A REVIEW OF THE NSP ENVIRONMENTAL MONITORING PROGRAM FOR THE ST. CROIX RIVER, WITH SPECIAL REFERENCE TO THE ANNUAL REPORT FOR 1968

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GENERAL COMMENTS

These reports of work by qualified consultants are reasonably good of their kind, and for the time and money expended in the study. They demonstrate that in 1968 the Saint Croix River area did not receive from the Allen S. King Plant any pollution of sufficient impact to damage major portions of local aquatic and terrestrial ecosystems. However, as with any piece of scientific work, numerous criticisms can be levelled at the investigations, and these are presented in detail under the later heading of particular comments. The following more general criticisms may be made first.

1. It is not easy, or inexpensive, to establish clearly the thermal and other effects of power plants upon rivers and lakes. Because there will be many more such power plants in the future, fired by a variety of fuels, it is imperative that several of those now being constructed in diverse locations be monitored with extreme care to really find out, not only (1) whether the plants avoid harming large areas of the local aquatic and terrestrial ecosystems, but also (2) how large are the areas which are demonstrably affected and how serious are the effects? The public has a right to know, and if the areas and effects prove to be exceedingly small over an extended period of years, it will be to the operators' advantage that this be demonstrated clearly and beyond question.

The program under review may meet the first requirement noted above (although a considerably longer period of investigation will be necessary to establish the point); it falls far short of meeting the second requirement, as may be seen in several sections of the particular comments. Far greater expenditure of time and money would be necessary to meet the second requirement, but in view of the rapid growth in power needs and the consequent proliferation of power stations, such an assignment of resources would seem readily defensible as providing guidelines for future development. The resources, and the studies, ought however to be provided by bodies other than the operators of the plants, so that full independence of investigation will not be open to question.

2. Biological data (for instance, on phytoplankton and fish) must be gathered for far longer than 1-2 years if an adequate preoperational baseline is to be established, because populations fluctuate markedly from year to year. To quote Dr. Brook, NSP's consultant on algae (report for 1966, p. 1, reiterated in the 1967 report): "Free floating algal populations show inexplicable year to year variations, sometimes

of ten but often as great as one-hundred fold in magnitude, so that only by extended surveys can any reliable conclusions be drawn about changes resulting from some altered environmental factor."

With only 1-2 years of data, and especially with the present rather meagre sampling program, it will probably be virtually impossible to detect any but major (perhaps catastrophic) changes in flora and fauna owing to pollution from the A. S. King Plant. Minor changes signalling a slow alteration of the river ecosystem will be very difficult to trace, and will require a careful long-term analysis which seems contrary to NSP's intentions according to newspaper reports. These reports indicated that NSP officials feel their monitoring program can soon be curtailed, owing to the lack of any danger signs to date. So far it cannot be said that the results are negative (proving no pollution), but only that they are inconclusive (not proving pollution).

It may be that if conditions upstream and downstream from the plant are sufficiently similar, the upstream monitoring will establish in time an adequate baseline. However, not enough effort has been devoted to testing or proving this proposition, which is very important to the success of the monitoring program. Present indications, as far as they go, suggest that phytoplankton and fish populations differ considerably in upstream and downstream sections of the river.

3. The basic design of the aquatic program ignores a very important aspect of monitoring -- that it is desirable to begin as near to the point source as possible. Effluent dilution often follows a power-law decline (i.e., a log/log plot of concentration versus distance from source yields a straight line) so that the effect of pollution falls off very steeply near the source and ever more gradually as distance from the source increases. Only if one begins near the source can the nature of the decline with distance be clearly established, for if one begins far away the variability induced by factors other than pollution may well mask the effect of pollution itself.

In my opinion, single stations plus a series of at least 3 radial transects of the river, beginning at the mouth of the effluent ditch and crossing the river upstream, across and downstream, would be far more satisfactory than the series of single stations and cross-sectional transects presently employed. The points on the radial transects should be closely spaced near the point source, and ever more widely spaced away from it, in order to establish firmly the nature of the pollution effects. At the very least, trials should be made of the effectiveness of radial transects in delimiting the area affected by pollution from the King plant. An example of such a series of radial transects is given in Figure 1.

4. The creel census as reported makes little contribution to the study as a whole, and should be abandoned as a needless expense unless the Minnesota Dept. of Conservation wishes to continue it for their own purposes and with their own funds. A creel census conducted within and outside the zone of heated effluent where the discharge canal enters the river could, on the other hand, be useful in determining whether the hot-water discharge improves or damages fishing very locally.*

5. A notable deficiency in the present aquatic monitoring program is the absence of any study of zooplankton populations, which are claimed by Churchill and Wojtalik ("Effects of heated discharges on the aquatic environment, and the TVA experience", at the American Power Conference, Chicago, April 1969) to be less tolerant of high temperature than the fish which feed on them.

6. It would be useful to have some indication of the emission by the plant of gaseous pollutants other than sulfur dioxide, for example nitrogen oxides and hydrocarbons.

7. Insufficient effort has been devoted to summarizing data, presenting them in meaningful graphic form (especially for year to year comparisons), and comparing them with relevant data elsewhere.

