Science and Politics in Early Nuclear Test Ban Negotiations

In a technical conference related to nuclear test ban negotiations in the late 1950s, Soviet and US scientists disagreed along national lines about the capabilities of scientific instruments, the validity of theories and the handling and interpretation of data.

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Scientists are no strangers to politics. Newspapers report daily about scientific experts participating in controversies such as low-level radiation, DNA testing, cloning or global warming. We find scientists testifying in congressional hearings, courtrooms and various committees on issues requiring both technical expertise and political judgment. This inseparability of science and politics is particularly visible in highly contested areas such as national security and nuclear arms control.[1]

In the late 1950s, when arms control negotiations between the USSR and the US revolved around monitoring a nuclear test ban, both sides sent scientific experts to discuss the technical details of the detection of nuclear weapons tests. In 1959, these technical meetings culminated in a controversy among scientific experts from both countries about the seismic detection and identification of underground nuclear explosions. The experts debated the validity of scientific data, facts, theories and instruments. They disagreed on a variety of technical issues and interpreted data along national lines. While the US scientists consistently emphasized the limitations of a monitoring system, the Soviet scientists used the same data to defend their government's claim that underground nuclear explosions could be adequately monitored. Uncertainties in scientific knowledge allowed for these different interpretations, and the debate among the scientists reflected the disagreement among the diplomats. The USSR and the US did not agree in 1959 about methods to detect small underground nuclear explosions and therefore settled for the Limited Test Ban Treaty of 1963, which excluded underground tests from consideration for the time being.

Scientists in arms control negotiations

How did scientists get involved in nuclear arms control negotiations? Beginning in 1946, the US consistently argued that arms control agreements without adequate control mechanisms were worthless. Physicists such as J. Robert Oppenheimer, Ernest O. Lawrence and Arthur H. Compton helped to work out the details of a future control system. However, negotiations with the USSR in the period from 1946 to 1957 did not lead to an arms control agreement. The US argued that a working control mechanism had to precede any political agreement, while the USSR insisted on "banning the bomb" first. To get out of this impasse, both sides searched for small, manageable first steps toward arms control.

During disarmament talks in London in 1957, both the US and Soviet diplomats suggested that a ban on nuclear testing could become such a first step. Again, US negotiators stressed that any agreement had to be accompanied by an adequate control system to prevent secret weapons testing and development. The details of such a system, they suggested, should be worked out by scientists from East and West in technical talks. Soviet Premier Nikita Khrushchev rejected such talks as deliberate attempts to delay the cessation of nuclear tests and argued that, in any case, technical experts could not help to solve political problems. In contrast, President Eisenhower insisted that no political talks would be held until scientists agreed on a monitoring system. "Studies of this kind," Eisenhower stressed in a 28 April 1958 letter to the Soviet premier, "are the necessary preliminaries to putting political decisions actually into effect. . . . In other words, with the practicalities already worked out, the political agreement could begin to operate very shortly after it was signed and ratified." A month later, Khrushchev accepted Eisenhower's proposal for technical talks.[2]

At that point, deep divisions in the US government about the technical possibility and political desirability of a nuclear test ban treaty became visible. While Eisenhower and his secretary of state, John Foster Dulles, tended to support a test ban, the Department of Defense and the Atomic Energy Commission opposed steps in that direction. A similar polarization was noticeable in the physics community: A politically influential minority around Edward Teller and Ernest Lawrence strongly favored a continuation of nuclear testing and opposed the pro–test ban recommendations of the President's Science Advisory Committee, which included physicists such as Hans Bethe. The selection of US scientists for the technical talks reflected the political divisions in the White House and among physicists. The US delegation included a test ban opponent (Ernest Lawrence), a moderate pro-

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FIGURE 1. CONFERENCE OF EXPERTS, Geneva, 1958. The Western delegation is on the right side of the table, the Eastern delegation on the left and United Nations representatives at the head of the table. (Courtesy of the Division of Rare and Manuscript Collections, Cornell University Library.)

ponent (Robert Bacher) and a "neutral" chairman (James Fisk).

