

14th ERDA AIR CLEANING CONFERENCE

SESSION IX

OPEN END

Tuesday, August 3, 1976
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OPENING REMARKS OF SESSION CHAIRMAN:

Session 9 is called "Open End" for a specific purpose. The intention is to open the session to short topics, to things that are in progress in the research labs but have not yet developed to the point where they are suitable for formal papers. It is to permit people to ask questions, to make comments, and to ask other people questions which will elicit useful answers. This is not intended to be a session for leftover papers or last minute formal papers, but, rather, a chance to discuss with colleagues many points which, perhaps, did not come up adequately or at all in the structured sessions. For that reason, the time allotted to each of the speakers will be brief.

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COORDINATION OF FIRE TERMINOLOGY IN ASTM*

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During the past few years, legal and technical questions have arisen concerning certain terms used in the field of fire technology. Such expressions as "non-combustible," "self-extinguishing," "slow-burning," have been challenged by government agencies and by fire protection engineers.

The American Society for Testing and Materials (ASTM) has in its various standards and testing committees subcommittees whose function is the development of appropriate terminology associated with the standards and test methods. When it became evident two or three years ago that the several different committees were each defining the same term in somewhat different ways, it was decided within the Society to set up coordinating groups each associated with a particular technology. At the present time two such working groups exist: WG-1 - concerned with terminology in the field of fire technology; and WG-2 - concerned with a similar function in regard to building materials.

Although in the interest of efficiency working groups are restricted as to numbers, committees having an interest in the particular technology are invited to provide representation and expertise. Thus, on Working Group I, we have representation from the Fire Tests Standards Committee, the Fire Hazards Standards Committee, committees on plastics, wood, rubber, textiles, and electrical insulating materials. Some members cover more than one area of interest, e.g., the National Bureau of Standards, ISO (TC-92 - International Fire Tests Standards), the Consumer Products Safety Commission, and the National Fire Protection Association.

WG-1 has accumulated a list of several hundred terms from within ASTM and from outside organizations. It has established priority lists for terms to be considered, and then proceeds to ballot on and discuss results of these ballots following the usual ASTM procedures. When coordinated terms have been finally agreed upon, they are sent to committees having an interest in these terms for comment and possibly further refinement.

It is proposed to publish terms in a particular field of technology in the form of a glossary which may eventually become a technical dictionary.

The two tabulations included herewith represent lists of terms which have been coordinated by WG-1 and are currently under various stages of review as indicated. We have found it necessary to go back to fundamental terms because in many cases the standard dictionary definitions are not deemed adequate. I present them here today for

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your general consideration with the idea that perhaps a similar coordinating effort should be made in the field of air-cleaning technology.

TERMINOLOGY IN FIRE TECHNOLOGY

I. Coordinated Terms Currently Under ASTM General Review

- ablation (n) - a self-regulating heat and mass transfer process in which incident thermal energy is expended by loss of material from exposed surfaces.
- afterglow (n) - emission of light, usually subsiding, from a material undergoing combustion, but occurring after flaming has ceased.
- char (v) - to form carbonaceous material during pyrolysis or during incomplete combustion.
- char (n) - carbonaceous material formed by pyrolysis or incomplete combustion.
- combustion (n) - a chemical process of oxidation that occurs at a rate fast enough to produce both heat and light either as glow or flames. Note: Some oxidation, such as that of hydrogen, emits radiation outside the visible spectrum.
- fire exposure (n) - the heat flux of fire, with or without direct flame impingement, to which a material, product, building element or assembly is exposed.
- flame (n) - a hot luminous zone of gas or matter or both in gaseous suspension that is undergoing combustion.
- flame spread (n) - the propagation of flame along or across a surface.
- flash point (n) - the lowest temperature, corrected to 101.3 kPa (1013 millibars) of pressure, of a sample at which application of an ignition source causes the vapor of the sample to ignite under specified conditions of test.
- glow (n) - (1) visible light given off by a substance because of its high temperature; (2) visible light, other than from flaming, emitted from a solid undergoing combustion.
- ignition (n) - the initiation of combustion. Note: The combustion may be evidenced by glow, flame, detonation or explosion. The combustion may be sustained or transient.
- pyrolysis (n) - irreversible chemical decomposition caused by an increase in temperature without oxidation.
- self-ignition temperature - (misleading term - not recommended).

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smoldering (n) - combustion of a solid material without flame, often evidenced by smoke. Note: Smoldering can be initiated by small or low-temperature sources of ignition, especially in loose materials, and may persist for an extended period of time after which a flame might be produced.

standard temperature/time curve - in fire testing, a curve used to define the prescribed furnace temperature with progressing time; that given in Method E-119, unless otherwise specified.

temperature (n) - the thermal state of matter as measured on a defined scale.

II. Coordinated Terms under Review by Interested Committees

fire severity (n) - the potential of a fire to inflict damage. Note: An evaluation of fire severity includes the properties - temperature, effective heat flux, rate of heat release, and duration - of the fire. Estimating fire severity also requires consideration of the thermal characteristics of a product, or component of a fire enclosure.

fire severity test exposure (n) - the measure of fire severity resulting from a standard fire test exposure. Note: As applied to ASTM methods E-119, E-152, and E-163 - the ratio of the area of the temperature/time curve of the average test furnace temperature to that under the standard temperature/time curve. In each case time is measured from the start of the test to the end of the test or time-of-failure; temperature is measured above a base point of 20°C (68°F).

flame spread rate (n) - the surface distance or area covered by a propagating flame per unit of time.

flame spread index (n) - a number or classification indicating a comparative measure of flame spread or flame spread rate as determined by a specific test method. Note: As applied to a particular test method, a flame spread index should be defined as to what it measures, e.g., distance, area, distance/time, or area/time, and should include the appropriate dimensions.

fire retardant (n) - a substance added to or incorporated into a material which by such incorporation or addition hinders the ease of ignition, flammability or combustibility of the resulting product.

smoke (n) - the visible and invisible airborne products - solids, liquids, and gases - evolved when a material undergoes pyrolysis or combustion.

DISCUSSION

FIRST: I'll exercise the chairman's privilege and make a comment. I'm one of the people that Mr. Gaskill has argued with about the term "smoke." I believe that optical density is an important characteristic of smoke and I asked him why he wished to redefine a word in common usage when it's not necessary. Perhaps, Mr. Gaskill will tell us why he prefers to call "the visible and invisible airborne products of a material that undergoes pyrolysis or combustion", "smoke" instead of "decomposition products."

GASKILL: "Smoke" is to be used to designate decomposition products by a particular process called pyrolysis. It is a special phenomenon in the field of fire. Although one can break up a product chemically and get a number of products out of it, I think we use "smoke" when discussing the phenomena associated with heat, flame, and radiation in fires. It is peculiar to that subject. Apparently, a number of my colleagues in ASTM agree with me.

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ULTRAHIGH EFFICIENCY SPARKPROOF ELECTROSTATIC PRECIPITATION

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Abstract

Low-conductivity nonmetallic plates for two-stage electrostatic precipitators allow design improvements and simplifications such that efficiencies of 99.99+ percent appear attainable at an air speed of 250 fpm and a pressure loss of 0.1 inch of water.

Text

In production designs, the two-stage electrostatic precipitator has been limited to filtration efficiencies of 85-95 percent. This performance has been satisfactory in many recirculating air systems, where little would be gained by higher efficiency operation. In many other situations however, including some in the nuclear industries, only those filtration efficiencies in the 99.9+ percent range are acceptable.

Over 25 years ago, an NRL team began to study possible improvements in two-stage electrostatic precipitator design, using the Westinghouse Model PH-12 Home Precipitator as a model. Our first effort was to place two of these units in series. The result was puzzling: the efficiency of two units in series was the same as that of either unit alone. This suggested localized areas of low filtration efficiency which were in the same locations in both units. We now began probing the downstream face of one of the units with a suction tube connected to an NRL Smoke Penetrometer. In a matter of minutes a definite pattern of low-efficiency areas was evident. It was clear that a significant fraction of the airflow through the precipitator (and this was shown to be true of other two-stage ESP's) was bypassing the collection fields through gaps between the high-voltage plates and the grounded aluminum housing. Closing these gaps with phenolic plastic strips and plates raised filtration efficiencies more than tenfold; to 99.5 percent or better. We now believe that at least part of the residual inefficiency was due to design factors which would be corrected automatically in the new design principle discussed below.

We were reaching for still higher efficiencies. Three of the modified precipitators were placed in series in a single housing. Efficiencies rose to levels beyond the sensitivity of the NRL light-scattering meter. However, a microbiological technique indicated efficiencies in excess of 99.999% for the triple unit. This was at an air speed of 250 fpm, and a resistance of about 0.1" of water. The finished device was large, heavy, and the modifications in the commercial design were all made and fitted by hand. It should be possible, however, to produce an ultra-high performance two-stage

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electrostatic precipitator following the principles of our no-bypass design.

One problem remained in the no-bypass design, and we are only now in the process of solving it. Filling the bypass gap between high-voltage elements and ground with plastic members is not entirely practical. Carbon tracking may occur, and with it permanent electrical leaks or dead shorts.

A new invention, the low conductivity collector plate, appears to make fully feasible the no-bypass concept in commercial designs. The new plates are nonmetallic, and have a low but appreciable electrical conductivity. The plates offer no significant resistance to the electrical current caused by the desposition of charge aerosol particles, but do not permit the concentrated and relatively high currents required to produce tracking of plastic insulation or inter-plate sparking. Other advantages are: (1) collector plates can now be supported solely at their edges, thus eliminating complex and heavy insulated support systems; (2) this change in turn allows more uniform airflow and the option of higher airflow, shorter collecting plates, or higher efficiency; (3) plate-to-ground sparking becomes impossible or improbable to any degree desired, and this in turn opens the way to; (4) the advantages of higher-voltage operation; and (5) the safe collection of highly flammable or pyrophoric dusts such as aerosols of uranium or plutonium.

DISCUSSION

FIRST: Could you tell me how these new devices behave in the presence of high humidity? Have you had a chance to check that?

FIELDING: No, we haven't. We're just getting underway. We've run a few tests.

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THE EFFECT OF EXPOSURE TIMES IN THE PREHUMIDIFICATION OF IMPREGNATED CHARCOALS *

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INTRODUCTION

An integral step in the test procedure for determining the trapping efficiency of methyl iodide at 25°C is the prehumidification of the impregnated charcoal. The specifications (1) direct that the prehumidification with air at $95 \pm 2\%$ RH be continued until the temperatures recorded at the entrance and exit of the charcoal bed are equal. Since this operation may require about five hours, it is convenient to allow the prehumidification to extend overnight (sixteen hours). This Note is concerned with the unexpected adverse influence on methyl iodide trapping by exposures of charcoals to prehumidification at long contact times.

RESULTS

The penetration of methyl iodide as a function of time of prehumidification is illustrated (Figures 1, 2 and 3) for a number of impregnated charcoals. After the designated prehumidification with air at $95 \pm 2\%$ RH the addition of the methyl iodide (two hours) and the desorption step (two hours) were made in air flows of 25 liters/min also at $95 \pm 2\%$ RH. The increased penetration with increase times of prehumidification is quite unexpected and it does not appear to have been reported hitherto. In the cases of no prehumidification, several minutes were allowed for the flow of air to reach a steady state before starting the dose; both the dose and the desorption steps used carrier air also at $95 \pm 2\%$ RH. The least penetration was observed with no prehumidification of the charcoal.

The weight-gains of the various samples were determined upon continuous exposure to air humidified at 95-96% RH. As shown in Figure 4, the gain in weight of these charcoals leveled off in about 20 to 40 hours, but the penetration of methyl iodide continued to increase after longer periods of prehumidification. After about 100 hours of prehumidification, the MBV-base charcoals leveled off at about 2.4% penetration, an unsatisfactory value (Figure 2). The penetration through KI_x impregnated charcoals also appeared to level off, but this occurred at a most unsatisfactory value (Figure 3). The leveling-off of the weight-gain after a relatively short time suggests that the quantity of adsorbed water is not the controlling parameter in the enhanced penetration of methyl iodide. It does not appear to be a question of filling the porous structure of charcoal with adsorbed water and thus blocking access to adsorption sites.

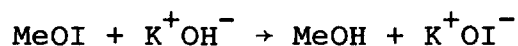
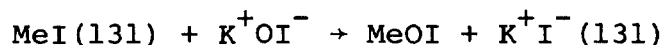
* This work was processed under ERDA Contract No. AT(49-1)-3627 (Division of Nuclear Fuel Cycles and Production) and the complete cooperation of John C. Dempsey, Contract Manager, is gratefully acknowledged.

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DISCUSSION

The specification that the prehumidification be continued until equal temperatures are recorded at the entrance and exit of the charcoal bed assures only that the heat of adsorption of water vapor has been reduced to a non-measurable quantity. One explanation may be that multilayers of adsorbed water are formed and the mass transfer of methyl iodide vapor across the interface is significantly reduced. The formation of multimolecular layers is similar to the formation of multimolecular clusters in imperfect gases (2) and there is no reason to expect that surface multilayers simply grow in depth over the entire surface and thus approach bulk water. Instead, the barrier layer of water may have finite openings between the clusters through which the methyl iodide may eventually diffuse given sufficient residence time. This model has suggested additional experimentation that is now in progress.

A reaction mechanism was suggested (3) for methyl iodide trapping in which the hyperiodite ion may be the reactive species, viz.



It may be possible that large surface coverage with adsorbed water interferes with the above mechanism. Since the reactivity of the prehumidified charcoal is recovered after drying, it may be a valid explanation.

An important aspect of the above findings is the relevancy to plant practice. The incoming air drawn into the charcoal filter may at times reach a high relative humidity. A. Gary Evans of the Savannah River Laboratory has noted (4) that while the humidity in experimental units may average about 70% RH, the daily high values after sundown may attain near saturation. Thus, a charcoal filter in operation may be subjected to a significant flow of saturated air. The extent to which such exposures influence the efficiency of a charcoal filter is as yet unknown.

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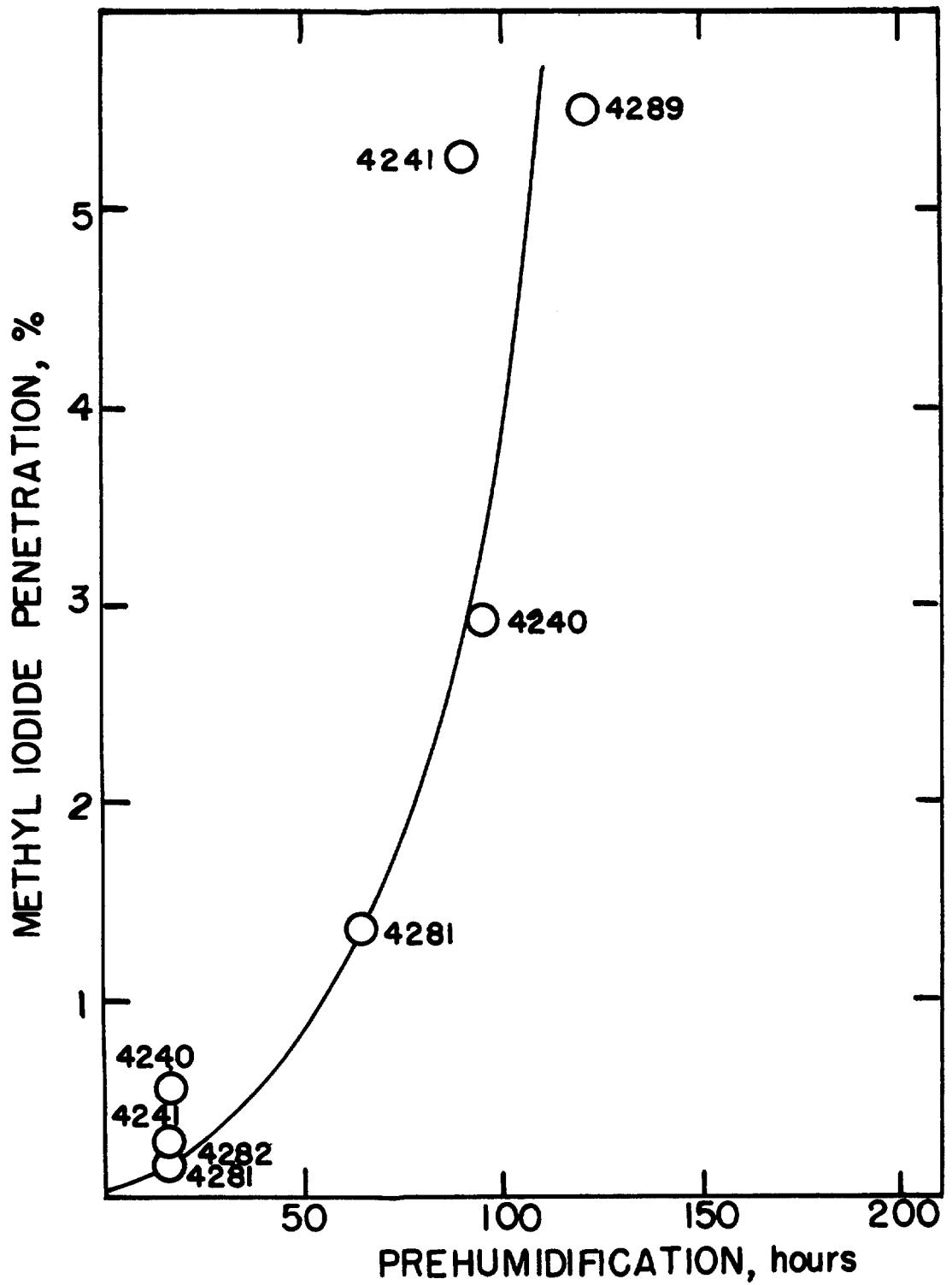


Figure 1: Penetration of methyl iodide as a function of the prehumidification time using iodate-iodide impregnations on Gx 202 (a wood charcoal).

