint was obviously related to what we have called in the above self-quotation "the limited capacity of a collective and prompt assimilation of the new developments within the discipline". But there was more. There was the difficulty of grasping, not as illuminated individuals but as a group, the mains directions that the developments of the discipline would have taken.
This apartness had also some consequences on publication habits. Italian physicists published firstly and mainly on Italian magazines ([3]): "Il Nuovo Cimento", "I Rendiconti dell'Accademia dei Lincei" and, starting 1930, "La Ricerca Scientifica". Often, papers published in "I rendiconti" were republished unchanged in "Il Nuovo Cimento". The same was true, to a lesser extent, for "La Ricerca Scientifica". Only a few papers were translated and published, with the same content, in foreign magazines, mainly german. Fermi too, particularly in his first years, followed this habit. However, he was among the few who published some papers only on foreign reviews. These publication habits were at the same time a manifestation of the apartness and a reinforcement of it. Papers published only in Italian magazines, in spite of their possible relevance, were often ignored, poorly or not at all quoted, and easily forgotten.
The ravaging effects of the war upset also the little world of Italian physics. Not only the research activity has been greatly slowed down during the war, but, at the end, the world appeared dramatically changed not only in its general political, economical and social traits but also from the viewpoint of the scientific community. For western countries the centre, not only from the scientific point of view, has been displaced on the other part of the Ocean. European countries suffered losses of eminent scientists, because of racial persecutions. Periphery started reconstruction as more peripheral than before. Therefore, no wonder if Italian solid state physics moved its first steps after the war with all the features and difficulties typical of a peripheral location. That situation was shared by all Italian scientific disciplines. However, as far as physics is concerned, not all the various subfields restarted on the same line. Nuclear and cosmic-rays physics had attracting and renowned reference points behind them; solid state physics, apart from the long lasting tradition in experimental research on ferromagnetic properties that the "Galileo Ferraris" National Institute helped survive through the war, had lost any reminiscence of the past and had to start from scratch in a situation in which, very rapidly, the funds began to be concentrated anew on nuclear physics.
To illustrate this general picture, we shall discuss three cases, chosen because of being particularly illustrative of our conclusions and of the fact that they cover three of the four decades studied.

I) Galvanomagnetic and thermomagnetic effects

Already in 1850 we find a work of G.A. Maggi ([4]) in which he reported to have observed an effect of the magnetic field on the thermal conductivity of iron. Later, the study of the influence of the magnetic field on the transport properties of solids was carried on by several Italian physicists. Among them we can not avoid mentioning Augusto Righi who in 1887, independently and about contemporaneously of Leduc, discovered what was later called the Righi-Leduc effect ([5]). Righi studied also the Hall effect: his first paper on the argument is dated 1883.
In the first decades of our century the Italian contributions have been primarily, but not exclusively, focused on the galvanomagnetic effects. The leading figure was Orso Mario Corbino, then in Roma. In fact not only he gave contributions whose importance was immediately recognized abroad, but, as we shall see, his theoretical approach characterized the Italian contributions in spite of the fact that it was not shared by all Italian physicists working in the field.

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[3] Also foreign physicists published their papers mainly on national magazines. But this habit had negligible consequences for those belonging to the scientifically leading countries.

[4] G.A. Maggi, Arch. de Genève,14, 132 (1850).

[5] A. Righi, Mem. Acc. Lincei, 4, 433 (1887); Rend. Acc. Lincei, 3-I, 481, 3-II, 6 (1887).

