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[Nettime-bold] Might a laboratory experiment now being planned destroy planet Earth

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Title: ***Might a laboratory experiment now being planned destroy planet Earth***

Might a laboratory experiment destroy planet Earth?

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Synopsis

Recently some concerns have been raised about the possibility that a high-energy ion-ion colliding beam experiment which just began at Brookhaven National Laboratory in the United States, and a similar one that is planned to begin some years hence at CERN in Geneva, might have cataclysmic consequences, hypothetically amounting to the disappearance of planet Earth. The probability that this happen is of course tiny. In the first part of this paper a popularised review is presented of the motivations for such concerns and of the extent they have been investigated, and in the context of such a popularised treatment the appeasing conclusions of these investigations are scrutinised. In the second part, in the light of this example, a terse analysis is provided of some scientific, ethical, political and sociological issues raised by the problematique associated with human endeavours which might entail a tiny probability of an utterly catastrophic outcome -- with special emphasis on related responsibilities of the scientific/technological community.

“First Commandment for experimental physicists: *Thou shalt put error bars on all your observations*.

First Commandment for theoretical physicists: *Thou shall get the sign right*.”

(Private Communication by Professor Sebastian Pease)

0. Recently concerns have been raised about the possibility that a major catastrophe -- possibly amounting to complete destruction of planet Earth -- result from an experiment which just began (summer 2000; for up to date information see http://www.rhichome.bnl.gov/AP/Status) at the Brookhaven National Laboratory (BNL) in the United States, and/or from a similar experiment under preparation at the European Centre for Nuclear Research (CERN) in Geneva, scheduled to begin there some years hence. Purpose and scope of this paper is to explain what these concerns are about, to report on various estimates [1-4] -- all of them appeasing -- of the risk, and to proffer some general scientific-political-ethical-sociological considerations related to this kind of issues; considerations whose interest should be however qualified by the avowed lack of *specific* scientific-political-ethical-sociological expertise of this author.

1. More detailed and professional analyses of the experiments in question and of their hypothetical danger are given in [3] and [2] (see also [4]). The presentation given below is largely in the nature of a popularised summary of these papers; it also points out certain aspects which to this author appear as possible shortcomings of these treatments. Some information on the publication history of these papers [1-4] is also provided below, as it might have some (minor) relevance with respect to the more philosophical considerations reported in the second half of this paper. This author profited from many discussions by voice and by e-mail with several colleagues, who will however remained unnamed to avoid any hint that they agree with what is written herein, for which the author feels he should take exclusive personal responsibility. An explicit thank is however due to Adrian Kent for having called my attention (via Pugwash) to this problematique.

2. The experiments in question realise collisions of heavy ions, produced by two beams of such particles colliding head-on against each other. The particles of the two beams are accelerated by huge (and very expensive -- many hundred million dollars) apparatuses ("colliders"). In the case of the Relativistic Heavy Ion Collider (RHIC) experiment at BNL, the particles in the two beams are gold ions (indeed, one can think of gold nuclei -- since the ionisation is almost complete), which get accelerated to a (planned) energy of *20 TeV* (*1 Tev=10^3 Gev=10^6 Mev=10^12 ev*), amounting to an approximate energy of *100 Gev* per nucleon (each gold nucleus is composed of *79* protons and *118* neutrons, altogether *197* nucleons). The energy in the centre-of-mass system when two particles collide adds up to *40 Tev*. In the CERN experiment (A Large Ion Collider Experiment -- ALICE), the energies per ion are expected to be *30* times larger, and the mass of each ion is also larger, but only marginally so: lead rather than gold, mass *207* (*82* protons and *125* neutrons) rather than *197*. The total energies in the centre-of-mass system (which in this case of colliding beams with equal energies coincides with the laboratory system; the situation is of course quite different in the case of one beam hitting a fixed target) are the largest ever realised in high-energy physics experiments; although of course one should actually talk, in this context, of energy *density* (the energies in macroscopic events, such as the collision of two billiard balls, or two cars, are much larger). It should also be noted that experiments with higher centre-of-mass energies *per nucleon* have been realised in machines which accelerate protons rather than ions.

In any case from some points of view these high-energy experiments explore a region of microphysics never before attained by man-made experiments -- although cosmic ray events with much higher centre-of-mass energies occur all the time and many of them have been experimentally observed. It is therefore justified to wonder whether the exploration of such (relatively) new phenomenology might give rise to surprises. Indeed it is precisely the hope to find something interesting that motivates these experiments and justifies their huge costs. Specifically, it is expected/hoped that in these experiments, immediately after the head-on collision, the nuclear matter that constituted the two colliding nuclei become effectively a "plasma" made up of *quarks* and *gluons* -- namely of the objects which elementary particle physicists consider nowadays as the ultimate constituents of matter. Such a plasma would to some extent mimic the situation that existed in the early Universe (if the origin of our Universe is correctly described by Big Bang scenarios); information on its structure is obviously of great scientific interest.

3. But could the "surprises" entail dangerous consequences?

While the experimental exploration of any new phenomenology always entails an unknown element, it is possible for elementary particle physicists to envisage which are the potentially dangerous developments, and to analyse the likelihood that they emerge. Indeed just such a task was recently assigned (perhaps a bit late in the game!) by the Director of BNL to four American physicists, who produced a Report [1], and who later issued another (significantly modified) version of it [3]; and a somewhat analogous analysis has been performed in parallel by three physicists associated with CERN [2]. These findings have also been commented upon by two other, quite distinguished, physicists, in a note published by *Nature* [4]. All these experts agree that the probability of any catastrophic outcome of the RHIC experiment is so small to justify proceeding with it without delay. Their arguments also apply, although to some respect with less cogency (but from some others with more cogency) to the ALICE experiment at CERN. A committee to analyse the matter has also been recently appointed by the Director General of CERN, Luciano Maiani.

It is however desirable that a larger community of experts than those specifically tasked to do so involve themselves in this problematique, and look critically at these analyses; and it is also desirable that, to the maximum extent possible, a larger community of responsible scientists, and of citizens, become informed. Moreover this problematique also entails considerations of a political and ethical character, which are to some extent independent of the technical details, but can only profit from a better understanding of them. It is for this reasons that this attempt is made to popularise (and in that context, to some extent, to also criticise) the findings of the experts who have looked at these issues [1-4], undoubtedly with greater scientific competence than I can muster, yet giving me occasionally the impression to be biased towards allaying fears "beyond reasonable doubt"; a posture quite understandable under the circumstances -- since undue fears might block experiments which promote scientific progress -- but which nevertheless constitutes, almost by definition, an odd stand for scientists (doubt being the seed of science; although, admittedly, only *reasonable* doubt!).

