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What is the causal impact of information and knowledge in stated preference studies?

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ABSTRACT

This paper reports the results of a stated preference experiment designed to test how information about a good's attributes provided in a survey affects knowledge, and how knowledge affects preferences for that good. A novel experimental design allows us to elicit subjects' ex ante knowledge levels about a public 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. Consistent with previous work, ex ante knowledge does affect stated willingness to pay. However, we find no significant marginal impact of knowledge on the mean nor the variance of willingness to pay for changes in the environmental good *conditional* on ex ante knowledge. Our results are consistent with a number of conceptual models of information processing and preferences, including confirmation bias, costly search, and timing differences in learning and preference formation.

1. Introduction

Understanding how agents respond to information when choosing amongst or valuing goods is important. The economics literature has set out a number of alternatives to "traditional" models of costless learning and complete information retention. For example, models of bounded rationality, costly learning, fatigue, and cognitive load all imply agents do not completely absorb new information (Caplin and Dean, 2015; Caplin et al., 2011; Gabaix et al., 2006; Sims, 2003). There is a tension in some cases between neutral information processing (Gabaix et al., 2006; Tversky and Kahneman, 1973, 1974) and deviations from this (Eil and Rao, 2011; Fernbach et al., 2012; Grossman and Owens, 2012; Rabin and Schrag, 1999).

How people respond to information takes on added importance in the use of stated preference techniques to estimate demand for public goods. Mitchell and Carson (1989) identified information provision as "amongst the most important and most problematic sources of error" in contingent valuation surveys. Respondents are often asked to value complex and (in many cases) unfamiliar goods, and it is unlikely that all or indeed most respondents will have well-defined preferences prior to elicitation (Gregory et al., 1995; Gregory and Slovic, 1997). Preference construction is affected by how the respondent processes the information presented to them, which information they select and their own prior knowledge about the good

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(Payne et al., 2000). Schläpfer (2008) argues that it is unlikely respondents will form consistent preferences unless the survey offers reliable contextual cues and Bateman et al. (2008) argue that failing to accommodate low informed respondents will lead to high variance willingness to pay (WTP) estimates.

Best practices in stated preference contingent valuation surveys are that the survey instrument includes background information about a public good project. Johnston et al. (2017) state that: "stated preference questionnaires should clearly present the baseline (or status quo) condition(s), the mechanism of change, and the change(s) to be valued and should elicit evidence that these pieces of information are understood, accepted, and viewed as credible by respondents". Baseline information in stated preference studies should be neutral, deemed technically correct by experts and seen as relevant by stakeholders and pilot participants. But how much baseline information is "enough"? There is a tension between providing sufficient information needed to establish "baseline conditions" and over-loading respondents with superfluous detail. This paper attempts to test how additional neutral and relevant information impacts knowledge about the good and estimated WTP for a change in that good. A novel experimental design is used to cast light on what the appropriate level of "baseline information" is in a contingent valuation study.

We are interested in the interaction between two important strands of the literature. First, how much a subject knows about a good *before a survey begins* is often correlated with their WTP (Cameron and Englin, 1997; Loomis and Ekstrand, 1998; Tkac, 1998): subjects tend to know more about things they care about or have experience with (Czajkowski et al., 2015a). Second, information about the management, characteristics and attributes of a public good *provided during a survey* has been found to both influence and 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 during a stated preference survey on WTP. Specifically, could the effects on stated WTP of providing information be driven by heterogeneity in the ex-ante information sets of people in the sample? Put another way, are less informed subjects more likely to be influenced by "new" information about the good? This relates to a normative question for survey instrument design: what is the appropriate *quantity* of information about a good to provide to subjects?[1]

This paper reports the results of an experiment designed to test for how providing information about the attributes of an environmental public good affects respondents' knowledge; and how that new knowledge affects the distribution of valuations for the public good *conditioning* on a subject's ex ante knowledge levels. We define information, learning and knowledge as follows: information is a specific fact; learning is being able to recall a specific fact from a choice set (e.g., in a multiple choice test); knowledge is a set of learned information. 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 experimentally 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 subject's ex ante level of knowledge. We are not aware of prior work in the literature which does this. Our design also provides an opportunity to compare different cognitive models of learning and stated valuation adjustment which has received increasing attention in the economics literature.

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. This is consistent with a model of imperfect learning and fatigue. Second, new knowledge about a good's attributes does not significantly affect valuations for that 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, the additional information did not affect the variance of the distribution of valuations.

Our paper contributes to the literature in several ways. First, 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 (Bergstrom et al., 1989; Boyle, 1989; Hoehn et al., 2010; Smith et al., 1988). As a result, we can estimate average treatment effects (ATEs) rather than intent to treat (ITT) effects.

Second, we are also able to test for the causal impact of information and learning, conditioning on ex ante levels of knowledge. Previous literature has shown that different ex ante levels of information can significantly change stated WTP (Cameron and Englin, 1997; Loomis and Ekstrand, 1998; 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. However, Boyle (1989) was not able to identify if this effect varies across the population by information sets.

[1] A related question we do not address here is: conditional on quantity, what is the right composition of information about a good to provide subjects. This paper focuses on the quantity question surrounding information provision (extensive margin) only but similar issues exist for the composition questions around information provision (intensive margin); see Munro and Hanley (2001) for a summary of early work on this issue.

