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Note

THE ROLE OF NUCLEAR EXPLOSIONS IN STUDIES OF IMPACT CRATERS

Dr. Bevan M. French's excellent personalized review article, The importance of being cratered: The new role of meteorite impact as a normal geologic process, in the February 2004 issue of *Meteoritics & Planetary Science* builds a strong case for impact as one of the primary processes affecting planets. His approach to this conclusion is taken from a historical perspective. He cites the year 1963 as a pivotal time, in which impact events came to be recognized as a common terrestrial occurrence. This foretold the eventual certification of impacts as a dominant means by which Earth-like planets and icy satellites around the giant planets have grown and modified their surfaces. However, probably because I do not recall that he attended, Dr. French left unmentioned a professional gathering the next year (May 16–19, 1964), in which key strides were made in proving the reality of impacts.

This was the Conference on Geological Problems in Lunar Research, organized by Dr. Jack Green (later, at Long Beach State University) and sponsored by the New York Academy of Sciences, in which 58 papers covering both volcanic and impact cratering processes were given (and documented in volume 123, article 2, 1965 of the *Annals of the Academy*). Attendees were almost a "who's who" in the burgeoning field of cratering, guaranteeing a lively intercourse centered around the still (at that time) vigorous debate on the origin of the abundant craters spread over the Moon. Of the diverse treatments of cratering during the Conference, two can be cited as decisive because their linkage demonstrated that impacts have struck Earth many times in the past and, by inference, were probably the best explanation of most circular features on the Moon. One was given by Mr. Michael Dence of Canada's Dominion Observatory summarizing his studies of Canadian craters, and the second by myself, at the time working in the Plowshare program ("peaceful uses of nuclear explosions for such engineering projects as canal digging, oil and gas recovery, large-scale mining") being conducted at the Lawrence Radiation Laboratory in Livermore (later renamed the Lawrence Livermore Laboratory or LLL).

To set the stage for the relevance of this pair of papers, some awareness of my background is needed. In his February 2004 paper, Dr. French has ably described what the "impacters" were basing their observations on and conclusions about the reality of the process. One of the central efforts was the program at the Dominion Observatory organized by C. S. Beals and carried out from a field and petrographic perspective by Michael Dence. When I arrived for the Conference, I was not cognizant of what that group had been learning.

In 1959, a year after receiving my Ph.D. from MIT, I joined LLL, initially as the first geologist hired to support the Nuclear Weapons Testing Program because field detonations of nuclear explosive devices had gone underground the year before. I had no prior experience in anything related to this, having confined my studies to classical geochemistry, sedimentology, and structural geology. In the presence of physicists and engineers, I learned fast about the phenomenology of nuclear explosions. My job largely consisted of going into the ground zero section of an underground explosion, mapping it, and helping to define the parameters, recorded in the rocks, needed by the physicists in calculating the behavior of the nuclear device (mainly its yield in kilotons). But, the physical effects of the explosion on the rock or alluvium surrounding the device were of no particular interest to my LLL colleagues (most were focused on applications rather than research).

That changed in the spring of 1960. A brilliant physicist, Dr. John Nucholls (who later became Director of LLL), had calculated (using a computer-based model) the mode of fracturing around a contained chemical explosion in the Winnfield (Louisiana) salt dome, part of the Cowboy program run jointly by DOD and the AEC to determine the degree to which explosions could be "muffled" (by being conducted in large mined-out spheres) to avoid detection during the Cold War. Dr. Nucholls (with insight I found rare in LLL physicists) wondered if the salt actually fractured as he predicted. I was assigned the task of mapping the actual fractures (which stood out as black filling by an injected carbon residue against the white salt) as I guided local miners who tunneled back into the bulged cavity around the detonated 1000 lbs of pelletol explosive. When I entered that cavity and collected salt from the edge, I noticed that the salt crystals had been deformed (I believe by plastic flow) into elongate shapes by the shock waves, and individual crystals showed sets of criss-crossing fracture or glide planes that fostered extension. At that moment, in just minutes, my career changed dramatically. I decided that someone should start examining, in detail, the rocks involved in chemical and nuclear explosions. I appointed myself and "bootlegged" these studies. As far as I know, I was the only person in the western world (I don't know about the Soviets) who actually studied nuclear explosion rocks in the early 1960s.