4. Three hypothetical dangers which might emerge from these experiments have been considered: (i) formation of a "black hole" or some other "gravitational singularity" in which surrounding matter might then "fall"; (ii) transition to another hypothetical "vacuum state", different, and lower in energy, than the vacuum state of our world (which would therefore be metastable); (iii) formation of a stable aggregate of "strange" matter, which might initiate a transition of all surrounding matter to this new kind of matter, with the result of completely destroying planet Earth (such a phenomenon would entail a great liberation of energy; hence, if it were to unfold quickly, it would result in a Supernova-like explosion).

The first ("gravitational") concern can be allayed by simple, hence quite reliable, order-of-magnitude calculations, that definitely exclude any such possibility.

The second concern can also be eliminated by estimates of the (very large) number of cosmic ray collisions, at higher centre of mass energy than those envisaged in these experiments, which have occurred in the past history of our Universe, and which should therefore already have triggered a transition to another vacuum if such a phenomenon were possible.

The third concern requires a more detailed analysis.

5. The corresponding, dangerous scenario goes as follows. (i) Suppose that a sufficiently large aggregate of strange hadronic matter (exist and that it) be stable (at zero pressure) -- or metastable, but with a sufficiently long lifetime for the subsequent developments to unfold. (Strange hadronic matter is nuclear matter formed not only by nucleons -- protons and neutrons -- but also by "strange" baryons, which are well known to exist, although none of them is stable; or, equivalently, but in terms of "more elementary" constituents, by the *quarks* that make up nucleons, but also by those other *quarks* which are called *strange* -- that are also known to exist, as constituents of those elementary particles also called *strange* -- perhaps because all of them are unstable and are therefore not normally present in our environment). Following current usage, we call such an aggregate of strange matter a *strangelet*. (ii) Suppose that *strangelets* are negatively charged. (iii) Suppose that a (negatively charged) *strangelet* is produced in the lab, in a collision-experiment with heavy ions, and stops there without previously breaking up and thereby disassembling.

Then, via the long range Coulomb (electric) force, it would attract (or, equivalently, be attracted by) a nearby atomic nucleus, fuse with it, and become a larger *strangelet*, whose charge might be initially positive due to the contribution of the positive charge of the nucleus (all nuclei have positive charge, and charge is conserved), but which would subsequently become again negative via emission of positively charged electrons (a standard phenomenon, known as beta decay) if the normal (ground) state of strange nuclear matter were negatively charged. The new (larger) *strangelet* would then fuse with another nucleus, and so on and on. In this manner all surrounding matter might get transformed into strange matter. If the entire planet Earth were to be so transformed, the final outcome would likely be a sphere of enormous density, with roughly the mass of the Earth and only, say, *one hundred meters* radius. An enormous amount of nuclear energy would be liberated in the process which, if it were a fast one, would result in an explosion of astronomical proportions.

This prospect represents clearly a very great, albeit hypothetical, danger. But how hypothetical?

Two approaches are possible to assess this. *One approach* relies on current theoretical understanding of nuclear and subnuclear physics, and tries to estimate the probability that the scenario mentioned above unfolds. A *second approach* relies on the observation that analogous collisions to those which are going to be realised in the experiments under discussion have already occurred in Nature (cosmic rays), without causing observable calamities; from this fact *upper bounds* can be inferred on the probability that these experiments have a catastrophic outcome.

The main disadvantage of the *first approach* is that it relies on a theoretical framework which is still imperfectly known -- and it moreover depends on computations nobody knows how to reliably perform. The *second approach* must also necessarily rely, to some extent, on our theoretical understanding of nuclear and subnuclear physics (and also astrophysics, see below), but of course much less so. In the context of both approaches prudence requires that, as a rule, uncertainties be generally replaced by "worst case assumptions", although this should be done "within reason". The analyses should in any case be conducted with a critical spirit -- not with the purpose to prove a conveniently appeasing conclusion -- indeed a prudent methodology (not really followed so far, to the best of my knowledge) should engage two groups of competent experts, a *blue team* trying to make an "objective" assessment, and a *red team* (acting as "devil's advocates") specifically tasked to make a genuine effort at proving that the experiments are indeed dangerous -- an effort that, *if successful*, might then be challenged by the *blue team* who might perhaps point out that such a conclusion could only be achieved by making too many too far-fetched worst-case assumptions -- and also perhaps by introducing too encompassing a definition of what "dangerous" means. A debate might ensue, which, if conducted in a genuine scientific spirit, would be quite enlightening for those who eventually have the responsibility to decide. But we shall return below to a discussion of these methodological issues.

For the moment we limit our presentation of the scientific aspects of the issue to a superficial if occasionally critical outline of the arguments and conclusions of the analyses performed so far [1-4] as we understand them; analyses which seem to us to have been in the nature of *blue team* treatments -- conducted of course in good faith by competent experts, but occasionally tainted by an excessive awareness of the public relations relevance of the exercise, perhaps at the expense of candour if not objectivity.

6. Let us begin by reporting on a risk assessment performed in the framework of the *first point of view*, namely based on the current theoretical understanding of the likelihood that the dangerous scenario described above unfold. We have seen that, in order for this to happen, three ingredients are necessary: (i) *strangelets* should exist (namely, be stable -- or at least long-lived -- at zero pressure); (ii) they should be negatively charged; (iii) there should be a nonnegligible probability that they be produced in the experiments in question. Let us consider each of these three items.

The possibility that *strangelets* might be stable (or metastable but long-lived --without external pressure) is quite conceivable on the basis of our present knowledge of nuclear and subnuclear physics, although nobody is really able to make a firm prediction in that respect, and perhaps the present body of evidence and understanding of nuclear and subnuclear physics might be interpreted as rather suggesting otherwise. Under these circumstances, it cannot in particular be excluded with any certainty that stable *strangelets* might exist, and moreover only at masses larger than, say, *300* nucleonic masses, so that they might in principle be produced in gold-gold or lead-lead collisions (which puts together a total mass of approximately *400* nucleonic units), but not, for instance, in the collision of two nuclei of iron (iron is a rather common element, both as part of celestial bodies and of cosmic rays, but its nucleus only has mass *56*, being composed of *26* protons and *30* nucleons). Indeed it is likely that *strangelets*, if they exist, are only stable (or long-lived) at relatively large masses. And they might be stable for arbitrarily large mass.

