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A Lagrangian plume model is described that has proven useful in water and air applications. It contrasts sharply with earlier Eulerian integral flux models even though they are shown to be equivalent. As an alternative and complementary approach, the Lagrangian formulation offers new insights into the problem. As a result, it furnished the first accurate statement of the Projected Area Entrainment (PAE) hypothesis that describes the assimilation of moving ambient fluid into the plume. The hypothesis allows — without tuning — average motion and dilution characteristics to be predicted for the first time. Further contemplation of the Lagrangian plume element resulted in the identification of the Negative Volume Anomaly (NVA). The NVA is an inconsistency in control volume conception resulting from the intersection of the cross-sections that bound it, causing the anomalous production of negative volume.

Although the Lagrangian plume model, UM, has been adopted by the U.S. Environmental Protection Agency, is used in over a dozen foreign countries, and has been verified independently, a pervasive bias against the approach makes it difficult to publish findings in the peer reviewed literature. A case study describing the problem is presented. This analysis suggests that the phenomenon is not unique to plume modeling. The contributing causes are perpetuated by the closedness of the peer review system. Recommendations are given for improving peer review procedures to open the process to inspection. They include simple measures modifying anonymity and allowing authors to submit to multiple journals simultaneously.

A LAGRANGIAN PHILOSOPHY FOR PLUME MODELING

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A major conclusion of this thesis is that science is not the open enterprise most of us would like it to be. In attempting to publish the Lagrangian plume model over a score of years, a pervasive bias against the approach has been revealed. The ramifications of this reality have been the dominating influences of my professional life. They have been at times debilitating and at others uplifting. The former are associated with personal isolation, disappointment, and vulnerability, while the latter are associated with finding kindred spirits to help overcome the negative aspects of peer bias.

I have found that wherever things go wrong, wherever there are abuses, there are generally also excellent people already struggling to overcome them. I admire them, thanks for the inspiration.

In particular, I wish to acknowledge the help and support of Don Baumgartner, Larry Winiarski, my daughter Erika Frick, Norbert Jaworski, Dave Young, and John and Helen Wade. Also, I thank all the members of my committee for their patience, help, and loyalty.

TABLE OF CONTENTS

TABLE OF CONTENTS, CONTINUED

LIST OF FIGURES

LIST OF FIGURES, CONTINUED

LIST OF APPENDICES FIGURES

APPENDIX 1

APPENDIX 6

LIST OF APPENDICES FIGURES, CONTINUED

A LAGRANGIAN PHILOSOPHY FOR PLUME MODELING

CHAPTER 1. INTRODUCTION

Plume modeling is the quantitative study of fluid jets and plumes, both liquid and gaseous. Compared to many others, it is a small discipline. Nevertheless, it is a varied one embracing work ranging from simple empirical formulas (Briggs, 1969) to elaborate finite difference and finite element models (Sini and Dekeyser, 1989). However, at the heart of the discipline, due partly to their computational economy, are integral models, specifically Eulerian integral flux plume models (e.g. Morton, Taylor, and Turner, 1956; Weil, 1974; Hoult, Fay, and Forney, 1969; Schatzmann, 1979), and, to a lesser but increasing extent, Lagrangian plume models (Winiarski and Frick, 1976; Teeter and Baumgartner, 1979; Frick, 1984; and Lee, Cheung, and Cheung, 1987; Cheung, 1991; and Baumgartner, Frick, and Roberts, 1993).

Integral models are by definition "averaging" models. That is, they predict the overall average behavior of plumes: mean dilution, mean trajectory, etc.. Conceptually then, they are relatively simple models although many, especially Gaussian Eulerian integral flux models, are mathematically elaborate and complicated. This apparent sophistication and the "flux" orientation seem to charm many of its most passionate proponents into believing that these models represent truly fluid dynamical models, i.e. that they are essentially equivalent to models based on the primitive equations of motion. However, one difference between fully fluid dynamical models and integral plume models is the use of closure schemes involving the pointwise parameterization of turbulence in the former as opposed to bulk, average, or *integral* entrainment hypotheses to close the continuity equation in the latter.

Entrainment is the process by which plumes grow, the act of assimilating ambient fluid into the plume interior. In both Lagrangian and Eulerian models entrainment is related to average properties and dimensions of the plume and ambient fluid. In comparison to the ambient fluid, plumes are characterized by relatively high levels of turbulent energy and other properties. Nevertheless, plume boundaries are defined rather arbitrarily to extend to regions of essentially ambient properties in the

Lagrangian model and to nominal, statistically defined limits in Gaussian integral flux models.

But, whatever the details, it is important to remember that the two models predict only average properties, such as dilution and rise. Thus, any prediction of profiles of properties depends on additional assumptions, for example, that the profile is Gaussian. Similarly, the Lagrangian model predicts the motion of only the center-of-mass of the plume element, not of every point across the cross-section.

The plume element is a fundamental modeling unit, a control volume, that is used to help to mathematically define the plume equations. It provides a framework for accounting for the mass, momentum, and other plume properties. It is a material volume (Batchelor, 1970) that moves through space, always, in the idealization, maintaining the original plume matter present at the defining moment at the source. In contrast, the Eulerian integral flux model conceives a stationary control volume through which the plume and ambient fluids flux, or flow, a seductively fluid dynamical notion which is, nevertheless, equivalent to the Lagrangian particle model.

The Lagrangian model has been notably successful in establishing the basic usefulness and goodness of the entrainment closure approach, whose efficacy helps justify the use of these models in the face of theoretically more appealing and rigorous primitive finite element and difference models. The *Projected Area Entrainment* (PAE) hypothesis which states that entrainment is simply proportional to the area projected by the plume element to the current and the ambient current speed, allows average plume properties to be predicted in a wide range of currents and conditions without the need for a tuned coefficient, i.e. its proportionality constant appears to be intrinsically 1.00. (Winiarski and Frick, 1978; Carhart, Policastro, and Ziemer, 1982; Frick, 1984; Lee and Cheung, 1990; Cheung, 1991; and Frick, Baumgartner, and Fox, 1994).

However, despite demonstrable success, it is difficult to publish findings derived from the Lagrangian plume model. In the final analysis, a pervasive and persistent bias is exhibited against the Lagrangian approach by Eulerian integral flux plume modelers and proponents. This bias results in the blocking of findings from the peer reviewed literature, which tends to be dominated by Eulerian integral flux modeling school of thought. This tendency is documented herein. Perhaps its most defining rationalization is the belief that the Lagrangian model is a solid body model whereas Eulerian integral flux models are "fluid dynamical." The bias is puzzling and troubling on for many reasons, but particularly in light of the fact that it is shown herein and elsewhere that the models are fundamentally equivalent.

While the models can be shown to be equivalent, the approaches are sufficiently different in viewpoint that they may be thought to be complementary, analogous to the principle of complementarity in physics in which light is studied alternatively as particles or waves, depending on which approach is most fruitful. This broadening of viewpoint, essentially an intellectual enrichening of the discipline, contributed to another important discovery, the *Negative Volume Anomaly* (NVA).

The NVA, or simply the anomaly, is a mathematical artifact. It results from the plume element being incompletely or erroneously defined. It is the result of the inadvertent intersection, in regions of high trajectory curvature, of the two cross-sectional faces that define part of the surface of the Lagrangian plume element. The intersection leads to a breakdown in the concept of the element and earlier equations based on it. Unless specific measures are taken to correct it, the artifact leads to the inclusion of essentially negative mass in the equation of continuity. Its ramifications, when present, include an overprediction of plume radius and the subsequent overprediction of plume entrainment, i.e. dilution.

Significantly, since equivalence between the two models exists even in the presence of the anomaly, it follows that it is intrinsically contained within the Eulerian integral flux plume model equations. Thus, whether or not one is a proponent of Lagrangian plume models, the problem needs to be fully understood before model tuning exercises are undertaken. Certainly the idea should be published and considered by the modeling community.

After becoming aware of its fundamental significance, the authors, Frick, Baumgartner, and Fox (1993) determined to publish work describing the NVA and its relationship to Lagrangian and, more significantly, Eulerian integral flux models (see

Appendix 1). It was hoped a way had been found to overcome the resistance that had been experienced with publishing the model and the PAE hypothesis.

However, the new effort encountered similar disapproval and bias, although, after this thesis was all but finished, the paper found in Appendix 1 was accepted for publication in a split review in which the editor made the decision to publish (van der Zee, 1994). It followed a concerted effort to identify an open forum. It also followed two rejections and five very negative reviews by the American Society of Civil Engineers' *Journal of Hydraulic Engineering*. Their tone was comparable to those of earlier rejections by the same journal of the Lagrangian plume model and the PAE (see Appendix 2).

An analysis of events relating to efforts to publish the Lagrangian plume model and the avowed breakdown and failure of a peer review system is given in Chapter 2, with supporting evidence given in the appendices. Beginning in Chapter 3 a more traditional technical development of the plume model is offered. The performance of the Lagrangian and Eulerian models is compared in Chapter 4. Conclusions and recommendations are given in Chapter 5.

CHAPTER 2. CONTROVERSY OVER CONCEPTS AND APPROACHES

INTEGRAL PLUME MODELS

The Lagrangian plume model simulates the gross behavior of turbulent jets and plumes, such as mean motion and dilution. It is characterized by various equations expressing physical laws, especially a conservation of mass equation expressed as an entrainment hypothesis. This is how it differs most from primitive equation models. Entrainment is the rate of assimilation of ambient fluid by the plume. Entrainment hypotheses makes it possible to close the plume equations without addressing turbulence and turbulent mixing more fundamentally. In this sense the Eulerian integral flux plume models are similar the Lagrangian model. In fact, they can be written to give equivalent predictions. This equivalence is demonstrated Chapter 4.

Nevertheless, the Lagrangian and Eulerian integral flux model approaches are radically different in viewpoint. The main difference is the basic modeling construct — the control volume — on which an accounting of various quantities is performed. The Lagrangian control volume, or plume element, moves at the average velocity of the plume fluid; it is a material element (Batchelor, 1970) that continues to contain, in its simplification, the molecules present at the point the source. In contrast, the Eulerian control volume is fixed in space with mass flowing, or fluxing, in some places across and in others parallel to its boundaries. These characteristics are also considered subsequently.

Plume modeling should not be considered to be an inflexible monolithic entity whose components are dictated unequivocally by physical principles. The inability to close the plume equations with a rigorously derived equation of continuity opens the field to a considerable degree to artistry, creativity, and good luck. Not surprisingly then, the different perspectives of the two approaches can lead to startlingly different ideas, especially entrainment hypotheses. The Lagrangian model leads naturally, or so it seems to this practitioner, to the Projected Area Entrainment (PAE) hypothesis. While the cylinder term of the PAE hypothesis is found in many plume models, in its totality it is different from any of the earlier statements devised for the Eulerian approach: previously the growth and curvature terms were generally unrecognized.

But, more profoundly, the Lagrangian approach gives insight into the Eulerian model that was not anticipated by its proponents: the Negative Volume Anomaly (NVA). The anomaly describes an inconsistency in the mathematical description of the Lagrangian plume model when trajectory curvature is large. While this appears to be a problem with the Lagrangian model, their equivalence proves that the Eulerian model is intrinsically subject to the anomaly. In short, the intersection of the cross-sections defining the plume element causes negative volume, or mass, to inadvertently creep into the traditional model equations on which earlier models are based. A feedback mechanism is created that can cause entrainment to be overestimated substantially.

THE PROJECTED AREA ENTRAINMENT HYPOTHESIS

The PAE hypothesis states that entrainment is proportional to the area of the plume element surface projected onto a plane defined by the local current vector (Winiarski and Frick, 1976, 1978; Frick 1984). Specifically, it is the flux of mass into the plume through this area. The area, and simultaneously the entrainment, changes as the plume element moves along the plume's trajectory. While the hypothesis was expounded in general terms at least as early as 1960 (Rawn, Bowerman, and Brooks), it was much later that the Lagrangian formulation precipitated its first accurate description in three terms: cylinder, growth, and curvature (Winiarski and Frick, 1978; Frick, 1984). Previously the growth and curvature terms were unrecognized or specifically neglected. For example, Schatzmann (1979) discounts the curvature term specifically, but without experimental justification. Even the growth term was frequently omitted, as, for example, by Hoult, Fay, and Forney (1969).

If the PAE hypothesis is basically correct, and evidence presented subsequently in this section suggests it is, poor results are inevitable when any of its terms are neglected. The amount of activity directed at entrainment hypotheses suggests that not only was the inadequacy of plume models generally recognized but the entrainment hypothesis was believed to be the cause. Yet, paradoxically, although poor agreement was observed, it did not stimulate an attempt to understand the source of failure as it might exist in an inadequate description of the PAE hypothesis. Instead, as Frick (1984) suggests and Cheung (1991) agrees, the preferred response was to tune coefficients or, when that did not work, to invent other entrainment hypotheses.

It is not claimed that tuning is an inappropriate response to engineering problems, in which it is often valuable. But, exercised prematurely, it tends to obliterate legitimate clues to natural phenomena. Ironically, it does so by doing what it is designed to do — to diminish the differences in limited domains between model predictions and observations. The point is that the base cause of disagreement may lie in the omission of appropriate terms, as is the case with PAE, or, it may be due to other errors and artifacts. One unwanted artifact, the Negative Volume Anomaly, is described in the next section. Premature tuning will tend to obscure the clues to better understanding that legitimate differences provide.

As long as these omissions, errors, or artifacts exist tuning is likely to be ineffectual because it targets the wrong entity for modification. In plume modeling, problems tend to be attributed to entrainment, whether or not it bears a relationship to the source of disagreement, as it does when the PAE hypothesis is incompletely expressed. While some improvement in a limited domain of the problem may seem apparent after tuning, the solution may actually degrade in other regions, with the concomitant complication of an erroneous "correction" to further obfuscate the path to a better solution. If tuning must be done to meet practical operational constraints, the solutions obtained should be considered provisional and a willingness to retreat to a previous level of ideation should be part of the discipline's paradigm.

The PAE hypothesis is important because *without tuning* it yields good agreement with observation, as was established by Winiarski and Frick (1976, 1978) and further by Frick (1984) using data recorded by Fan (1967). Their findings are corroborated by Lee and Cheung (1990) and Cheung (1991).

Thus, it may be concluded that the Projected Area Entrainment (PAE) hypothesis is a concise statement of forced entrainment, just like the Taylor entrainment hypothesis (Morton, Taylor, and Turner, 1956) is considered to be one for self-induced aspiration entrainment. In other words, the basic essence of the forced entrainment process is captured in a simple statement, one that describes the bulk of the process, in this case without tuning (a coefficient of unity).

Independent evidence supporting the Lagrangian model is given by Cheung (1991). In his thesis he describes and verifies the model JETLAG, developed by Lee, Cheung, and Cheung (1987) and Lee and Cheung (1990), which is patterned after the Lagrangian plume model (Frick, 1984). Frick, Baumgartner, and Fox (1993) demonstrate the approximate equivalence of the models; their work is given in Appendix 1. Comparability is further established by Cheung (1991) who shows that his "Eq. 3.19 reduces to the forced entrainment expression given by Frick (1984)." Thus, even though JETLAG is a 3-dimensional model with a 3-dimensional PAE hypothesis, what is said about it applies to a great extent to the model under consideration here. Cheung (1991) writes (the emphases are mine):

> "A LAGrangian JET model, 'JETLAG', for an arbitrarily inclined round buoyant jet in a current is presented in this chapter. The model has its root in the model developed for applications in cooling tower problems (Winiarski and Frick, 1976, 1978, Frick 1984). ***The essence of the model lies in the accurate determination of the 'forced entrainment' by the ambient current, often the dominant component. This eliminates the determination or tuning of any far field entrainment coefficients as required by most other models.*** The Lagrangian approach employed by the model in solving the conservation equations is another major difference with most other models. With an additive hypothesis for the entrainment terms and correction for data definition, this model gave reasonably good results when compared with basic experimental data of vertical buoyant jet in stagnant fluid or in a crossflow (Frick 1984).
>
> "The model was extended by Teeter and Baumgartner (1979) as UOUTPLM for application in water problems and it is one of the five mathematical models recommended by USEPA for calculating initial dilution. The main difference of UOUTPLM from the original version was the omission of drag term and equivalent virtual mass. UOUTPLM has not been adequately validated; the theoretical basis of the model was also strongly criticized by Alam et. al. (1982, 1984) in an evaluation of initial dilution models by field data. The model can only treat buoyant jets with 2-dimensional trajectories; even within this category of flows, it cannot handle some cases of interest (e.g. oblique jets or dense plumes in a current). ***Nevertheless, the simplicity, clarity of physical formulation of this model, and its performance for 2-dimensional jets in crossflow suggests the underlying approach is worth pursuing.*** When properly

interpreted in relation to traditional integral jet models, many of the previous theoretical objections can be removed (Lee & Cheung 1990)..."

"It will be shown that each term [of the PAE hypothesis] has its importance over different parts of the trajectory, so that none of the terms can be neglected. ***As pointed out by Frick (1984), many previous investigators have typically neglected at least one of the three terms; this may be the reason why variable entrainment coefficients have been found to be necessary in order to fit their model predictions to experimental data.***"

Perhaps most impressive is the extensive verification work Cheung (1991) presents. In a section titled "Relationship with other models," starting on page 63, Cheung states:

"All models developed for a buoyant jet in a current should be tested with the four basic flow regimes. Regardless of the particular theoretical formulation or good comparison in the prediction of a particular data set, model predictions should reproduce the correct asymptotic behaviour (Fischer 1979). JETLAG predicts trajectory and dilution variations which follow the power laws in the flow regimes as given in Table 2.3. The predictions of two other representative models by Chu (1975) and Schatzmann (1978), selected after an extensive evaluation of various mathematical models, are also shown together with experimental data by various researchers in Table 3.2. ***This clearly shows that the prediction of JETLAG is in good agreement with experimental data; the limitations ... of the other two models can also be seen from this table, e.g. Schatzmann's model cannot reproduce the correct behaviour in the buoyancy-dominated far field (BDFF)***; on the other hand, Chu's model is not applicable to the near field (stagnant ambient or weak current).

"Fig.3.19a) and b) shows examples of trajectory predictions for a momentum-dominated discharge and a buoyancy-dominated discharge, respectively. ***It clearly indicates the correct power law dependence of the trajectory in different flow regimes; (a) MDNF - MDFF - BDFF and (b) BDNF - BDFF. The smooth transitions from one regime to another are also demonstrated. These trajectory behaviours coincide with experimental observations*** (Wright 1977)."

The reason for presenting such extensive testimony is to convince the reader of the basic goodness of the Lagrangian model. This is considered necessary because the model has met substantial, but, in my opinion, unscientific and unfair, peer review opposition. My experience will be used to make general statements about the inadequacies of peer review. Obviously, persuading the reader to willingly consider such claims will be easier if the basic goodness of the science presented herein is not in doubt.

THE NEGATIVE VOLUME ANOMALY (NVA)

The Negative Volume Anomaly (NVA, or simply the anomaly) describes an inconsistency in the definition of control volume used in some integral hydrodynamical models. Speaking generally, it is important because control volumes are the fundamental building blocks of these models. Clearly, a significant inconsistency in basic model formulation will affect the overall quality of predictions whenever the conditions causing it are present. Thus, the NVA potentially affects plume modeling at the highest paradigm level. In that respect it is more compelling than the PAE hypothesis. While the latter could be presumably replaced by various entrainment hypotheses with comparable results, the NVA introduces unsuspected and unquantified errors into the model which can render tuning and other manipulation of the model nonsensical until it is itself addressed.

To appreciate the potential importance of the Negative Volume Anomaly consider Figure 1 which shows the ratio of dilution ratios at maximum rise and at overlap predicted by the EPA plume model UM (Baumgartner, Frick, and Roberts, 1993), a Lagrangian model subject to the anomaly. When using UM, instances of overlap, that indicate the presence of the anomaly, are brought to the attention of the user. In fact, in the default mode, the model is stopped at this point to avoid the NVA. This is in accordance with the EPA guidance document (Baumgartner, Frick, and Roberts, 1993) which recommends that additional dilution achieved beyond the point of overlap be neglected whenever it is substantial. The reason for this recommendation is that dilution can be greatly overestimated in these instances. Justification can be expressed in quantitative terms and therefore the constraint is properly limiting, if overly conservative.

The model was run in the default mode and then reconfigured and run a second time to obtain the dilution at maximum rise. When overlap occurs the latter dilutions are overpredicted because the NVA is present. The degree of overprediction is unknown but is believed to account for roughly half of the excess ratio above 1.0. Thus the anomaly does affect the way engineers and scientists use the model and should be considered by the modeling community.

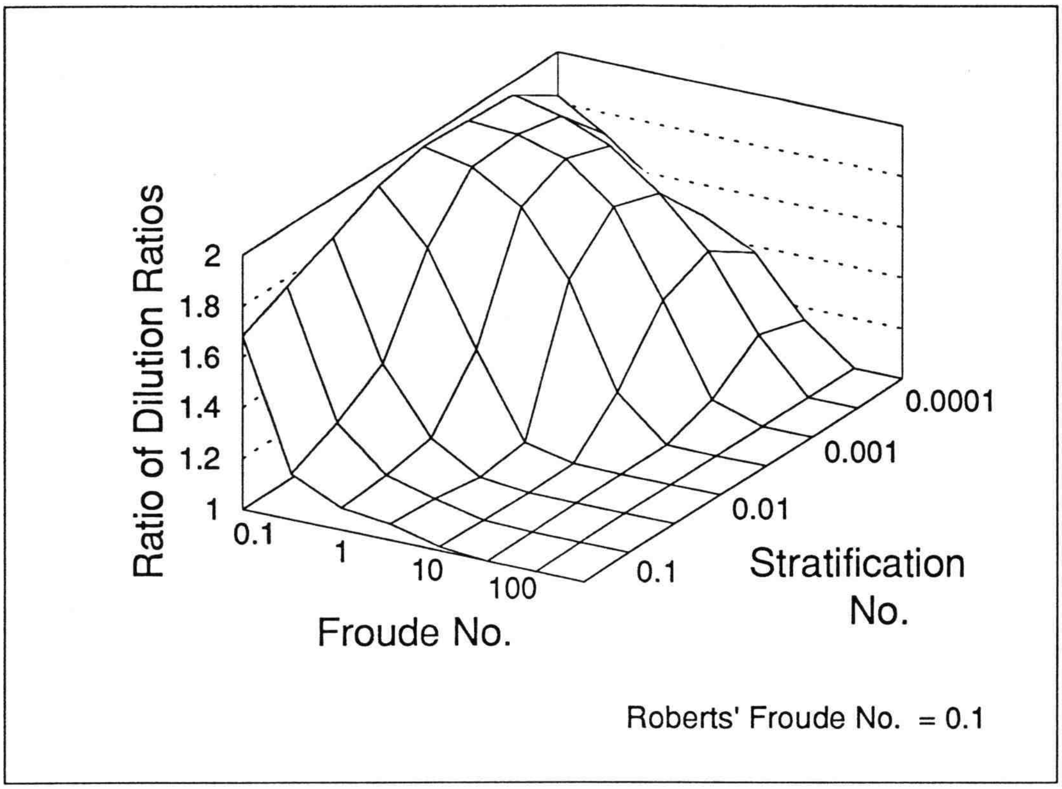

Figure 1. Ratios of dilution (NVA uncorrected dilution/NVA terminated dilution) as a function of densimetric Froude number and stratification parameter.

In Figure 1 the ratios of dilutions are plotted against the densimetric Froude number and the stratification number. The densimetric Froude number is defined as $v^2 \left(\frac{\rho_a - \rho_e}{\rho_e} gD\right)^{-1/2}$, where v is the effluent speed, ρ_a and ρ_e are the ambient and effluent densities, respectively, g is the acceleration of gravity, and D is the initial plume diameter. The stratification number is defined as $\frac{d\rho}{dz} \frac{D}{\rho_a - \rho_e}$, where $d\rho/dz$ is the ambient density gradient. The former is a relative measure of buoyancy and momentum initially present in the plume, small values corresponding to very buoyant discharges and large ones to jets. The latter is a measure of density stratification in the ambient fluid, large values indicating more stratification than small ones. The Roberts' Froude number, which is a relative measure of current strength, is defined as u^3/B, where u is the current speed and B is the buoyancy flux which is the product of the reduced gravity ($\frac{\rho_a - \rho_e}{\rho_e} g$) and the effluent flow per unit length of diffuser. The value of 0.1 has particular significance as a criterion separating predominantly aspiration (or Taylor) entrainment from predominantly forced entrainment (Roberts, 1977). Other similarities pertain, such as port spacing, etc..

Having concluded that the NVA is important, it is worthwhile to clarify a few related key concepts. For one thing, the anomaly is confusing until the meaning of volume is clarified. This is because volume is considered both as a scalar measure of magnitude and a reference to a topological entity, a physical object, the control volume.

With respect to the anomaly, the scalar volume is important in two equations: the entrainment equation and the scalar volume equation of the element. This dichotomy seems to be a source of confusion. This is because the scalar volume is computed by the entrainment equation, not the cylinder equation. In earlier models, the volume equation of the element, the equation for a cylinder, is used to derive its radius.

As for the physical object, the term control volume is customary in Eulerian integral flux models while plume element is customary in Lagrangian models. They are fundamentally different in that the Eulerian control volume is fixed in space (successive such stationary control volumes being considered) while the Lagrangian plume element moves with the fluid, i.e. is a sort of material volume. When concepts apply to both formulations in general, the term control volume is more common. It is important because it establishes the cross-sectional area exposed to the current.

The anomaly may affect various applications — for example, channel flow models and cloud models. In particular, it applies to the formulation of Lagrangian and Eulerian integral flux plume models. It may be understood as follows:

The anomaly emerges in instances of strong plume bending, or trajectory curvature, when the element faces that partially define the plume element surface intersect. In other words, it develops when the radius of curvature of the trajectory is less than the radius of the plume element. In such instances, an anomalous part possessing negative volume is created. This negative volume is an artifact of the mathematical definition, not a physical reality. As stated above, the total volume of the plume element is derived from the conservation of mass equation, specifically, the entrainment hypothesis. After it is computed it is used as an independent variable to estimate the corresponding radius. It is traditional to use the simple equation for a right cylinder for this purpose. This is called the round plume assumption.

The problem is that the round plume assumption breaks down and causes the radius to be overestimated when the radius of the plume is greater than the radius of curvature of the trajectory. Because the cross-sections must be constructed to be perpendicular to the trajectory to maintain flux vectors normal to them, the plume element faces intersect, which is the condition for the NVA to exist. In these instances the negative portion of the plume element acts to reduce the positive one. As a result, larger dimensions are inferred to make the volume of the anomalous plume element agree with the entrainment hypothesis. The process is a classic feedback mechanism in that increased radius leads to even more entrainment, magnifying the problem.

The mathematical and geometrical characteristics of the anomaly are described further in Chapters 3 and 4. For now it is important to simply understand that is a mathematical and conceptual problem, not a physical one. In speaking about the NVA it should clear that it is entirely a mathematical modeling artifact. The anomaly is not physically real even though words tend to confer physical reality to it. For example, to say "entrainment (or dilution) is increased" suggests that the effect is real but, in fact, refers only to the simulated entrainment.

An attempt to formulate a geothermal plume model, the original goal of this thesis, finally led to a general appreciation and understanding of the anomaly. The precipitating factor has been forgotten but earlier intimations helped clarify the problem. For example, it was encountered while modeling cooling tower plumes (Winiarski and Frick, 1976, 1978; Policastro et al., 1980). Such plumes often bend sharply at the source and, in simulations, the downstream plume element boundary will unrealistically interfere (overlap) with preceding portions of the simulated plume.

At the time, the sharp increase in simulated dilution was dismissed because it was reasoned that the excess mass would be entrained in a short time in any case and therefore the model was in overall agreement with reality. However, no procedure was developed to test this assumption, nor was it recognized that the cylinder equation has multiple roots and that the wrong one was being used in these circumstances (Frick, Fox, and Baumgartner, 1991). Finally, it was not anticipated that the problem might be more important in other regions of the trajectory, as for example near maximum rise. In retrospect, considering the Winiarski-Frick model tended to underestimate condensed plume length, the error is thought to degrade predictions of condensed core length in simulated cooling tower plumes.

The anomaly may be eliminated by modifying the earlier, anomalous plume element conception based on the unmodified round plume assumption. One way to do so is to integrate the plume element only over the positive part of the cross-section, as was done to obtain the corrected volume equation in Chapter 4. This is done to obtain the corrected results in Figure 2, lower curve. The approach preserves a semblance of the assumption while eliminating the anomaly. However, it is recognized at the outset that, while this modification may be satisfactory and represents a substantial improvement over earlier theory, in extreme cases, representing upstream intrusion, gravitational collapse, and other phenomena, it is likely to prove inadequate and may have to be replaced by a radically different conception.

Once eliminated, the corrected and uncorrected models may be compared. The effect of the NVA can be significant, as is shown in Figure 2. As explained previously, the solutions diverge when the radius of curvature of the trajectory becomes smaller than the radius of the element. The NVA-affected solution shows dilution increasing sharply as the radius of curvature of the trajectory becomes significantly smaller than the radius of the plume element. In contrast, the corrected solution grows less rapidly. At maximum rise the predicted dilutions differ by about 20 percent, with the uncorrected dilution being greater. Possibly, a systematic search would show that the anomaly can erroneously produce a one-third to one-half of total simulated entrainment in extreme cases. The procedure used to develop Figure 1 might be useful for identifying sensitive regions. Many of these points will be covered in more detail in subsequent chapters.

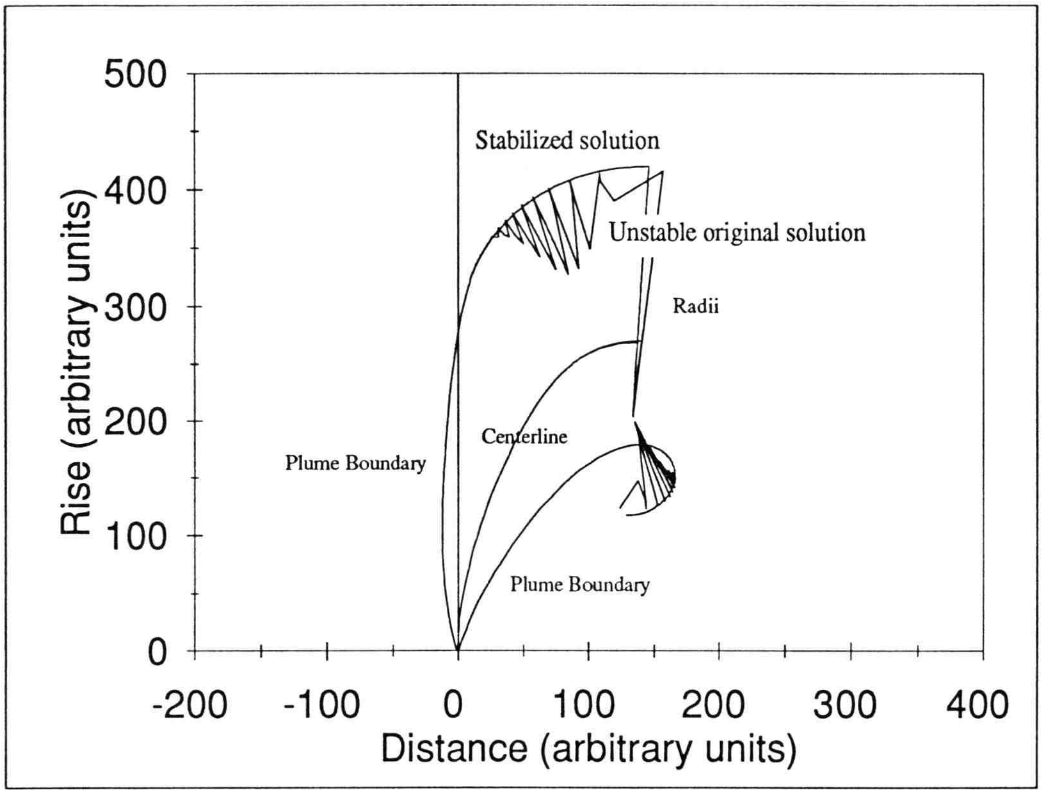

Figure 2. An example of the numerical instability that emerges in the original Lagrangian model when the NVA is corrected.

Only a few cases have been analyzed so far because the proposed correction introduces a numerical instability which is not easily corrected. Two attempts to remove the instability have been made, one with a vector version of the Lagrangian plume, similar to UM, and the other with an ordinary differential equation solver, ODESSA (Miller, 1993). The latter, which uses the Gear method, also suffers from the instability and the true solution is still somewhat uncertain. However, the Lagrangian and ODESSA models give equivalent results for the NVA uncorrected version of the model and similar results for the NVA corrected and stabilized form. An example of the instability involving the NVA corrected model is given in Figure 3.

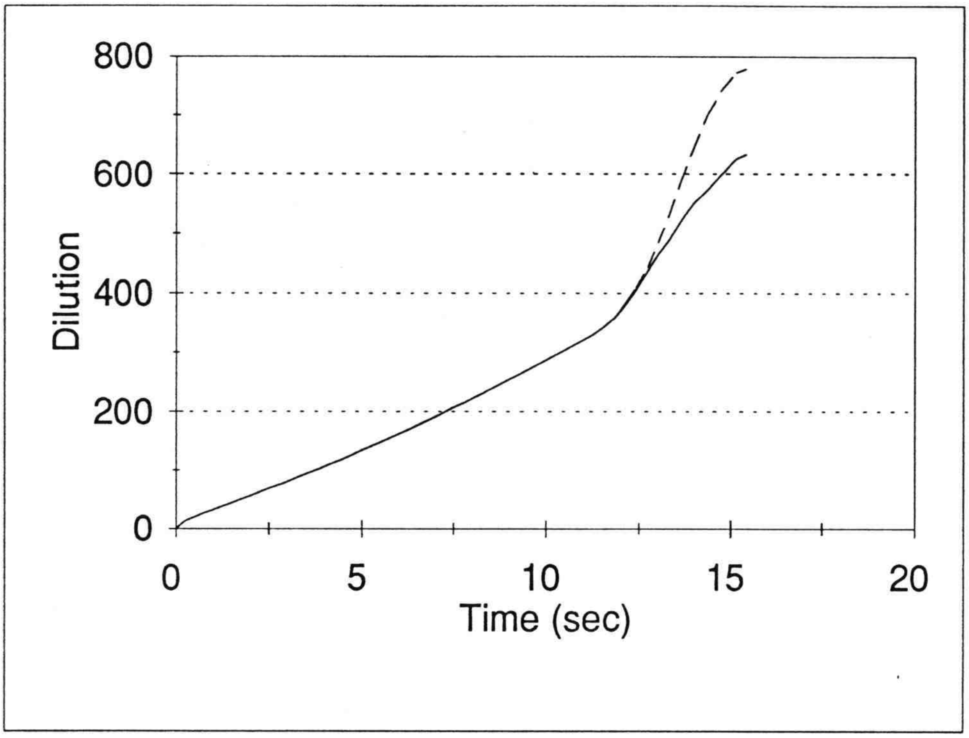

Figure 3. Dilution prediction of the ODESSA plume model with NVA (top) and NVA corrected and stabilized (bottom).

In many other important cases, without critical curvature, the anomaly is totally absent, which may be the main reason why it has not been identified previously. However, its manifestations may also be addressed indirectly, primarily through tuning and problem selection.

Many models are tuned, i.e., coefficients are used and adjusted to best fit model predictions with measurements. As explained above, the anomaly can undermine this process and yield spurious coefficients because differences between simulation and observation are likely to be interpreted to be legitimate and to be amenable to tuning. But, in fact, the influence of the anomaly is dependent on the relative presence or complete absence of critical curvature in measured plumes, which can only be established by determining whether the mathematical conditions for overlap exist. This is not done in many models and, as a consequence, the anomaly is a potential source of uncertainty in plume model tuning exercises. The resulting coefficients will be dependent on the mix of experimental cases used to tune them and, therefore, may be spurious.

In addition, the anomaly can contribute to models being limited to simpler behaviors. For example, Baumgartner (1993), reconsidering previous experience, believes that the anomaly contributed to the decision to limit the model PLUME (Teeter and Baumgartner, 1979) to the initial dilution region below the trapping level. While it is appropriate to do so when deficiencies in model theory are perceived, this process of problem selection is clearly limiting.

In conclusion, the Negative Volume Anomaly is important to the way in which at least one operational plume model, UM, is used — the presence of the anomaly being acknowledged in the EPA plume modeling guidance (Baumgartner, Frick, and Roberts, 1993). When present, it causes dilution to be overestimated. Limited work shows that the relative overestimation may be as high as 50 percent. These are significant conclusions and concerns. Yet, a paper explaining the anomaly, given in Appendix 1, was rejected twice by peer review committees of the American Society of Civil Engineers (ASCE). The causes for rejection are considered by the authors to be disturbing and significant in their own right and, therefore, are examined further in the rest of this chapter.

PHILOSOPHICAL CONSIDERATIONS OF MODELING

My advocacy of the Lagrangian plume model, the Projected Area Entrainment hypothesis, and the Negative Volume Anomaly has often been a frustrating experience. This has prompted me to analyze the process by which new ideas are brought forth before peers for scrutiny and evaluation. However, now that the Lagrangian model UM has been adopted by EPA, the PAE has been justified by Lee and Cheung, and a paper describing the NVA has been accepted for publication, the immediate frustrations have diminished.

But before then, I read books and articles and thought about peer review to make sense of my experience. I found that others also feel that they have been treated summarily by peer review. In fact, the peer review system appears to fail rather regularly and predictably when radically different ideas are presented. This is sometimes ironic, as for example in the case of ASCE which has a formal procedure for discussion. Such procedures are only effective when controversial ideas are allowed to be published in the first place.

As long as it preserves some semblance of its present form, peer review will always be an imprecise activity. The study by Ernst, Saradeth, and Resch (1993), which shows the variation in rankings by different reviewers of the same paper, confirms that fact. Nevertheless, I think relatively simple changes would help improve peer review. If we take our work seriously, peer review should be improved.

I think peer review could be improved and, because it has such pivotal importance to science and engineering, it would be worthwhile to improve it. Science would benefit. It could be said that the purpose of this section is to show that there are many other individuals who think so too. Hence it is loaded with quotes. In between, an effort is made to link them together in a philosophical way and to compare them to my experience.

Peer review is not always the benign, constructive force in science that our education leads us to believe it is. On the contrary, there are many destructive social undercurrents in peer review. To understand them, it is appropriate to consider some of the philosophical aspects of science in general and modeling in particular. This section winds through various philosophical themes associated with science to help put my views on science and peer review into a common framework. Among other ideas, it considers the implications of simplicity and sophistication, bias, complementarity, openness, controversy, and fun.

To begin, historically scientists have valued simplicity. Built into the scientific method is a tradition that is summed up by Occam's razor, that a minimum of assumptions be used to explain phenomena; i.e., the best of otherwise equally good explanations is the simplest one. Because it is based on a single, simple idea and requires no tuning, the PAE hypothesis conforms to that criterion.

But that tradition is disappearing in modeling. Unlike pure science, which tends to be analytical, modeling is synthetical. Instead of stripping away extraneous factors to identify the essence of a problem, models are aggregations of ideas. Typically, in plume modeling at least, both well known and less well known mechanisms, even hunches and guesses, are applied and used. In such an environment it is easy to lose sight of simplicity.

To use PAE and the Lagrangian plume model as an example, soon after their introduction (Winiarski and Frick, 1978) they began to attract criticism. Modelers did not like the Lagrangian approach, which, in its physics simplicity, was seen as unsophisticated, if not totally wrong. When equivalence between the approaches was presented to assuage that "concern", the criticism shifted to the alleged inadequacy of the PAE hypothesis. It was suggested that everyone knew the hypothesis was old (meaning obsolete) and had failed. Of course, the reasons for "failure" were not reviewed in the new light. Hence, what by virtue of its simplicity could have been considered a potential clarification of plume theory was kept out of the mainstream of the discipline on the basis of such ruses.

Of course, often the argument did not rise to even that level. To quote some of the critics by way of example:

> "[It] is exactly the way not to do plume models."

> "There were some concerns about the assumptions made, and the technical basis and validity of the [Lagrangian] model."

> "The only sound solution to the problem is [the one] that one of the authors did in a previous publication (Teeter and Baumgartner, 1979): to limit the valid range of the plume model."

> "The paper is, in fact, a critical discussion with an unfavourable [?] opinion of the so-called Eulerian and Lagrangian models.... But the authours have not presented an alternative except the introduction (a) of Eq. 19 [the corrected volume equation], *based on solid mechanics*, the justification of which is very much questionable...."

> "The almost trivial [Lagrangian plume model] ... has been superseded in a significant way by previously published work, [namely Schatzmann's, 1979],"

my emphasis. Contrast the latter quote to Cheung's (1991) findings. The objection to solid mechanics is also ironic in light of the demonstrated equivalence between the "objectionable" Lagrangian and the acceptable Eulerian integral flux approaches.

Synthesis and sophistication join forces to effectively usurp simplicity.

They can also hide bias. The objection to the Lagrangian approach suggests bias. The finding of bias is supported as a plausible explanation to observed reactions to the Lagrangian model. It should not come as a surprise that peer reviewers are often not open minded or objective. Campanario (1993) quotes Redner as saying "one of the roles of journals almost appears to be to sift out and reject really original contributions."

The conviction only came slowly to me that bias played a prominent role in the rejection of papers dealing with the Lagrangian model, the PAE hypothesis, and the NVA. At first I believed that the unfamiliarity of the Lagrangian approach was the primary impediment to understanding and acceptance. After all, the discipline was accustomed to solving plume problems with the Eulerian integral flux approach. The reasons that encouraged this viewpoint are forgotten, however, important individuals foster it. In *Fluid Dynamics*, G. K. Batchelor (1970) writes:

> "The Lagrangian type of specification is useful in certain special contexts, but it leads to rather cumbersome analysis and in general is at a disadvantage in not giving directly the spatial gradients of velocity in the fluid. We shall not need to use it in any systematic way, and it will be taken for granted in the following pages that an Eulerian specification is being employed. Nevertheless, the notion of *material* volumes, surfaces and lines which consist always of the same fluid particles and move with them is indispensable, and will often be employed within the framework of an Eulerian specification of the flow field."

Searching for acceptance, I developed an Eulerian plume model (Frick and Winiarski, 1975), adapted from Weil (1974), hoping to persuade my supervisor at the time, Larry Winiarski, to switch to it. That was the beginning of an effort that eventually led to an appreciation of various important principles of science. Ironically, it also had the immediate effect of winning me permanently to the Lagrangian approach which he advocated. This was because the two models agreed, as they should. As a bonus, the work identified an omission in Weil's paper and was published (Frick and Winiarski, 1975). However, after this auspicious start things became more difficult.

Even though the approaches are mathematically equivalent, they can be said to be complementary in the sense that together they give a fuller picture of the whole. In retrospect, it is apparent that the conventional Eulerian paradigm does not in itself give a good perspective for recognizing the existence and importance of the curvature and growth terms of PAE or the NVA. As a complementary viewpoint, the Lagrangian model, or paradigm, does.

Unfortunately, with respect to peer review, it is at the paradigm level that problems arise. This is because many scientists are proficient with only a limited number of them.

Bella (1987) defines paradigms as "a body of knowledge that provides disciplined guidance to community members." Thus, paradigms are shared by the community and give it a sense of unity. However, while paradigms provide a common framework and are often helpful in systematically developing a discipline, they can also blind its proponents to new ideas. Bella (1987) writes:

> "Paradigms thus give stability and rigor to the activities of a disciplinary community by directing members' attention to observations which can be addressed and evaluated by the community in an orderly manner. But these same filters may block out important observations. A community may be [averted] from critical observations and difficult questions that cannot be satisfactorily addressed under the guidance of its paradigms."

Bella (1987) thinks that the paradigm disciplines community activities at the same time that the activities generate innovations that modify or change the paradigm. A balance between the two is beneficial. Excessive discipline leads to a dogmatic system that stifles innovation. Too little can undermine the paradigm as fallacious ideas are adopted.

