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WHAT IS THE CAUSAL IMPACT OF KNOWLEDGE ON PREFERENCES IN STATED PREFERENCE STUDIES?

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What is the Causal Impact of Knowledge on Preferences in Stated Preference Studies?

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Abstract

This paper reports the results of a stated preference experiment designed to test for how information provided in a survey affects knowledge, and how knowledge affects preferences for a public good. A novel experimental design allows us to elicit subjects' ex ante knowledge levels about a good's attributes, exogenously vary how much new objective information about these attributes we provide to subjects, elicit subjects' valuation for the good, and elicit posterior knowledge states about the same attributes. We find evidence of incomplete learning and fatigue: as subjects are told more information, their marginal learning rates decrease. We find there is no marginal impact of knowledge on the mean nor the variance of WTP for changes in the environmental good; but that ex ante knowledge does affect stated WTP. Our results are consistent with preference formation models of confirmation bias, costly search, or timing differences in learning and preference formation. Our results raise questions about the purpose and effects of providing information in stated preference studies.

Keywords:

Learning, Information, Behavioral Economics, Decision Making Under Uncertainty

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D83, D81, Q51

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I. Introduction

One interesting feature of stated preference studies relative to other demand estimation methods is that subjects are presented with descriptive information about the attributes of the public good being valued before these values are stated to the researcher (Hanley, Shogren and White 2007). The information portion a survey is commonly justified because policy tethered to informed preferences is thought to be more efficient than policy tethered to uninformed preferences (Hanley and Barbier 2009). As a result, there is a large literature dating back to the early days of contingent valuation which examines how information provided in a survey about a public good's attributes and its management impact stated willingness to pay (Viscusi and O'conner 1984, Smith et. al. 1988, Boyle 1989, Bergstrom et. al. 1989, Hoehn and Randall 2002 and Berrens et. al. 2004).

In this paper, we use a novel experiment embedded in a stated preference study to identify the interaction of two important strands of this literature. First, how much a subject knows about a good before a survey begins is often correlated with their willingness to pay (Cameron and Englin 1997, Loomis and Ekstrand 1998 and Tkac 1998): subjects tend to know more about things they care about or have experience with (Czajkowski, Hanley and LaRiviere (2015). Second, information about the management, characteristics and attributes of a public good provided during a survey has been found to both influence and to not influence stated valuations (see Munro and Hanley 2001 for a summary of the early literature). A natural question, then, is how respondents' ex ante knowledge levels about an environmental good affect the causal impact of knowledge acquired about this good on willingness to pay (WTP) as measured in a stated preference study. Specifically, could the inconclusiveness of the literature on information effects be driven by heterogeneity in the ex ante information sets of the sample? Put another way, are less informed subjects more likely to be influenced by "new" information about the good?

This paper reports the results of an experiment designed to test for how providing information about the attributes of an environmental public good affects knowledge; and how that new knowledge affects the distribution of valuations for the public good *conditioning* on a subject's ex ante knowledge levels. Unlike previous literature, the novel experimental design allows us to elicit subjects' prior knowledge levels about a good's attributes; exogenously vary how much new information about the good's attributes we provide to subjects; elicit subjects' valuation for the good; and finally measure posterior knowledge states about the same attributes. Because we exogenously vary information in the experiment in addition to testing for knowledge states at the start of the survey, we can identify causal estimates for the marginal effect of new information provided on knowledge; and the marginal effect of knowledge on valuation for a good, conditioning on a subjects ex ante level of knowledge. This is not something which has been done before in the literature. Our design also provides an opportunity to compare different cognitive models of learning and stated valuation adjustment.

We embed the experiment in a stated preference survey concerning a population's willingness to pay for a project to regenerate coastal wetlands as a way of mitigating flood risk in Scotland. Wetland restoration reduces the costs of maintaining hard defenses by making use of the storm-buffering capacity of intertidal habitats, as well as providing additional flood storage during high tides and storm surges (King and Lester, 1995). Wetlands also provide amenity values by increasing habitat for wildlife, along with providing other ecosystem service benefits such as pollution reduction and improvements in water clarity (Barbier, 2011). Therefore, providing information about these attributes of coastal wetlands offers an ideal setting to identify the causal impact of knowledge on preference formation.

We used the following experimental design in our study. At the beginning of the survey we give subjects a nine-question multiple choice test over objective facts about historical flood events, flood protection and the ecological attributes of wetlands in order to elicit relevant prior knowledge levels. We

then randomly assign each subject to an information treatment (low, medium or high) based upon the number of questions they answered correctly. Each candidate piece of information about the good's attributes was related to exactly one of the nine multiple choice test questions. Importantly, because we identified the subject's relevant knowledge state at the beginning of the experiment, we can verify which information we give them is new. We then elicit agents' willingness to pay for a single wetlands restoration project which is uniform across all subjects. Lastly, we test subjects' retention of newly-provided information by giving them the same identical quiz at the end of the experiment which we gave them at the beginning. We are thus able to isolate how providing additional information about the good's attributes, which we can verify has been learned by comparing first and second quiz scores, affects their valuation for the good, while conditioning on different levels of a subject's *ex ante* knowledge.

There are two main results. First, giving subjects more information causes significant learning, although observed learning is incomplete. We also find that as subjects are told more information, their marginal learning rates decrease. The knowledge portion of the experiment is thus consistent with a model of imperfect learning and fatigue.

Second, we do not find that new knowledge about a good's attributes significantly affects valuations for the good. We do find systematic correlations between *ex ante* levels of information and valuations: *ex ante* more knowledgeable subjects valued the good less than *ex ante* less knowledgeable subjects. However, learning additional information did not affect these valuations holding the *ex ante* knowledge levels fixed. Further, additional information did not affect the variance of the distribution of valuations. As a result, we find that correlations between *ex ante* information and valuations and the impact of information on valuations are orthogonal.

Our paper contributes to the literature in a several ways. First, because of our novel experimental design, we can jointly test for how information affects knowledge and how knowledge affects preferences

in a unified framework. Previously literature which varies access to information about environmental goods only varies information without verifying that it has been learned (Smith et. al. 1988, Boyle 1989, Bergstrom et. al. 1989, Hoehn and Randall 2002 and Berrens et. al. 2004). As a result, we can estimate average treatment effects (ATEs) rather than intent to treat (ITT) effects in an experiment with information treatments directly as it pertains to valuation through knowledge.

Second, due to our design we are also able to test for the causal impact of information and learning conditioning on ex ante levels of knowledge. Previous literature shows, and our finding corroborate, that different ex ante levels of information can significantly change stated willingness to pay (Cameron and Englin 1997, Loomis and Ekstrand 1998 and Tkac 1998). In the closest experimental design to ours, Boyle (1989) finds that information about management costs and management practices can influence the variance but not the mean of the WTP distribution.¹ However, Boyle (1989) was not able to identify if this effect varies across the population by information sets. We find that neither the mean nor the variance of the WTP distribution is influenced by objective knowledge about the good itself, nor by knowledge of historical management practices.

Third, we focus very specifically on objective characteristics of the good which we can confirm were unknown to the subject. Other studies of information in a stated preference context focus on different forms of information such as redundant information (Viscusi and O'conner 1984), the relative cost of management versus a subject's income (Bergstrom et. al. 1989), and specificity of management actions (Boyle 1989). A different line of work combines theoretical modelling with stated preference studies to examine how differing levels of prior experience with a good affects how one estimates WTP in choice experiments (Czajkowski et. al. 2015a and Czajkowski et. al. 2015b). Our paper estimates the marginal

¹ Also like Boyle (1989) we only ask the WTP valuation question a single time.

impact of both information and acquired knowledge about public good attributes on WTP, conditional on prior knowledge levels.

Fourth, because we are able to test for ex ante and ex post knowledge states, vary information, identify that information creates knowledge and subsequently estimate WTP, we have the necessary features to link this large and somewhat older literature on information effects in contingent valuation to a newer economics literature on updating and valuation. Specifically, recent advances highlight how cognitive constraints, costly effort, endogenous search and confirmatory bias can influence both learning and subsequent valuation (Rabin and Schrag 1999, Caplin and Dean 2011, De Los Santos et. al. 2012, and Caplin and Dean 2015).² This is important, as recent evidence suggests there can be unexpected departures from neoclassical models of updating in stated preference studies (LaRiviere et. al. 2014).

Finally, because we simultaneously track both the causal effects of information on knowledge and knowledge on valuation, we are able to compare several different models of learning, information process and preference formation in a unified framework.³ To our knowledge this is the first such comparison in a stated preference survey. We are thus able to eliminate a large class of learning and valuation formation models used by our subjects in this study. Together with our results on learning, our results turn out to be consistent with three models of preference formation: 1) Confirmatory Bias (Rabin and Schrag 1999), 2)

² Understanding which model of learning and preference formation economic agents use in stated preference surveys is vital in order to develop mechanisms which lead to more efficient decisions and increased welfare (Sims 2003 and Bolton et. al. 2013). "Engineering" such mechanisms in the context of consumer choice relies on having a working model of preference formation (Bolton and Ockenfels 2012).

³ Testing for learning and updating in a unified framework is vital to parse between different models of updating. We claim that embedding exogenous variation in information is important because some "behavioral" models of preference formation can be explained instead by models of knowledge acquisition. A model of confirmatory bias, for example, posits that agents might perceive noisy signals to be in support of their priors even if the noisy signals are at odds with their priors (Rabin and Schrag 1999). The empirical prediction of that model is that new information might never alter the initial economic decision of an agent. However, an equally plausible explanation is that the subject never learned the information in the first place and, as a result, no updating could occur. This is one drawback of relying on experiments which estimate ITTs of information treatments. As a result, in order to horserace alternative models a joint test of both learning and updating on preference formation is needed.

heterogeneous preferences and endogenous costly information acquisition decisions similar in spirit to Caplin and Dean 2011 and 3) a timing lag between learning and preference formation.

The remainder of the paper is organized as follows: section two describes the survey and the experimental design in the context of the previous literature. Section three presents results. Section four discusses the results and concludes.

II. Survey, Experimental Design, and Hypotheses

Our experiment has four key components. First, the design allows us to test for how much information respondents possess about the good in question at the outset of the experiment: that is, to measure their ex ante knowledge about the good's attributes. Second, the design also allows us to test how much of the new information provided to respondents is learned. Third, we are able to observe how new knowledge induced by exogenously varying levels of information affect valuations for the good. Fourth, we can use the design to test whether our findings are consistent or inconsistent with several different models of learning and preference formation recently developed in the economics literature. This section describes the design in the context of these components.

Survey

We embedded an experiment within a stated preference survey in Scotland in 2013 in the context of current efforts by local government and the national regulator (the Scottish Environment Protection Agency, SEPA) to improve flood defenses along the Tay estuary in Eastern Scotland. Local councils and SEPA are concerned that current defenses are not sufficient to prevent major flooding episodes, given

changes in the incidence and magnitude of extreme weather events. Residents are also concerned: roughly 45% of people in the area purchase flood insurance.

In considering their options for decreasing the risks of flooding and expected flood damages, one option for regulators is to encourage the conversion of land currently used for farming to restore the estuarine and coastal wetlands which once characterized many of Scotland's east coast firths and estuaries. Such wetlands serve two major roles. For flood protection, wetlands offer a repository for temporary episodes of high tides, and mitigate enhanced flow rates from the upper catchment which may otherwise cause flooding. The degree of flood protection achieved depends on the size of the wetland created. Second, wetlands are a rich habitat for wildlife. As a result, wetlands offer a non-market benefit in the form of increased recreation (wildlife viewing) to the local community, as well as providing a range of other ecosystem services such as nutrient pollution reduction.