The second element of the dangerous scenario outlined above requires *strangelets* to be negatively charged (if they were positively charged, they would be repelled by ordinary nuclei due to the long range Coulomb force, which is sufficient to keep them sufficiently apart to exclude the initiation of any nuclear reaction -- just the same mechanism that prevents ordinary nuclei from initiating nuclear reactions among themselves, even when these processes are energetically favoured). This appears, on the basis of our present knowledge of nuclear and subnuclear forces, quite unlikely: the expectation -- to the extent that such calculations can be performed with any degree of reliability -- is that, if *strangelets* exist and are stable, their charge will be positive, albeit perhaps small (namely, if Z is their charge in standard units, and A their mass in nucleonic units, then a likely guess is that $0<Z/A<<1$ -- while for ordinary nuclei , or a little less). Of course, the very fact that the charge would be small hints at the possibility that it might end up being negative; but this appears most unlikely on the basis of rather elementary notions about the *strangelet*' s make-up in terms of *quarks* and known properties of different *quarks* (in particular, the values of their masses). However our present picture of a *strangelet* of mass, say, *300-400* is as a bound assembly of *900-1200 quarks*, and current theoretical understanding of the detailed internal structure of such an object is rather imperfect; the possibility that it might be seriously flawed, due to insufficient insight on collective multi-body phenomena, cannot be excluded.

The third element of the catastrophic scenario requires that *strangelets* be produced in heavy-ion collisions such as those realised in the experiments in question. This looks most unlikely. The reason is that, in such a high energy collision, an environment gets created in which all the elementary constituents -- be they *quarks* or subassemblies of few *quarks* such as nucleons or strange baryons -- have a lot of kinetic energy; the most natural outcome is therefore that many fragments fly out; it is very difficult for a very large object such as a *strangelet* (which, as we saw above, would be formed by a very large number -- *hundreds!* -- of constituents) to get assembled and to come out unbroken. On the other hand it is not easy to evince a quantitative estimate from such a qualitative analysis -- which is most likely to be basically correct, although again subject to the same caution mentioned above concerning a possible lack of theoretical insight on collective multi-body effects: there indeed is a theoretical model ("evaporation") which tends to predict somewhat larger probabilities of producing large agglomerates in collisions such as those under consideration, than other, generally considered more reliable, models do. However, graduating from qualitative statements to some kind of quantitative estimates seems to me a desirable development, since a large number of collisions will be realised in the experiments in question (approximately *20 billions per year* at RHIC, which is expected to run for *10 years*), and one would like to be quite certain that not a single dangerous *strangelet* gets produced, if indeed just one would be sufficient to initiate a catastrophic process.

Let us now pause a moment to ponder on the nature of these arguments, and on the significance in this context of terms and notions such as *likelihood* and *probability*.

When we talk about the likelihood that stable *strangelets* exist, and that they possibly be negatively charged, the notion of probability we invoke is associated with our imperfect knowledge of the laws of nature. This notion is rather different from that associated with probabilistic evaluations caused by our inability to predict exactly -- perhaps because of insufficient knowledge of the initial conditions -- the outcome of a physical phenomenon whose dynamics we do understand, which is instead the context in which we talk more usually, in ordinary life, about probabilities, for instance when we state that the probability to get, say, a *two* by throwing a dice, is *one sixth* (in this case the relevant "classical dynamics", while well known, actually entails a "sensitive dependence" on the initial conditions). It is indeed clear that the question whether negatively charged *strangelets* are or are not stable can in principle be settled; either one or the other alternative is true, and we eventually might be able to find out for sure (especially if the answer is positive), and thereafter no room would be left for probabilistic assessments. The probability of producing a dangerous *strangelet* -- *if such a stable or metastable object does exist* -- by running the RHIC or ALICE experiment for some time belongs instead to the second notion of probability: even if we had a much deeper knowledge of nuclear and subnuclear physics than we now have, we could never hope to go beyond a probabilistic assessment as regards the risk of producing a dangerous *strangelet* in such circumstances. This is a fundamental consequence of the quantum character of the laws of microphysics, as we understand them. However, we could -- if we knew enough -- be able to estimate accurately that probability, and thereby possibly to conclude it is small enough to exclude any reasonable concern.

On the other hand, at the current stage of knowledge, it would be desirable to provide some kind of probabilistic assessment for all the components of the catastrophic scenario -- in particular, for the three points mentioned above -- in order to come up with an estimate of the risk -- unless one can convincingly argue that this risk is certainly so tiny that any attempt to quantify it is useless. In the context of such an exercise, the question shall arise whether these probabilities -- of which the second and third are presumably quite small, presumably the latter more so than the former -- are *independent*, and should therefore be multiplied to get a final assessment. I have heard arguments that suggest this to be the case. I am not convinced. For instance, an important element which might, as it were *simultaneously*, affect all these evaluations would be some (possibly unexpected and perhaps *a priori* quite "improbable") feature of that very very-many-body problem which would have to be mastered in order to establish the properties of (heavy) *strangelets*. And it would obviously be fallacious to multiply the probabilities based on theoretical considerations, with those based on empirical evidences, as we explain below, after we have tersely reviewed this second line of argumentation.

7. Let us then survey what can be learned by taking the *second point of view*. High-energy collisions of heavy nuclei occur naturally, when cosmic rays impinge on heavenly bodies or among each other in the cosmos. Yet no catastrophic event has been so far attributed to such collisions. Does this provide sufficient assurance that no disaster will occur in the experiments under consideration? The short answer is, unfortunately, rather *inconclusive* -- indeed *negative* if one believes that the evaluation must be prudently made on the basis of "worst-case analyses". But before providing some details, let us inject two remarks.

