letters

Test-ban debate

I want to comment on the "Debate on a comprehensive nuclear test ban" (August, page 24). The debate focuses largely on questions of whether reliability of existing or future nuclear weapons could be assured under a comprehensive test ban. Both Hugh DeWitt and Robert Barker appear to accept as a common premise the need for reliability of our nuclear arsenal. If present US policy is to develop a first-strike capability (as ample evidence suggests), then of course a very high degree of reliability will be essential. I doubt underground testing can provide this degree of reliability. If, on the other hand, the US and Soviet nuclear arsenals are to serve only their stated purpose as deterrents to nuclear war, then a reduced level of confidence in their reliability is most desirable. Either power would be less likely to launch a first strike with weapons of unproven reliability, since its weapons might fail while its opponents' might not.

STEVE ARNOLD
Honeywell, Inc.
8/83 *Minneapolis, Minnesota*

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In their terribly stimulating essays, Hugh DeWitt and Robert Barker clarify two issues regarding a total nuclear test ban. DeWitt, unfortunately, makes an assumption which vitiates his argument (pro), but Barker (con) gives him the win anyway, since a logical extension of Barker's arguments leads to DeWitt's thesis.

DeWitt argues that weapons design can (and should now be) done in computers rather than underground. But he assumes that reliability will not be compromised, which raises the question: Why transfer the experiments from the field to the computer, if we continue to develop the weapons reliably? What difference would it make if the experiments were done in soil or in software? Concern for the environment does not seem central to DeWitt's argument; that word appears nowhere in the essay. In fact, wouldn't the transfer of the technology to computers lead to less intelligence information about the Soviets (and vice versa), hence to greater uncertainty, greater distrust, greater chance for misunderstanding and mistake?

Barker makes the assumption that transfer of the technology to computers would inevitably lead to degradation of reliability, and hence an increase of vulnerability. Although he does point out that this effect may not be completely symmetrical between the US and the USSR, it seems to me that the differential would be less important than the lessening of reliability on both sides. The net result of reduced reliability would likely to be a loss of interest in such systems on the part of the field commanders (and their superiors) who are charged with carrying out effective battle. General Brown may well opt to order up ten "good-ole" HE rockets he knows will work rather than one complicated high-tech gadget that has never been exploded. In the long run, wouldn't this lead to a reduction in the present love affair with nuclear weapons, and a de-emphasis of such weapons in favor of conventional ones?

The problem with nuclear weapons is not how to construct them reliably, or even how to not construct them, but how to prevent deploying reliable weapons. In the absence of an ability to prevent construction or deployment, it may be that an effective approach to nuclear disarmament would be to agree on a course that is ultimately guaranteed to construct and deploy unreliable weapons, and thus break our spiraling fascination with them.

ROBERT W. SCHMIEDER
Sandia National Laboratories
8/83 *Livermore, California*

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The title of the piece unfortunately does not describe the articles that follow. Hugh DeWitt has indeed presented the "pro" side of the question as he sees it. Robert Barker, on the other hand, provides background information on nuclear weapons that he believes debators of the issue should know about, but studiously avoids debate himself. He ends with the exhortation: "A real discussion is called for. An

