Pure & Appl. Chem., Vol. 65, No. 8, pp. 1815–1824, 1993.
Printed in Great Britain.

INTERNATIONAL UNION OF PURE AND APPLIED CHEMISTRY

in conjunction with

INTERNATIONAL UNION ON PURE AND APPLIED PHYSICS

Responses on the Report

DISCOVERY OF THE TRANSFERMIUM ELEMENTS

invited by the

IUPAC-IUPAP TRANSFERMIUM WORKING GROUP (TWG)

from

LAWRENCE BERKELEY LABORATORY, CALIFORNIA
(Albert Ghiorso and Glenn T. Seaborg)

JOINT INSTITUTE FOR NUCLEAR RESEARCH, DUBNA
(Yu. Ts. Organessian and I. Zvara)

GESELLSCHAFT FÜR SCHWERIONENFORSCHUNG, DARMSTADT
(P. Armbruster, F. P. Hessberger, S. Hofmann, M. Leino, G. Münzenberg, W. Reisdorf and K.-H. Schmidt)

followed by reply to the responses by

TRANSFERMIUM WORKING GROUP

Chairman: D. H. Wilkinson (IUPAP; UK); *Secretaries*: A. H. Wapstra (IUPAP; Netherlands); I. Ulehla (IUPAP; Czechoslovakia); *Members*: R. C. Barber (IUPAP; Canada); N. N. Greenwood (IUPAC; UK); A. Hrynkiewicz (IUPAP; Poland); Y. P. Jeannin (IUPAC; France); M. Lefort (IUPAP; France); M. Sakai (IUPAP; Japan).

Responses on 'Discovery of the transfermium elements'

I. RESPONSE FROM BERKELEY

We appreciate the opportunity offered by IUPAC to publish our comments on the report of the Transfermium Working Group "Discovery of the Transfermium Elements" as an accompaniment to its publication in Pure and Applied Chemistry. Unfortunately, this opportunity was not offered to us by IUPAP when the report was published in Progress in Particle and Nuclear Physics.

We appreciate the time and effort that the Transfermium Working Group (TWG) has put into the preparation of the report "Discovery of the Transfermium Elements", but nevertheless, we have some problems with the report.

We write to warn the readers that the report is riddled with errors of omission and commission. Many relevant references and much relevant data were omitted as a result of the selective use of references and data. Much of the experimental evidence is of a basically chemical nature and its proper evaluation would have benefited from the presence of more nuclear chemists on the TWG (which consists of seven members representing the IUPAP and, inexplicably, only two members representing the IUPAC). For example, a careful study by three nuclear chemists led to dramatically different conclusions (E.K. Hyde, D.C. Hoffman, and O.L. Keller, "A History and Analysis of the Discovery of Elements 104 and 105", Radiochemica Acta 42, 57-102 (1987)).

It is clearly a futile waste of effort and time to reopen the cases of elements 102 and 103, which were discovered more than 30 years ago with the approval of the suggested names by the IUPAC according to the standards of that era. Even more outrageous is the inclusion of element 101, discovered in classic experiments with name approval by the IUPAC, nearly 40 years ago. What is the purpose of this unprecedented maneuver? Obviously the accepted names for these elements should not, and are not going to be, changed. This conclusion is consistent with the statement in Part II (II.1 Introduction) of the TWG report "We reiterate the hope expressed in I.9 that our work will lead to the submission to IUPAC, by those most directly involved, of proposals, joint ones where appropriate, for a name for each element for which no name has yet been recommended by the IUPAC Commission II.2 on Nomenclature of Inorganic Chemistry."

This response is being presented to outline our position briefly - - yet hopefully in adequate detail -- so that the scientific world can have the point of view of scientists who have actually participated in the discoveries of these elements.

INTRODUCTION (cf. TWG report, pp. 1759-1761)

Among the "more elaborate expositions about the new elements" referred to on page1761,there are several references to papers by the Dubna investigators but no references to papers by the Berkeley investigators.

