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## Summary / Conclusions

1. The general weather regime between March 28 and April 5, 1979, across Eastern Pennsylvania was characterized by extreme inversion and stagnation conditions, rather untypical of springtime.

2. As a consequence, winds were very weak and variable most of the time, with TMI plumes and puffs drifting about slowly with very little dilution.

3. The most notable partial exception to this general flow picture was the sustained southerly flow on the afternoon and evening of March 28, carrying TMI plumes up to New York State and beyond.

4. Later that evening and night March 28/29, flow stagnation to the windward of the various Appalachian mountain ranges is evident from observations and from numerical model results (chapters 3 and 6). Respective TMI plumes will tend to be stalled across the general area of Cumberland County.

5. Details of the regional state of the atmosphere are very complex, as the advection of very warm air masses from the Gulf, progressing from higher levels down to the surface layer, occurs in individual surges timewise and spacewise.

6. The Appalachian ridges are influential in delaying the progress of the warm air masses towards the Northeast, deforming the pertinent warm fronts in a characteristic way.

7. Eastern Pennsylvania happens to be the meeting ground and "graveyard" of surface warm fronts from the southwest and cold fronts from northerly directions, making for additional complication of the flow picture.

8. Regarding local air currents in the vicinity of TMI out to 10 miles or so, the concept of the "dividing streamline" is used (chapter 4). This entails impaction of TMI plumes or puffs on some of the hills and ridges around TMI, and/or "fumigation" of such plumes or puffs down to particular hill sites at later times.

9. Plume impaction gives rise to very high concentrations of radioactive effluents at the affected hill sites (see chapter 7) and, therefore, very high dose rates. My estimate is that extreme effluent concentrations may be a factor of 100 to 1000 higher than concentrations computed from Woodard's Gaussian model, considering his accounting and averaging procedures. The factor quoted is, of course, for equal assumed release rates.

10. Concerning the apparent large discrepancy of absolute values of doses incurred between millirems claimed by the Defendants and hundreds of rems claimed by the Plaintiffs: It can be rationally explained by the abovementioned more realistic low meteorological dispersion factors, plus likely larger releases, plus proper consideration of the highly damaging beta-radiation.

11. Extreme effluent concentrations and respective doses have likely occurred many miles from TMI, at particular sites and times, contrary to the hypothesis of rapid fall-off with distance implied by the Defendants.

12. A review of boundary layer physics and model performance (chapter 5) shows that inherent uncertainty and variability do not allow the positioning of concentration maxima in space and time with any accuracy. In other words: Predicting when and where a concentration maximum will occur is not within reach of the most advanced dispersion models, much less of Woodard's Gaussian model. The best that models can do, if their physics is acceptable, is to predict the correct order of magnitude of concentration maxima, irrespective of their precise timing and location.

13. For the abovementioned fundamental reason alone, Woodard's "back-tracking" method for inferring the release rates from TLD data is not acceptable, even if the integration over space and time implicit in $\gamma$-dose computations mitigates the extreme variability and sensitivity found in plume or puff concentrations.

14. For the same reason, if for no other, computations of collective dose expressed as "person-rem" are invalid, based as they are on false claims of accurate knowledge of the amount and timing of releases, circumstances of release like wake effects, patterns of computed effluent concentration, and beta- and gamma-radiation damage to human tissue.

15. On top of my detailed critique of Woodard's dispersion model and proceedings, he is to be blamed for his complete failure to check the consistency of the various types of data available, and to comment on some large apparent discrepancies (see my chapter 1). This is an inexcusable offense against proper scientific methodology.

16. The practically complete lack of any meteorological analysis on the synoptic, regional or local scale in all official TMI reports up to now is striking and renders them invalid.

# Treatise on the TMI-2 Accident of March 28, 1979, particularly its meteorological aspects including transport and dispersion of the radionuclides released.

By I. Vergeiner, Ph. D.

