POSTSCRIPT TO *COPENHAGEN*
by Michael Frayn

[Prepared for the US production playscript edition.]

Where a work of fiction features historical characters and historical events it's reasonable to want to know how much of it is fiction and how much of it is history. So let me make it as clear as I can in regard to this play.

The central event in it is a real one. Heisenberg *did* go to Copenhagen in 1941, and there *was* a meeting with Bohr, in the teeth of all the difficulties encountered by my characters. He almost certainly went to dinner at the Bohrs' house, and the two men almost certainly went for a walk to escape from any possible microphones, though there is some dispute about even these simple matters. The question of what they actually said to each other has been even more disputed, and where there's ambiguity in the play about what happened, it's because there is in the recollection of the participants. Much more sustained speculation still has been devoted to the question of what Heisenberg was hoping to achieve by the meeting. All the alternative and co-existing explications offered in the play, except perhaps the final one, have been aired at various times, in one form or another.

Most anxious of all to establish some agreed version of the meeting was Heisenberg himself. He did indeed go back in 1947 with his British minder, Ronald Fraser, and attempted to find some common ground in the matter with Bohr. But it proved to be too delicate a task, and (according to Heisenberg, at any rate, in his memoirs) 'we both came to feel that it would be better to stop disturbing the spirits of the past.' This is where my play departs from the historical record, by supposing that at some later time, when everyone involved had become spirits of the past themselves, they argued the question out further, until they had achieved a little more understanding of what was going on, just as they had so many times when they were alive with the intractable difficulties presented by the internal workings of the atom.

The account of these earlier discussions in the twenties reflects at any rate one or two of the key topics, and the passion with which the argument was conducted, as it emerges from the biographical and autobiographical record. I am acutely aware of how over-simplified my version is. Max Born described the real story as not so much 'a straight staircase upwards, but a tangle of interconnected alleys,' and I have found it impossible to follow these in any detail (even where I can begin to understand them). In particular I have grossly understated the crucial role played by Born himself and by his pupil Pascual Jordan at Göttingen in formulating quantum mechanics (it was Born who supplied the understanding of matrices that Heisenberg lacked, and the statistical interpretation of Schrödinger's wave function), and of Wolfgang Pauli in Hamburg, whose exclusion principle filled in one of the key pieces in the puzzle.

But the account of the German and American bomb programmes, and of the two physicists' participation in them, is taken from the historical record; so is the fate of Danish Jewry; Heisenberg's experiences in Germany before and during the war, his subsequent internment, and the depression that clouded his later years. I have filled out some of the details, but in general what he says happened to him - at the end of the First World War, on Heligoland, during his nocturnal walk in Faelled Park, during the Berlin air-raid and his internment, and on his ride across Germany, with its near-fatal encounter along the way - is based very closely upon the accounts he gave in life.

The actual words spoken by my characters are of course entirely their own. If this needs any justification then I can only appeal to Heisenberg himself. In his memoirs dialogue plays an important part, he says, because he hopes 'to demonstrate that science is rooted in conversations.' But, as he explains, conversations, even real conversations, cannot be reconstructed literally several decades later. So he freely reinvents them, and appeals in his turn to Thucydides. (Heisenberg's father was a professor of classics, and he was an accomplished classicist himself, on top of all his other distinctions.) Thucydides explains in his preface to the *History of the Peloponnesian War* that, although he had avoided all 'storytelling', when it came to the speeches, 'I have found it impossible to remember their exact wording. Hence I have made each orator speak as, in my opinion, he would have done in the circumstances, but keeping as close as I could to the train of thought that guided his actual speech.' Thucydides was trying to give an account of speeches that had actually been made, many of which he had himself heard. Some of the dialogue in my play represents speeches that must have been made in one form or another; some of it speeches that were certainly never made at all. I hope, though, that in some sense it respects the Thucydidean principle, and that speeches (and indeed actions) follow in so far as possible the original protagonists' train of thought.

But how far is it possible to know what their train of thought was? This is where I have departed from the established historical record - from any possible historical record. The great challenge facing the storyteller and the historian alike is to get inside people's heads, to stand where they stood and see the world as they saw it, to make some informed estimate of their motives and intentions - and this is precisely where recorded and recordable history cannot reach. Even when all the external evidence has been mastered, the only way into the protagonists' heads is through the imagination. This indeed is the substance of the play.

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I can't claim to be the first person to notice the parallels between Heisenberg's science and his life. They provide David Cassidy with the title (*Uncertainty*) for his excellent biography (the standard work in English). 'Especially difficult and controversial,' says Cassidy in his introduction, 'is a retrospective evaluation of Heisenberg's activities during the Third Reich and particularly during World War II. Since the end of the war, an enormous range of views about this man and his behaviour have been expressed, views that have been fervently, even passionately, held by a variety of individuals. It is as if, for some, the intense emotions unleashed by the unspeakable horrors of that war and regime have combined with the many ambiguities, dualities, and compromises of Heisenberg's life and actions to make Heisenberg himself subject to a type of uncertainty

principle...' Thomas Powers makes a similar point in his extraordinary and encyclopaedic book *Heisenberg's War*, which first aroused my interest in the trip to Copenhagen; he says that Heisenberg's later reticence on his role in the failure of the German bomb programme 'introduces an element of irreducible uncertainty.'

Cassidy does not explore the parallel further. Powers even appends a footnote to his comment: 'Forgive me.' The apology seems to me unnecessary. It's true that the concept of uncertainty is one of those scientific notions that has become common coinage, and generalised to the point of losing much of its original meaning. The idea as introduced by Heisenberg into quantum mechanics was precise and technical. It didn't suggest that everything about the behaviour of particles was unknowable, or hazy. What it limited was the simultaneous measurement of 'canonically conjugate variables', such as position and momentum, or energy and time. The more precisely you measure one variable, it said, the less precise your measurement of the related variable can be; and this ratio, the uncertainty relationship, is itself precisely formulable.

None of this, plainly, applies directly to our observations of thought and intention. Thoughts are not locatable by pairs of conjugate variables, so there can be no question of a ratio of precision. Powers seems to imply that in Heisenberg's case the uncertainty arises purely because 'questions of motive and intention cannot be established more clearly than he was willing to state them.' It's true that Heisenberg was under contradictory pressures after the war which made it particularly difficult for him to explain what he had been trying to do. He wanted to distance himself from the Nazis, but he didn't want to suggest that he had been a traitor. He was reluctant to claim to his fellow-Germans that he had deliberately lost them the war, but he was no less reluctant to suggest that he had failed them simply out of incompetence.

But the uncertainty surely begins long before the point where Heisenberg might have offered an explanation. He was under at least as many contradictory pressures at the time to shape the actions he later failed to explain, and the uncertainty would still have existed, for us and for him, even if he had been as open, honest, and helpful as it is humanly possible to be. What people say about their own motives and intentions, even when they are not caught in the traps that entangled Heisenberg, is always subject to question - as subject to question as what anybody else says about them. Thoughts and intentions, even one's own - perhaps one's own most of all - remain shifting and elusive. There is not one single thought or intention of any sort that can ever be precisely established.

What the uncertainty of thoughts does have in common with the uncertainty of particles is that the difficulty is not just a practical one, but a systematic limitation which cannot even in theory be circumvented. It is patently not resolved by the efforts of psychologists and psycho-analysts, and it it will not be resolved by neurologists, either, even when everything is known about the structure and workings of the brain, any more than semantic questions can be resolved by looking at the machine code of a computer. And since, according to the so-called 'Copenhagen Interpretation' of quantum mechanics - the interconnected set of theories that was developed by Heisenberg, Bohr, and others in the twenties - the whole possibility of saying or thinking anything about the world, even the most apparently objective, abstract aspects of it studied by the natural sciences, depends upon human observation, and is subject to the limitations which the human mind imposes, this uncertainty in our thinking is also fundamental to the nature of the world.

