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WHAT IS THE CAUSAL EFFECT OF KNOWLEDGE ON PREFERENCES?

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What is the Causal Effect of Knowledge on Preferences?

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Abstract

We use a novel field experiment which jointly tests two implicit assumptions of updating models in a joint framework: that new information leads to new knowledge and that new knowledge can affect economic decisions. In the experiment, we elicit subjects' prior knowledge state about a good's attributes, exogenously vary how much new information about good attributes we provide to subjects, elicit subjects' valuation for the good, and elicit posterior knowledge states about the same good attributes. Testing for changes in knowledge jointly with changes in preferences allows us to horserace updating models more completely than previous studies since we observe ex ante and ex post knowledge states. Our results are consistent with a model of incomplete learning, fatigue and either confirmation bias or costly search coupled with unbiased priors.

Keywords:

Learning, Information, Bayesian Updating, Behavioral Economics

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D83, D81, Q51

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I. Introduction

It is important for economists to understand how individuals evaluate new information about a good, asset or strategic choice and subsequently make decisions. Take an individual deciding whether to purchase a new car or repair their current car which has broken down. That person would very likely i) spend time seeking out information about attributes of new cars, ii) learn that information and therefore acquire knowledge, and iii) then make a more educated and possibly different decision than they otherwise would have.

The economics literature has focused on the optimal amount of knowledge an agent should acquire in order to, on average, make the most efficient decision (Rothschild 1974, Hanna et. al. 2014, and Caplin and Dean 2015). To do so, much of the economics literature models agents as Bayesian updaters, although extensions and deviations of the Bayesian model have been developed to explain observed behavior (Rabin and Schrag 1999 and Schwartzstein 2014). Understanding which model of updating economics agents use is vital in order to develop mechanisms which increase welfare (Sims 2003 and Caplin and Dean 2015).

There are at least two implicit assumptions in any updating models commonly used in the literature. First, exposure to information is often assumed to have a one-to-one relationship with increases in knowledge. That is, if an agent is exposed to information, they learn that information. Second, increases in knowledge about the embedded attributes of a good are presumed to affect economic decisions. For example, if a consumer learns about a desirable characteristic embedded in a product, they are likely to value it more regardless of their previous valuation for the product. Thus, in any model of updating, information affects knowledge and knowledge affects decision making.

Given the reliance of updating models on these two assumptions, it is somewhat surprising that they have never been tested in a unified framework. This paper reports the results of a field experiment

designed to do just that. The novel experimental design allows us to elicit subjects' prior knowledge state about a good's attributes, exogenously vary how much new objective information about good attributes we provide to subjects, elicit subjects' valuation for the good, and elicit posterior knowledge states about the same good's attributes. Because we exogenously vary information in the experiment, we identify causal estimates for how information becomes knowledge and how knowledge affects valuations for a good. As a result, we are able to horserace several different models learning and updating (both Bayesian and non-Bayesian) in a natural field experiment.

Testing for learning and updating in a unified framework is vital to parse between different models of updating. Experimental evidence shows that empirical results consistent with deviations from the Bayesian model do exist (Eil and Rao 2011, Grossman and Owens 2012, and LaRiviere et. al. 2014). However, this literature is not able to parse between different models of updating because it is predicated upon changes in knowledge rather than the more primitive issue of information. We claim that embedding exogenous variation in information is important because some "behavioral" models of updating 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 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. As a result, in order to test the two implicit assumptions of the Bayesian updating model- and horserace alternative models- a joint test of both learning and updating is needed.

To test for learning and updating jointly using market data is challenging if not impossible. Lab environments are subject to common external validity concerns which are arguably more important in a context like micro-level updating since subjects plausibly attend to information in a lab differently than in the field. As a result, we utilized stated preference methods common in environmental economics

(Carson and Czajkowski 2014). Environmental economists use stated preference demand estimation methods when there is no market data which can be used to value a good, such as with non-use values of environmental or public goods. Importantly, recent work shows that if subjects believe a survey will be used to inform policy and is therefore consequential for respondents- as we show was the case in our study- it incentivizes subjects to truthfully reveal their valuation (Carson, Groves and List 2014, Vossler et. al. 2012, Vossler and Evans 2009 and Carson and Groves 2007).

In stated preference methods, agents are asked about their willingness to pay for a change in a public good or public service immediately after receiving information about it. Agents must incorporate this information into their existing ex ante information sets when making decisions about their willingness to pay for a particular level of provision for a public good.² As a result, stated preference methods are well-suited for implementing our experiment in an economically meaningful context.