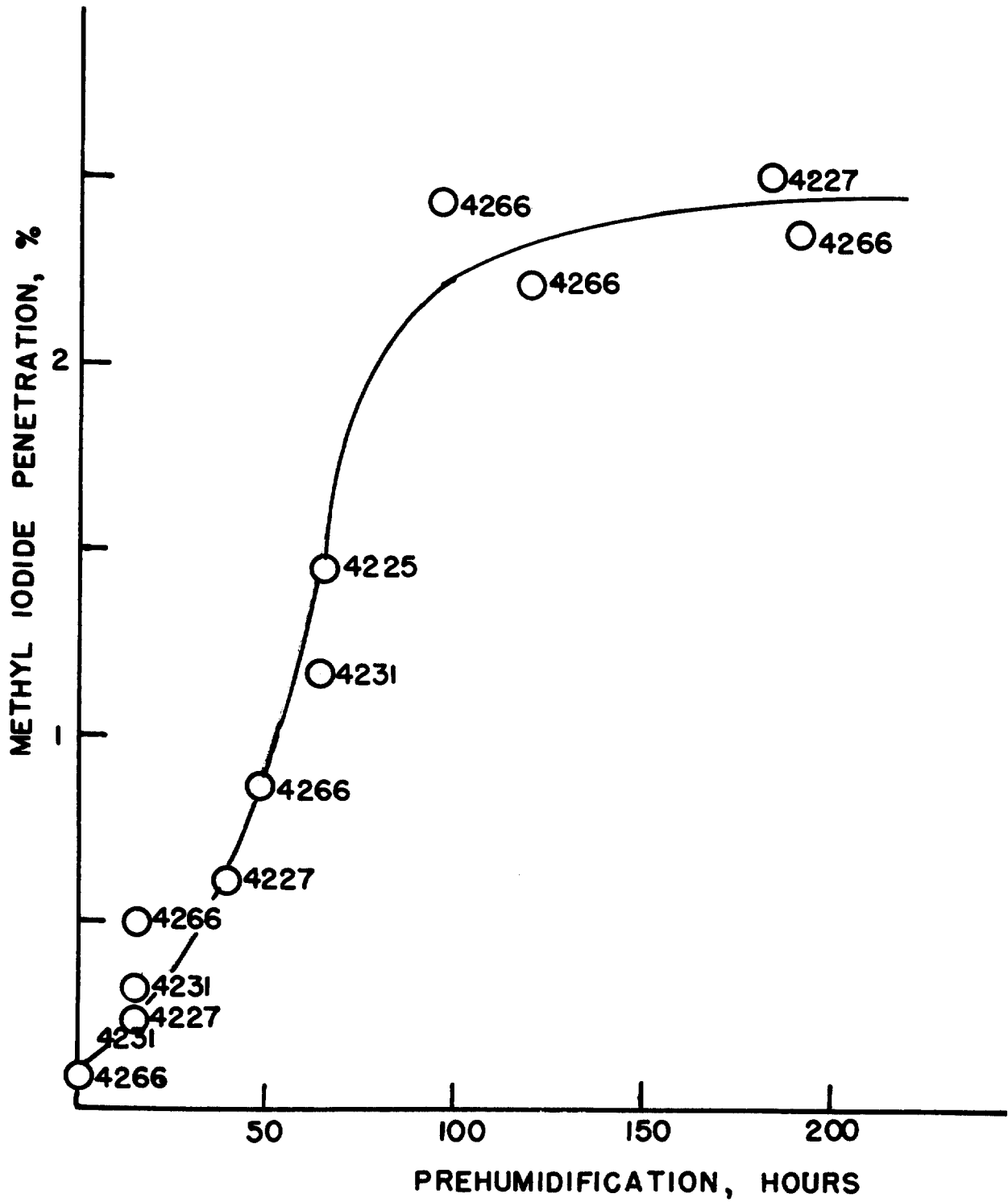


Figure 2: Penetration of methyl iodide as a function of prehumidification time using iodate-iodide impregnations on MBV (coal-base charcoal).

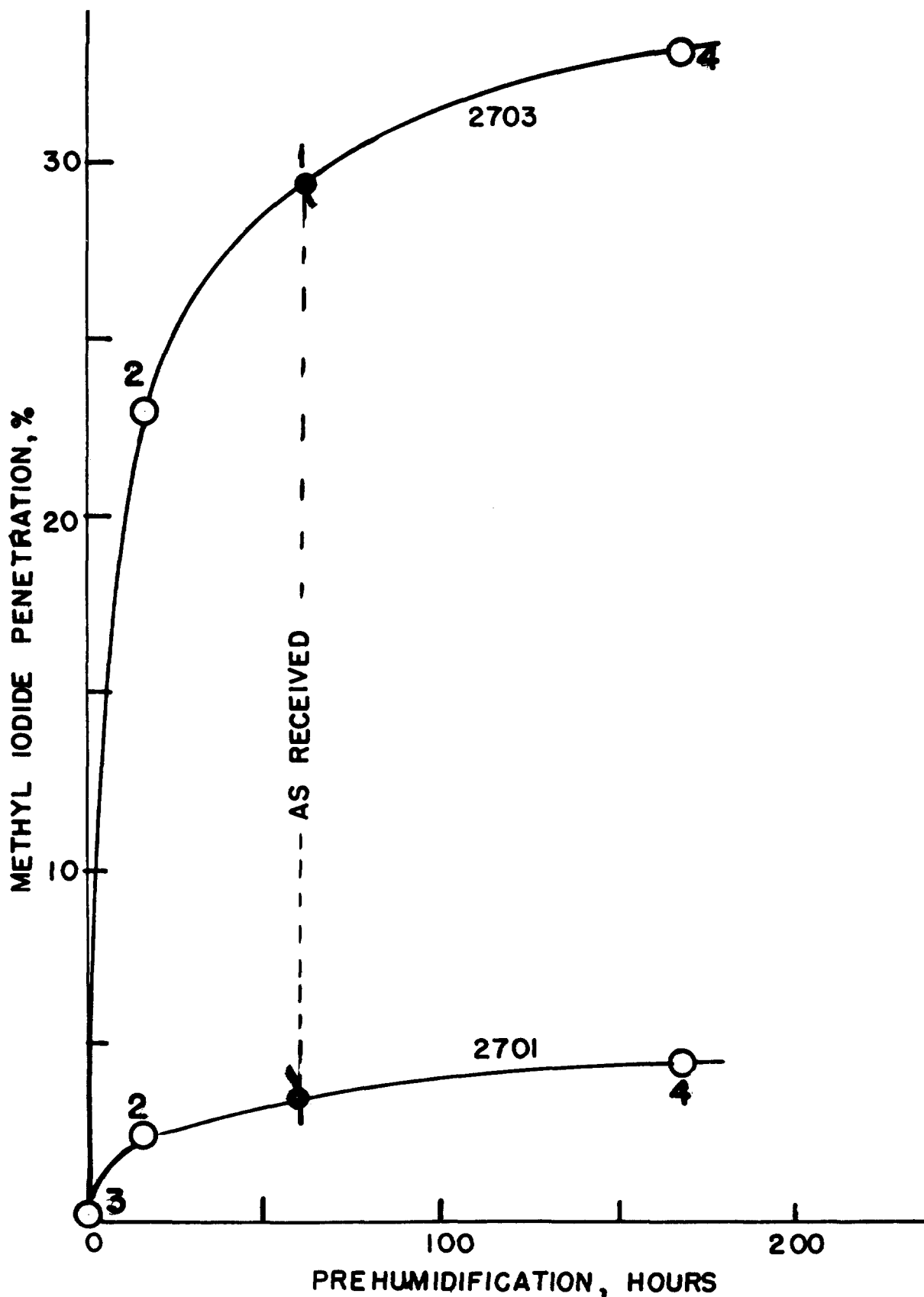


Figure 3: Penetration of methyl iodide as a function of prehumidification time for two KI_x impregnated charcoals.

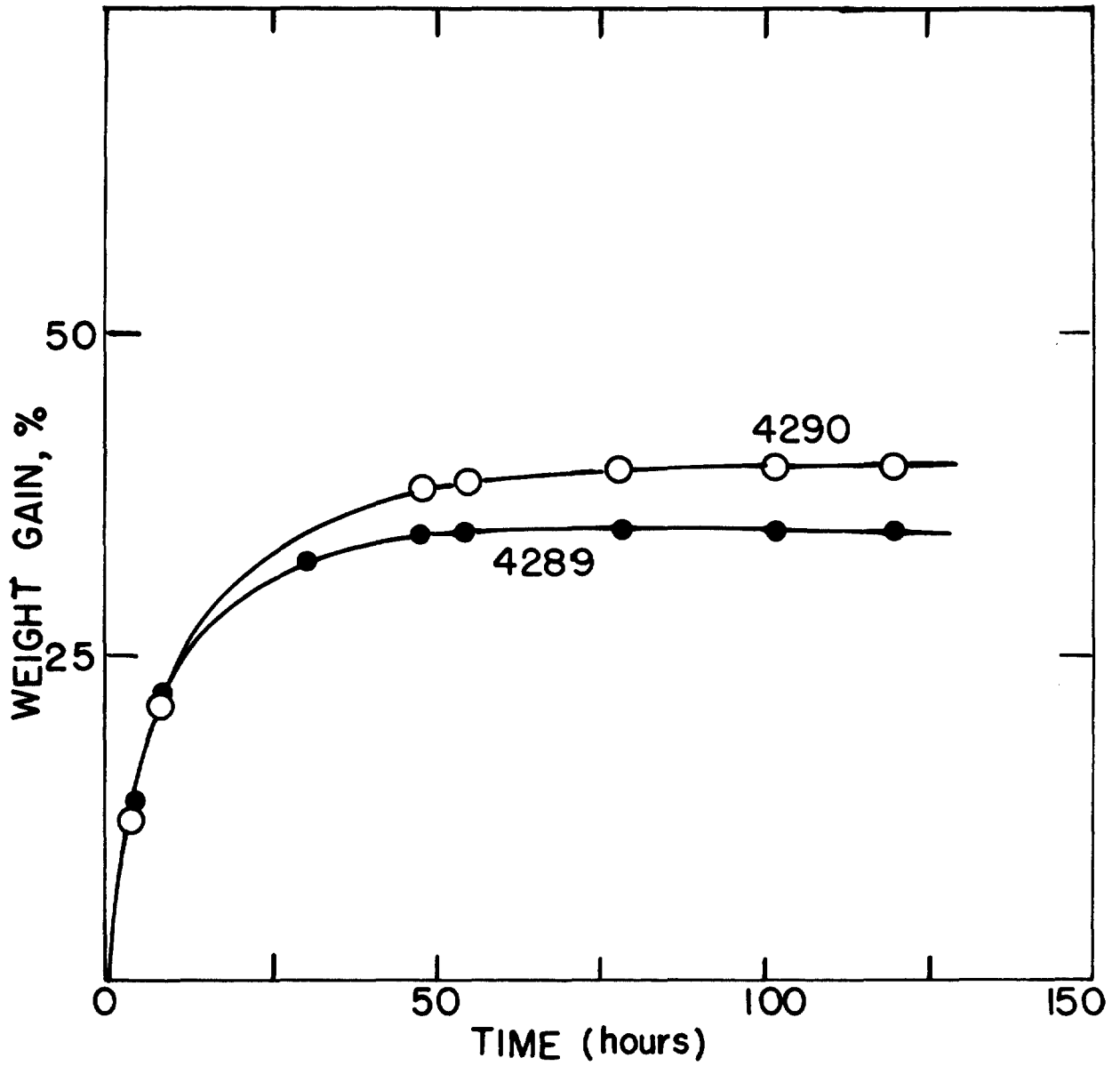


Figure 4: Weight-gain of two impregnated charcoals exposed continuously to humidified air (95-96% RH).

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DISCUSSION

FIRST: The MAST instrument is not specific for ozone. Would it not have been better to have used a chemiluminescence instrument that is specific for ozone? Also, I was wondering whether you should precondition the charcoal by heating it to 200°C for a period. Then you wouldn't have this phenomenon as the charcoal heated. In other words, is this a unique phenomenon for some volatile species that may be remaining in this charcoal and once you drive them off at a higher temperature, are permanently gone?

DEITZ: It is a fact that the MAST microcolombmeter responds to many oxidants. However, no signal was detected in the effluent gases from a thermal analysis of the charcoal up to ignition temperatures. Also, no iodine was emitted from crystals of potassium iodate and as previously mentioned, no emission from pre-heated crystals of potassium iodide.

A successful combination is KIO_4 plus KI; an impregnation containing this mixture does not require any preliminary treatment. The formation of KI is a desirable sink for radioiodine and it is very promising that these impregnations yield successful trapping of methyl iodide.

The iodine emission behavior ties very closely with Evans' desorption test at 180°C. We are also determining the emission at constant temperatures (160-250°C) and are finding linear curves for the emission of total iodine as a function of time at constant temperature.

KOVACH: Dr. Deitz, none of these charcoals is a real commercial charcoal. At least I've never run into any of the 2701 or any woodbase charcoal that is being used. So we don't really know if our good commercial carbons like the BC747 or the NSC585 fiber or the various carbons would behave the same way. These are all experimental charcoals.

DEITZ: We have used many commercial charcoals as impregnant carriers in the investigations now in progress.

KOVACH: They are commercial charcoals, but they are not being used in nuclear power stations, at least currently. The question I have is particularly related to the purity of the potassium iodine. I don't see the advantage of going to very high potassium iodide on charcoal that has up to 5% water soluble non-iodine. All sorts of junk, really, on the charcoal.

DEITZ: It is true that charcoals contain non-carbon constituents, but we are interested in particular in the emission of iodine after impregnation and whatever else is present does not appear to contribute significantly to the formation of gaseous iodine compounds.

KOVACH: At how high a temperature are you drying your experimental charcoals when you prepare them?

DEITZ: About 125°C.

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RIVERS: I assume the carbon labelled "2701" in your report was a sample of AAF 2701 which we sent you. That is not an experimental carbon; it is commercially available. 2703 is experimental. Could you elaborate on the notation "as received" on the slide?

DEITZ: The notation "as received" in Figure 3 corresponds to the methyl iodide penetration observed for the two samples (2701 and 2703) as received; the laboratory prehumidification was 16 hours. Both samples were then dried in a convection oven overnight at 105°C and immediately tested with no laboratory prehumidification; the penetration was very small (0.088 and 0.14%, resp.). Dried samples were then prehumidified for 16 hours and the penetration was 2.49 and 23.1%, respectively. Finally, the dried samples were prehumidified for 1 week (168 hours) and the penetration was 4.42 and 33.2%, respectively. We concluded that the samples as received had been exposed to water vapor for a period equivalent to about 60 hours of laboratory prehumidification.

RIVERS: I wonder what will be the effect on the overall penetration after going through the entire cycle under this exposure, e.g., after you heat it up to 230°C? Is this emission a significant mass or is it just a very tiny portion in spite of the fact that you can detect it so strikingly?

DEITZ: At present, we are using thermal analysis of impregnated charcoals as a measure of thermal stability. The charcoals are dosed with methyl iodide and immediately subjected to an analysis of the thermal desorption products. The order of magnitude of the products coming off at 180°C is about the same magnitude as Gary Evans finds in his desorption tests. The orders of magnitude of the iodine that we determine may be ascribed to the stability of the iodine complexes in the impregnated charcoals.

RIVERS: I was interested in the earlier one, coconut carbon 2701. Does it have a significant penetration when it's put through the cycle?

DEITZ: The results for methyl iodide penetration on 2701 and 2703 will be given in the next paper.

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IODINE EMISSION DURING A PROGRAMMED HEATING OF IMPREGNATED CHARCOALS IN CARRIER AIR

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INTRODUCTION

Unpublished results from this Laboratory have shown that there is considerable emission of carbon monoxide from impregnated charcoals heated (5°/min) in air flows at temperatures significantly below spontaneous ignition. This behavior raised questions as to the stability of the various iodide impregnations on charcoals when subjected to programmed heating in like manner; also, how would they compare to the stability of trapped iodine and/or methyl iodide on such charcoals? This Note is concerned with a thermal analysis of impregnated charcoals, reporting determinations of elementary iodine and organic iodides in air flows of 5 L/min.

PROCEDURE AND RESULTS

The effluent gases from the test procedure for the determination of the spontaneous ignition temperature (SIT) of an activated charcoal were sampled (126 ml/min) and the flow directed into a microcoulometric detector (MAST Instrument) (1,2). The charcoal was heated at 15°/min to 120°C and then at 5°/min until the SIT was attained. The MAST Instrument responds to elementary iodine and the output (mv) can be continuously recorded. A calibration was made against known vapor pressures of iodine crystals held at suitably low temperatures.

The emissions of molecular iodine are reported for three impregnated charcoals. The results for a KI_x-impregnated wood-base charcoal, 2701 (Figure 1), and for two (KIO₃+KI+KOH)-impregnated charcoals, 4289 and 4290 (3) (Figures 2 and 3), make an interesting comparison (see points in circles). The latter impregnations appeared to be more stable at a given temperature than the KI_x impregnation, a most desirable behavior with respect to DBA considerations. The small increase in I₂ emission (Figures 2 and 3) was followed by a marked decrease. This behavior raises interesting possibilities as to its significance in the nuclear application. Is the iodine changed to some thermally stable salt or is it converted to volatile organic compounds that are not counted by the microcoulometer?

To answer these questions, the effluent sample from the SIT test was passed first through a quartz tube (2 mm i.d.) heated to approximately 600-650°C and then directed into the microcoulometric detector (4). It is known that the alkyl iodides, such as methyl iodide, are pyrolyzed into elementary iodine and hydrocarbons (5). Hence, if volatile organic iodides are present in the effluent gases, the total iodine measured would be enhanced. The results (points in squares in Figures 2 and 3) demonstrate that this is indeed the case. Heating of impregnated charcoals must yield organic iodides. The KI_x impregnations are least stable, but these do not appear to form a significant amount of organic iodides. The new impregnations with oxyiodine

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salts (3) evolve less and, in addition, the emission after peaking at 250-260° decreases significantly until the actual SIT is approached.

CONCLUDING REMARKS

In the thermal analysis of some impregnated charcoals there are present chemical reactions that form volatile organic iodides at temperatures lower than ignition, thus raising the possibility of iodine release. The magnitudes of the SIT of nuclear grade charcoals appear to be of secondary importance.

The impregnations on charcoals of the oxyiodide-HMTA formulation are of greater stability than the present KI_x impregnations.

