Version: May 31, 2007

"Scenario Adjustment" in Stated Preference Research

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Selected Paper prepared for presentation at the American Agricultural Economics Association Annual Meeting, Portland, OR, July 29-August 1, 2007

Acknowledgements: This research has been supported in part by a grant from the National Science Foundation (SES-0551009) to the University of Oregon (PI: Trudy Ann Cameron). It employs original survey data from an earlier project supported by the US Environmental Protection Agency (R829485) and Health Canada (Contract H5431-010041/001/SS). This work has not been formally reviewed by any of the sponsoring agencies. Any remaining errors are our own.

Abstract

To assess demand for non-market goods, researchers must sometimes resort to direct elicitation of consumer tradeoffs with the use of surveys. Stated preference (*SP*) methods typically involve surveys of consumers wherein choice scenarios are posed to respondents and individuals are asked to indicate their preferred alternatives. As *SP* research has matured, much progress has been made to address a variety of well-known biases that can afflict demand estimates produced by these methods, but some concerns still remain. We use an existing survey designed to ascertain willingness to pay for private health-risk reduction programs to illustrate yet another potential source of bias. This bias is caused when not all respondents answer exactly the choice question they are asked and that the researcher intended for respondents to answer. *SP* researchers are familiar with the problem of outright "scenario rejection," where respondents may choose the status quo alternative because they reject the viability of the proposed alternatives. In contrast, we address the more subtle problem of "scenario adjustment," where respondents impute that the substantive alternative(s) in a choice set, in their own particular case, will be different than the survey instrument suggests. We demonstrate a strategy to control and potentially correct for scenario adjustment in the estimation of willingness to pay.

Keywords: value of a statistical life, value of a statistical illness profile, health risk reductions, stated preference, scenario rejection, scenario adjustment

JEL Classifications: Q51

1. Introduction

Researchers have widely considered the conventional problem of "scenario rejection" in stated preference (*SP*) surveys used to value non-market goods. Scenario rejection occurs when a respondent refuses to believe something about the stated choice context, and therefore prefers the status quo alternative (or refuses to make any choice at all). However, few researchers have looked at the more subtle problem of whether respondents subjectively update specific pieces of information provided in a choice scenario to make the context better match their own unique circumstances or beliefs. We define the more-general concept of "scenario adjustment" to apply when respondents do not feel the information in the stated choice scenario is entirely pertinent to them. Although they do not completely reject the scenario, they implicitly revise the information in the choice question to make it conform more closely to their specific experience and history. Using data from a stated preference survey on health risk reduction programs, we find evidence of scenario adjustment and offer a strategy whereby the adjustment can be corrected to allow the choice data to yield potentially more-accurate estimates of willingness to pay (*WTP*).

Adequate market data do not exist to address all types of economic questions. For example, life expectancy and future income expectations explain a number of different types of consumer choices, yet these are fundamentally unobservable variables (e.g. Dominitz and Manski (2004) or Manski (2004)). Preferences for public goods are not always expressed readily in the marketplace, either. Values consumers place on public policies pertaining to health or the environment are sometimes difficult to infer from market choices. To fill these information gaps, it is sometimes necessary to resort to *SP* methods. These methods rely on direct questions—specifically, hypothetical choice scenarios—posed to consumers in household surveys where respondents are given the opportunity to express their preferences across two or more alternative states of the world.

We take advantage of an existing stated preference survey concerning health risk reduction, described in Cameron and DeShazo (2006a). This survey is designed to elicit choices that allow the researcher to infer willingness to pay for private programs that reduce the risk that respondents will experience specific illness profiles. An illness profile consists of a description of the sequence of future health states associated with a major illness that a respondent may face over his or her remaining lifetime. The specific type of scenario adjustment we consider in this paper has to do with each respondent's acceptance of the stated latency of illness (i.e. the time until onset) in each illness profile that are described in the choice sets used in the survey.

Our assessment of the consequences of scenario adjustment, and our potential correction strategy is made possible because, after each stated choice question concerning alternative health-risk reduction programs, respondents are asked debriefing questions.[1] These debriefing questions sort out those who fully believe the information given in the scenario (and thus answer the choice question exactly as it was posed in the survey) from those who subjectively update the scenario information (and thus appear to have answered a somewhat different question). For example, some individuals underestimate the latency period before the benefits to the program would begin and express the opinion that the program's benefits, for them individually, would start sooner. Other individuals overestimate the latency before benefits from the program would begin and think the benefits would start later, for them, than the time actually stated in the scenario.

[1] See Figure 1 and 2 for examples from the survey.

The illness profiles in our survey are indexed to the respondent's gender and current age in order to make them more concrete. The sequence of adverse future health states for which risk might be reduced is described in terms of a future interval of sick time, followed (possibly) by an interval of post-illness recovered time, concluded (possibly) by some number of lost life-years, relative to the individual's nominal life expectancy without the illness. The econometric model is cast in terms of the present discounted time in each adverse health state and the individual's probabilities of facing each illness profile with and without the program. "Scenario adjustment" in this context, for example, might occur when a subject has a family history of heart disease at age 50. The choice scenario may describe heart disease that would lead to moderate and/or severe pain and disability starting at age 70. However, given his private knowledge of his family history, the subject might answer the question as though the proposed risk reduction program would begin to benefit him at age 50. Thus, the subject is answering a slightly different question than asked in the survey where the latency is reduced by twenty years. Debriefing questions allow researchers to know if an individual adjusts the scenario, by how much the scenario is adjusted, and, as we will show, allow the researcher to correct for the adjustment.

The paper proceeds as follows: the next section reviews the *SP* literature. Section 3 briefly describes the survey and data. Section 4 reviews the utility-theoretic choice model used to analyze respondents' program preferences. Section 5 discusses how to control for scenario adjustment and conveys our empirical results, and Section 6 concludes.

2. Stated Preference Literature

Among environmental economists, stated preference research evolved as one means to measure the non-market damages caused by events such as oil spills. For legal proceedings, there is a premium on model simplicity, so the early hypothetical choice tasks in these surveys tended to be streamlined pair-wise choices between one improved scenario concerning environmental quality at a single price versus the status quo. The goal was to estimate as precisely as possible the maximum price willingly paid for the environmental improvement. These were known as "contingent valuation methods," since the elicited values were contingent on the hypothetical market proposed to the consumer.[2]

In contrast, in the marketing and transportation mode choice literatures, stated preference measures evolved to help researchers understand consumers' marginal rates of substitution between different attributes of heterogeneous goods—i.e. different transportation modes, each with different levels of a common array of attributes including price, different modes of transportation (such as bus versus train versus private automobile), different travel times, wait times, amenities, and costs. In these literatures, more-complex models were appropriate. These stated preference methods were originally known as "conjoint choice experiments" with the notion of an experiment referring to the efficient randomized design of the mixes of attributes associated with each alternative. When these consumer choice exercises are conducted with larger heterogeneous samples of respondents, outside of a laboratory setting, they are

[2] For an accessible overview, we recommend the summary by Carson (2000).

sometimes called “field experiments,”[3] especially when the mix of attributes in any choice set is randomly assigned.

In conjoint choice experiments, random and efficient survey design is important. Randomness ensures that attribute levels are not highly correlated across alternatives, so that is possible to identify the separate influence of each different attribute. Efficiency is important so that the greatest precision in utility parameter estimates can be obtained with the smallest possible samples (since surveys can be very expensive).

While the ideal experimental design for survey choice sets is both random and efficient, choice sets also need to be realistic and believable to respondents. Complete randomization of attribute levels can lead to practical problems in a survey if randomly assigned attribute levels result in a combination of attributes that cannot exist in the real world. Louviere, et al. (2000) list four design objectives for experiments and one of them is market realism. Louviere (2006) notes “Unfortunately, adding realism is not a statistical design property.” Since it is extremely important to respondents that a choice scenario is plausible and realistic, this creates a tension between randomization and realism that can be difficult to balance. Therefore, the researcher should randomize levels of the attributes, but also make sure the choice sets are realistic and credible for respondents.

The survey employed in this paper is a conjoint choice experiment. In the survey data used for this paper, levels of attributes need to span both the domain of current real-world alternatives and the domain of potential future policies. It is important that the

[3] For more on stated preference research: see Louviere, et al (2000), a summary of the debate on using stated preference research is in a set of three articles: Portney (1994), Hanemann (1994), and Diamond and Hausman (1994). For details on performing a good stated preference study see Holmes and Adamowicz (2003). For contingent valuation methods see Mitchell and Carson (1989).

scenarios for both real and future world alternatives are credible. The randomization of attributes for current and future policies may mean that some mixes of illness profile attributes employed are less realistic or less plausible to some individuals. This issue of credible scenarios needs to be carefully considered by the researcher.