Firstly we like to emphasise that, if one tries to set an upper bound on the probability of a disaster occurring by arguments such as those just mentioned, and finds out that the upper bound thus obtained is not sufficiently small to conclude that the risk is small enough to be acceptable, this does by no means entail that the risk is indeed sizeable: it only indicates that that particular argument is not useful to provide confidence. In this respect the difference among what we dubbed above *first approach* and *second approach* must be emphasised: in the *first case*, one is trying to assess the *actual* likelihood that a dangerous outcome emerge; in the *second*, one is trying to find an *upper bound* to the probability that a catastrophe occur. Let us repeat the obvious: in the *first* case, if one gets from the analysis a probability that is not quite small, then concern is indeed appropriate; in the *second*, if it turns out that the computed probability is too large to provide assurance, this merely indicates that we do not have an argument that provides confidence, but it would be quite wrong to interpret such a finding as an indication that the probability of disaster has been shown to be sizeable, because the nature of the argument clearly prevents any such conclusion.

Secondly, we like to note an advantage of the *second approach*: it tends to be applicable to a larger variety of catastrophic hypotheses, rather than only to a particular kind of scenario. For instance -- if it did work -- it might also serve to exclude the risk that in high-energy ion-ion collisions such as those envisaged in the RHIC and ALICE experiments a different configuration of *ordinary* nuclear matter -- *not strange*: made up of *ordinary* nucleons -- be created which, if more tightly bound than the standard nuclear matter that constitutes standard (heavy) nuclei, might serve as "centre of condensation" for a transition of the nuclear matter of standard nuclei to this new configuration -- a transition which, if it were to involve a macroscopic chunk of matter, would be accompanied by a large release of energy and would therefore also entail catastrophic consequences. (For instance such a hypothetical, albeit implausible, more bound configuration of ordinary nuclear matter could be caused by a prevalence of the spin-orbit component of the nuclear force -- which can always be adjusted to be attractive -- over the central and tensor components, which provide instead most of the binding energy in standard nuclei. Such an anomalous "spin-orbit-bound" [5] configuration of nuclear matter would be characterised by large values of the relative angular momentum for every nucleon pair -- something that can indeed be in principle achieved -- and would therefore be very different from the configuration of the nuclear matter of standard (heavy) nuclei, and this might explain the metastability of such standard nuclear matter -- a metastability which might have a longer lifetime than that of the Universe, but might hypothetically be broken by a sufficiently energetic collision of sufficiently heavy nuclei -- giving thereby rise to a hypothetically catastrophic scenario analogous to that described above for strange nuclear matter -- although it is not clear in this case what the mechanism might be to cause the process to continue, so as to eventually involve macroscopic quantities of matter).

8. Let us then proceed to the *second approach* and report tersely two arguments which have been made (but also largely unmade) by BJSW [1], DDR [2] and JBSW [3], to provide, from empirical considerations based on cosmic ray phenomenology, confidence about the safety of the RHIC experiment. These arguments apply also to ALICE, but are less conclusive in that context. Of course, in order to be reliable, these arguments must refer to cosmic ray events analogous (in terms of the energies, and masses, involved) to those being envisaged in these experiments -- via direct evidence based on analogous cosmic rays events which have been actually measured, or via reliable extrapolations of such data (high energy cosmic ray data for heavy nuclei such as gold or lead are scarce).

BJSW [1] point out that analogous collisions to those that are planned at RHIC occur when cosmic rays hit the Moon. (The use of the Moon, rather than the Earth itself, for this argument is required because the majority of the cosmic rays that hit the Earth interact with its atmosphere before reaching the ground, and the atmosphere contains few heavy elements; hence in the case of the Earth the analogy with the collisions among *heavy* ions, or *heavy* few heavy elements, hence in the case of the Earth the analogy with the collisions among *heavy* ions, or *heavy* nuclei, is missing). Such collisions have occurred for a long time (the Moon is a *few billion years* old), without producing the catastrophic disappearance of our satellite. From this evidence they inferred [1] a very small upper bound on the probability that a catastrophe occur in the RHIC experiment, which would be quite appeasing, were it not for their failure -- as pointed out by DDH [2] -- to take due account of an important difference among the impact of cosmic rays on the nuclei in the lunar soil, and the collisions of heavy ions in the planned experiments. In the first case, a *strangelet* hypothetically produced in the collision would move with high speed relatively to the lunar matter and would therefore have a high chance to break up before coming to rest (to initiate the catastrophic scenario); in the second case the hypothetical *strangelet*, produced in a head-on collision of two ions, would be already almost at rest in the lab. Taking due account of this difference (and using in the related computations some rather extreme -- but perhaps not excessively so -- worst-case hypotheses), DDH [2] have shown that the safety margin provided by the persistence of the Moon essentially evaporates.

DDH [2] -- who also seemed bent at providing reassurance if at all possible, but insisted in trying to do so by the *second approach* (perhaps being motivated by lack of confidence on theoretical considerations alone; for instance they state that "*our understanding of the interactions between quarks is insufficient to decide with confidence whether or not strangelets are stable forms of matter*" [2]) -- tried then to review the relevant evidence based on cosmic ray phenomenology, but keeping in the process due account of the need to restrict attention to collisions in which a hypothetically formed dangerous *strangelet* would not be likely to break up before getting in equilibrium with the matter surrounding it.

Strangelets produced in cosmic space would eventually be swept into star matter (DDH [2] provide arguments that this would indeed happen, if the *strangelets* were negatively charged), and they would then cause

stars to blow up as supernovae, if the catastrophic scenario indeed prevails. But only about 5 supernovae per millennium are observed (and there are other well understood scenarios to produce at least some of them). In this manner DDH [2] obtain, as an upper bound to the probability of producing a dangerous *strangelet* in one year of running the RHIC experiment, the estimate 1/500,000,000 (o*ne over five hundred million*, namely *two billionth*). This argument also produces a bound for the ALICE experiment, which is however much larger.

DDH [2] state that this bound implies that "*it is safe to run RHIC for 500 million years*". A (substantially equivalent -- in operational terms! -- but) more correct -- albeit, perhaps, less appeasing -- language would state that this bound indicates that the time scale over which a catastrophe might emerge from the RHIC experiment is (at least) of the order of magnitude of *hundred million years*.

