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DRY DEPOSITION AND RESUSPENSION OF AEROSOL PARTICLES —
A NEW LOOK AT SOME OLD PROBLEMS*

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Some problem areas in the study of dry deposition and resuspension of aerosol particles are viewed from a different perspective. First some inferences are made about possible aerosol particle log-area distributions that could be found in the atmosphere and qualitatively about the roles of dry deposition and resuspension. The dry deposition process in each of five layers A through E (Aloft, Boundary, Canopy, Deposition, and Edaphic Layers) is considered separately and it appears that the overall process could be rate limited in any one (or more) of these layers. Two simple models for dry deposition to a smooth surface and to a canopy are presented. For the resuspension problem suggestions are made that a kinetic equation for the soil particle size distribution should be explored further, that the critical u_ required to move small particles may have an aerodynamic interpretation, that the soil particle size distribution dictates the saltation flux particle size distribution except for very large (~ 1 mm) particles, and that the convective diffusion equation is incapable of describing the suspended particle size distribution. A simple model is proposed for simultaneous deposition with some resuspension. The paper closes with an attempt to explain the physically meaningful variability of the resuspension factor.*

I. INTRODUCTION

This paper has caused me a great deal of anguish. The above is the third "official" title and I have tried many others. When George Sehmel sent out the call for papers I had a few ideas about atmospheric aerosols mostly derived from Junge's work and papers by Whitby et al. referenced later, some thoughts on dry deposition gained in part from conversations with Gifford, Lodge, and Vittori, and some very sketchy ideas about resuspension derived from seeing Sehmel's and Rosinski's results before they were published. So I sent George a broad enough title that I thought would cover what I wanted to say: "Atmospheric Aerosols: Sources, Concentrations and Sinks" and then set aside the month of August to write the paper. I see now I should have set aside a year or more! I have great empathy for Owen who five years ago wrote in his review paper⁽¹⁾ on similar topics: "all I seem to have achieved is a clearer understanding of why I am confused."

But a week ago I finally saw a way that I might at least hang all my random thoughts together. My plan is to take you on a tour, if you will, riding on an aerosol particle from the highest atmospheric levels, down to the ground as the particle dry deposits, and then back up again, as it's resuspended. From this perspective I will point out various aspects of the dry deposition and resuspension processes, problem areas where solutions are needed, and in a few cases some new ideas about how to approach the problems. I realize that by advertising this tour I am leaving myself an easy target for hard criticism, but does it really matter? Even if I do no more than arouse a storm of protest from more knowledgeable members of this group that my view is utterly erroneous, that too could be a contribution. Thereby the rest of us may better see which way to proceed in this difficult new field being addressed at this meeting. However I could do harm if someone who is even more a novice than I accepts what I say as valid. Therefore let me warn him (if there is anyone in this category!):

what follows is a tour guide; don't accept it as valid without taking the tour yourself!

II. ATMOSPHERIC AEROSOLS

First, look at the proposed vehicles and qualitatively at the roles of resuspension and dry deposition. For Figure 1 I have taken aerosol data from various references^(2 - 7) and tried to indicate different aerosol particle size distributions that might be found under different circumstances. The "combustion curves" are most uncertain. For particles larger than about 1 μm , dry deposition (including gravitational settling) is the dominant sink and resuspension can be considered as the dominant source especially if one includes "resuspension" of sea-salt and biogenic nuclei in this category, and forest fires, volcanoes, etc., are ignored. For smaller particles resuspension is possibly not too important but I don't think this has been firmly established. However dry deposition remains a strong contender with precipitation scavenging as the leading removal process for small particles and, on a climatologically averaged basis at most locations, dry deposition probably dominates for low level aerosol sources.⁽⁸⁾

Incidentally, Figure 1 illustrates one of the advantages of plotting the log-area distribution over, say, the number distribution: namely, thereby some of the "structure" of the size "spectrum" is revealed. A number distribution plot of the same data would differ little from the Junge distribution $\Delta N / \Delta \ln D \sim D^{-\beta}$, with β near 3. Other advantages are that it is "close" (although the log-length distribution would be "closer") to the diffusional attachment spectrum^(2,9,10) (i.e., it suggests to which particles a trace noxious pollutant would become attached by Brownian diffusion) and, for particles larger than about 1 μm , it gives a good indication of which aerosols significantly scatter visible electromagnetic radiation.^(11,12)

Although the primary thrust of this paper will be to inquire about some of the unknowns of dry deposition and resuspension as they relate

to atmospheric aerosol budgets, nevertheless I would like to comment here on the opposite viewpoint; namely, that studies of atmospheric aerosol budgets can help us understand dry deposition and resuspension. Although this viewpoint is by no means novel, especially in the use of sea-salt nuclei^(13 - 16) yet still much could be learned from such studies. For example, from Figure 2 and ignoring many complications, simple reasoning can yield a dry deposition velocity for the large particles of about 10 cm sec^{-1} , and for the Aitken nuclei, about 1 cm sec^{-1} . As opposed to many measurements similar to the above of aerosol concentrations in Eulerian frames fixed with respect to the earth's surface, there are few measurements in Lagrangian frames moving with an air mass. It seems to me that such measurements could yield substantial information about the sources and sinks of atmospheric aerosols but I am ill prepared to comment on the experimental difficulties of such undertakings. Instead I plan to emphasize various theoretical problem areas. This is not to suggest that I am qualified to address theoretical problems but it does appear that certain topics might profitably be examined from a novice's viewpoint.

III. DRY DEPOSITION

Understanding dry deposition is a difficult task if for no other reason than the many orders of magnitude of length scale involved. This is indicated in Figure 3 where I have also shown my own preference for identifying various layers, A through E. As Vittori emphasizes,⁽¹⁹⁾ the significance of identifying various layers is that in each, a smaller set of variables is sufficient. Often, when first approaching a complicated problem such as this one, it is helpful to try to identify the rate-limiting stage of the overall process. As I've indicated in the right hand column of Figure 3 and in more detail in Figure 4, it seems that the dry deposition process could be rate-limited in any of the layers, depending on the height of the release, meteorological conditions, details of the canopy, properties of the pollutant and characteristics of the sink. Consequently, as indicated in Figure 4, there seem to be plenty enough problems to go around: for meteorologists, aerosol physicists

and for a group which might be called ecologists among whom should be chemists, botanists and soil scientists! In the sequel I plan to indicate what I see to be the major problems in each of the layers A through E as we, so to speak, ride with the aerosol particle to the ground. Also, in places I will indicate ideas which may help toward solving some of these problems.

III.A Diffusion and Trajectories Aloft

If anyone should study Bauer's recent log-log plots⁽²⁰⁾ of σ_y and σ_z as a function of travel time he will appreciate the very great difficulties in this layer. I am prepared to contribute nothing to solving the problems except possibly the suggestion that we change our coordinate systems. That is, the atmosphere doesn't "recognize" our imposition of a Cartesian coordinate system; instead I believe we should follow Danielson's^(21,22) emphasis of the use of isentropic surfaces and discuss the diffusion of tracers in these surfaces as a function of wind shear, and perpendicular to them, as a function of potential temperature gradient. Indeed it seems that in the plots of $\sigma_z(t)$ for z above the boundary layer, we have made essentially no progress in accounting for atmospheric stability. Further I'd like to note my opinion of the questionable value of utilizing any diffusivity for the calculation of the subsequent motion of a specific release. After a few days we become limited by an "uncertainty principle" not arising from diffusion, but arising from our uncertainty about the state of the atmosphere at the time of the release. If you will, the noise of the initial data begins to dictate the diffusion of the pollutant.