In any case, multiple paradigms can be beneficial. In fact, complementary descriptions are valued elsewhere in science, particularly in physics. Barbour (1966) writes:

> "The wave-particle dualism of electrons in the above experiment is a feature of other entities also. Light in some situations (for example, interference effects) behaves as a wave, in others (for example, photoelectric effects) as a particle. Bohr used the word "*complementarity*" to refer to such sharply contrasting

> concepts,...[Such systems are complementary], rather than contradictory, since they do not occur in the same experimental situation." (my emphasis)

Thus, there are precedents for multiple approaches to problems. One of the benefits of complementarity is that it makes the physics community more receptive to controversy because controversy arises naturally in discourse about the best way to approach a problem. Because both approaches are accepted *a priori* as being relevant, the chances of success are increased. To appreciate its importance, one need only think about how stuck physics was when either the wave or particle paradigms were essentially exclusive.

The ramifications of not sharing paradigms or not respecting parallel ones can be serious. Peer reviewers may not be qualified to judge work outside of their paradigm. Worse, they may not recognize the limits of their own work. Without some form of accountability, this is a formula for disaster because of the myopia that can develop. The practical consequences may be serious, causing, for example, the suppression of legitimate ideas. Inadvertent or not, peer reviewers insensitive to other paradigms effectively abuse those who would bring new ideas to the discipline from different paradigms. As an analogy, Frick (1993), in analyzing the influence of static and dynamic jurisprudence on the legal perception of child abuse, relates how traditional static paradigms allow monstrous abuses to occur simply because they are not recognizable within the dominant paradigm. The slave system is such an example, albeit one in which the injustices were always apparent to some members of the community, particularly mothers (Bella, 1994).

Hence, without understanding and recognition of the limits of paradigms, even if that is only in the abstract sense, abuse is inevitable. Healthy controversy, as an incubator of emerging ideas, is suppressed. Without it the discipline stagnates. On an apparent absence of progress in hydraulics Bugliarello (1972) writes:

> "If one holds to Karl Popper's view, that science progresses through controversy, it becomes evident that the fields of hydraulics, hydraulic engineering, and fluid mechanics have experienced remarkably little controversy over the past 50 years. ...at the time when other fields in the physical as well as the social sciences have undergone enormous revolutions."

A lack of controversy, by which I mean a lack of healthy, open discourse in the journals, may thus be an indicator of a system in which bias and abuse exists. Controversy implies the existence of alternatives, and vice versa. An open system fosters alternatives, which emerge naturally in science. To maintain that major scientific disciplines cannot support alternative viewpoints is ludicrous. As creative individuals, scientists constantly spawn new ideas, many of which are ultimately discarded as unworkable. Nevertheless, these are the fertile fields upon which new directions are built. They are Kuhn's Revolutionary Science (1970). If peer reviewers have little toleration for alternative viewpoints, the discipline stagnates because new ideas are systematically squashed. A lack of openness and concomitant intolerance contributes to stagnation and bias. Controversy remains suppressed although the victims are aware of misconduct, bias, and abuse. Koestler (1964) writes:

> "The history of science abounds with examples of discoveries greeted with howls of laughter because they seemed to be a marriage of incompatibles — until the marriage bore fruit and the alleged incompatibility of the partners turned out to derive from prejudice."

and,

> "One of the conspicuous handicaps is the conservatism of the scientific mind in its corporate [e.g. peer review] aspect. The collective matrix of a science at a given time is determined by a kind of establishment, which includes universities, learned societies, and, more recently, the editorial offices of technical journals. Like other establishments, they are consciously or unconsciously bent on preserving the status quo — partly because unorthodox innovations are a threat to their authority, but also because of the deeper fear that their laboriously erected intellectual edifice might collapse under the impact. Corporate orthodoxy has been the curse of genius from Aristarchus to Galileo, to Harvey, Darwin, and Freud; Throughout the centuries its phalanxes have sturdily defended habit against originality."

McCutchen (1991) is even more unequivocal: "Peer review is a jungle."

Becoming credible becomes the dilemma scientists face trying to establish novel and creative ideas when their derivations are whimsically resisted, proof is still fragmentary, and a forum is denied. Unable to place ideas in a conventional framework, or unwilling to disguise them, as Mandelbrot did with his pioneering work on chaos

(Gleick, 1987), how does one claim they are rigorous and compelling if the established community is intolerant and unwilling to take risks?

The viewpoint from within the established community is quite different, which is part of the problem. The establishment considers itself informed and enlightened even as outsiders confront a closed-minded orthodoxy. Peer reviewers see themselves as the guardians of The Truth, not guarantors of legitimate standards. Abuses are not apparent within the paradigm. While peer review should assure written quality and discover obvious errors which might distract from a good technical discussion, it should not impair sustained discourse.

In his book, Kuhn (1970) breaks science into two parts, incremental science, or normal science, in which the dominant paradigmatic skeleton is systematically fleshed out by more or less expected findings, and revolutionary science which presents unexpected results (including anomalies) and is associated with paradigm shifts. Koestler's orthodoxy might be compared to normal science while the outsider represents revolutionary science. In apparent anticipation, Koestler (1964) writes:

> "The new territory opened up by the impetuous advance of a few geniuses, acting as a spearhead, is subsequently occupied by the solid phalanxes of mediocrity; and soon the revolution turns into a new orthodoxy, with its unavoidable symptoms of one-sidedness, over-specialization, loss of contact with other provinces of knowledge, and ultimately, estrangement from reality. We see this happening — unavoidably, it seems — at various times in the history of various sciences. The emergent orthodoxy hardens into a 'closed system' of thought, unwilling or unable to assimilate new empirical data or to adjust itself to significant changes in other fields of knowledge; sooner or later the matrix is blocked, a new crisis arises, leading to a new synthesis, and the cycle starts again."

From this perspective, "normal" in plume modeling is orthodoxy, represented by a rigid established paradigm (recall Bugliarello, 1972) maintained by peer review. It is a closed system in which blocking of alternative matrices has been institutionalized, largely by an unaccountable peer review system (McCutchen, 1991), but also by the excessive dependence on dimensional analysis, hasty tuning, and habit.

> "To undo wrong connections, faulty integrations, is half the game. To acquire a new habit is easy, because one main function of the nervous system is to act

as a habit-forming machine; to break out of a habit is an almost heroic feat of mind or character. The prerequisite of originality is the art of forgetting, at the proper moment, what we know [....] Without the art of forgetting, the mind remains cluttered up with ready-made answers, and never finds occasion to ask the proper questions." (Koestler, 1964)

In this light, the intuitive sense that mentors show for "new blood" is understandable. Engineers and scientists have forgotten to distinguish the difference between ideas that are valuable because they are simple and work and ideas developed in haste in response to a short term need. However, hastily contrived work is most harmful when it is used as a ruse for rejecting truly original work..

And, while this is a subjective judgement, it seems to me that beauty, elegance, and fun have been sacrificed on the altar of the peer reviewers' Truth. Koestler (1964) writes that Dirac once criticized Schrödinger for adjusting his equations to fit an experiment. Later, the original, simpler, and more elegant theory proved to be correct:

> "I think there is a moral to this story, namely that it is more important to have beauty in one's equations than to have them fit experiment." (Koestler, 1964)

Beauty is subjective and can be overstated. Nevertheless, in modeling ad hoc inventions, the trappings of "normal" plume modeling, should be kept in perspective and let go when more basic and simpler, and more beautiful, concepts are shown to work as well or better. Beauty and simplicity are related. There needs to be a commitment to being willing to back up to a previous level of ideation and to start over. In other words, science would be well served by an established community open to new ideas, even paradigm shifts. It should recognize that the clues to an emerging, potent, new idea are likely not to be overwhelming scholarship and evidence in the normal science sense, but simplicity and beauty.

But, in the final analysis, perhaps the simplest measure of a properly functioning science is the quantity "fun."

> "'It is remarkable', wrote Laplace, 'that a science which began with considerations of play has risen to the most important objects of human knowledge.' Thus at the very start of our inquiry we hit on a pattern — the discovery that a playful or *l'art pour l'art* technique provides an unexpected clue

to problems in a quite different field — which is one of the leitmotifs in the history of science." (Koestler, 1964)

Many of us experience excitement, fun, joy, and occasionally even ecstacy when doing science. They are the times devoted to studying problems, developing ideas, making discoveries, and even documenting them.

But, we also experience times of doubt, despair, and disillusionment. These are times after the reviews come back, reading venomous reviews, absorbing fatuous insults from anonymous reviewers, and wasting time reworking, rewriting, and resubmitting aging ideas. The Dr. Jekyll and Mr. Hyde sides of science. Regrettably, like the troughs between the crests of ocean waves, the bad times seem to last much longer than the good ones.

Why do we treat each other this way? Are scientists and engineers naturally immature and nasty? Or, is there something about the system that causes us to treat each other this way? In my correspondence to *Science*, given in Appendix 4, I tried to address these questions. In its own way, the ad for the book "A Ph.D. is not enough! A guide to survival in science" addresses them (Feibelman, 1994). In the remaining part of this chapter I try to shed more light on these issues.

BIAS IN PEER REVIEW

As is stated in the previous section, with regard to difficulties publishing ideas related to the Lagrangian plume model, PAE, and the NVA, I finally concluded that they derived from bias on the part of peer reviewers. The general literature provides some substantiation of that conclusion. In this section, specific instances of bias are examined.

There are various styles for tackling the issues. In one, the formal, dispassionate style of science, my experience can be summarized like this: the *Journal of Hydraulic Engineering* rejected the paper describing the Negative Volume Anomaly.

But science has many faces. Gary Miranda (1991) describes a form of double speak in science. He finds formal and peer review styles of discourse, the latter characterized by rhetoric, personal involvement, and passion. Forgoing all euphemisms, we might add ignorance, maliciousness, and bias, in other words, the whole gamut of human emotions. The peer review style appears in the rejections under consideration here. To use it to express the previous paragraph, I believe that it would be more accurate to say the paper was ridiculed and stonewalled.

There are many parallels. For example, an important paper which was first rejected outright is discussed by Companario (1993):

> "The prestigious *Journal of Chemical Physics* initially rejected Henry Eyring's classical 1935 paper on the activated complex in chemical reaction. The referee summarized his conclusions with these words: 'I have given considerable thought to the problems involved, and although I have not been able to resolve my uncertainties I have nevertheless become convinced that the method of treatment is unsound and the result incorrect.'"

One might ask why this reviewer was unable to transcend his personal boundaries? What were his loyalties? Is it conceit, closed-mindedness, or another conceptual or spiritual handicap that causes peers to impose their unscientific biases on the unsuspecting general community? An appropriate conclusion might have been: "Since I am unable to believe the ideas but am unable to find an error in the work, I should abstain from this review or recommend publication to allow the general community to debate its merits." This approach would reflect the attitude that the reviewer's job is to assure written quality and to identify errors that might distract from a good technical discussion in the journal. It is not the reviewer's job to act as guardian of the Truth, i.e., to create an imbalance between discipline and innovation that will result in dogmatism.

The review process at the *Journal of Hydraulic Engineering* (JHE) consisted of two distinct events, the initial review and a re-review. There was a re-review because we, the authors, decided to challenge the initial reviewers. It seemed expedient to do so, especially since we trust the editor. Thus we determined to engage the critics with rebuttals that would compel them to defend their criticisms rigorously or secure their consent to publish.

In retrospect, our expectations were idealistic. In reality, the editor chose to send the revised paper to new reviewers, thus the re-review. In my opinion, this act was an attempt to overcome what he suspected was a biased initial review. Perhaps he suspected that a challenge would only harden their positions. In that sense the action was positive and supportive. However, the new reviewers were given the rebuttals which clearly defined the dispute to them.

When he informed us of his actions we had misgivings, but being busy at the time, and hopeful, we acquiesced to the decision, which, in any case, had already been exercised. The outcome was probably predictable but proves useful for the purposes of this thesis. For me it is no longer difficult to imagine that, acting under a misplaced and inappropriate sense of loyalty to their peers, instead of to science, or, simply sharing the same biases, the new reviewers closed ranks behind their colleagues and accorded the paper the same treatment — unanimous rejection based on superficial pretexts (Appendix 3).

However, there was a notable exception. Of the three re-reviews, one, who will be called AA, is noteworthy for the effort expended by the reviewer. I think this individual was torn by conflicting emotions and beliefs. He wanted to be fair. In fact, he chastised one of the original reviewers for malevolent and careless criticisms:

> "The reviewer 'A' general comments seem to me to be totally off-the-wall. It is as if he or she only skimmed over the paper, missing its central point.... With not a single constructive comment, this is just a shoddy review; I feel sorry for the authors for having to bother to rebut it."

Of all the reviews, only AA fully acknowledges the central point of the paper, the anomaly, and struggles to come to grips with it. Yet, after elaborate argument, he concludes that negative volume is fine with him as long as it averages out to being positive!

> "I disagree with the authors' suggestion (more a demand, really) that what they call 'negative volume' should be left out of integral models.... The authors label this segment 'negative volume' and think somehow that the mathematically correct thing to do is to ignore it, i.e., leave it out of the plume integration....

> "However, all of the models under discussion assume for computational purposes that plume properties are symmetric with respect to the plume centerline....

> "[While defending earlier approaches] we do not have to artificially assume Δs = ds everywhere (i.e., assume parallel planes). The larger Δs on one side of the

> centerline is counterbalanced by the smaller Δs at an equal distance on the other side; even if $\Delta s < 0$ on one side and $> 2ds$ on the other side, their average will always be $= ds$;... When $\Delta s < 0$ regions are left out of the integration, this balance is destroyed, just like one person falling off the seesaw. Thus, what the authors see as a correction, leaving out 'negative volume', I see as an incorrection."

From my vantage point, this review is incredible. The reviewer accepts the notion of negative volume, ignoring the fact that negative volume multiplied by (positive) density yields negative mass! Globally or locally, the concept of negative mass is unknown in physics (a higher paradigm, in this case). It cannot be "averaged out" by arbitrarily adding (integrating in) extra mass to the equations. That we are unable to expose, through publication, such erroneous criticism is inconceivable to me. What good is the elaborate edifice for formal discourse in place at ASCE when the protagonists are barred from the forum?

It may be that the reviewer is rejecting the idea as a practical matter, although it does not appear to me to be the case. Of course, without knowing the extent of the effect of the anomaly on model simulations, it would be unwise to prevent it being debated on that basis. It is hard for me to imagine any other reason than bias to explain the rejection. To me, the seesaw analogy, for that is all it is, is simply a desperate rationalization for justifying preconceived concepts of what plume modeling should be. It is reminiscent of Plato's argument establishing Truth which Pirsig (1974) showed to be simply an analogy.

The experience at JHE presented a case of déjà vu. It was similar to my experience with the same journal over publication of the PAE hypothesis described earlier. That time the original version of a paper later published in *Atmospheric Environment* (Frick, 1984) was rejected on the basis of the reviews given in Appendix 2. As explained previously, that judgement has been totally discredited by Cheung (1991) and Lee and Cheung (1990) who demonstrate the basic model's superiority to Schatzmann's model. Furthermore, even at the time it was rebutted successfully. In a rebuttal I pointed out that Schatzmann had explicitly discounted curvature as contributing to entrainment whereas the paper showed it was significant.

From my readings and discussions with colleagues, it is clear that many scientists react to such setbacks with resignation. More likely they feel anger tempered by fear, for their careers can be seriously sidetracked or even ruined by such unwarranted attacks, which are, unfortunately, an occupational hazard imposed by the system upon hapless scientists. Hopefully, most determine to resubmit elsewhere. We are no different. The revised paper given in Appendix 1 was subsequently submitted to the International Association for Hydraulics Research (IAHR) for publication. It followed an unsuccessful effort to first persuade a senior editor of JHE to intervene benevolently in the review, see Appendix 4. This was done more to please a supporter than out of any hope for overturning the unanimous rejection. A reaction to his defense of the review system is also given in Appendix 4.

Before choosing IAHR we took the unusual steps of soliciting a few hydraulics and related journals about the likelihood of being published, given the abstract page to examine. This proved to be a good way to assess the review climate and receptiveness at various journals. Most queries were promptly answered, generally within three weeks. Letters and responses are given in Appendix 5.

In the case of the ASCE *Journal of Environmental Engineering* (JEE), the response was apparently delayed by the editor's background check into the causes for our letter, which mentioned the unsuccessful review at JHE. He actually contacted both editors involved in the JHE review to personally acquaint himself with the facts of the case. His supportive letter is given in Appendix 5. By the time it was received we had already decided to submit elsewhere.

The IAHR review is also presented in Appendix 5.

Some personal attempts to come to grips with events are given in Appendix 6. The effort I have expended in writing and rewriting convince me that I am not the best person to develop this topic. That is why, ultimately, this material was placed in appendices.

I came to the conclusion that efforts to understand the problem scientifically (like Kuhn, 1962, attempts to do), while being helpful, are unlikely to resolve the problems alone. Perhaps scientific revolutions, like political ones, are largely rooted in the relational agitation of "ordinary" scientists. Recognition of an abusive system will come from without peer review. Drawing the parallel to the development of opposition to slavery (Bella, 1994) seems appropriate.

Not that there will not be "normal" scientific efforts to reform peer review. This is part of the agitation. For example, a biomedical conference held recently focuses on peer review and presents solutions that are, it seems to me, incremental (normal science). Its authors support or mildly criticize peer review (Rennie, 1993). Some defend vested interests, while others are critical. A well represented topic deals with blinding and double blinding of reviewers. But no amount of blinding would have helped us because the bias is directed against the approach, not necessarily against the individuals. In fact, the lack of blinding may have helped Lee and his colleagues publish their work because their strong engineering credentials may have kept their critics from indulging their biases.

REFORMED PEER REVIEW

Bias is a fact of life in scientific peer review. It exists simply because science is a human enterprise and humans are known to harbor biases. That fact is not likely to change although it may become more or less true with time.

However, the mechanics of institutional peer review could be adjusted rather easily to decrease the consequence of bias. Relatively minor adjustments in peer review policies and procedures could rectify the worst peer review abuses. Simultaneous submission to journals and signed reviews are just two ways to try to help eliminate the worst effects of bias.

To help allay the fears of those who think that simultaneous submission would swamp the system, it could be combined with the obligation to inform each journal of other journals receiving the submission, allowing editors to decide whether or not to participate. Other suggestions to improve peer review and citations are given in Appendices 1, 4, and 6.

Sometimes peer review is defended on the basis that it depends largely on volunteer efforts. For example, Liggett (1993, Appendix 4) points out that JHE is so dependent. The implication is that authors should have more empathy for the "plight" of reviewers and tolerance for their decisions. However, it should not be assumed that simply because the system is established and staffed by largely well-meaning individuals that it is beyond criticism.

The institution of peer review has been shaped by the conscious, and unconscious, effort of humans. As such, not all of its rules have been designed with only the greatest good of science and scientists in mind. Certainly, at least commercial and personal interests have played a role. Even where institutional policies are the product of careful thought and reflection, they can become ineffective and harmful over time.

Revolution is only possible when an imperative for action exists, that is, when outrage sweeps away conventional impediments to decisive action. Whether and when it will happen in science is unpredictable. It will more likely be the results of relational discourse than of careful analysis. One reason why revolutions are not facilitated in science is given in the next section.

However, assuming that peer review were revolutionized, several practices and traditions would surely change radically. The concept of anonymity would very likely be changed. As things stand, anonymity provides opportunities to eliminate rivals with little justification and with very little risk. It helps empower those individuals willing to manipulate the system to promote their own limited aims. It may be the most important single privilege of peer review that, because it is easily abused, leads to mediocrity, stagnation, and abuse.

In apparent recognition of its destructive potential, some reviewers routinely sign their reviews (so I am told by colleagues, I have not seen one yet). However, while anonymity has created the antithesis of an open system and changes are needed, it does not follow that anonymity must be abandoned. The important concept is openness, not anonymity. In a reformed system anonymity might not prove corruptible and subversive.

For example, currently journals exercise power over authors by insisting that papers be submitted to only one journal at a time. This places those who present unpopular ideas and are rejected in a position of weakness by forcing them to submit to multiple consecutive reviews, each of which may takes months or years to complete. In this climate peer review anonymity has a devastating effect on the consideration and contemplation of new ideas. If, instead, authors were allowed to submit to multiple journals at once but were honor-bound to publish in the first one to accept their work, just as they are now honor-bound to submit to one journal at a time, the entire balance of power would be changed. More thoughts on this issue are presented in Appendix 4.

The way things are now it may be months or years before papers can resubmitted and published elsewhere, if ever; it is enough time for writers of shoddy reviews to escape responsibility. But, if other journals received the papers simultaneously, revolutionary science would come to light more quickly and it would be easy to discern bias in various peer review circles.

Some more or less revolutionary solutions for diminishing bias and other problems in peer review are offered by Campanario (1993), McCutchen (1991), Roy (1993), and others. See also Appendix 4.

THE INTELLIGENCE DEFECT

One last topic is worth developing before returning to plume modeling. It can be summed up by a question: why is a deficient peer review system maintained and perpetuated? It is one of the paradoxes of life that strength is often associated with weakness, and weakness with strength. It can be argued that the very central strength of science, the intelligence and creativity of individual scientists, is also one of its important weaknesses.

Consider the simple utterance of Nelson Kiang, who is quoted by Taubes (1993) writing on misconduct in science:

> "You can take a terrible system and if you put the right people in it, they'll somehow see something decent comes out of it, [but, better still is a system that] won't require an excessive sacrifice on the part of the members to do right."

This comment strengthens the complaints about peer review given previously and helps justify a call for reform. But, more importantly, it gives a clue to the reason why peer review generally escapes serious efforts to reform it, why, even though its problems are serious but simple, little is done to rectify them.

The reason is that often the "right people" are found to "see something decent comes out of it." Many scientists are devoted individuals who act diligently and cleverly, but alone, to overcome abuses. Whichever side of review they are on, they patch and repair, dealing with the symptoms of the problem. Being intelligent people, they often succeed in overcoming a bad system. But, as a result, little attention is paid to the underlying causes of system failure.

If I am typical, many scientists dream, or once dreamt, of being heroes. In fact, one could argue that there is in our society a hero mystique. Unfortunately, science needs heroes, and fortunately (I think) it seems to attract a disproportionate share of them. But, implicit in the mystique is system failure. Heroes are only needed when things go wrong. May there always be heroes in times of need, but better are systems that work without them. Peer review can be reformed to require less sacrifice on the part of the members to do right.

The irony of science is the inability or unwillingness of scientists to generally perceive that there are problems with peer review. As authors, they tend to have the ability and creativity to work around the system, in other words, to overcome it. They succeed despite the system. As reviewers and editors they often soften the edges of the system. But, as a result, they tend to dismiss, diminish, or forget the problems that plagued them. It becomes a rite of passage, a form of machismo.

Part of the problem is that scientists tend to apply analytical techniques to all problems. An analytical process would show that the necessary mechanisms exist at ASCE to assure that peer review is conducted properly. It is only the individuals who suffer from the system who complain and they are assumed to be complaining because they were rejected. The system effectively insulates itself from criticism. Bella (1994) writes:

> "Within the slavery system ... anything that went wrong was taken as evidence of the inferiority of slaves, thus justifying the slave system. Anything that went right was taken as evidence of the effectiveness of the system, thus justifying it. To people caught up within the system, all this seemed quite reasonable. The real question we face is this: What kind of evidence is required to expose the character of such systems and show how ordinary people become caught up within them?"

He goes on to show that recent scientific analysis of the slavery system would not have brought about its demise, however, relational evidence and discourse did. It was the arguments of many ordinary people, particularly mothers, who exposed the idea of slavery as unjust, not those of academicians.

The evidence to support this viewpoint is often vague and indirect. The psychology is probably similar to that of losing at chess. As cold analysts, there are nuances and taboos that we simply cannot address forthrightly. For example, consider J. D. Watson's (1993) five rules for succeeding in science (the first and fifth of which are considered further in Appendix 4). They are: (1) You have to avoid dumb people; (2) A scientist has to be prepared to get into deep trouble; (3) Be sure you always have someone up your sleeve who will save you when you find yourself in deep s—; (4) Never do anything that bores you; and (5) If you can't stand to be with your real peers, get out of science.

With the exception of Rule 4, they effectively reveal the dilemmas that dysfunctional scientific institutions impose upon individuals. While Watson is serious about the subject, he speaks euphemistically. For example, he says "dumb people," not bad, incompetent, arrogant, bigoted, or malicious people. He uses humor, because everyone enjoys wit, but perhaps also to enable him to speak about a subject that the collective machismo has made a taboo. In this way he lowers our inhibitions associated with this important, but neglected, topic. His personal approach helped him succeed and may help the voracious and wise reader, but it does little for the uninitiated scientist with a revolutionary idea facing a biased and insecure peer review panel.

If peer review is a bad system, there is nothing noble about maintaining it unchanged. The fact that most scientists and engineers are highly educated and ethical people who have the ability to make bad systems work most of the time is not a reason for condoning them.

It would be better to expand than to restrict the commerce of ideas. It is not that changing peer review procedures to allow authors to submit to multiple journals at once is necessarily the idea that will best optimize the way the system works, but it is easy to see that it would dramatically change it. What is lost if a subset of journals agrees to try it? Does anyone really believe that science will suffer from more of us participating in the general commerce of emerging ideas? (Further thoughts along these lines are given in Appendices 4 and 6.)

The need to construct and maintain objective and fair institutions transcends in importance the game playing, frontier attitudes, and the collective machismo of the community.

LOOKING BACK

Any work such as this one has a beginning, middle, and end that suggests a linear chronology was followed in its composition. Of course, that is not always the case; this chapter was written and revised last.

I have said I am not the best person to make the case I have made. I still believe that. But, considering Bella's argument (1994) it is clear how important relational discourse is for rectifying certain attitudes and injustices. It is not necessarily dependent on the pronouncements of the best intellects, in fact, they are likely to be oblivious to the problems identified here. Thus, in retrospect, my coming to the conclusion that Kuhn's work was basically inadequate in my quest for redressing grievances, seems now to make more sense. Scientific revolutions come about less from a logical progression of analytical arguments than they do from the consensus that develops through relational intercourse. This reflects the wisdom of he who observed that sometimes the science does not change until the old stalwarts die.

My thesis defense served to show how incomplete it is. Ideas emerged that deserve to be considered further but, if for no other reason than for lack of time, will not be. This is not the last word, only a small contribution to those who look at the system critically, understand its injustices, and seek to improve it. Most scientists seem to love science and want to help.

Let me summarize briefly a few ideas. I found formal review frustrating. It was difficult to establish the real objections anonymous reviewers had to the Lagrangian approach, the PAE, and the NVA. The system did not facilitate sustained discourse to identify them. It did not foster dialogue but antagonisms. The rebuttal process went unsupported. There was no way to tell if disagreement was legitimate, or whether there was a lack of interest or vested interest. There was no accountability.

What acts to salvage the situation is the informal process. The slow and uncertain process of gaining support by word of mouth, by finding mentors and patrons. In my case I was able to sustain sufficient momentum to carry me to this point. But, pity on those who are less fortunate. Consider the plight of those affected by the system at the formative stages of research. Unable to obtain support, their exposure to conferences and other organs of discourse are likely to be limited.

We selected the ASCE *Journal of Hydraulic Engineering* because we thought it was the most appropriate journal. ASCE represents our users more than any other organization and publisher. It embodies our paradigm and community. That fact makes events only more incomprehensible. Based on our experience, I think that it is clear that its discipline is exercised excessively rigidly, thus inhibiting innovation.

CONCLUSION

Plume modeling is a legitimate branch of science since some of it is based on physical principles and the resulting models are able to fairly prognosticate plume behavior. In fact, the Lagrangian model described herein, and equivalent Eulerian integral flux models, are able to predict plume behavior to a useful level of accuracy without tuning. Several critical reviews and independent efforts corroborate these conclusions.

However, while the Lagrangian plume model is useful as a tool for analyzing plume behavior, its use and development is resisted by the established plume modeling community. Rejection of papers on central modeling ideas, such as the Projected Area Entrainment (PAE) hypothesis and the Negative Volume Anomaly (NVA), are considered examples of peer reviewer bias, even though part of the observed reaction may be attributed to the way the ideas were presented.

It is known that major human enterprises are generally subject to human emotive forces. That is, the reaction to new ideas is not based solely on objective criteria and principles and cannot be predicted simply on that basis. To a degree, it is inevitable that all human institutions are fallible and imperfect.

However, beyond some level the imperfections are unacceptable. In science, the existence of pervasive bias defines this threshold. Specific instances to support the charge that part of the modeling community, namely peer reviewers, is biased against the Lagrangian plume modeling approach are given above. Numerous independent sources support the claim in general. Its existence adversely affects the otherwise positive rewards that scientists reap from their activities and diminishes the potential benefits to be derived therefrom by humanity.

While many recognize the pernicious effects of bias in science, the very competence of scientists makes it harder to attack the problem in a systematic fashion. There is an intelligence defect: the ability of scientists to work around problems actually interferes with their ability to recognize the problem and to find effective solutions.

The science community must stop empowering those who abuse the peer review system. A deficient system must not be condoned. The adverse effect of the intelligence defect should be recognized.

Excessive secrecy enables abusive peer review practices to occur. Relatively simple measures would make individuals accountable for their actions and improve the system, even without significantly altering the principle of anonymity. Allowing authors to submit to multiple journals simultaneously would be one such change.

At one time it may have been that humanity had the luxury of coexisting with byzantine and malevolent scientific institutions. That time has passed. If science adopts the general mores of society, where power, position, and wealth prevail, it will be to the ultimate disservice, perhaps even destruction, of humanity. For example, Bruce Rich (1994) speaks of the necessary "transparency" of institutions (like the World Bank) in order to respond to the impending environmental crisis. Once science was the inspiration for the Age on Enlightenment, a time in which the relative openness of science inspired the political sector to overcome the intrigues and injustices of an age in which ignorance, privilege, and unaccountable power prevailed.

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It is important to find ways to make science once again the institution for others to emulate.

CHAPTER 3. LAGRANGIAN PLUME MODEL THEORY

PERSPECTIVE

To understand the Lagrangian plume model and the controversy it has generated it is helpful to understand what could be called the basic model. The basic model is very similar to the UM, UMERGE, UOUTPLM, and OUTPLM models adopted by the U.S. Environmental Protection Agency (EPA) (Baumgartner, Frick, and Roberts, 1993; Muellenhoff et al., 1985; and Teeter and Baumgartner 1979). They differ slightly in the definition of entrainment. This chapter describes the basic dynamical equations and some of the numerical techniques and protocols that comprise the basic model. The main source is the UM model (Baumgartner, Frick, and Roberts, 1993), the latest EPA Lagrangian model.

The original models were developed for atmospheric and freshwater applications by Winiarski and Frick (1976). Teeter and Baumgartner (1979) adapted the model to marine applications and called it OUTPLM. This single source model became the basis for the multiple port diffuser merging plume model, MERGE (Frick, 1980). In turn, both models were modified and called UOUTPLM and UMERGE respectively (Muellenhoff et al., 1985). Most recently, UMERGE was generalized and enhanced further to include negatively buoyant discharges and background pollution. These improvements are included in UM. Other research focusing on the generalization to three dimensions and to geothermal applications continues (Frick, Baumgartner, and Fox, 1993).

UM and its predecessors share two distinguishing characteristics: the Lagrangian formulation and the projected area entrainment (PAE) hypothesis. The Lagrangian formulation offers comparative simplicity that is useful in developing the PAE. The projected area entrainment hypothesis is a statement of forced entrainment — the rate at which mass is assimilated by the plume in the presence of current. The basic idea for PAE is not original, as it was described in general terms at least as early as 1960 (Rawn, Bowerman, and Brooks). It is frequently used in incomplete form (e.g. Hoult, Fay, and

Forney, 1969) as explained by Frick (1984). In contrast, the Lagrangian models contain the first accurate expressions of PAE.

The traditional Taylor entrainment hypothesis (Morton, Taylor, and Turner, 1956) is also used and is the dominant source of entrainment under low current conditions.

The model was first verified by Winiarski and Frick (1976, 1978) and Frick and Winiarski (1975). It has been verified further or compared to other models by Frick (1980), Alam, Harleman, and Colonell (1982), Frick (1984), Baumgartner et al. (1986), Lee, Cheung, and Cheung (1987), Lee and Cheung (1990), Brandsma (1993), and others. The most extensive verification is found in Cheung (1991) who describes a three-dimensional Lagrangian model based on Frick (1984). This model is called JETLAG and is based on the works of Lee, Cheung, and Cheung (1987) and Lee and Cheung (1990). (The name LAGrangian JET model, or JETLAG, reflects the feeling they experience when they travel to the United States to present it). They show that the Lagrangian plume models using PAE predict the correct asymptotic behavior in a number of limiting conditions. Some of their conclusions are summarized in the previous chapter.

The atmospheric models have been verified by Carhart et al. (1982), Poliscastro et al. (1980), and Tesche, Jensen, and Haney (1979), and others.

Figures 4 and 5 provide an indication of the general quality of prediction. In Figure 4 the densimetric Froude number of the effluent is given by F_j: a measure of the ratio of momentum to buoyancy in the plume. Large Froude numbers indicate relatively high momentum while small ones indicate relatively strong buoyancy. The ratio of efflux velocity to current is given by k; a high value indicates relatively strong effluent velocity or low current speed.

Further evidence of performance is given by Frick, Baumgartner, and Fox (1993) which is reproduced in Appendix 1.

The Lagrangian model and its entrainment hypotheses are described below in some detail. To understand the model it is necessary to first have an appreciation of the basic model building block — i.e. the plume element. Given a firm appreciation of the plume element, the dynamical equations, conservation principles, entrainment hypotheses, and merging principles are more easily understood.

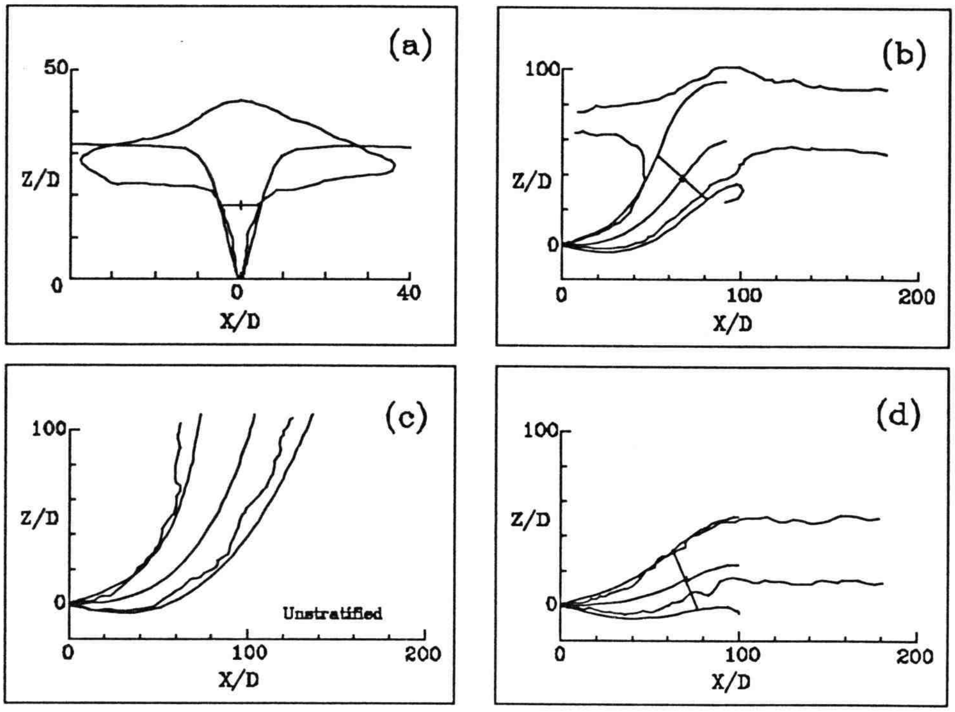

Figure 4. UM centerline and boundary predictions in stagnant ambient compared to Fan (1967). (a) Jet 10, (b) Jet 16, (c) Jet 22, unstratified, and (d) Jet 32.

BASIC LAGRANGIAN PLUME PHYSICS

The Plume Element

Lagrangian plume models differ from Eulerian integral flux models in that the basic modeling construct is a moving control volume which, in the ideal, continues to contain the molecules found in the element when it is first defined at the source. In contrast, the Eulerian integral flux models are built around fixed control volumes through which mass fluxes.

Both plume models are so-called initial dilution models, that is, they are valid near the point of discharge. Traditionally, they are considered to be valid between the point of discharge and the trapping level or surface, whichever is reached first. The trapping level is the depth at which the average density in the plume first equals the surrounding ambient density. In other words, it is the depth at which the plume would

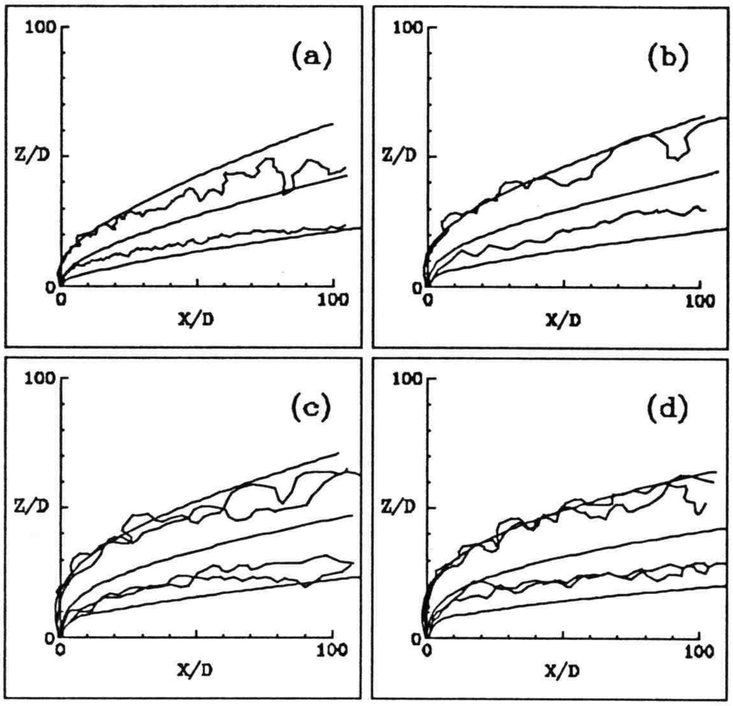

Figure 5. UM predictions in flowing ambient compared to Fan (1967). (a) F_j=10, k=8, (b) F_j=20, k=12, (d) F_j=40, k=16, and (d) F_j=80, k=16.

be in static, buoyant equilibrium in the absence of momentum and secondary forces. One of the goals of this work is to extend the region of validity of the models beyond the trapping level to the point of maximum rise. Roberts, Snyder, and Baumgartner (1989) and others believe that such an extension is feasible.

The shape of the element has important consequences on the dynamics of the plume in the initial dilution region because it determines the projected area of the element to which forced entrainment is directly proportional. That is what the Projected Area Entrainment (PAE) hypothesis is all about. Together with Taylor entrainment, they determine the growth of the element due to the assimilation of mass, and therefore, through the m in F = ma, determine the dynamics of the element center-of-mass, i.e. the element particle.

It is important to note that the shape of the element is not determined by the model. This is an important characteristic of not only Lagrangian models but Eulerian integral flux ones as well. The shape is specified by the modeler, consequently, as will be shown, the term fluid dynamics is a misnomer, for both models. Consider Figure 6 which shows the plume element in three stages of development. The Lagrangian model provides only an estimate of the element trajectory, i.e. s, the path of the center-of-mass of the plume element. It is shown as a solid line passing through the centers of the elements as if all the mass of the plume element were concentrated there. It does not independently predict the boundaries of the element. In other words, it is a simple particle model. As a result, since the Lagrangian and Eulerian integral flux models give equivalent results, given equivalent conditions and assumptions, it must be concluded that Eulerian integral flux models are also simply particle models.

The fact the both Lagrangian and Eulerian integral flux plume models are simple particle models is one of the major conclusions of this thesis. It is also believed to be the source of resentment and bias that the Lagrangian model elicits and encounters. It is a profound irony that critics of the Lagrangian model do not appreciate this fact.

In both formulations, the motion of the particle (the plume element's center of mass) is the only dynamic variable that is predicted by the plume model. Everything else must be assumed, inferred, or created. The shape of the element is established arbitrarily before the growth of the particle (i.e. entrainment) can be determined. In other words, the modeler determines how the shape of the plume is specified. Normally, a particular interpretation of the round plume assumption is used to establish the distribution of mass about the trajectory of the plume element; i.e. the plume element is usually assumed to be cylindrical in shape.

However, if the element is defined by a smooth surface on the exterior of the plume and by interior planes, or faces, that are perpendicular to the particle trajectory, and if the plume trajectory is curved, as it generally is, then an element exists that is not cylindrical but has the shape of a section of bent cone. Because the length of the element along the trajectory must be small for mathematical reasons, it is better to conceive of the element as a thin round wedge with a blunt or sharp edge. This is the element form assumed in UM. When the trajectory radius of curvature is smaller than the plume radius, a line of intersection develops, a physically unrealistic situation if the

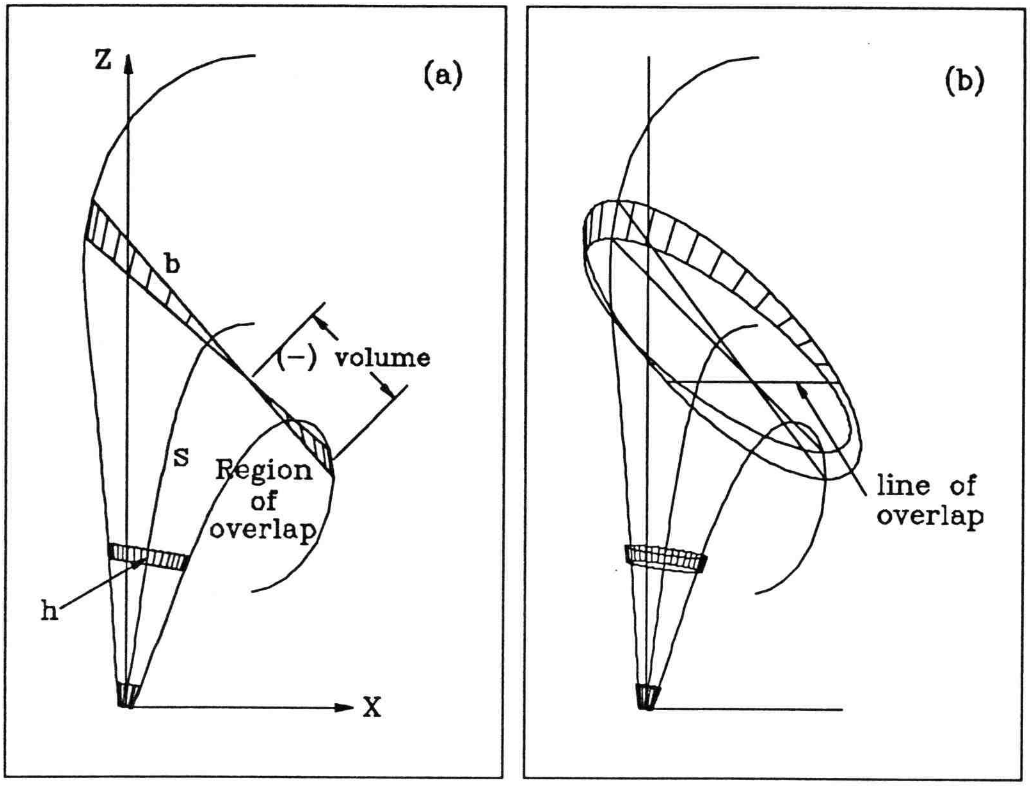

Figure 6. Plume trajectory, the element at three stages of development, and selected plume variables.

round shape is maintained at all costs. In other words, the two defining plume element cross-sections overlap, or actually self-overlap, since neighboring elements can also overlap. The concept of self-overlap, which is referred to as simply overlap (as opposed to overlap of neighboring elements), is equivalent to the NVA.

Secondly, the asymmetry in shape is not consistent with the general practice of symmetrically constructing a circular plume cross-section centered on the trajectory. Instead, it is recognized, and considered in a subsequent chapter, that the plume trajectory represents the center-of-mass of the plume element which generally is not at the center of the circular cross-section.

