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Dear Dr. Martin,

Thank you for your letter of 7 July. I am sorry you had trouble locating me and that Laval did not send on correspondence. My contract at Laval was terminated following publication of the article in Nature. I never cease to be astonished at peoples fear to deal with the most important problem mankind ever faced; the control of nuclear weapons. Despite the difficulties of being divorced from income, I have continued to press for further investigation in the hope this could avert potential catastrophe. Thus far this has proved abortive in the US, USSR, UK and Canada. I hope you will be able to lift the veil of uncertainty in your endeavours. I trust you will not object to expression of a chronicle of views and experience with a number of suggestions for research thrown into the comment.

HIGH YIELD WEAPONS.

The only direct observations capable of revealing the extent of odd nitrogen injection into the stratosphere by the high yield tests of 1962-3 appear to be those of Kondratyev and Nikolsky.[1] They used balloon borne actinometers, ie stratospheric based, to measure solar flux in the period from 1960 to the present. Nikolsky, private conversation, estimates the measurement accuracy as ± 0.3%. Following the high yield tests the mean solar flux was reduced by about 2.6% and it took 5 years for the atmosphere to recover. In one observation at the time of these tests, not reported in the above reference, the reduction was 8%. This is interpreted as due to absorption by an injected cloud of NO_2 prior to diffusive dispersal. The data are consistent with injection from these high yield tests if one assumes an efficiency of production four times that estimated theoretically in US studies and almost total injection. Having spent three months in Leningrad with Kondratyev and Nikolsky and getting to know them intimately, I am very confident of their integrity and capability. But the difference from the US theoretical estimate, and several other aspects of related analyses, imply a number of investigations should be undertaken to reveal the source of the discrepancy and enable accurate estimation of potential injection capability.

There is no way I can check the theoretical data without access to good computational facilities. But there are a variety of reasons which make the stated accuracy of the calculation doubtful. The variation in reaction rate coefficients between different laboratory measurements, as given for example in the NBS texts, is too large to permit the conclusion. There have been no allowances for the influence of pressure on the flow of energy from the detonation point and the chemical kinetics. Change in the relative significance of first and second order reactions must surely warrant examination. When the pressure is low enough,

one does not have equilibrium kinetics.

Conclusive study of the potential impact of nuclear detonations demands improved modelling of the temporal and spatial characteristics of the transfer of energy,(radiative, dynamic and chemical), with emphasis on the influence of pressure and excited species.

Ruderman and Chamberlain cited several reasons for doubting that past nuclear detonations had greatly modified the stratosphere. One relates to the carbon-14 data. They use C-14 as a tracer for the abundance and distribution of odd nitrogen on the grounds that the volume of space for their initial production from a one megaton detonation is the same. And since most of the C-14 from the high yield detonations of 1962-3 was observed to remain in the troposphere they concluded there was little odd-N injection into the stratosphere. They neglected the point that although the fireball volume is the same as the volume of C-14 production when the fireball has cooled to 2,000°K, (the cut off temperature for NO removal), the time at which this volume is attained is sufficiently later than the time of detonation, and C-14 production, for the region of NO production to be significantly displaced from the C-14 region. This displacement implies the C-14 trailed the odd nitrogen in the ascent of the fireball and indication the C-14 did not reach the stratosphere does not imply imply the odd-N remained in the troposphere.

Modelling of thermonuclear detonations should be sufficiently detailed to permit assessment of the validity of using radioactive products as tracers for the abundance and movement of odd nitrogen, paying particular attention to the limitations of C-14 as a tracer and the potential utility of Sr-90.

There was apparent confirmation that little odd-N reached the stratosphere from observation that there was little change in total ozone following the high yield detonations. But this conclusion was reached prior to the laboratory measurement of the rate coefficient for the NO,HO_2 reaction which showed that injection on the scale potentially possible from the 1962-3 tests should have lead to simultaneous increase in lower stratospheric ozone abundance and decrease in upper stratospheric abundance through curtailment of removal of odd oxygen by odd hydrogen. Evaluation of change following the high yield tests should have been based on examination of changes in the distribution of ozone and not total ozone. Relevant data are available though they have not been examined from this viewpoint.

Assuming the Sr-90 data are adequate and it is a suitable tracer, the coincidence of the time of arrival of Sr-90 in the southern hemisphere and the Mount Agung volcanic explosion suggests some rethinking of the events attendant that explosion are desirable. Umkehr observations by Pittock in Australia suggested a simultaneous change in the distribution of stratospheric ozone which was interpreted as a false measurement because of injection of dust into the stratosphere following the Mount Agung explosion but which precisely fits the kind of change in ozone one would anticipate from the scale of odd-N injection the Kondratyev/Nikolsky observations infer. The Australian data should be reassessed.

The Australian data may have a unique significance because of potentially greater deficiencies in data from other sources. Most of the northern hemispheric observation sites are located at latitudes with greater variability in ozone distribution. The natural variability is of the same order of magnitude as that one would anticipate fron the nuclear odd-N injection. Greater care is needed in reexamining these data from other sites. Detailed dynamical modelling and meteorological hindcasting is required. The greater distributional variation in the abundance of injected material, because of the closer proximity of the sites to the injection source is a further factor implying greater care in analysis is required. For other reasons, which I will outline later, the data from other low latitude sites, such as India and Egypt, have deficiencies which give the Australian data increased significance.

If one takes the Kondratyev/Nikolsky data as indicating the extent of potential odd nitrogen injection from a nuclear conflict, the estimate of the US National Academy of Sciences of 1975 of the potential change in global ozone should be increased by a factor of two; ie the ozone would be reduced to between 15 to 30 per cent of its normal atmospheric level. But there are grounds for believing a larger decrease possible. These are implied in part in the earlier comment of this letter. But they are even more forcibly implied by the specific observations which have been used to assert the unlikelyhood of significant ozone change from extensive thermonuclear detonations! Measurements of total ozone indicate an abundance prior to the 1962-3 high yield tests which was lower than the post test abundance. So it seemed clear that change due to past nuclear tests was negligible or at least much less than natural variability. But this conclusion was based on the assumption that the odd-N production could be directly related to yield and was independant of location and altitude of the point of detonation. There are aspects of high altitude detonations which have never been examined.

High Altitude Detonations.

