Models of Discovery and Creativity

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Editors

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Foreword

Since the origin of the modern sciences, our views on discovery and creativity had a remarkable history. Originally, discovery was seen as an integral part of methodology and the logic of discovery as algorithmic or nearly algorithmic. During the nineteenth century, conceptions in line with romanticism led to the famous opposition between the context of discovery and the context of justification, culminating in a view that banned discovery from methodology. The revival of the methodological investigation of discovery, which started some thirty years ago, derived its major impetus from historical and sociological studies of the sciences and from developments within cognitive psychology and artificial intelligence.

Today, a large majority of philosophers of science agrees that the classical conception as well as the romantic conception are mistaken. Against the classical conception, it is generally accepted that truly novel discoveries are not the result of simply applying some standardized procedure. Against the romantic conception, it is rejected that discoveries are produced by unstructured flashes of insight.

An especially important result of the contemporary study concerns the availability of (descriptive and normative) models for explaining discoveries and creative processes. Descriptive models mainly aim at explaining the origin of novel products; normative models moreover address the question how rational researchers should proceed when confronted with problems for which a standard procedure is missing.

The present book provides an overview of these models and of the important changes they induced within methodology. As appears from several papers, the methodological study of discovery and creativity led to profound changes in our conceptions of justification and acceptance, of rationality, of scientific change, and of conceptual change.

The book contains contributions from both historians and philosophers of science. All of them, however, are methodological in the contemporary sense of the term. The central values of this methodology are empirical accurateness, clarity and precision, and rationality. The different contributions realize these values by their interdisciplinary nature. Some philosophically oriented papers rely on historical case studies and results from the cognitive sciences, others on recent results from the computer sciences and/or non-standard logics. The historically oriented papers address central philosophical questions and hypotheses.

Acknowledgments

The editors are indebted to the Research Foundation – Flanders and to Gitte Callaert, Bert Leuridan, Friederike Schröder-Pander, and Stephan van der Waart van Gulik for their help in preparing the manuscript.

JOKE MEHEUS

Preface

At the end of October 1978, I had the privilege of organizing a conference on scientific discovery at the University of Nevada, Reno, USA. That was the first Guy Leonard (Memorial) Conference at UNR. Sam Goudsmit, co-discoverer of electron spin, then a professor at UNR after a distinguished career at Michigan, gave the opening lecture, "Physics in the Twenties", just a few days before his own death. The conference included around fifty participants from six countries, and the proceedings were eventually published by Reidel in two volumes. Herbert Simon and others working in artificial intelligence and neighboring fields had for some years focused on discovery and problem solving, but the Reno conference is often credited with helping to legitimize the topic for philosophers of science, epistemologists, and even some logicians.

As that conference ended, Lindley Darden remarked that it would be nice to assemble a similar group twenty years hence to determine what progress had been made. As it turns out, it was exactly twenty years later that Joke Meheus at Ghent University organized the conference to which the volume you are holding is devoted. The logic group at Ghent, headed by Diderik Batens, had by then devoted many years of research to developing logics that better capture the way in which people actually think in problem-solving contexts. They were, and have continued to be, among the most important "friends of discovery". By now work is well underway in many quarters on various philosophical or logical aspects of discovery, understood in a broad enough sense to include construction of novel models and research programs. Some of the most impressive work is being accomplished by Clark Glymour's group in the Philosophy Department at Carnegie-Mellon University in Pittsburgh, e.g., work on formal learning theory and on causal Bayes networks. Others, myself included, are taking a more historical approach.

Although Joke Meheus insisted that I be listed as a co-editor of this volume, I must confess that the International Congress on Discovery and Creativity was hers and Diderik's idea and that she deserves all praise for organizing the conference and for editing this volume. A great deal of effort was involved. The Ghent congress was on the same size scale as the one in Reno but, thanks in part to its more convenient location, more international. Joke even arranged a memorable visit to the beautiful Ghent City Hall, where we received an official welcome.

I want to express my warm appreciation to Joke and Diderik in particular and to Ghent University more generally for their wonderful hospitality, to the Research Foundation – Flanders that supported the congress, and to Lucy Fleet of Springer (the successor to Reidel and Kluwer in Dordrecht) for her ongoing support of the book project. Finally, thanks, of course, to the many contributors of papers to the conference, several of which appear here in slightly revised form.

THOMAS NICKLES

UNEXPECTED DISCOVERIES, GRADED STRUCTURES, AND THE DIFFERENCE BETWEEN ACCEPTANCE AND NEGLECT

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In June 1934 the Italian physicist Enrico Fermi published a paper in *Nature* entitled "Possible Production of Elements of Atomic Number higher than 92" (Fermi, 1934b). In this paper Fermi reported that by bombarding uranium with neutrons he and his team had produced an element which could be element number 93, that is, a transuranic element.

Two objections followed very quickly. One objection came from von Grosse and Agruss who pointed out that different chemical properties were to be expected from element number 93 than those displayed by the element produced by Fermi (von Grosse and Agruss, 1934a, 1934b). Hence, they suggested to recategorize the element as number 91. The other objection came from Ida Noddack (1934b), who also questioned Fermi's assumptions regarding the chemical properties of element 93 and suggested that the uranium nucleus could have split into several larger fragments which would be isotopes of known, light elements.

Although Fermi had formulated his findings very cautiously,[1] it was widely accepted within the scientific community that element number 93 had actually been produced. The two objections were only partly recognized. Meitner and Hahn tested the hypothesis raised by von Grosse and Agruss that the produced element could be protactinium—and proved the hypothesis wrong (Hahn and Meinter, 1935a, 1935b)—but nobody cared for the discussion of which chem-

[1]Fermi's wording was that the results "[suggest] the possibility that the atomic number of the element may be greater than 92" and that the evidence for concluding that it be element number 93 "cannot be considered as very strong" (Fermi, 1934b, p. 899).

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ical properties were to be expected of element 93. Noddack's objection was simply ignored. Neither her querying the chemical properties of element number 93, nor her proposal of the division of the nucleus were discussed—or even mentioned—by other scientists working in the field.