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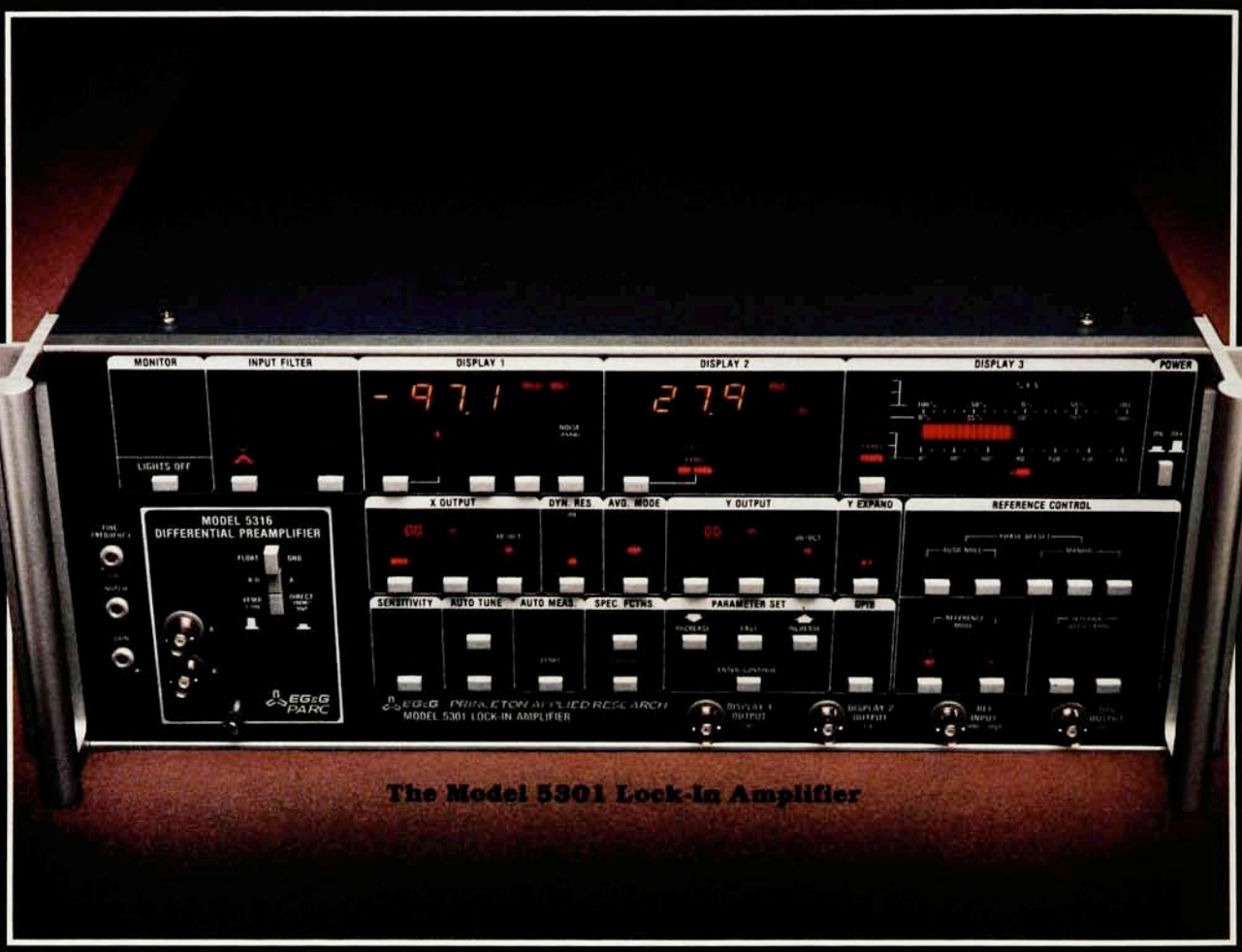

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informed debate should begin." Evidently, he was instructed by the Livermore Laboratory not to take sides.

Barker not only eschews debate, but sidesteps an issue of fundamental importance: that of continued operability of our stockpile. DeWitt addresses this point at some length, and quotes Bradbury, Garwin, and Mark's unequivocal "yes" answer to the question:

> Can the continued operability of our stockpile of nuclear weapons be assured without future nuclear testing? That is, without attempting or allowing *improvement* in performance, reduction in maintenance cost, and the like, are there non-nuclear inspection and correction programs, which will prevent the degradation of the reliability of stockpiled weapons?

I would like to know Barker's answer.

I believe that robust nuclear weapons *can* be designed without excessive penalty in performance. That is, once designed, built, and proof-tested, their continued operability in stockpile can be assured with appropriate inspection, correction, and remanufacture programs without requiring further nuclear tests. Bradbury, Garwin and Mark evidently believe that the nuclear weapons now in our stockpile are robust in this sense.

I do not know whether or not the nuclear weapons in our stock *are* robust. I believe, however, that they can be and should be. The possibility of a comprehensive test ban ever being acceptable to our government depends on it, as I believe both Barker and DeWitt would agree.