## 1. Introduction

While trying to make myself familiar with the case in the course of the last few months, I have reluctantly learned to accept that we are dealing with a large puzzle, quite unlike the usual situation for a meteorologist, where he has an emitter (vent stack) with given source strength, his task being to predict or explain concentrations of the released gases or particles ("aerosols") in the surrounding air due to variable wind and weather conditions, either on a short-term (accident) or long-term basis ("normal" operation).

If the meteorologist is very conscientious, he may also try to answer the question: What happens to the released stuff (poison) in the long run? Where does it end up? How and where will it be deposited on surfaces or transported further away?

### 1.1 Releases

In the case of TMI, the first and most obvious missing pieces are the number and kind of relevant isotopes, and the quantities and timing of the respective releases in the course of the first days and weeks after March 28.

This grave deficiency is not brought out in the reports by Daniel and Woodard presented on behalf of GPU = General Public Utilities, owner of the Three Mile Island plant, hereafter called "Defendants". Neither is it clearly acknowledged in the various official TMI reports by the NRC, the President's Commission or EG &G Idaho company or Rockwell International (Hanford Operations).

In fact, by discussing the consequences of the TMI disaster solely in terms of maps of alleged dose (millirems) inflicted on the surrounding population, or, even worse, in terms of one final number = the alleged total collective dose (person-rem), they leave the impression of perfect knowledge and control of events. Nothing could be further from the truth: The confusion, shock and des-organization of the operators and authorities during the first day or so into the accident are well-documented.

"Measured effluent data" are hard to find between a critical iodine filter cartridge for March 28 being lost, continuous vent measurements of radioactive noble gases being installed only after April 22, plant vent monitors designed only for very low radiation levels going off-scale immediately and staying so for a number of days, and in-plant area radiation monitors, occasionally off-scale too, responding to so many local influence factors that even trying to derive only relative release rates from them appears to be burdened with too many assumptions.

It is probably fair to say that a strongly entrenched belief had existed all along that such a "beyond design-basis accident" just could not happen in practice. We may even take this state of mind as an excuse, albeit disquieting, for the pitiful lack of suitable environmental monitoring during the critical first days.

What is more vexing is that the nuclear establishment with its scores of research centers, contractors and industrial expertise all across the country and billions of dollars on hand, investigating the first major accident in a commercial nuclear power plant worldwide, needed five years to find out that more than half the reactor core had suffered a meltdown. They finally called it "crumbling" of the core. One cringes in disbelief: such a catastrophic core breakdown, driving core temperatures up to about 5000 ° Fahrenheit, remained undetected by the thousands of in-plant instruments?

Maybe, but shouldn't we at least expect that at this point, after finally admitting the core meltdown, a radically new evaluation of the course of events and the environmental consequences would be undertaken by GPU? Nothing of the sort happened, except repeated regurgitating of earlier reports and results.

Turning back from this necessary excursion, credit should be given to R. Webb for his clear and convincing arguments (treatise submitted June 19, 1993) to the effect that, since the geometric configuration of the fuel rods had been lost in the partial meltdown, the exact course of events was just too complex to be simulated by models. Furthermore, important reactor operational data needed for computing releases just were not available - or were not made available - like data on times and duration of opening or closing of various valves of the Reactor Coolant System, amounts of water injected into the system and so forth. Also: Were the waste gas decay tanks ever vented to the atmosphere deliberately? We don't know. Daniel and Woodard don't say, although they give the impression that there had been no venting, and everything was under control. In other documents internal ventings with some leaking out to the atmosphere are suggested (e. g. on March 30), and helicopters were apparently looking for the respective plumes. The whole issue remains essentially fuzzy and unresolved, however. Alternative release modes and pathways are accidental leaks or bypasses, steam release valves or improper handling when changing filters. Webb also points to calculations by Baskes (Sandia Labs), which appeared to strongly support his "blow-out" hypothesis, leading to a total of more than 100 million Curies released (from Xenons and Kryptons alone). He states that the "plain implications" of these calculations had not been pursued in the various official reports.

