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Identifying and Measuring Nonuse Values for Natural and Environmental Resources

A Critical Review of the State of the Art



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Identifying and Measuring Nonuse Values for Natural and Environmental Resources

A Critical Review of the State of the Art

Health and Environmental Affairs Department

PUBLICATION NUMBER 316

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Section 1 OVERVIEW AND PURPOSE OF THE STUDY

The purpose of this study is to review the state of the art for identifying nonuse values for natural and environmental resources and for measuring them with the Contingent Valuation Method (CVM). The intent is to move beyond the taxonomic survey of potential biases which is found in a number of existing works such as Cummings, Brookshire and Schulze [1986] and Mitchell and Carson [1989]. We attempt to keep attention sharply focused on a set of basic issues which, in our view, should be at the core of contemporary debate concerning nonuse values and the CVM. These issues are: the theoretical foundation of nonuse values; the conditions under which nonuse values can be measured; the theoretical foundation of the CVM; and the extent to which responses to "willingness to pay" questions posed in applications of the CVM can be interpreted as values that represent a real economic commitment.

To set the stage for our study, we briefly sketch our major conclusions and describe the manner in which the study is organized. We must first, however, provide the reader with an appropriate perspective for the notion of "conventional wisdoms" and their importance for the method of inquiry used in the study, as well as for the manner in which our conclusions regarding nonuse values and the CVM *must* be interpreted.

"Conventional wisdom" refers to knowledge of what is true or right ("wisdom") that is established by convention, general consent, or accepted usage ("conventional").¹ Conventional wisdoms regarding one position or another may be implied by assertions of "fact" ("*this*" is the way it is, or "everyone knows that..."), by references or citations to many people that have accepted the position, or by appeal to substantive evidence which supports the position. Conventional wisdoms may not be correct. They can change when knowledge about what is true or right changes. *Assertions* of conventional wisdom for a position may not be correct in cases where "factual" statements are incorrect, citations of people supporting the position are incorrect (they do *not* support the position), or when evidence posited to support the conventional wisdom is shown to support a contrary position. Appeal to conventional wisdom can be a powerful argumentative tool when it imposes on opposing views the burden of proof that the conventional wisdom is incorrect for one or more of the reasons given above.

¹ American Heritage Dictionary [1982; pp. 319, 1386].

A review of the state of the art in a subdiscipline would typically involve the simple process of bringing together the relevant conventional wisdoms. In total, these wisdoms constitute the state of the art. All else equal, such a process would make our task a particularly easy one in the sense that there is certainly no shortage of conventional wisdoms regarding nonuse values and the CVM. As examples, the following exemplify a conventional wisdom concerning the composition of individual values and our knowledge of existence values that are based upon asserted fact and an appeal to substantive evidence:

"...total economic value is made up of five components: (1) onsite recreation use of the resource; (2) commercial use of the resource; (3) an option demand from maintaining the potential to visit the resource in the future; (4) an existence value derived from simply knowing the resource exists in a preserved state; and (5) a bequest value derived by individuals from knowing that future generations will be able to enjoy existence or use of a resource." Loomis, Hanemann and Kanninen [1991; pp. 412-413]

"Contingent valuation studies have also documented considerable willingness to pay for existence. For example, Schulze *et al.* (1983) found that estimates of willingness to pay to preserve visibility at the Grand Canyon were dominated by existence values. Other contingent valuation studies that found a willingness to pay for existence were reported by Brookshire, Eubanks and Randall (1983)." Boyce, Brown, McClelland, Peterson and Schulze [1989; pp. 306-307]

Here is an example of "fact" concerning a strong theoretical foundation underlying the CVM:

"Constructed markets [the CVM] enjoy a very strong theoretical foundation. (...) Constructed markets, in principle and in contrast to other benefit measurement techniques, can directly obtain WTP or WTA." Carson [1991; p. 123]

And here are examples of "facts" related to people's behavior in CVM studies: in offering values for environmental and natural resource goods, people do not behave strategically, they tell the truth.

"... experimental work, such as that reported by Vernon Smith [1980] is of interest. In experimental public goods markets with relatively weak incentives for accurate value revelation, Smith finds that most subjects accurately report their personal (induced) valuations." Randall, Hoehn and Brookshire [1983; p.638]

"... the specter of Samuelson's strategic bias proposition remained as a concern ... until the appearance of Vernon Smith's [1977] report of experimental evidence that further belied the strategic bias proposition." Cummings, Brookshire and Schulze [1986; p.16]

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In citing these examples, we do not mean to infer that all researchers concerned with nonuse value and the CVM accept the positions implied by the citations. Many do not and we attempt to draw this distinction in our later discussions. Statements of the type exemplified above are sufficiently common, however, to convey the impression of established conventions or accepted usage -- conventional wisdoms. This may particularly be the case for the unwary reader of the literature.

In any case, a state of the art assessment based upon *these* conventional wisdoms, and we document many related assertions in our study, would likely take the following form. Existence values "exist," they are measurable, and they are large, relative to any total value. The CVM enjoys a strong theoretical foundation and can be used to measure a broad range of values when implemented carefully. Finally, values derived from the CVM may be generally taken as representing truthfully reported values by participants in CVM studies. Surely, then, a method that enjoys this state of the art can serve as a basis for deriving reasonable values.

But What if the Conventional Wisdoms are Incorrect? Our review of the state of the art for nonuse values and the CVM addresses this question: are the conventional wisdoms regarding nonuse values and the CVM indeed correct? We examine the studies cited as providing evidence that existence values exist and are measurable, the factual basis of claims that the CVM is based upon a strong theoretical foundation; the studies cited as providing evidence that CVM subjects will generally report truthful values, and results from empirical studies regarding the extent to which people will actually pay amounts that they say they will pay in the CVM. Results from these analyses are laid bare for the reader's examination. Effectively, we ask the reader to join us in asking: are these results consistent with the conventional wisdoms for nonuse values and the CVM?

As will become apparent, our reading leads us to conclusions which are in stark contrast with the conventional wisdoms and state of the art characterized above. We hope to make clear, however, that these contrasts are not simply the result of differences in opinion between us and other scholars. They result from simply "listening" to the data that form the substance of the evidentiary pillars which are claimed to support these wisdoms. The reader, of course, will draw his or her own conclusions on this point. We attempt, although not always successfully, to facilitate broad participation in this regard by trying to maintain a level of exposition that

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may be accessible to the nontechnical reader.

A final observation may be useful in setting the stage for our study and for providing a context for our conclusions sketched below, particularly as they relate to the CVM. Over the last two decades or so, hundreds of studies have been conducted which make use of the CVM in one form or another.² The qualifying clause "one form or another" is important inasmuch as few if any of these studies are strictly comparable. The researcher's judgment plays a large role in the development and application of a CVM.³ Indeed, Carson and Mitchell [1992; p. 2] describe such judgment as it relates to questionnaire design in the CVM as a form of *art*:

"In any event, reliability and validity are at best relative concepts. A fair assessment of work during the last decade on *all* of the nonmarket valuation techniques is that none of them are (sic) automatically produce reliable and valid answers. A fair amount of *art* is required to assess the value of a natural resource. For hedonic pricing and travel cost analysis, this art comes primarily in the econometric specification; for contingent valuation, it comes largely in the form of question wording. In both cases the problem is that natural resources *do not* and *can not* have a *true* value which is context independent."⁴

Just a *few* examples of the many judgments required of the researcher in applying the CVM are:

- the amount and kinds of information regarding a good which is to be given to subjects.
- the framing of valuation questions.
- pretests of questionnaires and *interpretations* of results.
- the choice of a sample population and the choice of an elicitation mode.
- the treatment of zero and outlier values and the choice of statistical methods.

² The bulk of these studies focus on total, as opposed to nonuse, values.

³ For example, Carson *et al.* [1991; p. 3] argues that "In the course of designing a contingent valuation survey...the researcher inevitably must make and justify a number of design decisions which often have no obviously correct answers."

⁴ Many would argue that econometric specification in the CVM is more an art than a science. As but one example, see Bowker and Stoll [1988]. Without implying our acceptance of the argument, we also note Schulze's [1992] position that seems to argue that some natural resources may have values that *are* independent of context: those for which subjects attitudes are "crystallized."

While there exist works to which one may appeal for guidance in making judgments concerning one aspect of CVM design or another,⁵ there exists no common standard for "good" judgments regarding the many complex and interrelated issues which arise in the preparation and implementation of the CVM. While surveys have long been used to obtain information regarding public opinions and attitudes, there exists no compelling evidence that suggests that standards for survey design that have been developed for these uses are applicable in any comprehensive sense to the uses contemplated by CVM surveys. The difference between CVM surveys, wherein respondents are asked to truthfully reveal a real economic commitment to pay some amount of money, and surveys designed to elicit opinions, is made manifest in Mitchell and Carson's [1989; p. 188] description of the key problem facing the CVM practitioner:

"The key problem facing the designer of a CV study, we saw, is the novelty of valuing a public good, given the respondents' varying degrees of familiarity with the good being valued and how they currently pay for its provision."

Thus, as noted by Hoehn and Randall [1987; p. 241], "Not all CVM applications are created equal and differences among formats are likely to influence CVM performance." The problem noted above is that there exists no common standard that might be used for differentiating between a CVM that is "well-born" and one that is "poorly-born."⁶ One can find fault or point to weakness of one kind or another in virtually *every* CVM that has been conducted. But how important is one weakness or another? What does "important" *mean*? In any broad sense, the state of the art of the CVM is one wherein we cannot respond to these questions.

These questions relate to an issue that we regard as fundamental. To paraphrase Hoehn and Randall [1987], all *issues* regarding the CVM are not born equal. Thus, much has been written concerning biases in CVM value estimates that may be attributable to such things as

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⁵ Examples of basic references for questionnaire and survey design include Dillman [1978] [1983] and Schuman and Presser [1981].

⁶ For example, "None of these conclusions say anything about the validity of a particular CV study, of course. The multidimensional character of validity and the absence of a clear-cut criterion against which to compare CV values for public goods means that the validity of individual studies cannot be established in a definitive fashion. Each contingent valuation study should be able to survive scrutiny of its scenario and its estimation procedures for content validity, and should provide evidence for its theoretical validity in the form of either theoretically based regression equations or experimental findings based on split-sample comparisons." (Mitchell and Carson [1989; p. 209]) In these regards, one should note that most published CVM studies do not provide the full range of information required for the comprehensive analyses suggested by Mitchell and Carson [1989].

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the choice of a payment vehicle, the specificity of good descriptions, or whether or not a bidding process is used.⁷ We do not deny the importance of these issues. However, there exists a set of issues that are fundamental to the *raison d'etre* for the CVM which remain unresolved. Our argument is that the resolution of these issues should receive priority, in terms of our preoccupation with methods development questions, over those associated with survey design.⁸

These considerations are basic to our choice of an approach to the task of reviewing the state of the art for identifying and measuring nonuse and total values for environmental assets. We do not attempt a critical review of every CVM study. Our concern with design issues is generally limited to assessments of the theoretical foundation for WTP questions. Our concern is with what we describe above as fundamental issues that are relevant for *any* application of the CVM. Thus, *any* application of the CVM presupposes some theoretical or conceptual basis for operationally meaningful hypotheses which it is intended to test in one way or another. We then inquire as to the substance of the theoretical foundation which is assumed, implicitly or explicitly, in *all* applications of the CVM. *Any* application of the CVM assumes that the process in which subjects formulate values for a particular good is in some sense cogent and has some logical relationship with a well-defined choice set. Thus, we examine the extent to which these assumptions are supported by empirical evidence. *Any* application of the CVM "values" will reasonably⁹ approximate amounts that people will actually pay for a good. We inquire as to the extent to which empirical evidence supports these assumptions.

The conclusions that we draw are described in a general fashion as follows. In the main, we conclude that existing conventional wisdoms concerning separable nonuse values of an individual are simply incorrect. A conceptual basis for total values of a nonu*ser* can be justified, as can use and nonuse values of a user. In terms of use and nonuse values of a user, we know of no practical means by which existing conceptual models can be made operational, however. We conclude that any claim that the CVM enjoys a "strong theoretical foundation" is not based on unequivocal evidence. Empirical tests of the consistency between

⁷ See, as examples, Cummings, Brookshire and Schulze [1986], Mitchell and Carson [1989] and, more recently, Carson [1991], Carson and Mitchell [1992] and Schulze [1992].

⁸ For an example of what would appear to be an opposing view regarding priority, see Carson [1991] or Schulze [1992].

⁹ The question as to what "reasonably" might mean is taken up in Section 3, Do CVM Values Closely Approximate Real Economic Commitments?

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individual behavior in the CVM and the assumptions of received theory are at the *very* best inconclusive. Unequivocal evidence does not exist that would support the conventional wisdom that CVM subjects do not behave strategically. Finally, any assertion that CVM values are reasonable estimates of values which people hold for natural and environmental resources is without foundation, if by "reasonable" one means that CVM values are close approximations of amounts that people will actually pay for a good. Available empirical evidence suggests that large differences may exist between amounts that people say they will pay in a CVM and amounts that they will actually pay.

Our conclusions may fail to fully satisfy readers with purely academic interests in the CVM as a method for valuing public goods, as well as readers concerned only with the bottom line concerning the substance of values estimated with the CVM.

The academician may well inquire if our intent is to suggest the lack of any basis for intellectual interest in the CVM. This is certainly not our intention. For the experimental purposes which motivate academic interest in the CVM as a possible means for valuing public goods, the CVM may remain as a topic of interest for many scholars; it does for us. For these purposes, the academician may regard as interesting the simple finding that CVM values are "correct" in terms of having theoretically predicted signs (the values are positive, not negative). If, however, one is to put CVM values to uses which presume any large degree of accuracy, such uses enjoy neither theoretical nor empirical justification. Our conclusions are intended to suggest a need to carefully re-examine our "knowledge" of the substance and structure of the CVM, to recognize that many issues that we have regarded as being resolved in fact remain unresolved, and that until they are resolved the CVM is best viewed as a method that remains at a relatively infant stage of experimental inquiry. They then may be seen as an echo of the appeal by Kopp and Smith [1989; p. 607] for intellectual introspection as to what we know and don't know regarding these issues:

"[referring to the 'new level of specificity' for CVM values required by its use in CERCLA adjudications...] we should evaluate what we know and, equally important, what we don't know about valuing natural resources *as assets* that provide diverse services supporting a wide range of utility-generating activities."

To readers with interests only in the bottom line, *do* nonuse values exist and *can* they be measured with the CVM, we can only respond in the following way. Nonuse values can be justified conceptually. In terms of their measurement with the CVM, one can obtain estimates

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for them. How such estimates are to be interpreted is another issue. We know little at this point about the precise relationship between nonuse values or total values obtained with the CVM and values that reflect real economic commitments. The sparse evidence that we have in these regards suggests that this relationship will likely be very imprecise, with CVM estimates overstating real economic commitments by substantial amounts.

Our argument is structured as follows. The theoretical foundation of nonuse values is considered in Section 2. The theory of option values is discussed, and the evolution of other nonuse values is described and critically analyzed. Section 3 addresses the CVM as a means for measuring values for nonpriced goods by users and nonusers. Attention is focused on two questions: is the CVM based upon a strong theoretical foundation, and does existing empirical evidence demonstrate that people will actually pay what they say they will pay in the CVM? Conclusions are set out in Section 4.

A final observation is warranted before we move on to the arguments to be developed in our report. Our limited focus on the small set of issues that we argue to be central to assessments of nonuse values and the CVM has expository advantages and disadvantages. A major advantage is that it may serve to suggest caveats for uses of the CVM and to suggest priorities for future methods development research. A major disadvantage may be that, in limiting our discussions of colleagues' papers to aspects that relate to the issues of interest here, we fail to give full credit when it is due. A few examples may serve to make this point clear. We take sharp issue with interpretations given specific aspects of earlier works by such scholars as Bill Schulze, David Brookshire and Alan Randall, among others. Time and resources prohibit a broader review of all of their works, a review that would place the works that we cite in their appropriate state of the art context. Only in that context can the contributions of these scholars in providing intellectual leadership for imaginative and innovative inquiry regarding public goods valuation be fully appreciated. In terms of more recent works, we are particularly critical of positions taken vis-a-vis the set of issues of interest here by Robert Mitchell and Richard Carson [1989]. Their 1989 book is considered by many to be one of the "bibles" on the CVM. Positions taken by or attributed to these and other scholars that relate to what we refer to as "fundamental issues" are important for the conventional wisdoms of concern to us. We wish to make clear, however, that our critiques of aspects of work by these scholars are not meant to detract from the weight of their more general intellectual contributions to the state of the art.

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Section 2

OPERATIONALLY MEANINGFUL THEOREMS FOR NONUSE VALUES

A. OVERVIEW

There is a considerable literature on the benefits associated with public goods in general, and natural resources in particular. In reading a good part of this literature one is invited to accept as a matter of course that nonuse values are well established in economic theory and that their magnitude is large *vis-a-vis* total values associated with environmental resources. Moreover, the same applies with reference to the alleged "components" of nonuse values: option value, existence value and bequest value, to name but a few. In Table 1 we provide a small sample of citations which refer to such values. These references suggest a state of the art in which motive-related values enjoy a presumption of validity, are measurable, have been measured, and are "large" relative to any use-related value.

Our concern in this section is with this conventional wisdom concerning nonuse values. We address three interrelated sets of questions. First, to what extent are concepts of nonuse values based on operationally meaningful theorems in economics? Second, based on an assessment of the published works which are cited as providing empirical evidence of the existence of these many nonuse values, do the studies actually show what they purport to show: a derivation of nonuse values and their decomposition into various types of nonuse values? Finally, we ask: is there a well-reasoned rationale for our interest in nonuse values?

To provide the reader with an overview of the topics to be discussed in this section, we briefly outline the main line of argument.

We continually refer to the notion of *operationally meaningful theorems*, so we begin with a brief discussion of this concept (subsection B). The notion of a nonuse value, as it relates to the valuation of natural and environmental resources, has its origins in discussions related to a value associated with use: *option value* (subsection C). While, as we argue, the option value notion was lacking in terms of operational meaningfulness, it played an important role in enhancing economic analyses of decisions affecting unique and irreplaceable assets (subsection D).

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Table 2-1. Selected Citations Referring to Nonuse Values

"Many of the formal inquiries using CVM...give evidence that nonuse benefits of improvements in water quality (which include option value as well as existence value) tend to be some fraction of the use value (...) Other research...shows that existence value is greater than use value, and in the (Schulze *et al.*, 1983) case, is substantially greater." Madariaga and McConnell [1987; p. 936]

"...(citing, among others, Schulze *et al.* [1983]) the magnitude of the estimated option value in relation to the estimate of the expected consumer surplus has been much larger than expected from theory." V.K. Smith [1987; p. 283]

"Ambiguities may be present in existence values as well (as option value), but such information collected from well-run studies can be useful in making choices where resources might be irreversibly destroyed (Brookshire, Eubanks and Randall, 1983)." Kaiser, Brown and Davis [1988; p. 11]

"Contingent valuation studies have also documented considerable willingness to pay for existence. For example, Schulze *et al.* (1983) found that estimates of willingness to pay to preserve visibility at the Grand Canyon were dominated by existence values. Other contingent valuation studies that found a willingness to pay for existence were reported by Brookshire, Eubanks and Randall (1983). (...) While (earlier studies) provide conclusive evidence that some members of our society value the preservation of species or areas of special significance, they offer little indication of the pervasiveness of existence value." Boyce, Brown, McClelland, Peterson and Schulze [1989; pp. 306-307]

"The most popular decomposition (of total values) is between use and existence values. This happens because existence values typically are not measured by other benefit measurement techniques, such as travel cost analysis. The exclusion of existence values creates a bias in the travel cost analysis...." Carson [1991; pp. 127-128])

"In contrast to use values, which exist because people are physically affected by an amenity, existence values, or more generally nonuse values, embody the notion that a person need not visit a physical site or use services from that site to gain utility from its maintenance or improvement. The motives for existence values usually stem from vicarious consumption or stewardship concerns." Carson and Martin [1991; p. 393]

"...total economic value is made up of five components: (1) onsite recreation use of the resource; (2) commercial use of the resource; (3) an option demand from maintaining the potential to visit the resource in the future; (4) an existence value derived from simply knowing the resource exists in a preserved state; and (5) a bequest value derived by individuals from knowing that future generations will be able to enjoy existence or use of a resource." Loomis, Hanemann and Kanninen [1991; pp. 412-413]

"In addition to economic values of onsite recreation use and commercial uses of wildlife, there are many offsite user values. These include option, existence, and bequest values, all of which can be held by the general population as well as recreationists." Cooper and Loomis [1991; p. 448]

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Assume for the moment that we can estimate an individual's maximum willingness to pay (WTP), or *total value*, for such assets.¹⁰ In this case, the relevance of option or nonuse values, as components of total value, dissipates for other than pedagogic purposes. The 1964-1980 period saw many efforts to develop methods for measuring total WTP (subsection E). These efforts parallel in time the debate concerning option values, but are somewhat set apart from that debate.

From 1981 on, we begin to find option value theories merged with efforts to value public goods (subsection F). This is the period of time in which we see the ascension of nonuse values, particularly "existence" values, to an asserted position of primary importance. We examine in detail the studies which are most often cited in the literature as providing compelling evidence that nonuse values can be measured and that they constitute a large proportion of individual's total WTP for an environmental good, and briefly comment on other studies that explore means by which nonuse values might be measured. Efforts to theoretically decompose total values into use and nonuse components are reviewed in subsection G.

We conclude this section with an effort to bring all of these points together to the end of presenting our view of the importance and significance of nonuse values in terms of the state of the art for valuing natural and environmental resource values (subsection H).

B. WHAT ARE OPERATIONALLY MEANINGFUL THEOREMS?

Referring to economic theory in general, Samuelson [1947; p.4] defined an operationally meaningful theorem as:

... simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions. A meaningful theorem may be false. It may be valid but of trivial importance. Its validity may be indeterminate, and practically difficult or impossible to determine. Thus, with existing data, it may be impossible to check upon the hypothesis that the demand for salt is of elasticity -1.0. But it is meaningful because under ideal circumstances an experiment could be devised whereby one could hope to refute the hypothesis.

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¹⁰ This assumption sets aside questions as to the substance and reliability of WTP measures for natural and environmental resources derived with the CVM or other methods. These issues are addressed in Section 3.

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A theorem or hypothesis concerning values which individuals might place on nonmarket goods is operationally meaningful only if we can conceive of an experiment whereby the hypothesis might be refuted.

Our purpose in stressing the importance of distinguishing between theorems and operationally meaningful theorems is not simply pedagogical. The distinction is relevant for our critique of the literature on nonuse values in which we find a persistent failure to distinguish between *observable behavior* and the *unobservable motivations* (or tastes or preferences) of individuals.¹¹ Thus, in terms of the hypothesis that individual demand for any good is inversely related to the good's price, we can observe changes in prices and observe any changes in purchases of the good. We can readily conceive of observations that would refute the hypothesis. On the other hand, a theorem (implicit to a number of nonuse value studies reviewed below) stating that "individuals offer this value *because* they value the very existence (or any other asserted nonuse motive) of a good" is not operationally meaningful and can be given no higher status than an assertion. We cannot observe tastes or motivations. We cannot observe *why* a subject offers any particular value. One may have opinions as to motives that lead subjects to particular actions, but these are not to be confused with operational theorems.

C. IN THE BEGINNING THERE WAS OPTION VALUE

The nonuse value concept in environmental and resource economics had its origin in a scholarly debate concerning a very specific problem. The contributions of Burton Weisbrod [1964] and John Krutilla [1967] to this debate are widely viewed as seminal. In *Capitalism and Freedom*, Milton Friedman [1962; pp. 31 ff.] argued that a public facility, such as a national park, should be closed if it cannot be maintained by user fees. This view was challenged by Weisbrod, who posits that closure of the park could result in the land being put to residential or commercial uses. He further assumes that it would be prohibitively costly to revert to a park use of the land at some future date. Since the closure effectively represents

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¹¹ Samuelson [1947; p.3/4] offers an observation that we will later show comes uncomfortably close to describing some of the literature on nonuse values: "...only the smallest fraction of economic writings...has been concerned with the derivation of operationally meaningful theorems...We do not have to dig deep to find examples...Take a little bit of bad psychology, add a dash of bad philosophy and ethics, and liberal quantities of bad logic, and any economist can prove that the demand curve for a commodity is negatively inclined." Two studies that explicitly raise the question of the operational meaning of distinctions between use and nonuse values are Smith, Desvousges and Freeman [1985; p. 6-7] and Freeman [1992; p. 8ff.].

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an irreversible decision, we refer to the park as an irreplaceable asset.¹² Weisbrod then poses the question: how do we value such an asset?

Weisbrod's theory of option value consists of a line of deductive reasoning. There *may* be a number of individuals who, while they do not use the park or are infrequent users, would like to see the park retained and would be willing to pay some amount to see it retained. Since they do not use the park, or use it only infrequently, they do not pay significant fees in aggregate. Moreover, there exists no mechanism by which the fees which they *might* pay to keep the park open can be recognized. Weisbrod refers to the maximum amount of these fees which these nonusers would be willing to pay to keep the park open as an "option value." As the name implies, this value relates to the amount which nonusers and infrequent users would pay to retain their *option* to use the park at any future time. Note that option value as set out by Weisbrod is a *use*-related concept.

An individual's WTP for insurance, for having some asset on "stand-by," or for protecting one's options, is not limited to parks. Weisbrod notes the possible willingness of individuals who have never visited, and hope never to visit, a hospital to vote for bonds required to build or maintain a hospital.

There are two important aspects of Weisbrod's theory for present purposes. First, his premise is that individuals may be willing to pay for an asset that they do not use as a result of their preferences related to a desire to maintain their option to use it. He does not consider *other* motivations (e.g., bequest) which might lead an individual to the same behavior -- paying to maintain an asset which the individual does not use.¹³

¹² Some later writers have casually suggested that "irreplaceability" is somehow essential to the question of option demand or, more generally, nonuse values. It is useful to note that the characteristic of "irreplaceability" is just an extreme case of the situation posited by Weisbrod. He clearly envisaged less extreme situations as being relevant for option demand: "Infrequency and uncertainty of [future] purchase are not the only conditions bringing about a potential deviation of optimal private from optimal social behavior. In the present context there is another requirement: expansion or recommencement of production at the time any occasional-purchasers wish to make a purchase must be difficult (in time or resources) or impossible; this implies that storage of the commodity (service) must be expensive -- at the limit, impossible. In the case of a natural phenomenon such as Sequoia National Forest, if the trees were destroyed (allocated to alternative use), centuries would be required to restore them." (p.474).

¹³ This should not be taken as a criticism of Weisbrod's argument, so much as an implicit caveat that has been ignored by later writers.

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Secondly, Weisbrod does not offer his theory as being an operationally meaningful one.¹⁴ On the contrary, he notes the absence of markets by which relevant options might be purchased. Weisbrod sees the importance of option demand arising in situations where user charges fall short of the incremental cost of providing services. In such cases, recognition of option demand may dictate the necessity of public operation of the enterprise (p. 476).

These ideas are extended by Krutilla [1967] beyond the use-related setting of Weisbrod's arguments to include nonuse related considerations. Like Weisbrod, Krutilla's concern is with the underestimate of true social values of an irreplaceable asset which would result from a sole reliance on user fees. His argument extends naturally beyond user fees to any metric which captures only directly observed values from users. Krutilla makes compelling the case that *some* individuals will hold nonuse values for *some* irreplaceable assets.

Two aspects of Krutilla's arguments concern us here: his introduction of "existence" and "bequest" motives, and the operational meaningfulness of his conclusions.