Researchers have learned to address many possible biases in conjoint choice experiments. It is well-known that the preferences deduced from *SP* methods can be sensitive to many aspects of a survey's design and implementation. For example, estimates may be sensitive to question format (i.e. whether the choice concerns just the most-preferred alternative, or a rank-ordering of all alternatives), question order, and the presence or absence of a status quo alternative, etc.[4] DeShazo and Fermo (2002) and Hensher (2006) explore how respondents process different numbers of attributes and levels of complexity in stated choice experiments.

One problem that has long troubled researchers in *SP* surveys is the potential for respondents to register "protest responses." This is generally interpreted as a respondent's decision to report a certain value—often zero—not because they actually feel the value of the program is zero, but for some other reason (such as doubts about the viability of the proposed hypothetical program).[5] For example, even though a respondent places a high value on clean water, they might report they would not be willing to pay the cost of a proposed improvement to water quality because they do not feel that the program is technologically feasible.

[4] Boyle, et al. (2001) discuss the validity of conjoint rating, rank, and choice methods. Louviere (2006) discusses several other major issues in discrete choice studies.
[5] For more description of protest responses and protest bids, see Bateman, et al. (2002) and Champ, et al. (2003).

In open-ended contingent valuation questions, where the respondent is pressed to reveal a point estimate of their actual willingness to pay (rather than merely to indicate whether they would pay a particular stated price), objections to some aspect of the stated choice scenario are often registered as "protest zeros." Identifying these protest zeroes can be difficult. Bateman, et al. (2002) suggest several methods to identify these protest responses such as follow-up questions about why respondents answered the way they did and by identifying values of WTP that are greater than these individuals are actually able to pay.

From an empirical modeling standpoint, the challenge lies in how to construct a model to handle data where some zero values are true zeros and others merely represent a rejection of some aspect of the hypothetical choice scenario. There has been considerable applied research wherein the investigator considers the consequences of different possible treatments of zero values for willingness to pay. Bateman, et al. (2002) suggest an appropriate way to deal with protest responses is simply to delete them.[6] Champ, et al. (2003) suggest several methods such as trimming the upper values of contingent valuation questions if the answers are more than 10% of their income or identifying the values that have a large influence on the results and trimming those. These and many other strategies hinge on the availability of follow-up or debriefing questions which probe further when the individual conveys a zero value. Strazzera, et al. (2003) offer possible corrections for selection bias which is caused by protest zeroes in contingent valuation studies.

[6] See Jorgensen, et al. (1999) and Meyerhoff and Liebe (2006) who argue that censoring of protest responses may be inappropriate.

Similar to protest responses is the idea of “scenario rejection,” where individuals fail to accept some dimension of the stated choice scenario and thus do not make choices that reflect their preferences over the stated attributes of the good in question.[7] Scenario rejection may be a reason that a respondent gives a protest response. Any aspect of the scenario that the respondent does not find credible may lead them to fully reject the scenario. The importance arises when the *WTP* response of zero means that respondent does not believe some aspect of the choice scenario and not that they have a WTP of zero. Separating out scenario rejection zeroes from a true WTP of zero is important. Similar methods of correction as suggested in protest responses may be adequate to identify and correct for scenario rejection. [In our stated preference study of health-risk reduction programs, people chose “Neither Program” for a variety of different reasons, some of which are entirely acceptable and others that reveal some type of scenario rejection.]

In stated choice scenarios, the investigator must assume that survey respondents believe and accept the combinations of attributes bundled into each stylized alternative in the stated choice sets. However, this may not be a realistic assumption since consumers are heterogeneous and may understand (or relate to) a given choice scenario differently than other consumers. While survey respondents are asked to convey their choices contingent on the conditions described to them in the choice scenarios with which they are presented, even the most carefully pre-tested survey instrument will cause some individuals to have trouble imagining a world containing the exact alternatives described in the choice scenario. A respondent may refuse to “play along,” but if he or she tells us

[7] Even in real choice situations, a consumer may choose not to buy a product simply because the seller’s claims about it seem “too good to be true.” If the consumer could verify the product’s qualities, however, they would actually make the purchase. This suggests that scenario rejection and scenario adjustment may thus be fairly common in real markets, too.

that he rejects the scenario then there is the potential to test for systematic determinants of non-response to the choice question.

It is also possible that the individuals may not completely reject the choice scenario, but may find one or more of the stated alternatives implausible. The individuals may implicitly replace one or more of these implausible stated alternatives with something that they deem more plausible and then make their decisions based on these mental edits to the choice set and partake in scenario adjustment. Debriefing questions asked after the individuals make a choice can extract whether they have adjusted the scenario and by how much. If the survey extracts no debriefing information about these behaviors, the investigator is left to assume that the attributes the respondents used to make their decisions were exactly those stated in the choice set.

Closely related to scenario adjustment is the role of perceptions. Perceptions are an important determinant of choices, both real and hypothetical, that people make, and these perceptions influence their decisions. McConnell (1993) discusses in the context of the Comprehensive Environmental Response, Compensation, and Liability Act of 1980 (*CERCLA*) that people judge both whether environmental quality is damaged and which pollutants are the cause of the damage. Adamowicz, et al. (1997) suggest two methods for addressing the difference in objective versus perception data. In their model, the welfare impact is evaluated by looking at the difference between a base level of environmental quality and a target level of environmental quality. An agency has an objective measure of the environmental quality, but the respondents in the survey perceive a level of environmental quality. They suggest two approaches for correcting respondents' perceptions when they differ from the objective measure. One approach is to measure the difference in the perceived base and target levels and make sure that difference is equal to the difference between the agency's base and target levels. That way, the researchers are comparing the same level of change. The second approach, and the one that they employ, is to adjust the values of respondents who perceive the target value is lower than the objective value to make it equal to the objective value. If the perceived target level is equal to or greater than the agency's target level, then no change is made. They admit that this approach is correct only if individual's perceptions converge to the agency's target level over time. They do find that their parameter estimates differ when the objective and perceived data are used.

While outright scenario rejection may be relatively easy to detect, scenario adjustment—which is a matter of degree, rather than an all-or-nothing proposition—may be more insidious and more difficult to detect. To the fullest extent possible, it is imperative in *SP* research to do thorough pre-testing of choice scenarios for plausibility. Researchers now widely use methods borrowed from cognitive psychology applied to survey development.

In many substantial surveys, researchers are now careful to debrief respondents about "what they were thinking" when they answered the main choice questions. Some experts argue that it is difficult to ask a respondent to "time-travel" back to the instant in time when they made a prior choice and that debriefing questions must be posed tactfully. For example, it may go against the "norms of conversation" to ask someone to make a choice and then invite them, ex post, to reveal that they did not believe the choice scenario. Many people prefer to avoid confrontation, and others may find it somewhat impertinent to be asked "did you really believe the choice scenario," since this may imply that they should not have believed it, or that they were in some way naïve about its degree of realism.

An example from the previous literature where debriefing questions are used to discern scenario adjustment from previous literature is contained in a report on the "Montrose case" (Carson, et al. (1994)). Respondents are told in the choice scenario that the environment would recover from the effects of DDT and PCBs in a given number of years. Following the choice question based on this scenario, subjects are given debriefing questions. In the debriefing questions, they are asked whether they believed that DDT and PCBs could cause the problems stated in the scenario. The goal of this question is to determine who is rejecting the scenario. Next they were asked if they believe the natural processes would return to normal in the stated number of years. If they said they did not believe the stated recovery time, they were asked if they thought it was more or less than the stated time. This allowed the researchers to know who updated the time frame given in the scenario and whether they over- or under-estimated the time it would take, relative to the time specified in the choice scenario.

In a similar vein, Viscusi and Huber (2006) debrief their respondents to a survey about the value of water quality improvements by asking for subjective assessments of the probability that the program will actually produce the advertised benefits. Another example of scenario adjustment is contained in Burghart, et al. (2006). The authors introduce three scenario adjustment parameters into their theoretical model on climate change policy preferences. The usual maintained hypothesis would be that each of these parameters is equal to one. If it is not possible to reject the hypothesis, then the implication is that respondents, on average, both believe and pay attention to the corresponding attributes of the alternatives in the choice set. This restriction is relaxed, however, and the parameters are freely estimated. They turn out not to conform to the maintained hypothesis in the usual model. The estimated values of these adjustment parameters improve the predictive capabilities of the basic choice model and influence estimates of fitted willingness to pay for the good in question.