There appears to be only one relevant direct observation.[2] On the night of August 14-15 1959 observation of infrared atmospheric emission spectra from a balloon platform 28 kms. above Quebec showed a change in the spectra at dawn which could be relevant. An emission feature which could only have been from NO_2 vanished as the sun illuminated the gondola, and the relative amplitudes of this emission and the water vapour band in which it was embedded suggests an anomalously high abundance of NO_2. Later measurements have shown the ratio of column densities of water vapour and NO_x above 28 km. to be about 300 to 1. But the observed ratio of emission in this observation of 14-15 August 1959 was between 5 and 10 to 1. An NO_2 column density of about 10^{17} molecules $cm.^{-2}$ is implied in contrast to the natural abundance of about 3.10^{15} molecules $cm.^{-2}$. This assumes the H_2O, NO_2 bands have approximately the same strength. Around this time the USSR was conducting trials of a ballistic missile interceptor and nuclear warhead above the Kyzyl Kum desert and it has been my assumption that the NO_2 seen above Quebec on the 14-15 August could have resulted from such a

trial conducted some one to two weeks earlier. The winds and dispersion conditions would have been such as to take the injected material to Quebec before widespread dispersal through the atmosphere. Requests for relevant information saying yea or nay to this proposition from sources in the USSR and US have met with a stony silence though I know the trials took place.

If the detonation altitude was similar to the US Teak test a year earlier, one would expect a mirror point auroral disturbance to have occured. This might have been too far south east of the southern boundary of South Africa to take it into range of observers; unlike the Teak effects seen vividly in New Zealand and adjacent islands in the Pacific. Even if the detonation altitude was not suitable there should have been radio effects equivalent to those associated with Teak and Orange. Unfortunately, I do not have access to facilities which would make possible discernment of whether such a high altitude nuclear detonation took place on the appropriate date. I assume you have such access.

An investigation of the appropriate auroral and radio data would reveal whether the conjecture that the Quebec observation is related to a USSR nuclear test warrants examination. (This is by no means a trivial matter since other factors imply high altitude detonations could be far mor effective in producing odd-N than is currently assumed and the Quebec measurement is the only available one.)

If clearcut evidence can not be revealed from observational data, more extensive analyses of the flow of energy from high altitude detonations is essential.

Assumption that the prompt gamma radiation is all absorbed by the bomb debris, Koslow etc, is untenable. If one assumes the downward flow of energy follows a pattern of interactions leading to progressively longer wavelength x-rays until the wavelength is long enough to permit high transmission, a substantial fraction of the bomb energy will penetrate down to an altitude of 40 km.. A high altitude burst may produce chemospheric change akin to that of a solar proton event. Removal of upper stratospheric ozone through odd-N produced in this way leads to reduction in solar heating and temperature gradient promoting vertical instability and transfer of the injected odd-n into the lower stratosphere.

The yield from high level detonations in the period 1958-9 was about 10 Mt., and a presumption of a 20% efficiency in production of odd-N leads to the conclusion that one megaton of odd nitrogen may have been injected into the upper stratosphere, an injection equivalent to that of the later high yield detonations. But whereas the high yield detonations produced countervailing change in the upper and lower stratospheres, the initial effect of a series of high altitude detonations injecting NO_x only into the upper stratosphere would be a reduction in total ozone. Observation shows global total ozone reached a minimum shortly after the period of high altitude nuclear tests. The variation in ozone was markedly different from that of the two previous solar cycles.

If the high altitude detonations produced the observed decrease in global ozone, there are implications for

prior, more localised, change which warrant study. All injections took place with small solar zenith angles in conditions of weak circulation and dispersal. The injected clouds would produce severe local depletion of ozone above an altitude of 40km.. Subsequent removal of ultraviolet heating in this region would remove the 12°C per day heat input and lead to subsidence at a rate of about 1 km. per day. Descent over a period of about 10 days would lower the injected cloud to the altitude of peak ozone to produce severe depletion of total ozone locally.

Assuming normal stratospheric circulation conditions, the US test Teak should have lead to severe depletion of total ozone above India in the period 7-20 August 1958. The westward drift and varying vertical velocity gradient imply progressive dispersal in an equatorial belt and meridional dispersal so that maximal impact should have been in the vicinity of India. The time of exposure to stronger uv radiation than normal would be short enough that the biospheric impact could have been small. Around this time, the Indian data using the Dobson technique for ozone observation showed severe anomalies which were attributed to high levels of dust in the stratosphere.

If observational analysis and theoretical study of potential injection of odd nitrogen from high altitude detonations imply these have been potentially significant, the Indian ozone observations should be reexamined to see if the anomalous readings are compatible with change in ozone through high altitude nuclear detonations. If this turns out to be the case then the biospheric impact should be assessed.

A clue to the possible direction of such biospheric analysis lies in some studies of blindness in Indian cattle of a variety whose eyes appear particularly sensitive to uv radiation. Such a study has been reported in the Indian Journal of Cancer and preliminary indications indicate greater blindness in cattle alive at the time of the tests than those born thereafter. If such study reveals a correlation, research should be extended to studies of human blindness in India and the whole equatorial belt. The number of blind people in India is so high, about 5% of the population and a number equal to the total population of Australia, that relevant statistics should not be difficult to obtain and the possibility of horrendous impact from a single past nuclear test would be a salutory reminder of the potential danger from use of the current agreed stockpile of 200 such weapons and projected large increases in high altitude ballistic missile intercept capability.

But the whole spectrum of biospheric impact warrants more careful study and the studies suggested in the previous paragraph should be accompanied by a total system study whose form I will outline later.

Armed with detailed models for the impact of past nuclear detonations, and satisfactory interpretations of the dependence of odd-N yield on pressure, altitude, yield, location, time etc, one would be in a satisfactory position to assess the potential impact for various scenarios for potential nuclear conflict and misadventure.

Large Scale Nuclear Conflict.

There are obviously scenarios in which the

would be unaffected. The trend towards MIRV implies a potential warhead delivery situation in which there would be negligible injection of odd nitrogen into the stratosphere. But there are weapons design considerations which imply such a conclusion is unacceptable.

A characteristic of the very high yield weapons has received less consideration than it should. In terms of attack against soft targets such as cities the optimal detonation altitude for a one megaton device is between 10 and 20 thousand feet. When the yield is increased to 50 megatons the optimum detonation altitude yielding the same overpressure at the surface is between 50 and 100 thousand feet. The fireball from the one megaton explosion is small enough that the energy can be regarded as emanating from a point source. But with the 50 megaton device, exploding at much higher altitude, the density is low enough to allow much further penetration of non-vertically incident radiation into the atmosphere than relatively obtains for a 1 megaton explosion. The shock wave does not then come from a point source fireball but from an extended source firesheet almost parallel to the surface. So it is relatively more damaging and this characteristic has presumably been the feature which lead to its development and deployment by the USSR. It is an optimal weapon for use against a large urban target and consideration on potential chemospheric effects should be based on a scenario in which the 100 to 200 such weapons currently deployed are used against such targets.