The total iodine emission integrated to a specified temperature (e.g. 180°C) is a small fraction ($\sim 10^{-6}$) of the total iodine in the impregnation; the emission varies somewhat with both charcoal and impregnation formulation for a specified temperature.

The experimental procedure is now being applied to follow the iodine emission at constant temperatures for time periods up to 24 hours. These results will complement the desorption tests at 180°C already reported by A. G. Evans (DP 1280, Savannah River Laboratory).

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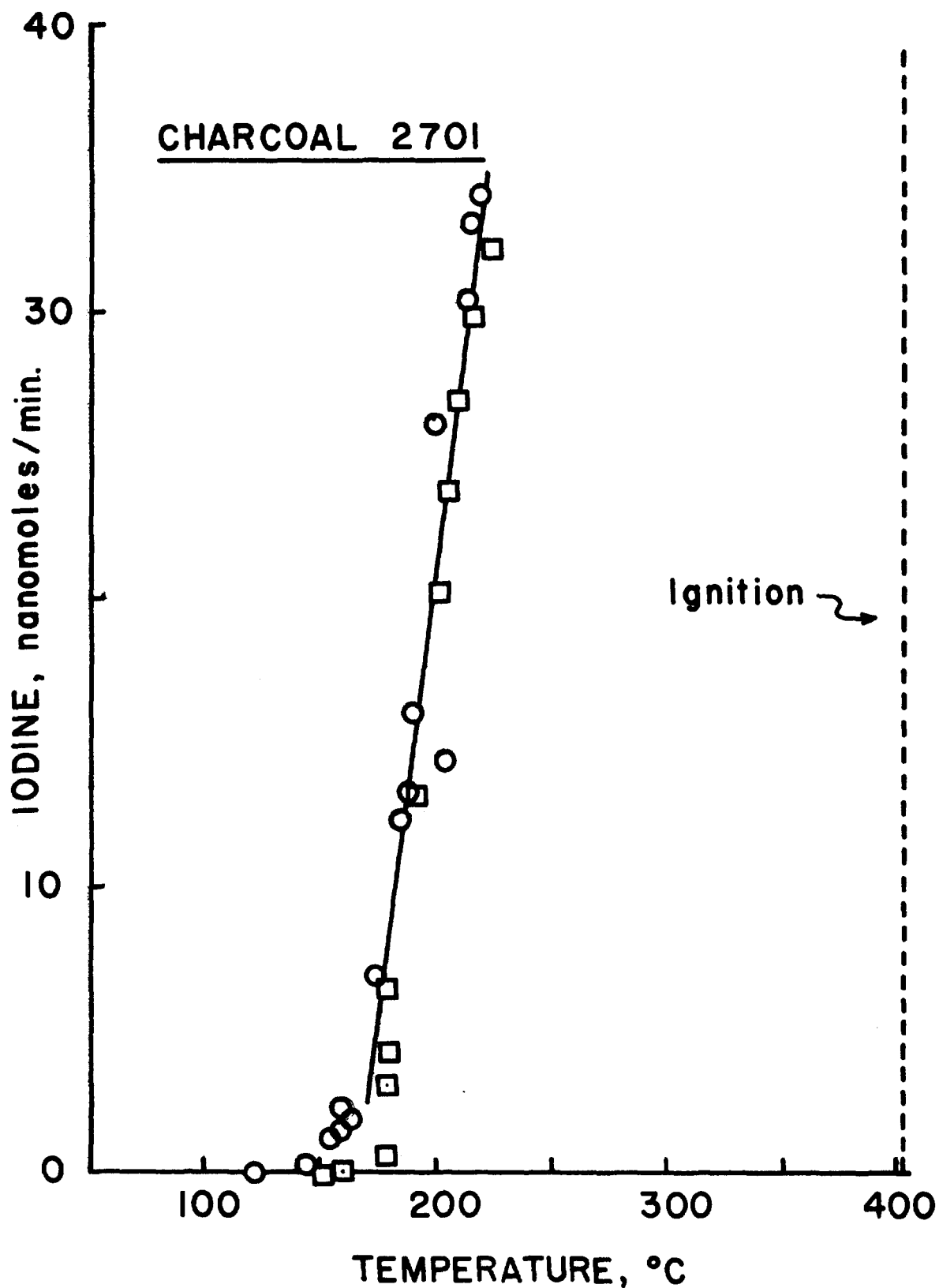


Figure 1: Emission of I₂ (points in circles) and of total iodine (points in squares) from KI_x impregnated charcoal

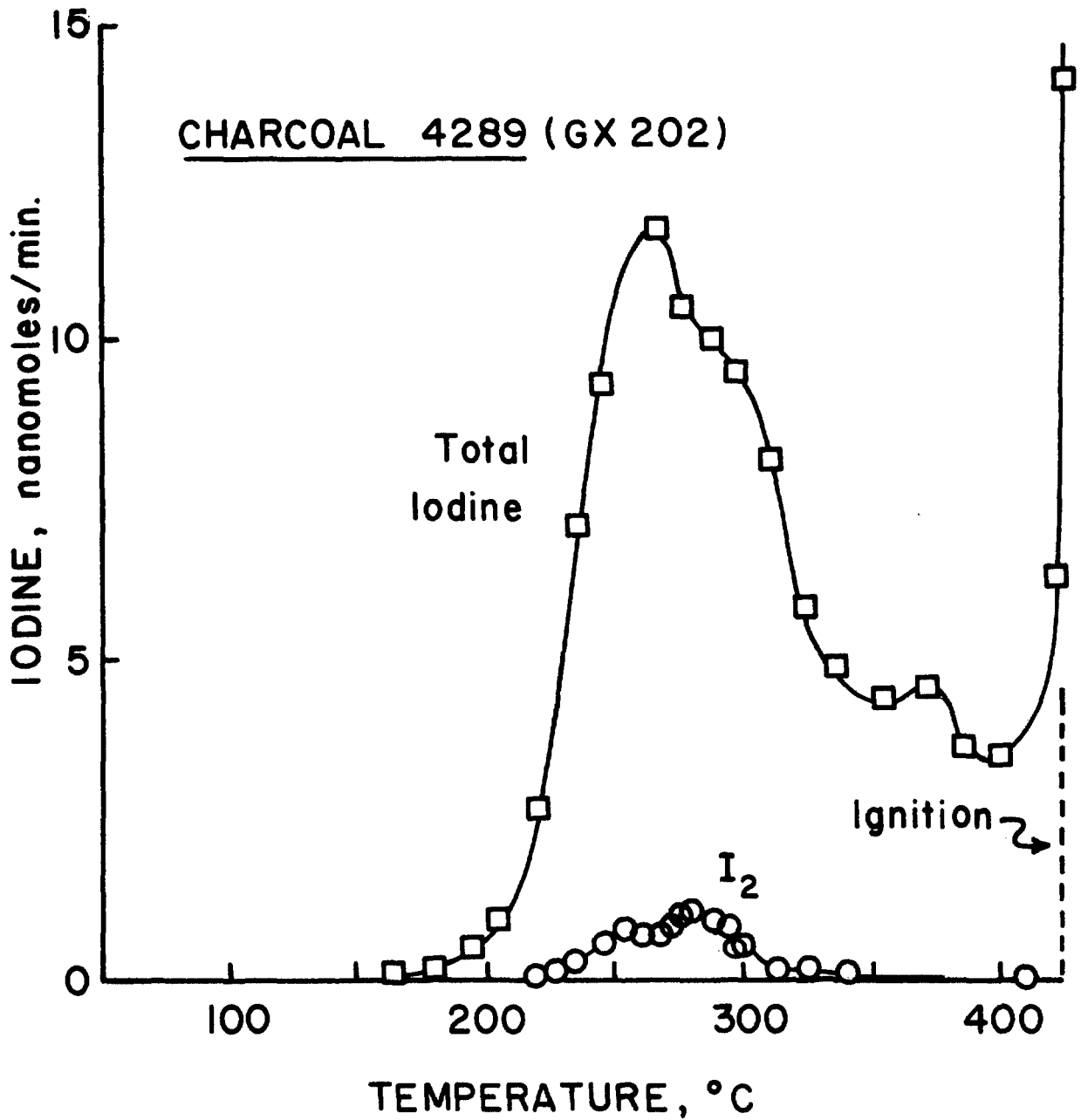


Figure 2: Emission of I₂ (points in circles) and of total iodine (points in squares) from a wood charcoal impregnated with KI+KIO₃+KOH+HMTA (3).

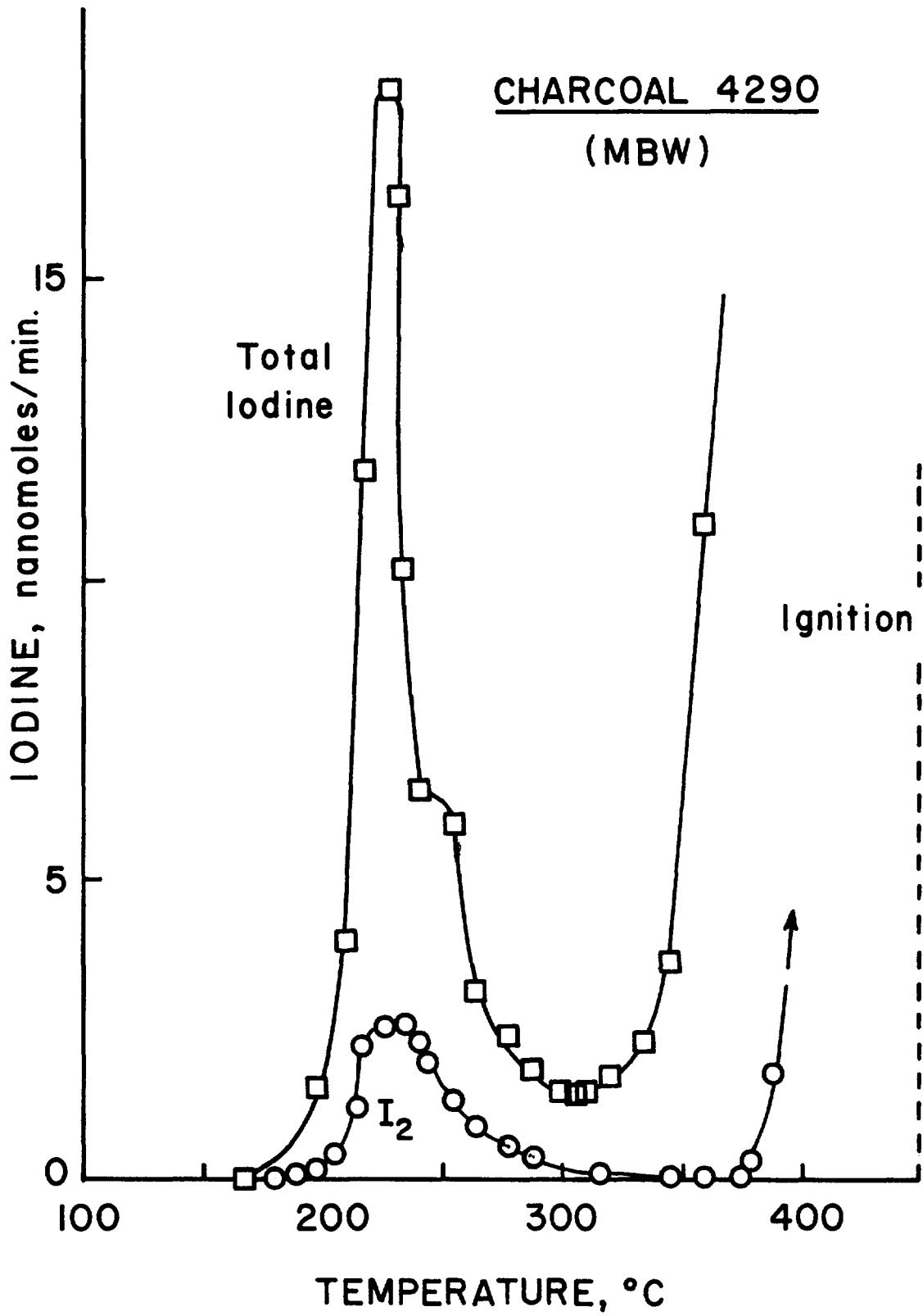


Figure 3: Emission of I₂ (points in circles) and total iodine (points in squares) for a coal base charcoal impregnated with KI+KIO₃+KOH+HMTA (3).

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DISCUSSION

RIVERS: The heating-elution curves shown are quite detailed, the result of some very sensitive measuring equipment. I hope everybody recognizes that the quantity of elution shown represents less than 0.01% penetration in the course of a test, even though the carbons were heated all the way to their ignition point. We reported some similar data at the 13th AEC Air Cleaning Conference. There is some evidence that a carbon with good physical adsorption properties (like coconut) will capture radioiodine well at lower temperatures, but release some of it on heating. A system with a lower initial adsorption, but strong chemical bonding of the iodine compounds, may have less overall penetration under conditions where the bed is subjected to decay heating. I was struck by the data in Staples paper in Session 4 which showed about 39% loss of adsorbed I_2 from the silver zeolite when it is heated to 150°C for substantial periods. Maybe we need a very careful look at the probable heating condition for these beds. Incidentally, would you expect the elution curve for 2701 to come back to the baseline before ignition, in the same manner as 4289 and 4290 samples did in Figures 2 and 3?

DEITZ: The iodine emission from 2701 (and 2703) does not return to the baseline before ignition. In fact, when viewed through a depth of about 10 cm, a purple cloud of iodine vapor is visible. The thermal stability of a KI_x impregnation on charcoal appears to be inferior to the samples 4289 and 4290 (Figures 2 and 3), the impregnations of which were basic solutions of $KIO_3 + KI +$ hexamethyltetra-amine.

FIRST: If you were to put dry air through your charcoal for 200 hours, would you return to your base reading?

DEITZ: Yes, but if you want to do it in less time, the charcoal can be placed in an oven. When the humidified charcoal is dehydrated, the initial low penetration is recovered. However, if the prehumidification is done at 95 and an attempt made to dry at 80 or so, the effort is analogous to trying to dry wet clothes on a wet day. Very little water would be removed from the charcoal. We are presently measuring the adsorption and desorption isotherms of water on impregnated charcoals in order to establish the extent of the hysteresis involved.

BELLAMY: I would like to make sure I understand this. All of the results you have just talked about are for new, unused carbons, so the effect cannot be compared to what Mr. Hunt talked about yesterday. Is that true?

DEITZ: Yes. As you know, our samples are now being aged by exposure to air flows drawn from the environment on the roof of the NRL Chemistry building.

BALLAMY: Can a committee just pick a standard prehumidification time and everybody run a test for the same number of hours?

DEITZ: We cannot solve this problem by committee work. It

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calls for some more experimental work. When the water layer builds on the surface of charcoal, a barrier is formed to the mass transfer of methyl iodide into the structure. More work must be done to understand the clustering of water molecules on the surface at high relative humidity and perhaps there are ways to minimize the effect.

KOVACH: What is the total air velocity that you are using in prehumidification? Is it the same as you use in the test?

DEITZ: The normal flow is twenty-five liters per minute but at times it is cut in half.

KOVACH: Have you looked at how clean the air is that you are using? Do you have any hydrocarbons? If it is compressed air, do you make sure you have absolutely no water vapor left from your compressors or any organic impurities?

DEITZ: The air supply of 25 liters per minute is passed through a bed of charcoal in a two inch diameter tube and then through a high efficiency particulate filter.

KOVACH: So you know for sure that you have no organic contaminant in the air stream. I am just trying to make sure. Is it water that causes the problem or are we facing other contaminants?

DEITZ: I believe that it is adsorbed water. When the charcoal is dried and the test started again, the same methyl iodide penetration is obtained after 16 hours. The behavior is quite reproducible.

MUNSON: I would just like to inquire if keeping the charcoal dry prevents this behavior, and I think you seem to say that. If the charcoal is continuously heated above the dew point, will the charcoal maintain its efficiency?

DEITZ: I don't know what is being done in industry. We would greatly appreciate some commercial information. Obviously, the cheapest procedure is to pull in outside air. When this is done, the increase in relative humidity at nightfall is an important factor.

WILHELM: Did you every try to get an x-ray picture of your charcoal? The charcoal is impregnated with potassium iodide and this forms a very small crystallite. If you run this charcoal in a wet atmosphere for a long time, the crystalite starts to grow. The surface area of the crystalites grow, and with this growth, the reaction on the surface becomes slower and slower because it has less surface. That could be a reason for what you have seen.

DEITZ: In reply to your first question, we have obtained quite a few electron transmission micrographs of charcoal, with and without impregnations. It is very interesting to see from the strong contrasting silhouettes where the impregnation is located. We will send you some photographs is you desire. The charcoal is amorphous material and the impregnations form minute crystals at random locations. These crystalites can best be studied with electron diffraction techniques. The electron diffraction results show the presence of single crystal patterns. These are present in the inner porosity of the charcoal.

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JONAS: Do you think one might distinguish between the amount of H_2O on the charcoal and the time of prehumidification, relative to its methyl radioiodide penetration, by prehumidifying the charcoal for 16, 25, 50 and 100 hours at a lower RH, e.g., 60 or 80% RH?

DEITZ: We are now prehumidifying samples at various relative humidities. Early researchers at NRL showed that dry charcoal and wet charcoal take up oxygen over periods of many days at a constant temperature in direct proportion to the moisture content. There is a very slow and continuous reaction of moist air with charcoal.

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AN EXPERIMENTAL INVESTIGATION OF THE RELATIONSHIP BETWEEN BED PACKING AND FLOW DISTRIBUTION

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Abstract

A testing program was conducted to evaluate different methods for filling carbon beds in terms of the resultant air flow distribution through the beds. Three filling methods were included:

- 1) Deposit carbon in the bed in an uncontrolled and random manner (with respect to fill rate and/or distribution) with no packing effort.
- 2) Uncontrolled deposition as in 1) above but followed by an attempt to pack by vibration.
- 3) Filled and packed simultaneously, using a gravity pack method.

The test results indicate that the filling method is a very significant factor in determining the quality of the resultant air flow distribution. A gravity packing method provided the best results while the uncontrolled fill with no effort to pack gave very poor results. The combination of an uncontrolled fill with subsequent packing by vibration also provided poor flow distribution.