SOME SCIENTIFIC CONSIDERATIONS (cf. TWG report, pp. 1761-1764)

This section includes a discussion of the inadequacies of the observation of reaction mechanisms as a means of establishing the atomic number of a new element (with which we completely agree), and yet such methods are accepted for the assignment of the atomic number when the criteria are applied later in the report. No mention is made in this section of the reliable method of assigning the atomic number of an alpha-particle-emitting nuclide through the observation of previously identified descendants (the method of genetic relationship), nor of the most definitive method which is chemical identification, and yet these are the only methods that have been proven to be efficacious for the discovery of new elements.

THE DISCOVERY PROFILES (cf. TWG report, pp. 1764-1795)

We have no objections to the somewhat cumbersome plan for the so-called discovery profiles, but we object to their selective application in some instances.

ELEMENT Z=101

We include a comment on this section to illustrate the grievous errors that can result when an evaluating group does not have the advice of several experienced nuclear chemists.

We think that the scientific community has always regarded the discovery of this key element as a classic of the **nuclear chemistry** method, but the TWG seems to miss the most important basis of the discovery, namely that it was **chemical**.

Clearly, one must not forget that the **essence of this discovery in 1955 (not 1958) was the chemical separation of element 101 from all other known elements.** Even if we had observed only one atom in the transfermium fraction, it would have been highly significant. We have the feeling that element 101 is included largely for the reason that "transfermium" relates to the number 100, a special number in the decimal system. We resent the inclusion of an element whose discovery more than met the standards of nearly 40 years ago. This made us wonder how insightful the rest of the report would be.

ELEMENT Z = 104

Our most serious quarrel with the TWG report is their treatment of the Dubna claims for the discovery of element 104. Perhaps it is understandable that it would be difficult to unravel the complicated series of Dubna internal reports (and oral reports at meetings) without having followed the events as they developed (as we were in a position to do). Over time, their reported half-lives of element 104 changed drastically.

The gist of the Dubna claim to the discovery of element 104 has been their "observation" of an isotope that decays by the process of spontaneous fission with the mass number 260, assigned on the unreliable basis of nuclear reaction systematics. For this, they reported a half-life of 0.3 seconds in 1964, 0.1 second in 1969, then a half-life of $80^{\pm}20$ milliseconds in 1976, and finally in 1985, the nearly correct half-life of 28 milliseconds (reported as 10-30 milliseconds by the Berkeley group as early as 1970, with the present best value of 21 milliseconds).

Thus, the Dubna claims of 1964 for the 0.3-second SF half-life of $^{260}104$ were invalid. Their original experiments performed inside the cyclotron were apparently flawed in that the intense neutron field produced fission tracks from uranium impurities in the nickel belt, and thus gave a diminishing background that mimicked radioactive decay.

There is no doubt that the Berkeley group definitely identified alpha-emitting isotopes of element 104 in 1969 (with mass numbers 257 and 259), through the reliable methods of genetic relationship, by observing the known daughters (alpha-emitting isotopes of element 102 with mass numbers 253 and 255).

For some reason, unknown to us, the 80-millisecond activity became their favorite, and they stuck to it for many years. **This prominent activity was not even mentioned in the profiles (discussion and assessment) sections of the TWG report.** We realize that Dubna now agrees that the 0.3 seconds → 0.1 seconds → 80 milliseconds SF activity does not exist, yet this "activity" was the highlight of their work on element 104 and claim to discovery. After it became clear that this work of G. Flerov, et. al., was clearly wrong, the Dubna group turned to the chloride volatility experiments of chemist I. Zvara, et. al., to try to sustain their claim to the discovery of element 104 and the TWG naively supported this attempt.