III.B Diffusion in and Deposition from the Boundary Layer

Figure 5 illustrates some of the problem areas in the boundary layer. Elsewhere I've commented some on the particle-break away problem⁽²³⁾ basically concluding that it is unsolved; on the problem of buoyant plume rise⁽²⁴⁾ suggesting that our present theories which ignore drag and heat transfer are inadequate; and on diffusion in shear flow⁽²⁵⁾ which soon becomes an overwhelming analytical problem. All that I'd like to do here is make two brief comments on the significance of accounting for the vertical

variation of the diffusivity, and on the problem of the boundary conditions at the earth's surface for the convective diffusion equation. These comments do not necessarily presuppose that using the convective diffusion in the boundary layer is accurate, only that it appears to be the best (and easiest) method available.

The first point can be seen from the steady state version of the convective diffusion in which the y dependence is here ignored

$$\bar{u}(z) \frac{\partial X}{\partial x} = \frac{\partial}{\partial z} \left[K(z) \frac{\partial X}{\partial z} + v_s x \right] \quad (1)$$

where v_s is the (positive down) settling velocity. If the differentiation on the rhs of (1) is carried out, then an effective settling speed can be identified as $v_s + \partial K / \partial z$. Thus if the diffusivity were treated as a constant, then we would be ignoring a term in the "settling speed" $\partial K / \partial z$. For example, if $K = k u_* z$ where k is von Karman's constant and u_* is the friction velocity, then by treating K as a constant we would be ignoring the value (positive = down) $k u_*$ which, for $u_* = 50 \text{ cm sec}^{-1}$, has the possibly significant value of 20 cm sec^{-1} . Physically, of course, this term reflects that there is a greater capacity for diffusion and therefore greater flux from regions where the diffusivity is larger. Whether or not this term (and indeed the settling term) is significant depends on the stability. Since these settling terms can cause substantial analytical difficulties it is worthwhile to check to see if the displacement $(v_s^{\text{eff}})t$ is large or small with respect to $(Kt)^{1/2}$. For stable conditions it can be significant and usually $\partial K / \partial z$ can not be ignored. For unstable conditions, however, as Deardorff⁽²⁶⁾ describes, there are very serious limitations of the convective diffusion equation itself. Probably we would be better off to follow Toba's lead⁽¹⁴⁾ and consider counterflowing mixing motions, in an attempt to account for thermals and the associated downdrafts. In this regard Figure 6 is interesting data obtained by Horst and Powell⁽²⁷⁾ which shows that even in neutral conditions the probability distribution of vertical gusts is not symmetric about $w = 0$.

The other point I wish to make here concerns the surface boundary condition to be used for the convective diffusion equation. This is a topic that Calder⁽²⁹⁾ has discussed at some length and Smith⁽³⁰⁾ has used Calder's formulation and indicated the resulting solution to the equation in a number of cases. Calder suggests that in the general case the boundary condition should be the "radiation" boundary condition familiar in thermal physics

$$\lim_{z \rightarrow 0} \left[K \frac{\partial x}{\partial z} + v_s x \right] = v_d x(x, y, z \rightarrow 0) \quad (2)$$

There are two major practical limitations of this formulation however. First, as it appears in (2) the deposition velocity, v_d , is no more than a parameter which ranges from zero (no flux and $v_s = 0$) to infinity (finite flux to a perfect sink i.e., with $x(z \rightarrow 0) = 0$). Thus studies separate from boundary layer meteorology are needed to specify v_d and indeed a major thrust of this paper will be to try to derive one. But of more significance is the quite formidable task of solving (1) subject to the boundary condition (2). This can be attested to by referring to Smith's paper and noting that even there the solutions are for simple $K(z)$ profiles and wind speeds independent of height. In view of this, in view of our inadequate knowledge of the diffusivities and further, in view of the questionable validity of the convective diffusion equation near the ground, it seems to me to be acceptable at the present time simply to obtain the deposition flux by taking the air concentration to be given, for example, by a Gaussian plume model for a nondepositing tracer and by multiplying this (empirical) expression by the deposition velocity. This method will overestimate both the flux and the air concentration (since it is not depleted) and therefore will lead from hazards analysis to conservative (safer) designs.

III.D The Deposition Layer

Although it would follow more logically to address problems in the canopy layer now, I'd like first to discuss problems in the deposition layer, not necessarily because they are simpler but because I'd like to have the results available before discussing deposition in a canopy. By the deposition layer I really mean the region above a smooth surface and in particular I'd like here to address the problem of deposition to a smooth surface in a wind tunnel. It might be thought that this problem is fairly well in hand with the predictive formulae available^(31 - 36) but instead I suggest that these formulae are mostly just empirical fits to data. All rely on the use of the convective diffusion equation (right next to the wall where large gradients and where diffusion step sizes of the order of the height above the wall would be expected to blatantly violate the "conditions" of its applicability!) and on the assumption of constant flux. Then, to yield predictive formulae, various assumptions are introduced for the "free flight distance" and/or for the variation of the "particle diffusivity" with height. In what follows I'd like to look at the problem in a different way. Ultimately it too leads to an empirical fit of the data but I did this because of lack of time to pursue it fully. Perhaps if the physical picture is right, future studies can lead to a complete theoretical analysis.

I credit the physical picture of this model to Joukowski's description of turbulence as described in Goncharov's book⁽³⁷⁾ and to Owen's admirable review paper.⁽¹⁾ Figure 7 schematically illustrates the picture of eddies at the wall and Goncharov quotes Joukowski as follows: "Vortex flow curls itself up into separate elliptical eddies . . . The centers of these elliptical eddies move horizontally at velocities which increase with the distance from the channel bottom . . . The elliptic filaments advance like a caterpillar tread." Consequently, as Owen suggests, it is probably realistic to assume not that the particles diffuse across the final viscous sublayer, but are convected across, basically by the advancing tread of Joukowski's caterpillar. Owen

goes on to show a resultant deposition velocity $v_d = R u_* \sigma^2$ where R is a constant ($R = 2.8 \times 10^{-4}$ for his fit to the data) and $\sigma = u_*^2 \tau / \nu$ is the Stokes number of the particle (τ is the particle relaxation time) based on the time scale for the viscous flow ν / u_*^2 , but he defers the theoretical development of the result to a later publication which I have not been able to find and which presumably is not yet published.

From this physical picture of convection of the particles across the viscous sublayer by the viscous "jet" at the leading side of Joukowski's caterpillar tread, an expression for the deposition velocity can be obtained from reasoning as trivial as the following. At the top of the tread, the pollutant's concentration is essentially a constant, say x_0 and it is convected down at a velocity proportional to the friction velocity, αu_* , where α is expected to be $O(1)$. Therefore the total flux down is $f \alpha u_* x_0$, where f is the fraction of the total surface area at which these viscous jets impinge. But of this total material in these jets only a certain fraction will be collected by the plate, depending on a collection efficiency E_j which is a function of particle size as well as possibly other parameters (electrical, phoretic effects, etc.). To determine E_j we could start, for example, with Fuchs' discussion⁽³⁸⁾ of the collection efficiency for a jet impinging on a flat plate but for now I propose to avoid this, leave the collection efficiency unspecified and write the flux to the surface as $f E_j \alpha u_* x_0$. As for the fraction f of the total surface at which jets are impinging, it is noted that with these jets there is carried a flux of x-momentum which by similar reasoning is $\beta f \alpha u_* \rho \bar{u}$ where β is another unknown constant near unity. This concept obviously closely follows Chamberlain's application of Reynold's analogy.^(39,40) But this flux is also the turbulent stress $\tau \equiv \rho u_*^2$ and therefore $f = u_* / \alpha \beta \bar{u}$. Consequently the deposition velocity for particles (dividing the flux by x_0) is

$$v_d = v_s + \frac{1}{\beta} \frac{u_*^2}{\bar{u}} E_j \quad (3)$$

in which I've added the settling velocity, a term independent of turbulence.