Third, we focus specifically on the 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'Connor, 1984), the relative cost of management versus a subject's income (Bergstrom et al., 1989), and specificity of management actions (Boyle, 1989). Our paper estimates the marginal impact of both information and acquired knowledge about public good attributes on WTP, conditional on prior knowledge levels.

In addition, because we are able to test for ex ante and ex post knowledge states, vary levels of 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 (Aadland et al., 2007; Caplin and Dean, 2015; Caplin et al., 2011; De los Santos et al., 2012; Rabin and Schrag, 1999). 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. To our knowledge, this is the first such comparison in a stated preference survey. 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) heterogeneous preferences and endogenous costly information acquisition decisions, similar in spirit to Caplin et al. (2011) and Caplin and Dean (2015) and 3) a timing lag between learning and preference formation.

2. 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.

2.1. Prior valuation studies

Early stated preference research began to question how the quantity and quality of information provided in surveys influences both the mean and variance of the WTP estimate. Results were mixed: some studies showed that increasing information provided did not affect mean WTP, but the variance of the estimate reduced with increasing information (Boyle, 1989; Bergstrom et al., 1989). Other studies showed that providing more positive information increased mean WTP (Bergstrom and Stoll, 1990) but that there is a level of saturation where further new information no longer affects the estimates (Munro and Hanley, 2001).

Further research questioned the role of a respondent's prior knowledge on uncertainty in the WTP estimate (Loomis and Ekstrand, 1998). Tkac (1998) used a quiz to test the respondent's prior knowledge on the good being valued and found that increased prior knowledge was positively correlated with WTP although these respondents were less receptive to new information. Hoehn and Randall (2002) extended this work and found that the effect of new information was uneven across respondents with some respondents revising their WTP upwards and some revising it downwards in response to new information.

More recent work has used "quizzes" as a means of testing respondent's prior knowledge of the good in question and investigated how this affects their WTP estimate and the interpretation of the information provided to them during the survey. Hasselström and Håkansson (2014) found that WTP differed significantly between the "detailed" and "fuzzy" information sets for low knowledge respondents but not for high familiarity respondents. Recent work on the valuation of cold water corals in Norway also used a quiz to examine respondent's knowledge and familiarity with the good (LaRiviere et al., 2014). An eight-question quiz grouped respondents into high and low knowledge following an initial presentation on cold water corals. LaRiviere et al. found that more knowledge led to respondents being more consistent in their choices, whilst those who scored above the mean were prepared to pay significantly more towards cold water coral protection. Using the same dataset, Sandorf et al. (2017) demonstrated that respondents with more knowledge were more likely to attend to the attributes in choice experiments. Jones et al. (2017) show that describing types of value which can be created from a project in different ways (e.g., not describing versus describing them) can impact WTP studies. Results indicated that between 78% and 94% of respondents learned something new and useful from the new information provided to them.

We embedded our experiment within a stated preference survey concerning a population's WTP for a project to restore coastal (estuarine) wetlands as a way of mitigating flood risk (known as managed realignment). Managed realignment offers several benefits over traditional hard concrete defenses. Estuarine wetlands make use of the storm buffering capacity of intertidal habitats, as well as providing additional floodplains during high tides and storm surges (King and Lester, 1995). In addition, wetlands also provide amenity value by increasing habitat for wildlife, along with numerous other ecosystem service benefits such as pollution reduction and acting as a nursery for early-life stages of fish and shellfish (Barbier, 2011).

When planning managed realignment schemes there is a need to engage with the general public as there is a legacy of local residents being opposed to such schemes (Ledoux et al., 2005).

We designed a contingent valuation survey to value a single specific managed realignment scheme on the Tay Estuary in Scotland. The survey was designed following the recommendations of Carson (2000)[2]. An initial focus group was held to refine both the information and the valuation portions of the survey with staff and students at the University of Stirling. This was followed by a pilot survey sent to 250 households within the study region, to which 50 households responded. The survey sample was restricted to Scottish residents within the local authority areas who would be responsible for paying for the managed realignment scheme via their local council tax. These restrictions were applied to the Scottish Phone Directory database, which holds the names and addresses of the population based on the Electoral Register, and 4000 households were randomly selected to take part in the final survey. Due to the experimental design, the survey could only be undertaken online, using a website we designed and operated via Survey Gizmo. Respondents were invited to take part through targeted postal mailings. Respondents received a letter on University of Stirling headed paper 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. Of the 4000 people contacted, 749 people completed the online survey with 593 usable (fully completed) responses: a response rate of 15%. The response rate is comparable with a similar UK wide stated preference survey for flood defense (12%) (Joseph et al., 2015), as well as other UK postal stated preference surveys that had response rates ranging from 11% to 22% (Burton et al., 2001; Hanley et al., 2010).

The order of the survey was as follows. Subjects were 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 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 policymakers 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. 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 nine questions reflect the kinds of additional information commonly provided in stated preference surveys. They cover ecosystem service co-benefits of the new managed realignment scheme, the historical baseline, and the environmental problem (here, increasing flood risks). Providing this type of background information is in line with best practices (Johnston et al., 2017). Each of the bullet points we provide is relevant for the case we consider. For example, in talking about the current baseline type of flood protection we include a question and bullet point about existing flood protection methods. We do the same regarding coastal wetlands and their impact on waterbird populations. In designing a survey instrument the designer must make a call about what background information to include and what not to include. This paper distils that background information about the project (e.g., historical flood protection and ecosystem relationships) into nine bullet points then randomly varies the amount provided to subjects.