'Uncertainty' is not a very satisfactory word to come at this. It sits awkwardly even in its original context. You can be uncertain about things which are themselves entirely definite, and about which you could be entirely certain if you were simply better informed. Indeed, the very idea of uncertainty seems to imply the possibility of certainty. Heisenberg and Bohr used several different German words in different contexts. Bohr (who spoke more or less perfect German) sometimes referred to *Unsicherheit*, which means quite simply unsureness. In Heisenberg's original paper he talks about *Ungenauigkeit* - inexactness - and the most usual term now in German seems to be *Unschärfe* - blurredness or fuzziness. But the word he adopts in his general conclusion, and which he uses when he refers back to the period later in his memoirs, is *Unbestimmtheit*, for which it's harder to find a satisfactory English equivalent. Although it means uncertainty in the sense of vagueness, it's plainly derived from *bestimmen*, to determine or to ascertain. This is reflected better in the other English translation which is sometimes used, but which seems to be less familiar: indeterminacy. 'Undeterminedness' would be closer still, though clumsy. Less close to the German, but even closer to the reality of the situation, would be 'indeterminability'.

Questions of translation apart, Heisenberg's choice of word suggests that, at the time he wrote his paper, he had not fully grasped the metaphysical implications of what he was saying. Indeed, he concludes that the experiments concerned are affected by *Unbestimmtheit* 'purely empirically.' He was not, as Bohr complained, at that time greatly interested in the philosophical fallout from physics and mathematics (though he became much more so later on in life), and he was publishing in a hurry, as Bohr also complained, before he had had a chance to discuss the work with either Bohr or anyone else. His paper seems to imply that electrons have definite orbits, even if these are unknowable; he talks about a quantum of light completely throwing the electron out of its 'orbit', even though he puts the word into inverted commas, and says that it has no rational sense here. The title of the paper itself reinforces this impression: *Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik.* Again there are translation problems. *'Anschaulich'* means graphic, concrete, 'look-at-able'; the title is usually translated as referring to the 'perceptual' content of the disciplines concerned, which again seems to suggest a contrast with their unperceived aspects - as if Heisenberg were concerned merely about our difficulties in visualising abstractions, not about the physical implications of this.

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The Copenhagen Interpretation of quantum mechanics has been scientific orthodoxy for most of this century, and is the theoretical basis (for better or worse) on which the century's dramatic physical demonstrations of nuclear forces have been constructed. But it has not gone unchallenged. Einstein never accepted it, though he could never find a way round it. The

mathematician Roger Penrose regards the present state of quantum theory as 'provisional', and quotes Schrödinger, de Broglie, and Dirac as forerunners in this view.

An alternative to the Copenhagen Interpretation, explaining the apparent superimposition of different states that appears at the quantum level in terms of a multiplicity of parallel worlds, was developed after the Second World War by Hugh Everett III, who had been a graduate student of John Wheeler, Bohr's associate in the famous paper which opened the way to an understanding of uranium fission. David Deutsch, who proposes an extreme version of Everett's ideas in his book *The Fabric of Reality*, claims that 'hardly anyone' still believes in the Copenhagen Interpretation. I have put this view to a number of physicists. They all seemed greatly surprised by it; but maybe I have hit upon precisely the supposed handful who remain in the faith.

Another follower of Everett (though he seems to differ quite sharply from Deutsch) is Murray Gell-Mann, who with Yuval Ne'eman revolutionised elementary particle theory in the sixties with the introduction of the quark, in its three different 'colours' and six different 'flavours', as the fundamental unit of the material world. Gell-Mann believes that quantum mechanics is the fundamental tool for understanding the universe, but he sees the Copenhagen Interpretation, with its dependence upon an observer and the human act of measurement, as anthropocentric, and as characterizing merely a special case that he calls 'the approximate quantum mechanics of measured systems.' I hesitate to express any reservations about something I understand so little, particularly when it comes from such an authority, but it seems to me that the view which Gell-Mann favours, and which involves what he calls alternative 'histories' or 'narratives', is precisely as anthropocentric as Bohr's, since histories and narratives are not freestanding elements of the universe, but human constructs as subjective and as restricted in their viewpoint as the act of observation.

The relevance of indeterminacy to quantum mechanics has also been challenged. A version of the famous thought experiment involving two slits has now actually been carried out in the laboratory (at the University of Konstanz). It confirms, as Bohr hypothesised, that while an unobserved particle seems to pass through both slits, so that it forms a characteristic interference pattern on a screen beyond them, any act of observation that attempts to determine which of the two paths the particle actually follows necessarily destroys the phenomenon, so that the interference pattern vanishes. But the experiment appears to suggest that, although the uncertainty principle is true, it accounts for discrepancies far too small to explain the loss of interference. The observation in the laboratory experiment, moreover, was carried out not, as in the old thought experiment, by hitting the particle involved with a photon, which transfers part of its energy to the particle and so alters its path, but by a way of marking with microwaves which has almost no effect on the particle's momentum.

Some physicists now accept that the loss of interference is caused by a much stranger and less quasi-classical aspect of the quantum world - entanglement. The notion was introduced by Schrödinger in 1935, and suggests that where quantum-mechanical entities become involved with each other (as with the particle and the photon), they form states of affairs which continue to have a collective identity and behaviour, even though their components have physically separated again. The difficulties in this are obvious, but there is no interpretation of quantum-mechanical phenomena that does not involve breathtaking challenges to the logic of our everyday experience.

For the references to all these developments see the bibliography at the end of this Postscript.

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What about my characters? Are they anything like their originals?

It's impossible to catch the exact tone of voice of people one never knew, with only the written record to go on, especially when most of what their contemporaries recall them as saying was originally said in other languages. There are also more particular problems with all three of my protagonists.

Bohr, for a start, was as notorious for his inarticulacy and inaudibility as he was famous for his goodness and lovability. He was fluent in various languages, but I have heard it said that the problem was to know which language he was being fluent in. Schrödinger, after his epic confrontation with Bohr in 1926, described him as often talking 'for minutes almost in a dreamlike, visionary and really quite unclear manner, partly because he is so full of consideration and constantly hesitates - fearing that the other might take a statement of his [Bohr's] point of view as an insufficient appreciation of the other's...' My Bohr is necessarily a little more coherent than this - and I have been told by various correspondents who knew him that in private, if not in public, he could be much more cogent and incisive than Schrödinger evidently found him.

The problem with Margrethe is that there is relatively little biographical material to go on. She and Niels were plainly mutually devoted, and everything suggests that she was as generally loved as he was. She had no scientific training, but Bohr constantly discussed his work with her, presumably avoiding technical language - though she must have become fairly familiar with even that since she typed out each draft of his papers. I suspect she was more gracious and reserved than she appears here, but she plainly had great firmness of character - in later life she was known as *Dronning* (Queen) Margrethe. She was always cooler about Heisenberg than Bohr was, and she was openly angry about his visit in 1941. According to Bohr she objected strongly to his being invited to the house, and relented only when Bohr promised to avoid politics and restrict the conversation to physics. Bohr himself always refused to be drawn about Heisenberg's trip in 1941, but she insisted, even after the war, even after all Heisenberg's attempts to explain, 'No matter what anyone says, that was a hostile visit.'

The problem with Heisenberg is his elusiveness and ambiguity, which is of course what the play is attempting to elucidate. The one thing about him that everyone agreed upon was what Max Born, his mentor in Göttingen, called 'his unbelievable quickness and precision of understanding.' The contrast with Bohr is almost comic. 'Probably [Bohr's] most characteristic property,' according to George Gamow, 'was the slowness of his thinking and comprehension.'