There are three inferences for potential odd-N production and injection into the stratosphere. All the odd-N will be injected. If odd-N production for thermal equilibrium conditions increases as the pressure decreases the efficiency of odd-N production will be higher than that which obtained in the high yield trials of 1962-3. The altitude of detonation begins to approach that where nonequilibrium chemistry could generate even more odd-N. This arises when a significant part of the energy is dissipated at altitudes where the increase in temperature of the atmosphere is insufficient for thermal dissociation of NO, ie a temperature of 2,000°K, since a large fraction of the N_2 dissociation by the incident radiation produces N doublet D which reacts rapidly with O_2 to form NO and the latter can not be removed subsequently through thermal dissociation. The detonation altitude marking the transition from thermal equilibrium to nonequilibrium conditions is around 40 km.. Since this is not far from a suitable detonation altitude for use of high yield weapons against soft targets it seems inadvisable to rule out a potential efficiency of formation of odd nitrogen as high as 5 to 10%. The USSR high yield weapons would then have the capacity to increase the odd-N abundance in the stratosphere by a factor of 500. An order of magnitude reduction in total ozone would be anticipated, placing the biosphere in jeopardy.

Hence, it is of vital importance to put theoretical studies of nuclear detonations and the effect of past tests on

on a more accurate footing. The high yield weapons represent just over one per cent of the weapons currently deployed and an accurate appraisal of the potential effect of a full scale conflict would have to examine all potential scenarios. This is far too extensive a task to deal with in a brief letter. And it is a task rendered doubly difficult by the lack of clear information on the weapon stockpile. For example, in the recent past the US have quoted the total east-west stockpile as about 10Kmegatons when in 1960 the stockpile was estimated, I believe reliably, as 60K megatons. The reason for the apparent decrease is that the delivery systems changed from aircraft to missiles and the warheads associated with the former have been disregarded in recent comment. Frankly, I see no reason to believe they have been dismantled by either side.

Because of these difficulties, I will not extend my comment unless you feel it would be worthwhile. In one sense, I consider it inappropriate to do so for I am convinced that the major potential protagonista, the USSR and US, are so aware of their capacity for mutual self destruction that they will desist so long as this MAD capability remains. But the possibility of a future imbalace through improved technology, particularly missile intercept, initiation through a third party because of increasing proliferation, and failure of command and control as in the fears expressed by York, Eisenhower's scientific advisor, remains. Insofar as full scale nuclear conflict is concerned, the only point to examination of potential chemospheric/biospheric impact on a global scale is to add further impulse to the deterrence in the belief that if the global living system can be shown to be under threat this provides a universally acceptable deterrent; all people and all nations.

There is, however, an additional factor which has not received enough attention; the possibility of disastrous chemospheric change through action which appears to be harmless because the consequences of such action have not been adequately studied.

Accidental Misadventure.

Consider the potential consequences if the USSR tests had taken place at their test site in the Kyzyl Kum desert in summer rather than the polar winter. Assume an efficiency of odd-N production of 0.8%, inferred from the Kondratyev/Nikolsky observations, in a total of six 50 megaton detonations. (The actual trials involved many detonations of smaller yield extending over a much longer period but it would have been quite feasible to have shortened the duration of the trials by working in the more favourable climate of Kyzyl Kum than Novaya Zembla, it would not have been difficult to ensure the yields were at the 50 megaton level and my purpose is simply to illustrate a fairly simple, bordering on trivial, point.)

Each test would lift some 0.4 megatons of NO into the stratosphere. Assume the expansion in ascent to the stratosphere followed by followed by self induced circulation change over the next few days would spread the injected material

uniformly into a region of a diameter of 1,000 km. and extending from an altitude of 25 km. upwards. (One of the factors which should form part of a study of nuclear effects on the chemosphere is the circulation changes following the changed heat input due to the injection. Investigation, for example, of the reason why the Sr-90 abundance from these tests reached a peak in the southern hemisphere a mere few months after cessation of the tests could prove significant.) The assumption is over simplified but the intent is merely to draw attention to the kind of odd-N mixing ratio which would prevail shortly after the test and the volume affected. The odd-N mixing ratio would increase by almost a factor of 200 over the unperturbed value to about 5.10^{17}. Total ozone in the affected region would reduce drastically in a day or two; long before the circulation could disperse the injected odd-N. The ozone deficient region would drift with the easterly circulation at a velocity of about 10 metres sec^{-1} gradually diffusing and filling in. The eastward drift would carry the ozone deficient region across the Ukraine, Europe and North America until diffusion arrested the successive exposure of these regions to one day's exposure to anomalously high uv radiation. The exposure would be repeated at weekly intervals for the duration of the tests. If the tests were repeated at daily intervals rather than weekly, the ozone deficient region would extend over 60 degrees of longitude and 10 degrees of latitude giving continuous exposure to intense uv over a period of one week to these regions. Contrast this situation with that which prevailed in the aftermath of the 1962-3 tests.

Injection then occurred in the absence of sunlight and in conditions of strong circulation and horizontal and vertical mixing. The gradual return of solar uv in the following spring through to the summer would result in progressive exposure at decreasing altitudes creating a tendency to drive high level injected odd-N into the lower stratosphere. Pittock's data suggest a 10% increase in ozone in the lower stratosphere above Australia. When the odd-N injection is not too large, the effect of the diminution of removal of ozone by odd hydrogen in the lower stratosphere is to create a one fifth power law dependence of ozone abundance on odd-N mixing ratio. The 10% increase could have been created by a 50% increase in lower stratospheric odd-N. (The lower stratospheric odd-N abundance in the Northern Hemisphere would have been much higher at this time, high enough to both eliminate ozone removal through odd-H and compensate for the loss in odd-H removal by additional odd-N removalof ozone and lead to no net change in lower stratospheric ozone. It is a trivial matter to work out the numbers involved so I will not go into details unless you think this desirable.) This implies an odd-N injection from the 1962-3 tests of the same extent as that assumed in the 6 hypothetical high yield detonations. But whereas the 1962-3 tests caused little change in total ozone, because they took place in the winter, these hypothetical summer tests introduce the possibility of localised diminution of total ozone

which could be very significant.

