Table 1. 2,3-Diphosphoglyceric acid and inositol pentaphosphate in erythrocytes from chick embryos and chicks. The embryo age is indicated as the day of embryonic development (DE) from start of incubation. "Pipped" means embryos that have pipped the shell but have not hatched. "Hatched" are chicks that have been out of the shell for 1 to 2 hours. Chick age is shown as day since the chick (DC) hatched. Enzymatic assay of 2,3-DPG was determined by reaction kinetics on perchloric acid extracts of erythrocytes. Trichloroacetic acid extracts of erythrocytes were chromatographed on Dowex 1-X8 formate columns. The 2,3-DPG was determined by reaction of portions of column fractions with chromotropic acid and by wet-ash phosphate analysis. IPP was assayed by wet-ash phosphate analysis of column fractions. Numbers in parentheses indicate the number of determinations and ± values are standard deviation.

Age	2,3-DPG (μmole/cm^3)			IPP (μmole/cm^3)
	Enzymatic assay	Phospho-glyceric acid	P_i	P_i
14-DE	1.62 ± 0.10 (4)	1.63 ± 0.53 (2)	1.57 ± 0.06 (2)	0.51 ± 0.01 (2)
15-DE	3.22 ± 0.20 (4)	4.14 (1)	4.02 (1)	0.38 (1)
16-DE	5.26 ± 0.12 (4)			
17-DE		5.43 (1)	5.88 (1)	0.42 (1)
18-DE	4.67 ± 0.30 (6)	5.09 ± 1.29 (3)	5.10 ± 0.21 (3)	0.48 ± 0.06 (3)
20-DE		3.89 ± 0.27 (2)	4.60 ± 0.51 (2)	0.79 ± 0.28 (3)
Pipped	4.61 ± 0.32 (8)			
Hatched	3.04 ± 0.12 (6)			
1-DC		0.76	1.13	1.15
3-DC		0.57	0.58	1.42
5-DC				2.69
8-DC				3.47

inositol polyphosphates in the erythrocytes of birds (*6*) and some turtles (*7*). This material, making up approximately three-fourths of the cellular organic phosphate, was first thought to be inositol hexaphosphate, phytic acid (IHP), but has recently been identified more accurately (*8*) as the 1,3,4,5,6-pentaphosphate of inositol (IPP). The supposition that the IPP in avian erythrocytes might serve as a regulator of hemoglobin function in a similar manner to that of 2,3-DPG in mammalian red blood cells, although not proved, is supported by studies in vitro (*3, 9*) and in vivo (*10, 11*). In fact, IHP binds more tightly to hemoglobins and is more effective in lowering oxygen affinity than is 2,3-DPG. The increases of IPP in the chick red cell shortly after it hatches (Fig. 1) undoubtedly accounts for the observed decrease in affinity for oxygen in whole blood during this period of development (*10, 11*)

We have been studying the changes in P_{50}, hemoglobin type, and organic phosphate in erythrocytes from newly hatched chicks (*10*). On extending these studies to embryos we were quite surprised to find high concentrations of 2,3-DPG, comparable to those in human red cells (4 to 5 μmole per cubic centimeter of packed cells), in the erythrocytes of the embryo shortly before hatching (Table 1). The amount of 2,3-DPG increases in the erythrocytes from the 14-day embryo to a maximum in the 16- or 17-day embryo and then drops precipitously in erythrocytes from embryos at the time of hatching (Table 1; Fig. 1). On the other hand, IPP is present in only small amounts in the erythrocytes until hatching and then accumulates rapidly.

The material we are measuring is identical with 2,3-DPG in that it possesses the characteristic elution position from Dowex 1-X8 with an ammonium formate buffer system (*12*), gives the appropriate color in assays for glycerate (*13*), the ratios of phosphate (*14*) to glycerate are 2 : 1, and stimulates phosphoglycerate mutase activity in an enzymatic assay for 2,3-DPG (*15*).

The significance of the presence of 2,3-DPG in the red blood cells of the chick embryo is not readily apparent. It accounts for approximately 45 percent of the cell phosphates and is the major organic phosphate in the chick embryo red blood cell during the last week of incubation. The accumulation and rapid elimination of 2,3-DPG from the erythrocyte of the embryo during this period is puzzling. In other studies we have found that 2,3-DPG interacts with chicken hemoglobins, shifting the oxyhemoglobin dissociation curve to the right just as it does with mammalian hemoglobins.

The sudden change from 2,3-DPG as the predominant organic phosphate of erythrocytes of the embryo to IPP shortly after hatching indicates an abrupt activation and inactivation of genes. It seems reasonable to propose that the sudden changes in oxygen tension that result from the changeover from passive diffusion into the egg to active respiration in the newly hatched chick may trigger these events. Whether these events may be related to the switchover from fetal to adult hemoglobin synthesis remains to be determined.

RUSSELL E. ISAACKS*
DONALD R. HARKNESS
Research Laboratories of the Veterans Administration Hospital and *Department of Medicine, University of Miami School of Medicine, Miami, Florida 33125*

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* Mailing address: Basic Sciences Research, Veterans Administration Hospital, 1201 NW 16 Street, Miami, Florida 33125.