We designed a contingent valuation survey to value a single specific wetlands reclamation project in the Tay estuary. In designing the survey, we took significant care to convey general information about the project in a neutral way consistent with Carson (2000). We held an initial focus group to refine both the information and the valuation portions of the survey. This was followed by a pilot survey with potential survey respondents and 50 people responded. The final survey was conducted throughout 2013; survey participants were randomly selected from the Scottish Phone Directory. Only people living within the local authorities affected by the flood defence scheme were selected to take part. The survey was conducted online through a website we designed and operated via Survey Gizmo. Respondents received a letter inviting them to take part in the survey and were given details of the survey website. A reminder card was sent two weeks after the first contact attempt. Subjects who completed the survey were given a £10 (\$16) Amazon gift card. A full copy of the survey is available upon request.

The design of the stated preference survey was as follows. Subjects were first told that their responses would help inform policy-makers improve the management of flooding in their local area. They were then given a nine question multiple choice quiz related to objective information about flooding, flood protection and wetlands. Each question was designed to correspond to different objective attributes of the proposed project, such as the way in which wetlands benefit wildlife and the proportion of homes in the study area which are currently classified as at risk from flooding. We justified the quiz as a way of informing policy makers how well this topic was being communicated to and understood by the community. The quiz was developed with academics specializing in flood risk management to ensure the questions and answers were appropriate and accurate. The set of all nine questions along with answers are in Appendix A. Respondents were then given either three, six or nine pieces of additional information about the attributes of the proposed flood defense strategy. Each piece of information provided to subjects corresponded to a single multiple choice question from the quiz in accordance with the randomized experiment design detailed in the next subsection.

The managed realignment (wetlands restoration) scenario was then detailed, including a map of where the scheme would take place, how many homes would be protected and the length of time before the defences would become active. The status quo scenario of continued reliance on existing hard defences with no managed realignment scheme was also included. The cost of the project was outlined and it was made clear that increases in respondent's council tax would fund the scheme. Council tax was a plausible payment vehicle as local authorities are responsible for funding flood defence in Scotland, whilst council taxes contribute around 20% of local authority budgets. We then elicited WTP for the managed realignment scheme using a payment card ranging from £0 to £150 and respondents were asked to tick all the amounts the household was WTP towards the scheme. The values were chosen based on feedback from initial focus groups. Screen shots of these information slides, the project description and the payment card are in Appendix B. Immediately following the WTP elicitation, respondents repeated the original nine question

quiz. We detail below how comparing the ex ante and ex post test score provides us with pre- and post-survey information levels. A series of debriefing questions followed, including statement questions regarding perceived flood risk, as well as a set of socio-demographic questions. Note that we chose not to ask valuations more than once in our survey because in our view that compromised the external validity of our results. Table 1 shows a summary of our survey's timing.

Table 1: Survey summary

1.	Subject begins survey
2.	Nine question multiple choice quiz
3.	Randomly assigned treatment group (conditional on quiz score)
4.	Managed realignment policy outlined, including costs, timescale and status quo scenario
5.	Respondents receive their additional three, six or nine pieces of information
6.	Elicit WTP for managed realignment scheme
7.	Second quiz
8.	Series of follow up questions regarding flood risk attitudes
9.	Socio-demographic questions

We chose to use a payment card for four main reasons. First, the government was only considering a single policy of a fixed design. As a result, any form of discrete choice experiment was inappropriate, since we were not interested in estimating the relative value of different attributes of the project or different ecosystem service values. Second, despite some recent evidence that repeated choices lead to more consistent stated valuations (Bateman et al. 2008 and Brown et. al. 2008), we note that this evidence does not carry over to our survey, since we do not use a repeated paired comparison (as in Brown et al) or consider a sequence of goods (as in Bateman et al). Third, we chose not to use a binary dichotomous choice (BDC) format due to the inherent noise and lack of statistical efficiency in a BDC format, whilst using a Double Bounded DC format often gives rise to inconsistencies between the initial and second bid response (Bateman et al, 20008). Finally, because the parameter of interest for us is the marginal impact of randomized treatment relative to a baseline (e.g., no new information) on WTP, we do not need to concern

ourselves with the anchoring problems faced by payment cards, since any anchoring effects would be shared by both subjects receiving more information and those receiving less information. Since we are interested in the treatment effect of information provision and not in the aggregate benefits of the project, we do not have to worry about any potential anchoring bias.

Experimental Design

As stated above, at the beginning of the survey, we gave subjects a nine question multiple choice quiz. Each question related to a single piece of information about the public good project. After the first multiple choice quiz, the number of correct answers, the specific questions answered correctly and the specific questions answered incorrectly were recorded for each subject. We then grouped respondents into *a priori* types as a function of the number of correct answers: low (L), medium (M) and high (H). *A priori* type L corresponds to 1-3 correct answers, type M corresponds to 4-6 correct answers and type H corresponds to 7-9 correct answers.

After subjects completed the initial quiz and their answers were recorded, we randomly assigned each subject to a treatment. A treatment in our case was an amount of information about the attributes of the good. Treatments could be low (L), medium (M) or high (H). Each treatment corresponds to a number (3, 6 or 9 for L, M or H respectively) of bullet points and/or figures conveying precise and objective information about the issue (flooding) or good (new coastal wetlands). Each bullet point and/or figure corresponds exactly to one question asked on the multiple choice questionnaire⁴. . As a result, after treatment assignment each agent can be summarized as a type/treatment pair in addition to information

⁴ The complete quiz, an example bullet point slide and the policy description slides are in Appendices A and B.

about their correct and incorrect answers. For example, a type treatment pair could be MH: a subject who answers between four and six questions correctly and who is then given all nine bullet points of information.

Importantly, respondents were always given information they answered correctly first before any additional information was given as dictated by treatment. For example, assume respondent A gets questions 2 and 7 correct and are in the L *treatment*. Respondent A is *type* L since they only got two out of 9 questions correct. The information set that they would then be provided with consisted of two bullet points associated with questions 2 and 7 and, additionally, one information bullet point selected at random from the remaining 7. Alternatively, assume respondent B gets questions 7, 8, and 9 correct and they are in the M treatment. They are *type* L since they scored three out of nine. Their bullet points would be the three bullet points associated with questions 7, 8 and 9 and three randomly chosen bullet points which correspond to questions 1 through 6.

The reason for not randomly selecting information is that we are concerned with the marginal effect of *new* information on learning and preference formation. In order for the experimental design to be valid, we must make sure that, on average, a type-treatment pair of LL is the proper counterfactual for type-treatment pair LM. If the information treatment does not span the agent's a priori information set (e.g., an individual's type), then the proper counterfactual cannot be ensured. Specifically, imagine the situation above in which respondent A gets questions 2 and 7 correct but their L treatment are bullet points associated with questions 3, 4 and 5. In that case, respondent A could test as a type M ex post when their information set is elicited later in the protocol.

Table 2: Type Treatment Pairs. Columns of the table represents the groupings (L, M, H) by the first test score and rows represent alternative treatments. To focus on the effect of new information we never treat subjects with less information than their ex ante knowledge.

Ex Ante Information Treatment	L	M	H
H	LH	MH	HH
M	LM	MM	--
L	LL	--	--

One useful way to represent the type-treatment pairs and treatment information sets is shown in Table 2. Columns represent the types (L, M, H) defined by the a priori test score, and rows represent the groups based upon treatment. In general, there are up to nine potential type-treatment pairs. It is important to note, however, that some of these pairs may be uninformative. For example, if someone has a high information level ex ante (type H) then they will learn no new information when given the low treatment. Alternatively, if someone has a low information level ex ante (type L) then they could learn new information when given the high treatment and subsequently have any ex post information level (L, M, or H). We therefore restrict ex ante H information types to receive only H information treatments and ex ante M information types to receive only M and H treatments to maximize the power of the experiment and focus on the effect of additional information.

After the quiz and the information treatments as stated above, subjects were all given identical background information about the flooding issue and coastal wetland creation project, the location of the floodplain and prospective new wetland, the possible flood mitigation benefits and the potential cost of the policy. At this point all agents were asked to select their maximum willingness to pay for the good. Finally, each agent was given the exact same quiz as at the beginning of the survey. Thus, at the end of the survey each respondent in a treated group is summarized by an initial set of quiz answers (a priori

information set), a type-treatment pair, a treatment information set (bullet points), a WTP response, and a second set of quiz answers (ex post information set).

An important contribution of our paper is that the novel experimental design provides us to opportunity to verify that information is actually learned. One cost of this design, though, is that we must give subjects a quiz before eliciting willingness to pay. Taking a quiz is admittedly uncommon for subjects before valuing a good. This external validity concern, though, is the cost of cleanly verifying that information provided was indeed learned. Furthermore, the external validity concern is only valid if taking a pre-survey quiz fundamentally alters the role of information and knowledge in a survey. We view this as possible but unlikely. To reiterate, we are primarily focused with the marginal impact of information and knowledge on the mean and variance of the WTP distribution rather than the absolute level of WTP.

Hypotheses

Combining the initial quiz, the information treatments and second quiz allows us to test for how subjects learn and what information processing procedure they are using in forming their valuation of a good. This subsection shows how our design allows us to compare different models of preference formation in stated preference surveys.⁵ Appendix D provides a more detailed treatment of these hypotheses.

Understanding how agents respond to information in stated preference surveys is important. The economics literature finds an increasing number of alternatives to a “naïve” model of costless learning and

⁵ New information could also change preference (taste) for some attribute (and so the mean of random utility function coefficient associated with this attribute) or change variance of the taste for this attribute. Put another way, new information could result in changing standard errors of a random utility model. It could also influence many preference parameters, possibly in different ways. For example, it could simultaneously change variance of all parameters in the same way, or influence the utility function error term – the scale parameter – so that choices become more / less random if scale is heterogeneous in the population. While these are important issues, in the current paper we restrict our analysis to updating behavior and effects on simple mean WTP for the mixed good and leave these issue to future work.

complete information retention. For example, models of bounded rationality, costly learning, fatigue, and cognitive load all imply agents do not completely adsorb new information (Sims 2003, Gabaix et. al. 2006, Caplin and Dean 2011, and Caplin and Dean 2015). Learning is important in our context because learning can affect a subject's valuation for a good once learned information has been processed. However, it is unclear exactly how learning about a good's attributes aligns with "neutral" information processing (Tversky and Kahneman 1973, Tversky and Kahneman 1974, Grether 1980 and Gabaix et. al. 2006) versus deviations from it (Rabin and Schrag 1999, Eil and Rao 2011, Grossman and Owens 2012, and Fernback et. al. 2013).