PARTICULAR COMMENTS**

A. FULFILLMENT OF THE OBJECTIVES OF THE INITIAL MONITORING PROGRAM PROPOSED ON 1 JULY 1966

The initial program outline has been followed for the most part, and some additional studies have been initiated, for instance those in plant pathology. However, the following questions may be raised concerning program fulfillment:

1. <u>Visual log</u>. It was proposed (p. 4) to keep a log for visual

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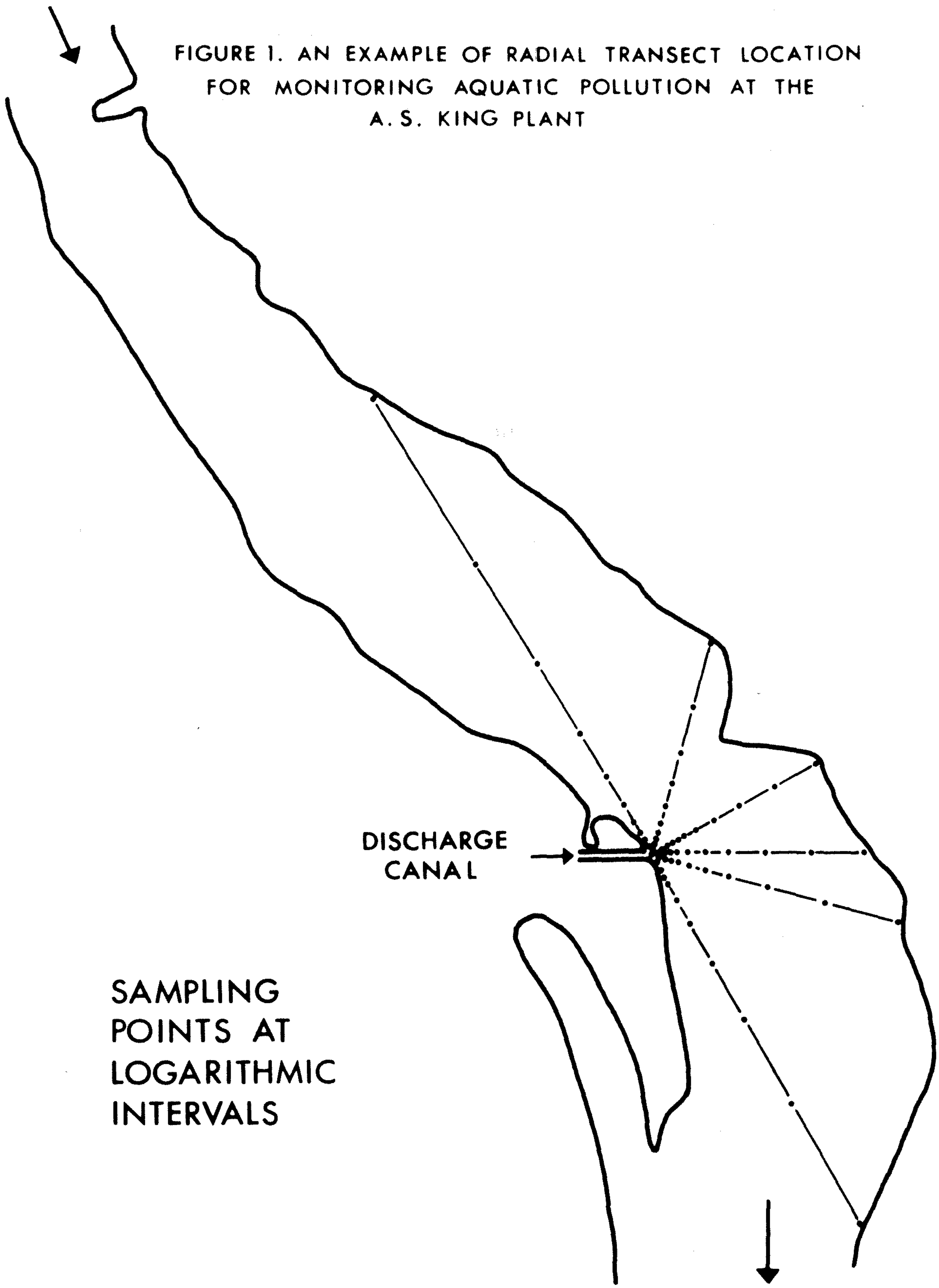
*Recent inquiry reveals that such a census is now being undertaken, but results have not yet been processed.

**Where page numbers, tables, and figures are cited, they refer to the 1968 report unless otherwise noted.

FIGURE 1. AN EXAMPLE OF RADIAL TRANSECT LOCATION FOR MONITORING AQUATIC POLLUTION AT THE A.S. KING PLANT



observations of significant phenomena, e.g., floating sewage, oil slicks, debris, odors, and the like. Apart from a few notes of floating debris appended to tables of water temperature, little appears in the reports regarding such phenomena; one would expect more to have been observable on occasion.

2. Additional physical and chemical analyses. There appears to be no coverage in the report of the additional analyses proposed (p. 5) following operation, i.e., turbidity at the outfall of the holdup pond, pH at this outfall and at the mouth of the discharge canal, and residual chlorine at the mouth of the discharge canal when chlorine is used in cooling towers or condensers.

