WATER RESEARCH TO 2020: ISSUES AND OPPORTUNITIES

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NWRI Contribution #86-28

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SUMMARY

Identifying emerging water issues, developing the knowledge base and introducing appropriate public policies to effectively and efficiently manage Canada's water resources for the next several decades is a major Canadian challenge. This challenge may profoundly affect the fabric of Canadian social and economic policies, and has the capacity to change the face of research within governments and universities.

This paper reflects water issues having environmental, social and economic consequences for the Canadian public and for which, according to the Final Report of the Inquiry on Federal Water Policy, federal leadership will be required. The research needs identified here largely pertain to knowledge gaps which inhibit effective policy advice to various levels of government.

Many emerging water issues are no longer small in scale and parochial in nature; they will be a manifestation of larger global phenomena, such as climate change which will be the backdrop for many of the large-scale water issues facing Canada over the next several decades. Recent estimates of acceleration of climate change puts the consequences within the amortization period of major engineering works. Within this context, public policy concerns will likely require research into such topics as long-range hydrological forecasting, drought forecasting and the use of non-standard proxy data to extend hydrological records, and incorporation of hydrological information into econometric modelling.

Managing toxic chemicals in the environment will require more focussed research on pathways, fate and effects of contaminants within land-water systems. Important developments will be required in the field of ecotoxicology to support environmental monitoring and interpretation. Greater knowledge is required on effects of chronic exposure of humans to toxins.

Knowledge gaps about arctic hydrology, aquatic ecology and toxic input place severe limits on long-range planning in the north. As yet it is not known whether toxic rain is a national concern. Climate change may well place greater stress on groundwater resources. Knowledge of persistence and fate of toxins in groundwater is poor.

Large savings can be made in the pollution control industry by operations research. Research into pollution control technologies have major economic implications for the Canadian environmental industry.

Socio-economic research is needed into legal and social implications of alternative environmental ethics, of various forms of demand management, of risk assessment, and means for dealing with uncertainty in public policy development.

To meet the challenge calls for a reassessment of the interrelationship of research and water management, and of the nature of university involvement in the national effort. Greater focus of effort around issues of national concern will be necessary. Some suggestions are provided.

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BACKGROUND

This paper was requested by the Science Council of Canada as part of its review of emerging water issues, research needs, and institutional mechanisms. The timeframe is that prescribed by the Council.

This paper is written from the perspective of water issues having environmental, social and economic consequences for the Canadian public and for which, according to the Final Report of the Inquiry on Federal Water Policy, federal leadership will be required. The research needs identified here largely pertain to knowledge gaps which inhibit effective policy advice to various levels of government. The topics covered are not priorized, are necessarily highly selective and do not imply that other types of water research are unimportant. Neither does this paper draw artificial barriers around basic, mission, or applied research; in almost all situations emerging water issues will require a blend of all three components to bring about a successful resolution.

INTRODUCTION

Identifying emerging water issues, developing the knowledge base and introducing appropriate public policies to effectively and efficiently manage Canada's water resources for the next several decades is a major Canadian challenge. It is a challenge which will profoundly affect the fabric of Canadian social and economic policies, has the capacity to change the face of research within governments and universities, has enormous potential for the Canadian environmental industry sector, and which can save billions by rationalizing short-term gains against long-term social and environmental costs.

Water issues in Canada have a pervasive influence within the Canadian social milieu. Partly this is historical; more importantly, Canada is running out of cheap, clean water. As outlined in the Final Report of the Enquiry on Federal Water Policy, Canadians and their various levels of government are now having to rethink the role of water both in terms of commodity value and as a pathway for adverse chemical impacts upon the environment and upon human health.

The nature of water issues is changing not only for quantitative and qualitative concerns, but also in scale. Many water issues which will emerge over the next several decades will no longer be local in scale and parochial in nature but will be increasingly complex in issue and uncertain of outcome. Some will be a manifestation of larger global phenomena, such as climate change. For many, such as atmospheric transport of pollutants, the impact will be felt far from the source of the pollutant. For many of these issues there will be profound scientific uncertainties. A challenge to governments will be to develop policy frameworks which can encompass uncertainty, especially for those water issues for which the consequences of policy deferral are too great.