In other models the problem of facial intersection is not treated, or even recognized. For example, if the element is assumed to be cylindrical, consecutive elements can not be positioned without overlapping each other in some places or without cavities emerging in others, i.e. without doing violence to the entire notion of mass continuity. These prohibitions apply to both overlap and overlap of neighboring elements (Cheung, 1991) though the latter is not treated herein. It is a serious deficiency, causing the plume radius and entrainment to be overestimated. UM issues a warning when overlap commences and, in its the default mode, terminates the initial dilution computation.

The plume is assumed to be in steady state. In the Lagrangian formulation that implies that successive elements follow the same trajectory. The plume envelope remains invariant while elements moving through it change their shape and position with time, each one experiencing identical motion. However, conditions can change as long as they do so over time scales which are long compared to the time in which a discharged element reaches the end of the initial dilution phase, usually maximum rise. The steady state assumption is needed to make it possible describe the length of the plume element (see Eq. 6) as a function of the instantaneous average velocity, its initial length, and the initial effluent velocity.

Thus, the length of the element does not, in general, remain constant but changes with time due to the different velocities of the leading and trailing faces. It follows that the radius of the element must respond to this velocity convergence or divergence, as well as to entrainment because the fluid is practically incompressible, though incompressibility is not assumed and the limiting Boussinesq approximations (Spiegel and Veronis, 1960) are not needed, an advantage of the Lagrangian approach.

The exterior surface of the plume element coincides initially with the edges of the orifice from which it issues (or the vena contracta diameter). By integrating from this known initial boundary condition the plume volume is calculated based on the entrained mass and the assumed element shape.

It is assumed that the properties of the plume at the boundary are indistinguishable from those in the adjacent ambient fluid. This has important implications, one being that drag is not an important force in plume dynamics. It also implies that mass crosses the projected area of the element at the speed of the ambient current.

Projected Area Entrainment (PAE) is a forced entrainment hypothesis that has been shown to work in moderate current without the need for a tuned coefficient (it is equal to unity, 1) (Frick, 1984). In cases of no current or light current the empirical Taylor entrainment hypothesis is needed as well. The Taylor hypothesis is an additive extra entrainment term which quickly diminishes in relative importance as current increases. While the model cannot be proven to be "correct", the fact that the theory predicts observed behavior well over a range of conditions without empirical tuning is not only desirable but persuasive.

Conservation Principles

The model includes statements of conservation of mass (continuity), momenta, and energy. Conservation of mass states that the initial mass of the element and that added, or entrained, over time is conserved. In modeling terms the element mass is incremented by the amount of fluid that flows over the outside boundary of the plume element in a given amount of time. The PAE prevents grossly excessive or inadequate entrainment which is one of the strengths of the hypothesis. Integrating the individual element projected areas confirms this fact. The PAE hypothesis dominates entrainment at moderate to high current speeds while the Taylor hypothesis is important at low current speeds (Roberts, 1977).

Similarly, horizontal momentum is conserved. The horizontal momentum, the product of the element mass and horizontal velocity, is increased by the horizontal momentum of the entrained fluid in the same time step. Vertical momentum, on the other hand, being subject to alteration by buoyancy, a body force arising from the density difference between the element and the ambient fluid, is not generally conserved.

Thermal energy is conserved and treated like mass and horizontal momentum, similarly incremented by adding an amount of energy equal to the product of a constant specific heat, the entrained mass, and the ambient temperature. Dissipation of kinetic energy, conduction, radiation, and other energy transfer mechanisms are neglected. An equation of state is used to obtain the densities of fresh and sea water in salinity and temperature ranges that are representative of terrestrial and coastal waters.

Entrainment and Merging

Entrainment is the process by which the plume incorporates or assimilates ambient fluid. It may be thought of as a process in which fluid flows into the plume interior through the exterior surface or as accretion followed by the redistribution of material. The former model is used here. Both are consistent with the projected area entrainment hypothesis but the latter form implies a redistribution of mass and a drifting of the center-of-mass, described in Chapter 4.

Several mechanisms of entrainment are considered: aspirated, forced, and turbulent, or eddy, diffusion (a form of entrainment). Aspirated (Taylor) entrainment is due to shear between the plume and the ambient and is present even when there is no current. It is driven by the low pressure induced in regions of high velocity, the resulting pressure gradient causing inflow into the plume. Thus the plume induces a flow field in the surrounding ambient fluid. Forced entrainment is due to the advection of mass (current) into the plume. Diffusion, while presumably always present, is only important beyond the zone of initial dilution and is neglected. It becomes dominant after the main entrainment mechanisms become small due to the steady reduction in shear between the plume and the ambient and the relative reduction of projected area. (However, the PAE growth term, described below, could be interpreted in terms of diffusion.) The transition to diffusion separates the near-field from the farfield. Strictly speaking, the latter dilution is not a part of the UM theory because UM is tentatively a near-field model. Instead, farfield diffusion is parameterized by the 4/3 power law attributed to Brooks (Tetra Tech, 1982) and others.

The PAE hypothesis, i.e. forced entrainment, consists of three terms. The first is proportional to the product of the length of the plume element along the trajectory and the plume diameter (the cylinder component), the second to the growth in diameter of the plume, and the third to the curvature of the plume trajectory that opens or closes area on the element surface. All are simply mathematical parts of the overall projected area that contribute to forced entrainment.

Taylor entrainment is proportional to the entire peripheral area of the plume element.

When adjacent plumes grow sufficiently they merge and entrain each other. Merging has the immediate effect of reducing entrainment of ambient fluid by reducing the contact area between the plume and its environs. Each of the four entrainment terms is decremented to a different degree as merging proceeds. In essence, merging simply necessitates some geometric corrections. Surface and bottom effects as demonstrated by Wood and Davidson (1990), or Coanda attachment (Akar and Jirka, 1990), are not modeled.

Only the merging of adjacent plumes discharging from linear diffusers (pipes) are considered here. This simplification helps to reduce the problem to two dimensions. Diffusers are assumed to be long so that end effects can be ignored and unbalanced internal diffusion is neglected.

Variations in the angle between the diffuser and the current are accommodated by mathematically reducing the spacing distance between adjacent ports by the appropriate trigonometric factor. Currents between 90 and 45 degrees may be handled in this way and lead to reductions of entrainment in agreement with measurements made by Roberts (1977). The modeling interface PLUMES (Baumgartner, Frick, and Roberts, 1994) provides a way of facilitating the appropriate conversions.

Typically diffusers are perforated on both sides. In a current the upstream plumes will then frequently merge with downstream plumes. This cross-diffuser merging is not simulated explicitly. In UM there are three ways to estimate the reduction in dilution due to cross-diffuser merging. The simplest way is to reduce the spacing between ports by a factor of two (i.e. spacing is equal to the diffuser length divided by the total number of ports). This method is justified by experience but it is not known with certainty how accurate it is. The effect may also be estimated by specifying the "background" concentration due to the upstream plume, which results in the prediction of a reduced effective dilution. A third method involves doubling the flow per port and increasing the diameter of the port to maintain approximately the same densimetric Froude number.

None of the methods account for the changes in density profile that the upstream plume effects on the downstream plume.

MATHEMATICAL DEVELOPMENT

Basic Model Theory

With respect to the foregoing discussion, it is emphasized that the element in Figure 6 is **not** cylindrical but is in general a section of a bent cone. The consequences of this fact cannot be overstated because the shape of the element determines the projected area which in turn determines forced entrainment, frequently the dominant source of entrainment. In general, a bent cone plume element has a projected area that differs substantially from the projected area of a simple cylinder. Thus, the growth and curvature terms are required to accurately describe the projected area of the plume element.

As has been stated, the superposition principle allows the entrainment terms to be combined. The projected area entrainment hypothesis states that where dm is the

$$\frac{dm}{dt} = \rho_a A_p u \tag{1}$$

infinitesimal amount of mass entrained in time dt, A_p is the projected area normal to the current, u, and ρ_a is the local ambient density. The element mass, m, is defined as $m = \rho \pi b^2 h$ (see also Equation 23). This hypothesis, neglecting Taylor entrainment for a moment, makes it possible to explain observed plume behavior in simple terms.

Equation 1 can be written in vector terms

$$\frac{dm}{dt} = -\rho_a \underline{A_p} \cdot \underline{U} \tag{2}$$

where the underline notation is used to indicate a vector. $\underline{A_p}$ is a vector in a vertical plane containing the current vector but pointing generally upstream (i.e. out of the element) and equal in magnitude to the projected area. $\underline{U}$ is the average velocity of the ambient flow through the projected area. The minus sign is due to the fact that $\underline{A}_p$ and $\underline{U}$ point in opposite directions so that their dot product is intrinsically negative.

To estimate the projected area it is necessary to first express mathematically how the length of the element, h, changes in response to changes in other plume properties. The reason h changes is due to the differences in velocities of the leading and trailing faces of the element which causes the faces to converge or diverge (separate) with time. Just how much depends on whether the local current velocity is less than or greater than the element velocity. Changes in h result in changes in the radius because mass is conserved, which affects the plume element projected area directly. The phenomenon is supported by dilution and radii data tabulated by Fan, 1967.

Referring to Figure 7, $\Delta|\underline{V}|$ is seen to be the difference in velocity at two opposing faces of the semi-infinitesimal element; the velocity vectors are proportional to the displacement vectors shown. (Also, it is worth noting, in both Lagrangian and Eulerian formulations the element is infinitesimal only along the trajectory. Thus it is a hybrid integrating volume which must be treated differently from truly infinitesimal volume elements.)

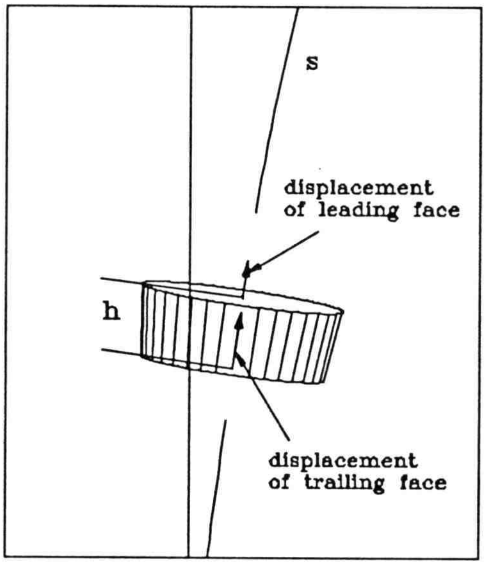

Figure 7. Convergence of element faces due to differences in face velocities.

Since the Lagrangian formulation deals with material elements and it is assumed the velocity is uniform, the faces separate or converge in time, proportionally to $\Delta|\underline{V}|$, i.e.,

$$\Delta h = \Delta|\underline{V}|\ \delta t \tag{3}$$

where δt is an arbitrary, but constant, time increment. Integrating Equation 3 and noting that the corresponding speeds and lengths are $\Delta|\underline{V}_o|$ and h_o, and, $\Delta|\underline{V}|$ and h respectively yields

$$\int_{h_o}^{h} dh = \delta t \int_{u_{so}}^{u_s} du_s \tag{4}$$

where $u_s = |\underline{V}|$ and $u_{so} = |\underline{V}_o|$. Equation 4 can be integrated to yield

$$h - h_o = (u_s - u_{so})\,\delta t \tag{5}$$

Finally, since δt can be chosen to be h_o/u_{so}

$$\frac{h}{h_o} = \frac{u_s}{u_{so}} \tag{6}$$

and $|\underline{V}|$ and h change proportionally. This relationship is supported by the numerical data given by Fan (1967).

Plume Dynamics

With the plume element defined, the remainder of the model may be developed. It is appropriate to begin a discussion of the Lagrangian plume equations with the equation of continuity, in other words, the entrainment equation. Equations 1 or 2 is a partial expression for entrainment; it states that the amount of mass added to the element in time dt is equal to the total mass flux through the element surface. The complete entrainment equation is a sum of the forced and Taylor entrainment terms as given in Equation 7

$$\frac{dm}{dt} = -\rho\, \underline{A}_p \cdot \underline{U} + \rho A_T\, v_T \tag{7}$$

where A_T is the area of the plume element in contact with the ambient fluid and v_T is the Taylor aspiration speed. Since, in the absence of merging, A_T wraps completely around the element it is not expressed as a vector. v_T is often related to an average plume velocity through a proportionality coefficient, α:

$$v_T = \alpha \, |\underline{V}| \tag{8}$$

where $|\underline{V}|$ is the average, or top hat, plume element velocity (but in other formulations it could be the centerline velocity with α scaled accordingly).

For plumes (jets with buoyancy) adequately described by a Gaussian profile (see a subsequent section entitled "Average and Centerline Plume Properties") a value of 0.082 is often attached to α. However, this value is based on the existence of a nominal plume boundary which encompasses only the central portion of the plume. The corresponding value for jets in stagnant ambient is 0.057. Lee and Cheung (1990) and Cheung (1991) maintain a variable coefficient, however, Frick (1984) gives arguments for using a constant α, at least tentatively. The conversion from nominal Gaussian plumes to a "top hat," or average, description of the plume element yields corresponding values of 0.116 and 0.081. According to Frick, the latter value is underestimated so that an average value for α of 0.1 is thought to be slightly conservative in terms of describing aspiration entrainment. It is the default value used by UM, however, provision are made to enable the user to change its value.

Strictly speaking, the areas are infinitesimal areas which might be indicated with the differential d prefix. This is because h is an infinitesimal distance. However, the model equations are finite approximations in which small algebraic values substitute for infinitesimal ones.

The entrainment hypothesis can now be quantified. Assuming no merging, dynamic gravitational collapse, or overlap (NVA), the Taylor aspiration area is simply

$$A_T = 2\pi b h \tag{9}$$

where b is the element radius. The reduction in this area due to merging is described subsequently. Dynamic gravitational collapse of plumes is considered by Frick et al. (1990). The NVA is described subsequently. A dynamic collapse algorithm and a NVA correction are not part of UM.

The projected area of the plume element is more difficult to derive than the Taylor entrainment area. Derivations of lesser and greater generality are given by Frick (1984) and Lee and Cheung (1990). A simpler derivation employing a rotation of coordinates is given below.

Since the current, $\underline{U}$, is a vector field it may be transformed into a useful coordinate system by known rules of vector rotation. A particularly useful coordinate system is the local coordinate system shown in Figure 8. The ambient velocity vector, i.e. the current, can be expressed as the sum of components in each of the local coordinate system directions,

$$\underline{U} = u_1\hat{e}_1 + u_2\hat{e}_2 + u_3\hat{e}_3 \quad (10)$$

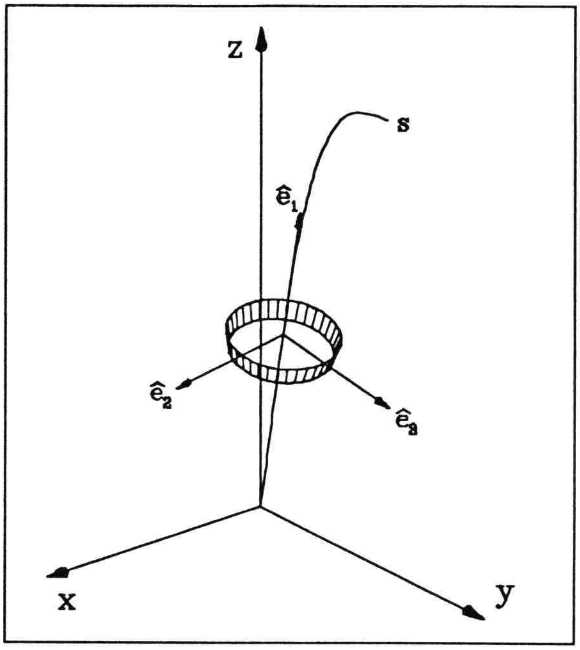

Figure 8. The local coordinate system.

where $\hat{e}_1$, $\hat{e}_2$, and $\hat{e}_3$ are the unit vectors in the direction of the trajectory, the horizontal normal to the trajectory, and in a vertical plane respectively. The vector $\hat{e}_3$ can be expressed in terms of the cross-product of $\hat{e}_1$ and $\hat{e}_2$:

$$\hat{e}_3 = \hat{e}_1 \times \hat{e}_2 \quad (11)$$

The unit vectors are derived by constructing a rotation matrix that transforms between the coordinate systems.

With respect to this coordinate system and each velocity component the corresponding projected areas are particularly simple, see Figure 9. Again ignoring merging, collapse, and overlap, the projected area associated with u_1, i.e. A_1, is simply an annulus that wraps around the plume

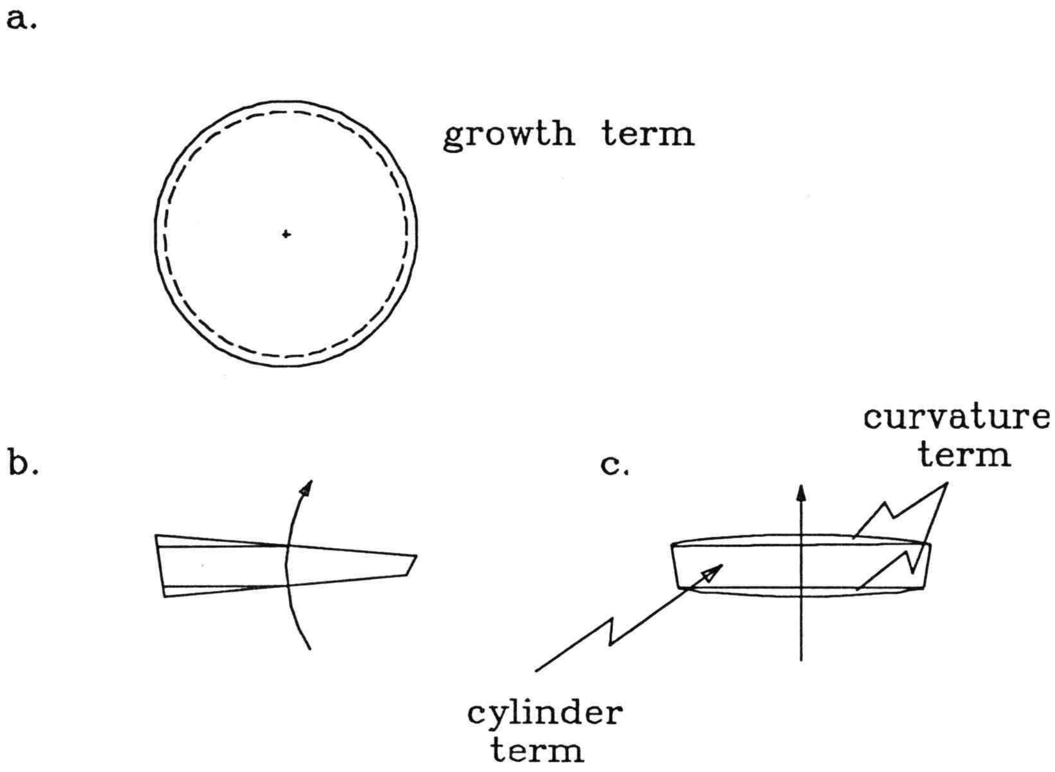

Figure 9. The projected area entrainment components: a) the growth area, b) side view of the element, and c) the cylinder and curvature area.

$$A_1 = \pi b \, \Delta b \tag{12}$$

where Δb is the difference between the radius of the leading and trailing faces of the plume element. This is the "growth" contribution to the projected area (see Figure 9a). The assumption is made that only the upstream portion of the area, half the circumference, has flow going through it (cf. Cheung, 1991). Furthermore, the flow in the wake is altered and is assumed to flow parallel to the plume surface.

The difference in radius over the length of the element is

$$\Delta b = \frac{\partial b}{\partial s} h \tag{13}$$

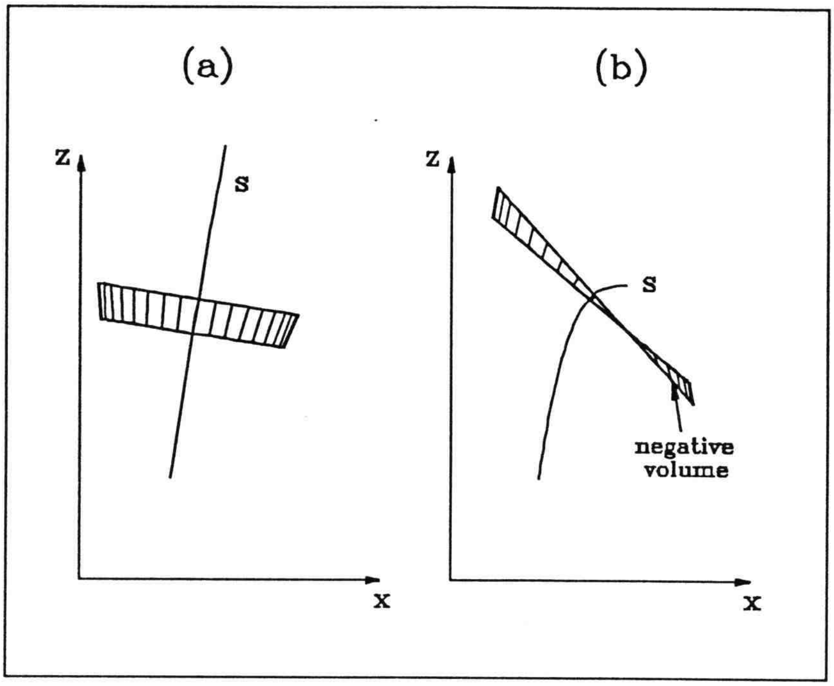

Figure 10. a) The plume element in a region of weak trajectory curvature and b) strong trajectory curvature (showing overlap).

where s is the distance along the centerline. The derivative is estimated from the difference in radius in successive program steps divided by the distance traversed.

Each one of the velocity components u_2 and u_3 has two projected area terms associated with it, one which is due to the curvature of the plume trajectory, the other simply being the projection of a cylinder (see Figure 9b and 9c respectively). Since only the two-dimensional problem is considered the u_3 component is ignored; its cylinder and curvature contributions are due to current flowing into the side of the plume element caused by directional changes with depth in the ambient flow.

The cylinder projected area is simply

$$A_{cyl} = 2bh \tag{14}$$

The change in direction of the average plume element velocity, $\underline{V}$, which is parallel to $\hat{e}_1$, over the length of the plume element h, in other words the curvature of the centerline s, produces the "curvature" component to the projected area. Since the faces defining the element are normal to s, in regions of trajectory curvature the element is deformed into a wedge shape. When the radius of curvature becomes smaller than the element radius overlap begins. Respective depictions are given in Figure 10.

The curvature component of the projected area is

$$A_{cur} = -\frac{\pi}{2} b^2 \frac{\partial \theta}{\partial s} h \tag{15}$$

where θ is the elevation angle of s. This area can be positive or negative depending of the sign of $\partial\theta/\partial s$ which is determined with reference to successive values of $\underline{U}$. A negative curvature and area has the effect of reducing the total projected area.

Historically the growth and curvature terms have either not been recognized or have been thought to be small compared to the cylinder term (Schatzmann, 1979). However, in general, it can be shown that all three contributions to the total projected area are important (Frick 1980, 1984; Lee and Cheung, 1990; Cheung 1991; Baumgartner, Frick, and Roberts, 1993). Earlier perceived inadequacies in the projected area entrainment hypothesis can be attributed to the omission of the growth and curvature terms.

That completes a discussion of entrainment.

Conservation of momentum is given by

$$\frac{dm\underline{V}}{dt} = \underline{U}\frac{dm}{dt} - m\frac{(\rho_a - \rho)}{\rho}\underline{g} \tag{16}$$

where m is the mass of the plume element ($m = \rho\pi b^2 h$), ρ_a and ρ are the ambient and average element densities respectively, and $\underline{g}$ is the gravity vector. $\underline{U}$ represents the average ambient velocity over the exposed plume surface. This point is worth emphasizing since the surface area is infinitesimal only along the centerline and can be extensive in the two dimensions orthogonal to the centerline, over which, therefore, the ambient velocity can vary significantly. In UM it is equal to the ambient velocity at the level of the particle, i.e. the center of the cross-section.

Equation 16 states that the change in momentum of the element is equal to the amount of momentum contributed by the entrained mass dm and the change in vertical momentum generated by the buoyant force. The implicit assumption is that drag is negligible or absent. This assumption is consistent with the conception of an element that has the same properties as the ambient at the outside surface of the element. Effectively, there are no shears that can generate drag. The inconsistency of this conception with the Taylor entrainment hypothesis is not resolved.

UM does not include interactions with solid surfaces and free surfaces. Users are simply warned when interactions with the surface and the bottom occur. In Muellenhoff et al. (1985) predicted dilutions were reduced by 10% when the sea surface was encountered. Other effects are also ignored, for example, since plumes rise in a matter of minutes the Coriolis force is neglected. However, very large plumes, like so-called hydrothermal mega-plumes, are likely to be affected significantly by the earth's rotation.

To quantify the buoyancy term in the conservation of momentum equation it is necessary to define the conservation of energy equation. It is approximated by

$$\frac{dmc_p(T-T_{ref})}{dt} = c_p(T_a-T_{ref})\frac{dm}{dt} \tag{17}$$

where c_p is the specific heat at constant pressure. T, T_a, and T_{ref} are the average element temperature, the ambient temperature, and an arbitrary reference temperature, respectively. More correctly, c_p would be represented by an integral. However, assuming that c_p is constant over the range of interest allows Equation 17 to be simplified,

$$\frac{dmT}{dt} = T_a\frac{dm}{dt} \tag{18}$$

Radiation, conduction, and diffusion are assumed to be small.

Like temperature, salinity is assumed to be a conservative property

$$\frac{dmS}{dt} = S_a \frac{dm}{dt} \tag{19}$$

where S and S_a are the average element salinity and the ambient salinity respectively. The symbol for ambient salinity should not be confused with average dilution of the plume, i.e., S_a is also used as the symbol for the average dilution of a plume cross-section.

The conservation of conservative pollutants is similarly formulated, however, since important pollutants, such as coliform, are subject to decay, a first order decay term is also included.

$$\frac{dm\chi}{dt} = \chi_a \frac{dm}{dt} - km\chi \tag{20}$$

where χ and χ_a are the concentrations of the species of interest in the element and ambient respectively and k is a first order decay constant.

The momentum equation includes the reduced gravity, $((\rho_a-\rho)/\rho)\ g$. It is evaluated once densities are determined. Densities are derived from the equation of state (SigmaT function) introduced by Teeter and Baumgartner (1979). This function is independent of pressure, limiting UM, by deep ocean standards, to shallow water. It is also limited to ordinary salinities and temperatures. For example, at the elevated salinity of 150 o/oo the error is about 10 percent sigma T units.

Boundary Conditions and Other Pertinent Relationships

To complete the model, the boundary and initial conditions must also be specified. The main boundary condition is the location of the source from which the subsequent position of the element may be determined by integrating the trivial relationship

$$\frac{d\underline{R}}{dt} = \underline{V} \tag{21}$$

where $\underline{R}$ is the radius vector of the particle, i.e. the center-of-mass of the element. To give an example of how the equations are solved in a finite difference model, the new $\underline{R}$ is

$$\underline{R}_{t+dt} = \underline{R}_t + \underline{V}_t \, dt \tag{22}$$

Another boundary condition is the initial plume radius. Initial conditions include the efflux velocity, the effluent temperature, etc..

Several other auxiliary equations and algorithms are required to perform simulations. For example, an algorithm is needed to interpolate between ambient measurements to give the desired values at the level of the particle. The ambient current, salinity, temperature, and background concentrations are found in this way. Also, because the Lagrangian plume equations require a very small time step initially, but not later in the simulation, a method of varying the size of the time step is used to control the relative amount of mass that is entrained during any one single step. This manipulation of the time step is done in the interest of computational efficiency.

The general computational procedure followed in the model is: 1) a time step is provided (guessed), 2) the entrainment equations are then used to determine the amount of mass that will be added given this time step, 3) this increase is then compared with the target mass increase and the appropriate adjustments are made to the time steps and the entrainment components to meet the appropriate mass doubling criterion, 4) the equations of motion and other model equations are solved, and 5) the new time step is established and the cycle is repeated.

The mass doubling criterion is used to scale the model time increment. The equations are stiff, in other words, a small time step is needed initially but later in the simulation a larger time step is satisfactory. Adjusting the time step to yield a doubling in mass every 100 simulation cycles provides both accuracy and computational speed.

It is important to recognize that some of the above equations are not always solved for the quantity on the left hand side of the equal sign. In other words, the dependent variable may be some other variable besides the one on the left hand side of the equal sign. For example consider Equation 23 which expresses the mass of the element in terms of its dimensions and the density:

$$m = \rho \pi b^2 h \tag{23}$$

For modeling purposes the radius, b, is not an independent variable, rather it is a dependent variable. Since mass is computed by integrating from its initial value using the entrainment, or continuity, equation, it is effectively an independent variable in Equation 23. In other words, Equation 23 must be inverted to solve for the radius. Thus

$$b_{t+dt} = \sqrt{\frac{m_{t+dt}}{\pi \rho_{t+dt} h_{t+dt}}} \tag{24}$$

Merging

The basic approach to handling plume merging is to 1) reduce the entrainment areas, both Taylor and forced, to account for the loss of exposed surface area that occurs when neighboring plumes interact with each other, and, 2) to confine the plume mass from each plume to the space between them that is known to be available from symmetry considerations. The underlying assumption is that neighboring plumes are identical and any interaction between them is equal and opposite in effect.

Considering Taylor entrainment first, the conditions of merging are depicted in Figure 11 where, for simplicity, no trajectory curvature is indicated. It is seen that the uncorrected Taylor entrainment area can be multiplied by a factor equal to the ratio of the exposed circumference to the total circumference to reduce it to the actual exposed area. The side of the plume element that is longer and larger in area due to trajectory curvature nearly compensates for the opposite side that is shorter and smaller.

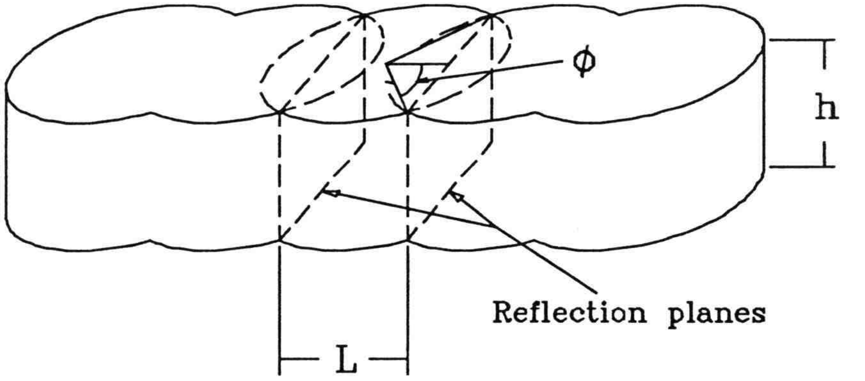

Figure 11. Merging geometry and reflection planes.

The appropriate ratio of correction is

$$a_T = \frac{\pi - 2\phi}{\pi} \tag{25}$$

where

$$\phi = \arctan\sqrt{\frac{4b^2 - L^2}{L^2}} \tag{26}$$

where ϕ is defined in Figure 11 and L is the spacing between adjacent ports. The same correction factor applies to the growth entrainment term.

While it is assumed that the current is perpendicular to the diffuser axis, the method may be used for angles between 45 and 135 degrees (90 degrees being equivalent to a current perpendicular to the diffuser) by multiplying L by the factor sin ψ where ψ is the angle between $\underline{U}$ and the diffuser axis. This method is justified by measurements of dilution of merging plumes (Roberts, 1977).

The correction factor for the cylinder projected area is simply

$$a_{cyl} = \frac{L}{2b} \tag{27}$$

Finally, the correction term for the curvature projected area entrainment contribution is

$$a_{cur} = 1 - \frac{2\phi}{\pi} + \frac{\sin 2\phi}{\pi} \tag{28}$$

Equations 23 and 24 must also be modified when merging occurs. As was pointed out in the previous section, the mass of the plume element is obtained by knowing the initial mass and integrating the entrainment equation. Given that the mass, average plume density, and element length are known, the element volume can be determined. Upon merging, the transverse dimension of the plume element (i.e. along $\hat{e}_2$) is assumed to be limited to a maximum length of *L*, the spacing distance. Effectively, a vertical plane half way between the ports acts as a wall or reflecting plane. This technique is common in air pollution modeling (Turner, 1970) where a fictitious mirror source is used to estimated dispersion in the presence of an actual physical barrier. With plume merging the sources are real.

Thus, the volume of the plume element can be thought to be the product of *h* and the area of a rounded rectangle, see Figure 12. This area is the quotient of the element volume and the length which, after simplification, becomes

$$\pi b_r^2 = \pi b^2 (1 - \frac{2\phi}{\pi}) + 2b^2 \sin\phi \cos\phi \tag{29}$$

where b_r is the unmerged round element radius and *b* is now the radius of the element in the vertical plane. In other words, *b* describes the plume element parallel to $\hat{e}_3$. Solving for *b*

$$b = \frac{\pi b_r}{\pi - 2\phi + 2\sin\phi \cos\phi} \tag{30}$$

the subscript $t+\delta t$ has been left off for simplicity. Since φ is larger than $\sin\varphi \cos\varphi$, *b* is larger than b_r.

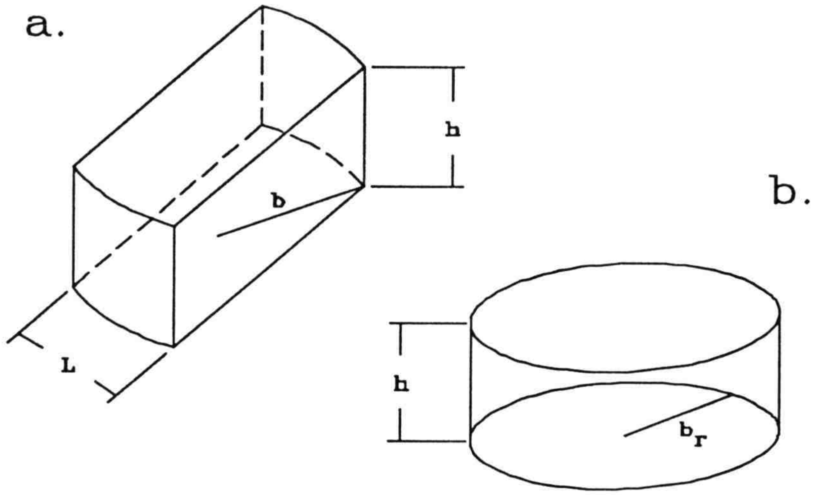

Figure 12. Derivation of dimensions in merging: a) merged element with volume confined between reflection planes, and b) unmerged element of equal volume.

Average and Centerline Plume Properties

The previous discussion is in terms of average plume properties because average plume properties are physically compatible with the average motion of the plume element. It is not reasonable to assume, as is done in Gaussian models, that centerline buoyancy can be used to accurately predict the average movement of the plume element (Frick, 1984). After all, the element is an entity which stretches from one boundary with the ambient flow to the other, albeit with widely varying properties in between.

On the other hand, centerline concentrations are of great concern because they have the greatest potential for causing adverse impacts on the environment. Fortunately, it is possible to estimate centerline properties once average properties are known. Plumes often exhibit predictable cross-sectional properties. For example, plumes discharged into quiescent fluid display a profile represented reasonably well by the Gaussian profile, very dilute at the edges and concentrated at the center. However, the Gaussian profile is not very compatible with the Lagrangian plume element because one extends to infinity while the other has definite boundaries. Consequently, another profile, the 3/2 power profile (Kannberg and Davis, 1976), which closely matches the Gaussian profile over the concentrated portion of its range, is used to determine the centerline concentration as a function of the average concentration, or dilution, that UM predicts.

The 3/2 power profile is expressed as

$$\Phi = \left(1 - \left(\frac{r}{b}\right)^{\frac{3}{2}}\right)^2 \tag{31}$$

where Φ is instantaneous scaling factor relating differences between the plume and the ambient of an appropriate property, such as the concentration of some pollutant or velocity, b is the plume radius, and r is the distance from the center of the plume to the point within the plume at which Φ is measured.

The peak-to-mean ratio is simply the ratio of the centerline to the average concentration, it is obtained from a flux integral. We start with the relationship for the average concentration

$$C_{avg} = \frac{\int_A C v dA}{\int_A v dA} \tag{32}$$

where C_{avg} is equivalent to the average concentration obtained from UM, C and v are the instantaneous concentration and velocity in the plume element, A is the cross-sectional area, and dA is the corresponding infinitesimal area. The peak-to-mean ratio is defined to be C_{max}/C_{avg}

$$\frac{C_{max}}{C_{avg}} = \frac{C_{max}\int_A v dA}{\int_A C v dA} \tag{33}$$

where C_{max} is the centerline concentration. The integrals are not easy to solve analytically and, therefore, are estimated numerically in UM.

It is illuminating to define limiting values of the coefficient. When dilutions and currents are large a simplification is possible. In this case the velocity can be considered constant and can be factored from the integrals, giving

$$\frac{C_{max}}{C_{avg}} = \frac{C_{max}\int_A dA}{\int_A C dA} \tag{34}$$

Using this approximation and assuming the 3/2 power profile a peak-to-mean ratio of 3.89 is found for round plumes. The corresponding ratio for a fully merged line plume is 2.22. However, the ratios vary and in much of the plume the peak-to-mean ratios are considerably smaller than these limiting values, in fact, near the source they often approach 1.0, depending on the uniformity of the source.

JUSTIFICATION OF PROJECTED AREA ENTRAINMENT

In 1989, Roberts, Snyder, and Baumgartner published three papers in ASCE (1989a,b,c) which measure and record the behavior of merging laboratory plumes in flowing, stratified environments. Although they did not set out to do so, their findings directly corroborate PAE, as shown below:

Given Equation 13a of Roberts, Snyder, and Baumgartner (1989a)

$$\frac{S_m q N}{b^{2/3}} = 1.08 F^{1/6} \tag{35}$$

where S_m is the centerline dilution in the plume, q is the diffuser volume flux per unit length, b is the buoyancy flux per unit length (i.e. the product of the reduced gravitational acceleration and the volume flux per unit length), F is a type of Froude number (u^3/b, where u is the current speed), and N is the buoyancy (Brunt-Vaisala) frequency

$$N = \left(-\frac{g}{\rho_a}\frac{d\rho}{dz}\right)^{1/2} \tag{36}$$

and $d\rho/dz$ is the ambient density gradient. And given Equation 13b of the same reference, viz.

$$\frac{z_e}{l_b} = 1.85 F^{-1/6} \tag{37}$$

where z_e is the rise above the port datum of the top of the fully merged wastefield and l_b is a buoyant length scale

$$l_b = \frac{b^{1/3}}{N} \tag{38}$$

Equations 35 and 37 may be combined. Then, noting that $q = Q/L$, where L is the length of the diffuser and Q is the diffuser total volume flux, and making the appropriate substitutions yields

$$S_m = \frac{1.08}{1.86} \frac{L z_e u}{Q} \tag{39}$$

The quantity $Lz_e u$ is, of course, just the flux through the projected area, which is the integrated form of PAE! The coefficient is within the general range described in the previous section.

This derivation proves, at least in an overall sense, that in sufficiently high current initial dilution is given simply by the quotient of the flux through the projected area of the wastefield divided by the source flux, multiplied by a constant factor. It is reasonable to assume that the integrated outcome is the result of adding fluxes given by applying the PAE hypothesis repeatedly over the plume trajectory. In other words, it is not reasonable to assume, a priori, that the entrainment occurs at a lesser rate than PAE at some points over its projected area while it occurs at a greater rate over another. Any such deviations are thought to be due to the aspiration effect of the Taylor entrainment coefficient which can be treated separately.

CHAPTER 4. LAGRANGIAN AND EULERIAN MODEL COMPARISON

INTRODUCTION

The Lagrangian model is sometimes specifically criticized for applying solid mechanics to a fluid dynamical problem, as is apparent from the peer review comments given in Appendix 3. If these negative peer review opinions are typical, as the review comments suggest, they should also be consistent. However, while it is true that it is largely a particle model, it is not true that Eulerian integral flux models are different in that respect — they are not fluid dynamical models as their proponents apparently believe. The belief, or misconception, to the contrary seems to be based on the fact that the latter models describe flux balances, not explicitly particle dynamics. However, they are called integral models because the fluxes are integrated over portions of the control volume surface. The equations are not formulated for individual grid points. Thus, they are not primitive Eulerian models in the finite element or finite difference sense. The integration is essentially an averaging process which results in most information about the system being lost. More correctly, a "fluid dynamic" model is never rigorously developed in the first place. Thus, they are particle models just as much as the Lagrangian model is and their views are inconsistent.

If the Lagrangian model were not attacked for being a particle model, whether Eulerian integral flux models are or not would be irrelevant. After all, the credit accorded a model ought to be based on the quality of prediction. But, since it seems to be rejected for primarily this reason, it is appropriate to present the facts to overcome the biases that elicit this treatment. Normally, it would be sufficient to show that the models are equivalent, as is accomplished below. However, whether or not this approach proves adequate depends in part on how honestly peer reviewers and other critics are willing acknowledge their biases.

The approach used here to show equivalence is simple. A "typical" Eulerian integral flux model is developed and presented. It is then compared to the Lagrangian model described in the previous chapter. The comparison shows that they are essentially equivalent, not only in the absence of strong plume bending but also when the negative volume anomaly is shown to affect the performance of the Lagrangian particle model. Since they are shown to be equivalent, it is concluded that the Eulerian integral flux models are also particle models and, furthermore, that they are implicitly subject to the negative volume anomaly.

The material in this chapter is adapted from Frick, Baumgartner, and Fox (1993) which is reproduced in Appendix 1.

AN EULERIAN INTEGRAL FLUX MODEL

An adaptation of the model attributed to Weil (1974) is chosen to represent Eulerian integral flux models. One reason is its relative simplicity. However, it is noted from the outset that his development is not self-consistent. In other words, the model equations developed with reference to the control volume shown in Figure 13 do not rigorously represent the depicted geometry. Figure 13 shows control volume faces that are parallel, which has mathematical implications that are not fully developed or understood. For the flux equations of this control volume to be formulated correctly, the angles between the integrated flux vectors and the control volume cross-sectional area vectors would have to be considered. In other words, since the average flux vectors are parallel to the trajectory they cannot also be normal to one or both cross-sectional surfaces of the control volume. To be formulated correctly, both normal and parallel fluxes would have to be specified.

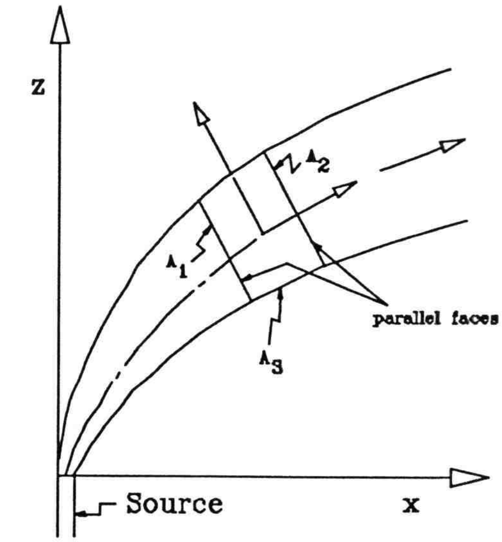

Figure 13. Schematic drawing of the Eulerian control volume, after Weil (1974). The conception of A_1 and A_2 as parallel faces is apparent.

This observation has more than academic importance. That only the normal flux balances, i.e. those parallel to the trajectory, are formulated is tantamount to saying that the schematic depiction is not used. Instead, the model uses the same control volume presented in the previous chapter. In other words, the cross-sections are not generally parallel and can intersect. Consequently, the model is subject to the negative volume anomaly, which is to say that the control volume can acquire a topological form which renders the formulation inadequate, anomalous, and erroneous. Intended or not, in zones of high curvature the mathematics is that of intersecting round plume cross-sections, with all built-in errors appertaining. It should not be surprising then that the two uncorrected models are equivalent when the negative volume anomaly is present.

Anticipating this complication, a standard, but ultimately erroneous, Eulerian integral flux plume model may now be developed. Basic to it are several integrated flux quantities defined as follows. The volume flux α:

$$\alpha = b^2 |\underline{V}| \tag{40}$$

where b is the radius of the control volume and $\underline{V}$ is the plume velocity vector. (π is a common factor in the equations that is implicitly incorporated.)