Another challenge discussed in this literature is the theoretical basis for the estimating specification and the strategies used to estimate stated choice models. In much of the early conjoint choice research in the marketing literature, the econometric specifications used to model choices were limited to linear and additively separable specifications that produced conveniently distinct scalar estimates of the “part-worths” of different product attributes. When all attributes enter linearly, the tradeoffs between price and the other attributes are readily identified by using the ratios of each coefficient to the coefficient on price. These specifications tend to be rather ad hoc local approximations.

Researchers then gradually developed a utility-theoretic framework for choice models. The framework most often used in environmental economics for choice scenarios is Random Utility Model (*RUM*). When using *RUM*, the researcher strives to model consumer choice as a function of the difference in indirect utility across the alternatives. The consumer chooses the alternative associated with the highest level of utility. With a sufficient number of observations, the underlying indirect utility function can allow for flexible patterns of substitution between non-price attributes in the implied willingness-to-pay function. If the specifications remain linear-in-parameters, these models can be estimated econometrically using conditional logit specifications (with the option of fixed effects specifications if there are multiple choice occasions per individual).

These models assume that individuals believe the entire choice scenario and if individuals make a choice based on different assumptions, the parameters may not be correct.

Stated preference surveys can be very useful if adequate market data do not exist, but researchers need to take great care in the design of the survey. It is impossible for researchers to fully anticipate the likely credibility of all dimensions of a randomized choice scenario from the perspective of each individual who might participate in the survey. After the researchers' best effort has been made to render the choice scenarios plausible to as many people as possible, the best response to residual scenario adjustments may be for the researcher to anticipate that they are inevitable in some proportion of cases and to plan for the option to correct for this behavior. Our paper differs from previous literature in that the goal of this paper is to illustrate how carefully worded debriefing questions can be used to net out certain types of scenario adjustment and how counterfactual simulation is possible to estimate preferences had each individual in the sample fully accepted this key dimension of the stated choice scenario. The contribution of this paper is to evaluate whether scenario adjustment occurs, to show how to control and correct for scenario adjustment, and to note the differences in willingness to pay for health risk reduction with and without the correction.

3. Available Choice Data

Since market data from which to infer individuals' demands for health risk reductions is not adequate, Cameron and DeShazo (2006a) use stated preference methods to elicit preferences for programs to reduce the risk of morbidity and mortality in a general population sample of adults in the United States.[8] In brief, the survey consists of 5 modules.[9] The first module asks respondents about their subjective risks of contracting the major illnesses or injuries used in the survey, how lifestyle changes would change their risk of these illnesses and how taxing it would be to implement these lifestyle changes.

The second module is a tutorial that explains the concept of an "illness profile." The sequence of future health states in an illness profile includes the number of years before the individual becomes sick, illness-years while the individual is sick, recovered/post-illness-years after the individual recovers from the illness, and lost life-years if the individual dies earlier than he would have without the disease. Then the tutorial informs the individual that he might be able to purchase a new program that would reduce his risk of experiencing each illness profile. Each illness-related risk-reduction program consists of diagnostic blood tests, drug therapies, and life-style changes.

The key module of each survey involves a set of five different three-alternative conjoint choice experiments where the individual is asked to choose between two possible health-risk reducing programs and a status quo alternative. Each program reduces the risk that the individual will experience a specific illness profile for a major illness or injury (i.e., one of five specific types of cancer, heart attack, heart disease, stroke, respiratory illness, diabetes, traffic accident, or Alzheimer's disease). Each illness profile is described to the respondents in terms of the baseline probability of experiencing

[8] Knowledge Networks, Inc administered an internet survey to a sample of 2,439 of their panelists with a response rate of 79 percent.
[9] For more detail on the survey, please see an annotated sample at: http://darkwing.uoregon.edu/~cameron/vsl/Annotated_survey_DeShazo_Cameron.pdf

the illness or injury, age at onset, duration, symptoms and treatments, and eventual outcome (recovery or death). The corresponding risk reduction program is defined by the expected risk reduction and by its monthly and annual cost.

Each choice exercise is immediately followed by a set of debriefing questions for the researcher to better understand the reasons that the individual made that choice. Some questions depend on the alternative chosen. For example, there are various perfectly legitimate economic reasons why individuals may prefer the status quo, including that the individual cannot afford either of the programs, they would rather spend money on other things, or they believe they will be affected by another illness before they contract either of these two. If respondents choose the status quo, they are asked why "Neither Program" is their preferred alternative. Included among these reasons are some unacceptable ones that reveal scenario rejection: "I did not believe the programs would work."

Other debriefing questions are asked regardless of which alternative the individual selects. One example is "About when do you think you would begin to benefit from each program?" This question is of great interest in this paper since it asks individual about the latency period to better understand whether they answered the question based on the latency period stated in the scenario or based on some different assumption. If the subject fully accepts the stated scenario, then the age at which the scenario states that benefits start will match the age at which the individual believes the benefits will start. However, if some individuals subjectively adjust the latency in the scenario to better fit their own beliefs, then these individuals may say that benefits will start either sooner than, or later than, the latency stated for each of the two illness profiles described in the choice scenario.

Module 4 contains additional debriefing questions that cross-check the validity of responses. Module 5 is collected separately from the survey and contains a detailed medical history, including which major diseases the individual has already faced, and other types of socio-economic data.

4. A Random Utility Choice Model

This paper builds off the theoretical model presented by Cameron and DeShazo (2006a). In that paper, it is established that stated choices appear to be best predicted by a model that involves expected discounted utility from durations in different future health states. Indirect utility is also modeled as additively separable, but quadratic, in present discounted expected net income. The most basic specification is a five-parameter model.

To understand the model, consider just the pairwise choice between Program A and the status quo alternative (N).[10] Define the discount rate as r and let $\delta^t = (1+r)^{-t}$. Let Π_i^{NS} be the probability of suffering the adverse health profile (i.e. getting "sick") if the status quo alternative is selected, and let Π_i^{AS} be the reduced probability of suffering the adverse health profile if Program A is chosen. The difference between Π_i^{NS} and Π_i^{AS} is $\Delta\Pi_i^A$, which is the risk reduction to be achieved by Program A. We assume that individuals do not expect to pay the annual cost of the risk reduction program if they are sick or dead. The sequence of health states that makes up an illness profile is captured by

[10] There is an analogous choice between Program B and the status quo alternative.

a sequence of mutually exclusive and exhaustive (0, 1) indicator variables associated with each future time period. These are defined as $1(pre_{it}^{A})$ for pre-illness years, $1(ill_{it}^{A})$ for illness-years, $1(rcv_{it}^{A})$ for recovered or post-illness years, and $1(lyl_{it}^{A})$ for life-years lost.

The present discounted remainder of the individual's nominal life expectancy, T_i, is given by $pdvc_i^A = \sum_{t=1}^{T_i} \delta^t$. Other relevant discounted spells, also summed from $t=1$ to $t=T_i$ include $pdve_i^A = \sum \delta^t 1\left(pre_{it}^{A}\right)$, $pdvi_i^A = \sum \delta^t 1\left(ill_{it}^{A}\right)$, $pdvr_i^A = \sum \delta^t 1\left(rcv_{it}^{A}\right)$, and $pdvl_i^A = \sum \delta^t 1\left(lyl_{it}^{A}\right)$. Since the different health states exhaust the individual's nominal life expectancy, $pdve_i^A + pdvi_i^A + pdvr_i^A + pdvl_i^A = pdvc_i^A$. Finally, to accommodate the fact that the individuals expect to pay program costs only during the pre-illness or recovered post-illness periods, we define $pdvp_i^A = pdve_i^A + pdvr_i^A$.