Krutilla, unlike Weisbrod, does consider tastes, preferences or motivations which might lead a nonuser to be willing to pay some amount to maintain an irreplaceable asset other than those which are related to any strict interpretation of an individual's desire to maintain his or her options for future use. He argues that an option demand may exist even among nonusers who place a value on the mere existence of such assets (p. 781), or among those with "bequest" motivations (p. 781, fn. 11). It is useful to consider carefully Krutilla's exposition of the *bequest* and *existence* motives which might give rise to an option value or, more generally, any nonuse value.¹⁵ All references by Krutilla to these motives are set out in

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¹⁴ In the strict sense Weisbrod does concern himself with the operationally meaningfulness of his claims as to the existence of option value. He argues clearly that: "This 'option value' should influence the decision of whether or not to close the park and turn it to an alternative use. But it probably will not exert any influence if the private market is allocating resources, because there may be *no practical mechanism* by which the entrepreneur can charge nonusers for this option. Schemes to charge them *can be imagined*, but non-coercive devices may be *extremely difficult to implement*." [1964; p.472, emphasis added]. He sets out an explicit procedure (p.472, fn.3) to elicit values from agents that do not use the park in a given year, noting clearly how difficult it would be to practically implement. He also explicitly recognizes the problem of free-riding responses to his procedure, but subsequent literature on demand-revelation suggests procedures that can address this problem (e.g., the Groves-Clarke Pivot Mechanism).

¹⁵ We note that in some parts of his arguments Krutilla does not distinguish between values reflecting motives related to the preservation of options and nonuse motives. For example, "An option (continued...)

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Table 2. We see in these statements no more than arguments as to *possible motives* which might lead a nonuser to value an irreplaceable asset.¹⁶ An individual's value for the asset may be motivated by preferences related to preserving an option for use, an option for use by future generations, or the mere existence of the asset. We find nothing in these arguments that would suggest Krutilla's intention to argue that these motivations are in some sense distinct and separable, and that one therefore looks to separate and additive values associated with the individuals preferences related to option, bequest and existence motives.

Krutilla made no claims for the operational significance of his expanded view of nonuse value.¹⁷ There is no suggestion in his work that one might observe monetary values derived from option, bequest or existence motives, other than in extremely imperfect ways.¹⁸ Krutilla looks to what he views as the compelling *a priori* case that *some* individuals hold such values as making a *prima facie* case for the conservation of unique environmental assets.

There is a subsequent literature which attempts to formalize the option value concept and to probe its relationship to total values which individuals might place on the loss of an asset. The following relationship evolved slowly: a total value, called Option Price (denoted OP) is the sum of Option Value (OV) and Expected Consumer Surplus from anticipated future use of the asset (ECS). Thus we have $OP \equiv OV + ECS$. Option value was viewed as motivated by

¹⁵(...continued)

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demand may exist...among others who place a value on the mere existence...." (p. 781) Contemporary conventions make such a distinction. Option value is use-related, and existence value, bequest value, etc. are related to nonuse motives.

¹⁶ Or, by logical extension, motives which might lead a user to value the resource at levels that exceed any reasonable use-related level.

¹⁷ Krutilla [1967; p.785] explicitly concludes that we "have virtually no knowledge about the possible magnitude of the option demand. (...) Obviously, a great deal of research in these areas is necessary before we can hope to apply formal decision criteria comparable to current benefit-cost criteria. Fully useful results may be very long in coming; what then is a sensible way to proceed in the interim?" The interim solution proposed is to use a pessimistic min-max decision rule (to determine the "*minimum* reserve to avoid potentially *grossly adverse* consequences for human welfare") quantified with "educated guesses."

¹⁸ He argues (p.781) as follows: "If a genuine value for retaining an option in these respects exists, why has not a market developed? To some extent, and for certain purposes, it has. Where a small natural area in some locality in the United States is threatened, the proposal is often purchased by the Nature Conservancy [...], a private organization which raises funds through voluntary subscription [...]. But this market is grossly imperfect."

Table 2-2. Krutilla on Existence and Bequest Motives

REFERENCES TO "EXISTENCE"

"...[referring to natural environments that have no close substitutes] it is not clear even on theoretic grounds that a comparison of the total area under the demand curve on the one hand and market receipts on the other will yield an unambiguous answer to the allocative question. When the existence of a grand scenic wonder or a unique and fragile ecosystem is involved, its preservation and continued availability are a significant part of the real income of many individuals [footnote 7]." p. 779.

[footnote 7: "These would be the spiritual descendants of John Muir, the present members of the Sierra Club, National Wildlife Federation, Audubon Society and others to whom the loss of a species or the disfigurement of a scenic area causes acute distress and a sense of genuine relative impoverishment."]

"There are many persons who obtain satisfaction from mere knowledge that part of wilderness North America remain even though they would be appalled by the prospect of being exposed to it.... An option demand may exist therefore not only among persons currently and prospectively active in the market for the object of the demand, but among others who place a value on the mere existence of biological and/or geomorphological variety and its widespread distribution [footnote 11]." p. 781.

[footnote 11: The phenomena discussed may have an exclusive sentimental basis, but if we consider the "bequest motivation" in economic behavior, discussed below, it may be explained by an interest in preserving an option for one's heirs to view or use the object in question."]

"If a genuine value for retaining an option in these respects exists, why has not a market developed?... perhaps of greatest significance is that the preservation of the grand scenic wonders, threatened species, and the like involves comparatively large land tracts which are not of merely local interest. Thus, all of the problems of organizing a market for public goods arise. Potential purchasers of options may be expected to bide time in the expectation that others will meet the necessary cost, thus eliminating cost to themselves. Since the mere existence or preservation of the natural environment in question satisfies the demand, those who do not subscribe cannot be excluded except by the failure to enroll sufficient subscribers for its preservation." pp. 781/2

REFERENCES TO "BEQUEST"

"... [within the context of technological progress, while other goods may realize continuous expansion] by reason of scientific discovery and mastery of technique, the supply of natural phenomena is virtually inelastic. That is, we may preserve the natural environment which remains to provide amenities of this sort for the future, but there are significant limitations on reproducing it in the future should we fail to preserve it." p.783

"We are coming to realize that consumption-saving behavior is motivated by the desire to leave one's heirs an estate... A bequest of maximum value would require an appropriate mix of public and private assets, and, equally, the appropriate mix of opportunities to enjoy amenities experienced directly from association with the natural environment along with readily producible goods. But the option to enjoy the grand scenic wonders for the bulk of the population depends upon their provision as public goods." "... some portion of the estate would need to be in assets which yield collective consumption goods of appreciating future value." p.784

"Given the irreversibility of converted natural environments, however, it will not be possible to achieve a level of well-being in the future that would have been possible had the conversion of natural environments been retarded. That this should be of concern to members of the present generation may be attributable to the bequest motivation in private economic behavior as much as to a sense of public responsibility." p.785

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a desire for insurance¹⁹, and led to extensive theoretical studies into the effects of riskspreading on the structure of an option value.²⁰ Potential impacts on the size of OV from asymmetric technologies were explored.²¹ The debate expanded to questions concerning whether or not OV might double-count consumer surplus²² and the conditions under which it might be negative in sign.²³ The importance of income and substitution effects on an OV was stressed. The role of uncertainty, both as to future demands and future conditions of supply, was explored at some length.²⁴

Detailed examination of the twists and turns in the option value literature during the 1970s is irrelevant for our primary interests. All of these studies share one thing in common with Weisbrod [1964] and Krutilla [1967]: none argues for the operational meaningfulness of the option value concept. While one does not find in this literature arguments that option value might in fact be measured in some separable sense, arguments concerning the operational meaningfulness of theorems and hypotheses related to other values do arise. Specifically, the issue of whether or not one might be able to measure the ECS component of an OP is addressed by Cicchetti and Freeman [1971] and Krutilla, Cicchetti, Freeman and Russell [1972], amongst others.

Beginning with the posited relationship between OP, OV and ECS noted above (viz., $OP \equiv OV + ECS$), and acknowledging our inability to observe OP and OV, these studies inquire as to

¹⁹ The notion of a *quasi-option value* is introduced by Arrow and Fisher [1974]. A quasi-option value is said to arise in instances where today's decision is not in the nature of a "once-and-forever preserve or don't preserve" choice concerning the asset, but one where we decide whether or not to postpone the "preserve or don't preserve the asset" decision until some future time at which more information is available. Quasi-option value is essentially a value for information (see Conrad [1980], Fisher and Hanemann [1987] and Freeman [1984a]).

²⁰ See, e.g., Schmalensee [1972], Cook and Graham [1977], and Zeckhauser [1969].

²¹ The notion here is that activities (e.g., hydroelectric power) which might replace an environmental asset (e.g., the Grand Canyon) may be affected by technological advance which lowers the price of their output, while such advances will not affect the environmental asset. On the contrary, with rising incomes over time, the demand (and therefore the price or value) of the environmental asset should rise. Moreover, the rent of some asset is directly influenced by the value of the next foregone alternative, so anything that changes the value of such alternatives will change the rent that must be imputed to an asset. See Krutilla, Cicchetti, Freeman and Russell [1972] for further discussion.

²² See Byerlee [1971].

²³ See Schmalensee [1972], Freeman [1984] and V.K. Smith [1984].

²⁴ See Graham [1981] and V.K. Smith [1979] [1987].

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how large OV would have to be to substantively affect decisions concerning the preservation of an environmental asset. For the social benefits from preserving the asset to be greater or equal to the cost of preserving the asset (denoted COST) we must have COST < θ + ECS, where θ denotes the unknown OV. At issue here is how large θ needs to be so as to bring about this condition. These studies proceed to estimate ECS for the purpose of comparing it with an estimate of COST and then deriving the "necessary" magnitude of θ if the asset is to be preserved. There is no claim that option value is being estimated directly or indirectly.

Along similar lines Schmalensee [1972] argues that the valuation of environmental assets using only measures of ECS might be accepted as a "best available technology" given that OV cannot be observed.²⁵ He arrives at this conclusion by showing theoretically that OV for an individual could well be positive or negative, depending on a myriad of unobservable characteristics of preferences of the individual. Arguing implicitly from the perspective of "the principle of insufficient reason" that there is no *a priori* reason for OV to necessarily take on positive or negative values, he suggests that the best guess at the likely value is zero. Tenuous as this argument is, it demonstrates well the perception of the literature at the time that one could not hope to ever operationalize the estimation of option value.

D. OPTION VALUE, PERSPECTIVE AND THE PRE-1980 STATE OF THE ART

It is important to place the literature concerning option value in its proper perspective in terms of the time at which the research was undertaken. The question to be addressed here is why the notion of option value was considered an important one at the time.

Until the early 1980s, the view held by *most* economists was that one could not hope to measure total values for public or nonmarket goods. This is *not* to say that *all* economists

²⁵ A brief comment is in order as to the operational meaningfulness of any theorems underlying efforts to measure ECS. Standard assumptions about individual behavior allow one to derive a theorem which essentially states that an individual's valuation for some future use is the present value of the satisfaction or utility which he expects to derive from that use. This specific value cannot be directly observed, however. As noted by Bohm [1975], conceptually ECS must consist of: the individual's perceptions of all likely future uses of the asset, the individual's valuation of each use, and the individual's subjective probability that the use will take place. Since these contingent perceptions and valuations cannot be observed either, efforts by Krutilla, Cicchetti, Fisher and Russell [1972] to measure ECS were based upon extrapolations of aggregate data. Aggregate visitation at parks is derived from projections of future populations and projected characteristics (income) of the future populations, which are then used to estimate future visitation. Values are derived from projected user fees (and considerations of travel costs). Aggregative probabilities of use are based upon past and projected participation rates of the population.

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held this view. Paralleling (in time) the option value debate were attempts to develop methods for obtaining total values for public goods.²⁶ It was generally the case that economists had accepted the proposition set out by Samuelson [1954] that efforts to derive such values by any means involving an individual being asked to reveal his true maximum WTP for a public good would be doomed by the incentives for individuals to behave strategically and free-ride. If the individual felt that his actual tax or payment would be linked to his expressed WTP, but that the public good's provision level did not depend on his expressed WTP, then the individual would understate the true WTP and thereby free-ride on the expected contributions of others. If such a link was not present, an individual interested in having the public good provided would be motivated to overstate a true value in hopes of affecting the decision concerning the provision of the good.

In an intellectual milieu where many believed that we could not observe the total value which individuals place on environmental assets, the importance of the option value concept was immediate. It established the principle that use-related values such as entrance fees *could understate* any measure of social benefits attributable to the asset.²⁷ Social decisions based on comparisons of costs with benefits so understated could then result in a misallocation of resources. In this context, the option value concept was surely important, notwithstanding its lack of operational meaningfulness. Even if it could not be observed and therefore measured, it served as a reminder to policy makers that decisions which are irreversible in nature should not be based on *strict* adherence to a benefit-cost test based solely on measurable benefits. The benefit side of the equation was claimed to be flawed, and perhaps substantially so.

Is the concept of an option or nonuse value of any importance in other contexts, particularly those in which we believe that we *can* measure an individual's *total* value for an environmental asset? Given the problem ("we can't measure total value") which motivated interest in the option value argument, one can only speculate as to whether Weisbrod [1964] or Krutilla [1967] would have thought about nonuse value at all if they had thought that an individual's maximum WTP for public goods was observable.

Whatever the answer to this question, however, it is difficult to imagine a motivation for one's

²⁶ See Freeman [1979] for a thoughtful overview of the state of the art for public goods valuation at the close the 1970s.

²⁷ To the extent that OV can theoretically be negative, however, a presumption that ECS is positive does not ensure that OP (\equiv OV + ECS) is always going to be positive.

interest in *disaggregating* a measure for maximum WTP into motive-related components (even if one could do so, which we will argue that you cannot). Accepting for the moment the relationship $OP \equiv OV + ECS$, if we know OP then what could possibly motivate our interest in knowing OV or ECS? We can think of no application of benefit-cost analysis or damage assessment for which we might wish to pick out OV or ECS and compare it with some component of cost.²⁸

One possible argument for wanting to disaggregate use and nonuse values, as we see it, is to apportion financing charges proportionately for some public good. The argument would run along the same lines introduced by Weisbrod [1964]. If we can readily identify the users of some resource or commodity, then there is relatively little conceptual or practical problem in getting them to pay for the commodity, providing it is a private good or has significant private good aspects (e.g., a subway system). The nonusers, however, cannot by construction be excluded from consumption if they do not reveal themselves by the act of consumption. If we could estimate the importance of nonuse value in relation to use value, however, we could determine the extent of a subsidy to come from nonusers *via* general taxation (and a rebate given users due to their being charged twice *via* taxes and tickets).²⁹

We do acknowledge one further reason for having to identify the use-value component of total value: uncertainty over the legal status of total values that include nonuse values, or that make no effort to factor them out. Prior to the ruling in *State of Ohio v. U.S. Dept. of the Interior* (880 F.2d 432, D.C. Cir. 1989) on July 14, 1989, the Department of the Interior had promulgated regulations in August 1986 for damage assessment in certain cases that limited damages to the lesser of restoration cost or lost use value. The Court struck down "the lesser

²⁸ Carson [1991; p.128, fn.6] makes this point very clearly. It is also apparent when one confronts the type of policy problems for which CVM is being used. Good examples include the World Bank's efforts to value alternative water supply scenarios in developing countries (see Briscoe *et al.* [1990]) or the Australian Government's efforts to decide whether or not to mine near the Kakadu National Park (see Imber, Stevenson and Wilks [1991]). Issues of nonuse values are mentioned in these studies, but there is no interest in attempting to measure them since the CVM provides estimates of total value which is all that is needed for the policy exercise. How reliable those estimates are is a matter for discussion in Section 3.

²⁹ Such a scheme is not so straightforward in practical terms, however. We must distinguish between values of non*users* and values of users which may include nonuse motives. An ethical question arises. If nonusers are to be taxed their value, what of the nonuse component of user's values (setting aside, for the moment, problems with measuring such a component)? The confounding implications of nonuse values *of users* for any practical considerations of an equitable rebate designed to offset double charges are immediately obvious.

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of" rule, and allowed lost nonuse values to be claimed as well as lost use values. Given the uncertainty over the legal status of nonuse value during this period, it is natural for litigants to want to have damage assessments prepared with and without nonuse values. Hence a derived demand for studies attempting to identify and measure nonuse values as a component of total values.

E. SEPARATE BUT PARALLEL ADVANCES IN METHODS FOR VALUING PUBLIC GOODS

During the period extending from the early 1960s through about 1980, a relatively small group of scholars was involved in efforts to develop methods by which one might obtain an individual's maximum WTP for a particular environmental asset. Two aspects of this research are important for the present line of argument. First, the public goods of interest for these researchers were not limited to those of the "irreplaceable" genre. Secondly, the research was concerned solely with obtaining a *total* value for the good under study. In initial empirical studies of environmental values, option or nonuse values are not at issue.³⁰

While we see in this period the early stages of efforts to develop a number of methods which might be used to value nonmarket goods, we limit our focus to the Contingent Valuation Method (CVM).³¹ Given the influence of Samuelson's [1954] earlier-mentioned "strategic bias" proposition on the economic profession's view of the public goods valuation issue, we should note that by the late-1970s the strength of that proposition was viewed by many as having been weakened by results from empirical tests of the strategic bias hypothesis presented by Vernon Smith [1977] [1979a] [1979b] and Bohm [1972] [1979].³² This view is assessed in some detail below in Section 3, *CVM Valuation Behavior and the Incentives Assumption*.

³² For a contrary view, see Freeman [1979; Chapters 5, 10].

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³⁰ The potential relationship between total values for environmental improvements and nonuse values is noted in other works, however. See, for example, Freeman [1979; p. 249].

³¹ The CVM was also known initially as the "bidding" or "asking" game. Experiments with two other methods, and their variants, were also taking place during this period: the Hedonic Price Method and the Travel Cost Method. For an overview of these methods, see, as examples, Freeman [1979], Cummings, Brookshire and Schulze [1986], Mitchell and Carson [1989], and Braden and Kolstad [1991].

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Throughout this period, the CVM was used to value a number of diverse natural resource and environmental goods. A nonexhaustive list of examples includes aesthetic visibility, recreation facilities, elk hunting, noise pollution, and the maintenance of instream flows.³³ Ignoring for the moment questions concerning the accuracy of measures derived with the CVM, our present concern is with one aspect of these studies: are the methods used to obtain estimates for values based on operationally meaningful theorems?

As a conceptual basis for their valuation methods, which involved simply asking people their maximum WTP for a posited environmental change, these studies appeal to the "bid curve" developed by Bradford [1970] and based on arguments introduced by Samuelson [1954]. For market goods one can logically derive from the premises of value theory testable hypotheses concerning behavioral responses (choices of quantities) to changes in prices. Such hypotheses are operationally meaningful inasmuch as we can conceive of experiments whereby individual responses to changes in prices can be observed. Similarly, one can conceive of experiments that change the quantity available and elicit the new demand price (or WTP) at the new quantity.³⁴

In the case of public goods, "quantity" changes may in fact involve changes in quality. This will be particularly common for environmental goods. Appeal can be made to received value theory to derive hypotheses concerning behavioral responses (changes in prices or values) to changes in quantity or quality. Such hypotheses are operationally meaningful inasmuch as we can conceive of experiments whereby the quantity or quality of a public good is changed, and then observe behavioral responses (a reported WTP). Samuelson [1954] and Bradford [1970] demonstrate that the appropriate measure of social benefits implied by this approach is the vertical summation of individuals' observed maximum WTP at each given quantity.

This discussion anticipates the problems with the CVM to be taken up in Section 3: how is the quantity or quality of the environmental good being changed communicated to the subject, and how are we to interpret the value which is "observed?"

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³³ See Randall, Ives and Eastman [1974], Rowe, d'Arge and Brookshire [1980], Brookshire, Ives and Schulze [1976], Loomis [1986] [1987], Brookshire, Eubanks and Randali [1983], Thayer [1981], Greenley, Walsh and Young [1981] and Daubert and Young [1981].

³⁴ As we discuss below in Section 3, when the change in quantity and WTP are *hypothetical*, a critical assumption must be accepted: that the "observed" WTP is an amount that individuals will actually pay.

F. MEASURING DISTINCT NONUSE VALUES WITH THE CVM

We return now to the topic of primary importance in this section: nonuse values. Beginning in the early 1980s, the focus of many empirical studies with the CVM turns to the *components* of total value estimates and to attempts to measure these components separately. We find this shift in emphasis in CVM research to be curious. All else equal, CVM researchers might have found the option value literature to be of little interest for them. Lacking a means for directly estimating total values associated with nonpriced goods, contributors to this literature viewed option value as a vehicle for arguing that available value measures based solely upon use might be expected to underestimate total values for nonpriced goods, particularly those of the irreplaceable genre. However, CVM researchers had the means for estimating total values. The rationale for concern with option values, or any other motive-related value, was then moot.

In any case, this focus quickly led to a proliferation of assertions concerning many nonuse values. Seemingly, to some, each of the *motives* suggested by Krutilla [1967] as potentially resulting in an individual's WTP to preserve environments would give rise to a *separable value*. Examples of the "values" which preoccupied many researchers from 1981 to the present day include: existence, bequest and option.

In early studies which attempt to measure such things as option, existence and bequest values, the conceptual basis from which testable hypotheses are to be drawn is incomplete or nonexistent. For option values, typically references to the theoretical arguments found in the option value literature are given as representing the theoretical foundation for the values to be estimated in the study. The operational meaningfulness of option value models aside, these references would be relevant only for estimates of *option* values. In terms of the conceptual foundation for *other* nonuse values to be estimated, the studies either ignore this issue (e.g., Sutherland and Walsh [1985] and Daubert and Young [1981]) or they simply refer to these values as those "outlined or proposed by Krutilla" (e.g., Walsh, Loomis and Gillman [1984; p. 15] and Greenley, Walsh and Young [1981; p. 657]).

In the balance of this section, we develop two arguments. First, we find in the literature pervasive references to studies that are said to have *demonstrated* that existence values can be measured, and that they are "large" relative to total values that individuals may hold for environmental goods. Most prominent among these, in terms of the frequency of citations, are the studies by Brookshire, Eubanks and Randall [1981], Schulze, Brookshire, Walther,

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MacFarland, Thayer, Whitworth, Ben-David, Malm and Molenar [1983], and Greenley, Walsh and Young [1981]. We examine these studies to the end of assessing the extent to which they in fact provide such demonstrable evidence of measured existence values and their relative magnitude.

Secondly, we briefly comment on several other prominent studies that attempt to measure nonuse values.³⁵ Our comments are primarily focused on an assumption that is seen to underlying all of these studies: use-related questions elicit *only* use-related values, and nonuse related questions will elicit *only* nonuse related values. Moreover, in some of these studies (those which elicit post-bid allocations of value across motives) one finds the presumption that the researcher can *a priori* define *all* motives that an individual might have for valuing an environmental good and that the questionnaire design can elicit values for any one of the motives of interest.

F.1 Three Early "Existence Value" Studies

We find in the literature what we regard as a conventional wisdom concerning existence values: they exist *apart from values associated with any other value-related motive*, they are measurable and have been measured, and they are "large" relative to total values for an environmental resource. The studies most often cited as providing the evidentiary basis for this wisdom are those by Schulze, Brookshire, Walther, MacFarland, Thayer, Whitworth, Ben-David, Malm and Molenar [1983], and Greenley, Walsh and Young [1981], and Brookshire, Eubanks and Randall [1983]. The first point we wish to make relates to the question of attribution: do these studies actually claim to have provided monolithic evidence for having measured existence values? To some extent, the answer to this question is "no." Some of these authors make clear the experimental, exploratory nature of their study and caveats relevant for their results. Thus, Brookshire, Eubanks and Randall [1983; p. 14] make clear that:

"There is some, *but nevertheless incomplete*, evidence that these contingent markets performed reliably...(however) some unexpected results were

³⁵ Most works conducted over the last five years or so focus simply on use value and "existence" or "intrinsic" value. In some cases "existence" or "intrinsic" would appear to be used simply as a synonym for "nonuse," without meaning to imply an argument that values associated with a specific motive are being measured (for example, see Smith, Desvousges and Freeman [1985]). Others, however, find compelling their description of values measured as those being associated with specific motives (see, e.g., Walsh, Aiken, Bjonback and Rosenthal [1987]).

obtained...we feel a guarded optimism about the use of contingent markets to estimate option prices. If we were to concede that the estimates generated in such markets are necessarily *indicative rather than reasonably precise* (and the empirical evidence on this point is still sketchy), we would argue (that indicative evidence is useful information)." *(emphasis added)*

Schulze, *et al.* [1983; p. 173] concede that their method, like other indirect estimation methods, are subject to errors.

The second point that we wish to make is directed to those who cite these works as having established the measurability and magnitude of existence values. We briefly examine these three studies to the end of assessing the extent to which they do indeed provide the compelling evidence implied by cited references to them.

The Schulze *et al.* [1983] study explores values for improved visibility in a group of national parks, including the Grand Canyon National Park. Two groups of subjects are interviewed in two different locations. The first location is a park site (the Grand Canyon National Park, or one of four other regional parks) and the second location is the subject's home (located in Denver, Los Angeles or Chicago). Those subjects interviewed at a park site are asked their WTP for a change in visibility, using as a payment vehicle a *daily entrance fee*. Those subjects interviewed in their homes (offsite) are asked their total WTP for the same changes in visibility at the parks, using as a payment vehicle the subjects' *monthly electricity bill*.

Implicitly, Schulze *et al.* [1983] assume the following. First, there are two, and *only* two, motives for valuing visibility changes: recreational use and "existence." Secondly, if a subject is interviewed at the park site and an entrance fee is the payment vehicle, *only use related motives* will be elicited by the WTP question. In other words, if the subject is onsite any response to a WTP question will be one for which any motives other than use will be repressed or forgotten. The resulting value is then interpreted as a pure use value. If subjects are interviewed offsite and the payment vehicle is the monthly electric utility bill, however, the *same* WTP question for a visibility change will now result in a value which reflect *all* valuation motives (limited, by assumption, to use and existence). Onsite, the subject forgets nonuse motives; offsite, they are remembered.

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This is the basis for Schulze *et al.*'s [1983] measurement of existence value. They have total values from offsite subjects. They have a total value from onsite subjects, but total values from onsite subjects are *assumed* to reflect only use motives and are then taken to be use values. Since subjects can have only two motives for valuing improved visibility, use and existence which then sum to a total value, the difference between their total values and use values is by definition an existence value.

We acknowledge the possible intuitive appeal of the following argument: if you ask a subject that is visiting a park how much more he'd pay to get in the park, he's thinking about getting in the park to use it. We find equally appealing, however, the possibility that subjects may be emotionally moved by the beauty of a natural environment, and in responding to a WTP question may reflect on any number of motives: existence, bequest, national pride, or even self-esteem. In our view, all that one can claim from this procedure is that it measured two sets of total values. We find no compelling basis for claims that onsite subjects offered pure use values.

A remaining question concerns the source for the oft-cited "large" existence values, relative to total values, which are claimed to have been identified in this study. As noted above, Schulze *et al.* [1983] define existence value as the difference between values obtained from offsite subjects who report a WTP as an addition to their *monthly* utility bill and values obtained from onsite subjects who report a WTP as an addition to a *daily* entrance fee.³⁶ These values do not differ to any great extent. Both values, the total value and the "use" value, are in the \$3.00-\$5.00 range. What then is the source for "large" existence values, which are the differences between these similar values? Schulze *et al.* [1983], *not the subjects*, convert these values to an *annual* value. Offsite values, in the \$3.00-\$5.00 range, are then multiplied by the frequency of utility bills: twelve. Onsite values, also in the \$3.00-\$5.00 range, are multiplied by the frequency of annual visitor days to the park, on average around two days per year. Using the lower bound of \$3.00 for illustrative purposes, total value is then \$3.00 times 12, and use value is \$3.00 times 2. Existence value is the difference between the two. The relatively "large" existence value (\$36 - \$6, or \$30) then reflects no more than the fact that 12 is larger than 2.³⁷

³⁶ We are aware of no argument that would suggest that specific payment vehicles elicit specific motives of an individual. We are aware that bids may be affected by the payment vehicle used (vehicle *bias*) but this is surely something different than what these researchers have in mind. Their vehicles, and sites, result in assumedly *un*biased bids which reflect *different motives*.

³⁷ The obvious question here is: if asked for an *annual* WTP would off-site subjects have reported a number (continued...)

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Brookshire, Eubanks and Randall [1983] were concerned with values associated with the preservation of herds of elk and long-horned sheep. They assume that their subjects have only three possible motives for valuing herds of elk herds or long-horned sheep. Specifically, the motives are "hunting use," "observation-use" and existence. In the simplest terms, subjects are asked if they hunt or observe the herd, and are asked a WTP question related to the preservation and enhancement of the herds. If they hunt, their value is interpreted as a use value. If they see the herd but do not hunt, their value is interpreted as an existence value.