RAY E. KIDDER
Lawrence Livermore National Laboratory
9/83 *Livermore, California*

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Robert Barker has based his opposition to a comprehensive nuclear test ban almost entirely on the need to maintain the reliability of our nuclear weapons through continued testing. Now the USSR would presumably have the same problem of maintaining reliability during a test ban. How wonderful if both sides could simultaneously lose confidence in their nuclear weaponry! It would greatly decrease the likelihood of a first strike, and it might remove the hair-trigger atmosphere that exists when a first strike is a real possibility.

JOHN E. TANNER
Exxon Nuclear Idaho Company
9/83 *Idaho Falls, Idaho*

AN AUTHOR COMMENTS: Both Steve Arnold and Robert Schmieder use "reliability" in a statistical sense and argue that reduced "reliability" would lead a nation contemplating a first strike to conclude that because only some fraction of their weapons might work, they could not accomplish such a mission. In the "reliability" (a word chosen by PHYSICS TODAY editors) section of my article, I used the word "confidence" throughout to bring attention to the fact that, without testing and with the inevitable age-related changes that occur in nuclear weapons, the situation may well arise in which one might believe that *no* weapons of a given type will work. If this weapon is a major element of the deterrent force, "confidence" in the deterrent will be undermined and, I argue, a major instability will be introduced into international relations. While it can be argued with some credibility that reduced reliability in first-strike weapons is a good thing, I have yet to hear good arguments for the thesis that loss of confidence in second-strike, deterrent weapons is a good idea.

In the concluding paragraphs of my PHYSICS TODAY article, I state there are those who "... assert that the United States can, without testing, maintain confidence in its stockpile..." Ray Kidder apparently is a member of that group who unfortunately are not enlightened as to the basis for their belief. Neither to my knowledge was the original letter of Bradbury, Garwin and Mark. My observation of this group is that it is generally composed of those whose relationship to nuclear design, if it exists at all, is theoretical rather than applied. It may be nice to argue from a theoretical perspective that perfection is possible; it is a different matter to design, engineer, manufacture and age a real device. It would be helpful to their argument to cite a single precedent of appropriate complexity.

Rather than side-step the issue, I believe it is addressed directly in the section that was labeled "reliability" and contains my discussion of "confidence." I know of no way for real people, real governments and real manufacturing facilities to achieve Kidder's theoretical goal. Loss of confidence is but one of several negative impacts of a comprehensive test ban which must ultimately be evaluated in any debate of a CTB.

Faith in Kidder's logic is undermined early in his letter by the conclusion that only because of instructions from the Laboratory did I not "take sides." Conversation with either Hugh DeWitt or myself could have informed him of the history of both articles. He has correctly defined the purpose of my article—to provide background for informed debate. I continue to believe the debate is worthwhile if it encompasses all issues and sticks to facts rather than fantasies.

ROBERT B. BARKER
Lawrence Livermore National Laboratory
9/83 *Livermore, California*

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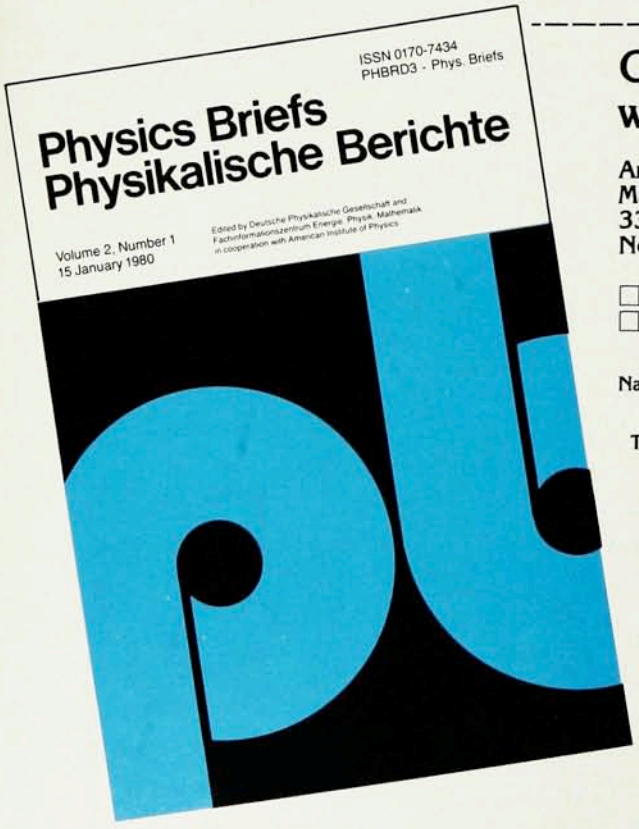