The first step, in mid-1961, was to be assigned once again to supervise the mining out of a contained chemical explosion, this time in ashfall tuff at the Nevada Test Site (NTS). Fractures in the surrounding medium proved to be rare, as the tuff responded by being compressed into a compact material. As part of this study, I also investigated the glass types found above the base of the spherical cavity formed at the site of the Rainier event (the first underground nuclear explosion in history) conducted in similar tuff. Reddish to yellowish tuff beds near the device were melted into a liquid that fell rapidly into the cavity base before the roof literally fell in, causing tuff blocks to be incorporated in the melt lens. That melt cooled quickly into obsidian-like dense black glass. The blocks partially to completely vesiculated into white pumice-like glass. A third type of glass, pinkish in color and containing metallic copper (from device wires), was found as droplets and coatings in fractures into which vaporized (this is a supposition) tuff was inserted out to 30 m or more beyond ground zero. This last glass was totally devoid of water (to the limits of analytical detection), and the obsidian and pumice types were very low in water content. The original tuff contained from 14 to 17% by weight of H_2O. This evidence suggests that the explosion was a dehydrating process that drove off water to locations beyond the near environs of the compressed tuff. I have since speculated that this complete removal of water from "wet rock" during a big explosion (which, in its earlier phases, at least resembles the mechanics of impact cratering) by shock and/or superheating was relevant to the argument that tektites (water-free) could develop from water-bearing terrestrial rocks subjected to impacts.

My best opportunities for examining shocked rocks came in the first half of 1962. In February, a 5 kiloton nuclear device was exploded in granite at NTS, the experiment given the code name "Hardhat." It was fully instrumented so that the shock pressure gradient from ground zero (where pressures were in excess of a megabar) outward was fairly precisely known. Afterward, the surrounding rock was drill cored, and samples from all distances into ground zero were recovered. I had thin sections made of this rock core at intervals from 20 cm to a meter, extending out to 30 m from the transient cavity wall (like Rainier, the roof fell in). When I examined these sections under a petrographic microscope, amazing things appeared—almost none being described in the literature on rock deformation (I was then totally unaware of the very few papers describing rocks from impact structures but knew of Eugene Shoemaker's studies of craters at NTS and Meteor Crater in Arizona and had heard something about coesite from Ed Chao's work). Among the peculiarities I observed were the increase in microfracturing as ground zero is approached, kinking of biotite, and conversion of the feldspars into an isotropic state (later I learned this was being called "thetomorphs" or "diaplectic glass"). But, most unusual to me were the multiple sets of thin, planar "fractures" in both quartz and feldspar. (I was not sure that this was a proper identification and speculated that these might be slip planes.) We now know that they are disordered silicate structures. Only later did I discover that these would soon be named "planar deformation features" or PDFs. Still, having taken a course from H. Fairbairn at MIT on petrofabrics, I had the foresight to measure the PDF orientations by U-stage and found that the majority followed what is known as the omega and pi directions relative to the c (optic) axis.

In March of 1962, a small nuclear explosion, "Danny Boy," was conducted in basalt to assess its cratering efficiency (a prelude to a Plowshare plan to dig a second Panama canal). For the first time, workers analyzing the postshot core for signs of where the true crater wall was relative to ground zero (critical to analyzing device performance) failed to find evidence of the boundary. I had about 60 thin sections made from the core and found evidence of the crater wall by an abrupt increase in microfracturing, then almost none (as the drilling entered the zone of fallback rock that fell against the wall). This cost $600 (for the thin sections) compared with several hundred thousand dollars spent in the failed postshot diagnostics program. Sadly, few of my LLL colleagues seemed to appreciate this approach (studying the rock itself as a recorder of shock wave effects).

A super event, named "Sedan," occurred in July of 1962. This was the detonation of a 100 kiloton device buried at a shallow depth in Yucca Flats alluvium at NTS. This produced a crater that was 400 m wide and 110 m deep. I watched this event from a distance of just 8 km, sitting on the side of a welded tuff cliff (that swayed noticeably from the traveling shock waves) atop Rainier Mesa. Within weeks, I was permitted to enter the ejecta blanket around Sedan and collect almost 100 samples consisting of variably shocked quartzite, limestone, granite, welded tuffs, carried by streams into the Yucca Flats basin, and the alluvium itself. Relying on thin sections, I concentrated on shock effects in the quartzites and was able to arrange the samples (which had been located at different distances from ground zero) into a suite that showed progressive shock damage. PDFs abounded in these rocks. Some quartzites were now completely glass (but with no internal flow, thus preserving their original shapes), and a few had actually melted. Granites converted entirely to glass, but with texture preserved, were also found. I concluded then that intense shock pressures could produce wondrous features in a wide variety of rocks, which I needed to report within the literature. (Note: I have donated all these thin sections and those from other explosion and impact rocks to the Mineral Sciences Department at the Smithsonian Institution in Washington, D.C. to preserve them for others to study.)