Dulles expressed the hope that scientific and political issues could be separated in the upcoming technical talks. Asked in a press conference on 10 June 1958 whether the US scientists would receive any political guidance, he said that the experts were given "complete authority to work on this matter [of how to detect nuclear explosions] as a purely scientific technical matter, to use their best judgment and report to us accordingly. I do not anticipate that there will be any need for political guidance."

In July and August of 1958, experts from the USSR, Czechoslovakia, Poland and Romania met in Geneva with their colleagues from the US, the UK, France and Canada. (See figure 1.) In 30 sessions, the experts discussed techniques of recording acoustic, hydroacoustic and seismic waves and of monitoring radioactive debris and radio signals, to detect nuclear explosions in the atmosphere, the oceans, on the ground and underground. In addition, they gave some thought to the detection of explosions in space. Based on these considerations, the experts discussed the requirements for a monitoring system. The detection of underground nuclear explosions turned out to be the most difficult task, and the experts agreed that the associated difficulties determined the number of control posts of the monitoring system and its detection threshold. To make matters more complicated, the experts had to extrapolate from seismic measurements of only a single underground explosion, the small yield (1.7 kiloton) US test known as Rainier of 19 September 1957. (See figure 2.) Although the details were often contested, the Geneva experts nevertheless agreed "that it is technically feasible to establish, with the capabilities and limitations indicated [in the report], a workable and effective control system to detect violations of an agreement on the worldwide suspension of nuclear weapons tests." With this document in hand, the diplomatic negotiations for a test ban began in Geneva on 31 October 1958.[3]

Arms control supporters hailed the agreement as a good start for the diplomatic negotiations. Eugene Rabinowitch, editor of the *Bulletin of the Atomic Scientists*, expressed the hope "that once an international problem has been formulated in scientifically significant terms, scientists from all countries, despite their different political or ideological backgrounds, will be able to find a common language and arrive at an agreed solution."[4] This hope was short-lived. US underground tests conducted after the Geneva conference provided new seismic data. Those data and their interpretation became the focal point of political and technical debates between the USSR and the US, culminating in the meetings of Technical Working Group 2 in November and December 1959.

Technical Working Group 2

In October 1958, the United States completed Operation Hardtack II, a comprehensive series of 37 weapons and safety tests, which included seven underground explosions. Five of the underground tests produced significant seismic waves, which made it possible to test the Geneva recommendations for the detection of underground explosions.

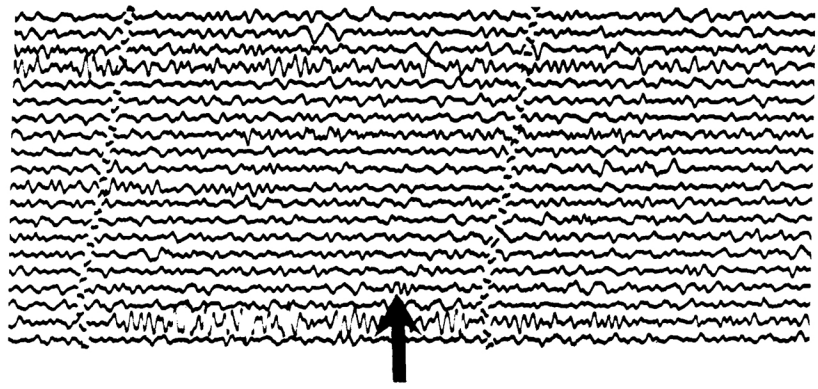

FIGURE 2. SEISMOGRAM showing the underground nuclear test named Rainier, which occurred on 19 September 1957, as recorded at Fairbanks, Alaska. The arrow points to the explosion signal, which is difficult to distinguish from the background of seismic noise. (Courtesy of Carl Romney.)