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AN AUTHOR REPLIES: In their letters, Steve Arnold and Robert Schmieder both question what they perceive as a basic premise underlying my arguments in favor of a comprehensive nuclear test ban. To answer them, I think it is helpful to ask what is the purpose of the nuclear arsenals now possessed by two superpowers. If either side regards its nuclear weapons as intended for war fighting in any future conflict, then there is very little chance of ever attaining a test-ban treaty or any realistic reductions in the present nuclear arsenals. In this case, there will always be great pressure to continue nuclear testing, both to modernize the present nuclear weapons and to improve them so that the other side will not obtain an advantage. On the other hand, if both sides can agree that the primary purpose of nuclear weapons is solely to deter a nuclear conflict, then I believe that a nearly complete ban on further nuclear testing is possible and desirable. The existing huge stockpiles of nuclear weapons and effective delivery systems possessed by both sides represent a formidable deterrent against either side's using them first, either as a first strike or in a smaller conflict. The present deterrence capability will persist for years to come even if no further improvements in nuclear-bomb technology are made. However, in view of the long history of tension between the superpowers, it is most likely that any US president will ever agree to a complete test ban treaty, *if* there is a serious possibility of degradation of our nuclear stockpile in years to come. This was a basic premise behind my article, as was clearly pointed out in the final sentence of Ray Kidder's letter.

Both Arnold and Schmieder also suggest that degradation of nuclear stockpiles might be a good thing since reduced confidence in reliability would lessen the likelihood that these weapons would be used in future conflicts. This is a nice idea, but in my opinion it is completely unrealistic. I doubt very much that either side would ever accept a political agreement, that is, a test-ban treaty, that would result in eventual unrealiability of nuclear weapons as a deterrent force. The possibility of an asymmetry in the assumed degradation of the stockpiles of the two superpowers makes this idea even more unlikely. However, a realistic test-ban treaty might be accepted by the superpowers if confidence in the deterrence capability of existing stockpiles could be maintained. For this reason, I spent considerable effort in my article to bring out the fears of the American nuclear-weapons establishment that aging nuclear weapons in our present stockpile could not with confidence be replaced by newly manufactured bombs in the future. In his letter, Kidder expresses this point very clearly, as did weapons experts Bradbury, Garwin and Mark in their 1978 letter to President Carter.

Barker discusses in his article a number of significant technical matters in nuclear-weapons development and testing, but he evades a fundamental question. Namely, can a thoroughly tested and proven American bomb design of the present be reliably manufactured in future years? Kidder asks this question again in his letter, and Barker does not answer in his reply to Kidder. We all agree that nuclear bombs are complex high-technology devices and that, from the point of view of those people charged with guaranteeing the reliability of the American stockpile, periodic nuclear testing gives added confidence. Statements emanating from the weapons labs mentioned in my article suggest that American bomb designs are by now so sophisticated and delicate that they cannot be reliably manufactured in the future without nuclear testing. In effect, modern bomb designs effectively would preclude a comprehensive test ban treaty if they *require* nuclear testing in the future. I suggest that it is possible to design reliable nuclear bombs that can be manufactured in the future and not require future testing.