I. Introduction

For all ESF (Engineered Safety Feature) Systems, Regulatory Guide 1.52 (Reference 1) requires that the flow distribution within carbon adsorber beds must not vary by more than $\pm 20\%$ from the average. For Non-ESF Systems, the NRC (Nuclear Regulatory Commission) has recently published ESTB No. 11-2 (Reference 2), which also requires a maximum flow deviation of $\pm 20\%$ from the average.

In addition, it is instructive to estimate the effect of velocity on performance. Some calculations have been performed using a method developed in CVI Technical Note No. 26 (Reference 3) to evaluate relative performance at various velocities. Penetration of CH_3I was estimated to increase by 70% to 90% for a 20% increase in velocity. A 50% increase in velocity could increase penetration by 200% to 300%, depending on the quality of the carbon. These increases were estimated assuming inter-particle diffusion to be the rate limiting step. Therefore, poor flow distribution can have a very significant effect on adsorber performance, due to increased penetration in the high velocity areas.

Another consideration is the weathering effect. The rate of weathering is related directly to velocity. Therefore, if the velocity is 50% higher through some area of the bed, then the adsorbent would be expected to weather 50% faster in that area, an important economic consideration.

Thus, uniform flow distribution is important because of regulations, performance and economic factors. Good design practice, therefore, demands that steps be taken to assure a uniform flow through the adsorber.

One of the most important design considerations, although frequently not given adequate consideration, is to insure that the flow resistance of the adsorbent be uniform throughout.

It is also sound engineering practice to make the flow resistance through the adsorber large relative to the inlet/outlet resistances. When this condition is met, any tendency to promote a non-uniform flow pattern by the inlet/outlet sections is overpowered by the larger flow resistance in the adsorber and the flow is thus smoothed out.

With these design principles in mind, it is significant to note that carbon will exhibit a 300% variation in flow resistance if the packing density is allowed to vary from minimum to maximum as indicated in Reference 4.

From previous testing reported in Reference 5, the packing density can be substantially affected by the packing method. It was found that a gravity pack method provides the maximum packing density while an uncontrolled fill generally produces the minimum packing density and an uncontrolled fill followed by mechanical shock (vibration) provides an intermediate packing density.

The effect of various packing methods on flow distribution was evaluated by measuring the flow distribution through a section of a full scale adsorber with the adsorber being filled and packed by various methods. This paper describes the test program and discusses the results.

II. Experimental Procedure

The test was conducted on a specially constructed element out of a full scale adsorber. Figure 1 is a sketch representing a typical all welded adsorber in the plan view. Figure 2 is a cross-section of the test apparatus in a side view. The test apparatus can be described as representing an element or slice taken from one full scale bed along with its associated one half inlet and one half outlet air plenum (see Figure 1). The test section was 6" in width (direction normal to the 2 dimensional flow plane) but all other dimensions are representative of the full scale system. The test unit was oriented vertically to facilitate filling of the carbon.

The test apparatus was considered to be a very realistic model of production assemblies because:

- 1) The depth of carbon in the test apparatus represented, for practical purposes, the minimum depth of carbon in full scale systems. The test section had a bed depth of 2-1/8 inches, the full scale systems vary from a minimum of 2-1/8 to 2-3/16 inches for nominal 2 inch beds.
- 2) Since the standard inlet and outlet plenums handle air from two carbon sections while the inlet and outlet plenums for the test assembly handled air to only one carbon section, the test assembly inlet/outlet plenums were one-half the width of their full scale system counterparts. Therefore, the air velocities were the same for the inlet and outlet plenums in the test assembly as in the full scale system.
- 3) The test assembly used a bed length of 7 feet, representing the longest bed that had been manufactured to that time by CVI. Thus, in all the significant dimensions, (i.e., in the plane of flow) the test assembly corresponded to a full scale system.

The test apparatus featured holes in the inlet and outlet air plenums at locations A2 thru A7 and B1 thru B6, as shown in Figure 2, through which a pitot tube or hot wire anemometer could be inserted to determine the air velocities in the inlet and outlet plenums. The faces of the unit (the outer sides of the inlet and outlet plenums - see Figure 2) were made of plexiglass so that the correct positioning of the pitot tube and hot wire anemometer could be visually confirmed. The holes were taped over except when in use and then the probes were "packed" around the hole to preclude air leakage.

Air flow was provided by a Spencer Turbine unit with a manually operated throttling valve and orifice flow meter to provide flow control. A schematic drawing of the test setup is illustrated in Figure 3.

It was intended that the flow be adjusted to provide an average superficial bed velocity of approximately 40 fpm for the first tests. However it was subsequently determined that the actual velocity was 35.4 fpm; thus, for consistency, the remaining tests were conducted at nominally the same flow rate of 35.4 fpm.

Test Procedure

Air velocities on the order of 40 fpm (standard face velocity in I_2/CH_3I removal systems) are very difficult to measure reliably with conventional equipment. This is even more true in the presence of a much higher velocity normal to the face velocity. Therefore, rather than attempting to measure the face velocity directly, the volumetric flow rate through various carbon sections was determined directly by changes in flow in the inlet and outlet plenums. The face velocity was then calculated directly from the volumetric flow rate and the face area for the various sections.

Therefore, the primary measurements taken in the tests were velocities in the air inlet and outlet plenums at measuring stations A2, A3. . . A7 and B1, B2. . . B6, as shown in Figure 2, measuring the velocities in the direction of the plenum axis.

Referring to Figure 2, the air enters the test unit at the bottom of the inlet plenum. As the inlet plenum is traversed from bottom to top, some of the air stream is continuously withdrawn from the inlet plenum and passes through the carbon bed to the outlet plenum.

Since the pressure gradient through the bed is much larger than the pressure gradient in the direction along the length of the inlet/outlet plenums, it follows that the air flow through the carbon is essentially normal to the flow in the plenum.

Each section of the test assembly must satisfy the continuity equation. The section in Figure 2 illustrates a typical flow balance based on the Section 2-3. Applying the continuity equation to the section illustrated in Figure 2 (changes in pressure are negligible in terms of density change so continuity is expressed in terms of volumetric flow rate).

$$\dot{V}_{B2} - \dot{V}_{B3} = \dot{V}_{A3} - \dot{V}_{A2} \quad (1)$$

where the symbol \dot{V} is used to indicate the volumetric flow rate.

Applying the continuity equation to the inlet plenum for the section shown in Figure 2:

$$\dot{V}_{2,3} = \dot{V}_{B2} - \dot{V}_{B3} \quad (2)$$

Summarizing, the approach used in the test program was to measure the plenum velocities, in the direction of the plenum axis, from which the volumetric flow rates ($\dot{V}_{B1}, \dot{V}_{B2}, \dots, \dot{V}_{B6}$ and $\dot{V}_{A2}, \dot{V}_{A3}, \dots, \dot{V}_{A7}$) were determined.

A velocity traverse was performed for several representative sections prior to the test from which a centerline coefficient was determined. Throughout the test, the centerline velocity was measured and the centerline coefficient was used to calculate the volumetric flow rate at that section.

III. Data Analysis

Having determined the flow rates in the air plenums at various stations, the change in flow rate between any two adjacent stations in the air plenum can be equated to the flow rate through the carbon between those two points (see Equation 2). Having determined the flow rate, the average superficial face velocity can be calculated for the area between the stations, using data from both the inlet and outlet plenums.

The plenum velocities at points A2 thru A7 and B1 thru B6 are entered as data into a computer which calculates the corresponding volumetric air flow rate at each of the measuring stations. The computer also sums the flow rate at each elevation, for example, $\dot{V}_2 = \dot{V}_{A2} + \dot{V}_{B2}$. Assuming the air flow through the carbon is perpendicular to the screen for the reasons given previously, the total plenum flow rate at any elevation will be constant and equal to the total flow rate into the unit. Thus, it would be expected that $\dot{V}_1 = \dot{V}_2 = \dot{V}_3 = \dot{V}_4 = \dot{V}_5 = \dot{V}_6 = \dot{V}_7$, although the measured values are certain to vary somewhat from station to station because of measurement error.

Since the calculation of the face velocities ($V_{1,2}, V_{2,3}$ thru $V_{6,7}$ in Figure 2) are determined from the flow differences $\dot{V}_{B2} - \dot{V}_{B1}, \dot{V}_{B3} - \dot{V}_{B2}$, etc., the calculated face velocities are very sensitive to small deviations in flow rate. In effect, the procedure used involves numerically evaluating the derivative of the flow (dV/dx) in the inlet or outlet plenum, this derivative being proportional to the corresponding face velocity through the carbon. It is well known that the derivative of a "non-smooth" function can be very erratic which underscores the need for smooth flow rate data in the inlet and outlet plenums.

Therefore, the measured flow rate data in the plenums were smoothed, subject to the condition that the total plenum air flow rate at any elevation was a constant to satisfy the principle of continuity. These steps were accomplished by first normalizing the data from the outlet plenum to that of the inlet plenum as follows. Prior to smoothing the data, the computer averages the indicated flows from the inlet and outlet plenums for each elevation to obtain the indicated total air flow rate at each station. These were then averaged to obtain the average total air flow rate.

The average total flow rate was then used as the flow rate at the inlet and outlet, i.e., at points A1 and B7. The average flow from all stations was used rather than the measured flow at A1 and B7 because the measured flows at these points were considered suspect due to inlet/outlet effects. The velocity traverses performed at both of these measuring stations confirmed this. These velocity traverses across the plenums generally indicated a flat velocity profile, typical for fully developed turbulent flow except at the sections A1 and B7. There was a steep velocity gradient from front to back at B7. Therefore, if the velocity probes were not placed exactly right, the measured velocity would be significantly off. Section A1 appeared to have a vena contracta effect to complicate measurement at that station.

The measured flow rate for the outlet plenum at each interior station is then subtracted from the average total air flow rate to obtain the corresponding flow rate for the inlet plenum. Thus, for each elevation (except A1 and B7), there are two data points representing the flow rate in the inlet plenum, one datum point being measured directly, the other calculated from the average total air flow rate and the measured flow rate at that elevation in the outlet plenum. These data were then subjected to a least squares curve fitting procedure to obtain a "best fit" curve which was then used in subsequent calculations to represent the best estimate for flow rate in the inlet plenum at any elevation.

Figure 4 is a reproduction of a typical work sheet to illustrate the above procedure (in this example the carbon was filled by pouring, i.e., loosely packed). The pitot tube data is handwritten at the top of Figure 4. The seven inlet and seven outlet plenum velocities are then entered into the computer which calculates and prints out the inlet and outlet plenum flow rates, the total flow rate for each station and the average total air flow rate. The flow rates in the outlet plenum were then subtracted from the average total air flow rate by hand and are shown handwritten next to the outlet flow rate column.

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The inlet plenum flow rate data used in the curve fitting procedure is plotted in Figure 5 for the example case with the measured flow rate data represented by the circle symbol and the flow rate data calculated from (the average total air flow rate less the outlet plenum flow rate) represented by the square symbol. The end points, corresponding to positions at 0 and 80 inches from the inlet are based on the average total air flow rate at the inlet and zero flow at the outlet, respectively, to satisfy the principle of continuity.

It can be seen from Figure 5 that the data measured in the inlet plenum and the data measured in the outlet plenum agree quite well.

The data, as tabulated in Figure 4 and plotted in Figure 5, were used to determine a best fit curve for a 2nd order equation, using the method of least squares. The end points at 0 and 80 inches from the inlet were weighted to force the curve to go through those points. The resultant best fit curve is shown in Figure 5. From an examination of Figure 5, the data points look reasonably consistent and are fitted well by the 2nd order equation. It should be noted that mathematically, by assuming a 2nd order equation, the face velocity is forced to vary linearly from one end to the other. This can be seen in the data of Figure 6 thru Figure 15. The reason for this is that the face velocity is determined, in effect, by differentiating the flow rate along the length of the plenum as:

$$V = \text{Constant} \cdot \left(\frac{\Delta \dot{V}}{\Delta x} \right) = \text{Constant} \cdot \left(\frac{d\dot{V}}{dx} \right) \quad (3)$$

where

V = average face velocity for carbon over increment Δx

$\Delta \dot{V}$ = incremental change in volumetric flow rate between stations

Δx = incremental distance between stations

Since the flow rate data is fit by a second order equation:

$$\dot{V} = Ax^2 + Bx + C \quad (4)$$

and

$$V = \text{Constant} \cdot \left(\frac{d\dot{V}}{dx} \right)$$

then

$$V = \text{Constant} \cdot (2Ax + B) \quad (5)$$

Therefore, V is forced to be a linear function of x (distance). If a 2nd order equation did not fit the data so well (for example, see Figure 5), then a higher order equation should be used. However, the curve fits were all sufficiently good that it was decided that a higher order curve fit would not be justified and would not be statistically meaningful based on the number of data points. It might also be noted that if the flow data of Figure 5 formed a straight line, the constant "A" in Equation 5 would be zero and the velocity would be constant across the bed. The data in Figure 5 obviously could not be considered to fit a straight line and therefore, by inspection, it could be concluded that the face velocity is non-uniform.

The "best fit" curve from Figure 5 is then taken to represent the true flow distribution for the inlet plenum and the quantity (average total air flow minus inlet plenum flow) the flow distribution for the outlet plenum. The flow distribution for the inlet plenum can then be used to calculate the face velocity profile using Equation 5.

The face velocity profile based on the data from Figure 5 is plotted in Figure 6 pictorially as velocity arrows at various elevations.

For this test, featuring a loosely packed bed, it can be seen that the face velocity is far from constant, varying from a minimum of 19.5 fpm to a maximum of 57.1 fpm.

Results

The results of the tests are presented in Figures 6 thru 15. The test apparatus is represented schematically along the ordinate in each of the figures with the measuring stations shown by number. The numbering system is consistent with that used in Figure 2 with Station No. 1 at the inlet, Station No. 7 at the outlet and with Stations No. 2 thru No. 6 approximately evenly spaced between. The average velocity between measuring stations is shown as an arrow at the midpoint between stations. The magnitude of the velocity is read from the abscissa.

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Each of the figures includes pertinent test information including the Test Number, a description of the fill technique, percent maximum packing density and measured flow resistance.

The maximum packing density is based on the highest measured density from all in-house tests on a given carbon. The static pressure differential was measured at each of the five interior stations (2 thru 6) to determine the average flow resistance for each fill of the test bed except for Test No. 26.

Data taken on carbon filled by pouring, without control over distribution or rate of fill, is plotted in Figures 6 thru 8. The result was a series of loosely packed beds, as evidenced by the measured percent of maximum packing density and the measured flow resistance. It is apparent that the resultant face velocities are very non-uniform.

Test No. 26 (Figure 9) used the same uncontrolled pouring method. However, after pouring, an electric vibrator was attached to the side of the test assembly and energized. Operation of the vibrator continued until the carbon quit settling (surface of carbon dropped about 3" during vibration). Additional carbon was added to bring the level up to normal height. The apparent density increased from 28.0 to 29.7 lbm/ft³ with vibration or from 85.4% to 90.5% of maximum-packing density. It is apparent from Figure 9 that the velocity is still very non-uniform, varying from 17.8 fpm to 51.5 fpm. Given a cursory consideration, it might be expected that vibration should improve the flow distribution somewhat over the data from Figures 6 thru 8 since there was some increase in packing density. However, the carbon used in Test No. 26 had an abnormally large mesh size which would have the effect of decreasing the flow resistance. Also, variation in packing density within a bed is another mechanism which can create flow channeling. The characteristic response of any non-rigid mechanical system to vibrational excitation is the formation of several nodes and antinodes (i.e., minimal and maximum displacements). It would be expected that the packing density would vary accordingly from one location to another, being maximum at the antinodes and minimum at the nodes. Thus, it is considered likely that vibration could very well make any existing flow channeling worse, even though the average packing density and flow resistance are increased. For comparison, the same large mesh carbon was used with a gravity pack technique. The corresponding data are plotted in Figure 14 and 15 and will be described later.

Data on gravity packed carbon beds are plotted in Figures 10 thru 15. The data in Figures 10, 11, 12, and 13 show high average flow resistances with a $\Delta P/L$ of 0.420 to 0.458 in. w.g./in. of bed depth compared to 0.171 to 0.192 in. w.g./in. for the uncontrolled, poured beds of Figures 6 thru 8. Also, the measured packing density was 100% of maximum compared to the 86.5% for the loosely packed beds. Most significant, the corresponding flow distribution was much improved, particularly the data in Figures 11, 12, and 13.

A closer examination of the data represented in Figure 10 indicates that these data are not as reliable as data taken in the other runs. The data from Run No. 6 and Run No. 7A (Figure 10 and 11) were taken on the same packed bed (i.e., same fill) under the same conditions and at the same flow rate so that the velocity distribution should be the same. Yet the flow from Run No. 7A (Figure 11) is much more uniform. In examining the plenum flow data of Run No. 6, the data are more scattered than for any of the other runs. The plenum flow data from Run No. 6 and Run No. 7A are shown for comparison in Figure No. 16 and Figure No. 17, respectively. By inspection, the plenum flow data of both Figures 16 and 17, could be well represented by a straight line, which would mean a uniform face velocity. For contrast, refer to Figure 5 where the plenum flow is decidedly non-linear (i.e., the face velocity is non-uniform).

An index of the scatter is the error of fit (i.e., the square root of the sum of the differences squared) which is computed and printed along with the other curve fitting data. The error of fit for the data in Figures 10 thru 13 is summarized in Table 1.

Table 1

Figure No.	Error of Fit
10	6.98
11	4.77
12	4.80
13	4.36

Therefore, it is reasonable to assume that the actual flow distribution from that particular fill is more nearly represented by the data of Figure 11 than by the data of Figure 10.

The data represented in Figures 14 and 15 were taken on gravity packed beds but with an abnormally large mesh size carbon. This is the same carbon for which data were plotted in Figure 9 (Test No. 26). In

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comparing the data for gravity packed beds with the data on a bed packed by vibration, it can be seen that the gravity packed bed provided a much better flow distribution.

V. Summary

Proper packing of carbon beds is an important factor in providing good flow distribution which, in turn, is a prerequisite to the following:

- 1) Conformance to Standards and Regulations
- 2) Minimizing weathering effects and premature carbon replacement with attendant economic impact.
- 3) Good adsorber performance

Proper packing requires both a uniform packing density throughout the system and a near maximum packing density for the given material.