Four years later, the hypothesis was raised once more—now by Hahn and Straßmann—that the nucleus had split into two fractions (Hahn and Straßmann, 1939a). But this time the suggestion was not ignored, on the contrary, it received an immediate, overwhelming attention and was unreservedly accepted.

Several historians of science as well as some of the historical actors have later dealt with the issue why Noddack's suggestion was ignored while Hahn and Straßmann's was accepted. Their interpretations of Noddack's proposal vary considerably. Among the historical actors looking back, Glenn Seaborg says of Noddack's paper that it "intimated the possibility of the nuclear fission reaction" (Seaborg, 1989, p. 379), while Straßmann, on the contrary, calls her suggestion a mere "accidental hit".[2]

A similar divergence of opinion can be found among the historians. Herrmann rhetorically asks if Noddack's suggestion can "be taken as the prediction of nuclear fission, as is sometimes advocated? Not really, because Ida Noddack herself does not consider her suggestion of a novel nuclear process to be meaningful enough to test it experimentally" (Herrmann, 1995, p. 53). Van Assche, on the contrary, asks "[a]s seen now, the whole publication was a recipe to discover fission, an experimental discovery that took another four years to be made and understood. How was it possible that this advice was ignored?" (van Assche, 1988, p. 206).

This confusing pattern of interpretations reflects some fundamental, recurring philosophical questions regarding unexpected discoveries, such as: Which are the constraints that make a discovery unexpected? If these constraints preclude the phenomenon, when is it rational to violate them? And is it possible that different people can rationally operate with non-identical constraints? In the following I shall give a brief account of the discovery of nuclear fission,[3] focusing on the objections to Fermi's results in 1934 and the hypothesis raised by Hahn and Straßmann in 1938/39. I shall base my account on an analysis of conceptual structures and argue that these show individual differences that may explain how different scientists can come to operate with non-identical constraints.

[2]Orig. "Zufallstreffer" (Krafft, 1981, p. 210).
[3]For an extended account of the discovery of nuclear fission, see Andersen, 1996.

1. The Conceptual Analysis

The conceptual structures of interest in this historical development are mainly taxonomic. In my analysis I shall draw on the theory of taxonomic concepts which has been developed by Kuhn. I shall argue that on the background of this theory it can be explained not only how anomalies may trigger various kinds of discoveries, but also that differences between the conceptual structures of individual scientists may explain the diverging assessments of such anomalies and on this background why some scientists accept a discovery while others reject, neglect or ignore it.

According to Kuhn's theory, a taxonomic conceptual structure is established by grouping objects into similarity classes .[4] This grouping is not determined by necessary and sufficient conditions, but by similarity between the objects within the category and difference to objects from contrasting categories. Importantly, there are no restrictions on which features can be used to judge the objects similar or dissimilar. On the contrary, anything one knows about those objects can be used in the classification. But basing a taxonomy on similarity and difference instead of explicit definitions only works if it can be assumed that no objects fall between the similarity classes. If an object does, that is, if judged by different features it seems to belong to two contrasting categories, it violates the expectations regarding which objects exist and how they behave, in short, it is an anomaly. Such anomalies may be of different sorts. They may suggest that the objects of a given category within the taxonomy behave differently than expected, but without suggesting changes to the boundaries of other categories in the taxonomy. Or they may suggest that yet another category exists within the taxonomy, but that this is simply an additional category of previously undiscovered objects such that the new category does not affect the boundary of the previously known categories. Or, most severely, they may suggest that the previously assumed category boundaries do not hold, that is, that the taxonomy must be restructured in order to work consistently. Whereas the two former kinds—changes in the characteristic features of a given category and addition of a new category to an existing taxonomy—are changes that can be assimilated within the existing taxonomic structure, the latter kind changes the taxonomic structure itself.

As it has often been pointed out, dramatic changes are only made if the triggering anomaly is somehow felt to be severe. According to the similarity account of taxonomic concepts, the severeness of an anomaly is connected to a phenomenon called *graded structures*. On a similarity account of concepts, all instances of a concept need not be equally good examples. On the contrary,

[4]This account will have to be very brief. For a full account, see e.g. Andersen et al., 1996; Chen et al., 1998; Nersessian and Andersen, 1997.

some instances may be better examples than others by being more similar to each other or more clearly dissimilar to instances of contrasting concepts. This variation in the status of instances is called a concept's 'graded structure'.[5]

These graded structures may explain why not all anomalies are equally severe. If an object is encountered that, judged from different features, is a *good* example of two contrasting concepts, this will be a severe anomaly, as it clearly questions the adequacy of the conceptual structure. On the contrary, if an object is encountered that, judged from different features, is a *poor* example of two contrasting concepts it may not call the conceptual structure in question, but just suggest that further research may be necessary to find out whether a new category exists or whether the existing categories may show some additional features that allow the objects to be unequivocally assigned to one of them. An analysis of graded structures may thus explain why a given anomaly is judged severe or unimportant, and thus why a restructuring of the taxonomy is accepted or not.

In the following I shall present an analysis of the graded structures of the concepts involved in the discovery of nuclear fission in order to explain the reactions to various anomalies and to the different claims to new discoveries.

2. Nuclear Physics

At the beginning of the 1930s the nucleus was conceived of as a collection of individually existing protons, electrons and α-particles (Gamow, 1931). After the neutron was discovered in 1932, the nuclear electron hypothesis was no longer necessary, and the nucleus was conceived as existing of protons and neutrons which possibly clustered together in α-particles.[6]

In accordance with the view of particles existing individually within the nucleus, Gamow had developed in 1928/29 a quantum mechanical theory of α-decay in which he treated nuclear disintegration as a tunnelling phenomenon (Gamow, 1929a, 1929b). On this theory, only particles up to the size of the α-particle were energetically capable of tunnelling the potential barrier.

In 1934 Curie and Joliot discovered that they could induce radioactivity in light elements by bombarding them with α-particles (Curie and Joliot, 1934). Due to the potential barrier, α-particles could only be used for bombarding light elements, and Fermi therefore suggested to use the electrically neutral neutron as projectile instead. Fermi and his collaborators started with a systematic investigation, "irradiating all the substances [they] could lay [their] hands on" (Segré, 1970, p. 75). They reported that for a large number of elements of any

[5]See e.g. Barsalou, 1992, ch. 7.3.2 and Lakoff, 1987, ch. 2 for an overview of the psychological literature on graded structures.

