Letters

Disputed Discovery of Element 105

The recent letter of G. N. Flerov (2 Oct.) raises questions concerning the claims of my group to the discovery of element 105. Flerov's major concern seems to be that neither the formal presentation (*1*) of our results in *Physical Review Letters* (29 June) nor the account of this work by Holcomb in *Science* (15 May, p. 810) give adequate recognition to results obtained by Flerov and his associates that claim the discovery of an isotope of element 105 which decays by spontaneous fission (*2*).

With regard to the publication of the paper in *Physical Review Letters* on element 105, I must respond simply that the paper had been completed before the Dubna preprint was received. Holcomb's report on the other hand was based on an invited paper which I delivered at the Washington meeting of the American Physical Society on 28 April 1970, not long after receipt of this preprint. Our translation of this document was completed on 10 April 1970. In retrospect, it is clear to me that it would have been prudent to insert in press a reference to the new Dubna results, and I apologize to Flerov and his group for not doing so.

It is certainly debatable whether laboratory preprints should be considered as publication in the open literature (there is at least one major research facility in this field which does not receive the Dubna preprints), but that is not my major concern here. It is clear that the work in the two laboratories is completely independent and essentially concurrent (our first detection of the 1.6-second $^{260}105$ alpha activity was in November 1968, but the data were inadequate for publication).

I would like to raise the basic question of what constitutes the *discovery* of a new element. It seems to me that the *discoverer* is the one who first *proves* that he has indeed found a new element. Our published work demonstrates beyond question that we have identified the isotope $^{260}105$ by linking it genetically to its well-known lawrentium daughter, ^{256}Lr. This was done both by the alpha-recoil milking of the daughter and by a time-correlation analysis of mother-daughter events. On the other hand the Dubna discovery of a 2-second spontaneous-fission emitter is *still open to question* as to the identity of the atomic number involved. They have attempted to link this discovery with data published in an earlier preprint (*3*) in which the discovery of element 105 was attributed to the detection of isotopes which decayed by alpha particle emission. In our *Physical Review Letters* communication we discussed this earlier work and showed that it was completely contradicted by our experiments. I would certainly agree that it is *possible* that the 2-second spontaneous-fission activity arises by a branch decay of $^{260}105$ (our present experiments set a limit of about 20 percent) or, more likely, from $^{261}105$, but I believe that it is by no means firmly established that the spontaneous fission is due to element 105.

My lack of confidence in experiments based exclusively on the detection of spontaneous-fission activity stems from our own work as well as that of others. Nothing presented in the Dubna preprint of February 1970 alters my conviction that this mode of decay is not sufficient by itself to conclusively demonstrate that a new element has been formed. Witness the fact that this same controversy between the Dubna and the Berkeley groups has prevailed for several years over our competing claims to the discovery of element 104. In this case a 0.1-second (formerly 0.3-second) spontaneous-fission activity was assigned to $^{260}104$ by the Dubna group and was not confirmed by our work. On the other hand we have positively identified the alpha emitters $^{257}104$, $^{259}104$, and $^{261}104$ by mother-daughter experiments similar to that performed with $^{260}105$. In addition we have found a 10-millisecond spontaneous-fission activity that we believe is due to $^{258}104$, but its positive identification suffers from aforementioned difficulties.

Our position in regard to the naming of these two elements is very straightforward. We believe that we have found and characterized isotopes with these atomic numbers in a clear and unambiguous manner, and to illustrate our confidence we have proposed the names rutherfordium (Rf) for element 104 and hahnium (Ha) for element 105. If our findings stand over a period of time they will be recognized in the traditional way—acceptance by the scientific community and its established nomenclature committee. If, on the other hand, it becomes obvious that prior or concurrent work should take precedence, then presumably other names should and will be recognized.

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References

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2. G. N. Flerov, Yu. Ts. Oganesian, Yu. V. Lobanov, Yu. A. Lazarev, S. P. Tretiakova, Joint Institute for Nuclear Research Report JINR 4932 (1970), unpublished.
3. G. N. Flerov, V. A. Druin, A. G. Demin, Yu. V. Lobanov, N. K. Skobelev, G. N. Akapiev, B. V. Fefilov, I. V. Kolesov, K. A. Gavrilov, Yu. P. Kharitonov, L. P. Chelnokov, Joint Institute for Nuclear Research Report JINR-P7-3808 (1968), unpublished.

Science Teachers: Ignored in a Crisis

Spurned may not be quite accurate; perhaps *ignored* describes better what is happening to science teaching today. Recently a 14-member commission presented a report (*1*) on pesticides and their relationship to environmental health to the Secretary of Health, Education, and Welfare. More than 150 additional university, government, and industrial scientists contributed advice, information, or services to the commission. The problems of environmental pollution by pesticides were reviewed thoroughly in the report and 14 recommendations were made to repair the damage already done and to prevent future instances of this type of pollution.