The x-momentum deficit flux β:

$$\beta = b^2 |\underline{V}| (v_x - u_x) \tag{41}$$

where v_x and u_x are x-components of the plume velocity vector and the current velocity respectively.

The z-momentum deficit flux γ:

$$\gamma = b^2 |\underline{V}| v_z \tag{42}$$

where v_z is the vertical component of the plume velocity vector.
The temperature deficit flux δ:

$$\delta = b^2|\underline{V}|(T-T_a) \tag{43}$$

where T is the plume temperature and T_a is the ambient temperature. And the salinity deficit flux ε, which, since it assumed air as a medium, is not part of Weil's (1974) development:

$$\epsilon = b^2|\underline{V}|(S-S_a) \tag{44}$$

where S is the plume salinity and S_a is the ambient salinity. The differential equations corresponding to Equations 40 through 44 follow.

The equation of continuity used here is

$$\frac{d\alpha}{ds} = 2ab|\underline{V}-\underline{U}| + |\underline{U}|\{b\cos\theta\frac{\partial b}{\partial s} + \frac{2b}{\pi}\sin\theta + \frac{b^2}{2}\sin\theta\frac{\partial\theta}{\partial s}\} \tag{45}$$

where a is the dimensionless shear or aspiration entrainment coefficient, θ is the elevation angle of the trajectory, and s is the distance measure along the trajectory. Equation 45 is taken from Frick (1984) and differs radically from Weil's (1974) entrainment hypothesis.

The compound term in Equation 45 is the Eulerian form of the Projected Area Entrainment (PAE) hypothesis. Unlike earlier entrainment assumptions, it includes terms proportional to both $\partial b/\partial s$ and $\partial\theta/\partial s$. In contrast, Weil used an entrainment hypothesis simply proportional to the radius of the plume and vertical velocity of the plume element. Weil's entrainment assumption seems to produces excessive growth in some cases and is not adopted, however, when it is used, the two models still give equivalent predictions.

The remaining flux differential equations are

$$\frac{d\beta}{ds} = -b^2 v_z \frac{\partial u_x}{\partial z} \tag{46}$$

$$\frac{d\gamma}{ds} = b^2 \frac{\rho_a - \rho}{\rho} g \tag{47}$$

$$\frac{d\delta}{ds} = -b^2 v_z \frac{\partial T_a}{dz} \tag{48}$$

and

$$\frac{d\epsilon}{ds} = -b^2 v_z \frac{\partial S_a}{\partial z} \tag{49}$$

where ρ is the element density, ρ_a is the ambient density at the level of the element's center-of-mass, and g is the acceleration of gravity.

The equations are solved for the secondary variables in the following way. The temperature at step i+1 is

$$T_{i+1} = T_{ai} + \frac{\delta_{i+1}}{\alpha_{i+1}} \tag{50}$$

and, correspondingly, the salinity is

$$S_{i+1} = S_{ai} + \frac{\epsilon_{i+1}}{\alpha_{i+1}} \tag{51}$$

The density ρ is a function of pressure, salinity, and temperature. The equation of state, σ_T, has been formulated for large ranges of temperature and pressure including deep sea geothermal releases. Such generality is possible because the Boussinesq approximation is not needed in the Lagrangian model to simplify the analysis, a reason for employing it in Eulerian integral flux models. Equation of state details are not given here because the work is fragmentary and unfinished. But, in general,

$$\rho_{i+1} = \sigma_T(p_{i+1}, S_{i+1}, T_{i+1}) \tag{52}$$

where p, the pressure at depth. In deep geothermal applications it is necessary to recalculate the pressure and also temperature changes due to potential temperature effects. Attempts to generalize the model in this way remain incomplete, consequently the simple equation of state introduced by Teeter and Baumgartner (1979) is use here.

The new velocity vector is written as

$$\underline{V}_{i+1} = \frac{\beta_{i+1}}{\alpha_{i+1}}\underline{i} + 0\underline{j} + \frac{\gamma_{i+1}}{\alpha_{i+1}}\underline{k} \tag{53}$$

where $\underline{i}$, $\underline{j}$, and $\underline{k}$ are the primary horizontal, secondary horizontal, and vertical unit vectors respectively. The y-component is set arbitrarily to zero since the model is two-dimensional.

The new vector displacement is given by

$$\Delta\underline{R} = \underline{V}_{i+1}\Delta t \tag{54}$$

the distance along the trajectory by

$$s_{i+1} = s_i + \Delta s \tag{55}$$

where $\Delta s = \Delta|R|$, and the radius vector is

$$\underline{R}_{i+1} = \underline{R}_i + \Delta\underline{R} \tag{56}$$

The new radius is calculated from

$$b_{i+1} = \sqrt{\frac{\alpha_{i+1}}{|\underline{V}_{i+1}|}} \tag{57}$$

representing the standard round plume assumption, at issue in this paper.

MODEL EQUIVALENCE

As explained in Chapter 1, the inconsistent treatment by peer reviewers, their biases and cherished, but fallacious, ideas sometimes lead peer reviewers to reject worthwhile work. If the detrimental effects of such treatment is to be overcome, it is necessary to identify and expose its fallacies. Frick (1984) attacked one fallacy that held that the elaborate trappings of Gaussian Eulerian integral flux models, using complicated profiles and relationships between various quantities, makes them superior to simpler Lagrangian and Eulerian integral flux models. He suggested that the resulting neglect of mass outside the nominal boundary in fact rendered these models inferior for predicting average plume motion.

Another fallacy that contributes to the peer review rejection of the Lagrangian plume model is that it is a particle model inappropriately applied to fluid dynamics, while Eulerian integral models, of which they approve, are perceived to be fluid dynamics models. In fact, both models are "particle" models.

The particle model fallacy can be exposed by showing the Lagrangian and Eulerian integral flux models are essentially equivalent, using similar assumptions. This exercise serves also to establish the effect of the Negative Volume Anomaly (NVA) on plume behavior.

Three cases are simulated and compared, one without and two with the NVA present. In all cases the two models give equivalent predictions.

Figure 14 presents a case in which overlap does not occur, as can be appreciated by the fact that the cross-sections (diameters) do not intersect. This is verified further by the models which are designed to report the onset of overlap or intersection. The diameters predicted by both the Eulerian (Figure 14a) and the Lagrangian (Figure 14b) models are shown at approximately equally spaced intervals along the trajectory. They are constructed symmetrically about and perpendicular to the trajectory, which is consistent with established practice. However, as is established subsequently, this practice is grossly deficient.

The current was deliberately chosen to produce conditions in which the cross-sections almost intersect at the bottom boundary in the high curvature region. While current can prevent cross-sections from intersecting, it can also contribute to it, as in counter-flowing buoyant plumes.

The centerlines and plume envelopes predicted by both models are nearly identical (Figure 14c). Differences in the range of one percent are attributed to the use

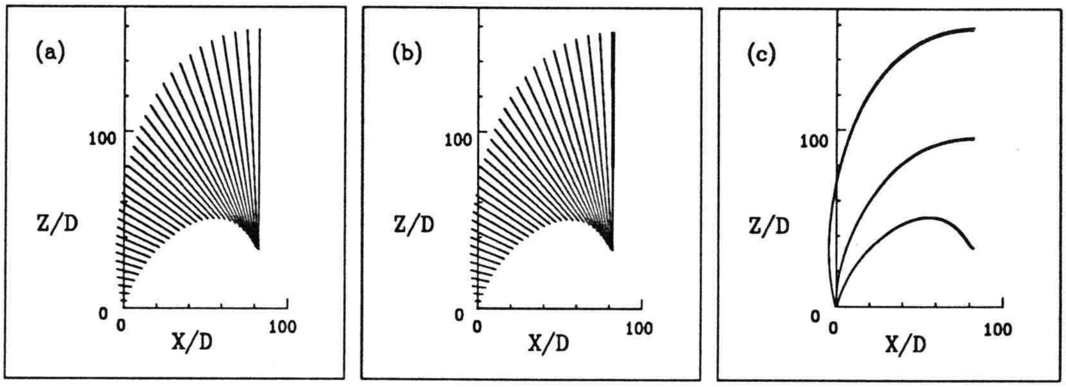

Figure 14. Eulerian and Lagrangian model comparison: a) Eulerian model predicted diameters, b) Lagrangian model predicted diameters, and c) edges and centerlines.

of the Boussinesq approximation in the Eulerian model and computational artifacts. They are similar in magnitude to those found by Frick and Winiarski (1975) but are small compared to the effect of the negative volume anomaly inherent in Equations 24 and 57 when overlap is significant, as is shown subsequently.

A second case with overlap, i.e. with the negative volume anomaly evident, is shown in Figure 15a. In this case, with no ambient current, the cross-sectional diameters intersect over part of the trajectory (not shown), creating the anomalous hook near maximum rise. In these circumstances the Lagrangian model is formulated erroneously, as is explained in detail below. Nevertheless, both models continue to give equivalent predictions. Despite extensive differences in formulation, control volume conception, and the "Lagrangian" anomaly described previously, the approaches are mathematically equivalent. Furthermore, equivalence is not dependent on the particular entrainment hypothesis used for the models as Weil's entrainment assumption (Weil, 1974) also produces agreement.

The final example (Figure 15b) represents a vertical discharge to weak current, comparable to a large source in the deep ocean (Baker et al. 1989). It is similar to the case shown in Figure 13 which shows the intersection of the element. The plume rises through a relatively unstratified layer before encountering a stable, less dense, layer near

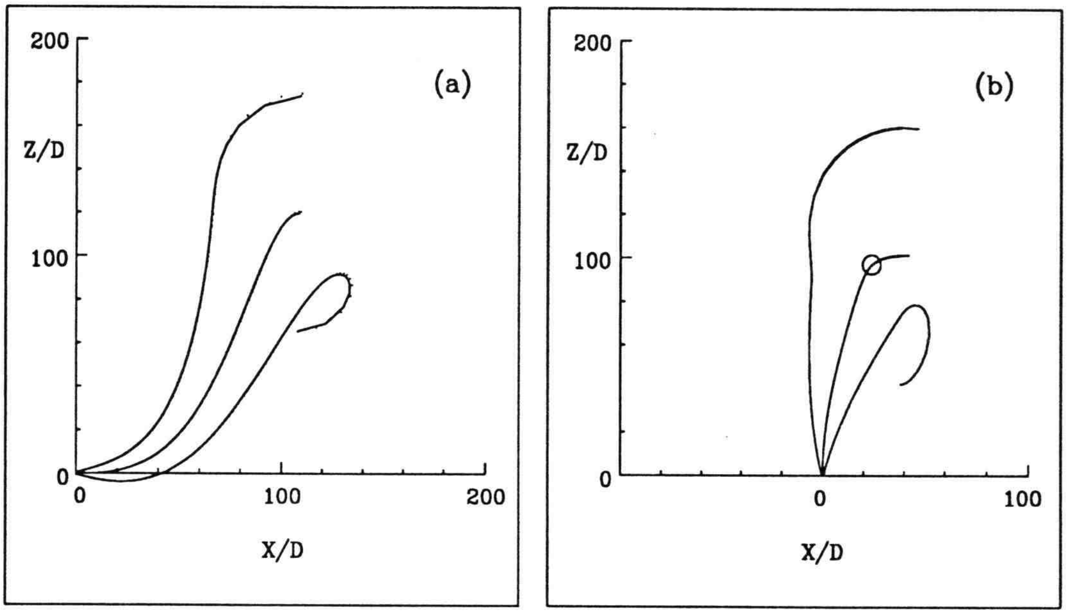

Figure 15. Additional comparisons of **both** models showing equivalence when overlap occurs: a) horizontal discharge into quiescent ambient, and b) vertical discharge into flowing ambient.

the level of maximum rise. At this level the current also increases with the net result that overlap begins. The conditions, though chosen for their demonstrative value, are not unrealistic. Once again, the two models are both equally in error.

These examples prove that the Eulerian integral flux models are equivalent to Lagrangian plume models given equivalent assumptions. Therefore, given that Eulerian integral flux models are accepted for the purpose of simulating fluid plumes, it is not appropriate to reject the Lagrangian on the basis that it is a particle model. Eulerian integral flux plume models are as much particle models as are Lagrangian models. It would be much more constructive to accept the Lagrangian paradigm as a complementary modeling viewpoint. The development of the PAE hypothesis and the identification of the NVA show that it is beneficial to maintain both approaches.

CAUSES AND EFFECTS OF NVA ON PREDICTION

The preceding section shows that the Lagrangian and Eulerian integral flux plume models are equivalent given the same assumptions. That conclusion is based on equivalence of three simulations of three entirely different flow cases, two of which involve the NVA. In this section the causes and effects of the NVA on model prediction are analyzed. In particular, the magnitude of the effect is established.

An important consequence of the anomaly is that entrainment is overestimated. For example, Figure 14 depicts a case in which the trajectory curvature is fairly constant and comparatively small so that overlap does not occur, i.e. the NVA is not present. Consequently, the growth in radius, and entrainment, over the length of the trajectory is uniform and gradual. In contrast, in Figure 15b the trajectory curvature is greater and the NVA does occur. In the very short distance where trajectory curvature is highest, i.e. where intersection is pronounced (shown circled), the predicted dilution increases sharply and the anomalous hook is created.

The hook appears because in standard practice the plume's diameters are constructed without regard to the physical consistency of the definition of the plume element. In other words, if the plume element were actually constructed of material disks connected by flexible and expandable sides containing the original fluid (plus entrained fluid), the complications introduced by curvature would become apparent. Standard practice suggests the disks could pass through each and, considering that neighboring plume elements exists, that the material substance of each element can occupy the same space occupied at the same time by other elements. This is, of course, is physically unrealistic.

Before elaborating on this topic further it is worthwhile to distinguish two related concepts. In this discussion volume has a double meaning, scalar measure of size and topological identity. Because it is confusing to simultaneously speak of volume in both connections, *let the topological form of the volume, which represents a real object, be called control volume or plume element.*

The anomaly derives from inconsistent definitions of objects (control volumes) used by those formulating plume models. They portray them as cylinders or bent cones and fail to consider the adjustments that must be made when faces are inclined to each other beyond a critical angle, i.e. beyond the point at which the element faces begin to intersect or overlap. When they do the object is no longer simple and topologically unified. Instead, it is one cleaved by a line of intersection which divides two parts possessing negative and positive volumes. Equations 24 and 57 are not derived for such a topologically complex object, in fact, the complex object is physically unreal.

The reason that one part has negative volume has nothing to do with the intentions of modelers. The complication comes about inadvertently when modelers apply the volume equation for a disk to solve for the radius of the plume element. In doing so, modelers are unaware of its valid limits, which are that the elements faces must not be overlapped. If they are, the integration limits which are implicit in the formula violate physical reality. In particular, the portion on the overlapped side of the "cylinder" has a mathematically negative length which integrates to a negative volume. The compound topological object, i.e. control volume, no longer conforms to reality.

The negative volume anomaly leads to the overestimation of entrainment because the radius is overestimated. This follows because as soon as the element faces intersect, as shown in Figure 16, the negative volume of the portion on the overlapped side of the line of intersection decreases the total volume of the compound object. Since volume is literally derived from the entrainment hypothesis the radius is forced to adjust to make the total volume of the "cylinder", now composed of positive and mathematically negative parts, equal to it. Calculated from Equation 57, the radius is correspondingly, and erroneously, larger. Simply stated, the mathematics do not conform to the physics of the situation and all PAE terms in Equation 45 and its Lagrangian counterpart are inadvertently and erroneously increased.

The overlap problem was recognized earlier. Teeter and Baumgartner (1979) avoided it by limiting the valid range of the PLUME model. However, more typically, due to the simplified treatment of the control volume in Eulerian developments, it is simply overlooked. Since the Eulerian approach deals with fluxes through surfaces that,

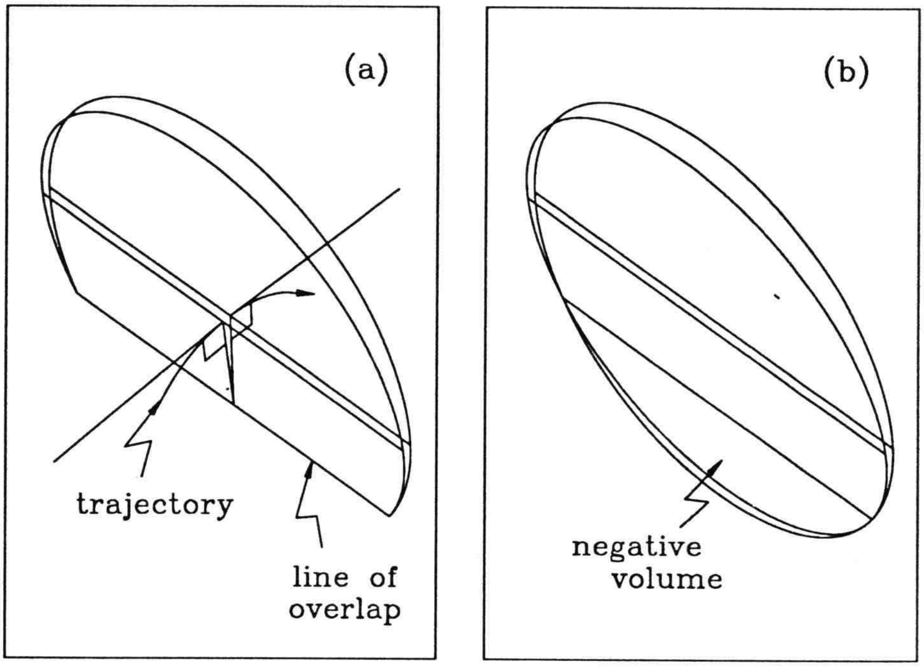

Figure 16. Plume element details: a) consistently depicted, and, b) traditional depiction showing the anomalous negative volume.

it is believed, can be located arbitrarily, only schematic sketches of the control volume are usually constructed (cf. Figure 13). Furthermore, since Equation 24 is implicit in the Eulerian formulation (Equation 57) the intersection of control volume faces is not readily apparent since the simple cylinder equation is not used explicitly. Even when cross-sections perpendicular to the trajectory are used (e.g. Davidson, 1986) intersection is not anticipated or addressed. Thus, the careless use of schematic drawings has contributed to the problem being overlooked. Inadvertently, the negative volume anomaly causes unintended or ill-defined flux quantities to be integrated at control volume surfaces.

PROPOSED IMPROVEMENT

As a first order improvement of earlier practices, the topologically new control volume object proposed by Frick, Baumgartner, and Fox (1993) is used to replace the standard control volume or element. It preserves the round plume assumption but is limited the unoverlapped portion of the volume. The inclusion of negative volume in existing Eulerian and Lagrangian models is actually unintentional and is the result of integrating the volume across the full diameter of the element. Defining the limits of integration to extend from the upstream end of the diameter to the point of intersection eliminates the negative volume from the integration and yields a more realistic formula for the control volume from which the radius may be derived. The corrected volume is expressed by

$$Vol = \frac{\pi}{2}hb^2 + hd\sqrt{b^2-d^2} + hb^2\sin^{-1}\frac{d}{b} + \frac{2}{3}\frac{h}{d}\sqrt{(b^2-d^2)^3} \tag{58}$$

where d is the distance from the center to the point of intersection. Note that when $d = b$ and the volume is multiplied by density that Equation 58 reduces to Equation 24.

Equation 58 cannot be solved analytically for the radius, however, approximation techniques may be used to find b. The use of a simple bisection technique in the Lagrangian model gives the results shown in Figure 17, which corresponds to Figure 15b.

Several improvements are achieved by adopting the new element or control volume. Most importantly, the radius and entrainment of the plume do not grow as rapidly as they did previously. This is directly correlated to the elimination of the sharp curvature, caused by the anomaly, discernible at the point where the centerlines first diverge, a region pinpointed in Figure 15b. Thus the corrected model leads to more rise and less trajectory curvature and entrainment. In the present case the overall entrainment is reduced by about 15%. While this change may not seem large it should be remembered that it derives from only a small part of the total trajectory. It is also sufficiently large to suggest that modelers may be tuning their theories inappropriately by adjusting entrainment coefficients to mathematical artifacts.

In addition, the top of the simulated plume does not undulate as much as before, in fact, its vertical excursion beyond the point of maximum rise is considerably less than the vertical excursion of the center-of-mass, which exhibits the characteristics of a Brunt-Vaisala wave. The same is true for the bottom of the plume because the negative volume portion is absent. This trend better approximates experimental data which also

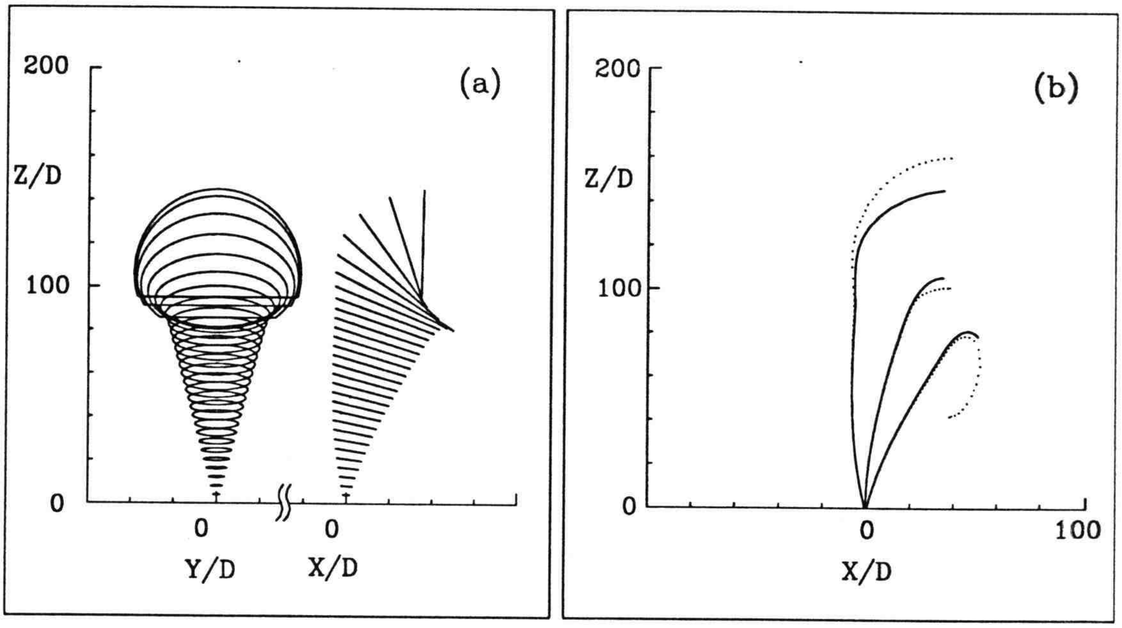

Figure 17. Lagrangian model comparison showing the effect of the volume correction: a) orthogonal views of the corrected plume, and b) comparison of edges and centerlines (corrected model: solid lines).

do not show pronounced oscillations (cf. Figure 4b and 4d).

These points could be made more clear if the simulations were carried out beyond the point of maximum rise. However, beyond that point the entrainment assumption is not well formulated or verified. Also, as is so often the case, the correction of one problem leads to the identification of another. Once the negative volume anomaly is eliminated in the proposed way it becomes apparent that the relationship between the traditional conception of plume centerline and plume element has changed. If overlap is pronounced the entire plume element mass can be above the trajectory, clearly an undesirable side effect of the traditional conception.

Thus, in the Lagrangian formulation the next logical step is to position the plume element so that its center-of-mass coincides with the corresponding point on the trajectory. In other words, plume diameters are no longer simply drawn with their centers on the predicted plume trajectory which, in the Lagrangian formulation, is clearly the trajectory of the center-of-mass of the plume element. Thus, Equation 58 must be solved simultaneously with the equation for the center-of-mass of the element, an exercise that has been accomplished in the absence of overlap ($d \geq b$) by Frick, Fox, and Baumgartner (1991).

With a new emphasis on the center-of-mass, it follows that the center-of-mass trajectory must drift due to the asymmetric addition of mass (entrainment) to the plume element, a concept that is also considered by Frick, Fox, and Baumgartner (1991). They found that these corrections, in the absence of the negative volume anomaly, can lead to changes in simulated maximum penetration and dilution of about 15 percent. This application of particle dynamics and resulting implications, though far reaching, need not make fluid dynamicists uncomfortable. The equivalence of both models makes the parallels inescapable. Since equivalence has been established without actually damaging the basic Eulerian integral flux modeling edifice, perhaps further exploration along these lines will lead to other positive findings.

CHAPTER 5. CONCLUSION AND RECOMMENDATIONS

SUMMARY

This work is noteworthy for its use of the Lagrangian formulation, considering the majority of models in its class are Eulerian integral flux models. In addition, two fundamental ideas are developed, the Projected Area Entrainment (PAE) hypothesis and the Negative Volume Anomaly (NVA).

The PAE hypothesis describes entrainment due specifically to current, i.e. forced entrainment. In contrast, in quiescent ambient fluid or in low current, Taylor entrainment is an important component of overall entrainment. The PAE hypothesis holds that entrainment is simply proportional to the area of the plume element surface projected onto a plane defined by the local current vector (Winiarski and Frick, 1976, 1978; Frick 1984). It is defined to be the flux of mass through this area which changes as the plume element moves along the plume's trajectory. Described as early as 1960 (Rawn, Bowerman, and Brooks), the Lagrangian model PAE hypothesis was the first accurate mathematical description of the hypothesis.

PAE is a simple way to close the plume equations of motion and is worthwhile because it enables modelers to predict gross plume behavior, such as average motion and dilution, without the tuning of coefficients (Frick, 1984; Lee and Cheung, 1990; Cheung, 1991). It applies in the initial dilution region of plumes in moderate to high current.

The Negative Volume Anomaly (NVA) is an inconsistency in the definition of control volume used in some hydrodynamical models. Speaking generally, it is important because control volumes are the fundamental building blocks of such models. A significant inconsistency in their basic formulation affects the overall quality of predictions whenever the conditions causing the inconsistency are present. This happens when plume bending, or trajectory curvature, is pronounced. In these cases, the element faces that define portions of the plume element surface intersect, creating an anomalous part possessing negative volume. This negative volume is an artifact of the mathematical definition, not a physical reality.

The anomaly is related to the inappropriate application of the round plume assumption. Traditionally, to apply the assumption, the simple equation for a right cylinder is used to derive the radius. The scalar value of the volume itself comes from the entrainment equation and the equation of state. The method breaks down and causes the radius to be overestimated when trajectory curvature implies the plume element faces intersect, in other words, when the NVA exists. This is because the negative portion of the plume element reduces the positive one. Thus, larger dimensions are inferred to make the volume of the anomalous plume element agree with the entrainment hypothesis. The process is a classic feedback mechanism in that increased radius leads to even more entrainment, magnifying the problem (Frick, Baumgartner, and Fox, 1993.)

The PAE hypothesis eventually helped to establish the Lagrangian plume model as a legitimate alternative to Eulerian integral flux models because the model predicts well over a large range of conditions. The NVA, on the other hand, represents a fundamental error in earlier Lagrangian models and is responsible for the degradation of the model when overlap occurs. Considering the general equivalence between the two model approaches, it is natural to wonder whether the anomaly exists in the Eulerian formulation. A simple comparison of similarly formulated models of both types shows that it does. Consequently, the NVA affects both approaches.

The degree to which the NVA affects model solutions will ultimately determine whether the anomaly is important from a practical point of view. This work shows differences in dilution of about 20% in some cases. It is possible that differences twice as large may ultimately be attributed to the anomaly under certain conditions yet to be identified. However, further work awaits the successful attack of the numerical instability that arises when the basic model is corrected for the NVA.

PUBLICATION EXPERIENCE

It has proven very difficult to publish these findings. The seminal work describing the PAE was eventually published in the peer reviewed literature (in *Atmospheric Environment*; Frick, 1984) after a protracted and unsuccessful attempt to publish in the ASCE *Journal of Hydraulic Engineering*. This is ironic in light of the subsequent publication, in another journal, of Lee and Cheung's work (1990) supporting the Lagrangian model using PAE and the extensive work of Cheung (1991). More recently, similar difficulties have been encountered with the NVA.

After many years of soul searching and thought about circumstances surrounding these experiences it has become clear to me that an ingrained bias against the approach virtually precludes the objective consideration of the model and its results. In my opinion, the reaction to the model is pathological, if indeed scientific pathology is the refusal to confront physical absurdities, i.e., negative mass, when they are clearly presented as such. After all, the NVA is essentially exactly that, a statement of a physical fantasy which exists in extant plume models.

A paper similar to the one given in Appendix 1 was submitted to the ASCE *Journal of Hydraulic Engineering* despite the earlier negative experience because it is the appropriate forum for the work. ASCE represents the users of the Lagrangian plume models more than any other organization. This judgement is supported by the Editor of the *Journal of Environmental Engineering* (see Appendix 5.) Also, we believed that the publication of Lee and Cheung (1990) signaled a thaw in the review climate at ASCE.

However, promoting the Lagrangian plume model, the PAE hypothesis, and the NVA has been both frustrating and enlightening. Clearly, I would have preferred having the model accepted immediately upon initial submission. As it happened, I have aged a score of years and have forfeited various benefits of reputation which might have accrued had events smiled upon this work more consistently.

On the other hand, I have learned a great deal about what science is, other than what I thought it was when I was a naive graduate student. Science is a social enterprise (Bella, 1987), which has many positive connotations. However, Koestler (1963), McCutchen (1991) and other point out, it also means that it is affected by closed mindedness, bias, and other human shortcomings.

In the effort to publish the work it became apparent that supporters of the Lagrangian model and the editors and peer reviewers are talking past each other. Kuhn (1970) commented on the tendency for this to happen when revolutionary science comes into contact with normal science. Stated less euphemistically, there is a bias against the approach.

Editors are unable to transcend their paradigm. James Liggett (1993) insists on the competency of his reviewers (see Appendix 4) in defending the decision to reject the work. He does so in the face of clear evidence that contradicts this claim: when pressed the reviewers apparently will accept the notion of negative mass rather than entertain the Lagrangian model as a valid alternative to traditional models. Negative mass violates all scientific paradigms and hence renders the reviewer's objections invalid.

When it is finally accepted by the community that bias is indeed a problem the solution will be found and will be found to be rather simple and painless. I suspect the current objections are partly grounded in people's fears of losing the privileges that the present system confers on them.

Finally, I do not question the right of the ASCE *Journal of Hydraulic Engineering* or others to publish what they want, that is their prerogative. However, many reviewers and editors seem to believe that to reject work it must be found to be inadequate and basically flawed in some way, instead of simply finding that the work is not wanted because it is outside their domain of interest. The NVA is not flawed, the reviewers objections are. At least, we have given sufficient evidence to show that the idea should be debated by the open community. I suggest that the failure of the system to respond appropriately to our submission is an indication of the reinforcing cycles described by Bella (1994). In this connection, comparing slavery with peer review is appropriate in that both exhibit similar behaviors.

RECOMMENDATIONS

This thesis gives evidence that bias exists in science, particularly in peer review. This bias unfairly blocks legitimate work from being published. Improving the quality of peer reviewers, as some have suggested, would help reduce the deleterious effects of bad reviewers, however, since scientists already tend to be individuals of unusually high quality, it is unlikely that conditions will improve substantially in this way.

On the other hand, scientific institutions and practices have been created by humans, and can be changed by humans. Secondly, there is no reason to believe that the present system is better than other systems that remain untried. In particular, the acceptance of revolutionary science would be better assured by the opening of existing systems. The negative influence of bias is more apparent in open systems than in anonymous systems. Thus, any procedural changes that open systems would also tend to encourage individuals to respond more open-mindedly to new ideas.

As has been suggested, one way to open the system is to make signed reviews standard practice. That would tend to discourage some of the worst peer review abuses. However, the system might be opened in ways that do not sacrifice anonymity.

I suggest that if authors were allowed to submit to multiple journals the balance of power in scientific publication would shift dramatically. If this practice were adopted, authors would be honor bound to publish in the journal that first accepted their contribution. Abuses perpetrated by biased (or malicious) reviewers would tend to come to the attention of editors rather quickly after discovering that such work was accepted elsewhere. The revision of manuscripts that often goes on now between submitting to different journals would no longer tend to mask the true bias that exists.

A common reaction to this proposal is given by James Liggett (1993, see Appendix 4). It contends that the system would be inundated. However, it is not by any means certain that would be the case, as is pointed out in Appendix 4. Furthermore, the basic idea could be modified to produce the desired effect. For example, authors could be required to inform each journal of the other journals to which a work is sent, that way editors could decide promptly whether or not they wished to have the paper undergo full review. If not, they could promptly inform the authors.

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APPENDICES

APPENDIX 1. NEGATIVE VOLUME ANOMALY MANUSCRIPT

INTRODUCTION

The manuscript given in this appendix was submitted to the *Journal of Hydraulic Research* on 28 October 1993. It was accepted conditionally for publication on 6 March 1994.

The positive outcome of the JHR review does not invalidate the conclusions drawn in this thesis regarding peer review. Though bias is not universal in peer review, neither is it uncommon. Certainly it produces a troublesome aberration of desirable and healthy peer review that weakens science in general. Consequently, it is appropriate to pursue the earlier path of identifying bias in science and offering remedial measures.

While some improvements were made before the latest submission, the paper given in this appendix is substantially similar to the one rejected by the *Journal of Hydraulic Engineering*, as described in this thesis. The review comments on which the JHE rejection was based are given in Appendix 3.

For brevity, the paper's reference section is omitted. References may be found in this thesis.

COVER LETTER

October 27, 1993

Dr. Paul Kolkman
Editor, *Journal of Hydraulic Research*
International Association for Hydraulic Research
Rotterdamsweg 185
PO Box 177
2600 MH Delft, Netherlands

Dear Dr. Kolkman:

Enclosed is a paper we submit to you for possible publication in your journal. Thank you for your positive response to our previous letter soliciting your opinion on the probable openness of your reviewers to its main topic, the negative volume anomaly. Our previous submission to ASCE Hydraulics Division was sobering, to say the least. Except for one, the reviewers did not acknowledge the anomaly, and, the one who did otherwise give a good review concluded that "what the authors see as a correction, leaving out 'negative volume', I see as an incorrection." He could have left that to a larger audience to decide.

I think the treatment accorded the paper at ASCE conceals other issues. Since its inception, the Lagrangian model has been denigrated by various, some of them prominent, ASCE members. To be blunt, I think they feel that the Lagrangian association to particle physics takes away from their pretensions to being fluid dynamicists. We are also critical of Gaussian models and the excessive dependence on tuning that characterizes the field. These observations are based on my experience with earlier work which was eventually published by *Atmospheric Environment* in 1984. I tried to publish it in ASCE *Journal of Hydraulic Engineering* in 1980. It covered many of the same topics later addressed by Lee and Cheung (1990), to whom we are indebted.

We submitted the present paper to ASCE at Don Baumgartner's urging, after all, ASCE does speak to our primary audience, and I thought, in view of the Lee and Cheung work, that the ASCE climate had become more benign.

Judging from your letter, your experience with your reviewers is likely to be more like my experience with *Atmospheric Environment* than with ASCE. We have revised the paper again in preparation for IAHR. In any case, I thought you should be informed about our background.

Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

MANUSCRIPT SUBMITTED TO *JOURNAL OF HYDRAULIC RESEARCH*

The manuscript submitted to the International Association for Hydraulic Research (IAHR) *Journal of Hydraulic Research* is given below.

IMPROVED PREDICTION OF BENDING PLUMES

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ABSTRACT

Plume models can define control volumes, or elements, that are frequently inconsistent with the overall geometry of the problem, tending to cause entrainment to be overestimated. The errors derive from an incongruity in plume and element conceptions that makes it impossible to properly construct the plume from the envisioned elements. The intersection of the round element faces, when plume diameters and trajectory curvature are large, causes the inadvertent and anomalous generation of negative volume and hence a faulty integration of the plume equations. A comparison of equivalent Eulerian integral flux and Lagrangian models shows that both are susceptible to the problem even though it is identified specifically in the latter formulation. A modified plume control volume is proposed and its performance is demonstrated. The development yields clues to plume behavior pertaining to, for example, the distribution of mass in the element.

INTRODUCTION

The use of Eulerian integral flux models based on flux balances through fixed control volumes is well established in plume modeling. Significant progress was made with such models after the introduction of the Taylor entrainment hypothesis, which expressed the proportionality of entrainment (flow into the plume) to the diameter and velocity shear between plume and quiescent ambient fluid (Morton, Taylor, and Turner, 1956). Proposed after the Gaussian profile describing the distribution of properties within the plume was well developed (e.g. Albertson et al., 1948), its success encouraged the search for an analogously succinct statement of forced entrainment — induced by the current or wind (e.g. Hoult, Fay, and Forney, 1969). As noted by Lee, Cheung, and Cheung (1987) and others, the results have been disappointing and have prompted successively more elaborate attempts, as by Schatzmann (1979), to solve the problem.

The Lagrangian plume model is an alternative way of viewing the problem, using a moving plume element as the basis for developing the plume equations. Though radically different in formulation, it is equivalent to the Eulerian integral flux model as is shown subsequently. First used by Frick and Winiarski (1975) and described by Winiarski and Frick (1976, 1978), little effort was made to accommodate a similarity profile. While not all Eulerian plume models are Gaussian, this difference and the new control volume did lead to a succinct statement of forced entrainment: the Projected Area Entrainment (PAE) hypothesis (Frick, 1984). It states that forced entrainment is simply the product of the ambient density, current speed, and the area of the plume element projected onto the current. A simpler form was introduced earlier (Rawn, Bowerman and Brooks, 1960).

The hypothesis has been extensively used and verified. The original freshwater Lagrangian model (Winiarski and Frick, 1976) was adapted to marine applications by Teeter and Baumgartner (1979) by introducing a generalized the equation of state. Their model, OUTPLM, was further modified for merging plumes and renamed UMERGE (Muellenhoff et al., 1985). Recently it was generalized further and, as UM, is included in the EPA PLUMES modeling interface (Baumgartner, Frick, and Roberts, 1993). Other Lagrangian models using PAE include JETLAG, a three-dimensional plume model developed by Lee and Cheung (1990) and Cheung (1991), and, atmospheric plume models developed by Winiarski and Frick (1978). The latter were evaluated by Policastro et al. (1980) and Carhart et al. 1982.

Cheung (1991) gives extensive verification evidence based in part on earlier work by Lee, Cheung, and Cheung, 1987, and Lee and Cheung, 1990. He shows that JETLAG with PAE better predicts asymptotic trajectory and dilution parameters than do Chu's (1975) and Schatzmann's (1979) models. Also, it predicts the correct power law dependencies of rise and their transitions in different flow regimes. Baumgartner, Frick, and Roberts (1993) show that the hypothesis is consistent with experimental results obtained by Roberts, Snyder, and Baumgartner (1989). Lee (1992) suggests that turbulence theory should support PAE, an idea developed further in Lee, Rodi, and Wong (1993).

For the sake of illustration, Figure 1 gives a comparison between JETLAG (solid lines), UM predictions (broken lines), and raw data (Fan, 1967) for a range of densimetric Froude numbers and efflux to current ratios, k. Horizontal and vertical distances are expressed in diameters. The general goodness of prediction is apparent, particularly in the faithful reproduction of the bending (high trajectory curvature) phase. The differences can be attributed to different implementations of the Taylor hypothesis.

The negative volume anomaly, the main topic of this paper, is an inadvertent consequence of earlier model formulations using the round plume assumption. When present it causes the radius to be overestimated, and, by coupling to the entrainment hypothesis, cascades to the element mass and other plume variables. In extreme cases predictions degrade severely, contributing to earlier models being limited to the initial dilution region near the source (Muellenhoff et al., 1985). Fortuitously, this limitation was helpful in the establishing veracity of PAE. Also, the anomaly is frequently not present (even beyond the trapping level). In the cases shown in Figure 1 the curvature and radii are sufficiently small so that the anomaly is avoided.

Fan does describe conditions in which the model exhibits the anomaly, for example, in Figure 2. The hook is evidence of its existence, as is explained subsequently. Since it asserts the existence of negative volume, and mass, the anomaly is clearly spurious and unwanted. It is a classical feedback mechanism that amplifies until the turning of the element to the prevailing current reduces the curvature. It may be correlated to the onset of various plume phenomena, such as upstream lateral

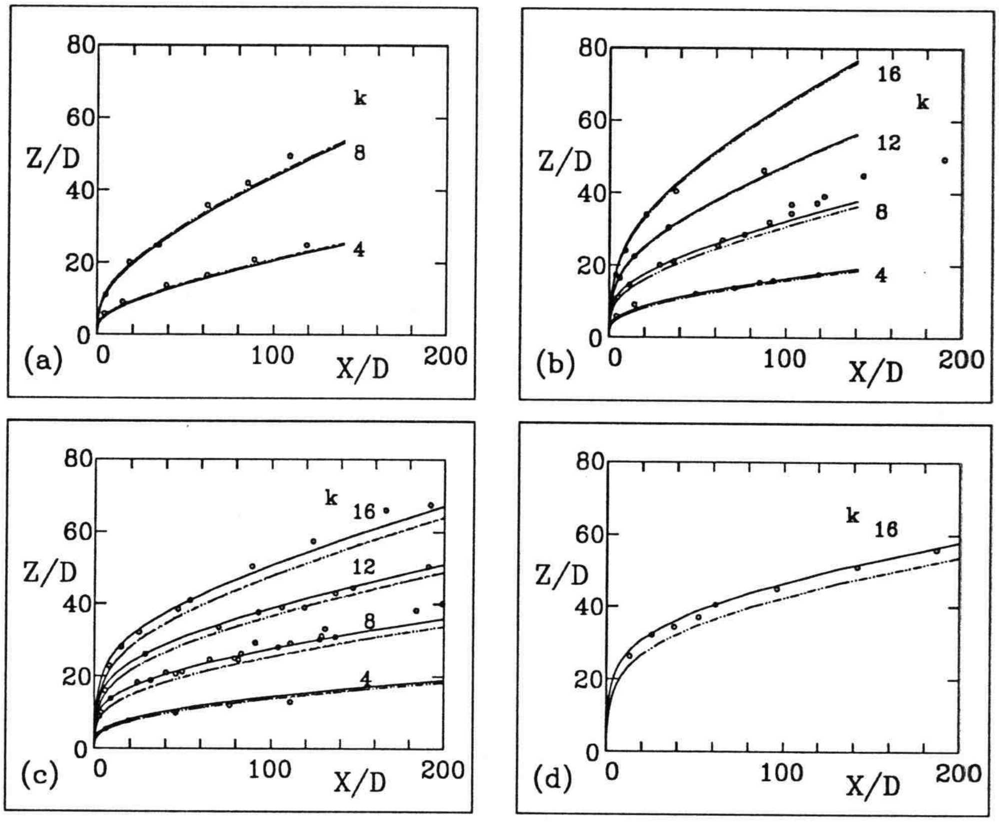

Figure 1. Trajectory comparisons between UM (dashed) and JETLAG (solid), Lagrangian models using PAE. Densimetric Froude numbers 10, 20, 40, and 80 for (a) through (d) respectively. Data after Fan (1967).

spreading (anvil formation) and increased internal diffusion (Frick et al. 1990).

The use of unrealistic, idealized forms of the plume element is the reason the problem was unrecognized until now. In all affected models the control volume is treated either as a right cylinder (e.g. Weil, 1974) or unintersected section of bent cone (e.g. Frick, 1984, and Davidson, 1989). The common equation for a cylinder is then used to derive the radius, the volume being obtained from the entrainment hypothesis. But, when the faces of the element intersect, or overlap, which they do when curvature is large since the faces are defined to be perpendicular to the trajectory, this method is invalid. In such cases a purely mathematical, negative volume is created and inadvertently a larger, erroneous radius, conforming to a physically unreal object, is derived.

The anomaly is important because degraded model performance encourages unjustified tuning of coefficients. In other words, anomalous behavior is mistaken for bona fide deficiencies in legitimate model theory, particularly entrainment. Many entrainment coefficients and mechanisms have been introduced to account for "observed" discrepancies between prediction and observation. But, if strong bending occurs their justification is dubious because it is not known whether physical phenomena or mathematical artifacts account for the deficiencies. Gaussian models are especially susceptible to modification because they use nominal radii, arbitrarily defined and always well within the plume, which tend to obscure various dynamic effects (Frick, 1984).