As a young man Heisenberg seems to have had an appealing eagerness and directness. Born described him as looking like a simple farm boy, with clear bright eyes, and a radiant expression on his face. Somebody else thought he looked 'like a

bright carpenter's apprentice just returned from technical school.' Victor Weisskopf says that he made friends easily, and that everyone liked him. Bohr, after their first meeting in 1922, was delighted by Heisenberg's 'nice shy nature, his good temper, his eagerness and his enthusiasm.' There was something about him of the prize-winning student, who is good at everything required of him, and Bohr was not the only father-figure to whom he appealed. He had a somewhat similar relationship to Sommerfeld, his first professor in Munich, and in his difficulties with the Nazis he turned to two elders of German physics for counsel, Max Planck and Max von Laue. His closest friend and colleague was probably Carl Friedrich von Weizsäcker, who was younger than him, but it is striking that during his internment the person he chose to confide his explanation of the Hiroshima bomb to was not Weizsäcker, who was interned with him (although he may well have discussed it with him already), but the 66-year-old Otto Hahn.

The American physicist Jeremy Bernstein says that 'he had the first truly quantum-mechanical mind - the ability to take the leap beyond the classical visualizing pictures into the abstract, all-but-impossible-to-vizualise world of the subatomic...' Cassidy believes that a great part of his genius was his 'ability to adopt a serviceable solution regardless of accepted wisdom.' Rudolf Peierls stresses his intuition. He would 'almost always intuitively know the answer to a problem, then look for a mathematical solution to give it to him.' The obverse of this, according to Peierls, is that 'he was always very casual about numbers' - a weakness that seems to have contributed to his downfall - or his salvation - in the atomic bomb programme.

Margrethe always found him difficult, closed, and oversensitive, and this propensity to be withdrawn and inturned was exacerbated as life went on - first by his political problems in the thirties, and then by his efforts to reconcile the moral irreconcilables of his wartime work. His autobiographical writing is rather stiff and formal, and his letters to Bohr, even during the twenties and thirties, are correct rather than intimate. Throughout the period of their closest friendship they addressed each other with the formal *Sie*, and switched to *du* only when Heisenberg also had a chair.

The conversations that Heisenberg claimed such freedom to recreate in his memoirs are stately. Much more plausibly colloquial is the transcript of David Irving's long interview with him for *The Virus House*, Irving's history of the German bomb programme, though he is still (naturally) watchful. In the transcripts of the relatively unguarded conversations that the German atomic team had among themselves during their internment, where Heisenberg emerges as the dominant figure, both morally and practically, a certain hard-headed worldlinesss can be detected. He is much concerned with professional prospects, and with how they might make some money out of their wartime researches. When one of the others says that if they agree to work on atomic matters under Allied control they will be looked down upon as traitors 'in the eyes of the masses', Heisenberg replies: 'No. One must do that cleverly. As far as the masses are concerned it will look as though we unfortunately have to continue our scientific work under the wicked Anglo-Saxon control, and that we can do nothing about it. We will have to appear to accept this control with fury and gnashing of teeth.'

There was always something a little sharp and harsh about him, something that at its best inspired respect rather than love, and that after the war occasioned really quite astonishing hostility and contempt. Even Samuel Goudsmit turned against him. Goudsmit was an old friend and colleague; when the investigators of the Alsos mission, the Allied agency for gathering intelligence on German atomic research, for which he was working, finally broke into Heisenberg's office in 1945, one of the first things they saw was a picture of the two of them together that Heisenberg had kept there as a memento of happier days. But when Goudsmit subsequently interrogated Heisenberg he found him arrogant and self-involved. Goudsmit had understandably bitter feelings at the time - he had just discovered the record of his parents' death in Auschwitz. Heisenberg was also caught in a false position. Confident that his team had been far ahead of the Americans, he offered Goudsmit his services in initiating them into the secrets of uranium fission. (Goudsmit did nothing to correct his misapprehension, which gave Heisenberg, when the truth finally came out, grounds for returning Goudsmit's bitterness.) In his superficial and strangely unimpressive book on Alsos, Goudsmit wrote about Heisenberg and his team with contemptuous dismissal, and in the year-long correspondence in the American press that followed its publication, accused him of self-importance and dishonesty.

Weisskopf gave a reception for Heisenberg during his trip to America in 1949, but about half the guests - including many people from the Los Alamos team - failed to appear, explaining to Weisskopf that they didn't want to shake the hand of the man who had tried to build a bomb for Hitler. Even Cassidy, who gives full measure to Heisenberg as a physicist in his biography, is notably cool and cautious in his assessment of Heisenberg's role in the German bomb programme. Ronald Fraser, the British intelligence officer who escorted Heisenberg back to Copenhagen in 1947 (the British seem to have been frightened that he would defect to the Russians, or be kidnapped by them) replied to Irving's inquiry about the trip in tones of patronising contempt that seem slightly unhinged. 'The whole story of "a kind of confrontation",' he wrote to Irving, 'in the matter of his 1941 natter with Bohr in the Tivoli Gardens [sic] is a typical Heisenberg fabrication - maybe a bit brighter than a thousand others, but like them all a product of his *Blut und Boden* guilt complex, which he rationalises that quickly that the stories become *for him* the truth, the whole truth, and nothing but the truth. Pitiful, in a man of his mental stature.'

The historian Paul Lawrence Rose, who has focussed upon Heisenberg as an emblem for what he regards as the general failings of German culture, also takes a remarkably high moral tone. In a paper he wrote in 1984, entitled *Heisenberg, German Morality and the Atomic Bomb*, he talked about Heisenberg's 'guff,' his 'self-serving, self-deluding claims,' and his 'elementary moral stupidity.' After a further fourteen years research Professor Rose returned to the subject in 1998 in a full-length book which was published after the play was produced, and which has attracted considerable attention, *Heisenberg and the Nazi Atomic Bomb Project: a Study in German Culture*. His contempt for Heisenberg remains unmoderated. He believes that Heisenberg failed, in spite of his perfect readiness to serve the Nazi regime, because of his arrogance and wrong-headedness, and because he embodied various vices of German culture in general, and of the Nazi regime in particular, whose values he had absorbed.

It is a difficult book to read - Rose can scarcely quote a word of Heisenberg's without adding his own disparaging qualification. Here is a selection of his interjections on two facing pages taken more or less at random: '... self-incriminating... a somewhat inadequate explanation... this inconsistency... the falseness of these lame excuses.... a characteristic Heisenberg lie... Heisenberg's usual facile rationalising ability... Heisenberg then went on glibly to recollect... the delusory nature of Heisenberg's memory...'

You wonder at times whether it wouldn't look better if it was handwritten in green ink, with no paragraph breaks. Rose seems to be aware himself of the effect he is producing. He realises, he says, that some readers may 'find distasteful the recurrent moral judgments passed on Heisenberg.' They may also, he thinks, be put off by what seems a 'lack of sympathy with German culture' - he cannot say, he confesses, that his 'British background' has made him entirely sympathetic to it. He is at pains to distance himself from any unfortunate echoes that this attitude may awaken: he hopes that readers will not accuse him of 'unthinkingly preaching a crude view of German "national character," whatever that term may mean.' What he is concerned with, he explains, is not that at all, but 'the enduring nature of what one might call the "deep culture" of Germany... In this book I have tried to penetrate into how Germans think - or rather, perhaps, used to think - and to show how radically different are German and what I have termed "Western" mentalities and sensibilities.' It is this that underlies what he calls, without apparent irony, 'the Heisenberg problem.'

Some of his evidence induces a certain dizziness. He quotes without comment, as the epigraph to a chapter, a remark by Albert Speer, the Nazi Minister of Armaments: 'I do hope Heisenberg is not now claiming that they tried, for reasons of principle, to sabotage the project by asking for such minimal support!' It's true that any claim to have sabotaged the project, particularly for reasons of principle, would represent an astonishing departure from Heisenberg's habitual caution on the subject. But the question is not what Speer hoped, but whether Heisenberg *did* make such a claim.