[6]For an account of the nuclear electron hypothesis, see Stuewer, 1983.

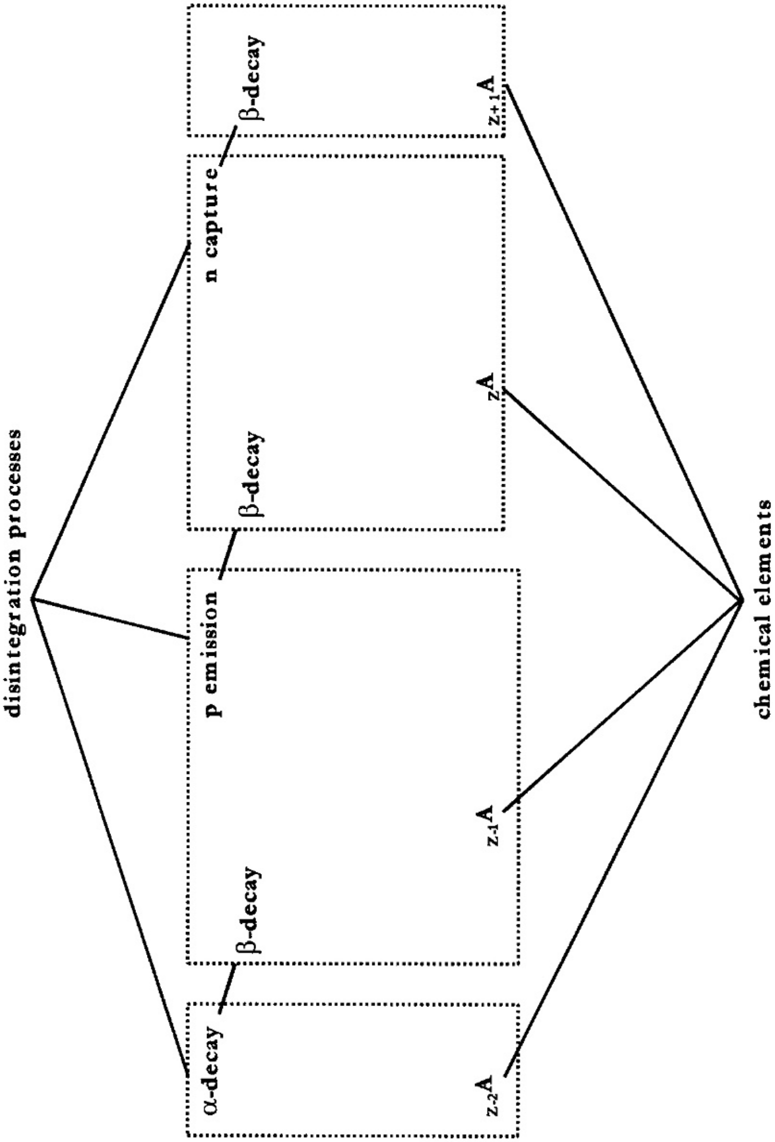

Figure 1. Main taxonomy of disintegration processes and its connection to the taxonomy of elements.

Wir geben nun eine Übersicht der erzwungenen Umwandlungen. Bei jedem Reaktionstyp sind nur die Eigenschaften aufgeführt, die ihn von den übrigen Prozessen unterscheiden; soweit keine besondere Angabe gemacht ist, gelten also die obengenannten allgemeinen Regeln.

(α, d). Scheint nur in einem einzigen Fall beobachtet zu sein (75). Daß das Deuteron als unzerlegter Kernsplitter auftritt, ist wegen seiner geringen Stabilität in der Tat unwahrscheinlich.

(α, p). An diesem Prozeß wurde die künstliche Kernumwandlung durch RUTHERFORD und CHADWICK entdeckt. Er ist energetisch besonders günstig, wenn der Ausgangskern ungerades Z und gerades N hat, da er zur Erhöhung von Z um eine, von N um zwei Einheiten führt.

(α, n). Bei diesem Prozeß wurde das Neutron entdeckt. Er ist energetisch günstig bei Kernen mit geradem Z und ungeradem N.

(α, γ). Nicht beobachtet. Da der Prozeß an sich mit großer Energieausbeute möglich sein sollte, muß die Erklärung in der geringen Emissionswahrscheinlichkeit der γ-Strahlung liegen (§ 34). Einfangung ohne Emission einer Strahlung ist (§ 4) nach den Erhaltungssätzen unmöglich.

Figure 2. Extract from von Weizsäcker's *Die Atomkerne* which treats all possible induced radioactive processes in the form of a list of all possible permutation of p, n, d, α and γ as projectile and decay products, respectively.

atomic weight, neutron bombardment would produce unstable elements which disintegrated through the emission of β-particles (Fermi, 1934b).

The next step was to investigate the primary processes that lead to the β-radiating elements. The original group consisted of Fermi, who had already achieved international reputation as a theoretical physicist, and the two physicists Amaldi and Segré, but they soon recruited the chemist D'Agostino in order to make the chemical separations necessary for identifying which elements were produced in the disintegration processes. Identification of the produced elements would then reveal the primary process by which it had been produced. The group reported that three main processes were possible: α emission, proton emission and neutron capture (Fermi, 1934b, p. 898).[7] This established the main taxonomy of artificially induced disintegration processes and its connection to the taxonomy of elements (fig. 1).

[7] α emission was identified for Al, Cl and Co, proton emission for Ph, S and Zn, and neutron capture for Br and I.