To further simplify notation, let $cterm_i^A = \left[\left(1-\Pi_i^{AS}\right)\right] pdvc_i^A + \Pi_i^{AS}\, pdvp_i^A$ and let $yterm_i^A = \left[-pdvc_i^A + \Pi_i^{AS}\, pdvs_i^A + \Pi_i^{NS}\, pdvl_i^A\right]$. The expected utility-difference that drives the individual's choice between Program A and the status quo can then be defined (there will be an analogous term for the utility difference between Program B and the status quo):

$$\begin{aligned}
\Delta E_{S,H}\left[PDV\left(V_i^A\right)\right] = \beta_0 &\left\{\left(Y_i - c_i^A\right) cterm_i^A + Y_i\, yterm_i^A\right\} \\
&+ \beta_1 \left\{\left(Y_i - c_i^A\right)^2 cterm_i^A + Y_i^2\, yterm_i^A\right\} \\
+ \alpha_1 \left\{\Delta\Pi_i^{AS}\, pdvi_i^A\right\} &+ \alpha_2 \left\{\Delta\Pi_i^{AS}\, pdvr_i^A\right\} + \alpha_3 \left\{\Delta\Pi_i^{AS}\, pdvl_i^A\right\} + \varepsilon_i^A
\end{aligned} \tag{1.1}$$

The five terms in braces can be constructed from the data, given specific assumptions about the discount rate[11].

In the sense of Graham (1981), the option price for the program is the maximum common certain payment that makes the individual just indifferent between paying for the program and enjoying the risk reduction, or not paying for the program and not enjoying the risk reduction. The annual option price $\hat{c}_i^A$ that makes the expression in equation (1.1) exactly equal to zero can be calculated as

$$\hat{c}_i^A = Y_i - f^{-1}\left(\frac{\left(\beta_0 + \beta_1 Y_i\right) yterm_i^A + pterm_i^A + \varepsilon_i^A}{-\left(\beta_0 + \beta_1 Y_i\right) cterm_i^A}\right) \tag{1.2}$$

Where $f(Y) = \left(\beta_0 + \beta_1 Y_i\right) Y_i = \beta_0 Y_i + \beta_1 Y_i^2$, so that $f^{-1}(\cdot)$ is the solution to a quadratic form. Then, the expected present value of this stream of payments must be calculated over the individual's remaining nominal lifespan:

$$E_{S,H}\left[PV\left(\hat{c}_i^A\right)\right] = cterm_i^A\left[\hat{c}_i^A\right] \tag{1.3}$$

Finally, to convert this expected present-value option price into a measure that Cameron and DeShazo (2006a) call the "value of a statistical illness profile" (*VSIP*), we normalize arbitrarily on a 1.00 risk change by dividing this *WTP* by the absolute size of the risk reduction to produce:

$$VSIP = E_{S,H}\left[PV\left(\hat{c}_i^A\right)\right] \Big/ \left|\Delta\Pi_i^A\right| \tag{1.4}$$

The *VSIP* depends upon the entire illness profile and all of the parameters in equation (1.1). It is the closest counterpart, in this model, to the conventional idea of the "value of

[11] In this paper, we assume a common discount rate of 5%. In Cameron and DeShazo (2006b), the consequences of assuming either a 3% discount rate or a 7% discount rate are explored. Work in progress involves the estimation of individual-specific discount rates simultaneously with these stated choices concerning health risk reduction programs, using additional data on intertemporal choices by a separate sample of respondents from the same population.

a statistical life" (*VSL*) employed in the mortality risk valuation literature, such as discussed in the meta-analysis by Viscusi and Aldy (2003). The *VSIP* can be used to compare willingness to pay for health risk reductions for differing age groups and illness profiles.

The data suggest, however, that the simple five-parameter model in equation (1.1) is dominated by a specification that is not merely linear in the terms involving present discounted health-state years. First, we factor out the probability differences in the final terms in equation (1.1) as:

$$\begin{aligned}&\alpha_1\left\{\Delta\Pi_i^{AS}\,pdvi_i^A\right\}+\alpha_2\left\{\Delta\Pi_i^{AS}\,pdvr_i^A\right\}+\alpha_3\left\{\Delta\Pi_i^{AS}\,pdvl_i^A\right\}\\&=\Delta\Pi_i^{AS}\left[\alpha_1 pdvi_i^A+\alpha_2 pdvr_i^A+\alpha_3 pdvl_i^A\right]\end{aligned}$$

Where $j = A, B, N$, and $pdvX_i^N = 0$ for $X = i, r, l$. This simple linear specification fails to explain respondents' observed choices as well as a model that employs shifted *logarithms* of the $pdvX_i^j$ terms. We considered a form that was fully translog (including all squares and pairwise interaction terms for the three log terms). If we retain only those terms where the α coefficients are statistically significantly different from zero, this final term becomes:

$$\Delta\Pi_i^{AS}\begin{bmatrix}\alpha_1\log\left(pdvi_i^A+1\right)+\alpha_2\log\left(pdvr_i^A+1\right)+\alpha_3\log\left(pdvl_i^A+1\right)\\+\alpha_4\left\{\log\left(pdvl_i^A+1\right)\right\}^2+\alpha_5\log\left(pdvi_i^A+1\right)\log\left(pdvl_i^A+1\right)\end{bmatrix}\tag{1.5}$$

Finally, because the opportunity for longer durations in each health state is correlated with the youth of the respondent, we allow the α coefficients to differ systematically with the respondent's current age wherever this generalization is warranted by the data. This leads to a model where $\alpha_3 = \alpha_{30} + \alpha_{31}age_i + \alpha_{31}age_i^2$, and analogously for α_4 and α_5. This quadratic-in-age systematic variation in parameters permits non-constant age profiles for our *VSIP* estimates, and our data tend to produce the usual higher values during middle age and lower values for younger and older respondents.

5. Controlling for Scenario Adjustment with Respect to Latency

Our goal in this paper is to assess the extent to which scenario adjustment, with respect to the latencies of the illness profiles described in the choice sets, affects the estimated preference parameters in the model. We do this by allowing the parameters in the general model to vary with individual responses to our key debriefing questions.

The working version of the general model involves a total of 13 parameters—β_0 and β_1 which capture the marginal utility of net income (i.e. expenditure on all other goods and services), the five basic α parameters appearing in the illness profile term in expression (1.5) above, plus the three pairs of coefficients on the age_i and age_i^2 terms that shift α_3, α_4 and α_5.

We initially allow each of the thirteen parameters in the general specification to differ systematically with individuals' responses to the debriefing questions about when (and whether) they are likely to benefit from each program. The specific ways the utility parameters are permitted to vary are described in Section A, and a possible correction method is described in Section B.

A. Variables used to quantify scenario adjustment

After each choice scenario, respondents are asked debriefing questions about when they believe that the benefits of each proposed program would begin, for them personally. Figure 2 gives one example of the specific wording of our debriefing questions. Based on the answers to each of these questions, we define the following two variables.

- $1(never_i^j)$ is an indicator variable that takes a value of 1 if the individual responds by checking "Never (Program would not benefit me)."
- $MOEL_i^j$ is an approximately continuous variable defined as the "minimum overestimate of the latency". The variable $MOEL_i^j$ is zero if the interval checked in Figure 2 contains the stated latency for the illness from the corresponding choice scenario. In this case, the time the benefits begin, in the opinion of the individual, is essentially the same as the latency stated in the choice scenario. $MOEL_i^j$ has a positive value equal to the lower bound of the checked time interval if that interval lies above the latency stated for that illness in the choice scenario (i.e. the individual overestimates the latency period). $MOEL_i^j$ has a negative value equal to the upper bound of the checked interval if that interval lies entirely below the stated latency (i.e. the individual underestimates the latency).

In Section B, we will use these variables as sources of systematic heterogeneity in the estimated utility parameters in the general choice model used in Cameron and DeShazo (2006a). First, however, we will explore the apparent relationships between these opinions and observable characteristics of respondents.

Propensity to say "Never (program will not benefit me)"

We use a simple binary logit model to examine how a wide variety of characteristics of the respondent, and attributes of the health risk targeted by each program, help to explain the value of the indicator variable $1(never_i^j)$. Ten different health risk-reduction programs are considered by each respondent, in five sets of two, with each set including the status quo as a third alternative. In total, therefore, 15,040 substantive illness profiles and health-risk reduction programs are considered in the 7,520 choice scenarios analyzed in this paper. For 11,566 (76.9%) of these illness profiles, respondents indicated that they would never benefit from the risk-reduction program.

Models 1 and 2 in Table 2 show the results from an ad hoc binary logit model to explain $1(never_i^j)$. The logit specification suggests that people are more likely to say that a particular program will never benefit them if they are female, the attendance of some or graduation from college. People who are less likely to say the program will never benefit them tend to have lower incomes, feel that they are at subjective risk of getting this disease or other major diseases, people who have, on average, more room to improve their health habits, are single parents and are people with dual-incomes who will have a child at home at the onset of the disease.