But this bound is only applicable if the catastrophic phenomenon is fast enough to yield a supernova event. If the process is slow, so that no visible supernova explosion emerges, a different approach is needed. DDH [2] then argue as follows. Firstly they observe that if the process is excessively slow, then one need not worry: in particular if it would take more than *ten billion years* to destroy the Earth, no concern seems appropriate, since anyway we expect that *ten billion years* hence planet Earth will be engulfed by a much enlarged Sun, which by that time will have become a red giant star. Hence, they focus on the intermediate range of a hypothetical scenario that is not so fast to yield a supernova explosion, yet it is fast enough to cause reasonable concern in terms of the Earth getting destroyed before its natural death, as predicted by current astrophysical expectations, occurs. In this context, they look at the increased luminosity of stars that would be caused if some of them were destroyed, even relatively slowly, by *strangelets*, and they get again an upper bound of the same order of magnitude, or perhaps -- if worst-case assumptions are made to model the destruction of a star caused by the *strangelet* mechanism -- a bound *one hundred times larger* -- which would indicate that the time scale over which a catastrophe might emerge from the RHIC experiment is still quite large, of the order of (at least) *million years*.

But this part of their analysis seem to me somewhat unconvincing -- in particular, their modelling of the dynamics of star destruction via the *strangelet*-caused mechanism. This point is, however, moot, since the DDH [2] bound is invalidated [3] if one takes account of the possibility that *strangelets* be metastable -- with a lifetime short enough for them not to be "eaten" by stars once they are formed in interstellar space, yet long enough to cause a catastrophe when they are produced at rest in the lab.

It seems in conclusion that the empirical evidence from cosmic rays yields no appeasing upper bound on the probability of producing a dangerous *strangelet* in the experiments in question, at least if one insists that any such bound, to be entirely reliable, should be obtained by treating uncertainties via worst-case hypotheses. Indeed JBSW [3] (rather in contrast to BJSW [1]) state:

"*By making sufficiently unlikely assumptions about the properties of strangelets, it is possible to render both* *of these empirical bounds irrelevant to RHIC. The authors of Ref. [2]* [namely, DDH] *construct just such a model in order to discard the lunar limits: They assume that strangelets are produced only in gold-gold collisions* [this is imprecisely stated -- the assumption is that strangelets be stable only at masses large enough, of the order or larger than the mass of two gold nuclei]*, only at or above RHIC energies, and only at rest in the centre of mass* [this is also imprecisely stated]*. We are sceptical of all these assumptions. If they are accepted, however, lunar persistence provides no useful limits. Others* [presumably a call to Ref. [5] is missing here, which reads: "*We thank W. Wagner and A. Kent for correspondence on the subject of strangelet metastability*"; indeed, this Reference is quoted nowhere in JBSW [3] !]*, in turn, have pointed out that the astrophysical limits of Ref. [2] can be avoided if the dangerous strangelet is metastable and decays by baryon emission with a lifetime longer than sec. In this case strangelets produced in the interstellar medium decay away before they can trigger the death of stars, but a negatively charged strangelet produced at RHIC could live long enough to cause catastrophic results. Under these conditions the DDH bound evaporates.*"

9. There is one other point that does not seem to have been taken quite into consideration in these analyses: namely the possibility that the catastrophic scenario, rather than ending up in the destruction of the entire planet

Earth, yield "only" a local calamity. This might, for instance, possibly be the case if *(i)* there were a valley of stability or metastability for *strangelets* of masses, say, from *300* (in units of nucleon mass) to some finite mass *B* -- analogous to the situation for standard nuclei, except for the fact that, in the dangerous *strangelet* case, there would be a lower mass limit (here arbitrarily guessed at *300*) and at least some negatively charged specimens would also be included among the stable and metastable *strangelets*; and if moreover *(ii)* heavier *strangelets* had a sufficiently large probability to *fission* into such stable or metastable *strangelets*. If the various lifetimes and cross sections for the various decays and reactions were properly adjusted, a chain reaction might be initiated by the production of a negatively charged *strangelet* in the lab and it might result in a nuclear explosion, which might however stop before reaching astronomical proportions. Such fine-tuning of parameters might look contrived, hence unlikely; but it would not be the first time that Nature surprises us: who could have *a priori* guessed that standard nuclear physics was so finely tuned, not only to allow the creation of controlled nuclear reactions, but even to organise naturally such an experiment on our planet, over geological times, in the Uranium-rich mines of Gabon? Moreover, what about the observation (anthropic principle?) according to which, of the infinitely many other possibilities, quite a number must be excluded since, if they had prevailed, we would not be here to argue about them.

10. But let us abandon such far-fetched speculations, to try and summarise this part of our discussion. To this end it is perhaps both expedient and instructive to quote GW [4]:"*If strangelets exist (which is conceivable), and if they form reasonably stable lumps (which is unlikely), and if they are negatively charged (although the theory strongly favours positive charges), and if tiny strangelets can be created at RHIC (which is exceedingly unlikely), then there just might be a problem. A new-born strangelet could engulf atomic nuclei, growing relentlessly and ultimately consuming the Earth. The word 'unlikely', however many times it is repeated, just isn't enough to assuage our fears of this total disaster.*"

GW [4] then go on to report that, by relying on what we called above the *second approach*, sufficiently small upper limits can be put on the risk probability. They report the lunar argument of BJSW [1], without mentioning the criticism of it by DDH [2], and quote the BJSW conclusion ("*cosmic ray collisions provide ample reassurance that we are safe from a strangelet-initiated catastrophe at RHIC*" [1]), and likewise they quote uncritically DDH [2] ("*beyond reasonable doubt, heavy-ion experiments at RHIC will not endanger our planet*"); and they appeasingly conclude that "*even though the risks were always minimal, it is reassuring to know that someone has bothered to calculate them.*" Unfortunately this conclusion, to the extent it relies on the *second approach*, seems to me to be by now somewhat unjustified -- as we have tried to explain above. This is, to some degree, reflected in the modified flair of JBSW [3] relative to BJSW [1]: indeed the sentence from BJSW [1] quoted by GW [3] -- which was the final sentence of this Report [1] commissioned by the Director of BNL, and was indeed introduced by a rather peremptory "*we demonstrate that*" -- is no more to be found in JBSW [3], and it is replaced there by the following final paragraph of the introductory section (which indeed follows the one we quoted above, at the end of § 8):

"*We wish to stress once again that we do not consider these empirical analyses central to the argument for safety at RHIC. The arguments which are invoked to destroy the empirical bounds from cosmic rays, if valid, would not make dangerous strangelet production at RHIC more likely. Even if the bounds from lunar and astrophysical arguments are set aside, we believe that basic physics considerations rule out the possibility of dangerous strangelet production at RHIC.*"

production at RHIC."