The point is that whereas the threat from nuclear conflict is a strong deterrent, past experience from the USSR high yield tests seems to indicate that repetition would have no significant impact and there is nothing to deter such action. Yet it would be exceedingly harmful if it led to sufficiently great ozone removal on a regional scale as to produce excessive uv exposure on this scale. I see,for example, no reason why China would be deterred from conducting such trials. Yet if they did so, and the conditions were such as to cause critical injury to the USSR, retribution by the USSR would seem an inevitable consequence. Harmful action through blind ignorance is the thing I fear most.

And similar harmful action could follow from ignorance of the potential impact of high altitude detonations. Moscow is presently surrounded by one hundred ABM interceptors whose warheads I assume to have the same yield as the US Spartan missile, about 2.5 megatons. I conjecture the potential injection of 25 megatons of odd-N into the stratosphere if this defensive system is ever used. One can not rule out the possibility that reentry of unidentified vehicles or space debris, particularly in conditions of international tension, might induce use of this defensive system. I doubt the target discrimination capability of the radars involved is good enough to identify a real warhead and the paralysing effect of radiation and ionospheric perturbation on their radars from detonation of their first interceptor warhead implies the total defensive system must be used against an attack or they run the risk of having it incapacitated or destroyed before it can be used. Because odd-N insertion would be restricted to altitudes above 40km., the USSR might be relatively unharmed, at least initially. But consider the implications attendant use of the system in summer when the prevailing winds and descent through removal of upper level ozone could carry the cloud of injected odd-N above North America, covering an area of the same size as the US, with the increase in odd-N above North America of a factor of One thousand giving the capacity to reduce total ozone by over an order of magnitude and expose North America to intense uv for a week or so.

Such misadventures are feasible from current capability. There is a growing risk that the range of potential misadventure may increase, aided by continuing failure to come to grips with reality.

There is a disquieting character to the continuing assumption that nuclear detonations in space can have no environmental impact. This reached the height of absurdity in a proposal in the early sixties that explosion of several thousand megatons at high altitudes could create a temporary Van Allen belt of sufficient intensity to remove decoys accompanying a missile warhead and enable recognition and intercept of the warhead. It was further exemplified in the US National Institute of Sciences Report of 1975 when the authors rejected the notion that high altitude detonations should be

considered in spite of the facts that ABM interceptors are deployed and current and future defence capability rests on the intelligence gathering and information exchange capability of satellites. Efforts to achieve a ballistic missile intercept capability can be expected to continue and the threat posed by developments ~~of~~ whose use would generate unacceptable environmental change, though not in the eyes of those turning a blind eye to such a potentiality, remains. There is, however, a potential intercept capability which might have a beneficial effect. This will be outlined later.

Investigation of the potential effect of nuclear conflict should not be restricted to a formal, stylized study of all out attack and counterattack by the defence systems currently deployed and under development but should include all aspects of potential trials and weapon usage where there is a possibility the controllers concerned are ignorant of the potential impact of their action and there are grounds for believing severe geophysical impact could ensue.

Assessment of these grounds demands more thorough investigation of biospheric characteristics than has yet been undertaken.

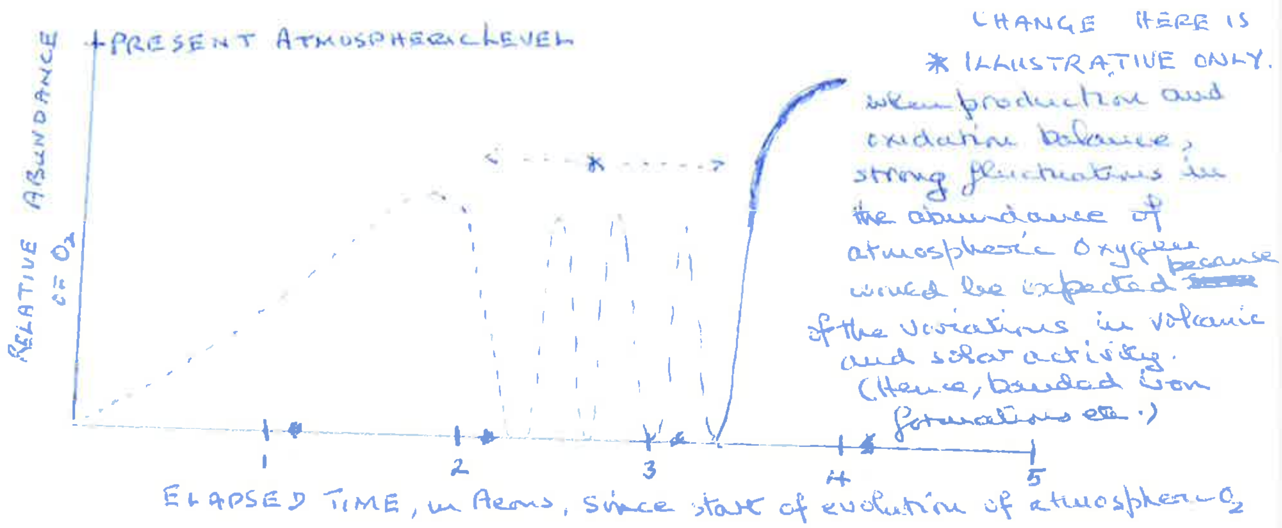
Interaction with the Biosphere.

Arguments of the kind advanced by Lovelock, New Scientist, July 1980, contain an oversimplification which is unwarranted. He notes that the argument that oxygen evolved from procarytoids, given strong support by fossil data, implies these could not have been prevented from developing because of the absence of an ozone screen since they must have been present when there was no oxygen and no ozone screen. He uses this argument to voice the proposition that the development of terrestrial life was not contingent on the presence of an ozone screen. I believe the argument unacceptable since it neglects the possibility of other means for formation of atmospheric oxygen and ozone. (I share his view that the primitive fossil forms bear such resemblance to current simple procarytoids as to suggest that their current character and behaviour could be a guide to the kind of processes taking place in primitive evolution and, by implication, to appraisal of the effect of environmental change on the existing biosphere. Phytoplankton reproduction is critically dependent on a balance between influx of nutrient and outflow of phytoplankton in regions of simultaneously appropriate vertical transport and solar illumination. There seems no reason to suppose their early evolution took place at greater depths below the water surface than at present. And since current phytoplankton are affected by uv radiation it seems appropriate to assume the primitive equivalents were similarly affected.) The alternative hypothesis of escape of hydrogen has attractive features. Not the least of these is an apparent impossibility of deriving a quantitative model for the temporal development of atmospheric oxygen and ozone though, at first sight, the possibility of deriving such a model from hydrogen escape seems equally intractable.