Corbino's first paper on the argument was published in 1911 ([6]) and deals with what was later called the "Corbino effect". The most relevant aspect of this work is given by the use, instead of the usual rectangular plate, of a disc with a circular hole at its centre: the electrodes carrying the current are placed on the internal and external circumference and the magnetic field is perpendicular to the disc. In these conditions the primary current is radial and the effect of the magnetic field is that of producing a circular current: this is the "Corbino effect". Corbino did not say how he had conceived or borrowed the idea of using a disc instead of a rectangular plate. The fact is that 25 years before, i.e. in 1886, Boltzmann ([7]) had published a theoretical paper in which the Hall effect is studied not only in the usual conditions but also in a circular plate, that is in a configuration identical with Corbino's. Did Corbino know of the Boltzmann's paper? We do not know the answer. We only know that, Trabacchi, a Corbino's assistant and coworker, cited Boltzmann's papers in a work published in 1918 ([8]). We also know that none of the foreign physicists who worked on and discussed about Corbino effect, recalled that the essential features of the theoretical treatment of the disc had been already given by Boltzmann. Nor did it L.L. Campbell in his beautiful review book on the galvanomagnetic and thermomagnetic effects published in 1923 ([9]). Anyway, Boltzmann's papers were not the only possible source of inspiration for Corbino. During the nineteen century circular plates had been widely used for the study of electromagnetic effects; we may recall Faraday's disc and Barlow's wheel. Maggi himself used a circular plate for searching a magnetic field effect on the thermal conductivity of iron.

From an historical point of view, the major interest in the first of Corbino's papers is perhaps given by the fact that he used a theory of electrical conduction based upon the existence of two types of carriers which Corbino called the positive and negative ions or, in later papers, electrons. Now the point is that, in those times, the dominant theory of electrical conduction was based on the existence of only one type of charge carriers, namely the electron. No surprise then if the first paper published abroad on Corbino effect ([10]) is simply an interpretation of Corbino's experimental results in terms of the monistic theory of conduction.

The choice of Corbino in favour of the dualistic theory may be defined as "strong" in the sense that Corbino committed himself to the existence of two types of charge carriers, to the temperature dependence of their concentrations, and to its general validity. However, Corbino did not say a word to justify his choice and kept silent on this fundamental issue till 1920.

We can not follow here even the main lines of development of the study of galvanomagnetic effects in Italy after the basic Corbino's work of 1911. We will restrict ourselves to some highlights and comments. 1915 may be considered as the year in which the Italian studies on galvanomagnetic effects assumed the contour of a line of research. The IX volume of "Il Nuovo Cimento" opens with an article by Hall on "A possible explanation of the Hall and Ettingshausen phenomena", followed by a paper by Corbino and a lengthy article by Volterra, both on galvanomagnetic effects. But that is not all. In addition to these three papers, that volume contains five more papers on galvanomagnetic effects. Other papers will be published in the three following years and this highly productive period culminated in 1918 with the paper by Corbino on magnetoresistance.

Viewed as a whole, this Italian work is characterized by three features. The first is the committment to the dualistic theory of conduction. The second, is the kind of theoretical approach, which may be defined as typical of mathematical physics. It was clear from the beginning: Corbino's aim was more oriented to the determination of currents and potentials distribution than to understand how the magnetic field influences the transport properties. Of course in Corbino's equations we find the carriers' mobilities, but their role is auxiliary and unproblematic. The third feature is given by the apartness of the Italian group with respect to the international community. Apart from the choice of the dualistic theory in a context of widespread diffusion of the monistic one, it must be stressed that, in spite of the attention given by foreign physicists to Corbino effect in terms of extended and more accurate measurements and of alternative theoretical interpretations, the Italian group ignored foreign contributions and rarely

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[6] O.M. Corbino, Nuovo Cim. 1, 397 (1911); Phys. Zeit. 12, 561 (1911). Preliminary accounts of this work have been published in "I Rendiconti dell'Accademia dei Lincei" (1911).

[7] L. Boltzmann, Anz. Acad. Wiss. Wien, p.77, (1886); Phil. Mag. 22, 226 (1886).

[8] G.C. Trabacchi, Il Nuovo Cim. 16, 197 (1918).

[9] L.L. Campbell, "Galvanomagnetic and thermomagnetic effects", London 1923.