Increasing cost of remedial actions to resolve water issues will change certain of the ground rules which govern research. It will be necessary to place the cost of environmental damage within a much larger socio-economic framework, especially as governments are now making closer links between environment and economy. As an example, the cost of controlling toxic discharges by legislating zero discharge is so large that it will be necessary for research to provide very specific answers to rates of environmental degradation, relative importance within and between environmental compartments, potential for ecosystem recovery, and specific consequences of inaction. This implies that research into major water issues will become increasingly focussed around policy requirements. This does not imply constraints to freedom of enquiry, but rather that the total effort will necessarily become more focussed.

1. CLIMATE CHANGE

Climate change provides the backdrop for many of the large-scale water issues which Canadians will face over the next several decades. Not only is it of fundamental concern for the long-term economic health and social well-being of the Canadian life-style, but recent estimates of acceleration of climate change puts the consequences within the amortization period of major engineering works. Climate change is an integral part and a source of great uncertainty in many emerging water issues. The importance to water managers is that as water resources are used to their limit the importance of variability or change has major impact on economic planning.

Long range hydrological forecasting research must be greatly improved and explicitly linked to predicted shifts in precipitation patterns. This will directly affect such issues as large-scale river diversions, drought research, water demand forecasting, and changing water levels. Research will be required to define the consequences of predicted temperature changes upon ice-cover of large lakes (especially the Great Lakes), and upon the kinds of ecological changes which are likely to occur in northern aquatic systems in response to changed temperature regimes. It is not known to what extent aquatic life may suffer from changing chemical composition of the atmosphere, including ozone depletion. A major research need will be the development of simulation modelling techniques with which to evaluate alternative policy options for dealing with the socio-economic implications of climate change.

2. HYDROLOGICAL/ENGINEERING/PHYSICAL

2A. Undoubtedly the single most complex engineering issue which Canadians may face will be that of LARGE-SCALE DIVERSIONS. The political and engineering ramifications have been widely discussed; the scientific concerns are less well known. The climatic, hydrological, and ecological implications of large scale diversion of northward flowing rivers is largely a matter of informed speculation as is the extent of social repercussions. For all aspects, basic and site-specific knowledge of the physics, biology and chemistry of northern aquatic ecosystems is insufficient for policy development purposes.

2B. An important aspect of long-term water demand forecasting is episodic DROUGHT. This is especially germane for the prairie provinces where significant supply/demand imbalances currently exist. The policy response to transboundary flow across the western provinces is apportionment. Drought, especially prolonged drought, coupled with a shift to greater natural variability in precipitation, has the capacity to cripple the prairie economy. Because hydrological records are insufficiently long and contain much internal variance, effective drought forecasting requires greater knowledge of drought over recent history. This requires integration of geochemical and biological research of proxy data with the shorter period of recorded discharge. For agriculture, the prospect of climate change and episodic drought requires that major research should be directed to improved irrigation techniques in order to conserve water, inhibit erosion, and to control the rate of salinization of western soils. The prospect of climate change and episodic drought have major implications for agricultural soil/water policies which are too numerous to deal with here.

2C. Managing the physical and chemical consequences of point and diffuse sources in large drainage systems requires detailed knowledge of the DYNAMICS OF SEDIMENT SOURCES AND PATHWAYS of fine-grained (silt and clay) materials. For water management, knowledge of: transport beyond edge-of-field, in-stream pathways, storage and remobilization, is poorly understood. This knowledge is central to rationalization of agricultural impacts upon surface water resources. As the principal carriers of many contaminants in aquatic systems, fine-grained sediments are central to detection and control of point and diffuse sources of toxic chemicals. Considerable effort must be expended to enhance the primary knowledge base, then to develop appropriate predictive models for point and diffuse management.

2D. Of particular importance to prairie regions within the next decade, and potentially beyond the year 2000 if climatic predictions prove correct, will be the spreading problem of soil SALINIZATION. Water is both the cause and the cure. Agricultural development, especially irrigation agriculture, has the potential to profoundly alter downstream and groundwater quality. Research will be required to predict the ecological and, therefore, economic consequences of increasing salt content of prairie lakes and reservoirs. Such research must be incorporated into drought research because desalinization requirements will be a competing demand within a long-term demand-forecasting framework.

2E. Application of REMOTE SENSING to water resource management issues is underdeveloped. With increasing sophistication and resolution of airborn and satellite platforms, and of multi-spectral imaging of surface and groundwater coupled with the computational power of the micro-computer, applications of remote imaging and data collection to water quantity and quality issues merits serious effort over the next several decades. In a country as large as Canada, remote sensing has the potential for cost-effective alternatives to more expensive on-site data collection techniques for a variety of water management issues.