So did he or didn't he? Rose doesn't tell us, and the only reference he gives is Gitta Sereny's new book, *Albert Speer: His Battle with Truth*. The allusion is to the crucial meeting at Harnack House in 1942, mentioned in the play. Speer said in his memoirs that he was 'rather put out' by the very small amount of money that Heisenberg requested to run the nuclear research programme. In an earlier draft of the manuscript (the 'Spandau draft'), says Sereny, he had added in brackets the remark that Rose quotes - and Heisenberg, she says, 'did in fact try precisely that after the war.'

So he *did* make the claim! But when and where? Sereny doesn't tell us. The only references to the smallness of the sums of money he asked for that I can find in the record are the one quoted, by Speer himself, and another by Field Marshal Milch, Goering's deputy in the Luftwaffe, who was also present at the meeting. There's certainly nothing about it in Heisenberg's memoirs, or in Robert Jungk's book, *Brighter Than a Thousand Suns*, or in Heisenberg's long interview with Irving, or in the other two obvious places, his interview with *Der Spiegel* in 1967, when Irving's book was published, or his review of the book in the *Frankfurter Allgemeine Zeitung*. I hardly like to put myself forward to fill the gap, but so far as I know the only reference he made to the subject was posthumously and fictitiously in my play.

Sereny, like Rose, is markedly unenthusiastic about Heisenberg in general. She goes on to argue that Heisenberg's claims about his intentions in meeting Bohr in 1941 'are now shown by Speer's Spandau account to be false', though quite how this is so she doesn't explain. About what she calls 'the facts' of the Copenhagen meeting she is remarkably brisk. In the conversation '...which Bohr subsequently reported to his associates at the Niels Bohr Institute, Heisenberg had made his political stand crystal clear. His team, he told Bohr, had gone some way towards discovering a way to produce an atom bomb. Germany was going to win the war, probably quite soon, and Bohr should join them now in their efforts.'

The idea that Heisenberg was inviting Bohr to work on the German bomb is on the face of it the least plausible out of all the possible interpretations that have been offered. It is completely at odds with what Weisskopf recalls Bohr as saying in 1948, and with what Bohr is on record as telling Chadwick at the time. In any case, the suggestion that Heisenberg thought he might be able to import someone half-Jewish into the most secret research programme in Nazi Germany is frankly preposterous.

So what is Sereny's evidence for her account of the meeting? At this point the sense of vertigo returns, and one begins to have the feeling that one is in an Escher drawing, where the stairs up to the floor above somehow lead back to the floor one is already on, because the only reference she gives is... Powers, Heisenberg's great champion, in *Heisenberg's War*.

And it's true - Powers *does* quote an opinion to this effect (and it's the only possible source for it anywhere, so far as I know). He says he was told by Weizsäcker that some person or persons unnamed in Copenhagen, 44 years after the event, had told *him* that this is what Bohr had said he had believed Heisenberg's intention to be. One might think that this is rather faint evidence. In any case, even if it really is what Bohr believed, it is of course not what Weizsäcker believed, or Powers either. They are reporting Bohr's alleged belief as a possible misapprehension on his part which might have explained his anger. Indeed, Powers's own reading of the situation is precisely the one that Sereny claims to be discredited by Speer's remark.

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Goudsmit gradually modified his opinion, and his final judgment on Heisenberg, when he died in 1976, was a generous one which goes some way to expunging the dismissive tone of his book: 'Heisenberg was a very great physicist, a deep thinker, a fine human being, and also a courageous person. He was one of the greatest physicists of our time, but he suffered severely under the unwarranted attacks by fanatical colleagues. In my opinion he must be considered to have been in some respects a victim of the Nazi regime.'

Robert Jungk, one of the few authors who have ever attempted to defend Heisenberg, modified his opinion in the opposite direction. In *Brighter than a Thousand Suns*, originally published in 1956, he suggested that the German physicists had managed to avoid building nuclear weapons for conscientious reasons, and quoted Heisenberg as saying that, 'under a dictatorship active resistance can only be practised by those who pretend to collaborate with the regime. Anyone speaking out

openly against the system thereby indubitably deprives himself of any chance of active resistance.' But Jungk later changed his mind, and described the notion of passive resistance on the part of the German physicists as a 'myth'. He had contributed to spreading it, he said, out of an 'esteem for those impressive personalities which I have since realized to be out of place.'

For a really spirited and sustained defence Heisenberg had to wait until Powers published his book in 1993. It is a remarkable piece of work, journalistic in tone, but generous in its understanding and huge in its scope. A little too huge, perhaps, because Powers is unable to resist being side-tracked from the main narrative by the amazing byways that he perpetually finds opening off it. I recommend it particularly to other dramatists and screenwriters; there is material here for several more plays and films yet.

His central argument is that the Allied bomb programme succeeded because of the uninhibited eagerness of the scientists to do it, particularly of those exiles who had known Nazism at first hand, and who were desperate to pre-empt Hitler; while the German programme failed because of the underlying reluctance of scientists in Germany to arm Hitler with the bomb, however strong their patriotism, and however much they wanted to profit from the possibilities for research. 'Zeal was needed,' he says; 'its absence was lethal, like a poison that leaves no trace.'

But he goes further, and argues that Heisenberg 'did not simply withhold himself, stand aside, let the project die. He killed it.' He tries to show that at every point Heisenberg was careful to hold out enough hope to the authorities to ensure that he and and his team were left in charge of the project, but never enough to attract the total commitment and huge investment that would have offered the only real hope of success. 'Heisenberg's caution saved him. He was free to do what he could to guide the German atomic research effort into a broom closet, where scientists tinkered until the war ended.'

Cassidy, reviewing the book in *Nature*, described it as a good story, but insisted that 'as history it is incredible.' Rose dismisses it as 'entirely bogus' and 'a scholarly disaster'. Powers acknowledged ruefully, in a recent letter to the *Times Literary Supplement*, that he had failed to convince any historian who had pronounced upon the matter.

The play is not an attempt to adjudicate between these differing views of Heisenberg's personality, or these differing accounts of his activities. But it would have been impossible to write it without taking *some* view of Powers' version of events, so here, for what it is worth, is a brief summary of the case, and of my own hesitant view of it. The evidence is confused and contradictory, and making any sense of it involves balancing probabilities and possibilities almost as indeterminable as Heisenberg found events inside the atom.

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Some of the evidence undoubtedly appears to support Powers's thesis in its stronger form, that Heisenberg deliberately sabotaged the project.

In the first place there are two scraps of direct testimony. One is a message brought to America in 1941 by a departing German Jewish academic called Fritz Reiche. It was from Fritz Houtermans, the German physicist who had just realised that if they could get a reactor going it would produce plutonium, and that plutonium would be a fissile alternative to the U235 that they could not separate. Reiche testified later that he had passed it on to a group of scientists working at Princeton, including Wolfgang Pauli, John von Neumann, and Hans Bethe. As Rudolf Ladenburg, the physicist who arranged the meeting, recorded it afterwards, Houtermans wanted it to be known that 'a large number of German physicists are working intensively on the problem of the uranium bomb under the direction of Heisenberg,' and that 'Heisenberg himself tries to delay the work as much as possible, fearing the catastrophic results of a success.'

Rose dismisses Houtermans as a proven liar, and records that Reiche later appeared to withdraw his belief in Heisenberg's opposition to the project. But neither of these objections seems immediately relevant to the consistency of Reiche's and Ladenburg's testimony.

The second scrap of evidence is even more direct, but much more dubious. Heisenberg's American editor, Ruth Nanda Anshen, records receiving a letter from him in 1970 in which he claimed that, 'Dr Hahn, Dr von Laue and I falsified the mathematics in order to avoid the development of the atom bomb by German scientists.'