In summary, I remain persuaded that, contrary to Daniel and Woodard, we are a long ways away from knowing the truth on the amount and timing of the releases. This fact, by itself, renders their dose computations essentially worthless. As if this were not enough, a look at the core "inventory" shows that, in addition to the noble gases Xe and Kr being considered exclusively in all official reports, literally dozens of fission products were also present in enormous quantities of hundreds of millions of Curies, with short half-lives and intense beta- and gamma-activity. No attempt had been made to account for these highly damaging gases and tiny particles. or to show that they had all been retained 100 % within the building complex, which indeed is not credible.

Webb has shown that "blowouts " bypassing the filters likely occurred. And even filters just don't have 100.000 % efficiency for all kinds of fine and ultrafine particles.

What about other parts of the puzzle, some missing, some known? I defer the human and biological evidence towards the end, by no means implying that it is least important.

1.2 Meteorological data: Transport and Dispersion

The Defendants do make use of wind and temperature data from their own on-site meteorological tower, although they don't look at vertical profiles, just taking the wind at the 100 ft-level. Unbelievably, they did not collect (or at least they do not discuss or show) any outside piece of data: airport hourly meteorological data from Harrisburg, Middletown, Lancaster, Reading and many others, radiosonde wind and temperature profiles from Washington, Pittsburgh and Albany, routinely published synoptic weather maps and the like.

The hilly topography surrounding TMI is frighteningly complex: high enough to cause plumes from the TMI vent stack to occasionally impact on the nearby slopes, and certainly deflecting the local and regional airflow up to the crest height of the Allegheny mountain ranges, or up to the base of the extremely strong temperature inversion present during the decisive first days into the accident.

Winds dropped to far below normal, indicating plume meandering and possible stagnation and consequent dangerous accumulation of highly contaminated air parcels, hanging around and raising the times of exposure to the population. The Defendants do not show a single weather map, no trace of the local or regional mountain topography. There is no attempt to graphically illustrate at least one radioactive puff or plume travelling across the countryside, demonstrating its effects.

Even if, in our eyes, all the elements of their "trajectory calculations" are defective or highly questionable (the releases, the assumed wind field out to 50 miles turning instantly and "unisono" to the "rhythm" of the wind measured on the tower, the unscientific, unproved way of taking account of the topography) - they certainly must have believed their calculations, or else they would not have published the resulting millirems?

1.3 Measured dose rate readings (Geiger counter)

They ran up to some hundreds of mR/hr around the plant, and up to 1200 mR/hr at 600 ft above TMI-2 (were the helicopters in the plume?), quoted from the "Sequence of Events", Report of the Task Group on Health Physics and Dosimetry, exhibit H in: Defendants' Reply Brief in Support of their Motion for Summary Judgement. Surely these numbers are only a tiny selection from the mass of data and information available to the Defendants. They make no attempt to interpret even one of these values, much less to cross-check them with other types of data, although all the expensive expertise is theirs. True enough: due to the wind shifts and intermittent releases, plume locations and concentrations must have fluctuated quite a bit.

Ground survey teams apparently were

often in the wrong place at the wrong time.

But is this an excuse to discard the measurements altogether? And why were "no helicopter flights conducted during the evening of March 28 when major releases occurred"?