In early January 1959, US negotiators introduced the preliminary seismic analysis to the diplomatic conference in Geneva. According to US seismologists, the new data showed that it was considerably more difficult to detect and identify small underground nuclear explosions than previously thought by the Geneva experts. Soviet diplomats refused for some months to take the new data into account, but, in November 1959, they finally gave in to diplomatic pressure and sent their seismic experts for another round of technical talks with their US and British colleagues.

Both the Soviet and US delegations included scientists who had participated in the 1958 Geneva Conference of Experts, among them the chair of the Soviet delegation, geophysicist Yevgeny Fedorov, and his US counterpart, physicist James Fisk. Besides Fisk, the most active members of the US delegation included Department of Defense seismologist Carl Romney, academic seismologist Frank Press and physicists Wolfgang Panofsky, Harold Brown and Hans Bethe. The small British delegation, headed by physicist William Penney, played only a marginal role. In the Soviet delegation, Fedorov was supported most actively by seismologists Yurii Riznichenko and Ivan Pasechnik and theoretician Vladimir Keilis-Borok.[5]

The technical meetings began with an extended debate about the working group's objectives. Which issues should be discussed by the assembled experts, and which ones should be left to politicians and diplomats? Both the US and Soviet scientists present had received broad terms of reference from their respective governments. In the US, a group of top-level policymakers, including the secretary of state and delegation chairman Fisk, had developed such a political framework only days before the technical conference began. The US terms of reference instructed the scientists, first, to update the estimates of detection capabilities in light of the new data; second, to discuss possible improvements of a monitoring system; third, to find criteria for distinguishing between earthquakes and underground explosions; and finally, to agree on criteria for on-site inspections.

The Soviet agenda excluded renegotiation of detection capabilities. Fedorov's main objective was to facilitate a political agreement for a nuclear test ban. Consequently, he defended the conclusions reached earlier by the Geneva Conference of Experts, which had provided a technical foundation for such an agreement. To save this foundation, he treated the Geneva report as a legal and binding document, and reacted impatiently when US scientists suggested that the new seismic data required a reevaluation of the 1958 agreement. In Fedorov's view, the new data and their US interpretation were merely "views" and not scientific facts such as the conclusions of the Geneva experts. Throughout the conference, Fedorov and his experts not only challenged the US experts' evaluation of the new data, but also their instruments and theories, and the use of certain fundamental statistical and seismological concepts. Soviet scientists consistently presented an optimistic view of detection capabilities, while their US counterparts emphasized the detection-related difficulties that would beset a monitoring system.[6]

Instruments

A major debate developed at the 1959 talks around the capabilities of seismic instruments. In their new measurements, the US scientists had used standard short-period seismographs of the "Benioff" type, named after Caltech seismologist and instrument specialist Hugo Benioff. (See figure 3.) Challenging the performance of the Benioff offered the Soviets an opportunity to attack the validity of the new seismic data. Although the US seismologists regarded the Benioff as the best available instrument for the detection of short-period seismic waves, the Soviets maintained that in 1958 both delegations had agreed on a somewhat different and ultimately superior device. Furthermore, the Soviet instrumentation expert Pasechnik tried to prove that Benioff's instrument considerably distorted seismic signals. Fisk rejected Pasechnik's attack as unfounded and emphasized that no existing instrument, not even any Soviet seismograph, fulfilled the 1958 Geneva specifications. In any case, Brown argued, the performance of the Soviet standard short-period instrument known as the SVKM could not compete with that of the Benioff. Attempts by US scientists to convince their Soviet colleagues that the Benioff, in fact, fulfilled the specifications for seismic instruments agreed upon in 1958 drew only sarcastic comments from Fedorov: "You know the Bible was subject to many interpretations and exegeses, but I think the report jointly put together by us would not allow of a variety of interpretations. With regard to the seismic apparatus section, we think the section was clearly and succinctly worded."[7]