MOEL (minimum overestimate of the latency, continuous variable)

We explore the determinants of our continuous measure of the "minimum overestimate of the latency" using an ordinary least squares (OLS) model. The $MOEL_i^j$ for a program is known only if the individual does not state that they expect never to benefit from the program (i.e. if $1(never_i^j) = 0$). Thus, we have a maximum of 12,596 observations on $MOEL_i^j$. For many respondents and many programs, the interval during which the individual expects to benefit from the program includes the onset time specified in the illness profile. For these individuals and programs, $MOEL_i^j = 0$, signaling no scenario adjustment with respect to the latency period. In this study, this happens for 3,693 of the 12,596 programs for which $MOEL_i^j$ information is available. Latency is overestimated for 1,424 programs and the latency is underestimated for 7,479 programs. The mean value of $MOEL_i^j$ is -8.123 (with a minimum of -58 and a maximum of 29).

Models 3-5 in Table 2 indicate the significant determinants of $MOEL_i^j$. The coefficients on age and age squared are highly significant. Individuals who are more likely to overestimate the latency period are older, consider themselves to have room to improve their health habits, live in a dual-income household, or will have a child at home at the onset of the disease. Individuals who are more likely to underestimate the latency before the program's benefits will begin are those who have not attended college, currently have an illness, feel that they are at subjective risk for this illness, or currently have a child. The length of the stated latency is also an important determinant of $MOEL_i^j$. Not surprisingly, a longer stated latency period in the scenario makes respondents more likely to underestimate the latency.

B. Netting out the effects of scenario adjustment

Respondents' answers to the debriefing questions following each stated choice question suggest that a substantial share of our respondents may not have taken at face value all of the descriptions of the illness profiles provided in the choice scenarios. The empirical results in the previous section suggest that the tendency to adjust the choice scenario may vary systematically with the type of respondent and with the nature of the illness profile in question (e.g. pain, long latency, etc.).

The intent behind a stated preference study, however, is to induce individuals to accept the stated choice scenario and to respond conditional on that acceptance. If respondents reinterpret the question before they answer, then some of the maintained hypotheses behind the random utility model that produces estimates for utility parameters are violated. We use $1(never_i^j)$ and $MOEL_i^j$ to control and correct scenario adjustment with respect to the latency attribute and these measures should work more generally on other attributes that are affected by scenario updating.

Our two scenario adjustment variables, $1(never_i^j)$ and $MOEL_i^j$, are included in the basic model as variables which are allowed to shift each of the basic parameters. The respondents giving the information about whether they adjusted the scenario are the very same respondents who report their program choices. We acknowledge that using the same sample for both the dependent variable and for $1(never_i^j)$ and $MOEL_i^j$, which are included on the right hand side of the equation, may lead to an endogeneity problem. We feel, however, that that the latency period is a determinant of the expected utility-difference that drives the individual's choice between Program A, Program B and the status quo. Since the respondent first reads the scenario, then determines his or her perception of the latency period and then makes a program choice, that $1(never_i^j)$ and $MOEL_i^j$ should be included on the right hand side of the equation and do not create an endogeneity bias.

We make the following substitution for each of the thirteen basic preference parameters in the general specification in Cameron and DeShazo (2006a)[12]:

$$\alpha_k = \alpha_{k1} + \alpha_{k2} 1(never_i^j) + \alpha_{k3} MOEL_i^j \qquad (2)$$

Results for these fully generalized models are contained in Appendix 2, available from the authors. In the body of this paper, in Table 4, we report only the results for a parsimonious version of the model that retains only those shift variables which are individually statistically significant.

Model 1 of Table 4 shows the utility parameter estimates when the possibility of scenario adjustment is completely ignored. Model 2, in columns 2 through 4 reveal the results when scenario adjustment is accommodated. We label the first column in Model 2 as "Corrected," since these are the main effects of the net income and illness profile variables, controlling for $1(never_i^j)$ and $MOEL_i^j$. The ideal situation (full acceptance of the stated latency of benefits) corresponds to $1(never_i^j) = 0$ and $MOEL_i^j = 0$ for all respondents and all programs. These corrected estimates constitute the utility parameters that would be predicted under those conditions. Column 3 shows the shifts in these parameters induced by $1(never_i^j)$ which equals one if the respondent states that they will never benefit from the program. Column 3 shows the shifts in these parameters for $MOEL_i^j$ (i.e. the extent to which the individual over or underestimates the latency described in the choice set for each year).

The magnitude of some of the shift parameters in Model 2 are striking. In the uncorrected model, the estimate of the (dis)utility associated with the linear term in

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[12] In a set of preliminary models, we employed both $1(never_i^j)$ and a pair of indicator variables for over- or underestimation (relative to none) to shift each of the α parameters in the general model.

discounted sick-years is -47.9. This negative effect could be overwhelmingly undone if the individual believes that the program in question will never benefit them since the coefficient on the interaction term involving $1(never_i^j)$ is 212.7 and is strongly statistically significant. Even if the individual does not report that they will never benefit from the program, each one-year increase in the individual's minimum overestimate of the latency reduces the implied marginal (dis)utility of a discounted sick-year by a very significant 7.08. The corresponding term in the (dis)utility of a discounted lost life-year is reduced by 4.09 for each year by which latency is overestimated.

The magnitude of the shift parameters is large, but to appreciate the overall effects of these parameter changes on demand estimates, it is necessary to calculate fitted willingness-to-pay (or *VSIP*) estimates. As shown in Table 5, *VSIP* estimates are calculated for an individual aged 30, 45, or 60 years-old, who earns an income of $42,000 per year. Five illness profiles that differ in years of sickness, latency of sickness, and whether the result is recovery or death, are constructed for each age group. The illness profiles in Table 5 involve sickness with a full recovery, sickness followed by death, and sudden death. The period of sickness lasts for either one or five years, as indicated. Next, two latency periods are considered. In the first pair of columns in Table 5, we assume that each illness profile starts immediately (i.e. the illness has no latency period). In the second pair, we assume that the illness profile involves a latency of 20 years (i.e. the onset of the disease is 20 years in the future). In each pair of columns, the uncorrected *VSIP* numbers are the values implied by the uncorrected model in Table 4. The corrected *VSIP* numbers are implied by the baseline coefficients in the corrected model in Table 4, which corrects for any scenario adjustments reported by respondents.

Table 5 shows that for the "No Latency" illness profiles, when the benefits start immediately, the corrected estimates are mostly higher than those models that do not take scenario adjustment into account. The most dramatic differences are for long and fatal illnesses for 60-year-olds, where the uncorrected model suggests a *VSIP* of less than $1 million, whereas the corrected estimate is $6.93 million. The only exceptions, where the corrected estimates are lower than the uncorrected estimate, are some of the illness profiles that have a full recovery. The difference in the corrected and uncorrected *VSIP* estimates suggests that if scenario adjustment is not taken into account, willingness to pay measures may be downward biased. This bias may result in the recommendation that some public policies that affect individuals with no latency (when benefits start immediately) should not be implemented when, in fact, it may actually be welfare-increasing to put these policies into effect.

In contrast, the corrected estimates for illness profiles that have a latency of 20 years are almost all lower than the uncorrected estimates. Many of these estimates are not significantly different from zero. The only two anomalies, where the corrected estimates are higher, are for the illness profiles of 30-year-olds who will recover. This suggests that failing to take into account scenario adjustment could cause some public policies that address long-latency health risks to be implemented when, in fact, they are not actually welfare-enhancing from the current perspective of most age groups.

The trends in the differences in these *VSIP* values show the importance of acknowledging and correcting for scenario adjustment. Accurate estimates help guide good public policy decisions that are welfare-improving.

6. Conclusions

Researchers have addressed many problems in *SP* research such as protest responses and scenario rejection. This paper addresses the related and more subtle problem of scenario adjustment.

Using debriefing questions in a stated preference survey on willingness to pay for health risk reductions, we were able to see that some respondents tend to adjust a given scenario. We use two variables to quantify the scenario adjustment. One is a binary variable that is equal to one if the respondent said that the program will never benefit them. The second variable is a measure of how much a respondent under- or over-estimates the latency period before benefits of the program would begin to occur.