Rather than seek a theoretical prediction for the jet collection efficiency at this time, I have chosen to estimate it from available deposition velocity data and from approximate solutions to similar problems, especially those with which I am familiar from precipitation scavenging studies.^(41,42) For the inertial term in the collection efficiency I have used the deposition velocity data obtained by Liu and Agarawal,⁽⁴³⁾ see Figures 8 and 9. Interestingly but of no obvious significance, a choice of $\beta = k$, von Karman's constant, is convenient. Further, it is encouraging that, as seen in Figure 8, the collection efficiency for other viscous flows⁽³⁸⁾ is not too dissimilar from the present result, and that, as seen in Figure 9, the predicted critical Stokes numbers, smaller than which there is no collection for potential jet flow⁽³⁸⁾ and for viscous flow about a sphere⁽³⁸⁾ is not poorly reflected by the proposed approximate empirical fit through Liu and Agarawal's data. For the contribution to E_j from Brownian diffusion of the particles I propose at present to use Chamberlain's expression⁽³⁹⁾ modified by the Schmidt number dependence deduced for flow in the neighborhood of a stagnation point.⁽⁴⁴⁾ Finally, ignoring electrical effects and thermophoresis, but adding a diffusiophoretic deposition velocity for the case of water vapor evaporation or condensation which can either be written as in Goldsmith or May⁽⁴⁵⁾ or more conveniently as given by Horst⁽⁴⁶⁾, then the proposed approximate expression for the deposition velocity is

$$v_d = v_s + 10^3 \text{ cm sec}^{-1} \left(\frac{\dot{m}''}{1 \text{ g cm}^{-2} \text{ sec}^{-1}} \right) + \frac{u_*^2}{\beta \bar{u}} \left[10^{-3/St} + \frac{\beta}{\gamma} Sc^{-0.6} \right] \quad (4)$$

where $\dot{m}''$ is the water vapor flux (+ve if condensation), St is the Stokes number $\tau u_*^2/\nu$ in which τ is the particle's relaxation time and ν is the coefficient of kinematic viscosity of air, $Sc = \nu/D$ is the Schmidt number and D is the particle's diffusion coefficient.

In Figure 10, (4) is plotted for $\beta = \gamma = 0.4$ and for different values of u_* and $\dot{m}^0$, the value of $\dot{m}^0 = 1 \text{ mm hr}^{-1}$ (or $\dot{m}^0 = 0.1 \text{ g cm}^{-2} \text{ hr}^{-1}$) being a not unreasonably high value for wet surfaces on warm days at moderate humidities. From Figure 10 it can be noticed that diffusiphoresis can significantly influence dry deposition (whereas, unless there is no heat transfer from the surface through supporting structures, thermophoresis is expected to be negligible⁽⁴⁷⁾). In Figure 10 are plotted some data points obtained by Sehmel⁽⁴⁸⁾ and by Clough⁽⁴⁹⁾ and the comparison is encouraging. Because of this I believe the above qualitative discussion could profitably be re-examined with greater rigor, although for practical problems one should keep in mind that usually the particle-size distribution is not a stagnant variable.

III.C Deposition in a Canopy

Returning now to the problems of dry deposition in a canopy, it seems reasonable that it is necessary to include the filtration effect of the canopy elements. Lodge⁽⁵⁰⁾ calls the earth's surface "the world's largest, crummiest fiber filter" which I think well emphasizes the point. Since I have not seen any other models of deposition in a canopy, I propose to start from as crude a model of the fluxes as is shown in Figure 11. Basically I again envision Joukowski's caterpillar tread which at the front end "dumps" the flux $\propto u_* x_0$ into the canopy and, at the back (although I've drawn it on the right) "picks up" $\propto u_* x_c$. A flux $v_s x_0$ flows to the ground independent of the caterpillar. Now I assume that in addition to the jet deposition there is a deposition which depends on the filtration efficiency of the canopy, symbolized by the efficiency per unit length, E_c' , and on the mean wind in the canopy, $\bar{u}_c$. The problem then is to solve for the concentration in the canopy, x_c , and eliminate it to find the deposition velocity.

To do this, consider the volume element of the canopy of length δx , height H and width W shown in Figure 12. If for the moment the flux $v_s x_0$ is ignored, a continuity equation can obviously be written as

$$\begin{aligned}
& \frac{\partial}{\partial t} \left[x_c H W \delta x \right] \delta t + \frac{\partial}{\partial x} \left[x_c H W \bar{u}_c \delta t \right] \delta x \\
& = \alpha u_* x_0 W \delta x \delta t - \frac{u_*^2}{\bar{u}_g} \frac{E_j}{\beta} x_0 W \delta x \delta t \\
& - E_c' \delta x H W \bar{u}_c \delta t x_c - \alpha u_* x_c W \delta x \delta t
\end{aligned} \tag{5}$$

for which it is assumed that turbulence in the canopy is sufficient to mix x_c uniformly. For steady-state and x -independent conditions (5) yields an expression for x_c . Then, upon defining v_d as the net flux $\alpha u_* x_0 - \alpha u_* x_c$ divided by x_0 , the result is

$$v_d = v_s + \frac{\epsilon}{1 + \epsilon} \alpha u_* + \frac{1}{1 + \epsilon} \frac{u_*^2}{\bar{u}_g} \frac{E_j}{\beta} \tag{6}$$

where again I have added v_s and in which ϵ depends on the canopy's filtration efficiency per unit length, E_c' and height, H , via

$$\epsilon = \frac{H E_c' \bar{u}_c}{\alpha u_*} \tag{7}$$

where $\bar{u}_c$ is the mean wind in the canopy which quite possibly is linearly related to u_* .

I think equation (6) is an interesting result. If $\epsilon = 0$, then (6) reduces to the result (3) for deposition to a smooth surface; ϵ is small for short canopies ($H \rightarrow 0$), poor filtration efficiency ($E_c' \rightarrow 0$) or $\bar{u}_c \rightarrow 0$ which might be interpreted to indicate low penetrability — that much seems reasonable. On the other hand, for a very efficient canopy filter, $\epsilon \rightarrow \infty$ and (6) becomes

$$v_d = v_s + \alpha u_* \tag{8}$$

which for particles smaller than about $10 \mu m$ (depending on the magnitude

of the unknown parameter α) is essentially independent of particle size. This seems to be a reasonable result, too, if one envisions a canopy so efficient (eg., so deep) that no matter what size of particle enters, eventually it will be collected by the vegetation. Perhaps an application of this result will explain Semonin's observations⁽⁵¹⁾ that the concentration of sea-salt nuclei inland from Hilo, Hawaii, fell at the same rate regardless of particle size.

At this time I do not propose to address the problem of relating ϵ and especially E_c' to measurable properties of the canopy (biomass, vegetation type, etc.). However if someone is so inclined I would refer him to Davies' recent book on air filtration⁽⁵²⁾ as a good starting point.