The letter itself has apparently vanished from the record. Rose nonetheless accepts it as beyond doubt genuine, and sees it as a yet more blatant attempt at self-justification. It is not, however, called into evidence by Powers, even though it would appear to support his case, and he mentions it only in his notes, and with the greatest reserve. Jeremy Bernstein, who seems to me the best-informed and most fair-minded of all Heisenberg's critics, and whose book *Hitler's Uranium Club* will be relied upon in understanding the scientific considerations that follow, dismisses it as 'incredible' and 'a chimera'. It is entirely at odds with Heisenberg's careful moderation in all his other references to the matter, and the inclusion of Hahn and von Laue in the plot is nonsensical. Hahn was a chemist, not a physicist, and, as will be plain from what comes later, had no knowledge whatosever of the relevant mathematics, while von Laue is famous as an outspoken opponent of Nazism who never worked on the German nuclear programme at all.

So much for the direct evidence, true or false. All the rest of the evidence is indirect, and relates to whether Heisenberg did actually have some understanding of the relevant physics and concealed it, or whether he failed out of ignorance. It centres on the question of critical mass. This is the amount of fissile material (U235 or plutonium) which is large enough to support an explosive chain reaction, but small enough not to explode spontaneously. An estimate of this amount was crucial to the decision about proceeding with a serious nuclear weapons programme because of the enormous difficulty and expense of separating the U235 from the U238 that makes up the vast bulk of natural uranium, and the length of time it would take to develop a reactor capable of transmuting the uranium into plutonium. At the beginning of the war it was believed by scientists on both sides that the answer would be in tons, which put the possibility of producing it beyond practical consideration. The idea became imaginable only when two scientists working in Britain, Rudolf Peierls and Otto Frisch, did the calculation and realised quite

how fast the reaction would go with fast neutrons in pure U235, and consequently how little fissile material you would need: not tons but kilograms. (The various ironies associated with this are explored in the play, and I will not repeat them here.)

Powers argues that the idea never became imaginable in Germany because Heisenberg 'cooked up a plausible method of estimating critical mass which gave an answer in tons.' He believes that Heisenberg 'well knew how to make a bomb with far less, but kept the knowledge to himself.'

There is a certain amount of evidence that the German team did at one point arrive at a much lower figure for the critical mass - indeed, for one in kilograms, that bore some relation to the estimate made by Frisch and Peirls, and to the actual mass of the Hiroshima bomb (56 kg). Manfred von Ardenne, who was running an alternative nuclear programme for the German Post Office, later claimed in his memoirs that in the late autumn of 1941 he was informed independently by both Heisenberg and Hahn that they had worked out the critical mass for a U235 bomb and found it to be about 10 kilograms. This information was subsequently withdrawn by von Weizsäcker, who told him that he and Heisenberg had decided that a U235 bomb was impossible (because the heat of the reaction would expand the uranium too fast for it to continue). But Heisenberg, so far as I know, never commented on this, and von Weizsäcker, according to Bernstein, 'essentially denied' that any such conversation ever took place.

As Bernstein says, it is difficult to know what to make of all this - it is 'one of several brick walls anyone who studies this subject runs into.' I think it's difficult to take von Ardenne's recollection entirely literally. Hahn, as I noted before, plainly had no understanding of the mathematics, nor of any of the other issues involved, and, as we shall see, had to have them explained to him by Heisenberg later. On the other hand (and this story has more other hands than a Hindu god), in von Weizsäcker's report on the possibility of an American bomb programme, written in September 1941, he talked about the destructive effects of a bomb weighing 5 kg. Then again, in February 1942 a brief progress report for German Army Ordance, authors unnamed, suggested without further explanation a critical mass of between 10 and 100 kg. And at the crucial meeting with Speer at Harnack House in June 1942, when Field Marshal Milch asked him how large an atomic bomb would have to be to destroy a city, Heisenberg replied, or so he said in his interview with Irving, that it, or at any rate its 'essentially active part', would have to be 'about the size of a pineapple.'

In the end, though, I believe that the crucial piece of evidence lies elsewhere, in a source that was denied to everyone who wrote about Heisenberg until recently - the transcripts of the Farm Hall recordings. Bernstein, Powers, and Rose were the first commentators to have access to them.

Though of course they still don't reach the same conclusions from them.

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The story of Farm Hall is another complete play in itself. Sir Charles Frank, the British atomic physicist, in his admirably fair and clear introduction to the text of the transcripts that was published in Britain, regrets that they were not released in time for Dürrenmatt to make use of.

At the end of the war troops of the Alsos mission, to which Goudsmit was attached, made their way through what was left of the German front line and located the remains of the German reactor at Haigerloch, with the intention of finally reassuring themselves that Germany would not be able to spring some terrible nuclear surprise at the last moment. They also seized the team of scientists themselves, making a special armed sortie to Urfeld, in Bavaria, to collect Heisenberg from his home. Hechingen, the nearby town where the team was based, and Haigerloch itself were in the French sector. The scientists were abstracted secretly, from under the noses of the French, and brought back to Britain, where they were held, under wartime laws and without anyone's knowledge, in a former Intelligence safe house - Farm Hall, near Cambridge. The intention seems to have been partly to prevent their passing on any atomic secrets to either of our other two allies, the Russians and the French; partly to forestall any discussion of the possibility of nuclear weapons until we had completed and used our own; and partly, perhaps, to save Heisenberg and the others from the alternative solution to these problems proposed by one American general, which was simply to shoot them out of hand.

They were detained at Farm Hall for six months, during which time they were treated not as prisoners but as guests. Hidden microphones, however, had been installed, and everything they said to each other was secretly recorded. The existence of the transcripts from these recordings was kept as secret as that of the prisoners. General Groves, the head of the Allied bomb programme, quoted from them in his memoirs (1962), and Goudsmit plainly had access to them, which he drew upon in his book on Alsos, but the British Government, perhaps to protect the feelings of the former detainees, some of them now prominent in post-war German science, perhaps merely out of its usual pathological addiction to secrecy, continued to block the release of the papers themselves. Even Margaret Gowing was refused access when she wrote her official history of British atomic policy in 1964, and David Irving was refused again, in spite of strenuous efforts, for *The Virus House* in 1967. The ban was maintained until 1992, when the Government finally gave way to a combined appeal from leading scientists and historians.

The German originals are lost, and the translation was plainly done under pressure, with little feeling for colloquial nuance, but the transcripts are direct evidence of what Heisenberg and the others thought when they were talking, as they believed, amongst themselves. The ten detainees represented a wide range of different attitudes. They ranged from Walther Gerlach and Kurt Diebner, who had both been members of the Nazi party, to Max von Laue, who had been openly hostile to the regime, who had never worked on the atomic programme, and whose inclusion in the party seems on the face of it mysterious. Their conversations over the six month period reflect a similarly wide range of attitudes and feelings. The general tone is pretty much what one might expect from any group of acadmics deprived of their liberty without explanation and cooped up together. There is, as one might suppose, quite a lot of complaining, scheming, and mutual friction.

One thing, though, seems to me to emerge quite clearly: for all practical purposes German thinking had stopped at a reactor, and there had been no eagerness at all to look beyond this to the possibility of weapons. Their shocked comments in the

moment of unguarded horror that followed the announcement of Hiroshima are particularly revealing. The internees had been given the news by their (almost) endlessly sympathetic and urbane gaoler-cum-host, Major Rittner, at dinner-time, but Heisenberg had not believed it until he had heard it with his own ears on the BBC nine o'clock news. 'They were completely stunned,' reported Rittner, 'when they realised that the news was genuine. They were left alone on the assumption that they would discuss the position...'

'I was absolutely convinced,' says Heisenberg, in the conversation that followed, 'of the possibility of our making an uranium engine [reactor] but I never thought that we would make a bomb and at the bottom of my heart I was really glad that it was to be an engine and not a bomb. I must admit that.' Weizsäcker says that he doesn't think that they should make excuses now for failing, 'but we must admit that we didn't want to succeed.' Gerlach: 'One cannot say in front of an Englishman that we didn't try hard enough. They were our enemies, although we sabotaged the war. There are some things that one knows and one can discuss together but that one cannot discuss in the presence of Englishmen.'