The primary purpose of the effort described in this paper was to evaluate the effect of different methods for filling carbon beds on the resultant air flow distribution through the beds. The results of this testing program indicate that the gravity packing method provides a more uniform flow distribution than either no packing or packing by vibration.

This result is not surprising considering that, properly done, a gravity packing technique produces the maximum packing density and provides an inherently uniform packing density. On the other hand, it is considered improbable that a system of any size could be vibrated uniformly throughout, suggesting very strongly that the packing density would also be non-uniform.

References

1. USAEC Regulatory Guide 1.52, June 1973
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3. Parish, H., "CVI Technical Note No. 26", November 9, 1972
4. Pittsburgh Activated Carbon Specification Sheet 23-108, Type PCB Granular Carbon
5. Parish, H., "CVI Technical Note No. 30", November 6, 1975

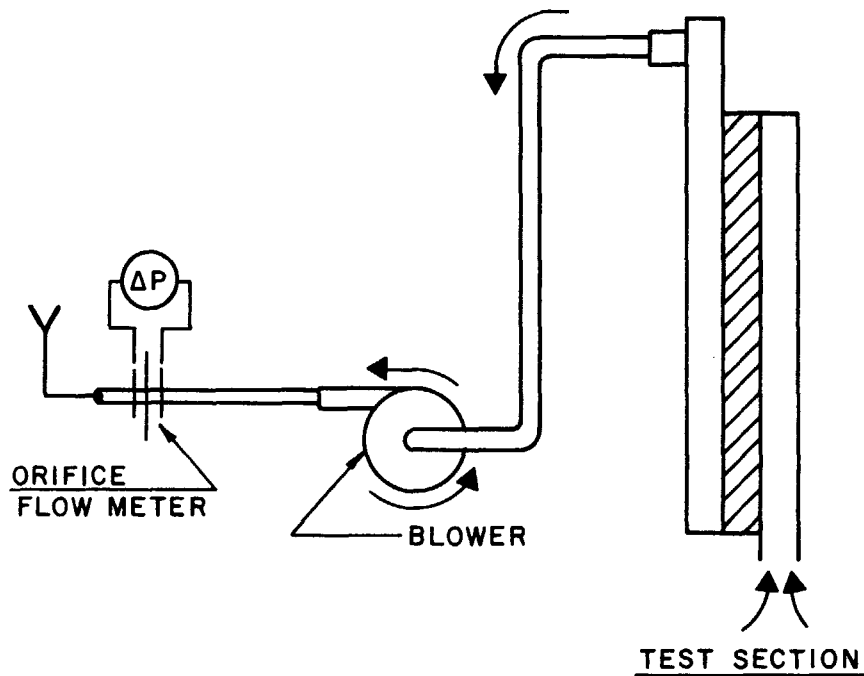


Figure 3. Flow schematic for test setup.

STATION	P _{IN} (IN.W.G.)	V _{IN} (FPS)	P _{OUT} (IN.W.G.)	V _{OUT} (FPS)
1	0.310	36.66	0	0
2	0.186	28.82	.003	3.66
3	0.160	26.81	.009	6.34
4	0.120	23.15	.035	12.50
5	0.046	14.33	.073	18.05
6	0.007	5.59	.155	26.31
7	0	0	.260	34.07

ENTER SEVEN INLET VELOCITIES
 ? 36.66,28.82,26.81,23.15,14.33,5.59,0
 ENTER SEVEN OUTLET VELOCITIES
 ? 0,3.66,6.34,12.5,18.05,26.31,34.07

STATION	INLET FLOW RATE (CFM)	OUTLET FLOW RATE (CFM)	TOTAL FLOW RATE (CFM)
1	137.5	0.0 - 126.6	137.5
2	108.1	13.7 - 112.9	121.8
3	100.5	23.8 - 102.8	124.3
4	86.8	46.9 - 79.7	133.7
5	53.7	67.7 - 58.9	121.4
6	21.0	98.7 - 27.9	119.6
7	0.0	127.8 - 0.0	127.8

AVERAGE FLOW RATE (BASED ON FLOW RATE) = 126.6 CFM

PRINT BED FLOW RATES AND VELOCITIES ? (1 - YES, 2 - NO)

? 2

CONTINUE ? (1 - YES, 2 - NO)

? 2

Figure 4. Sample work sheet indicating inlet, outlet, and total plenum flow rates.

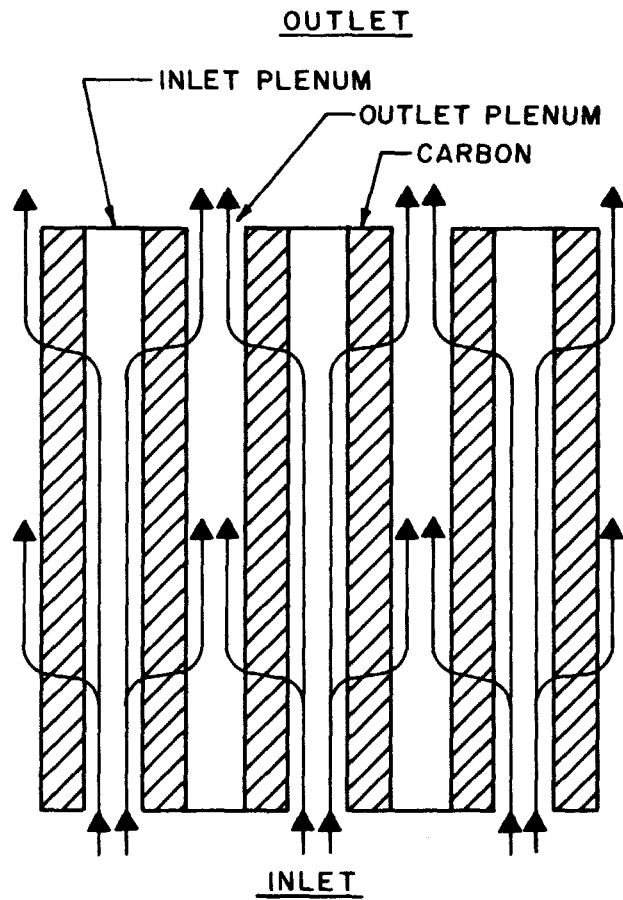


Figure 1. Cross section of typical all-welded adsorber in plan view.

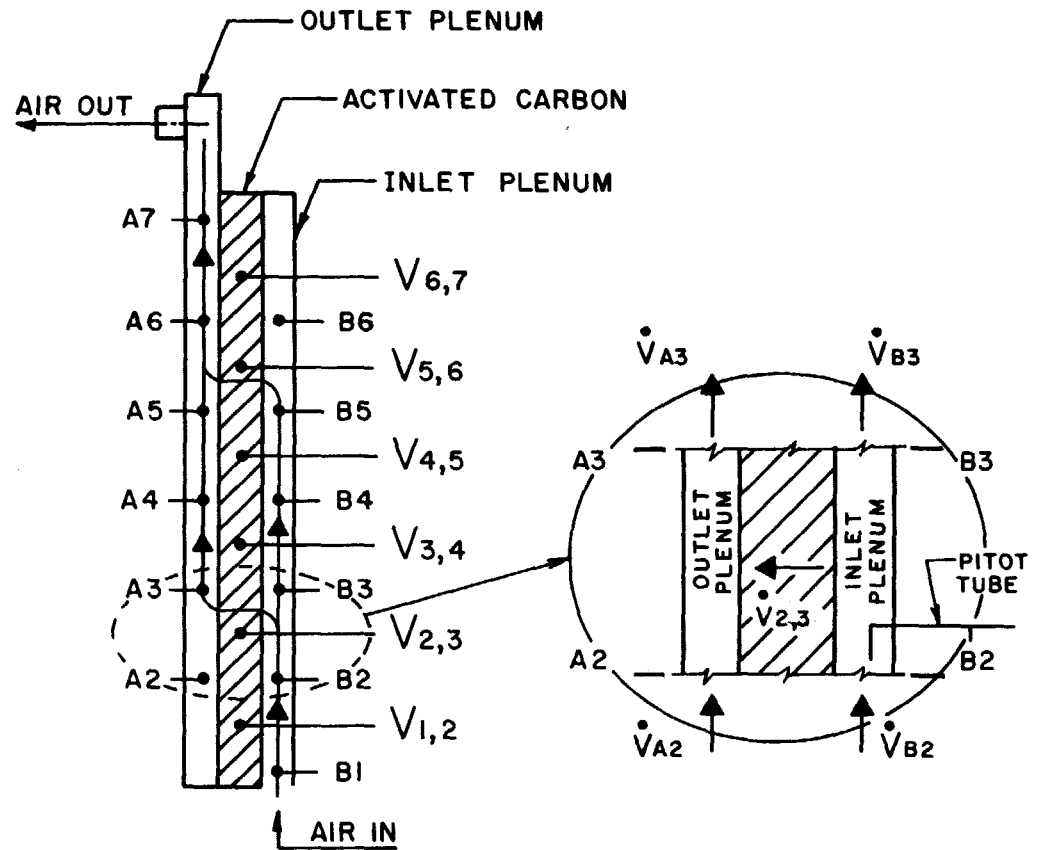


Figure 2. Cross section of apparatus in side view.

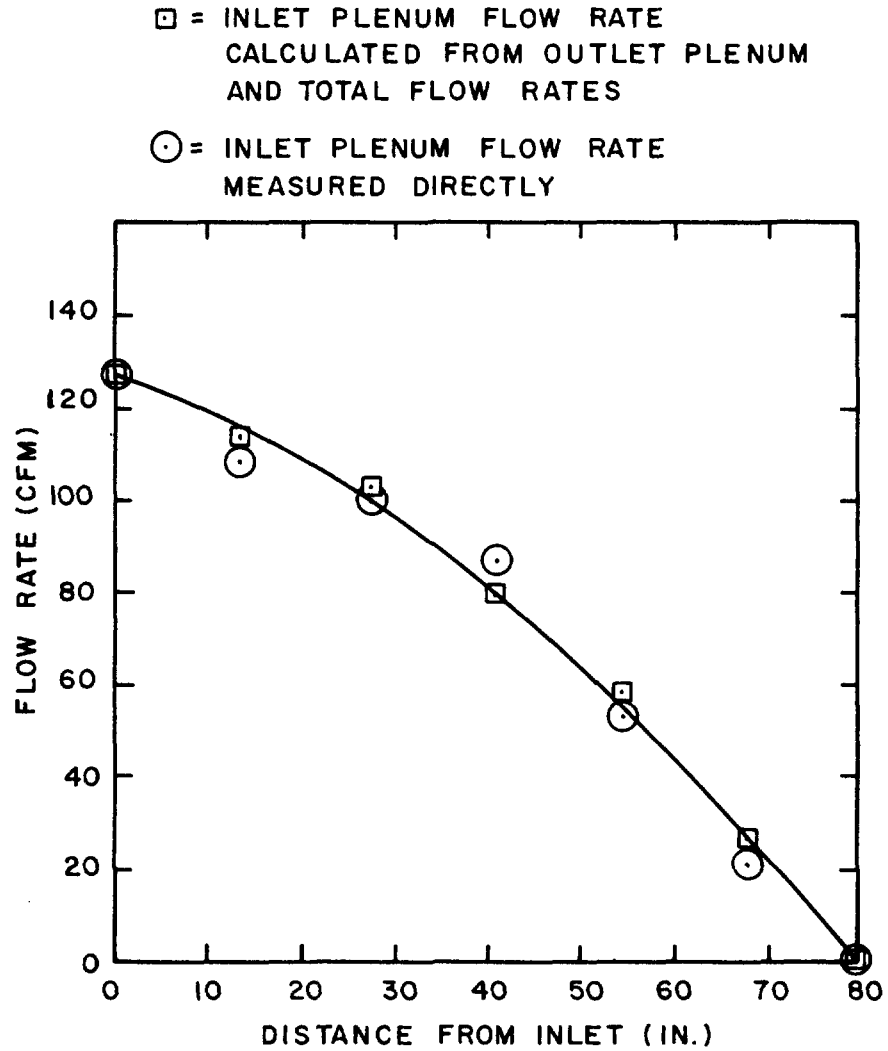


Figure 5. Sample indicated plenum flow rate and best fit curve for Test No. 3A.

TEST DATA

- 1) TEST NO. - 3A
- 2) FILL TECHNIQUE - POURED (LOOSE)
- 3) % OF MAXIMUM PACKING DENSITY - NOT MEASURED
- 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = 0.192 IN·W·G/IN

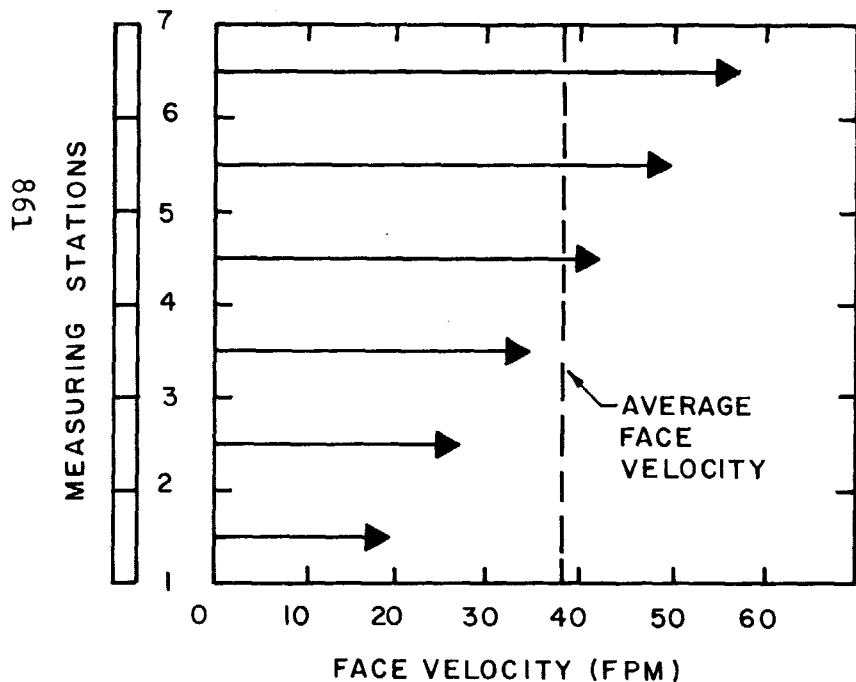


Figure 6. Face velocity distribution - Test No. 3A.

TEST DATA

- 1) TEST NO. - 9
- 2) FILL TECHNIQUE - POURED (LOOSE)
- 3) % OF MAXIMUM PACKING DENSITY - 86.5 %
- 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = 0.171 IN·W·G/IN

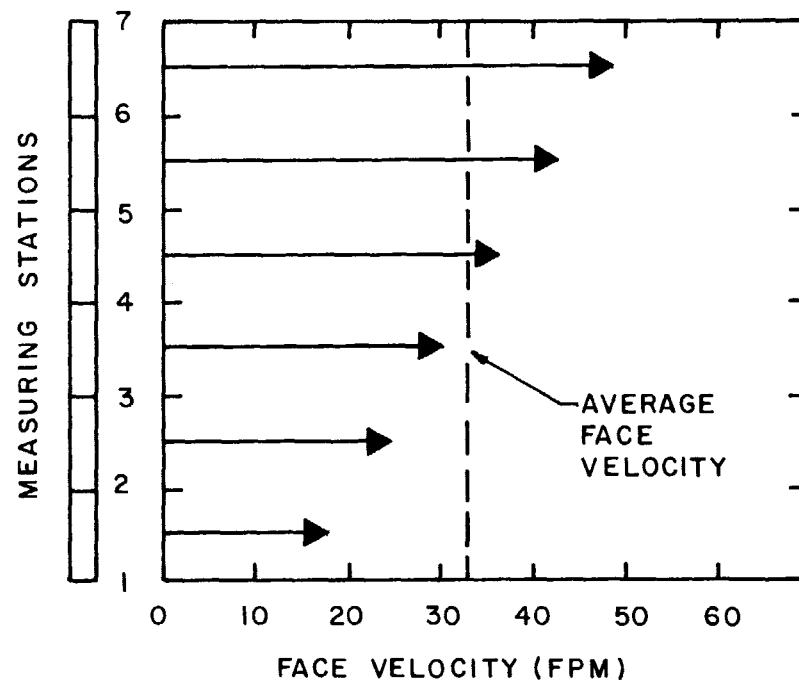


Figure 7. Face velocity distribution - Test No. 9.

TEST DATA

- 1) TEST NO. - 11
- 2) FILL TECHNIQUE - POURED (LOOSE)
- 3) % OF MAXIMUM PACKING DENSITY - 86.5 %
- 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = 0.171 IN-W·G/IN

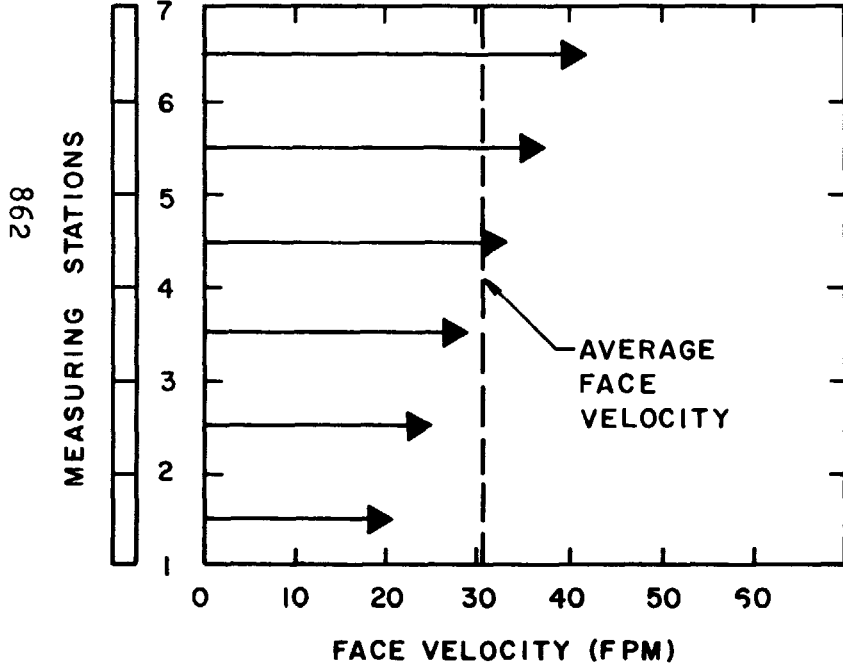


Figure 8. Face velocity distribution - Test No. 11.