2F. INFORMATION TRANSFER TECHNIQUES in the field of hydrological monitoring are not new. The ability to extrapolate beyond gauged sites is, however, underdeveloped for the purpose of network optimization. New techniques will be increasingly important for supplementing existing records and for predicting hydrological response of ungauged basins. The application of this research will become increasingly important for assessing Canada's water resources (quantity), especially as climate change renders historical discharge records increasingly inadequate for long-term water demand and allocation forecasting.

2G. The general field of HYDROLOGICAL MODELLING is not, of itself, a major research need for water management issues. This is a well established field where significant new breakthroughs are unlikely. This will, of course, continue to be a rich area of academic research for testing hypotheses, examining physical processes and exploring fundamental relationships. A major research need will be for techniques which incorporate hydrologic forecasting into econometric modelling.

2H. The development of ENGINEERING MODELS applicable to site-specific water issues will remain a low-key research activity. Issues such as ice-jam management, river engineering works, and coastal erosion prediction and management will benefit from improved understanding of interaction of water with boundary media. The objective of research in this field will be to improve the economics of problem management and to reduce the probability of unanticipated consequences.

3. TOXIC CHEMICALS

Toxic chemicals are undeniably Canadians' primary environmental and water-related concern. From the perspective of water management, toxic chemicals can be grouped into two interdependent issues -- public health and environmental health. There is some evidence that toxic chemical management programs are beginning to produce a positive response in certain aquatic systems. On the other hand, the complexity of environmental response to combinations of many toxic substances and, ultimately, exposure by man to multiple chemical intake is poorly understood.

Within North America at least, there is some room for optimism that toxic burden to environmental systems will begin to decline over the next decade. This will reflect increasingly stringent controls over release and disposal, more rigid registration procedures, and restrictive effluent criteria. With continuing innovation in biotechnology we may expect to see a slow movement towards alternatives to chemical pest management in agriculture and forestry. Regardless of these beneficial trends critical knowledge gaps must be filled, both to promote those policies which will accelerate these trends, and to deal with the chemical burden already in the environment plus those that will be added to the environment by design or by accident. The impact of toxic substances upon the environment appears to be also controlled by what have been perceived as issues of non-toxic significance -- for example: are nutrient control policies in the Great Lakes having the effect of exacerbating toxic phenomenon?; is sediment transport in rivers the cause of long-term chronic exposure to delisted hydrophobic chemicals?; is climate change likely to produce the greatest agricultural demand for those same lands which suffer greatest toxic deposition from atmospheric sources?, are observed changes in species composition in aquatic systems produced by chronic exposure to toxins? The knowledge gap seems endless!

The following facts or reasonable assumptions are pertinent to the development of a research strategy for toxic chemicals for the next several decades.

(i) Life cycle management of chemicals, while applicable for managing industrial chemicals, will not resolve environmental concerns for chemicals which are released into the environment as part of their use pattern (e.g. agricultural chemicals).

(ii) Policies which control agricultural chemicals (or, alternatively, control land use) must rely on knowledge of pathways, fate and effects of target chemicals within drainage systems in order to make cost-effective judgements on economic consequences of policy options.

(iii) Knowledge of pathways, fate and effects of target chemicals provides the basis for effective post-implementation monitoring of chemical management policies.

(iv) It is unlikely that there will be sufficient political will to embrace a zero-discharge philosophy without further evidence of adverse ecological and human health effects. Therefore, success in advocating increasingly restrictive chemical policies will rely on improvement in the knowledge base. Regardless of arguments about net economic benefits to GNP of an environmental industry, escalating costs of preventative technologies for affected industries will become so expensive that there will be disincentives to intervene with policy initiatives except where the environmental or social cost can be proven.

(v) While policies pertaining to human health will probably be driven by more conventional hazard/exposure/risk toxicological assessment, policies pertaining to aquatic ecosystem health will require impact verification for which a major expansion of the knowledge base of pathways, fate and effect of multiple chemicals and their degradation products will be needed.

(vi) There will be increasing public demand for chemical monitoring and assessment in aquatic systems. This is likely to take three forms: (1) systematic assessment of presence and effects of toxic chemicals under federal or federal/provincial mandates; (2) broadly-based analysis of trends and impacts through such vehicles as state of environment reporting, and (3) calculation of threat to human health based, quite possibly, on the allowable daily intake concept.