[10] E.P.Adams, Phil. Mag. 27, 244 (1914).

quoted them. If one reads only Italian papers on the subject, he gets the clear impression that the study of galvanomagnetic effects was carried on mainly in Italy with scarce and minor contributions by foreign countries. Eventually, Corbino will be forced to assume a more critical approach by the intervention of another Italian physicist, Michele La Rosa. So, while Corbino (and the other Italians) ignored for years the signals coming from abroad, he answered with impressive engagement to La Rosa's remarks. It is perhaps impossible to find a better example of what we have called the apartness of Italian physics community.

However, before discussing the La Rosa-Corbino debate, we must say some words on what must be considered the crowning achievement of the Italian group: the paper by Corbino on magnetoresistance published in 1918 ([11]). In an article published two years before, Elena Freda ([12]) showed the possibility of explaining the magnetoresistance effect on the basis of the dualistic theory. As Corbino explicitly acknowledged in his paper, he was firmly convinced that dualistic theory could not explain magnetoresistance effects. Anyway, when eventually Corbino dealt with them, he exploited all the potentialities of the dualistic approach. He showed that magnetoresistance effects in a rectangular plate can occur only in the presence of two types of carriers; that the effect is larger in circular plates; and, finally, that the Hall coefficient depends on the applied magnetic field only if two types of carriers are present. Furthermore, combining the relations obtained for the resistance and the Hall coefficient as functions of the magnetic field, it would have been possible, through appropriate measurements, to obtain the concentrations and mobilities of the two types of carriers. We now know that the relations obtained by Corbino, though qualitatively correct, were quantitatively wrong. But, of course, Corbino's contemporaries did not know or could prove that. Therefore, Corbino's results should have been cheered as at least interesting achievements, also because the subsequent experimental work of Trabacchi (8) seemed to support the relations found by Corbino. But that did not happen (we are here referring to the international community): the large majority of physicists simply distrusted dualistic theories and Corbino's calculations were probably considered no more than a formal exercise.

Now to Corbino-La Rosa debate. In a note of a paper published in 1918 in "Il Nuovo Cimento" ([13]) La Rosa writes:

> "*Drude's hypothesis about the existence of a double current of electrons of opposite sign may explain the positive Hall effect, but, as it is known, this hypothesis had shown untenable for various theoretical and experimental reasons during the following development of the entire electronic theory*".

And one year later, again in a note:

> *".....Prof. Corbino has published a work on this argument [La Rosa is referring to the article of 1918 we have just discussed] in which the presence of two kind of electrons is supposed. As it is known, this hypothesis is not supported by experimental facts, and leads to very great difficulties in the development of other branches of the electrons' theory.....so that every effort towards an explanation of the facts in the framework of the generally accepted theory (which admits of negative electrons only) maintains all its value*" ([14]).

The answer of Corbino to La Rosa remarks was uneasy. In a period of thirteen months (June 1920-July 1921) he presented to the Accademia dei Lincei five memoirs in which he strove to parry the blow. He acknowledged that

> "*A further formulation [of the theory] must ideally lead.....to the dismissal of those hypothesis which do not fit well into the general frame of the physical phenomenology. In this sense, we must consider as a true*

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[11] O.M. Corbino, Nuovo Cim. 16, 185 (1918.

[12] E. Freda, Il Nuovo Cim., 12, 177 (1916).