Since the Benioff did not have exactly the same characteristics as the short-period instrument recommended by the Geneva Conference of Experts, the Soviets felt justified to reject the new data as essentially irrelevant for the detection problem. US chairman Fisk dismissed the Soviet position as "debating points" and "quibbles" and insisted on scientific, not legalistic, arguments. On the other side, Fedorov expressed amazement "to see that you, scientists, with all these facts, still state: 'With our marvellous [*sic*] Benioff we obtained all that we could have obtained, all that the control system could have obtained with the instruments that we suggested for it.' We cannot accept such a statement."[8]

Throughout the conference, both sides came back repeatedly to this issue, offering new evidence for the superior or inferior performance of the Benioff. As in essentially all unresolved controversies during the conference, the Soviets and Americans found themselves on opposite sides of the fence. The dividing line was clearly

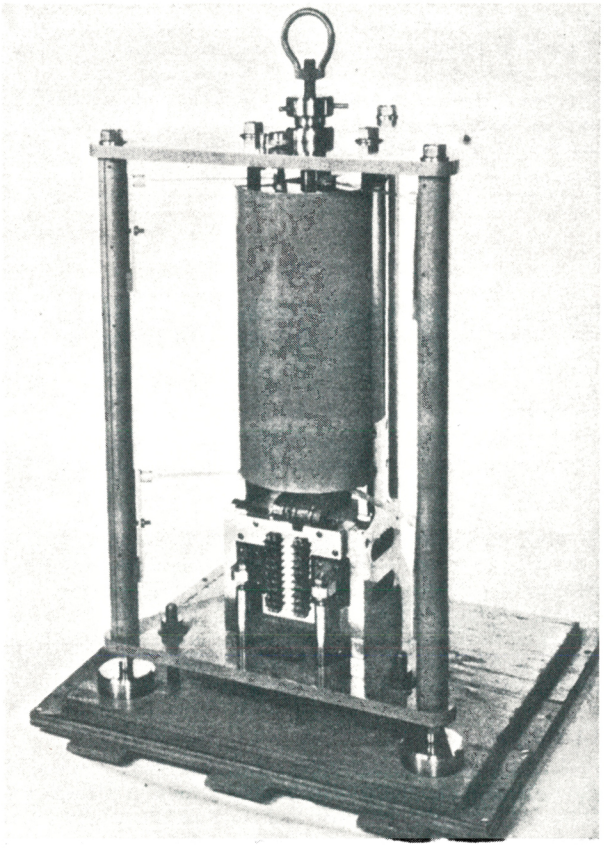

FIGURE 3. BENIOFF SEISMOGRAPH. Soviet and US scientists debated the capabilities of this instrument, which is shown here without its outer covers. (From H. Benioff, in *Advances in Geophysics*, vol. 2, H. E. Landsberg, ed., Academic Press, New York, 1955, p. 233.)

drawn: no Soviet scientist would argue for the Benioff, no American against it. The scientists did not agree if and to what extent other seismographs, existing or not, could have provided better data. The interpretation of what a certain instrument could and could not measure differed along national lines.

Decoupling theory

In 1958, at Teller's suggestion, scientists at the RAND Corp had developed a new theory about how underground explosions could be concealed. The theory suggested that the amplitude of seismic signals from an underground nuclear explosion could be reduced by as much as a factor of 300, if the nuclear device were exploded in a large underground cavity. The implications for a test ban treaty were obvious: An explosion 20 times the yield of the Hiroshima bomb (15 kt) would not be detected by the monitoring system. Both the US and Soviet governments realized that if the theory turned out to be valid and if such large underground cavities could be constructed secretly, it would strike a serious blow to the test ban negotiations. Highly technical arguments for the validity of the theory were presented at the 1959 talks by two American physicists—RAND physicist Albert Latter and Bethe. Latter, one of the theory's principal authors and a declared opponent of a test ban agreement, and Bethe, an outspoken supporter of arms control measures, both argued for the validity of the theory.[9]