From the above policy-related concerns, we can draw certain conclusions concerning aquatic toxic chemicals research over the next several decades.

3A. Pathways, Fate and Effects Research.

Knowledge of pathways, fate and effects of toxic chemicals is essential not only for developing effective public policies that affect chemical use and discharge, but also for state of environment reporting. Such knowledge is critical if the gap between science's ability to measure chemicals in the environment and its lesser ability to interpret such data, is to be narrowed.

This research will continue to have five practical objectives: (1) understanding the nature of ecological response to toxic chemicals, including potential for and rate of recovery (2) producing practical means for public agencies to survey presence/absence and to make useful inferences about the ecological consequences, (3) providing data to assist in interpreting the implications for human health in terms of direct or indirect consumption of the contaminated resource, (4) providing data for assessing damage to an economic resource (e.g. fish), and (5) providing information required for pesticide registration.

The knowledge base is currently meager, with research isolated by traditional disciplinary boundaries, and the national effort poorly focussed. The toxicological properties of the vast majority of chemicals now found in the environment are poorly known, and properties of degradation products frequently not known at all! Knowledge of pathways through which chemicals are transported through and between environmental compartments is rudimentary; research into pathways, especially that performed by public agencies, tends to falter at the boundaries of agency mandates (air/water/forests/soils/etc.). Answers to questions such as: how far do toxic chemicals move?, under what conditions?, in association with what other chemicals?, how long will it last?, with what degradation products and with what ecological and/or human health implications?, are essential for formulating effective control policies. Few epidemiological studies relate observed toxins in the environment to human health. Such studies tend to be uncertain in outcome because of unknown causal or functional relationships between observations of chemical presence and observations of cancer morbidity and/or mortality. Very little is now known of total toxic intake by humans or what the maximum "acceptable" intake should be. While it appears that food is a quantitatively more important pathway than potable water, there is no agreement on human effects of chronic exposure to low levels of toxics in drinking water.

Research into pathways, fate and effects requires major new efforts into chemical structures, physico-chemical relationships, and biological cycling (food chain dynamics). It requires major new initiatives which investigate the transfer of toxic substances across land surfaces, down rivers, through the land-lake interface, and fate in receiving water bodies. In this regard, serious under-representation of lotic biology in Canadian academic research requires immediate attention if policy action in river basins is to be effective.

3B. Aquatic Ecotoxicology

This field has been widely identified as critical to assessment of toxic contaminants in aquatic systems. For environmental management purposes, this is the study of the environmental effects of groups of chemicals found at ambient concentrations. Whereas the toxicology of single chemicals establishes the "safe" concentration, the premise of ecotoxicology is that environmental systems (here, water) are subjected to many kinds of chemicals for which synergistic or antagonistic effects upon biological communities are quite different.

There has been much research into toxic effects of individual chemicals upon specific target organisms, both from the point of view of biomonitors, and of physiological response. Much of this work both in government and in universities has and continues to be of a pharmacological nature, or related to toxicological properties of specific chemicals for, for example, agricultural and food and drug administrative purposes. The broader issue of ecological response is, however, poorly understood due to complexity of the subject. Future research in ecotoxicology can be divided into two timeframes; in the shorter term our knowledge of ecological response will be severely limited by continuing deficiencies in our primary understanding of ecology of aquatic systems and of the rate of response to various degrees of environmental and chemical change. For practical purposes, therefore, requirements for environmental assessment over the next one to two decades will necessarily focus on development of sophisticated bioassay techniques which provide objective information from which environmental managers can make judgements about the potential for ecological hazard and risk in aquatic systems. In the longer term the objective must be to understand and quantify ecological behaviour and response to ambient levels of toxins in order to put in place better schemes for maintenance and use of aquatic resources.

3C. Toxic Chemical Monitoring

Governments are expected to provide continuing and authoritative assessments of trends and significance of toxic chemicals in the aquatic environment. Such programs should also provide information for assessing efficacy of remedial policies, and for building primary data bases for simulation modelling of linkages between biogeochemical realities and socio-economic implications of policy alternatives.