3. Algal studies. There appears to be no discussion, in the three reports issued so far, of the point in the river at which a truly planktonic algal community develops, and of tributary sources of algae to the river. These problems were singled out for study in the initial program (p. 7), which also indicated that periphyton would be studied in the immediate area of the plant to determine the influence of heat in the discharged circulating water. The nearest periphyton site (no. 4) appears to be located considerably beyond the zone of major heat influence indicated in the latest annual report (see water monitoring program, p. 2-3).

4. Fish studies. The initial program proposed (p. 7) studies of the food habits of the principal fishes, and the distribution of these fishes throughout the year (my italics). The reports so far do not provide information on these points, apart from some comparison of fish abundance in separate sampling periods -- in 1968 they were spring and summer for trap- and gill-netting, summer and fall for electro-fishing, and spring, summer and fall for trawling.

5. Air monitoring. As part of the air monitoring program it was stated (p. 12) that detailed information would be reported for unusual situations which occur within each month. Detailed information of such a kind has not been discussed in the reports so far. It was also proposed (p. 12) that a qualitative survey of sources of SO_2 in the general environment would be made at intervals to assist in the evaluation of instrumental results. This survey does not appear to have been carried out, apart from some speculation regarding possible sources which accompanies the data sheets for 1966.

B. DISCUSSION OF THE REPORT FOR 1968

1. Pagination of reports. To facilitate study, citation, and reference, the reports should be paged continuously from beginning to end, including all tables, figures, plates, etc.

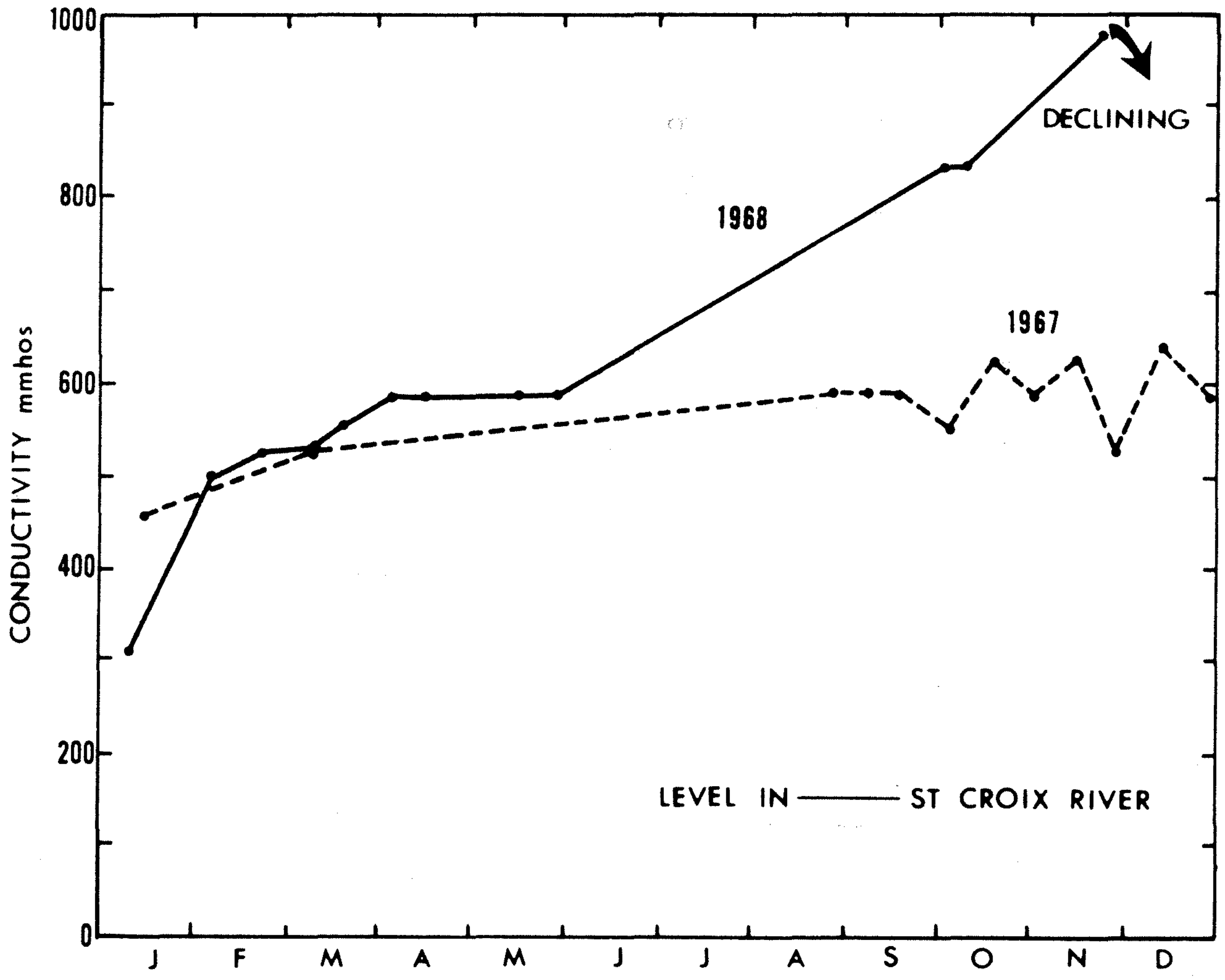
2. Location of discharge canal. From the report (and the initial monitoring program) it is difficult to ascertain just where the discharge canal enters the St. Croix in relation to the monitoring range 70. The location of the canal should be shown on all relevant maps (e.g., Map I of water sampling locations, which follows the summary of water analyses).

