sensitive to ECT. Memories acquired during the 3 years before ECT were impaired, but memories acquired before that time were not affected. The results therefore appear to confirm the hypothesis, originally formulated by Ribot (*12*), that the susceptibility of a memory to disruption is inversely proportional to its age. The validity of this conclusion depends, of course, on how well the remote memory test satisfies the requirements it was designed to meet. The most important of these is that names from each time period must be forgotten at the same average rate. Although this point is difficult to establish definitively with any retrospective method, considerable effort was made to meet this requirement by minimizing various kinds of sampling bias (*6*).

If resistance of memory to amnesic treatment can indeed increase over a period of years, then there must be two distinct consequences of the passage of time. First, material in memory becomes resistant to disruption over the years. Second, the same material becomes gradually more difficult to recall. Thus, names from 1971 to 1972 were remembered rather well, but were forgotten after ECT. Conversely, names from earlier time periods were remembered rather poorly, but were not affected by ECT. The neural substrate of memory apparently changes with the passage of time, such that resistance develops as forgetting occurs.

The frequent observation that retrograde amnesia can cover a time period of minutes, hours, or days has usually been interpreted to indicate that the memory storage process is labile for only a short period of time after learning and is then consolidated into a more stable state (*1*). The findings reported here indicate, however, that retrograde amnesia can sometimes cover a period of years. Accordingly, the development of resistance to amnesic treatment need not depend on the rapid transition from a labile to a stable memory process. The development of resistance can presumably depend on gradual changes in neural mechanisms that subserve enduring, relatively stable modifications in function.

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7. Five programs were taken from each of the 16 years, 1957 to 1972. Popular exposure to the programs was estimated from viewing data provided by the A. C. Nielsen Company. (i) In households having a television set, the percentage of time spent watching television has not varied by more than 2 percent for any year since 1965. Comparable data were not available for earlier years. Between 1965 and 1972 the percentage of American households having a television set rose from 94 to 97. (ii) Individual Nielsen ratings, which were provided for 81 percent of the 80 programs in the test, indicated that the popularity of the programs selected for each time period was quite similar ($F = 1.0$, $P > .3$). The validity of the test as a measure of remote memory for specific time periods has been tested in two ways. (i) High school students failed to recognize the names of programs broadcast when they were very young, but recognized more recent programs as well as or better than adults. (ii) Adults who had resided outside North America for a period of years did poorly on questions about programs broadcast while they were away. These results and details of test construction are presented in Squire and Slater (*6*).
8. The results were submitted to a three-way analysis of variance with repeated measures and then to an analysis of simple main effects to make before and after comparisons for each 2-year time period [Q. McNemar, *Psychological Statistics* (Wiley, New York, 1962), pp. 323–333].
9. Unilateral nondominant ECT impairs verbal memory less than bilateral ECT; however, it may relieve depressive symptoms somewhat more slowly. See M. Fink, in *Psychobiology of Convulsive Therapy*, M. Fink, S. Kety, J. McGaugh, T. A. Williams, Eds. (Wiley, New York, 1974), pp. 1–17; G. d'Elia, in *ibid.*, pp. 21–34.
10. We recently administered both forms of the television test to 16 persons who had received a course of 5 to 17 (mean = 9.9) bilateral ECT treatments about 6 months previously. Their test scores closely matched the scores of 56 control subjects (*6*). The two groups did not deviate from each other by more than 4 percent for any time period.
11. I. Janis [*J. Nerv. Ment. Dis.* **3**, 359 (1950)] reported that some remote memories for past personal events had not recovered by 10 to 14 weeks after a course of 8 to 27 bilateral treatments.
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Appraising Proposed Federal Standards for Water Resources Investment

In publishing articles on topics with respect to which most of its readers have no background, *Science* has a special obligation to select these articles carefully and to be certain that they and the references in them are balanced and accurate. A case in which these criteria have not been met is the recent article by Cicchetti, Davis, Hanke, and Haveman (*1*). The article is a critique of certain aspects of proposed federal standards for water resources planning and investment (*2, 3*). The weaknesses of the article are three. First, the authors do not make the reader aware of the context, including the scholarly background, from which the proposed new standards were derived. Second, on theoretical issues the authors do not adequately cite opposing views. The reader is thus not made aware that some of the authors' views are not universally, or even widely, accepted by scientists. Third, many of the references to the document reviewed by the authors are incomplete or inaccurate.

The standards under review by the authors are derived from a theory of public investment planning called multiobjective analysis. This theory was first set out in the classic *Design of Water-Resource Systems* (*4*). A more recent version can be found in *Guidelines for Project Evaluation* (*5*). In this latter work multiobjective planning is recommended for all economic sectors, not just for water resources. According to multiobjective theory, public projects should be designed explicitly in terms of a wide range of social, economic, and environmental objectives. This is a generalization of the traditional "economic" benefit-cost analysis that the proposed standards were meant to supplant. The authors neither describe this major theoretical development in project planning nor do they indicate that it provides the intellectual foundations for the proposed new standards; as a result, it is difficult for the reader to judge the nature and significance of the standards.