In a letter written fourteen years later von Laue complained that, during their conversations at table in the following weeks, 'the version was developed that the German atomic physicists really had not wanted the atomic bomb, either because it was impossible to achieve it during the expected duration of the war or because they simply did not want to have it at all.' Von Laue's account of the elaboration of this sanitised 'version' (*Lesart* in German) has been seized upon by unsympathetic commentators, and contrasted with the encouraging prospects for atomic weapons that some of the physicists had undoubtedly held out to the Nazi authorities at various times during the earlier part of the war.

Well, we all reorganise our recollections, consciously or unconsciously, as time goes by, to fit our changed perceptions of a situation, and no doubt Heisenberg and his fellow-detainees did the same. But Bernstein locates the origins of the *Lesart* in those immediate reactions to the announcement of Hiroshima on the nine o'clock news. If this is so then I can only say that the team began to get their story together with quite remarkable spontaneity, speed, presence of mind, and common purpose. If they all thought as fast as this, and co-operated as closely, it's even more surprising that they didn't get further with the bomb.

To me, I have to say, those immediate and unprepared reactions suggest quite strongly that the first part of Powers's thesis, at any rate, is right, and that there *had* been the 'fatal lack of zeal' that he diagnosed. Perhaps Gerlach's claim, unchallenged by the others, that they had actually 'sabotaged the war' suggests at the very least a consciousness that quite a lot of stones had been left unturned.

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But do the transcripts support Powers's contention that Heisenberg 'cooked up a plausible method of estimating critical mass which gave an answer in tons, and that he well knew how to make a bomb with far less, but kept the knowledge to himself"?

One preliminary point needs to be cleared out of the way first: the question whether Heisenberg understood an even more fundamental point, the difference between a reactor (which is operated by slow neutrons in natural uranium, or some other mixture of U238 and U235) and a bomb (which functions with fast neutrons in pure U235 or plutonium). Goudsmit, who plainly had access to the transcripts when he wrote his book at Alsos, seems to have thought they supported his view that Heisenberg didn't. Before the transcripts were published Rose shared Goudsmit's dismissive view.

But, according to the transcripts, what Heisenberg tells Hahn that same night, when Gerlach, their Nazi co-ordinator, has retired to sob in his room, and they are finally alone together, is that 'I always knew it could be done with 235 with fast neutrons. That's why 235 only [presumably = "only 235"] can be used as an explosive. One can never make an explosive with slow neutrons, not even with the heavy water machine [the German reactor], as then the neutrons only go with thermal speed, with the result that the reaction is so slow that the thing explodes sooner, before the reaction is complete.'

Bernstein (unlike Goudsmit) reads this and what follows as showing that Heisenberg *did* understand the difference between a reactor and a bomb, 'but that he did not understand either one very well - certainly not the bomb.' Rose now seems to accept that Heisenberg's remarks do indicate that he realised the bomb would have to be fissioned with fast neutrons (though he shows that in the past Heisenberg had been toying with the idea of some kind of vast exploding reactor).[1]

This same conversation between Heisenberg and Hahn, when they were alone together on that terrible night, seems to me also to resolve the question of Heisenberg's understanding of the critical mass beyond any reasonable doubt. He takes Hahn through what he believes to be the relevant calculation and tells him that the answer is 'about a ton.' I can't see any earthly reason why he should be rehearsing a fabricated calculation or a fabricated answer at this stage, in a private conversation with someone he seems to have trusted, after the German team are out of the race and in custody, and after someone else has in any case already built the bomb. If he had had the right calculation and the right answer up his sleeve all the time, now would surely have been the moment to produce them. I find it much more plausible that he was telling the simple truth when he said to Hahn just before this that 'quite honestly I have never worked it out as I never believed one could get pure 235.'

Earlier on in the evening, it's true, when everyone was present during the conversation immediately after the news bulletin, Hahn says to Heisenberg: 'But tell me why you used to tell me that one needed 50 kilograms of 235 in order to do anything.' (To which Heisenberg replies that he wouldn't like to commit himself for the moment.) This does seem to suggest that

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1 Bernstein takes the trouble to explain in his book what few other commentators do - the difference between slow and fast neutrons: 'By definition, slow neutrons move with speeds of the order of a few kilometers a second, about the speeds that molecules at room temperature move in a gas. That is why these neutrons are also referred to as thermal. Fast neutrons, the kind that are emitted in many nuclear processes, move at speeds of tens of thousands of kilometers a second.'

he *had* made a calculation of some sort earlier, as von Ardenne claimed - though it also surely destroys once and for all the improbable proposition that Hahn had been involved in it, or had made some kind of estimate of his own. Perhaps Heisenberg had made not so much a calculation as some kind of guess or estimate. Even if *was* a serious calculation, it seems most unlikely that it was the right calculation, or that it was one he had adhered to.

This is made clear to me (at last) by Jeremy Bernstein. I should explain that when I first read the Farm Hall transcripts, before I wrote the play, I was using the bare uncommented text published in Britain, unaware that there was also a completely different edition published in the US, incorporating Bernstein's detailed commentary. After the play was produced and published he was kind enough to send me it, and it illuminated a great many matters that I had not understood before. These are after all scientists talking to scientists, and they are reported verbatim with all the ellipses of spoken conversation, and with a further haze cast over the proceedings by translation. Bernstein is both a distinguished journalist and a professor of physics, and he has a long acquaintance with the history of atomic research. (He recalls being given the bare plutonium core of a bomb to hold on the Nevada test site in 1957; 'it was slightly warm to the touch, since plutonium is marginally radioactive.') He has a thorough understanding of the scientific issues involved, and is the ideal guide to the physics - though a slightly less percipient one, I think, to the psychology of the physicists.

I'm pleased to discover for a start that he takes the same view of Heisenberg's admission to Hahn about never having worked out the critical mass. He believes that it has to be taken at its face value, and he asks how it can be reconciled with the figure of 50 kg recalled by Hahn. He demonstrates that when Heisenberg attempts to do the calculation for Hahn he 'gets it wrong at every level' - he does the arithmetic wrong, and is in any case doing the wrong arithmetic. 'Knowing how scientists work,' says Bernstein, 'I find it implausble that he ever did the calculation correctly before. One can imagine even a Heisenberg forgetting a number - he was, in any case, not very good with numbers - but it is very difficult to imagine his forgetting a general method of calculation, a method that once led him to a more reasonable answer.'

The calculation of the critical mass is not the only thing that Heisenberg got wrong that night. Even when he revealed to Hahn that he understood how the critical mass could be reduced by the use of a reflective shield he suggested a material, carbon, that would have had the opposite effect to the one intended. Carbon is a good moderator for a reactor, and Heisenberg's proposing it for the 'tamper' in a bomb, says Bernstein, 'shows he was thinking like a reactor physicist, which, for the last two years, he was.'

These were of course Heisenberg's first thoughts off the top of his head in the wake of Hiroshima. A week later, with the help of what few details the newspapers had given of the two bombs, Heisenberg offered all his fellow-internees a lecture in which he presented a complete and considered account of how the Allies had done it. The inclusion in the lecture of quite fundamental matters, argues Powers, together with the questions which his hearers asked, make it clear that it was all news to everyone present except his closest associates. 'What the Farm Hall transcripts show unmistakably,' he says, 'is that Heisenberg did not explain basic bomb physics to the man in charge of the German bomb program [Gerlach] until after the war was over.' They 'offer strong evidence that Heisenberg never explained fast fission to Gerlach.' At the end of the lecture, says Powers, 'the German scientists, given a second chance, would have been ready to start building a bomb.'

Bernstein sees the lecture very differently. He demonstrates that Heisenberg's exposition is still marred by quite fundamental misconceptions. Heisenberg now seems to have 'the first inkling' of how to calculate the critical mass (though he still does the arithmetic wrong), but is not much nearer to the practicalities of building a bomb than his audience. What the novelty of a lot of this material suggests to Bernstein is simply that communications between the different sections of the German project were very poor.