3. Coal pile drainage. There is a continued rise in conductivity and sulfate from January to November 1968 (see Figure 2), rather than a fluctuation as claimed. The subsequent decrease noted may be temporary, because there was also a temporary decrease in December 1967 and January 1968. Complete analysis of these discharge waters (along with the river samples already being analyzed) would probably help to understand this phenomenon, as might a survey of fluctuations in ground-water level at the site.

4. Water temperature in summer. The water temperature surveys included only one comprehensive study, on 8 Aug. 1968, and the summary of that study is very difficult to visualize without any graphic presentation. Plots of the isotherms have been made by NSP, and should have been included as part of the report, becuase they would make immediately apparent the location and extent at that time of the warm-water zone created by the plant discharge. This information is of vital importance in designing (or re-designing) a truly adequate program of biological monitoring, and more such surveys should be undertaken. Such studies should be detailed and frequent, and should also be designed to determine whether warm water is prone to move downstream close inshore, where littoral invertebrates are being sampled.

In the annual report for 1966 it was stated that recorders were being installed to facilitate making circulating-water heat budgets. There appears to be no discussion of such budgets in the later reports, although some data on total heat discharged to the river are included in the tables of 1968 water temperatures in the St. Croix.

FIGURE 2. SEASONAL CHANGES IN CONDUCTIVITY OF COAL PILE DRAINAGE



5. Water temperature in winter. No comprehensive survey of winter heating appears to be envisaged, along the lines of the above-mentioned single summer survey. Such winter studies would be of considerable value, and should be undertaken. Changes in dimensions of winter zones of warm water should also be studied in relation to periods of plant shutdown, so that possibilities of temperature shock to fish acclimated to warm winter water can be examined.

6. Ice and open water. The survey of ice thickness and open-water extent could well be expanded to include stations farther downstream, at little cost. A graphic presentation of ice-thickness, from 0 at the point of discharge to a maximum value in the region of more or less constant downstream thickness, could be prepared for the period when the ice is thickest. This would show clearly the true extent of the winter influence of the heated discharge in any given year, and would therefore be of considerable interest.

7. Macro-invertebrates. In this section it might be wise to modify the following sentence on p. 4: "These data indicate that the operation of the Allen S. King Plant, which began in early 1968, had no effect on the biology of Lake St. Croix as measured by the macro-invertebrates." All that can be said is that this survey, in the particular sites examined and by the techniques used, did not show any effect of plant operation upon the macro-invertebrates. The survey methods were neither highly sensitive nor very extensive, so that minor changes could well be missed, as could major changes which might have taken place closer to the point of discharge of heated effluent than any site sampled.

Some questions might be raised about the inferences drawn in this study, and their statistical justification, for example:

(a) Is the "stability" of the left bank significantly different from that of the right bank (p. 5, table 3)? Does stability differ significantly from year to year?

(b) Why present the regression lines in Fig. 1 which presumably are not significant according to data on rank-order correlation? In any case, both regression lines (y on x, x on y) should be included, because one variable does not depend on the other. For Fig. 1B, is the regression really $Y = 17.16 + 0.358X$; the intercept looks more like 18.6?

(c) Is the coefficient of variation (and therefore the "stability") significantly different as between insects and non-insects, justifying the statement (p. 7) that the insect group is more stable than the non-insect group? Again, why a regression line, and if one, why not two?

(d) It is claimed (p. 8) that "No significant biological changes occurred in Lake St. Croix over the three years of study, but this statement (like that on p. 4) should be qualified, especially because macro-invertebrates were not studied well within the zone where heated water from the discharge canal raised river temperatures substantially (as shown by the detailed survey on 8 August 1968).

(e) Are the data on standing crop (<u>not</u> productivity as stated) significantly different for the three ranges (60, 80, 97) shown in Table 6 (p. 9)?

Some attention ought to be paid to differences in specific composition of the fauna from site to site and year to year. They are not mentioned in the report, although, for example, the number of clam species dropped from 7 and 9 in 1966 and 1967 to 3 in 1968, while the numbers of individual clams were 9, 26 and 5 for those years. Amphipod species numbered 4 in 1966, 7 in 1967, and 3 in 1968; while numbers of individuals were 34, 35 and 17 respectively. Caddis fly species were also least numerous in 1968. Such phenomena deserve more extensive study.

