Fantappiè's "final relativity" and deformations of Lie algebras

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Abstract The rigidity of the real semisimple Lie algebra $\mathfrak{so}(4, 1)$ was first proved in a brief paper published by Fantappiè in 1954 (Rend. Accad. Lincei Ser. VIII 17:158–165, 1954). The purpose of this note is to provide some historical context for this work and discuss why no further developments of this result were pursued by Italian mathematicians at the time.

1 Biographical notes on Fantappiè

Luigi Fantappiè was born in Viterbo in 1901. He studied mathematics in Pisa, at the Scuola Normale Superiore, where he graduated under the direction of L. Bianchi in 1922. Under the guidance of Bianchi his first mathematical research was on algebraic topics, namely directed to the study of Riemann zeta function (Fantappiè 1973, vol. I). Soon after graduation he moved to Rome, where he was in direct contact with two of the leading mathematicians of the time: Francesco Severi and Vito Volterra. Severi was, at that time, the rising star of Italian mathematics. He had just moved from Padua to Rome and despite the fact that he was a geometer he was appointed as professor of analysis. His reputation was undisputed and, after one year he was elected Dean of the Università "La Sapienza". Fantappiè was offered a position as assistant to Severi, a role that would imply taking notes of his lectures and implicitly following his advice. At the same time, however, Fantappiè had a chance to contact Vito Volterra, perhaps the

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most internationally renowned Italian mathematician in those years, though already in the second half of his career. Among his many and widespread scientific interests Volterra pioneered functional analysis in Italy. It was the outcome of such different interactions that stimulated Fantappiè to study a new class of functionals, generalizing previous works by Volterra from the real to the complex case. Beginning in 1924, Fantappié defined *analytic functionals* and relatively rapidly his contributions to this field were recognized internationally (see Lützen 1982; Struppa 1987 for a historical overview of this theory). As a result of his efforts he was promoted professor and appointed successively to Cagliari, Palermo, Florence and Bologna.

In the ensuing years his relationship with each of his mentors diverged. Volterra considered Fantappiè *his prize pupil*, as Weil recalls (Weil 1992, p. 48). However, they had conflicting political opinions. While Fantappiè was a fervent Fascist from his arrival in Rome, Volterra was one of the leading figures of opposition to the regime. In 1932 Volterra was dismissed from the University for his refusal to take the oath of loyalty to the Fascist government. This was not enough to close scientific contacts between the two men, which were, however, definitely interrupted after the anti-Jewish legislation in 1938; Fantappiè welcomed the new law in front of the old Jewish professor. Conversely his relation with Severi grew stronger. Severi became the leading mathematician in Italy and the one with direct connections with the Government.

Fantappié was an ardent supporter of Severi, and they developed a close friendship strengthened by their sharing similar political, religious and scientific viewpoints. In 1933 Fantappié accepted a position as foundation professor of mathematics at a new University in Saô Paulo, Brazil. His mission was both to establish the new mathematics Department and to strengthen relations with the large expatriate Italian community, a mission not devoid of political import. He remained in Brazil from 1933 to 1939,[1] founding the local mathematical newspaper, guiding the more promising students in research centered around analytic functionals and promoting the ideas of the Fascist regime in meetings and conferences throughout the country. In 1939 he left Brazil and went back to Rome where he was offered the Chair of Higher Analysis and was appointed vice president of the newly founded National Institute of Higher Mathematics (INDAM), directed by his former mentor Severi. During WWII he often travelled to Spain, where he developed a close connection with the University of Catalunya, and Portugal, and began enlarging his research interests. Starting in 1942 he proposed a mainly philosophical theory centered on the concept of *sintropy* which, in his intentions, should connect physics and biology and explain "finalistic phenomena". In later years he also proposed a "mathematical" proof of the existence of God and devoted some attention (in conferences and in published papers) to parapsychological phenomena (see Fantappiè 1993). Apart from these unconventional interests, from the late 1940s, his attention was drawn to the role of topological groups in physical and mathematical theories: he lectured on topological groups at INDAM and published papers centered on the role of symmetries in relativity theory. This was to be his last mathematical interest; in the summer of 1956 he suffered a fatal heart attack. Some

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[1] See Zornoff Táboas (2008) for a detailed description of Fantappiè's influences on Brazilian mathematics.

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additional details on his life can be found in the numerous obituaries (a quite complete one is Fichera 1957) and in the introduction to the first volume of his collected works Fantappiè (1973).

2 Fantappiè's final relativity

In looking at Fantappiè's list of scientific publications (as, for example, recorded in Fantappiè 1973), one sees a distinct gap between 1943 and 1948. This gap may not be immediately apparent since it does not consist of a complete lack of new publications. All his works, in those years, were not mathematical but focused on the development of *sintropy* with scientific considerations showing up just in the background.

The theory of analytic functionals which had established his reputation was largely put aside. The problem of extending analytic functionals to more variables could not be easily solved by the techniques at hand in his school (as is nicely explained by Struppa in Struppa 1987). However, he did acknowledge it in lecture notes for a course in Barcelona in 1948.