III.E The Edaphic or Edacious Layer

Then that brings us to the edaphic layer and essentially the problem of resuspension. However before turning to that I would like to make a couple of comments. The problem here is to understand the rate at which contaminants are transported into the depths of the soil. The movement of particles, whether soluble or insoluble, seems to be pretty well dictated by the movement of soil moisture. This in itself is an extremely difficult problem to solve and about which much has been written by soil scientists (eg., see references 53-55 and references therein). The case of sorption of gases by soils and by vegetation is even more difficult because this can proceed with or without water. Here the interested researcher should consult Hills' admirable papers⁽⁵⁶⁻⁵⁸⁾ as well as studies by soil scientists both experimental (eg., 59 and references therein) and theoretical (eg., 60 and references therein).

IV. RESUSPENSION

Now I'd like to comment on the resuspension problem, starting from the ground and working up, but first I'd like to restrict the

scope. As you well know, the entire problem of resuspension is extremely complex. I've tried to organize the facets of the problem in Figure 13 but then gave up labelling the diagram because it became too complex! The few things I would like to say about the problem are generally associated with the thick solid lines to the left of the figure. Even then I will essentially completely ignore the important problems of the movement of aerosols in the soil,^(53 - 55, 61 - 63) of determining the size distribution of specific spills, eg., plutonium^(64,65), and of predicting the decrease in the surface concentration with time. (However I would like to note my skepticism that it's an exponential process, eg., see reference 66; a "power law" may fit the data well and better reflect the physics if the physics is just an increase in contaminated volume with time.) Thus I plan to ignore many of the problems of interest at this meeting and instead concentrate on questions associated with atmospheric aerosols such as: What is the soil particle size distribution? Is it reflected in the airborne size distribution? How is the airborne concentration influenced by soil and meteorological conditions? What is the vertical distribution of resuspended aerosol particles? And so on.

IV.A Aerosol Particle Kinetic Equation

As a first step toward considering the size distribution of the resuspended aerosol, it is reasonable to consider the soil particle size distribution. Some data obtained by Bagnold⁽⁶⁷⁾ and by Chepil⁽⁶⁸⁾ is shown in the log-probability plot of Figure 14. As is well known (eg., see Fuchs⁽³⁸⁾) this type of graph paper is constructed so that if the variate is log-normally distributed then it will graph as a straight line. It is seen in Figure 14 that the log-normal distribution leaves quite a bit to be desired and especially Chepil's data suggests that the particles agglomerate, reducing the concentration of smaller particles from that expected for a log-normally distributed variable. Later at this meeting we will hear more about this very important aspect of the problem from Rosinski et al.,⁽⁶⁹⁾ to whom I am indebted

for calling my attention to the phenomenon a few years ago during personal communications.

What I would like to do here is comment on the theoretical aspect of the problem. As is well known, Kolmogoroff⁽⁷⁰⁾ predicted that particles subjected to breakage into smaller sizes would be distributed log normally. I find his theoretical development rather difficult to follow but the result seems reasonable if one qualitatively examines the problem as follows. Let the random number m_1 be the mass of a specific particle after the first breakage. Then $m_1 = b m_0$ where m_0 is the mass of the original particle and b (for "breakage") is a random number between 0 and 1 which gives (one of) the fractions into which the original particle is broken. Similarly, assuming b is independent of the size of the particle, $m_2 = b b m_0$ and so on, so that after n breakages, there are 2^n particles and the mass of any one of them is $m_n = b^n m_0$ or $\ln m_n = n \ln b + \ln m_0$. Now implore the central limit theorem which gives us that for n large enough and for only weak conditions on the probability density function for b , then $\ln m_n$ will have a normal distribution. Admittedly the above analysis needs "cleaning up" but I don't propose to do it here; I think the essence of it is right and that's all I want for now. Since in fact the data shows that the size distribution of soil particle is not log normally distributed, I'd like to suggest a different way to proceed.

The proposal is to obtain a kinetic equation for the particle size distribution. For example, let $f(m,t) dm$ be the number of particles ("m-particles") per unit volume if desired, at time t whose mass is from m to $m + dm$. This number changes during δt by losing some through their breakage and via attachment to other particles, and by gaining some, through the breakage of larger particles and the coagulation of smaller particles. Let the number of m-particles lost by breakage during δt be $b \delta t f(m,t) dm$ where b is the breakage rate, and by attachment be $\bar{a} \delta t f(m,t) dm$ where $\bar{a}$ is an attachment

rate, averaged over all other particles present. The number gained during δt from breakage of particles of mass $M(>m)$ is $P(M \rightarrow m) b \delta f(M,t) dM$, where $P(M \rightarrow m)$ is the probability that upon breaking a particle of mass M , one of the pieces will have mass m . The gain via coagulation of smaller particles is familiar from the similar problem in cloud physics.⁽⁷¹⁾ From the above one can write down a general kinetic equation but there really isn't much point in doing it since the resulting nonlinear integro-differential type of equation is notoriously difficult to solve. What I'd like to do here is drastically simplify the problem and see where it leads. However, I do hope that others will consider this problem further.

Then consider the simple case (which I have never seen anyone study before) of only breakage and no coagulation. The resulting kinetic equation is obviously

$$\frac{\partial f}{\partial t} dm = - b f dm + \int_m^\infty dM P(M \rightarrow m) f(M,t) \quad (9)$$

Now $P(M \rightarrow m)$ must be specified and many options are available. A realistic one should probably contain the idea that the smaller the particle, the more difficult it is to break. If all particles were broken exactly in half then I think that would be described by $P(M \rightarrow m) = 2 \delta(m - M/2) dm$, the factor 2 arising because there are two pieces. Here I'd like to look a little at the case when the large particle is broken into two pieces of random size, subject only to the constraint of mass conservation. I have concluded that this gives $P(M \rightarrow m) = 2 dm/M$. The general solution to the resulting equation is rather unwieldy. I get

$$f(m,t) = e^{-bt} \int_0^\infty d\lambda w(\lambda) e^{b\lambda t/m^{2\lambda}} \quad (10)$$

where there is a continuum of eigenvalues λ , with weighting function $w(\lambda)$. I have had nowhere enough time to explore this solution fully, but let me at least mention the steady state solution

$$f(m) = c_1/m^2 \quad (11)$$

This is especially interesting if instead of using $f(m) dm$, the number of particles with mass m to $m + dm$, we use $f(r) dr$, the number of particles with radius r to $r + dr$. Since $f(m) dm = f(r) dr$ then (11) gives

$$f(r) = c_2/r^4 \quad (12)$$

which is the Junge distribution!

However interesting this result is I don't think it should be taken too seriously without exploring the full equation further. That is, what I think should be taken seriously are attempts to put the most reasonable physics into the full kinetic equation describing both attachment and breakage, and then as best one can, solve the resulting equation to see if this method can explain the observed data shown at the start of the section. This could be a life-time's task (not a week's!) so I plan to quit here and move on to the next phase: getting the soil particles in motion.

IV.B Beginning Motion of Individual Particles

You are familiar with Bagnold's ^(67,72) results reproduced in Figure 15 that demonstrate the minimum u_* required to move monodisperse aerosols along a flat surface. Bagnold ⁽⁶⁷⁾ and others ⁽⁷³⁻⁷⁵⁾ have seemingly satisfactorily explained the large-particle part of this curve but all I've found about the small-particle part ($\lesssim 100 \mu m$) are suggestions (eg., see Fuchs ⁽³⁸⁾) that intermolecular forces begin to become important. What I'd like to suggest here is that perhaps the full curve can be explained by simple aerodynamic arguments.