## 1.4 Airborne activity

Concentrations of Xe-133 in air apparently were not measured until eight days into the accident, when most of it was over. However, air samples of Iodine-131 are quoted in the previously mentioned Sequence of Events. The same comments apply: No interpretation, no cross-checking with other types of data. One specific example is presented here to drive home that point. Take two of the quoted values, converted to SI units:

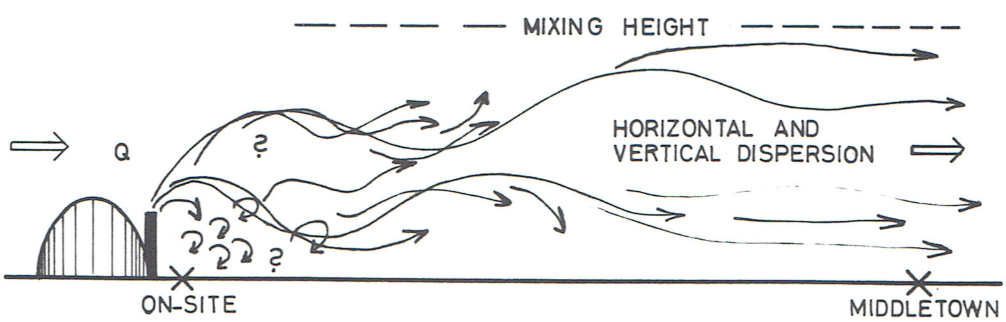
March 28:

2.27 p. m. (local time) 12 n Ci I-131/$m^3$ (maximum) at Middletown, roughly 5 km straight north from TMI

4 - 6 p. m.: On-site air samples indicate up to 200 n Ci/$m^3$ I-131, that is roughly 17 times the maximum concentration at Middletown.

These afternoon hours are characterized by fairly steady winds from the south at ≈ 5 m/s speed. They also coincide with a likely "blow-out" release according to R. Webb.

Fig. 1.1: *Schematic TMI plume on the afternoon of March 28. Elevated release or ground release?*



Thus, a fairly organized plume should have been established on average (see Fig. 1.1). Let us start at Middletown. With any of the more unstable Pasquill-Gifford classes and the mixing height (Washington radiosonde, Fig. 2.2) of ≈ 1200 m, the plume should have been close to well-mixed there, independent of the effective release height, such that the Gaussian equ. 5.1, notwithstanding its severe shortcomings, should yield a fairly robust relation between the release rate Q and the local concentration quoted above. The release rate comes out to be

$$Q = 30 \text{ m Ci I-131/s or}$$
$$= 30\,000 \ \mu \text{ Ci/s.}$$

Compare this with the nearest value quoted by Woodard (Table 5.1, Smoothed Iodine Release Rate Data used in Dose Assessments) of 22.7 μ Ci/s!

This is less than a thousandth part of our value, and his neighbouring values are even less. How did Woodard arrive at his numbers? One wonders how he obtained the excellent agreement between calculated versus measured concentrations of Iodine-131 in air (averages!) in his Fig. 5.3.

On-site iodine concentration measurements pose a further problem, albeit not nearly as aggravating as the above discrepancy in release rates by a factor of 1000. They would fit in rather well with an elevated release at, say, 60 or 70 m a.g. and stability class between B and C. A ground release suggested otherwise by cavity effects (see chapters 4 and 5) is difficult to argue here: Even assuming uniform mixing across a building wake of size 170 ft x 250 ft, on-site concentrations of I-131 should then have been ≈ 1600 nCi/m$^3$, whereas only 200 nCi/m$^3$ are quoted. Of course, it could be that releases had dropped by late afternoon, or that a very plausible stabilization of the lowest atmospheric layers would have cut off much of the iodine "signal".

Again, this example is intended not so much to give final answers as to solicit the kind of cross-checking that should have been done.

## 1.5 Radiological Environmental Monitoring Program (REMP)

Only some segments of this program are being mentioned here under separate titles, a comprehensive critique being beyond the scope of this treatise.

REMP, which comes in two versions, routine and expanded emergency REMP after March 29, included analyses of gross α, gross β, samples of water, precipitation, milk, fish and other food. Perhaps the spirit of REMP is best expressed by the resumé of an Interim Report on TMI offsite emergency REMP by Porter-Gertz Consultants Inc.:

"The resultant potential incremental radiological doses to the general population for all pathways considered were generally less than 4 millirem and as such would be less than the dose that would be received from a transcontinental plane flight."