Our study is designed to both identify the causal effect of knowledge on preferences and parse between different models of preference formation in stated preference surveys. To identify how knowledge is created, its effect on preferences, and to parse between different models of preference formation, we estimate the following two equations separately defining treatment coefficients to convey the marginal impact of treatment on each outcome of interest:

$$Q2\ Score_i = X_i'\gamma + 1\{LL_i, LM_i, LH_i\}\Gamma_{LL} + 1\{LM_i, LH_i\}\Gamma_{LM} + 1\{LH_i\}\Gamma_{LH} + 1\{MM_i, MH_i\}\Gamma_{MM} + 1\{MH_i\}\Gamma_{MH} + 1\{HH_i\}\Gamma_{HH} + \varepsilon_i \quad (1)$$

$$WTP_i = X_i'\gamma + 1\{LL_i, LM_i, LH_i\}\omega_{LL} + 1\{LM_i, LH_i\}\omega_{LM} + 1\{LH_i\}\omega_{LH} + 1\{MM_i, MH_i\}\omega_{MM} + 1\{MH_i\}\omega_{MH} + 1\{HH_i\}\omega_{HH} + \varepsilon_i \quad (2)$$

Equations (1) and (2) include a vector X of self-reported subject specific demographic characteristics.⁶ In equations (1) and (2), as before, the two capitals stand for the ex-ante score and the

⁶ In the first specification controls act to verify that assignment is random. Put another way, the average effect of additional information on scores (e.g., the various treatment effects) should not be affected by demographic control variables. Conversely, when estimating the effect of WTP on the controls, it could be the case that the effect of additional information on willingness to pay could vary systematically with demographic characteristics. If those demographic characteristics are also correlated with preferences for the good, then adding in controls could affect the estimated coefficients of treatment on WTP.

information treatment respectively (e.g., the treatments in Table 1). There are two left hand side variables which we consider separately. The first is *Q2 score*; it is the score respondents achieve in the second quiz they complete, and thus measures ex post knowledge of the good. The *Q2 score* specification therefore measures actual learning that occurs conditional on ex ante information levels and treatment. Each coefficient Γ_{JN} measures the marginal effect of being treated with additional information on second quiz scores for subjects who are in ex ante information knowledge group J when being in the N information treatment. For example, Γ_{LH} is the marginal effect on second quiz scores of being presented with three additional information bullet points relative to being presented with the M information treatment but being in the L ex ante knowledge group.

The second equation is defined similarly with stated willingness to pay (*WTP*)⁷ conditional on ex ante information levels and treatment. For example, ω_{LH} is the marginal impact of being supplied with three additional objective information points on a subject's stated valuation relative to receiving the M information treatment (e.g., nine versus six total bullet points).

We estimate both regressions by OLS with robust standard errors. We also estimated the equations using interval regressions and fit both a normal and spike distribution to the WTP data to estimate both the mean and variance of treatment's impact on WTP. Results are consistent across specifications so we focus here on the OLS estimates, since our primary interest is the marginal impact of information on knowledge and of knowledge on WTP, rather than on WTP levels *per se*. OLS is the simplest method to construct this conditional different of means. We also run an instrumental variables regression using information treatments as an instrument for knowledge in order to estimate the causal impact of knowledge on WTP. We describe that econometric model below.

⁷ In what follows, we adopt a simple, non-parametric approach to estimating respondents' WTP – we conservatively assume that their WTP is equal to the lower bound of the selected payment card interval (i.e., we use Kaplan-Meier estimator of WTP). Neither using mid-points of the selected intervals, nor adopting parametric approach to estimating sample-level WTP (presented towards the end of the paper) qualitatively changes the results.

By jointly analyzing the coefficient estimates of equations (1) and (2) we can evaluate whether the results of our experiment is consistent or inconsistent with different models of learning and preference formation. For example, estimating $\Gamma_{LM} = \Gamma_{LH}=0, \Gamma_{MH}=0$ jointly with $\omega_{LM}=0, \omega_{LH}=0, \omega_{MH}=0$ implies that we fail to reject the null hypotheses that no learning and no preference updating takes place. Alternatively, estimating $\Gamma_{LL} + \Gamma_{LM} = \Gamma_{MM}, \Gamma_{LL} + \Gamma_{LM} + \Gamma_{LH} = \Gamma_{MM} + \Gamma_{MH} = \Gamma_{HH}$ and $\omega_{LL} + \omega_{LM} + \omega_{LH} = \omega_{MM} + \omega_{MH} = \omega_{HH}, \omega_{LL} + \omega_{LM} = \omega_{MM}$ means that we fail to reject a null hypothesis of complete learning and valuation updating completely dependent on attribute information. Appendix D details several alternative models consistent with alternative sets of coefficient estimates.

III. Results

4000 questionnaires were distributed to homeowners within the areas potentially affected by the managed realignment scheme. 749 people completed the online survey with 593 usable (fully completed) responses: a response rate of 15%. The response rate is acceptable and comparable with a similar UK wide stated preference survey for flood defense (12%) (Rotimi et al 2015) , as well as other UK postal stated preference surveys (Mentzakis et al 2014; Burton et al 2001; Hanley et al 2010) that had response rates ranging from 11 % to 22%.

Table 3 shows the distribution of Type–Treatment pairs in our sample. Only twelve subjects scored 7, 8 or 9 on the first quiz. As a result, there are only 12 *a priori* type H subjects meaning there are only 12 subjects in the HH treatment. We over sampled from the LL type-treatment group in order to balance the power in estimating treatment effect relative to the information treatments most commonly found in the field (e.g., type L ex ante types).

Table 3: Socio-demographic comparisons between the type-treatment pairs

	Type - Treatment Pairs					
	LL	LM	LH	MM	MH	HH
Income						
Under £15,000	18%	16%	2%	16%	11%	22%
£15,000-£19,999	13%	13%	13%	12%	14%	0%
£20,000 - £39,999	24%	23%	38%	41%	41%	33%
£40,000 - £69,999	27%	26%	33%	20%	20%	33%
£70,000 - £99,999	10%	11%	7%	9%	9%	11%
Over £100,000	8%	10%	7%	2%	5%	0%
Education						
Secondary school	24%	23%	15%	20%	19%	22%
Sixth form/college	24%	18%	26%	26%	25%	11%
Undergraduate degree	22%	29%	34%	28%	29%	11%
Post-graduate degree	29%	30%	26%	26%	27%	56%
Economic activity						
Employed	58%	70%	73%	59%	69%	40%
Unemployed	42%	30%	27%	41%	31%	60%
Property status						
Property owner	76%	79%	84%	89%	87%	90%
Other	24%	21%	16%	11%	13%	10%
Gender						
Female	46%	34%	35%	43%	42%	60%
Male	54%	66%	65%	57%	58%	40%
Age						
18-29	8%	11%	16%	6%	11%	10%
30-39	17%	18%	24%	9%	11%	0%
40-49	16%	18%	23%	22%	19%	10%
50-59	26%	24%	16%	25%	26%	30%
60-64	8%	9%	10%	13%	8%	10%
65 and over	26%	20%	11%	25%	25%	40%
Observations	151	78	72	97	94	12
Note: Socio-demographic information by treatment status						

Table 3 also reports socio-demographic characteristics by treatment type. Characteristics of the local authority populations revealed our sample was not fully representative of the local population in a few ways. The age groups 40- 49 years, 50-59 years and 65 and over were well represented in the survey whilst the youngest age group (18-29) was under represented (9% of sample compared to 22% in population). Males

were also over represented in the survey (58% compared to 47%). 63% of respondents worked full time compared to 50% of the overall population. The modal income group was £20,000–£39,000 which was similar to the median income of the local authorities (£26,000). Over 80% of the sample owned their own homes compared to local authority average of 64%. Importantly, because our main goal in this paper is estimating average treatment effects rather than the population’s willingness to pay, differences between sociodemographic characteristics in our sample relative to the population are of second order importance.

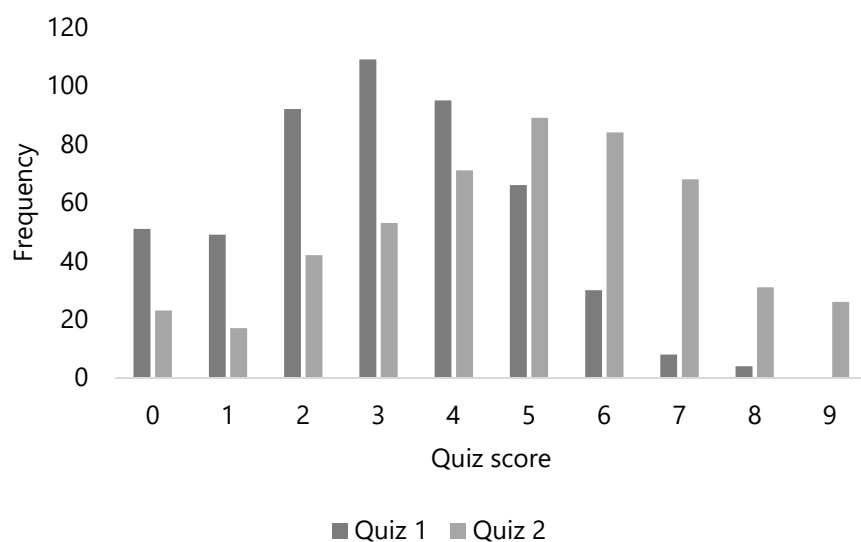


Figure 1: Comparison of the first and second quiz scores

Treatment and Learning

Figure 1 compares the first and second quiz scores of subjects. Comparing the first and second quiz scores it is clear that subjects scored significantly more on the second quiz compared to the first quiz (mean quiz one = 3.05, $S.E = 0.08$ and mean quiz two = 4.86, $S.E = 0.10$). This is evidence that respondents were less informed about the good and the project before the survey, relative to after.

Table 4 shows the coefficient estimates of regression (1) with Second Quiz Score as the dependent variable and information treatment groups as independent variables. LL, MM and HH are defined as mean quiz 2 scores and LM, LH, and MH defined as the marginal impact of additional information on second quiz score. To highlight treatment effects, we exclude a constant in this regression specification.⁸ We report four specifications: the full sample with and without a host of self-reported demographic controls like education, sex, age, etc.; and only individuals who self-reported perceiving their responses as being consequential to the likelihood of the wetlands project going ahead, again with and without controls. In each specification, the control variables do not significantly alter the estimated treatment effects. We take this as evidence that we properly randomized treatment.⁹

Table 4: Second quiz score on type-treatment pair

Variables	(1)	(2)	(3)	(4)
LL	3.530*** (0.0562)	4.058*** (0.464)	3.429*** (0.105)	5.105*** (0.546)
LM	0.945* (0.442)	1.290*** (0.125)	1.096* (0.481)	1.316** (0.289)
LH	0.484 (0.324)	0.555 (0.282)	0.0375 (0.465)	0.258 (0.386)
MM	5.402*** (0.148)	6.064*** (0.509)	5.132*** (0.243)	6.716*** (0.769)
MH	0.875*** (0.133)	0.914*** (0.122)	1.003*** (0.147)	1.076*** (0.183)
HH	8.167*** (0.208)	8.622*** (0.107)	8.143*** (0.253)	9.424*** (0.279)
Observations	504	431	247	179
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	0.867	0.886	0.877	0.918

Robust standard errors in parentheses. ***, **, * represent significance at 1%, 5%, 10% level, respectively. Dependent Variable is second quiz score. LM, LH and MH are defined as the marginal effect of additional information. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, and environmentalist. Columns (3) and (4) include only individuals who did not perceive results as being inconsequential. To highlight treatment effects, we exclude a constant in this regression specification.

⁸ Also as before, some observations are dropped when control variables are included since some subjects chose to not respond to questions about where they lived and their level of education.

⁹ Balancing tables available upon request confirm we randomized properly.