Before the New York Academy of Sciences Conference, I also began to conduct controlled experiments, first by firing a flat-nosed projectile from a 16-in battleship cannon, set up at LLL's Site 300, at enclosed targets of rock. This just fractured the rock. Then, I fortuitously invented a new way to shock rocks and minerals—the implosion tube, consisting of brass or steel cylinders (about 4 cm in diameter) hollowed in the center and filled with small rock or mineral cores (1 cm wide) or loose grains of individual mineral species. The tube was then placed along the central axis inside a larger aluminum tubular casing backfilled with a liquid explosive. Thus, the sample tube was subjected to an inward pressure. All the features I had observed in the nuclear explosion rocks were duplicated. Across a 5 mm gradient, variations in degree of shock metamorphism (a term I did not devise but came to adopt when I learned of the work of others that used that name) were equivalent to about 250 kilobars (calculations showed that the maximum pressures attained were on the order of four-tenths of a megabar). Among these features were maskelynite (in Danny Boy basalt), isotropic calcite, and shock-lithified quartz grains. This last feature duplicated a product I call "instant rock," which I had collected in the debris apron around some small chemical cratering explosions, in which loose Ottawa sand grains was used to backfill the emplacement holes but which were shock-lithified to a very cohesive "rock" lying about the ejecta.

I had not yet reported these observations as oral or written papers at the time of the NY Academy Conference. As I recall, I heard Michael Dence's paper on petrographic features in Brent, Clearwater Lakes, and other reputed Canadian impact craters before I gave my paper, A comparison of features characteristic of nuclear explosion craters and astroblemes. From the Dence paper, I realized that the features I'd observed in nuclear/chemical explosions were precisely those he described, except his rocks had experienced post-impact alterations. But, the most significant conclusion I reached (and stated in my presentation) was that these explosion craters, having been instrumented by pressure gauges, generated hundreds of kilobars of overpressure (shock wave-induced), and therefore, one could reasonably infer that the impact structures were formed by pressures of similar magnitudes. In other words, I offered the idea that a convincing proof of impact origin of certain craters or astroblemes was that their rocks contain evidence of modifications by very high pressure shock waves much like those produced in similar rocks by nuclear explosions. No known terrestrial tectonic or volcanic deformation process is known to produce such high pressures in a surface or near-surface setting. I think that this conclusion holds today despite claims by diehard volcanologists that astroblemes or fresh craters associated with shock features somehow have a volcanic origin (to my knowledge, no pressures as high as 100 kb have ever been measured, and most geoscientists cite pressure limits of 20–30 kb during explosive eruptions). The calculations on the mechanism of impact cratering show convincingly that a very high energy release causes high pressure shock waves that are distributed over a localized volume situated near the surface at the time of the causitive event. The only reasonable explanation for this is extraterrestrial: that kinetic energy comes from a high speed incoming meteoritic mass, which can reach huge numbers according to K.E. = MC^2.

From that day in New York forward, I entered a new phase in my career—advocate for extraterrestrial impact cratering and promoter of shock metamorphism as the decisive evidence that bespoke of a high pressure, rapid formative process of an explosive nature. I do not recall whether Bevan French or I or someone else coined the term "shock metamorphism." I did learn after the NY Conference of an abstract by D. L. McIntyre in 1962 at the AGU Western meeting that introduced "impact metamorphism" to describe planar features at the Clearwater Lakes dual impact site in Canada.

I met Bevan soon thereafter, probably during the August 1965 Lunar Geological Field Conference in Bend, Oregon (again, largely organized by Jack Green). He was in the first phases of learning about shock effects in crater rocks. Then, on a November bus trip across Missouri, as a field excursion to several cryptoexplosion structures (noted for their shatter cones) in that state, enroute to the 1965 Geological Society of America annual meeting in Kansas City, he and I dreamt up the idea for the Conference on Shock Metamorphism of Natural Materials, held in 1966 at the Goddard Space Flight Center. Later, we edited the Proceedings volume of the same name that, after publication in 1968, became almost the "bible" in the field of shock-induced cratering.