Following the quiz, the managed realignment 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 the natural flood defense would take to become effective. A status quo scenario of continued reliance on existing hard defenses with no managed realignment was also included. The cost of the project was described as being increases in respondent's council tax to fund the scheme; respondents were told that this cost was currently subject to some uncertainty, motivating the use of a payment card. Council tax was a plausible payment vehicle as local authorities are responsible for funding flood defense in Scotland. We then elicited WTP for the managed realignment scheme using a payment card ranging from £0 to £150: respondents were asked to tick all the amounts which the household was WTP towards the scheme. The values were chosen based on feedback from initial focus groups. Immediately following the WTP elicitation, respondents repeated the original nine-question quiz. A series of debriefing questions followed, including 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.

To foster policy consequentiality, several reminder cues indicating that the results would be shared with policymakers were included in the survey, specifically:

> "The price you choose will be used to inform the local authorities and the Scottish Government when deciding future flood defense options in the Tay Estuary."
> "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 defense policy is chosen"

We chose to use a payment card format for three 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

[2] A copy of the survey is available on request from the corresponding author.

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

Box 1: Elicitation scenario in the contingent valuation survey.
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 defense 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 defenses (seawalls) 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 realized.

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 defense policy is chosen.

in estimating the relative value of different attributes of the project or different ecosystem service values. Second, 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 format often gives rise to inconsistencies between the initial and second bid response (Bateman et al., 2008; Whitehead, 2002). 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. Moreover, as we are interested in the treatment effect of information provision and not in the aggregate benefits of the project in this paper, we view anchoring bias as a second order problem. However, we recognize that payment cards are not widely viewed as incentive compatible (Carson and Groves, 2007). In particular, there are concerns that payment cards can lead to participants under-revealing demand for the good and that in many cases payment cards do not follow an implementation rule.

However, our scenario and the payment card we employ reflect the recommendations of Vossler and Holladay (2018) who considered incidences where payment cards can be incentive compatible. Our payment card was presented as a series of yes/no votes and we asked subjects to answer "yes" to each payment level that the household would definitely be willing to pay. We highlighted that the overall cost of the wetlands project was uncertain, explaining why a range of payments options were offered. The elicitation scenario is provided in Box 1.

It is useful to reemphasize the goal of the experiment at this stage: we are interested in the causal marginal impact of a larger quantity of information relating to a good's attributes on stated WTP. There are other types of information, which are important (e.g., reminders of budget constraints, statements about consequentiality, etc.). We focus narrowly on the quantity of information about public good attributes. A slightly different question regards the *content* of a *given* amount of information or public good project, but we leave that analysis for future work.

2.2. 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

Table 2
Type - treatment pairs.

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

Columns of the table represent 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.

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 knowledge 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 questionnaire. 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.

Importantly, respondents were always given information they answered correctly before any additional information was given as dictated by treatment. For example, assume respondent A gets questions 2 and 7 correct and that they 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. At no point was a subject told the number of questions which they answered correctly, that they correctly answered any particular question, nor that the information they were given related to quiz material. In this respect, the only difference between a standard stated preference survey and our survey before eliciting WTP was a short multiple choice quiz at the beginning.

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.

The type-treatment pairs and treatment information sets are summarized 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. There are up to nine potential type-treatment pairs 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 WTP 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 experimental design provides us with the opportunity to verify that information is actually learned. One cost of this design, though, is that we must give subjects a quiz before eliciting WTP. 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.

From an experimental design perspective, we chose not to incentivize learning by paying subjects for answering correctly, in order to mimic common practice in stated preference studies as closely as possible. This decision provides external validity within the stated preference literature where best practices say nothing about incentivizing learning, nor how incentivizing learning might impact stated WTP (Johnston et al., 2017). We are not concerned with experimenter demand effects because (save the pre-survey quiz discussed above) the survey mimics best practices for a payment card survey. Finally, in the econometric analysis, we intentionally weight each question equally in order to align the empirical design with the research question of identifying how the *quantity* of information in the survey impacts WTP measures, conditional on ex ante knowledge levels. We are interested in learning about both how people use the amount of learned knowledge in stated preference surveys and in how economists can improve on survey design. Given that common practice is to give survey subjects a wide variety of information about the attributes of the good being valued, equal weighting provides the appropriate counterfactual.

2.3. 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. Appendix A provides a more detailed discussion about how the hypotheses fit into the literature on behavioral economics as discussed in the first paragraph of the introduction.

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:

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

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

Eqs. (1) and (2) include a vector X of self-reported subject specific demographic characteristics.[3] In Eqs. (1) and (2), as before, the capital letter pairs (e.g. LH) stand for the ex ante score and the information treatment respectively (e.g., the treatments in Table 2). There are two left-hand side variables which we consider separately but in both cases, we leverage experimental variation in treatment and use a simple OLS econometric model. Due to clean, experimental variation, OLS is sufficient to describe the causal effect of treatment on our outcomes of interest, although we corroborate our results using parametric estimation of WTP distributions.