Setting aside the imposed limitation on nonuse motives, the method used by Brookshire Eubanks and Randall [1983] was clearly an imaginative effort to explore nonuse values, and demonstrates one way to measure them. If we obtain a value from a self-reported nonuser and we can believe this reported status, then the value elicited can be defined as a nonuse value. However, a problem arises with their use values, and therefore any comparison across values. Their "use" value is a pure use value *only* if one assumes that hunter-subjects did not have or "forgot" any observer-use or nonuse motives in the formulation of their reported values. Similarly, observer-use values are pure use values only if subjects did not have or "forgot" any other nonuse motives.

Greenley, Walsh and Young [1981] set out to measure values associated with maintaining water quality in a river used for recreation purposes. Water quality in the river is threatened by a proposed plant whose waste discharges to the river would have irreversible quality effects. Subjects are asked their WTP for water quality improvements to "enhance recreational enjoyment." (p. 664). They are then asked to state other values: a value for postponing the development decision that would have the effect of polluting a river, an existence value, and a bequest value. The value of "enhancing recreational enjoyment," interpreted as a use value, and the value of postponing the development decision, interpreted as a quasi-option value, are seemingly added (p. 667). Curiously, while Greenley, Walsh and

³⁷(...continued)

^{\$3.00} times 12, and would on-site subjects have reported a number \$3.00 times 2? This issue relates to a fundamental problem which is explored in some depth below in section III.C: do CVM values reflect a *real economic commitment* on the part of subjects? While there exists a limited amount of empirical evidence relevant for this question as it applies to a *one-shot* commitment for a payment, there exists *no* evidence relevant for assessments of CVM responses involving a series of payments. Even if one assumes that CVM subjects will *actually* pay a reported amount one time, an assumption that enjoys less than overwhelming empirical support, there exists no basis for assuming that he or she will continue such payments. Thus, if annual values are to be used for analyses then subjects should be asked for an annual value.

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Young [1981] describe their bequest and existence values as "...the values that would remain in the absence of recreation use" (p. 669), they are not added to use and option value "...because of the high probability of upward bias." (p. 669).

Greenley, Walsh and Young's [1981] subjects are implicitly of the "forgetful" genre. When asked for their WTP for a specific motive, in formulating a WTP response the subject is presumed to repress all motives other than the one stated by the interviewer. We know of no basis in economics which would support the notion that one can change the quantity of a good, ask the subject for only a specific set of motives related to this change in the quantity of the good, and then observe the change in price attributable *only* to the asked-for motives.³⁸ We then find no reasonable basis for giving motive-related meaning to the individual nonuse values set out in the study, or for meaningful comparisons between use and nonuse values.

At the time that these three studies were conducted, they were innovative efforts to explore new ground in the area of valuing nonmarket goods. Brookshire, Eubanks and Randall's [1983] study provided early evidence of values for environmental goods on the part of nonusers; note that this value is a *total* value (albeit one that Brookshire, Eubanks and Randall [1983] attribute to an existence motive). We conclude, however, that these studies do *not* provide compelling evidence that separable motive-related values can be measured or that existence values are in any sense "large" relative to total values.

F.2 Examples of Other "Nonuse Value" Studies

A number of other studies have been conducted over the last decade which empirically explore different approaches to the measurement of nonuse values. Many provide some degree of positive evidence that individuals may hold values for an environmental asset that is independent of any notion of use.³⁹ Evidence as to our ability to quantify these values is much weaker, however, other than in the case of values obtained from self-identified nonusers. This is to say that evidence for our ability to obtain values of nonusers of environmental goods must be viewed as compelling, setting aside all considerations of what

³⁸ In these regards, we may agree in principle with the argument of Madariaga and McConnell [1987] that "motives matter" (pp. 938-940) in discussions of why individual's might hold values for a resource that are not use related. Bishop and Welsh [1992; p. 12] seem to suggest that an understanding of motives might be important for distinguishing between unique and non-unique environments, or (p. 20) for assessing the validity of the CVM. Precisely how motives are to be identified and measured, or how they are to be put to these uses is unclear, however.

³⁹ See Freeman's [1992] discussion, however, of the relevance for such studies of how "use" is defined.

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these values might mean *vis-a-vis* real economic commitments (a topic explored in Section 3). One must be much less sanguine as to our ability to differentiate between use and nonuse values *of a user*. Still weaker is evidence from empirical studies that attempt to measure *components* of nonuse values. Several examples drawn from better-known studies may serve to make these points.⁴⁰

We wish to differentiate between nonuse values that refer to values of a nonuser and nonuse values of a user. To this end, we refer to nonuser and nonuse values, respectively. As noted above, on conceptual grounds we see no problems with nonuser values. Such values have been identified by Brookshire, Eubanks and Randall [1983], Samples, Dixon and Gowen [1986], Boyle and Bishop [1987] and Desvousges, Smith and McGivney [1983], amongst others.

Our concern is with nonuse values as defined above. We have no quarrel with the notion that users may have such values. Our quarrel is with the extent to which these values have been measured in empirical studies conducted to date.⁴¹ One approach that has been used in efforts to measure nonuse values, as well as components of nonuse values, involves asking subjects for their total value for an environmental good, after which they are asked to allocate this total value among various nonuse categories. One sees this approach in the studies by Sutherland and Walsh [1985], Walsh, Loomis and Gillman [1984], Walsh, Sanders and Loomis [1985] and Walsh, Aiken, Bjonback and Rosenthal [1987]. As an example of this approach, Sutherland and Walsh [1985] ask subjects their maximum WTP to protect water quality at a given site. Subjects are then asked to report the percentage of this total value attributable to the following:

- (i) Payment to visit Flathead Lake or River this year, in addition to travelling or lodging expenses.
- (ii) Payment for the opportunity to visit the Lake or River in the future at the same level of water quality and fishing conditions.
- (iii) Payment to preserve water quality in Flathead River and Lake. The value to the subject from knowing that good water quality exists there.
- (iv) Payment to preserve water quality in Flathead River and Lake. The value to the subject from knowing that future generations will have good water quality.

⁴⁰ See Freeman [1992] for a similar assessment.

⁴¹ Advances in the development of conceptual models that effectively separate use and nonuse values are described below in subsection G. As noted in these discussions, however, there have been no successful efforts to implement these conceptual models.

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Applying percentages reported for items (i) through (iv) to the total bid of each subject, Sutherland and Walsh [1985] interpret the value associated with (i) as a use value, and that with (ii), (iii) and (iv) as option, existence and bequest values, respectively.

The following assumptions are implied by this approach. First, since subjects are instructed that the percentages i-iv should total 100% (p. 284), subjects can have *only* value-motives related to use, option, existence and bequest, as these motives are perceived *by the investigators*. Second, subjects know values associated exclusively with each of these motives. One can only speculate as to what subjects might have had in mind in making allocations of this type. Just how subjects might differentiate between values described in questions (ii)-(iv) is surely questionable. There is an obvious potential for subjects to view as "cues" terms such as "knowing that future generations will have a good water quality." As pointed out by Freeman [1992; pp. 45-46], these kinds of allocation approaches are without theoretical justification. We find no substantive basis for the attribution of values derived in this class of studies to specific nonuse motives.

Many other studies of nonuse values were being conducted during this period, some of which were remarkable for their efforts in using techniques for design that were state of the art at that time, and for the attention given to theoretical underpinnings for their valuation process. Two examples of studies in this class, which involve subjects being directly asked for use and nonuse values, are Desvousges, Smith and McGivney [1983] and Desvousges, Smith and Freeman [1985].

Desvousges, Smith and McGivney [1983] obtain values for a number of different water quality levels in the Monongahela River. Essentially, they ask subjects for two specific values. The first value related to possible use of the river, defined by the investigators as an option price. The second value was obtained where subjects were instructed to think only in terms of satisfaction that they would obtain from changes in water quality that excludes any use of the river on their part. This latter value was interpreted as a preservation or existence value.⁴² The extent to which an existence or nonuse value was, in fact, measured by this latter question, depends on a number of considerations. In responding to the option value question,

⁴² Notwithstanding the elicitation of this value, it is not used in their analyses of values. This seemingly reflected their concerns for issues similar to those discussed below.
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did subjects in fact focus only on use-related motives and "repress" any nonuse motives? Moreover, subjects had nothing in the way of motivation as to why they should imagine a set of circumstances in which they would not have access to the river.⁴³

Desvousges, Smith and Freeman [1985] value changes in risk association with policies concerning the disposal of hazardous materials. Subjects are asked to value reduction in risks *to the household* associated with a change in policies concerning the disposition of hazardous materials. They are then asked to value an *additional* regulation that would reduce ecological risks, but would not further reduce household risks. The latter value is referred to as an "existence" value. Desvousges, Smith and Freeman [1985; ch. 6] acknowledge problems associated with labeling values for the potential motives which might give rise to the value, and suggest a more general label, "intrinsic" value. Indeed, this study contains an extraordinarily thoughtful and comprehensive review of theoretical aspects relevant for nonuse values. Once again, however, their success in obtaining separable use and nonuse values with their empirical survey procedures turns on the extent to which subjects, in their introspective formulation of values associated with questions concerning household risks, in fact repressed any nonuse motives that they might have associated with the posited policy for hazardous waste disposal.⁴⁴ As in the case of Desvousges, Smith and McGivney [1983], the relevance of this issue is immediately apparent: the potential for double-counting.

Results from these studies demonstrate that nonusers may hold values for environmental goods. The *magnitude* of nonuse values is questionable, however.

Another example of efforts to measure nonuse values is the "tree killing" experiments of Boyce *et al.* [1989] [1991]. For present purposes, the aspect of this study that is of interest concerns the use of an incentive-compatible auction institution⁴⁵ to elicit WTA and WTP for a

⁴³ McConnell [1983] and Freeman [1992] suggests a means for explaining to subjects a set of circumstances that would preclude their use of a resource which is consistent with economic theory: assume that prices for goods that are necessary complements to the good are so high as to preclude use ("choke prices.") We evaluate this approach in Section 2G, A Formal Decomposition of Total Values into Use and Nonuse Components. It was not employed in the studies discussed here, however.

⁴⁴ For the existence value one must assume that subjects accepted the distinction between household and ecological effects attributed by the researchers to the additional regulation.

⁴⁵ We question the *strength* of the financial incentives that were used in this experiment in Section (continued...)

Norfolk Pine tree, which is a little house plant. In one set of experiments, subjects were told that if they did not buy the tree (the WTP case) or sell it back to the experimenter (the WTA case), the tree would be destroyed "then and there."⁴⁶ In another case, subjects were asked WTA and WTP for a tree, but were told nothing about the disposition of the trees left in the hands of the experimenters.

Boyce *et al.* [1989] [1991] define "existence value" (in the [1989] version of the study, "intrinsic value" in the [1991] version) as the difference between bids obtained in the "kill" case and those obtained in the "no-kill" case. They find that subjects generally offered higher WTP or WTA valuations for the "kill" case than they did in the "no kill" case. These differences are found to be statistically significant, leading to the conclusion that "intrinsic values" *associated with the trees (i.e., the environmental good)* are significantly positive.

The difficulty with this interpretation of results is that the investigators are assuming that the subject sees the difference in the treatments as they do. Thus, Boyce *et al.* [1991; pp. 20-21] acknowledge that caution must be used in attributing the values obtained in the "kill" case to nonuse values that reflect motives and preferences related to the environment:

We should point out that the exact commodity valued in these experiments could be the source of some debate. Due to the variety of definitions for intrinsic values found in the literature, it may be argued that intrinsic values is not what was captured in these experiments.⁴⁷

⁴⁵(...continued)

^{3,} CVM Values and Real Economic Commitments. Nonetheless, this study is exemplary in at least employing some institution for eliciting total values that has known demand-revealing properties in theory.

⁴⁶ Actually the destruction took place in a separate room, but did occur at the conclusion of the experiment.

⁴⁷ They continue (pp. 20-21): "The fact that individual trees were destroyed, rather than all Norfolk Island pine trees, may have different implications than if the whole species was destroyed. Nonetheless, some element of moral responsibility seems to have been captured." This extension of their argument, and expansions in Schulze [1992; pp. 29-31], appear to relate to their discussions of WTA-WTP differences. As a justification for WTA values being large relative to WTP they argue that, in the case of WTA for allowing the extinction of a species, the individual may view his acceptance of compensation as his taking upon himself the *moral responsibility* for extinction. Such is not the case with a WTP value. Boyce *et al.* [1991] claim that this difference *may* account for WTA-WTP differences for actions related to species extinction.

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Indeed, it is not at all clear that differences in bids obtained in the "kill" and "no-kill" cases are in any way attributable to intrinsic or any other values associated *with the tree*. While bids in the "no-kill" case may plausibly be taken as subjects' values for the tree, bids in the "kill" case could with equal plausibility have nothing to do with the tree *per se*, but with avoiding an *action* which subjects found disgusting or unpleasant. The extent to which the "kill" case captures total value for the tree, while the "no-kill" case captures only use value, may then be problematic.

G. A FORMAL DECOMPOSITION OF TOTAL VALUES INTO USE AND NONUSE COMPONENTS

There have been several attempts to formally define what is meant by nonuse value and to propose a conceptual decomposition of total value into use and nonuse components. The most widely cited of these are McConnell [1983], Randall, Hoehn and Meier [1989] and Randall [1991].⁴⁸ We take McConnell's [1983] work as being representative of this approach and briefly review his arguments below.⁴⁹ To anticipate the results of this review, we find that these approaches are internally consistent but impossible to render operational.

The key analytical concept underlying rigorous analyses of nonuse value has been the expenditure function. This is nothing more than the smallest amount of money that an individual would require, at given (relative) prices, to attain some specified level of utility. It views the consumer as minimizing the cost of attaining this given baseline level of utility, subject to the constraint that the consumer may purchase goods only at prevailing prices. Thus, we write this minimum level of income or expenditure as a function of prices and utility levels. If the utility level that must be attained rises, then the minimum level of income

⁴⁸ Building on results in Neill [1988], Larson [1991] presents intriguing results on the possibility of using transactions-based data to estimate total value for nonusers. The key insight here is to use the restrictions of demand theory, especially the "adding-up requirement" which derives from the observance of the budget constraint, to be able to infer nonuse values. Rather than attempting to identify private goods that are complementary to the nonmarket good in question, these results focus on finding goods that are instead Hicks-neutral. The upshot is that zero nonuse value is not *imposed* as part of the process of inferring total value, as is the case with traditional complementarity assumptions in the tradition of Mäler [1974] and Bradford and Hildebrandt [1977]. No empirical application of this approach has been published, and there is still considerable uncertainty as to the precise data requirements for implementation. Nonetheless, it raises the possibility of a more thorough comparison of CVM values with transactions-based values than has been possible in the past.

⁴⁹ Randall [1991] makes use of a curious scenario (equation 10.5a on p. 306) in which the resource exists at some initial level Q⁰ but is somehow priced *out of existence* via a choke price on existence itself.

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needed to attain it will rise.⁵⁰ If there is an across-the-board increase in prices, then the minimum level of income must rise if the original level of utility is to be maintained.

The expenditure function is useful because it allows one to directly define equivalent changes in income levels for some parametric change in prices or baseline utility. It is also possible to generate compensated demands directly from the expenditure function: they are simply the partial derivative of the expenditure function with respect to the price of the good in question. Thus, specifying an expenditure function for an individual gives the analyst all of the information needed to undertake cost-benefit analysis and to measure demand.

To the extent that the consumer's utility function is parameterized by some *exogenous* variable, the expenditure function will be similarly parameterized. This simple extension is of the utmost importance to understanding how one can derive a decomposition of total value into use and nonuse components. When we say that a utility function is "parameterized" by some variable, all we are saying is that if that variable changes then utility will change. Most importantly, to call such a variable "exogenous" is to say that the consumer does not have any control over it: it is not a choice variable, in the sense that food purchases are typically a choice variable.

Consider adding some resource R to the consumer's utility function. It follows that the expenditure function will be parameterized by the level of R, since the level of R affects the consumer's level of utility. McConnell [1983] carefully specifies two sets of scenarios which conceptually capture a total and a nonuse value. Use value is then simply the difference between total and nonuse value. We examine these scenarios below.

Defining Total Value G.1

It is important to state total value from the resource in comparable terms if one is to make operational use of this decomposition. Total value is defined with two scenarios, which we will refer to as 1 and 2. In scenario 1, the resource is at some reduced level, but prices are at their baseline level. We will refer to the minimum amount of income required by the individual to maintain baseline utility with this scenario as Y₁. In scenario 2, both resource level and prices are at their baseline level. The minimum amount of income required for the individual

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⁵⁰ Strictly speaking, it will never decrease. If we assume local non-satiation and positive prices then the statement in the text is correct. We will maintain these auxiliary assumptions unless otherwise specified. See Deaton and Muellbauer [1980; p. 37ff] or Jehle [1991; p.159] for a rigorous statement of the relevant properties of the expenditure function.

to attain baseline utility under this scenario is defined as Y_2 . Total value is then just the minimum income needed to attain baseline utility level in scenario 1, minus the minimum income needed to attain the baseline utility level in scenario 2, or $TV = Y_1 - Y_2$. It is the minimum amount of income that leaves the individual indifferent between a state of the world in which the resource is at its present level and one in which the resource is diminished in quantity or quality.

G.2 Defining Nonuse Value

Two additional scenarios are specified by McConnell [1983; p. 260] for the purpose of defining nonuse value.⁵¹ In each scenario, the consumer is assumed to attain the same level of baseline utility employed in scenarios 1 and 2.

Scenario 3 is where the individual faces reduced levels of the resource R and "choke" prices for all private goods that are complementary to R^{52} A choke price is defined as any price that results in the use of the complementary good being driven to zero. In such a situation, there can be no use associated with R, since use is made impossible by use-related prices that are prohibitively high. Thus, in scenario 3, the consumer is not using the resource and the level of the resource is reduced by some given amount. We refer to the minimum amount of income required for the individual to maintain baseline utility levels with this scenario as Y_3 .

In scenario 4, the individual enjoys the original level of the resource but faces choke prices for all of the goods complementary to the resource. McConnell [1983; p. 259] is careful to point out that the choke prices applicable in scenario 4 need not be the same as the choke prices applicable in scenario 3, since they will depend on the level of the resource in general. In any event, as in scenario 3, the consumer in scenario 4 does not use the resource (prices are too high), but it continues to exist at its original level. Refer to the minimum amount of income required for the individual to maintain baseline utility with scenario 4 conditions as Y_4 .

In both scenarios 3 and 4, the individual faces choke prices such that use of the resource is prohibited. The only difference between the two scenarios is the quantity or quality of the

⁵¹ Freeman [1992; pp. 14-21] extends this formal decomposition to further distinguish "nonuse value" from "existence value".

 $^{^{\}rm 52}$ More generally, any goods whose level can be chosen by the consumer and that are complementary to R.

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resource. Any difference between Y_3 and Y_4 can then *only* reflect nonuse effects on the individual's well-being associated with differences in the quality or quantity of the resource. Thus NUV = $Y_3 - Y_4$ is McConnell's [1983] nonuse value.

G.3 Defining Use Value

Use value (UV) is simply the residual between total value and nonuse value, as these are defined above. Thus UV = TV - NUV.

G.4 Measurement Problems

Given these definitions of total value, nonuse value, and use value, how can one measure them? McConnell [1983; p. 261/2] considers problems associated with this question carefully.

The first problem is that we must make sure that the sample of consumers are asked to value all four scenarios.⁵³ This follows from the fact that expenditure functions must be evaluated at the same price and resource level vectors. McConnell [1983; p. 261] states this problem in the following way:

The significance of this result is striking when one considers the logistics of measuring existence value. Such measurements must sample users and nonusers. However, the conclusion that existence values differs for users and nonusers implies that estimates of willingness to accept compensation for those who have no *in situ* use cannot be extrapolated from *in situ* users. In effect, two different types of surveys are needed: one for users and one for nonusers. In addition, even for identical preferences and weak separability of R [the resource level], changes in the price vector change the marginal value of R. Thus, in the case of resources that have national prominence, it is necessary to sample people from all over the country.

The reason that we need to sample people from all over the country is that different parts of the country will have different baseline prices, as well as access to the resource. Since we must have comparability of the expenditure functions in all respects except those identified in the above-mentioned four scenarios, we can only *hope* to ensure this by having a national survey. This problem can be overcome, of course, with a sufficiently large survey budget. Thus, it does not render the proposed decomposition nonoperational.

The second problem is hinted at in the above quote. If we do not confront each survey respondent with all four scenarios, then we must make a strong assumption that all

⁵³ We say four scenarios, despite one of them being the baseline, since there will probably need to be some questions asked to establish what baseline prices and resource access are for any individual.

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respondents to the different scenarios have identical tastes and face the same price vectors. If we did not make this assumption, we could not legitimately compare the four expenditure levels elicited from the distinct samples. One response to this would be to suggest that one just collect sufficient socio-economic data so that an expenditure function can be generated by regression means. This would allow one to compare the minimum income requirements of otherwise distinct samples, by substituting in common (e.g., mean) values for the regressors. The difficulty with this solution is that the explanatory power of regressions of this kind⁵⁴ has been notoriously poor in the CVM literature. The premise of this problem, however, is that we do not have a large enough budget to run the proscribed surveys. Hence this problem, while serious, does not render the decomposition nonoperational.

The third problem is one that goes well beyond the measurement of nonuse values, but does take on particular significance when one is attempting to elicit nonuse values to the end of obtaining measures of WTA. McConnell [1983; p. 263] explains the problem well:

... it is hard to resist speculating on the logical consequence of the pervasive use of existence values in cost-benefit analyses. One view might be quite pessimistic. Suppose that we settle on compensating surplus as the appropriate criterion for project evaluation. Any project which changes any of the characteristics that make up the quality of life will require measurement of compensation. Some kind of contingent market mechanism would have to be used. Because *in the case of existence value, by definition we have no tests for bad answers, we in effect have no way of distinguishing between bribery and compensation.* One might argue that any tool that can apparently be used to thwart measures of progress so easily is of questionable value. *(our emphasis)*

To complete this thought, he then provides an optimistic counter-argument based on vague concepts from disciplines other than economics. The point is that *economics* has no way of making the measurement of existence value operational, particularly when one is attempting to elicit the minimum compensation (WTA) required to return the economic agent to a baseline level of utility. This problem does render the decomposition nonoperational with respect to WTA measurement, but has more to do with the failure of the "measuring instrument" than with the conceptual decomposition itself. On the other hand, if there is no way to measure the

⁵⁴ Admittedly the existing literature has only focused on estimating bid functions, and has done no work as far as we aware on estimating or recovering expenditure functions of this kind. Nonetheless, we remain pessimistic that better results are likely to be found when one tries estimating or recovering the necessary expenditure functions.

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concepts involved in the decomposition, there is nothing refutable about the statement that nonuse value is X% of total value.

Is it possible to design a CVM to elicit Y_1 , Y_2 , Y_3 and Y_4 for an individual using only WTP formulations, so as to avoid this problem? In principle there is: simply run the CVM scenarios "in reverse." Posit that the baseline is the hypothetical in which resource does not exist *and* all goods complementary to the resource have choke prices. Ignoring the potential for subjects with negative NUV, this will ensure that WTP questions are conceptually well-posed since any provision of the resource and/or any reduction in choke prices will not make the individual worse off. Thus, we would be eliciting WTP values rather than WTA values, avoiding the problem posed by McConnell [1983; p. 263] discussed above.

Serious problems remain, however, even in this WTP context. What if there does not exist a private good that is complementary to the use of the resource? More likely, what if such goods, if found, turn out to be complementary to *other* nonmarket resources? How can we then say that elicited nonuse values are solely attributable to the resource in question? We have no answers to these questions.

It is telling that *there have been no published CVM studies that attempt to use this formal framework to measure nonuse value.* Randall [1991] presents a formal framework of this genre, but *none* of the empirical estimates he records (p. 317-320) employ it.

Freeman [1992; p. 38] explicitly states how the CVM scenarios need to be posed in order to elicit nonuse value using essentially the formal framework explained above, but finds no empirical studies that do so. He correctly concludes that one reason to

"...withhold judgement as to the magnitude of nonuse values is that many of the studies described here have not used definitions of nonuse values that are solidly based in individual preference theory. This is especially true where nonuse values have been elicited from users without specifying the conditions leading to nonuse and where supposed option values have been estimated. In a sense, there has been too much measurement without sufficient attention to the theoretical development of the concepts purported to be measured." (p. 49).

Randall [1991; p. 312] also correctly notes that a formal conceptual framework defining nonuse value places the focus on precise scenario construction. He then asserts that

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"Clearly, researchers can obtain valid estimates of total value, existence value, and various kinds of use value (in a deterministic or uncertain context) by conducting, *de novo*, a CVM exercise" (p. 313). Such *de novo* exercises have yet to be conducted.

H. CONCLUSIONS

The notion that values which many individuals will place on environmental resources will include a nonuse component is accepted by economists concerned with issues related to the valuation of nonmarket goods. In principle, there is nothing objectionable about the notion. Central to the economist's theory of value is the importance of an individual's *motives* or *preferences* as they affect the individual's choice of actions which yield satisfaction. Operationally meaningful theorems in economics imply observable behavior of individuals in response to changes in economic circumstances. The premises on which these theorems are based take as given the individual's preferences.

There is nothing in this theory which places use-related limits on an individual's preferences. "Fitting" into the economist's theory of value are any number of instances of economic actions of the nonuse genre: a person's provision of financial support to poor children in developing countries that they may never meet; a vote for higher taxes to support education by a retired, childless family; or an individual's willingness to pay higher taxes to maintain a national park which he has never visited and never intends to visit. In the economist's application of received value theory as a basis for estimating values which individuals place on nonmarket goods, concern is simply with observations of individual behavior (stated or observed WTP) in response to changes in circumstances (changes which affect the quantity or quality dimensions of the nonmarket good).

Our concern in this section has *not* been with the question "do nonuse values exist?" For some individuals they surely do. Rather, our concern has been with three interrelated questions. Are there operationally meaningful theorems which might lead to the specific measurement of nonuse values? Do we in fact have a body of credible evidence which shows that nonuse values, and particularly components of any nonuse value, are particularly "large?" Finally, why are we interested in such values?

Referring to the many asserted *components* of a nonuse value, we find nothing in the way of operationally meaningful hypotheses which would permit the estimation of values attributable

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to specific motives of individuals. Of course, we are not the only economists to take this position. Smith, Desvousges and Freeman [1985; pp. 6-5, 7-5] observe that "...there is no basis for estimating pure existence values from observations of changes in market prices or quantities ... Presently, there is not a strong *a priori* basis for identifying which motive, or set of motives, is most important for these values."⁵⁵ Mitchell and Carson [1989; p. 67] argue that "While we believe CV surveys are capable of measuring benefits that include a nonuse dimension, we are less optimistic about their ability to obtain meaningful estimates of separate component values." We find no credible basis for claims related to either the measurement of existence and other motive-related values or claims for the "large" relative size of such values.

Why are we interested in nonuse values? If one has no means for measuring an individual's maximum WTP for an asset, the importance of nonuse values *could* be substantial despite our inability to measure them. This is the issue that motivated early interest by scholars in the option value notion. The option value notion and later nonuse value concepts may point to serious shortcomings in benefit-cost analyses of alternative uses of assets that are based solely on use. This interest logically extends to a context in which we are measuring maximum WTP for an asset only if one can point to a use to which such measures might be put. We find no examples in the contemporary literature in which it is suggested that, rather than measure maximum WTP for the asset, the appropriate procedure is to measure use-value, nonuse value #1, nonuse value #2, etc., and then sum them.

What we find in the literature is the obverse case: determine maximum WTP, and then disaggregate it. To what end? To what use? As we stated earlier, we cannot imagine a valuation problem which would require the comparison of only nonuse values to the cost of some action affecting a natural resource or environmental asset. As noted by Carson [1991; p.128, fn.6], "...efforts to decompose a WTP response into use value and existence value have probably received too much attention given its policy relevance (because WTP is already the desired welfare measure)..."