Urey's contention that inhibition of photodissociation of water vapour through screening by molecular oxygen implied hydrogen escape could not be responsible for the growth of terrestrial oxygen is not germane since the bottleneck is the rate of vertical transport of all hydrogenic material to the exosphere and, in any case, the rates of direct and indirect photodissociation of water vapour were inadequately established at the time of his proposal. But since there is no way the past rates of diffusion to the exosphere can be reliably established development of an escape model seems an impossible task. But there may be an indirect way of getting ar around the difficulty.

One must first assume the water vapour mixing ratio above the tropopause has been invariant. Then consider the fate of molecular hydrogen formed through the reaction $H + HO_2 \rightarrow H_2 + O_2$. H_2 from this reaction forms in a narrow altitude belt at the mesopause. The time for photodissociation is so long that loss this way can be disregarded. The variation in diffusivity with altitude is such as to suggest that all molecular hydrogen formed from this reaction at the mesopause passes to the exosphere where it can be dissociated and the hydrogen then escape. The time to reach the exosphere and escape is much less than the thirty years required for photo-dissociation.

Calculation of the rate of hydrogen formation as a function of time and O_2abundance is trivial. Assuming a rate coefficient for the reaction of 2.10^{-13}, the time for formation of atmospheric oxygen, including that currently in a bound state, can be calculated as 4,000 million years. Presumably, one can assume the growth of atmospheric oxygen bega -n with the onset of volcanic activity. But growth of the oceans created an increase in oxidation and the development of atmospheric oxygen involved a competition between formation through hydrogen escape and removal through oxidation. Assume this would continue until the oxidation reached a saturation point with the atmospheric oxygen abundance monotonically increasing thereafter. The variation of atmospheric oxygen with time would take the form shown.

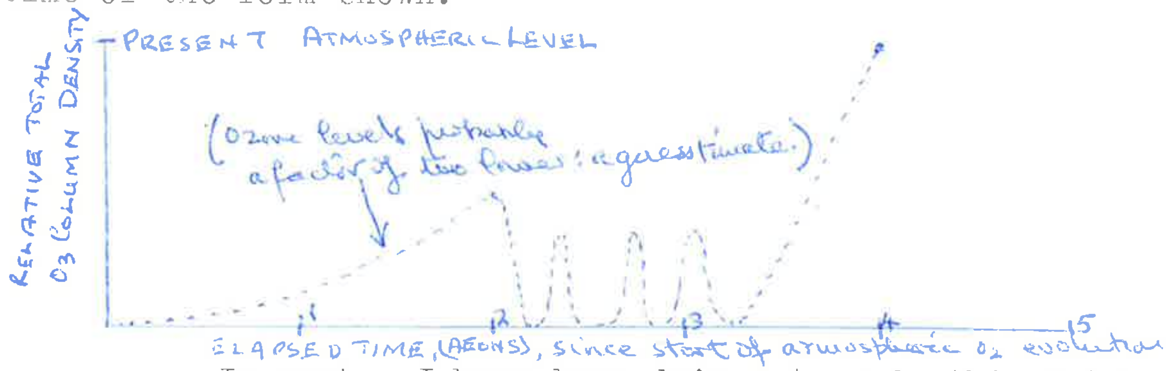


This is an extremely simple model with many deficiences. I can give you details on the calculation if you wish but I am not so much concerned with the absolute accuracy of the model as with derivation of an approach which might enable one to gauge the role of the ozone screen in evolution and thus potential inferences on the impact of gross perturbation of ozone.

My views on the development of the ozone screen are, I am afraid, as much at variance with those expressed in the literature as the foregoing thoughts on oxygen evolution.

I first determine the height and temperature of the stratopause as a function of oxygen abundance. Assuming the temperature of the tropopause fixed at its current value, this gives the vertical temperature gradient of the stratosphere as a function of oxygen abundance. Then from assumptions on a relation between vertical diffusivity and temperature gradient, I combine the chemistry and diffusion to derive a variation in ozone profile and total abundance with oxygen abundance. The current view in the literature seems to be that total ozone rapidly peaked after the onset of oxygen growth because of the higher pressure at which peak oxygen photodissociation occurred, and decreased thereafter. But this entirely neglects the variation in vertical stability and transport.

The net result is a variation in total ozone with time of the form shown.



In part , I have been driven towards this interpretation through views expressed in correspondence with Christopherson of Oslo. He pointed out to me that there was geological data suggesting that primitive plant life, circa 400 million years ago, was relatively insensitive to uv illumination. I think it not unreasonable to adopt a hypothesis of a slight change in Darwinian philosophy; survival of the fittest to adapt to environmental change, particularly the change of surface insolation.

I do not see how one can begin to talk sensibly about the potential effect of ozone change through thermonuclear detonation malpractice without some kind of model for the impact of ozone change on a geological time scale; a model in which the complexities of the interdependence between species, from phytoplankton to man, can begin to emerge.

Studies of the impact of ozone change should begin with the procarytoids since these are the only living organisms capable of forming amino acids directly from the natural elements.

The first biospheric question you should ask is whether Australia could survive if the procarytoids were destroyed, the ozone diminution which could result in their destruction and the conditions under which this diminution might occur.

Such study should then be extended to consideration of why different procarytoids evolved at different stages in the geological past. A reason for suggesting this as an initial approach is that these species are relatively simple and they seem to represent an initial building brick in the process of evolution.

Then one might try to piece together some concepts on how the progressive evolution of vegetal forms might have been influenced by changing uv illumination and move on to related studies of the evolution of insect life and more complex organisms.

Armed with the kind of ideas on interdependence between species which such study would bring out one would be in a better position to formulate plans for the study of the potential impact of ozone change, through anthropogenic idiocy, on the current biosphere.

And I suppose one ought to include direct study of man's capacity to survive with appropriate thoughts on skin cancer, blindness and sun tan lotions etc though I personally feel it is simple enough for man to stay out of the uv or use protective covering so that the real threat will lie in effects on biospheric species on which man is wholly dependent though I would be at a loss to say unequivocally what these are.

Just as assessment of the possibility of ozone change through thermonuclear detonations and evaluation of the potential impact on the biosphere are dauntingly difficult, so the issue of control of nuclear weapons poses problems which will be equally difficult to resolve.

THoughts on Arms Control.