For the Soviets, the decoupling theory presented a major obstacle to a test ban agreement. Consequently, Fedorov at first refused to discuss the theory. When the American scientists insisted on a scientific exchange, the Soviet experts attacked the theory's underlying complex hydrodynamical arguments. Seismologist Konstantin Goubkin concluded that such a big cavity had essentially no effect on the seismic signal, and Mikhail Sadovsky suggested that a large cavity might even produce an increase in the signal's amplitude. The American theory of a 300-fold signal decrease stood against the Soviet suggestion of a signal increase. Again, the disputes ended without agreement and in personal confrontation. Shooting against Latter, Fedorov pointed out that the RAND physicist had even published his anti–test ban views in a book coauthored with Teller. "Why has he [Latter] come here?" Fedorov challenged Fisk. "To help us finish with the testing of weapons or to prevent that?"[10] Again left without an agreement, the Soviets rejected the decoupling theory as speculation and an act of faith.

Political equations

A central disagreement at the 1959 talks developed around the question of how to handle the data from underground explosions. Were all data points equally reliable for a determination of the explosion's seismic magnitude, or should one assign statistical weights for the calculation of the mean? Or should one even exclude certain measurements that seemed to deviate substantially from a mean value? Riznichenko, for example, pointed out that records from two seismic stations, Woody and Barrett in southern California, gave magnitude readings far smaller than other stations did for the same explosion. (See figure 4.) He argued that data points from those two stations should be excluded because they apparently led to a systematic underestimation of magnitude values. In addition, he dismissed all data from stations that recorded the explosion at distances between 1100 and 2500 kilometers, claiming that magnitude determinations at stations in that so-called second, or shadow, zone were unreliable. He declared that "before using the points in the second zone in calculating [magnitude values] for explosions, one should make the relevant analysis and exclude the systematic part of the divergencies of the points in this zone. . . . Until this analysis is made all one can do is simply to refrain from using the points in the second zone, which is what I had to do."[11]

The decision of what constituted a reliable data point shaped also the best fit for the data. (See figure 5.) Romney suggested that the magnitude data revealed a simple relation between explosion yield Y (in kilotons) and seismic magnitude M—namely, $M = 3.65 + \log Y$. The Soviets excluded data for the smaller explosions and recalculated some magnitude values, and argued that the function $M = 4.2 + 2/3 \log Y$ fitted the data much better. The seemingly minute differences between the two functions had major political ramifications. The American fit suggested that, for example, a 1 kt underground explosion would produce a seismic magnitude of 3.65, about half a magnitude less than in the Soviet interpretation. This difference, which seems hardly relevant to a nonseismologist, translated into very different problems for the monitoring system. The 1958 Geneva Conference of Experts had estimated that the annual number of earthquakes worldwide with a magnitude larger than that of a 1 kt explosion was 10 000. Based on the new seismic data, the American seismologists suggested instead a number of 25 000, while the Soviets pushed the number down, at first to 7400, and in their final report to 3000. To detect

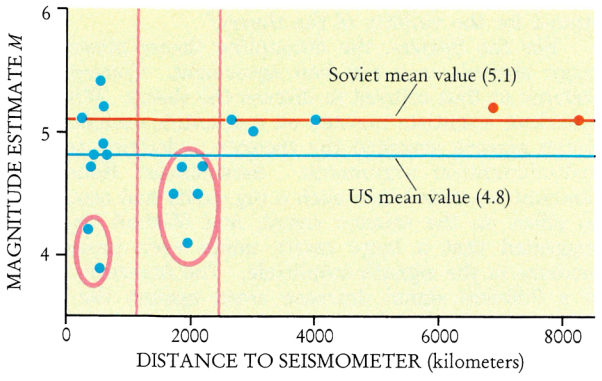

FIGURE 4. MAGNITUDE DATA from seismological monitoring of the 22 kt Blanca nuclear test of 30 October 1958. Blue points represent US data; red points, Soviet data. Soviet scientists argued for the exclusion of the encircled data points on seismological and statistical grounds. US scientists rejected this procedure. Thus the two sides calculated different mean values for the seismic magnitude of the Blanca explosion.