There is compelling need for major new research initiatives into monitoring techniques which will improve the efficiency and effectiveness of aquatic toxic chemical data collection and assessment programs. In the next decade research must produce new biological techniques for effects assessment of ambient water quality. These will be grafted onto standard surveillance and analytical programs. Over the longer term, however, the proliferation of chemicals of concern, the greatly expanded geographical areas of concern, and the inability of conventional programs to define ecological response, make it unlikely that public agencies will be able to afford to collect and analyze ambient data using conventional collection and single chemical analytical procedures. A major research challenge will be the development of new surveillance technologies related to stress assessment, early warning indicators, etc.. Essentially, these new technologies will be the fruit of mission-oriented research into ecotoxicology, with surveillance technique being one very significant form of technology transfer to meet operational requirements of public agencies.

3D. Water Quality Objectives

Over the next two decades water quality agencies will have to totally rethink the types of water quality objectives used to characterize acceptable levels of toxins in water. Research will be required to develop water quality objectives based upon aspects of ecological response to combinations of ambient chemicals. Initially, this will focus upon response of target organisms; later, however, it must include response of higher levels of ecological organization to energy and material flux within the ecosystem. This research must include explicit hazard assessment related to probability of exposure (risk).

3E. Analytical Methods

To support toxic chemicals research and operational program requirements there will be a need for increasingly sophisticated chemical analytic techniques. This will have two aspects; one will be the necessity to develop new analytical technologies (e.g. MS/MS) to increase the speed, accuracy and detection levels of chemicals of concern. The second aspect , however, is the development of new screening techniques which use biochemical assay technique as a means of reducing the number and frequency of routine chemical analyses. Analytical methods development will shift, therefore away from use of lower and lower detection limits as a primary investigative tool, to an integrated biogeochemical approach which can provide ecological and mass transfer information as well as data on specific chemicals.

A major development over the next two decades should be a series of new field-applicable assay techniques for toxic stress. A number of recent developments have promise, including bacterial assays and enzymatic response of larger organisms.

4. ACID/TOXIC "RAIN" (LRTAP)

Research needs for acid rain control will shift from primary understanding of aquatic system behaviour, to predictive techniques for assessing regional (aquatic) response to legislated emission reductions. In the near term there will have to be some shakeout of existing data collection programs through development and application of several forms of sensitivity analysis. The objective will be to increase efficiency and effectiveness of existing government programs for assessing social and economic consequences of future emission reduction options. Combined with this should be research into questions of long-term impairment of soil and forest fertility. Significant effort should be devoted to technology development for stack emission control.

It is probable that within the next decade it will be necessary to provide clear understanding of the nature and extent of atmospheric deposition of toxic substances (so-called "toxic rain"). Initially, this will be achieved by enhanced monitoring of atmospheric deposition and by investigating potentially significant diffuse sources of airborne toxins (e.g. air/water exchange of toxics in the Great Lakes, long-distance impacts of agricultural chemical drift). Coupled to this should be significantly increased study of recovery potential of aquatic ecology and of chemical deposition in northern environments. The objective will be to establish whether there is a significant problem. If the answer is yes, then research will be required to produce specific information for shaping federal policy requirements.

5. GROUNDWATER

There will be increasing concern over quality and quantity of groundwater resources, especially as climate change may dictate substantial increases in groundwater utilization. The quantity issue will be addressed by increased data collection programs. There will be need, however, for research into: consequences of climate change upon recharge rates and potential for irreversible change caused by drawdown and loss of permeability due to compaction of Pleistocene sediments. Groundwater quality issues will include such issues as nitrogen and chloride contamination, and salinity concerns associated with agriculture. For both quantity and quality purposes research is needed to increase the efficiency of data collection and interpretation in a field in which data acquisition costs are extremely high.

Increasingly, however, emphasis will be on contamination of groundwater resources. While this will frequently be site-specific and related to waste disposal, leaking underground storage facilities, etc., there will be a larger research need for expansion of knowledge of contaminant transport, persistence and degradation in aquifers, and for methods for safe in-ground disposal of toxic and radioactive wastes. For some parts of Canada it will likely be necessary to develop models which predict spatial and temporal trends of aquifer contamination for socio-economic forecasting purposes. For example, are the short-term gains of pesticide application worth the cost of long-term aquifer contamination? The ability to define alternatives assumes major importance given the large economic cost which accrues either from loss of the resource or from remedial requirements.