⁵⁵ Further, referring to motivations for pure existence value, Smith, Desvousges and Freeman [1985; p.6/7] note that "Definitions can be considered in part a matter of taste. A set of definitions can be considered useful if it furthers the research objectives and leads to useful answers to meaningful questions and if the definitions have the virtue of distinguishing between cases where use of a site generates observable data and cases where no meaningful data can be obtained by observing market transactions."

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Finally, recall that initial interest in nonuse values was motivated by concern for unique, irreplaceable assets. A question then arises which is of concern to many researchers (see, e.g., Freeman [1992; pp. 52-53]). What is the relationship between "unique" resources and nonuse values?⁵⁶ Are nonuse values limited to unique resources? Freeman [1992; pp. 52-53] interprets the literature as suggesting that "ordinary" resources or subpopulations of a widely dispersed wildlife species are not likely to generate significant nonuse values and, in the case of damages to resources, nonuse values are likely to be unimportant where recovery from the injury is quick and complete. Freeman [1992; p. 53] correctly points out that substantial problems arise in giving operational meaning to the idea of uniqueness. In economic terms uniqueness" may be defined only in the eyes of the beholder. Taking an extreme example, to some there could be any number of substitutes for the Grand Canyon, reflecting the view that "if you've seen one canyon, you've seen them all." In the end, of course, the relationship between uniqueness and nonuse values must remain an empirical question, one about which we understand little at this point in time.⁵⁷

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⁵⁶ See Bishop and Welsh [1992] for a discussion of this question.

⁵⁷ Within a somewhat different context, we again take up the question of substitutes below in Section 3.

Section 3

USING THE CONTINGENT VALUATION METHOD FOR ESTIMATING VALUES FOR NATURAL RESOURCE AND ENVIRONMENTAL DAMAGES: ARE CVM VALUES REAL ECONOMIC COMMITMENTS?

A. INTRODUCTION

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A number of CVM studies have attempted to estimate components of nonuse values for natural and environmental resources. In Section 2, we set aside all questions related to the accuracy or substance of values estimated with this method. We now take up these questions.

It is useful to state explicitly the assumptions which underlie the economist's theory of value. We will refer to these assumptions throughout this section.

ASSUMPTIONS OF VALUE THEORY⁵⁸

We assume that individuals are motivated to make choices by a desire to maximize their well-being, referred to as utility. In their efforts to do this, any individual is assumed to:

P Have well-behaved preferences over their choice set. Specifically, assume that preferences are *Complete* (any two consumption bundles can be compared⁵⁹ in terms of utility), *Reflexive* (any consumption bundle is at least as good as itself)⁶⁰, *Transitive* (if bundle *A* is preferred to bundle *B*, and *B* is preferred to bundle *C*, then *A* is always preferred to *C*), *Monotonic* (more of a good⁶¹ is better than less), and *Quasi-concave*⁶² (implying that average consumption bundles are weakly preferred to

⁶¹ Commodities that the agent does not like are called "bads". In most practical applications one can just reverse the directions of axes and measurement to ensure that we are always talking about commodities that are "goods" in the sense defined by the Monotonicity axiom.

⁶² A common way to state this axiom is that the indifference curves (the level sets of the utility (continued...)

⁵⁸ For completeness we might add: **U** Have well-behaved preference over all uncertain goods consumed. The axioms of expected utility theory are typically stated in addition to those presented in **P**. They result in the consumer being able to determine the expected utility of some uncertain commodity as simply the weighted sum of the utilities of the constituent certain commodities, with the weights being given by the subjective probability of each of the constituent commodities actually occurring.

⁵⁹ The word "compared" admits of indifference. Thus the agent could well say that given his current knowledge of the two bundles he does not prefer one to another. This statement is perfectly consistent with the Completeness axiom.

⁶⁰ To non-economists this axiom sounds trivial and silly, but it is needed to ensure that certain logical properties of the system of preferences are satisfied. If it were not true the axiom system would admit contradictions at every step of virtually every proof.

extreme bundles). In the present context we would add a requirement that the consumer know the existing quantities and qualities of goods that he does *not* have a choice over, since these could well affect his preferences.

B Have a well-defined budget constraint. Specifically, the consumer knows his current disposable income, his ability to borrow for current consumption, and all of the prices of commodities he can choose.

We must carefully consider the implications of these assumptions. Assumption **P** implies the following. The set of goods over which utility is defined, which we will refer to as a "choice set"⁶³, may include relatively few or many goods. There may be many goods that provide utility to the individual, but that are not objects of choice. Examples include the many environmental and public goods from which the individual may derive utility. He "knows" that these goods exist, he may be aware of the satisfaction that he derives from them, but they are not objects of choice. For the subset of goods over which utility is defined, his preferences across these goods are assumed to be complete, reflexive, transitive, monotonic and quasi-concave. Such preferences are said to be *well-behaved*.

For our purposes, it is important to point out that the composition of an individual's choice set may change from time to time. Such changes may be frequent or infrequent. Goods once in the set may be discarded and new ones added. Given any change in the composition of his choice set, the individual is then presumed to behave *as if* he had re-evaluated all goods in the new choice set to the end of determining his well-behaved preferences among the various goods. This re-evaluation process is very important. It involves the individual's assessment of such things as how various goods might be preferred over others, the extent to which one good or set of goods might be substitutable for other goods ("if I buy this good, I may not want to buy that good: they both basically serve the same purpose"), and the complementarity of goods ("if I buy more of this good, I may want to buy more of that good"). The implications of this presumed re-evaluation process, referred to by some as the process of "researching one's preferences," will be discussed later.

⁶²(...continued)

function) have superior sets (the consumption bundles preferred to those on the indifference curve) that are convex. In more homely terms, this axiom ensures that indifference curves are bowed into the origin.

⁶³ One may find the term "choice set" occasionally used more narrowly to describe the subset of goods in the utility function that are objects of choice. This distinction becomes important only when one considers public goods or rationed private goods.

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Let us summarize the salient implications of assumption **P** that are of interest here. In value theory, a consumer's choice among goods is based on the assumption that individuals behave as if they are *aware of goods which they might acquire*, that their tastes and preferences guide them in their choice of goods to be included in a choice set, and that they have established well-behaved preferences for goods in this set. In the balance of this section, we will refer to this setting for individual choice as a *well-defined choice set*.

Assumption **B** implies an *incentive structure* within which the individual's acquisition of goods takes place. Given a fixed and finite income, the individual's purchases of additional units of the good involves an opportunity cost: he can acquire the good *only* by giving up the acquisition of other goods. He then has an incentive to make utility maximizing choices, since income spent for one good is not available for expenditures on other goods. In this sense, the consumer can only reduce his utility by failing to make "good choices" as to how to spend his income.

Based on the value assumptions given above, one may derive the "law of demand" -- the quantity of any good which the individual will purchase is inversely related to the price of the good.⁶⁴ Based on these assumptions, one can deduce the following proposition which is fundamental to any effort to value natural resource or environmental damages:

THE WTA (WTP) PROPOSITION

Take an individual with some amount of income, Y, who enjoys access to some natural or environmental resource measured in quantity or quality terms as Q. This combination of Y and Q yields some level of utility to the individual. Denote this initial level of utility U. Let the quantity or quality of this resource change by an amount ΔQ . For the individual to remain at the level of utility U, the change in Q must be offset by a change in income.⁶⁵ This change in income is the individual's maximum willingness to pay in the case where ΔQ is positive, or his willingness to accept in the case where ΔQ is negative.

⁶⁴ Providing that the good is "normal", in the sense that the consumer would demand more of it if his income increased while all prices stayed constant, or that the good is not strongly "inferior" (non-normal).

⁶⁵ Since the resource is a nonmarket good, an increase (decrease) in Q does not impose a (budgetary) cost on the individual and must therefore increase (decrease) the individual's utility or wellbeing. We wish to impose a cost on the individual for the change in Q, but the cost must be such that the individual is no better or worse off than he was before the change.

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As a theoretical argument, this WTA (WTP) proposition is perfectly legitimate and is well grounded in theory. It is operationally meaningful *for some types of Q*, types of Q wherein we can imagine an experiment where Q is *actually* changed, the individual is constrained to his initial level of utility, and an *actual* change in Y (the WTA or WTP) is observed.

It is asserted that the CVM provides a means by which just this kind of "thought experiment" can be conducted. This assertion must rely on two critical assumptions. First, one must assume that one can deduce the following "modified" WTA proposition from the assumptions of value theory given above:

THE "MODIFIED" WTA (WTP) PROPOSITION

Take an individual with some amount of income, Y, who enjoys access to some natural or environmental resource measured in quantity or quality terms as Q. This combination of Y and Q yields some level of utility to the individual. Denote this initial level of utility U. State a *hypothetical* change in the quantity or quality of this resource in an amount $\triangle Q$. Ask the individual the *hypothetical* maximum (minimum) amount that he would be willing to pay (accept) for the change $\triangle Q$, and assume that in responding, the individual understands that "maximum" (minimum) means that he is to remain at the initial level of utility U. The observed amount that the individual states is the amount of income that he would in fact pay (accept) given an actual change in Q.

Secondly, one must assume that the assumptions of value theory can be shown to apply to subjects in a CVM experiment.

The literature abounds with theoretical discussions which presume acceptance of these assumptions. Indeed, the conceptual foundation argued in virtually every CVM paper relates to Hicksian or Marshallian surplus measures which derive directly from utility theory. The acceptance of these assumptions as a matter of course is made particularly explicit by Richard Carson. The following are a few examples in this regard:

"Constructed markets [the CVM] enjoy a very strong theoretical foundation. (...) Constructed markets, in principle and in contrast to other benefit measurement techniques, can directly obtain WTP or WTA." Carson [1991; p. 123]

"When there are thought to be substantial nonuse values, then contingent valuation (Mitchell and Carson [1989]) is the only economic valuation technique capable of correctly, from the perspective of economic theory, measuring both use and nonuse benefits of visibility improvements." Carson *et al.* [1991; p. 2]

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However, one finds *no example* of a study in which someone has *demonstrated* that the Modified WTP Proposition can, in fact, be deduced from the assumptions of value theory.⁶⁶ It is then not too surprising that the casual acceptance of the two assumptions given above does not go without challenge. As a good example, V.K. Smith [1986; p. 173] states categorically that "... we *do not have a model of how individuals will respond to CVM questions.*"

Referring to the argument that the direction of errors in the CVM can be identified by simply considering optimal strategies for CVM subjects within a simple model, V.K. Smith [1986] notes that any effort in this regard requires the assumption that individuals *will* react strategically in their responses, in effect taking the process seriously. Indeed, the major problems with the CVM as seen by Smith [1986] lie in our lack of understanding of the extent to which the assumptions of value theory are satisfied in applications of the CVM. He proposes (p. 172) the following questions as summarizing the "principal problems" with the CVM or any method based upon hypothetical questions:

- Will each respondent really take the decision circumstances seriously, since there are no tangible incentives to do so?
- Is an individual capable of processing the information involved in what is often a new or unfamiliar set of conditions, and responding with his or her actual valuation, even though this value would ordinarily be derived after time for consideration?
- Does an individual's response require repeated experience to form an appraisal of the valuation of the hypothetical question?

The lack of any demonstrated nexus between the assumptions of value theory and the theoretical construct which, by assumption or assertion, must underlie the Modified WTP proposition which is used in implementing the CVM is a persistent source of difficulties for many economists. This is particularly the case in terms of the "incentives" assumption **B**. For example, in their summary of the difficulties with the CVM, Braden, Kolstad and Miltz [1991; p. 12] make the following observation.

⁶⁶ Hoehn and Randall [1987] set out a theoretical construct from which they claim one can deduce operationally meaningful theorems concerning behavior in the CVM with observable differences in experience and information. Their model fails to address the *critical* difference between contingent and real markets, however: the difference between real and hypothetical goods and payment. See the discussion below in a subsection of B.1 (p. 3-24), <u>Asserted Incentives for Truth-telling in the CVM: The Dichotomous Choice Approach</u>.

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"The root of the difficulties is that most of these methods [like the CVM] rely on intentions, ideals, or behavior expressed in hypothetical circumstances. Intentions are typically costless to express, or nearly so, which means that they may not be considered as carefully as are real consumption choices. Many economists are loathe to base economic values -- values that will be used to allocate real resources -- on information that does not grow out of *real economic commitments*." (our emphasis)

The quotations given above certainly do not exhaust available examples of economists' opinions concerning the assumptions that one must accept to say that the CVM provides a means by which one can meaningfully implement the experiment implied by the Modified WTP Proposition. We note a common thread in expressed concerns with values derived with the CVM: *do subjects in the CVM accurately report the amount of money which they would in fact pay if the good in question was made available to them*? In the succinct terms used by Braden, Kolstad and Miltz [1991]: do CVM values represent a *real economic commitment*?

Our concern then is with the extent to which CVM values may be taken as a real economic commitment. For one to accept the notion that CVM values represent a real economic commitment by individuals participating in the CVM study, it seems reasonable to require that substantive evidence exist which would support one or both of the following propositions:

INDIVIDUAL BEHAVIOR IN CONTINGENT MARKETS IS CONSISTENT WITH THE BEHAVIORAL IMPLICATIONS OF THE ASSUMPTIONS OF VALUE THEORY

and/or

THERE EXISTS A CREDIBLE BODY OF EMPIRICAL EVIDENCE THAT DEMONSTRATES THAT CONTINGENT VALUES CLOSELY APPROXIMATE REAL ECONOMIC COMMITMENTS

These propositions relate directly to our discussion of the assumptions of value theory, the WTP Proposition and the Modified WTP Proposition. In the balance of this section, we examine the existing literature to the end of assessing the extent to which there exists credible evidence which would support one or both of these propositions. Arguments and evidence bearing on the theoretical proposition are reviewed in subsection B. Evidence pertaining to the extent to which CVM values approximate real economic commitments is reviewed in subsection D.

B. IS INDIVIDUAL BEHAVIOR IN CONTINGENT MARKETS CONSISTENT WITH BEHAVIOR ASSUMED IN VALUE THEORY?

We noted above the argument that we do not have a "hypothetical" analog to our theory of value. The fact remains, however, that there exists a large literature which implicitly or explicitly assumes that the theory of value, and operationally meaningful theorems and propositions which can be derived from this theory, can essentially be "transferred" to the hypothetical setting of the CVM. Assertions that the CVM "enjoys a very strong theoretical foundation" suggest a conventional wisdom. It is the acceptance of this wisdom that leads to the presumption that CVM values entail the same real economic commitment as do market-based values. However, advocates of this position have yet to explicitly set out this "theoretical foundation." Its existence is only asserted. Indeed, there exists a plethora of such conventional wisdoms which must be addressed in any assessment of the extent to which individual behavior in the CVM is consistent with the theory of value upon which it is assumed to be based.

Since the presumed real economic commitment of CVM values rests upon the presumption that an individual's valuation behavior in the CVM is reasonably consistent with that assumed in the theory of value, we inquire as to the extent to which empirical evidence supports this presumption. In subsection B.1, we examine literature relevant for the "incentives" assumption **B**. In subsection B.2, we examine evidence relevant for CVM values being drawn from well defined choice sets, assumption **P**.

B.1 CVM Valuation Behavior and the Incentives Assumption

A critical requirement for the Modified WTP Proposition is that an individual, in response to a posited contingent change in the quantity of some good, will report a value which is the maximum amount that he would pay to see the change realized. In this section, we address the question: in reporting this value, will the individual's behavior be consistent with assumption **B** of value theory?

The incentives question is of the utmost importance for any claim that CVM values represent a real economic commitment. Indeed, the asserted "solid theoretical foundation" claimed for the CVM rests on the assumption that CVM subjects have *the same incentives* to reveal real economic commitments as they do in real markets: given fixed incomes, they are aware of the sacrifice in terms of their acquisition of other goods implied by their "commitment" to pay for

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the CVM good. We focus on two sets of issues that we view as being particularly important for the incentives question. The first set of issues primarily involves a behavioral question: do CVM subjects behave in a manner consistent with value assumption **B** by reporting their true WTP for a good, or do they behave *strategically* and report a "number" which reflects something other than their true valuation of the good? The second primarily involves a methodological question: is subject behavior affected, in terms of incentives for truthful reporting of values, by whether the WTP question is stated as a dichotomous choice or as a request for a WTP amount? Each of these questions is considered in turn.

<u>Strategic Behavior, Strategic Bias, and CVM Values</u>. Strategic behavior *per se* is not undesirable in the CVM. Some *types* of strategic behavior, most importantly truth-telling, is required if values offered by subjects are to be taken as representing a real economic commitment. The strategic *bias* issue relates to types of strategic behavior whereby subjects report values in a CVM that do *not* represent a real economic commitment. This type of behavior, suggested by Samuelson [1954] as being unavoidable in questionnaire approaches to estimating values for public goods,⁶⁷ involves subjects reporting values which they hope will influence the results of a survey in directions that they view as being favorable to them.

Strategic bias may reflect any number of motivations. Subjects may report a "value" for the purpose of making a statement concerning strongly held ideological views regarding the good in question. If a subject feels that a particular public good will be provided regardless of the level of any payment determined in a survey, he may purposely "free-ride" on the contributions of others and report a very low value in the hopes of minimizing the amount that he will ultimately have to pay for the good. Alternatively, the individual may feel that he will *not* ultimately pay for the public good in question, but that the decision as to whether or not the

⁶⁷ V.K. Smith [1992; fn. 1] suggests that in a later paper Samuelson [1958] was more optimistic concerning the prospects for eliciting people's values through questionnaires. Smith refers to Samuelson's statement following reference to the strategic bias issue: "... I do not wish to be too pessimistic. After all, the world's work does somehow get done. And to say that market mechanisms are non-optimal, and that there are difficulties with most political processes, does not imply that we can never find new mechanisms of a better sort [...as an example regarding strategic bias] Interrogate people for their tastes with respect to public goods in such large homogeneous groups as to give each respondent the feeling that his answer can be a 'true' one without costing him anything extra." [1958; p. 334] In his later considerations as to how one might devise a system of "benefit taxation" which would in some sense make people pay for what they get, he concludes (p. 336) "Instantly, you will discover that the same game-theory reasons that compel rational men to hide their desires for public goods will be motivating them to hide their consumers' surpluses from different product configurations."

good will be provided depends upon the results of the survey. He may then be motivated to *over-value* the good in the hope of having the good provided (at no real cost to him). In what follows, we consider the type of strategic behavior that has been of primary concern to most CVM researchers: free-riding.

One finds in the CVM literature the conventional wisdom that *it is well established* that subject behavior in the CVM will *not* in any substantive sense be characterized by free-riding. A number of examples may provide the reader with some feel for this wisdom:

"Compared with the experiments [from the experimental economics literature], which show a low level of strategic behavior, we conclude from the evidence that strategic behavior in properly designed CV surveys should be even lower. The evidence is sufficiently extensive and consistent, we feel, to shift the burden of proof to the shoulders of those who would continue to challenge the validity of all CV studies on grounds of strategic behavior. This judgement should not be taken to imply that the CV analyst can totally dismiss the possibility of bias caused by strategic behavior (...) some types of CV studies are more prone to strategic behavior than others." Mitchell and Carson [1989; p. 162]

"However, several types of evidence tend to corroborate the reliability of CVM results. First, CVM results are consistent with preferences revealed by actual choice behavior (Tolley, *et al.*, 1984) ... empirical evidence appears inconsistent with the conventional models of strategic misstatement (e.g., the strong free-rider hypothesis discussed by [V.L.] Smith [1980] and the strategic bidding model of Brookshire *et al.* [1976])." Hoehn and Randall [1987; p. 227]

"... the specter of Samuelson's strategic bias proposition remained as a concern ... until the appearance of Vernon Smith's [1977] report of experimental evidence that further belied the strategic bias proposition." Cummings, Brookshire and Schulze [1986; p. 16]

"Studies by Bohm, Scherr and Babb, and Smith [1977] tend to indicate that fears among economists relating to gamesmanship are exaggerated." Bishop and Heberlein [1979; p. 928]

"[citing Smith [1977], Scherr and Babb [1975] and Bohm [1972]] Empirical evidence thus far does not support the existence of strategic bias among consumers." Schulze, D'Arge and Brookshire [1981; p. 156]

"... survey work with consumers has failed to show any evidence of strategic bias in valuing public goods. This result agrees with the experimental work of Grether and Plott [1979] and Smith [1977], who also failed to find evidence of strategic economic behavior in experimental settings." Schulze *et al.* [1983; p. 153]

Statements of the type exemplified above invite the reader to accept as "fact" that there is strong, unequivocal evidence that would support one or more of the following statements:

STATEMENT ONE: valuation behavior of subjects in the institutional environment used in experimental economics can be assumed to characterize behavior in the institutional environment used in the CVM.

and/or

STATEMENT TWO: notwithstanding perhaps substantive differences between the valuation institutions used in experimental economics and that used in the CVM, subject behavior in the CVM is reasonably *similar* to that observed in the institutions used in experimental economics.

and/or

STATEMENT THREE: within the valuation institution of the CVM, it has been demonstrated that subjects do not behave strategically.

Moreover, statements one and two presume that experimental studies show little free-riding in a conclusive way. This issue is taken up as a digression at the end of this subsection. We next review existing evidence which is relevant for each of these statements.

<u>Experimental Institutions and the CVM</u>. We begin with the first statement: valuation behavior of subjects in the institutional environment used in experimental economics can be assumed to characterize behavior in the institutional environment used in the CVM.⁶⁸ In experimental economics, two valuation institutions have been primarily used for research focused on free-riding behavior: the Smith (or Unanimity) auction and the Voluntary Contribution Mechanism.

The Smith auction is introduced and developed by V.L. Smith [1977] [1979a] [1979b] [1980]. These are the works most often cited by CVM researchers as providing evidence that subjects do not generally free-ride. The institutional characteristics of the typical Smith auction which are salient for our discussions are the following:

⁶⁸ One might also interpret this statement as simply saying that institutions don't matter. Of course, this statement involves an issue that is basic to all research conducted in the field of experimental economics: *institutions do matter.* This is to say that individual choice behavior depends, in general, on the institutional environment within which choices are to be made. We mention it here for the following reason. In subsection C, Do CVM Values Closely Approximate Real Economic Commitments?, we inquire as to evidence that might support the position that people will actually pay amounts that they say that they will pay in the CVM. This inquiry may be viewed as setting aside questions regarding the relevance of economic theory or, more generally, valuation institutions, for assessing the substance of CVM values.

(a.1) Subjects are given an interest-free loan at the outset of the experiment; this loan can only be used to "buy" shares of a public good.

(a.2) Subjects receive a monetary reward (a payoff) which depends upon the number of units of the public good that is produced (how this number is determined is explained below). For example, they may each receive (say) \$10.00 if 6 units of the good is produced, \$12.00 if 7 units are produced, and so on. Thus, in essence, the subject does not "buy" or "acquire" a public good. His "shares" in the good are "sold" to the experimenter at the end of the experiment for money. This is his payoff.

(a.3) Subjects face "induced" values. At the outset of the experiment, they are told the costs of providing one, two, three, or any number of units of the public good. The number of units of the good to be "provided" is determined by the total amount of bids by all subjects. If total bids cover the cost of (say) 5 units, then 5 units of the public good are "provided." If total bids cover the costs of 10 units, 10 units are provided, and so on.⁶⁹ Subjects are aware of costs for "producing" different levels of the public good. Thus, in combination with (a.2), subjects know the value of producing different numbers of units of the public good. The value question faced by one subject is within the context of what *all other subjects* are bidding. Finally, this valuation context is one wherein subjects are not required to "research their preferences" for the good in question. Their incentives are rather to research their *strategies* for maximizing their payoffs.

(a.4) When a subject offers a willingness to pay for some quantity of the public good, he must *actually pay* his offered WTP out of the loan.

(a.5) Each subject can "veto" the proposed outcome, in which case all subjects receive no payoffs⁷⁰ and the public good is not "produced."

The Voluntary Contribution Mechanism has been used by a number of researchers for experiments concerned with the free-riding behavior of subjects. Examples include Isaac, McCue and Plott [1985], Harrison and Hirshliefer [1989], Marwell and Ames [1979] [1980], and Schneider and Pommerehne [1981]. The salient institutional characteristics of the Voluntary Contribution Mechanism are the following:

(b.1) Subjects are given an interest-free loan at the outset of the experiment; this loan can be used to contribute to the provision of the public good. The number of units of the public good that is "provided" is determined by dividing the total dollar amount of bids from all subjects by the assumed unit cost of providing the good. Thus, if bids from all subjects total \$500.00 and the unit cost of providing the good is \$50.00, 10 units of the good are "provided."

⁶⁹ In the popular "balanced budget" version of the Unanimity Auction subjects receive rebates of monies collected which exceed the cost of providing the number of units of the good determined in the auction.

⁷⁰ Apart from a fixed payment of about \$3.00 or so for showing up at the experimental session.

(b.2) Subjects receive a monetary reward (a payoff) which depends upon the number of units of the public good that is produced. It may also depend on the number of subjects participating.

(b.3) Subjects face "induced" values (see a.3 above).

(b.4) When a subject offers a willingness to pay for some quantity of the public good, he must *actually pay* his offered WTP out of the loan.

The reader requires no more than a passing knowledge of the institutional structure of the CVM to join us in responding to the question: is the valuation institution of the CVM *in any substantive way* similar to these experimental institutions? In the CVM, subjects are neither given a loan as in (a.1) or (b.1) nor do they actually pay their stated WTP as in (a.4) or (b.4). They have no veto power concerning the outcome of the experiment as in (a.5). They do not receive monetary rewards determined by the outcome of the experiment as in (a.2) or (b.2). They are typically not given cost information as in (a.3) or (b.1).⁷¹ Their values are not couched in terms of the behavior of all other subjects as in (a.3) or (b.1).⁷² They do not have induced values as in (a.3) or (b.3). Most importantly, one *must assume* that CVM subjects are motivated to research their valuation of the public good, whereas subjects in both the Smith auction and Voluntary Contribution Mechanism have explicit incentives to research strategies that will maximize their payoffs. These observations lead us to conclude that there are few important *similarities* between the valuation institutions used in the relevant works in experimental economics and those used in the CVM.

<u>The Similarity of Behavior in the CVM and Experimental Institutions</u>. Statement Two poses the question: notwithstanding what we have shown to be substantive differences between the valuation institutions used in most experiments and that used in the CVM, does there exist evidence that might establish that subject *behavior* in the CVM is reasonably similar to *behavior* observed in the Smith auction or the Voluntary Contribution Mechanism?

⁷¹ One may wish to argue, however, that when the dichotomous choice approach is used in the CVM, a cost is implied.

⁷² These contrasts of the CVM with the Smith auction are nicely set out in Brookshire and Coursey [1987; p. 559] in the following way. "The CVM procedure outlined previously does not ask the individual to consider the valuation question in the context of what other individuals are bidding. Additionally, the CVM procedure does not present information to the household pertaining to the cost of the alternative (levels at which the public good is provided). Both of these elements are included by necessity in a Smith auction procedure."

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Unfortunately, there have been very few studies that provide empirical results that are relevant for this question. We do not find studies that compare free-riding behavior in CVM and Voluntary Contribution Mechanism institutions. We find two studies that compare values for a public good derived with the CVM and variations of the Smith auction: Brookshire and Coursey [1987] and Brookshire, Coursey and Schulze [1990].

Brookshire and Coursey [1987] examine three elicitation institutions: a field CVM, a Field Smith auction (SAF), and a Laboratory Smith auction (SAL). In each case, they elicited WTA and WTP valuations for the public good. The public good was residential tree density in a neighborhood in Fort Collins, Colorado. In the WTP exercises, they asked subjects to value increments of 25 and 50 trees from a baseline of 200 trees in a nearby park. In the WTA exercises, they asked subjects to value decrements of 25 and 50 trees from a baseline of 260 trees.⁷³ Thus, their overall experimental design consisted of three elicitation institutions (CVM, SAF, and SAL), two valuations bases (WTP and WTA), and two levels of change in the resource (25 trees or 50 trees).