Table 4 has two key features. First, within every specification providing more information to subjects increases retained information (knowledge). Some of these increases, though, are not statistically significant (see below). Second, the rate of information retention varies somewhat across specifications. However, the pattern of decreasing retention as more information is provided (e.g., LH coefficient smaller than LM coefficient) is consistent across specifications.

Turning to the hypothesis tests for learning, we can reject the hypothesis that no learning occurs. In each specification, the estimated coefficients on LM and MH are significantly different from zero. Similarly, we can reject the null hypothesis that subjects exhibit complete retention: the coefficient on LH is not statistically different from zero. Further, the point estimates for the coefficient on LM and MH indicate that subjects retain between 1 and 1.3 pieces of information for each three new pieces of information given for “low levels” of new information (e.g., three new pieces) but only .04 to .26 pieces out of three for “large levels” of new information.

As a result, we fail to reject the null hypothesis of both incomplete learning and fatigue. The marginal ability of subjects to learn new information is clearly decreasing in the volume of new information provided in every specification. It is also clear from the coefficients on LL, LM and LH that information monotonically increases scores (similarly for MM and MH). We take this as evidence that our information treatments cause subjects to learn, but that learning is incomplete. This is evidence that the experimental design for the causal effect of not just information, but also learning on WTP for the public good is valid.

Treatment and Willingness to Pay

Table 5 shows summary statistics for WTP levels by treatment status. 83% of the sample were willing to pay towards the managed realignment scheme with a sample mean willingness to pay of £44.77

per annum ($S.D = 46.21$). The self-stated main reason for respondents not being willing to pay any amount was not being able to afford to contribute (26%).

Table 5: Comparison of willingness to pay across the type-treatment pairs

Type - Treatment Pairs	Percent of Zero Bids	Median WTP	Mean WTP	Standard Deviation	Observations
LL	19%	20	45.17	48.12	151
LM	14%	45	51.47	48.51	78
LH	19%	20	42.01	46.52	72
MM	19%	20	37.99	39.67	97
MH	13%	30	47.66	48.51	94
HH	0%	45	45.00	34.18	12
Total	17%	30	44.77	46.21	504

NOTE: Includes all completed surveys.

Table 6: Valuation on Treatment

VARIABLES	(1)	(2)	(3)	(4)
LL	45.17*** (4.765)	67.82*** (3.745)	58.33*** (4.790)	64.17*** (6.766)
LM	6.309 (6.040)	-3.368 (6.992)	0.167 (8.532)	-9.236 (8.720)
LH	-9.460** (2.264)	-10.55*** (2.143)	-2.719 (8.169)	-5.920 (8.491)
MM	37.99*** (7.170)	57.22*** (3.238)	36.89** (10.63)	37.08*** (5.842)
MH	9.670 (8.143)	7.592 (5.049)	13.21 (14.53)	7.810 (11.35)
HH	45** (12.51)	59.65* (21.69)	37.14* (16.17)	24.03 (13.82)
Observations	504	431	247	179
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	0.489	0.617	0.537	0.717

Robust standard errors in parentheses. ***, **, * represent significance at 1%, 5%, 10% level, respectively. Dependent Variable is stated valuation. LM, LH and MH are defined as the marginal effect of addition information. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, and environmentalist. Columns (3) and (4) include only individuals who did not perceive results as being inconsequential.

Table 6 shows the coefficient estimates of regression (2) with WTP as the dependent variable and treatment group as independent variables. We report four specifications: the full sample with and without controls and the subset of subjects self-reporting their responses as being consequential with and without controls. In all cases, demographic controls have the expected sign. For example, environmental group members and subjects owning flood insurance were both willing to pay more for restored wetlands; as a result, we don't report those estimates in this paper to focus attention to treatment effects.

There are three interesting aspects of Table 6. First, there are some differences in stated valuations by whether subjects said that they believed the survey would be used for policy decisions or not. In some cases there are level differences (e.g., the MM and LH coefficients in specification (2) and (4)). In all most cases the standard errors increase as well (e.g., LL standard error in (2) versus (4)). The standard error result is expected given the decrease in sample size. The literature on stated preference surveys notes that stated preference elicitation is only reliable if the surveys are viewed as consequential by subjects (Carson, Groves and List 2014, Vossler et. al. 2012, Vossler and Evans 2009 and Carson and Groves 2007). We find some evidence here that belief in consequences changes stated WTP.

Second, the mean WTP varies by ex-ante levels of information. For example, people in the LL group had significantly higher valuations than those in the MM group (p-value for significant difference in specification 4 less than .01). Similarly, we reject the null hypothesis that $\omega_{LL} + \omega_{LM} = \omega_{MM}$ at 5% and 10% levels for specifications (3) and (4). We fail to reject the high information analog though (e.g., fail to reject $H_0: \omega_{LL} + \omega_{LM} + \omega_{LH} = \omega_{MM} + \omega_{MH} = \omega_{HH}$). While we fail to reject the full information hypothesis, this is possibly due to imprecise estimates. This result is consistent with the literature that ex ante information and experience levels with a good are correlated with WTP (Cameron and Englin 1997, Loomis and Ekstrand 1998 and Tkac 1998).

Third and most important for our study, the marginal effects of information on stated WTP, which we confirmed becomes knowledge at imperfect but statistically significant positive rates, is not statistically

different from zero in every specification. The one exception is that there is a significant and negative marginal effect of the three pieces of information in the LH treatment which we verify above *were not learned* for specifications (1) and (2). This result implies large amounts of unlearned information lead to lower stated WTP. Taken at face value, it could be that subjects take the additional information as evidence they are not informed and that uncertainty about the accuracy of their information set affects valuations, similar to LaRiviere et. al. (2014). However, the literature on stated preference surveys finds that stated preference elicitation is only incentive compatible if the surveys are viewed as consequential by subjects (Carson, Groves and List 2014, Vossler et. al. 2012, Vossler and Evans 2009 and Carson and Groves 2007). As a result, we disregard this single difference between specifications (1) and (2).¹⁰

Learning and Willingness to Pay

Lastly, our experimental design gives us the ability to test for the causal effect of learning on WTP directly. We are not aware of another study which has this feature. Because we observe what a subject knew before the treatment, exogenously provide information, elicit WTP and then observe what the subject knew ex post, we can both observe learning and then relate observed learning to observed differences in stated WTP. The normal confounding factor in this analysis is that subjects who knew less to begin have a greater opportunity to learn. However, we can control for the number of new pieces of information each subject sees. Due to our experimental design, then, we can get around this problem.

Figure 2 shows the correlation between exposure to new information and learning new information and the correlation between learning new information and WTP in panels (a) and (b) respectively. We define a variable called *New Info Bullets* which is defined as the number of new pieces of objective information shown to a subject. For example, if a subject answered four questions correctly on the first quiz and was

¹⁰ We note, though, that, our study on the coefficient estimate on LH concerns the marginal effect of knowledge on preferences, not the strength of preference or the valuations associated with them. To that end, the coefficient on LH estimates the marginal impact of additional information on stated valuation. Issues raised by lack of consequentiality are differenced out by the LL coefficient in our study,

assigned to the M information treatment, they would be exposed to two new pieces of information. We also define a variable called *Info Bullets Learned* which is defined as the number of new pieces of information which the subject learned. Put another way, *Info Bullets Learned* is the number of correctly answered questions on the second quiz which they subject both didn't correctly answer on the first quiz and which was subsequently provided in an information bullet. (This lets us be certain the subject learned the bullet point due to the information presented as opposed to guessed the correct answer on the second quiz randomly). Hence, *Info Bullets Learned* is less than or equal to *New Info Bullets* by definition. Lastly, the ratio of *Info Bullets Learned* to *New Info Bullets* we call the "retention ratio".



Figure 2: Opportunity to Learn, Learning and Willingness to Pay. Data is jittered to show density.

Panel (a) confirms the findings about learning and updating: despite a couple of subjects who are outliers there is a clear positive relationship between being treated with new information and learning. The relationship, though, between WTP and learning is less clear. If anything, it appears there is a negative relationship between learning and WTP. However, panel (b) does not control for ex ante information levels: for example, the subjects who learn more information could more likely to have less ex ante information as

well. Our analysis controls for this artifact breaking the marginal effect of learned information into ex ante level of information bins.

We estimate the effect of learning on WTP directly in 3 different ways. First, we estimate an instrumental variables regression using the random assignment of information treatments in the experiment as an instrument for knowledge. The learning results above show that the instrument is relevant and random assignment in the experiment gives us a valid instrument which meets the exclusion restriction. In each IV regression the first stage results are highly significant. Second, we estimate WTP by how much information different subjects had an opportunity to learn given treatment. Third, we estimate WTP by how much additional information subjects were shown that they did *not* learn. This final approach informs whether subjects exhibited free disposal of information.

We use two different IV specifications: first, we restrict the estimating sample to be the ex ante low knowledge group. Second, we restrict the ex ante medium knowledge group. In neither case do we use any information on the ex ante H information group: recall we are interested in the effect of new knowledge formation on WTP. The information treatments provide exogenous variation in new knowledge created only in the ex ante L and M groups. That combined with a small sample in the HH group cause us to not use the HH group data in this analysis. We do not pool the ex ante L and ex ante M types, condition on quiz one scores and use the different information treatments as instruments. That specification would assume the marginal impact in knowledge is identical across subjects. This would be an unwise assumption since the LL and MM treatment groups exhibit significantly different stated valuations. We can provide the pooled results upon request but they are consistent with contamination from the ex ante knowledge valuation differences.

Table 7: IV regressions of Valuation on second quiz score (Ex Ante Low)

	(1)	(2)	(3)	(4)
Quiz 2 Score	-0.132 (4.491)	-2.781 (3.824)	-0.979 (7.980)	-6.683 (6.534)
Constant	46.59** (18.69)	43.57** (18.73)	61.71* (32.40)	67.22** (34.08)
Observations	301	258	135	94
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	0.001	0.061	0.003	0.115

Standard errors in parentheses. ***, **, * represent significance at 1%, 5%, 10% level, respectively. Dependent Variable is stated valuation. LL, LM and LH are first stage instruments. Constant term indicates the mean WTP for ex ante low types in the LL treatment. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, and environmentalist. Columns (3) and (4) include only individuals who did not perceive results as being inconsequential.

Table 8: IV Valuation on Treatment (Ex Ante Medium Knowledge)

	(1)	(2)	(3)	(4)
Quiz 2 Score	11.06 (7.900)	8.243 (7.199)	13.18 (9.622)	11.36 (8.528)
Constant	-21.74 (46.21)	-15.38 (43.53)	-30.73 (54.37)	-55.15 (49.29)
Observations	191	169	105	84
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	-0.163	-0.001	-0.232	0.118

Standard errors in parentheses. ***, **, * represent significance at 1%, 5%, 10% level, respectively. Dependent Variable is stated valuation. MM and MH are first stage instruments. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, and environmentalist. Columns (3) and (4) include only individuals who did not perceive results as being inconsequential.

Tables 7 and 8 show the IV estimation results of the ex ante low and ex ante medium knowledge groups respectively. For both groups and in every specification, the causal impact of knowledge on valuation is not statistically different from zero. We take this as complimentary evidence that there is no knowledge based preference updating.

In Appendix C we supplement these finding with estimation results which allow for valuations to vary with both the number of new pieces of information learned and the amount of new information which is unlearned across different treatment groups. In those specifications we find that in no case did extra

learned or unlearned information significantly impacts valuations (e.g., we observe free disposal of information for all ex ante knowledge levels).