Our survey concerns a population's willingness to pay for a well-publicized project in Scotland to regenerate of coastal wetlands as a way of mitigating flood risk. Wetlands act are a form of flood protection because they allow an outlet for storm surges or high rainfall incidents. In addition, wetlands also provide amenity value by increasing habitat for wildlife. As a result, individuals living in flood plains are less likely to have their property damaged by flooding if new wetlands are created in the area in addition to enjoying increased wildlife abundance and diversity. Therefore, providing information about these two well-defined attributes of coastal wetlands offers an ideal setting to horserace alternative learning and updating models.

² While the stated preference literature acknowledges the importance of previous experience and information (Cameron and Englin (1997) and Czajkowski, Hanley and LaRiviere (2015)), there is no paper which is able to identify the causal effect of information on WTP isolated specifically through learning. Such decisions include their maximum willingness to pay for an improvement in environmental quality, or for a reduction in expected damages.

We use the following experimental design to parse between models of learning and updating: 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 ecological attributes of wetlands 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 possible piece of information we gave subjects was related to exactly one of the nine multiple choice questions. Importantly, because we identified the subject's exact knowledge state at the beginning of the experiment, we can verify which information we give the subject is new to them. 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 we gave them at the beginning. Thus, we are able to isolate how providing additional information, which we verify has been learned by comparing first and second quiz scores, affects valuation for the good while conditioning on different levels of a subject's *ex ante* knowledge.

There are two main important results from the field experiment. The results of the purely informational part of the experiment show that higher information treatments cause significantly more learning in subjects, even though that 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 incomplete learning and fatigue.

The results of the updating portion of the survey show similar discord with a standard neoclassical Bayesian model. We find that additional information about a good's attributes does not significantly affect valuations for the good. However, 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 about good attributes did not affect these valuations.

The updating result in conjunction with the incomplete learning result is consistent with two different models of learning and updating. The first is a model of imperfect learning and confirmation bias. 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 not opportunity to update valuations because the differing levels of information embedded in different information treatments were not actually learned.

The second possible model is one of costly search and unbiased priors: agents could use costly effort in the field before the experiment to seek out and learn information up to the point where the expected marginal cost of learning is less than the expected marginal benefit. If endogenously formed and unbiased beliefs about good attributes are valued according to underlying heterogeneous preferences, additional information will not affect pre-existing valuation levels. Furthermore, ex ante levels of knowledge could easily correlate with valuations in a systematic way: for example, people in the flood plain may both know more about wetlands flood mitigating potential and be willing to pay more for them. This type of model bears some similarities to recent models of costly attention (Caplin, Dean and Martin 2011, Hanna et. al. (2014), Schwartzstein (2014), Caplin and Dean 2015, and LaRiviere et. al. (2015)). The challenge for future work is to create an experiment parsing these two different models- and other yet to be determined models- of learning and valuation.

A key contribution of our paper is that due to our novel experimental design, we can confirm with certainty that our treatment effects are over new knowledge as opposed to new information. 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.

A second main contribution of our paper is that we jointly test for how information affects knowledge and how knowledge affects preferences. As a result, we can horserace different models of updating. For the updating procedure, we find evidence consistent with both 1) confirmatory bias and 2) heterogeneous preferences and endogenous information acquisition coupled with incomplete learning and fatigue. Our findings motivate research providing a understanding the welfare implications of both of these models and further experimental and field work parsing between them.

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 three key components. First, the design allows us to test for how much information respondents possess about the good in question at the outset of sampling: that is, to measure their ex ante knowledge. Second, the design also allows us to test how much of the new information is *retained*. Third, we are able to observe how a priori and new (retained) information affect willingness to pay for a public good. This section describes the design in context of these components.

Survey

We conducted a field experiment as part of a stated preference survey in Scotland in 2013. We set the survey in the context of current efforts by local government and the national regulator (the Scottish Environmental 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 also are concerned: we find that many 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 re-build 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 otherwise may cause flooding. The amount of flood protection is commensurate with the size of the wetlands 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 removal.

In order to gauge the public's willingness to pay for restoring wetlands, we undertook a stated preference survey. Subjects were invited to participate in the survey via repeated mailings and radio and newspaper advertisements.³ Subjects who completed the survey were given a £10 (\$16) Amazon gift card. The survey was conducted online through a website we designed and operated. Each subject who participated was given a unique identifier code. In the stated preference survey we embedded the field experiment described below.