Brookshire and Coursey's [1987] analyses focus on their assessments of WTP and WTA disparities. The free-riding question was not central to their inquiry. Their data on means, medians, standard deviations, and number of observations in each cell (Table 1, p. 561), however, allows for a rudimentary assessment of the question of interest to us.⁷⁴ For a crude comparison of the means of the treatments of interest here, we can conduct a simple *t*-test of the hypothesis that any two samples have the same mean, allowing for them to have different standard deviations. The exact critical mean values for this test are as follows: for the CVM-SAL WTP comparison and 25 (50) tree increment, 0.034 (0.27); for the CVM-SAL WTA comparison and 25 (50) tree increment, 0.0048 (0.0059). Thus, in three of the four possible comparisons, these critical values suggest that the CVM and SAL institutions generate *different* average valuations. We caution, of course, that this is a rudimentary and

⁷³ Note that the commodities being valued in the WTA and WTP exercises are not the same. One values a decrement from 200, the other an increment from 200.

⁷⁴ A request was made to David Brookshire for the raw data used in this study and in Brookshire, Coursey and Schulze [1990]. Following his move from the University of Wyoming to the University of New Mexico, Brookshire found that the computer center at Wyoming had accidently purged his data files. At the time of our request it was thought that data requested by us was among the lost data. It now turns out that these data are available. Unfortunately we discovered this too late to allow analyses of these data to be included here.

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parametric test, but is all that can be undertaken with the available statistics.⁷⁵ Given the non-Gaussian nature of most such data, we have no basis for claiming that the test undertaken has much in the way of statistical power.⁷⁶

The data reported in Brookshire, Coursey and Schulze [1990; Figure 2, p.185] is very difficult to assess since it is in the form of a graph with no information about standard deviations. The impression seems to be that the hypothetical WTP CVM values (in Part I of their experiment) are about 50% higher than their "Smith auction" counterparts.⁷⁷ In the case of the WTA valuations, there appears to be a more dramatic difference, with the CVM values being about 100% higher than the "Smith auction" values. Of course, such "eyeball" impressions have little if any weight, but we can do no better without access to the raw data.⁷⁸

In summary, we find no evidence that might justify a claim that CVM values and values elicited with a Smith auction experiment are similar or dissimilar. Their relationship remains as an open question.

<u>Free Riding in the CVM Institution</u>. Our third and final statement asks the question: within the valuation institution *of the CVM*, does evidence exist that demonstrates that subjects do not behave strategically? We find four studies most often cited as

⁷⁷ The institution here was a modification of the Smith Auction introduced by Coursey and Smith [1984].

⁷⁸ As noted above, these data were not available to us at the time of this writing.

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⁷⁵ We can list, by way of information, the ratios of the medians although there is no way to infer whether or not these are statistically significantly different from unity. They are as follows: for the CVM-SAL WTP comparison of 25 (50) trees, 1.89 (1.24); and for the CVM-SAL WTA comparison of 25 (50) trees, 27.6 (21.4).

⁷⁶ In the Brookshire and Coursey [1987] (hereafter BC) experiment, only 2 of the 8 SAL experiments actually terminated in non-zero bids (see Table 2, p.562). This means that the tentative valuations listed and used by BC for the final round of these experiments were not what the subjects ended up facing: they paid zero, or were compensated zero, as per the "rules of the game" with the Smith Auction used for these experiments. BC appear to have used valuations that the subjects entered in the last round whether or not they met the group fund requirement or were vetoed. The validity of this procedure is arguable. In any event, the real economic commitment of the subjects in those cases was zero. If one substitutes a zero valuation for all of the SAL experiments that failed to converge, the averages drop dramatically. Specifically, they drop from \$7.31 (\$12.92) in the SAL-WTP experiment for 25 (50) trees to \$6.00 (\$0.00), and from \$17.68 (\$95.52) in the SAL-WTA experiments for 25 (50) trees to only \$0.00 (\$6.98), respectively. Since there is no effect on the corresponding CVM values, which were much larger than the SAL numbers that BC reported, these adjustment would strengthen the conclusion that there is a significant difference between valuations elicited in the CVM and the SAL experiments.

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providing evidence that is relevant for this question: Bohm [1972], Bohm [1984], Rowe, D'Arge and Brookshire [1980] and Brookshire, Ives and Schulze [1976]. We consider each of these studies in turn.

Bohm [1972] is a landmark study that had a great impact on many researchers in the areas of field public good valuation and experimentation on the extent of free-riding. The commodity was a closed-circuit broadcast of a new Swedish TV program. Six elicitation procedures were used. In each case except one the good is produced, and the group gets to see the program, if aggregate WTP equals or exceeds a known total cost. Every subject received SEK50 when arriving at the experiment, broken down into standard denominations.

Bohm employs five basic procedures for valuing his commodity. In Procedure I, the subject pays according to his stated WTP. In Procedure II, the subject pays some fraction of stated WTP, with the fraction determined equally for all in the group such that total costs are just covered (and the fraction is not greater than one). In Procedure III, the payment scheme was unknown to subjects at the time of their bid. In Procedure IV, each subject pays a fixed amount. In Procedure V, the subject pays nothing. For comparison, a quite different Procedure VI was introduced in two stages. The first stage, denoted VI:1, approximates a CVM, since nothing was said to the subject as to what considerations would lead to the good being produced or what it would cost him if it was produced. The second stage, VI:2, involved subjects bidding against what they thought was a group of 100 for the right to see the program. This auction was conducted as a discriminative auction, with the 10 highest bidders actually paying their bid and being able to see the program.

No formal theory is provided to generate free-riding hypotheses for these procedures. Procedure I is deemed (p.113) the most likely to generate strategic *under*-bidding, and procedure V the most likely to generate strategic *over*-bidding. The other procedures, with the exception of VI, are thought to lie somewhere in between these two extremes. Note also that explicit admonitions *against* strategic bidding were given to subjects in procedures I, II, IV and V (see p.119, 127/129). Although no theory is provided for VI:2, it can be recognized as a multiple-unit auction in which subjects have independent and private values. It is well-known

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that optimal bids for risk-neutral agents⁷⁹ can be well *below* the true valuation of the agent in a Nash Equilibrium, and will never exceed the true valuation (see Cox, Smith and Walker [1984]). Unfortunately, there is insufficient information to be able to say how far below true valuations these optimal bids will be, since we do not know the conjectured range of valuations for subjects.

The major result from Bohm's study was that bids were virtually identical for all institutions, averaging between SEK 7.29 and SEK 10.33.

We suggest that there is no basis to claim dogmatically that Bohm [1972] has shown that strategic behavior is absent in "real-life" experiments, let alone that his results can be so interpreted concerning the CVM. Bohm [1972; p.125] himself concluded that his results were compatible with the view "...that people respond in an 'irresponsible fashion' ... to hypothetical questions." His results are important for suggesting a methodology for attacking this problem, but it is premature to draw too strong a conclusion in this respect.⁸⁰

Bohm [1984] uses two procedures that elicit a real economic commitment from individuals, albeit under different (asserted) incentives for free-riding. The two procedures are used to extract a lower and an upper bound, respectively, to the true average WTP for an actual good. Each agent in group 1 was to state his individual WTP, and his actual cost would be a

⁷⁹ Bohm [1972; p. 126] discusses the possibility of what he terms "auction fever", in which subjects get caught up in the fight over a few objects (e.g., sports trophies). The upshot would be a form of the "winner's curse", in which the subjects end up paying more for the object than they, themselves, would have liked to pay *ex post*. If this type of irrationality applied to these subjects then their bids might exceed their true valuation, offsetting the logic presented in the text. We prefer to interpret the available data on the presumption that agents are rational.

⁸⁰ These results are used by Mitchell and Carson [1989; p.147 especially] in an effort to generate some numbers on the "percentage of true WTP measured in experimental studies." They use the results from procedure VI:2 as a benchmark, arguing that they come closest to being true WTP since a real economic commitment was required (although this was also the case for procedures I-IV). Of course, as noted above and disregarding the "auction fever" hypothesis of Bohm [1972; p.126], the institution used in this case would lead us to expect these observed bids to understate true valuations, but by how much we cannot easily say. Thus, using the reported data for VI:2 as "true WTP" results in an upward bias in the percentages Mitchell and Carson [1989; p.147] report. Further, they compare the average contributions in each procedure to the average for VI:2, resulting in numbers on the propensity to free-ride of 74%, 85%, 71%, 74% and 85% for procedures I-V, respectively. The raw data does not appear to be particularly symmetric, however, and indeed medians tend to be much lower than means in all of these cases. If one uses the ratio of medians instead of means these propensities drop to 50%, 70%, 50%, 65%, and 70%, respectively. Moreover, these are also inflated values since the benchmark values for VI:2 are biased down from their true values.

percentage of that stated WTP such that costs for producing the good would be covered exactly. This percentage could not exceed 100%. Subjects in group 2 were asked to state their WTP. If the interval estimated for total stated WTP equalled or exceeded the (known) total cost, the good was to be provided and subjects in group 2 would only pay SEK500. Subjects bidding zero in group 1 or below SEK500 in group 2 would be excluded from enjoying the good.

In group 1, a subject only has an incentive to understate if he conjectures that the sum of the contributions of others in his group is greater than or equal to total cost minus his true valuation. Total cost was known to be SEK 200,000, but the contributions of (many) others must be conjectured. It is not possible to say what the extent of free-riding is in this case without further information as to expectations that were not observed. In group 2, only those subjects who actually stated a WTP greater than or equal to SEK500 might have had an incentive to free-ride. Forty-nine subjects reported exactly SEK500 in group 2, whereas 93 reported a WTP of SEK500 or higher. Thus, the extent of free-riding in group 2 could be anywhere from 0% (if those reporting SEK500 indeed had a true WTP of exactly that amount) to 53% (49 free-riders out of 93 possible free-riders).

The main result reported by Bohm [1984] is that the average WTP interval between the two groups was quite small. Group 1 had an average WTP of SEK827 and group 2 an average WTP of SEK889, for an interval that is only 7.5% of the smaller average WTP of group 1. Thus, the conclusion in this case must be that if free-riding incentives were present in this experiment they did not materially affect the outcome.⁸¹

One can question, however, the extent to which these results generalize. The subjects were representatives of local governments, and it was announced that all reported WTP values would be published. This is not a feature of most CVM studies, which often go to great lengths to ensure subject confidentiality. Thus, while this experiment does provide evidence

⁸¹ The following calculation suggests this conclusion. The average WTP of the 50% of the population of 274 subjects who had an incentive to understate their WTP (group 1) was 7.5% below the average WTP of the remaining 50% who had an incentive to overstate their WTP (group 2). Adjusting for sampling error with a 95% one-sided confidence interval, the average WTP of the whole population, if placed in group 1, would be at most 32% below the average WTP of the whole population if placed in group 2. Although this 32% represents the combined effect of the understatement incentive in group 1 and the overstatement incentive in group 2, the free-rider (or understatement) incentive could still account for (at most) this 32%, assuming that nobody responded to the overstatement incentive. We are grateful to Peter Bohm for this interpretation of his data.

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of an absence of free-riding behavior in a context in which WTP involved a real economic commitment, the results do not transfer without qualification to the CVM.

Brookshire, Ives and Schulze [1976] (BIS) undertake a series of CVM exercises designed, in part, to test for the importance of strategic behavior. They offer an explicit model of the chain of logic that a subject *could conceivably* go through when deciding whether or not to respond truthfully, and then propose to test it indirectly. They are very clear as to what they posit:

"If respondents in our example assume the mean bid of the sample survey is used to set the entrance fee and an individual with perfect information has an 'honest bid' greater than the mean bid, there exists an incentive to make a false high bid to bias the sample mean upward." (p.327; emphasis added).

These are crucial assumptions that are not obviously applicable to any CVM, unless they are built in by design as in Rowe, D'Arge and Brookshire [1980]. Given these assumptions, BIS proposed to

"... undertake to demonstrate that strategic behavior, when carefully analyzed in the context of our particular bidding game, can be easily recognized. This, in turn, will enable us to make an evaluation of the extent of the problem." (p.327/8)

The specific bidding game that they propose assumes that each individual has perfect information with respect to the average bid of all others in the CVM (p.328). Two further implicit assumptions here are (i) that CVM responses will actually have some influence on the social decision, and (ii) that none of the other individuals are able to revise their bids after the subject in question gives his bid. In effect, this last assumption means that all subjects must be expected to rationally conjecture what the mean bid of the other respondents is, since the assumption as stated could only be literally true in general for the last respondent.⁸²

Thus far in the argument, one can appreciate the rationale for the assumptions, if one is to be able to say anything about strategic behavior without a reference valuation institution (such as a Vickrey auction). One can even imagine setting up a CVM in which these assumptions are met. However, BIS added a final assumption, for which a rationale is not developed: that true

⁸² BIS have some discussion (p.328) about preferences being identical across all individuals, but this is simply an analytical vehicle to work in the assumption that the respondent in question knows the mean bid of everybody else (since he knows his own bid). Identical preferences is surely less palatable than making the direct assumption that the respondent conjectures the mean bid of everybody else, since this allows some heterogeneity of preferences from the outset.

valuations are distributed normally. Specifically:

"If we assume that honest bids are distributed normally, bias of the type discussed above will tend to 'flatten' the distribution dramatically. This implies substantial numbers of very high and very low bids relative to the mean bid if dishonest responses constitute a significant fraction of the survey." (p.329)

Turning to the actual survey that was implemented, as noted by the authors, there is no mention of sample means to respondents. The WTP question is posed (p.334) with no qualifying information as to the bids of others. Hence, the authors must rely on the respondents behaving as if they correctly conjectured the sample mean.

Turning to the data, they conclude as follows:

"Examining the distribution of bids (...), we note that it is not flat in either the total grouping or the disaggregated cases." (p.340)

Unfortunately, statistics are not presented that might suggest negative kurtosis,⁸³ which is the prediction of their strategic behavior hypothesis. One cannot generally rely on "eyeball assessments" of sample data distributions in this manner to draw reliable statistical conclusions.

While an imaginative effort to investigate the possibility of strategic behavior in the CVM, we suggest that the BIS "distributional model" does not provide a basis for unequivocal conclusions regarding such behavior.

Rowe, D'Arge and Brookshire [1980] (RDB) take up the suggestion of BIS (p.345) to actually give the sample mean information to respondents and see if they want to revise their bid. In this way, a subject who revises his bid can be presumed to be doing so strategically. This is a plausible means by which one might meaningfully extend the assumptions of BIS: by "hard-wiring" them into the CVM design. They correctly point out the problems with the simple BIS model of strategic behavior:

⁸³ It is not clear whether or not the data reported in Figure 7 (p. 341) of BIS reflects all of the raw data. Rowe, D'Arge and Brookshire [1980; p. 15], in discussing their "trimming" procedures, imply that the same procedures were used in BIS. If this is the case, such trimming would bias the results against the proposed hypothesis of strategic bidding.

"... for the individual to accomplish this goal [of strategic bidding], a great deal of information is necessary. For instance, the sample size, the previous bids, and whether or not they are the last respondent are necessary to bid strategically with an assured outcome." (p.6)

To overcome these informational problems, RDB give 40 subjects the mean bid of the other (supposed) bidders and then told them that they would have to pay the average bid and not their own. RDB do not say whether these subjects were given information on the full sample size or whether or not they were the last respondent. This could obviously matter. If the subject is just one respondent in an arbitrarily large group, then he has little hope of shifting the mean, whatever his bid.⁸⁴ Similarly, if the subject is included among the first few respondents, and there are many more to follow, he can scarcely affect the average one way or the other.

Turning to the data, we are concerned with the use of an automatic "trimming" procedure to eliminate data which could support the hypothesis of strategic bidding:

"First, as in all results reported in this paper, zero bids were analyzed, under criteria suggested by Randall *et al.* [1974] and Brookshire *et al.* [1976], where bids greater than 10 SD from the mean were deleted." (p.15)

This procedure may not have been innocuous. Although RDB do not report the results with or without the use of the procedure, their conclusion hints that it did make a difference:

"... if zero and large bids are closely analyzed and possibly rejected, strategic bias, if it exists, has a negative effect upon the bid distribution." (p.15)

The actual data analysis undertaken is complicated by the presence of many different treatments in the one design, making it difficult to undertake direct tests of the hypothesized negative kurtosis of the bid distribution with strategic bidding. Instead, they test in a regression context for the significance of any interaction of a dummy representing "being environmentally inclined" and a dummy for "provision of the sample mean." They find no significant interaction for either dummy variable, and conclude that significant strategic behavior is absent. Why this interaction term should have a nonzero expected value under the hypothesis of strategic behavior is not obvious. If, for example, there were as many proenvironmental as anti-environmental respondents in this group of 40, their bid deviations could be offsetting.

⁸⁴ Implicitly this line of argument assumes that the agent feels constrained not to give "incredibly large" bids, perhaps because he rationally fears that such "outliers" will be dropped.

The RDB study is an imaginative effort to extend the earlier inquiry of BIS. Like the BIS study, however, it explores the possible use of a method for identifing strategic behavior that has no apparent theoretical or empirical rationale. One cannot appeal to results from the study as having established the presence or absence of strategic behavior in applications of the CVM.

<u>Free-riding in the Experimental Literature: A Digression</u>. Given that the CVM valuation institution is substantively dissimilar to those used in experimental economics, and that there is no firm basis for arguing that behavior in these two sets of institutions is similar, we have argued that one cannot readily justify drawing inferences from findings in experimental economics to subject behavior in the CVM. Regardless of their direct relevance for the CVM, the question remains as to what one might conclude from studies of free-riding behavior conducted in experimental economics. In this regard, we are interested in the question: are findings of an absence of pervasive free-riding in experimental studies as monolithic as suggested by citations of the type exemplified above?

In terms of the free-riding behavior of subjects, applications of the Smith auction are mixed and clearly do not support the general conclusion that free-riding is absent. Two factors in particular render such a claim false. First, the fact that the collective decision tends to be the efficient one when there is agreement does not mean that each individual has truthfully revealed his preferences, which is what incentive-compatibility or "demand revelation" require. As Smith [1979b; p.208] points out very clearly:

... the mean bids differ from the corresponding Lindahl equilibrium bids. Consequently, although the Auction Mechanism provides public good quantities that approximate the Lindahl equilibrium quantity the private good allocations do not approximate the Lindahl equilibrium quantities. [This] is because subjects with low endowment [...] tend to contribute less, while subjects with high endowment [...] contribute more, than is required for a Lindahl allocation.

These results are quite general to the many other induced-value experiments conducted with the Smith auction (see Banks, Plott and Porter [1988, p.314], for example). What they mean is that some individuals over-contribute and others under-contribute, and that they do not do so at random.

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Second, the success rate of the Smith auction is not high, and when the group fails to come to an agreement in the induced-value control experiments this means that at least one subject has not revealed his preferences truthfully. Smith [1979a] observed a failure rate of about 10%, Smith [1979b] a failure rate of 20%, and Banks, Plott and Porter [1988] a failure rate of 50%. When one allows for these failures, the efficiency of the Smith auction is statistically about the same as a direct contribution mechanism for which free-riding is predicted and confirmed.

The clearest example of this result is provided in Smith [1979b; Table 5, p.207]. The average contribution was 9.10 units with the Smith auction over ten experiments. The average contribution was 7.3 units when a different mechanism for which free-riding is predicted was used. The average reported for the Smith auction excludes those experiments which failed to reach agreement. In a note to this table, Smith indicates that the average for the Smith auction drops from 9.10 to 7.9 if the disagreement outcomes are included and given a value of 3.33 (which is the theoretical free-riding prediction). If one includes disagreement outcomes and assigns them their actual value, zero, one obtains an unconditional average provision level of only 6.3 for the Smith auction, which is *below* the average provision level of the free-rider procedure (7.3). On the other hand, Banks, Plott and Porter [1988; Table I, p.316] report significantly higher (unconditional) provision levels with the Smith Auction than with a free-rider mechanism. The appropriate conclusion is that the efficiency of the Smith auction is sensitive to the specific environment in which it is used. Strategic behavior is observed in some, but not all, environments.

The implications of results from applications of the Voluntary Contribution Mechanism for freeriding behavior are also inconclusive.

There exists a set of studies using the Voluntary Contribution Mechanism that report results showing very little free-riding behavior on the part of subjects. Most notable among these studies are Chamberlin [1974], Marwell and Ames [1979] [1980], and Schneider and Pommerehne [1981]. An example of conclusions suggested in these works is the following: "(...) individuals did systematically behave as free riders (...) but the extent to which free riding occurred was not great." (Schneider and Pommerehne [1981; p. 702]) Since these studies

are given detailed attention in Mitchell and Carson [1989; pp. 133-136, 139-143, 146-148],⁸⁵ we will not repeat that detail here.

There also exists a good number of other studies which draw *diametrically opposite conclusions*.⁸⁶ As examples, Isaac, McCue and Plott [1985; Table 2, p. 61] find pervasive free riding. Actual contributions by subjects as a percent of true (induced) valuations are found to be only 37%, 19%, 12%, 9% and 9% in trials one through five, respectively. Harrison and Hirshliefer [1989; Table 7, p. 216] also find pervasive free-riding behavior in their experiments with the Voluntary Contribution Mechanism. Subject contributions as a percent of true (induced) values for all trials averaged but 32%. Further, Harrison and Hirshliefer's [1989; p. 218] theoretically based prediction for "perfect" free riding is a value for the ratio of subject contributions to true values of 33%. Thus, their findings were consistent with the perfect free riding prediction. Along similar lines, Kim and Walker [1984; p. 4] conclude that "Free riding behavior in the reported experiment was overwhelming, systematic, and very much in accord with economic theory." Other studies involving applications of the Voluntary Contribution Mechanism that report results similar to those given above include Andreoni [1988], Isaac and Walker [1988a] [1988b] [1991], and Isaac, Schmidtz and Walker [1989].

Thus, if one focuses solely on the works by Chamberlin [1974], Marwell and Ames [1979] [1980], and Schneider and Pommerehne [1981], one might conclude that, in terms of behavior in the Voluntary Contribution Mechanism, the case is effectively closed: there is very little in the way of free riding. When these works are viewed within the context of the more complete literature that is relevant for this question, it is clear that monolithic "conclusions" one way or the other are simply not justified.

Summary. Do the studies reviewed above provide the compelling evidence that would

⁸⁵ We note other studies cited in these regards by Mitchell and Carson [1989; p. 135] in support of their argument that voluntary demand revelation mechanisms might "outperform" incentive compatible mechanisms: Sherr and Babb [1975] and Grether and Plott [1979]. Sherr and Babb [1975] do not use induced values, so they have no basis by which strategic bidding can be identified in either their control group or in the group which participates in a form of a Pivot Mechanism experiment. References to Grether and Plott [1979] as providing "evidence" related to free-riding must be viewed as gratuitous: Grether and Plott [1979] do not address this issue.

⁸⁶ Referring to the studies cited above that find little free-riding, Kim and Walker [1984; p. 5] observe that "The outcome furthermore suggests that an explanation of the previous, contrary experimental results [which refute the free-rider hypothesis] is likely to be found in one or more of the invalidating factors that were present in previous experiments."

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justify the conventional wisdom that CVM subjects must generally answer truthfully, as opposed to strategically, in the valuation institution used in the CVM? It is our view that one cannot draw such evidence from results that have been obtained in experimental economics. Institutions used in these studies are, on their face, dissimilar to that used in the CVM, and there exists no evidence that would unequivocally establish that, in terms of how subjects behave in one institution or the other, subject behavior in the CVM is similar to that in the Smith auction or the Voluntary Contribution Mechanism. In any case, one can draw no definitive conclusions regarding free-riding behavior from studies conducted in experimental economics: one set of literature says yes, another says no. Finally, the few studies concerning free-riding behavior in the CVM valuation institution provide results that are at best inconclusive. We then find that the conventional wisdom regarding strategic behavior in CVM studies to be lacking in terms of any unequivocal empirical foundation.

<u>Asserted Incentives for Truth-telling in the CVM: The Dichotomous Choice Approach</u>. The use of dichotomous choice methods has become increasingly common in applications of the CVM. This method involves a subject responding "yes" or "no" to a question that asks whether or not he would be willing to pay some stated amount for the environmental good under study. The growing use of this approach is seemingly based on a general acceptance of still another evolving conventional wisdom: that the dichotomous choice approach yields "incentive-compatible" results, in that subjects will respond truthfully to CVM questions posed as a dichotomous choice. The argument that a dichotomous choice approach to applications of the CVM will result in "incentive-compatible" (truth-telling) values is exemplified by the following:

"In a policy referendum model with individually parametric costs, truth-telling is the individually optimal strategy." Hoehn and Randall [1987; p. 237]

"We also showed that the discrete-choice referendum model was incentivecompatible in the sense that a person could do no better than vote yes if her WTP for a good being valued by this approach was at least as large as the tax price, and to vote no if this was not the case. This finding offers the possibility of framing contingent valuation questions so that they possess theoretically ideal and truthful demand-revelation properties." Mitchell and Carson [1989; p. 151]

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On what basis are we to accept as "fact" the claim that individuals must tell the truth in applications of the CVM using the dichotomous choice approach?

It is well known in social choice theory that simple majority rule is incentive-compatible if there are only two objects of choice and each agent has well-defined preferences over the two objects. This result does not generalize to voting over three or more objects. What is crucial to these results, however, is that the voter's utility be affected by the outcome of the voting process. We do not question the theoretical result that dichotomous choice is incentive-compatible under this assumption, but we do question the relevance of the assumption in CVM applications.

Hoehn and Randall [1987; p. 237] make explicit the assumptions underlying their conclusion that the dichotomous choice approach elicits truthful responses. First, the respondent must *believe* that the public good will be provided or a policy implemented if a plurality of citizens (CVM subjects?) approves it. Secondly, he must *believe* that approval is conditional on each person paying the "cost" set out in the dichotomous choice question. Thirdly, uncertainty as to how others will vote is assumed to provide the individual with an incentive to participate in the "referendum." Fourthly, CVM participants place some positive value on the opportunity provided by a CVM exercise for influencing policy. And fifth, the individual will invest some positive effort in determining an individually desirable stated valuation (p. 238).

Hoehn and Randall [1987; p. 240] do not claim the existence of empirical evidence to verify their assumptions. They do, however, suggest that their model yields unambiguous and refutable consequences, and that for several such consequences existing evidence (however sparse) tends to be confirming rather than refuting. To the extent that these latter claims extend to their conclusion that the dichotomous choice approach yields incentive-compatibility, we must agree with Hoehn and Randall [1987; p. 238] that their assumption (described above as assumptions four and five) may indeed be "controversial." We would add assumptions one through three to the list of controversial assumptions. Let us briefly sketch the substance of this controversy.

Basically, assumptions one through four can be taken as implying that the individual cares about the good being valued and that he believes that the CVM survey will actually affect the decision to provide or not to provide the good: his utility will be affected by the outcome of the

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survey. He must also believe that provision of the good will actually depend on his payment of the cost set out in the CVM questionnaire. Satisfaction of these assumptions would require, in our view, particularly perverse behavior on the part of CVM subjects. Notwithstanding the fact that *both* the good to be provided *and* the subject's payment are hypothetical in the CVM, the subject is assumed to view them both *as being real*. Perhaps there exists means by which subjects might be fooled into believing that the good and payments are real. We are unaware of empirical evidence that subjects can be so fooled.

As something of an aside, if subjects *could* be fooled into believing that goods and payments in the CVM were real, we should note the concerns of some CVM researchers about the relationship between truth-telling (nonstrategic behavior) and the hypothetical nature of the CVM which are at direct odds with Hoehn and Randall's [1987] arguments. Some CVM scholars look to hypothetical goods and payments, a *failure* of Hoehn and Randall's [1987] assumptions, as being "the solution" to avoiding strategic behavior in CVM studies. In presenting examples of this position and it's critics, we caution the reader that there exists no empirical data which might support or refute the idea that observations of strategic behavior decline with more hypothetical valuation institutions.