There were also external reasons, both social and personal, for this interruption. During 1943 Rome, the city where he was living, experienced the worst years of the WWII period. Despite propaganda to the contrary, it was clear by that time that the Axis powers had lost and in July that year the northward Allied advance had begun. Also, in July, the Allied bombing of Rome commenced with approximately 3000 casualties in a single day. It was going to be but the first of more than 50 bombings on the capital, in subsequent months. At the time, Fantappiè was in his villa just outside Viterbo, his birthplace, only 90 km from Rome; it is in fact reported that he later missed one of the INDAM's meetings due to difficulties in reaching Rome. Viterbo was also bombed toward the end of 1943. At the end of July the Fascist government was overthrown and Mussolini arrested. This marked the start of a long and difficult period of social unrest. By the beginning of September the new Italian government switched allegiance to the Allies and German military forces occupied the city. In October 1943, Nazis raided the Jewish ghetto in Rome, deporting more than 1.000 in concentration camps; all Italian young males unwilling to take arms against the Allies had to hide to escape immediate execution. It is no surprise that under such exceptional circumstances the University was essentially closed. Even after Rome's liberation on the fifth of June 1944 the situation did not get any better. Severi, a prominent symbol of deposed Fascist regime and Fantappié's mentor and personal friend, was suspended from all his University positions (Goodstein and Babbit 2012). Despite Fantappiè's outspoken Fascist support, there is little information he was similarly sanctioned. However, he was burdened by the additional role of INDAM's vice presidency until 1948 (though, according to Roghi (2005), Severi got back to his duties, not officially, already in 1946).

On a personal level, his life went through major changes as well. The death of his beloved mother, with whom he was living, was followed quite closely by his marriage with Maria Quadrani, a family friend, in 1946.

Be it as it may, whether social, personal or scientific reasons prevailed in such turbulent years, no new papers on pure mathematics were published by Fantappiè, in those five years. A new interest, however, arose: the role of symmetries in physical systems, with a special emphasis on relativity theory. In 1949 a note (Fantappiè 1949) appeared on *Costruzione effettiva dei prodotti funzionali relativisticamente invarianti.*[2] This was the prelude to an almost complete change of direction in his scientific interest. Though he kept on assisting former students and young colleagues, in Rome as well as in Barcelona and Saô Paulo, in their efforts to further develop his theory of analytic functionals, his own research focused on the role of symmetries in physics. His last student, the physicist G. Arcidiacono, was the only one to follow him along this path. This change of direction was reflected also in his INDAM lectures (INDAM lectures at that time were analogous to nowadays PhD courses). In the difficult academic year of 1945/1946, the first after the end of WWII, he abandoned analytic functionals, the traditional subject of his course, and lectured on *Mathematical tools of modern physics and Unitary Theory of the Universe.*[3] In 1950/1951 he switched again and, until his death in 1956, his lecture courses were devoted to *Topological Groups and their application to Physics*[4] (Roghi 2005; Fantappiè 1961).

Fantappiè's researches on symmetries in mathematical physics had to face a rather peculiar state of the art.

The history of relativity theory in Italy has been much treated in the literature (e.g., Bergia 2005; Pastrone 1998), and we will only recall some key points. After a very intense debate in the 1920s between a relativistic and an anti-relativistic side, by the 1930s the situation was settled in favor of the first. This was not completely independent of the fact that the work of Levi-Civita was in favor of relativity theory, and he had a great scientific reputation. However, relativity was studied more in mathematics than in physics departments, and after Levi-Civita was discharged from the University as a consequence of racial laws, it started fading also from the attention of Italian mathematicians. Therefore, when Fantappiè undertook his researches in the field, this neglection of the topic was quite evident.

Lie theory was a somewhat analogous situation. It is not too excessive to say that soon after Lie's work Italy became one of the leading nations in pursuing this line of research. Following the work of Cremona and Corrado Segre, who immediately saw how fruitful such theory could be, a number of people contributed relevant results (see Cogliati 2014 for a much detailed description of the earliest reaction to Lie theory, concentrated on infinite-dimensional continous groups). The first name to be mentioned is certainly that of L. Bianchi, whose treatise Bianchi (1918) rapidly became a standard in years to come. It was followed and surrounded by work of Enriques, Fano, Fubini, the already-mentioned Levi-Civita, Medolaghi, Segre and Vivanti, all between the end of the 19th and the beginning of the 20th century. A special mention should be given to the work of the young Eugenio Elia Levi, at the outbreak of the 20th century, that gave him a rapidly growing reputation abruptly interrupted by his death in action in

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[2] An effective construction of relativistically invariant functional products; MR 0044942.

[3] Strumenti matematici della fisica moderna e Teoria Unitaria dell'Universo.

[4] Gruppi topologici e loro applicazioni fisiche.