Parametric estimation of Willingness to Pay

While there is no evidence in our experiment that treatment affected mean valuations, it is possible that treatment could have affected the variance of the distribution of valuations. Czajkowski et. al. 2015a and Czajkowski et. al. 2015b show that in both Bayesian and non-Bayesian models of updating, additional knowledge about a good can affect the variance in addition to the mean of WTP distribution. In either a Bayesian or non-Bayesian model, more information serves to move agents closer to their fully informed valuations, even though in some models like costly attention or confirmatory bias, agents are less likely to change these valuations.

We perform a joint test of treatment on the variance and mean of the WTP distribution. To do so, we use individual interval WTP responses to fit several parametric distributions of WTP.¹¹ We calculated the CDF of the selected distribution at the upper bound of each individual's WTP and subtracted the CDF evaluated at the lower bound of their WTP.¹² This is simply the probability that the individual's WTP is in the range described by the lower and upper bound, indicated by the selected bid and the next highest bid (conditional on the parametric distribution sample-level parameters), and is this individual's contribution to the likelihood function. By adding up the individual contributions and maximizing the resulting function with respect to the distribution parameters, we effectively use maximum likelihood method to fit a parametric distribution to our interval data.

¹¹ Recall that the survey elicited respondents' WTP using the payment card approach. Respondents were asked to select the maximum bid which they would be willing to pay. This reveals that their WTP is equal or higher than the selected bid and lower than the next higher bid (not selected), effectively providing the information about the interval in which respondents' true WTP is.

¹² In the case the numerically calculated difference of the CDFs was 0, we took the PDF of the distribution evaluated at the lower of the bounds.

Formally, let LB_i and UB_i indicate individual i 's lower and upper bounds of WTP, respectively. This individual contribution to the likelihood function, conditional on an assumed parametric WTP distribution described by a known CDF is:

$$L_i = \Pr(LB_i \geq WTP_i > UB_i) = CDF(UB_i, \beta_i) - CDF(LB_i, \beta_i), \quad (1)$$

where β_i is a vector of distribution parameters (e.g., mean and standard deviation for the normal distribution) which can be made individual-specific by making them functions of individual-specific characteristics.¹³

Table 9: Treatment-specific estimates of the mean and standard deviation of the Gamma distributed WTP

	(1)		(2)		(3)		(4)	
	Mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.
LL	52.82***	67.10***	63.48**	74.92***	67.66***	77.96***	137.54***	76.16***
	(5.89)	(8.45)	(25.38)	(25.98)	(11.25)	(14.37)	(50.95)	(15.02)
LM	5.92	-0.19	-5.06	-3.21	-2.29	-9.51	-39.35	-15.33
	(10.31)	(13.83)	(11.79)	(7.68)	(17.28)	(20.56)	(37.42)	(11.17)
LH	-11.47	-5.67	-2.84	-1.97	-3.61	6.78	-6.41	-2.94
	(11.71)	(15.65)	(11.49)	(7.74)	(20.23)	(24.08)	(5.68)	(2.30)
MM	42.24***	50.07***	66.59**	77.22***	40.00***	47.61***	87.22**	56.07***
	(5.46)	(7.20)	(27.51)	(26.97)	(7.03)	(8.89)	(38.57)	(15.38)
MH	13.63	16.24	14.31	9.31	16.79	14.91	10.68	5.30
	(9.32)	(12.46)	(11.42)	(7.10)	(12.40)	(15.09)	(10.58)	(4.55)
HH	42.69***	35.42***	76.36**	85.33***	33.87**	33.93***	167.23***	82.37***
	(12.97)	(9.49)	(36.51)	(29.83)	(14.33)	(11.20)	(51.48)	(16.63)
Observations	504		432		247		179	
Controls	N		Y		N		Y	
Consequential Sample Only	N		N		Y		Y	
AICc/n	5.4233		5.2861		5.5929		5.4433	

Standard errors in parentheses. ***, **, * represent significance at 1%, 5%, 10% level, respectively. LM, LH and MH are defined as the marginal effect of addition information. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, and environmentalist. Columns (3) and (4) include only individuals who did not perceive results as being inconsequential.

Because there is no a priori or theory driven information about what the distribution of WTP in the population is, we tried over 20 most commonly used parametric distributions to see which one fits our data

¹³ The models were estimated using a custom code developed in Matlab, available from <https://github.com/czaj/DistFit> under CC BY 4.0 license.

best. Appendix E provides the summary of the comparison. We found that, of the distributions we tried, the gamma distribution provided the best fit, in terms of the lowest finite sample corrected Akaike Information Criterion level.¹⁴

Using the estimated parameters of the Gamma distribution we then simulated the mean and standard deviation of the WTP distribution. The simulation was based on the Krinsky and Robb (1986) parametric bootstrapping technique. Using the vector of the estimated coefficients and the variance-covariance matrix we took 10^4 multivariate normal draws, and for each of the draws we drew 10^4 points from the gamma distribution. These are then used to derive moments of the distributions, while step 1 draws provide the estimates of the standard errors of these moments. The results are provided in Table 9.

Consistent with our OLS findings, we do not find an effect of addition information on mean WTP. What Table 9 adds is the variance results. In each specification there is no impact of additional information on the variance of willingness to pay. This result is somewhat surprising: even though addition information does not impact mean WTP, a model of Bayesian updating suggests that addition information should tighten the distribution around an (unchanging) mean (Czajkowski et. al. 2015a and Czajkowski et. al. 2015b). This non-result is inconsistent with a model of Bayesian updating, even if type L or M subjects would update around an unbiased (albeit imperfectly informed) mean WTP. As a result, this finding is inconsistent with a model of heterogeneous preferences with Bayesian updating and costly search. As we discuss in the next section, it is also not a smoking gun for any non-Bayesian updating model. While a puzzle, we urge caution as this is a single study's result insofar as knowledges lack of effect on updating in a single elicitation survey.¹⁵ Replication in other contexts is needed.

¹⁴ We used AICc instead of comparing the LL values, because the distributions could differ in the number of parameters.

¹⁵ A study with the same design but with a choice experiment (CE) elicitation format would be better suited to address that question. A payment card only offers a single observation per subject whereas a CE allows for a multiple observations between subjects. As a result, a CE format is better suited to estimate the signal to noise ratio across subjects in different treatments (e.g., the scale parameter). This is an intriguing line of future research.

Endogeneity concerns

Even within our design in which information treatments are exogenous there could be a potential concern of endogenous effort related to learning. Assuming that learning is costly, if a subject cares about a topic it could be that they are willing to use more effort in order to retain information provided about that topic in the course of a survey. As a result, the estimated effects of both learning and excess information on WTP could still be considered to be endogenous.

The experimental design allows us to address this issue by using the randomly assigned information treatment as an instrument for both learning and excess information while conditioning on ex ante information levels. We propose the following Instrumental Variables approach: the first stage uses groupings of the number of newly provided information bullets from regression (3) as an instrument for learning. We then take the predicted value of bullets learned and use it as the independent variable of interest in the second stage regression. We do this for the entire sample in addition to breaking the sample into subjects who were only in the ex-ante L information grouping.

We do not report the regression output here but the IV regressions show very imprecisely estimated zeros for both learning and excess information. This result is robust to restricting the sample to only subjects in the ex-ante L information grouping. When we restrict the sample to only include subjects who stated they are confident the survey will be used to inform policy, the IV point estimates increase to average more than zero but they stay insignificant.

IV. Discussion & Conclusion

This paper reports the results from a novel experiment to identify the causal effect of learning and knowledge about an environmental good's attributes on subjects' valuation for that good. We designed an experiment which identifies ex ante knowledge levels, exogenously varies information provided to

subjects, elicits valuations for the good using stated preference methods, and finally identifies ex post knowledge levels. The results for learning show that providing subjects with more new information causes significantly more learning in subjects. However, we find that observed learning is incomplete. We also find the likelihood that a subject learns a piece of new information decreases as the subject is presented with increasing amounts of new information, a result which is consistent with models of fatigue. Our findings therefore suggest that learning is imperfect and varies with the amount of new information presented.

The results of the valuation portion of the experiment show that exogenous increases in knowledge about the good's attributes (both increased flood protection and increased wildlife abundance) did not alter subjects' valuation for the good; our evidence is thus consistent with an absence of knowledge-based preference formation within a survey. Information learned also has no significant effect on the variance of stated WTP. Ex ante knowledge, however, matters a great deal to the value people placed on the wetlands project.

The learning results we find, coupled with the valuation results, are consistent with three different models of updating and preference formation: one consistent with neutral information processing and two which depart from the neoclassical model. The first possible model is a neoclassical model of costly search similar in spirit to Caplin and Dean (2011): agents use costly effort to seek out and learn information up to the point where the expected marginal cost of learning is less than the expected marginal benefit.¹⁶ If endogenously acquired knowledge about "good" attributes are valued according to underlying heterogeneous preferences, the provision of additional knowledge will not affect pre-existing valuation levels. Furthermore, ex ante levels of knowledge could easily correlate with valuations in a systematic way:

¹⁶ Our results are somewhat different in that we study information gathering rather than choice sets.

for example, people in the flood plain may both know more about the flood mitigating potential of wetlands and be willing to pay more for wetland restoration.¹⁷

Second, our results are consistent with imperfect learning coupled with confirmation bias similar to Rabin and Schrag (1999). Importantly, this interpretation requires that we tested for both learning *and* changes in valuations. Without verifying that learning occurs- even incomplete learning- it is possible that subjects have no opportunity to update valuations, because the differing levels of information embedded in different information treatments were not actually learned.¹⁸

Third, it could be that learning can occur instantaneously but preference formation takes time. Given that our experiment takes place over 10-25 minutes, we are not able to identify any changes in valuation which may take longer to evolve. While we are not aware of any models of preference formation with this dual timing feature, it is one possible explanation for our findings. Indeed, this explanation would be consistent with recent literature related to the difficulty of identifying a causal impact of advertising on internet purchases (Lewis and Rao 2015 and Blake et. al. 2015).

Understanding the causal impacts of knowledge on economic decisions is very important: firms and governments spend large sums of money to educate the public about the benefits and costs of different goods, services and actions. Because our results are consistent with three different models of preference formation, there are several possible welfare implications. If models of confirmatory bias or endogenous search with heterogeneous preferences are driving our results, large scale information campaigns could still be effective but for reasons associated with choice set salience (Caplin and Dean 2015). Alternatively if there is a time lag between knowledge formation and preference formation large information campaigns

¹⁷ This type of model also bears some similarities to other recent models of costly attention (Caplin, Dean and Martin 2011, Hanna et. al. 2014, and Schwartzstein 2014).

¹⁸ Similar models have recently been used to explain some individual's reluctance to believe scientific evidence for climate change.

could have large, albeit delayed effects. This second explanation is consistent with recent work which has difficulty identifying a causal link between online advertising and sales in the short run (Lewis and Rao 2015 and Blake et. al. 2015). More research parsing between these models is therefore needed.