[13] "*L'ipotesi del Drude dell'esistenza di una doppia corrente di elettroni di segno opposto può giustificare l'effetto Hall positivo, ma come si sa, essa per varie ragioni di ordine teorico e sperimentale si è dimostrata insostenibile, nel successivo sviluppo di tutta la teoria elettronica.*" M. La Rosa, Nuovo Cim. 15, 89 (1918)

[14] *".....il Prof. Corbino ha pubblicato un lavoro su questo argomento, nel quale si ammette la presenza di due specie di elettroni. Come si sa, questa ipotesi non è suffragata da fatti sperimentali, e conduce a gravissime difficoltà nello sviluppo delle altre branche della teoria degli elettroni,.....cosicchè ogni tentativo diretto a ricondurre la spiegazione dei fatti nel quadro* della teoria generalmente ammessa (che suppone l'esistenza dei soli elettroni negativi) conserva tutto il suo valore." M. La Rosa, Nuovo Cim. 18, 39 (1919)

progress every effort to explain on the basis of the presence of only one kind of electrons, the negative ones, those facts which seem to call for the presence, postulated by Drude, of two kind of carriers: the positive and the negative" ([15]).

This statement though important as the first critical reflection on his own theoretical approach, may be misleading. In fact, one might expect that Corbino would have adopted a more critical approach towards dualistic theory or/and would have contributed to achieve a "true progress" by trying the monistic one. His choice was completely different. In the memoirs cited and in two others published in 1926, Corbino strove to show that the application of Lorentz's theory and of its modifications to galvano and thermo magnetic effects leads to "categorical contradictions" and to predictions that are at odds with experiments.
With the two memoirs of 1926, Corbino ended his contribution to the studies of galvanomagnetic effects. Italian physicists, not belonging to what we may call Corbino's group, kept on studying these effects till late thirties. But the isolation of the Italian contribution became more pronounced. The advent of quantum mechanics and of the band theory of solids was simply ignored by Italian researchers who kept on studying galvanomagnetic effects.

II) A paper on paramagnetism
Miss Rita Brunetti was appointed to a chair in Ferrara in 1926, but owing to the poor state of the physics institute, she worked for the next two years, before leaving for Cagliari, at the physics institute of Bologna. The paper that we shall discuss was presented on the seventh of April 1929 to the Accademia dei Lincei and was never published elsewhere ([16]). The title was "Theory of paramagnetism for ions in strong molecular fields". However, to follow the path of discovery, we must go back a little. In Bologna Miss Brunetti began to study the optical properties of rare earth ions in liquid and solid solutions and in crystals. We will report on these works elsewhere. What interests us here, is that Miss Brunetti, in dealing with these problems, became familiar with the possible influence of molecular or crystalline fields on the optical properties of rare earth ions when in solutions or in crystals. Her next step was the study of the possible contribution of the highest of the two allowed multiplet states to the paramagnetic behaviour of the Ce^{3+} ion ([17]). To explain her experimental results, Miss Brunetti supposed that the molecular field in some way increases the effective charge of the nucleus of the cerium ion so that the separation between the two multiplet levels should be lower than that expected. As we shall see, the effect of molecular fields on multiplet levels will become for Miss Brunetti the clue for finding the way to the solution of the puzzle presented by the paramagnetic behaviour of the ions of the iron group.
When Miss Brunetti looked at this problem, the state of art was as illustrated in fig. 1, taken from the paper of E. C. Stoner ([18]). Curve 2 is the theoretical number p of Bohr magnetons calculated using Hund's formula (1925) ([19]), according to which
$p = g\,[(J(J+1)]^{1/2}$

This formula ([20]) should apply when the separation between the multiplets (corresponding to different J values) is large when compared with KT. With two exceptions (Eu^{3+} and Sm^{3+}), this formula describes the paramagnetism of rare earth ions ([21]).

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[15] "*Una elaborazione ulteriore deve idealmente condurre .....alla eliminazione di quelle ipotesi che non si assestano bene nel quadro generale della fenomenologia fisica. E in tal senso è da considerare come un vero progresso ogni tentativo di spiegare con la presenza di elettroni di una sola specie, i negativi, quei fatti che sembrano richiedere l'esistenza postulata dal Drude, di due specie di centri mobili: i positivi e i negativi*." O.M. Corbino, Rend. Acc. Lincei, 29, 282 (1920).