In surprising retrospective reasoning, involving (1) "an unpublished document in the possession of the TWG", (2) calculations by members of the TWG itself ("after consulting an expert on statistical methods, the TWG made its own analysis"), (3) "Calculations by Zvara and a member of the TWG", (4) inaccurate and erroneous recollections of a talk (which we heard and upon which we made notes) given by Zvara at a meeting of the American Chemical Society in San Francisco in 1968 and (5) Dubna unpublished preprints of uncertain actual dates of issue, the TWG concludes that some chloride volatility experiments as early as 1968 established the decay by spontaneous fission of the 3-second $^{259}104$ (whose discovery as an alpha emitter was not published by Berkeley scientists until 1969). We feel strongly that such retrospective treatment of 20-year-old Dubna data by members of the supposedly impartial TWG is highly irregular and that the drawing in 1991 of ex post facto conclusions that the Dubna group was entirely unaware of 20 years before (as evidenced by their publications at that time) is manifestly unfair.

Dubna scientists reported in 1971 that they observed a 7% SF branching decay of the alpha emitting 3-second $^{259}104$, discovered at Berkeley two years earlier. Actually, even today, there is no proof positive that $^{259}104$ even has a 7% SF branching decay. The spontaneous fission observed at Dubna could easily be due to nearby nuclide(s) of lower Z that have been recently discovered to have SF branch decays.

It is interesting to note that the TWG report itself, in describing the volatility experiments performed in 1969, notes that the Dubna investigators maintained that their experiments confirmed the 0.3 second period. They quote the Dubna assertion, "It shows positively that the effect was not caused to an appreciable extent by the decay of nuclides undergoing spontaneous fission with half-lives of 0.014 and 3.7 sec."

There is no credible evidence that Dubna investigators were dealing with, or thought at the time they were dealing with, the 3-second isotope of element 104 ($^{259}104$) at any time before its discovery at Berkeley in 1969 in these early (1968 and 1969) volatility experiments.

No mention is made in the profiles (discussion) sections of the first meaningful chemical identification of element 104 at Berkeley in 1970 by the reliable ion exchange separation method.

On the basis of this record, we cannot--indeed, in all honesty, should not--accept the TWG CONCLUSION, "The chemical experiments in Dubna (69Zv99 with 70Zv99) and the Berkeley experiments (69Gh01) were essentially contemporaneous and each show that element 104 had been produced. Credit should be shared."

Acceptance of this conclusion would be a disservice to the scientific community. Our suggested name for element 104, should be adopted.

ELEMENT Z = 105

Experiments at Berkeley in 1970 unequivocally established the existence of an isotope of element 105 with the mass number 260 (half-life 1.6 seconds and alpha energy 9.1-Mev) by the reliable method of genetic relationship (observation of the known daughter 8.4-Mev alpha activity, 30-second $^{256}103$).

Earlier and contemporaneous work at Dubna based on their usual observation of decay by spontaneous fission was inconclusive, as is characteristic of this method for the assignment of atomic number. Volatility experiments at Dubna performed in 1971 were similarly inconclusive.

An identification by another group (V.A. Druin, et. al.) occurred at Dubna in 1971 of an isotope of element 105 with the mass number 260 or 261 (half-life 1.4 seconds and alpha energy 9.1-Mev) by the Berkeley method of genetic relationship (observation of a daughter alpha activity of half-life 35-seconds and alpha energy 8.3-8.6-Mev, presumed to be due to $^{256}103$). Even if the latter daughter identification is correct, this identification followed the Berkeley discovery experiment by a year.

The TWG CONCLUSION states "Independent work reported in 1970 from Berkeley (70Gh02) and from Dubna (71Dr01) was essentially contemporaneous and equally convincing. Credit for the discovery should be shared". We disagree with the assessment "essentially contemporaneous and equally convincing," and suggest that our suggested name "hahnium" be adopted for element 105.

ELEMENT Z = 106

Experiments at Berkeley by a Berkeley-Livermore collaboration in 1974 led to the definite identification of $^{263}106$ (half-life of 0.9 seconds and alpha energy 9.06-Mev) by the observation of its previously known alpha-decaying daughter $^{259}104$ and granddaughter $^{255}102$ (use of reliable method of demonstration of genetic relationship). This observation was confirmed by a group at the Oak Ridge National Laboratory (C.E. Bemis et. al., Physics Division Annual Progress Report (1974), Chemistry Division Annual Progress Report (1975)).