Unilateral nuclear disarmament may be more likely to promote than prevent imbalance and instability. It is simply a means for avoiding responsibility for setting up detailed arms control procedures. For a time, and in a bipartisan relation, the doctrine of mutual assured destruction may work. But, in the long run, proliferation and increasing simplicity in the construction of nuclear weapons will cause such a multiplication of potential sources of imbalance and instability as to imply requirement for more organised means for control of nuclear weapons.

The problem is not so much reaching agreement as making sure agreement can be verified. Certain verification will demand such loss in sovereignty that it could be several generations before adequate systems can be agreed and introduced. The thermonuclear age began a generation ago and there has been scant progress since then.

I doubt the feasibility of building a bridge towards conditions enabling verification through confrontation and concealment of the facts about weapons impact. There is a section of Mac Donald's submission to the US Congress on the

potential effect of largescale operation of SSTs which rings true however false the SST argument may be; particularly the points 2, (1) and (2) on page 4 of the submission. Because of the long residence time and chemical reactivity, the stratosphere is the element of the global life support system most likely to be stressed by anthropogenic activity to the point where the system is threatened. When this point has been so repeatedly hammered into the minds of people throughout the world that it is accepted as axiomatic there may be a hope of motivating them to demand unified action towards suitable vefification. The fact that it will take a long time to reach such a state of affairs may deter some but it should be a stimulant to make sure we get there.

In the meantime, balanced conditions to deter large scale nuclear attack should be maintained with thinking directed towards weapon system development which would so increase the risk in such full scale attack as to provide an absolute deterrence. This has been an aspect which has been a personal preoccupation for some years, was hinted at earlier in this comment, and may warrant some amplification since it appears to go counter to both the theme of nuclear arms control and current weapons development policy.

To see the point one should turn back to the thinking that went on in the panic following the USSR IRBM trials and the Sputnik launch; the panic outlined by York and one in which I was heavily involved. We launched into an immediate effort to assess the feasibility of detecting ballistic missile launch from satellites. This was soon proved to be practical and adequate satellite systems, (MIDAS etc), have been operating for almost a generation. Simultaneously, it was realised that ICBMs were most sensitive to attack in this phase, when the burning rocket could be detected, and proposals were advanced for ballistic missile boost intercept; (BAMBI). BAMBI was hardly a practical proposition for, as York noted, it would require the enormous fleet of at least 3,000 satellites in continuous operation. But what York and others failed to realise, understandably so because of the state of the art at the time, was that there was a way to circumvent the farcically crippling cost of the BAMBI satellites.

If one can develop a defence system in which the rocket motor is destroyed during launch and the warhead dumped back in the vicinity of the launch site, at a cost less than the cost of the ICBM, the resultant removal of ballistic missile capability should be a powerful stimulus for agreement to remove the missiles and agree to a verification program.

It was with this kind of thought in mind that I initiated a research study on high energy CO_2 lasers in 1965 which led to development of the transverse excited arc laser and discovery that a laser capable of knocking out a ballistic missile during the launch phase could be built. I will spare you the details of the various ways in which timely target detection, identification and intercept might be provided at reasonable cost. My intent in describing this proposition is to make sure you

are aware that my concept on the way in which nuclear weapons control should be approached differs completely from that of the unilateral disarmers,(CND etc), and those multilateralists, and these seem to represent the balance of those concerned, who fall back on mutual assured destruction as the panacea.

Comments on and studies of the impact of extensive thermonuclear detonations on the chemosphere and biosphere should be so arranged and conducted as to prevent proponents of unilateral disarmament using them to create imbalance and instability and thought through in such a way as to stimulate motivation for new methods of arms control verification and interim weapons system structures.

When your letter arrived, I thought it inappropriate to answer by citing work done additional to my brief comment in Nature. It seemed more sensible to give a general overview on thoughts about the issue. This involves less effort and briefer communication, despite the inordinate length of this letter. But it may be sensible to indicate the form and extent of my involvement in the issue and changes in viewpoint as they have developed in the past 22 years to help you decide whether there is merit in these activities.

Personal Activity.

In 1957 I was charged with responsibility for devising ways in which the ballistic missile threat might be met through passive detection systems and initiated a variety of research studies to this end in close cooperation with the US, particularly the Advanced Research Projects Agency in Washington. This involved observational and theoretical studies of the launch and reentry phases, appraisal of the influence of the environmental background on detectability and development of necessary components and instrumentation. This was a fairly extensive program involving, for example, sending research teams to Ascension Island, Cape Canaveral, Thule in Greenland, Albuquerque and various sites in Canada, and observations as far afield as Point Barrow , Alaska and Mendoza, Argentina. Brevity, and to a limited extent a need for confidentiality still remaining, inhibits detailed description of these activities. But they gave a reasonably broad picture of the range of problems involved in ballistic missile characteristics which is, I believe, germane to missile control and disarmament.

Assessment of the environment involved a wide range of atmospheric observations using CF-100 and U-2 type aircraft, extensive balloon borne measurements with a wide variety of instruments in gondola payloads of the order of 500 to 1,000 lbs. and rocket observations of airglow and seeding effects. With this context, two conclusions emerged following the observation of August 14-15 1959[2]

Ozone removal was primarily controlled by minority constituents of which odd H could have primary importance because the observation of NO_2 in this observation and the failure to confirm the observation in later measurements-delayed by 2 years-suggested a variability in NO_2 abundance not consistent with odd N as the primary agent destroying ozone. (At this stage the

chemistry of odd O,N,H was in a rather primitive state and it seemed probable that the anomaly in the observation could be the result of natural variability of odd-N, a conclusion shown to be probably false by later observations, rather than injection from nuclear detonations.)

In contemplating the character of stratospheric ozone, the most significant point seemed to be the large disparity in the chemical energy of the ozone layer- about 3,000 megatons - and estimates of the available energy in the nuclear stockpile of about 60,000 megatons. The potential effect of extensive nuclear detonations appeared to be massive increase in total ozone.

I tried to stimulate Canadian and US governmental interest with this potentiality as a possible basis for motivation in arms control. Interest was lukewarm and fragmentary and support for necessary research gradually filtered away while efforts had to be directed into other channels of more conventional armaments. Personal contributions at the ozone conferences in Arosa, 1960, and Albuquerque, 1964, at the International Congress of the Aeronautical Sciences, Stockholm, 1962 and the Radiation Commission meeting in Leningrad in 1964 attempted to reflect indirectly on this view. The view became clearly erroneous when Crutzen began to develop the odd-N argument in the early seventies and any relevance in these and parallel contributions at the time is restricted to the observational data. These are not entirely irrelevant; measurement, for example, showing that the intensity of day hydroxyl airglow was equal to the nightglow intensity, indicates that removal of odd oxygen at the stratopause is controlled by the H, OH reactions and not $O + O_3$ which then enables one to calculate the height and temperature of the stratopause as a function of oxygen abundance and thus evolve an argument for the past variation of stratospheric temperature gradient, stability and total ozone.

