Heisenberg and the bomb

SIR — Walker[1] misunderstands some essential issues involved in the Farm Hall documents, asserting that Werner Heisenberg's "competence" was somehow at issue in an extended exchange with Samuel Goudsmit, the physicist who led the Allies' *Alsos* mission into Germany. Goudsmit, who did not equate genius with infallibility, set Heisenberg in the company of Einstein and Bohr[2], but questioned whether Heisenberg and his wartime colleagues had reached a working understanding of fast fission weapons. A claim to such knowledge was implicit in the suggestion that some of the German physicists had refrained from building bombs for the Nazi regime on moral grounds[3].

The *Alsos* mission discovered evidence of considerable confusion in both the administration of the several competing German uranium projects and in their physics, including a secret report by Gerlach from late 1944 alluding to nuclear explosives involving tons of uranium, rather than the tens of kilograms fast fission weapons actually require. Objecting that Goudsmit's book *Alsos*[4] had overstated the case, Heisenberg insisted that he, at least, and some other theoreticians in his circle had understood the key issues of fast-fission physics and plutonium breeding. Like any practicing scientist, Goudsmit distinguished between superficial speculation and the kind of working knowledge from which practical consequences can flow, and carefully analysed the record for evidence that the Germans possessed such knowledge. Goudsmit did from 1949 onwards[5] acknowledge Heisenberg's awareness of plutonium breeding as a theoretical possibility (the Germans did not succeed in creating even microscopic quantities of the element with which to experiment). No evidence that Heisenberg brought forward, however, documented the clear conception of a fast-fission bomb.

Heisenberg's conversations at Farm Hall reveal now, as they did to Goudsmit when he reviewed them in 1945, the thinking of a physicist who has considered the broad possibilities of nuclear weapons and some of the difficulties involved in their construction, but whose picture of how they would work is not clear. This becomes especially evident in Heisenberg's response at Farm Hall on 6 August to the news of the Hiroshima explosion. When he moved from general comments to actually estimating the minimum size of a uranium bomb, that day, his reasoning revealed some basic misconceptions about fast-fission bomb physics. Heisenberg's model was a reaction spreading from the centre to the periphery of a uranium-235 sphere, fissioning 10^{24} uranium-235 nuclei in the process. He determined the minimum size of the sphere by requiring that diffusing neutrons must collide with and fission this many nuclei before reaching the surface. On that basis, he estimated the radius of the sphere as 54 cm, which he said gave a minimum mass of 1 ton (Actually, this much uranium weighs about 13 tons. Walker, inexplicably, reports Heisenberg's estimate as "hundreds of tons"). The appropriate criterion for a self-sustaining reaction, however, is that neutron generation exceed neutron loss through the surface, which gives a very different result. Nor do actual fast-fission explosions go to completion: outward expansion of the fissioning material halts the chain reaction very quickly — a decisive concern in fast-fission bomb design, which Heisenberg assures Hahn applies only to slow-neutron chain reactions (The Hiroshima bomb, in fact, fissioned only about 2% of its uranium[6]).

The disparity between the actual (bare) critical mass of 60 kg and Heisenberg's working value of a ton or more helps, of course, to explain his utter incredulity at the news of Hiroshima, because it defines the difference between a dauntingly difficult task and an impossible one. (The Allied project, ironically, gained decisive impetus when Frisch and Peierls in early 1940 *under*-estimated the critical mass as 600 grams and so made the goal seem less difficult[6].) Heisenberg, a week after the jolting lesson in critical mass provided by Hiroshima, revised his first, ill-conceived estimate, and presented a more elaborate calculation, which placed the critical mass at tens of kilograms. His lecture at Farm Hall on 14 August, however, is a primitive, unsophisticated treatment of the subject, treating a bomb as though it were a reactor. A second week's thought would have shown Heisenberg the unreliability of his argument and allowed him to discover some mistakes (using the total cross section to determine the mean free path introduces a spurious factor of one half in the critical mass). Walker, however, counts the lecture as proving Heisenberg's mastery of fission bomb physics, as if being able to arrive at a rough understanding after being given the fact of the bomb were the same as possessing it beforehand.

In Walker's view, the different courses of the German and Allied programmes can be fully explained by a single, well-informed decision taken by German Army Ordnance on the basis of wartime priorities not to proceed with a full-scale effort. Beyond oversimplifying things, this reasoning imputes to the Germans an understanding of the implications and difficulties of a commitment to build bombs that is obvious now but was not then. The result is a view that excludes from examination the large and consequential differences between the situation of the Allied scientists and that of their counterparts under totalitarianism, without which this history becomes merely an exercise in charting the German bureaucracy and of guessing what the German physicists might have done had they known more and been given better support. These are just the kinds of errors and confusions we hope historians will protect us *against*.

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Proterozoic carbon cycle

SIR — Des Marais *et al.*[1] argue persuasively for a non-uniformitarian view of the Proterozoic carbon cycle, envisioning a substantial buildup of the sedimentary organic carbon reservoir between 2.5 and 0.6 Gyr (billion years) ago. Having in its favour several elements of plausibility, the new hypothesis should stimulate the field; I propose that some residual questions be viewed in this light.

An uncomfortable corollary of the new concept is the relative constancy of δ_{carb} (the natural abundance of ^{13}C in marine sedimentary carbonates) in the face of a pronounced increase of ^{13}C abundance in organic matter (δ_{org}) over the same time interval (see figure). Defining δ_{in} as the ^{13}C abundance in the total carbon input to the atmosphere–ocean system and f_{carb} and f_{org}, respectively, as the fractions of carbon buried in inorganic and organic form ($f_{carb} = 1 - f_{org}$), then keeping δ_{carb} relatively stable within the constraints of the crustal carbon isotope mass balance, $\delta_{in} = f_{carb}\delta_{carb} + f_{org}\delta_{org}$ would require that increasingly larger fractions of isotopically heavier C_{org} go along with increasingly smaller proportions of C_{carb} with quasi-constant δ_{carb} (or, equivalently, the