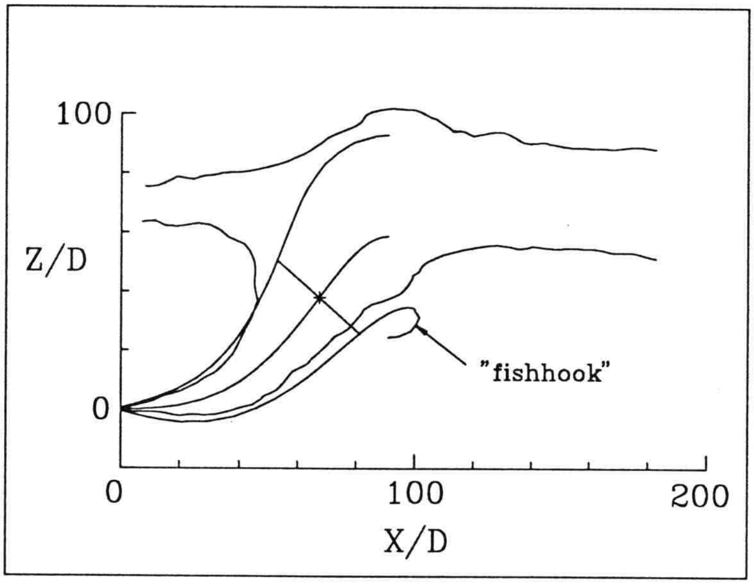

Figure 2. Example of a plume simulation affected by the negative volume anomaly.

Because they are equivalent formulations, the anomaly should affect Eulerian model solutions also, a deduction which we demonstrate by developing both models. It is found that they are equivalent whether or not the anomaly is present. The origin of the anomaly is analyzed and an alternative element is proposed, a modified round plume assumption. Implications on plume modeling are examined and provide additional insights. For example, the traditional practice of symmetrically depicting the plume diameter about the "centerline" trajectory of the plume is found insupportable because it is inconsistent with the center-of-mass trajectory in regions of strong bending.

MODEL DEVELOPMENT

The negative volume anomaly is identified in and associated with the Lagrangian plume model. However, since Lagrangian and Eulerian models are alternative but equivalent formulations the anomaly must pervade both (uncorrected) formulations. In other words, Eulerian integral flux plume models are subject to the anomaly as well. A demonstration showing that there is equivalence between uncorrected Eulerian and Lagrangian model simulations proves this assertion, showing both models share similar limitations.

An Eulerian integral flux model

The model chosen for its relative simplicity to represent Eulerian integral flux models is adapted from Weil (1974). The equations, with the exception of the entrainment function, are developed with reference to the control volume shown in Figure 3. It is important to note that the faces of the depicted control volume are parallel, which has mathematical implications that are not fully appreciated. For this conception to be valid the angles of the integrated flux vectors, i.e. normal and tangential fluxes, must be considered. Thus, contrary to expectations suggested by the standard depiction, the model is subject to the negative volume anomaly — proving, irrespective of intent, that in zones of high curvature the mathematics is that of intersecting round plume cross-sections.

The Eulerian integral flux plume model uses integrated flux quantities defined as follows. The volume flux α:

$$\alpha = b^2|\underline{V}| \tag{1}$$

where b is the radius of the control volume and $\underline{V}$ is the plume velocity vector. (π is a common factor in the equations that is implicitly incorporated.)

The x-momentum deficit flux β:

$$\beta = b^2|\underline{V}|(v_x - u_x) \tag{2}$$

where v_x and u_x are x-components of the plume velocity vector and the current velocity respectively.

The z-momentum deficit flux γ:

$$\gamma = b^2|\underline{V}|v_z \tag{3}$$

where v_z is the vertical component of the plume velocity vector.

The temperature deficit flux δ:

$$\delta = b^2|\underline{V}|(T - T_a) \tag{4}$$

where T is the plume temperature and T_a is the ambient temperature. And the salinity deficit flux ε, not given in Weil (1974) but needed here:

$$\epsilon = b^2|\underline{V}|(S - S_a) \tag{5}$$

where S is the plume salinity and S_a is the ambient salinity. The differential equations corresponding to Equations 1 through 5 follow.

The equation of continuity used here is

$$\frac{d\alpha}{ds} = 2ab|\underline{V}-\underline{U}| + |\underline{U}|\{b\cos\theta\frac{\partial b}{\partial s} + \frac{2b}{\pi}\sin\theta + \frac{b^2}{2}\sin\theta\frac{\partial\theta}{\partial s}\} \tag{6}$$

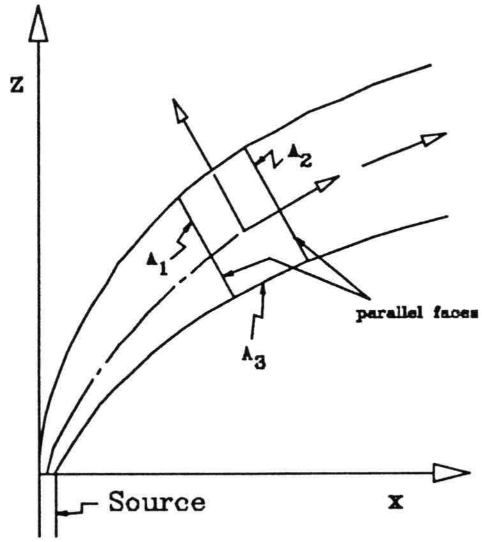

Figure 3. Schematic drawing of the Eulerian control volume, after Weil (1974). The conception of A_1 and A_2 as parallel faces is apparent.

is the dimensionless shear or aspiration entrainment coefficient, θ is the angle the trajectory makes with the horizontal axis, and s is the distance measure along the trajectory.

The compound term in Equation 6 is the Eulerian form of the Projected Area Entrainment (PAE) hypothesis given by Frick (1984). Unlike many entrainment assumptions, it includes terms proportional to both $\partial b/\partial s$ and $\partial\theta/\partial s$. In contrast, Weil used an entrainment hypothesis simply proportional to the radius of the plume and vertical velocity of the plume element. Weil's entrainment assumption produces excessive growth and therefore is not adopted, however, when it is used, the two models still give equivalent predictions.

The remaining differential flux equations are

$$\frac{d\beta}{ds} = -b^2 v_z \frac{\partial u_x}{\partial z} \tag{7}$$

$$\frac{d\gamma}{ds} = b^2 \frac{\rho_a - \rho}{\rho} g \tag{8}$$

$$\frac{d\delta}{ds} = -b^2 v_z \frac{\partial T_a}{dz} \tag{9}$$

and

$$\frac{d\epsilon}{ds} = -b^2 v_z \frac{\partial S_a}{\partial z} \tag{10}$$

where ρ is the element density, ρ_a is the ambient density at the level of the element's center-of-mass, and g is the acceleration of gravity.

The equations are solved for the secondary variables in the following way. The temperature at step i+1 is

$$T_{i+1} = T_{ai} + \frac{\delta_{i+1}}{\alpha_{i+1}} \tag{11}$$

and, correspondingly, the salinity is

$$S_{i+1} = S_{ai} + \frac{\epsilon_{i+1}}{\alpha_{i+1}} \tag{12}$$

The density ρ is a function of pressure, salinity, and temperature. The details are not of concern here, however, the equation of state, σ_T, can be formulated to be valid for fresh and seawater for large, essentially geothermal, ranges of temperature and pressure because the Boussinesq approximation is not used.

$$\rho_{i+1} = \sigma_T(p_{i+1}, S_{i+1}, T_{i+1}) \tag{13}$$

where p, the pressure at depth, is retained for generality but is not a factor in this paper. In deep geothermal applications it would be necessary to recalculate the pressure and also temperature changes due to potential temperature effects.

The new velocity vector is written as

$$\underline{V}_{i+1} = \frac{\beta_{i+1}}{\alpha_{i+1}}\underline{i} + 0\underline{j} + \frac{\gamma_{i+1}}{\alpha_{i+1}}\underline{k} \tag{14}$$

where $\underline{i}$, $\underline{j}$, and $\underline{k}$ are the primary horizontal, secondary horizontal, and vertical unit vectors respectively. Since the model is two-dimensional the y-component is zero.

The new vector displacement is given by

$$\Delta\underline{R} = \underline{V}_{i+1}\Delta t \tag{15}$$

the distance along the trajectory by

$$s_{i+1} = s_i + \Delta s \tag{16}$$

where $\Delta s = |\Delta\underline{R}|$, and the radius vector is

$$\underline{R}_{i+1} = \underline{R}_i + \Delta\underline{R} \tag{17}$$

The new radius is calculated from

$$b_{i+1} = \sqrt{\frac{\alpha_{i+1}}{|\underline{V}_{i+1}|}} \tag{18}$$

representing the standard round plume assumption, at issue in this paper.

The Lagrangian model

At a fundamental level, the Lagrangian model incorporates the same ideas as the Eulerian and they are equivalent, providing that equivalent assumptions are used and the length of the element is allowed to vary (Frick, 1993; Frick and Winiarski, 1975). However, the respective viewpoints are substantially different, helping to establish the growth and trajectory curvature terms of the PAE hypothesis in the Lagrangian formulation (Frick, 1984), and other properties, specifically the negative volume anomaly.

After some evolution the element now corresponds to the conception shown in Figure 4. As may be seen, the plume element is constructed from two cross-sections separated by a small distance. Figure 4 shows the element in subsequent stages of development, showing a decrease in the length of the plume element, *h*, as the velocity along the trajectory decreases, a typical condition because frequently the effluent velocity is greater than the ambient velocity. This contraction is central to the Lagrangian model development and is discussed further below. The effects of growth and curvature on the element are also apparent. A profoundly overlapped element with intersecting faces is shown near maximum plume rise. While the length of the element is exaggerated, other details are faithfully depicted.

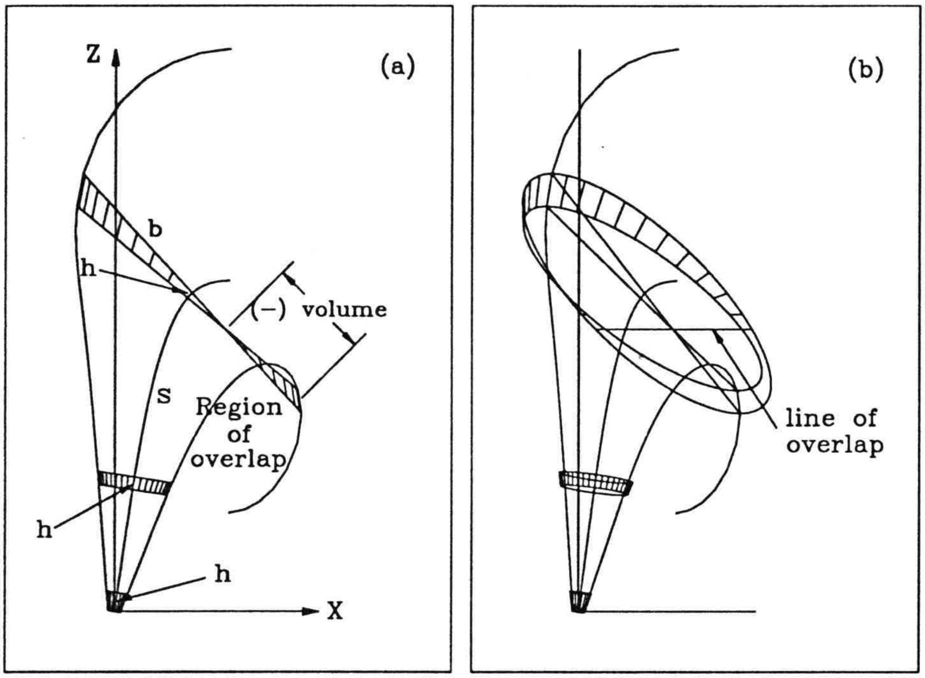

Figure 4. The Lagrangian plume element at three stages of development. Note overlap and variations in the length of the element. (a) side view, and (b) oblique with positive part of element hatched.

The plume element length change is expressed by

$$h_{i+1} = \frac{|\underline{V}_{i+1}|}{|\underline{V}_i|} h_i \tag{19}$$

where h is the length of the element along the trajectory. This relation explains why dilution and radii (squared) are not, in general, directly proportional to each other, a fact suggested by experimental data (e.g. Fan, 1967) and included in the solution given by Morton, Taylor, and Turner (1956). It is fortuitous that the average velocity serves adequately in describing this aspect of plume behavior even though it is known that the actual material element deforms rapidly and is not a coherent entity for long.

The crucial property described by Equation 19 establishes a viewpoint on the plume element, that the Eulerian formulation lacks, which makes it possible to identify both $\partial b/\partial s$ and $\partial \theta/\partial s$ as important ingredients in the entrainment process. They follow from a careful derivation of the projected area of the plume element (Frick, 1984). Again, while the two formulations are effectively equivalent given identical assumptions, the ideas that emerge from their development are considerably different.

Major model equations include the entrainment,

$$\frac{dm}{dt} = \rho_a bh\{2\pi a|\underline{V}-\underline{U}| + \pi|\underline{U}|\cos\theta\frac{\partial b}{\partial s} + 2|\underline{U}|\sin\theta + \frac{\pi}{2}b|\underline{U}|\sin\theta\frac{\partial\theta}{\partial s}\} \qquad (20)$$

where m is the mass of the element and t is the time.
The equation of momentum is

$$\frac{dm\underline{V}}{dt} = \underline{U}\frac{dm}{dt} - \frac{\rho_a - \rho}{\rho}g \qquad (21)$$

Conservation of energy equation can be expressed in terms of the enthalpy, H, since the specific heat at constant pressure changes considerably under geothermal conditions:

$$\frac{dmH}{dt} = H_a\frac{dm}{dt} \qquad (22)$$

where conduction and radiation are neglected. Temperature can be obtained by reference to the appropriate enthalpy. The non-linear effects arising from the profile of properties across the plume are neglected but will sometimes prove to be significant.

Conservation of salinity is given by

$$\frac{dmS}{dt} = S_a\frac{dm}{dt} \qquad (23)$$

The equations defining the new location of the plume element are the same as for the Eulerian model. However, the radius of the plume element at time $t+\Delta t$ must be recalculated as a function of h and the volume of the element. Earlier Lagrangian models, including the recent model of Lee and Cheung (1990), calculate the radius from the mass of the element by

$$b_{i+1} = \sqrt{\frac{m_{i+1}}{\pi\rho_{i+1}h_{i+1}}} \qquad (24)$$

which is once again the standard implementation of the round plume assumption corresponding to Equation 18. In the following section, Equations 18 and 24 are assumed so that a comparison of the basic models may be made. Subsequently they are shown to be incorrect when the plume element faces intersect, causing the negative volume anomaly.

MODEL EQUIVALENCE

The Lagrangian approach is less familiar to many readers, and the traditional relationship between the Lagrangian formulation and particle dynamics may cause anxiety about their use in fluid dynamics (see page 71 of Batchelor, 1971). These concerns, perhapse appropriate in certain applications, can be dispelled here by showing the models are equivalent. Doing so is appropriate in its own right as it allows conclusions to be drawn about both formulations. Most importantly, the negative volume anomaly, identified in earlier formulations of the Lagrangian plume model, can be shown to exist in Eulerian integral flux plume models.However, in a limited way the equivalence of the Eulerian and Lagrangian models was demonstrated previously (Frick and Winiarski, 1975).

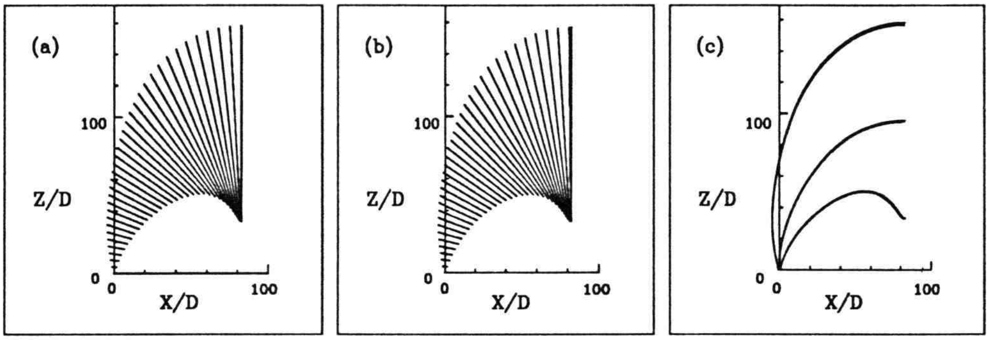

Figure 5. Eulerian and Lagrangian model comparison: (a) Eulerian model predicted diameters, (b) Lagrangian model predicted diameters, and (c) edges and centerlines.

Model equivalence is first established in Figure 5 for a case in which overlap does not occur, as is apparent by inspection of the non-intersecting diameters. The diameters predicted by both the Eulerian (Figure 5a) and the Lagrangian (Figure 5b) models are shown at approximately equally spaced intervals along the trajectory. They are constructed symmetrically about and perpendicular to the trajectory, consistent with established practice. The current is chosen to produce conditions in which the cross-sections almost intersect at the bottom boundary in the high curvature region. While current can prevent cross-sections from intersecting, it can also contribute to it, as in counter-flowing buoyant plumes.

The centerlines and plume envelopes predicted by both models are nearly identical (Figure 5c). Differences in the range of one percent can be attributed to computational artifacts and the use of the Boussinesq approximation in the Eulerian model. They are similar to those found by Frick and Winiarski (1975) and are small compared to the effect of the negative volume anomaly inherent in Equations 18 and 24 when overlap is significant, as is shown subsequently.

A case involving the negative volume anomaly is shown in Figure 6a. In this case, with no ambient current, the cross-sectional diameters intersect over part of the trajectory (not shown), creating the hook near maximum rise. Nevertheless, both models continue to give equivalent predictions. Despite extensive differences in formulation, control volume conception, and the "Lagrangian" anomaly described previously, the approaches are mathematically equivalent. Furthermore, equivalence is not dependent on the particular entrainment hypothesis used for the models as the use of Weil's entrainment assumption (Weil, 1974) also produces agreement.

A final example, Figure 6b, is a vertical discharge in weak current, representative of a large source in the deep ocean (Baker et al. 1989). It is similar to the case shown in Figure 4 which shows the intersection of the element. The plume rises through a relatively unstratified layer before encountering a stable, less dense, layer near the level of maximum rise. At this level the current also increases. The conditions, though chosen for their demonstrative value, are not unrealistic and agreement is again attained.

When the anomaly is present, entrainment is overestimated. For example, Figure 6 shows that the plumes grow excessively in very short distances where trajectory curvature is highest (i.e. where intersection is pronounced.) This is especially noticeable in Figure 6b where dilution increases fourfold in the short distance in which intersection occurs (circled). In Figure 5, where trajectory curvature is fairly constant and comparatively small, the growth in radius over a similar distance is more uniform and gradual. The hook appears again because diameters are constructed without regard to the physical consistency of the definition of the plume element.

In conclusion, the negative volume anomaly affects both, uncorrected models.

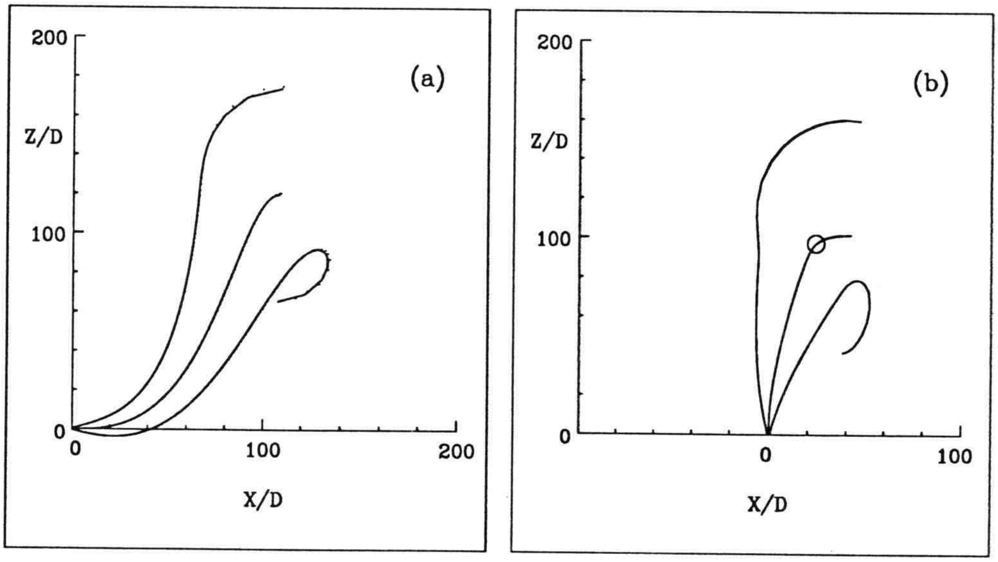

Figure 6. Additional comparisons of **both** models showing equivalence when overlap occurs: (a) horizontal discharge into quiescent ambient, and (b) vertical discharge into flowing ambient.

THE PROBLEM — CONTROL VOLUMES

The negative volume anomaly derives from the inconsistent definition of the control volume which conceives of the element as a cylinder or bent cone and fails to consider the adjustments that must be made when faces are inclined to each other beyond a critical angle, i.e. beyond the point at which the element faces begin to intersect or overlap. When this happens the volume, derived from the entrainment equation, no longer produces the correct radius when the simple equation for a cylinder (viz. Equation 18 or 24) is used to derive it.

The negative volume anomaly leads to the overestimation of entrainment because the radius is overestimated. This follows because as soon as the element faces intersect, as shown in Figure 7, a negative volume forms that affects the simulation because it subtracts from the positive part of the volume. Since volume is derived from the entrainment hypothesis the radius is forced to adjust to make the total volume of the "cylinder", now composed of positive and mathematically negative contributions, equal to the volume established by the entrainment equation. The radius calculated from Equation 24 is correspondingly, and erroneously, larger. Simply stated, the mathematics do not conform to the physics of the situation and all PAE terms in Equations 6 and 20 are inadvertently increased.

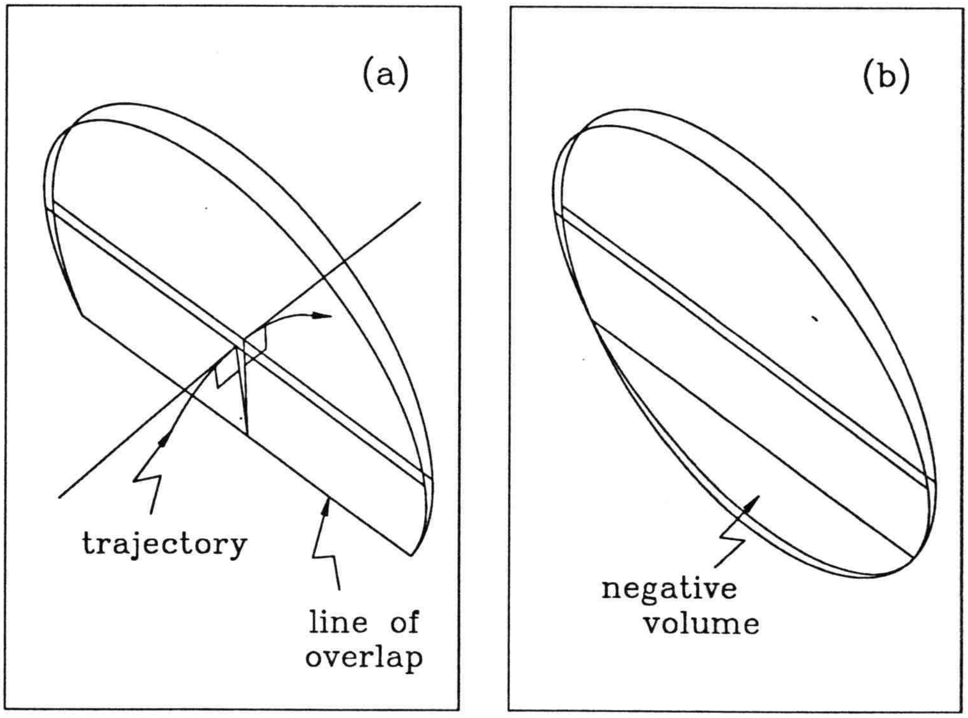

Figure 7. Plume element details: (a) consistently depicted, and, (b) traditional depiction showing the anomalous negative volume.

The overlap problem was recognized earlier. Teeter and Baumgartner (1979) avoided it by limiting the valid range of the PLUME model. However, more typically, due to the simplified treatment of the control volume in Eulerian developments, it is simply overlooked. Since the Eulerian approach deals with fluxes through surfaces that, it is believed, can be located arbitrarily, only schematic sketches of the control volume are usually constructed (cf. Figure 3). Furthermore, since Equation 24 is implicit in the Eulerian formulation (Equation 18) the intersection of control volume faces is not readily apparent since the simple cylinder equation is not used explicitly. Even when cross-sections perpendicular to the trajectory are used (e.g. Davidson, 1986) intersection is not anticipated or addressed. Thus, the careless use of schematic drawings has contributed to the problem being overlooked. Inadvertently, unintended or ill-defined quantities are integrated at the flux surfaces: the negative volume anomaly.

PROPOSED IMPROVEMENT

As a first order improvement of earlier practices, we propose a replacement volume for the standard control volume or element which, while preserving the round plume assumption, is confined to the unoverlapped portion of the volume. The inclusion of negative volume in existing Eulerian and Lagrangian models is actually unintentional and is the result of integrating the volume across the full diameter of the element. Defining the limits of integration to extend from the upstream end of the diameter to the point of intersection eliminates the negative volume from the integration and yields a more realistic formula for the control volume from which the radius may be derived. The corrected volume is expressed by

$$Vol = \frac{\pi}{2}hb^2 + hd\sqrt{b^2-d^2} + hb^2\sin^{-1}\frac{d}{b} + \frac{2}{3}\frac{h}{d}\sqrt{(b^2-d^2)^3} \qquad (25)$$

where d is the distance from the center to the point of intersection. Note that when $d = b$ and the volume is multiplied by density that Equation 25 reduces to Equation 24.

Equation 25 cannot be solved analytically for the radius, however, approximation techniques may be used to find b. The use of a simple bisection technique in the Lagrangian model gives the results shown in Figure 8, which corresponds to Figure 6b.

Several improvements are achieved by adopting the new element or control volume. Most importantly, the radius and entrainment of the plume do not grow as rapidly as they did previously. This is directly correlated to the elimination of the sharp curvature, caused by the anomaly, discernible at the point where the centerlines first diverge, a region pinpointed in Figure 6b. Thus the corrected model leads to more rise and less trajectory curvature and entrainment. In the present case the overall entrainment is reduced by about 15%. While this change may not seem large it should be remembered that it derives from only a small part of the total trajectory. It is also sufficiently large to suggest that modelers may be tuning their theories inappropriately by adjusting entrainment coefficients to mathematical artifacts.

In addition, the top of the simulated plume does not undulate as much as before, in fact, its vertical excursion beyond the point of maximum rise is considerably less than the vertical excursion of the center-of-mass, which exhibits the characteristics of a Brunt-Vaisala wave. The same is true for the bottom of the plume because the negative volume portion is absent. This trend better approximates experimental data which also do not show pronounced oscillations (cf. Figure 2).

These points could be made more clear if the simulations were carried out beyond the point of maximum rise. However, beyond that point the entrainment assumption is not well formulated or verified. Also, as is so often the case, the correction of one problem leads to the identification of another. Once the negative volume anomaly is eliminated in the proposed way it becomes apparent that the relationship between the traditional conception of plume centerline and plume element has changed. If overlap is pronounced the entire plume element mass can be above the trajectory, clearly an undesirable side effect of the traditional conception.

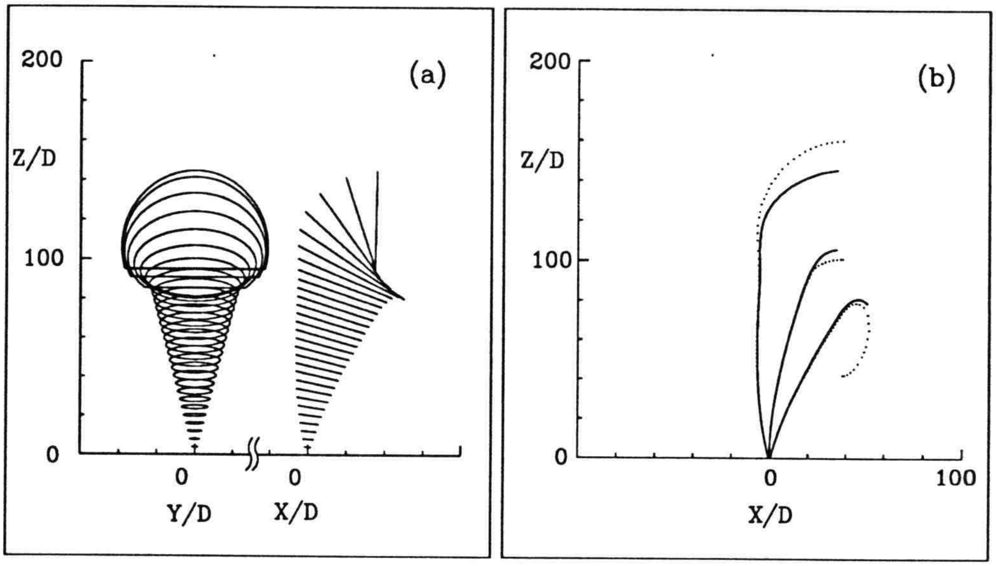

Figure 8. Lagrangian model comparison showing the effect of the volume correction: (a) orthogonal views of the corrected plume, and (b) comparison of edges and centerlines (corrected model: solid lines).

Thus, in the Lagrangian formulation the next logical step is to position the plume element so that its center-of-mass coincides with the corresponding point on the trajectory. In other words, plume diameters are no longer simply drawn with their centers on the predicted plume trajectory which, in the Lagrangian formulation, is clearly the trajectory of the center-of-mass of the plume element. Thus, Equation 25 must be solved simultaneously with the equation for the center-of-mass of the element, an exercise that has been accomplished in the absence of overlap ($d \geq b$) by Frick, Fox, and Baumgartner (1991).

With a new emphasis on the center-of-mass, it follows that the center-of-mass trajectory must drift due to the asymmetric addition of mass (entrainment) to the plume element, a concept that is also considered by Frick, Fox, and Baumgartner (1991). They found that these corrections, in the absence of the negative volume anomaly, can lead to changes in simulated maximum penetration and dilution of about 15 percent. This application of particle dynamics need not make fluid dynamicists uncomfortable. The equivalence of both models makes the parallels inescapable. Since equivalence has been established without actually damaging the basic Eulerian integral flux modeling edifice, perhaps further exploration along these lines will lead to other positive findings.

ACKNOWLEDGEMENTS

This work was supported in part by the EPA Office of Science and Technology and the EPA Office of Wetlands, Oceans, and Watersheds. The work is in partial fulfillment of Walter Frick's doctoral thesis requirements at Oregon State University, Department of Civil Engineering. This is US EPA contribution #ERL-N-158 and University of Arizona Environmental Research Laboratory Publication #93-12.

LIST OF SYMBOLS

α	volume flux
β	x-momentum deficit flux
γ	z-momentum deficit flux
δ	temperature deficit flux
ε	salinity deficit flux
θ	angle of trajectory with the horizontal axis
ρ	average plume density
ρ_a	ambient density at the level of the element's center-of-mass
a	dimensionless shear or aspiration entrainment coefficient
b	control volume, or plume element, radius
g	acceleration of gravity
h	length of the plume element
H	plume enthalpy
H_a	ambient enthalpy
$\underline{i}$, $\underline{j}$, and $\underline{k}$	Cartesian unit vectors
k	efflux to current ratio
m	mass of the element
p	pressure at depth
$\underline{R}$	plume radius vector
s	distance measure along the trajectory.
S	plume salinity
S_a	ambient salinity
t	time
T	plume temperature
T_a	ambient temperature
$\underline{U}$	current vector
u_x, u_y, u_z	components of $\underline{U}$
$\underline{V}$	plume velocity vector
Vol	plume element volume
v_x, v_y, v_z	components of $\underline{V}$

APPENDIX 2. 1980 REVIEW FRAGMENTS

1980 REVIEW

This appendix presents evidence supporting the conclusion that bias among peer reviewers is a relatively common peer review problem. The review history of the PAE starting in 1980 illustrates the problem. The paper, "Projected area in plume modeling," was the first to encounter bias and to be rejected for the kind of unscientific reasons identified by Kuhn (1970), Koestler (1964), and others. Among these reasons is the inability of reviewers to transcend the established paradigm. The tendency for them to "sturdily defend habit against originality," i.e. to act under biasing influences, is apparent in the excerpted review dated 10/16/80. The entire review in question consists of only two sentences:

> "The almost trivial work described in this manuscript has been superseded in a significant way by previously published work. See, for example:
>
> Schatzmann, M. The integral equations for round buoyant jets in stratified flows. Journal of Applied Mathematics and Physics (ZAMP) Vol. 29, pp. 608-630, 1978.
>
> ____________. An integral model of plume rise. Atmospheric Environment v. 13 no. 5, pp. 721-731, 1979.

In addition to bias, the nastiness to which McCutchen (1991) refers is evident. Whether the holders of these negative emotive reflexes are aware of them or not is not clear. They may be totally sublimated and rationalized. Kuhn (1970) speaks of the fear among established scientists that "Previously completed work on normal projects would now have to be done again because earlier scientists had failed to recognize and control a relevant variable," thus making them consciously defensive. In any case, this thesis argues that the structure of the peer review institution makes it susceptible to bias and that relatively simple measures would rectify the worst abuses.

The 1980 paper was resubmitted and eventually went through something like an appeal process. In the process, it was sent by the editor to another associate editor (apparently inadvertently, because he passed it back to the original editor). However, this third editor also contacted me personally to inform me that he thought the paper deserved to be published but that, in the existing climate, he doubted it would be. This rather candid and courageous correspondence shows, I think, how aware scientists actually are of the problems of peer review even without the benefit of what Kuhn and others have to say. If the system works in such instances as this it is due to the efforts of individuals, not to any inherent stability in the system itself. In other words, the dysfunction in peer review is self-evident, that is not the problem. The problem is the almost pathological toleration we, as scientists, have for a bad system.

One of the final rejections of the 1980 paper, dated 1/30/82, is presented below. It suggests that the adherents of the established paradigm are unable to discern the difference between dimensional terms and tuned coefficients on the one hand, and entrainment terms, derived to conform to a hypothesis, on the other. For example, note the insistence that a drag concept is used in the Lagrangian model, *if only one in which the drag coefficient is equal to zero!* This criticism was levelled despite the fact that drag was theorized not to exist because at the boundaries, at which such a force would be exerted, there is no gradient of ambient and plume properties to promote such a force.

What was noteworthy in the 1980 review was that the peer review was not unanimous in its rejection of the paper. One reviewer, after the resubmission, requested further revisions while another recommended the paper be published:

> "The revised manuscript is improved considerably and the comments I made on the first review have been addressed faithfully [and the paper should be published after minor revisions]." (Anonymous, 1981)

This contrasts to the JHE response to our 1992 paper which was entirely negative.

The work presented in "Projected area in plume modeling" was revised and subsequently published in *Atmospheric Environment* (Frick, 1984).

REVIEW OF 1/30/82

> 1. The paper starts with a fundamentally wrong premise. The abstract states: 'There is much experimental evidence showing that forced entrainment ... is directly proportional to the stream projected area'.
>
> On the contrary, there is no experimental evidence whatsoever - at least known to this reviewer - that would suggest that statement. The only two supporting references (namely, 10 and 14) are quoted entirely out of context and relate to shallow water diffuser plumes under very strong cross-flow where the usual degree of freedom in entrainment processes is suppressed.

Thus, the entire development of Frick's model is a) without any physical basis and b) contrary to known results. a) the model fails to address the physical mechanism by which entrainment would occur. In fact, the model is entirely kinematic (geometric) in nature, and does not make use of physical principles that would describe how a turbulent region (i.e. the plume) is growing in size in present [sic] of a surrounding ambient (flowing or stagnant) fluid. Nothing is said about the dynamics of the flow (e.g. shearing, local generation of turbulence, or convection of turbulence; are equilibrium assumptions justified? etc.) and under which dynamic situation the entrainment could possibly be of the type assumed by the author? b) The model is clearly wrong in the limit of small crossflow? The assumptions of the flow distribution around the plume (i.e. entrinnment on jets; Richards on thermals)? In fact, the several justifications on bottom of p. 8: "The hypothesis is consistent ... profiles (16)." are all either erroneous, quoted out of context or intelligible [sic]. The model is at variance with the fact that pressure measurements in jet cross-sections indicate a non-zero pressure force (a drag) a fact that is neglected in the momentum equation.

2. The author claims the model is "verified" with "no empirical coefficients" (p. 17, 27). This calim is not substantiated at all. a) The model is not compared to any basic equipment which would demonstrate that the postulated entrainment mechanism is correct, nor does the author describe the design of any such experiment. b) The model contains empirical coefficients just as much as any other model: The assumed witdth (and is [sic] relation to the mean flow quantities) has just as much the charactyer of a coefficient, there is an aspiration entrainment coefficeint and the drag coefficient ($C_D = 0$) is neglected. In fact, the characterization of any turbulent flow does require coefficients. c) The supposition is made that tables 1 and 2 prove the validity of the model in comparison to others. First of all, all of the performance measures are rather subjective in the final analysis and second, other models have equally good success in prredicting two plume features only.

Finally, the author's aversion to coefficients is not at all shared. In this reviewer's opinion a sound physically based plume model may be one with several coefficients but each of those should be based on simple special flow configurations for which the coefficient is (reasonably) well established from basic experiments. This process is quite different from "tuning".

3. Apart from the above fundamental criticisms, the development of the geometric relations is exceedingly awkward. Why not simply develop the analytic function for a bent-over cone and then describe its projection onto the stream cross-sectional plane. If secondary factors (such as curvature and vertex angle) are attributed such great importance, one is left with the uneasy feeling that several assumptions made in the geometry relationships could be equally important?

In summary, the present paper adds <u>nothing</u> to an improved understanding of plume dynamics.

APPENDIX 3. 1992 REVIEW

INTRODUCTION

The reviews of the 1992 submission and resubmission, five in all, are given here. The resubmission was similar to the paper given in Appendix 1. It is worthwhile to add that we resubmitted the paper because the associate editor handling the review is himself a trustworthy plume modeler. Secondly, the comments received on the original submission could all be rebutted. Comments and rebuttals are given below.

The readers of this thesis will undoubtedly reach widely differing opinions regarding its merits and the way I have confronted my critics. Some may applaud my efforts while others condemn them. Perhaps a greater measure of diplomacy or belligerence on my part would have made a difference. On the other hand, if individuals were socially graceful or powerful, would they gravitate to science? Put another way, why cannot the peer review system function properly irrespective of the character of the scientist and his peers?

In principle, a bad review can be rebutted. The fact is that without extremely active intervention from the editor of the journal, a rebuttal, no matter what its merits, has almost no chance of changing the original peer opinions. The whole idea is a community fantasy that denies the existence of inappropriate human behavior which McCutchen (1991) so aptly describes. Rebuttal does not work because reviewers do not admit mistakes, it is as simple as that. And, in the current climate of peer democracy, based on individual versus community rights, in which the editor plays a weak role, basically counting the votes, the system, viewed from the best interests of the science as a whole, is dysfunctional.

In any case, in four of the five reviews it is plain that, at best, we and the reviewers, in Kuhn's words, are talking past each other. In addition, I believe two of the latter reviewers, privy to the comments made by the original reviewers, treat the paper superficially, distance themselves emotionally (i.e., rationalize), and close ranks behind their fellow peers. Neither of them ever even openly acknowledge, much less appreciate, the significance of the negative volume anomaly.

The fifth reviewer does appreciate the significance of the anomaly and provides a thoughtful, considerate, and even beautiful review. However, he too is stuck in the established paradigm and, in Kuhn's words, devises ad hoc modifications of the established theory to eliminate any apparent conflict. In his case, he is willing to accept the notion of negative volume, preserving the status quo, rather than accepting the publication of the paper.

In summary, consider the way of determining the age of a horse -- by counting its teeth. There is an old adage about fruitless arguments in which the antagonists quibble about a point (the horse's age) while neglecting to examine the evidence (count its teeth). That is what is happening with the negative volume anomaly. My experience is that tangible evidence need not be considered by reviewers, if something offends, and it can be anything, all they have to do is cite someone else's work in opposition or simply make derogatory (and emotional) general statements.

If nothing could be done about it it would be distressing but endurable. However, much can be done to rectify the problem. To reiterate briefly, the case for "no-fault" publication advocated by McCutchen (1991) is appealing. Allowing scientists to submit simultaneously to several journals would also basically alter the chemistry of peer review.

INITIAL REVIEW: REVIEWER A

Comments on "Elements and control volumes in plume modeling."

The paper is, in fact, a critical discussion with an unfavourable opinion of the so-called Eulerian and Lagrangian models to be used for calculating the spread of vertical plumes in a cross-flow, or when the ambient fluid is calm. But the authours have not presented an alternative except the introduction(a) of Eq.19, based on solid mechanics, the justification of which is very much questionable, and (b) of an expression for entrainment, which does not take into account the asymmetrical and deep penetration of the ambient cross-flow into the plume. Their expression of entrainment has very little in commen with those given in the literature in plumes and jets. A similar expression was also suggested by Schatzmann, requiring five tuning factors. This indicates the inadequacy of such an approach. In addition to k-E models there are, both theoretical and experimental results, for the cases considered in the paper. I wonder whether the authors are aware of them.

I stongly object the use of name Eulerian and Lagrangian models, they make no sense, and are misleading.

REBUTTAL OF REVIEWER A COMMENTS

For the sake of brevity, not all aspects of the review history of the Lagrangian model is presented herein. However, the responses to review comments were always extensive. Whether or not they were effective, at least, we took the reviewers comments seriously.

March 6, 1992

Dr. James Liggett
Journal of Hydraulic Engineering
Cornell University
273 Hollister Hall
Ithaca, NY 14853-3501

Dear Dr. Liggett:

Your letter declining our paper, "Elements and Control Volumes in Plume Modeling," has been received. In light of the referees' negative comments, thank you for inviting us to revise and resubmit the paper, it supports our convictions. Also, we would welcome your reading the revision as you indicated you might.

The enclosed revision is substantially different from the initial manuscript. We address the topics that were criticized by the referees and provide additional information that should prevent other readers from raising similar objections. We also enclose point-by-point rebuttals to their comments for your and their review.

We believe that the paper should not be declined on the basis of vague objections or a claim that our analysis is doubtful. The reason is that we identify a very specific error in earlier Lagrangian models, i.e. the omission of a treatment of overlap, and show that Eulerian models give similarly erroneous predictions. We also identify other important properties which need to be considered by the plume modelers. These findings cannot be conveniently dismissed without proof that they are for some reason erroneous. This the referees have failed to do. Therefore, it will be a disservice not only to us but to the ASCE plume modeling community as well if they do not consider their opinions more thoughtfully.

Your acceptance will be an important step in the work receiving the open scrutiny it deserves. We are prepared to defend the work and are confident that no substantial errors will be found.

Sincerely,

Signed Walter E. Frick

A Rebuttal of Referee A

1) Our work is a "critical discussion" of Eulerian and Lagrangian models.

This is not a criticism of our work and we agree with it. To say essentially, as we do, that earlier Eulerian and Lagrangian plume models do not correctly develop the mathematics of the plume element and the corresponding control volume can be interpreted to be very critical of established practices. But, we prove our assertion directly by identifying the problem and offering a correction for it (Equation 25). Then we show how it affects the predictions of the Lagrangian formulation. Finally, by equivalence we show indirectly that the Eulerian formulation intrinsically contains the same error. The methodology follows accepted scientific practice, i.e. we show consistency with earlier theory before establishing new principles.

Our analysis provides opportunities to those in both paradigms to propose work which will correct earlier models. Also, by pointing out a fundamental error in modeling practice (control volume construction) we potentially make it possible for all modelers to extend their theories to regions, viz. the region beyond the trapping level, which present conventional wisdom, as represented by Referee B, holds to be out of range of the established approaches.