[16] R. Brunetti, Rend. Acc. Lincei, 9, 754 (1929).

[17] R. Brunetti, Nuovo Cim., 6, 85 (1929).

[18] E.C. Stoner, Phil. Mag. 8, 250, (1929).

[19] F. Hund, Zeits. Phys. 33, 855 (1925.

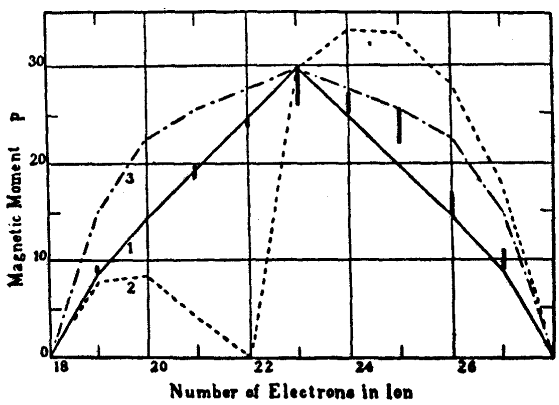

Fig. 1 Observed and calculated numbers of Bohr magnetons for the ions of the iron group. (From ref. 18)

The vertical lines represent the experimental data for the ions of the iron group. No question that they can not be fitted by Hund's formula. Curve 1 represents the formula suggested in 1927 by Bose ([22])

$$p = [4S(S+1)]^{1/2}$$

and obtained supposing that only the spins contribute to the magnetic moments. It fits well the experimental data for the first ions of the iron group, but the magnetic moment of the ions containing more electrons are definitely greater than those calculated. Furthermore, Bose gave no theoretical justification of the hypothesis he used. Finally in 1928, Van Vleck ([23]) showed that, while Hund's formula works well, as already known, when the multiplets' separation is high with respect to KT, the formula that must be used in the opposite case - small multiplets' separation with respect to KT - is

$$p = [4S(S+1)+L(L+1)]^{1/2}$$

This formula is reported as curve 3 in the figure.

Stoner (18) and Brunetti showed, contemporaneously and independently, the way to the solution. We present the two approaches side by side in table I in order to emphasize similarities and differences.

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[20] Hund proposed its formula before the introduction of the electron spin. Therefore, the quantum number J was then the total angular momentum defined as the sum of the orbital angular momemtum of the outer electron and the angular momentum of the "rest of the atom". After the discovery of the electron spin, Hund's formula was read as we use today.

[21] The paramagnetic behaviour of these ions must be described by a non-approximate formula because of the fact that their multiplet separation is comparable with KT.

[22] D.M. Bose, Zeits. Phys. 63, 864 (1927).

[23] J.H. Van Vleck, Phys. Rev. 31, 587 (1928).

TABLE I

BRUNETTI	STONER
The multiplet separation in the ions of the iron group is much smaller than that of rare earth ions. An estimate indicates that there is a factor of about ten. The molecular field may then break up the L-S coupling, destroying the multiplet levels in the case of the iron group but not in the other.	*In the iron group, the electrons responsible for the paramagnetism are the outer ones. In the rare earth case they belong to an inner shell and do not take part in the molecular bond. Therefore, in the case of the iron group there will be a strong "l" interaction not only inside the ions but also with neighboring ions.*
The molecular field, supposed having a randomly oriented simmetry axis, aligns the angular momentum along its axis.	*The "l" interaction between neighboring ions tend to orient the angular momentum along a definite direction.*
Calculates the energy levels in a magnetic field supposing that spin magnetic moments orient freely in the external field.	*Treats the "l" interaction between neighboring ions as an effect of a virtual magnetic field Hi. Supposes that the virtual field is parallel or antiparallel to the external field. Spin magnetic moments orient freely in the external field.*