The first LHS variable considered 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 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. This provides the right apples to apples comparison across treatments.

The second equation is defined similarly with stated WTP (*WTP*)[4] 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 fitted both a normal and spike distribution to the WTP data to estimate both the mean and variance of treatment's impact on WTP. Since results are consistent across these specifications we focus here on the OLS estimates, as our primary

[3] 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 WTP 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.

[4] 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.

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.

For all of our analysis, the correct control group for any treated group to identify the causal impact of information on preferences conditional on a particular ex ante information must be subjects with the same level of ex ante information. For example, the proper control group for a subject in the LH treatment is a subject in the LL treatment. Both subjects test into a specific amount of ex ante knowledge (e.g., L) but are randomly assigned information treatments H versus L (e.g., nine pieces of information versus three). As a result, LH types are identical to LL types in every way with respect to the amount of ex ante knowledge as it relates to the quiz except for LH types being exposed to six “unknown” pieces of information. The same logic holds for types LM versus LL. Comparing LM to LH compares six versus three unknown pieces of information conditional on ex ante L information levels. Comparing MH to MM compares three new pieces of information conditional on M ex ante information levels.[5] As a result, the correct counterfactual for any comparison are subjects with the same level of ex ante information but different information treatments. To this end, comparing ex ante L types to ex ante M types is not the main comparative static of interest since those subjects have different levels of ex ante information (e.g., the average number of correct answers in L versus M is two versus five).

By jointly analyzing the coefficient estimates of Eqs. (1) and (2) we can evaluate whether the results of our experiment is consistent or not 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 fully dependent on attribute information. See Appendix B for a mapping of coefficient estimates to alternative models of learning.

3. Results

Table 3 shows the distribution of Type –Treatment pairs in our sample. Twelve subjects scored 7, 8 or 9 on the first quiz. As a result, there are only twelve *a priori* type H subjects meaning there are only twelve subjects in the HH treatment. 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 (e.g., type L ex ante).

Table 3 also reports socio-demographic characteristics by treatment type. Comparing the characteristics of the local authority populations to our sample revealed our sample was not fully representative. 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 the 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%. This would have implications for calculating the aggregate WTP for the managed realignment scheme and appropriate weightings would need to be applied.

3.1. Treatment and learning

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

Fig. 2 compares total correct, incorrect and “I don’t know” responses across the first and second quizzes. There is little difference between the incorrect responses between the two quiz rounds, however, the proportion of “I don’t know” responses falls significantly between the two rounds. This suggests that people who were unsure in Round 1 were those who read the new information more carefully and learned this new information. Those who guessed incorrectly but were not told they had guessed incorrectly perhaps did not engage with the new information provided. For the remainder of the analysis “I don’t know responses” were treated as “incorrect responses”, however, we are interested in the information learned by respondents as judged by the increased quiz score, not the difference between incorrect and I don’t know respondents.

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 the second quiz score. To highlight treatment effects, we exclude a constant in this regression specification.[6] We report four specifications: the full sample with and without self- reported demographic controls including survey round, education, gender, flood threat indicators, property owner, and environmentalist and only

[5] The design permits some ex ante heterogeneity within Type-Treatment pairs. For example a subject in LL could view zero, one, two or three new pieces of information. On average, though, subjects in the LM group will see more. We show this explicitly below. Average differences preserve internally valid experimental design. Having treatment groups defined as the number of correct answers was not feasible given our sample size.

[6] 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 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. Not all subjects in each Type-Treatment pair answered all socio-demographic questions. We investigate this in detail below.

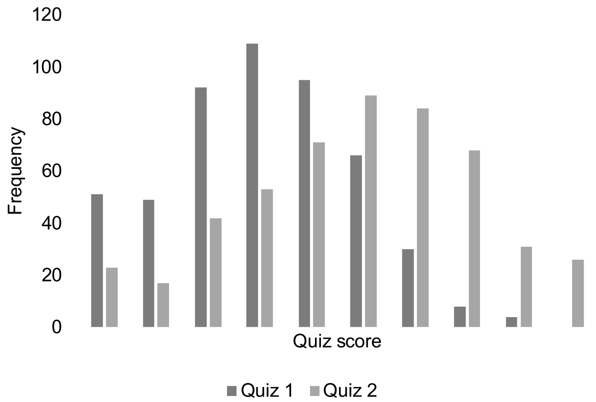

Fig. 1. Comparison of the first and second quiz scores.

individuals who self-reported perceiving their responses as being consequential to the likelihood of the managed realignment scheme taking place, 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.[7]

Table 4 has two key features. First, for every specification, providing more information to subjects increases retained information (knowledge). Some of these increases, though, are not statistically significant. Second, the rate of informa-

[7] Balancing tables available upon request confirm we randomized properly.