Experimental Design

The design of the stated preference survey was as follows: subjects were told that their responses would help inform policy and management of flooding in their local area (the survey was funded by the Scottish Environmental Protection Agency, Scottish Natural Heritage and the Scottish government). They were then given a 9 question multiple choice quiz related to objective information about flooding, flood protection and wetlands. The quiz was justified to respondents as a way of informing policy makers how well this topic was being communicated to and understood by the community. Respondents were then

³ We show demographic characteristics of subjects relative to the population in the Tay estuary below. A copy of the advertisement is available on request.

given objective information about flooding, flood protection and wetlands. Each piece of information provided to subjects corresponded to a single multiple choice question from the quiz. We then elicited willingness to pay for a specific wetlands restoration project in the Tay Estuary which was identical for all participants. Finally, the subjects were given the exact same nine question quiz followed by a series of debriefing questions.

In this survey, we embedded the following field experiment. 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 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 exam 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 or good. Each bullet point and/or figure corresponds exactly to one question asked on the multiple choice questionnaire. The complete quiz, an example bullet point slide and the policy description slides are in the Appendix A. As a result, after treatment assignment each agent can be summarized as a type/treatment pair in addition to information 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 (e.g., the high information treatment). The experimental design is displayed graphically in Figure 1.

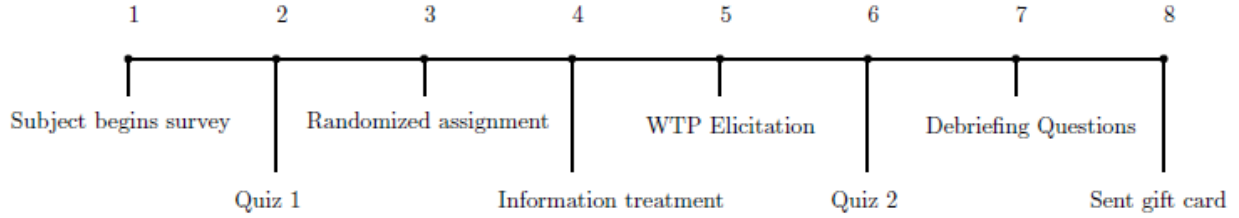


Figure 1: Experimental design

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 they would be provided 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 updating. 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.

One useful way to represent the type-treatment pairs and treatment information sets is shown in Table 1. 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.

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

Table 1: 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.

After the quiz, and before the information treatments, subjects were all given identical baseline information as to the potential cost of the policy and other background information given to all survey participants. The information treatments were displayed after this uniform information. At this point all agents were asked to select their maximum willingness to pay for the good – wetlands restoration – from a payment card of 20 different prices starting at zero and increasing to “greater than \$150”. They were only allowed to choose one of these values. Finally, each agent was given the exact same quiz as at the beginning of the survey in addition to a set of personal characteristic and debriefing questions. 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).

Our novel 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 to subjects before purchasing or valuing a good. This external validity concern, though, is the cost of cleanly verifying that information was indeed learned.

There is a valid concern about using stated preference valuation methods rather than market data in this paper even though recent literature finds evidence that it elicits truthful valuations. We argue that these concerns are ameliorated by using a randomized control group. Any bias introduced by use of stated preference should be differenced out when the treatment group is compared to the control group since we are interested in marginal effects of information and knowledge. The stated preference concern is only valid if the economist believes the specific *amount* of information we provide in M and H information treatments relative to the L treatment interacts with stated preference methods in a unique way.

Hypotheses

Combining the initial quiz, the information treatments and second quiz allows us to test for how subjects learn and what information updating procedure individuals are using in forming their willingness to pay estimates. This subsection introduces each type of learning and updating procedure. It shows how the information treatments allow us to identify the updating rule used in the practice.⁴

⁴ 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 - is 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.

The economics literature finds an increasing number of alternatives to costless learning and information retention. For example, models of bounded rationality, costly learning, fatigue, and cognitive load have agents not completely adsorbing new information into their information set (Sims 2003, and Gabaix et. al. 2006). Learning is important in our context because learning can affect a subject's valuation for a good.⁵ While neoclassical models often assume that agents use learned information and Bayesian updating to refine their preferences, there is evidence that individuals may filter additional information through priors in a way that confirms whatever preference or bias they may have previously had (Rabin and Schrag 1999, Eil and Rao 2011, Grossman and Owens 2012, and Fernback et. al. 2013). While previous levels of knowledge about a good have been shown to affect mean and variance WTP in a way consistent with Bayesian updating, those studies are not designed to be able to parse between alternative updating models (Christie and Gibbons 2011, Czajkowski et. al. 2014, and LaRiviere et. al. 2014). Our study fills these gaps.