8. <u>Phytoplankton</u>. It may be noted that the nanno-plankton, often important to primary productivity and to food-web dynamics, has not been investigated. It also seems odd that there are no phytoplankton stations (for chlorophyll and productivity analyses as well as algal counts) in the main body of the river between no. 4 just upstream of the discharge canal and no. 6 four miles downstream. (Station no. 5 is in a protected position in Bayport Bay.) One might expect stations to be located much closer below the discharge point, to detect how immediately any effect of thermal or other pollution might manifest itself. In particular, one might hope to examine whether the heated-water discharge serves as a source of inoculum for certain species of algae favored by the conditions there. (For instance, do blue-green algae develop earlier or in abnormally high densities in the heated zone, and so provide an inoculum capable of initiating early or especially

heavy blooms downstream?) The detailed studies carried out on 20 Sept. 1968 are inconclusive on this point, particularly because different species showed differing patterns of abundance, and an east wind may have been a complicating factor. As suggested by Prof. Brook (p. 7) there may also be a possibility of algal destruction in the cooling system of the NSP plant, but because of the depth distribution of algae in relation to the depth of the intake pipe was not determined, such a phenomenon could not be established with certainty.

An important observation is that over the three years studied there have been striking annual differences in the abundances and times of "blooming" of various algal species, pointing to the need for many more than 2 years of pre-operational study to establish satisfactory base-lines. (Some indication of the statistical significance of the annual differences would seem appropriate here.) The substantial upstream/downstream differences which have been observed to vary from year to year (see p. 2-3 and p. 7), may be of particular importance. Such differences make it difficult to rely upon post-operational upstream data as a substitute for the pre-operational downstream data needed to serve as a baseline for measurement of power-plant effects upon the river.

It is claimed tentatively (p. 9) that sewage effluents might be responsible for high rates of photosynthesis at stations 3 and 4 downstream from Stillwater, and station 7 downstream from Hudson; but the highest rates were measured at station 1 near the head of the lake. It would seem important to do enough experiments for all stations simultaneously to determine the statistical significance of conclusions on this point. The September peak of algal standing crop seems to have been missed in the experiments, with a long gap between 9 July and 3 October 1968. It might be worthwhile to carry out more experiments, adding samples from the zone of heated discharge, and to analyze the productivity of a complete set of waters from various stations in that zone.

There does not appear to be a study in 1968 of the depth distribution of phytoplankton, despite the observation in the previous year that deep-water samples often show the greatest abundance. It was also remarked at that time that a study of the effect of heated effluent upon the vertical distribution of phytoplankton would be of interest, and possibly very significant. The statement still holds.

9. Attached algae. The study in 1968 does not include any treatment of data by Hohn's method, which was employed in 1967 as the most useful technique available for assessing the condition of rivers by analysis of attached algae. If the technique is now regarded as being of little value, some explanation should be given. Although the numbers of species encountered in 1967 showed little variation between sampling stations, the abundance of algae per unit area of slide surface varied widely from station to station in 1968, as did the numbers of individuals for several major species in October (table 3 of the report). It is pointed out (p. 2) that current flow-rate may affect results with the technique used. Therefore it would be useful if comparative data on flow-rates were available for all stations.

In the study of algal pigments (p. 2) methods are not specified. For an assessment of the reliability of the data, it is necessary to have information on this point.

The claim is made (p. 3) that "the King Plant discharge does not seem to affect the abundance of attached algae." However, no slides were installed within the zone where heated discharge has a strong effect on water temperature. Moreover, numbers of algae per unit area may be comparable at the same time that specific composition of the algal floras at different stations varies widely (compare stations 4 and 5 in table 3 of the report). Many more data would be needed to establish the truth of the statement quoted.

10. Fish. Despite the stated intention that this survey was designed to detect pollution effects, there is little indication that it is different from a normal river or lake survey. There appears to have been no attempt to look for fish congregating in the heated discharge, especially in winter -- when the possibility of shock following temporary plant shutdown ought to be examined. Possible avoidance of the heated zone in summer ought also to be investigated. Studies on growth rates in and out of the heated effluent might be considered, as might studies on predator/prey relationships.

As in the case of phytoplankton, there is much year-to-year variation in the data (see tables 12-14), pointing to the need for pre-operational studies over a considerably longer period than 2 years.

Moreover, no treatment of the data for statistical significance has been included in the report. In the report for 1967 (p. 36) the importance of variation in year-classes of crappies is noted as responsible for "Boom or Bust" fishing. Other important factors affecting abundance of fish may be flooding, annual variations in temperature, changes in predator populations, and disease.

The 1964 fisheries survey of Lake St. Croix by Duane Shodeen (Investigational Report No. 283, Minn. Dept. of Conservation) showed appreciable upstream/downstream variation in fish populations. This suggests that upstream monitoring will not necessarily provide sound baseline data against which to assess pollution effects downstream.

Sampling methods (and/or timing and placement) may affect results. For instance, trapnet catches of carp increased in 1968, but gillnet and trawl catches declined. Whether the differences are significant apparently has not been tested.

11. <u>Creel census</u>. The survey period reported on should correspond to that for the other activities, i.e., December 1967 through November 1968.