REMP might be best characterized in satirical terms: How to go about searching for contaminations which one doesn't wish to find with a mass of expensive experts and gadgets?

## 1.6 TLD-data

TLD-readings from 20 fairly close-in sites are, notoriously enough by now, the only environmental radiological piece of information used by Woodard to "Back-calculate" releases of noble gases, thereby purporting to calibrate the Daniel source term as a basis for all his dose calculations. To the uninformed layman, Woodard's claim sounds convincing enough:

"During the periods of greatest releases from the plant during the accident, the winds were steady and were blowing directly over some of the TLDs. Therefore, the plume measurements made by those TLDs provide direct evidence of the gamma radiation dose present during the periods of significant TMI releases. Because it is also known that the greatest portion of these releases was xenon, the TLD measurements, as analyzed by the techniques discussed here, enable us to estimate the amount of radiactive material leaving the plant." (Woodard Affidavit, Jan 15, 1993).

In order to appreciate just how weird and unfounded this claim is, and how fallacious the method of relying on TLD-data must be, some fairly elementary considerations are sufficient. A thorough critique of TLD-performance and TLD-environmental monitoring philosophy will, I understand, be supplied by a number of scientists. I have, of course, made myself familiar with this issue by talking to colleagues in the field of radiation physics and radiation chemistry.

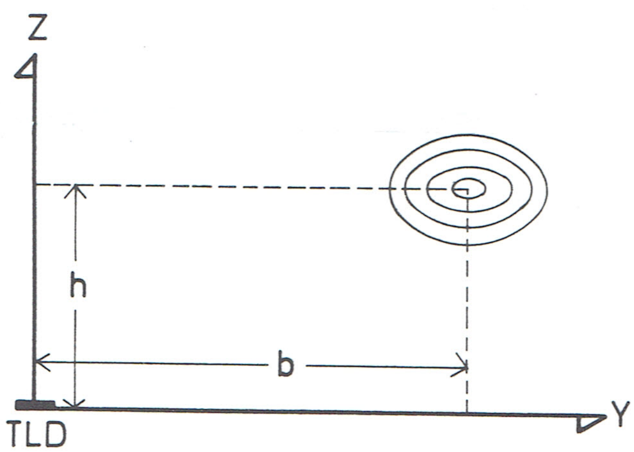
To demonstrate a main point, let us look at the standard basic law for a γ-radiating point source (see e. g. Slade, 1968):

$$\dot{D}_I = \gamma \cdot \frac{A}{r^2} e^{-\mu r} \cdot (1 + k\mu r) \tag{1.1}$$

$\dot{D}_I$ = dose rate at receptor (TLD)
A = source activity (Curie)
r = distance between source and receptor
μ = coefficient of attenuation
γ-constant, for example γ = 0,0142 $R \cdot m^2/(h \cdot Ci)$ for Xe - 133
(1 + kμr) "quick and dirty" approximation of "dose buildup factor" due to Compton scattering.

Using this law, I compute the dose rate received at a TLD on the ground from a sufficiently concentrated+) line source (plume) directly overhead and at various distances b sideways (see Fig. 1.2 looking along the plume) by integration.

Fig. 1.2: *TLD reading from elevated plumes*




A near-cosine angle of incidence reduction in sensitivity of the TLD has also been assumed.
I compare results for two very schematic "gammas":
"soft γ " = 100 keV: μ = 2 x $10^{-2} m^{-1}$, dose buildup not included because of TLD cutoff
"hard γ " = 1 MeV, μ= 0,80 x $10^{-2} m^{-1}$, dose build up included (k =1,3)

+) No finite-plume approximation, no frills!

To be specific, the following parameters are used for both gammas, the resulting dose rate D$_\gamma$ being proportional to $\frac{\gamma \cdot Q}{\bar{u} \cdot h}$ :

$\gamma$ for Xe - 133 as above

Plume source strength Q = 50 x $10^6$ Ci in 3 hr
Wind strength $\bar{u}$ = 5m/s
Height of plume h = 60 m.