Consider the implication of the survey design on the ability to parse between which updating procedure agents are using. For simplicity we estimate the following two equations:

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

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

In equations (1) and (2), X is a vector of subject specific control variables.⁶ In equations (1) and (2), as before, the two capitals stand for the ex-ante score and the information treatment respectively (e.g., the

⁵ Previous literature shows that mean and variance of WTP have been shown to be influenced by changes in agents' information set and that properly controlling for how information can influence such changes with stated preference data is important (MacMillan et al. 2006; Aadland et al. 2007; Hoehn et al. 2010, Czajkowski et. al. 2013 and LaRiviere et. al. 2014).

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

treatments in Table 1). There are two left hand side variables which we consider separately. The first is *score* and that specification measures actual learning that occurs conditional on ex ante information levels and treatment. The second is willingness to pay (*WTP*) conditional on ex ante information levels and treatment.

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}	--	--

Table 2: 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.

Score

It is important to understand the implications of treatment on information retention in order to reject the feasibility of various hypotheses dictating updating behavior. One useful way to think about information pairs is summarized in Table 2. Table 2 has ex ante information levels on the x axis and ex post information levels on the y axis. There are three important features about Table 2. First, there are three information pairs that should not be feasible if individuals can recall information: 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. This is equivalent to assuming perfect recall and can be tested empirically. 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

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.

equation) are the increase in score given by the estimated coefficients Γ_{LL} , Γ_{MM} , and Γ_{HH} . There are three information pairings in which some learning should occur: LM, LH and MH. The effect of these information pairings on score is given by Γ_{LM} , Γ_{LH} , and Γ_{MH} . Third, it is possible for information acquisition to be incomplete. For example, it could be the case that an individual of type L is given treatment H and has ex post information M. Put another way, subjects characterized by treatment status LH could have ex post M information levels.

Now consider the significance of coefficients which would be consistent with three different updating rules introduced above:

No Learning – $H_0: \Gamma_{LL} = \Gamma_{LM} = \Gamma_{LH} > 0, \Gamma_{MM} = \Gamma_{MH} > 0, \Gamma_{HH} > 0$

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

Complete Learning – $H_0: \Gamma_{LM} = \Gamma_{MM} > 0, \Gamma_{LH} = \Gamma_{MH} = \Gamma_{HH} > 0, \Gamma_{LL} \neq \Gamma_{LM} \neq \Gamma_{LH}$

In this case, the information treatment fully determines ex post information levels.⁷

Incomplete Learning – $H_0: \Gamma_{LL} < \Gamma_{LM} < \Gamma_{LH}, \Gamma_{MM} < \Gamma_{MH}$

In this case, type L individuals can learn but they can't fully learn in the high information treatment.

Fatigue – $H_0: \Gamma_{LM} - \Gamma_{LL} > \Gamma_{LH} - \Gamma_{LM}$

⁷ Note here that we are only concerned with updating behavior. We discuss the implications of different ex ante information levels below.

In this case, retention rates are higher when the subject is provided less information. Note that fatigue can jointly occur with costly or incomplete learning.

WTP

Conditional on learning, there is still a question of how prior information affects WTP relative to being exogenously provided with additional information (e.g., being in one treatment versus another). This is what distinguishes learning and updating in our study. For example, it is not necessarily the case 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 three models below that use the WTP estimating equation (2).

Bayesian updating –

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

Subjects' WTP is determined by the ex post level of information, assuming information is retained. This assumes that information has a uniform effect (e.g., prior information levels don't matter, only information levels at time of WTP elicitation).

Confirmation Bias–

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

In this case, the endogenous acquisition of information ex ante fully dictates how additional information affects WTP of agents. Agents could interpret learned information as confirming what they already understood regarding their preferences for the good, consistent with confirmatory bias. 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.

We note that there is another feasible explanation to observing both learning and confirmation bias: agents could use 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 (Rothschild 1974). We are not aware of any work in the literature which creates sufficient variation to identify between these two models.

Behavioral Information overload– $H_0: \omega_{LH} \neq \omega_{MH} = \omega_{HH}, \omega_{LM} = \omega_{MM}$

Regardless of the updating rule, there could be a distinct behavioral reaction to being given significantly more information than the subject already has which is different from that which occurs during updating when the marginal amount of information is not as great. Our design allows us to test for this model.