TEST DATA

- 1) TEST NO. - 26
 - 2) FILL TECHNIQUE - POURED & VIBRATED
 - 3) % OF MAXIMUM PACKING DENSITY - 90.5 %
 - 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = NOT MEASURED
- NOTE: THIS DATA TAKEN ON BC-727 WITH AN ABNORMALLY LARGE MESH SIZE

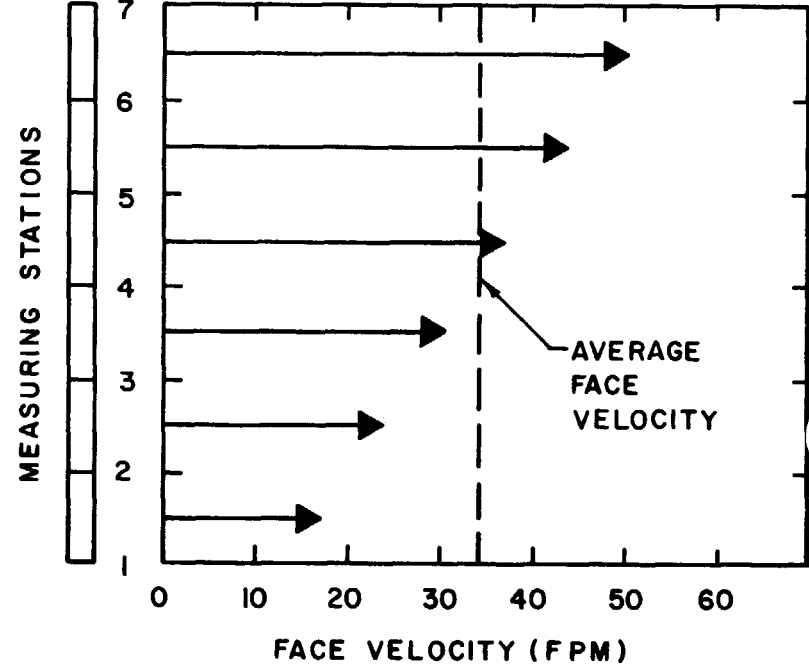


Figure 9. Face velocity distribution - Test No. 26.

TEST DATA

- 1) TEST NO. - 6
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - NOT MEASURED
- 4) AVERAGE FLOW RESISTANCE ($\Delta P/L$ AT 35.4 FPM) = 0.420 IN·W·G/IN

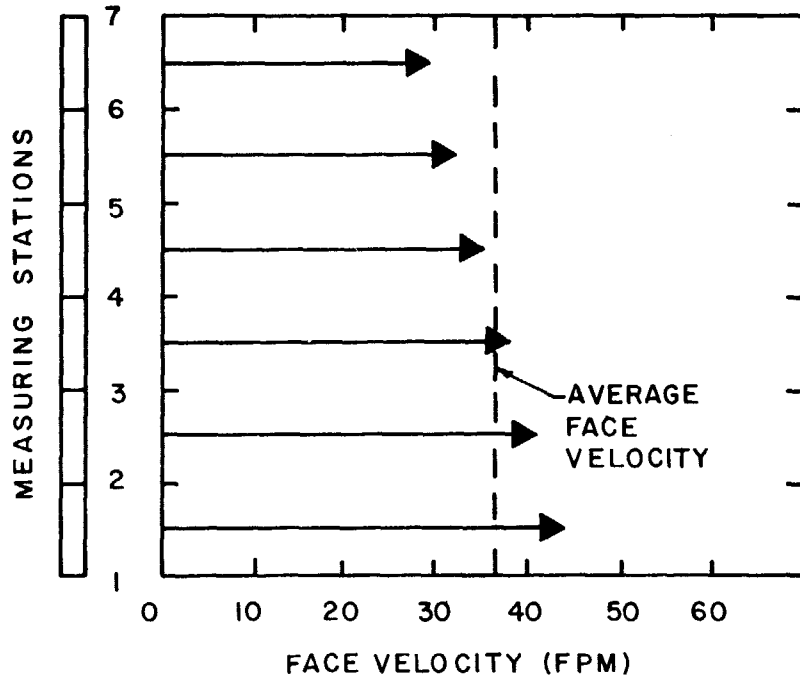


Figure 10. Face velocity distribution - Test No. 6.

TEST DATA

- 1) TEST NO. - 7A
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - NOT MEASURED
- 4) AVERAGE FLOW RESISTANCE ($\Delta P/L$ AT 35.4 FPM) = 0.420 IN·W·G/IN

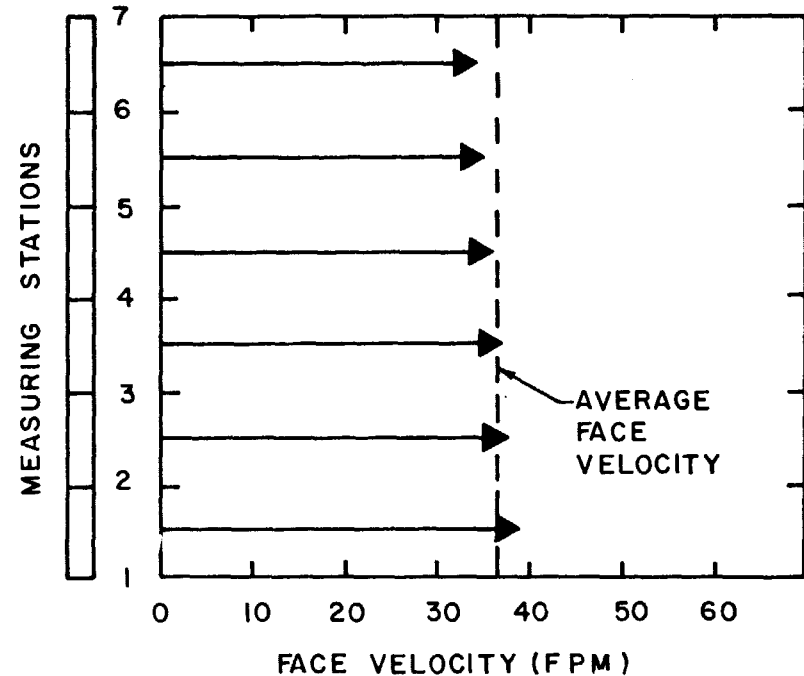


Figure 11. Face velocity distribution - Test No. 7A.

TEST DATA

- 1) TEST NO. - 14
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - 100 %
- 4) AVERAGE FLOW RESISTANCE ($\Delta P/L$ AT 35.4 FPM) = 0.458 IN·W·G/IN

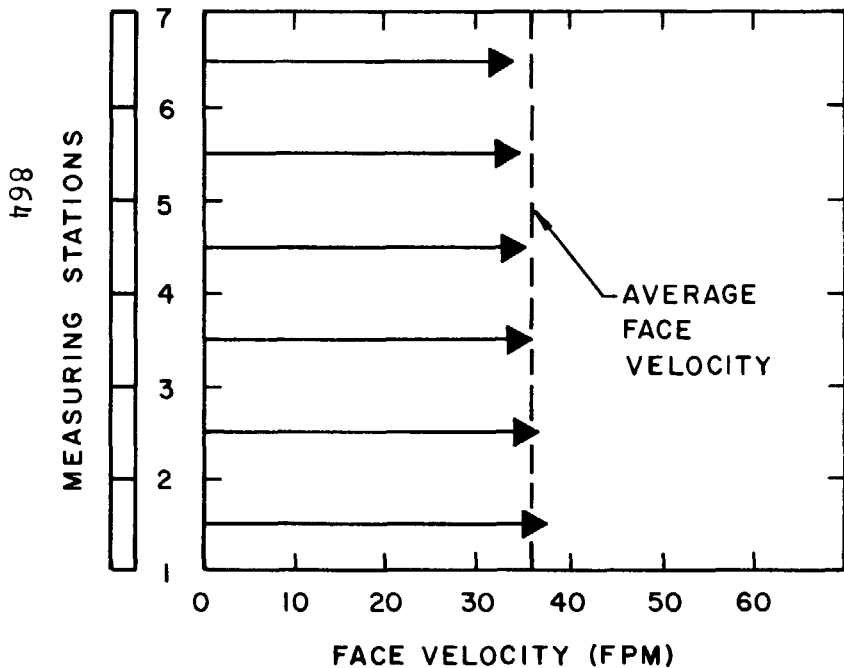


Figure 12. Face velocity distribution - Test No. 14.

TEST DATA

- 1) TEST NO. - 15
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - 100 %
- 4) AVERAGE FLOW RESISTANCE ($\Delta P/L$ AT 35.4 FPM) = 0.458 IN·W·G/IN

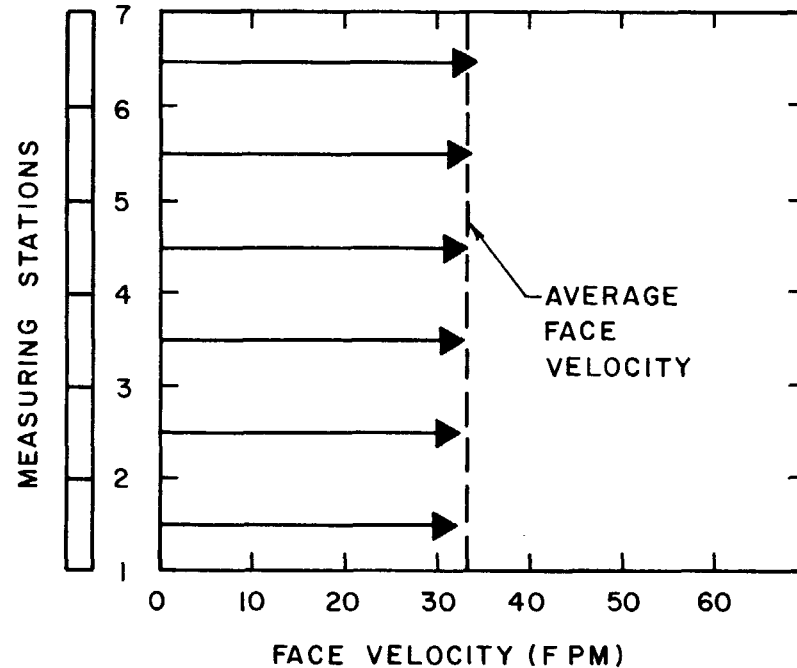


Figure 13. Face velocity distribution - Test No. 15.

TEST DATA

- 1) TEST NO. - 23
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - 100 %
- 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = 0.298 IN·W·G/IN

NOTE: THIS DATA TAKEN ON NEW BC-727 WITH AN ABNORMALLY LARGE MESH SIZE

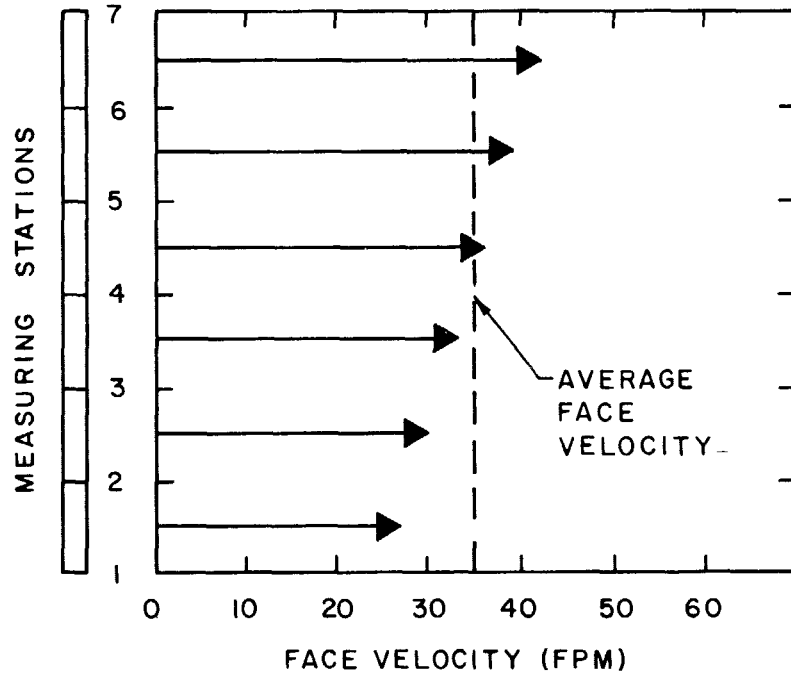


Figure 14. Face velocity distribution – Test No. 23.

TEST DATA

- 1) TEST NO. - 24
- 2) FILL TECHNIQUE - GRAVITY PACKED (DENSE)
- 3) % OF MAXIMUM PACKING DENSITY - 100 %
- 4) AVERAGE FLOW RESISTANCE
($\Delta P/L$ AT 35.4 FPM) = 0.298 IN·W·G/IN

NOTE: THIS DATA TAKEN ON NEW BC-727 WITH AN ABNORMALLY LARGE MESH SIZE

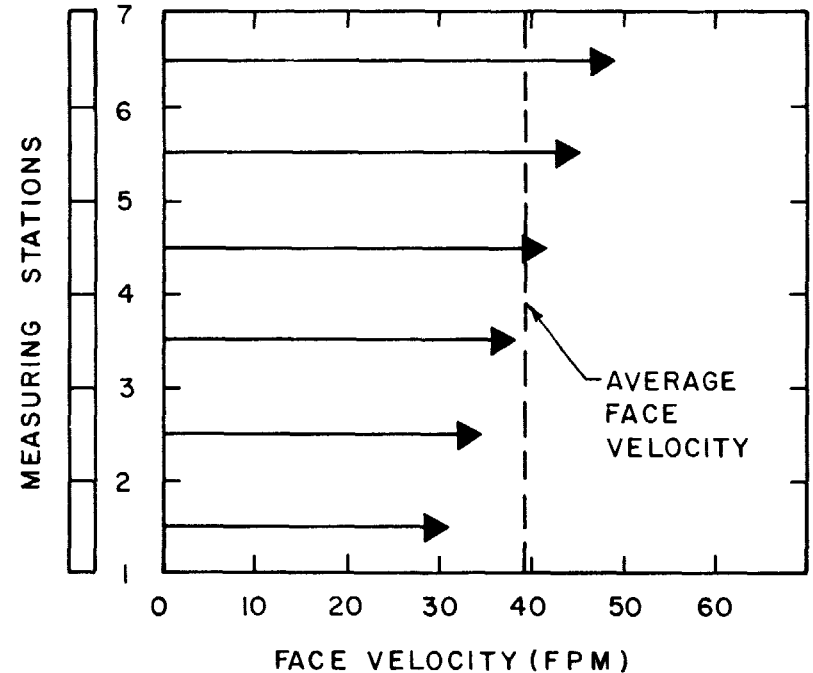


Figure 15. Face velocity distribution – Test No. 24.

□ = INLET PLENUM FLOW RATE
CALCULATED FROM OUTLET PLENUM
AND TOTAL FLOW RATES

○ = INLET PLENUM FLOW RATE
MEASURED DIRECTLY

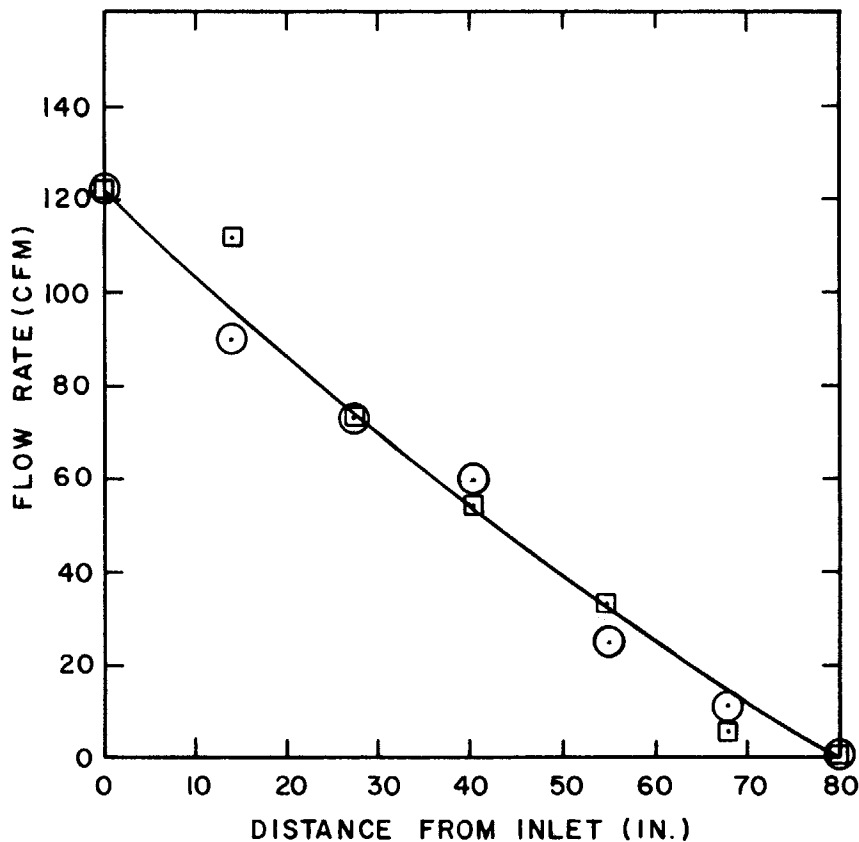


Figure 16. Indicated inlet plenum flow rate and best fit curve for Test No. 6.