Schmieder suggests that I am arguing for bomb design by computers and without underground testing. This is hardly true. Weapons designs are very thoroughly analysed by computers now and always will be. Regardless of the computing power of any weapons laboratory, a new design or a significant change of an existing design will always require testing before it is manufactured for the American stockpile. Thus a realistic test ban treaty will simply stop the introduction of new designs into the stockpile. He also questions why I don't mention environmental effects of nuclear testing. The reason is simply that environmental effects of underground nuclear testing are minuscule compared to the effects of a nuclear war of any kind. However, it is worth noting that even underground tests occasionally vent. When the Baneberry test in 1970 vented, a cloud of radioactive dust went several thousand feet above the Nevada Test Site and was eventually tracked as far as North Dakota.

Barker ends his article by calling for an informed debate on the effects and desirability of a comprehensive test-ban treaty. In fact, this debate is well underway. Even if Barker chooses not to address the points made in my

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article, I think he cannot lightly dismiss the letter of Bradbury, Garwin and Mark to President Carter in 1978. These men have spent their professional lives in the weapons establishment and are not engaging in fantasies in their arguments for a comprehensive test ban.

HUGH E. DEWITT
Lawrence Livermore National Laboratory
9/83 *Livermore, California*

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In your informative debate on a comprehensive nuclear test ban, Robert Barker advances a number of arguments for the necessity of tests and calls for an informed debate. Such an informed debate is made very difficult in the face of military secrecy. There are, however, some considerations that scientists can confidently advance. Experience with large-scale accelerators has shown that components can be reliably tested so that the completed machine can confidently be expected to work; I do not know of any case of failure of the ultimate test. Uncertainty exists only to the extent that some unknown physics is involved that can only be experimentally tested with the full device.

In the case of a high-energy heavy-ion accelerator, not enough is known about the instability of the high-current ion beams and so on. In the case of the bomb, I believe that the equation of state of fissile material at high temperatures and pressures is not sufficiently known. If they were faced with a possible test ban, Livermore scientists could engage in a truly scientific program of computations of the equation of state—by Monte Carlo methods, for example—and test the results experimentally in conjunction with implosion codes incorporating radiation transport and so on.

My experience with such work in laser fusion research makes me think that this is possible, even though nonmilitary work on the subject is hampered by US government regulations forbidding certain lines of research. This makes it difficult to judge the accuracy of predictions of physical theory combined with well tested two- or three-dimensional codes. The dynamical stresses, accelerations, heat flows and so on produced by the electromechanical parts of nuclear devices are calculable, and they can be measured without detonating the bomb itself. Fearing nuclear proliferation, the US government will not release the theoretical work and the test data, and so the scientific community cannot judge these matters reliably.

On the other hand, if it is possible to

design weapons systems without bomb tests, it may be argued that the question of a test ban is irrelevant. If the military does not yet have scientists as good as the people at CERN, Fermilab or Stanford who design high-energy machines, or as good as the original team that predicted bomb performance with devastating accuracy, then better people could be hired.

It seems much more important to persuade the scientific community both that better bombs are not needed and that the effort is a waste of good people. We can, I think, face with equanimity the degradation of the existing stockpile; at least some of the miserable things won't go off. I, for one, am not in the least confident that our chance of survival is significantly increased thereby.

If the Russians find that their stockpile has not degraded, would they not conclude that ours hasn't either? But even if it has, this is still no reason for the deterrent to fail. Surely the balance of terror does not depend on whether a few devices fail to explode. Many of us now believe that the exciting story of the evolution of life on this planet will come to an end if the arms race continues much longer. It is galling to think that the vested interests of 10 000 individuals are responsible for it.

HANS MOTZ
9/83 *Oxford, England*

Defining determinism

In his article "How random is a coin toss?" (April, page 40), Joseph Ford makes a series of statements on randomness, determinism, continuum and infinity that are quite surprising to me. First of all, he seems to adhere to a definition of determinism that is at variance with the simple classical "nothing happens without cause." On the other hand, this traditional type of determinism has never been in contradiction to our daily experience of how "minute causes may cause big effects," as far as I see. Ford's argument cannot convince me that recent developments in nonlinear dynamics could have any impact on this familiar picture of classical macroscopic determinism.