"Another point that deserves emphasis relates to the strategic hypothetical bias argument. The point attributed to Rick Freeman ... is important and bears repeating. There is no strategic bias so long as the CVM is strictly hypothetical. If it is hypothetical, then the respondent knows his answer won't affect any policy, and there is no incentive to misrepresent preferences. But if it is hypothetical, there is no great incentive to go through the effort and cost of sharp calculation to elicit true preferences. This is the real conundrum in the method and underlies my initial skepticism about the CVM." Rosen [1986; p.196]

"Neither the empirical evidence nor the theoretical arguments convinced me that strategic bias is liable to be significant. Sherwin Rosen does raise a point: Supposing I am asked, "From now on will you use the survey data?" That is, will survey data form the basis of our judgements? *Then, indeed, I suppose one might have some problems.*" Arrow [1986, p. 183, emphasis added]⁸⁷

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⁸⁷ Arrow continues "But let's not think that far ahead. This means the whole discussion about Vickrey auctions and the like [in experiments testing the CVM], which are basically incentivecompatibility methods, are really beside the point. I don't think this has much to do with the basic issue." (p.183, emphasis added) Thus, it would appear that Arrow might forgive concern with strategic bias so long as it is couched within the context of the ends sought in CVM research during the mid-1980s: experimental efforts by academicians to explore the feasibility of the CVM as a means for valuing public goods.

"... if respondents to such a [CVM] survey do not believe the survey will have any impact on policy or outcomes, then no incentives for bias exists. The hypothetical nature of such surveys may then aid in eliciting bids that are not strategically biased." Schulze, D'Arge and Brookshire [1981; p. 156]

Mitchell and Carson [1989] "show" that the discrete-choice referendum model is incentivecompatible in a two-paragraph discussion (pp. 148-149) concerning research which focuses on political markets and includes a citation to Hoehn and Randall [1987]. The political markets literature to which they refer does, as they suggest, argue that the yes or no format to a voting question can be incentive-compatible. This conclusion rests, however, on some of the same assumptions required by Hoehn and Randall [1987]: subjects care about the good and they believe that their vote will actually influence the outcome. Mitchell and Carson [1989] speak to these requirements as they are relevant to the CVM in only one way. They note that with discrete-choice models, where voters regard control of the quantity of the good and its tax price to be exogenous, it has been shown that there is no rational reason for individuals to vote, inasmuch as their chance of influencing decisions is small if there are many voters. Mitchell and Carson [1989; p. 149] seem to simply assume this problem away with the statement: "Voting is assumed to be motivated by a sense of social and civic responsibility." This assumption runs counter to Brennan and Buchanan's [1984] argument that when people feel that they cannot influence decisions, they may vote symbolically. The Brennan and Buchanan [1984] argument would appear to be dismissed out of hand by Mitchell and Carson [1989; p. 149, fn. 33] with the following statement of their preferred alternative interpretation of the argument: people may behave in a more public-spirited manner in political markets than they do in private good markets.

What can we conclude regarding the correctness of the emerging conventional wisdom that the dichotomous choice approach is incentive-compatible, and that the approach results in truth-telling in applications of the CVM? The nature of the valuation institution used in the CVM is one wherein the assumptions given as requisites for the approach to be incentive-compatible are not satisfied. We know of no obvious means by which one might fool subjects such that the assumptions might be satisfied.⁸⁸ We then find no unequivocal basis for the

⁸⁸ As a further justification for the use of the dichotomous choice approach, a number of writers argue that the approach is easier to administer, "easier" in the sense that subjects are not required to introspectively determine their precise value for an unfamiliar good. For example, Bowker and Stoll [1988; p. 374] argue that "It is simple to administer because no interviewer is required. Respondents are not faced with intricate bidding schemes; moreover, respondents do not have to contemplate exact (continued...)

conventional wisdom.89

B.2 CVM Behavior and Well-defined Choice Sets

Value assumption **P** requires that individual values derive from a well-defined choice set. The CVM typically invites the subject to introduce a "new" good into this set. Is the individual's valuation for the newly introduced good derived from a well-defined choice set? We will shortly comment on the possible implications of values derived from choice sets which are *not* well-defined.

The importance of well-defined choice sets for true valuations, valuations which reflect a real economic commitment is discussed in a number of studies. The relationship between well-defined choice sets and true values is described by Freeman [1986] within the context of the need for an individual to "explore" preferences.

"It is conventional to assume that individuals have well defined preference orderings and that they know the shape of their indifference curves. Thus, if we observe an individual to accept a trade-off between income and some other good, we believe that he has revealed something about his preference ordering (...)the inference that revealed trade-offs reflect true values or preferences is correct *only if individuals do in fact have full knowledge of their preference orderings* (...) Suppose that due to (...) the introduction of a new good, an individual has an opportunity to choose from among a set of consumption bundles that are unfamiliar to her (...) It seems plausible that she might experiment with several different consumption bundles before settling into a new equilibrium position. This experimentation can be viewed as an effort to explore an unfamiliar part of her preference ordering. We can only accept revealed preferences as reflecting true preferences after this exploration has been completed. Therefore I want to define the true value of the environmental good as that substitution between income and the environmental good which

⁸⁹ These discussions abstract from the somewhat unique data analysis problems that arise with the use of the dichotomous choice approach. In these regards see Hanemann [1984], Cameron [1988], Cameron and James [1987], Kriström [1990] and McConnell [1990]. Numerical examples of problems associated with choices for functional forms used for analyses of dichotomous choice data are given in Bowker and Stoll [1988] and Imber, Stevenson and Wilks [1991].

⁸⁸(....continued)

values for resources for which payment is not customary." This may arguably be the case in instances where it is immediately obvious to a subject that his value is well above a stated value. In all other cases, if we are to suppose that the subject's response reflects a real economic commitment, he must determine his value for the good (as with a response to an open ended question) before a meaningful response can be made to the "yes"/"no" question. Such contemplation by subjects is presumed in our use of dichotomous choice data. For example, Bowker and Stoll [1988; p. 373] argue that "The results of the responses for all subjects were subsequently analyzed...to determine the expected value which respondents place on the resource."

we would observe after repeated trials or opportunities for the individual to alter her consumption position." Freeman [1986; p. 150]

The introspective process of preference research required for the individual to form a well defined choice set seemingly involves two interrelated things: time and information. Particularly in instances where the new good is unfamiliar to the individual, time may be required if the individual is to "explore" unfamiliar parts of his preference orderings. The conjectured importance of time and information is also emphasized by Hoehn and Randall [1987] as follows.

"The time and resource constraints of the CVM context may introduce two sources of error into the value formulation process. First, information errors may arise as complex policy information is communicated to the respondent by the CVM format. Errors may be left uncorrected due to time constraints on repetition and review. Thus, the time constrained process of communicating complete information may introduce an additional source of uncertainty into the policy scenario as perceived by the respondent. Second, once policy information is assimilated, the process of evaluation -- of selecting a bid -- may also be cut short by limited time and decision resources." Hoehn and Randall [1987; pp. 229-230]

But the information requirement for a well-defined choice set is said to have another dimension of potential importance: information as to relevant substitutes and complements for the good being valued. Referring to the customary process by which benefit-cost studies for individual public goods are typically conducted in isolation (where substitutes and complements are *not* considered), Hoehn and Randall [1989] conclude that valuations which exclude relevant substitutes and complements may be misleading.⁹⁰

"For instance, consider only the problem of species preservation, a relatively small subset of environmental concerns. Conventional benefit cost logic demonstrates the nontrivial benefits for each of a limited number of representative species. There seems little reason to doubt that a similar level of benefits could be demonstrated for many other endangered species -- at least when each is evaluated independently. Yet biologists suggest that there are literally hundreds of thousands of species in danger of extinction. Surely, in this sort of policy environment, conventional procedures overlook some crucial element of the evaluation problem." Hoehn and Randall [1989, p. 550]

⁹⁰ We note in passing the challenge to a number of Hoehn and Randall's [1989] conclusions by Quiggin [1991] as well as the response of Hoehn and Randall [1991].

Thus, to continue Hoehn and Randall's example, a well-defined choice set for the individual (whose value is at issue in the benefit-cost study) who is to add "preserve species X" to his choice set may require consideration of many *other* species whose preservation might substitute (given the individual's preferences) for species X.⁹¹

The importance of the individual's consideration of complements and substitutes in developing a well-defined choice set is emphasized by V.K. Smith [1992; p. 72] in the following way:

"The framing of the commodity to be valued using CVM must reflect an understanding of how people perceive it, what people consider related goods (either complements or substitutes), and how people understand the process involved in altering their consumption patterns as part of adjusting to an exogenously imposed change in the good of interest."

Suggested implications of values derived from choice sets that are *not* well defined are also addressed by a number of studies. For example, Hoehn and Randall [1989; p. 550] state that their analyses "... demonstrate that conventional procedures (which ignore substitutes) systematically overstate net benefits...." Of course, this claim presumes that the values so derived reflect true WTP. Accepting this presumption for the sake of the present line of argument, valuation behavior under such circumstances would be clearly inconsistent with the basic theoretical postulates which are claimed to underlie the CVM.⁹²

Taking as given the views discussed above concerning the potential importance of time, substitutes and complements for CVM values reflecting well-defined choice sets,⁹³ in the following subsections we briefly comment on studies that address these issues.

⁹¹ Samples and Hollyer [1989] find CVM values that are sensitive to assumptions regarding multiple species and the sequence of valuation questions. Contrary findings regarding sequencing effects are reported in Boyle and Bishop [1985] and Boyle, Welsh and Bishop [1989].

⁹² See Bishop and Welsh [1992; pp. 12-15] for a discussion of the implications of substitutes for the aggregation of values from distinct CVM studies.

⁹³ We find distinctions between information related to a commodity, information regarding substitutes and complements, and information regarding the decision context (or "rules of the hypothetical game") to be blurred and confusing. Moreover, we perceive the need to distinguish between providing subjects information *about* substitutes and complements and providing subjects with information about the *possible acquisition* of substitute or complementary goods *via* the mechanism provided by the CVM. Time and resource limitations have not allowed us to flesh out the sources of our discomfort with these distinctions and to develop constructive criticism as to the roles of such things as time and information in the CVM. Our discomfort with the present state of understanding of these issues is reflected in the "cautious" manner in which we draw conclusions from the studies examined below.

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<u>Time and Preference Research</u>. We find two studies which are directly relevant to the time and preference research requirements for well-defined choice sets. The first is the study by Whittington, *et al.* [1992]. In this study, two groups of subjects were recruited in three large villages in the Nsukka district of Anambra in Nigeria for participation in a CVM study. None of the villages had an operational water supply system. The good to be valued was access to a community water spigot which would eliminate the necessity to make long trips to a nearby stream for the purpose of obtaining potable water. An alternative good was a private indoor spigot.

An on-the-spot CVM value was obtained from one of the groups of subjects. For the second group, the good was described and then subjects were told that they would meet the following day, at which time a WTP would be elicited. Subjects were invited to think about the good and the maximum amount that they would be willing to pay to acquire the good. Subjects were encouraged to discuss these questions with their family, friends and neighbors. WTP values from the group given "time to think" were found to be significantly *lower* than values obtained from the group that responded to value questions on the day of the survey. Thus, the Whittington, *et al.* [1992] experiment suggests that individuals may require more time to sort through their preferences, to form a "new" well-defined choice set over which preferences are well-behaved, than is typically provided in applications of the CVM.⁹⁴

Arundale [1991] replicates Whittington, *et al.*'s "time to think" procedures. The good valued in this study is a program to reroute the transportation of hazardous materials around major population centers in New Mexico, thereby reducing threats to public health and safety posed by possible accidents involving carriers of hazardous materials. Her subjects were primarily students in introductory economics classes at the University of New Mexico. One group of students was given a standard application of the CVM: the good was described and WTP was elicited immediately thereafter. A second group of students was given a written description of the public good. They were told that they would be asked their maximum WTP for the good during a meeting to be held in *two days* time. They were asked to take the description home with them and, if they wished, to discuss it with family and friends. Two days later subjects

⁹⁴ This CVM involved face-to-face interviews with subjects. In cases where the CVM uses mail surveys, there is no way to impose a "time to think" structure on subjects. The extent to which recipients of mail surveys read them, reflect on the questions, perhaps discuss them with family members, and then respond to the value questions is problematic.

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were given a written description of the good and a WTP elicited. The results parallel those of Whittington, *et al.* [1992]: WTP from the second group of subjects that were given "time to think" were significantly lower than those obtained from the first group.

Results from these studies may be taken as suggesting that CVM values can be sensitive to whether or not subjects are give "time to think."⁹⁵

There are additional CVM studies which focus on a question which may be viewed as indirectly related to the time-preference research question examined above: does a subject's CVM bid change over time? This question is referred to as the "bid stability" of the CVM value. Note here that we are not necessarily inquiring as to the effects of time on the subject's formulation of a well-defined choice set as we did above. Instead, we ask: given a subject's reported bid at one point in time, however that bid might be formulated, if the subject is asked to value the same good at a later point in time, will he report the same value?⁹⁶ This line of inquiry would parallel the time-to-think line of inquiry only if one had good reason to believe that subjects think about their valuation of the good between the times of the first and second CVM questions. Subjects are not asked to perform such a task in the bid stability literature reviewed here.

An example of this type of study is Kealy, Montgomery and Dovidio [1990]. In this exercise, CVM values for a private good (a chocolate candy bar) and a public good (a de-acidification program for lakes in the Adirondack region of New York State) were obtained from a set of subjects. Two weeks later, the same set of subjects were again asked for their WTP for the private or public good; each set of subjects valued only one of the goods. After analyzing the stability of CVM bids obtained over the two week period, it is concluded that subjects "... appear to have stable preferences for both the private and public goods" (p. 255). It must be noted, however, that these subjects were undergraduate students whose participation in the experiment partially fulfilled their course requirement for an introductory psychology course.

⁹⁵ An obvious question for future research is suggested by these studies: how will *shorter* amounts of time to think about their bids effect subject behavior? Does "preference research" require 30 minutes, an hour, or several hours? This question should also be addressed with some controls for the other things that subjects can do during the allotted "time to think", so that any differences in values can indeed be attributed to the time needed for preference research. Also, we note that reference to these procedures as "time to think" includes time to discuss values with family and friends.

⁹⁶ This is referred to in the literature as "reliability."

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Thus, they may well have acted in a (consistent) manner to satisfy their instructor, rather than reporting the true WTP. Moreover, prior to obtaining subjects' re-bid for the CVM good at the end of a two-week period, subjects were *reminded of their original WTP* bid (p. 250). Thus, while suggestive, these results must be interpreted within the context of the author's caveats: "We caution the reader that there is a strong possibility that our comparison of first-period report with second-period report behavior could be overly favorable to the predictive validity of CVM. Subjects may be reluctant to contradict their previous response, even if it overstates their true WTP." (p.258)

Loomis [1989] uses the CVM to obtain WTP values for improving water quality at Mono Lake in California. Two populations are surveyed: randomly selected households throughout the State of California and onsite visitors at Mono Lake. Some 3 to 6 months following the original survey, subjects that participated in the survey were re-contacted with a cover letter that indicated to the subjects that the new survey was a follow-up survey to the one that they had previously taken, and that interest was in their "current" values for Mono Lake. The correlation between initial values and follow-up values ranged between 42% and 78%. When comparing the stability of values obtained in the original and follow-up surveys between the general population and the visitor population, Loomis [1989; p. 83] concludes that "Overall, little can be said regarding the relative stability of values. While it seems plausible that visitors have a better defined image of the commodity to be valued than general households. visitors' definition of the commodity may deteriorate as the time from their last visit lengthens." When visitor and general population data are pooled, Loomis [1989; p. 83] reports relatively stable values in the sense of high correlation between initial and follow-up values. He does not address issues related to the extent to which subjects might have incentives to appear consistent in their responses to the two surveys.

A final example of this class of studies⁹⁷ is seen in the study by Reiling, Boyle, Phillips and Anderson [1990]. These authors focus on the stability of CVM values for a seasonal good that is valued during different seasons. Their "good" involves the control of late-season black flies along a section of the Penobscot River in the State of Maine. Unlike the Loomis [1989]

⁹⁷ Jones-Lee, Hammerton and Philips [1985] also report a test-retest procedure for CVM values related to transportation risks. A sub-set of 210 subjects responding to a CVM questionnaire were asked to re-answer one question (included in the original questionnaire) a month later. They find no statistical difference between original and recall responses but note that the standard deviation for recall responses were almost twice the mean response in the original interview (p. 67).

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study which tests and re-tests the same population, Reiling *et al.* [1990] survey two different samples from the same population at two different points in time. One sample is surveyed during August and September, which is at the peak of the black fly season. The second sample is surveyed during late October and November. Subjects in each survey are asked their maximum WTP for three levels of control: 60%, 75% and 90% reductions in the number of adult black flies. Information concerning attitudes and household characteristics are used to test the hypothesis that the two samples had the "same" characteristics (p. 131). Only two characteristics were significantly different between the two samples: household size and a second characteristic that is not identified by the authors (p. 132). Reiling, *et al.* [1990] find no significant difference in values for any of the three levels of control between the two groups of subjects. They then conclude that CVM values are temporally reliable.

Studies that directly address the issue as to whether subject behavior in CVM studies is consistent with the "time to think" requirements for well-defined choice sets suggest that CVM values may be sensitive to the amount of time that subjects are given to formulate their values. In other words, on-the-spot values obtained in typical applications of the CVM may lack real economic content since subjects have not fully researched their preferences. The three examples of "bid stability" studies reviewed suggest reasonable correlations between values obtained with a test-retest procedure, but the direct relevance of these findings for the time-to-think question is unclear. The bottom line is that we have no definitive evidence regarding the time-to-think question. The limited evidence which does exist, along with the more general state of our ignorance concerning these important considerations, would seem to be sufficient to impose a great deal of modesty on the part of CVM practitioners as to any claim for a "solid theoretical foundation" underlying the CVM.

<u>Information, Substitutes and CVM Values</u>. The notion that changes in information given CVM subjects will affect values reported for a particular environmental good is argued by Cummings, Brookshire and Schulze [1986; p. 54-55] and derived as a testable hypothesis from an extended model of consumer choice by Hoehn and Randall [1987]. There are a number of studies which develop empirical results which may be interpreted as testing the hypothesis that changes in information result in significant changes in subjects' valuations.

The implications of such findings are of direct relevance for our inquiry as to the consonance of individual behavior in applications of the CVM and the assumptions of value theory: *either*

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individual's reported values are not based upon well-defined choice sets *or* different information sets represent *different* "new" goods which the individual is asked to place within his choice set. Both of these implications raise serious questions as to the interpretation of CVM values for a particular good.

In the former case, a real economic commitment on the part of the individual may not be attributed to the CVM value. In the later case, different information regarding a good arguably *should* change the individual's valuation. To a large extent, new information may be viewed as having the effect of presenting the subject with a new good. The problem which arises is that one must then be prepared to specify the "correct" quantity and quality of information, and therefore the "correct good," which must be given to subjects. But how does one correctly describe the good?

Bergstrom, Stoll and Randall [1989] test the hypothesis that CVM values will increase in response to subjects being given "perspective" and "relative" information concerning the relationship between bids and incomes. An initial WTP is obtained from student subjects for facilities that provide access to a well-known river in central Texas used for recreation. Subjects are then told the percent that this bid represents of the subject's annual income, and are then allowed to revise their bid. The revised bid, expressed as a percent of income, is then compared with other expenditures (for such things as clothing, rent, etc.), and subjects are allowed once again to revise their bid. Finally, subjects are told the cost of the river access program, and that their previous bid, if paid by "everyone," would be insufficient to cover the program costs, and are allowed again to revise their bid. Changes in bids at each opportunity for bid-revision are found to be statistically insignificant. Final bids, however, are found to be significantly greater than initial bids. The "significance" of these results is problematic, however, given the process of continually "prodding" subjects used in this experiment (see Bergstrom, Stoll and Randall [1989; p. 689, fn. 1]).

The effects of "service information," which is information concerning uses of a commodity, on CVM values are examined by Bergstrom, Stoll and Randall [1990]. A mail survey is used to elicit CVM values for three scenarios (concerning hunting bag levels and fish catch levels) related to the protection of wetlands along a large section of Louisiana's coast. One set of recipients of the mail survey was given "service information" not given to the other set:

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information concerning other *positive* services provided by the wetlands (wildlife encounters; natural scenery and isolation or remoteness). The authors find (p. 620) that the provision of "service information," as provided in their study, significantly increases subject's CVM values.

We next consider the extent to which it can be claimed that CVM subjects, in the formulation of their value for a specific environmental good, take into consideration implicit trade-offs with *other* public goods which might substitute for, or be complementary to, the specific good.

In typical applications of the CVM, subjects are asked to value a single environmental good which is generally posed as arising from the adoption of a given environmental policy. Environmental policies may effect multiple dimensions of the environment, however. In such cases, the single-good procedure then values each element of a multidimensional policy separately, thereby ignoring substitution or complementary effects which might attend implementation of the policy. On theoretical grounds, such an approach is argued by Hoehn and Randall [1989] to be conceptually flawed.⁹⁸

The potential empirical magnitude of this flaw is examined in a study by Hoehn [1991]. One group of Chicago subjects was asked to value an 83% air quality improvement in the Grand Canyon National Park. A second group of subjects was asked two questions. First, their valuation of a 100% air quality improvement in Chicago. Following their response to this question, they were asked to value a joint program: a 100% air quality improvement in Chicago *and* an 83% air quality improvement in the Grand Canyon National Park. The mean value for the program to improve air quality in the Grand Canyon National Park and in Chicago was \$83.00/household and \$179.00/household, respectively, when the programs were valued in isolation. The *joint* program was valued at \$190.00/household, however. These data imply that there is considerable substitutability between air quality improvements in Chicago and the Grand Canyon National Park. The extent of this substitution cannot be derived from these data, however, inasmuch as the joint value of \$190.00 cannot be allocated between air quality in Chicago and the Grand Canyon.⁹⁹

⁹⁶ Boyle, Reiling and Phillips [1989] consider the effects of varying prices of substitute goods on CVM values for one specific good. Their study points to the need to consider the effects of changes in prices of substitutes which result from a policy action on CVM values (p. 300).

⁹⁹ See Loehman and De [1982] for a similar study where an attempt is made to value components of health effects from air pollution.

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Neill [1991] addresses these issues in a series of CVM experiments using undergraduate students in introductory and intermediate economics classes at the University of New Mexico. She obtains subject valuations for (i) a program which protects the squawfish from extinction; (ii) a program to reroute the transportation of hazardous materials around major urban centers in New Mexico; and (iii) a package of eight *environmental* programs which include (i) and (ii).¹⁰⁰ In experiments (i) and (ii) subjects value one good. In experiment (iii) subjects valued eight goods.

Neill [1991] finds that values for programs (i) and (ii), when valued in isolation, are significantly higher than those obtained when the programs are included with substitute goods. Specifically, the average WTP for the squawfish in (i) is \$48, but drops to only \$22 in (iii). With respect to hazardous materials transportation, she finds that the average WTP falls from \$157 in (ii) to only \$29 in (iii).

There is one additional finding in the Neill [1991] study which is of interest. In an experiment (iv) she asked subjects to value the squawfish in a CVM format. Prior to posing the WTP question, however, subjects have described to them the seven environmental goods used in experiment (iii). In contrast to experiment (iii), she does *not* ask subjects in experiment (iv) to state a WTP for the other seven environmental programs. The objective of this variation was to see if the WTP for the squawfish is affected by simply providing subjects with information about substitutes. Neill [1991] tests the hypothesis that the value of the squaw fish obtained in (i) is the same as that obtained in (iv), and fails to reject the hypothesis. This result, along with results from her comparisons of (i)-values with (iii)-values, suggests that significant differences in bids obtain *only* when subjects are allowed to "acquire" substitute goods with the same CVM medium used to "acquire" the squawfish. Simply describing substitute goods leave unaffected the value attributed by subjects to the squawfish.

Schulze, McClelland and Waldman [1991] report a CVM experiment involving subjects' values for reduced air pollution in the Denver, Colorado area. Subjects are asked their WTP for (i) visibility improvements associated with a specific air pollution control program, then (ii) a

¹⁰⁰ The other six environmental goods involved: eliminating health threats from radon gas in homes; improving air quality in the City of Albuquerque; improving water quality in the Rio Grande; eliminating mercury contamination in New Mexico's lakes; clean-up of groundwater contamination in the City of Albuquerque; and a waste recycling program.

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separate WTP for health improvements from the pollution control program, and then (iii) their *total* WTP for visibility and health improvements associated with the pollution control program. Forty percent of the subjects submitted the *same* bid for (i), (ii) and (iii). In other words, these subjects offered a stated WTP of (e.g.) \$50.00 for the visibility improvement, \$50.00 for the health improvement, and \$50.00 for *both* health and visibility improvements. Schulze, McClelland and Waldman [1991] interpret these findings as supporting the notion that individual values for any one effect are "embedded" in a value for some broader environmental good, or that individuals view the government's provision of public goods as something akin to a joint production process. In valuation terms, however, their findings would appear to be equally consistent with the hypothesis that the set of goods are viewed by subjects as perfect substitutes.

For completeness, in closing these discussions, we note the recent study by Kahneman and Knetsch [1992a] which argues that the value of any one or a set of environmental goods is somehow "embedded" in values for environmental goods in general.¹⁰¹ This line of argument parallels an anecdote offered by Kahneman and Tversky [1979] which suggested that, in allocating scarce incomes, individuals might consider purchases in a manner that would be consistent with their having "mental accounts" for different classes of goods. While the "mental accounts" notion was not particularly new and can be given simple meaning using the economists' notion of weak separability, it did serve to prod CVM researchers' interest into questions of substitutes and their relevance for CVM values, and in the problem of defining the range of substitutes which might be relevant for particular types of environmental goods (see, e.g., Schulze, *et al.* [1983]).

In any case, Kahneman and Knetsch [1992a] empirically test one interpretation of the mental accounts notion, which they call "embedded values." An embedding effect occurs when the WTP for one good is found to be insignificantly different from the WTP for a more-inclusive good. Their results are interpreted by them as indicating that CVM values obtained for a good that is included in a more comprehensive good cannot be differentiated from those obtained

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¹⁰¹ For a related argument that subject values for one endangered species may represent a general value for *all* endangered species, see Bishop and Welsh [1992; pp. 7, 15].

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for the more comprehensive good.¹⁰²

B.3 The "Solid" Theoretical Foundation for the CVM: Closing Remarks

Lacking as we do a conceptual economic model which describes in some axiomatic sense the manner in which we expect subjects to behave in the CVM, the position taken implicitly or explicitly by many CVM researchers is that one can simply appeal to received value theory as providing such a theoretical foundation. A critical inference is drawn here. Acceptance of the applicability of received theory of value in real markets to subject behavior in the CVM is used to infer the *real economic commitment* of values observed in real markets to values derived with the CVM. Let us briefly summarize the results of our review of literature which is relevant for any assessment of the solidness of the theoretical foundation underlying the CVM.

We find in the literature limited evidence relating to the consistency of subject behavior in CVM studies with key assumptions in value theory: the incentives assumption and the assumption regarding well-defined choice sets.¹⁰³ In terms of incentives, our concern is with the incentives that CVM subjects have for determining and then reporting their true values for a good, "true" values in the sense that they closely approximate the maximum amounts that they would in fact pay for a good. When payments are hypothetical, subjects *may* have incentives to behave strategically, to report values which are not "real" values in the sense of reflecting a real economic commitment. We have noted the conventional wisdom accepted by many CVM researchers that "evidence" exists which justifies the general expectation that CVM subjects will not engage in such behavior (ergo, their values reflect a real economic commitment). We have shown that this."wisdom," and its inferential conclusions, lack a substantive foundation. Valuation institutions used in experimental economics are very different from those used in the CVM. There does not exist unequivocal evidence that would suggest that subject behavior in the CVM is reasonably "like" subject behavior in experimental

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¹⁰² We must note the ongoing controversy concerning the weight that is appropriately given to Kahneman and Knetsch's [1992a] results. In this regard, see Burness *et al.* [1991], V.K. Smith [1992], Harrison [1992a], and Kahneman and Knetsch [1992b].

¹⁰³ We have abstracted from questions related to the consistency of subject behavior in the CVM with the axioms of revealed preference (see Samuelson [1948], Houthakker [1950], and Varian [1982] [1983] [1988]). This line of inquiry, addressed by Adamowicz and Graham-Tomasi [1991], remains at an infant stage of development. While their approach represents an imaginative first step in efforts to examine subject behavior in an explicit revealed preference context, any meaningful interpretation of their numerical results is subject to important qualifications.

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institutions. There does not exist a body of studies that unequivocally demonstrates the lack of pervasive strategic behavior in valuation institutions used in the CVM *or* those used in experimental economics.