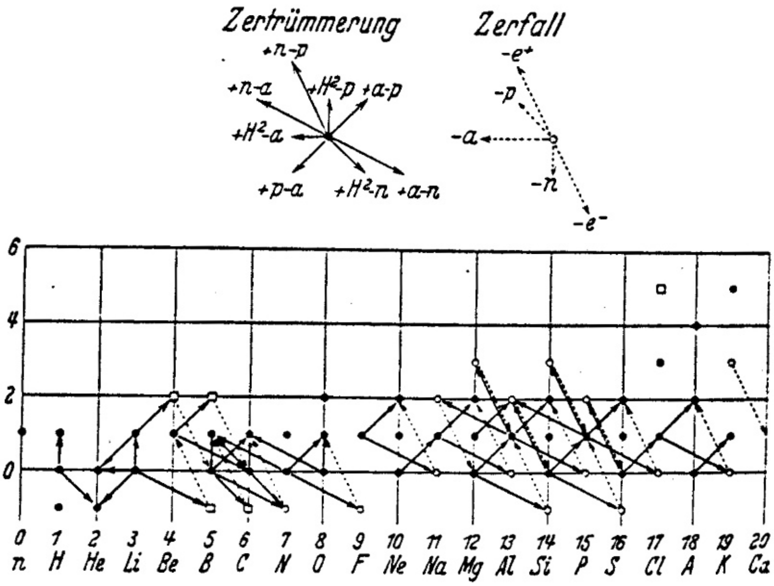

Figure 3. Checker-board like diagram of possible nuclear transmutations. From Meitner, 1934.

This taxonomy was in fine accordance with Gamow's theory of decay which precluded decay products larger than the α-particle.[8] In the years that followed, Gamow's result that only particles up to the size of the α-particle could be emitted would become tacitly accepted in the whole scientific community to such an extend that the mere possibility of larger decay products would never be mentioned (fig. 2).

Likewise, the diagrams and notations which were developed could only represent the idea that a projectile hits a nucleus which as a result transformed into another nucleus by the emission of a particle (fig. 3). The range of the taxonomy seemed well-defined.

Having established this taxonomy of artificially induced disintegration processes, Fermi and his team took special interest in heavy nuclei. The general instability of the heaviest elements might give rise to successive β-decays, and possibly that could lead to a transuranic element (fig. 4).

When they bombarded U with neutrons they discovered at least 5 different disintegration processes with different half-lives: 10 sec., 40 sec., 13 min. plus at least two more periods from 40 minutes to one day (Fermi, 1934b, p. 899). But where did they belong (fig. 5)?

[8] Fermi referred explicitly to Gamow's work in several papers, see e.g. Fermi et al., 1934, 1935.

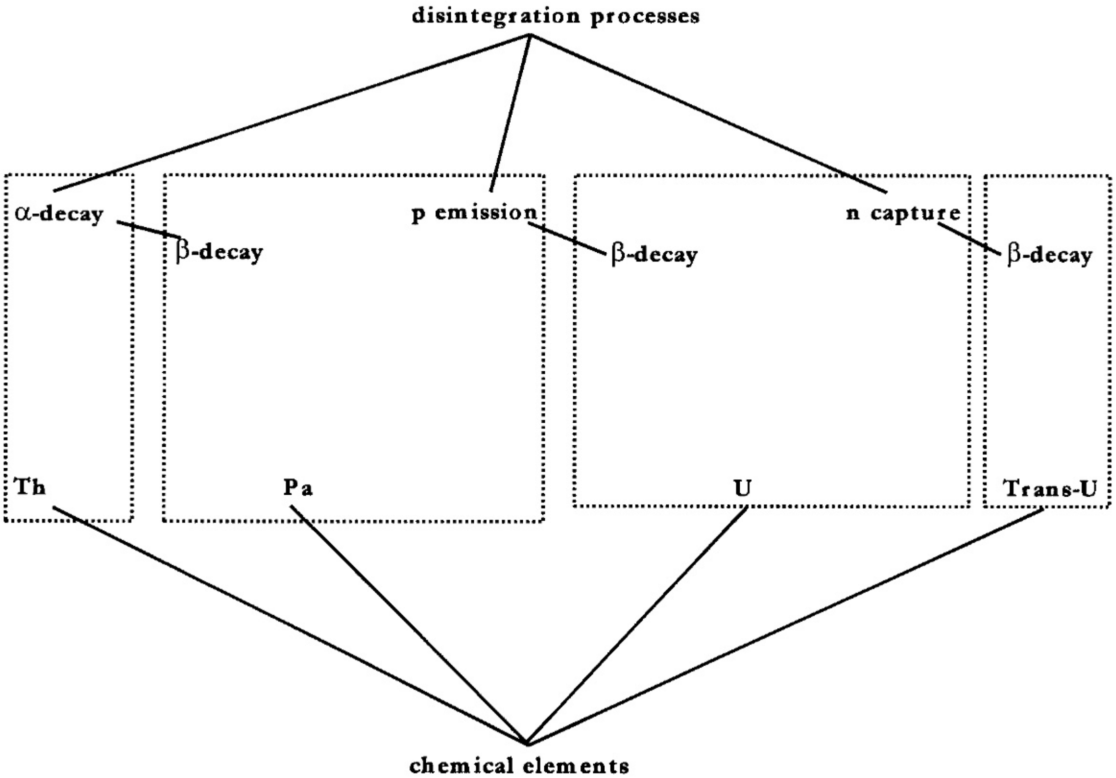

Figure 4. Taxonomy of disintegration processes when bombarding uranium with neutrons and the connection to the taxonomy of heavy elements. The β-decay series following the primary disintegration processes might give rise to transuranic elements.