We urge some caution in interpreting our findings, as further research is needed to identify their robustness and portability. The external validity of our findings outside of a stated preference context is also uncertain. It is possible that in relatively low stakes micro level decisions, economic actors deviate from decision rules used in other circumstances. Decisions made with higher stakes and by experienced decision makers must be evaluated as well. Further, more experimental designs are needed to parse between the three candidate models of preference formation updating we sketch in this paper. Lastly, our findings don't imply that stated preference surveys are in some way flawed: the same biases we observed in our experiment have been observed in the lab and with revealed preference data. That said, the main implication of our study for stated preference design is straightforward: new information provided to respondents in a survey does not appear to affect the mean or the variance of the WTP distribution. Rather, it is respondents' *ex ante* (pre-survey) level of knowledge which affects the value they place on the environmental good.

Finally, respondents learn some of the information provided to them, but the marginal effects of this information are declining. Furthermore, it appears that the marginal value of moving from a small to a large amount of new information is zero since our subjects did not learn such information. Thinking more deeply and in line with more recent work in economics, our results imply that survey respondents could exhibit some of the same "behavioral" updating rules recently discussed in the literature. Similar mechanisms developed to address those issues in revealed preference markets could be useful in stated preference markets as well.

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Appendix A: Quiz

A Short Questionnaire

Please answer the following nine questions about flood defence and the Tay Estuary to the best of your knowledge. We would really like to find out how much people know about the Tay Estuary. This will make it easier for the Scottish Government and local authorities to let you know what is taking place in your area now and in the future.

1. In the Tay Estuary what percentage of homes are at risk from flooding?
 - a. Less than 3%
 - b. Between 3% and 5%
 - c. Between 6% and 8%
 - d. More than 9%
 - e. I don't know
2. How much money is invested annually in river and coastal defence in Scotland?
 - a. Between £10 million and £30 million
 - b. Between £30 million and £50 million
 - c. Between £50 million and £70 million
 - d. Between £70 million and £90 million
 - e. I don't know
3. Historically, the main type of coastal flood protection in Scotland has been:
 - a. Beach replenishment and nourishment
 - b. Planning regulations to limit development on flood plains
 - c. Concrete sea walls and rock armouring
 - d. Managed realignment
 - e. I don't know
4. Managed realignment schemes have the potential to provide:
 - a. A lower level of protection from flooding
 - b. No protection from flooding
 - c. A greater level of protection from flooding
 - d. The same level of protection from flooding
 - e. I don't know
5. Coastal wetlands are beneficial to fisherman because:
 - a. Wetlands do not benefit fisherman
 - b. Wetlands provide a food source for fish
 - c. Wetlands provide spawning grounds for fish
 - d. Wetlands act as a 'no take zone' thereby helping to preserve fish stocks
 - e. I don't know
6. Coastal wetlands are beneficial to wildlife because:
 - a. Wetlands do not benefit wildlife

- b. Wetlands are less polluted than other coastal habitats
 - c. Wetlands provide a food source for wildlife
 - d. Wetlands are less likely to be disturbed by humans
 - e. I don't know
7. Managed realignment schemes involve the loss of land to the sea. The land most likely to be lost is:
- a. Agricultural land
 - b. Residential land
 - c. Disused brownfield land
 - d. Seafront land
 - e. I don't know
8. The Scottish Government has a legal duty to the European Union to protect coastal wetlands because:
- a. Wetlands are important recreational assets
 - b. Wetlands are important fishing grounds
 - c. Wetlands are important habitats for waterbirds
 - d. Wetlands are important natural flood defences
 - e. I don't know
9. Which of the following is one of the main causes of decline of shelduck (a waterbird) in the Tay Estuary?
- a. Commercial fishing
 - b. Coastal erosion
 - c. Port operations
 - d. Oil spills
 - e. I don't know

Appendix B: Screenshots

Figure B1: Quiz Questions as seen by subjects

ID 2

In the Tay Estuary what percentage of homes are at risk from flooding? *

- ☐ Less than 3%
- ☐ Between 3% and 5%
- ☐ Between 6% and 8%
- ☐ More than 9%
- ☐ I don't know

ID 3

How much money is invested annually in river and coastal defence in Scotland? *

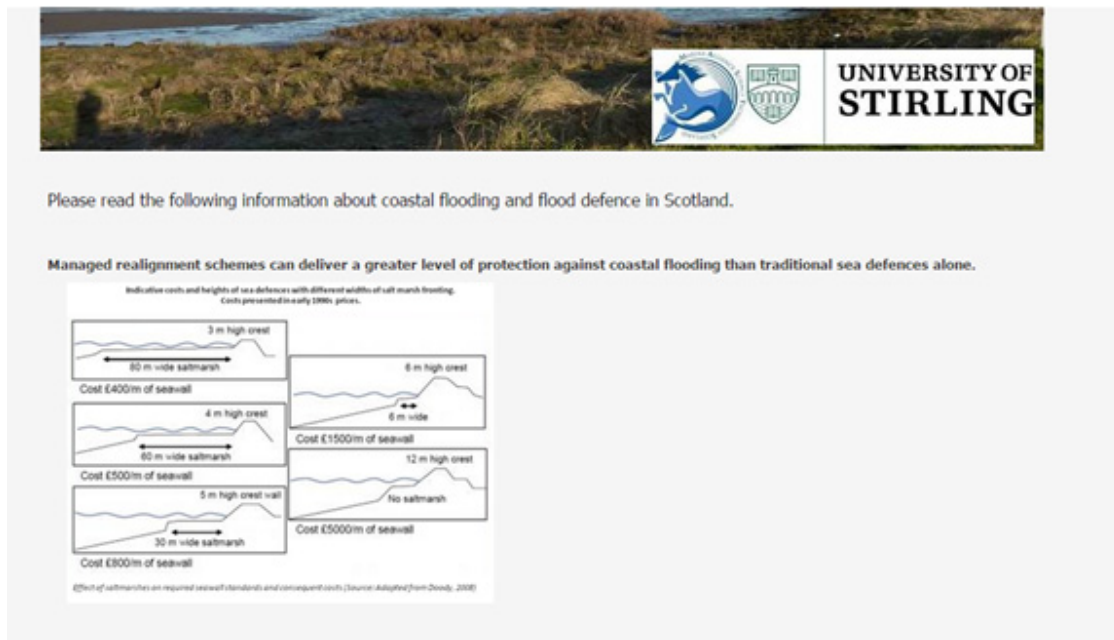
- ☐ Between £10 million and £30 million
- ☐ Between £30 million and £50 million
- ☐ Between £50 million and £70 million
- ☐ Between £70 million and £90 million
- ☐ I don't know

ID 4

Historically, the main type of coastal flood protection in Scotland has been: *

- ☐ Concrete sea walls and rock armouring
- ☐ Beach replenishment and nourishment
- ☐ Managed realignment
- ☐ Planning regulations to limit development on flood plains
- ☐ I don't know

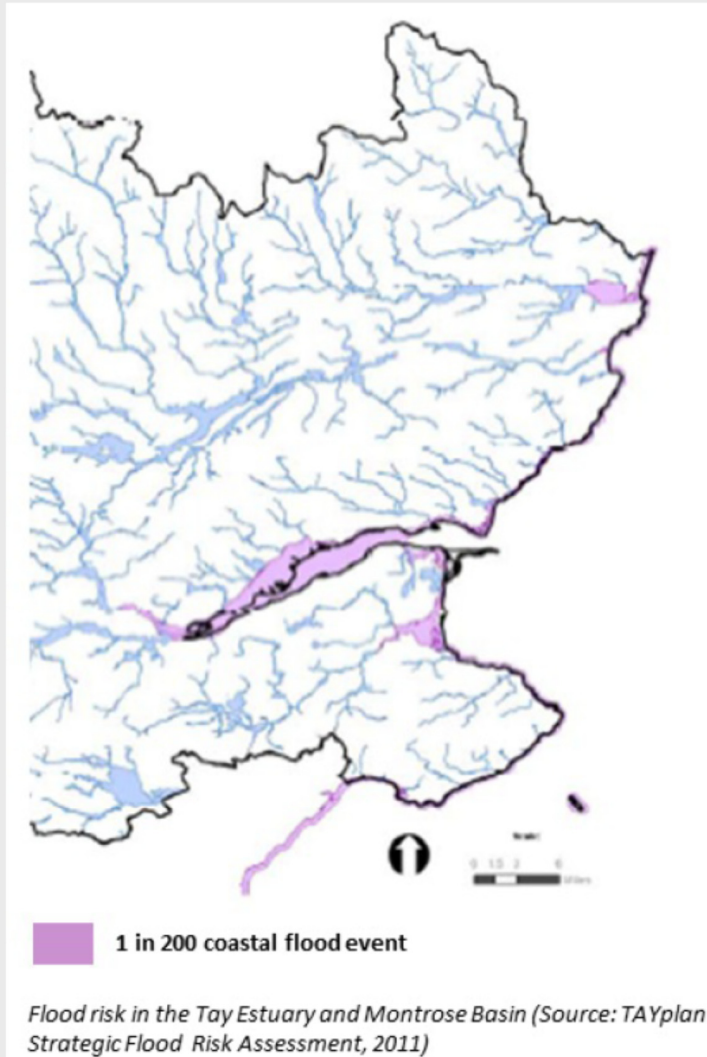
Figure B2: Example Bullet Point Screenshot



NOTE: This screenshot corresponds to quiz question number 4 in Appendix A

Figure B3: Example Bullet Point Screenshot

In the Tay Estuary and Montrose Basin approximately 4% of homes lie within a floodplain.



NOTE: This screenshot corresponds to quiz question number 1 in Appendix A

Figure B4: Example Bullet Point Screenshot

Historically, the main form of flood defence in Scotland has been hard engineered structures such as sea walls, rock armour and groynes.



Groynes



Sea walls



Rock armour

NOTE: This screenshot corresponds to quiz question number 3 in Appendix A

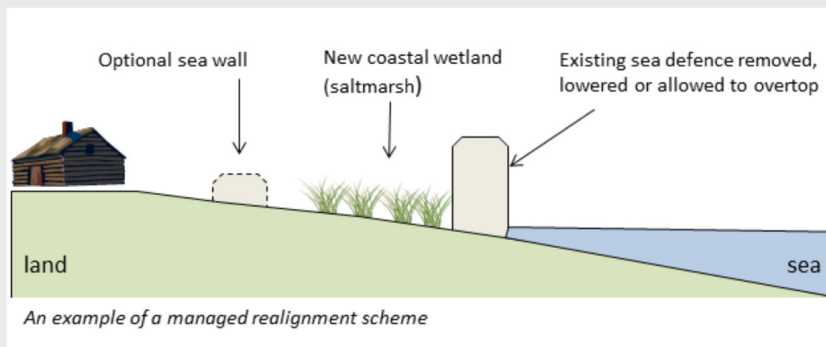
Figure B5: Policy Description Screenshot

Future Flood Defence

Coastal flooding accounts for 29% of the overall flood risk in the Tay Estuary. Coastal flooding is caused by abnormally high spring tides and storm surges. The Scottish Government and local authorities are considering the future river and coastal defence options for the Tay Estuary as part of a commitment to deliver flood risk management plans by 2015. Managed realignment is one of the potential flood defence options for the Tay Estuary.