In 1965, lacking support to enable continuance of studies related to the potential impact of nuclear weapons, I switched the work of my laboratory into the laser activity mentioned earlier and having about 60 staff available for the work it did not take long to get the breakthrough envisaged. Presentations on the potential impact of nuclear detonations on ozone were restricted to a one day presentation to US Defense Dept., AEC and University experts in Washington, a short presentation to staff of the US Arms Control Agency at the State Department in Washington and, as a member of the Committee on Atmospheric Environment of the AIAA and at the request of the chairman, a brief comment on potential atmospheric pollution intended for publication in the AIAA Journal but later withdrawn since the concensus view of the committee was that it was overly research oriented. I mention these only to indicate the comment in Nature did not arise out of a void and not to imply that anyone would derive benefit from reading the contributions and comments.

Family circumstances forced me to resign from the Canadian Civil Service post in November, 1969. Following a year

abroad, and at the suggestion of Canadian Government Officials, I accepted the offer of a professorship at Universite Laval, Quebec, financed by the government ...."To lead a research team studying atmospheric physics."

The promised support failed to materialise and after I had rehoused and reestablished my family in Quebec I was told it would not be forthcoming. The atmospheric research program I had initiated was to be terminated on the grounds that there was no longer a defence interest and passed to a university because the scientific merit remained. The notion that inhibition of thermonuclear conflict was not a relevant defence issue had never crossed my mind and I was utterly shattered by the misrepresentation and duplicity. Fear to undertake the necessary study is perhaps understandable because of public criticism if the notion proved illfounded but to lie in such a way as to eliminate the potential for future employment was outrageous and inexcusable.

So, apart from a little lecturing, my time at Laval had to be restricted to putting ideas down on paper and giving a few presentations at scientific meetings. I wrote a fairly lengthy report, around 600 pages, in fulfillment of contractual obligations which was rejected out of hand by those responsible for the contract and when I suggested it might be issued as a document from the Centre de Recherche sur les Atomes et les Molecules at Laval the reply came that this would be prevented. So copies of this draft are not available. In any case, being now ten years old, it is outdated, being overtaken for the greater part by more recent work. But a few of the ideas may still have merit and a brief description of them gives a framework for outlining later activities.

The report dealt with atmospheric chemistry, iterative interaction between chemistry and dynamics, approaches to interpretation of natural and anthropogenic perturbation and the spin off of suitable research into areas of defence, foreign policy and commercial exploitation. The chemistry had to involve the kind of interpolation I had attempted in appraisal of the photolysis of wet ozone, but in the much more complex context of added odd-N, and is of little current relevance. The applications concepts are hardly relevant to your expressed immediate interest. But there may be facets of the thoughts on iterative interaction between chemistry and dynamics coupled into interpretation of natural and artificial perturbation which are worth mentioning.

The feature that the temperature field deriving from the photochemical environment generates a circulation which modifies the photochemistry and circulation in an iterative interaction leading to a quasi steady state condition is obvious enough. But what is less obvious is the physical relationships which govern this interaction, the extent to which observations may present a misleading picture of the physical processes and a probable need for observations in an alternative format.

The description of diffusive mixing is the central difficulty. Does the eddy diffusion equation give an accurate

physical description? Is it being misused in modelling? The first place where I tried to consider iterative interaction was in an extension of Will Kellogg's ideas on mesospheric/upper stratospheric behaviour. This in an effort to think through an alternative interpretation of the overall meteorological influence of solar activity. (I am not sure we will ever be able to understand the potential effect of anthropogenic perturbation, or feel convinced of the validity of models, if we can not interpret the effect of such natural perturbation. And the study has a direct relevance to the interpretation of the possible impact of past nuclear tests.)

I make the assumption that diffusivity is a function of overturning and dependent on the vertical gradients of velocity and temperature. For want of a better expression, I use the eddy diffusion equation with the eddy diffusion constant a function of the above variables and from the iterative combination of chemistry and motion derive steady state values for temperature and composition as a function of latitude at solstice. The underlying concept is that the thermospheric O provides a reservoir of energy whose diffusion to lower altitudes modifies the temperature gradient and controls the eddy diffusion constant. The feedback in the system ensures diffusion is large in the high latitude winter and greatly increased by a suitable input from solar activity. Vertical transport then takes place in short term localised bursts. Strong instability in the lower thermosphere/mesosphere resulting from solar activity is a highly sporadic affair and there is no way in which it can be represented by averaged eddy diffusion coefficients based on mean, and in my view crude, observations of composition. One should attempt to provide a model where the details of temporal and spatial characteristics are the focus of interest. Overall behaviour can be very dependent on localised short term events.

This is my main quarrel with current activities. They tend to rely on averaging processes which smooth out the very functions which should be central to observation and theory. It is almost as though they were attempting to assess the probability of being struck by lightning by taking the average vertical gradient of the electric potential and using it to define the probability of producing lightning strokes.

There were several motivations behind the work. I thought it desirable to search for an explanation of Kulkarni's point that the biennial variations in stratospheric winds and ozone was related to solar activity, dying away around sunspot minimum. One can, of course, always derive an explanation by assuming enough feedback in the oversimple iterative interaction model I employed and it could be far removed from reality. Only more detailed observation will establish whether it makes sense. Its primary virtue, if it has any, lies in indicating the kind of observations which may be necessary in groping towards more accurate pictures of atmospheric behaviour. There is a secondary motivation; attempting to formulate a model for tropospheric meteorological change as a function of solar activity. This uses the same thought on the dependence of diffusivity on the vertical

but gradients of velocity and temperature for the high latitude winter stratosphere coupling into the mesosphere/lower thermosphere. One derives a pattern of spatial/temporal change consistent with observation and potentially capable of giving an explanation. But it would take too long to describe and I had better move on to other matters.

One can only hope to delve into the potential impact of past nuclear detonations on the stratosphere through use of past observations if one can eliminate the uncertainty in variations due to solar activity. And the possibility that such natural perturbation has many features in common with some kinds of perturbation through thermonuclear detonations suggests that study of the influence of solar activity could be of direct value in interpreting the potential effect of such detonations.