Also in 1974, Dubna investigators reported the observation of a spontaneous fission activity with a half-life of 4-10 milliseconds, which they assigned to $^{259}106$ on the basis of reaction systematics (again using these unreliable methods for the identification of atomic number). We now know that this assignment was erroneous in that the observed spontaneous fission activity was due primarily to $^{256}104$, the daughter of $^{260}106$, and not to element 106 (as demonstrated in 1984 by another team of Dubna investigators).

Therefore, we agree with the TWG CONCLUSION "Independent work reported in 1974 from Berkeley-Livermore (74Gh04) and from Dubna (740g04) was essentially contemporaneous. The Dubna work is highly important for later developments, but does not demonstrate the formation of a new element with adequate conviction, whereas that from Berkeley-Livermore does".

Therefore, we and our co-workers will assume the right to suggest a name for element 106 and will do so at the proper time.

SUMMARY (cf. TWG report, pp. 1796-1797)

1. ELEMENT Z = 101 The TWG report failed to recognize the definitive chemical identification of an isotope of element 101 performed in 1955. This was the same reliable ion-exchange separation method used, during the preceding six years, for the discovery of elements 97, 98, 99 and 100.

2. ELEMENT Z = 102 The TWG CONCLUSION is silent on the definitive earlier contributions of the Berkeley investigators to the discovery of element 102. The TWG erroneously assumed that interference from the 1.8-second ^{250m}Fm invalidated the discovery of alpha-particle emitting $^{254}102$ via the chemical identification of its alpha-particle-emitting daughter ^{250}Fm.

3. ELEMENT Z= 103 We think that the TWG treatment of this element was the best in the report and we agree with the TWG CONCLUSION.

4. ELEMENT Z = 104 Here we have our most violent disagreement with the TWG report. The TWG did not recognize the central role that the non-existent 0.3 sec.→ 0.1 sec.→ 80 ms. SF activity (assigned to $^{260}104$) played (for 20 years!) in the Dubna claim to the discovery of element 104 (until 1985) and erroneously accepted their untenable alternate claim (the chloride volatility experiments) to the discovery of element 104.

5. ELEMENT Z = 105 We do not agree with the TWG CONCLUSION that the Berkeley and Dubna work "was essentially simultaneous." The definitive Berkeley work clearly preceded the Dubna work.

6. ELEMENT Z = 106 Although we agree with the TWG CONCLUSION, we believe the TWG should have more clearly described the role of the Dubna team of A.G. Demin, et al. in disproving (in 1984) the Dubna claim to the discovery of element 106.

SOME REFLECTIONS

We believe that the demand of the Dubna investigators that their meeting with the TWG should be postponed from the originally scheduled time to a time after the meetings with the Berkeley and GSI investigators was a ploy that paid off very well for them. Thus the Dubna group had the advantage of "having the last word" and especially the extraordinary and highly questionable advantage of having the TWG "collaborate" with them in "retrospective re-evaluation" of old data, an advantage that was never available to the Berkeley investigators. Also, although it may be a gratuitous comment, we feel we must, in all honesty, point out that the TWG erred in its reliance so much on one of its members to compose its report; the downgrading of chemical contributions seems to be one consequence of this.

Albert Ghiorso

Glenn T. Seaborg

Lawrence Berkeley Laboratory
1 Cyclotron Road
Berkeley, CA 94720, USA
22 September 1992

II. RESPONSE FROM DUBNA

The published materials are the result obtained by a group of renowned scientists in the course of an extensive and thorough analysis of the problem of discovering new elements. By establishing the TWG, the authorizing bodies of the IUPAP-IUPAC intended to get an objective opinion on the priorities in discovering a number of new elements in the transfermium region. The result has turned to be more extended and profound than expected. The criteria for the discovery of a new element of the Periodic System proposed by the TWG are based on a great amount of varied experimental and theoretical data. At the same time they possess the necessary generality. Due to this they will hopefully not impose rigid restrictions upon future research in which there can occur unforseen but promising possibilities. The very fact of developing such criteria is an important event in the life of the scientific community. We are thankful to the TWG for their efforts and hope that this feeling is shared by our colleagues.