As a non-scientist I can't offer any opinion on the physics. To my eyes, I have to say, Heisenberg does seem to have come a remarkably long way in a week - if, that is, he was starting more or less from scratch. And he surely must have been. It's really not plausible that he hadn't recollected more by this time if he actually had done the work. The conclusion seems to me inescapable: he hadn't done the calculation. If he had kept the fatal knowledge of how small the critical mass would be from anyone, as Powers argues, then it was from himself.

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In the end, it seems to me, your judgment of Heisenberg comes down to what you make of his failure to attempt that fundamental calculation. Does it suggest incompetence or arrogance, as his detractors have claimed? It's possible. Even great scientists - and Bernstein agrees that Heisenberg was one of them - make mistakes, and fail to see possibilities that lesser men pick up; Heisenberg accepted that he had made a mistake in the formulation of uncertainty itself. And I think we have to accept Bernstein's judgment that, although he was the first person to be able to grasp the counter-intuitive abstraction of quantum mechanics, he was not so good at the practicalities of commonsense estimates and working arithmetic.

Or does the failure suggest something rather different? An unconscious reluctance to challenge the comforting and convenient assumption that the thing was not a practical possibility? Comforting and convenient, that is, if what he was trying to do was *not* to build a bomb. Is it all part of a general pattern of reluctance, as the first and more plausible part of Powers's thesis suggests? If so, you might wonder whether this reluctance was a state definite enough to be susceptible of explanation. Heisenberg was trapped in a seamless circle which explains itself: he didn't try the calculation because he didn't think it was worth doing - he didn't think it was worth doing because he didn't try it. The oddity, the phenonomenon that requires explaining, is not this non-occurence but its opposite - the escape of Frisch and Peierls from that same circle. It seems almost like a random quantum event; in which case, of course, it is no more explainable than its not happening.

After the war, certainly, Heisenberg was not just passively reluctant about any military application of nuclear power, but very actively so. In the 1950s, when there was a proposal to arm Federal Germany with nuclear weapons, he joined forces with Weizsäcker and others to fight a vigorous campaign that entirely and permanently defeated it.

There is also one small piece of evidence about his attitude during the war that Powers rather curiously doesn't comment on: the question of the cyclotron.

At the crucial meeting between Heisenberg and Speer in 1942, which seems finally to have scuppered all possibility of a German bomb, Heisenberg is reported to have emphasised the need to build a cyclotron. A cyclotron could have been used, as the cyclotrons in America were, for isotope separation, the great sticking-point in the German programme. In the account of this meeting in his memoirs Speer says: 'Difficulties were compounded, Heisenberg explained, by the fact that Europe possessed only one cyclotron, and that of minimal capacity. Moreover, it was located in Paris and because of the need for secrecy could not be used to full advantage.' Powers mentions this, but does not go on to the obvious corollary: that if Speer's recollection is accurate, then Heisenberg was plainly lying, because he knew perfectly well that there was a second cyclotron to hand - at Bohr's institute in Copenhagen. This would suggest that his apparent anxiety to lay his hands on a machine that might actually separate some U235 was not quite what it seemed. Or, at the very least, that he placed Germany's war aims below his desire to protect Bohr's institute.

Perhaps Speer is simply wrong. It seems uncharacteristic of Heisenberg to have risked such a blatant falsehood, and he makes no mention of it in his own accounts of the meeting. All the same, when he went back to Copenhagen in 1944, after Bohr had fled, to adjudicate a German proposal to strip the institute of all its equipment, presumably including the cyclotron, he seems to have contrived to leave it even then still in Danish hands.

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One of the forms of indeterminacy touched upon in the play is the indeterminacy of human memory, or at any rate the indeterminability of the historical record. There are various examples which I left out, for fear of making the play even more tangled than it is. Some, such as the difficulties about the amazingly realistic figure for the critical mass that von Ardenne recollected being given by Heisenberg and Hahn in 1941, I have already mentioned in this Postscript. There were others. A minor one concerns whether there were two ships sent to load the Jews of Copenhagen for deportation, as some witnesses recall, or a single one (named as the Wartheland). A more significant point of dispute is the drawing which Heisenberg did or didn't make for Bohr during their meeting in 1941.

According to Hans Bethe, who was one of the team at Los Alamos, Heisenberg drew a rough sketch to show Bohr the work that was being done in Germany. Bohr evidently took it to Los Alamos with him when he went, because Bethe (and others) recall it being passed around at a meeting there. Bethe told Powers that Bohr believed it represented a bomb; but the consensus of opinion at the meeting was that it was a reactor. However, Aage Bohr, Niels's son, a physicist himself (and another Nobel prizewinner), who was with his father in Copenhagen during Heisenberg's visit, and with him again in Los Alamos, was absolutely insistent that there was no drawing.

If the story is true it might help to explain Goudsmit's insistence, in the teeth of the evidence from Farm Hall, that Heisenberg couldn't tell the difference between a reactor and a bomb. It would certainly cast doubt on Heisenberg's recollection that the entire discussion with Bohr in 1941 took place during the walk, and that Bohr broke off the conversation almost as soon as it was broached. It seems improbable to me that Heisenberg would have risked putting anything down on paper, and if even so he had then I can't see why he didn't seize upon it after the war, to support his claim that he had hinted to Bohr at the German research on a bomb. I suppose it's possible that Bohr made the sketch himself, to illustrate to his colleagues at Los Alamos what he thought Heisenberg was getting at, but the truth of the matter seems to be irretrievable.

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I have had many helping hands with this play, both before it was produced in London and since. Sir John Maddox kindly read the text for me, and so did Professor Balázs L Gyorffy, Professor of Physics at Bristol University, who made a number of corrections and suggestions. I am also indebted to Finn Aaserud, the Director of the Niels Bohr Archive in Copenhagen, and to his colleagues there, for much help and encouragement. Many scientists and other specialists have written to me after seeing the play on the stage. They have mostly been extraordinarily generous and supportive, but some of them have put me right on details of the science, for which I am particularly grateful. They also pointed out two mathematical errors so egregious that the lines in question didn't make sense from one end to the other - even to me, when I re-read them. All these points have now been addressed, though I'm sure that other mistakes will emerge. So much new material has come to hand, in one way or another, that I have extensively overhauled and extended this Postscript to coincide with the production of the play in New York.

One matter of dispute that I have not been able to resolve completely concerns the part played by Max Born in the introduction of quantum mechanics. The matter was raised (with exemplary temperance) by his son, Gustav Born, who was concerned about the injustice he felt I had done to his father's memory. I was reluctant to make the play any more complex than it is, but I have since made adjustments both to the play itself and to this Postscript which go at any rate some way to meeting Professor Born's case. We are still at odds over one line, though, in which Heisenberg is said to have 'invented quantum mechanics'. I am quoting the judgment of other physicists here (including one not especially sympathetic to Heisenberg), but I realise that it is a huge over-simplification, and that it seems to compound the original injustice committed when Heisenberg was awarded the Nobel Prize in 1932 'for the creation of quantum mechanics', while Born had to wait another 22 years to have his

part acknowledged in the same way. The trouble is that I have not yet been able to think of another way of putting it briefly enough to work in spoken dialogue.

The American physicist Spencer Weart, in a letter to Finn Aaserud, very cogently pointed out that the calculation of the critical mass was much harder than I've made it seem for Heisenberg once Bohr has suggested it to him. 'Perrin failed to get it and his publication of a ton-size critical mass subtly misled everyone else, then Bohr and Wheeler failed, Kurchatov failed, Chadwick failed, all the other Germans and Russians and French and British and Americans missed it, even the greatest of them all for such problems, Fermi, tried but missed, everyone except Peierls... Physics is hard.'