You might find it worth while discussing the issue with Kulkarni at Aspendale, particularly if you follow up the suggested alternative explanation for Pittock's observations.

If you want me to give further details justifying my belief that observational techniques and procedures currently in use are hopelessly inadequate, I would be happy to do so. In conditions where the impact of perturbation is dependent on local sporadic events, where the critical event may have a duration of hours, there is a need to move two orders of magnitude closer to global real-time observation.

The possibility of a stratospheric change through the increasing abundance of carbon dioxide seemed worth considering. The thought was that modification of the temperature gradient in the lower thermosphere/stratosphere could alter the vertical stability and modify diffusivity in the conditions of peak instability. Thoghts on the potential impact of injection of CFM played a lesser role in my activities at the time, partly because some years previously I had arranged a program investigating chlorine/ozone reactions in the chemistry department of the University of Montreal and these appeared to indicate such underlying complexities that I did not feel I could usefully contribute.

I felt that the general preoccupation with the potential impact of SSTs and CFM in the early seventies was misguided, diverting attention from the far more serious potential effect from extensive thermonuclear detonations. Shorn of any support for such a viewpoint, I thought it worthwhile submitting the note to Nature, in the hope, however slight, of changing this orientation. The degree of interest aroused in Canada can be gauged from the fact that the officials controlling my contract at Laval, in an immediate verbal communication upon hearing of the submission, insisted they must not be mentioned therein. They made it plain they would have stopped the submission if this had been possible. So I had to quit and spent the next two years in limbo, doing some teaching and putting the past aside. The teaching was humdrum, and at times unpleasant, so I judged I should make a series of attempts to have the issues

reopened in North America and the Soviet Union.

Through 1977-8, based in Toronto, (York University), and London, Canada, (University of Western Ontario), I made a series of contacts in Canada and the US in university and governmental, official and political, circles. Since the venture was a private one, funded by myself, there were limitations on the contacts available but through visiting a number of universities and research institutions and attendance at AGU meetings I was able to get an up to date picture of the state of the art of chemospheric research and discuss much of the theme of the early content of this letter. I presented a paper on my thoughts on the effect of solar activity, tied into the question of the potential impact of thermonuclear detonations, at the IAGA/IAMAP meeting in Seattle.

Privately, most scientists expressed concern and interest. Ralph Cicerone, U. of Mich., Chang at Livermore were among the 50 or so with whom I had some sort of discussion. But there seemed to be no way in which I could move those exercising political/financial muscle to move towards suitable action.

Simultaneously, I had been corresponding with Kondratyev in Leningrad, who I had known since 1965, to try to arrange similar contact and discussion in the USSR. In the summer of 1978 word came from the Soviet Union that I would be welcome to spend the months of September to December there, lecturing on the topic at the University of Leningrad and the Main Geophysical Observatory, which would give the opportunity for the exchange of views I sought.

There was substantial scientific interest but attempts to move towards governmental/political action, where the State Committee for Science and Technology under Kirillin was clearly the responsible agency, met with a cold shoulder. Nevertheless, the reception by concerned scientists was favourable enough to give hope of motivation for undertaking suitable research which might in the end lead to appropriate political action.

Kondratyev asked me to write a monograph on the subject a couple of weeks before I was due to leave. So, in some haste, I wrote a 250 page draft. There was no time to make a copy. Nor could I present adequate graphs and complete references in the time available and with the administrative difficulty of chasing up the references in the Academy library. I assumed a copy of my hand written draft would be sent to me after I had left the USSR. It was hardly practical to do the graphs and references without being able to look at the original text. When the translation into Russian was complete, I was asked for the accompanying figures and references. I replied that I would put these together as soon as I had a copy of the text I wrote. The copy was never sent; I can only assume the discourtesy is due to the stupid rigmarole of their security system. So I recommend to anyone who is visiting the USSR and could find themselves in a similar position to take a few sheets of

carbonpaper along as a necessary precaution.

After my return from the USSR, I paid a couple of visits to North America and made many attempts to present the views and obsevational data to which I had been exposed there.
This was accompanied by extensive correspondence, primarily with politicians of whom I have contacted about 50 of all shades of political opinion.

To little avail, as far as I can tell. The difficulty with politicians is that they never seem able to answer a direct question. And when it looks as though they are getting close to doing so they plead ignorance but never seem to wish to have that ignorance dispelled.

I wish you well in your task. My attempts to bring the issue into the open have been an unmitigated personal disaster. The result has been unemployment for many years and no prospects of ever changing the situation. You can imagine the attitude of prospective employers when they enquire what I have been doing. "Attempting to prevent a thermonuclear war or an equivalent disaster", is the kind of reply which takes them so far away from the frames of reference in which they live as to seem to leave no room for further question!

If you are able to carry out the task and the points I have been trying to raise can be shown to contain a germ of truth and commonsense, you will do me a great favour. It might even create a situation in which I could find a job.

Are there any jobs in Australia for errant Canadians? Particularly for one who would like to forestall nuclear disaster through failure to understand the implications of impact and who has the necessary contacts in the US and USSR to create a route to this end if only a single responsible politician would have the courage to give the support which would make this possible...

With best wishes,
Yours sincerely,

John Hampson

John Hampson.

References;-
1. K. Ya. Kondratyev and G. A. Nikolsky, Doklady Academia Nauk. 243, pp 607-610, 1978. (Geophysica.)
A more fulsome account, in English, was sent to a meeting in Toronto but not published. I have a rather battered copy which might be xeroxable if you are interested and can not get a copy.

2. Problemes Meteorologiques de la Stratosphere et de la Mesosphere, Presses Universitaires de France, 1966. Page 433, Figure 11.

I quote these two references because you may not have seen them and they contain the only relevant direct observations of NO_2. Much of U.S. work is well documented & so I refrain from citing it. In contrast, Russian comment has been brief and heavily circumscribed. So it might be of interest to add a Russian generalised view; namely, a recent book P.T.O.

of Kondratyev's, "Radiative Factors of the Contemporary Climatic Change" published in June of last year contains such information in Chapter II.

28 July.

I must apologise for delaying a reply to your letter. It's arrival coincided with visits from my children and offspring and so family reunion took priority. Many find my conclusions so bizarre that I refrained from action through fear that my comments could prove more harmful than beneficial. And the stress of financial insolvency brought about by my actions has had a severe effect on my wife. Very high blood pressure induced a minor stroke and it is essential to avoid creation of a state of tension which could be harmful to her.