It is difficult to believe that a creel census as carried out here can be an adequate tool in testing pollution effects, because of the variability in amount and type of fishing pressure. For example high water greatly decreases the amount of bank fishing (p. 7) and probably boat fishing as well, with a consequent reduction in catch. Ice-fishing also varies greatly with ice conditions from year to year (p. 7), so that if a creel census is to be useful many years of preoperational data will be needed. Also, creel census is particularly subject to upstream/downstream differences and fluctuations (in type and amount of fishing pressure, bait used, etc.), so that upstream data cannot easily be substituted for preoperational downstream data. Once again, there ought to be some investigation of the zone of heated water adjacent to the point of effluent discharge, to see if fishing is improved there. This is probably the most useful way to employ a creel census in pollution studies.*

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*Apparently this is now being done.

Large year-to-year variations are apparent for this as for other biological parameters. For a good but puzzling example, the average number of fish caught per man-hour, by section of the river censused, varied from 0.327 to 0.605 in 1968 (table 14), but in 1967 the range was far greater, from 0.235 to 1.363. Whether the differences are significant has not been tested. At any rate there appears to be no correlation between the sectional catch-rates in 1967 and those in 1968.

As a final point, it has been suggested that poor weather caused a decline in boating on the river from 127,000 hours in 1967 to 76,000 hours in 1968. However, over the same period shore camping rose more than 2-fold*, and water-skiing was up from 5,000 to 7,000 hours. As a part of creel census activity, some inquiry might be initiated to account for such variations.

12. Air monitoring. In this section the study by the Wisconsin Natural Resources Department is omitted, for inadequate reasons. It is not sufficient to state that "some results were at wide variance from NSP's recorders", and to cite one unusual instance of the same SO_2 level being reported at all five stations monitored. A study of the correlation (or lack of it), between the NSP and Wisconsin recorders would seem to be called for, together with a detailed examination of possible factors involved in any discrepancies.

It would be much more helpful to have data presented on maps rather than as bar graphs of the kind given. Relationships between stations, and their distances from the King Plant, could be much more readily appreciated from maps. In addition, it would be of interest to have a complete set of graphs showing the per cent of time air concentrations exceeded successively higher atmospheric SO_2 levels. Such graphs were shown for 1966 and 1967 at stations 8 and 9, but were omitted for 1968. If I read the data correctly, they would show a marked increase of SO_2 in 1968 at both stations. Incidentally, there seems to be a contradiction in the report for 1967, in that the graph for frequency of occurrence of increasing air concentrations of SO_2

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*Apparently owing partly to a change in definition of such activity.

shows station 9 as distinctly more polluted than station 8, whereas the bar graphs for quarterly number of SO_2 readings over 0.05 ppm (and the following tables) indicate that station 8 was the more polluted in the winter quarter when almost all high readings were observed.

In the air monitoring summary for 1968 it is stated (p. 2) that "A minor effect of the plant can be deduced in the spring, summer, and fall quarters." This conclusion stands unsupported, and the evidence for it should be adduced in a quantitative manner, preferably with supporting graphs or maps. Again, it is stated (p. 2) that "Sulfation rates are higher than in 1967", but no supporting quantitative data are discussed to establish how much higher, or why. Figure 3 shows the sort of thing that might be done, and demonstrates a distinctly higher sulfation rate from May to August 1968 than for the same period in 1967 at station 5, where high sulfation rates are generally observed. The effect is not clear at station 9 close by the King Plant. In this connection, station 9 is situated so that very few winds blow from the plant to the station (see wind roses in the report). Therefore, although closest to the plant, this site does not exhibit the highest sulfur levels. Relocation north or south of the plant should be considered.

Station 26 is noted as exhibiting the maximum mean sulfation, but apparently no investigation has been made to determine why this should be so. (Station 26 is the westernmost station, and almost due west of the King Plant, so that winds seldom blow from the plant and the local municipalities to the station.)

No effort seems to have been made in the report for 1968 to compare and correlate air SO_2 with sulfation rates. Figure 4 shows very little correlation, and this observation should be the subject of detailed inquiry, to find out the reason for the lack of correlation, and which of the two methods of sulfur estimation is more significant as a measure of plant emission. In this connection A. R. Meetham's book "Atmospheric Pollution, Its Origins and Prevention" (3rd ed., 1964, revised by D. W. Bottom and S. Cayton) indicates that sulfation is falling out of favor as a technique for monitoring sulfur emission.

Unfortunately, data for sulfur and dustfall are not compared with data from other rural, urban and industrial sites. Such a comparison