WWI. But all this attention faded rapidly. When a few years later U. Amaldi published his works concerning infinite transformations groups, he got almost no reaction from the mathematical community ("The work of U. Amaldi [...] left no trace. This should not surprise us if we consider that also the fundamental work of Cartan on finite transformations groups was basically ignored until the 60 s." (Rogora 2008, p. 64)[5]). It was the beginning of a long eclipse, during which Lie theory received a very limited attention in the Italian School of Mathematics. It is significant that in the standard comprehensive book on Italian Mathematics between the two world wars (Brigaglia and Scimone 1998), the name of Lie appears only twice, and always in connection to applications, either in differential geometry, following the work of Bianchi, or in the integrability of mechanical systems.

Thus when Fantappiè undertook his new research, there was scant attention given in Italy to either of these areas. Conversely, there was much activity internationally which had generated a wealth of new results. Fantappiè himself was somewhat aware of this gap and would mention to his students his limited mathematical tools in comparison with those used by some foreign colleagues (I owe this remark to a personal communication with Prof. F. Succi, one of Fantappiè last students).

Fantappiè was guided by the idea of realizing what he himself calls "an Erlangen program for physics", a classification of possible physical theories through their group of symmetries. With these words he ended (Fantappiè 1952, p. 290):

> From the previous, it then follows that, just as for the classification of geometries given by Klein in his Erlangen's program, also for possible "physics", or better "physical universes" as above specified, the most natural possible classification is the one in terms of the fundamental groups used to define "equality". Let us remark that here the term "fundamental group" has to be understood as maximal group of symmetries of the physical system.[6]

The specific purpose of this first paper was to classify linear operators invariant under coordinate transformations. Though he lacked a description in terms of group representations and despite not clarifying on which function spaces such operators should act, he was moving toward the same goals as one can find in the contemporary works of Segal (among many others) that we will review below.

In this Erlangen-kind of perspective the passage from classical mechanics to relativity theory can be interpreted as a change in the underlying stability group of the theory. As already explained in Minkowski (1909), while classical mechanics is invariant under the action of the 10-dimensional Galilei group, special relativity is invariant under the group of Poincaré transformations.[7] The former group can be recovered

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[5] Il lavoro di U. Amaldi [...] passò praticamente inosservato. La cosa non deve stupire se si considera che anche gli importantissimi lavori di Cartan sui gruppi continui finiti non furono praticamente letti fino agli anni '60.

[6] "Da quanto precede, infine, segue allora che, come già per la classificazione delle geometrie data da Klein nel suo programma di Erlangen, anche per le "fisiche" possibili, o meglio per gli "universi fisici" possibili, sopra specificati, la classificazione più naturale che si può fare è proprio quella che si ottiene *in base al gruppo fondamentale, che serve a definire l'uguaglianza.*"

[7] Also referred to as the group of inhomogeneous Lorentz transformations.

from the latter as a limit letting a certain parameter c, naturally interpreted in physics as the speed of light, go to $+\infty$.

It is quite natural to ask, at this point, whether this procedure can be iterated, i.e., if the Poincaré group can be replaced by other group of symmetries of which it is the limit, if this extension is unique, if it is further deformable and, finally, when this procedure will stop. In fact already in (Fantappiè 1952, p. 288), it was remarked that:

> It may happen that the 10-parameters Lorentz group, which will be denoted by Lz_{3+1}^{10}, in the evolution of our scientific knowledge, will face an analogous fate as the 10-parameters Galilei group Gl_{3+1}^{10} of classical physics and will end up being replaced by a more general group, like the group of isometries Ds_{3+1}^{10} of De Sitter space-time. This group, in fact has Lorentz group as limit case, when curvature of space-time goes to 0, as well as Lorentz group gives as classical limit Galilei group when the speed limit is sent to ∞".[8]

He postponed details of this limit procedure to a future publication.

This question is, in fact, described by Freeman Dyson as part of one of the "missed opportunities" listed in his famous Gibbs' Lecture (Dyson 1972). In his opinion the reason for such missed opportunities was that physicists and mathematicians were "neglecting to talk to each other", and in the specific case Dyson stresses the missed opportunity to foresee an argument in favor of cosmological expansion. To be honest, it would be rather anachronistic to expect an approach like the one of Fantappiè at the beginning of the 20th century, as Dyson advocates. De Sitter's work is basically contemporary with the works of Elie Cartan on the classification of real semisimple Lie algebras (Cartan 1914) and the idea that such Lie algebras could somehow be generic in the set of all Lie algebra laws was yet to come. The missed opportunity that we would rather concentrate on is on how Fantappiè missed the chance to anticipate the theory of Lie group contractions and deformations, an idea for which, as we will see in the next paragraphs, times were ripe at the beginning of the 1950s.