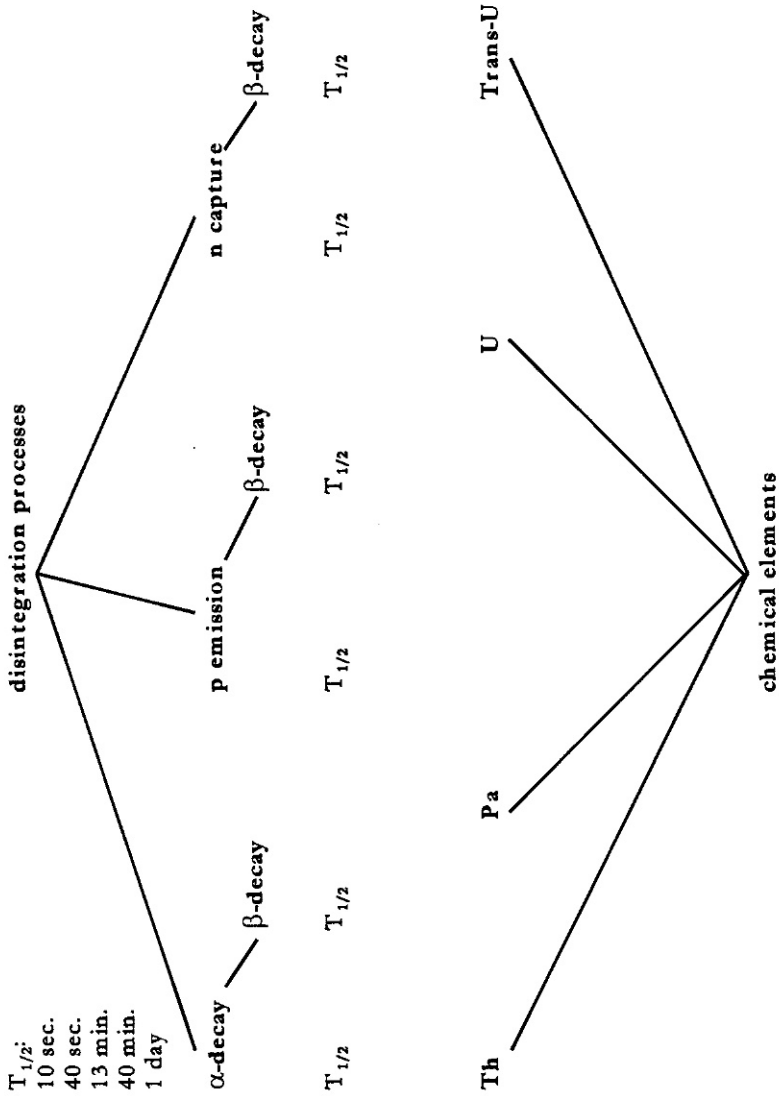

Figure 5. Half-lives could be used to identify different disintegration processes, but first it had to be determined which disintegration processes had which half-lives.

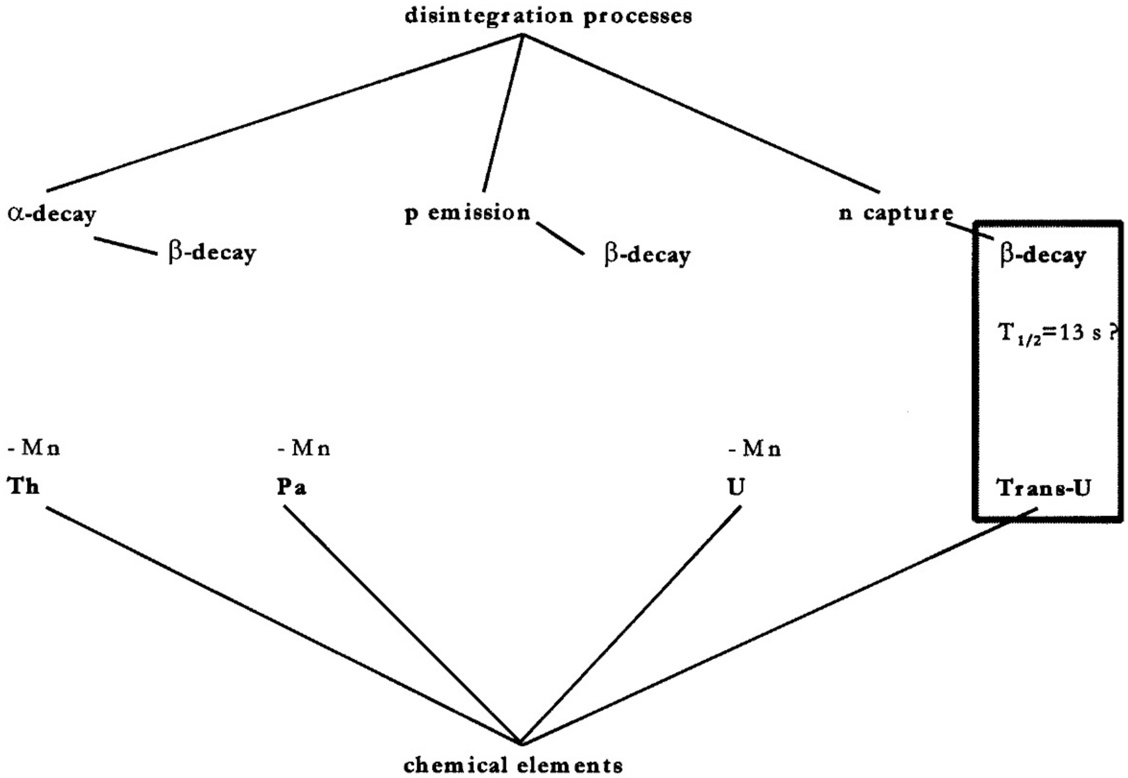

Figure 6. Manganese precipitation processes showed that the element produced in the 13 min. process could not be any of the known heavy elements (indicated in the diagram as -Mn). It was therefore categorized as a transuranic element.