There are many theoretical issues on which the authors do not adequately cite opposing or alternative views. Anyone who peruses *Guidelines for Project Evaluation*, for example, will see this clearly. Two examples can be cited here by way of illustration. The authors say that "the basic purpose of public investments is to correct for private market failures . . ." (*1*, p. 723). Compare Eckstein: "Some government activities are intended to redistribute income" (*6*, p. 13). The authors also state: "the ideal pricing structure would equalize marginal social costs and marginal social benefits" (*1*, p. 727). Compare Marglin: "The futility of basing pricing policy on such simple rules as 'price equal to marginal cost' . . ." (*7*, pp. 91–92).

Finally, citations of the document are incomplete (the authors give no page references to the document except for an initial reference to the entire document, and references to the document are in important instances inaccurate). Several examples can be presented here. The authors refer to the fourth of the four accounts in the document as an income redistribution account (*1*, p. 723). In fact, the fourth account is called the "Social Factors" account. Four subclassifications are given for this account, of which income distribution is but one. [The others are life, health, and safety; emergency preparedness; and a catchall "other" category (*2*, p. 24146).] As another example, the authors say that "the procedures for evaluating environmental effects exclude a major component of environmental impact: the significance of options in the context of irreversible decisions" (*1*, p. 725). Yet the document refers to "irreversible commitments of resources to future uses" (*2*, p. 24146) as a factor to consider under the heading of beneficial and adverse effects on the environment, and later a reference is made to "beneficial effects resulting from the preservation of freedom of choice to future resource users by actions that minimize or avoid irreversible or irretrievable effects or, conversely, the adverse effects resulting from failure to take such actions" (*2*, p. 24162). The authors might wish to argue that the proposed standards do not give enough emphasis to the question, but surely it is misleading to state that the procedures "exclude" it.

As a final example, the authors state that ". . . the WRC [Water Resources Council] presents in voluminous detail the beneficial environmental effects that can stem from water undertakings and the procedures for their quantification; it treats the possible adverse effects in but a single sentence" (*1*, p. 725). The accuracy of this statement can easily be tested, since finding more than one sentence treating the adverse environmental effects of water undertakings proves it wrong. This is not difficult, since the document refers to adverse environmental effects on pages 24159, 24160, 24161, 24176 (table 3), 24187 (table), and 24192. It is worth noting that the authors use this flagrantly inaccurate reference as support for their proposition that the proposed standards will lead to an overcounting of the environmental benefits of water resources projects as compared to their environmental costs.

To conclude, the editors of *Science* cannot be commended for publishing an article that caters so little to the interests of its readers in receiving balanced and accurate information.

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3. A later version of these standards has been approved by the President and published as *Fed. Reg.* **38** (No. 174) (1973), part III, p. 24778. A proposed version that preceded both the final version and (*2*) can be found in *Report to the Water Resources Council by the Special Task Force: Principles for Planning Water and Land Resources* (U.S. Water Resources Council, Washington, D.C., 1970). This earlier version is, in my view, superior to the subsequent revisions.
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In his technical comment Major criticizes the editors of *Science* for accepting for publication our article which presents a critique of the proposed standards for water resources planning and investment. His claims are that the article does not (i) discuss the scholarly works on which the proposed standards rest, (ii) cite opposing views, and (iii) provide complete or accurate references to the Water Resources Council documents.

To Major's first charge we plead *nolo contendere*. The volumes which he cites are substantive additions to the public investment planning literature and, in a rough way, have guided the development of the proposed standards. The theory of multiple objective planning which they exposit is relevant to planning when numerous objectives are explicitly stated and trade-offs among them are well defined. In a public investment area such as water resources, designed to yield intermediate services to producers (irrigation, flood control, navigation), this situation does not prevail. Our position, which we believe is widely shared by economic analysts, is that the fundamental rationale for public sector activity in the water resources area is the failure of the market system to yield these services because of external effects or public goods problems. It seems doubtful that the federal government dredges rivers or builds dams as part of antipoverty or regional income redistribution policy. Hence, our critique rests on the standard analytical basis of benefit-cost analysis —the objective of maximizing national economic efficiency. This objective makes it possible to evaluate in a consistent, analytically sound framework *all* effects of a project which convey welfare changes on people, and it also avoids the potential for double-counting which the multiple objective framework of the proposed standards invites. Moreover, it is less subject to abuse and misuse in the hands of a construction agency evaluating its own projects.

We also concede Major's second point. The objective of our article was to criticize the proposed standards from the perspective of the accepted body of standard welfare economics. It was not to "cite opposing . . . views" on "many theoretical issues." As Major well knows, these issues have been examined thoroughly in the economics and planning literature. Moreover, although we would immediately consider that "some government activities are intended to redistribute income," it seems unlikely that rivers and harbors projects have such an intent. Also, a quick look around is all that is necessary to convince one that efforts to increase the efficiency of government pricing or user charges policy have been "futile."

Major's final point is nit-picking. For instance, he can find a number of references in the document to the adverse environmental effects of water projects only if he is willing to count essentially the same sentence several times. To do so, however, serves as a good example of the practice of double-counting which we feel is invited by the proposed standards.