In the 1954 paper (Fantappiè 1954), Fantappiè proved that the inhomogeneous Lorentz group is, truly, a limit of the De Sitter group, which we would nowadays simply call the pseudo-orthogonal group $O(4, 1)$. This proof is the mathematical core of his communication. He then proceeds to outline the basic ideas of a new relativity theory, which he calls *final relativity*, and to which he will devote some of his residual energies in one of his last published works (Fantappiè 1955, p. 158) emphasis as in the original):

> And in this Note we will show, indeed, that the Lorentz group is just the *limit case* of "another group", continuously depending on a parameter R, when $R = \infty$.

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[8] "Può accadere che il gruppo di Lorentz, che indicheremo con Lz_{3+1}^{10} (a 10 parametri) nella progressiva evoluzione delle nostre conoscenze scientifiche, finisca col subire una sorte analoga a quella del gruppo di Galileo Gl_{3+1}^{10} (pure a 10 parametri) della fisica classica, e cioè finisca con l'essere sostituito da un altro gruppo più generale, per esempio dal gruppo Ds_{3+1}^{10} dei movimenti in sè del cronotopo di De Sitter, che dà proprio il gruppo di Lorentz come caso limite, quando il raggio di curvatura del cronotopo tende all'∞, così come il gruppo di Lorentz dà come caso limite il gruppo di Galileo, quando la velocità della luce si fa tendere all'∞."

Since we will show that this other group *cannot be the limit* of a different group, we will call this new group the *final group*, and we will denote it with Fn_{3+1}^{10}.[9]

It has to be said that, from a physical viewpoint, his efforts did not generate results that matched his expectations. Despite the fact that his last student, Arcidiacono, devoted most of his scientific career to the development and promotion of Fantappiè's final relativity (see Arcidiacono 1988, and the bibliography therein), his construction was at best ignored and in some case openly criticized. A reflection of the rebuttal by the scientific community can be found in the increasingly sceptical reviews Arcidiacono's papers received in Mathematical Reviews.

It is not, however, our aim here to judge whether the physical theory of final relativity received the attention it deserved. Our point here is to clarify to what extent and why also Fantappiè (and his students) missed a substantial mathematical opportunity, namely that of pioneering the theory of Lie group and Lie algebra deformations which was being developed at the same time in the USA.

3 Fantappiè's rigidity proof

In this section, we will describe in more detail the content of Fantappiè (1954). In particular, we are interested in how Fantappiè proved that the Lorentz group is the limit of a semisimple real Lie group which is not further deformable. This last result, in particular, is what we now term a rigidity result and would be proved with rather different techniques and considered to be standard, while, at the time, could have led to very interesting new speculations.

We were referring to "Lie group deformations," but, to tell the truth, Fantappiè's first deformation result concerns more the action on a specific homogeneous space than the group itself. Starting from the Lorentz group Lz_{3+1}^{10} together with its action as group of isometries of the flat Minkowski space $\mathbb{R}_{3+1}^4$, he sought for a group G converging, in some limit, to Lz_{3+1}^{10} and acting on a manifold M. Such manifold was required to fulfill the following:

1. dim $M = 4$;
2. $G = \text{Iso}(M)$ has dimension 10;
3. M is a semi-Riemannian manifold of signature $3 + 1$.

It has to be mentioned that in Fantappiè (1954), there is no explicit explanation of the meaning of one group being the limit of another one; it is just intended that there will be a group $G = G(R)$ depending on some parameter R such that when $R = \infty$ then $G(\infty) = Lz_{3+1}^{10}$. We will further comment on this point later on.

The reasons for asking for the deformed "cronotope" M to verify the above requests is explained, in the above-mentioned paper at page 159, as follows:

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[9] *E in questa Nota mostreremo, per l'appunto, che il gruppo di Lorentz è proprio il caso limite di un "altro" gruppo, dipendente con continuità da un parametro R, per R = ∞, e poiché dimostreremo che questo nuovo gruppo non può più essere limite di un altro gruppo diverso, chiameremo questo nuovo gruppo il gruppo finale, e lo indicheremo con Fn_{3+1}^{10}.*

Now, the most certain information, valid in the most general case, is clearly what, referring to *integer numbers* (which cannot vary continuously) should coincide with the known ones, in the limit case.[10]

This statement, given with no clarification on the topologies involved, may look at first somewhat vague. Though it makes perfect sense, since, no matter what topology is put on some topological space of cronotopes, whatever it may be, functions from such space to $\mathbb{Z}$ takes constant values on continuous paths. We just remark that no effort is made throughout this paper and subsequent works, on clarifying what a *space of cronotopes* could be.