The empirical results from this survey on willingness to pay for risk reductions suggest that some individuals update some aspects of a scenario given in a survey so that it better applies to their own personal situation. Debriefing questions given immediately after the survey question of interest can allow researchers to discern those individuals who answered the scenario as it was written from those who updated the scenario. The debriefing questions can also find out by how much individuals adjusted the scenario before answering the choice question, which allows researchers to counterfactually simulate what the individual's response would have been if they had answered the stated question. Correcting for scenario adjustment allows for more accurate estimates of willingness to pay.

Our estimation shows that the counterfactually simulated *VSIP* estimates that are corrected for scenario adjustment differ from those estimates that have not been corrected. This suggests the importance for recognizing the individuals that update the scenarios and correcting for it. Programs that benefit people now have mainly higher estimates when corrected and programs that benefit people 20 years in the future have mainly lower estimates. These differences in measures of willingness to pay could make the difference between enacting a policy that is cost-effective and not enacting it. They could also make the difference between enacting and not enacting a policy that is actually not cost-effective. Therefore, using debriefing questions to evaluate the responses by individuals should be a regular component in surveys. Correcting for scenario adjustment should be a regular routine component when doing empirical work on stated preferences.

Figure 1 – Example of a Choice Scenario

Choose the program that reduces the illness that you most want to avoid. But think carefully about whether the costs are too high for you. If both programs are too expensive, then choose Neither Program.

If you choose "neither program", remember that you could die early from a number of causes, including the ones described below.

	Program A for Diabetes	**Program B for Heart Attack**
Symptoms/ Treatment	Get sick when 77 years-old 6 weeks of hospitalization No surgery Moderate pain for 7 years	Get sick when 67 years-old No hospitalization No surgery Severe pain for a few hours
Recovery/ Life expectancy	Do not recover Die at 84 instead of 88	Do not recover Die suddenly at 67 instead of 88
Risk Reduction	10% From 10 in 1,000 to 9 in 1,000	10% From 40 in 1,000 to 36 in 1,000
Costs to you	$12 per month [= $144 per year]	$17 per month [= $204 per year]
Your choice	○ **Reduce my chance of diabetes**	○ **Reduce my chance of heart attack**
	○ **Neither Program**	

Figure 2 – Example of Corresponding Debriefing Questions Used to Correct for Scenario Adjustment

You may have chosen Program A, Program B, or neither. Regardless of your choice, we would like to know when, over your lifetime, you think you would first need and benefit from the two programs (if at all).

Your answers below may depend upon the illness or injury in question, as well as your current age, health and family history.

Around when do you think you would begin to value highly the risk reduction benefits of each program?

Select one answer from each column in the grid

	Program A to reduce my chance of diabetes	Program B to reduce my chance of heart attack
For me, benefits would start		
Immediately		
1-5 years from now		
6-10 years from now		
11-20 years from now		
21-30 years from now		
31 or more years from now		
Never (Program would not benefit me)		

Table 1: Descriptive Statistics (Programs offered = 12596)

For Models to Explain Scenario Adjustment

Variable	Mean	Std. Dev.	Min	Max
Dependent Variables				
Will never benefit from program* $1(never_i^j)$	0.076			
Minimum overestimate of latency** $MOEL_i^j$	-8.123	12.268	-58	29
Minimum overestimate if latency overestimated $MOEL_i^j > 0$	7.723	6.451	1	29
Minimum overestimate if latency underestimated $MOEL_i^j < 0$	-15.151	10.759	-58	-1
Attributes of stated illness profile				
Duration of pain/disability (months if less than 60)	35.794	37.999	0	192
1(Longterm pain/disability) (>60 months)	0.288	0.453		
Age/gender/income of respondent				
Age of respondent (years)	49.920	14.944	25	93
1(Female)	0.504			
Income ($10,000)	51808	33815	5000	150000
Educational attainment				
1(Less than HS)	0.104	0.305		
1(High School)	0.337	0.473		
1(Some College)	0.251	0.433		
Objective health status				
1(Have same illness)	0.040	0.195		
Count of other major illness	0.294	0.578		
Subjective health risks				
Subjective risk, same illness	-0.223	1.242		
Subjective risk, other illness	-0.242	0.861		
Avg room to improve health habits	3.446	0.831		
Respondent's household structure				
Size of household	2.572	1.258		
1(Have kids)	0.287	0.452		
1(Single parent)	0.017	0.129		
1(Dualinc-w/ or w/out kids)	0.647	0.478		
1(Have kid at onset)	0.029	0.169		
1(Single parent & kid at onset)	0.001	0.030		
1(Dual-income & kid at onset)	0.023	0.150		

* Benefits Never has a greater number of programs (13,615).

** 29.3% of the minimum overestimate of latency (*MOEL*) observations are equal to zero. *MOEL* = 0 if the respondent's subjective latency interval contains the latency stated in the survey.

Table 2: Correlates of different measures of scenario adjustment

	1 - Binary Logit $1(never_i^j)$	1 - Binary Logit $1(never_i^j)$	3 – OLS $MOEL_i^j$	4 –OLS $MOEL_i^j$	5 – OLS $MOEL_i^j$
Attributes of illness profile					
Duration of pain/disability (months if less than 60)	-0.001	-0.000	0.033	0.033	0.012
	(0.57)	(0.50)	(11.38)***	(11.37)***	(4.65)***
1(Longterm pain/disability) (>60 months)	0.157	0.155	0.502	0.499	0.578
	(1.97)**	(1.95)*	(2.07)**	(2.06)**	(2.76)***
Some demographic characteristics of respondents					
Age of respondent (years)	0.006	-	0.314	0.311	0.012
	(0.45)		(6.92)***	(6.87)***	(0.15)
Age-squared (100s of years)	-0.010	-	-0.116	-0.113	-0.078
	(0.79)		(2.70)***	(2.64)***	(1.10)
1(Female)	-0.375	-0.381	-0.205	-	-
	(5.61)***	(5.71)***	(0.99)		
Educational attainment					
1(Less than HS)	0.254	0.213	-1.832	-1.876	-1.712
	(2.09)**	(1.77)*	(4.79)***	(4.93)***	(5.21)***
1(High School)	0.274	0.246	-0.673	-0.701	-0.559
	(3.27)***	(2.98)***	(2.56)**	(2.68)***	(2.47)**
1(Some College)	0.143	0.136	-0.239	-0.256	-0.375
	(1.64)	(1.57)	(0.86)	(0.92)	(1.56)
Objective health status					
1(Have same illness)	-0.187	-0.222	-2.554	-2.542	-2.125
	(0.99)	(1.18)	(4.70)***	(4.67)***	(4.52)***
Count of other major illness	-0.116	-0.146	-0.567	-0.555	-0.640
	(1.99)**	(2.61)***	(2.97)***	(2.90)***	(3.88)***
Subjective health risks					
Subjective risk, same illness	0.342	0.343	-1.115	-1.116	-1.411
	(10.15)***	(10.20)***	(10.54)***	(10.56)***	(15.42)***
Subjective risk, other illness	-0.152	-0.147	-0.039	-0.043	0.269
	(3.23)***	(3.12)***	(0.25)	(0.28)	(2.01)**
Avg room to improve health habits	0.081	0.094	-0.973	-0.974	-0.976
	(2.01)**	(2.36)**	(7.40)***	(7.41)***	(8.60)***
Latency Period					
Stated latency	-	-	-	-	-0.250
					(2.22)**
Stated latency squared	-	-	-	-	-0.001
					(0.78)

Latency and age interaction	-	-	-	-	-0.013
					(3.50)***
Latency and age squared interaction	-	-	-	-	0.000
					(2.77)***
Latency and female interaction	-	-	-	-	-0.025
					(3.25)***
Respondent's household structure					
Size of household	-0.144	-0.140	-0.118	-	-
	(3.54)***	(3.70)***	(0.88)		
1(Have kids)	0.167	0.219	-1.987	-2.208	-0.663
	(1.42)	(1.96)*	(5.38)***	(8.27)***	(2.81)***
1(Single parent)	-0.578	-0.564	-1.858	-1.794	-2.058
	(2.48)**	(2.48)**	(2.20)**	(2.15)**	(2.85)***
1(Dualinc-w/ or w/out kids)	-0.017	-	0.701	0.625	0.754
	(0.22)		(2.87)***	(2.74)***	(3.83)***
1(Have kid at onset)	-0.064	-	14.445	14.371	2.557
	(0.16)		(11.11)***	(11.07)***	(2.22)**
1(Dual-income & kid at onset)	0.173	-	-2.681	-2.601	-2.354
	(0.37)		(1.84)*	(1.78)*	(1.87)*
Constant	2.720	2.708	-17.957	-18.157	8.782
	(6.85)***	(15.76)***	(14.36)***	(14.64)***	(3.55)***
Observations	13626	13626	12596	12596	12596
Log L			0.12	0.12	0.35
R-squared	-3550.8	-3552.8			

Absolute value of z statistics in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%, ^Sample size is smaller for Models 2 and 3 since they do not include those individuals who said the program would never benefit them.