As we can see, the conceptual approach is different: while Stoner links the possible effect of the molecular field to the fact that the paramagnetic electrons are in outer or inner shells, in Brunetti's view the reference term is the multiplet energy separation. The two points of view are not equivalent, but, since their role was mainly that of sound arguments in favour of the breaking of the L-S coupling by the molecular field, they did their job. No wonder then if the two models, in spite of their different conceptual foundation and structure, led to substantially the same results. The reduced contribution to the paramagnetism of the orbital moments arises from the fact that they are pre-oriented by the molecular field. The formulas obtained are:

$$p = [4S(S+1) + (1/3)L(L+1)]^{1/2} \quad \text{(Brunetti)}$$

$$p = [4S(S+1) + f(KT/mH_i)L(L+1)]^{1/2} \quad \text{(Stoner)}$$

where in Stoner's formula m is the orbital magnetic moment and f is a function which can take on the values between 0 and 1.

Stoner's formula, more flexible than Brunetti's, has the advantage of covering continuously the gap between the two extreme cases discussed above and reported in fig 1. Brunetti, on the other hand, comments:

"*We do not exclude that, for a particular distribution of the ions in the molecule, one could get an orbital contribution greater or smaller than (1/3)L(L+1)*" ([24]).

The way was traced. The treatment of Brunetti and Stoner was characterized by models in which the reduced contribution of the orbital moments comes out as a purely classical effect. The quantum mechanical treatment of the quenching of the angular momentum would have been given soon afterwards. In the book by Van Vleck on elec tric and magnetic susceptibilities ([25]), published in 1932, the problem was presented as settled for good. Van Vleck referred to Stoner's paper but did not mention Brunetti's contribution. Both contributions instead, had been quoted by Sommerfeld at the Solvay Conference on Mag netism held in Brussels in 1930 ([26]). But Sommerfeld went on by following Stoner's paper. This is not surprising. Apart from Stoner's international reputation and long lasting activity on magnetic properties, Stoner's paper was clear, well presented and based on the essential distinction between inner and outer shells for the paramagnetic electrons. Brunetti's paper on the other hand, was less brilliant. Brunetti's paper has been rapidly forgotten. Also this audience is p robably familiar with Stoner's contribution but unaware of Brunetti's work. The publication of the paper in "I Rendiconti" certainly limited its diffusion abroad. However, what about Italy? Apart from the memorial paper written by Zaira Ollano after Brunetti's death in 1942 ([27]), as far as we know, there are no Italian quotations of Brunetti's work. Polvani too, in his paper dedicated to a century of Italian Physics (1839-1939) ([28]), quoted Brunetti's work on Ce^{3+} ions we referred to above, but did not say a word on her more important contribution on the quenching of angular momentum. If we would to describe this case in a few words, we should have to speak of isolation into the apartness.

III) Electri cal properties of metallic films

In a paper published in the Physical Review in 1898, Miss Stone ([29]) reported that electrical resistivity of thin silver films is larger than that of metal samples of normal dimensions and that resistivity becomes very large if the film is thin enough. Stone's work received great attention by physicists, mainly experimental; in a few decades the literature on the argument amounted to hundreds of papers.

In 1901, J.J. Thomson put forward the hypothesis that the increased resistivity was to be connected with the reduction of the electron mean free path due to surface reflections ([30]). However, it was soon found that the thickness dependence of the mean free path predicted by Thomson well known formula for diffusive reflections was too weak for very thin films. In 1910 L. Houllevigue ([31]) suggested that very thin films are made up of grains and that electrical conduction between two grains takes place only when they come into contact. In 1917, R.W. King ([32]) tried to put the granular

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[24] "*Non escludiamo che per particolare distribuzione degli ioni nella molecola si possa avere un contributo più grande o più piccolo di (1/3)L(L+1) da parte delle orbite elettroniche*".

[25] J.H. Van Vleck, "Electric and magnetic susceptibilities", Oxford, 1932.