One may think now that the criteria will be of assistance in developing a general approach to the evaluation of results obtained by different research groups and in this sense they will contribute into the progress of investigations. In general the TWG has taken an objective approach to its conclusions on the priorities in discoveries reported in the LBL, FLNR and GSI papers. It has acknowledged an approximately equally important contribution of the three laboratories into the discovery of transfermium elements.

At the analysis of the priorities the TWG was to make a well reasoned choice from different and sometimes opposite viewpoints and concepts. That is why every participant of the research on new elements may have some concrete remarks referring to the materials presented by the TWG. It seems to us that the TWG conclusions underestimate the importance of some Dubna papers on the elements 103, 104 and 105. But we have agreed to cooperate with the TWG and have thus assumed an obligation to respect its decision.

It should be noted here that already in the beginning of the seventies the Dubna laboratory has suggested to the IUPAC the idea of the present TWG. Initially, as early as in 1968, basing on its own data Dubna drew the IUPAC's attention to the fact that the adoption of names for elements 102 and 103 was hasty. In accordance with the IUPAC tradition, its

Commission on Nomenclature of Inorganic Chemistry deals also with the names of elements. It became clear at the time that the absence of nuclear chemistry and nuclear physics experts on this board created great difficulties in solving the problems of naming man made elements since what it really required was a judgement on the conclusiveness of evidence of the discovery. Due to this the IUPAC authorizing bodies accepted the Dubna proposal and in September 1974 established a Committee of experts on the given issue (Chairman - Prof. J. Lewis, Cambridge, UK). Unfortunately, it never got down to work. The scientific community knows next to nothing about this episode.

ON THE NAMES OF TRANSFERMIUM ELEMENTS

The tradition of the majority of scientific fields is to give names in honour of scientists who have made the decisive contribution into the discovery itself or into the development of the corresponding fields. The greater part of the proposals on the names of transfermium elements follows this tradition - here one finds the names of outstanding researchers of the structure and transformations of atoms and nuclei. Due to this already schoolchildren learn the names which are milestones in the history of fundamental sciences. The same, though probably in a smaller extent, can be referred to the place where the discovery has been made. Everyone understands that the discovery and its authors are just the top of an iceberg the body of which is the efforts and achievements of a great number of people who have contributed into the scientific, methodical and technical basis of the research.

It was not the task of the TWG to recommend names of new elements. Following the logical sequence of steps the IUPAC and IUPAP are now to consider this problem in accordance with the conclusions of the TWG established by them.

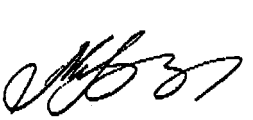
Names, as known, are suggested by the authors of papers that have been recognized to possess the priority in the discovery. It is not only an honorary privilege of the discoverers but also an acknowledgement of their intellectual property as well as of the efforts and expenses of the laboratory where the discovery has been made.

The right of the authors of a discovery to give a name signifies undoubtedly that they are bearing a moral responsibility to the scientific community. This responsibility is shared also by the bodies issuing "official" recommendations on the names.

The situation with names of elements becomes more complicated when a discovery is recognized to be the result of joint efforts of two different groups or when a conclusion is made that the two groups have discovered the element simultaneously and independently of each other. As is seen from the commented report this takes place with elements 103, 104 and 105 for which the TWG has recognized the credit of the authors from Berkeley and Dubna to be equal. Each of the groups has suggested its own names which makes it difficult to take a decision. The best way out here could be a joint proposal of elements names formulated by the authors of priority papers. We have brought forward the initiative of convening such a meeting of authors during the work of the TWG already (at that time, we were thinking yet about all the three laboratories) and are ready to make everything possible for the realization of this principle. The joint proposal of names would base on the TWG conclusions but some other points could be also taken into account. It is of little doubt that the IUPAC and IUPAP will accept such a proposal as the basis for their official recommendations on the names.