□ = INLET PLENUM FLOW RATE
CALCULATED FROM OUTLET PLENUM
AND TOTAL FLOW RATES

○ = INLET PLENUM FLOW RATE
MEASURED DIRECTLY

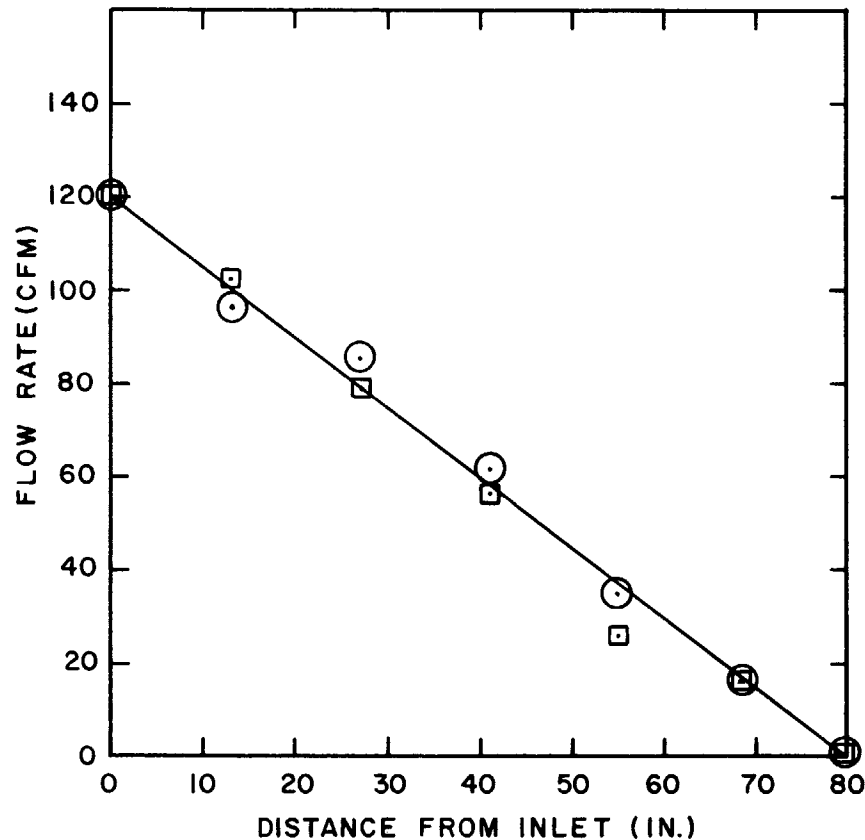


Figure 17. Indicated inlet plenum flow rate and best fit curve for Test No. 7A.

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DISCUSSION

FIRST: Would you tell me how you measure 30 feet per minute with a pitot tube?

PARISH: Because of the time limitations, I didn't have time to discuss this in sufficient detail, I am afraid, but what I tried to indicate was that we did not try to measure the face velocity in and out of the carbon. What we measured was the velocity down the plenum, which has a much higher velocity than 30 to 40 feet per minute.

FIRST: How does this relate to the differential velocity over your face?

PARISH: If you would like to take the time, we could go back to the first slide which shows the test unit. The flow down the plenum is on the order of 1,500 feet per minute which you can measure quite well. If you know the difference in flow from one station to the next, you can then calculate the flow through the carbon from the continuity equation. From the volumetric flow rate through the carbon over the area between stations and from the area you can calculate the velocity.

FIRST: Your calculation may be correct, but it doesn't prove your point.

PARISH: I would like to say one other thing in that respect. I have some slides available which will appear in the paper that I could also show you. Using this technique, we measured the flow in the inlet plenum and outlet plenum individually and then compared the difference between the total flow and the flow in the outlet plenum. We got a very high degree of consistency, which should relieve your mind as to the validity of that approach.

BURCHSTED: Did you make any measurements directly downstream of the bed itself?

PARISH: You are talking about face velocity?

BURCHSTED: Outlet velocity.

PARISH: At the face of the bed?

BURCHSTED: Yes.

PARISH: Not in this effort. We had tried that previously, and I think this is the point that Dr. First was getting at. It is a very difficult thing to do. We tried it before. We have gotten some numbers, but we decided that it was basically an unsatisfactory approach, and that is why we went to this scheme for testing.

BURCHSTED: I suspect you get a lot of deviation, according to where you make the measurement across the bed. If you have directional effects, there will be no uniformity to the flow velocity.

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STRAUSS: The first question I would raise was asked by you, Mr. Chairman, and I noted the answer. The second is a remark, but first I want to know what bed thickness you have.

PARISH: This is a two inch bed. It actually measured about two and an eighth. It was intended in every way to be consistent with the standard conventional filter bed.

STRAUSS: I agree with you fully that the flow distribution is very important to get uniform removal of impurities. Because the bed described in our paper that you referred to is 10 times deeper, there are some differences in filling and in flow distribution. The velocity decrease we mentioned is due to the geometric form and is nearly uniform over the total crosssection of such a deep bed.

MARBLE: We tested a 4" flat bed 1" from the back face at 40 FPM. We found no real uniformity of flow. Some readings were as high as 500 FPM. How could you get such uniform readings in your system and how did you take the side vectors into account?

PARISH: I can understand. You would have difficulty trying to measure 40 feet per minute. That's correct.

MARBLE: I think that is what Dr. First was getting at.

PARISH: I would agree with him, too. I agree that you and everybody else would have difficulty measuring 40 feet per minute with a pitot tube. The thing I am trying to get across is that we did not attempt, in this effort, to measure 40 feet per minute. If there is time, I would like to go back to that first slide.

MARBLE: How did you take care of the fact that you had vectors in the plenum?

PARISH: Let me try to explain it this way: We measured the flow, not the velocity, to certain discreet sections of the bed. From the flow, we can calculate the velocity. The velocity normal to the bed, the 40 feet per minute nominal velocity, is insignificant compared to the velocity we are measuring, which, as I pointed out earlier, was on the order of 1,500 per minute.

STIEHL: There are 2 effects you are demonstrating: the change of pressure drop of a bed by varying the packing density and the change of velocity distribution in the experimental duct system by varying the pressure drop. Did you measure the influence of packing density to the linear velocity distribution in a rectilinear flow direction?

PARISH: That would be possible, and we could use that to demonstrate, say, the effect of a non-uniform packing density. However, one of the things that we are illustrating here would be obviated with that approach. In other words, if you are flowing straight in and straight out, then you would not have a variation in ΔP across the bed, which is what led to the non-uniformity in the first few slides. Yes, you could measure the flow uniformity if you flowed directly in and out, but that is not directly representative of typical filter systems.

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STIEHL: I think you mix two principles. One is the change of density of the packed bed and the other is the distribution of the velocity in between the two beds in the channel in the duct where you approach the layer itself.

PARISH: We did, to an extent, mix them. We treated the first one by itself. In the first several slides that I showed you, we were measuring the effect of the velocity between two beds. In the third from the last slide, we introduced the effect of variations in packing density within the bed itself, illustrating that it overwhelmed the plenum velocity effect. Therefore, it was only a qualitative illustration of the effect of variations in packing density within the bed. By varying the packing density, or distribution, if you will, we completely reversed the picture. Instead of having the flow tending to be highest on the outlet end, now the flow was highest on the inlet end; even though the plenum velocity effect was still there. So I agree, it was not real clean, but we did show, I think, the significance of the effect.

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A CONTAINMENT AND RECOVERY SYSTEM FOR FUEL-REPROCESSING PLANTS*

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Abstract

We identify tritium containment and removal problems in a fuel-reprocessing plant and explore conceptual process designs for reducing emissions to the environment to below 1 Ci/day. The conceptual design recommended would allow an air atmosphere in the reprocessing-plant hall, and would use a continuous-catalytic-oxidizer/molecular-sieve-adsorber cleanup system to maintain a $40\text{-}\mu\text{Ci}/\text{m}^3$ tritium level ($5\ \mu\text{Ci}/\text{m}^3$ HTO) against 180 Ci/day leakage from components and process piping.

Containment Goals

Environmental Impact

Most fuel reprocessing plants routinely emit about 1500 Ci/day. The lowest practicable level for most tritium-handling facilities is around 3 Ci/day or 10^{-6} times the daily inventory handled, whichever is least. The goal in the fusion-reactor-engineering community is to reduce the total tritium loss to the environment to below 1 Ci/day. This level will be used as a standard in this study.

Losses from a fuel-reprocessing-plant containment hall with an approximate volume of $350\ 000\ \text{m}^3$ could be kept at this low level only if the tritium concentration within the gas cover (inside the hall) were kept low (i.e., around $40\ \mu\text{Ci}/\text{m}^3$). In addition, the entire wall surface would have to be either hermetically sealed or covered with a rare-metal getter (scavenger). The getter approach can be rejected on the grounds of cost and scarcity of materials; it would take 10 t of cerium, titanium, or some other rare metal to cover the walls of a $350\ 000\text{-m}^3$ containment hall with a 1-mm-thick foil. At ambient temperatures, a stainless steel shell (alloys 304, 316, or 321) 2 mm (1/16 in.) thick would provide the needed permeation barrier⁽¹⁾. The necessary welding and construction technology already exists. Such a shell, erected within a concrete building, would make it possible to meet our permeation standard (below 1 Ci/day) even in the case of an incredible accident in which 2.3 kg of T_2 (the assumed total inventory of a fusion facility) were ignited and dumped, releasing HTO or T_2O or both into the containment hall and quickly increasing the concentration up to 24.5 ppm ($65\ \text{Ci}/\text{m}^3$).

The hermetic shell would be assembled on standoffs or studs cast into the concrete shell of the building. This would create a space between the building and the stainless steel liner. Tritium levels in the space would have to be kept below $5\ \mu\text{Ci}/\text{m}^3$. If we arbitrarily

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state that the facility must be capable of containing T₂, HTO, or T₂O at the high accident-level concentrations described above (65 Ci/m³), and of reducing the concentrations to 40 μCi/m³ in 3 days of containment-hall air processing, then we can compute the length of the standoff required. If a 24.5-ppm spill leaks into this space for 3 days at 0.03 Ci/day (the permeation through 2 mm [1/16 in] of stainless steel) and if the level within the space must remain below 5 μCi/m³, the standoff distance for a 350 000-m³ hall must be 79 cm. This release, under the worst possible meteorological dispersion conditions ($\chi/Q < 6.7 \times 10^{-4}$), would result in a minimal 0.002 mrem exposure to the general public. The design concept is economical and very safe; it would also qualify as providing for secondary containment of the hall.

Occupational Safety

The routine safety of the worker must be the prime consideration in setting normal ambient operating levels within the hall. We have selected 40 μCi/m³ of gaseous tritium as the ambient containment hall level [8 times the maximum permissible concentration (MPC)] for very particular health and safety as well as state-of-the-art hardware reasons. A worker can be exposed at 5 μCi/m³ (100% MPC) for 40 hours per week without any clothing protection if the HTO fraction remains below 12% by volume. Such environments (with once-through air) are not atypical of production facility work environments in the U.S. Mound⁽²⁾ is demonstrating 40 μCi/m³ in a recirculating highly-processed system, and Los Alamos⁽³⁾, on a smaller scale is demonstrating 0.5 μCi/m³ for 10⁶ isolation between a processed glove box (at 1 Ci/m³) and a room. For design purposes, the level chosen for normal operating conditions should be well below 100% MPC. ERDA recommends⁽⁴⁾ that levels of 20% MPC should be used for design purposes. Such a standard would limit work assignments to 8 hours per week in a containment hall with a 40 μCi/m³ tritium concentration and would further require analytical evidence for HTO fractions below 2%. As an alternative, the worker could be protected by light-weight suits that would not interfere with his needs for agility and dexterity. Such light-weight suits (overalls with plastic over- and under-garment) with an independent air supply typically⁽⁵⁾ can provide protection factors of 100 or more if changed hourly, and ventilated suits can provide factors up to 1400. Thus, it can be seen that the nominal level of 40 μCi/m³ offers many operational options, while still remaining close to the economic and safety optimum.

As mentioned above, there must be some assurance that our operational design could maintain HTO levels below 2%, and this question is critical to its practicality. The HTO can be produced from pure T₂ or HT by isotopic exchange with H₂O in the water-laden air within the hall, by autoradiolysis of T₂ in air (oxygen), by catalytic conversion of T₂ or HT on active metal surfaces exposed, and by radiolysis of T₂ or HT in air caused by radiation fields. If care is taken to eliminate hot, precious-metal (catalytic) surfaces, excess water vapor, and a buildup of T₂, then the formation of HTO can be kept at manageable levels. Estimates of the conversion rates have been made by fitting available data^(6,7) to a rate equation. The results are as follows:

$$\text{HTO} = T_2(0)[1 - \exp(-kT_2(0)\theta)] , \quad (1)$$

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where θ is time for conversion in hours, $T_2(0)$ is the initial T_2 concentration in $\mu\text{Ci}/\text{m}^3$, k is the rate constant ($k = 29 \times 10^{-10} \text{ m}^3/\mu\text{Ci}\cdot\text{h}$ at 100% humidity; $k = 6.24 \times 10^{-10} \text{ m}^3/\mu\text{Ci}\cdot\text{HR}$ for dry air), and HTO is the calculated concentration of HTO in $\mu\text{Ci}/\text{m}^3$. At $T_2(0)$ levels of $40 \mu\text{Ci}/\text{m}^3$, the rate of formation of HTO would always be less than $1.0 \times 10^{-6} \mu\text{Ci}/\text{m}^3\cdot\text{h}^{-1}$. The conversion rate does not get significant until levels of $65 \text{ Ci}/\text{m}^3$ (over 10^6 times larger than those planned for in design) are approached. For the incredible accident case, $11 \text{ Ci}/\text{m}^3$ of HTO would be found in the first hour. From this analysis, we see that it is extremely important to keep T_2 at low concentrations.

The above predictions of HTO formation have been made assuming T_2 conversion by isotopic exchange and autoradiolysis in humid air. The effects of the availability of substantial areas of active metal surfaces on this conversion rate have been examined elsewhere⁽⁷⁾. A comparison of oxidation rates under various conditions is made in Table I. It can be seen that, overall, the enhancement effect results in concentrations some 500 times over those in dry air. However, it is clear from the HTO conversion rates that when the concentrations are kept low (i.e., $40 \mu\text{Ci}/\text{m}^3$) and when the air within the containment hall is continuously processed and not allowed to sit stagnant for very long periods of time, the percentage of HTO present will remain well below the 2% figure desired for the design case (20% MPC HTO).

Process System Leakage

Containment Hall Processing

Now that it has been established that a tritium gas (2% HTO) level of $40 \mu\text{Ci}/\text{m}^3$ is a safe working environment within the hall, we can proceed to examine the constraints that this level places on the process units within the containment hall. Experience in our facility at LLL and at other tritium research and development facilities in the U.S. suggests that whether a continuous air recirculation or a once-through air exhaust system is used, the ambient room levels will

Table I HTO levels (in $\mu\text{Ci}/\text{m}^3$) produced under various containment hall conditions by oxidation of T_2 (initial T_2 concentration = $40 \mu\text{Ci}/\text{m}^3$).

Time (h)	Dry air	Humid air	Catalyzed by steel*	Rad. field on steel**
1	9.96×10^{-7}	4.01×10^{-6}	9.75×10^{-5}	0.6×10^{-3}
2.5	2.49×10^{-6}	1.00×10^{-5}	2.44×10^{-4}	1.58×10^{-3}
50	4.98×10^{-5}	2.01×10^{-4}	4.88×10^{-3}	$\sim 3.1 \times 10^{-2}$
2 000	1.99×10^{-4}	8.00×10^{-3}	1.92×10^{-1}	~ 5.0
10 000	9.98×10^{-3}	4.00×10^{-2}	0.96	~ 25.0

* In humid air.

** Radiation field taken to be 40 000 R/h, on steel-catalyzed surfaces in humid air.

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ultimately be dependent upon the cleanliness of the work, i.e., on how much outgassing of tritium occurs from leakage and deadsorption. The ambient level for a given room volume depends on the air replacement rate within this volume and the tritium outgassing rate. Our operating experience with once-through air exhaust systems suggests that 20 changes of air per hour are needed to keep room levels at about $15 \mu\text{Ci}/\text{m}^3$. Of course, when laboratory operations are not clean, the level will increase correspondingly.

In Table II, we give the results of a mass-balance analysis relating the maximum tritium outgassing rate that can be tolerated (based on the $40\text{-}\mu\text{Ci}/\text{m}^3$ level) to given air-flow rates in the $350\ 000\text{-m}^3$ hall.

A processing system that could maintain the $40\text{-}\mu\text{Ci}/\text{m}^3$ level is shown in Fig. 1. This system would catalytically oxidize the T_2 and HT to HTO and capture the HTO on a molecular sieve. Operation at high temperature (350°C) would permit oxidation of tritiated hydrocarbons. The higher-boiling refractory organics could be trapped by an activated carbon column. Waste air would be recycled for the most part into the hall itself, with a small bleed stream exhausted to the stack, hopefully below $40 \mu\text{Ci}/\text{m}^3$. The processed air would enter the hall near floor level, where most of the workers would be located. The bulk flow of the reprocessed air would be $58.8 \text{ m}^3/\text{s}$ ($100\ 000 \text{ cfm}$) or better. The air velocity near the worker might be in excess of $3.3 \text{ m}/\text{min}$ (10 fpm); however, the net velocity upward through the interior of the containment hall would be only $0.33 \text{ m}/\text{min}$ (1 fpm) or so. Thus, the fact that air is blowing past the worker ($< 120 \text{ fpm}$) does not constitute a safety enhancement.

Spill Accident

The waste-gas processing system, the consequences of an accidental spill of the entire inventory (2.3 kg T_2), and estimated worker re-entry times after such a spill must now be considered. This problem is a difficult one because of the transient behavior of T_2O and T_2 adsorbed layers on all exposed surfaces, but neglecting the adsorption-controlled outgassing problems, we can make an estimate of the processing time required to reduce the $65\text{-Ci}/\text{m}^3$ concentration from the incredible accident, to the $40\text{-}\mu\text{Ci}/\text{m}^3$ operating level. Using the Mound Laboratory studies⁽⁸⁾ for the determination of the catalytic oxidation kinetic rate data, a plug-flow reactor can be designed.

Table II Maximum tolerable outgassing within containment hall to maintain $40\text{-}\mu\text{Ci}/\text{m}^3$ level.