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Meteor-Generated Infrasound

In their report Donn and Balachandran (*1*) have made a complex problem appear overly simple. They suggest that, independent of other present means of meteor detection, they can determine a meteor influx rate with the use of acoustical methods. On the contrary, I suggest that their present speculation linking the pressure waves they record to meteors is not well founded. No cause-and-effect relationship has been established. All the supporting evidence they cite is indirect.

The following points stand out with respect to their hypothesis:

1) Recent theoretical treatments by both Tsikulin (*2*) and ReVelle (*3*) predict that an air wave signal with a period of 3 seconds (at maximum signal amplitude) as shown in figure 1 of Donn and Balachandran (*1*) at a range of about 50 km requires for its production a very large object ($\gtrsim$ 1 m in diameter, depending on the Mach number of the meteor). The theoretical analysis assumes that under certain flow conditions the meteor-atmosphere interaction can be treated with the use of a cylindrical blast-wave, line-source model. Specific details can be found in ReVelle (*3*). Such a large

body would very likely produce audible sounds as well, in the vicinity of the entry or, at the very least, would produce visual radiation effects of a nature so striking that reports in the mass media would have been common. The altitude range they propose for their "meteor" source is in fact consistent with that of such a large body. It should also be noted that Donn and Balachandran (*1*) were fully aware of these theoretical predictions.

2) The influx rate of such large bodies is not precisely known, but it certainly does not exceed about 10^{-22} cm^{-2} sec^{-1} (*4*). Within a circle 200 km in radius, this corresponds to an influx rate of about four objects per year (for masses $\simeq 10^2$ to 10^3 kg). Thus, the number of *potential* sources is not great. Because the hypersonic line source is highly directional, it is likely to be difficult to detect the source even under optimal conditions at the ground unless the source-observer geometry is also optimal. Atmospheric refraction and the local background noise level may be substantial, depending on the circumstances at the time of entry. Air waves from large bodies such as the Allende (*5*) and the Lost City meteorites were not recorded (at ranges as close as 1000 km from ground detection equipment) by conventional U.S. Geological Survey and National Oceanic and Atmospheric Administration microbarograph systems (*5, 6*). Also, no signals of meteoric origin were recorded by Donn and Balachandran on the instruments described in their report, when such sources were known and quite close (*7*). Thus, the occurrence and detection of meteor-generated infrasound is not so simple a problem as the authors' "many signals" would suggest.

3) Classified microbarograph results released by Shoemaker and Lowery (*8*) and reported in terms of specific influx rates by Gault (*9*) show that signals from air wave objects [meteors (see the discussion at the end of Gault's paper)] have been recorded, but by very sensitive classified military microbarographs (whose primary use is for the detection of air waves from nuclear explosions in the atmosphere). Even though data for specific meteors have not been released, the minimum mass detectable at the ground appears to be $\simeq$ 1 to 10 kg, which is in agreement with present theoretical predictions (*3*). On the basis of the influx information of Gault (from the work of Shoemaker and Lowery), an influx rate of about 130 meteors per year over an area 200 km in radius is predicted at this mass. [McCrosky's rate at this mass is about 20 per year (*4*).] The wave frequency predicted theoretically for such objects, however, is in the range 2 to 5 hertz at the ground, depending on the actual source-observer distance and on the minimum meteor size and Mach number combination which can produce a detectable amplitude. Thus Donn and Balachandran's interpretation of 0.3 hertz as indicative of meteor source parameters seems incorrect. Relatively good agreement between the predicted wave period and the observations of Shoemaker and Lowery (*8*) and of Hilton *et al.* (*10*) (Apollo 15 reentry data) can be found in ReVelle (*3*).

What is really needed, therefore, is a joint acoustic-photographic (and/or radar) detection effort for air wave signals from meteors (*2, 3*). Only then can a direct cause-and-effect relationship be established. This, of course, should be supplemented by an active theoretical research program. Such efforts are currently under way both at the National Research Council of Canada and at the University of Michigan.

It has been some 7 years since the scientific community was made aware of the existence of classified air wave signals from meteors. Unfortunately the end product of these data, the influx rate, is rarely quoted, probably because the details of this prediction have not been released. Such results and procedures are certainly of interest to those involved with the lunar seismic program for detecting meteoroid impacts (*11*). The ability to predict meteor parameters (size, mass, and other properties) on the basis of the specific meteor signals is of great value in this little known mass range. In addition, a better understanding of the propagation of weak nonlinear disturbances in the atmosphere may be possible with the use of these signals.

I make a strong plea for the declassification and publication of these data.

DOUGLAS O. REVELLE
Astrophysics Branch,
National Research Council of Canada,
Ottawa, Ontario K1A 0R8, Canada

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In considering ReVelle's (*1*) objections to our report (*2*), we would like to point out first that science proceeds by deductions, often from indirect evidence. However, in our report we carefully explained why this signal must be from a very fast-moving elevated source and ruled out all possible sources except meteors (or flying saucers!). ReVelle found no fault with the basic data or with the arguments leading to our conclusions. It would be very helpful if he can give an alternative explanation of the observed events.