11. In conclusion the main arguments to allay fears of a catastrophic outcome of the experiments RHIC at BNL and ALICE at CERN is (i) the unplausibility, on theoretical grounds, that stable or (sufficiently long-lived) metastable *strangelets* with negative charge (i. e.., "dangerous *strangelets*") exist, and (ii) the hunch that, even if they do exist, the probability that even a single one of them be created in these experiments is exceedingly small (but how small is small enough? -- more on this below).

People might feel that (iii) the empirical arguments based on cosmic-ray phenomenology, even if not totally convincing, provide additional confidence. Perhaps so. Yet I have also read that each of the 3 arguments, (i), (ii)

respectively (iii), can be interpreted as providing (tiny) upper limits, call them respectively , to the probability that a catastrophe occur, and that these 3 small numbers should be multiplied to obtain a final estimate of the upper limit to the probability of a catastrophe, , since these 3 estimates are based on independent arguments. I think this is unconvincing as regards the first two probabilities, and , because the arguments that lead to them are not quite independent, as pointed out above; and it is, in my opinion, definitely incorrect as regards the third probability, , which is based on *empirical* considerations rather than *theoretical* analyses.

(Indeed, imagine you are tasked to estimate the probability to draw a black ball from a box. The *theoretical* information you have is that the box contains two balls, one black and one white; you also know *empirically*, from a number of previous draws, that the black ball came out about half the times. So your estimate based on *theory* is that the black ball has probability 1/2 to be drawn; your estimate based on *empirical data* also suggests it has probability 1/2 to be drawn; do you then conclude the probability is 1/4 ? I would not have introduced parenthetically this trivial argument, were it not for the fact that an eminent colleague indeed suggested the probabilities, , mentioned above should be multiplied, and he has not yet recognised -- to the best of my knowledge -- that he was wrong on that count; so, in deference to his scientific eminence, I must continue to doubt whether my trivial example is really applicable. Let the reader judge).

Let us moreover recall the fundamental difference among the probability, call it , that a "dangerous *strangelet*" exist, and the probability, call it , that such a "dangerous *strangelet*" be produced in a particular experiment. In particular (once it is well defined by providing a precise definition of "dangerous *strangelet*", mainly by specifying its minimal lifetime) is a probability that originates from our ignorance of the theoretical framework and our inability to perform reliable computations; it would become exactly one or nil if we had a complete understanding of the matter, namely if we could ascertain with certainty whether dangerous *strangelets* do or do not exist (for instance, we know for sure that no stable nucleus exist which is composed only of neutrons -- although we also know that neutron stars, kept together by the long-range gravitational force, can exist and indeed most probably do exist). Acquiring such knowledge is of course entirely possible; and of course knowing for sure that dangerous *strangelets* (with mass less than, say,) do not exist ( ) would be the most convincing way to allay any concern about the ion-collider experiments at BNL ( ) and CERN ( ). The value of -- a quantity which of course only makes sense if dangerous *strangelets* do exist, and only for experiments in which they can *in principle* be produced (in particular, the total mass of the two projectiles should exceed the mass of the *strangelet*) -- is instead never exactly zero. Indeed, by definition, it is a positive number, although it could nevertheless be sufficiently small to allay any reasonable concern -- indeed this is what most expert seem to think, even if they do not seem (so far) able to come up with a quantitative estimate (for this reason I used above the term "hunch" -- although the justification for my doing might be viewed as mot, inasmuch as it is merely based on the difficulty to come up with a reliable quantitative estimate, see below).

12. My personal assessment of the situation concerning the ion-ion experiments at BNL and CERN is not one of serious concern, because I have confidence on the assessment of the experts who unanimously state there is no danger. I am, however, a bit disturbed by what I perceive as lack of a very sustained effort at getting more quantitative estimates of the various probabilities involved in this problematique than I saw reported so far. I wonder in this context about the ratio of the funds that have been devoted to such an endeavour, relative to those that have

gone and are going into the experimental set up. If that ratio is less than, say, a few percent, I would -- in my admitted naiveté -- feel puzzled and disturbed (however, as a theoretical and mathematical physicist, I might be biased in this assessment).

In this context, I hope (and expect) the Committee tasked by the Director-General of CERN to look into the matter will do a thorough job, and I also hope their findings will be adequately scrutinised by the expert community after they are published. The usefulness of such open discussions is of course obvious, and it has indeed been demonstrated by the progress in understanding these matters that has resulted from the fruitful reciprocal criticism among the two experts groups BJSW and DDH [1-3].

13. But I am also somewhat disturbed by what I perceive as a lack of candour in discussing these matters by many -- including several friends and colleagues with whom I had private discussions and exchanges of messages -- although I do understand their motivations for doing so. Many, indeed most, of them seemed to me to be more concerned with the public relations impact of what they, or others, said and wrote, than in making sure the facts were presented with complete scientific objectivity

This is, of course, a subjective assessment, for which I must take personal responsibility. It has, in any case, motivated me to also try and outline below some scientific-political-ethical-sociological considerations related to this kind of issues, which are perhaps of more general validity than referring to this particular case, although this is a significant example which I keep in mind throughout these reflections.

14. First of all it is clear to me that risk evaluations can be reliably done only by *experts*: in this particular case by experts on nuclear and subnuclear physics, as well as, to the extent relevant, on astrophysics etc.; and also by experts on the evaluation of the risk of mishaps which are both "extremely unlikely and extremely catastrophic". And it is of course appropriate that, to the maximal extent possible, those who are tasked with making such evaluations not be affected by any "conflict of interest". If an experiment potentially dangerous is being planned by a group in a lab, it is of course desirable that risk evaluations be performed by scientists who have no interests vested in the performance of that experiment. This is, of course, not always easy -- since the more knowledgeable experts are often to be found just among those who are also most keen to see the experimental results in question.

It is also obvious that an adequate investment in risk evaluation should be made before the funds already spent in the preparation of an experiment render its cancellation exceedingly wasteful and therefore hard to decide, even if there were good reasons to do so.