Secondly, I do not believe that either classical physics or quantum mechanics have ever been based on the assumption of "infinite computational and observational precision." To the contrary, most of us physicists learned very early in life that no parameter can be measured with absolute precision on a continuous scale. I don't think we need arithmetic complexity theory to get that. Certainly, no physicist has ever made use of an "incalculable irrational number having positive Kolmogorov complexity," and I do hope that no one ever lost his time on calculating anything, either $\sqrt{2}$ or π or e to a precision of more than a hundred decimals.

So, Ford may readily do away with his undefinable numbers. I would be very surprised if that had any effect on any one of the theories by which we try to describe the basic features of what we experience as physical reality.

E. BIEDERMANN
7/83 *Boeblinger, West Germany*

In response to Keyworth

It was distressing to read the letters (September, page 11) responding to George Keyworth's thoughtful and civilized Guest Comment. In May, Keyworth was "pained" to call the APS Council's resolution on nuclear war "arrogant." In September, his pain is perhaps relieved as he sees how far his critics excel him at *ad hominem* invective. One correspondent calls Keyworth "inexperienced and shortsighted" and speaks of his "emotionalism." A second correspondent (who probably has not bothered to read a single scientific paper by Keyworth) says that he holds his position "not as the result of any distinctive contributions to science but as a reward for his dedicated service to the Los Alamos nuclear-weapons factory." This correspondent seems to feel that "first-rate scientists" (possibly including himself; Fellows of APS, perhaps?) should be above "insulting and threatening" criticism by second-raters such as the President's science adviser—criticism which he describes as "intimidation" and "repression." A third correspondent calls Keyworth "a company man" and "politically motivated," and a fourth elegantly dismisses his ideas as "crap."

These letters read like a burlesque of an unusually entertaining faculty meeting. Unfortunately, they are meant in earnest. Anyone who reads them will see how easily we in APS lose our heads and abandon our commitment as scientists to civility, reason and truth, once we enter the political arena. We, and the Council, must balance the good we may do for the external world against the harm we surely do to our science, our Society and ourselves.

JAMES E. FELTEN
University of Maryland
9/83 *College Park, Maryland*

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I applaud APS President Robert Marshak's firm support for the APS Council resolution on nuclear war (May, page 9). If Presidential Science Advisor George Keyworth is shocked to see APS stepping into the political arena (May, page 8), then he may have lost sight of the reality that nuclear war is more than just a game in global politics.

The days are gone when physicists need only concern themselves with physics, politicians with politics and clergymen with religious matters. We embrace the separation of state and religion only to the extent that one does not dictate to the other. All disciplines of human endeavor have always been and are ever more intimately intertwined with one another. The issuing of considered statements on nuclear war, whether they are from a body of physicists or Catholic bishops, ought to be taken seriously and viewed by politicians as contributions from responsible citizens of the planet Earth rather than as intrusions into the political process.

If we recognize the interdependence of all nations, all elements of societies, and all walks of life, and if we learn to use dialog rather than rhetoric as the principal form of communication, then we may graduate from this precarious Age of Information to the Age of Enlightenment!

BILLY W. LOO
University of California
7/83 *Berkeley, California*

Error in history

C. P. Snow's *The Physicists* was reviewed in PHYSICS TODAY (September 1982, page 75). I would disagree with Joan Bromberg's conclusion, that despite its errors in fact, it "is an excellent book to hand, with suitable cautions, to undergraduate students in physics and history." In my opinion, the book is just too unreliable to be used. Trivial mistakes—James Franck becomes two people, James and Josef—should have been corrected before publication. It is a disservice to Snow that they were not. But other errors make one think that perhaps Snow didn't really know how physics is done, or worse, preferred a good story to the truth. For instance, Snow describes Fermi's early prediction of transuranic elements, and says (page 91), "It was a pity, people thought later, that Rutherford, who had died shortly before, wasn't on the scene. It was just the sort of problem Rutherford would have seen straight through." This is fantasy—Rutherford died in 1937, more than three years after Fermi's results, and no one "saw through" the problem. Painstaking persistence solved it. Snow then tells a muddled story of the fission discovery, and decides that physicist Lise Meitner was Otto Hahn's "collaborator" who had "complete trust in her old chief." In fact, the two