First for the large particles (without going into a lot of detail about torques, lifts, the Magnus effect and so) basically, to move

a particle, the aerodynamic force $(1/2) \rho_a \bar{u}^2 C_D (\pi d^2/4)$, where C_D is the drag coefficient, must overcome the gravitational force, $f (\pi/6) \rho_p g d^3$ where f is a fudge factor. If $\bar{u}$ is taken to be the familiar log profile, i.e., $\bar{u} = u_* [2.4 \ln(z u_*/\nu) + 5.84]$ evaluated at $z = d/2$ then one gets the result that u_* varies as $d^{-1/2}$. Elsewhere⁽⁷⁶⁾ I put some reasonable numerical values in this and essentially get the expression shown in the figure which fits the data:
 $u_* = 30 \text{ cm sec}^{-1} (d/400 \text{ } \mu\text{m})^{1/2}$, using $f = 1.14$ and $\rho_p = 2.6 \text{ g cm}^{-3}$.

Now for the small particles. But first, what is meant by "large" versus "small" particles? In the above it was assumed that the air velocity in the neighborhood of the particle was given by the log profile which essentially reduced to $\bar{u} = 5.84 u_*$. But for a very small particle buried in the viscous sublayer (beneath the tread of Joukowski's tractor?), the velocity there would be less than u_* . Therefore it seems that a distinction should be made between large and small particles when the particle diameter is the size of the viscous sublayer $\delta \sim \nu/u_*$. Interestingly, for $\nu = 0.15 \text{ cm}^2 \text{ sec}^{-1}$ and $u_* = 15 \text{ cm sec}^{-1}$ then $\delta \sim 10^{-2} \text{ cm} = 100 \text{ } \mu\text{m}$ and this essentially is at the minimum of Bagnold's curve.

For particles of diameter $d < \delta$, it would seem to be more appropriate to characterize the air velocity experienced by the particle as $(d/\delta) u_* = (d/\nu) u_*^2$. Then equating the two forces (or torques) as for the case of a large particle, we get

$$\frac{1}{2} \rho \left[\frac{d}{\nu} u_*^2 \right]^2 C_D \frac{\pi d^2}{4} \sim \frac{\pi}{6} \rho_p d^3 g \quad (13)$$

and therefore the required u_* to dislodge a particle with $d < \delta \sim 100 \text{ } \mu\text{m}$ varies as $d^{-1/4}$. Again, upon substituting reasonable numerical values into (13) I get close to the curve shown "fit" to Bagnold's data⁽⁷⁶⁾

$$u_* = 28 \frac{\text{cm}}{\text{sec}} \left(\frac{10 \text{ } \mu\text{m}}{d} \right)^{1/4} \quad (14)$$

IV.C Collective Motion of Polydisperse Aerosols

But in the real world with polydisperse aerosols, as Bagnold emphasizes,^(67,72) once a few particles are in motion an avalanche or sandblast effect occurs, moving many. The reason for this is simply that the momentum of an airborne particle is of the order of 10^3 times larger than the equivalent volume of air. Elsewhere⁽⁷⁶⁾ I asked the question: how many particles are thereby dislodged? and I'd like to sketch the answer here. Suppose we take as suggested by Lumley and Panofsky⁽⁷⁷⁾ that the probability distribution of the horizontal wind gusts are Gaussian about the mean value $\bar{u}$ (eg., as given by the log profile) and with variance u_*^2/k^2 , where k is von Karman's constant. From the previous section we have that a "large" particle will be dislodged if $|u| \geq G^{1/2} r^{1/2}$ where $G = (2/3) f(\rho_p/\rho_a) g$ and $f = 1.14$. If the contribution to the motion of large particles arising from negative values of u can be ignored, then the probability that a large particle will be moved is the probability that $|u| \geq G^{1/2} r^{1/2}$. Integrating over the Gaussian distribution of u gives $1/2 \operatorname{erfc}[\{(Gr)^{1/2} - \bar{u}(r)\}/\{2^{1/2} u_*/k\}]$. This is plotted in Figure 16 with arbitrary normalization along with some data.

The conclusion is that for large particles, and probably also for small particles if the ballistic effect is evaluated, the particle size distribution is not significantly influenced by the wind's capability to dislodge the particles; if the horizontal wind is sufficient to dislodge some particles, it will essentially dislodge all. Consequently it would seem that it is not the probability distribution of horizontal wind fluctuations that dictates the particle size distribution in the lowest layers but it is the soil particle size distribution itself. However an exception to this occurs at the largest particle sizes, at the far right of the figure. Perhaps this explains Bagnold's repeated findings⁽⁶⁷⁾ of a particle weight distribution curve at large particle sizes which fall as D^{-9} . That is, perhaps

this result reflects no more than the upper tail of the distribution curve for the horizontal wind fluctuations in his wind tunnel.

IV.D Dust Storm Fluxes: Creep, Saltation and Suspension

Since I have been irreverent with everything else in this report I don't plan to stop now: I plan to question Bagnold's result⁽⁶⁷⁾ that the horizontal flux of material, $q \sim (u_*')^3$. During a dust storm, the vertical flux of horizontal momentum to the surface is $\tau' = \rho(u_*')^2$. I assume that this momentum is finally carried to the surface by the particles at a "deposition velocity" $\bar{v}_s + \alpha u_*'$ where $\bar{v}_s$ is the mass-average settling speed. Then if there is this vertical flux of horizontal momentum to the surface, the "concentration" of horizontal momentum in the dust layer is the flux divided by the deposition velocity, i.e., $\overline{mNu_p} = \rho(u_*')^2 / (\bar{v}_s + \alpha u_*')$, where $\bar{N}$ is the average number density of particles, $\bar{m}$ is their average mass and $\bar{u}_p$ their average horizontal velocity. But $\overline{mNu_p}$ is also the horizontal mass flux. Therefore the horizontal (saltation) flux is $\Gamma = \rho(u_*')^2 / (\bar{v}_s + \alpha u_*')$, in units, for example, of $g \text{ cm}^{-2} \text{ sec}^{-1}$.

To get the flow rate per unit horizontal, crosswind distance, then Γ must be multiplied by the height of the saltation layer. If it is estimated that the vertical velocity of a particle ejected from the ground is approximately the same as the velocity of an incoming particle, i.e., $\bar{v}_s + \alpha u_*'$, then the ejected particle will rise to a height h given by the conservation of energy: $(1/2) m (\bar{v}_s + \alpha u_*')^2 = mgh$. Then multiplying Γ by h gives the flow rate per unit width

$$q = c \frac{\rho}{g} (u_*')^2 [\bar{v}_s + \alpha u_*'] \quad (15)$$

where I've replaced the factor of $1/2$ by a constant c of order 1 since this crude analysis can't support such definiteness. Equation (15) is Bagnold's result only if $u_*' \gg \bar{v}_s$. Otherwise, for large ($\gtrsim 100 \mu\text{m}$) particles, and moderate wind speeds ($u_*' \lesssim 50 \text{ cm sec}^{-1}$)

but depending on the unknown parameter α , one obtains $q \sim (u_*')^2$. I haven't been able to find enough data to test (15).

IV.E Eddies, The Vertical Distribution and the Suspension Flux

But the above isn't very significant one way or the other: it may make a factor of 2 difference whereas in other aspects of the problem there are order-of-magnitude uncertainties. One of these very difficult areas is in predicting the amount of material that is resuspended. Perhaps the best we can do is say that the suspension flux is a certain percentage of the saltation flux, but: as a percentage of the total flux, Bagnold⁽⁶⁷⁾ says that the suspension flux "except in the case of a very dusty sand, appears to be very small indeed" and Chepil's result⁽⁷⁹⁻⁸¹⁾ (quoted here from WMO Technical Note No. 59⁽⁸²⁾) is: "according to the type of ground, about 3 - 40% is turned to dust (suspension), 5 - 25% roll and slide along the surface, and 50 - 75% are transported in saltation." It is difficult to glean from this last quote whether it means by weight or by number but with that type of variability the choice of interpretation doesn't matter much!