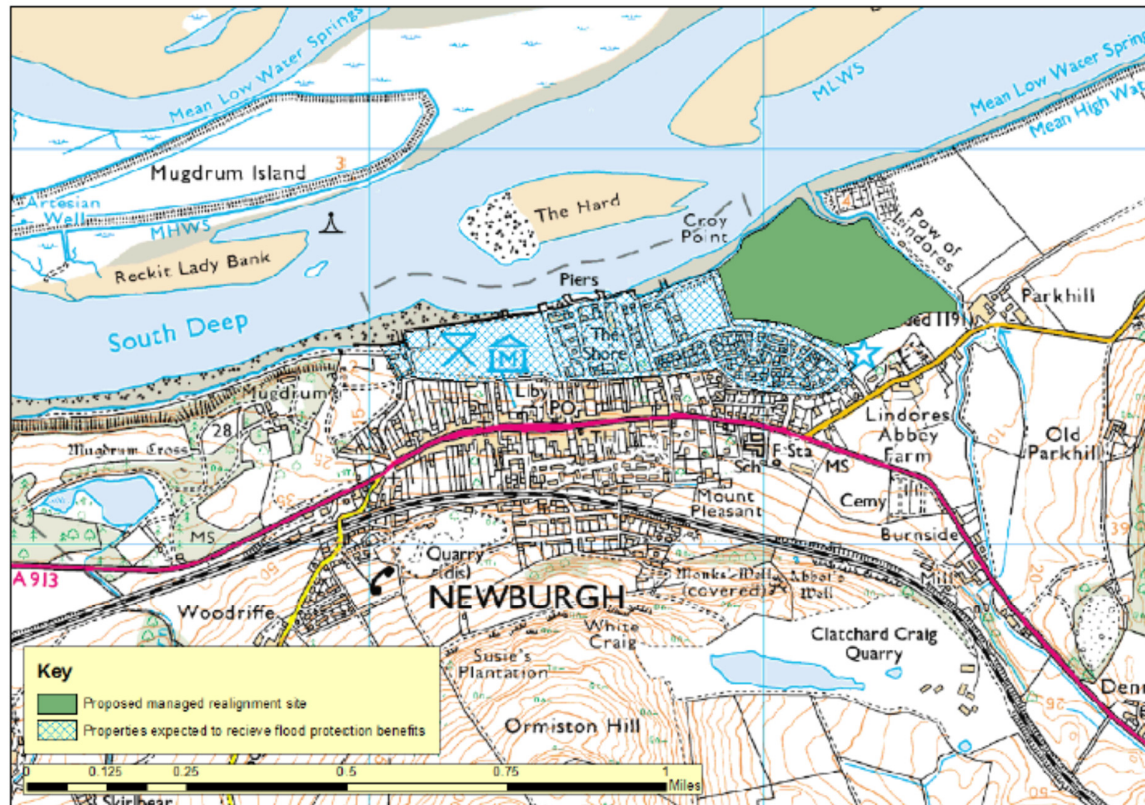
What is Managed Realignment?

Managed realignment is where part of the existing sea defence is removed and the area behind it is allowed to flood. This flooding creates coastal wetlands known as saltmarshes. These coastal wetlands can protect the land against storm surges, coastal erosion and sea level rise. By allowing an area of land to flood constructed sea walls needed to protect residential areas and businesses can be shorter in height and thus constructed at a lower cost (see picture below).



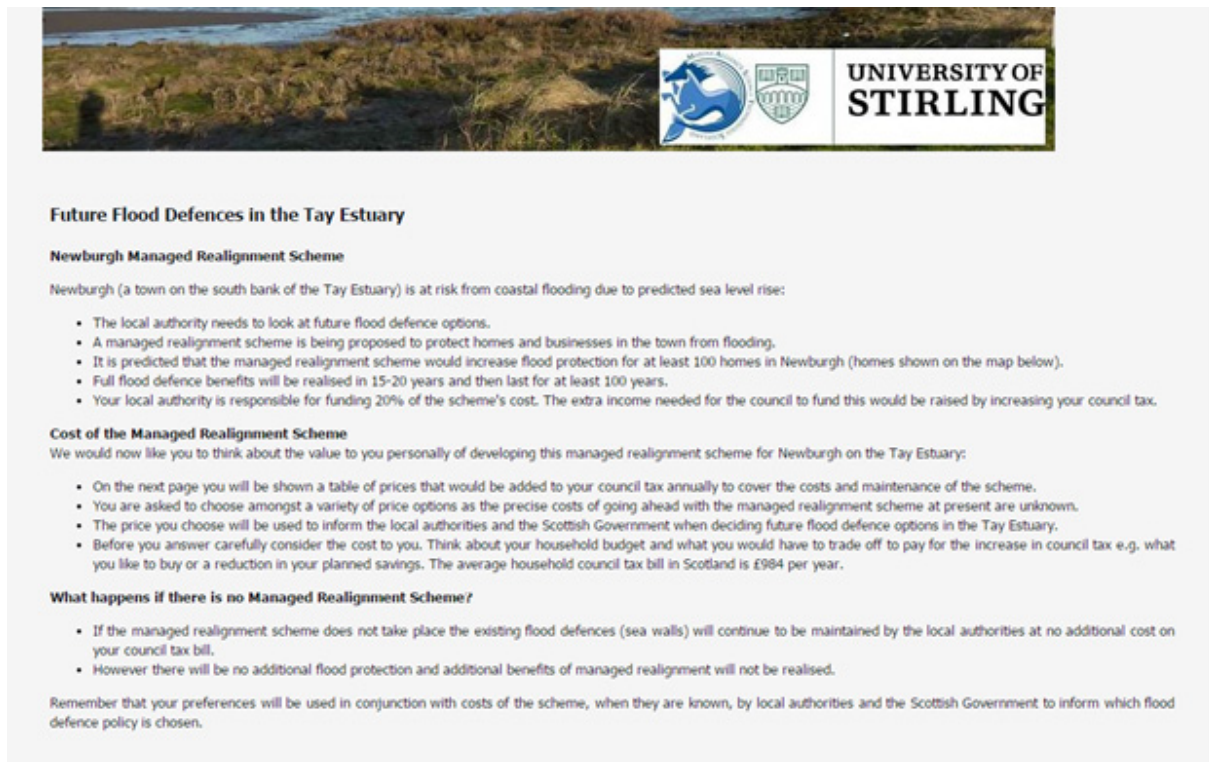
On the next page you will learn more about flood defence in Scotland and what managed realignment means for you.

Figure B6: Screenshot Description of Site



Proposed managed realignment scheme at Newburgh in the Tay Estuary

Figure B7: Policy Description Screenshot



Future Flood Defences in the Tay Estuary

Newburgh Managed Realignment Scheme

Newburgh (a town on the south bank of the Tay Estuary) is at risk from coastal flooding due to predicted sea level rise:

- The local authority needs to look at future flood defence options.
- A managed realignment scheme is being proposed to protect homes and businesses in the town from flooding.
- It is predicted that the managed realignment scheme would increase flood protection for at least 100 homes in Newburgh (homes shown on the map below).
- Full flood defence benefits will be realised in 15-20 years and then last for at least 100 years.
- Your local authority is responsible for funding 20% of the scheme's cost. The extra income needed for the council to fund this would be raised by increasing your council tax.

Cost of the Managed Realignment Scheme

We would now like you to think about the value to you personally of developing this managed realignment scheme for Newburgh on the Tay Estuary:

- On the next page you will be shown a table of prices that would be added to your council tax annually to cover the costs and maintenance of the scheme.
- You are asked to choose amongst a variety of price options as the precise costs of going ahead with the managed realignment scheme at present are unknown.
- The price you choose will be used to inform the local authorities and the Scottish Government when deciding future flood defence options in the Tay Estuary.
- Before you answer carefully consider the cost to you. Think about your household budget and what you would have to trade off to pay for the increase in council tax e.g. what you like to buy or a reduction in your planned savings. The average household council tax bill in Scotland is £984 per year.

What happens if there is no Managed Realignment Scheme?

- If the managed realignment scheme does not take place the existing flood defences (sea walls) will continue to be maintained by the local authorities at no additional cost on your council tax bill.
- However there will be no additional flood protection and additional benefits of managed realignment will not be realised.

Remember that your preferences will be used in conjunction with costs of the scheme, when they are known, by local authorities and the Scottish Government to inform which flood defence policy is chosen.

Figure B8: Payment card used by all subjects

Please select either yes or no to each of the prices presented in the table that your household would definitely be willing to pay in additional council tax each year to fund the development of a managed realignment scheme at Newburgh on the Tay Estuary. *

	Yes	No
£0 *	☐	☐
£5 *	☐	☐
£10 *	☐	☐
£15 *	☐	☐
£20 *	☐	☐
£30 *	☐	☐
£40 *	☐	☐
£50 *	☐	☐
£60 *	☐	☐
£70 *	☐	☐
£80 *	☐	☐
£90 *	☐	☐
£100 *	☐	☐
£110 *	☐	☐
£120 *	☐	☐
£130 *	☐	☐
£140 *	☐	☐
£150 *	☐	☐
I am prepared to be in excess of £150 *	☐	☐

Appendix C: Additional WTP Specifications

In addition to IV regressions, we account for the effects of learning on WTP in a second way to account for differing opportunities to learn (based upon different information treatments) in a different way. To do so, we estimate the following regression:

$$WTP_i = \alpha + X_i'\gamma + 1\{0 - 3 \text{ New Info Bullets}\} * (\text{Info Bullets Learned}) \beta_L + 1\{4 - 6 \text{ New Info Bullets}\} * (\text{Info Bullets Learned}) \beta_M + 1\{7 - 9 \text{ New Info Bullets}\} * (\text{Info Bullets Learned}) \beta_H + \varepsilon_i \quad (3)$$

In equation (3) the coefficients of interest are β_L , β_M , and β_H . Each coefficient shows the causal effect of additional learned information conditional on the amount of new information present. For example, β_L represents the effect of learned information conditional on starting off in the low ex ante information group.

TABLE C1: WTP on Learning

VARIABLES	(1)	(2)	(3)	(4)
<i>0-3 New Bullets Shown* Info Bullets Learned</i>	-0.877 (2.429)	0.0790 (2.283)	-2.071 (3.838)	0.852 (4.313)
<i>4-6 New Bullets Shown* Info Bullets Learned</i>	0.480 (1.309)	0.266 (1.347)	0.359 (2.147)	0.368 (2.394)
<i>7-9 New Bullets Shown* Info Bullets Learned</i>	0.987 (1.677)	0.860 (1.388)	1.565 (3.080)	2.126 (2.179)
<i>Constant</i>	44.47*** (3.089)	63.07*** (12.11)	51.28*** (4.766)	53.51** (21.16)
Controls	N	Y	N	Y
Incentive Comp	N	N	Y	Y
Observations	504	431	247	179
R-squared	0.001	0.252	0.003	0.354

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent Variable is WTP. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, environmentalist and perceived consequentiality indicators. Columns (3) and (4) includes only individuals who perceive results as being consequential.

Results from regression (3) are shown in Table C1. We find no evidence in any specification that there is any causal effect of learning on stated WTP for the public good considered here. This non-effect does not vary as a function of the previous amount of information. However, the estimates are quite noisy: the ratio of the point estimate of each coefficient to standard error of the estimate is quite low. This is evidence there is likely to be heterogeneity in the effect of learning on WTP. These results are robust to binning subjects according to *Info Bullets Learned* as we've done with *New Bullets Shown*.