[26] A. Sommerfeld, in "Le Magnetisme - Rapports et discussions du VI-ème Conseil de Physique tenu à Bruxelles du 20 au 25 octobre 1930 sous les auspices de l'Institut Intemationale de Pyhisique Solvay, pp 17-19, Paris, 1932.

[27] Z. Ollano, Il Nuovo Cim., 19, 213 (1942).

[28] G. Polvani, "*Il contributo italiano al progresso della fisica, negli ultimi cento anni*", in "*Un secolo di progresso scientifico italiano: 1839-1939*", pp 555-699, Roma, 1939.

[29] I. Stone, Phys. Rev. 6, 1 (1898).

[30] J.J. Thomson, Cambr. Phil. Proc. 11, 120 (1901).

[31] L. Houllevigue, Compt. Rend. 150, 1237, (1910).

[32] R.W. King, Phys. Rev. 10, 291 (1917).

hypothesis on a quantitative basis, assuming random grain distribution during the film deposition and introducing some simplifying assumptions.

We will not go on discussing the various modifications of the granular hypothesis. Only one comment. The striking feature of thin films' story before the second world war is that the two hypothesis, mean free path reduction and granular constitution, had been largely viewed as mutually exclusive. It is not easy to understand why. Maybe that the almost despairing discrepancies between experimental data obtained by different authors had induced a diffuse mutual distrust among experimentalists. Then, it was perhaps easier than normally it is, to discard others' data than to put under discussion our own hypothesis. Only in the thirties, the awareness of the many deceitful pitfalls waiting for the unsuspicious thin film researcher began to diffuse and first reproducible data obtained in high vacuum began to appear. So, all the data accumulated before (and many of those obtained in the thirties too), are characterized by not sufficiently controlled experimental conditions and therefore appear to us as worthless of any explanation effort.

Italian contribution in this field is mainly related to the name of Eligio Perucca, then in Torino. Perucca began to work on thin films along with but independently of Pierucci ([33]) during the late twenties and ended about ten years later. With respect to the best experimental conditions and facilities available abroad, Italian researchers in this field suffered heavy drawbacks. Pierucci, experimented with the deposition of a tungsten filament of a worned out bulb. Perucca, in his review paper ([34]) presented at the bicentenary celebration of Galvani's birth held in 1937 in Bologna, acknowledged with a tenous vein of sadness the impossibility for his laboratory of keeping the pace of the necessary technological innovations. Therefore, you will be no surprised in hearing that the results obtained by Italian researchers in this field have not been so good as those obtained abroad, in spite of the experimental skill of Perucca. Our main interest lies, in this case too, in a particular hypothesis assumed for explaining the experimental data. Perucca believed, during about all the period of his work in the field, that the increased resistivity of films could not be explained either by mean free path or granular constitution theories. Therefore, already in his first paper published in 1930 ([35]), Perucca suggested that

> "*...metallic films a few atomic diameter thick are not conductive, that is they do not contain conduction electrons; a metal has normally a superficial layer which is metallic but not conductive. Metallic films are insulators till they become twice thicker than the insulating superficial layer..... The hypothesis is certainly audacious and forces us to consider from a new viewpoint an entire set of fundamental phenomena*".

Perucca did not worry about the plausibility of his hypothesis with respect to the available theoretical background nor did he make any effort to give it an independent theoretical foundation. He maintained his hypothesis without substantial modifications almost till the end of his work on metallic films. Only in 1937, in the review paper already cited, Perucca began to assume a more cautious stand with respect to his "audacious" proposal. In his last paper on the subject, published in 1938, Perucca, spurred by his new experimental results, revised his position towards the granular hypothesis (which he had considered untenable), and wrote in the abstract:

> "*.....The consequent strengthening of granular hypothesis suggests to consider premature every other attempt of pure theoretical interpretations of the anomalies (electron free path reduction; deficiency of conduction electrons) till experiments on metallic films of well defined crystalline structure, possibly monocrystalline, could be carried out*" ([36]).