At the time, I was on the faculty of the University of Houston, having left LLL in September of 1964. But I made one last contribution to Plowshare after leaving. Plowshare, conjoined with Conoco Oil and El Paso Natural Gas, was to conduct a deep nuclear explosion near Farmington, New Mexico (the Gasbuggy event) to see if this improved recovery of natural gas from a sandstone. John Nucholls gave his usual review of how the explosion proceeds, using computer-derived illustrations organized into a two minute movie. Despite the Winnfield salt experience, he placed the fractures as equi-spaced "spokes" around the growing spherical cavity. The chief geologist of Conoco rose to his feet exclaiming, "that all looks very nice, but what is happening in the blank areas between the fractures!" No one had an answer, but someone remembered that I had the Mining Engineering Department at UC-Berkeley make detailed measurements of a variety of physical properties in Hardhat core. This was included in a final in-house report I wrote before leaving LLL. Some 200 people went into a holding mode as a DC-3 flew the 32 copies of this report from the Livermore Library to Las Vegas. Within four months, an ad appeared in *Geotimes* seeking three people capable of carrying on what I had done in my shocked rocks studies. Gasbuggy took place, but the gas yield was below expectations. I have been told that they found shock effects in core recovered in rock around ground zero. Unfortunately, Plowshare died as a multimillion dollar program a few years later, mainly a victim of the worldwide clamor to cease nuclear testing programs. The Soviets tried similar practical applications for a few years thereafter, but their efforts ended abruptly with the ratification of treaties to prohibit the legitimate testing of weapons.

In 1966, I spent the summer with Michael Dence at the Dominion Observatory where I studied samples and thin sections from the various Canadian craters. I had a NASA grant to specifically study the West Hawk Lake impact structure in western Ontario. Enroute, I stopped off at the University of Iowa and received samples of the Manson (Iowa) core from Richard Hoppin. While in Canada, I made thin sections from these and later reported that Manson was indeed an impact structure, as had been surmised by Hoppin and his colleagues. In 1968, I mentioned the gist of this work in a paper given at the Meteoritical Society (which I had joined in 1967) annual meeting in Vienna, Austria. Later, in 1992–1993, after I had retired, I did a complete study of the Manson core and reported the results in GSA Special Paper 302. I gave an extended summary of these later Manson results in a paper at the Meteoritical Society's meeting in Vail, Colorado in 1993.

In 1967, I took a leave of absence from the University of Houston to work at the Planetology Branch at Goddard. There, I made use of a variety of versatile instruments—electron microscope, electron microprobe, etc.—to study the shocked rocks I had collected. I elected to stay at Goddard. From 1969 into 1970, I was a Principal Investigator of lunar samples, charged specifically with characterizing the shock effects that I found. However, my circumstances changed by early 1970, and I found it necessary to move into a new field (remote sensing) thus, largely shutting down my impact work (except to use Landsat to search for new impact structures; this is described in section 18 of my current NASA Web site called the Remote Sensing Tutorial [http://rst.gsfc.nasa.gov]).

I was privileged to be in on the ground floor of modern impact crater science. In retrospect, I consider the three most important contributions I made to that field to be: 1) the aforementioned determination that high pressure nuclear explosions cause the same petrographic features as seen at impact sites and, hence, offer solid proof of a kinship in the mode of deformation and response of rocks subjected to shock waves; 2) the concept of the "shock log," applied to my study of West Hawk Lake core into the fallback breccias, which plots the degrees of shock effects at different depths into this part of the crater (this confirmed quantitatively the observations that these effects vary irregularly in intensity owing in part to mixing as the crater walls collapse inward); and 3) the first postulation I know of (developed one afternoon in a true moment of inspirational insight) that the lunar highlands consist largely of multiple ejecta blankets piled up from millions of impacts.

Over the past 45 years, I have had a totally unexpected but very satisfying career being what I call an "exotic geologist." This has involved working on nuclear explosions, terrestrial impact craters, lunar rocks, and remote sensing of the Earth— nothing traditional in my college preparations. It has been fun, and I am grateful to the Meteoritical Society both for their earlier support and for publishing this letter that allows me to amplify the record on shock metamorphism and impact cratering.

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