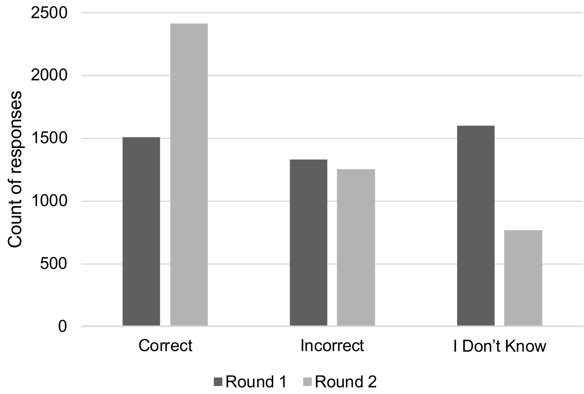

Fig. 2. Comparison of correct, incorrect and I don't know responses for quiz rounds 1 and 2.

Table 4
Regression of the second quiz score on type-treatment pairs.

Variables	(1)	(2)	(3)	(4)
LL	3.53***	4.07***	3.43***	5.15***
	(0.06)	(0.46)	(0.10)	(0.56)
LM	0.95*	1.30***	1.10*	1.32**
	(0.44)	(0.13)	(0.48)	(0.29)
LH	0.44	0.56	0.038	0.26
	(0.32)	(0.28)	(0.475)	(0.39)
MM	5.40***	6.06***	5.13***	6.72***
	(0.15)	(0.51)	(0.24)	(0.77)
MH	0.88***	0.91***	1.00***	1.08***
	(0.13)	(0.12)	(0.157)	(0.18)
HH	8.17***	8.62***	8.14***	9.42***
	(0.21)	(0.11)	(0.25)	(0.28)
Observations	504	431	247	179
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	0.87	0.89	0.88	0.92

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.

tion 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 0.04 to 0.56 pieces out of three for "high levels" of new information.

As a result, we fail to reject the null hypothesis of 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.

3.2. Willingness to pay effect

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 WTP of £44.77 per annum (*S.D* = 46.21). The main reasons for not being prepared to pay were not being able to afford to contribute (26%) and belief that it is the Scottish Government's responsibility to fund flood defense (27%). Other potential reasons for not being willing to pay were i) respondents not believing that managed realignment is an effective flood defense ii) respondents not believing there is a need to invest in flood defenses and iii) respondents preferring to spend their income on other things

Table 5
Comparison of WTP 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
Regression of WTP (mid-points of selected interval ranges) on type-treatment pairs.

Type - Treatment Pairs	(1)	(2)	(3)	(4)
LL	45.17***	67.82***	58.33***	64.17***
	(4.77)	(3.75)	(4.79)	(6.776)
LM	6.31	−3.37	0.17	−9.24
	(6.04)	(6.99)	(8.53)	(8.72)
LH	−9.46**	−10.55***	−2.72	−5.92
	(2.26)	(2.14)	(8.17)	(8.49)
MM	37.99***	57.22***	36.89**	37.08***
	(7.17)	(3.24)	(10.63)	(5.84)
MH	9.67	7.59	13.21	7.81
	(8.14)	(5.05)	(14.53)	(11.35)
HH	45.00**	59.65*	37.14*	24.03
	(12.51)	(21.69)	(16.17)	(13.82)
Observations	504	431	247	179
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	0.49	0.627	0.547	0.72

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 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.

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 respondents who were most concerned about flooding were willing to pay more for the managed realignment scheme. Including the demographic controls does not significantly alter the effects of the treatment groups on WTP. We henceforth leave out any discussion of demographic controls to focus attention on treatment effects.

There are three aspects of Table 6. First, there are some differences in stated valuations according to whether subjects said that they believed the survey would be used for policy decisions or not (that is, whether they believed their responses would be outcome-consequential). In some cases, there are level differences (e.g., the MM and LH coefficients in specifications (2) and (4)). In most cases the standard errors increase as well (e.g., LL standard error in (2) versus (4)). Consistent with previous field studies on consequentiality we find evidence that beliefs about consequentiality are positively correlated with stated WTP (Herriges et al., 2010; Hwang et al., 2014; Interis and Petrolia, 2014; Vossler et al., 2012; Vossler and Watson, 2013; Czajkowski et al., 2017).

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 0.01). 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; Tkac, 1998).

Third, 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

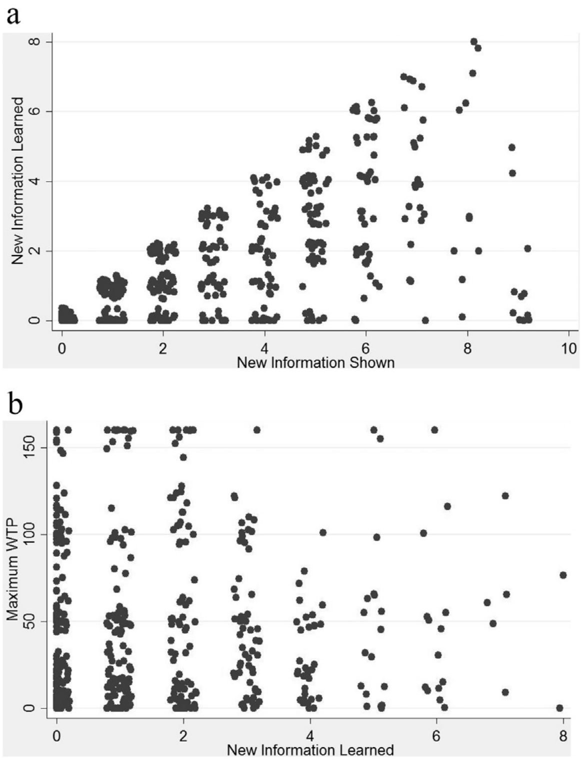

Fig. 3. A: New information learned versus new information shown i.e. opportunity to learn. The average number of new bullets show is 3.11, the average number learned is 1.38. n = 482. B: New information learned and maximum willingness to pay. Mean retention rate by treatment: LL = 0.51, LM = 0.55, LH = 0.50, MM = 0.54, MH = 0.54, HH = 0.63. n = 482.

stated WTP. 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)[8].