FIGURE 3. SEASONAL CHANGES IN SULFATION RATE AT (STA. 9), NEAR (STA. 5) AND FAR FROM (STA. 15) THE KING PLANT

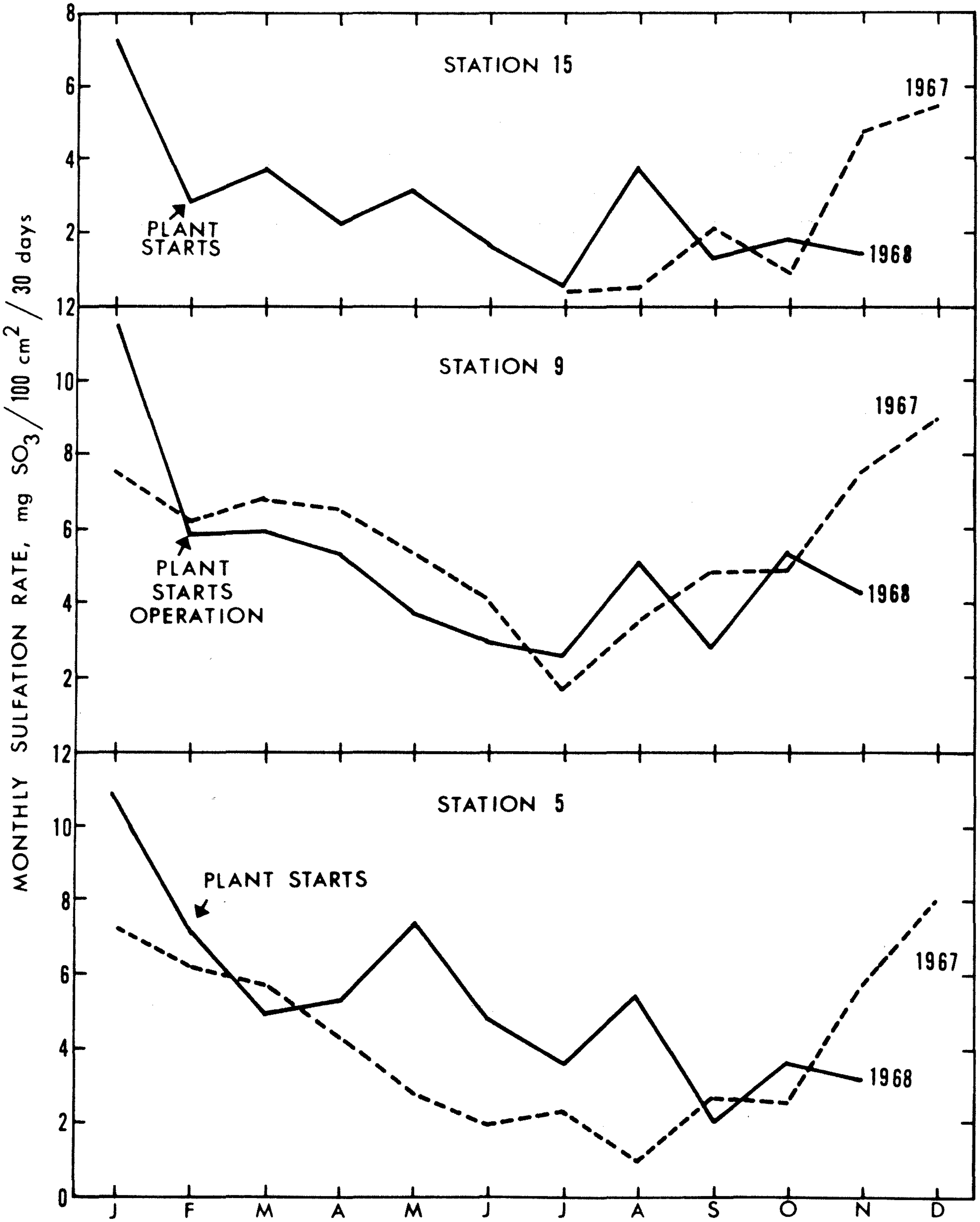
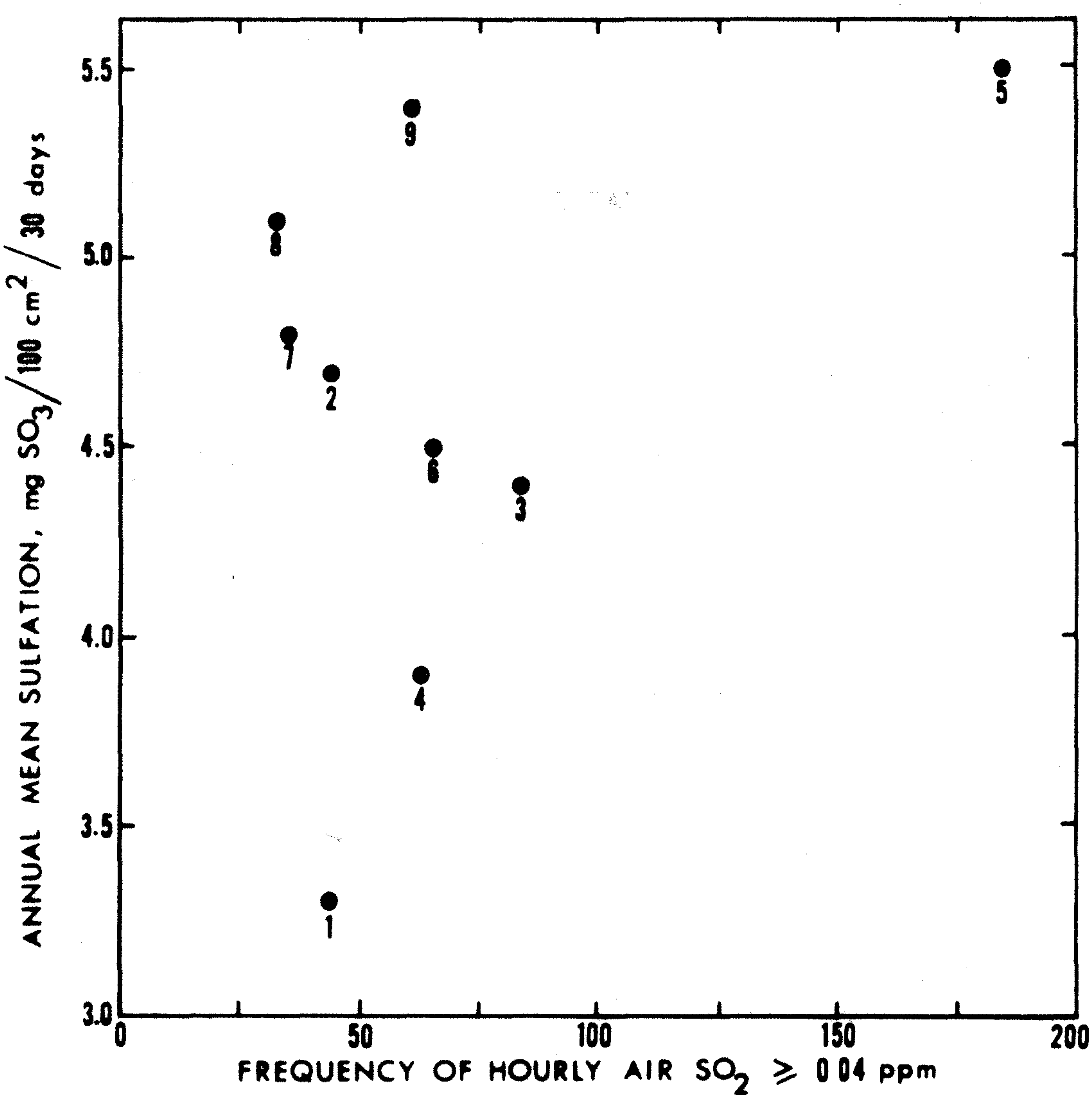