Thus, we help make both the Eulerian integral flux and Lagrangian plume models more competitive with other approaches. We think that members of ASCE will appreciate this and believe it is the best place for us to publish because it is primarily the civil engineering community that depends on our models.

2) Our opinion of Eulerian and Lagrangian models is unfavorable.

We disagree, unless Referee A means that earlier models can benefit from improvement. Clearly, we have proven our commitment to the Lagrangian model by consistently using it and promoting it for well over 10 years (e.g. Teeter A.M. and D.J. Baumgartner, 1979; Winiarski L.D. and W.E. Frick, 1976). By showing the basic equivalence between the two models in our paper, we obviously also promote the Eulerian model. Our record at EPA, and now at NOAA, proves that we are vigorous advocates of both approaches.

3) Eulerian and Lagrangian models perform unfavorably in calculating the spread of vertical plumes in a cross-flow, or when the ambient is calm.

This is not generally true as our changes show, e.g. in the revised introduction which presents predictions of a number of experimental cases. It should be clear especially from Figure 1 that, even without the corrections which we develop, the basic plume models (both Eulerian and Lagrangian using the Projected Area Entrainment (PAE) hypothesis) predict quite well, in some cases even beyond the trapping level. This should not surprise anyone considering that experimental work shows that frequently the average dilution achieved is simply the ratio of flow through the plume cross-section divided by the total effluent flow (e.g. Equations 13a and b, Roberts, Snyder, and Baumgartner, 1989[1]), effectively the cornerstone of our theory.

---

[1]. Starting with Equations 13a and 13b it can be shown that

$$S_m = \frac{1.08}{1.86}\frac{L z_e u}{Q}$$

where S_m is the minimum dilution in the plume, L is the length of the line plume perpendicular to the current, z_e is the height between the source and the maximum rise of the plume, u is the ambient current, and Q is the total flow rate or the line source. The average dilution is approximately twice the minimum dilution. This is precisely a statement of the Projected Area Entrainment (PAE) hypothesis in integrated form.

Using the Lagrangian approach we were the first to recognize the descriptions of forced entrainment based on PAE were deficient in other models (e.g. Hoult, Fay, and Forney, 1969). Our formulation of PAE is the first one to be consistent with the integrated projected area of plumes (Frick, 1981). By accurately depicting the PAE hypothesis we simultaneously conform to experimental findings (Roberts, Snyder, and Baumgartner, 1989) and achieve a simplicity (as evidenced by our waiving additional coefficients) that eludes other modelers.

We emphasize that these predictions are made using a single constant coefficient -- none in PAE and one in the Taylor entrainment hypothesis. We are confident that the engineering community will recognize the value of having models that offer simultaneously high performance and simplicity. Achieving this goal gives users confidence that the model will work outside the limited range of experimental conditions. This is important to engineers, many of whom recognize the folly of overtuning models to data.

The counterflow evident in Figure 1b of the changed text is not predicted per se, but it is not the purpose of the paper to do so. However, our theory does shed light on the cause of this behavior and may ultimately help in its prediction. We address this issue further in Frick et al. (1990).

4) No alternatives are presented except for Equation 19 and an expression for entrainment.

This is incorrect. Equation 25 presents an alternative, corrected formulation for describing the geometry of plume elements and control volumes. This equation is important in plume modeling and potentially in other integrated flux problems as well (for example, channel flow problems). We analyze the need for the equation, define it based on fundamental principles, and demonstrate its importance.

5) Equation 19 is based on solid mechanics which is questionable.

It might be said that the Lagrangian model is based on solid mechanics since, in predicting the trajectory of the plume element, we strictly predict only the motion of the center-of-mass of the plume element. But this criticism applies equally to the Eulerian models since we establish its equivalence with the Lagrangian model. **In other words, the Eulerian integral flux models are intrinsically solid body models.**

If we were to abhor solid body physics in fluid dynamics under all circumstances, both of these models must clearly be discarded. This clearly contradicts accepted engineering practice which embraces the integrated flux approach. Furthermore, if we were to condemn two approaches which obviously give useful results, as we demonstrate, shouldn't we be equally stern with dimensional analysis? It seems to us that, while A's criticism might be appropriate in a journal of theoretical mathematical physics, it hardly applies to engineers who must often sacrifice rigor, and purity, to make progress on problems which can otherwise not be analyzed effectively or at all.

6. The entrainment function does not take into account asymmetrical and deep penetration of the ambient into the plume.

This is incorrect.

Even though additional work remains to be done by both Lagrangian and Eulerian integral flux modelers, we do take the first steps in considering the **asymmetry** of the problem in establishing the overlap problem (Equation 25) and correctly depicting the distribution of mass about the center-of-mass. We go further in identifying the effect of **asymmetric** entrainment on the motion of the center-of-mass (further developed by us in: Frick, W.E., C.G. Fox, and D.J. Baumgartner, 1991.)

It is true that our model does not deal with the details of the redistribution of tracers within the plume element once the mass is entrained. However, mass is more correctly addressed than in earlier integral flux models, as we have demonstrated by proving the equivalence of the earlier models.

7. The [PAE] entrainment function has little in common with those given in the literature [on] plumes and jets.

This is incorrect to the extent that most forced entrainment functions include the cylindrical entrainment component of PAE as proven by Frick (1984). Schatzmann (1979) also includes the growth term and confirms Winiarski and Frick's (1978) identification of the curvature term. However, he assumes, erroneously and without proof (Frick, 1984), that the latter is of second order significance. Furthermore, Frick (1980) shows that integrating all three components of the projected area results in an area consistent with Equations 13a and b of Roberts, Snyder, and Baumgartner (1989a), proving that the cylindrical component, common to many other models, must be augmented by introducing a tuned coefficient, if the other two terms are ignored.

We agree however that the similarities end there. Unlike Schatzmann we introduce no additional coefficients in moving from the stagnant to the flowing. We are pleased that A notes that, in comparison, Schatzmann depends on five tuning factors in his model. In contrast, our success is guaranteed by simply recognizing basic geometric realities that determine the correct formulation of the Lagrangian plume element and the Eulerian integral flux control volumes.

8. Are the authors aware of the k-epsilon approach?

Yes we are. It is generally not used in the class of plume models under consideration here. We wonder whether k-epsilon theory can predict a single clearly demonstrable fact about plume behavior. For example, could it be used to independently derive the PAE, which expresses the fact that overall forced entrainment into plumes is simply the flow through the total projected area of the plume as demonstrated by Roberts, Snyder, and Baumgartner (1989a) and others? Our model is consistent with that empirical fact.

9. I [,Reviewer A,] strongly object [to] the use of name Eulerian and Lagrangian models, they make no sense and are misleading.

We disagree. We consider our model to be a Lagrangian model, expressing time as the independent variable, and following a modified material element. In other words, as a gross simplification we perceive an element which is bounded by a network of molecules which, although it stretches with time, remains intact. We know these are gross assumptions but, again, they give the same results as other published plume models given the same assumptions. So the other models are subject to the same implicit assumptions. The usage is largely consistent with Batchelor (1970).

The Eulerian models have other names and we mention some of them: integral flux, Gaussian, etc.. Whatever the name, the control volume can be thought to be fixed in space, that is, we consider a succession of stationary control volumes over which the fluxes are computed. This is characteristic of the Eulerian paradigm and hence the name.

With this interpretation the usage is neither non-sensical nor misleading.

But the name applied is not the issue. We could describe our model without mentioning "Lagrangian" and we could critique and compare other models using the names of the authors. This might make it appear we were critical of their work personally, which is not our point. Future readers would be less likely to see the generality of our argument and think we were criticizing specific works. Finally, the reviewer did not offer any preferred terminology for us to consider.

10. The paper should not be published [because] lack of analysis make the conclusions doubtful.

This is a fallacy. It could be said much more accurately that a lack of analysis in earlier models make their validity doubtful and that we set out to help put them on a sounder footing.

Our analysis proves that the Lagrangian and Eulerian models are, given identical assumptions, identical. Thus, even before we really begin to forge new paths, the level of analysis is at least comparable with other published approaches. If the Lagrangian approach had been developed first, we hope the Eulerian modelers would do the same. We then go on to prove that earlier concepts of plume elements and control volumes are erroneous under certain conditions. Therefore, assuming we have made no substantive errors (and none have been identified), the level of analysis is more advanced than that of previous published attempts.

To say that our results are doubtful is different than saying they are wrong. If we were wrong Referee A would say exactly how we erred and we would be obliged to correct the error or withdraw the paper. We present all the important equations needed to describe the Lagrangian model and the basics of Weil's model (1974) so that this can be accomplished. Still, the fact that Referee A cannot identify a substantial error does not prove that none exist.

Furthermore, we admit that engineers frequently deal with problems which are potentially dangerous and doubt is an important and legitimate component of their judgement, effectively a part of a factor of safety. It is therefore a justifiable criterion for blocking a work from publication. However, doubt can only be invoked to prevent publication if the claims made by the submitters serves to reduce the factor of safety, i.e. makes engineering practice less conservative. In fact, we are saying that existing practice is less conservative than it should be (it yields higher dilutions than are justified). Consequently, doubt should work in favor of having the paper published so that the community at large has the opportunity to assess our claims.

Referee A could have said that he was unconvinced by the scope of verification and insisted on more justification to overcome doubt on his part. We would be happy to respond and, at on our own initiative, have done so in the revision. But the doubt expressed is vague. The best we can do is to treat it as a philosophical issue. As such, we would like to put it in a context that every civil engineer can appreciate.

Suppose our paper had to do with bridge construction and had been submitted for publication just as work on the Tacoma Narrows Bridge was getting underway. Suppose that the paper used a new analytical approach to show that the bridge would be inherently unstable in high winds. Suppose further, that the new analysis was completely described and shown to be equivalent to earlier analyses of proven bridge design. Would a prudent engineer, unable to identify a single specific instance in which the analysis was demonstrably deficient, withhold this paper from the community for no other reason than an unsubstantiated notion of doubt? As another philosophical point of consideration, what would be the reaction of the community to this action after the failure occured? While the present case is clearly not as dramatic, we do not see how it is philosophically different.

Our general approach has been established for over 15 years. In that time no one, despite considerable agitation and opposition on the part of some, has identified a single fundamental error in our analysis that can be quantified. On the contrary, several members of ASCE have recognized the inherent simplicity and value of our analysis (e.g. Lee and Cheung, 1990). This despite the fact that an error, overlap, existed for anyone to identify.

We have no illusions about the fact that our ideas will eventually be superseded. But whose will not? In the meantime, we think plume modeling stands to gain by initiating a discourse about our work through publication.

11) Marginal comments.

page 2.

♦ [...strong bending and] small [radii]? Response: We definitely mean large radii, otherwise the plume sections do not overlap.

♦ [Error goes unnoticed] by whom? Response: If this problem were recognized in the Eulerian paradigm, the b^2 in equations Equations 1-10 and 18 would be replaced by a more complicated expression where applicable: in regions of overlap.

♦ In plume spread there is no internal diffusion. Response: Upstream lateral spreading and increased internal dilution are intended as two separate concepts.

page 4.

♦ Crititicism of Figure 1. Response: Figure 1 material is borrowed from Weil as indicated. Obviously, neither Weil or anyone else has previously considered the effect of overlap. We do not agree that an equation of a form similar to Equation 3 is inapplicable in the "overshoot" region (we assume to be the region beyond the trapping level). Certainly Weil solved the equations up to and beyond the point of maximum rise (Weil, 1974).

♦ "and other sources" has been removed. We meant Spiegel and Veronis (1960) and others.

page 5.
♦ "Is dimensionless." Response: Comment incorporated.

page 6.
♦ An explanation is required for Equations 15 to 17. Response: This part has been revised.
♦ [The round plume assumption can only be used] when the ambient is very low or stagnant. Response: We disagree that the comment should be incorporated. The round plume assumption is used even in moderate and high currents.

page 7.
♦ [Should] width [be added to Figure 2]? Response: The letter b is used to represent the width, or the radius. The figure (now Figure 4) has been totally revised.
♦ Three [different stages of development in Figure 2]. Response: Comment incorporated.
♦ [Is Equation 19] solid mech[anics]? Response: We have elaborated on this issue extensively in the revision, especially to the introduction.

page 8.
♦ [With regard to Equation 19] what about large velocity fluctuation causing deep penetration? See also Schatzmann. Response: We are not sure about the Schatzmann reference. Once again, we point to the equivalence to the Eulerian models to make the point that this equation is inherent in that paradigm. We can also point to the extensive track record that the Lagrangian model enjoys thanks to extensive use in regulatory work and other verification studies.
♦ [Top-hat models are] not applicable in a cross-flow. Response: Again, we point out that Eulerian models, with which the Lagrangian model can be made to be equivalent, are used in such problems.

page 9.
♦ Gaussian profiles do not occur in plumes with a cross-flow, unless it is very low. Response: Since we are aware of Fan's work (1967), as well as other studies which show the bifurcated cross-sections of plumes in crossflow, we are aware of this problem. We are, of course, not at all dependent on the Gaussian assumption, but simply point out that their dynamics are effectively top-hat (averaged) also. The referee's criticism is routinely ignored, e.g. by Schatzmann (1979).
♦ Fig 4 does not show a rapid rise? Is there a stratification? Response: Yes there is. We hope this is clearer in the revision.

page 10.
♦ I.e. Eqs 19 & 24 are not applicable. Response: Equation 19 is still used. We agree, Equation 24 is inapplicable to overlapped cross-sections and must be corrected, as we do (see Equation 25).

page 11.
♦ [Is radius in Equation 24 also a function of] curvature? Response: No.

REFERENCES [Omitted for the sake of brevity. See the bibliography.]

INITIAL REVIEW: REVIEWER B

The authors address the problem of numerically simulating the dispersion of buoyant jets in a cross-flow. They state that most of the problems with integral models occuring in zones with large trajectory curvature are caused by an insufficient mathematical formulation of the models. They propose a correction of control volume shape, in order to avoid overlapping of plume elements in those zones, and introduce such a corrected model.

The referee is in complete disagreement with the authors. He does not even share their physical notion of the problem.

The mathematical formulation the authors present is that of a buoyant jet. In a stably stratified environment such a jet reaches a terminal height of rise. If the cross-flow velocity is zero or weak, the plume tends to overshoot the equilibrium height and flows downwards (see Fig. 7). Trajectory curvature is large, a sort of anvil formation can be observed. If that happens, the flow can no longer be regarded as a jet, dispersion is no longer dominated by internal jet turbulence. The cloud drifts with the ambient flow and its further dispersion is governed by ambient turbulence (if there is any). Another type of model (advection-diffusion) has to be applied. The only sound solution to the problem is that one of the authors did in a previous publication (Teeter and Baumgartner, 1979): to limit the valid range of the plume model.

REBUTTAL OF REVIEWER B COMMENTS

A Rebuttal of Referee B

In the first paragraph of comments, B simply summarizes the general content of our work. It needs no response from us. The same holds true for the first three sentences of the third paragraph in which B reviews common knowledge with which we are in agreement. The remaining comments, which are critical, are addressed in detail below:

1. [If] [t]rajectory curvature is large, a sort of anvil formation can be observed.

This is not generally true. Frick et al. [1990] demonstrate that anvil formation does not occur until overlap (which is a mathematical consequence of the incorrect description of the plume element and control volume in the Lagrangian and Eulerian formulations) is relatively pronounced. Hence many cases without anvils exist that are subject to overlap in earlier approaches. The latter are appropriately addressed by our theory and are the main topic of our work. Earlier Eulerian and equivalent Lagrangian models systematically overestimate radius and dilution because they neglect overlap. We think B will agree that we should not propagate an identified conceptual and mathematical error in plume models.

While the correction has immediate practical importance, its greatest value, depending on how common the error is, may be that it identifies a serious conceptual error in the construction of the control volumes in general.

2. If that happens, the flow can no longer be regarded as a jet, dispersion is no longer dominated by internal jet turbulence.

Our revision makes it clear that we are primarily interested in cases of incipient anvil formation (see Frick et al., 1990, for additional details).

Turbulence is a funny concept: everyone accepts it and pays lip service to it but none really understand how it works in plumes or know how to parameterize it. The obligatory reference to turbulence offers the appearance of rigor without commitment.

We don't pass the buck. We note with considerable satisfaction that overall plume dilution from forced entrainment has been found more than once to be proportional to the product of the projected area of the plume and the ambient current velocity (Roberts, Snyder, and Baumgartner[2], 1989; Rawn, Bowerman, and Brooks, 1960). We are the first to correctly parameterize this experimental reality. Interestingly enough, the ambient current has no direct connection to the turbulence in the plume.

3. The cloud drifts with the ambient flow and its further dispersion is governed by ambient turbulence (if there is any).

This is incorrect. First, we have already stated that our main topic deals with plumes without anvils. The mathematical occurence of overlap is not associated with anvil formation until it is well advanced, and until it is, normal plume behavior is observed. Second, Roberts, Snyder, and Baumgartner (1989) make clear that turbulence in plumes does not collapse until after maximum rise is reached (Equation 11, Roberts, Snyder, and Baumgartner, 1989). Our work applies to this region.

4. Another type of model (advection-diffusion) has to be applied [in the region of overlap].

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[2]. Starting with Equations 13a and 13b it can be shown that

$$S_m = \frac{1.08}{1.86}\frac{Lz_e u}{Q}$$

where S_m is the minimum dilution in the plume, L is the length of the line plume perpendicular to the current, z_e is the height between the source and the maximum rise of the plume, u is the ambient current, and Q is the total flow rate or the line source. The average dilution is approximately twice the minimum dilution. This is precisely a statement of the Projected Area Entrainment (PAE) hypothesis in integrated form.

This is incorrect for the same reasons discussed in 3. But even in their absence we disagree. Our submission is to an engineering journal in which such phenomonological techniques as dimensional analysis are used to study entrainment. The criterion used to justify them is that they work. Our theory is based on sound, if basic, principles of physics. We meet a rigorous standard. To a similar level of agreement we show that our theory works. Therefore, at this level of reality, our model is appropriate. It is not reasonable to hold us to a more stringent standard of engineering "purity", despite the fact that we do.

5. The only sound solution to the problem is that one of the authors did in a previous publication (Teeter and Baumgartner, 1979); to limit the valid range of the plume model.

This is false.

We cannot prove it but every scientist and engineer surely believes that solutions exist to all physical problems. This problem is certainly not any different in this regard and therefore a solution other than no solution exists. Certainly empirical evidence shows there is a solution. Therefore, there must be a "sound solution" and the criticism is false.

If everyone is forced to wait until the perfect theory exists little will ever be published. This is an unreasonable expectation for us, or anyone, to meet. We have already provided support for our argument that our theory is valid at least for a subset of conditions involving overlap for which anvil formation is neither used nor necessary in proving our conclusions. Therefore, we have something worthwhile to offer.

6. The referee is in complete disagreement with the authors.

Assuming, as we do, that Referee B is not simply nurturing a bias, it is incorrect for B to disagree with us (see our rebuttal to 7, below).

Moreover, we think that our changes will help produce better understanding and tolerance of our approach while bringing B into agreement with our approach. For example, our comparisons to data given in Figures 1 and 2 show that the basic Lagrangian model is competitive with any other Eulerian integral flux plume model (e.g. Schatzmann, 1979). In other words, unbiased observers will find our predictions to be of higher quality than those of most, if not all, other plume models. If these unbiased observers are then instructed to weigh more heavily predictions, otherwise equal, obtained by a model using the fewest coefficients, there is no doubt our model would be chosen to be the best.

Our own plume modeling comparison study (Baumgartner, Muellenhoff, and Frick, 1989) shows that the basic Lagrangian model using the PAE agrees more closely to the other four EPA plume models than any of the others do to each other. Independent surveys cited in the revised paper show that the basic Lagrangian model comes in first or second among more than a dozen plume models. These successes and the above responses to B's criticisms should make agreement possible.

7. He [B] does not even share their physical notion of the problem.

Again, assuming, as we do, that Referee B is not simply nurturing a bias, it is incorrect for B to disagree with us. If B's criterion for not sharing our physical notion does not simply reflect a determined, but unscientific, refusal to consider an equivalent approach and B accepts integral flux models, then equivalency makes it impossible to claim that he does not share our physical notion of the problem.

We appreciate B's sentiment because it gives us the opportunity to elaborate on a visceral issue whose existence would be denied by our critics but probably has more to do with the almost hysterical and unintelligible opposition our approach elicits than any substantive objection (yet to be elaborated).

The Eulerian and Lagrangian approaches are like two sides of a body, which match identically where they join (physical reality itself). For B to say he does not even share our physical notion of the problem is like someone saying he does not recognize one side of his body. The equivalence of the approaches is not just a philosophical statement we learned in school, we prove the equivalence in the paper. We prove it by showing that the two approaches give the same results. Thus, B cannot logically deny sharing our notions unless the Eulerian approach is rejected as well, in which case he should not be making the rules because he is not playing the game.

Frequently, all civil engineers know is that our model works well, they do not even think about the fact that they use a Lagrangian approach. But modelers, perhaps thinking that they are cleverer, recognize that the model is based on solid body dynamics and sometimes hastily condemn it because it is not a "fluid dynamical" model. The irony is, of course, that the Eulerian integral flux models they value are intrinsically solid body models as well! We prove that through equivalence and it is true by virtue of the fact that the Eulerian integral flux models, in a dynamical sense, average the properties across the control volume. (Even Gaussian models do; they also only predict the motion of the center-of-mass[3].) It is exactly to overcome this impediment to fair consideration that we take such pains to prove equivalence.

Once this fact is understood two options remain: reject both models or, recognizing that the models have been useful in describing plume behavior, accept the Lagrangian model as a full partner and explore this "solid body" model further to determine its potential fully. There is not much "fluid dynamics" in dimensional analysis either but we do it because it is useful. As Joseph Lee said in defending the Lagrangian approach before critics at the Keulegan Conference at MIT, to paraphrase, "The Lagrangian approach [and the Projected Area Entrainment Hypothesis] answers many questions about plumes and we should give it a chance [to prove itself]," (Lee, 1990). This would be an enlightened path for all of us to take. We think many are ready to take it.

8. An additional ramification.

Referee B might contemplate the ethical ramifications of the state of affairs that exists with respect to our work: Is he aware that several works published in ASCE journals are based on our work, most notably that of Lee and Cheung (1990)?

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[3] Not necessarily the same as the center-of-mass of the full plume.

We think it is appropriate that work germane to other work published by ASCE should also be published.

REFERENCES [Omitted for the sake of brevity. See the bibliography.]

RE-REVIEW: REVIEWER AA

This review is the only one of five which acknowledges and addresses the negative volume anomaly. Clearly, the reviewer takes pains to write a good and exhaustive review, however, the talking past each other of which Kuhn (1970) speaks is evident. AA writes:

Review of "Elements and Control Volumes in Plume Modeling"

This has been a difficult and most extraordinary review assignment for me, and I have reviewed well over 100 papers over the years, mostly in the field of atmospheric dispersion. First, it is unusual to be called in as an additional reviewer to try to resolve strong disagreements between authors and the original reviewers. Second, I have never encountered such a disparate exchange among individuals in an "objective" technological field. It is like the blind men arguing with each other over the nature of the beast, an elephant, as they each tactually explore only a small part of it. The beast here is plume modeling, and there is more to it than there first seems.

The reviewer "A" general comments seem to me to be totally off-the-wall. It is as if he or see only skimmed over the paper, missing its central point and attacking it on grounds that have nothing to do with the paper or that misrepresents its point of view (which is poorly expressed, but it is there if you read the paper in its entirety). With not a single constructive comment, this is just a shoddy review; I feel sorry for the authors for having to bother to rebut it.

On the other hand, reviewer "B" did read the paper, as is evidenced by his or her elegant and succinct opening statement of what the paper is about (the authors could benefit by emulating this writing style). The more I re-read the paper, the more I feel in basic agreement with reviewer "B", although I have much more to say about what bothers me about this paper.

Like reviewer "B", I too do not share much of the authors' physical notion of the problem. Unfortunately, I do not know how to express "why" in 25 words or less. (This reviewer has a reputation, and even an award, for detailed, long, critical, and constructive reviews; I beg the patience of both authors and editor.) Since I have found literally dozens of statements in the paper to disagree with, it is hard to know where to begin. Let us start with the central issue.

The central issue of the paper, which it takes the authors 16 pages single spaced at 14 characters an inch to begin discussing, concerns the question of how to model plume properties in regions of sharp curvature in plume trajectory - specifically, where the plume radius of curvature is less than the radius of the plume itself. (The paper does not put it this way. In fact, it hardly puts it any way at all. It often fails to define its terminology, to say succinctly what it is discussing and why, and to make its points clearly.) In such a case, the authors believe that it is a "conceptual and mathematical error" to include, in a plume integration assuming a round plume, the segment on chord defined by a line on the plane(s) of integration centered on the local center of trajectory curvature. They label the volume defined by these segments as "negative volume" and appear to assume, with really no argumentation to support this course of action, that the best way to treat this volume is to ignore it, i.e., omit it from the area or volume of integration in integral plume models.

The authors assert these propositions for two classes of models, which they apparently conceive as the only two classes of integral (bulk properties) plume models and on which they spend considerable space and words to "prove" their equivalence. (Given the same closure and round, symmetric plume assumptions, I see no reason not to expect their equivalence; however, the paper's demonstration of equivalence is a single computational example with totally unspecified boundary conditions; the "Eulerian" model class uses differential equations for integrals of plume properties over planes intersecting the plume perpendicular to its trajectory axis and solves them by integrating over increments of displacement on the trajectory axis, ds. The so-called "Lagrangian" model class uses time derivatives for integrals of plume properties over volumes of infinitesimal thickness defined by planes intersecting the plume perpendicular to its trajectory axis, solving these equations by integrating over time of travel. However, they would be truly Lagrangian (motion-following) only if all the fluid in the volume traveled at the same velocity, the mean velocity; since ambient fluid is constantly entrained into the plume and velocities vary across its cross-section, obviously this condition is not met. I would have to label such models as "quasi-Lagrangian" or "bulk-Lagrangian"; true Lagrangian models follow parcels of fluid material which distort due to velocity shear and which do not lose or gain material.

I disagree with the authors' suggestion (more a demand, really) that what they call "negative volume" should be left out of integral models. Since the Eulerian and quasi-Lagrangian models are practically equivalent, let me use the Eulerian as an example, because I find it conceptually less problematic. Consider the spacial derivatives of properties between two planes of integration, both perpendicular to the trajectory axis, spaced ds apart at the axis. Unless the local trajectory is straight, these planes are not parallel as illustrated in Fig. 3 of the present paper or in Fig. 1 of Schatzmann (1979). They are non-parallel, as in Fig. 1 of Davidson's (1986) control volume discussion of Schatzmann's model (J. Climate and Applied Meteorology 25, 858-867) or as in Figs. 4 and 7b of the present paper. If the plume bends very sharply and the local radius of trajectory is less than the plume radius, then the planes of integration intersect within the plume, as pictured in Fig.7b. Then the local displacement between the planes, Δs, say, indeed reverses sign in the plume segment on the far side of this intersection from the plume centerline. The authors label this segment "negative volume" and think somehow that the mathematically correct thing to do is to ignore it, i.e., leave it out of the plume integration.

However, all of the models under discussion assume for computational purposes that plume properties are symmetric with respect to the plume centerline. When we write a derivative in ds of bulk plume properties integrated over these planes, we do not have to artificially assume Δs = ds everywhere (i.e., assume parallel planes). The larger Δs on one side of the centerline is counterbalanced by the smaller Δs at an equal distance on the other side; even if $\Delta s < 0$ on one side and $> 2ds$ on the other side, their average will always be = ds; since symmetry has been assumed, the spacial gradients of plume properties are symmetric about the axis and the resultant sums (integral of gradients over two spacial dimensions on planes intersecting the plume) are the same as if $\Delta s = ds$ everywhere. When $\Delta s < 0$ regions are left out of the integration, this balance is destroyed, just like one person falling off the seesaw. Thus, what the authors see as a correction, leaving out "negative volume", I see as an incorrection.

Furthermore, in regions of very sharp plume curvature, this is all pretty much moot anyway. Any model using a symmetry assumption is wrong or at least doubtful. Such regions will always involve considerable asymmetry. In the case of plumes reaching the "trapping level" (termed "equilibrium": height in atmospheric literature), the ambient density stratification is often much stronger on one side of the plume than on the other. Even if the ambient gradient is constant, the plume concentration and velocity excesses are skewed as the trapping level is approached, because the least diluted portions of plume material penetrate farther than the most diluted portions. In addition, with the vertical component of turbulence being squashed in this stage, the validity of any rising-stage entrainment assumption is deteriorating.

In their rebuttal to Reviewer B, the authors say, "Our revision makes it clear that we are primarily interested in cases of incipient anvil formation." I did not find this clear at all, after reading the paper twice. Then they say "our main topic deals with plumes without anvils"- so who knows what they are really primarily interested in. With major chunks of the paper devoted to a passioned defense of the "PAE" model and to demonstrating the equivalence of their Eulerian and a quasi-Lagrangian models, it is hard to discern their primary interest. Be that as it may, I think reviewer B would agree with me that, in the case of incipient anvil formation, no "plume" model presently formulated gives a realistic picture of the concentration or velocity fields. Turbulence is being squashed, while more and less diluted parts of the plume are tending towards different levels, while the hydrostatic and dynamic pressure fields in three dimensions are important in the anvil and horizontally spreading/vertically collapsing portions of the plume, while wave drag is removing a large part of the vertical momentum, and nothing is nearly symmetric except possibly about a vertical plane through the plume axis (if there is no current/wind direction shear). The best one can hope for from a plume model in this region is a good estimate of the vertical extent of the trapping layer. (What more do you need?)

Because of the above-mentioned phenomena not accounted for, the authors' ambition to fix plume models with a center-of-mass correction appears to me to be wishful thinking. Either much more complicated, three-dimensional numerical modeling will be required, or, more pragmatically, laboratory-based empirical corrections.

There are other modeling deficiencies not mentioned which are of more importance than the questions discussed here. One that was only hinted at in this paper is the difference between in-plume mass and effective mass, which includes the inertial effect of "added mass" outside the plume but coupled to it through pressure forces. The "added mass" term is discussed at some length in Davidson (1989) (Atmospheric Environment 23 341-349). Briggs (1975) (chapter in Lectures on Air Pollution and Environmental Impact Analyses, Amer. Meteorological Soc., Boston) derived integral plume equations from basic equations of fluid mechanics with considerably more rigor than Schatzmann and others. By assuming only that the ambient flow is initially non-rotational, he showed that drag forces such as assumed by Schatzmann cannot exist; this goes back to a 19th century axiom that potential flow is frictionless, and has been experimentally confirmed by Coelho and Hunt (1989) (J. Fluid Mech. 200, 95-120). By integrating the equations over an infinite plane while defining the plume by $|\Delta\rho|$ or concentration >0, Briggs' approach seems to avoid any "solid body" assumptions, which the present authors appear to think is implicit in all integral plume models. They also seem to think all such models assume round plumes, but the above model is completely general in this regard, absorbing arbitrary velocity and density perturbation distributions into "shape factors" which are assumed to vary slowly with position (because they are always of order unity). Briggs also showed that the added mass part of vertical momentum could be expressed as a line integral around the periphery of the plume of the velocity potential in the ambient. From the laboratory measurements of Richards (1963) on horizontal thermals, which showed that the flow outside the thermal boundary was indeed potential flow, he estimated the added mass part of vertical momentum as 1.3 times that of the in-plume fluid. This is a very significant factor in calibrating models to give both good trajectory and dilution predictions (Davidson, 1989).

I mention the above because the authors of the present paper seem unaware of a vast body of literature with plume models outside the two classes discussed in this paper. Some of these models even use a different set of conservation equations, with a closure different than entrainment. Briggs (1975), Table 1, gives a very succinct summary of several dozen plume models to that date.

Speaking of entrainment, large segments of this paper sidetrack into a bordering-on-emotional defense of the "PAE" entrainment model. At the top of page 7, I get the distinct impression that the authors are bitter about losing one or more grants and want to vent their feelings about it here. This seems quite inappropriate for technical journal article.

The authors champion the PAE assumption as one that requires none of those bad, nasty, low-life empirical constants to make the model fit the data. I disagree. The model is constructed so as to give quite respectable trajectory fits using constants equal to 1, which looks nice on paper and saves a minuscule bit of computational time. At the same time, the predicted radii are too fat because they include, in effect, "added mass". (This happens in any "one radius does all" model, as explained by Davidson). The PAE assumption (Eq. 6) has four entrainment terms, the first of which is a convention Taylor entrainment term (but should not the velocity scalar only be the velocity excess parallel to the plume axis?) For a quasi-horizontal plume, this term is the order of Θ times the largest term, where Θ is the plume inclination in radius; similarly, the fourth term, a curvature term, is of the order of Θ^2 times the largest. For small Θ we can neglect these terms and use ds $\approx$ dx, cos$\Theta \approx 1$, and plume velocity $\approx$ U to simplify and approximate Eq. 6 with

$$2\, db/dx \approx db/dx + (2/\pi)dz/dx$$

where b is the plume radius and z(x) describes the plume trajectory ($\sin\Theta \approx \tan\Theta = dz/dx$). The first term on the right resembles the one Schatzmann (1979) creates by (inappropriately) invoking the Leibniz rule, but is 1/2 as large (a good thing -

Schatzmann's entrainment equation is much more complicated, but because his version of this term nearly equals the left side, entrainment is altered radically and plume radius can blow up in situations where it should not - see Davidson (1986), Fig 3). I do not know why the above is 1/2 as large, but because it is, the final result is $db/dz \approx 2/\pi = 0.64$. This happens to be very close to best trajectory fit values of entrainment constants in other models the authors seem somewhat contemptuous of.

I can find many more statements in this manuscript to disagree with, but have already covered the larger issues. The most glaring faults are that so many words are used to say so little, there is repeated harping, sometimes bordering on ranting, on certain little themes (like PAE) not central to the alleged subject of the paper, the mathematical correction (or incorrection) proposal is promoted like some long overlooked key to accurate plume modeling, while larger issues are ignored. In summary, it is much ado about very little.

RE-REVIEW: REVIEWER BB

Comments on the paper entitled "Elements and Control Volumes in Plume Modeling," by Frick, W. E., Baumgartner, D. J. and Fox, C. G.

The authors show in this paper that a Lagrangian plume model is equivalent to the model formulated using the Eulerian method. The establishment of a relation between Eulerian and Lagrangian formulations is a useful exercise and would lead to better understanding of plume dynamics.

One of the problem encountered in a Lagrangian formulation, as pointed out by the author, is the overlap of the plume element with the neighbouring elements of the plume. The overlap is severe when the curvature of the path is large compared with the width of the plume, and the authors have proposed a procedure in the paper to made correction for the overlapping problem. Calculation were conducted for horizontal and vertical discharge into a horizontal crossflow. The results for the path and the width of the plumes obtained using the Lagrangian model were found to be the same as the those obtained by the Eulerian model. the correction for the plume overlapping has lead to a small improvement of the Lagrangian model, but the model still breakdown if the width is large compared with the longitudinal length scale of the plume.

The major difficulty of this paper, in my view, has been the attitude of the authors and the manner in which this attitude is presented in the paper. The paper has a rather long introduction and many unnecessary comments on the virtue of one model versus the other. Many discussions are non-specific and metaphysical. The involvement of solid-body mechanics in the description of the plume motion is entirely unnecessary. The paper in its present form is not acceptable for publication in the ... .

It is difficult for this reviewer to be specific about how the paper should be revised. I suggest that the paper be significantly shorten perhaps to the form of a technical note. The authors are asked to concentrated on the the result obtained by the modified Lagrangian model and on the relation between the Lagrangian and Eulerian models. Materials irrelvant to the model comparison, such as those presented in Figures 1 and 2, should be eliminated. Unsubstantiated charge and general statement should also be eliminated.

RE-REVIEW: REVIEWER CC

Comments

I have no difficulty with the correspondence between Eulerian and Lagrangian calculations - my problem is that each is only applicable in particular areas, e.g. if we are doing a Lagrangian calculation we not only want the elements of the plume to be non overlapping but also non-interfering. It seems to me therefore that the Lagrangian calculations should only be applied when each element has become similar to an advected two-dimensional thermal. The Lagrangian calculation should not be applicable in the Gaussian region where it seems to be obvious that the velocity-distribution implies the transfer of mass between the Lagrangian elements. This is not the case in the advected thermal region.

We cannot deny that the projected area entrainment hypothesis with the Lagrangian calculation gives reasonable results (Lee, 1991), incidentally I don't believe the POE hypothesis was described by Rawn, Bowerman and Brooks for the case similar to that of a rising plume - rather they applied a continuity equation to the full depth of the plume's rise). There are however problems with this method for a jet in a coflow and some problems for the case where the cross flow is small. For a large crossflow however the coefficient used in the bent-over region is a constant with a value of 1. (This value is related to the definition of the plume width.) However, this model does not describe the essential fluid mechanics of the flow. There must be a great deal of difference between the Gaussian region and the vortex region and this is disguised by this method of approach. I believe this is important if the model is to be general.

Finally, in the Gaussian region the paper does not mention the known experimental fact that the entrainment in the plume region is much greater than that in the jet like region (a universal constant is not appropriate).

The paper concentrates on the overlapping of the Lagrangian elements without taking into account the basic fluid mechanics and on this basis I feel that this paper should be rejected.

APPENDIX 4. AN APPEAL TO OVERTURN NEGATIVE PEER REVIEW

A LETTER OF APPEAL

October 6, 1993

James A. Liggett
Cornell University
273 Hollister Hall
Ithaca, NY 14853-3501

Dear Dr. Liggett:

Some time ago, you may recall, we submitted a previous version of the enclosed paper, "Improved prediction of bending plumes," for publication in the ASCE *Journal of Hydraulic Engineering*. In a review and re-review, five peers unanimously rejected the paper and you were moved to decline the submission. Subsequently, you agreed to an appeal of the decision, an offer we have been contemplating ever since. We feel that you and the associate editor are acting as honest brokers in this review, however, we have little assurance that an appeal will be any more open than the ASCE peers are to our ideas. Thus, in addition to trying herewith to obtain clarification from you on the procedural and other aspects of an appeal, we broadened our search for an appropriate journal. I will explain.

The ASCE review is the latest in a series of disappointing peer review performances that I have personally experienced in trying to have our work published in the *Journal of Hydraulic Engineering*. Both the work and the negative peer review experience are such a central part of my professional experience that I have become interested in similar instances and am working them into my dissertation. Of course, as soon as one is sensitized to an issue one tends to become aware of it everywhere. Indeed, once one begins to look one finds evidence of peer review abuse and other misconduct in the scientific literature.

Let me describe to you some of the factors and thoughts that go into our decision to communicate with other editors, along with yourself. In the process, let me briefly explain what I think is wrong with many peer review systems and why I think our work deserve to be published.

One of my professors, David Bella, teaches a course called "Contemporary Technology." The name is a bit misleading, though not inappropriate. Basically it is a philosophy course dealing with technological issues. His students read such books as Robert Pirsig's "Zen and the art of motorcycle maintenance," which, interestingly enough, was submitted to about 150 publishers before one was found to publish it. Dr. Bella talks about the stability of social enterprises, including peer review, and draws parallels to physical systems. For example, there are intrinsically safe and unsafe ways to build systems, ostensibly, to serve the same purpose. Social systems have analogous problems.

In 1980 I submitted a paper to the Hydraulics Division similar to my paper "Non-empirical closure of the plume equations," (Frick, 1984). It was rejected on the basis that it was superseded by Schatzmann's work (1979). Yet, later, properly citing the work, some of the same elements found in it were published by Lee and Cheung (1990) in "Generalized Lagrangian model for buoyant jets in current," in *Journal of Environmental Engineering*. Of course that is not the *Journal of Hydraulic Engineering* but nevertheless it thoroughly debunked the main criticisms levelled at the earlier work. If any doubt remained it was dispelled by Cheung (1991, particularly pages 63-64) who explicitly shows the superiority of the JETLAG model over Schatzmann's.

Furthermore, while the models that we have developed at EPA (Teeter and Baumgartner, 1979; Muellenhoff et al., 1985; Baumgartner et al., 1993) can be freely criticized by anyone and have it published by ASCE, for example, as did Alam et al. (1982), and while hundreds of members of ASCE and other engineers and practitioners use the models, the EPA and other principals who have developed these models are effectively barred from its forums. Consider the words of Ian Wood who delivered the Thirteenth Hunter Rouse Hydraulic Engineering Lecture (Wood, 1993):

> "Finally, a model in which the plume is treated as a set of noninterfering advected elements, and each element is followed, has been developed. This model uses the maximum of the normal entrainment and the *forced entrainment* introduced by Frick (1984). This latter assumption is that all the ambient fluid on the upwind side of the exposed portion of the fluid element is entrained (Lee and Cheung 1991). **Some of these models have become industry standards** and Roberts (1991) in his recent review discusses the regions of applicability of some of them." (My bolding)

Recognizing that the *Journal of Hydraulic Engineering* failed completely to publish one of the seminal contributions that prompted them, how can such conclusions be reconciled with the treatment that our work is accorded? And, having failed previously, what assurance is there that it has not failed again? In my opinion, there is a complete decoupling between reality and ASCE's responsibilities to its membership actively using the models. The ASCE peers have broken faith with its general membership.

As I say above, like my professor, Dr. Bella, I have become interested in the causes of misconduct in science, though one could argue that misconduct is not an issue here. Perhaps, we are just talking about the erosion of trust which ultimately hobbles science and robs it of its ability to serve the public.

However, in "Misconduct: views from the trenches" Gary Taubes (193) quotes Nelson Kiang who says about systems:

> "You can take a terrible system and if you put the right people in it, they'll somehow see something decent comes out of it, [but, better still is a system that] won't require an excessive sacrifice on the part of the members to do right."

That is really the issue, peer review is a system basically flawed. It works "properly" in some associations of individuals and "fails" in others. One of its flaws is the rule that authors can only submit to one journal at a time. Imagine what would have happened to Robert Pirsig's book if he had been obliged to follow that rule! Instead, what if authors could submit to multiple journals but where required to publish in the one that first accept the contribution? Also, in using re-reviews, as ASCE did, how do the original critics ever learn about their errors without access to our rebuttals? How are they made accountable for flippant reviews? Finally, editors could take a more active role in screening rejected papers to make some effort between differentiating the very bad, which peer review recognizes, from the very innovative, which peer review has a penchant for failing to recognize. Some of these issues are coming increasingly to the attention of scientists. For example, consider the agenda of the Second International Congress on Peer Review in Biomedical Publication held recently in which Drummond Rennie observes: "[E]ditors will have to get used to the idea that their own practices are a suitable subject for inquiry," (Rennie, 1993).

Again, I hope I do not sound personal, you are suffering from the system as we are . We doubt that there exists a journal whose editor would ignore five unanimous negative reviews to publish a work.

On the other hand, our work has been innovative, our track record proves that claim. Furthermore, the paper at issue defines a fundamental problem common to most plume models under the identified conditions. We unequivocally point out the anomalous "existence" of negative mass in plume models. Ironically, its consideration is appropriate in Wood's paper itself. There should have been no question that this paper should have been submitted to the membership at large for consideration and debate. Yet, four of the ASCE reviewers dealt with it superficially. Only one acknowledged the central point of the paper, but, concluded, paradoxically, that *negative mass* was all right with him as long as it averaged out.

As is stated above, we honestly believe that you and the associate editor have tried to treat us fairly and we have no complaints on that account. We are thankful for and will consider an invitation to test your appeal process. However, we ask for some assurance that your editorial staff is sincerely interested in giving our work genuine and substantive consideration. Its actions on our behalf are likely to be resented by the affected peers, though my experience is that an open audience reacts much differently. In any case, we hope that you will make your leadership aware of this issue. There are some loose cannons among your peer reviewers; in my opinion, they have revealed a lack of understanding and disregard for basic physics. But, that is ASCE's problem. Nevertheless, if this epistle ultimately results in procedural changes at ASCE, to make it more stable and less needy of benign and heroic human intervention, that would make our efforts, and yours, worthwhile.