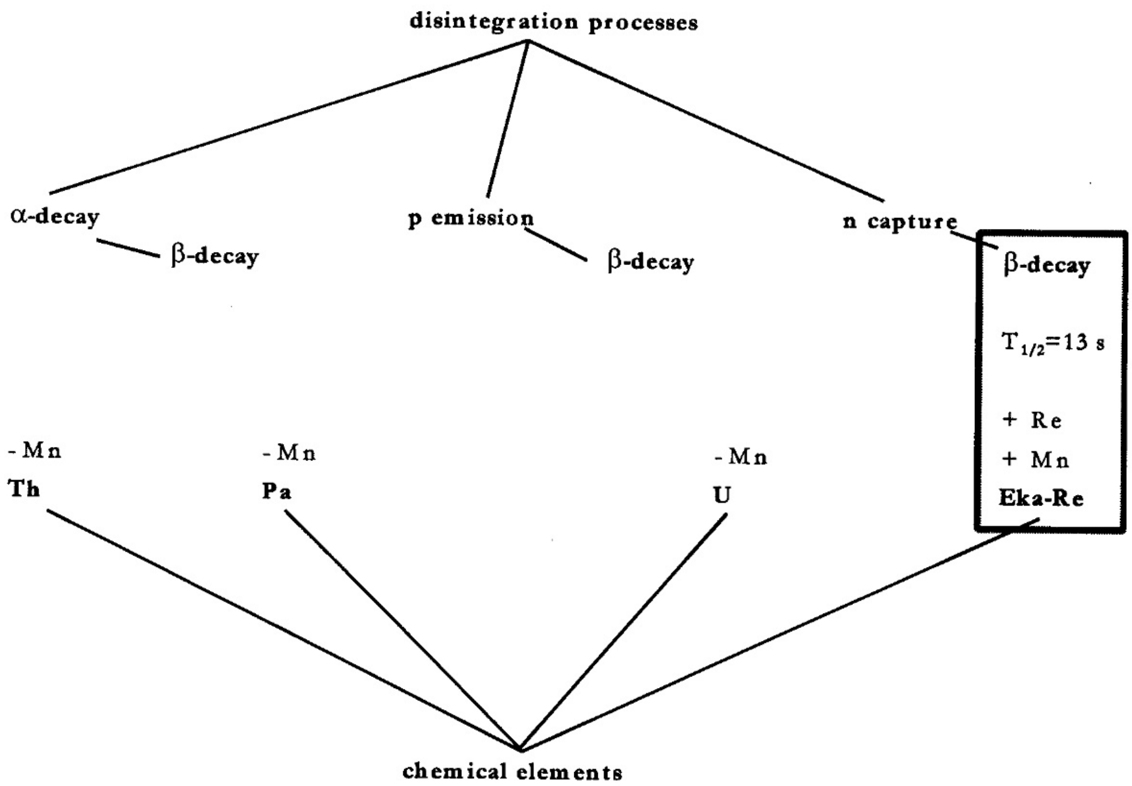

Figure 7. Since the element produced in the 13 min. was chemically homologous with manganese and rhenium (indicated in the diagram as +Mn and +Re) it was hypothesized to be element number 93 and belong to the same subgroup in the periodic table as rhenium. It was given the name Eka-Re.

Concentrating on the element with the period of 13 min., they showed that a manganese precipitation process would separate this element from "most of the heaviest elements" (Fermi, 1934b, p. 899), and they concluded that "this negative evidence about the identity of the 13 min. activity from a large number of heavy elements suggests the possibility that the atomic number of the element may be greater than 92" (fig. 6).

They hypothesized that "if it were an element 93, it would be chemically homologous with manganese and rhenium" (Fermi, 1934b, p. 899) and reported that this hypothesis was supported by the results of another precipitation process using rhenium sulphide (fig. 7).

However, they also noted that elements 94 and 95 would probably not be easy to distinguish from element 93 and that consequently "valuable information on the processes involved could be gathered by an investigation of the possible emission of heavy particles" (Fermi, 1934b, p. 899). Hence, given that chemical characteristics might not be conclusive, they referred to the desirability of including decay characteristics in the classification as well (fig. 8).

The discovery of transuranic elements was therefore a very *expected* discovery. The taxonomy of artificially induced disintegration processes indicated that transuranic elements might very well be produced, and it provided the classificatory means by which to find them.

Although Fermi was initially very cautious in his claim of having discovered the first transuranic element, the reaction from the scientific community was unreserved congratulations. Or, rather, *almost* unreserved congratulations. Two objections were raised shortly after Fermi's first publication of the results. The first came from von Grosse and Agruss (1934a, 1934b). On the basis of Mendelejeff's periodic law they questioned Fermi's assumptions regarding the chemical properties of element 93. However, what they questioned in this paper was solely how the element Eka-Rhenium would behave,[9] but not whether the element 93 would be Eka-Rhenium, that is, whether it would belong to the same group in the periodic table as rhenium (fig. 9).

Von Grosse and Agruss further criticized the process which Fermi's team had used to rule out protactinium,[10] and reported that according to their experiments the new element could very well be protactinium. However, Meitner and Hahn showed that this was not the case (Hahn and Meinter, 1935a, 1935b) (fig. 10).

Whereas the specific suggestion to recategorize the element as protactinium was discussed—and rejected—within the scientific community, their criticism of Fermi's assumptions regarding the chemical properties of Eka-Rhenium re-

[9]More specifically, von Grosse and Agruss questioned whether the highest oxide of Eka-Rhenium would form an acid under the conditions of Fermi's experiment, or whether it would precipitate with the manganese carrier.

[10]Fermi had used a very short-lived isotope of protactinium which made the chemical operations very difficult.