One way that we might try to approach the problem (albeit with reluctance) is via the convective diffusion equation. From the last section we have the horizontal mass flux in the saltation layer τ . The total number concentration can be estimated to be $\tau/(\bar{m}\bar{u}_p)$ where $\bar{m}$ is the average particle mass and $\bar{u}_p$ might be reasonably approximated as $\bar{v}_s + \alpha u_*$. Then, very crudely, $N_0 = O(\tau/(\bar{m}\bar{u}_p)) \simeq 10^3$ particles cm^{-3} , or the mass density of the particles is similar to the mass density of air. If the particle size distribution in the saltation layer is a reflection of the soil particle size probability density function $f_s(D)$ then the number of particles in the saltation layer which have diameter D to $D + dD$ is $N(D,0) dD = N_0 f_s(D) dD$. If the convective diffusion equation describes the vertical distribution of the contaminant then for the implied conditions,

$$\frac{\partial}{\partial z} (c u_*' z \frac{\partial N}{\partial z}) = - v_s(D) \frac{\partial N}{\partial z} \quad (16)$$

where $N(D,z)$ is the number of D-particles at height z and I've used for the eddy diffusivity $K(z) = c u_*' z$ where c is a constant probably near von Karman's constant. The solution to (16) is

$$N(D,z) = N_0 f_s(D) \left(\frac{z}{h} \right)^{v_s(D)/c u_*'} \quad (17)$$

where h is the height of the saltation layer.

Figure 17 shows a plot of (17) compared with some of Sehmel's data.⁽⁵⁾ I've cheated some by taking $N_0 f_s(D)$ to fit his 0.3 m data. Further, for the two curves with $\rho_p = 1$ (v_s for water drops) I chose $c u_*' = 12 \text{ cm sec}^{-1}$ to fit the $z = 1 \text{ m}$ data and then let the $z = 10 \text{ m}$ prediction fall where (17) predicts. It doesn't do a very good job: it misses the maximum and falls too rapidly for large D . There's also a curve for $\rho_p = 2.66 \text{ g cm}^{-3}$ showing that then $c u_*'$ can be chosen larger. But the main point to be gleaned from this is that even with all this fudging, the convective diffusion equation doesn't explain the data very well — just as might have been expected!

Elsewhere⁽⁷⁶⁾ I've also tried to set up a flux balance equation, using the obvious idea that to lift a particle, the vertical velocity w must be greater than $v_s(D)$ and therefore if the probability distribution of w , $P(w)$ is known, the vertical distribution of the particles might be calculated. If as suggested by Pritchard et al.,⁽⁸⁵⁾ $P(w)$ has an "exponential tail" then with some major approximations one still gets an exponential fall-off of $N(D,z)$ at large D . This is illustrated in Figure 17 by the curve erroneously labelled as Gaussian $P(w)$. However if one plots the upper end of the $P(w)$ data of Figure 6, see Figure 18, then a power-law fit to $P(w)$ seems to do quite well. If this and the soil particle number distribution $f_s(D) \sim D^2$ are put into a crude flux balance equation, then the result is the curve marked

$P(w) \sim w^{-n}$ which seems to have the correct large D behaviour. Although this is encouraging there is still a long way to go as you can easily verify by yourself if you'll try to write a flux balance equation: you need to know not only $P(w)$ but how far the eddies propagate vertically. Indeed this takes us back to Joukowski's caterpillar treads, now in the case when they're cast off from the surface. Goncharov⁽³⁷⁾ has mounted what seems to me to be a monumental effort attempting to describe this process but about his final formula concludes (see p. 196 of the English translation of his book): "Here κ , ℓ , and b are functions of z which cannot be determined either theoretically or experimentally." Such a diagnosis for any model is rather devastating!

In view of this and the inexorable march of time, I plan to leave this topic now, shaking my head, and suggesting that for some time yet we'll have to assume that the vertical flux is a simple fraction of the saltation flux.

IV.F A Fraction of Deposited Material Resuspended

The above was an implicit attack on the problem of simultaneous resuspension and deposition when resuspension dominated. Here I'd like just to glance at the inverse problem of deposition with a fraction of the deposited material resuspended. In particular, look at canopy flow and from the point of view of Joukowski's caterpillar. Earlier I had for the deposition velocity

$$v_d = v_s + \frac{\epsilon}{1 + \epsilon} \alpha u_* + \frac{1}{1 + \epsilon} \frac{u_*^2}{\bar{u}} \frac{E_j}{\beta} \quad (6)$$

where, recall, ϵ is related to the canopy's filtration efficiency. The opinion that I'd like to express here is that if simultaneous resuspension occurs in the canopy flow case, then I suspect that it is the "jet" term which is modified. My thought is that in the limit, the trailing end of Joukowski's caterpillar tread would completely frustrate the leading edge, removing all that is deposited. Yet I wouldn't expect

much to be resuspended from within the canopy. Consequently I suggest that when there is resuspension (6) should be modified to

$$v_d = v_s + \frac{\epsilon}{1 + \epsilon} \alpha u_* + \frac{\delta(d)}{1 + \epsilon} \frac{u_*^2}{\bar{u}} \frac{E_j}{\beta} \quad (18)$$

where $0 \leq \delta(d) \leq 1$, depending on the amount of effective jet deposition ($\delta = 0$ for no jet deposition) and where δ probably strongly depends on particle size, just as Bagnold's critical u_* curve implies. In some cases, if $\epsilon = 0$, $\delta < 0$ but this brings us back to the problem of resuspension with some deposition.

Again there's not enough data available to test this result, but at least I would like to call your attention to Figure 19 where by proper choice (of the many parameters!) in (18), it is possible to fit Chamberlain's data⁽⁸⁶⁾ for the deposition to real and to sticky, artificial grass. For the lower curve I took $\epsilon \alpha / (1 + \epsilon) = 0.038$ and $\delta = 0$ (i.e., no jet deposition on real grass) and for the upper curve (sticky grass) I added the last term in (18) with, eg., $\delta = \beta = 0.4$. The above combination of constants could be fit with $\alpha = 1$ and $\epsilon = 0.0395$ but obviously there are many other possibilities for the parameters. However perhaps this concept is interesting enough to pursue.

IV.(G H I J) K — The Resuspension Factor

In spite of my previous plan to avoid problems not of interest in atmospheric aerosol studies, I feel obliged to come to the rescue of the poorly treated resuspension factor.^(87,88) It seems to be very fashionable to criticize it; I plan to defend it. Does anyone propose that the relation between the air concentration, x and the surface concentration, x_s is $x \sim x_s^2$? As for the oft-quoted remark that the resuspension factor varies by eleven orders of magnitude (eg. 89,90) I suggest that: that's an error. With $x = K x_s$, the resuspension factor can be any finite (dimensional number between 0 and of the order of δ^{-1} , where δ is the depth of the pollutant's penetration in the soil. That's an infinite number of orders of magnitude! But the point is: this variation is physically meaningful, so rather than waste time criticizing K , let's try to understand its variability.