15. Such risk evaluation exercises yield eventually a probabilistic estimate, which is arrived at after detailed investigations of catastrophic scenarios, that often entail lots of detailed computations, in which context choices must often be made among "most plausible" and "worst-case" hypotheses. Prudence of course suggest that the latter be preferred, but judgements may vary. It is therefore quite possible that different groups of experts end up with final evaluations which do not quite tally. Just for this reason it is generally desirable that more than one team be engaged in such analyses -- as already mentioned above, a desirable technique is to task a *blue team* to perform as *objective* an analysis as possible, and a *red team* to act as "devil's advocate", namely to try deliberately to prove that the experiment in question is indeed dangerous -- of course always using sound scientific arguments. At the end the two teams should compare their findings, and especially the analyses which led to those results. This approach is also desirable to minimise the possibility that a risk scenario be altogether ignored. I understand such a procedure is (more or less) generally followed whenever a potentially dangerous enterprise is undertaken, for instance the construction of a nuclear power plant. In such a case generally the *blue team* is organised by those who plan the plant, and the *red team* by those who authorise its construction.

Once such an analysis has been performed, it should preferably end up with quantitative conclusions, in the guise of probabilistic estimates -- possibly yielding a range of values. Then the nontrivial question arises of what the *acceptable* value for the probability of a disaster happening should be -- namely a value small enough that the risk be considered worth taking. This of course depends quite significantly on the magnitude of the catastrophe that might occur if things went wrong, and on the advantages that accrue by proceeding with the project. Especially when the gains are purely scientific, there is clearly the danger that an excessively prudential approach end up in impeding scientific progress.

Anyway I suggest some kind of benchmark to assess "acceptable" probabilities of extreme catastrophes might be set by the probability of some such impending "natural" catastrophe. I understand for instance the probability that an asteroid with diameter over 10 Km hit our planet -- an event which would most likely put an end to the human presence on Earth -- is estimated to be of the order of *one over one hundred million* per year.[6] It is probably correct to argue that any man-made risk of total catastrophe should be smaller than any natural risk -- but it seems to me reduction by an extra small factor -- *one tenth, one hundredth, one thousandth*, perhaps *one over ten thousand* -- should be sufficient. Hence I would probably advise in favour of authorising a *worthwhile* undertaking -- be it a scientific experiment or some other worthwhile human enterprise -- if I were *reliably* guaranteed it entails a risk of ultimate catastrophe per year less than *one over one trillion* (probability per year less than).

Of course what I have in mind here are major undertakings, which involve large investments of funds technology expert manpower – and which therefore, as it were automatically, entail a careful and responsible behaviour. The situation is quite different if consideration is extended to actions which can be performed on a much smaller scale – as is for instance the case for certain experiments in molecular biology and genetic engineering. Indeed simple arithmetic shows that the human experiment on Earth is unlikely to last for many more centuries if all humans on this planet were to exercise a hypothetical *individual* right to engage in an activity that entails a probability per year to cause a global catastrophe!

Considerations of this kind – paradoxical as they may appear – underline the obvious need to pay special attention to all those scientific/technological developments that entail the risk of major catastrophes being produced by small scale endeavours – involving few individuals.

On the other hand, even in the context of major kind of enterprises such as those discussed above, I will not be surprised to find many who strongly disagree with the figure suggested above -- -- possibly because they think that upper bound to the probability of ultimate catastrophe per year should be set to a much smaller value, quite likely because they would argue it should be to "zero". Those who make the latter argument would probably not be impressed if I pointed out to them (and rigorously proved, if not convincingly to them, certainly to the satisfaction of every knowledgeable physicist), not only that there indeed is a *nonzero* probability that our planet Earth explode within the next second, but moreover that every act of their life -- including each time they breath -- has a *nonzero* probability to cause the end of the world, in whichever way they like to define this event as well as the term "to cause"!. (The point is of course that *zero* is a very special number -- indeed, it was recognised as a number only a few centuries ago -- it is *qualitatively* different from any small number, however small that may be: and of course the *nonzero* numbers we are talking about here are indeed much smaller than those we indicated before -- perhaps might be a representative wild guess!).

The discussion in the previous paragraph seemed to lead us astray. In fact, it raised two important, and related, issues: (i) Who should, in the end, make decisions? (ii) How to cope with the fact that the majority of people are simply unable to comprehend a probabilistic argument?

16. Granted that (fortunately) it is not up to me to take decisions, who then should decide? Let us firstly -- for the sake of making the argument concrete -- focus on the RHIC experiment: should a controversy about its safety emerge and should the laboratory governance get overruled, who should/could finally decide whether it should proceed? Since the experiment takes place in the USA and it is funded by American taxpayers' money, clearly the decision-makers in this instance are the relevant US institutions: the President, Congress, the judiciary. But to the extent that experiment puts at risk the entire planet Earth, is it fair that the (non American) majority of world citizens have no say whatsoever on the matter?

In any case, even US citizens may have some say in this matter only in a very indirect way, namely to the extent those who decide are their representatives -- within that context of representative democracy which still seems the better system of governance human kind has been able to devise so far. In any case, what I think is really important is that American citizens, as well as the rest of us who live on this planet, have a reasonable assurance that, within the decision-making system that eventually decides, there exist an adequate capability, and will, to make a competent and objective evaluation of the risk.

As for the CERN experiment, the situation is somewhat less clear-cut. CERN is an European institution located between France and Switzerland. I understand the final decision, on such matters as authorising a potentially dangerous experiment at CERN -- if it ever had to be taken at a political level beyond the CERN governance -- would be taken by French rather than Swiss authorities. Somebody told me this was arranged so that such decisions could not be subject to a popular referendum -- a practice common in Switzerland, and inexistent in France. I do not know whether this gossip is true; I was informed that the legal department at CERN has cogent arguments to sustain the validity of this choice (arguments I would certainly not dare to challenge, nor indeed even to scutinize: not my cup of tea). In any case I am in no position to judge whether or not such a decision was wise. As it is, if a controversy were to emerge over whether to go ahead with the ALICE experiment at CERN, and if the CERN leadership -- who are to begin with the natural decision-makers on this matter -- were eventually overruled (a development I would not *a priori* like), then the final decision would rest with the French decision-making system (be it the executive, legislative, or judicial power, as the case may be): the decision-making system, to be sure, of a democratic and highly civilised country, which however does not have a spotless record on such matters.

(Suffice to recall the following episode. Years ago -- perhaps in the aftermath of the catastrophe associated with the use of AIDS-infected blood for transfusions -- an "independent" National Committee was created in France by President Mitterand to assess potential "great risks"; and it was granted the power, somewhat unusual in France, not only to select which topics to focus upon, but also to publish its findings, without having to ask for a governmental permission to do so. That Committee did use such powers, for instance in the context of its assessment of the potential risks associated with the fact that a new TGV (fast train) track was planned to pass close to a nuclear power station -- the TGV path is a very delicate political issue, with important electoral implications. Perhaps for this reason -- in any case, without any explanation being proffered -- that Committee was soon after, very suddenly, abolished -- via a one-line rider in the context of the omnibus budget bill).