In this highly schematic setup, the TLD would "see" the following signals D$_\gamma$:

| | b = 0 | 60 | 120 | 180 | 240 | 300 m | sideways |
|---|---|---|---|---|---|---|---|
| soft $\gamma$ | 106 | 32 | 4,70 | 0,60 | 0,08 | 0,02 | mR/hr |
| hard $\gamma$ | 451 | 207 | 63 | 23 | 10 | 4,6 | mR/hr |

This little table speaks for itself:
The sensitivity of the TLD reading to the sideways displacement of the plume is large for soft gammas, somewhat less for hard gammas.

This result, of course, couldn't be new:
Jan Beyea had already noted that TLD's as few as the 20 operational ones on March 28 could miss a concentrated plume altogether if it extended across a "window" between them, and that narrow plumes with dispersion so poor could be found in nature which were capable of delivering on the order of 50 rems at the point of impaction, assuming a noble gas release of "only" seven million Curies. Sensitivity may be less in other situations. Some more examples will be given in chapter 5.4.

This is only the beginning of a long story, however. The Defendants present a brief argument "Response of Teledyne Dosimeters to Xe-133", which tries to leave the impression that this TLD would more likely over-respond than under-respond, given the most probable plume geometries: Their study, however, tends to reinforce the point that the TLD response depends critically on the mix of isotopes (photon energies) present. In other words: You should know the $\gamma$-energy spectrum (which, of course, you don't) in order to evaluate the TLD.

Even worse, the above-quoted study remains in the range of several tens of millirems. Did the Teledynes really cover a dynamic range from tenths of millirems to, say, hundreds of rems? Or did they rather get saturated at, say, one Rad or lower, as would be the case for "high-sensitive", "environmental" dosimeters? This suspicion would be entirely in line with the fact that the TLD's as part of the routine REMP (Radiological Environmental Monitoring Program) were designed to measure low doses only in the course of routine operations, and with the apparent deeply rooted conviction that a catastrophic nuclear accident just couldn't happen, which, inspite of all the evidence to the contrary, even persisted right into the accident. Doses on the order of tens or hundreds of rems seem to have been unimaginable to anyone!

Accepting all the TLD's shortcomings, their readings should still have been higher, considering the lot of hard gammas present in the radioactive cloud, and the overwhelming human and biological evidence (see further down).*)
The low readings are suspect for more than one reason. Take, for example, the much-quoted measured TLD-dose at Middletown of 9,1 mrem (through April 28!), and compare it with a computed dose incurred in a mere three hours from Xe-133 alone, assuming that the receptor-TLD was submerged in the plume on the afternoon of March 28 (see the iodine check further up). That plume was fairly wide at ≈ 30 ° and close to well-mixed vertically. Accepting a source strength of 50 x $10^6$ Ci in 3 hours, i. e. Webb's "blowout", the local concentration C of Xe-133 at Middletown would be ≈ 2 x $10^{-3}$ Ci/$m^3$, which, applying the "gamma-submersion factor" for Xe-133 of 3,3 x $10^{-5}$ (mrem/hr)/(nCi/$m^3$), translates to a γ-dose of ≈ 200 mrem incurred in only three hours! This "gamma-submersion factor", by the way, is in perfect agreement with equ. (1.1) integrated over a half-space, with the concentration C constant everywhere

$$\dot{D}_I = 2\pi \cdot \gamma \cdot C \cdot \frac{1+k}{\mu} \qquad (1.2)$$

where k is around 5 for soft gammas.

The computed 200 mrems would, of course, be reduced by TLD cutoff for soft gammas, and by another factor of 25 if we chose to believe Daniel's release rates (which we don't). But on the other hand, not one of

*) There are hints that one or more dosimetres were dismissed as "outlanders" or as "erratic". Obviously, these must be checked.