We consider below his itemized points seriatim:

1) ReVelle has made a serious error here in assuming that the amplitude is peaked at 3 hertz. In the last paragraph of our report we noted that the dominant energy for the case shown lies between 0.3 and 3 hertz. His mistake was to deduce peak energy from the record illustrated, which is from an instrument with a 0.1- to 1-hertz passband and thus emphasizes the low-frequency component. This record was utilized for illustration because the presentation of the variations of time lags among the components of the tripartite array is much clearer for the lower-frequency (longer) waves. However, we also operate another tripartite array with a passband of 1 to 10 hertz. Our two instrument arrays were installed for purposes other than meteor-sound detection, and their passbands break near the middle of the frequency range of the meteor signal so that we cannot obtain a continuous energy spectrum. But the signal recorded on the same magnetic tape from our array with a 1- to 10-hertz passband shows higher amplitudes, with the peak amplitude at 2.5 hertz. Figure 1 presents a comparison of simultaneous signals from both passbands and clearly shows the stronger high-frequency components.

The high-frequency peak of the signal and its low amplitude (of the order of 1 μbar) certainly suggest a source smaller than 1 m and closer to the threshold mass detectable at the ground of 1 to 10 kg suggested by ReVelle. Also, it is well known [see, for example, Young (*3*)] that the Fourier analysis of an N-wave shows an energy distribution in a wide band of frequencies both higher and lower than the basic N-wave frequency. The higher frequencies are manifest as audible sound detectable at long range only from large sources because of strong attenuation at these frequencies. The observed frequencies are dependent on the source, the attenuation, and the instrument characteristics, and we should emphasize that our instrumentation was not set up to study meteors at the time of these measurements.

2) ReVelle seems unfamiliar with the

principles of refraction of sound in the atmosphere. For an elevated close source, sound following steeply inclined ray paths suffers little refraction and is easily detected at the surface. This has been shown by Tsikulin [(*4*), figure 19, p. 56], a reference cited by ReVelle. For distant sources ray paths are controlled in a complicated way by the temperature-wind structure of the atmosphere so that rays reflect between the stratosphere and the surface giving strong shadow zones; this effect, which has been well documented by computed ray-tracings in many publications by us and others (*5*), explains the anomalous occurrence and absence of signals at long range from the Lost City and Allende meteorites. At no time did we claim that the details of meteor sound were not complex. What we tried to show is that, with the reasonable assumption of a meteor source for our signals, certain logical deductions about trajectory could be made and that, with a grid of stations, flux estimates might be possible for bodies capable of generating infrasound.

3) The critical reference to Shoemaker and Lowery (*6*) and Gault (*7*) seem very inappropriate. The former is a short abstract which mentions that acoustic detection of meteors has occurred. We agree. ReVelle then implies that Gault (*7*) has data on influx rates that are significant because they were made by very sensitive instruments used for the detection of air waves from nuclear explosions. But we use equally sensitive instruments in the detection of acoustic waves from nuclear explosions and rockets, and we were the first to publish unclassified results in this country. Also, in this section, ReVelle returns to the importance of the erroneous peak at 0.3 hertz. As we noted in the first point, the peak energy is at 2.5 hertz, with strong recorded energy being spread over the range from 0.3 to 3 hertz. Atmospheric attenuation, which increases as the square of the frequency, has certainly reduced the higher-frequency portion of the spectrum, as well as its continuation beyond 3 hertz. Hence, rough estimates of the quoted flux rates for the source sizes deemed significant by ReVelle are in perfect agreement with our observations so far.

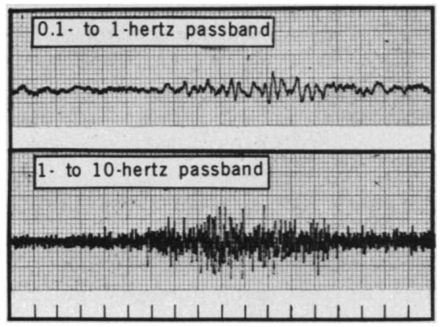

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Fig. 1. Acoustic signal for 20 October 1973 recorded by instruments with the passbands indicated. Both signals were recorded on analog magnetic tape with identical gain settings in both the recording and playback electronics. The maximum amplitudes were at 2.5 hertz. Time marks at the bottom indicate 4-second intervals.

We believe that our results appear to give the first published documentation of meteor infrasound other than the handful of cases from huge visible fireballs. Although we completely agree about the need for a combined program of photographic, radar, and acoustic detection, we explained in our report the possible difficulties associated with the detection of a large portion of acoustic meteors by photographic and radar methods. We agree with ReVelle that the release of classified documentation will aid measurably in this problem and can provide a further test of our conclusions.

William L. Donn
Lamont-Doherty Geological Observatory of Columbia University, Palisades, New York 10964, and
City College of New York, New York 10031

Nambath K. Balachandran
Lamont-Doherty Geological Observatory of Columbia University

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