The necessity of looking together at G and M (but the same result would hold true without this request) is imposed by the fact that the author choses to rely on a much older result commonly credited to Bianchi,[11] namely that if dim $M = 4$ and dim Iso(M) = 10 then M has constant curvature C. Having said so, Fantappiè utilized the classification of such spaces up to isometries to prove that the requirement on the signature of the semi-Riemannian structure forces M to be a sphere of radius R (where, obviously, $R = 1/C^2$) and that in the limit $R \to \infty$, the metric on this spheric cronotope reduces to the usual one on the flat Minkowski space $\mathbb{R}^4_{3+1}$. At this point he only needed to compute Iso(M), which he calls the final group Fn^{10}_{3+1}, and to show that $\text{Fn}^{10}_{3+1} \simeq O(4, 1)$ as a topological (in fact Lie) group.[12]

The second part of the note is centered on the proof of the fact that Fn^{10}_{3+1} cannot be further deformed, and is opened by some considerations about Fn^{10}_{3+1} being simple. At this point (page 164) the argument goes as follows:

We can remark that the existence of an invariant subgroup translates into vanishing of some structural constants and, therefore, simplicity translates into the fact that some such constants should be nonzero.[13] If a simple group is a limit of another group, with the same number of parameters, i.e., can be obtained by another depending continuously on a variable α for $\alpha \to \alpha_0$, then its structural constants will also depend continuously on α, and therefore, all those that are nonzero for $\alpha = \alpha_0$ will remain $\neq 0$ for α in a neighborhood of α_0. In particular, if the limit group is simple, the variable group should also be simple for α close to α_0; in other words a simple group can only be limit of simple groups.[14]

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[10] Ora, le informazioni più sicure, che si possono avere nel caso più generale, sono evidentemente quelle che, riferendosi a *caratteristiche espresse da numeri interi* (le quali non possono variare con continuità) debbono coincidere con quelle note, del caso limite.

[11] It is maybe worthwile to recall here that, as mentioned, Bianchi was Fantappiè's thesis advisor and most probably his main source of informations for anything concerning differential geometry.

[12] The choice of giving a special notation like Fn^{10}_{3+1} for what is nothing but a pseudo-orthogonal group is not completely clear. On one hand, it is obvious that Fn stands for final. On the other hand since Lz stands for Lorentz and Ga for Galilei, it is difficult not to guess that Fn could stand also for Fantappiè and to ask oneself whether such ambiguity is intended.

[13] An invariant subgroup H in G identifies, via integration, a Lie ideal $\mathfrak{h} \subseteq \mathfrak{g}$. By choosing a suitable basis of $\mathfrak{g}$, one can indeed transform the ideal condition $[x, h] \in \mathfrak{h}, \forall h \in \mathfrak{h}, \forall x \in \mathfrak{g}$ into vanishing of a subset of structural constants with respect to this basis.

[14] Si può osservare infatti che la presenza di un sottogruppo invariante si traduce nell'annullarsi di varie costanti di struttura e quindi la *semplicità* del gruppo si traduce nel fatto che almeno alcune di queste costanti

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This argument is considered to hold for complex simple groups and implies rigidity as a consequence of the classification of complex simple Lie groups: no other complex simple Lie groups in dimension 10 exists. It may be considered of some interest that Fantappiè does not refer to Cartan's original paper but to his later work describing compact forms of complex simple Lie groups (Cartan 1929). He then looks at the same argument from a real point of view, recalling that $O(5)$, $O(4, 1)$ and $O(3, 2)$ are the only real simple Lie groups of the right dimension, and remarking that one of these cannot be deformed into another (but about this apparently innocent statement see the quote that ends this section).

Let us now comment on such proofs.

As for the first part, it has to be noted that the fact that Lz_{3+1}^{10} is deformed by Fn_{3+1}^{10} is proved by Fantappiè's by differential geometric techniques (in a way his deformation takes place in the category of isometry groups of semi-Riemannian manifolds), though it is possible to prove it by purely algebraic techniques. And, in fact, as we shall see in the next paragraph, the existence of such deformation was considered to be quite obvious by contemporaries. Unicity, which is less trivial, was proved by algebraic means by Wigner's student W.T. Sharp in his Ph.D. thesis at Princeton (1960), as credited by Lévy-Nahas (1967). It is difficult to say whether Lévy-Nahas knew Fantappiè's paper (published in Italian on a not so widely known journal).

As for the proof of rigidity of Fn_{3+1}^{10} (which, we recall, is nothing but the real semi-simple Lie group $O(4, 1)$) his argument is at the same time convincing and specious. Truly, he centers the main point: semisimplicity is an open condition and thus remains unchanged under small perturbations. However, his argument does not meet modern expectations for rigor. The fact that he is talking about structural constants seems to suggest that he is considering Lie algebras rather than Lie groups, a distinction he never made explicit, thus somewhat obscuring his reasoning. In this respect, his work looks closer to papers on Lie theory at the start of the 20th century, regardless as they are of worries about global considerations. This despite the fact that in the meanwhile, mainly on the basis of H. Weyl's work, topology had entered Lie theory in a substantial way, as clearly explained in Hawkins (2000). While Fantappiè did acknowledge that he had such a distinction in mind, although not clearly expressed, it has to be noticed that the vanishing of some structural constants is strongly dependent, at the Lie algebra level, on the choice of a preferred basis, and since he makes no comments on dependence on the choice of this basis, nor on the choice of a canonical one, his argument cannot be regarded as completely conclusive. It has to be remarked that, if correct, this argument would work for a large class of real semisimple Lie groups. Fantappiè, however, does not seem to be interested in extending the range of his results beyond this specific case.