6. THE NORTH

Emerging aquatic concerns in northern Canada will be dictated by a complex blend of economic, strategic (i.e., geopolitical), touristic, native, and sovereignty issues. While it is impossible to predict when, or in what order, these are likely to impact on aquatic concerns, it is certain that all will have to accommodate a basic fact - - namely, that basic understanding of northern aquatic ecology, of aquatic physics, and of fluvial and sediment dynamics is inadequate. Although northern aquatic systems are not fragile in a conventional sense, recovery from thermal or chemical stress takes much longer. In the extreme case of climatic warming, it is not known what the nature of ecological response will be.

Major river impoundments, either for hydroelectric purposes or for diversion to the south are likely to have unforeseen consequences on northern terrestrial and aquatic ecology, and on thermal transport into arctic environments. Not only is it likely that this will exacerbate climatic shifts in Canada's north, but also such engineering programs may suffer extreme adverse economic consequences imparted by substantial changes in precipitation regimes due to climate change.

Increasing arctic haze is likely to have important effects upon arctic hydrology due to changing albedo of the lower atmosphere. Arctic haze is, in part, a visible manifestation of toxic input into northern aquatic systems by atmospheric processes. To what extent this will endanger freshwater tundra and boreal ecology over the next several decades is largely unknown. Because of the central importance of the Canadian arctic in global circulation patterns, Canadian research into chemical deposition and ecological response in northern environments will have geopolitical as well as national consequences. It is in the north that aquatic effects of global transport of toxics from developing countries is likely to have the greatest impact.

It is likely that petrochemical interest in the north will assume major importance within the next two to three decades. This includes development south of sixty but which impacts northward flowing rivers. Disposal of toxic wastes from this industry, spill management, etc., will require new research initiatives to establish adequate aquatic protection, especially in view of native life-styles on many of these rivers.

In the water quality field, continued economic development -- urban, mining, petrochemicals -- will require new knowledge which equates effluent standards with resiliency of aquatic habitat. Current water quality standards have questionable scientific validity in northern aquatic systems. In this context, research over the next two decades must focus upon stress assessment techniques. This is similar to toxics research noted above, but has one very major difference -- namely, that of defining the degree to which fragile northern habitats may be chemically stressed before irreversible change or collapse occurs.

Over the next several decades it is probable that several more national parks and/or reserves will be declared. This is likely to parallel an increasingly viable tourism industry. This, and northerners expressed interest in maintaining a pristine environment will require environmentally compatible development.

Aquatic research required in the north is, and will continue to be, largely focussed upon basic research into the nature and behaviour of northern ecosystems, and upon improved techniques for water quantity and quality measurement and forecasting. Efforts must, however, be coordinated so that the effort is compatible with identified policy concerns. Because of cost, logistics, distance, problem complexity, small manpower, and maintenance of research focus, it may be wise to consider greater coordination of research activities. The situation is not unlike that of Australia where information needs for policy initiatives in rural and remote parts of the country were coordinated by the Commonwealth Scientific and Industrial Research Organization (CSIRO).

7. ENVIRONMENTAL INDUSTRY

Associated with most of the above discussion is the premise that Canadians will demand major improvements in water treatment and pollution control technology, especially in the toxics field, over the foreseeable future. Just for the next decade estimates of required improvements in urban infrastructure are staggering. All of this in one way or another is related to protection of the aquatic environment, whether directly through effluent management, or indirectly by stack emission management. In the near-term research into systems design is capable of major gains in sewage-handling efficiency without recourse to capital expansion; not only will this free up large amounts of capital for utilization elsewhere in the economy, but will have a direct beneficial effect on ecological and recreational aquatic resources. Research over the longer term will be applied to removal and detoxification of dangerous chemicals from stack and sewage effluents both for municipal and for closed-loop industrial purposes.

Water managers, especially at the federal level, must not loose sight of the the fact that Canada has the potential of becoming a world leader in this field. The direct returns to the Canadian economy of an environmental industry are potentially huge. A major commitment to technological R&D over the next two decades at least, will be the entry price into this profitable field. Here is another direct link between environmental objectives and compatible economic objectives.

8. SOCIO-ECONOMIC RESEARCH

This paper has assumed that there will be no fundamental change in societal attitudes and, therefore, of political will, especially in regards to toxic chemical management. While not diminishing the need for biogeochemical science, there is urgent need for research into legal implications of existing and alternative environmental ethics. Just as there is need for 'hard' science input into public policy formulation, there is equal need for social science input into such policy alternatives as 'reverse onus of proof', or some form of environmental bill of rights.