3.3. *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 with 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.

Fig. 3 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 Information Shown* 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 *New Information Learned* which is defined as the number of new pieces of information which the subject learned. Put another way, *New Information Learned* is the number of correctly answered questions on the second quiz which the subject both didn't correctly answer on the first quiz and which was subsequently provided in an information bullet. This lets us be certain that on average subjects learned the bullet point due to the information presented as opposed to guessing the correct answer on the second quiz randomly. Hence, *Info Bullets Learned* is less than or equal to *New Information Shown* by definition. Lastly, the ratio of *New Information Learned* to *New Information Shown* we call the "retention ratio".

Panel (a) confirms the findings regarding 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,

[8] 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.

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 artefact breaking the marginal effect of learned information into ex ante level of information bins.

3.4. 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,b) 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 is some models like costly attention or confirmatory bias, agents are less likely change these valuations.

We perform a joint test of treatment on the variance and mean of the WTP distribution.[9] Consistent with our OLS findings, we do not find an effect of additional information on mean WTP and there is no impact of additional information on the variance of WTP. This result is somewhat surprising: even though additional information does not impact mean WTP, a model of Bayesian updating suggests that additional information should tighten the distribution around an (unchanging) mean (Czajkowski et al., 2015a,b). Even if type L or M subjects update around an unbiased (albeit imperfectly informed) mean WTP, 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.[10] Replication in other contexts is needed.

4. Discussion and conclusion

This paper reports the results of a novel experiment to identify the causal effect of learning and knowledge about an environmental good on how people value 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. 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. There is, however, an endogeneity concern: if a subject cares more about the topic it could be that they are willing to use more effort in order to retain the additional information provided. This was tested for by including personal relevance and motivation variables when regressing type-treatment pairs on the second quiz score. Flood risk characteristics were not significant, which suggests there was no relationship between personal motivation and learning.

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, 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 et al. (2011): agents use costly effort to seek out and learn information up to the point where the expected marginal cost of learning is equal to the expected marginal benefit[11]. If endogenously acquired knowledge about "good" attributes is 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: for example, people in the floodplain may both know more about the flood mitigation potential of wetlands and be willing to pay more for wetland restoration. This type of model also bears some similarities to other recent behavioral models of costly attention (Caplin et al., 2011; Hanna et al., 2014; Schwartzstein, 2014).

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

[9] Full discussion the analysis and table of results can be found in Appendix B.

[10] 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 marginal impact of knowledge on preferences across subjects in different treatments (e.g., the scale parameter). This is an intriguing line of future research.

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

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[12].

Third, it could be that learning can occur instantaneously but preference formation takes time. Given that our experiment takes place over 10–25 min, 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. Further, from a stated preference survey design perspective, the 10–25 min time interval is the relevant one.

A drawback of this research was not including a question on preference uncertainty following the WTP question. Loomis and Ekstrand (1998) demonstrated in their work on owl conservation that respondents with a higher prior knowledge of the good were more certain about their preferences. A useful addition to this survey would have been to explore whether preference uncertainty was influenced by respondents' prior knowledge or the additional information presented to them. Furthermore, allowing respondents more variability in the payment card format i.e. being able to tick the amounts they are certain they would pay and being allowed to leave a gap between that and the amounts they certainly would not pay, as used by Hanley et al., (2009) may have also shown differences between the type-treatment groups.

We recognize that a nine-question multiple choice quiz is an imperfect measure of knowledge. There are two issues: one that the test is multiple choice and the other that it is only nine questions. However, almost all OECD countries use multiple choice questions to test students' knowledge of use. The SAT is the most important single test for college placement in the U.S. and is almost entirely multiple choice. Economics faculties also often use multiple choice tests to evaluate student knowledge. Another issue is that the length of the multiple choice quiz is too short. Here we rely on the law of large numbers: on average less informed subjects will perform worse on a multiple choice quiz than more informed subjects. Because we give the test to over 500 subjects, on average the people that perform well on the test know more and the people that perform worse know less. While one individual might get lucky, we have a sufficiently large sample size so that on average L types know less about the information the quiz than M types who know less than H types. Put another way, a single low knowledge type who is classified as an M due to luck will wash out on average. As further evidence that types are not random, L types learn more when given the H treatment relative to the M treatment relative to the L treatment. In sum, because of the widespread use of multiple-choice tests in education, the law of large numbers and the statistically significant evidence that subjects in different ex ante groups learn relatively more when given additional information, we are confident in the validity of using a nine-question multiple choice quiz to get signal on knowledge levels.