In closing, to give you an idea what the climate is like in this rarefied atmosphere of rejected manuscript submission, I have written to five different journals so far, explaining briefly our experience at ASCE and enclosing the title page. My favorite example of the exchange, based on the prominence of its author, is enclosed. In all, four editors responded within about three weeks time. One was a very warm invitation to submit, the others where polite suggestions to submit elsewhere. I must add that the reasons given were editorial ones — generally that the topic was not a main interest for the journal. Such reason are appropriate and cheerfully understood. In my opinion, many of the reason given by your reviewers are neither competent nor appropriate, as I think an assessment by qualified referees would confirm.

Thank you again for your consideration.

Sincerely,

Signed Walter E. Frick

[Submitted with references, not given here. See the Bibliography.]

EDITOR'S RESPONSE

Journal of Hydraulic Engineering
Editor: James A. Liggett
Cornell University
273 Hollister Hall
Ithaca, NY 14853-3501

October 15, 1993

Mr. Walter E. Frick
U.S. Environmental Protection Agency
Hatfield Marine Science Center
Newport, OR 97365

Dear Mr. Frick:

Thank you for your letter of October 6. Your comments on peer review are very interesting. I agree that it is flawed and all of us have seen abuses. The primary problem is that no one has suggested a better system. In matters such as the publishing decision in the *Journal of Hydraulic Engineering*, a benevolent editor acting alone might function better for a time, but like all systems with benevolent dictators, it would eventually collapse.

ASCE is a bureaucratic organization with tight rules on how the journals are operated. Apparently, the Board of Directors has historically sought to maintain rigid control. The publishing decision has been by vote of the reviewers. Each paper was sent to two or three reviewers. If the vote was a tie, it was sent for a tie-breaker review. When I became editor, I made it clear that I had no interest in simply being a counter of the votes without any real say in what is published. My mode of operation has led to numerous clashes with ASCE Publications, but it has also gained more flexibility. The vote rule still stands, but I (and the associate editor) get a vote and I can count votes in a flexible manner, essentially eliminating some that I deem to be from reviewers that have not sufficiently studied the paper. As a result I have declined papers that the reviewers approved and have accepted papers when the reviewers have recommended rejection. Since I am the only one who sees all of the papers, that is the only way I know to maintain some semblance of uniform quality.

I read most of the papers. I cannot read carefully all of the papers; there are simply too many. The ones that I tend not to read, or at least not to read in detail, are those where I have confidence in the reviewers and the associate editor. I have continually tried to impress on the associate editors, with the directive to pass on to the reviewers, that part of our job is to help authors express their ideas in a form that is publishable. Indeed, I consider a rejection of a paper somewhat of a failure on our part to provide sufficient aid and advice to the author. Lack of time prevents us from pursuing that goal as vigorously as we would like.

The mistake that I particularly want to avoid is failing to publish innovative papers. I have observed that some subfields are "closed" to new ideas from those not already anointed. As an example I currently have assigned a paper to an associate editor to help put it in a condition that is publishable. The author has a new idea (There is a serious question of whether it is a "good" idea, but the profession can decide.), but after a couple of revisions he cannot put it in a form that can be accepted. The paper will eventually be published.

The primary problem in attempting a major correction of the system is simply time. ASCE uses volunteers entirely. Some other journals pay an editor but still depend on volunteer reviewers. All of our associate editors are volunteers. None of us gets reduced workloads or extra credit for these activities. Thus we must limit the amount of time that is spent on journal activities. I have argued at ASCE for a paid editor, but that is not going to come about anytime soon. Nevertheless, the system seems to work well but with some blemishes.

You cite as one of the flaws the rule against simultaneous submission. There is no practical way that we could operate without such a rule. First, we would be overwhelmed by papers that are submitted simultaneously. Not only would the paid ASCE staff have to be increased, but the number of editors would have to expand tremendously. We already have trouble finding good referees and such a system would greatly compound the problem. Soon referees would refuse to look at a paper or would find that reviewing takes over their work time. If only one-have of our authors made simultaneous submissions, we would be in serious trouble. The situation is very different than in the commercial publishing field where all, even the reviewers, are paid and where the primary criterion is not quality but financial viability.

In the matter of peer review, after reading thousands of reviews, I cannot identify any as deliberately false or malicious, although some have been wrong, thoughtless, incompetent, and self-serving. A few reviewers will accept almost anything and a few others would have declined Bernoulli's *Hydrodynamica*. I do, as your letter suggests, take an active role in screening rejected papers (and accepted papers). That role consists of reading the reviews carefully, usually reading the paper, evaluating the comments and the recommendation of the associate editor, and in case of doubt sending the paper to people in which I have confidence to make a fair, informed recommendation.

Since the referees are volunteers, the only way that they are made accountable is through their own sense of ethics and service and by a sense of preservation of their professional reputations. I have rarely seen anyone go far professionally who does not produce conscientious reviews. If the system is "basically flawed," if it is a "terrible system," if it is to be replaced, what system is better? I am reminded of Churchill's statement: "Democracy is the worst form of government, save all others."

The associate editors decide who should review a paper, but I sometimes make suggestions. I did so in the case of your paper. At least three of the reviewers are persons in whom I have a large measure of confidence and respect; the other two I know less about but have no reason to believe that they are not competent. Most of the reviews (with some exception) have generally been thoughtful and analytical. The paper is in a field that I know something about but cannot pretend to have the expertise of the three reviewers whom I know. I see nothing in the reviews to suggest that they are self-serving or that the reviewers are not open to new and innovative ideas.

I can get additional reviews, with or without additional referees. However, I am not willing to overrule the current reviewers and if I go for additional reviews, the result is likely to be the same. Indeed, I don't know how to do better since some of the reviews come from people that I (and you) respect most, people that I know to be fair and expert in this field. Thus when you say "...I think and assessment by qualified referees would confirm [that] the reason given by the current referees are neither competent nor appropriate," you have had the most competent referees in the field.

I regret that I cannot be more positive.

Sincerely,

Signed James A. Liggett

CRITIQUE

There is no question in my mind that Dr. Liggett is making every effort to be personally fair in his treatment of us. However, I am reminded once again of Erika Frick's (1993) statement on the legal perception of child abuse, relating how traditional static paradigms allow monstrous abuses simply because they are not recognizable within the paradigm. As I quite expected — and as Kuhn (1970) says about normal and revolutionary science — we are talking past each other.

Dr. Liggett chooses to ignore two important points, one of which is made in the letter — the negative volume anomaly itself. Negative volume is the key concept in the disputed work. Except for the one reviewer, it is totally ignored in the discussion. The concept of positive volume and its linkage to reality is common to every scientific paradigm, yet, try as we will, we cannot get the peers or the editors to recognize that this basic tenet of science is being violated in earlier Lagrangian and, by equivalence, Eulerian integral flux models. THEY DO NOT WANT TO ACKNOWLEDGE IT! That is the crux of the problem.

The other point Dr. Liggett chooses to ignore is the indication from one of his own reviewers, the only one that addressed the negative volume anomaly, that at least one of the initial reviewers had given a shoddy review.

Again, while we hardly believed that we would change the editor's mind given unanimously negative reviews, the information is there for a strong, well-rounded, and unbiased editor to reach the conclusion that the paper deals with a legitimate scientific topic which the peers are unable to review in an unbiased manner.

A REACTION SUBMITTED TO *SCIENCE*

The correspondence between Dr. Liggett and me could have continued and may eventually have evolved into a satisfying meeting of the minds. However, to continue seemed pointless. It seems that at best, we are talking past each other, at worst there may be no intention on his part to ever acknowledge the fundamental problem separating us — the NVA and JHE's determination to jam the issue.

Again, it seems that the best way to make progress is to get the problem out in the open. Consequently, I determined to continue the dialogue by trying to publish a letter in *Science* on our exchange. I wrote the following letter to the *Science* Letters Editor:

20 Oct 93

Ms. Christine Gilbert, Letters Editor
Science
1333 H Street, NW
Washington, DC 20005

Dear Ms. Gilbert:

I read with much interest J. D. Watson's playful "Succeeding in Science: Some Rules of Thumb," (Science, Vol. 261, 24 Sep 93). His first rule is that you have to avoid dumb people. His fifth and final one is that if you can't stand to be with your real peers, get out of science.

While somewhat tongue in cheek, his rules are right on target, although they are not very helpful. For example, if you can't stand your real peers and get out of science, how can you succeed? It could be: "If you wish to succeed at science, you may have to overcome, circumvent, outlive, persuade, or otherwise learn to deal with your peers." My personal response to this rule reaffirms me as a scientist.

But, more importantly, you cannot avoid dumb people, because it is inevitable that in peer review you will encounter them — frequently. If you really are good at your science then, sooner or later, you may think of something that no else knows or suspects. Furthermore, even after you explain your discovery it will not be readily understood or accepted. Under these circumstances everyone is "dumber" than you are, at least on this one point.

I think herein lies the paradox of peer review. Sufficiently removed from their experience and knowledge, how can two or three individuals judge the merit of an idea which someone smarter, at least with respect to it, has discovered or created? Frequently, they cannot. In cases such as this the proper decision should be to widely disseminate it, recognizing that some in the general audience are more likely to understand and appreciate it. They can begin placing it in new contexts that eventually make it understandable to the larger community, or, reveal its fallacy.

Here is a criterion for recognizing good peer review. Good reviewers expect to judge contributions from people who are "smarter." And, while they can detect some errors and unoriginal work, and even good new ideas in their realm of knowledge, they recognize a legitimate foreign idea. And, they clear the way for others to evaluate it — the general community.

Bad peer review is xenophobic.

To avoid bad peer review every reviewer should ask this question, "Is the work comfortably within my realm of understanding?" If it is then it is likely that the reviewer can recognize significant and original work and detect errors. These are likely to be the "unadventurous nibblings at the margin of truth rather than quantum leaps" (Lock, 1985). If it is not, the reviewer should try to determine whether a recommendation to the larger audience is appropriate or whether to abstain.

I have discussed peer review with colleagues and usually find it vigorously defended. One reason given is that the results are largely acceptable. They are bound to be since some of the most educated and sincerest, individuals, scientists and engineers, serve on review panels. Nelson Kiang states:

> "You can take a terrible system and if you put the right people in it, they'll somehow see something decent comes out of it, [but, better still is a system that] won't require an excessive sacrifice on the part of the members to do right." (Gary Taubes 1993)

Few suspect abuse, however, I sense their convictions are conditioned by habit and cherished ideals. As just one of its many conflicts, "[The system] often allows excellent manuscripts to be criticized by referees with vested interests or contrary views," (Campanario, 1993).

In a current debate with an editor of an engineering journal, I criticized their peer review procedures. He countered with Churchill's famous statement: "Democracy is the worst form of government, save all the others." I agree, let's keep peer review. However, the point is there are many forms of peer review, even as there are many democracies. The latter are evolving, and, from the inscription in the Jefferson Memorial, should. To insist that our democracy is the best will surely result in it becoming the worst. I say we apply the same kind of innovation we do to science to the institutions that govern it.

Like some of us, Watson has lost his innocence about science and peer review. He is effectively saying that the system is often hostile to the most creative individuals and must be manipulated or circumvented by rules of survival. To twist Kiang's maxim just a little, the system offers sufficient temptation to invite abuse and theft. The problem is not new, as anyone who has read Koestler's (1963) (beautiful) account knows.

We sometimes forget that science is an empirical art. That means that after arguing about the horse's age awhile, we eventually get around to counting its teeth. I say we apply this practice to peer review. Let's try a system where authors are allowed to simultaneously submit to as many journals as they wish, being bound to publish in the first one that accepts their papers.

"It will swamp the system" summarizes the automatic response I get. I think not, not permanently. At the risk of having a paper accepted by younger, less prestigious journals, safe "nibblings" will continue to be sent to the appropriate ones. Another reason given is that editors and peer reviewers are already too busy. Too busy doing what? Clinging to privileged information for profit or obstruction? Are we really going to suffer from openness? Our own self-interests aside, does anyone really think it would be bad for science for more of us to share more generally in the commerce of emerging ideas?

However, it will fundamentally change the balance of power in the scientific publishing business. The best judges of original work will finally have an effective mechanism for indelibly attaching their names to original work and getting it published promptly. Nibblings will become bites. The same indelibility may discourage multiple submission of less than mediocre work. Dead wood clinging to established privileges, will be more promptly pruned to make room for new growth. Other measures could further improve the system (McCutcheon, 1991).

In 1972 Bugliarello observed about my discipline, hydraulic engineering and fluid mechanics, that it was stagnating from lack of controversy. Bad peer review is xenophobic. As "smart" people, let's take the hint from Watson. Let's stop privilege and restore the fun.

References [omitted for brevity, see the Bibliography]

Sincerely,

Signed Walter E. Frick

ON THE LETTER PUBLISHED BY *SCIENCE*

A letter based on the one in the previous section was published by *Science* (Frick, 1993). Only one of the two major ideas was published, the one calling on reviewers to relinquish control when they encountered ideas which they were unable to prove or disprove. The more viable idea of modifying peer review procedures was omitted. Recognizing that I would probably have to settle for half a loaf, I responded to the galley as follows:

Dr. Christine Gilbert
Letters Editor
Science

Dear Dr. Gilbert:

Thanks for considering my letter [accounting data]. In your condensation you capture one of my two main points, though I think the one less amenable to change, i.e. individual behavior.

In the spirit of your proposal would it be possible to change the two last sentences in the line with the following?

"...they recognize when they cannot reject foreign ideas with confidence. Relinquishing control and, if necessary, suspending their convictions, they step aside to let the general community evaluate them."

On another philosophical level, speaking as individuals, and in line with my other point, the bigger issue is shaking up journalistic practices. really revolutionizing them, to try approaches that would open up science and wrest control from some of the individuals and institutions now bottling it up. I am persuaded that Watson speaks euphemistically — dumb means bad, incompetent, arrogant, malicious, and bigoted. To think the impact of these individuals is trivial is naive. There are many creative thinkers who cannot survive the climate "dumb people" create, either because of their passive personalities or lack of the security of their positions (life itself, in some cases). Their creative energy is squeezed out of the system — lost. My point is that we can no longer afford as a society to let science be governed by those who are simply the most arrogant, most nasty, most privileged, or most obstinate. We need to restructure practices and institutions to neutralize them to give creative individuals access to their audience.

My points are base on experience gained in a part of the scientific landscape no doubt much less open and fair than the environment found at *Science*. My efforts to espouse a couple of unpopular ideas would have expired long ago before the closed gates of peer review except for the support of a few gutsy individuals who provided moral and financial support. I think these people are great, heroes always are. What should bother us all is that we need them. Even your distinguished journal plays a role in perpetuating an increasingly inadequate system.

Sincerely,

Signed Walter E. Frick

APPENDIX 5. SEARCH FOR AN APPROPRIATE JOURNAL

INTRODUCTION

Once it became apparent that the *Journal of Hydraulic Engineering* would not publish the paper "Improved prediction of bending plumes," it became necessary to identify an alternative journal. The review and re-review process at JHE had been a lengthy and debilitating one, it was not the kind of experience we were anxious to repeat. However, we continued to feel strongly the work should be published. Consequently, we ultimately decided to pursue a somewhat unorthodox approach. We first submitted the abstract to five editors of five different journals, asking them to give us their opinion as to the likelihood for publication. The queries were promptly answered and, after Dr. Liggett's unequivocal rejection (see Appendix 4), we sent the paper to the journal which expressed the most interest in the abstract: the *Journal of Hydraulic Research.* The letters and responses are given below.

THE *JOURNAL OF FLUID MECHANICS*

July 14, 1993

Dr. G.K. Batchelor
Editor, *Journal of Fluid Mechanics*
Cambridge University Press
Edinburgh Bldg.
Shaftesbury Rd.
Cambridge DB2 2RU, England

Dear Dr. Batchelor:

We have just finished revising a paper on a mathematical model to improve the prediction of strongly bending plumes. It is a Lagrangian model which alone places it under suspicion. (I had been working on the Lagrangian model for some years before I became aware of your considered opinion of the approach on page 71 of the 1970 reprint of *An Introduction to Fluid Dynamics*). In additions, it incorporates the projected area entrainment hypothesis which has been criticized in the past.

However, as is explained in the attached abstract, the paper identifies an anomaly in the traditional use of the round plume assumption while demonstrating the equivalence between the Eulerian integral flux and the Lagrangian approaches.

If you believe this may be of interest to the JFM readership we would submit it right away for review. An earlier version of the paper was rejected by the

ASCE *Journal of Hydraulic Engineering* and we sense the editor may be reluctant to consider it again.

Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

University of Cambridge
Department of Applied Mathematics and Theoretical Physics
Silver Street, Cambridge CB3 9EW
July 23, 1993

Dear Dr Frick,

Professor Julian Hunt (Assoc. Ed. of JFM) and I have both thought about the question posed in your letter of 14th July. The conclusion we have reached is that JFM referees would be likely to see the paper as largely concerned with questions of accuracy and convenience of different plume models. The part of the system that involves fluid mechanics in an essential way is the entrainment process, but we judge from the abstract that this is not discussed in the paper. We think a journal concerned with industrial or environmental fluid mechanics would be more suitable than JFM.

This is a hasty opinion, but I am sure it is better to send the paper elsewhere than to risk ultimate rejection after the time-consuming process of consulting JFM referees.

Yours sincerely,

Signed George Batchelor

THE *JOURNAL OF HYDRAULIC RESEARCH*

August 31, 1993

Editor, *Journal of Hydraulic Research*
International Association for Hydraulic Research
Rotterdamsweg 185
P. O. Box 177
2600 MH Delft, Netherlands

Dear Editor:

We have just finished revising a paper on a mathematical model to improve the prediction of strongly bending plumes. An earlier version was rejected by the

ASCE *Journal of Hydraulic Engineering*. We sense that the topic is controversial and that its editor may be reluctant to consider it again. Because we continue to believe in the veracity and importance of our ideas, we feel compelled to find a better way to identify promising publishers than by sequentially sampling various peer review climates. Thus, our request for your guidance and opinion.

Enclosed is the title page of the manuscript. The paper identifies an anomaly in the traditional use of control volumes and the ubiquitous round plume assumption while demonstrating the equivalence between the Eulerian integral flux and the Lagrangian approaches. It is of concern not only to the hundreds of engineers and scientists who use the US EPA plume models UOUTPLM, UMERGE, and UM, but anyone using control volumes in high curvature flow regions.

We are not asking for a guarantee. However, if you believe this may be of interest to your readership, and your reviewers are receptive to new ideas, we would submit it right away for review.

Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

IAHR *Secretariat*
Rotterdamseweg 185
PO Box 177
2600 MH Delft, The Netherlands

13 Sept. 1993

Dear Dr. Frick,

I read with interest your letter dd. August 31, 1993. Although I am not familiar myself with the topic of your proposed paper, I think that potentially it is suited for publication in our Journal. Although the ASCE *Journal of Hydraulic Engineering* has much in common with our Journal, our readership is more research oriented.

However, your paper will be handled in the normal way; it will be reviewed by two experts. Our actual acceptance rates is in the order of 50%. Please take into account that your topic is very specialized. This means that care has to be taken about a presentation which draws attention also to interested, but less specialized readers.

Enclosed you find our "instructions for authors".

Yours sincerely,

Signed Paul Kolkman
Editor of the Journal

COASTAL ENGINEERING

September 15, 1993

Editorial Office, *Coastal Engineering*
P.O. Box 1930
1000 BX Amsterdam
The Netherlands

Dear Editors:

We have just finished revising a paper on a mathematical model to improve the prediction of strongly bending plumes. An earlier version was rejected by the ASCE *Journal of Hydraulic Engineering*. We sense that the topic is controversial and that its editor may be reluctant to consider it again. Based on the enclosed title page of the manuscript, could you tell us whether you think your readership would be interested in our contribution?

Briefly, the paper identifies an anomaly in the traditional use of control volumes and the ubiquitous round plume assumption while demonstrating the equivalence between the Eulerian integral flux and the Lagrangian approaches. We think it would be of interest not only to the hundreds of engineers and scientists who use the US EPA plume models UOUTPLM, UMERGE, and UM, but anyone applying traditional control volume conceptions to model flow in high curvature flow regions.

We are taking this path to avoid the time consuming process of determining the responsiveness of specific peer review circles to new ideas. This has already happened. Thus, your guidance is valued and appreciated, while we recognize that your appraisal is also subject to uncertainty. Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

EDITOR-IN-CHIEF
H. F. Burcharth
Aalborg University
Department of Civil Engineering
DK-9000 Aalborg, Denmark

September 22, 1993

Dear Dr. Frick,

Thank you very much for your letter of 15 September regarding the possible submission of a paper to our journal.

I have asked the Editor, Dr. Burcharth, about the suitability of your article and, as you can see from the enclosed, he feels that the paper could be better submitted to a journal devoted to Environmental Engineering.

Thank you for considering our journal.

Yours sincerely,

Signed F.G. Koning

TELLUS

September 15, 1993

Editors, *Tellus*
Editorial Office
Arrhenius Laboratory
S-10691 Stockholm, Sweden

Dear Editors:

We have just finished revising a paper on a mathematical model to improve the prediction of strongly bending plumes. An earlier version was rejected by the ASCE *Journal of Hydraulic Engineering* and we sense that its editor may be reluctant to consider it again. *Tellus* has a reputation for publishing controversial work, although, compared to some of the pioneering contributions *Tellus* has published, our work should hardly be considered so. Based on the enclosed title page of the manuscript, could you tell us whether you think your readership would be interested in our contribution?

Briefly, the paper identifies an anomaly in the traditional use of control volumes and the ubiquitous round plume assumption while demonstrating the equivalence between the Eulerian integral flux and the Lagrangian approaches. We think it would be of interest not only to the hundreds of engineers and scientists who use the US EPA plume models UOUTPLM, UMERGE, and UM, but anyone applying traditional control volume conceptions to model flow in high curvature flow regions.

By sending you this letter, we are trying to avoid the time consuming process of blindly determining the responsiveness of specific peer review circles to new ideas. This has already happened. Thus, your guidance is valued and appreciated, while we recognize that your appraisal is also subject to uncertainty. Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

Tellus
Editorial Office
Arrhenius Laboratory
S-106 91 Stockholm

27 September 1993

Dear Dr. Frick,

We have considered your inquiry in your letter of 15 September 1993 as well as the enclosed title page of the manuscript "Improved prediction of bending plumes".

With regard to the typical readership of Tellus A and Tellus B, our judgement is that your paper would reach a rather limited group of readers. Thus believing that welcoming a submission of your paper to Tellus would do you a disservice, we suggest that you approach a more appropriate journal for publication. We like to thank your for considering Tellus for publication of your work.

Sincerely,

Signed Hilding Sundqvist
Editor in Chief, Tellus A

THE *JOURNAL OF ENVIRONMENTAL ENGINEERING*

August 31, 1993

Dr. Steven C. McCutcheon
Editor, *Journal of Environmental Engineering*
U.S. EPA Environmental Research Laboratory
College Station Road
Athens, Georgia 30613
Dear Steve:

We have just finished revising a paper on a mathematical model to improve the prediction of strongly bending plumes. An earlier version was rejected by the ASCE *Journal of Hydraulic Engineering*. We sense that the topic is controversial, and that Dr. Liggett may be reluctant to entertain it again. As editor we think both he and the associate editor tried to be as fair and helpful as they could, however, our peers in ASCE treated the work in a generally superficial and cavalier fashion. It was a sobering experience.

Because we continue to believe in the veracity and importance of our ideas, we feel compelled to find a better way to identify promising publishers than by sequentially sampling various peer review climates. Thus our request for your guidance and opinion.

Enclosed is the title page of the manuscript. The paper identifies an anomaly in the traditional use of control volumes and the ubiquitous round plume assumption while demonstrating the equivalence between the Eulerian integral flux and the Lagrangian approaches. It is of concern not only to the hundreds of engineers and scientists who use the US EPA plume models UOUTPLM, UMERGE, and UM, but anyone applying traditional control volume conceptions to model flow in high curvature flow regions.

We are not asking for a guarantee. However, if you believe this is of interest to your readership, and your reviewers are *receptive* to new ideas, we would submit it right away for review.

Thank you very much for your help.

Sincerely,

Signed Walter E. Frick

Journal of Environmental Engineering
Editor: Steven C. McCutcheon, Ph.D., P.E.
U.S. EPA Environmental Research Laboratory
960 College Station Road
Athens, Georgia 30605-2720

Dear Walter:

I received your August 31, 1993 letter on your submittal to the *Journal of Hydraulic Engineering*. Since I received your letter, I have had the opportunity to talk with Phil Roberts about the first two rounds of review. In terms of ASCE policy when we transfer a paper from one journal, we do transfer the comments from the previous rounds of review and we consider those before we make a decision on whether to go forward on the review.

I must say since we spoke of the work about two years ago, I am disappointed that you have not been able to publish the work in the *Journal of Hydraulic Engineering*. I believe that journal would be the ideal place to publish new approaches to simulating plume behavior. There is, however, a difference in the scope between the two journals. Like *Hydraulics*, this Journal also pays attention to theoretical developments, but we go further to ask than any contributions have a strong emphasis on the practice of environmental engineering. We focus on how the theoretical advance will impact practice in designing diffusers and other waste disposal alternatives. Both journals are receptive to new ideas but we consider broader practical contexts.

As a result, we offer a different emphasis in review, but I must say that it is quite a concern that the theoretical aspects of your work has not passed the review at this point. From my conversations with you and from reading your abstract, I sense that is the focus and strength of your contribution, not the practical application of plume models. Depending on whether I find bias, I would expect to back to at least one of the reviewers. I would of course reserve judgement until I had a chance to look over the comments.

If you believe very strongly that the reviewers did overlook an important contribution, then I would be quite willing to consider the paper further. But I must strongly warn you that we may likely reach the same conclusion if it is not clear how your Lagrangian method better estimates plume entrainment. I cannot judge your chances with this journal, without seeing the paper and the comments. I would therefore encourage you to revise the paper (I believe you are going to do this anyway) and send me a copy of the manuscript and the review comments. I would appreciate your written response to the comments. I will scan over the paper at that point and give you an assessment of what your chances may be. It may be appropriate to consider a technical note or another journal. You can then make a final decision at that point. I would encourage you to give me a call at (706) 546-3301. I am very pleased that you were able to follow through with the work we've talked about in the past. I hope if the paper is not published in one of the ASCE journals that you do find an appropriate place to put this material in print. With best personal regards, I am,

Yours very truly

Signed Steven C. McCutcheon

APPENDIX 6. EXCERPTS FROM THE DRAFT THESIS

KUHN'S CONCEPT OF PROGRESS IN SCIENCE

Introduction

Thomas Kuhn, in his book "The structure of scientific revolutions" (1970), describes two basic states of science, which, for our purposes, are assumed to apply to engineering as well, to a greater or lesser extent: normal science and scientific revolution. Scientific revolution is further divided into three sub-states: crisis, proliferation of paradigms, and paradigm shift. Each is associated with scientific progress.

The first state, normal science, exists when a single paradigm governs the activities of the entire community. The purpose of scientific endeavor under normal science is to reveal, or discover, the individual manifestations of nature implied by the paradigm. That is, its purpose is to flesh out the paradigmatic skeleton, not to promulgate new theory. In normal science progress is cumulative and, to the extent that the general nature of the results are anticipated, contributions are unexceptional.

The inevitable result of normal science is the discovery of anomalies, behavior violating the expectations derived from the governing paradigm. The community reacts in various ways, either reconciling the anomaly within the paradigm, or eventually reaching a state of crisis in which the anomaly is generally recognized but unresolved. In this state, the orderly sequence of work dissolves as the science becomes receptive to theories no longer derived from the established paradigm, i.e. paradigms begin to proliferate. However, many in the community will continue to defend the established paradigm despite its falsification and the simultaneous verification of new paradigms. In Kuhn's words: "They will devise numerous articulations and ad hoc modifications of their theory in order to eliminate any apparent conflict."

Eventually the anomaly is resolved, often by outsiders who are not as committed to the established paradigm. For example, Dalton, who revolutionized chemistry with the principle of fixed proportions, was a meteorologist trying to solve problems related to moisture in the air. The discipline is then redefined under a new paradigm which accounts for the anomaly while explaining much of existing experience. Normal science is re-established.

In many instances the revolution necessitates the redoing of previously completed work. For example, after the discovery of Roentgen (X) rays Kuhn states that "Previously completed work on normal projects would now have to be done again because earlier scientists had failed to recognize and control a relevant variable."

Much of this was described by Koestler (1964) six years earlier:

> "There are certain analogies between the characteristic stages in history of an individual discovery, and the historical development of a branch of science as a whole. Thus a 'blocked matrix' in the individual mind reflects some kind of impasse into which a science has maneuvered itself. The 'period of incubation', with its frustrations, tensions, random tries, and false inspirations, corresponds to the critical periods of 'fertile anarchy' which recur, from time to time, in the history of every science. These crises have, as we saw, a destructive and constructive aspect. In the case of the individual scientist, they involve a temporary retreat to some more primitive form of ideation--innocence regained through the sacrifice of hard-won intellectual positions and established beliefs; in the case of a branch of science taken as a whole, the crisis manifests itself in a relaxation of the rigid rules of the game, a thawing of the collective matrix, the breakdown of mental habits and absolute frontiers...."

Koestler is also more insightful about the process which makes the outsider so important. The reason they are able to play such a prominent role is that they "bisociate" ideas from two paradigms:

> "I have coined the term 'bisociation' in order to make a distinction between the routine skills of thinking on a single 'plane', as it were, and the creative act, which, as I shall try to show, always operates on more than one plane. The former may be called single-minded, the latter a double-minded...."

In this light the inhibitive role of peer review, as defender of the established paradigm, is evident.

The Established Paradigm

As explained briefly in Chapter 1, the established paradigm is the Eulerian integral flux plume modeling approach and its basic tenets, most notably associated entrainment hypotheses, the round plume assumption, and the standard conception of the control volume. This definition, while somewhat arbitrary, is the paradigm with which Winiarski identified when the Lagrangian model was first being developed. It is described in some detail in subsequent chapters.

The question of who determines to which paradigm one belongs is an interesting one. Mandelbrot and "normal" scientists apparently disputed the issue (Gleick, 1985). I conclude that even choosing one's scientific identity is a matter of paradigms, a bit like a new child identifying with and attempting to join other children in the sandbox. Different strategies of joining may be involved, for example, the child may subordinate itself to be accepted. Whether it is or not, the original activities of the group are likely to continue uninterrupted. A more dramatic strategy is to force its way into the group. In this case, if successful, the ongoing activities of the original group are likely to change, resulting in a paradigm shift.

Membership into engineering communities is traditionally attained through subordination (an initiation process). To quote Kuhn: "Scientists [Engineers] cannot reject paradigm and still remain scientists [engineers]."

The sandbox analogy seems appropriate. Carrying it a step further, the peer reviewers are individuals guarding the entrances to the sand box. They are appointed guardians of the common turf. Depending on conditions within the sandbox and the mandate the group confers upon them, they might be friendly or bullies.

Reactions to Crisis and Paradigm Shifts

The reaction of the established community to crisis and paradigm shift can be quite irrational while still being predictable. Once again, the intangible and relative nature of crisis should be remembered--the community may be quite unaware of any crisis existing. Consequently, its members are likely to react with dismay or indignation to any suggestion of anomaly. Recall Mandelbrot's experience.

Kuhn (1970) states:

> "...the choice [of paradigms] cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue....Each group uses its own paradigm to argue in that paradigm's defense."

He goes on to say that

> "To the extent...that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms."

They are at fundamental cross-purpose, a famous example being that of Berthollet and Proust and their debate on chemical reactions occurring in fixed proportion. "Each collected impressive experimental evidence for his view. Nevertheless, the two men necessarily talked through each other, and their debate was entirely inconclusive" (Kuhn, 1970).

Current State of Plume Modeling

In the context of this discussion, we might wonder what is the state of the established plume modeling paradigm? The children in the sand box will likely insist that it is normal science, or normal engineering. They argue that their paradigm, anchored by well-behaved cylindrical control volume integrations, provides a suitable mathematical and theoretical framework for solving the plume problem. The process of fleshing out the paradigmatic skeleton consists primarily of defining the entrainment function and other refinements, a process which is facilitated by dimensional analysis. They love the status quo and reject the notion that a crisis may exist.

However, in "Elements and control volumes in plume modeling," Frick, Baumgartner, and Fox (1992) give reasons to indicate that plume modeling is in fact already in a state of crisis. (The trustees are planning to replace the sandbox with other equipment.) They identify the main ingredients of crisis: a proliferation of entrainment hypotheses and concomitant tuning of coefficients, omission of basic physics from the integral flux paradigm, and the basic inconsistency of the definition of the cylindrical control volume. The control volume inconsistency implies the existence of negative volume, and hence negative mass, under specific conditions, i.e. high trajectory curvature. It is a compelling anomaly because it is at odds with all scientific paradigms!

The hard part is persuading a sufficient number of modelers to recognize the crisis, propose alternative paradigms, and, ultimately, to select a new paradigm and re-establish normal science. This is intrinsically different from Kuhn's conception and may be due to the nonspecificity of dimensional analysis.

One alternative interim paradigm has been proposed, though, as I have indicated, not in its "proper" (Kuhnsian) sequence. It is the Lagrangian plume model with a redefined plume element, or control volume. It is recognized that this paradigm serves mostly to clarify the crisis and it is not claimed that it will be the paradigm which ultimately emerges after the crisis is resolved. Of course, the principle of complementarity suggests both paradigms be kept under any circumstance. Nevertheless, while the Lagrangian paradigm has led to some valuable insights into plume modeling and the old established paradigm, particularly the identification of the negative volume inconsistency, its use has also encountered numerous difficulties, including numerical instabilities which tend to develop as the theory becomes more detailed, which suggest that it is far from the ideal platform that the community would like to have.

Predictably, in light of the experience of Mandelbrot (Gleick, 1985) and the observations of Kuhn (1970), the proponents of this and the established paradigm are talking through each other. The simplification of the Projected Area Entrainment hypothesis bolstered by extensive verification (Policastro et al., 1980; Carhart et al. 1982; Tesche, Jensen, and Haney (1980); Frick, 1984; Baumgartner et al. (1986), Lee and Cheung (1990), and Cheung (1991)), and its implications on the viability of numerous competing ad hoc entrainment hypotheses submitted over time, is simply dismissed by most members of the established school. The verification implicit in proof of equivalence of the established paradigm with the Lagrangian plume model given equivalent assumptions and the re-evaluation of the established paradigm that it necessitates is devalued. Now the falsification, i.e. the demonstration of the spurious existence of negative mass in the established paradigm, is studiously ignored or rejected. In the words of one reviewer of "Elements and control volumes...," "What the authors see as a correction [excluding negative volume], I see as an incorrection" (Anonymous, 1992). End of debate. No chance of open discourse in the general community ever develops because the ideas are suppressed by the peer review committee. Circumventing the system by publishing outside the community (e.g. in *Atmospheric Environment*) is effective, if at all, only over a great period of time because such interdisciplinary literature is given less credence or receive less exposure than does community literature. There is no substitute for publishing in the right place. Peer reviewers know all this.

Clues Obliterated, Crisis Obscured

Plume modeling is not typical science, typical science being more analytical while modeling is more synthetical. An important limitation of plume modeling, and much modeling, is the existence of weak links in the chain of equations, assumptions, and hypotheses comprising the model. The melding of numerous disparate scientific components into a model makes it inevitable that some parts are better than others. Historically, for example, in plume modeling the entrainment function has been identified as the weakest part. Because this part is weak it attracts the attention of the best minds in the established paradigm. However, as inferred from the new paradigm, a host of other relationships previously thought to be well behaved, particularly the definition of the model control volume, are in fact not themselves well defined.

The weak links of the chain introduce a degree of uncertainty into plume modeling which is compounded by the existence of uncertainty in available verification data. This uncertainty has a unique effect on modeling which is not shared to a great extent by ordinary science. It obscures the existence of crisis by smearing ordinary measures for detecting anomaly. As a result, established modeling paradigms, even more than scientific paradigms, are resistant to criticism because the crisis is hard to substantiate unambiguously. In other words, the data allow opposing conclusions to be drawn. In such circumstances, community decisions are likely to be based on unscientific factors such as the prestige of the researcher. Outsiders are at a distinct disadvantage in such an environment. This is why Frick, Baumgartner, and Fox (1992) have selected the negative volume anomaly to establish that the crisis state exists -- it cannot be disputed without violating broader governing paradigms. At the time it seemed to be a promising and appropriate tool for picking the locks on the doors of peer review.

But, shockingly, the negative volume anomaly is being denied by members of the established paradigm. To quote Kuhn (1970) once again: "They will devise numerous articulations and ad hoc modifications of their theory in order to eliminate any apparent conflict" (although my experience shows that studied neglect is a more common way of denying the anomaly). Details are given in Appendix C. A specific example of such an ad hoc articulation pertaining to the plume modeling crisis is given below:

> "However, all of the models under discussion assume for computational purposes that plume properties are symmetric with respect to the plume centerline. When we write a derivative in ds of bulk plume properties integrated over these planes, we do not have to artificially assume $\delta s = ds$ everywhere (i.e., assume parallel planes). The larger δs on one side of the centerline is counterbalanced by the smaller δs at an equal distance on the other side; *even if* $\delta < 0$ *on one side* and $> 2ds$ on the other side, their average will always be $= ds$; since symmetry has been assumed, the spacial gradients of plume properties are symmetric about the axis and the resultant sum (integral of gradients over two spacial dimensions on planes intersecting the plume) are the same as if $\delta s = ds$ everywhere. When $\delta < 0$ regions are left out of the integration, this balance is destroyed, just like one person falling off the seesaw. *Thus, what the authors see as a correction, leaving out "negative volume", I see as an incorrection.*"[4]. (Anonymous, 1992) (my emphasis)

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[4]. The review, of which this quotation is part, is given in its entirety in Appendix C. Except for this part where we talk entirely past each other, the review is actually quite innovative and even positive. The reference to the seesaw is interesting in light of the playground analogy.

Members of the physics community would recognize that integrating mass over the fanciful volume of the established paradigm is erroneous. However, peer review is not allowing that frame of reference, M_2, to intersect the established paradigm. Consequently, the obvious bisociations will not occur because the rigid thought control of the established paradigm prevents the necessary intersection of paradigms to take place. Without an appreciation of complementarity, the proponents of the two paradigms are destined to talk through each other.

The way to real understanding of the plume problem is not obvious and the consequences of uncertainty are more important than they appear. The two paradigms, which can be shown to be equivalent given equivalent assumptions, should not simply be "tweaked" and tuned to fit selected data. The value of the Lagrangian paradigm is its ability to reveal characteristics of the model that are latent or intrinsic in the other. Ultimately, it makes it apparent that a whole range of effects exists, attributed by the established paradigm to entrainment, that owe their existence instead to the myriad of other sources of error in the established model. Simply tweaking the entrainment functions to make the model fit some partial data set or other is not an acceptable way to truly solve the problem. Such a process, while apparently improving performance in one area, e.g. prediction of rise height, may actually degrade and obscure the proper solution in other areas, such as prediction of dilution.

In this confusion there is good reason to be concerned about the role of dimensional analysis and tuning. While recognizing the profound value of dimensional analysis to engineering, it should be recognized that because it provides only functional relationships which must be tuned, the very information for assessing the quality of a relationship, the difference between theory and experiment, is obliterated. Everyone knows, and everyone forgets, that dimensional analysis is at most as good as the list of parameters that go into it. To give just one example, it has not contributed to the identification of the roles of curvature and growth in correctly expressing PAE, as subsequent events, notably the publication of Lee and Cheung (1990) and Cheung (1991) has revealed.

Social Aspects of Plume Modeling

The foregoing discussion of community reaction to crisis and paradigm shift make it apparent that scientific transactions during times of crisis and paradigm shifts tend to be at cross-purposes. In modeling, I contend that the social interactions, especially peer review, are likely to be even more irrational, negative, and fruitless. The uncertainty in modeling, exemplified by indefinite or ambiguous verification results, allow extraneous unscientific criteria to emerge as determinants of so-called quality and acceptance. Combined with anonymity, the established paradigm can be fashioned into a formidable fortress.

The most important, and destructive, instantiation of this condition is the exercise of power to deny the existence of crisis and competing paradigms access to the scientific media which should play an active role in fostering debate over the anomaly. The motivation can be competition for funds, the threat of losing prestige gained from earlier work likely to be overturned by the new paradigm, closing of ranks behind other members of the established paradigm, or something as trivial as a casual derogatory remark made publicly, like that of the expert in Chapter 1, embarrassing if shown to be wrong. The mode of execution is likely to be through positions of power, such as professorships, and appointment to peer review committees. The latter facilitate rearguard actions by virtue of the fact that the individuals are not accountable for their actions. Again, the uncertain nature of modeling make it especially susceptible to such aberrant behavior while helping its practitioners to rationalize and moralize their actions.

Kuhn observes that "Scientists cannot reject paradigm and still remain scientists. This has undoubtedly driven some to desert science." This philosophical abstraction conceals the real human, and community, costs of unenlightened (and anonymous) peer review. But, whereas Kuhn appeared content with letting a crisis develop and then to wait for the *Überscientist* to propose a new paradigm, I contend this tolerance of the status quo is foolish. Why would scientists, the one group in the world most committed to rational thought, leave scientific revolutions to chance[5]? Worse than that, why would scientists tolerate a system which systematically attempts to destroy the thinkers of the fundamental ideas on which the fundamental paradigms are based?

> "The great ideas in science in the next few years will be those not yet thought of. The system ought to select people likely to think them, but, alas, it is inherently biased against such speculation. Granting agencies want certainty, and reviewing peers fear unexpected discoveries by their competitors." (McCutchen, 1991)

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[5]. The answer is obvious. In lieu of systematic ways for encouraging new ideas and in the presence of oppressive peer review, chance is about the only agent for change remaining.

Is it not worthwhile to search for ways to improve peer review, without destroying it, to better foster new ideas? Surely, the paradigm of peer review itself is overdue for a paradigm shift.

Soldiers and Patrons of Revolution

Kuhn identifies the states of science and speaks about the inevitability of crisis, noting that science is imminently equipped to discover anomaly. In other words, scientific revolution is not only inevitable but also desirable, even though it is resisted. Since revolution occurs at all levels and scales of science, sooner or later all scientists are likely to experience one. One would think that our education would prepare us for that eventuality and even teach us to look forward to it. But, on the conduct of the revolution Kuhn is less insightful, as already noted.

Surely exceptional individuals emerge, though, once again, modeling complex systems presents its own unique obstacles to would-be revolutionaries. A paradigm that addresses the anomaly while explaining substantial parts of established science is ultimately deduced. What Kuhn does not emphasize, however, is the fact that at least one other individual must recognize the merits of a new paradigm before it has a chance to be considered by science, never mind revolutionalizing it.

Science can be, and, to a large degree, is, a (representative and constitutional) democracy: peer review governed by a constitution, the established paradigm. It generally rejects contributions that are believed not to conform to the paradigm (Bella, 1987). This power is exercised by *anonymous* and established scientists. What they see and reject may never become the subject of discourse in the community because the work may be altered or even withdrawn, forever. They are anonymous to insulate them from biasing influences. However, anonymity allows them to exercise their own biases more or less indiscrimminately. McCutchen (1991) thinks it would be better to have negative reviewers sign their reviews and only positive reviewers remain anonymous. He also advocates no-fault publication. I mention his ideas at this point only to suggest to the reader that the existing attitude on the best peer review procedures is by no means a unanimous one.

There is no appeal to another power. For example, Kuhn states that "One of the strongest, if still unwritten, rules of scientific life is the prohibition of appeals to heads of state or to the populace at large in matters scientific." Historically, rejected authors either give up or attempt to publish elsewhere. Yet, revolution is as important to the progress of science as normal science and revolutionary ideas do get through.

So how are revolutionary ideas exposed and debated when they are rejected by the peer review system, as they are likely to be? Sometimes it is done stealthfully, for example, Mandelbrot, one of the key contributors to early chaos theory, frequently disguised his most revolutionary ideas in innocuous terms. Lorenz published in the relatively obscure journal of meteorology *Tellus*, leading one physicist to bitterly exclaim afterwards, "*Tellus!* Nobody reads *Tellus*" (Gleick, 1985).