But the incentives issue is not limited to speculations as to whether or not CVM subjects are truthful in their reporting of values. Let us suppose that subjects do *not* behave strategically. Indeed, our experiences with applications of the CVM leaves us with the feeling that many, if not most, subjects do not purposefully lie (we have no proof one way or another, of course). The relevance of the incentives issue nonetheless remains. With the good and the payment hypothetical, we must ask: what incentives do subjects have to make *good* choices, to undertake the process of researching their preferences to the end of determining what they would *really* be willing to pay for the good in question?

The core of the incentives issue is seen in the following question: do people make the same kinds of choices when asked what they *might* pay for something as they do when they are faced with circumstances where they *must* pay to acquire something? Arrow [1986; p. 183] comments that he finds "... the hypothetical bias concerning payment more serious than that about commodities. This is the concern of those who follow the economists' tradition which criticizes hypothetical questions. Verbal answers don't hurt the way cash payments do." A basis for such concern is suggested by results reported by Kealy, Montgomery and Dovidio [1990]. They examine the hypothesis that preferences are the same among the two groups of subjects, one that is asked for hypothetical payment and one that is asked for actual payment. They reject this hypothesis, a result which they interpret as suggesting that "...in the hypothetical situation preferences are less well formulated because subjects have *less incentives to seriously contemplate their actual willingness to pay."* (p. 257, emphasis added)

As another, perhaps more indirect, example, Smith and Desvousges [1987] find significant differences in marginal valuations of risk with declining risk levels that result from changing information relating to baseline risks of premature death. Economic theory would predict that an individual's values for additional incremental reductions in risk would decline as baseline risk levels decline. This is a simple application of the principle of diminishing marginal utility. However, in Smith and Desvousges' [1987; p. 108] application of the CVM they find *increasing*



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marginal valuations for risk reductions as baseline risk levels are reduced.¹⁰⁴ Were these CVM subjects "irrational" or did subjects simply have less incentives to seriously contemplate their actual willingness to pay? In our view, one would look to the latter hypothesis to explain the behavior observed in the Smith and Desvousges [1987] experiment.¹⁰⁵

We have also argued that use of the dichotomous choice approach in applications of the CVM does not resolve the incentives problem. The assumptions underlying the assertion that this approach is incentive-compatible require a valuation institution wherein goods and payment are viewed by subjects as being "real" and that they affect the subject's utility, in direct contrast to the setting of the CVM wherein both goods and payment are hypothetical.

We have also examined the literature for evidence pertaining to the notion that CVM subjects report values which are derived from well-defined choice sets. Our review of available evidence, which is admittedly sparse, suggests that there is little that would firmly establish a position in this regard one way or another. CVM values may be sensitive to the amount of time that subjects are given to think about their valuation of the good. The context in which subjects consider substitute and complementary goods may also have substantive effects on CVM values.

Moreover, the value which one obtains from an application of the CVM may be affected by the amount and kinds of information given to subjects. Bergstrom, Stoll and Randall [1990] demonstrate significant *increases* in CVM values that result from providing subjects with information concerning desirable services provided by an environment. But this raises a critical question which is acknowledged by these authors: would CVM values *decline* if subjects had described to them *negative* aspects of the environment? Within the context of their wetlands study, such negative attributes might include such things as "(...) exposure to

¹⁰⁴ More precisely, subjects were randomly assigned base-line risk levels. The theoretical expectation would be that subjects assigned low base-line risk levels would value a given reduction in risk less than subjects valuing the same risk reduction but assigned higher base-line risk levels. Jones-Lee, Hammerton and Philips [1985] obtain similar results. More than 40% of their subjects gave the same values for different levels of risk reduction (p. 67, section B.a). Overall, however, they do find responses to some questions that reflect consistency with diminishing marginal utility (see, e.g., p. 66, result A.c).

¹⁰⁵ We acknowledge a range of other possible explanations for these observations. Any or all of the many other problems with the CVM discussed in this report may be relevant for explaining the observations.

insects, poisonous snakes and hungry alligators." (p. 620) Thus, they correctly conclude (p. 620) that "Information is also a critical component of contingent valuation studies, but the effects of potential information biases are poorly understood."

Is there evidence of any substantive kind that might support the unequivocal conclusion that the CVM enjoys a "strong" theoretical foundation? In the light of the results summarized above, we must conclude that there is not.

C. DO CVM VALUES CLOSELY APPROXIMATE REAL ECONOMIC COMMITMENTS?

After a review of literature related to the theoretical foundation of the CVM, the reader may or may not be moved to share our conclusion that this foundation is weak at best. An important issue remains, however. One might take the following position: maybe the theoretical basis for the CVM is not so "solid," but the CVM "works" anyway. Referring to the possibility of strategic behavior, perhaps institutions *don't* matter or perhaps CVM subjects do not behave strategically, notwithstanding the paucity of evidence that they do not. Theory and institutions aside, perhaps the CVM still yields values which reflect real economic commitments.

Such a methodological position is not without precedent in economics. For example, in response to criticisms of the relevance of value theory, Friedman [1953; p. 15] has taken the line of argument that our models may be valid, even if one or more of the assumptions underlying them are not. If behavior predicted by the model reasonably approximates actual behavior, then the model is a legitimate tool for empirical analysis, at least until a better model is developed.

We do not attribute this position to any of our colleagues. But, in considering this argument, we are not simply poking at a straw man. This argument is directly relevant to the incentives issue discussed in *CVM valuation and the incentives assumption*. The question posed there, setting aside possible strategic behavior, was: do subjects in the CVM have incentives to make the same kinds of "good" choices that they have when faced with actual payment? This section's focus on the question "do CVM values approximate real economic commitments?" may then be viewed as an extension of our earlier concern with the incentives question.

To the end of examining this general position, we begin in *Comparing CVM results with results* from indirect estimation methods: Inferential evidence of real economic commitments? with a

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brief discussion of studies that compare CVM results with results from indirect estimation methods. *CVM values and real economic commitments* examines results from studies that directly address the question: will subjects *actually* pay amounts that they *say* that they will pay in the CVM.

C.1 <u>Comparing CVM Results with Results from Indirect Estimation Methods:</u> Inferential Evidence of Real Economic Commitments?

One set of efforts designed to assess the validity of CVM values focuses on comparisons with those derived with estimation methods that essentially rely on values indirectly implied by consumer behavior. The methods most often used for these comparative purposes are variations of the travel cost method (TCM) and the hedonic price method (HPM). While not exhaustive, examples of these and other comparative studies include the following: Bishop and Heberlein [1979], Bishop, Heberlein, Welsh and Baumgartner [1984], Blomquist [1984], Brookshire, Thayer, Schulze and D'Arge [1982], Brookshire, Thayer, Tschirhart and Schulze [1985], Cummings, Schulze, Gerking and Brookshire [1986], Desvousges, Smith and McGivney [1983], Hoehn and Randall [1985], Loehman [1984], Knetsch and Davis [1965], Smith, Desvousges and Fisher [1986], and Thayer [1981].¹⁰⁶ Summaries of these studies and their results are found in Cummings, Brookshire and Schulze [1986; Chapter 6] and Mitchell and Carson [1988] [1989; Chapter 9].

The relevance of studies comparing results from a CVM with those from the TCM or HPM for the question of interest here, whether or not CVM values reflect a real economic commitment, is at best indirect. Suppose that one obtains value measures with applications of the CVM and the HPM for a particular environmental good. Suppose further that statistical analyses result in the rejection of the hypothesis that the two values are different; i.e., the CVM value is statistically indistinguishable from the HPM value within some confidence limits. Does this finding establish that the CVM has yielded a "true" or "accurate" value for this particular good, in the sense of a value that reflects a real economic commitment? Of course, the response to this question is: *only* if one is prepared to assert that the HPM value is "true," "accurate," or reflects a real economic commitment. Few, if any, would be prepared to defend such an assertion, however. This is to say that we have no basis for knowing in any precise way the relationship between HPM values or TCM values and real economic commitments that

¹⁰⁶ See, also, Mitchell and Carson [1984], Tolley and Randall [1985], Walsh, Sanders and Loomis [1985], Sorg, *et al.* [1985], Walsh [1986], and Sorg and Nelson [1986].

subjects may be willing to make for public goods. Thus, V.K. Smith [1986; p. 174] correctly notes that "Comparisons of indirect [HPM and TCM] and CVM estimates are largely useless unless we can bound the nature of the errors associated with the indirect estimates." Mitchell and Carson [1988; pp. 188 and 190] note that,

"Although suggestive, these comparisons have their limitations...it is well recognized that estimates based on the indirect methods...are themselves prone to error, owing to that fact that their indirect relationship with the good being valued necessarily requires the use of largely arbitrary assumptions to arrive at a WTP amount...convergent validity involves comparing two estimates, neither of which can be assumed to represent the unmeasured variable. Although it is reassuring when the two measures of the same concept are close to one another, the possibility exists that both are inaccurate."¹⁰⁷

All of this is to simply say that a showing that CVM values for a particular good are "close" to those derived with an indirect estimation method obviously demonstrates no more than that the two methods yield similar results. While such a showing may be interesting, it does *not* provide conclusive evidence that the CVM has captured a real economic commitment by subjects.

Setting aside our inability to unequivocally link results from comparative studies to demonstrations of real economic commitments, the similarity between value estimates derived with the CVM and indirect methods *for some environmental goods* is remarkable. For example, Cummings, Schulze, Gerking and Brookshire [1986] estimate values associated with the provision of municipal infrastructure in boomtowns using the HPM and the CVM. They fail to reject the hypothesis that mean values for the elasticity of substitution between wages and municipal infrastructure calculated from the two methods are the same. Smith, Desvousges and Fisher [1986; p. 289] find a close correspondence between values for water quality derived with the CVM and a "simple" travel cost model. In 13 of 15 comparison studies considered by Cummings, Brookshire and Schulze [1986; pp. 100, 101], CVM values were within +/-50% of values derived with indirect methods.

¹⁰⁷ Mitchell and Carson [1989; p. 205] note the "closeness" of CVM values and TCM or HPM values in some studies, but correctly argue that "...it must be noted that the benefits measured by the two types of methods [CVM and TCM/HPM] are not strictly comparable. Travel cost analysis, for example, is an ex-post welfare measure which tends to exclude existence values, whereas contingent valuation is an ex-ante measure and includes these values...hedonic price studies...make the questionable assumption that all of the benefits of air quality are capitalized into residential housing prices."

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Of course, in a number of cases CVM values are *not* found to be "close" to values derived with indirect methods. This observation leads Cummings, Brookshire and Schulze [1986; Chapter 6] to pose the question: are there common characteristics among goods for which CVM and indirect value estimates are "close" and those for which CVM and indirect values are not close? They loosely suggest that such might be the case. Drawing on the paradigm of a perfectly competitive market (pp. 102-104), they define as "Reference Operating Conditions" (ROCs) a subject's familiarity with a commodity, their valuation/choice experience with a commodity, little uncertainty, and the use of a WTP measure (p. 105). They then suggest that a parallel exists between goods that satisfy these ROCs and those for which CVM values were found to be "close" (within +/-50%) to values derived with indirect methods.

Unfortunately, Cummings, Brookshire and Schulze's [1986] view of the implications of this parallel obfuscates the issue of the accuracy or real economic commitment of CVM values. Values from an estimation study wherein ROCs are in some (undefined) sense satisfied would seemingly be accepted as "true" or "accurate" by them.¹⁰⁸ They appear to assume that indirect methods may be taken, on their face, as having satisfied the ROCs. Thus, they appear to view closeness of CVM values to values derived with indirect methods to imply closeness of CVM values to "true" values reflecting a real economic commitment. We have noted above the absence of any basis for this line of deduction, a "lack" noted by others.¹⁰⁹ The "reference accuracy" discussions of Cummings, Brookshire and Schulze [1986] may be viewed as useful in terms of a call for the profession's attention being focused on means by which the accuracy and reliability of CVM measures might be assessed. A great deal more attention is required, however, as to the nature of possible "conditions" required for any criterion for reference accuracy.¹¹⁰

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¹⁰⁸ This would certainly appear to be the case in their discussions of the ROCs in Chapter 6. However, in Chapter 13 (pp. 230-231), the authors acknowledge the infant stage of our knowledge of criteria for accuracy in CVM measures, discuss other possible ROCs, and call for "...imaginative thinking and research relevant to the specification of precise and defensible ROCs." (p. 230)

¹⁰⁹ "...there are no grounds for considering travel cost- or hedonic price-based measures to be more accurate than CV-based measures." Mitchell and Carson [1989, p. 204]. See also V.K. Smith's [1986] critique and Mitchell and Carson [1988, pp. 188-90].

¹¹⁰ Mitchell and Carson [1988], and Carson, Hanemann and Mitchell [1986], among others, argue that a political market model may be preferable to a consumer market model for the valuation of public goods. This preference is justified on the grounds that people value public programs and amenities in referenda. Mitchell and Carson [1988; p.189] argue that "Acceptance of a referendum model would imply quite different ROCs; for example voters often make binding choices about amenities with which they have relatively slight familiarity." As discussed above the key descriptor here is "binding," which (continued...)

Thus, for the purpose of our inquiry regarding the real economic commitment of CVM values, results from comparative studies offer little in the way of substantive evidence one way or another.

C.2 CVM Values and Real Economic Commitments

We now focus attention on the extent to which CVM values *have been shown* to approximate *actual* amounts that people are willing to pay for a good. We must note that "hard" evidence relevant for this question as it applies to environmental goods is very limited. This limitation reflects the fact that received valuation methods cannot be generally applied to non-deliverable public goods (e.g., we cannot actually auction a hypothesized improvement in air quality). This limitation raises an obvious question: are results from experiments comparing CVM and "real" values for private goods and public-like goods relevant for assessments of CVM values attributable to such things as environmental goods?

The importance of a subject's familiarity with a good for assertions that CVM values are at least "reasonable" is emphasized in most discussions of the CVM (see, e.g., Cummings, Brookshire and Schulze [1986; ch. 6]). Mitchell and Carson [1989; p. 188] argue that "The key problem facing the designer of a CV study, we say, is the novelty of valuing a public good, given the respondents' varying degrees of familiarity with the good being valued and how they currently pay for its provision." *If* one accepts this argument, and setting aside any other problems with the CVM, one might then argue that the hallmark of a "good" CVM design is making the subject as familiar as possible with the good being valued. A "perfect" design would then be one where the subject is demonstrably familiar with the good being valued. Common, private goods surely meet this criterion. If the CVM value for such goods (strawberries or a painting) does not closely approximate real values that people will actually pay for these goods, we may ask: how can one expect a *better* predictive performance of the CVM when the good being valued is unfamiliar? Thus, *for the limited purposes of this section's inquiry*, results from private goods.

One study which addresses the question of interest here is reported by Dickie, Fisher and

¹¹⁰(...continued)

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implies that individuals view the referendum as real. The weight of this argument when applied to a hypothetical referendum where the good to be provided and payment for the good is hypothetical is debatable.

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Gerking [1987].¹¹¹ They obtain values for a pint of strawberries. They use the CVM, and they actually sell the strawberries to households. At a 1% significance level, they fail to reject the null hypothesis of structurally identical demand equations estimated from actual sales and CVM data (p. 75).¹¹²

A number of other studies involving "familiar" goods find significant differences between CVM values and market values, however. Bishop and Heberlein [1979; pp. 928-929] find significant differences between CVM estimates for subjects' WTA for goose permits and WTA values based upon actual cash payments.¹¹³ Bishop and Heberlein [1986] obtain CVM and actual cash values for deer permits. They found (pp. 130-131) that WTP values were significantly overstated in the CVM relative to the cash market. Referring to the "familiarity" requirement for real economic commitment in CVM values conjectured by Cummings, Brookshire and Schulze [1986], Bishop and Heberlein [1986; p. 134] note that "Clearly, if contingent valuation is capable of giving unbiased estimates of real values, it should have done so here." Their results, however, "... indicate bias. People were more willing to sell their goose hunting permits for real dollars than they indicated they would be in the contingent market. Preliminary results from the deer study indicate that in an auction framework, CVM will overestimate willingness to pay (...) money is a powerful stimulus and real money is more powerful than hypothetical money" (Bishop and Heberlein [1986; p. 134]).

Evidence concerning the real economic commitment of CVM values for private goods is offered by Seller, Stoll and Chavas [1985]. They use the CVM to obtain WTP values for improvements in boat docking facilities in four lakes in Eastern Texas. Values are derived with a Travel Cost model, with an open-ended CVM study, and with a dichotomous choice CVM study. Two results from this study are of interest for our purposes, although the

¹¹¹ A companion study which focuses on actual sales of strawberries and values for strawberries obtained in a Vickrey auction is Brookshire, Coursey and Schulze [1987].

¹¹² While Dickie, Fisher and Gerking [1987] find no significant difference in the distribution of values from actual sales and the CVM, their price estimation equation based on CVM data performed poorly in predicting quantities demanded at given prices. This finding leads to their conclusion that CVM data "...may be best utilized in aggregate form." (p. 75)

¹¹³ Mitchell and Carson [1989; p. 195-199] dispute these conclusions. Bishop and Heberlein [1986; p. 127-8] respond by questioning the *ex post* data manipulations and imputations employed in their critique. Hanemann [1984] also re-evaluates the original studies conclusions, demonstrating the extreme sensitivity of results to alternative statistical assumptions.

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evidence presented is very weak.¹¹⁴ First, both CVM applications yielded a demand relationship with a positive slope for one of the lakes (Lake Somerville). The explanation offered for this anomaly bears on the "familiarity" issue discussed above: "It appears that specifying a contingent market under conditions where boaters were not used to paying a launch fee may have caused problems (...) This seems to add to the evidence that the contingent valuation instruments used to collect data for analysis must be designed so that behavior by the respondent is as familiar as possible" (Seller, Stoll and Chavas [1985; pp. 169, 174]). Secondly, CVM values from their open format questionnaire resulted in *negative* surplus measures for two of the lakes (Lake Conroe and Lake Houston). These results, along with similar results from a third lake (Lake Livingston),¹¹⁵ are interpreted by the authors in the following way: they "... seem to indicate that people reported they were willing to pay less for an annual ramp permit than they already paid in total launch fees over the year on a per visit basis" (Seller, Stoll and Chavas [1985; p. 169]).

Setting aside the questions raised above in *Measuring Distinct Nonuse Values With The CVM* as to what was actually measured in their experiment, Boyce *et al.* [1989] use an incentivecompatible experimental institution¹¹⁶ in a study designed to elicit subjects to reveal their WTA and WTP for a Norfolk pine, as discussed earlier. They also elicited CVM values from the same subjects. Given the data they report on sample means and medians, one can easily gauge the relationship between hypothetical CVM values and values that represented a real economic commitment. Using means from their data, hypothetical CVM values over-state real economic commitments by 27%, 117%, 85% and 150% in the WTP-NoKill, WTP-Kill, WTA-NoKill, and WTA-Kill experiments, respectively. Using median values, which is arguably more

¹¹⁴ In a later study Sellar, Chavas and Stoll [1986] demonstrate that the anomalous results obtained from data concerning Lake Conroe, discussed below, are attributable to a mis-specification of the estimation model used for analyses reported in the present [1985] study. The implications of such model mis-specification for results related to value estimates for the remaining three lakes are unclear, but suggests the need to view all of the results in the [1985] study with healthy skepticism.

¹¹⁵ Surplus was not calculated for the fourth lake, Lake Somerville, because "... the demand curve was not downward sloping, lay in the fourth quadrant and was considered unreliable." (p. 171).

¹¹⁶ Caution must be used in interpreting these results given that the "buyout distribution" of Boyce *et al.*'s [1989] [1991] prices are skewed. This then may give rise to a "payoff dominance" problem which can effect the incentive properties of their valuation institution. For a discussion of the payoff dominance problem see Harrison [1989] [1990a] [1990b] [1992b]. We note the ongoing controversy as to the significance of payoff dominance. Critics include D. Friedman [1992] and Cox, Smith and Walker [1992]. See Drago and Heywood [1989], Hey [1991], Kagel and Roth [1992], Roth [1988] and Merlo and Schotter [1992] for arguments that support the payoff dominance issue. Harrison [1992b] reviews the debate.

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reliable given the likely skewedness of the data, CVM values over-stated real economic commitments by much larger percentages: 82%, 150%, 150% and 400%, respectively. These data suggest that values elicited with the CVM can be highly inflated with respect to the real economic commitment that subjects would actually make for a good.

Neill [1991] directly addresses the question as to the extent to which subjects will actually pay amounts that they say that they will pay in the CVM. She compares the valuations elicited for an art object using a CVM and a Vickrey auction in which the subjects were required to actually pay for the object out of their own pockets.¹¹⁷ The art object was a small, framed painting of a Southwest rural scene by an unknown Navajo Indian painter. Sixteen subjects were given a series of "training experiments" in the use of a Vickrey auction with induced values.¹¹⁸ They then participated in a Vickrey auction for the art object. Forty-one subjects participated in a CVM experiment in which maximum WTP for the art object was obtained. Average valuations were \$37.04 and \$9.49 for the CVM and the Vickrey auction, respectively, with medians of \$30 and \$6. Neill [1991] tests and rejects the hypothesis of no significant difference between the distribution of bids obtained from the two experiments.¹¹⁹

Six of Neill's [1991] subjects participated in both the CVM and the auction, but were *not* included in the auction data analyses of which were described above. Their behavior provides a particularly stringent test of the extent to which the CVM constitutes a real economic commitment. Neill [1991] elected not to use the same subjects in the CVM and the auction since there was a chance that this could cause biases in favor of the CVM as subjects may have felt obliged to actually pay the amount they hypothetically recorded so as to avoid embarrassment. A possibility exists for the opposite bias, of course, if subjects are angered by being asked to actually pay for something that they thought involved an honest and

¹¹⁷ This qualification is unusual in most experiments, since it is common practice in these settings to endow subjects with some cash with which to bid. The effect of such endowments has not been examined in a controlled manner. To allow for subjects that might be cash-constrained during the experimental sessions, an interest-free loan for the weekend was arranged. The experiment was conducted on a Friday afternoon and subjects knew that the loan had to be repaid by 5:00 pm the following Monday. A loan contract was signed between the eventual winner and an impecunious research assistant of the experimenter (who had been privately given a large pot of cash by the experimenter for this purpose). The loan was repaid as agreed.

¹¹⁸ Actually there were 22 subjects in this stage of the experiment, but 6 are ignored for reasons explained below.

¹¹⁹ The CVM did not mimic the rules of provision of the Vickrey auction. Instead, it followed the traditional CVM in not specifying explicitly what the rules of provision of the public good would be. This weakens the conclusions that can be drawn from Neill's [1991] experiments, since it is *conceivable* that just altering the (hypothetical) rules of provision in the CVM to match those used in the (non-hypothetical) Vickrey auction could alter elicited WTP in the CVM.

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hypothetical survey. This is a "trick" used by many salesman: asking a subject how much they would be willing to pay for a good, and then confronting them with a (smaller) actual price. These six subjects volunteered to participate in the auction after being privately told that it involved them bidding for the art object out of their pocket.¹²⁰

Neill [1991] finds the six in-sample responses to be consistent with the out-of-sample responses analyzed above. The subjects reported CVM valuations of \$65, \$7, \$25, \$25, \$100 and \$5, for an average of \$37.83; their corresponding auction values were \$18, \$6.27, \$2.50, \$12, \$8 and \$5, for an average of only \$8.63. Using a matched pairs Wilcoxon test, she rejects the null hypothesis that these values are the same at a 4% critical level, even though the sample is quite tiny.

Turning next to experiments involving goods that are public in nature, Kealy, Montgomery and Dovidio [1990] examine the predictive validity of CVM values for actual cash payment for a private good (a candy bar) and a public good (a deacidification program for lakes in the Adirondack region). One group of subjects is asked their WTP for one of these goods with the understanding that they are to actually pay their offered amount in two weeks. A second group is asked for a WTP, but within a hypothetical (CVM) context. Two weeks later, the second group of subjects were asked for an *actual* payment. The authors pose the question: when asked for a WTP for a good, does the subject's knowledge that he must actually pay a reported amount affect the amount that is reported? They test two hypotheses. The first is the hypothesis that preferences are the same among the two groups of subjects. They reject this hypothesis, a result which they interpret (p. 257) as suggesting that "...in the hypothetical situation preferences are less well formulated because subjects have less incentives to seriously contemplate their actual willingness to pay." The second hypothesis relates to the extent to which WTP amounts initially reported by each of the two groups predict behavior at the end of two weeks. They find that, for both goods, initial verbal reports were reasonably

¹²⁰ They had participated in a bargaining experiment held just prior to the Vickrey auction (during which their CVM values were elicited), and asked if they could participate in the next session. They were told about the experiment before any of the other 16 subjects for the auction had entered the room.

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correlated with actual payment.¹²¹ They also examine the effect of foreknowledge that subjects will be required to actually pay their stated WTP on the degree of correlation between hypothetical WTP and actual payment behavior. They conclude (pp. 259-260) that "... individuals are more likely to overstate than to understate their [hypothetical] WTP when they are not expecting to have to make an actual payment (...) foreknowledge of an obligation to pay in accordance with one's verbal statements of willingness to pay has a positive impact on the predictive validity of contingent values...."

Another experiment with a public-like good is reported by Seip and Strand [1989]. A sample of 101 Norwegians were asked in personal interviews their WTP 200 Norwegian kroner for membership in the Norwegian Association for the Protection of Nature (Norges Naturvernforbund, NNV), which is the largest and best established private environmental organization in Norway. Sixty-four of the 101 subjects responded "yes," indicating that they would hypothetically pay the stated amount. A short time later, the 64 subjects that answered yes in the CVM study were sent letters encouraging them to join the NNV at a membership cost of 200 kroner. There was no reference in these letters to the earlier CVM study. One month later, a second mailing was sent to subjects that had not joined the NVA as a result of the first letter. Again, reference was not made to the initial CVM study. At the end of the second mailing, only *six* of the 64 "yes" respondents in the CVM had actually paid the 200 kroner to join the NVA.

A sample of 25 of the 58 "yes" respondents in the CVM study who had not responded to invitations to do what they had said that they would do were interviewed by telephone. Emphasizing the scientific nature of their study, the interviewers reminded the subjects of their "yes" response to the CVM questionnaire and the two mailings which offered them the opportunity to do what they had said that they would do. The subjects were asked for reasons underlying their lack of response to invitations to join the NNV. Twenty-four of the 25 subjects indicated that their WTP expressed in the CVM study was an expression of their WTP for environmental goods in general, not their willingness to pay for the NNV in particular! When asked if they would like to change the WTP offered by them in the original CVM study,

¹²¹ They offer the following caveat, however. "We caution the reader that there is a strong possibility that our comparisons of first-period report with second-period behavior could be overly favorable to the predictive validity of the CVM. Subjects may be reluctant to contradict their previous response even if it overstates their true WTP." (p. 258)

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17 of the 25 subjects indicated that they would *lower* their response. Seip and Strand [1989; p. 3] conclude that the results are "... discouraging, by indicating that the CVM can imply quite serious biases of overvaluation, in particular when (like here) the good to be valued is rather abstract and it is difficult to attach concrete environmental values to it."

Duffield and Patterson [1992] use mail surveys to obtain three sets of values for a fund to be established for the purpose of leasing water rights to be used for the preservation of in-stream flows in a set of Montana rivers.¹²² One set of subjects (Cash-TNC) are asked to actually make a tax deductible contribution to an *actual* fund, the "Montana Water Leasing Trust Fund," that had been established by the Montana Nature Conservancy. A second group (Hypo-TNC) was asked a hypothetical question: if contacted in the next month with a request to make a tax deductible contribution to the Montana Water Leasing Trust Fund, how much would they be willing to contribute? For the third group (Hypo-UM), both the fund and payment was hypothetical. They were told that a fund *could be* established. They were then asked the maximum amount that they would contribute to the fund if contacted in the next month with a request for a contribution.¹²³ Comparisons of results from Hypo-UM with those from Cash-TNC and Hypo-TNC are made difficult by differences in the design and follow-up procedures used. Unlike procedures used in the Cash-TNC and Hypo-TNC experiments, subjects in Hypo-UM did not receive a brochure describing the trust fund, and extensive follow-up mailings were used in the Hypo-UM experiment. There were no follow-up mailings in the Cash-TNC and Hypo-TNC experiments. We therefore only compare results from the two similar experiments: Cash-TNC and Hypo-TNC.

