explanations, which have accounted for certain anomalies but not yet all: it is an endless process.

The same can be said of so-called cosmic theories about the origin and duration of our Universe, all of which theories consist in extrapolating on a grand scale physical laws observed on our small scale.One of these discussions in vogue at the moment is: does the Universe come from a formless cloud that condensed into cascade of smaller worlds? or was there an initial dense ball which exploded into various fragments? All hazardous speculations, especially when they are mixed with philosophical ideas and proofs of the existence of God, which any honest scientist, or simply any honest man for that matter, should, I think, oppose on the grounds that they are impermeable to the mind; receive them politely but not with conviction.

The same phenomenon of incertitude is to be found in the sphere of the infinitely small, that of the atom. Not that we have to reject this "scientific progress" in the matter, that is to say the almost infinite succession of theses which have all explained certain phenomena very well, whilst leaving certain other phenomena to one side. Basically, we should make use of the laws of Physics concerning corpuscles, but it would be better not to believe in them. We should be wary of the scientist who doubts nothing; the greatest of them are undoubtedly those who have given priority to the principle of incertitude, as a threshhold which is not reason's to cross.

Even Pascal was mistaken in insisting to Père Noël, the Jesuit scientist, that there was a perfect void above the column of mercury in the barometer. But his idea, tainted with error though it was, allowed him to give the best possible account of the phenomenon of atmospheric pressure. His physics law was not absolute, but he invented the barometer, and in that he was a genius.

The ignorant man can conceive nothing; he who will not admit error is incapable of action. A good scientist is neither the one nor the other. For he has to take decisions; *a bet has to be taken to get started.*

Probable opinions. One could deduce from this that one should not speak of "proofs" in experimental sciences, but rather of more or less probable opinions. Certain Jesuits, always in advance of an idea, even employed this term of the decisions of their "grave doctors". It is in fact an interesting idea: to know whether it is applicable to moral questions is not my aim; as the other said: *I am not content with the probable, I told him, I am looking for what is sure.* Speaking for myself, I admit my incompetence. But I would readily speak of probable opinions in the experimental sciences, which provokes the shocked reaction: «Your pure reasoning then only ends in non-certitude!» Yes, because it leaves its proper domain when it applies itself to the experimental. And even though probable, the scientific process is useful and necessary, if only to rid us of our little idols and superstitions, in which the ignorant unwillingly and unknowingly believe; not to mention the idols of Science, be they persons or ideas.

So how to decide? According to the well known principle, that between two complementary events, one of very slight probability and the other of a probability close to 1, you have to bet on the second. And if the probabilities are equal, say so and, as far as possible, avoid deciding.

The Carbon 14 affair. A small parenthesis on the matter of the tests carried out on the Holy Shroud, where the scientific procedures have proved to be extremely debatable. I depend on the excellent reports in the CRC for this subject. One could find at least three properly scientific faults; I class them in order of importance, starting with the least.

1. The eventual open mixing of the samples with no laboratory control has more to do with conjuring than with scientific experimentation. Such sleight of hand, moreover, is more common than you would think in Physics, especially when there is a desire to force the experiment to confirm a preestablished thesis. Careers can sometimes depend on this.

2. The statistical analysis of the published results: a very small number of measurements presenting a wide scatter ($\chi_2 \simeq 6.4$) do not allow the famous "law of errors" to be applied (the normal law), nor can any conclusion therefore be reached as to the dating of the cloth; still less a declaration of absolute proof backed by mass publicity. Dr Tite, the spokesman, is playing a grotesque rôle here as scientific guarantor of the results. A true scientist would have been much more cautious in his conclusions. One thing is clear: the peculiar politics of the scientific world have prevailed in this affair over plain intellectual honesty. But it is quite common for people who have shown remarkable mental agility between the ages of 20 and 30 to run out of steam and then, in desperation at having no further original ideas, to settle down to the pursuit of honorary posts. At the same time, they keep their competence in a very small field so detailed that it disappears before everyone's eyes. When they conduct an experiment, they necessarily find the results they need! The results that do not shock. So, is the scientific world corrupt? No more than any other, and, I would add, less than another, for there remains a whole army of research scientists who work in obscurity in a way that is totally disinterested; I have met too many of them not to be sure of their existence. It is those people who save science.

This *Nature* report, co-signed by twenty one persons, should normally have been followed by a proper experts' report, comprising all the intermediary results. In any case, the dossier of all this must exist somewhere. Even though unpublished, it should still be made available on request. For example, many unproven theorems are published in mathematics with a view to lightening the articles (the papers lack space); but the demonstrations are always carefully kept in reserve for anyone interested. It is the rule, and is part of the scientific ethical code. It is a rule that should suffer no exception.

3. Overlooking other scientific measurements that do not agree with a recent dating. Some of them are much more trustworthy than the Caarbon 14 test (it would be very interesting, for example, to concentrate the technical means at our disposal on confirming the eventual traces of coins; that dating would be "almost infallible"). To ignore less doubtful measurements and to present the more doubtful as though they were absolute results, is evidence of irresponsibility; it is a betrayal of science, covering a false procedure by an improper vulgarisation.

I believe that your idea, Father, of obtaining a criterion for scientific decision by means of probabilities is entirely within the correct logic of the problem. If it were realisable, it would not prove the date strictly speaking; not like "2+2 = 4", but it would give an answer to those who contradict: «Don't be absurd; you have thrown tails, when we know that there is a thousand to one chance of throwing heads!»

Frequencies and subjectivity. Here comes the big problem: If we are to decide through probabilities, how are they to be estimated? There are two, almost complementary, ways for they do not apparently apply to the same events.

There are probabilities defined by **frequencies**, which apply to repetitive events. If I toss my coin a thousand times, and "tails'