III. Results

Survey and Questionnaire

All participants for the survey were selected from the Scottish Phone Directory. Only people living within the local authorities affected by the flood defense scheme were selected to take part. In total 4000 households were contacted by mail and invited to take part in an online survey. A reminder card was sent two weeks after the first contact attempt. A third reminder card was sent after that. Of 4000 people invited 749 people completed or partially completed the online survey with 504 responses completed in

sufficient detail to be used in the analysis, typical response rates for mail-out stated preference surveys in the UK.⁸

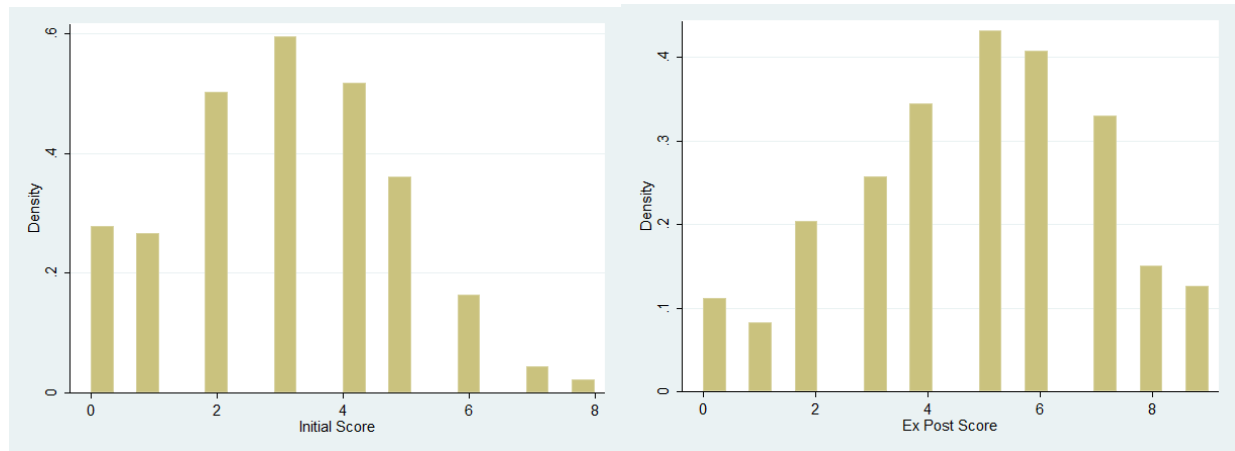


Figure 2: Quiz score histograms by test. Ex post quiz scores include only the treated grouped

Summary Statistics

Self-reported socio-demographic statistics that the sample was representative of the local authority areas sampled in terms of age ($\chi^2(6) = 63.04, p < 0.01$) and gender ($\chi^2(1) = 6.71, p < 0.01$). The mean income band was £20,000 - £30,000 and half the respondents worked full time. Some 69% of the respondents reported being insured for flood damages.

At the start of the survey each respondent answered identical nine question multiple choice quizzes concerning objective information about the historical flood protection and reclaimed wetlands. This quiz was then repeated for all respondents after they stated their WTP for all subjects. Figure 2 shows the histogram of subjects' scores in quiz one and quiz two for all subjects who took both quizzes. Figure 2 shows that there was a significant difference in the scores for quiz one (mean= 3.08, SD=1.76)

⁸ We also had a large control group not given the pre-survey quiz which we exclude from this analysis and address in LaRiviere et. al. 2015.

and quiz two (mean=5.19, SD=2.23). It is important to note that 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 subjects in the HH treatment. The complete composition of treatment and control groups is shown in Table 3. We oversampled 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.

TABLE 3: A Priori Type – Treatment Pairs

LL	151
LM	78
LH	72
MM	97
MH	94
HH	12
Note: n = 504 total subjects. For none of the analysis in this paper do we include the control group. Those results are available upon request from the authors.	

Information, Learning and Updating

Table 4 shows the coefficient estimates of regression (1) with Second Quiz Score as the dependent variable and treatment group as independent variables. To highlight treatment effects, we exclude a constant in this regression specification.⁹ 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 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 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.

TABLE 4: Score on Treatment Group

VARIABLES	(1)	(2)	(3)	(4)
<i>LL</i>	3.531*** (0.148)	3.886*** (0.508)	3.429*** (0.210)	4.919*** (0.606)
<i>LM</i>	4.400*** (0.252)	5.180*** (0.586)	4.525*** (0.316)	6.275*** (0.627)
<i>LH</i>	4.986*** (0.333)	5.719*** (0.586)	4.563*** (0.431)	6.538*** (0.683)
<i>MM</i>	5.446*** (0.164)	5.824*** (0.534)	5.132*** (0.246)	6.464*** (0.612)
<i>MH</i>	6.300*** (0.191)	6.756*** (0.536)	6.135*** (0.249)	7.565*** (0.653)
<i>HH</i>	8.167*** (0.200)	8.405*** (0.539)	8.143*** (0.244)	9.234*** (0.736)
Observations	504	431	247	179
Controls	N	Y	N	Y
Consequential Sample	N	N	Y	Y
R-squared	0.867	0.885	0.877	0.915

Robust standard errors in parentheses

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

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

Table 4 has two key features. First, within every specification providing more information to subjects increases retained information. Some of these increases, though, are not statistically significant (see below). Second, the rate of information retention varies somewhat across specifications. However, the larger patterns of increased retention is constant regardless of the specification.