and identify a small nuclear explosion of 1 kt against a background of similar signals from 25 000 earthquakes was of course a much more difficult task than for 3000. In short, the American interpretation of the data translated into substantially more problems for a future monitoring system, while the Soviet seismologists interpreted the data in a way that seemed to make the monitoring task even easier than had been expected by the 1958 Geneva conference.[12]

Riznichenko argued that "any real specialist, especially any experimenter, must know that, in order to get a refined and accurate result, you must pick out only reliable data and try to disclose all the systematic trends that may be apparent in the data and not just brush them aside. . . ." But how does one define "reliable data" and "systematic trends"? The American scientists replied to Riznichenko's challenge with a two-pronged counterattack: Romney rejected his seismological arguments, and mathematician John Tukey questioned his statistical analysis. Romney insisted that the exclusion of data points from the shadow zone was not justified seismologically. He emphasized that American seismologists did not use "fallacious statistical tricks or specious seismological arguments applied to reject valid data which does not agree with other ideas we might have. All of the data is used, and used properly." Attacking Riznichenko's statistical procedures, Tukey accused the Soviet scientist of distorting the numbers and concluded that a statistician who would follow the Soviet approach "would be looked at in a very peculiar way by almost any other statistician in the profession."[13]

During the whole conference, the US scientists defended their new interpretation, which challenged the 1958 conclusions, while Soviet scientists interpreted the same set of data to defend the validity of the 1958 report.

Magnitude determination

Differences in determining the seismic magnitude of a nuclear explosion led to an aggravation of the dispute. In their often-heated exchanges, both sides quoted the leading authorities in magnitude determinations, Caltech seismologists Charles Richter and Beno Gutenberg, to support their interpretations. The concept of an earthquake magnitude, as originally defined by Richter, applied only to local earthquakes. An extension to distant earthquakes and explosions was not unambiguous and led to three different earthquake magnitude scales, which did not exactly coincide. Even worse, the numerical relation between the scales was not well defined. This uncertainty again allowed for different interpretations of the magnitude data.

Riznichenko argued not only that the Americans had used the wrong magnitude scale, he also implied that they had done so deliberately to get the results they wanted. "Not trying to be serious now," Riznichenko began, "and not speaking about anyone in particular, an unobjective seismologist who was trying to get the lowest possible readings of magnitude and the smallest quantitative values for magnitude would probably prefer to use the [local magnitude scale] and not the [unified magnitude scale]." For Romney, Riznichenko's presentation was "an incredibly complicated distortion of the magnitude scales," and Romney added that he was "happy that the inventors of the scale were not forced to sit through it as we were." Neither the official nor the private meetings resolved the dispute, leaving an open controversy about magnitudes, which in Fedorov's view was "boiling down to horse-trading." In the face of all this uncertainty, Fedorov maintained, there was no need to rewrite the 1958 agreement.[14]

Epilogue

Both delegations at the 1959 talks repeatedly expressed the hope that an objective scientific discussion of the same data, instruments, methods and theories must lead to an agreement about the scientific facts. But the facts of one group were dismissed by the other as mere views, opinions and positions. Charges of biased calculations, questionable assumptions and "making the data fit" were traded back and forth. With each side accusing the other of ulterior political motives, the conference ended in dispute. The unresolved problems were handed back to the diplomats.