Yu. Ts. Oganessian

Flerov Laboratory of Nuclear Reactions
Joint Institute of Nuclear Research
Dubna, Russia
12 October 1992



I. Zvara

III. RESPONSE FROM DARMSTADT

The research group at the Gesellschaft für Schwerionenforschung highly appreciates the efforts of the International Union of Applied Chemistry and the International Union of Applied Physics to solve the longstanding problem of the priority of discovery of the heaviest elements by appointing the Transfermium Working Group. This international group of renowned experts in nuclear physics and chemistry, headed by Sir Denys Wilkinson, established criteria for the discovery of a new element and on the basis of this judged on the priorities of the discoveries of the transfermium elements. Members of this group were scientists from countries not involved in the discovery of a new element.

The criteria for the discovery of new elements were developed after a careful study of the literature and after visits to the involved laboratories. Permanent contact was established with the researchers concerned by distributing the protocols of the TWG meetings. Only this procedure made it possible that the criteria were adapted to the most recent experimental developments.

The most important conclusions of this report are:

- Heavy element research is a vast and fruitful field of research, which made large progress in recent years.
- Our knowledge about the heaviest elements and the interpretation of the underlying physics such as nuclear spectroscopic properties, the nuclear stability and the production is the combined result of nuclear research of several laboratories.
- The main contributions have been made by the Lawrence Berkeley Laboratory at Berkeley, The Joint Institute of Nuclear Reactions at Dubna, and The Gesellschaft für Schwerionenforschung at Darmstadt.
- The three laboratories contributed approximately equal parts in the development of the field of research.
- GSI has the priority for the discovery of the elements 107 and 109 and the main contribution to the discovery of element 108. The work at JINR Dubna on element 108 carried out at the same time shall be acknowledged.

Though we generally agree with the spirit of this report, we realize the difficulties to point out specifically the contributions to the discovery of a new element even more as these experiments are always at the limit of the experimental possibilities. We specifically believe that element 108 has been discovered at GSI. The detection of daughter products alone is insufficient for identification of a new element, however, may become conclusive in combination with the results obtained at GSI. Besides, we accept the conclusions of the transfermium working group, being aware of the fact that never again such a highly renowned group of experts will review the field in such detail.

We are aware of the great contribution of the JINR to the investigation of the heaviest elements in two major points:

- The production of the heaviest elements by formation of weakly excited compound nuclei using targets close to lead.
- The change of spontaneous-fission systematics in the transactinide region.

Both of these ideas were finally confirmed in our research work.

After a thorough discussion of the discovery and naming of elements among scientists of the Berkeley, Dubna, and Darmstadt groups with the aim of presenting to IUPAC a common proposal for all the transfermium elements we proposed, in accordance with the TWG conclusion, names for the elements 107, 108, and 109 to the IUPAC in a letter dated September 4, 1992. We did this after we realized that it was impossible to find a common solution for the elements 101 to 106 between the Berkeley and Dubna groups. We were, however, able to settle all naming problems between Darmstadt and Dubna.

The proposed names are the following:

Element 107 should be named "Nielsbohrium" (Ns).

This name was originally proposed by G. N. Flerov, JINR Dubna, for element 105. In recognition of the discovery of synthesizing elements using lead and bismuth based reactions, the method used by us for the synthesis of the elements 107 to 109, we adopt this name for element 107. We fully agree with the Dubna group that Niels Bohr highly merits to be honoured by the name of an element, and we propose the name for element 107 together with our colleagues from JINR. This suggestion will also help to solve the controversy of naming element 105. Formally the confirmation of this proposal was communicated to IUPAC in a letter of common agreement between Dubna and GSI by a letter dated September 17, 1992.

Element 108 should be named "Hassium" (Hs).

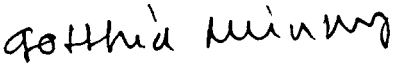
Darmstadt was the former capital of the German state of Hesse. "Hassia" was the Latin name of the state in the middle ages. Our laboratory was founded in 1969 by the initiative of physicists and chemists from Hesse, and it is since then strongly supported by the state government. So we intended to follow an old tradition to name an element after the place of discovery.