In this vein I'd like to close the paper with a crude prediction of K. First it seems that many of us have been away from the farm too long. Farmers have been dealing with resuspension for centuries, but they call it wind erosion. Thanks to soil scientists such as Chepil⁽⁹¹⁾ and Woodruff⁽⁹²⁾ empirical and semi-empirical formulae are now available to predict wind erosion. In their paper, Gillette et al.⁽⁹³⁾ present one form of the formula developed by Chepil and Woodruff which can fairly conveniently be used to predict the amount of soil lost from a field in tons of soil per rod width per hour. Or, if we desire only climatologically averaged predictions, then I refer you to Agricultural Handbook No. 346⁽⁹⁴⁾ (available at a cost of 25 cents!) from which we can estimate the soil loss in tons per acre of field per month from anywhere in the contiguous states. For example: find the erosion which is expected to occur next March from a 1,320-foot, north-south field in the vicinity of Midland Texas if the field is smooth and flat, if dry sieving indicates that 25% of the soil fractions are greater than 0.84 mm in diameter and if the field has 800 pounds per acre of flat wheat stubble. Answer: 40 tons per acre during March! In October, 21 tons per acre!

The point of the above is to suggest that many of the difficulties associated with predicting K are already solved for us: the importance of vegetation, soil moisture, soil particle size, cloddiness, ridge roughness, etc. Thus there are available tables and charts to estimate the erosion rate

$$E'' = E(I', R', C', L', V)$$

where

I' is a soil erodibility index

R' is a soil ridge roughness factor

C' is a climatic factor

L' is a field length along the prevailing wind erosion direction

and

V is equivalent quantity of vegetative cover.

The specific functional form for $\dot{E}$ can be found in Gillette et al's. paper, by returning to the original papers by Chepil, (95 - 99) or it can be inferred from Agricultural Handbook No. 346. Thus let's assume we know the erosion rate. It's usually given in units of tons of soil per acre per month but if we multiply the numerical value by the conversion factor of about 3.5×10^{-9} then the answer will be in cm of eroded soil (or cm^3 per cm^2) per sec.

Now consider the resuspension of some noxious contaminant spilled on a field of erodibility $\dot{E}$. Let $c(\xi, \eta)$ describe the distribution of the contaminant, eg., let $c(\xi, \eta) d\xi d\eta$ be the number of curies in the soil (to all depths) under the area element $d\xi d\eta$ about ξ, η . The volume of soil eroded during dt from location ξ to $\xi + d\xi$, η to $\eta + d\eta$ is $\dot{E} dt d\xi d\eta$. Therefore, if the volume concentration of the contaminant (assumed to be uniform with depth) is $c(\xi, \eta) d\xi d\eta / (\delta d\xi d\eta)$ where $\delta = \delta(t)$ is the depth to which the contaminant has penetrated, then the rate of contaminant removal is $\dot{E} d\xi d\eta c(\xi, \eta) / \delta$. Assume a fraction r of this is resuspended, the rest moving by creep and saltation. Then the air concentration downwind (i.e., in the x-direction) at location x, y, z from the source at (ξ, η) is

$$d\chi(x, y, z; t, \xi, \eta) = \frac{r \dot{E} d\xi d\eta c(\xi, \eta)}{\delta 2\pi \bar{u} \sigma_y \sigma_z} \exp \left\{ - (\dots) \right\}$$

where σ_y and σ_z are functions of the distance $(x - \xi)$. I've left the argument of the exponential function blank; filling it in with $(y - \eta)^2 / 2\sigma_y^2$, $z^2 / 2\sigma_z^2$ and a deposition term are unnecessary complications here. Then to obtain the contribution to the air concentration from all sources, obviously

$$\chi(x, y, z; t \dots) = \frac{r \dot{E}}{\delta 2\pi \bar{u}} \int \int_{\text{Strewn Field}} d\xi d\eta \frac{c(\xi, \eta)}{\sigma_y \sigma_z} \exp \left\{ - (\dots) \right\} . \quad (19)$$

It's true that it's rather inconvenient to extract a resuspension factor from (19). However, if the sampler is located a considerable distance from the strewn field (so that $x - \xi \approx x$) then (19) becomes

$$x(x,y,z; t \dots) \approx \frac{r \dot{E}}{\delta 2\pi \bar{u}} \frac{Q_0}{\sigma_y(x) \sigma_z(x)} \exp \left\{ - (\dots) \right\}$$

where Q_0 is the total quantity of contaminant spilled. (Incidentally since the flux to the surface downwind is a deposition velocity times this x , then for σ_y and σ_z increasing linearly with x , this gives a surface concentration downwind which varies as $1/x^2$ which fairly well fits the Rocky Flat's data, see Figure 20.) Now if a surface concentration is defined by $x_s = Q_0/A$, where A is the area of the strewn field, then the resuspension factor is

$$K = \frac{x}{x_s} \approx \frac{r \dot{E}}{\delta 2\pi \bar{u}} \frac{A_0}{\sigma_y \sigma_z} \exp \left\{ - (\dots) \right\}.$$

Finally, for a distance downwind such that $\sigma_y \sigma_z \sim A_0$ and for y and z small this becomes essentially

$$K \approx \frac{r \dot{E}}{2\pi \bar{u} \delta} = \frac{3.5 \times 10^{-9} \text{ cm sec}^{-1}}{1 \text{ ton acre}^{-1} \text{ mo}^{-1}} \left(\frac{r \dot{E}}{2\pi \bar{u} \delta} \right). \quad (20)$$

It's true that one has to fairly brutally approximate the above analysis in order to obtain a resuspension factor, and because of this I do side with the critics, but the main point I wanted to emphasize was that the variability of K is physically significant. For example, if in (20) we put for a very erodible field, $\dot{E} = 100$ tons acre⁻¹ day⁻¹ and $r = 10\%$, $\bar{u} = 10$ m sec⁻¹ and $\delta = 1$ mm then $K \sim 10^{-7}$ m⁻¹. On the other hand for $\dot{E} = 1$ ton acre⁻¹ yr⁻¹, $\delta = 100$ cm, $\bar{u} = 1$ cm sec⁻¹ then $K \sim 10^{-18}$ m⁻¹, i.e., eleven orders of magnitude smaller!

V. CONCLUSIONS

My anguish mentioned in the introduction prevails through to the conclusions: what conclusions can be drawn from a tour such as this? As your tour guide I hope I was successful in calling your attention to interesting problems. However, again, don't believe everything you saw; revisit it yourself after consulting the reference materials. Also watch your step!

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FIGURE LEGENDS

- Figure 1. Possible log-area distributions of surface level aerosols that might be measured under different circumstances. Data base from references 2 through 7.
- Figure 2. A replot of Aitken nuclei data from Hogan et al.⁽¹⁷⁾ and large particle data from Parkin et al.⁽¹⁸⁾ showing the measured gradient of atmospheric aerosol concentrations from expected source regions.
- Figure 3. A suggested division of the total dry deposition problem and a qualitative indication of where the process may be rate limited.
- Figure 4. Further qualitative indications of the time scales involved in the dry deposition process, evaluated either from $\delta t = \delta x/v$ or $\delta t = (\delta x)^2/D$, and illustrating that the process may be rate limited in various layers, depending on where the displacement time is largest.
- Figure 5. A schematic diagram of dry deposition problem areas in the boundary layer.
- Figure 6. Data obtained by Horst and Powell⁽²⁷⁾ for the probability density function for the vertical wind speed at various elevations for a neutral boundary layer. There is a suggestion that at small heights the Gill anemometer may not respond well to the large wind speeds.⁽²⁸⁾
- Figure 7. A schematic illustration of Joukowski's caterpillar developing in a turbulent boundary layer. The picture was sketched from motion picture frames in Goncharov's book.⁽³⁷⁾