For political commentator Robert Gilpin, writing in 1962, the culprit for the failure of Technical Working Group 2 was obvious. The Soviets, he wrote, had "sought to discredit the new American data mainly through specious scientific arguments; when this technique proved impossible and embarrassing to them they shifted to the tactic of employing contrived technicalities." On the other side, the Soviets blamed the American scientists for "tendentious use of one-sidedly developed material for the purpose of undermining confidence in the control system. . . ."[15]

How much political guidance did the scientists receive from their governments? Were they directed to uphold a specific political position and to "massage" the data and facts until they fit their government's political objectives? The verbatim records of the meetings show that Fedorov and his colleagues tried to save the conclusions of the 1958 Geneva Conference of Experts under all circumstances. They used the uncertainty in the scientific data, theories and concepts to support the official Soviet view, which aimed at an immediate agreement on a comprehensive nuclear test ban. However, the official records show no evidence that the Soviet scientists did not genuinely believe in the validity, or at least the scientific justifiability, of their arguments. On the other side of the table, the US scientists were guided by the broad terms of reference developed by top-level US policymakers. But those terms of reference did not include any instructions to emphasize technical disagreement with the Soviet sci-

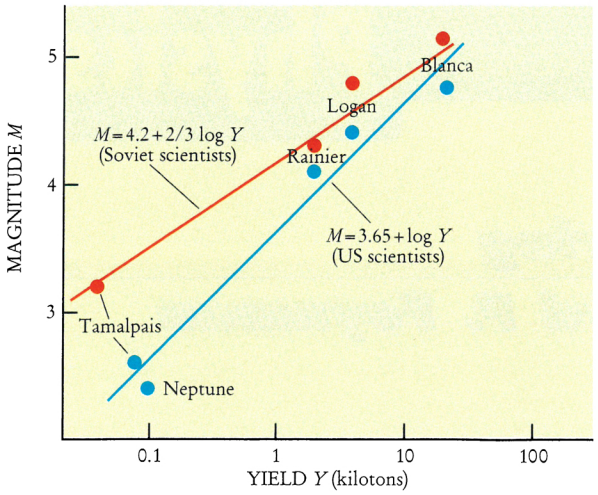

FIGURE 5. DIFFERING INTERPRETATIONS of the magnitude values of five US underground explosions. Blue points represent US calculations; red points, Soviet calculations. The two resulting fits for the relationship between magnitude and yield had considerably different consequences for the monitoring of underground nuclear weapons tests.

entists. Fisk and his colleagues were genuinely concerned about the limited seismic detection capabilities in light of the new data. Mistrusting the Soviets, they wanted a system that would work "adequately." Despite a considerable range of options among the US experts on what constituted an "adequate" control system, they agreed that the new data were relevant to the detection problem and that the optimistic interpretation by the Soviet scientists was not warranted.

The US and Soviet experts did not find a common language to solve the problems of the diplomats, because their own language of seismology, physics and statistics allowed different readings with significantly different consequences for international relations. The two delegations disagreed about the capabilities of instruments, the value of theories, how to handle raw data and how to define a widely used scientific concept such as seismic magnitude. In the end the position taken by an individual scientist correlated fully with his nationality, despite major differences of views on test ban issues among the US experts, and very likely also among their Soviet colleagues. The controversy ended without an agreement.

After years of fruitless diplomatic negotiations, the 1962 Cuban missile crisis moved both sides to make a concentrated effort toward achieving nuclear arms control. In 1963 the nuclear powers agreed on a Limited Test Ban Treaty, which prohibited nuclear tests in the atmosphere, outer space and underwater. However, testing continued underground, only marginally restricted by the 1974 Threshold Test Ban Treaty, which limited underground tests to a maximum yield of 150 kt. The controversy about seismic capabilities shaped nuclear arms control negotiations from the late 1950s until 1997, when an agreement on a comprehensive test ban treaty was reached. And the controversy shaped seismology: In the early 1960s the Department of Defense increased government funding for R&D in seismology by more than a factor of 30, transforming the small academic discipline into a major academic-industrial-military endeavor.[16]

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