Turning to the hypothesis tests for learning, we can reject the hypothesis that no learning occurs. We focus on specification 3 for our hypothesis testing. The null hypothesis that $H_0: \Gamma_{LL} = \Gamma_{LM} = \Gamma_{LH}$ is rejected at the 1% level (F-stat of 11.09). Similarly, we can reject the null hypothesis that subjects exhibit complete retention. The null hypothesis $H_0: \Gamma_{LH} = \Gamma_{MH} = \Gamma_{HH}$ is rejected at the 1% level (F-stat of 18.03).

We fail to reject, though, 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 specifications 3 and 4. Similarly, it is 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. This motivates our updating horserace.

Willingness to Pay

Figure 3 shows a histogram of maximum willingness to pay all subjects who completed the survey. Figure 3 shows that demand for this good is downward sloping and that subjects exhibit non-trivial anchoring around 50, 100 and 150 pounds. Since we are concerned with the effect of treatment on WTP here, though, these anchoring effects are unimportant unless there is a correlation between anchoring and different treatments. We view this possibility as unlikely. Importantly, there is significant heterogeneity in WTP for this good.

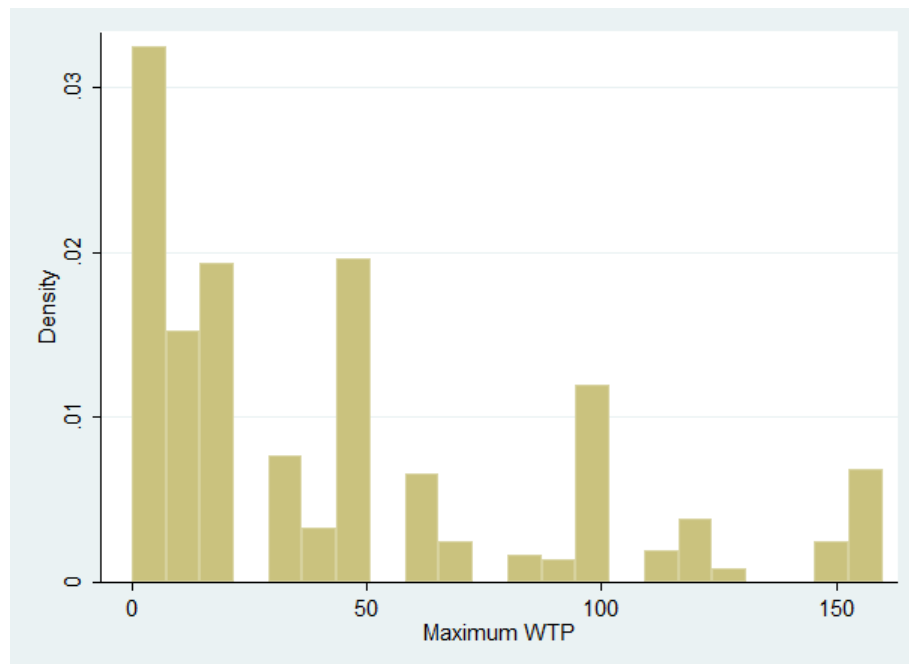


FIGURE 3: Histogram of WTP for all subjects. N = 504.

Table 5 shows the coefficient estimates of regression (2) with WTP as the dependent variable and treatment group as independent variables. We assume *LL* is the baseline group in this regression specification and coefficient estimates are the marginal effect of different treatments relative to that group.¹⁰ We report four specifications: the full sample with and without controls and only individuals who self-reported perceiving their responses as being consequential with and without controls.

In the first two columns of Table 5 there is no statistically significant effect of any treatment on WTP relative to the *LL* group. There are two implications: first, the marginal effect of information within the *L* ex ante information set is not significant. Second, the effect of different ex ante levels of information are also statistically insignificant: the *MM*, *MH*, and *HH* coefficients are all insignificant.