A further limitation of our survey could be the high proportion of respondents supporting the proposal (83%). Flooding and flood risk is an emotive issue, and as the results show a high number of respondents supported the scheme. This raises the question as to whether our findings would be valid for a less preferred or less familiar good. Respondents who were most concerned about whether their home or local area would be protected were likely to be solely interested in the flood reduction benefits of the good (information on which was presented to all respondents in the standard elicitation scenario) and chose to ignore additional information on ecosystem service provision when forming/stating their preferences. This may not be the case for a more unfamiliar or less well received good.

The fact that the new information provided to respondents in our survey did not affect the mean or the variance of the WTP distribution could have implications for stated preference instrument design if the results are shown to be externally valid. We find that respondents' ex ante (pre-survey) level of knowledge affects the value they place on the environmental good. Respondents learn some of the information provided to them, but the marginal effects of this information are declining. In line with more general work in economics, our results imply that survey respondents in our study could exhibit some of the same "behavioral" updating rules recently discussed in the literature. Similar mechanisms developed to address these issues in revealed preference markets might be useful in stated preference markets as well if the results from this single study are externally valid. In that sense, we view this paper as one of many linking behavioral updating and preference formation models to stated preference surveys.

However, we urge some caution in interpreting our findings, as further research is needed to identify their robustness and transferability within stated preferences. 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 should, therefore, be evaluated as well. Further, more experimental designs are needed to parse between alternative models of learning, preference formation and value updating which we sketch in this paper.

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

Table A1
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}	–	–

Appendix A. Alternative models of updating

In this paper we are able to horserace several different learning and preference formation: 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).

A.1 Learning hypotheses

One way to think about the causal effect of information on knowledge is from estimating Eq. (A1).

$$Q2\,Score_i = X_i'\gamma + 1\left\{LL_i,\ LM_i,\ LH_i\right\}\Gamma_{LL} + 1\left\{LM_i,\ LH_i\right\}\Gamma_{LM} + 1\left\{LH_i\right\}\Gamma_{LH} + 1\left\{MM_i,\ MH_i\right\}\Gamma_{MM} + 1\left\{MH_i\right\}\Gamma_{MH} + 1\left\{HH_i\right\}\Gamma_{HH} + \varepsilon_i \tag{A1}$$

Table A1 highlights the possible ex ante and ex post information levels. 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) is 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 the 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: $\Gamma_{LM} = \Gamma_{LH} = 0$, $\Gamma_{MH} = 0$

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.

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: $\Gamma_{LH} < \Gamma_{LM}$

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.

A.2 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 Eq. (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 Eq. (A2). In order to interpret each model, there are restrictions on the learning results which must coincide with the valuation based preference results.

$$WTP_i = X_i'\gamma + 1\left\{LL_i,\ LM_i,\ LH_i\right\}\omega_{LL} + 1\left\{LM_i,\ LH_i\right\}\omega_{LM} + 1\left\{LH_i\right\}\omega_{LH} + 1\left\{MM_i,\ MH_i\right\}\omega_{MM} + 1\left\{MH_i\right\}\omega_{MH} + 1\left\{HH_i\right\}\omega_{HH} + \varepsilon_i \tag{A2}$$

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 the 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 (Caplin et al., 2011).

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 are 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.

1) 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 factors rather than knowledge of the good's attributes drives heterogeneous preferences for goods.

Table A2
Comparison of different parametric distributions fitted to the interval WTP data.

	Parameters	LL	AICc/n
Normal	2	−1582.89	6.33
Logistic	2	−1576.22	6.30
Extreme Value	2	−1682.91	6.73
Generalized Extreme Value	3	−1399.22	5.63
t Location Scale	3	−1582.89	6.36
Uniform	2	−1579.70	6.32
Johnson SU	4	−1376.31	5.56
Expotential	1	−1372.33	5.47
Lognormal	2	−1378.31	5.52
Loglogistic	2	−1381.19	5.53
Weibull	2	−1356.27	5.43
Rayleigh	1	−1706.46	6.80
Gamma	2	−1354.35	5.42
Birnbaum-Saunders	2	−1373.11	5.50
Generalized Pareto	3	−1362.63	5.48
Inverse Gaussian	2	−1403.87	5.62
Nakagami	2	−1413.76	5.66
Rician	2	−1706.46	6.82
Johnson SB	4	−1370.03	5.54
Johnson SL	4	1376.31	5.48
Poisson	1	−10,033.66	39.84
Negative Binomial	2	−1376.12	5.51

2) 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.

We perform a joint test of treatment on the variance and mean of the WTP distribution. We use individual interval WTP responses to fit several parametric distributions of WTP.[13] 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.[14] 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 the maximum likelihood method to fit a parametric distribution to our interval data.

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:

$$L_i = Pr\left(LB_i \geq WTP_i > UB_i\right) = CDF\left(UB_i, \boldsymbol{\beta}_i\right) - CDF\left(LB_i, \boldsymbol{\beta}_i\right), \text{(A1)}$$

where $\boldsymbol{\beta}_i$ 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.[15]

Because there is no a priori or theory driven-guidance on the shape of the distribution of WTP in the population, we tried over twenty most commonly used parametric distributions to see which one fits our data best (Table A2) 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.[16]

[13] 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.

[14] 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.

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

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

Table A3
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	5.42		5.29		5.59		5.44	

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.