This is not to say I would prefer that Switzerland rather than France be the ultimate decision-making authority over whether or not to make an experiment at CERN: indeed my clear preference is that such decisions be taken by CERN’s own decision-making system. Yet I can't help asking myself how the citizenry in Switzerland views this matter.

17. We are indeed coming again here to a crucial point: the role of citizens, who in democracy should ultimately be the determining element in decision-making. But to play this role responsibly, citizens should possess some understanding of the basic facts; yet this is sometimes, nay often when decisions concern scientific issues, next to impossible for the "man in the street", or for the "housewife in the kitchen" (to mention some politically incorrect, yet realistically quite relevant, stereotypes). A relevant example in this context is precisely the point mentioned above (end of § 15), namely the difficulty to understand probabilistic arguments, in particular those based on very small, yet finite, probabilities; arguments which are essential to form an informed opinion about any assessment of the risk of very major catastrophes having very low probability of occurring.

It seems to me in this context the scientific community has a great responsibility. There is of course a strong temptation to take a "let the matter to us" attitude. But clearly this is neither quite right nor politically viable, nor, indeed, generally acceptable: indeed some of my elementary particle colleagues, whose tendency towards such a stand I found somewhat disturbing when I broached with them the potential danger of high energy ion-ion collision experiments (especially when it tended to take the subliminal form of simply joking the matter away), were themselves quite unwilling to accept such an attitude in the matter, say, of molecular biology experiments involving gene manipulation: there, some of them kept insisting, we know the dangers are real, hence oversight over what scientists (i.e., biologists -- not fellow physicists!) are up to is needed.

18. There also is a strong temptation for many in the scientific community to view these matters primarily in terms of public relations. This may be subliminal or deliberate; it may be good or bad (in my judgement).

A deliberate effort to inform the public -- particularly those closer at hand, who might influence local political decision-making -- on the scientific activities being undertaken is, in my opinion, a legitimate part of the public relations game, especially in major labs which require large public funding: and in this context even a certain amount of hype (of the results) and minimisation (of the dangers -- if any) is acceptable, especially if the task to propagate these notions is performed by public relation professionals rather than by the scientists themselves -- who should instead, I submit, rather keep a low profile, indeed be modest-minded, or at least act modestly: preferably the former, but at least the latter, to respect a scientific etiquette which is part of a more civilised way of living (a context, however, where the danger that bad behaviours eventually prevail over good ones looms large -- especially in those scientific environments, such as contemporary "high-energy" experimental physics, where the main traits required of top scientists are managerial skills and aggressiveness rather than thoughtful scholarship).

What however must by all means be avoided is any attempt at obfuscation in the context of scientific analyses. This seems so obvious not to require any mention. But in fact it is not obvious at all. And it seems to me a scrutiny of the papers [1-4] on which this discussion has largely focussed provides as good an illustration as any, of the potential pitfalls that may emerge when scientific writings are somewhat affected [1-3]-- or even primarily motivated [4] -- by the concern "not to alarm the public"; not to mention editorial policies, such as that underlining the decision by *Nature* to refuse to publish DDH [2] on the grounds it was of no interest to their readers, which smack of deliberate attempts to manage public opinion rather than to inform it.

The possibility that, on certain topics, the public may indeed "become alarmed" by reading a scientific paper (or quotations from it) does exist, since it is -- rather, it should be -- in the nature of such writings to avoid peremptory statements; while, when treating of extreme potential dangers, only peremptory statements (excluding any possibility that such risks might indeed materialise) are adequate to allay the fears of the public. And the concern is in my opinion justified that an "alarmed public" might lead to "irrational" decision-making, which might interfere with sound scientific progress, and perhaps end up in damaging the public interest (which, we trust, is indeed vested in the promotion of sound scientific progress). But any attempt by the scientific community to remedy this situation by resorting to lack of candour and transparency is in my opinion unwise and dangerous. For two, equally important, reasons.

19. If the scientific debate gets muted or distorted -- or altogether suppressed -- because the imperative "not to alarm the public" takes precedence over the objective candour and the open confrontation of points of view which is a main characteristic of the scientific method, then the danger of eventually indeed making some silly mistakes increases significantly. This has been illustrated time and again in the context of military research, when such debates (for instance on the effects of radiation on military personnel and civilian populations) were altogether suppressed -- especially in totalitarian societies; but not only there -- by imposing military secrecy. But even in the context of open scientific research, one should not forget that grossly mistaken assessments have sometimes been made by well meaning most competent scientists (a classical example being the famous pronouncement by Lord Rutherford -- foremost nuclear physicist of his times -- that any prospect of exploiting nuclear energy was "moonshine"). The only cure against this risk is the scientific method of completely honest, completely candid, give-and-take open debate among competent practitioners. As we already noted above (end of § 12), this has indeed been once more demonstrated by the scientific exchanges that resulted in the JBSW [3] revision of BJSW [1].

20. Another, no less important, drawback of any deviation by the scientific community -- under the banner of combating alarmism -- from the practice of completely unrestrained open candid unobfuscated transparency in all their utterances, is the (justified!) lack of confidence by the general public in the "integrity of scientists", that is eventually likely to result from any hint that such behaviour is prevailing. Such a lack of confidence is particularly deplorable precisely in the context we are discussing here. Indeed I would like to conclude this paper by reaffirming my strong belief that it is most desirable that any decision on the kind of matters we have been discussed herein be, if not always taken -- which would be politically impossible, and indeed ethically dubious in a democratic context -- certainly always primarily influenced by the scientific community, rather than by demagogues or charlatans or, at best, incompetent generalists. But this will become impossible -- at least in a democratic context (to which I assume few would like to renounce: particularly those of us who had some chance to experience the alternative!) -- if such a (justified!) lack of confidence by the general public in the scientific community will eventually prevail.

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Back

$Z / A \cong 1/2$

$\approx 10^{-7}$

P_1, P_2

P_3

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p

$p < P = P_1 P_2 P_3$

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P_1, P_2, P_3

p_1

p_2

p_1

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$p_1 = 0$

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$A_c \cong 420$

p_2

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