TABLE 5: Maximum WTP on Treatment Status

VARIABLES	(1)	(2)	(3)	(4)
<i>LM</i>	5.303 (6.962)	-3.046 (7.569)	0.167 (10.36)	-8.619 (12.18)
<i>LH</i>	-3.864 (6.918)	-12.89* (6.974)	-2.552 (11.68)	-15.09 (11.75)
<i>MM</i>	-7.396 (5.831)	-10.74* (5.649)	-21.45** (8.456)	-26.15** (10.07)
<i>MH</i>	2.636 (6.570)	-3.724 (6.487)	-8.237 (9.411)	-18.44* (9.974)
<i>HH</i>	-1.364 (10.34)	-9.054 (12.92)	-21.19 (14.54)	-40.21** (18.15)
Constant	46.36*** (4.060)	67.34*** (12.31)	58.33*** (6.484)	68.82*** (20.66)
Controls	N	Y	N	Y
Incentive Comp	N	N	Y	Y
Observations	504	431	247	179
R-squared	0.008	0.261	0.031	0.390

Robust standard errors in parentheses

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

Dependent Variable is WTP. *LL* treatment group is baseline. 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.

¹⁰ 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 education attainment.

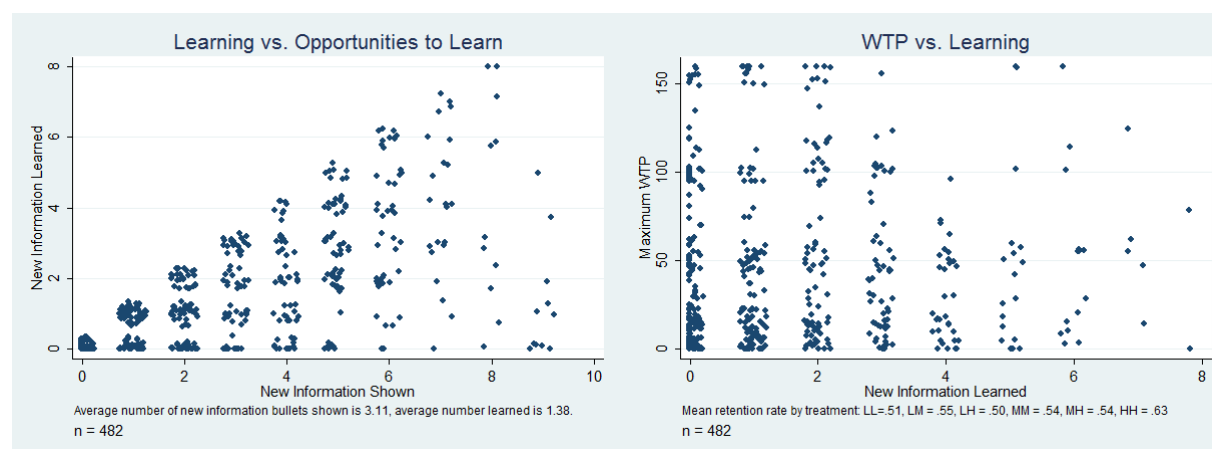
Adding control variables and trimming the sample to only subjects viewing the survey as consequential alters the significance of coefficient estimates of treatment on WTP. This is to be expected as controls increase the signal to noise ratio of treatment. Columns (3) and (4) have treatment effects of *MM*, *MH* and *HH* are significantly different from the *LL* treatment. However, the significant differences are driven by ex ante information levels: the *LL*, *LM* and *LH* treatments are not significantly different from each. Neither are *MM* and *MH* (pairwise F-stat of 1.23). As a result we fail to reject the null hypothesis that there is a causal effect of knowledge- which we confirm does occur in the previous subsection- on WTP. However, we reject the null hypothesis that ex ante information does not affect WTP. As a result, ex ante information predicts valuations.

Turning to hypothesis testing, the regression specification matters. Consider the first model with no controls nor compatibility trimming. We fail to reject the null hypothesis that only information treatment matters for WTP is $H_0: \omega_{LH} = \omega_{MH} = \omega_{HH}, \omega_{LM} = \omega_{MM}$ (p-value = .25, F-stat = 1.39). The null hypothesis of confirmatory bias is $H_0: \omega_{LL} = \omega_{LM} = \omega_{LH} > 0, \Gamma_{MM} = \Gamma_{MH} > 0$. We also fail to reject that null hypothesis (p-value = .18, F-stat = 1.61). We also fail to reject the null hypothesis that all treatment groups are the same (p-value .37, F-stat 1.09) and or that the effect of treatment on WTP is jointly zero. In sum, for the unrestricted sample, we fail to reject any updating model.