Table B1
IV regressions of WTP on second quiz score (ex ante low knowledge).

	(1)	(2)	(3)	(4)
Quiz 2 Score	−0.13	−2.78	−0.999	−6.68
	(4.49)	(3.82)	(7.98)	(6.53)
Constant	46.59**	43.57**	61.71*	67.22**
	(18.69)	(18.73)	(32.40)	(34.08)
Observations	301	258	135	94
Controls	N	Y	N	Y
Consequential Sample Only	N	N	Y	Y
R-squared	<0.01	0.06	<0.01	0.12

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.

Using the estimated parameters of the Gamma distribution we then simulated the mean and standard deviation of the WTP distribution (Table A3). 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.

Consistent with our OLS findings, we do not find an effect of additional information on mean WTP. In each specification, there is no impact of additional information on the variance of WTP. This result is somewhat surprising: even though additional information does not impact mean WTP, a model of Bayesian updating suggests that additional 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.

Appendix B. Additional willingness to pay specifications

B.1 Instrumental variables regression

We use two different IV specifications: first, we restrict the estimating sample to be the ex ante low knowledge group (Table B1) Second, we restrict the ex ante medium knowledge group (Table B2). In neither case do we use any information on the ex ante H information group: 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. For both groups and in every specification,

Table B2
IV regressions of WTP (ex ante medium knowledge).

	(1)	(2)	(3)	(4)
Quiz 2 Score	11.06	8.24	13.18	11.36
	(7.90)	(7.20)	(9.62)	(8.538)
Constant	−21.74	−15.38	−30.73	−55.15
	(46.21)	(43.53)	(54.37)	(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.

Table B3
Regression of WTP on learning.

VARIABLES	(1)	(2)	(3)	(4)
*0-3 New Information Shown * New Information Learned*	−0.887	0.08	−2.07	0.85
	(2.43)	(2.28)	(3.84)	(4.31)
*4-6 New Information Shown * New Information Learned*	0.48	0.27	0.36	0.37
	(1.31)	(1.35)	(2.15)	(2.39)
*7-9 New Information Shown * New Information Learned*	0.99	0.86	1.57	2.13
	(1.68)	(1.39)	(3.08)	(2.18)
Constant	44.47***	63.07***	51.28***	53.51**
	(3.09)	(12.11)	(4.766)	(21.16)
Controls	N	Y	N	Y
Consequential	N	N	Y	Y
Observations	504	431	247	179
R-squared	<0.01	0.25	<0.01	0.35

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.

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.

We supplement these findings 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).

B.2 Opportunity to learn on willingness to pay

We account for the effects of learning on WTP in a second way to consider for differing opportunities to learn (based on different information treatments) (Eq. (B1)). In Eq. (B1) 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.

$$\begin{aligned} WTP_i = \alpha + X'\gamma + 1\left\{0-3\,New\,Information\,Shown\right\} * (New\,Information\,Learned)\ \beta_L \\ + 1\left\{4-6\,New\,Information\,Shown\right\} * (New\,Information\,Learned)\ \beta_M \\ + 1\left\{7-9\,New\,Information\,Shown\right\} * (NewInformation\,Learned)\ \beta_H + \varepsilon_i \end{aligned} \tag{B1}$$

We find no evidence in any specification that there is any causal effect of learning on stated WTP (Table B3). This. n-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 the 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 *New Information Learned* as we've done with *New Information Shown.*

B.3 Unlearned information and willingness to pay

We repeat regression (B1) but change the continuous variable to *Excess Info*. We define *Excess Info* to be the amount of unlearned new information provided to subjects (Eq. (B2)). We would like 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

Table B4
Regression of WTP on excess information.

Variables	(1)	(2)	(3)	(4)
*0-3 New Information Shown * Excess Info*	−1.81	−2.91	−3.89	−6.41
	(2.90)	(2.87)	(4.12)	(4.35)
*4-6 New Information Shown * Excess Info*	2.00	0.52	2.46	1.86
	(1.74)	(1.64)	(2.63)	(2.52)
*7-9 New Information Shown * Excess Info*	−0.63	−2.06*	0.86	0.54
	(1.40)	(1.25)	(2.32)	(2.33)
Constant	44.12***	64.38***	50.48***	55.30***
	(2.66)	(12.33)	(4.315)	(21.10)
Controls	N	Y	N	Y
Consequential	N	N	Y	Y
Observations	503	430	247	179
R-squared	<0.01	0.26	<0.01	0.37

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.

quantity of information provided to subjects could be important in many economic situations.

$$WTP_i = \alpha + X'\gamma + 1\left\{0-3\,New\,Information\,Shown\right\} * (Excess\,Info)\;\beta_L + +1\left\{4-6\,New\,Information\,Shown\right\} * (Excess\,Info)\;\beta_M + 1\left\{7-9\,New\,Information\,Shown\right\} * (Excess\,Info)\;\beta_H + \varepsilon_i \tag{B2}$$

We find only weak to no evidence that there could be an effect of excess information on WTP (Table B4). 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 Information Shown*). However, this effect doesn't persist across different levels of newly shown information (e.g., *4–6 New Information Shown* or *7–9 New Information Shown*). We have performed the same regression controlling for the amount of learned information and the results are similar.

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