Footnote 14 continued

debbono essere *diverse da zero*. Se quindi un gruppo semplice è *limite* di un altro gruppo, con lo stesso numero di parametri, si ottiene cioé da un altro, dipendente con continuità da una variabile α per $\alpha \to \alpha_0$, anche le costanti di struttura di questo dipenderanno con continuitá da α, e quindi tutte quelle che sono diverse da 0 per $\alpha = \alpha_0$, resteranno pure $\neq 0$ per α abbastanza prossimo ad α_0. In particolare, dunque, se il gruppo limite è *semplice*, anche il gruppo variabile deve essere *semplice* per α vicino ad α_0, cioé *un gruppo semplice non può essere limite che di un gruppo pure semplice*.

In modern texts, the rigidity of real semisimple Lie algebras is proved by vanishing theorems for Lie algebra cohomology. Even if the Whitehead's lemmas were already known at the time (the original Chevalley–Eilenberg paper dates back to 1948), their link with rigidity was not still understood and would not be for some years (Nijenhuis and Richardson 1964; Gerstenhaber 1964). Still it is possible that a rigorous proof following Fantappiè initial suggestion, i.e., avoiding cohomological arguments, could be given already at the time. It is interesting to note the following remark by Goze and Lütz remark about the chance of proving rigidity of semisimple Lie algebras by direct algebraic computations in (Goze and Lutz 1981, p. 145):

> If you ask some specialists on Lie algebras about this question, he first will tell you that rigidity of semisimple Lie algebras is an evidence, since they are classified: for every n, there is only a finite number of non isomorphic semisimple structures on $\mathbb{C}^n$; hence it is clear, due to continuity, that the only possible limit points are in the class itself. Then you wonder about continuity versus finiteness; naively you think that you could approach μ_0[15] jumping from one class to another. You claim for some details. . . At this point you get the following precisions:
> (i) everybody knows that any Lie algebra with $H^2(\mathfrak{g}, \mathfrak{g}) =$ is rigid;
> (ii) it is well-known that semi-simple Lie algebras satisfy $H^2(\mathfrak{g}, \mathfrak{g}) = 0$, hence are rigid.
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> This continuity versus cohomology is not an evidence for nonalgebraic-minded people, of course. If you throw an eye in the literature you discover that (i) is an important result of Nijenhuis and Richardson and that (ii) is a complicated consequence of the existence of Weyl basis using Whitehead Lemmas or alternatively some computations on spectral sequences. . .On the whole algebraic evidence, isn't it?

4 Comparison with Segal and Inönü–Wigner

We will now consider two related and contemporary works using approximately the same arguments that are usually credited as the starting point for the deformation (and contraction) theory of Lie algebras. It has to be noticed that they both predate the work of Fantappiè by a few years; assigning priority is not a major concern. It is to be assumed that during and immediately after the war scientific communication was not at its best. Switching research topic under such circumstances may quite well result in a limited knowledge of the relevant literature. We have no reason to believe that Fantappiè knew such papers, we rather have an indication of the opposite; a collection of reprints he owned is preserved in Ferrara library, and there is no mention of them (Gambini and Pepe). Rather than discussing priorities, we will focus on differences between these works and the one of by Fantappiè.

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[15] μ_0 Is the semisimple Lie algebra structure whose rigidity we would like to show.

4.1 The work of I.E. Segal on contractions

The first paper we consider is by Irving Ezra Segal (1951), at that time a very young professor in mathematical physics at the University of Chicago. The work was described, in an obituary (Baez et al. 1999) *a wide-ranging article* covering a very large array of algebraic and analytical approaches to quantum field theory.

The focus of this work is to approach physical systems from an operator algebra point of view, which keeps track of symmetries. Observables are identified inside algebras of operators on Hilbert space (or irreducible representations of C^*-algebras). The group of symmetries appears in determining such algebras; observables are generators of unitary representations in bounded or unbounded operators. The largest part of the paper consists of a very careful analysis of a number of technical problems which are encountered when representing locally compact groups (or rather their group C^*-algebra) on Hilbert spaces, and when confronting continuous and smooth vectors under such representations.[16] On page 255 Segal clearly states that he wants to clarify in what sense "one physical theory is a limiting case of the other", a situation that, in his words, from the point of view of operator algebras "is quite difficult to define precisely". This difficulty suggests to him to restrict considerations to deformations of Lie algebras. In a very modern approach (which was rediscovered 20 years later), he considers what we nowadays call the algebraic variety of Lie algebra laws and declares deformations to be continuous paths on such a variety. He furthermore proves that any compact Lie algebra is rigid, i.e., it does not admit a nontrivial deformation; in his words "the following proof of this fact makes strong use of a suggestion of Harish Chandra". As for the more general case that includes the noncompact real semisimple Lie algebra $\mathfrak{so}(4, 1)$ analyzed by Fantappiè, he says (Segal 1951, p. 257):

> It seems plausible that the result just obtained should be valid without the assumption of compactness, and it can be seen from Cartan's classification of the real simple groups (by the method we use later in connection with the conformal group) that this is true in many particular cases. However, we know of no proof of the general result.