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[33] Probably the two began to work on metallic films more or less in the same period. However, the first published report is that of Pierucci, Rend. Acc. Lincei, 7, 400 (1928).

[34] E. Perucca, Nuovo Cim. 11, 531 (1937).

[35] "*.....pellicole metalliche dello spessore di pochi diametri atomici non sono conduttrici, cioè non contengono elettroni di conduzione; un metallo è normalmente fornito di una pellicola superficiale metallica non conduttrice. Le pellicole metalliche sono isolanti finchè non raggiungono uno spessore doppio di quello che compete allo strato superficiale isolante.....L'ipotesi è certamente ardita, ed essa obbliga a considerare sotto un punto di vista nuovo tutta una categoria di fenomeni fondamentali*." E. Perucca, Nuovo Cim. 7, 50 (1930).

[36] "*.....Il conseguente consolidarsi della teoria granulare induce a ritenere prematuro ogni altro tentativo di interpretazione teorica pura delle anomalie (riduzione del cammino libero degli elettroni; deficiente numero di*

Was it a suspended judgment or an elegant way to give up? Perhaps a mixture of both.
Perucca's case reminds in some way that of Corbino. But the differences are many. We will touch upon only two. The relations between Italian and international physics community, though the same from the basic viewpoint of centre-periphery interaction, have changed at least in one relevant aspect. The Italian physicists who, like Perucca, were working on up to date topics, had, from Corbino's times, changed the audience which now was primarily the international one. This change is clearly reflected in the references network of Italian papers and was already noticeable in Brunetti's articles we talked about before. Another hint is given by the fact that in Perucca's case the criticism to his hypothesis came, as far as we know, only from abroad and was promptly answered by Perucca. We have not so deep a knowledge of the Italian physics community for saying with certainty when this important change took place. However, we believe that the period can be reasonably placed in the late twenties ([37]).
There is another important difference between Corbino's and Perucca's case. Corbino did know and master Lorentz's theory of metallic conduction: simply he decided, and, as we have seen, he was allowed to do so, to ignore it. Perucca instead had probably some problem in dealing with the new born band theory of solids and in evaluating his hypothesis with respect to it. However, both cases are but different manifestations in different situations of what we have called the apartness of Italian physics community.
This reminds us of the fate of solid state physics in Italy during the thirties. In the first four decades of our century, the fraction of papers published in "Il Nuovo Cimento" and devoted to the physics of condensed matter (including liquids, but by far, mainly solids) is about eighteen per cent (1). However, to this production does not correspond a constant quality level. In the thirties, particularly the late ones, the quality crisis is evident. Two may have been the main reasons. The first was related to the already recalled difficulty of the Italian physics community of promptly assimilating the new developments within the discipline, in this case, mainly quantum mechanics and its application to crystals. The second may have been the attraction of nuclear physics so brilliantly advertised by the successes of Fermi's group.
By the way, those were the years in which the physics of solids as a definite subfield began to emerge in Germany, England and United States on the wave of the unifying theoretical approach provided by quantum mechanics. Now it should be clear to us why Italy missed the appointment.

elettroni di conduzione) finchè non si sperimenti su pellicole metalliche a struttura cristallina ben definita, possibilmente a struttura monocristallina." E. Perucca, Nuovo Cim. 17, 365 (1938).

[37] The relationship between Italian physics community and the international one was clearly more complex than it may appear from our report. To be more precise, at least on one point. Also for Corbino, from the scientific point of view, the audience was primarily the international one. It was in fact the international community the one which used, discussed, improved, extended and explained in the framework of the monistic theory Corbino's effect. However, the evaluation of Corbino's work and achievements made by the Italian community was largely independent from the international context but essentially based on the national one. To some degree this was also true in Perucca's time. But while Corbino's papers were clearly written for the Italian audience, Perucca wrote with the international community as a reference point.