An even more unusual example is that of James H. Wiborg, a businessman and self-taught scientist. In a private publication, Theoria Primarius (Wiborg, 1992), he presents a sort of TOE (Theory of Everything) which predicts several outstanding physical phenomena, currently being attacked by general relativity and multi-dimensional string theory, in a straight-forward, standard mathematical fashion. Now whether or not his paradigm is revolutionary and viable or not is irrelevant if it goes to the grave with him. Wiborg's book represents his commitment to finding that one adherent to champion and carry on the work. By induction, which has mathematical as well as social implications, other adherents are found.

The Role of the Editor, Judiciary Branch of Peer Review

Revolutionary ideas become the subject of discourse by the community at large because a way is found to circumvent the pseudo-democratic, unaccountable, and paradoxically autocratic, peer review system of normal science. It is as if the United States, or other great democracies, were run by governments not scrutinized by a free press. In the foregoing examples, the authors themselves do the circumventing. However, perhaps the most common circumvention is also the most obvious: peer reviewers and editors knowingly circumvent the system and use criteria of discrimination which transcend or ignore the paradigm to which they are, or are supposed to be, bound.

For example, recently the journal *Nature* published a paper that claimed that water had "memory." The editors knew that they would be criticized for their decision beforehand, yet, citing their commitment to a broader authority than the governing paradigm and peer review community, they published it anyway. "While [editors] do not want the system to be flooded by substandard contributions, where there is a fundamental disagreement, the journals should expose the issue" (Bella, 1992), which *Nature* did. The editor essentially serves in a judiciary capacity.

While the editor is in general not a specialist in the field in question, normally (s)he is a scientist who shares super-paradigms with the peers. In the present case, within the super-paradigm lies the basic premise that negative volume has no validity in science. Given that peer reviewers of the established paradigm deny this prohibition, an editor not actively practicing in the sub-paradigm, i.e. as non-specialist, may still overrule the verdict of the peer review process.

My experience shows this is unlikely. Though editors may have the knowledge and authority necessary to overrule the peers in a review, they tend to not become involved for understandable reasons. Negative volume is exactly what Frick, Baumgartner, and Fox (1992) identify exists in the established paradigm (as well as in earlier versions of their own). This falsification of the established paradigm should not be ignored and should be submitted to the community at large, to force an open discourse and debate to occur. Instead, the road of least resistance is usually taken. The editor simply counts votes and relays the bad news. Once again, there is little in the peer review system that is likely to change this state of affairs.

Conclusion

Thomas Kuhn's book, "The structure of scientific revolutions" (1970), provides a useful paradigm for understanding the scientific process. Its influence even extends to the popular culture in Joel Barker's film: "Discovering the future: the business of paradigms" (ca. 1992). Kuhn describes a process of normal science and scientific revolution, both of which contribute to the advancement of science, as the two important scientific processes. Most of his ideas, as well as important ideas about the process of creation, were anticipated by Koestler (1964).

Normal science is dominated by an established paradigm which serves as a framework for inquiry. Inevitably, anomalies are discovered which cause crisis and ultimately lead to a new paradigm. There is evidence that the process often deviates from this simple conception. In particular, anomaly is often not really recognized as such until after the old paradigm is already replaced (Lightman and Gingerich, 1992) and, consequently, crisis tends to be denied. The beneficial influence that complementarity might provide is absent.

The reactions to paradigm shift or claims of crisis tend to be negative and predictable. Peer review plays an important role in keeping a crisis from being identified. In plume modeling, and undoubtedly other modeling and engineering fields, approximate analytical techniques, e.g. dimensional analysis, further help to obscure crisis and insulate the established paradigm from revolutionary forces.

Kuhn does little to describe the makeup of the revolution and fails to fully recognize the inhibitory nature of peer review and the strategies used by researchers to circumvent this conservative institution. He is happy with the concept of *Überscientist.* In fact, a cadre of revolutionaries ultimately works to make the revolution acceptable.

PARADIGMS AS LANGUAGE FOR UNDERSTANDING PLUMES

A New Language

Many scientific papers develop and advocate very specific technical discoveries, often with rather superficial reference to the overall philosophical framework within which they exist. As has been pointed out above, this is possible because an accepted paradigm, or common language, exists. This language is understood by peer reviewers who evaluate a new contribution for its overall compatibility with the established paradigm.

Not surprisingly, as our model did not emerge from the established paradigm, more often than not, our contributions have been rejected by the peer review process (Frick, 1984 being the exception; in a process that circumvented the engineering community it was published in *Atmospheric Environment*). We do criticize some of the specific principles and methods of the established paradigm but the reason for rejection has more to do with the general differences in paradigms. Since my readers may not know the language of the Lagrangian paradigm but wish to learn it, it is appropriate to present a short background discussion which will give a philosophical framework to help understand it.

In this chapter, a rather general discussion of my notion of science as it existed some time ago (Frick, 1991) will be presented. In rereading this earlier material which I wrote while trying to come to grips with my own experience, I have found many parallels with Kuhn (1970) and have made some changes to adjust the discourse to reflect his more familiar philosophical paradigm, a task which was easily achieved. This discussion leads naturally to the examination of specific plume behaviors that shape some of the important ideas in the Lagrangian paradigm.

Science, a Maze, Plume Modeling

Science is like a maze, a metaphor used by Kuhn (1970), albeit sparingly. Similarly, Richard Feynmann compared science to a room with multiple exits, basically a maze. A maze has an entrance, and presumably an exit, its goal, (Kuhn argues that science is evolutionary and presumably has no exit). It does, however, have dead ends. Near the entrance to the maze are signs placed there by other scientists who came before. Some signs point the way while others warn of dead ends. The signs are sometimes contradictory and become less frequent deeper in the maze until they disappear altogether.

Any maze can be traversed simply by following a wall with a finger, never losing touch (L'Amour, 1987). Consequently anyone can do it, given sufficient time. (There is also no reason to assume our maze is two-dimensional.) Also, thanks to Aristotle science is an infinite maze for by virtue of analysis every corridor can be split in two, and two again, ad infinitum. This was happening before Galileo's time with the theory about the orbits of the sun and planets about the earth. Thus, recognizing dead ends can be a tricky business.

A scientist is given a lifetime to solve the maze, or at least to be honored for finding the best trail for others to follow. Because time is of the essence scientists abhor dead ends, even though that knowledge is useful. In addition to having recorded their ways through the maze they have also developed, through experience, rules to help them recognize the most promising direction. The rules are the paradigm, which represents a systematic pursuit of knowledge involving the recognition and formulation of a problem, the collection of data through observation and experiment, and the formulation and testing of hypotheses.

The paradigm provides an efficient way to examine and analyze the clues found along the way, either as part of the path through the maze or dead end. Apparently the scientist can examine each clue, as by measuring it, and conjure up a statement that interprets it, as in a hypothesis. The hypothesis can then be subjected to a procedure invented by the scientist or someone else to test its veracity. This process can be repeated with different hypotheses and tests as frequently as desired, providing there is time.

Bad measurements can cause a test to fail and indicate a direction that points to a distant dead end. So can a bad test on good measurements. If errors are made it can lead many in the wrong direction and result in great loss of time. If the scientist gives up the quest, for whatever reason, without discovering his error, the signs he leaves will frustrate those who follow.

As powerful and useful as the paradigm may be, it is also part of the maze. In other words, the paradigm changes our perception of the maze, making some paths, the most promising ones if the paradigm is a good one highly visible while rendering false trails virtually invisible. And, vice versa.

Paradigms are the most extensive and constant entities in science. Usually we adhere to several paradigms which nest and overlap. In our specialty we use the simplest statement of paradigm possible, what I have been calling the established paradigm. Paradigms are constant in the sense that the basic skeleton remains fixed, however, scientists are constantly fleshing out the skeleton in a process Kuhn calls normal science.

When paradigms do change, and they do, the change tends to be profound, a process Kuhn calls scientific revolution--to him normal science is unexceptional. For example, the Greeks established the Ptolemaic paradigm of the universe, which was earth centered. It formed the basis for humanity's perception for millenia. However, when good measurements like Brahe's became available, scientists, like Kepler and Galileo, improved our knowledge of the maze primarily by identifying the dead ends to which the signs at that time lead, for it was a rather haphazard system of navigation before that time. There was a shift in paradigm, a scientific revolution, and, as a result, few go down the-sun-about-the-earth corridor anymore.

Once the shift is made the new paradigm is quickly established and refined, becoming an institution much like washing one's hands before surgery, a practice which, by the way, was initially rejected by peer review. And for a while rapid progress is possible because clues along the way are efficiently measured and interpreted. Guided by the paradigm, a whole new perception of the maze becomes apparent as new hypotheses are developed and evaluated.

Still, dead ends are inevitable. Within Kuhn's paradigm, these are the anomalies. In the late 1800's many physicists were in a dead end called the ether hypothesis, busily opening new corridors which were also dead ends, partly because the wave paradigm was supreme and there was no notion of complementarity. As Kuhn explains, what happens is that a number of paradigms may be proposed each casting the maze into a different light. The tolerance of the discipline's practitioners to outside ideas increases and ultimately, frequently someone from another orientation, provides a new governing paradigm that resolves the anomaly.

In the case in question, no one had examined the clue at the entrance, which had to do with the speed of light, very carefully (compare what is happening in plume modeling). In fairness, maybe the means were not yet available. Eventually Michelson and Morley did examine the clue and developed an experiment to illuminate it, pardon the pun. This process helped create, notably by people like Einstein, Bohr, Schrödinger and others, the theory that then pointed in other directions, see Lightman and Gingerich (1992) for an alternative interpretation. Thus a new sign was put up and people stopped wasting time going down the wrong fork. It's not hard to recall people from dead ends because the maze is not a physical place and journals and other means of communications have been developed to publicize relabeling of the forks. Putting it in Joel Barker's terms (ca. 1992), when there is a paradigm shift everyone starts over. In our terms, there is a certain amount of consolidation and re-evaluation, a return to an earlier level of ideation and innocence (Koestler, 1964), not to a pre-scientific state.

The method for generating hypotheses hasn't really changed, except that most scientists recognize a general quality of hypotheses which excludes religious and occult elements (no longer part of the overall scientific paradigm) as not having been very fruitful for establishing promising directions in the scientific maze in the past. That is why we have chemistry and not alchemy. Otherwise, the hypothesis seems to be the product of the creative act, on which Kuhn (1970) is relatively silent. We found that laying out all the known pieces as carefully as possible and replacing old, worn pieces with better ones facilitates the creative process. This is part of the discipline of science. Koestler (1964) theorizes that creativity is a process of bisociation, the interaction of two intersecting paradigms--the junctions of paths in the maze analogy.

Also part of the discipline is the general guideline to value simplicity as established by Occam's razor. Its most common form is the attitude "There must be a better way." This seems to be a rather simple way of avoiding using Aristotle's gift excessively. It is part of a larger, more immutable paradigm of science.

The clues deeper in the maze are more complex, retaining a retinue of clues found nearer the entrance at each step. (This is not to say science becomes more difficult, just like a maze does not become more difficult, because our level of understanding is constantly being improved by work that sheds more light on the process.) Consequently, the hypotheses also begin to build and depend upon one another and tend to become compound hypotheses, that is, theories or models. At least this is the way it seems to be in the plume modeling maze. Soon the quality of the individual component hypotheses becomes a factor. Thus, as another practice, scientists rate different parts of their theories: hypotheses that can't be substantiated very well but seem reasonable and are necessary are called assumptions. Hypotheses that have worked well not only in one field but in many are called laws. In between are, for lack of a better term, still hypotheses.

Normally, scientists value laws over hypotheses and assumptions, no matter how pertinent the latter are to the theory. Thus the ingredients of a hypothesis are chosen, wherever possible, first from laws, second from other hypotheses, and only when absolutely necessary from assumptions. Of course, "normal" behavior sometimes breaks down under stress, i.e. during times of crisis.

Successively deeper entry into the maze requires the eventual upgrading of assumptions to hypotheses and hypotheses to laws. This is normal science. It does explain, however, why passing up clues for convenience can be a problem later on.

Paths can diverge, which implies that some paths eventually converge again with other paths. Each path is a paradigm or sub-paradigm. Sometimes clues can be examined to try to determine which path is the shortest and easiest. However, both paths have different clues which may be useful deeper in the maze and consequently, if the resources are available, it is sometimes prudent to direct traffic along both paths, i.e. to nurture multiple paradigms to facilitate creativity.

A maze is often thought of in connection with leisure, serving no other useful purpose (cf. Koestler's (1964) discussion of the importance of play). This connotation may be appropriate for some branches of science, but usually uses for science are eventually found. In fact, the fruits of some sciences are so urgently needed that methods for rapidly appraising and analyzing the clues, if in a cursory manner, have been established. This could be a distinction between engineering and science although engineers are generally scientists and will be considered to be such here. They know that some of their theories will be superseded upon further examination but feel that temporary gains justify the practice. Safety factors are established to mitigate any potential negative side effects.

One would conclude that, as a consequence, engineers would be most amenable to changing paradigms and complementary approaches. In fact, I think they are the most resistant. Once again, not always without reasons.

Dead Ends in Plume Modeling

The plume modeling maze is possibly more difficult than some others because the clues are either hard to measure or our meters are still too crude to measure them accurately and with precision. Either way, the resulting uncertainty makes it is harder to interpret clues than they might otherwise be. It is hardly amazing then that different researchers have established different paths through the maze. In fact, it is amazing that so few do take substantially different paths. It is likely that some of the paths are dead ends, not converging paths, explaining some of this reticence. If they are, it is worthwhile to identify and relabel the way. As the number of plume modelers is limited (e.g. how many people want to know about the fate of sewage?), it is important to call down the dead end corridors to alert others, and to direct their attention to newly posted information.

Of course, considering the uncertainty, some diversity of quest is healthy. It is wise to establish different paths; in plume modeling there are the Eulerian integral flux modeling, Lagrangian plume modeling, finite difference, volume, and element modeling paths, to name a few. That the Eulerian integral flux and Lagrangian approach are converging paths has been established particularly for plume models by Frick, Baumgartner, and Fox (1991). Lee and Cheung (1990) and Cheung (1991) further support this conclusion. Clues from both paths will be found relevant here.

Interpreting and Re-interpreting Clues

The clue found at one of the forks along the way where signs point in different directions has to do with the definition of the radius of the plume. Previously, when researchers examined jets in quiescent ambient fluid, the path revealed that plumes and jets, special cases of plumes without buoyancy, have properties concentrated near their center and possess a profile that in simple cases approximates the binomial or Gaussian distribution.

At this point, one group of modelers, convinced of the statistical significance of the Gaussian profile chose one fork and labelled it "Gaussian plume modeling." This label is a bit misleading because some Gaussian plume modelers, as will be seen, took the other fork. Consequently, we will add to the label: "nominal radius," and call it the "nominal radius Gaussian plume modeling" path or something like that. Nominal connotes the fact that the chosen radius has an arbitrary relationship to the statistical properties of the profile and not to the plume as a physical entity.

The way they interpreted the clue was to conclude that the definition of the radius was a relative matter that would not affect the solution. Some of them might have concluded that the concentrated core of the plume should be the focus of attention and that the model should be a centerline model. In any case, they concluded that a statistic of the Gaussian profile, a multiple of the distance to the standard deviation of the profile would become the nominal radius.

The other group of modelers considered the dynamical significance of the definition and concluded that the radius of the plume must include the entire plume. Others in the group may just have thought that this path was easier and therefore worth pursuing, if for only a little while. They labelled their path "top-hat" to emphasize the importance of the averages of various properties, like buoyancy as applied to the total mass. (We note however that some Gaussian modelers took this path also, i.e. they tried to confine the Gaussian distribution to the entire plume cross-section.)

Returning to the dynamical significance of the top-hat radius definition, the key to the clue at the fork is the momentum equation, you know the one that says $F = ma$, where F is the force acting on the mass m and causes an acceleration a. The top-hatters concluded that the trajectory of the plume element must be a property of the total buoyancy of the plume, therefore, the top-hat radius.

(It is important to recognize, for subsequent purposes, that both models are particle models despite the fact that the plume element has finite dimensions and, in the nominal radius model, an explicit distribution of properties. That is to say both models predict only the motion of the center-of-mass of the plume element, however the element is defined.)

The fact that scientists split at this juncture would normally indicate that no satisfactory experiment was devised to test the nominal and top-hat radius hypotheses or that the measurements were sufficiently uncertain so that the results were ambiguous. Rather than wait for conclusive proof one way or the other they went on, feeling secure in their judgement.

However, scientists are human and the possibility that some more subjective concerns came into play should not be ruled out. Perhaps neither group felt inclined to make the changes that would be required to go the other way. This becomes part of the overall uncertainty that can later obscure key criteria and tests. In any case it is hard to imagine that they thought their paths would converge again.

Maybe it is worthwhile to consider again the clues, hypotheses, and test results to see if new conclusions can be drawn.

Figures 1 and 2 show the predictions of two models described by Teeter and Baumgartner (1979): a Lagrangian top-hat model, OUTPLM, and a nominal radius Gaussian plume model, PLUME. Also shown are the predictions for another nominal radius plume model described by

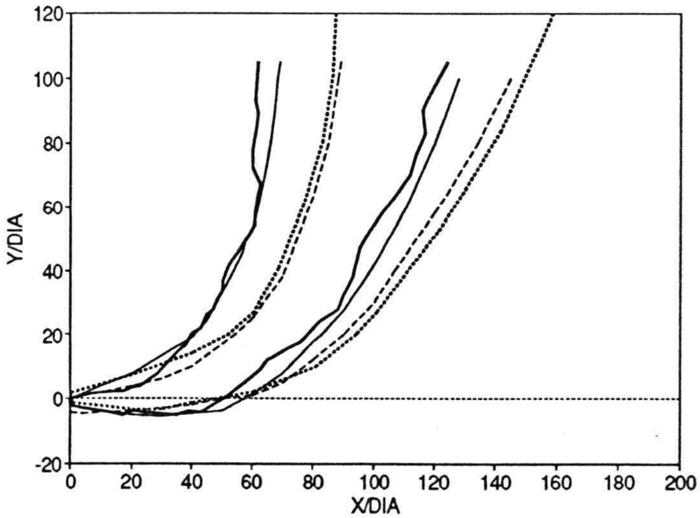

Figure 1. Comparison of OUTPLM (light solid line), PLUME (light dashed), and Schatzmann (heavy dashed) compared to data observed by Fan (Run 22, heavy solid).

Schatzmann (1979)[6]. All predictions are compared to plume measurements reported by Fan (1967). Fan's data show the visible outlines of the plume while the models show the top-hat and nominal-radius outlines, as appropriate.

By examining Fan's work we find that he carefully reproduced some of his experiments to establish the repeatability of his technique. In addition to photographic data, of which Figure 1 is an example, he also made centerline and cross-sectional measurements. We conclude that his work has high quality and suffers from less experimental uncertainty than the 15 to 20% often attributed to this kind of work.

The comparison, while limited, is not uncharacteristic. It shows that the top-hat model better approximates the trajectory and width of the plume. When the competing theories are examined to give further insight into the models we find that the top-hat model uses a single tuned constant while the Schatzmann model uses multiple coefficients. Thus, the top-hat model should be picked again by the cut of Occam's razor.

The fact that the nominal radius Gaussian plume model is thought by many to be the best plume model (cf. Cheung (1991) who selects Schatzmann (1978) and Chu (1975) for comparison to JETLAG), in contradiction to this conclusion, makes it worthwhile to start the maze again to study the messages at previous forks.

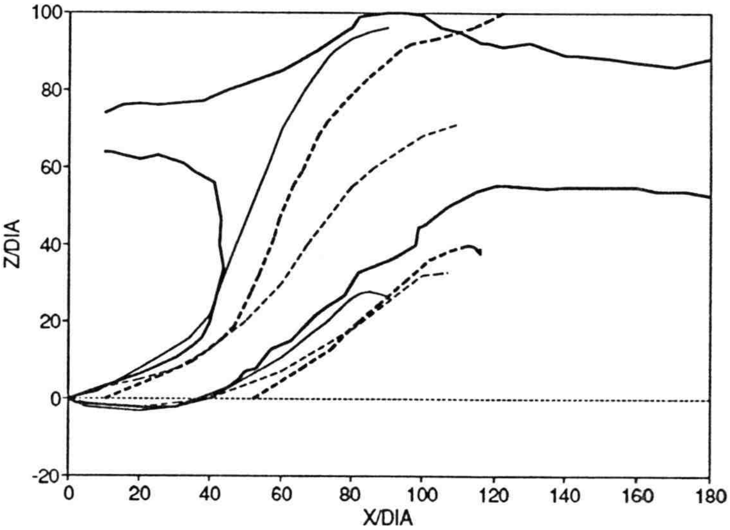

Figure 2. Same as Figure 1 using Fan's Run 16.

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6. Schatzmann's published trajectory of Fan's Run 16 does not correspond exactly to Fan's data in that the aspect ratio of the vertical and horizontal scales is not quite true.

A Short History of Nominal Radius Gaussian Plume Modeling

The Gaussian plume models, as a class, seem to originate from experiments on jets discharged in quiescent ambient fluid (Albertson et al., 1950) and similar work. Jets are streams of fluid which have the same density as the surrounding ambient fluid in which they exist; they possess momentum, not buoyancy. In the absence of density stratification (or cross-current) jet fluid flows in a straight line, on the average. This is the degenerate case of a complex trajectory whose solution is trivial. In a dynamic sense it practically has no trajectory at all.

As was noted earlier, in this simple environment it is found that the cross-sectional profiles of velocity or tracer quantities have a distinct binomial, or Gaussian, character. Furthermore, their boundary is not discernible as a distinct surface, especially in the absence of dye for visualization. Consequently, the radius became to be described in terms of its statistical properties describing, for example, the distribution of velocity over the plume cross-section. The fact that this definition might be at odds with efforts to predict the trajectories of buoyant plumes, or jets and plumes in a cross-current, was not apparent because experiments affecting these concepts were not done until after many of the main ideas were firmly established.

As a model for jets in quiescent ambient fluid the nominal radius Gaussian model is successful. However, as the forces are parallel to the direction of motion, the trajectory is degenerate and potential dynamical contradictions are not revealed.

Douglas Fox (1970) used the Gaussian model to predict the maximum penetration of buoyant plumes into stably stratified, but quiescent, ambient fluid, as shown in Figure 3. The trajectory degenerates again to a straight line but comes to an end, at maximum penetration. Again it was possible to fashion agreement with observations without actually solving the complex trajectory problem by tuning the coefficient of the aspiration entrainment hypothesis, also know as the Taylor hypothesis, after Taylor in Morton, Taylor, and Turner (1956). Interestingly, the fit requires a smaller coefficient than for top-hat models, a fact that was interpreted to be consistent with the nominal radius hypothesis.

The fact that the top-hat model does an equally good job predicting the *average* penetration (Figure 4) and predicts increasingly more penetration if the entrainment coefficient is reduced, mimicking the Gaussian solution, seems to have gone largely unnoticed. Also unnoticed was the fact that the Gaussian profile plays no substantial direct part in the dynamics of the plume.

Contradictions only become apparent after the theory is applied to horizontally discharged buoyant plumes and to jets in a cross-flow, conditions under which the trajectory no longer degenerates to a straight line. In essence this was the first test for the dynamical part of the theory, which failed, though, of course, the failure went unrecognized. The failure suggests that there exists a unique combination of plume element mass and total buoyant force which results in the prediction of the observed trajectory. Frick, Baumgartner, and Fox (1992) call this mass the "dynamic mass."

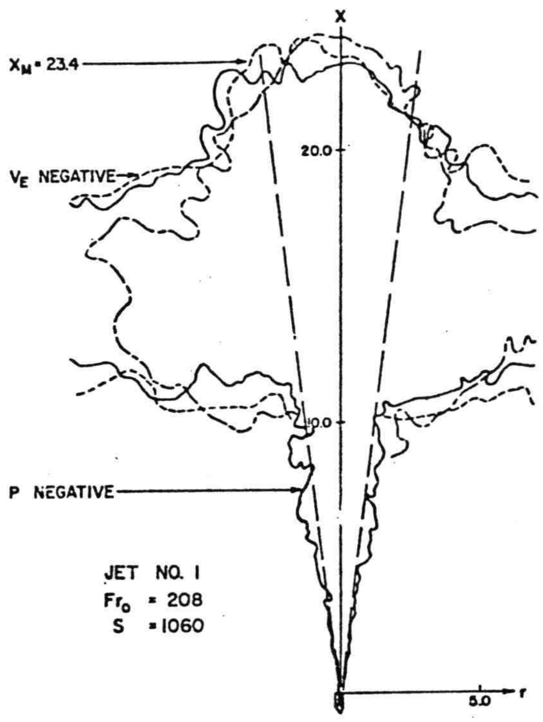

Figure 3. Prediction of a nominal-radius model by Fox (1970) for plumes discharged vertically into stratified ambient fluid.

The fact that the test failed was not acknowledged however, possibly because the observed behavior was already outside the paradigm. In fact, as can be seen from Figure 1, the nominal radius model doesn't even predict the trajectory on the side of the top-hat trajectory where the buoyant core, or cores, because these plumes bifurcate, are located. The top-hat model succeeds because it more closely

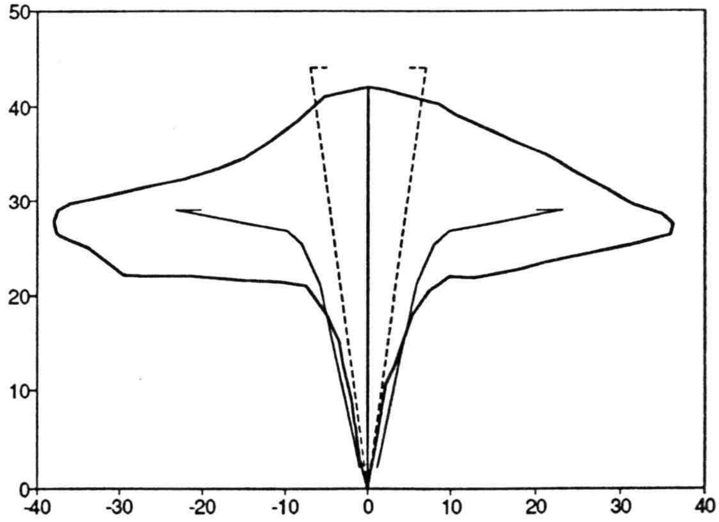

Figure 4. Model predictions for a top-hat model (OUTPLM) and a nominal-radius model (PLUME) compared to data taken from Fan (1967). (Frick, ca. 1981)

mimics the dynamic mass and the total buoyancy. Thus, even though the top-hat model is less sophisticated with respect to the details of the profile, it is more consistent with the average physics of the problem.

This evidence should have disqualified the nominal radius hypothesis and the Gaussian models that used it, i.e. the nominal radius Gaussian paradigm. A dead end sign should have be posted. It is not known for certain why it was not. Experimental uncertainty may be the reason that is cited to explain discrepancies, it certainly clouds the decision making process. However, it is also possible that there was a reluctance to upgrade or switch models, a form of scientific inertia. It is one reason why scientific revolutions are resisted.

Upgrading Plume Models

At the time the Eulerian integral flux plume modeling paradigm was first established, models were simple. The entrainment hypothesis (the equation of continuity) and momentum equations and other equations, collectively called the non-entrainment equations co-existed, in commensurately primitive forms. The purpose of normal science was to develop these crude ideas fully, in hopes of solving the plume problem.

Thus, early on, the entrainment hypothesis was little more than an assumption (e.g., Morton, Taylor, and Turner, 1956; Weil, 1974) to make an analytical solution of the equations possible. The non-entrainment equations were also incomplete imitations of their more rigorous counterparts, viz. the Navier-Stokes equations. For example, the equation of state was often assumed to be linear, i.e. to have a constant bulk modulus of expansion (Hirst, 1971; Kannberg and Davis, 1976), a practice that leads to catastrophic failure of the plume models in some cases, as is shown by Frick and Winiarski (1978). Figure 5 shows that for the case of an initially buoyant thermal water plume discharged into fresh freezing water, the plume, after rising briefly, unexpectedly sinks to the bottom.

Through the process of normal science, the models were gradually upgraded, e.g., a non-linear equation of state was introduced (Teeter and Baumgartner, 1979). This change was timely considering the availability of more powerful computers and work accurately defining the density of seawater over common temperature and salinity ranges.

Primarily, however, much effort was devoted to upgrading the entrainment hypothesis which was perceived to be the major obstacle between the model and better simulation. It was targeted for upgrading even though the non-entrainment equations remained in a basically

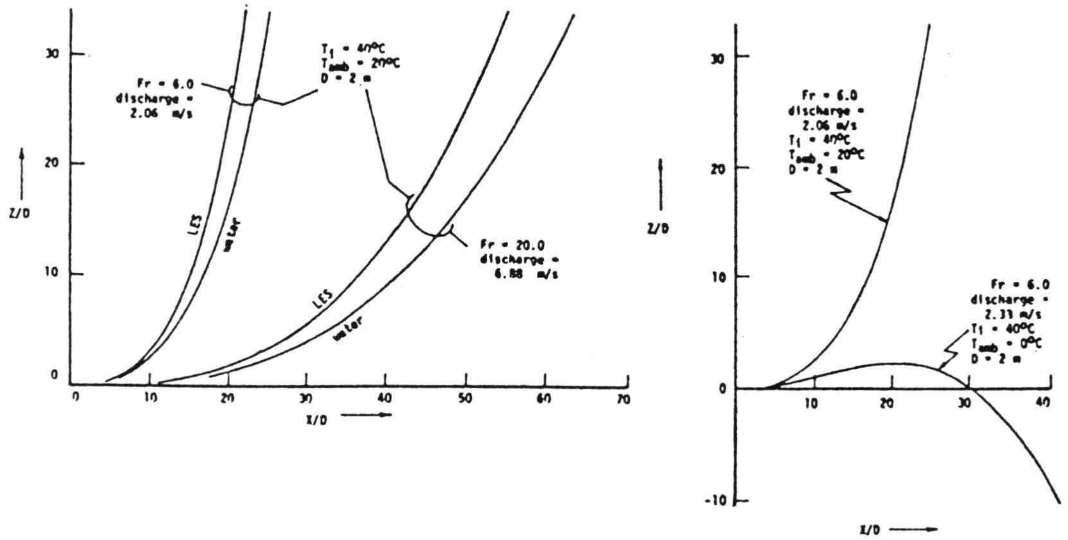

Figure 5. The effect of making various equation of state assumptions. LES refers to linear equation of state (constant bulk modulus). The others allow for curvilinear effects.

primitive form, in contradiction to the upgrading principle described earlier. That principle, in fact, requires a return to simpler entrainment hypotheses while the non-entrainment equations are upgraded. Paradigm shifts, even small ones, require basic ideas to be re-evaluated, cf. Koestler's (1964) return to a previous level of ideation. An upgrade of these basic equations could for the first time take Lagrangian and Eulerian integral flux models beyond simple particle mechanics to the mechanics of distributed mass.

The need to upgrade the non-entrainment equations is evident from the work of Frick, Baumgartner, and Fox (1992) and Frick, Fox, and Baumgartner (1991). In the course of attempting to extend the validity of numerical initial dilution plume models beyond the trapping level, that is, beyond the level of average zero buoyancy of plumes to which the earlier EPA plume models are confined (Muellenhoff et al., 1985), they found several theoretical deficiencies in the earlier Lagrangian plume model. In an example of bisociation, the intersection of the Eulerian and Lagrangian paradigms, the use of a material element in Lagrangian modeling, not a fixed control volume, precipitated these conclusions.

This example demonstrates why it is to the benefit of the discipline as a whole to maintain more than one paradigm. The problem of convergence of the element faces was explained previously. This key concept necessary for equivalence between the two basic models establishes the element shape as an important component of the overall problem. In other words, it made it apparent that close attention must be paid to realistically defining the plume element. The work demonstrates that control volumes cannot be arbitrarily defined, if such a definition means that inconsistencies are introduced into the model as they are. For example, in the case of facial intersection, the immediate problem of intersection can be avoided arbitrarily by defining the element such that intersection is prevented. However, in that case, faces perpendicular to the trajectory can no longer be maintained and the dynamical equations must be changed to account for the flux of non-orthogonal vectors. This is not being done in the established paradigm. Instead, the inevitable "anthropogenic" discrepancies introduced into the model are falsely attributed to deficiencies in the entrainment function.

Thus, Frick, Baumgartner, and Fox (1992) show that the particle dynamics of existing plume models is inadequate. After correcting the equations, they show that the improvements effect a reduction of approximately 15% in dilution, a level high enough to evoke the unwarranted tuning of entrainment coefficients. The conclusions are inescapable because they derive from well known laws which have priority over hypotheses, including any tuned entrainment hypothesis. They are part of a larger, transcending paradigm.

Several deficiencies were identified by pursuing this line of research, normal science in the Lagrangian paradigm. First it was found that the definition of plume elements in regions of high trajectory curvature, as is frequently encountered between the trapping level, the depth at which a plume's average density first equiliberates with the ambient density, and maximum rise, leads to difficulties in defining the plume element because the leading and trailing faces of the element will often intersect, or overlap, due to their construction perpendicular to the curving centerline. Then it was discovered that the standard equations for calculating the element volume and plume radius are incorrect under these conditions. Consequently the radius and, through feedback, the entrainment is generally overpredicted and it is no longer hard to imagine that entrainment functions are being tuned to compensate for deficiencies that are being created by anthropogenic errors in other parts of the model. In other words, simulated entrainment is in part a function of analytical artifacts!

Since it is also shown that an analogous top-hat Eulerian integral flux model gives equivalent results, the established paradigm is subject to the same errors.

Frick, Baumgartner, and Fox (1992) also observe that the center-of-mass of even an un-overlapped element and the center of the round cross-section are not co-located. While this fact may seem irrelevant to the argument, the redistribution of mass that it implies changes the shape of the plume and indirectly the entrainment. Furthermore, since entrainment is generally asymmetric, the trajectory should include an apparent motion (drift) simply due to the addition of mass to the plume element. In other words, it is governed not just through dynamic forces and inertia, but drifts because entrainment implies the element must be continuously redefined consistent with the constantly changing distribution of mass. This invalidates the earlier particle models -- the idea that entrainment can be treated as if mass addition were occurring at the center of mass of the element. A quantitative example comparing the basic model with one containing

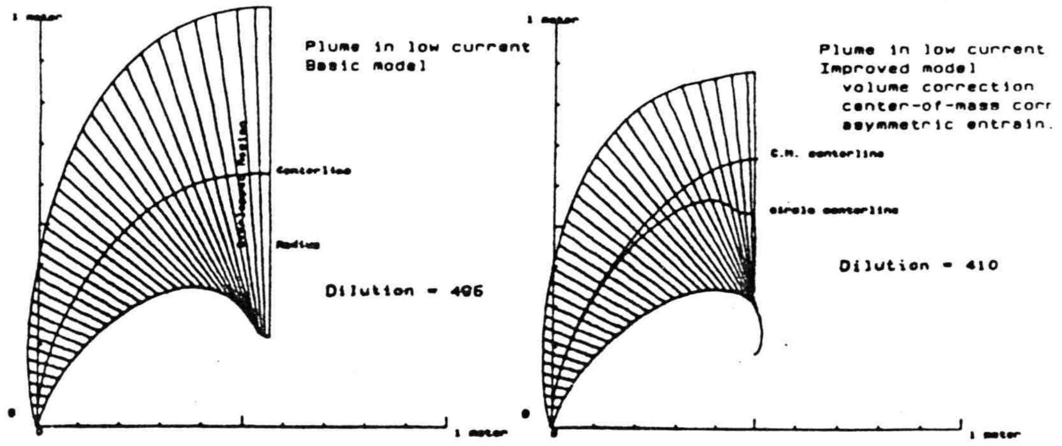

Figure 6. a) basic model predictions. b) enhanced mass distributed model using overlap, the center-of-mass correction, and asymmetric entrainment.

these adjustments is shown in Figure 6.

The important point to establish is that the cumulative effect of these mechanisms on plume behavior is substantial, being larger than the relatively fine differences that prompt changes to entrainment hypotheses. Thus the entrainment equation, admittedly a weak link in the modeling chain, tends to be a lightning rod for activity. But, in light of the evidence given here, it is apparent that simple entrainment hypotheses should be maintained at least until more rigorous non-entrainment equations are established. It can not be over-emphasized that the plume equations are not interchangeable tuning mechanisms. While all the equations are linked, tweaking the entrainment equation is not an alternative to fixing the momentum equation.

There is another argument that is worth making that supports very simple entrainment hypotheses. The acknowledged uncertainty in plume dilution data is about 20%. Suppose the entrainment function could be expanded in a Taylor series. Then one might expect the major term to account for about 50% of the natural variation in entrainment. The second term 25%, and so on. Clearly, the third term would be buried in the noise of the experiment and would be, by line of reasoning, better left out of the model. Ignoring this concern will add another layer of uncertainty to the model (Larimore and Mehra, 1985) and will be counter-productive. In other words, the model will be overtuned.

Maze Flotsam

There are several other observations to glean from a traversal of the known part of the plume modeling maze. All in themselves are comparatively unimportant, but taken together they are significant.

One concern is using dissimilar but related data for verification without discriminating their differences. A good example is the simultaneous use of photographic plume data and so-called centerline measurements. A figure borrowed from Fan (1967) illustrates this problem beautifully as shown in in Figure 7. In verifying models the data are often mixed together without pointing out the disparity between the center-of-mass of the plume cross-section and the maximum centerline concentration point (which are also shown in Figure 7 in reference to the cross-section also taken from Fan). To put it another way, nothing is said about the fact that the assumed plume center, as defined by photography, has no simple connection to the measured centerline trajectory obtained by moving a sensor in a vertical plane through the center of the plume. The discrepancy is pointed out by Frick (1984) and demonstrates that the entrainment coefficients obtained by tuning to this data would depend entirely on the mix of dissimilar data adopted by the individual modeler. Cheung (1991) accounts for this difference in his data.

When fluids are withdrawn from plumes for analysis, as is commonly done, it is often not clear from the description of experiments whether it is done iso-kinetically or not. Iso-kinetic sampling intercepts only that fluid directly impingent on the sampling orifice, i.e. the sampler acts like a sharp ring that neither sucks nor blows. If sampling is not iso-kinetic it is really impossible

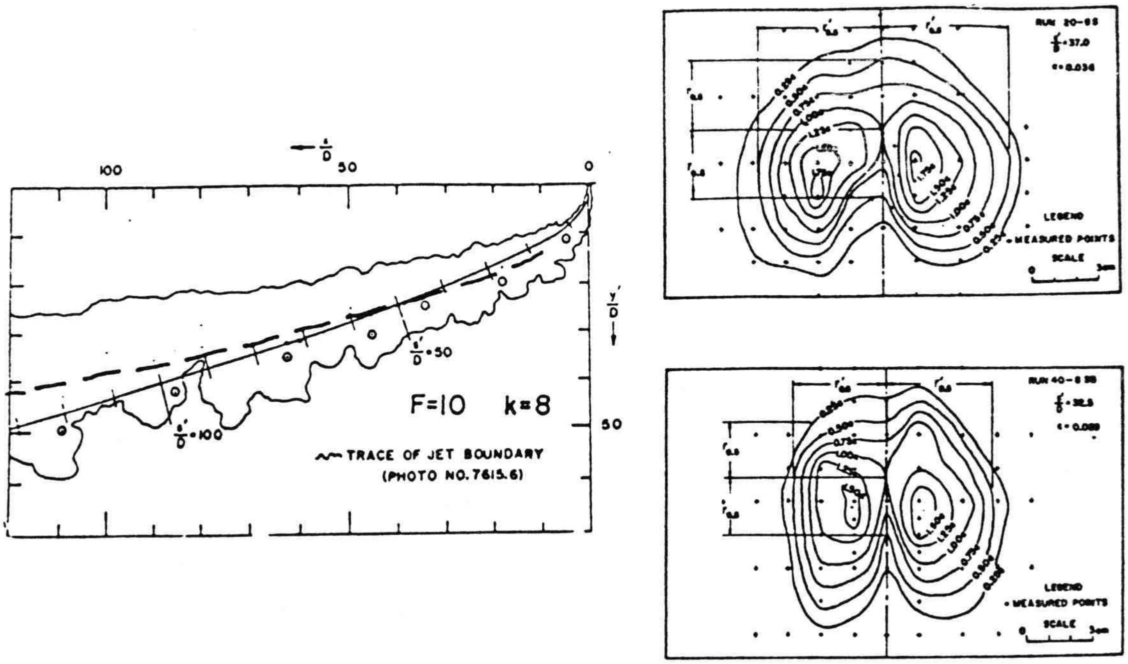

Figure 7. Relationship between different rise statistics, approximate center-of-mass is shown by heavy dashed line; and cross-section. Adapted from Fan (1967).

to determine the spatial averaging that is occurring and to which the data are subject. Of course, the probe itself will disturb flow patterns.

While the dilution of plumes is a central concern to environmentalists, concentration is not necessarily the best statistic for verifying plume models. The problem with centerline concentration measurements is that often the so-called centerline concentration does not represent a maximum concentration (Fan, 1967; Cheung, 1991). On the other hand, defining average concentrations is difficult because the area of the cross-section (as well as the parameters of concern) must be accurately known. A 10% error in diameter translates into a 21% error in average dilution. Thus, while plume modelers cannot ignore concentration measurements, it is often better to use trajectory measurements for tuning. Of course, the differences between the various types of trajectory measurements must be respected, as is pointed out above, and they should be quantified. However, a 10% error in plume rise is only a 10% error and therefore has potentially more value for verification.

Some doubts about plume modeling are hard to articulate, which is vexing in itself. In this category are doubts about the use of coefficients. Coefficients are multipliers which are not necessarily constants. While they are frequently necessary because they embody empirical evidence which is otherwise hard to formulate, it is hard to imagine why we would want to develop theories which depend on variable coefficients. Would the universal gravitational constant be nearly as compelling if it were a variable? The whole idea behind theories is to have equations composed of variables and constants. Yet the coefficients are not variables nor constants. The best that can be done is to place such theories under the empirical category to use only when necessary. However, as soon as it becomes evident that a model consisting of variables and constants predicts as well as an empirical model using coefficients the latter should be replaced.

Finally, the use of peak-to-mean ratios should be consistent with the model in question, notably statistical and top-hat models. More recognition should be given to the fact that peak-to-mean ratios depend not only on the model in question but also on merging, current, averaging definitions, and other factors. Used correctly, the uncertainty in the appropriate value for a peak-to-mean ratio in given circumstances is probably in the range of 50%. It loses all meaning with respect to recent laser visualization experiments depicting the pronounced lumpiness in plumes (Papantoniou and List, 1989).

Recommendations

While several main conclusions and recommendations are presented at the end of the thesis, it seems appropriate to conclude this chapter with several that relate to the topics of experimental and modeling techniques.

1) When it applies to basic research to develop plume models, concerned individuals should assure that proper equation upgrading sequence is observed (a previous level of ideation) and that the level of refinement of the assumptions and the entrainment function is subordinated to the level of refinement in the non-entrainment equations.

2) In the absence of exceptionally good reasons justifying additional terms, entrainment functions should be simplified to include at most 2 to 3 tuned coefficients until the appropriate degree of equation upgrading is achieved.

3) Steps should be taken to encourage the upgrading of empirical models as outlined in this work. Models using variables and constants should be preferred over models using variable coefficients (purely empirical models) under the range of conditions where their performance is demonstrably comparable.

4) Further development of nominal radius plume models should not be encouraged.

5) Nominal radius plume models should be withdrawn from circulation as soon as suitable substitutes become available. Gaussian integral flux models should be redefined in terms of the dynamic mass.

6) The dynamic mass concept should be adopted. Relationships between the dynamic mass and other quantities, such as centerline dilution, should be developed. The use of peak-to-mean ratios to compare between different statistics should be thoroughly reviewed and developed.

7) Collection of data suitable for making discriminating judgements between theories should be encouraged.

8) Iso-kinetic sampling should be instituted wherever sampling involves withdrawal of fluid from the plume.

9) The simple particle model (existing Eulerian integral flux and Lagrangian plume models) should be phased out as it is replaced by distributed mass (center-of-mass) models.

10) The round plume assumption should be modified to account for plume element facial intersection, or overlap.

The recommendations apply to basic research. In general, they do not impinge on EPA's short-term strategy of using empirical models to provide estimates for plume behavior under special conditions. i.e. conditions not well treated by fundamental scientific plume models. However, as implied, the standards for empirical models should also be raised.