Duffield and Patterson's [1992; Tables 2, 3 and 4] results for residents and nonresidents¹²⁴ are summarized below in Table 3. Column 2 is the number of delivered questionnaires.¹²⁵

¹²⁵ Excluding questionnaires returned due to change of address or incorrect address.

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¹²² We gratefully acknowledge permission by John Duffield to use these data.

¹²³ Duffield (personal communication) points out a potential weakness in this question form: some subjects *may* have interpreted the question as implying that they would be contacted in a month for a requested WTP. Such interpretations could affect, among other things, the response rate.

¹²⁴ Response rates and reported WTP were consistently higher for non-residents than for residents.

	Number Delivered	Number Returned	Amount of Contribution					
Experiment			\$0	\$10	\$25	\$50	\$100	\$250
Cash-TNC	4650	511	349	70	59	24	8	1
Cash-TNC %	100	11	68	14	12	5	2	0.2
Hypo-TNC	2067	481	264	106	67	31	13	2
Hypo-TNC %	100	23	55	22	14	6	3	0.4

Table 3-1. Results from the Duffield and Patterson [1992] experiment

Column 3 is the number of questionnaires that were returned to experimenters. Columns 4-9 are numbers of respondents reporting WTP between zero and \$250.

A question of immediate importance for our inquiry as to differences between hypothetical values and values that represent a real economic commitment arises from the large differences in response rates: is the response rate of 11% for the questionnaire requesting real payment, which is smaller by half than for the questionnaire requesting hypothetical payment, simply a random occurrence or does it imply a nonresponse bias? It is difficult to ignore the potential implications of the differences in response rates between these experiments involving real and hypothetical payments, differences seemingly attributed to nonresponse bias and/or free-riding behavior by Duffield and Patterson [1992; p. 26]. If one ignores these differences the estimate of the sample average WTP is based on the percentage of returned questionnaires represented by subjects that bid \$10, \$25,\$50, \$100 or \$250, yielding an average household value¹²⁶ of \$9.40 for actual cash contributions and \$12.70 for hypothetical contributions. The CVM overestimates real economic commitments by some 35%. If, instead, one takes nonresponse as being indicative of a zero value, guite different estimates of population WTP are obtained: \$0.98 if estimates are based on real economic commitments, and \$2.97 if estimates are based on CVM results. In this case, the CVM overestimates real economic commitments by 203%. Admittedly, this is only one example of an alternative treatment of nonrespondents, but it does illustrate the possible bias of the CVM.

In any case, Duffield and Patterson's [1992] results suggest an overestimate of real economic commitments from values obtained with the CVM. The extent of this overestimation in their study depends critically on how one interprets and then deals with the nonresponse question.

¹²⁶ The sum of contributions weighted by the percent of "households" (returned questionnaires) offering the contribution.

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D. CONCLUDING REMARKS

Do CVM values reflect a real economic commitment on the part of subjects? This question is central to concerns for the accuracy, reliability, or economic meaningfulness of values derived with the CVM. Our response involved a search of the literature for evidence that would support one or both of the following propositions. First, there exists a strong theoretical foundation for the CVM which implies behavior in CVM studies that parallels behavior in real markets. Secondly, people will actually pay amounts reported in CVM surveys.

We have argued that there exists no theory in economics that might serve as a source for operationally meaningful hypotheses related to individual behavior under *hypothetical* circumstances. Virtually all references to economic theory found in the literature are based on a common assumption: that our theories regarding individual behavior in *real* market settings wherein goods are actually exchanged and actual payments are made can in some way be "transferred" to the valuation setting of the CVM.

We then inquired as to the existence of empirical evidence that might make such a transfer intellectually palatable. In other words, we ask: does substantial evidence exist that would support the claim that subject behavior within the CVM valuation institution is reasonably similar to behavior assumed in economic theory? We first looked to the extent to which the CVM valuation institution provides market-like incentives for "good choices," or for truthful reporting of values. We argued that appeal to empirical results from experimental economics as providing evidence that CVM subjects will not generally free-ride involves critical assumptions that are arguable, at best, and at worst, unfounded. The valuation institutions used in experimental economics are patently dissimilar to those used in the CVM in important ways. Moreover, there is no evidence that unequivocally establishes that subject behavior in the CVM is similar to subject behavior in the institutions used in experimental economics. Nor does there exist unequivocal evidence concerning the pervasiveness of free-riding in the CVM valuation institution institutions used in experimental economics. There simply exists no basis for monolithic statements regarding free-riding behavior in the CVM one way or another.

Continuing our search for evidence related to the theoretical foundation underlying the CVM, we also probed the extent to which CVM subjects might make valuation decisions drawn from

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a well-defined choice set in a manner that parallels that assumed in economic theory. Based on our review of the few studies that have examined this issue, at best the evidence is inconclusive. CVM values have been shown to be reasonably stable over time. Their "predictive validity," the extent to which CVM values are reasonable predictors of amounts that people will actually pay, is shown to be questionable however. Significant variations in CVM values themselves *can* result from changes in the amount of time that subjects are given to "think about" their valuations, in the types of information given to them, and in the context in which substitute goods are presented to them.

One may wish to argue that changes in CVM values which attend changes of the type discussed above are not random and are predictable. Thus, the expansion of choice sets via the introduction of substitutes *should* result in lower values, as they typically do. But this simply says that if one "tweaks" the information setting provided by the CVM questionnaire in certain respects, values may move in predictable ways. Given the present state of the art, however, this argument cannot be taken as implying "strength" for our understanding of the theoretical foundation of the CVM. We have no theory by which we might systematically define correct or incorrect "tweaks." This is to say that, given the *possible* sensitivity of CVM values to information, we do not know the "right" kinds or amounts of information that should be given to subjects. Given the possible sensitivity of CVM values to substitutes, we do not know the "right" context within which substitutes should be presented to subjects, the "right" *kinds* of substitutes that should be presented, or the "right" number of substitute goods that should be presented.

In terms of the randomness or predictability of CVM values, there is another dimension of this issue which we have not touched upon in our discussions of the theoretical foundation of CVM values but which should be mentioned. Our theory of value would lead us to expect that CVM values, if they reflect true values, would vary in predictable ways with changes in specific individual or household characteristics. In general, *these* kinds of predictions *are* observed in CVM studies. For example, CVM values are typically positively related to a subject's income. Such observations may then be taken to suggest that CVM values are not simply "noise" or that they are not random. They *are* interesting and they *are* suggestive. They do not, however, provide the basis for arguing that the CVM enjoys a strong theoretical foundation,

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just that the CVM *can* generate values that are consistent with *some* of the most rudimentary predictions of economic theory. Contrast this type of "test" of the consistency of CVM responses with economic theory and the type of test employed to econometrically test the fundamental restrictions of demand theory (e.g., Deaton and Muellbauer [1980; ch. 3]), and it is apparent that these "tests" of the CVM are partial and uninformative at a fundamental level.

We must conclude that it has *not* been demonstrated that the CVM enjoys a strong theoretical foundation. Claims to the contrary, and there are many, are based on the *assumption* that economic theories related to individual behavior in contexts involving real transactions with real payments are relevant for the hypothetical context of the CVM. We do not find empirical evidence that might suggest that this assumption is credible.

The second proposition that we examine involves the question: will people actually pay amounts reported in CVM surveys? Most empirical studies that compare CVM values with values obtained from valuation institutions in which truth-telling is known to be a dominant strategy for subjects involve private goods. The relevance of such studies for an assessment of our second proposition is straightforward. If CVM values for familiar, private goods are not close approximations for values which subjects will actually pay, there are good reasons for expecting that CVM values for less familiar public goods will not closely approximate real values that subjects will pay. The little evidence that exists regarding this issue is inconclusive. Results from available studies suggest that the distribution of CVM values for strawberries closely approximates the distribution of values that people actually pay for strawberries. CVM values overstate actual amounts that subjects will pay for a Southwest painting by a factor of five. Only 10% of subjects making CVM commitments for payments to support an environmental fund in Norway were actually willing to pay the amount given by them in the CVM. CVM estimates for values that subjects will contribute to a fund to maintain in-stream flows in Montana overestimates actual contributions by anywhere between 35% and 200%, depending on how nonresponse data are interpreted.

These few studies concerning the relationship between CVM values and actual payments do not, of course, constitute a preponderance of evidence which might support a conventional wisdom as to whether or not CVM values closely approximate amounts that people will actually pay for a good. There then exists no basis for a monolithic conclusion in this regard.

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Section 4 CONCLUSIONS

Over the last twenty years or so, there have been many published and unpublished papers written on the topic of nonuse values and the use of the CVM as a means for measuring them. During the bulk of this period, the CVM was generally accepted for what is was: an experimental method that might have promise as a means for valuing public goods. The late 1970s and early 1980s were marked by a period of intellectual excitement during which innovative and imaginative individuals employed innovative and imaginative methods in efforts to advance the state of the art for applying the CVM to the task of benefits measurement.

In the absence of a theory from which operationally meaningful hypotheses might be derived concerning individual behavior within the hypothetical valuation institution that is fundamental to the CVM, CVM researchers had only one recourse: to "transfer" the deductive logic that applies to consumer behavior in real market contexts to the CVM valuation institution. A parallel assumption is seen in more recent efforts to frame the CVM as a referendum process: *assume* that individuals will view the CVM process as *real* rather than hypothetical.¹²⁷

The social and intellectual stakes were relatively low during this period, however. Expectations for the accuracy of CVM value estimates, in terms of the extent to which CVM values approximated "true" values, were extraordinarily modest. For the intended purposes of value estimates derived with the CVM,¹²⁸ evidence suggesting that CVM values might approximate "true" values within an order of magnitude could be greeted with enthusiasm. Thus, there were *some purposes* for which an estimation method that estimated "true" damages of \$0.5 billion at \$0.1 billion or \$2 billion might be useful. An obvious problem continued to nag at CVM researchers, however: they had no means for determining "true" values with which CVM values might be compared for the purpose of assessing "accuracy" as defined above.

¹²⁷ Hoehn and Randall [1987] make clear their "key assumption" (p. 229, fn. 3) that "...the respondent believes that the benefit information generated by CVM will have some influence on the eventual policy decision." (pp. 228-229).

¹²⁸ These were primarily benefit-cost analyses of public projects or proposed public policies.

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Theory aside, *accuracy* aside, "methods development" experiments with the CVM could be justified simply on the basis of one's identification of an intellectually interesting issue. Bishop and Heberlein [1986] describe the character of research during this period in the following way:

"Past research [with the CVM] has not been as conducive to real methodological progress as it might have been for two reasons. First, it was probably necessary for CVM to go through a prescientific stage. Most of the history of CVM brings to mind children with a chemistry set pouring chemicals at random into a test tube to see what will happen. (Perhaps the most recent installment is to 'stir in' a Vickrey auction) Second, there has been very little truly basic research on CVM. Most of the research has had to justify its existence by claiming to address real world problems. Methodological research had to be done as a add-on to these applied studies. It is little wonder that after 20 years, we are still debating such basic issues as whether iterative bidding improves accuracy." (pp. 146-147)

Bishop and Heberlein [1986] go on to conclude:

"A great deal has been learned about CVM, but so much is unknown even now. We do know that CVM is the most promising technique for applying an economic yardstick to many of the nation's seemingly most valuable environmental and resource commodities. Enough positive evidence has accumulated to warrant a major investment in full development of the contingent valuation method." (p. 147)

Has the CVM moved beyond the situation described by Bishop and Heberlein [1986]? Has our understanding of the substance of nonuse values and CVM values advanced beyond the methods development stage? A response to these questions is of particular relevance given contemporary conditions wherein nonuse values and the CVM have been pressed into policy applications which *presuppose* that the relevant state of the art has advanced substantively beyond the pre-scientific stage. We refer here to the use of nonuse values as measured by the CVM in CERCLA and OPA litigation, *not* as measures indicative of potential orders of magnitude values but as compensatory damages which individuals *are to actually pay*. Uses of the CVM for *these* purposes implies that it provides an "economic yardstick" that has well-calibrated inches and fractions of inches. Can the CVM "yardstick" be used in this manner?

In requiring of the CVM a well-calibrated yardstick, are our requirements too stringent? Are *any* economic measures commonly used in the private and public sector so well calibrated? What of market prices? Pratt, Wise and Zeckhauser [1979] examine the following questions.

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Is there an equilibrium distribution of sellers' prices? At equilibrium, may sellers with identical characteristics charge different prices? They examine prices quoted for thirty-nine standardized products by businesses in the Boston metropolitan area. The number of businesses surveyed for each product varies from four to twenty-two (pp. 206-207). Market prices for these standardized products are shown to vary substantially. The ratio of maximum to minimum observed prices ranges from 111% for a Raleigh Grand Prix 10-speed bicycle or a 500 gallon container of fuel oil to 667% for a Dennman styling brush. Notwithstanding the possibility that non-negligible search costs may account for some part of these deviations, one plausible hypothesis is that buyers are less than fully informed about the distribution of sellers' prices. Reminiscent of the "familiarity" issue of concern to CVM researchers, Pratt, Wise and Zeckhauser [1979; p. 211, fn. 16] find that the standard deviation of prices is less (about one-third) for frequently purchased goods than for infrequently purchased goods, indicating that "Less frequent purchase also implies that buyers will know less about the distribution of sellers prices."

Moreover, differences between stated intentions and actual actions are not unique to the CVM. Marketing research for common, generic goods faces the same problems. For example, Kalwani and Silk [1982] find significant correlation between stated purchase intentions and actual purchase behavior, but show that absolute correlations may be as low as 50%.¹²⁹ The role of, and difficulty of measuring, *intensity* of intentions for predicting behavior is stressed in this literature. This point is also stressed by Schuman and Presser [1981; p. 271], and underlies Schulze's [1992] arguments concerning "crystallized" values.¹³⁰

These considerations are clearly relevant for our intellectual inquiry as to the accuracy of CVM values and possible limits for expectations in these regards. They may obfuscate, however,

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¹²⁹ See, also, Morrison [1979]

¹³⁰ Schuman and Presser [1981] emphasize the importance of crystallized *attitudes* for reliable attitudes, which are attitudes that are stable over time. "Values" as used by them appear to us to refer to subjects preferences and tastes, as opposed to their cognizance of some amount that they would pay for some good. Our reading of Schulze [1992] suggests that he is referring to crystallized values in this latter sense. Thus, he suggests that crystallized values have direct implications for the need for concern with contextual issues related to survey design. We do not grasp the logic for the extension of crystallized attitudes to specific monetary values held by a subject for a good that are somehow predetermined and fixed.

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the central issue that we have in mind with our "yardstick" analogy. If we wish to deduce "the" value of a good based on observed market prices, in the light of varying prices, we may argue about the appropriate summary statistics. But there is no question as to the interpretation of the measures with which we are dealing. We can learn if people are paying these prices. The nexus of such prices to real economic commitments is not in question. This is not the case with CVM values. Our concern with the relationship between CVM values and real economic commitments is not arguing that CVM subjects should have the same value. Our theory tells us the opposite: we would *expect* different "prices," or different values from different people. The argument is rather that we cannot establish what the CVM is measuring. The variation of concern is that between the CVM measure and what one supposes that it is measuring: a real economic commitment. This is to say that the units on our "yardstick" are in deviations of CVM values from actual values held by subjects.

In summary, we have examined the literature concerning nonuse values and the CVM and find a great deal in the way of assertions of fact and conventional wisdom that suggests that substantial progress has been achieved in the development of strong theoretical foundations for nonuse values and the CVM, and in advancing the CVM as a reasonably accurate yardstick for measuring economic values.¹³¹ We have examined these assertions and the evidence claimed to support them in considerable detail. We now review the conclusions that we draw from our analyses.

A. A THEORY OF NONUSE VALUE

Several authors claim to have described an economic theory wherein utility depends on nonuse values. With that theory, we have no quarrels. However, we find no basis for imputing specific motives to an elicited WTP. We cannot pretend to "know" why individuals offer a value for a good, since we cannot observe motives. We then know of no way by which efforts to decompose nonuse values into components might be justified. Thus, claims that "existence values" exist, have been measured and are "large" relative to total values, are, in our view, without foundation.

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¹³¹ For example, Carson and Mitchell[1992; p. 1, emphasis added] observe: "Any nascent methodology requires a certain amount of break-in time in which its basic operational principles are developed. *That period has passed* for contingent valuation..."

A conceptual basis for decomposing total values into use and nonuse components is clearly offered by McConnell [1983] and Freeman [1992]. We concur with their assessments of the difficulties that will attend any effort to make these decompositions operational. It is telling that *none* of the studies that claim to measure nonuse values as a component of total value employ these formal decompositions. Until such frameworks can be made operational, the best that can be done at this point in time is to obtain total values from users and nonusers. Reference to values of nonusers as a nonuse value is a matter of expositional choice.

B. MEASURING NONUSE VALUES WITH THE CVM: THE "STRONG" THEORETICAL FOUNDATION FOR THE CVM

We find no evidence that would support an unequivocal claim that the CVM enjoys a "strong" theoretical foundation. We find no basis for the conclusion that CVM subjects have, or do not have, incentives for truthfully reporting their preferences, or that they will not behave strategically. One cannot appeal for "evidence" supporting such a conclusion to results from empirical studies in experimental economics. The valuation institution used in experimental economics differs from that used in the CVM in critical ways. Existing studies do not unequivocally establish that behavior in the CVM is "like" behavior in valuation institutions used in experimental economics or that strategic behavior is not prevalent in CVM valuation institution itself. While *some* studies conducted in experimental economics report minimal strategic behavior, an equal number report pervasive instances of strategic behavior.

Incentives for truth-telling in referendums noted in the political markets literature cannot be uncritically "transferred" to the institutional setting of the CVM. Such incentives have *not* been shown to exist in hypothetical political "markets."

Empirical studies that focus on the consistency of subject behavior in the CVM with theoretically assumed behavior based upon well-defined choice set are few in number and at best inconclusive. While there exists no monolithic evidence one way or another, it would appear that typical applications of the CVM *may* not allow subjects the time required for them to fully research their preferences. Moreover, the kinds and amounts of information given subjects *can* significantly affect their reported values, and we do not understand the definition of "appropriate" kinds or amounts of information which they "should" be given. Finally, subjects may not consider, as they are *assumed* to consider, relevant substitutes and complements for goods valued with the CVM. Again, with the present state of the art, we do

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not fully understand the nature of appropriate substitutes or complements to which subjects should be exposed, or the context in which substitutes or complements should be presented to subjects.

C. THE REAL ECONOMIC COMMITMENT IMPLIED BY CVM VALUES

Uses of CVM values which presume any reasonable degree of accuracy must assume that, theoretical "niceties" aside, in the end the CVM will yield value estimates that closely approximate values which reflect a real economic commitment. We must leave to the reader his or her definition as to what "reasonably approximate" might mean: within 10%, 25%, 50%, 100% or 200%? We argue that experiments that compare CVM values with real economic commitments for familiar, private goods are relevant for this particular issue. In homey terms, if the CVM can't "get it right" with familiar private goods then one is hard put to argue that it will do so with unfamiliar, public environmental goods.

We find that CVM values may reasonably approximate amounts that individuals will pay for strawberries. CVM values overestimate amounts that individuals will pay for a painting by some 400%. Only 10% of subjects that *say* that they will pay to join a conservation organization in an application of the CVM do so when given an opportunity. CVM values for amounts that people say they will contribute to a fund to maintain in-stream flows overestimate amount that they will actually pay by 35% or 200%, depending on how non-response data are interpreted. While these few studies cannot justify conclusive evidence regarding the extent to which CVM values might reflect a real economic commitment, we regard them as justifying healthy skepticism as to the extent to which economic value can be attributed to CVM values.

We feel compelled to place our admittedly dismal conclusions concerning the present state of the art for the CVM into perspective. It is our view that conclusions regarding the state of the art of the CVM are "dismal *only* for a context in which one wishes to view it as a method by which one can estimate monetary values that enjoy some reasonably close proximity to real economic commitments. Such uses of the CVM are clearly, in our view, premature. The state of the art of the CVM has simply not advanced to a state wherein such uses are credible. As a vehicle for providing public policy makers with general *insights* regarding order of magnitude comparisons of *potential* social benefits and social costs associated with public decisions, the

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"promise" of the CVM remains.¹³² What the CVM cannot provide with any degree of reliability are "precise" quantitative measures.

We recognize the perception that society needs a means for measuring nonuse values for environmental resources *now*. This perceived need cannot, however, justify putting to use a method which is *demonstrably* inadequate for its intended use. The impetus for skipping over the completion of a *conclusive* experimental development process for the CVM did not, in the main, originate with CVM scholars. It originated from uncritical acceptance of the method by social decisions makers as a "best available" technology for speaking to pressing contemporary problems. Our assessment of the present state of the art of the CVM leads us to what we view as an unavoidable conclusion: for uses that require that the term "value" will imply some nexus with real economic commitments of people, it has yet to be demonstrated that the CVM as currently applied is up to the task.

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Appendix A:

SUGGESTIONS FOR FURTHER STUDY

Our assessment of the CVM results in two general conclusions. First, there may be large disparities between amounts that individuals *say* that they will pay for a public good within the hypothetical context of the CVM and amounts that they will actually pay. Secondly, large disparities in CVM values may result from CVM applications which provide subjects with different contexts relevant for their formation of a well-defined choice set.

The disparity between CVM values and real economic commitments *per se* is not a major problem *if* such disparities *can be predicted*. We do not know if these disparities can be predicted. However, large variations in CVM values associated with alternative valuation contexts would not pose major problems *if* we could define a "correct" context and *if* remaining variations in values could in fact be predicted. In what follows, we set our suggestions as to an approach that might be taken to explore these issues.¹³²

Define V_{ic} as some value measure (mean, median, or some index of a distribution of values) derived with an incentive-compatible valuation mechanism (such as a Vickrey auction) and V_{cv} as a comparable value derived from an application of the CVM. Let $\alpha = V_{cv} - V_{ic}$. Our interest is with the determinants of α . That is, can α , the disparity between a CVM value and a value reflecting a real economic commitment, be explained? We may posit that α systematically varies with a number of arguments:

$$\alpha = f(x_1, x_2, ..., x_n).$$

Our attention is then focused on the questions: what *are* the arguments x_i and, quantitatively, how is α affected by each argument?

To explore these questions, one would proceed along lines where CVM studies are undertaken in parallel with valuation methods that are demonstrably incentive-compatible. The

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¹³² These suggestions are certainly not original. A bias function, and how it might be measured, is discussed in Freeman [1979], for example. Cummings, Brookshire and Schulze [1986; pp. 232-235] call for research that is focused on measuring the magnitude of errors in the CVM. Mitchell and Carson [1989; p. 147] are also concerned with forms of bias estimation.

difference between a CVM value and a value which is known to represent a real economic commitment is the bias that we hope to predict. Initial efforts to explore the bias function would focus on private goods with varying familiarity characteristics. The case for beginning with private goods was made above: if the CVM cannot "get it right" with familiar private goods it is difficult to see how it might do so with unfamiliar public goods. Examples of candidates for incentive-compatible valuation institutions to be used for analyses of private goods include the Vickrey auction, the English auction and the Becker, DeGroot and Marschak [1964] valuation mechanism.¹³³

We emphasize the need to design incentive-compatible valuation institutions in ways that parallel valuation questions posed in the CVM. Conventional designs for laboratory experiments involve subjects being given an endowment of money and being asked the amount of this *increase in income* that they would pay for a particular good. The valuation context for the CVM is quite different, of course. Subjects are not given money. They are asked their maximum WTP for a commodity where "payment" must result from a reallocation of the subject's present pattern of expenditures from their existing income. The experiments of Neill [1991] demonstrate a design for the Vickrey auction that is comparable to the CVM: subjects are not given initial money endowments and bids must be paid "out of pocket" by subjects.

A major methodological challenge must arise in efforts to apply parallel CVM and incentivecompatible studies to robust analyses of the bias function. This is particularly the case for those components of f(.), discussed below, that involve substitute and complementary goods. We are aware of no applications of incentive-compatible valuation institutions developed in experimental economics that involve multiple goods.¹³⁴ This is simply to say that the extension of any method's incentive-compatible properties to applications involving multiple goods will require careful scrutiny.

¹³³ The "appropriate" valuation mechanism to be used for the purpose of eliciting real economic commitments must be selected carefully, of course. One cannot assume that any one particular mechanism yields incentive compatible results in all cases. For example, Kagel, Harstad and Levin [1987] note that experimental subjects were much more likely to actually adopt truth-telling strategies in English auctions than in Vickrey auctions, despite the two institutions having identical theoretical predictions. Harrison [1992c] emphasizes the difficulty of parameterizing the Becker, DeGroot and Marschak [1964] procedure so as to avoid the "payoff dominance" problem.

¹³⁴ We refer here to multiple goods that are heterogeneous. There certainly exist multiple-unit extensions of the Vickrey auction that are incentive compatible *if* the goods are homogeneous. See Forsythe and Isaac [1982] for a discussion of these issues.

The process of identifying potential determinants of differences between CVM values and real economic commitments is basic to our proposed research agenda. While we cannot fully anticipate what these determinants may be shown to include, our review of the literature suggests a point of departure. The contemporary state of the art is one in which the following variables may be hypothesized as having significant influences on biases in the CVM: the nature of the good, in terms of subject's familiarity with its purchase and consumption; the inclusion of substitute and/or complementary goods¹³⁵; information given to subjects; and time given subjects for the formulation of their values.

A particularly important set of variables relate to questionnaire design. As correctly noted by Carson *et al.* [1991; p. 3], "In the course of designing a contingent valuation survey suitable for use in benefit/cost decision-making, the researcher inevitably must make and justify a number of design decisions which often have no obviously correct answers." A fundamental question then arises: on what objective basis can such design decisions be assessed? As we have noted above, there presently exists no set of reasonably specific and widely accepted criteria or standards as to a "good" design. The result is a state of the art in which the "correctness" of any particular design is justified on the basis of the particular researcher's priors or simple assertions of "success."¹³⁶ We think it unlikely that any one set of standards could be developed for assessing "good" designs which would be applicable to any and all goods that might be valued with the CVM. In the end, the proof of a good design must be found in its performance: what are

the effects of alternative designs on the proximity of the resulting CVM values to real economic commitments?¹³⁷

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¹³⁵ We note here the distinction between simple descriptions of substitute or complementary goods and the context wherein subjects are allowed to value or "purchase" these goods.

¹³⁶ Carson *et al.* [1991; p. 10] notes the difficulties in designing a description of a "visibility" good which balances simplicity (that may seriously distort reality) and complexity (which may overwhelm the cognitive capacity of subjects). The justification of the approach that is ultimately used is seemingly based upon an *a priori* belief that "...in spite of the additional complexity of our approach, respondents find it more plausible and therefore easier to understand." (p. 11) Similarly, "... a draft instrument was developed which was then tested and revised repeatedly until threats to its validity *had been identified and satisfactorily addressed.*" (p. 6, emphasis added) At issue here are criteria used for defining a comprehensive search for "threats to validity," the definition of "validity," and the meaning of "satisfactorily addressed."

¹³⁷ If the researcher can obtain values for a good which are demonstrably incentive compatible, why then use the CVM? A response to this question is fairly straightforward: the potential strength of the CVM is that it provides a way to obtain large numbers of observations from geographically dispersed populations at costs which can be reasonably modest compared to those that would be required to sample populations with laboratory techniques.

If some degree of success is realized in explaining differences between CVM and incentivecompatible values for private goods, the next phase of research would move to experiments with the bias function where *deliverable* public goods are valued. This would necessarily be a longer term research program. An important part of this program would be the search for an incentive-compatible valuation institution (for example, a variant of the Pivot Mechanism). An even longer term research program involves research related to the design of experiments that employ incentive-compatible methods to value *nondeliverable* public goods. Our hope is that the demand for incentive-compatible methods applicable to the valuation of nondeliverable public goods will stimulate research that will expand the number of alternative valuation methods available for this purpose. An example of exploratory research related to this end is seen in the efforts of Cummings and Harrison [1991] to validate an "Inference Game" mechanism.

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