For the water industry climate change will change the basic assumptions which underlie long-term socio-economic planning. More important, however, than predicting specific directions for socio-economic research in the water field over the next two decades, is the necessity to greatly improve the linkages between biogeochemical and socio-economic inputs to policy formulation for long-term water planning. Socio-economic research will be required to evaluate various forms of demand management, including water pricing mechanisms and water conservation practices. This has particular application where major diversions are anticipated, especially in view of projected precipitation changes. As an alternative to supply augmentation, particularly in prairie environments, serious attention must be given to opportunity costs associated with restoration of impaired surface waters.

In the normative field of policy analysis, the socio-economic implications of water policy cannot be predicted on a causal basis. Methods must be found to develop conceptual and policy frameworks which can handle the complex and uncertain relationships which underlie the environment-economy linkages both for current and 'futures' scenarios. Models tailored to specific concerns should be developed which provide insight into policy alternatives. This will require new combinations of physico-chemical and economic modelling. As an example, there is a much broader and largely unresearched area of information transfer modelling (spatial extrapolation) for water management purposes. It has broad implications for developing public policy options; it will incorporate physical and statistical components which relate the physical and chemical dynamics of drainage systems to socio-economic consequences. This type of management modelling could become a valuable tool for discriminating amongst many possible policy options for economic development within large drainage systems. Such models will also have important applications for assessing land use options, especially those which entail use of agricultural chemicals.

9. HOW TO MEET THE CHALLENGE

Because of what seems a bottomless pit of environmental science needs together with rapidly decreasing timeframes for achieving essential knowledge breakthroughs and information synthesis, it is clear that science policy direction and national leadership will be required to achieve timely results. As a general observation, governments will have to accept the realization that the Canadian scientific establishment is undermanned and under-resourced to meet these new research requirements. Our educational establishments must come to grips with the fact they are often teaching outmoded environmental science, are failing to provide adequate training and direction, and that leading Canadian water scientists are frequently located in government institutes. The situation is so severe that new relationships must be established between government research establishments (which have critical mass) and universities (which can provide specialized knowledge, expertise and manpower training).

To accomplish what has been outlined above calls for a reassessment of the interrelationship of research and water management. Clearly, "how" things are done both in research and in water mangement will necessarily evolve. Some suggestions follow:

(i) greater focus must be provided to the national effort.

(ii) research must be more coordinated around those issues for which policy guidance will be required.

(iii) improved interaction between university and government research facilities should be a policy goal.

(iv) improved functional integration of disciplinary expertise now segregated into several government departments.

(v) funding agencies should consider greater coordination of research activities for northern research.

(vi) greater international coordination of research dealing with significant policy concerns.

(vii) stronger linkages between researchers in Canada and those in other world-class research facilities.

To achieve much of what will be needed over the next several decades requires that thought be given to educational and training programs in the environmental field. This will be applicable both to researchers and to water managers. Indeed, water management and research must be explicitly linked if future policy objectives are to be achieved in an increasingly complex world.

In the past, water managers and researchers have tended to operate within a sectoral world, carrying out research and making decisions based upon their particular jurisdiction or mandate. While this approach has served Canada in the past, increasing complexity of issues, the complex interrelation of quantity and quality concerns, and the general backdrop of climate change, requires that water managers will require an increasingly sophisticated blend of talents and tools. Serious thought must be given to the question of where the next generation of water managers will come from. How will they achieve backgrounds which include such topics as decision theory and critical path analysis, risk assessment, plus a systematic background in physical, economic and social sciences?

In Canada, water resource science is broken into various disciplines with accompanying disciplinary biases. While that may be effective for training of disciplinary research scientists, the multidisciplinary requirements of the water research field are seriously underrepresented in Canadian tertiary educational. Additionally, there is a need for more coherent water management training within the context of a professional school. Indeed, the lack of adequate professional training is an important reason why universities have had little influence in the mainstream of public policy development in the environmental field. A major challenge to our university system, therefore, will be the ability to adjust to major new national needs of this type, not only in the management field but also in refocussed areas of more traditional disciplines. This challenge is not just a question of more money; it is a challenge to the management structure and decision-making processes of our major universities. Successful response to this challenge will have the effect of making universities more accountable to national requirements. The issue is vital insofar as, in the long term, our universities are the very foundation of the success or failure of national environmental policies.