Unlearned Information and WTP

We repeat regression (3) but change the continuous variable to what we define as *Excess Info*. We define *Excess Info* to be the amount of unlearned new information provided to subjects. We would like to be able to determine if incomplete learning directly affects stated WTP. If it does then it is evidence that there is no free disposal of information. Put another way, the total quantity of information provided to subjects could be important in many economic situations. As a result, we estimate the following regression:

$$WTP_i = \alpha + X'_i\gamma + 1\{0 - 3 \text{ New Info Bullets}\} * (\text{Excess Info}) \beta_L + 1\{4 - 6 \text{ New Info Bullets}\} * (\text{Excess Info}) \beta_M + 1\{7 - 9 \text{ New Info Bullets}\} * (\text{Excess Info}) \beta_H + \varepsilon_i \quad (4)$$

Results from estimating equation (4) are shown in Table C2. We find only very weak to no evidence that there could be an effect of excess information on WTP for the public good. For example, there is a consistent but insignificant negative effect of excess information on WTP for subjects shown only a small amount of new information (e.g., *0-3 New Bullets Shown*). However, this effect doesn't persist across different levels of newly shown information (e.g., *4-6 New Bullets Shown* or *7-9 New Bullets Shown*). We've performed the same regression controlling for the amount of learned information and the results are similar.

TABLE C2: WTP on Excess Info

VARIABLES	(1)	(2)	(3)	(4)
<i>0-3 New Bullets Shown* Info Bullets Learned</i>	-1.809 (2.902)	-2.909 (2.866)	-3.887 (4.119)	-6.408 (4.349)
<i>4-6 New Bullets Shown* Info Bullets Learned</i>	1.999 (1.744)	0.523 (1.644)	2.462 (2.626)	1.863 (2.520)
<i>7-9 New Bullets Shown* Info Bullets Learned</i>	-0.627 (1.401)	-2.062* (1.250)	0.863 (2.321)	0.540 (2.328)
<i>Constant</i>	44.12*** (2.662)	64.38*** (12.33)	50.48*** (4.315)	55.30*** (21.10)
Controls	N	Y	N	Y
Incentive Comp	N	N	Y	Y
Observations	503	430	247	179
R-squared	0.006	0.256	0.012	0.365

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent Variable is WTP. Control variables in columns (2) and (4) are survey round, education, gender, flood threat indicators, property owner, environmentalist and perceived consequentiality indicators. Columns (3) and (4) includes only individuals who perceive results as being consequential.

Appendix D: Alternative Models of Updating

Because we are able to horserace several different learning and preference formation models in this paper, we list possible findings as consistent or inconsistent with different models. If we reject a null hypothesis, we therefore reject the model associated with it. Alternatively, if we fail to reject a null hypothesis, our findings are consistent with that particular model of learning or preference formation (insofar as preferences are related to valuations).

Learning Hypotheses

One useful way to think about the causal effect of information on knowledge from estimating equation (1). There are three important features about Table D1. First, there are three information pairs that are not feasible because we did not use redundant treatments: ML, HL and HM. For example an individual with an ex ante high information set should never lose information because they are reminded of a subset of information they already knew. Second, there are three information pairs in which minimal or no learning occurs: LL, MM and HH. The effect of these information pairings on learning (e.g., the first equation) are the increase in score given by the estimated coefficients Γ_{LL} , Γ_{MM} , and Γ_{HH} . Third, there are three information pairings in which some learning might occur: LM, LH and MH. The marginal effect of new information on score is given by Γ_{LM} , Γ_{LH} , and Γ_{MH} .

Now consider the significance of coefficients which would be consistent with different types of learning. We are able to horserace three different models of learning.

1) No Learning –

$$H_0: \Gamma_{LM} = \Gamma_{LH} = 0, \Gamma_{MH} = 0$$

Table D1: Ex ante information and ex post information levels. Importantly, the cells in this table do not necessarily correspond to any particular treatment. This table represents all possible scenarios, assuming perfect recall, for how much information a subject can have after treatment assuming that each updating rule is feasible.

Ex Ante Information \ Ex Post Information	L	M	H
H _{info}	Γ_{LH}	Γ_{MH}	Γ_{HH}
M _{info}	Γ_{LM}	Γ_{MM}	--
L _{info}	Γ_{LL}	--	--

In this case, only *a priori* information determines subsequent second quiz scores.

2) Complete Learning – $H_0: \Gamma_{LL} + \Gamma_{LM} = \Gamma_{MM}, \Gamma_{LL} + \Gamma_{LM} + \Gamma_{LH} = \Gamma_{MM} + \Gamma_{MH} = \Gamma_{HH}$

In this case, the information treatment fully determines ex post information levels.¹⁹

3) Incomplete Learning– $H_0: \Gamma_{LM} > 0, \Gamma_{LH} > 0, \Gamma_{MH} > 0$

In this case, type L individuals cannot fully learn in the high information treatment.

In addition, subjects could exhibit fatigue when they learn. For example, the ability of subjects to retain the marginal piece of information could decrease in the amount of information they are provided with.

4) Fatigue - $H_0: \Gamma_{LH} < \Gamma_{LM}$

¹⁹ Note here that we are only concerned with updating behavior. We discuss the implications of different ex ante information levels below.

In the case of fatigue, retention rates are higher when the subject is provided with less information. Note that fatigue can jointly occur with incomplete learning. Further, fatigue could be a cause of incomplete learning if, for example, subjects in the LH and LM information treatments do not have significantly different levels of ex post knowledge.

Preference Formation/Valuation Hypotheses

Conditional on learning, there is still a question of how new knowledge about the good's attributes affects WTP. Our design and the empirical specification in equation (2) allows us to control for ex ante attribute knowledge levels so that the marginal effect of knowledge can vary as a function of ex ante knowledge levels. This is what distinguishes learning and preference formation in our study. For example, it is not necessarily the case that the two individuals that have the same amount of retained information after treatment have the same WTP for the good. Given the design of this experiment, we can horserace different models of how additional information affects WTP. To do so, we consider the models below that use the valuation estimating equation (2). In order to interpret each model, there are restrictions on the learning results which must coincide with the valuation based preference results.

1) Knowledge-based preference updating – $H_0: \omega_{LM} \neq 0, \omega_{LH} \neq 0, \omega_{MH} \neq 0$

Knowledge based preference updating implies that subjects' WTP is determined by the ex post knowledge levels of knowledge. In order for this result to be consistent with knowledge-based rather than information-based updating, though, this finding must coincide with either complete or incomplete learning. If instead this empirical result occurs jointly with no learning then this empirical finding would be consistent with *information*-based preference updating rather than *knowledge*-based valuation updating.

It is possible that different types of knowledge-based preference updating are more informative, from a scientific perspective, than others. Consider the following:

2) Ex-post knowledge preference updating–

$$H_0: \omega_{LL} + \omega_{LM} + \omega_{LH} = \omega_{MM} + \omega_{MH} = \omega_{HH}, \omega_{LL} + \omega_{LM} = \omega_{MM}$$

Ex-post knowledge preference updating implies that only ex post knowledge levels correlate with valuation and ex ante levels do not. It is consistent with knowledge about good attributes having a uniform effect (e.g., prior knowledge levels don't matter, only knowledge levels at time of WTP elicitation).

3) Ex ante Knowledge-based preferences– $H_0: \omega_{LM} = \omega_{LH}=0, \omega_{MH} =0$

If learning occurs but valuations do not change as a function of new knowledge then valuations are not a function of ex post information levels. In this case, the endogenous acquisition of ex ante knowledge before the experiment fully dictates the WTP of agents. There are three different models consistent with this result. First, agents could interpret learned information as confirming what they already understood regarding their preferences for the good, consistent with confirmatory bias as in Rabin and Schrag (1999). As a result, confirmation bias is joint hypotheses across both the learning regressions (e.g., there must be either complete or incomplete learning) and the results of the confirmation bias coefficients above.

Second, agents could use endogenously chosen levels of costly effort before the experiment to learn up to the point where the marginal cost of learning is less than the expected marginal benefit (e.g., learning increases welfare from a decrease in decision errors). If these endogenously acquired priors are unbiased relative to underlying heterogeneous preferences, additional information will not affect pre-existing valuation levels. This model bears similarity to bandit models of costly search where heterogeneous

preferences create variation in the marginal benefit of obtaining knowledge (Rothschild 1974, Caplin and Dean 2011 and Hanna et. al. 2014).

Third, this result is also consistent with a timing gap between learning and preference formation. It could be that subjects exhibit knowledge-based preference formation but preferences take time to form and are only formed upon reflection. We are not aware of any economic model which posits that preferences are formed in this way but we cannot rule it out as an explanation. We are also not aware of any extant experimental design designed to parse between the first two models although identifying this third model could conceivably be performed but eliciting valuations at different points in time.

There are two caveats to confirmation bias and endogenous costly effort. First, nothing in principle prevents endogenous costly effort and confirmation bias from occurring simultaneously. Indeed separately identifying these two models could be challenging to future researchers. Second, if endogenously acquired knowledge levels is correlated with preference then we expect to observe: $\omega_{LL} \neq \omega_{MM} \neq \omega_{HH} \neq 0$. For example, a consumer who has a higher WTP to attend a tennis match may know more about tennis, *ceteris paribus*.

4) No Knowledge-based preferences— $H_0: \omega_{LL} = \omega_{MM} = \omega_{HH}, \omega_{LM} = \omega_{LH} = \omega_{MH} = 0$

If learning occurs but knowledge is orthogonal to preferences then there would be no statistically significant difference between ex ante levels of information, ex post levels of information and value for the good. This finding would indicate that other factor than knowledge of the good's attributes drive heterogeneous preferences for goods.

5) Knowledge based Preferences & Information Overload—

$$H_0: \omega_{LH} = 0, \omega_{LL} + \omega_{LM} = \omega_{MM}, \omega_{MM} + \omega_{MH} = \omega_{HH}, \omega_{MH} \neq 0, \omega_{MH} \neq 0$$

Assuming that learning occurs and that ex post knowledge levels matter, there could be a distinct behavioral reaction to being given large amounts of new information in preference formation. Being treated with significantly more information than a subject already possesses as knowledge could feasibly affect preference formation directly. This has similarities to a model of costly preference formation where more new knowledge leads to higher processing costs. If we observe full learning, and moderate amounts of knowledge matter for WTP (e.g., $\omega_{LM} \neq 0$, $\omega_{MH} \neq 0$) but the marginal value of knowledge decreases in the amount of knowledge (e.g., $\omega_{LH} = 0$) it is evidence that preference formation costs, if they exist, increase in the amount of new knowledge.

Appendix E: Comparison of different parametric distributions fitted to the interval WTP data

	Parameters	LL	AICc
Normal	2	-1,582.89	6.33
Logistic	2	-1,576.22	6.30
Extreme Value	2	-1,682.91	6.73
Generalized Extreme Value	3	-1,399.22	5.63
t Location Scale	3	-1,582.89	6.36
Uniform	2	-1,579.70	6.32
Johnson SU	4	-1,376.31	5.56
Exponential	1	-1,372.33	5.47
Lognormal	2	-1,378.31	5.52
Loglogistic	2	-1,381.19	5.53
Weibull	2	-1,356.27	5.43
Rayleigh	1	-1,706.46	6.80
Gamma	2	-1,354.35	5.42
Birnbaum-Saunders	2	-1,373.11	5.50
Generalized Pareto	3	-1,362.63	5.48
Inverse Gaussian	2	-1,403.87	5.62
Nakagami	2	-1,413.76	5.66
Rician	2	-1,706.46	6.82
Johnson SB	4	-1,370.03	5.54
Johnson SL	4	1,376.31	5.48
Poisson	1	-10,033.66	39.84
Negative Binomial	2	-1,376.12	5.51



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