Table 3: Descriptive Statistics – Policy Choice Models
(n = 15040 illness profiles and associated risk reduction programs)

	Mean	Std.dev.	Min.	Max.
Program attributes				
Monthly program cost	29.87	28.71	2	140
$\Delta\Pi_i^j$ = Risk change achieved by program	-.003406	.001669	-.006	-.001
Stated Illness profiles				
Latency (in years)	19.58	12.02	1	60
- 1($never_i^j$)	.07686			
- 1(Latency under-estimated)	.5458			
- 1(Latency over-estimated)	.1025			
- $MOEL_i^j$	-7.471	11.96	-59	29
Sick years (undiscounted)	6.499	7.169	0	52
$pdvi_i^j$ = Present value of sick-years	2.208	2.514	0	16.28
Recovered years (undiscounted)	26.05	13.02	1	64
$pdvr_i^j$ = Present value of recovered years	.4774	1.370	0	15.90
Lost life-years (undiscounted)	10.81	10.29	0	55
$pdvl_i^j$ = Present value of lost life-years	2.567	2.931	0	17.80
Subjective Adjustments to Illness Profiles				
Attributes of individuals				
Annual income	50852	34065	5000	150000
Age at time of choice	50.38	15.11	25	93
Systematic selection from RDD contacts				
$\hat{P}(sel_i) - \overline{\hat{P}}$ = Difference between fitted response/nonresponse and population average	.6769	3.363	-.3159	17.94

Table 4: Policy Choice Model (Parsimonious; alternatives = 22560)

(Parameter) Variable	Model 1: Uncorrected Coefficients	Model 2: Corrected Coefficients (if $*MOEL_i^j = 0$)	Model 2: $*1(never_i^j)$	Model 2: $*MOEL_i^j$
$(\beta_{00}\times 10^5)[\text{first income term}]$	5.183 (8.30)***	8.071 (10.69)***	-	0.225 (5.14)***
$(\beta_{10}\times 10^9)[\text{second income term}]$	-.1992 (4.22)***	-.2109 (4.15)***	.7656 (3.05)***	-
$(\alpha_{10})\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-47.89 (5.35)***	-57.32 (5.04)***	212.7 (3.91)***	7.083 (7.24)***
$(\alpha_{13})\left[P(sel_i)-\overline{P}\right]\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]$	3.372 (2.34)**	3.853 (2.45)**	-	-
$(\alpha_{20})\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-16.49 (1.76)*	-57.93 (5.77)***	-	-
$(\alpha_{30})\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-580.1 (3.25)***	-858.3 (4.28)***	-	4.092 (3.26)***
$(\alpha_{31})age_{i0}\cdot\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	20.46 (2.82)***	43.15 (5.41)***	-	-
$(\alpha_{32})age_{i0}^2\cdot\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-0.1874 (2.70)***	-0.3719 (4.97)***	-	0.0064 (7.39)***
$(\alpha_{40})\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	199.3 (2.41)**	281.8 (3.11)***	395.6 (4.51)***	-
$(\alpha_{41})age_{i0}\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	-7.786 (2.32)**	-15.71 (4.31)***	-5.197 (3.69)***	-
$(\alpha_{42})age_{i0}^2\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	0.0739 (2.27)**	0.1365 (3.90)***	-	-0.0013 (3.12)***
$(\alpha_{50})\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	102.4 (1.40)	129.6 (1.62)	-348.0 (3.77)***	-4.301 (3.90)***
$(\alpha_{51})age_{i0}\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	-4.484 (1.57)	-6.680 (2.16)**	-	-
$(\alpha_{52})age_{i0}^2\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	0.0561 (2.10)**	0.0624 (2.17)**	0.0752 (3.28)***	-
Log L	-11694.646	-10993.394		

[a] Corrected utility parameters are purged of scenario adjustment as captured by systematic differences in these parameters for alternatives where stated latency was not accepted by the respondent.

Table 5: Values of statistical illness profiles (mean [5th, 95th percentiles])
Without and with correction for illness scenario adjustment (Income = $42,000)

		No latency		Latency of 20 yrs	
Age	Illness profile	Uncorrected	Corrected	Uncorrected	Corrected
30	1 year sick, recover	$ 2.49 [1.3,3.94]	$ 3.2 [2.43,4.07]	$ 1.54 [0.77,2.49]	$ 1.94 [1.43,2.50]
	5 yrs sick, recover	3.75 [2.59,5.16]	3.94 [3.13,4.86]	2.32 [1.60,3.20]	2.35 [1.87,2.90]
	1 year sick, then die	4.14 [1.67,6.80]	6.52 [4.89,8.40]	4.42 [3.26,5.97]	1.67 [0.97,2.42]
	5 yrs sick, then die	4.19 [1.39,7.21]	7.02 [5.05,9.12]	4.57 [3.51,6.00]	1.99 [1.42,2.65]
	Sudden death	**4.26** **[1.30,7.38]**	**5.74** **[3.96,7.64]**	**4.35** **[2.97,6.04]**	**1.42** **[0.55,2.28]**
45	1 year sick, recover	2.33 [1.20,3.75]	2.68 [1.93,3.48]	1.33 [0.64,2.15]	1.27 [0.82,1.72]
	5 yrs sick, recover	3.56 [2.45,4.92]	3.47 [2.73,4.33]	2.08 [1.44,2.84]	1.68 [1.29,2.12]
	1 year sick, then die	4.59 [2.99,6.55]	7.61 [6.39,9.09]	2.53 [1.95,3.21]	-0.93 [b] [-1.59,-0.37]
	5 yrs sick, then die	4.44 [2.73,6.66]	8.48 [7.04,10.14]	2.66 [2.16,3.32]	-0.39 [b] [-0.89,0.04]
	Sudden death	**4.57** **[2.88,6.58]**	**6.10** **[4.88,7.39]**	**2.43** **[1.71,3.19]**	**-1.37** [b] **[-2.15,-0.70]**
60	1 year sick, recover	2.21 [1.07,3.46]	2.04 [1.31,2.75]	1.11 [0.55,1.67]	0.3 [-0.08,0.63]
	5 yrs sick, recover	3.26 [2.19,4.5]	2.86 [2.19,3.62]	1.66 [1.22,2.11]	0.59 [0.27,0.87]
	1 year sick, then die	2.40 [0.98,4.03]	6.41 [5.26,7.82]	1.27 [0.57,1.91]	-2.76 [b] [-3.79,-1.97]
	5 yrs sick, then die	0.92 [b] [-0.6,2.58]	6.93 [5.65,8.48]	1.23 [0.67,1.78]	-1.85 [b] [-2.63,-1.27]
	Sudden death	**3.46** **[1.88,5.13]**	**4.97** **[3.83,6.18]**	**1.39** **[0.52,2.09]**	**-3.2** [b] **[-4.32,-2.33]**

[a] Based on random draws from the joint distribution of the estimated parameters.
[b] Respondents were given no opportunity to express negative willingness to pay, so negative simulated values should be interpreted as zero WTP.

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APPENDIX – Alternative specifications

Under- or over-estimate of latency (ordered discrete variable)

We also considered a second specification for over- or under-estimating the latency. The ordered categorical variable $ordered\_latency_i^j$ is explored in the context of an ordered logit model. The variable $ordered\_latency_i^j$ is an ordered categorical variable that takes on the value 0 if the upper bound of the age interval checked among the selections in Figure 2 is lower than the stated age of onset given in the choice scenario. It takes the value 1 if the age interval checked in Figure 2 contains the stated age of onset, and take a value of 2 if the lower bound of the age interval lies strictly above the stated age of onset in the choice scenario

Results for this model are displayed in Appendix A in Table A-2. Individuals who are more likely to overestimate the latency of the illness are those who have finished only high school, have temporary or long-term pain in the illness profile stated in the scenario or have a kid at the stated onset of the disease. Individuals who are more likely to underestimate the length of the latency tend to have a lower income, have either this illness or another major illness, have a subjective risk for this illness, have kids, or will have a kid at the stated onset of the disease.