From this citation it is evident that he was well aware that the rigidity of real semisimple Lie groups was an interesting and open problem. More than once in his papers and commentaries in the years to follow he expressed his regret for the fact that the starting idea of deformations of Lie algebras was credited mostly to the work of Inönü and Wigner, omitting mention of his 1952 paper. As an aside let us remark that in this work not only does he consider deformations of Lie algebras but also more general deformations of a *compound system*, which consists of a locally compact topological space M, a continuous action of a locally compact group G on Ms and a G-invariant measure on M; a compound system is a topological analog of the set of data Fantappiè is deforming to analyze his *cronotope*, cleared of the differential data.

In the last part of his paper Segal proceeds to consider a specific situation quite close to the one in Fantappiè (1952). The rigid group of symmetries he ends up with

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[16] It is no surprise the theory of smooth vectors in unitary irreps will be definitely set up by Eward Nelson, one of the first Ph.D.'s students of Segal.

is the 15-dimensional conformal group $SO(4, 2)$. Considering its natural action on the 6-dimensional space is one way to treat at the same time the De Sitter, anti De Sitter and Poincaré group actions on suitable Minkowski space-times, and thus realize Fantappiè's deformation as a true limit inside a wider Lie group. Segal shows that $SO(4, 2)$ is not further deformable and sets the foundations of its application to physics, in a program to clarify some aspects of standard special relativity. It is reasonable to think that since he was able to prove rigidity of $SO(4, 2)$, the rigidity of $SO(4, 1)$ was also clear to him, but no explicit mention of this fact appears in his paper.

Most of Segal's work, in later years, was devoted to the development of a new relativity theory based on the conformal group (in fact its universal cover), much as Arcidiacono devoted its life to building up in details *final relativity* (Arcidiacono 1988). It is possible that Fantappiè's work and its later developments could be included as a special case in this wide-ranging theory. However, as Nelson states (Baez et al. 1999, p. 661):

> Segal's work on the Einstein universe as the arena for cosmology and particle physics is a vast unfinished edifice, constructed with a handful of collaborators.

A fate is somewhat curiously shared with Fantappiè's final relativity.

4.2 The work of Inönü–Wigner on Lie group contractions

Let us consider now some comments on (Inönü and Wigner 1952), a paper which had significant impact on mathematical physics, as evidenced by the 54 citations censed in Mathscinet from 1985 onwards. The introduction (p. 510) starts as follows:

> Classical mechanics is a limiting case of relativistic mechanichs. Hence the group of the former, the Galilei group, must be in some sense a limiting case of the relativistic mechanics' group, the representations of the former must be limiting cases of the latter's representations. There are other examples for similar relations between groups. Thus the inhomogeneous Lorentz group must be, in the same sense, a limiting case of the de Sitter groups.[17]

Thus the initial intuition of Fantappiè (1952), for what concerns the existence of the deformation, is stated, immediately, as a fact (as we saw it goes back to Minkowski itself). In fact such an idea was folklore in US mathematical physics of the time (e.g., "If this group[18] is regarded as the group of motions of a flat three plus one dimensional Riemannian space, it may be regarded as the limit for zero curvature of the group of motions of De Sitter space" in (Thomas 1941, p. 113). The second remark is that their emphasis is shifted from the beginning from Lie groups to their representations (Inönü–Wigner 1953). This is no surprise, since Wigner was one of the pioneers in the use of representation theory techniques in physics. This makes even

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[17] The plural here refers to the fact that from a purely Lie theory point of view the Poincaré group may be reached as a contraction both of SO(4, 1) and SO(3, 2). A possibility that in Fantappiè (1952) is excluded by requirements on the signature of the space-time manifold.

[18] The inhomogeneous Lorentz group.

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more evident how such a concept is never explicitly stated in any Fantappiè's papers. In fact a distinction he made in his lectures between the group and its adjoint (see, for example, the posthumous lecture notes of his INDAM course Fantappiè 1959) could be rephrased in terms of left and right action of the same group on a manifold (and on functions on the manifold); a rephrasing which would give a much easier understanding of some statements.

Going back to Inönü–Wigner, the authors proceed to give a definition of the contraction of Lie groups, which has to be understood as the analog of the otherwise undefined limit in Fantappiè (1954). It is described in terms of structural constants in the Lie algebra, much as by Fantappiè, but with no doubt in more exact terms. Differently from Segal they do not insist on an abstract approach but they give a very clear contraction procedure from an operational point of view. Though such contractions are not as general as Segal's degenerations, as remarked already in the text,[19] they allow very explicit computations in a number of cases. It has to be said that the first joint work of Inönü and Wigner was published in an Italian journal (Inönü and Wigner 1952) exactly in the same years in which Fantappiè was developing his theory, and this paper establishes the source of interest in Lie contractions, as explained by the first author in (Inönü 1997).[20] However, this journal is a journal in physics and it is possible that Fantappiè was unaware of it.