Element 109 should be named "Meitnerium" (Mt)

Lise Meitner is one of the outstanding women in nuclear science to whom we feel especially obliged. She was one of Germany's most eminent physicists and was forced to emigrate in 1938, shortly before the discovery of fission by Hahn and Strassmann, the work of whom she initiated.

In contrast to the common use in naming the elements up to element 105, whose names were published at or shortly after their discovery, we made these names public about ten years after the discovery of "Meitnerium" and the examination of our work by the international commission of experts, in a ceremony at GSI Darmstadt on September 7, 1992.

We express our confidence that the leading bodies of IUPAC and IUPAP will accept our name proposals.



G. Münzenberg

on behalf of GSI Heavy Element Research Group

P. Armbruster, F.P. Heßberger, S. Hofmann, M. Leino, G. Münzenberg, W. Reisdorf and K.-H. Schmidt

Heavy Element Research Group
Gesellschaft für Schwerionenforschung
Darmstadt, FRG
29 September 1992

IV. RESPONSE OF TRANSFERMIUM WORKING GROUP TO CRITICISMS

The Transfermium Working Group (TWG) was disbanded on submitting its final report to IUPAP and to IUPAC in 1991. However, we, its members, have carefully considered the reaction of Messrs Ghiorso and Seaborg to that final report and, as individuals, we make the following agreed statement.

1. The TWG consisted of nine men of goodwill who, conjointly and severally, spent some thousands of hours over a period of five years, including seven week-long meetings in seven different countries, three of the meetings being in the laboratories of chief concern, in a microscopic and scrupulous analysis of the discovery of the transfermium elements. We were not self-appointed nor did we proffer our services: we were invited to serve by, and were appointed by, IUPAP and by IUPAC through their appropriate official channels. We were utterly without bias, prejudice or pre-commitment and had no connection with any of the laboratories of chief concern; we did not care who had discovered the elements in question but agreed to find out.

2. The three laboratories of chief concern were, both at our meetings with them and in writing, invited to bring to our notice any matter or publication that they considered to be of relevance; in particular they were, at any early stage, sent a list of the publications known to the TWG with the invitation to add to it anything that might have been missed and that the laboratories wished to be considered by us. It sits ill with Messrs Ghiorso and Seaborg that they should now accuse us of having overlooked publications to which they failed to draw our attention at that time. None of the allegedly overlooked material would have affected our final judgements to the slightest degree.

3. We do not consider that it would be profitable to respond point by point to the scientific criticisms of Messrs Ghiorso and Seaborg; even less do we feel it necessary to respond to those of their criticisms that are not in this category. The reaction from Berkeley is chiefly valuable in illustrating two of the conclusions of our report: (i)"... since different sections of the scientific community may have different views as to the importance and reliability of interpretation of different forms of scientific evidence, the bringing into that belief [that the formation of a new element had indeed been established] may well occur at different times and at different stages of the accumulation of the evidence." (ii)"... we have been acutely conscious of the ever-present temptation ... to wring more from such hardly-gained data than those data are truly able to yield." We might here also recall our conclusion following that second quotation: "... our task ... has been not so much to weigh the facts as to weigh their interpretation."

4. **After detailed examination of all the criticisms from Berkeley we do not find it necessary in any way to change the conclusions of our report.** [Berkeley points out, we grant, that there are six misprints in our report, and we are grateful to them for this helpful emendation, but they are indeed no more than misprints and would not mislead anyone sufficiently involved in the matter to have pursued our reasoning to the level of detail at which they are found.]

(D. H. Wilkinson)
5 May 1993
on behalf of TWG

Correspondence address:
IUPAC Secretariat
Bank Court Chambers
2–3 Pound Way
Templars Square, Cowley,
Oxford OX4 3YF, UK

R.C. Barber, N.N. Greenwood, A.Z. Hrynkiewicz, Y.P. Jeannin, M. Lefort, M. Sakai, I. Ulehla, A.H. Wapstra, D.H. Wilkinson