Table A-1: Policy choice model with all interaction terms (Alternatives = 22560)

	Model A1	Model A2		
(Parameter) Variable	Uncorrected	Corrected	$*1(never_i^j)$	$*MOEL_i^j$
$\left(\beta_{00}\times 10^5\right)\left[\text{first income term}\right]$	8.387 (10.03)***	8.387 (10.03)***	-2.702 (0.76)	0.248 (4.11)***
$\left(\beta_{10}\times 10^9\right)\left[\text{second income term}\right]$	-2.385 (3.86)***	-2.385 (3.86)***	10.235 (2.95)***	-0.027 (0.64)
$(\alpha_{10})\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-58.359 (5.05)***	-58.359 (5.05)***	248.650 (3.87)***	7.233 (7.13)***
$(\alpha_{13})\left[P(sel_i)-\overline{P}\right]\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]$	3.892 (2.15)**	3.892 (2.15)**	6.055 (0.60)	0.012 (0.08)
$(\alpha_{20})\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-51.663 (4.52)***	-51.663 (4.52)***	-60.728 (1.12)	1.177 (1.00)
$(\alpha_{30})\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-1019.412 (4.11)***	-1019.412 (4.11)***	499.341 (0.49)	5.900 (0.36)
$(\alpha_{31})age_{i0}\cdot\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	48.701 (4.80)***	48.701 (4.80)***	-19.464 (0.47)	-0.309 (0.41)
$(\alpha_{32})age_{i0}^2\cdot\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-0.412 (4.24)***	-0.412 (4.24)***	0.144 (0.36)	0.012 (1.47)
$(\alpha_{40})\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	339.442 (3.13)***	339.442 (3.13)***	484.391 (0.81)	-3.979 (0.41)
$(\alpha_{41})age_{i0}\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	-17.555 (3.95)***	-17.555 (3.95)***	-7.705 (0.33)	0.308 (0.72)
$(\alpha_{42})age_{i0}^2\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvl_i^A+1\right)\right]^2$	0.148 (3.44)***	0.148 (3.44)***	0.032 (0.15)	-0.006 (1.24)
$(\alpha_{50})\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	141.815 (1.55)	141.815 (1.55)	-416.324 (0.89)	-13.371 (1.42)
$(\alpha_{51})age_{i0}\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	-6.993 (1.95)*	-6.993 (1.95)*	-0.117 (0.01)	0.434 (1.07)
$(\alpha_{52})age_{i0}^2\cdot\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]\cdot\left[\log\left(pdvl_i^A+1\right)\right]$	0.063 (1.85)*	0.063 (1.85)*	0.101 (0.58)	-0.005 (1.20)
Log L	-11694.646	-10948.179		

Table A-2: Correlates of MOEL as a discrete variable

	1 – Ordered logit $MOEL_i^j$	2 – Ordered logit $MOEL_i^j$	3 – Ordered logit $MOEL_i^j$
Attributes of illness profile			
Duration of pain/disability (months if less than 60)	0.004	0.002	0.002
	(5.05)***	(1.93)*	(1.99)**
1(Longterm pain/disability) (>60 months)	0.064	0.094	0.095
	(0.93)	(1.30)	(1.32)
Some demographic characteristics of respondents			
Age of respondent (years)	0.036	0.000	-
	(2.72)***	(0.00)	
Age-squared (100s of years)	-0.029	0.003	-
	(2.34)**	(0.15)	
1(Female)	0.005	-	-
	(0.09)		
Educational attainment			
1(Less than HS)	-0.939	-0.940	-0.936
	(6.80)***	(6.68)***	(6.67)***
1(High School)	-0.040	-0.005	-0.007
	(0.57)	(0.07)	(0.10)
1(Some College)	-0.202	-0.207	-0.209
	(2.62)***	(2.57)**	(2.60)***
Objective health status			
1(Have same illness)	-0.679	-0.654	-0.651
	(3.20)***	(3.01)***	(3.00)***
Count of other major illness	-0.119	-0.137	-0.132
	(2.08)**	(2.28)**	(2.23)**
Subjective health risks			
Subjective risk, same illness	-0.132	-0.200	-0.201
	(4.38)***	(6.25)***	(6.28)***
Subjective risk, other illness	-0.081	-0.031	-0.028
	(1.86)*	(0.68)	(0.62)
Avg room to improve health habits	-0.155	-0.174	-0.178
	(4.30)***	(4.59)***	(4.72)***
Latency Period			
Stated latency	-	0.013	0.010
		(0.24)	(0.31)
Stated latency squared	-	-0.003	-0.003
		(6.86)***	(7.70)***
Latency and age interaction	-	0.003	0.002
		(1.35)	(1.86)*
Latency and age squared interaction	-	-0.000	-0.000
		(3.19)***	(4.71)***

Continued...

Respondent's household structure			
Size of household	-0.011	-	-
	(0.29)		
1(Have kids)	-0.284	-0.097	-
	(2.64)***	(1.13)	
1(Single parent)	-1.204	-1.319	-1.387
	(2.80)***	(3.06)***	(3.25)***
1(Dualinc-w/ or w/out kids)	0.107	0.120	0.107
	(1.55)	(1.82)*	(1.67)*
1(Have kid at onset)	1.809	0.155	-
	(6.80)***	(1.03)	
1(Dual-income & kid at onset)	-0.330	-	-
	(1.13)		
Observations	12596	12596	12596
Log L	-4259.161	-3697.929	-3698.915

Absolute value of z statistics in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

APPENDIX B – Available from the authors

Table B-1: Minimal Model (Alternatives = 22560)

	Model B1	Model B2		
(Parameter) Variable	Uncorrected	Corrected	$*1(never_i^j)$	$*MOEL_i^j$
$\left(\beta_{00}\times10^5\right)\left[\text{first income term}\right]$	5.342 (9.17)***	9.991 (12.98)***	-1.787 (0.54)	0.409 (7.40)***
$\left(\beta_{10}\times10^9\right)\left[\text{second income term}\right]$	-2.160 (4.61)***	-2.014 (3.33)***	9.731 (2.84)***	-0.026 (0.64)
$(\alpha_{10})\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-27.053 (4.56)***	-37.493 (4.99)***	109.601 (2.75)***	5.348 (7.75)***
$(\alpha_{13})\left[P(sel_i)-\overline{P}\right]\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]$	3.297 (2.29)**	3.475 (1.90)*	5.121 (0.50)	-0.033 (0.23)
$(\alpha_{20})\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-21.870 (2.35)**	-37.893 (3.43)***	-60.407 (1.13)	0.993 (0.86)
$(\alpha_{30})\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-30.409 (5.97)***	-36.974 (5.89)***	190.347 (5.79)***	6.594 (11.12)***
Log L	-11726.31	-11073.051		

Table B-2: Parsimonious Minimal Model (Alternatives = 22560)

	Model B1’	Model B2’		
(Parameter) Variable	Uncorrected	Corrected	$*1(never_i^j)$	$*MOEL_i^j$
$\left(\beta_{00}\times10^5\right)\left[\text{first income term}\right]$	5.342 (9.17)***	9.816 (14.00)***	-1.900 (0.57)	0.387 (10.18)***
$\left(\beta_{10}\times10^9\right)\left[\text{second income term}\right]$	-2.160 (4.61)***	-1.800 (3.58)***	9.425 (2.76)***	-
$(\alpha_{10})\Delta\Pi_i^{AS}\log\left(pdvi_i^A+1\right)$	-27.053 (4.56)***	-37.184 (4.97)***	103.398 (2.72)***	5.398 (7.98)***
$(\alpha_{13})\left[P(sel_i)-\overline{P}\right]\Delta\Pi_i^{AS}\left[\log\left(pdvi_i^A+1\right)\right]$	3.297 (2.29)**	3.786 (2.39)**	-	-
$(\alpha_{20})\Delta\Pi_i^{AS}\log\left(pdvr_i^A+1\right)$	-21.870 (2.35)**	-43.664 (4.45)***	-	-
$(\alpha_{30})\Delta\Pi_i^{AS}\log\left(pdvl_i^A+1\right)$	-30.409 (5.97)***	-36.855 (5.89)***	188.932 (5.74)***	6.619 (11.22)***
Log L	-11726.31	-11074.305		