On the other hand, Inönü and Wigner never discuss the more difficult problem of the uniqueness of deformations; in a way, this is reasonable since they take the opposite point of view of contraction. In their approach, a reasonable question could have been the following: given a Lie group G list all its possible contractions. Answering such question would requite a complete classification of all its subgroups: a rather ambitious program. Still it is this work, with its limits, that is nowadays commonly credited as the starting point of Lie algebra deformation theory. It should be no surprise, since Wigner advised many students and coworkers to pursue this avenue, being thoroughly convinced of its fruitfulness in mathematical physics.

5 Conclusions

The work of Fantappiè that we discussed may be considered simply as a partly failed attempt by a mathematician during the end of his career. However, it is clear that his weaknesses, which we emphasized in comparison with the work of Segal and Inönü–Wigner, were not accidental. The *missed opportunity*, in the words of Dyson, was that of foreseeing a fruitful field of study in the direction of Lie algebra deformations

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[19] "The above considerations show a certain similarity with those of I.E. Segal. However, Segal's considerations are more general than ours as he considers a sequence of Lie groups the structure constants of which converge towards the structure constants of a non isomorphic group. In the above, we have considered only one Lie group but have introduced a sequence of coordinate systems therein and investigated the limiting case of these coordinate systems becoming singular. As a result of our problem being more restricted we could arrive at more specific results." (Inönü and Wigner 1952, p. 514).

[20] This historical note was presented for publication in the proceedings of the mentioned Workshop. However, such proceedings were never published. It is possible to find it on the internet at http://ysfine. com/wigner/inonu.

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and their applications to mathematical physics. His initial intuition was certainly not without interesting developments, and his timing was good, as this work was developed at around the same years as the pioneering work of Segal and Inönü–Wigner; even more so if one considers that Fantappiè was, at that time, INDAM's vice president and therefore certainly in a position to have a lasting influence on future research directions in his country.

However, several factors conspired to hinder the success of this initial intuition. First and foremost, of course, Fantappiè's own interest was much more centered on pursuing a new "final relativity theory" than in understanding the mechanism of symmetries deformation. Two factors, probably, contributed to this. A lack of interest in abstract considerations: this could be influenced by the fact that in the central years of his career, abstractness in Italy was viewed negatively as "Jewish mathematics" (as explained at various points in Guerraggio and Nastasi (2005) and, as an example, in the infamous Evola 1940). To elaborate further on this point Fantappiè was not avoiding generalities: he sought to construct a "final relativity" purely on the basis of its symmetries, for example. But he always concentrated on the operational sides of his theories rather than on its foundations, as he did in postponing a functional analytic treatment of analytic functionals.

Furthermore he aimed at a complete theory of physics guided by symmetry which, together with his syntropy theory applied to biological systems, would reflect a complete and rational view of the Universe. The idea of deriving such a grand scheme from few principles followed from his strongly religious views and from his attempts, more and more intense in the last years of his life, to reconcile faith and science; and effort to which he felt almost compelled (see some personal recallings in AA.VV. 1972).

Aside from Fantappiè's personal motivation and beliefs it is important to understand the context and work environment; he worked on his "final relativity" in relative isolation. No other Italian mathematician would share his interests in Lie theory in those years, and the global weakness of the Italian school of algebra, especially for what concerned Lie algebras and their representations, was to remain unchanged for many years to come. For what concerns the more physical aspects of his construction it has to be said that in Italy no one took up the heritage of Levi-Civita, who died in scientific and social isolation, after racial legislation excluded all Jews from teaching in Italian Universities. Thus Fantappiè's work, written in Italian, in Italian journals, and devoted to themes which were not much considered by his local contemporaries, remained substantially underdeveloped. It is easy to think that, if health problems had not prevented Fantappiè from accepting an invitation to visit Princeton in 1951, the situation would have been very different. Segal, who was working at Princeton at the time, had the chance to interact with Harish–Chandra who was laying the foundations of the representation theory of semisimple Lie groups. Wigner, who had established his reputation with the use of representation theoretical arguments in particle physics, was also well positioned for his own students to advance his ideas about group contractions (he met Inönü exactly in 1951). The contrast with the two environments, at that time, could not be sharper.

It is sometimes said that Fantappiè's work was neglected due to his beliefs held in religion, philosophy and politics. It is certainly possible that some of this more extreme attitudes (e.g., interest on parapsychological phenomena) may have alienated

him to an extent. However, it is also clear that internal mathematical reasons can be sufficient to explain why his works remained underdeveloped after his death. More than his personal situation, it was the effect of country's long period of isolation from the international mathematics community that is why after his death, Fantappiè's intuitions about deformations of Lie groups remained one of the "missed opportunities" in the Italian mathematics research of the time.

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