When we restrict our sample to subjects who are most likely to truthfully reveal their willingness to pay and include demographic controls, the results change. We argue that it makes sense to use the restricted sample in this case: valuations for this good are likely correlated with factors like age and exposure risk. Secondly, the literature shows that consequentiality matters for attaining true valuation elicitation in a stated preference survey (Vossler et. al. 2014 and Carson et. al. 2014). With the restricted sample we reject all models but the Confirmatory Bias/Costly Attention models. The causal effect of marginal information is not significant and differences in ex ante information is significant. This second finding only becomes clear in the restricted sample.

Learning and WTP

We also test for the causal effect of learning on WTP directly. 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 issue.



(a) (b)
Figure 4: Opportunity to Learn, Learning and Willingness to Pay. Data is jittered to show density.

Figure 4 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 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 was provided the 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”.

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) doesn’t 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.

In order to account for the effects of learning on WTP, we estimate the following regression:

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

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.

Results from regression (3) are shown in Table 6. 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*.

TABLE 6: WTP on Learning Conditional on Being Treated

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.

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:

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

Results from estimating equation (4) are shown in Table 7. We find only very weak evidence that there could be an effect of excess information on WTP for the public good. We stress that this evidence is not robust and there is no clear pattern to it. 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 7: WTP on Excess Info Conditional on Being Treated

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.

Endogeneity concerns

There is still the potential concern, though, of endogenous effort related to learning. Assuming that learning is costly, if a subject cares about a topic it could be that there are willing to use more effort in order to retain information provided about that topic. As a result, the above estimate effect 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 experimental design to parse between different models of learning and updating with respect to subjects' valuation for a public good: flood protection. 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, consistent with models of fatigue. Our findings therefore suggest that learning is probabilistic 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. Conversely, ex ante knowledge matters a great

deal. As a result, we find evidence of costly learning and fatigue in the information based analysis. For the updating procedure, we find evidence consistent with both 1) Confirmatory Bias (Rabin and Schrag 1999) and 2) heterogeneous preferences and endogenous information acquisition decisions similar in spirit to Caplin and Dean 2015.

Due to our novel design, we can confirm with certainty that our effects are over new knowledge about a good's attributes as opposed to simply being provided information about a good's attributes. The design, eliciting ex ante and ex post knowledge levels, is the first of its kind to be implemented in the field, to our knowledge. 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.

Our results add to a literature which shows systematic deviations from individual subjects updating in accordance with a strictly neoclassical Bayesian framework in the sense that our results are consistent with confirmatory bias (Eil and Rao 2011, Grossman and Owens 2012, and LaRiviere et. al. 2014). However, our findings are similarly consistent with a model of heterogeneous preferences and endogenous information acquisition decisions. While the welfare implications of confirmatory bias have been developed in various settings (Rabin and Schrag 1999), the welfare implications of Bayesian updating models with endogenous search coupled with probabilistic and fatigued learning are less well understood in the context of valuation. Specifically, the marginal cost of learning is often assumed to be linear (Rothschild 1974). More theoretical work and experimental work parsing these two models, seems promising.

Lastly, we urge caution in interpreting these results more generally as well: it is possible that in relatively low stakes very micro level decisions, economic actors deviate from decision rules use in other circumstances. Decisions made with higher stakes and by experienced decision makers must be evaluate

as well. Further, more experimental designs are needed to parse between candidate models of updating in the context of richer class updating procedures, both Bayesian and non-Bayesian, we sketch in this paper.

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PART 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

Figure A1: Example Bullet Point Screenshot

eview / Test Mode
Show/Hide Comments
Invite Others to Test
Survey Settings
Testing Tools



Please read the following information about coastal flooding and flood defence in Scotland.


Managed realignment schemes can deliver a greater level of protection against coastal flooding than traditional sea defences alone.

Indicative costs and heights of sea defences with different widths of salt marsh fronting.
Costs presented in early 1990s prices.

3 m high crest 80 m wide saltmarsh Cost £400/m of seawall	6 m high crest 6 m wide Cost £1500/m of seawall
4 m high crest 60 m wide saltmarsh Cost £500/m of seawall	12 m high crest No saltmarsh Cost £5000/m of seawall
5 m high crest wall 30 m wide saltmarsh Cost £800/m of seawall	

Effect of saltmarshes on required seawall standards and consequent costs (Source: Adapted from Doody, 2008)

Coastal wetlands create new saltmarshes and mudflats which provide food sources for a wide variety of wildlife.




Prior to Realignment
Coast defences present
Little intertidal habitat

NOTE: This screenshot corresponds to quiz question number 4.

Figure A2: Policy Description Screenshot

view / Test Mode

Show/Hide Comments Invite Others to Test Survey Settings Testing Tools



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.



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