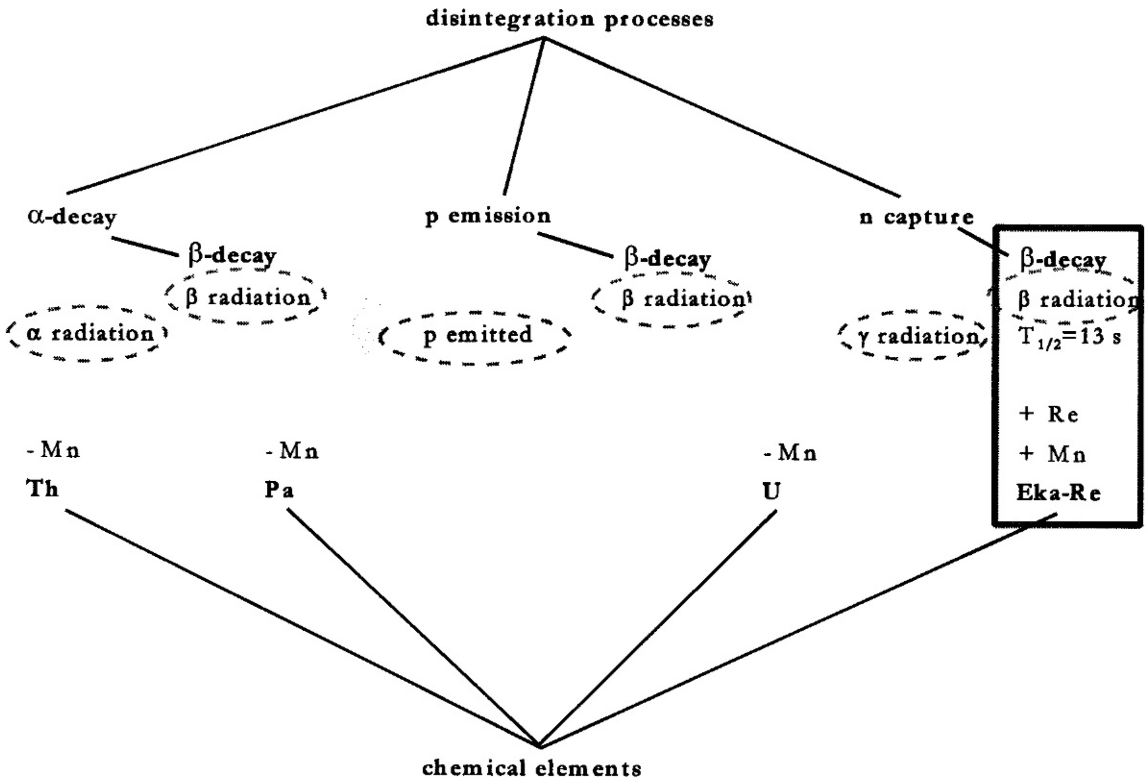

Figure 8. Since it was difficult to categorize the transuranic elements only on the basis of chemical behaviour, including other characteristics from the taxonomy of disintegration processes, such as radiation, would be advantageous.

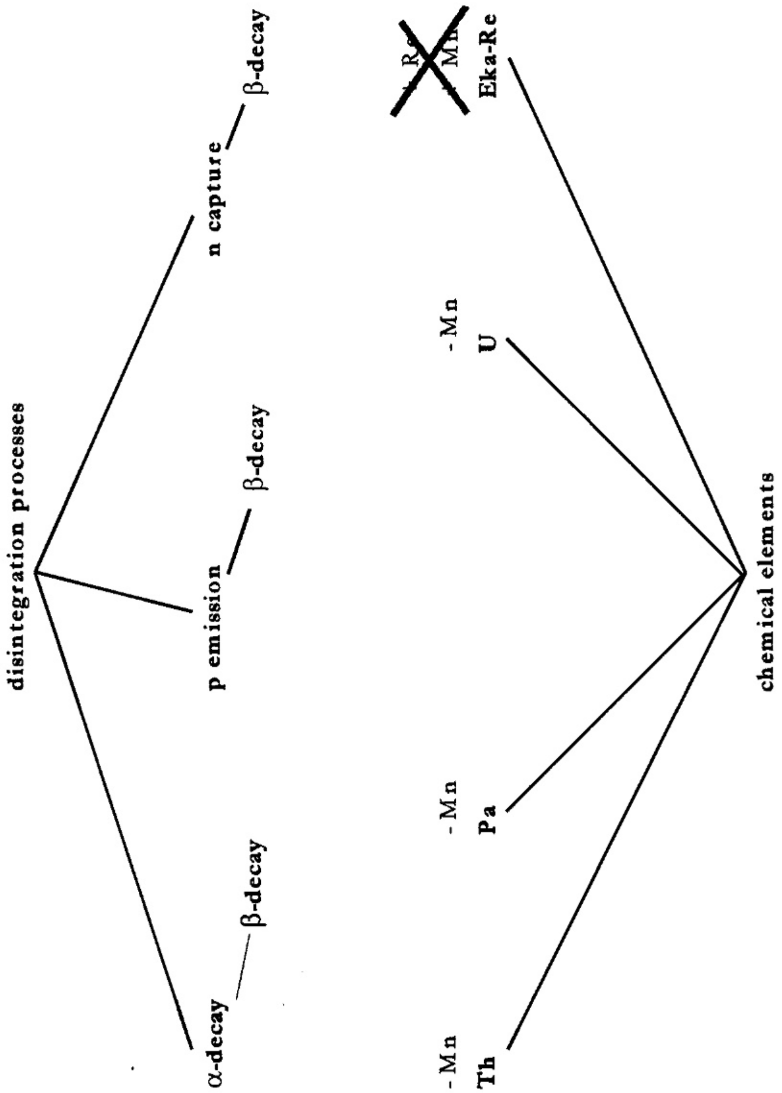

Figure 9. Von Grosse and Agruss questioned whether element 93 would be chemically homologue to rhenium and manganese.