FIGURE 4. RELATIONSHIP BETWEEN AIR SO_2 AND SULFATION RATE AT NINE STATIONS



would be valuable, and was attempted in a very minor way for dustfall in 1966.

13. Plant pathology. It is unfortunate that four of nine plots of wild vegetation were interfered with. Protection against tree-cutting, mowing, etc., should have been arranged as far as possible beforehand. Because these plots contained very different sorts of plant communities, it would seem desirable to have established artificial plots planted to sensitive species (e.g., white pine seedlings, alfalfa), so that response would not be influenced by community variability. Possibly potted plants, grown in a uniform soil, would have been even better; and some of these could also be exposed to SO_2 in laboratory studies as baseline material to establish pollution effects.

The lichen plots are also rather variable (6 species, on 4 different tree species), and probably inadequate in number for results to have statistical significance.

Because the pines in the metropolitan area are already in considerably less than optimal condition, as shown by Prof. Grether, one would expect them to be particularly liable to sulfur pollution. It would seem desirable to visit these pines (and indeed other vegetation plots) more frequently than 5 times in the year (April 29, July 2, Aug. 13, Sept. 1, Oct. 29), and particularly at times when the individual sites have recently been subjected to fumigation by the plume from the King Plant smokestack, to determine whether such fumigations have any effect upon these species (or others in the vicinity). A mobile recorder, establishing SO_2 levels at various sites where the plume is observed to touch down, would be a useful adjunct to such studies.

One might ask whether the observation of Stoklasa, that SO_2 causes premature autumnal coloring (see report for 1967, p. 2) has been followed up, and also whether Skye's observation of bark acidification (report for 1968, p. 22) by urban pollution is being tested in the vicinity of the King Plant.

Finally, some mention should be made of the levels of sulfur dioxide pollution at which effects on various plant species might be expected. These could then be related to the levels observed so far by air monitoring in the vicinity of the A. S. King Plant.

SUMMARY

The environmental monitoring program for the A. S. King Plant on the Saint Croix River has shown that in 1968 substantial pollution damage was not observed in any major portion of the river or the surrounding land. However, the program is far from adequate to detect minor changes of a sort which might be cumulative and have a serious long-term impact. The program suffers greatly from inadequate sampling, and especially from the absence of aquatic stations well within the zone of heated discharge close to the mouth of the effluent ditch. In view of the rapid proliferation of generating plants, far more detailed studies are desirable in order to provide guidelines for future design elsewhere. The costs of such studies should not be borne by the operators of the plant, but by state and/or federal agencies.*

*Estimates by Professor W. C. Walton of the University Water Resources Research Center provide a guide to the costs of a more thorough monitoring effort. His proposal (dated April 1966) for a comprehensive research program, entitled Effects of Heat Rejection and Stack Gases on Biota, Fisheries and Hydrodynamics of Upper Lake St. Croix and on Air Quality, envisioned a team of more than 20 persons and a 5-year cost of approximately $2 1/2 million. It did not include a plant pathology study.