Some correspondents have objected to Heisenberg's line about the physicists who built the Allied bomb, 'Did a single one of them stop to think, even for one brief moment, about what they were doing?', on the grounds that it is unjust to Leo Szilard. It's true that in March 1945 Szilard began a campaign to persuade the US Government not to use the bomb. A committee was set up - the Committee on Social and Political Implications - to allow the scientists working on the project to voice their feelings, and Szilard also circulated a petition among the scientists, 67 of whom signed it, which mentioned 'moral considerations,' though it did not specify what exactly these were.

But the main stated reasons for Szilard's second thoughts were not to do with the effects that the bomb would have on the Japanese - he was worried about the ones it would have on the Allies. He thought (presciently) that the actual use of the bomb on Japan would precipitate an atomic arms race between the United States and the Soviet Union. The Committee's report (which Szilard himself seems to have written) and the petition stressed the same points. By this time, in any case, the bomb was almost ready. It had been Szilard who urged the nuclear programme in the first place, and at no point, so far as I know, while he worked for it (on plutonium production) did he ever suggest any hesitation about pursuing either the research or the actual manufacture of the bomb.

I think the line stands, in spite of Szilard's afterthoughts. The scientists had already presented their government with the bomb, and it is the question of whether the German scientists were ready or not to do likewise that is at issue in the play. If Heisenberg's team *had* built a bomb, I don't think they would have recovered very much moral credit by asking Hitler to be kind enough not to drop it on anyone - particularly if their objection had been the strain it might place upon post-war relations among the Axis powers.

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One looming imponderable remains. *If* Heisenberg had made the calculation, and *if* the resulting reduction in the scale of the problem had somehow generated a real eagerness in both the Nazi authorities and the scientists, could the Germans have built a bomb? Frank believes that they could not have done it before the war in Europe was over - 'even the Americans, with substantial industrial and scientific advantage, and the important assistance from Britain and from ex-Germans in Britain did not achieve that (VE-Day, 8 May 1945, Trinity test, Alamogordo, 6 July 1945).' Speer (who as armaments minister would presumably have had to carry the programme out) suggests in his memoirs that it might have been possible to do it by 1945, if the Germans had shelved all their other weapons projects, then two paragraphs later more cautiously changes his estimate to 1947; but of course he needs to justify his failure to pursue the possibility. Powers makes the point that, whatever the timetable was, its start date could have been much earlier. Atomic energy in Germany, he argues, attracted the interest of the authorities from the first day of the war. 'The United States, beginning in June 1942, took just over three years to do the job, and the Soviet Union succeeeded in four. If a serious effort to develop a bomb had commenced in mid-1940, one might have been tested in 1943, well before the Allied bomber offensive had destroyed German industry.'

If this 'serious effort' had begun only after Heisenberg's visit to Copenhagen, as the play suggests might have happened if the conversation with Bohr had gone differently, then even this timetable wouldn't have produced a bomb until late 1944 - and by that time it was of course much less likely that German industry could have delivered. In any case, formidable difficulties remained to be overcome. The German team were hugely frustrated by their inability to find a successful technique for isolating U235 in any appreciable quantity, even though the experimental method, using Clusius-Dickel tubes, was of German origin. They could have tried one of the processes used sucessfully by the Allies, gaseous diffusion. This was another German invention, developed in Berlin by Gustav Hertz, but Hertz had lost his job because his uncle was Jewish. (It was, incidentally, the delays in getting the various American isotope-separation plants to function which meant that the Allied bomb was not ready in time for use against Germany.)

The failure to separate U235 also held up the reactor programme, and therefore the prospect of producing plutonium, because they could not separate enough of it even for the purposes of enrichment (increasing the U235 content of natural uranium), so that it was harder to get the reactor to go critical. The construction of the reactor was further delayed because Walther Bothe's team at Heidelberg estimated the neutron absorption rates of graphite wrongly, which obliged the designers to use heavy water as a moderator instead. The only source of heavy water was a plant in Norway, which was forced to close after a series of attacks by Norwegian parachutists attached to Special Operations Executive, American bombers, and the Norwegian Resistance. Though perhaps, if a crash programme had been instituted from the first day of the war, enough heavy water might have been accumulated before the attacks were mounted.

If, if, if.... The line of ifs is a long one. It remains just possible, though. The effects of real enthusiasm and real determination are incalculable. In the realm of the just possible they are sometimes decisive.

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Anyone interested enough in any of these questions to want to sidestep the fiction and look at the historical record should certainly begin with:

Thomas Powers: *Heisenberg's War* (Knopf 1993; Cape 1993)

David Cassidy: *Uncertainty: The Life and Science of Werner Heisenberg* (W H Freeman 1992)

Abraham Pais: *Niels Bohr's Times* (OUP 1991) - Pais is a fellow nuclear physicist, who knew Bohr personally, and this, in its highly eccentric way, is a classic of biography, even though Pais has not much more sense of narrative than I have of physics, and the book is organised more like a scientific report than the story of someone's life. But then Bohr notoriously had no sense of narrative, either. One of the tasks his assistants had was to take him to the cinema and to explain the plot to him afterwards.

Werner Heisenberg: *Physics and Beyond* (Harper & Row 1971) - In German, *Der Teil und das Ganze*. His memoirs.

Jeremy Bernstein: *Hitler's Uranium Club, the Secret Recordings at Farm Hall*, introduced by David Cassidy (American Institute of Physics, Woodbury, New York 1996)

or the British edition of the transcripts:

Operation Epsilon, the Farm Hall Transcripts, introduced by Sir Charles Frank. (Institute of Physics Publishing 1993)

Also relevant:

Heisenberg: *Physics and Philosophy*. (Penguin 1958)

Niels Bohr: *The Philosophical Writings of Niels Bohr* (Oxbow Press, Connecticut 1987)

Elizabeth Heisenberg: *Inner Exile* (Birkhauser 1984) - In German *Das politische Leben eines Unpolitischen*. Defensive in tone, but revealing about the kind of anguish her husband tended to conceal from the world; and the source for Heisenberg's ride home in 1945.

David Irving: *The German Atomic Bomb* (Simon & Schuster 1968) - in UK as *The Virus House* (Collins 1967). The story of the German bomb programme.

Paul Lawrence Rose: *Heisenberg and the Nazi Atomic Bomb Project* (U of California Press 1998)

Records and Documents Relating to the Third Reich, II German Atomic Research, Microfilms DJ29-32. (EP Microform Ltd, Wakefield) Irving's research materials for the book, including long verbatim interviews with Heisenberg and others. The only consultable copy I could track down was in the library of the Ministry of Defence, London

Archive for the History of Quantum Physics, microfilm. Includes the complete correspondence of Heisenberg and Bohr. A copy is available for reference in the Science Museum Library, London. Bohr's side of the correspondence is almost entirely in Danish, Heisenberg's in German apart from one letter.

Leni Yahil: *The Rescue of Danish Jewry*, (Jewish Publication Society of America, Philadelphia 1969)

There are also many interesting sidelights on life at the Bohr Institute in its golden years in:

French & Kennedy, eds: *Niels Bohr, A Centenary Volume* (Harvard 1985)

and in the memoirs of Hendrik Casimir, George Gamow, Otto Frisch, Otto Hahn, Rudolf Peierls, and Victor Weisskopf.

For the subsequent challenges to the Copenhagen Intepretation:

David Deutsch: *The Fabric of Reality* (Allen Lane 1997)

Murray Gell-Mann: *The Quark and the Jaguar* (W H Freeman 1994; Little, Brown 1994)

Roger Penrose: *The Emperor's New Mind* (OUP 1989)

The actual 'two-slits' experiment was carried out by Dürr, Nonn, and Rempe at the University of Konstanz, and is reported in *Nature* (3 September 1998). There is an accessible introduction to the work in the same issue by Peter Knight, and another account of it by Mark Buchanan (boldly entitled 'An end to uncertainty') in *New Scientist* (6 March 1999).