Air flow		Volume changes per hour	Outgassing tolerated (Ci/day)
(m^3/s)	(cfm)		
0.36	607	1/24	1.0
5.88	10 000	0.05	18.0
58.8	100 000	0.5	180.0
2352	4 000 000	20.5	7200.0

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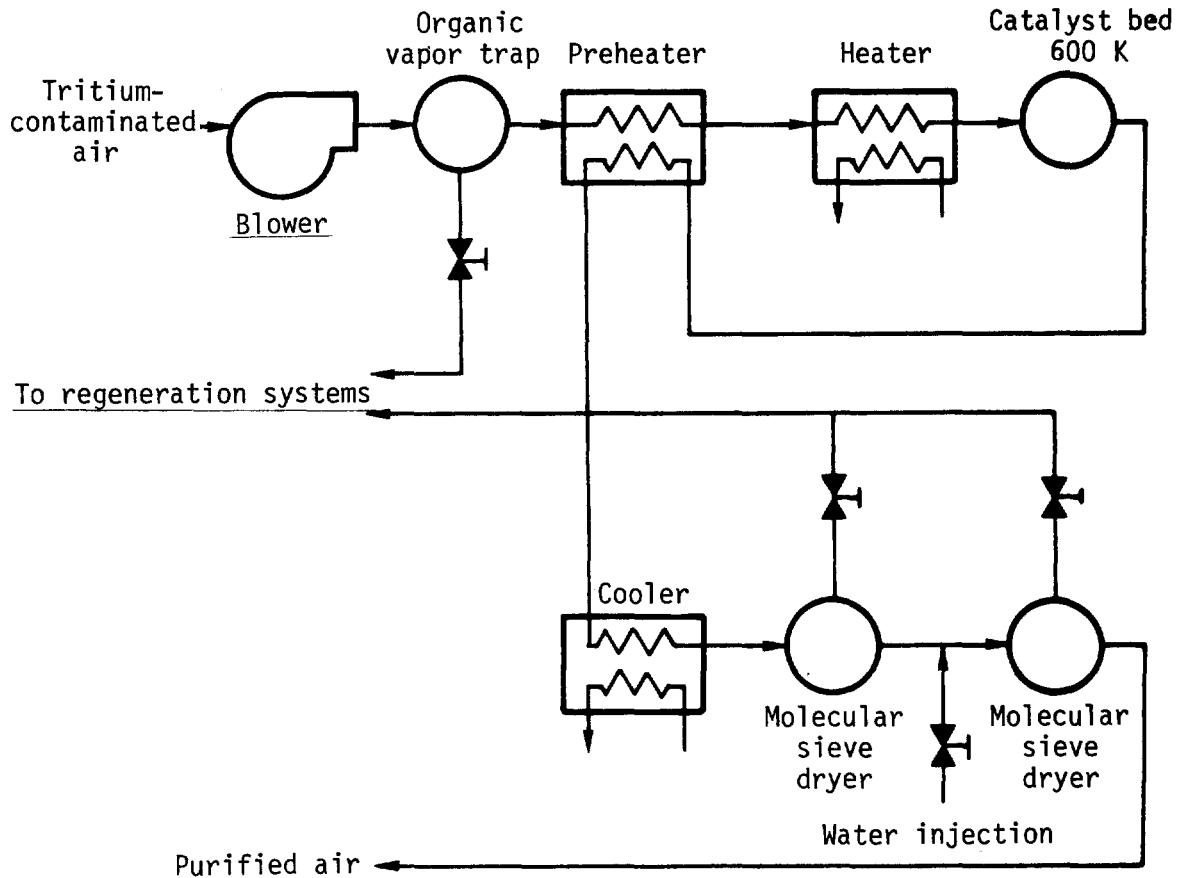


Figure 1 Catalytic oxidation and molecular-sieve adsorption of tritium.

A schematic for such a recirculating tritium-removal system is shown in Fig. 2. The tritium (T_2) concentration (in g/m^3 of total gas) is given as T and subscripted according to location. It is assumed that the containment-hall atmosphere is well mixed and that the volumetric removal of gas (mostly T_2O) is negligible compared to the recirculating flow Q_2 .

A mass balance on the containment hall can be made by expressing the difference between inlet and outlet concentrations and the decrease in the prevailing level:

$$T_2(\theta) - T_1(\theta) = -\frac{V}{Q_2} \frac{dT_2(\theta)}{d\theta}, \quad (2)$$

where θ is the process time and V the containment hall volume. A similar balance can be made around the removal process unit:

$$Q_2[T_2 - T_1] = Q_3 T_3 \quad (3)$$

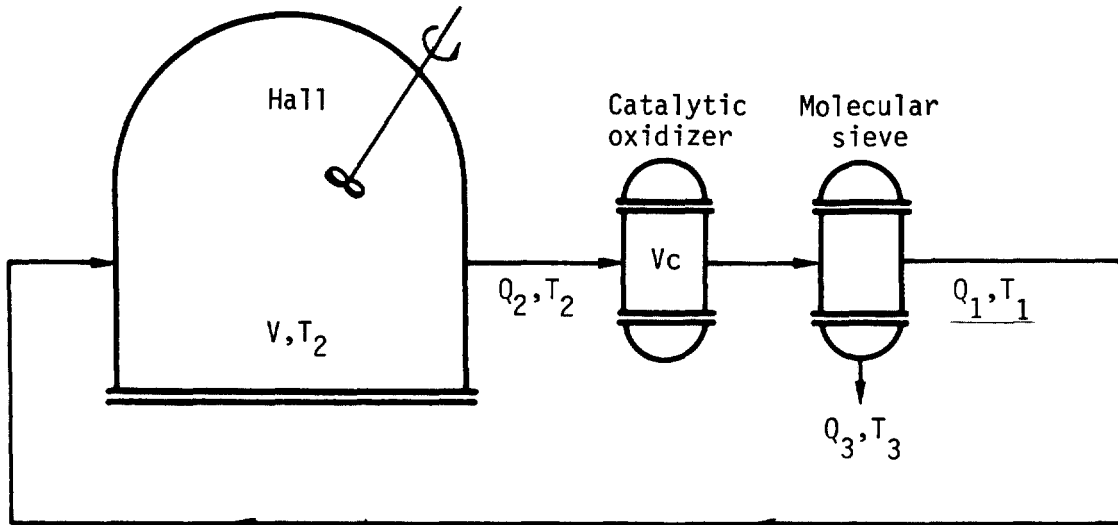


Figure 2 Model for prediction of tritium spill cleanup dynamics.

or

$$\frac{T_1}{T_2} = 1 - \frac{Q_3 T_3}{Q_2 T_2} \quad (4)$$

The plug-flow-reactor design equation can be written⁽⁹⁾ as:

$$\frac{T_1(\theta)}{T_2(\theta)} = \exp\left[-k \frac{V_c}{Q_2}\right], \quad (5)$$

where V_c is the catalyst volume and k is the first-order kinetic rate constant,

$$k = 2.27 \times 10^5 \exp[-7100/RT], \quad (6)$$

as determined at Mound⁽⁸⁾. Although this plug-flow approach is simplistic in that it neglects pore diffusion and dispersion effects, for low-temperature (300 K) operation, these effects are insignificant.

Eliminating $T_1(\theta)$ between Eqs. (2) and (5) results in:

$$T_2(\theta) \left[1 - \exp\left(-k \frac{V_c}{Q_2}\right) \right] = \frac{V}{Q_2} \frac{dT_2(\theta)}{d\theta} \quad (7)$$

Integrating,

$$\int_{T_2(0)}^{T_2(\theta)} \frac{dT_2}{T_2} = - \left[1 - \exp\left(-k \frac{V_c}{Q_2}\right) \right] \frac{Q_2}{V} \int_0^\theta d\theta,$$

results in

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$$\frac{T_2(\theta)}{T_2(0)} = \exp \left\{ - \left[1 - \exp \left(-k \frac{V_c}{Q_2} \right) \right] \frac{Q_2}{V} \theta \right\}. \quad (8)$$

In Table III, the results of the model calculations relating safe "re-entry times" as a function of required catalyst volumes are shown. Catalyst beds larger than 200 m³ are not practical because of the cost; thus, re-entry times range from around 1 day to 6 months for reasonable bed designs. Re-entry times of several days to a week or so would be acceptable; catalyst volumes from 10 m³ to around 100 m³ are therefore needed. There are many areas where costs can be cut if an optimization of the design is made to suit the particular specialized requirements for the containment hall processing.

We have looked briefly at one way of containing tritium in a fuel-reprocessing plant so as to limit the release to the environment to 1 Ci/d. The use of a gas cleanup system to provide a safe working environment and to make re-entry of personnel possible within a reasonable time after a large spill has been discussed.

Table III Tritium reduction after the incredible accident
 [Q = 58.8 m³/s (100 000 cfm); T₂(0) = 65 ci/m³;
 V = 350 000 m³; Temp. = 300 K].

Elapsed time (h)	Tritium level (μCi/m ³) for various catalyst volumes			
	0.17 m ³	1.7 m ³	16.7 m ³	167 m ³
30 (1 day)	6.05 × 10 ⁷	3.21 × 10 ⁷	1.71 × 10 ⁵	0
600 (1 month)	1.54 × 10 ⁷	48.03	0	0
3 000 (4 months)	6.56 × 10 ⁴	0	0	0
10 000 (1 year)	2.12 × 10 ⁻³	0	0	0
4 years	0	0	0	0
Approximate plant cost (millions of dollars)*	0.1	1.0	10.0	100.0

*Basis: honeycomb catalyst at 60 000 dollars/m³, constituting 10% of plant cost.

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REDUCING AIR CLEANUP SYSTEM COSTS

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I. Introduction

The majority of Air Cleanup System specifications issued this last year, for Air Cleanup System manufacturers to bid to, have contained numerous errors. These errors range from omissions and conflicting requirements to more stringent requirements than are required by government specifications and are obtainable by the industry. This increases the engineers time and manufacturers time and costs in resolving and correcting these errors, thus frequently delaying the schedule and increasing the Air Cleanup Systems cost.

II. Causes and Solutions

During the past years we have reviewed many such technical specifications in support of our Sales Department in preparing bid proposals for Air Cleanup Systems. As an engineer, my interest is in establishing the technical requirements of the equipment and proposing a cleanup system design that meets the users requirements, as he states them in his specification.

But what the engineer wants or needs in his Air Cleanup System is not always what he indicates in his technical specifications to bidders. A few reasons are apparent.

First: He is often an H.V.A.C. or Mechanical Engineer, newly hired by the expanding Architect Engineer or utility, who has not worked with specifications of Nuclear Air Cleanup Systems before, and is scheduled to issue his Air Cleanup System Technical Specification in a couple months.

Second: He has quickly read a number of the government regulations and specifications, and has become lost in the paper mill of confusing and sometimes conflicting requirements.

Third: He has reviewed a number of product catalogs, some of which are obsolete, and has become confused by the alternatives.

Fourth: He looks for a quick way out of his dilemma to issue his technical specification on schedule -- and he apparently has two or three alternatives.

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The first alternative, one that has been used many times, is to obtain a copy of a previously issued Air Cleanup System specification from another division of his company, that is working on another nuclear plant. Using this as a guide, he uses the same text, changing the CFM and instrumentation to fit his particular requirements. But his new technical specification now has inherited any errors that the original specification had, and perhaps some more stringent requirements than his air filter units may really need.

Examples to illustrate this include:

1. Eight independent omissions in one specification, listed as controlled by the specification, for heating coil fin configuration and materials, prefilter test requirements, HEPA filter type for MIL-F-51068, HEPA filter test requirements, and fan drive arrangement and other construction features.

2. Incorrect HEPA filter section flow rates $2\frac{1}{2}$ times the capacity of other parts of the Air Cleanup System.

3. Air flow measurements in the middle of an Air Cleanup System housing, accurate to within 5% of the system's rated flow, with instrument terminals for remote readout and recording. The air flow velocity pressure the engineer was going to measure was 0.006 in. w.g., while the air flow velocity pressure at the inlet and outlet were 0.1 in. w.g.; a still difficult but measurable figure.

4. A charcoal adsorber section with a 12-inch head of charcoal above the filter beds with viewing ports and lights above the charcoal to verify that it would not settle after properly filling the 4-inch beds. This required unnecessary stainless steel, welding and inspection, windows, lighting and wiring, and charcoal.

A second alternative, used less frequently, is for the engineer to lift the most stringent requirements from several specifications and the highest performance levels from the various product catalogs he is able to obtain in his limited time. But this leads to some conflicting or unrelated performance requirements in his specifications, when the highest performance is used from various products, and more stringent requirements than his Air Cleanup Systems may really need.

Examples to illustrate this include:

1. Charcoal adsorbent impregnated with 5% TEDA, but with the charcoal ignition temperature above the requirements of the RDT M16-IT at a value obtainable only with a 2% TEDA or 3% KI₃ Impregnation.

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2. Moisture and over pressure dust loading tests for one HEPA filter from each roll of filter media used on filters meeting the requirements of MIL-F-51079 with QAS testing. The price of this destructive testing was twice the price of the total quantity of HEPA filters to be built per the MIL Specs and individually tested at a QAS station.

3. Prefilters of 85% ASHRAE Dust Spot Test efficiency to be individually tested with 0.3 micron DOP smoke per military Standard 282.

4. Air Cleanup Systems used in normal recirculating control room air and auxiliary buildings, for air that men are working in, which have the following component requirements:

- A. Withstand temperatures up to 300 degrees Fahrenheit.
- B. Withstand a radiation dose of 1×10^6 Rads.
- C. Include a water deluge system for fire extinguishment in the charcoal adsorber due to decay heat from adsorbed radioactive iodine.

In preparing a proposal, we assume that the engineer really wants what his technical specification states, and therefore attempt to propose and price a design which meets his specification. Some Air Cleanup System manufacturers question unusual requirements with the engineer, some include it in their design and pricing, and some omit it, taking an exception to the specification. Now the Engineer must wade through proposals which include and omit various items in their price, and to the Purchasing Department, it is like pricing apples and oranges.

An option which we believe results in a better technical specification, is for the engineer to review and design his Air Cleanup Systems using ORNL-NSIC-65, or its replacement when available, ERDA 76-21, as a guide and handbook. In addition, his specification should require that the design, construction, and testing of the systems and their components are to meet the requirements of ANSI N-509 and 510, as applicable. These standards call out the remaining government and industry standards, which therefore do not need listing again in your technical specification. Add to this, consultation with one or more of the manufacturers who have built Air Cleanup Systems to a number of government and engineers' specifications, and are familiar with what is required and what performances in components are available, and you will have a better, more precise, technical specification for your Air Cleanup Systems.

Conclusion

By working with your engineers, the Air Cleanup System manufacturers, or a knowledgeable independent consultant, can supply them with information they are unaware of and can obtain information that they are having difficulty obtaining. We can help them design their system and work out their technical problems so that they may arrive at a satisfactory design to meet the Air Cleanup System performance requirements and prepare a technical specification that correctly and precisely identifies these requirements.

An accurate specification identifying what is really required, reduces the Air Cleanup System manufacturer's time and cost in preparing bid proposals, thus reducing his operating costs, and eliminates unnecessary repetitive phone calls to the engineer from each Air Cleanup System manufacturer. Thus, the engineer reduces his initial specification preparation time, reduces the number of calls he receives to clarify his specification, reduces his time required to evaluate the proposals submitted in response to his specification, and reduces the price manufacturers ask to meet his specification. In addition, he becomes more knowledgeable about the requirements and performance his Air Cleanup Systems will meet.

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ABSOLUTE FILTERS: EFFECTIVE FILTERING MEDIA

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The amount of effective filtering media in an absolute filter, i.e. that media which is available to remove particulates, is a function of the method used to construct the filter. The effective filtering media is not necessarily equal to the total amount of filtering media in the filter.

This may seem like a profound statement of oversimplification, but I believe it is relevant now for two reasons:

1. On August 1, 1976, we learned that, for the first time a separatorless filter passed all of the testing performed by Edgewood for QPL listing.
2. Certain data have been presented here which are in disagreement with data gathered from experience in evaluating the performance of separatorless filters over extended periods of service life.

Experience indicates that separatorless or molded type filter applications have approximately 3 times the service life of filters of conventional construction with flat roll media and corrugated separators. This experience is derived from applications where filters have been in service for periods ranging from six months to three years.

The phenomenon of effective filtering media vs total filtering media was detected during tests to determine the reasons for the proven extended life of the molded filter element.

As a part of these tests, filters constructed with both molded filter packs and separator type filter packs were subjected to surface dust loading by ASHRAE test dust without lintens. After the filters had been surfaced loaded, they were cut apart and samples of the filtering media were removed. It was noted that the media from filters made with separators showed alternating bands of black and white throughout the depth of the filter. The black bands indicated those areas of the filtering media that had been exposed to the test dust. The white bands indicated those areas where the crowns of the separators had pressed against the filtering media to block the effective use of the media.

The percentage of effective filtering media in a separator type filter was calculated by measuring the band width between the crowns of the separators and the widths of the white bands indicating blockage of the media by the crowns of the separators. From these measurements, the total effective area of filtering media for various types of size 24" x 24" x 11-1/2" filters were calculated as follows:

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	TYPE FILTER PACK		
	Mini-Pleat	Standard	Molded
Width of Separator Shielding Band-In	.06	.08	.05
Width of Media-Crown to Crown-In	.30	.43	.70
Effective Media-Crown to Crown-In	.24	.45	.65
Percent Effective Filtering Media	80.0	81.4	92.9
Total Filtering Media - Sq. Ft.	292.3	263.1	289.4
Effective Filtering Media - Sq. Ft.	233.8	214.2	268.8

It is concluded that the greater proven service life of the molded filter element is the result of greater area of effective filtering media and the lower initial pressure drop of the molded element.

The affects of the filter media thickness have not been evaluated at this time. Standard roll filtering media is used in thicknesses of 15 to 19 mils. Molded filter elements are made from media of 22 to 25 mil thicknesses.

DISCUSSION

FIRST: Do I understand from the slide you showed that those pleats are floating in air and don't touch each other? You showed 100 per cent of the filter medium available.

CADWELL: That was an error, it should have been 92.9%. That's why I made the point of extended service life for the separatorless filter. We find that when you have this per cent of filter media in the filter, airflow resistance is equal to a single layer of paper at equal filtration velocity.

DEMPSEY: I am very glad to see this trend toward increasing the capacity of filters. It is a very important matter down the road for waste management. Lately, we have heard a great deal about at least two manufacturers in Europe who place twice as much paper in the same area. I would be very interested to know if anyone has tested them for performance.

CADWELL: I think Chuck Skaats may be testing some now.