- Figure 8. A plot of Liu and Agarwal's deposition velocity data⁽⁴³⁾ in a form to yield an estimate of the jet collection efficiency and showing Davies'⁽³⁸⁾ and Langmuir and Blodgett's⁽³⁸⁾ predictions for the collection efficiencies for similar problems.
- Figure 9. Similar to Figure 8 but using log-log coordinates to emphasize the theoretical cut-off Stoke's numbers⁽³⁸⁾ and to show the proposed approximate empirical fit. More accurate fits could be devised but the above seems adequate for present purposes and is conservative in the sense of the last section, desiring to over-estimate the deposition velocity.
- Figure 10. The deposition velocity as given by (4) and some data points from Sehmel⁽⁴⁸⁾ and Clough.⁽⁴⁹⁾ Notice that (4) is evaluated using $\rho_p = 1 \text{ g cm}^{-3}$ but Sehmel's data is for $\rho_p = 1.5$.
- Figure 11. A schematic diagram for the first iteration of contaminant fluxes in a canopy (γ^{-1} in the figure should be β^{-1} to be consistent with text).
- Figure 12. Definition of symbols for continuity equation.
- Figure 13. An attempt to indicate some of the complexities in the resuspension problem.
- Figure 14. Data from Bagnold⁽⁶⁷⁾ and Chepil⁽⁶⁸⁾ showing measured size distributions of specific sand (Bagnold) and airborne (Chepil) soil particles.
- Figure 15. Bagnold's critical u_* required to move monodisperse aerosol particles along a flat surface.⁽⁷²⁾
- Figure 16. Particle number distribution curves. The rightmost curve shows the size distribution which turbulent winds with $u_* = 20 \text{ cm sec}^{-1}$ could dislodge from the surface. For interest, some data obtained

Figure 16. (continued) by Junge,⁽⁷⁸⁾ Sehmel and Lloyd⁽⁵⁾ and the author⁽⁷⁾ are also shown.

Figure 17. The log-volume distribution of resuspended aerosol particles. Data from references 5, 7, 78, 83, and 84, especially from Sehmel and Lloyd.⁽⁵⁾ The three curves with specified cu_* are predictions from the convective diffusion equation; i.e., (16).

Figure 18. A limited range of the $P(w)$, 7.5 m curve of figure 6 from Horst and Powell.⁽²⁷⁾

Figure 19. Deposition with simultaneous resuspension. Data from Chamberlain.⁽⁸⁶⁾ For curve (1), equation (18) was used with $\delta = \beta = 0.4$ and the parameters ϵ and α necessary to fit the data with (13); curve (2), when $\delta = 0$.

Figure 20. An approximate plot of the surface concentration east of the Rocky Flats spill, data from Krey and Hardy.⁽¹⁰⁰⁾

SURFACE-AREA DISTRIBUTION, $\Delta S/\Delta A$ AND $(\mu\text{m}^2/\text{cm}^3)$

COMMON NAMES:
AITKEN NUCLEI
CONDENSATION NUCLEI
LARGE IONS

PRECIPITATION SCAVENGING DRY DEPOSITION AND EVAPORATION

LARGE PARTICLES

PHOTOCHEMICAL AEROSOLS

GRAVITATIONAL SETTLING

**DUST
SAND**

SOIL PARTICLES

0 400 800

MILES

----- $\geq 1\mu\text{m}$ PARTICLES

———— AITKEN NUCLEI

MULTI-LAYER MODEL FOR DRY DEPOSITION

NEED: SOURCE HEIGHT, POLLUTANT'S VERTICAL DISTRIBUTION, $\bar{u}$, $K(z)$, τ , CANOPY CHARACTERISTICS, POLLUTANT AND SURFACE PROPERTIES.

ORDER-OF-MAGNITUDE OF SCALES FOR MULTI-LAYER MODEL

Z(m)	LAYER	CATEGORIZATION	LENGTH	"MIXING" VELOCITY (cm sec ⁻¹)	DIFFUSIVITY (cm ² sec ⁻¹)	DISPLACEMENT TIME (SEC)
10 ⁵						
10 ⁴	A	STRATOSPHERE	√5 km		5 x 10 ³	10 ⁷
		TROP. <div style="display: inline-block; vertical-align: middle;">ABOVE</div> <div style="display: inline-block; vertical-align: middle;">IN</div> <div style="display: inline-block; vertical-align: middle;">MIXED LAYER</div> 	1 km		10 ⁵	10 ⁵
10 ³	1 km		1 km	10 ²		10 ³
		STABLE	100 m		10 ⁴	10 ⁴
10 ²	B	NEUTRAL	100 m		10 ⁵	10 ³
		UNSTABLE	100 m		10 ⁶	10 ²
10 ¹						
10 ⁰	1 m	FOREST, CITY <div style="display: inline-block; vertical-align: middle;">LIGHT</div> <div style="display: inline-block; vertical-align: middle;">STRONG</div> WINDS	10 m	10 ¹		10 ²
			10 m	10 ²		10 ¹
10 ⁻¹	C	TREES, SHRUBS <div style="display: inline-block; vertical-align: middle;">LIGHT</div> <div style="display: inline-block; vertical-align: middle;">STRONG</div> WINDS	1 m	10 ¹		10 ¹
			1 m	10 ²		10 ⁰
10 ⁻²	1 cm	SHRUBS, GRASS <div style="display: inline-block; vertical-align: middle;">LIGHT</div> <div style="display: inline-block; vertical-align: middle;">STRONG</div> WINDS	10 cm	10 ¹		10 ⁰
			10 cm	10 ²		10 ⁻¹
10 ⁻³	1 mm	LAWNS, SURFACES <div style="display: inline-block; vertical-align: middle;">LIGHT</div> <div style="display: inline-block; vertical-align: middle;">STRONG</div> WINDS	1 cm	10 ¹		10 ⁻¹
			1 cm	10 ²		10 ⁻²
10 ⁻⁴						
10 ⁻⁵	D	PARTICLES (RADIUS)	100 μm	~50		10 ⁻³
			10 μm	~5		10 ⁻²
			1 μm	~10 ⁻²		10 ⁰
			0.1 μm	~10 ⁻⁴		10 ²
			0.01 μm		10 ⁻⁴	10 ⁰
			0.001 μm		10 ⁻²	10 ⁻²
10 ⁻⁶	E	GASES	100 μm		10 ⁻¹	10 ⁻³
			100 μm			

① METEOROLOGY: DETERMINE FLOWS AND DIFFUSIVITIES ABOVE AND IN CANOPIES

② AEROSOL PHYSICS: DETERMINE DEPOSITION TO MET-SMOOTH SURFACES

③ CHEMISTRY/BIOLOGY: DETERMINE THE SORPTION OF GASES BY REAL SURFACES

PROBLEM AREAS OF DIFFUSION IN THE BOUNDARY LAYER (EVEN IF THE GRADIENT FLUX ASSUMPTION IS VALID)

K_i or σ_i as
A FUNCTION OF
STABILITY AND $\bar{u}$

DIFFUSION IN
SHEAR FLOW

MESOSCALE
DIFFUSION

BUOYANT
PLUME
RISE

PARTICLE
BREAK-AWAY

$$\frac{\partial x}{\partial t} = \nabla \cdot (K \nabla x - \vec{v} x)$$

$$K = K(z)$$

DOWNWASH

BOUNDARY CONDITIONS

JOUKOWSKI'S
"CATERPILLAR
TREAD"

GRID
SUPPRESSED
TURBULENCE →

DEVELOPING
BOUNDARY
LAYER →

TIME

DRY DEPOSITION TO SMOOTH SURFACES

POLLUTION FLUX IN A CANOPY

POLLUTION BUDGET IN A CANOPY

SOME COMPLEXITIES OF RESUSPENSION

DEPOSITION WITH RESUSPENSION