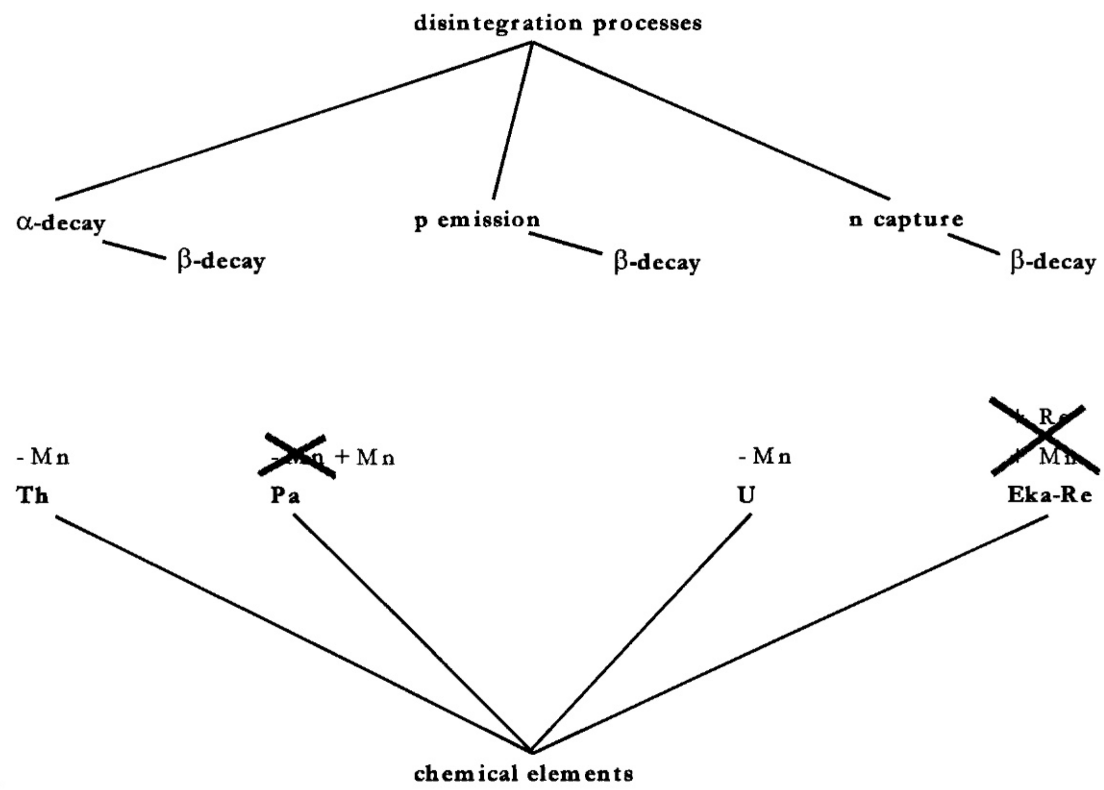

Figure 10. Von Grosse and Agruss reported that according to the precipitation processes they had conducted, the new element could be protactinium.

mained unnoticed. Von Grosse published a paper a few months later in which he both substantiated his claim regarding Eka-Rhenium, and also pointed out that element 93 might not even be Eka-Rhenium, but could instead belong to a transition group which would imply a completely different set of chemical properties (von Grosse, 1934) (fig. 11).[11]

Still, there were no indications in any of the papers from the Rome or the Berlin teams that they seriously discussed whether element 93 would have the chemical properties which Fermi had assumed in his classification.

The second objection came from Ida Noddack (1934b). She too pointed out that element number 93 might not have the chemical characteristics which Fermi had assumed in his identification, especially regarding the rhenium precipitation process. However, the alternative she suggested was much more radical than the alternative which von Grosse and Agruss had proposed. She pointed out that several known elements would behave like Fermi's new element in the manganese precipitation process. But these were all much lighter than uranium and could not be the product of any of the artificially induced disintegration processes contained in Fermi's taxonomy. She therefore suggested two different processes which could possibly lead to the production of light elements: either a long series of successive transformations, or the division of the nucleus into several large fractions (fig. 12).

There was no reaction at all from the scientific community to Noddack's suggestion. Apparently, these suggestions could simply not be taken seriously. According to Gamow's droplet analogy, which treated disintegration as a tunnelling phenomenon, disintegration processes *had* to be one nucleus transmuting into another nucleus of almost the same size by releasing a small particle. On this model, there was no way a nucleus could divide into a few large fractions.

What Noddack suggested was not filling out a well-defined gap in the taxonomy like Fermi's suggestion was. The potentiality of the taxonomy of artificially induced disintegration processes clearly did not include the division of the nucleus. Whereas discovering transuranic elements was highly expected, discovering that the nucleus had split into large fractions would not only be unexpected, it would be highly revolutionary, demanding changes in the principles underlying the taxonomy. This did not seem necessary, neither to Fermi and his group, nor to anybody else.

During the four years to follow, several discoveries were made that had not been expected, but which could all be included in the taxonomy without changing its underlying principles (fig. 13). The process 'neutron chipping' was introduced as a simple addition to the taxonomy which had not been expected

[11] von Grosse assumed the transition group to start with protactinium, hence the transuranic elements would have chemical properties similar to this element.

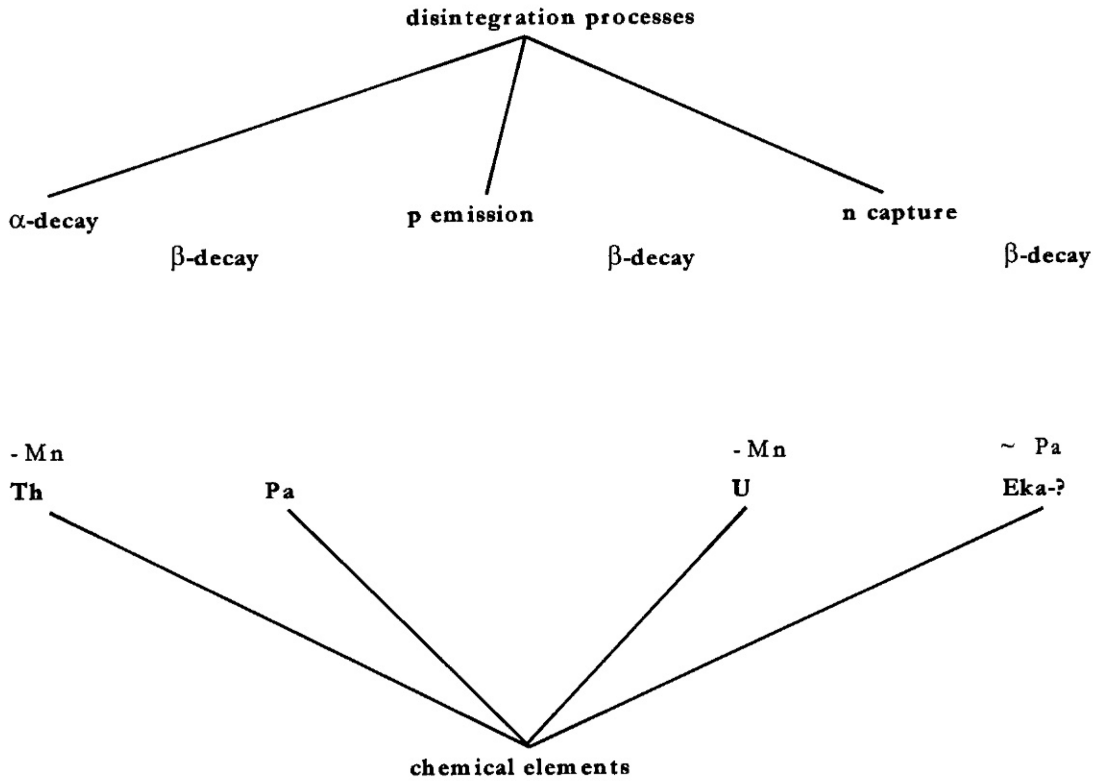

Figure 11. In a later paper von Grosse argued that element number 93 could belong to a transition group like the lanthanides. This could mean that element number 93 had chemical characteristics similar to those of protactinium.