We are concerned about the bureaucratic inclination to measure particular kinds of so-called "benefits," which tend to be but special categories of the general concept of gains in national economic welfare. This inclination leads to double-counting and is especially troublesome if the agency pleads lack of expertise when it comes to measuring costs. As economists, we see great flexibility in the national economic efficiency account to include, albeit only once, all of the types of benefits and costs that are discussed by Major and the Water Resources Council. Also, we are wary when the agency that constructs also performs the evaluation analyses.

To repeat the main thrust of our critique, it seems unwarranted for water resource planners to establish a set of standards emphasizing nonefficiency effects and secondary impacts, when their performance in accurately appraising the relatively easy-to-measure primary efficiency effects has been so inadequate. We say this while at the same time acknowledging the relevance of information on these nonefficiency impacts to project appraisal and choice.

In conclusion, we have two principal concerns. First, a separation of evaluation and construction must precede any accounting change. Second, when well-defined special interests benefit from a construction project, the collection of user costs is absolutely necessary.

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Morpholine as Olfactory Stimulus in Fish

Cooper and Hasler (*1*) reported electrophysiological evidence for retention of olfactory cues in homing salmon. They found significant differences in the magnitude of the evoked electroencephalographic (EEG) response to 1 percent morpholine (10^4 mg/liter) for homing coho salmon exposed to morpholine at 5×10^{-5} mg/liter as fingerlings 1 month before smolting as compared to salmon not exposed to morpholine as fingerlings.

Salmon and trout are considered to be macrosmatic on the basis of both behavioral and electrophysiological observations. Recent electrophysiological studies have shown that both salmon and trout respond to certain amino acids at extremely low concentrations, and that stimulatory effectiveness is closely related to molecular structure (*2*). A variety of chemicals such as alcohols, aliphatic acids, and amines, which are highly odorous to humans, are not always stimulatory to these fishes (*2, 3*). There is no evidence, behavioral or electrophysiological, indicating the involvement of olfaction in the detection of morpholine by fish. Wisby (*4*), who first studied the effect of morpholine, found that concentrations as low as 10^{-5} and 10^{-6} mg/liter were repellent to coho salmon fry. However, he failed to show that the salmon detected morpholine by olfaction. The concentrations of morpholine used by Cooper and Hasler (*1*) are well within the range in which morpholine is repellent to salmon fry, whether by olfaction or not. The data reported here suggest that the morpholine effect on which their research is based may be a nonspecific irritational effect elicited by a physiological mechanism other than olfaction.

Rainbow trout (*Salmo gairdneri*) were studied by methods described (*2, 5*). The effect of morpholine at lower concentrations (0.01 and 0.1 percent) is primarily inhibition of the spontaneous background activity followed by a slight afterresponse. At 1 percent, which is the same concentration employed by Cooper and Hasler and approximately 10^8 times higher than that of the lowest threshold determined electrophysiologically for the most stimulatory amino acids, the background EEG activity is slowly replaced by an oscillatory potential that is not terminated by rinsing. Figure 1A shows a typical effect induced by application of 1 percent morpholine in the nares for 10 seconds. The response shown here is for the last of three consecutive stimuli. This morpholine effect differs from the normal olfactory response (for example, Fig. 1B) in that (i) there is a long delay in the morpholine reaction, (ii) the effect builds up gradually and is sustained over a long period after rinsing, and (iii) this period increases with repeated stimulation. This evidence suggests that the morpholine solution may penetrate deep into the olfactory epithelium and cause a nonspecific irritational effect at nonspecialized cell surfaces. It also seems likely that the effect is caused by the high *p*H of the 1 percent morpholine solution (*p*H of 10.2). The normal olfactory response in fish is highly *p*H-dependent and is almost entirely inhibited at *p*H higher than 9 (*6*).

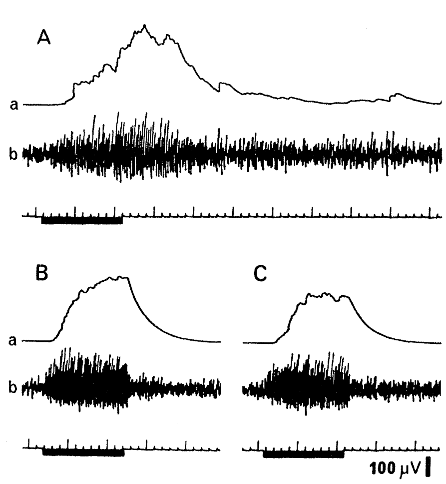

Fig. 1. (A) Effect of 1 percent morpholine on EEG activity of the olfactory bulb of rainbow trout. This record is for the last of three consecutive stimuli, each of 10-second duration and applied at 2-minute intervals. In this record and those in (B) and (C), the upper traces (a) show the integrations of the lower (b). Heavy lines below each record indicate the duration of the stimuli; small hatch marks, 1 second. (B) Typical response to $10^{-5}M$ L-serine. (C) Response to $10^{-5}M$ L-serine after three consecutive applications of 1 percent morpholine (10-second duration, at 2-minute intervals), followed by rinsing for 5 minutes. The response magnitude is reduced compared to (B).