Signaling Probabilities in Ambiguity: on the impact of vague news

Dmitri Vinogradov
University of Glasgow

Yousef Makhlouf
Nottingham Trent University

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JEL Classification: C90, D01, D81

Keywords: ambiguity-aversion, ambiguity premium, Ellsberg experiment, vague news.

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Adress for Correspondence: Dmitri Vinogradov, Adam Smith Business School, University of Glasgow, Gilbert Scott Building, University Avenue, Glasgow G12 8QQ. Email: Dmitri.Vinogradov@glasgow.ac.uk

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Abstract

Vague, or imprecise, news may affect decisions by changing either the fundamentals, or the associated uncertainty, or both. We show response to vague news is shaped by ambiguity attitudes, yet with qualitative differences for different levels of risk, on top of ambiguity, conveyed. The decision functional consists of a probabilistic term and an ambiguity premium; the latter depends on both risk and ambiguity, implying differential responses of ambiguity-neutral, -averse and -seeking subjects to probabilistic, as well as non-probabilistic news. In a two-color Ellsberg experiment with signals we obtain ambiguity attitudes matter more for non-probabilistic and less for probabilistic, though still imprecise news. For vague news conveying a relatively high probability of success, subjects exhibit insensitivity to the ambiguity component, unless explicitly facing similar news of different degrees of precision. Possible explanation is in either flat ambiguity premiums, or the cognitive inability to process the risky and the ambiguous components simultaneously.

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1 Introduction

News affects behavior of individuals and whole markets even if the conveyed message lacks precision. In March 2016, the Fed's Chair Janet Yellen commented[1] on the US monetary policy by saying "I consider it appropriate for the Committee to proceed cautiously in adjusting policy," - and the US markets rose already during her speech.[2] Although the exact probability of future rate hikes and their timing remains unknown, evidently even vague messages matter for investment decisions. Imprecise policy communication is not the only instance of this effect. Qualitative corporate news (lacking numbers and hard evidence) stimulates trading activity of short sellers (von Beschwitz et al., 2017) and drives stock prices even if it bears little factual information (von Beschwitz et al., 2015; Boudoukh et al., 2013). Service quality signals of various precision and reliability, available through online reviews and ranking systems, influence consumer choices (Vermeulen and Seegers, 2009). Players' decisions in game shows are sensitive to moderators' comments even if those are ambiguous.[3] Although vague messages may communicate information relevant for decisions, one cannot neglect "... the ambiguity of this information, a quality depending on the amount, type, reliability and "unanimity" of information, and giving rise to one's degree of "confidence" in an estimate of relative likelihoods..." (Ellsberg, 1961). A message, or news, therefore consists of an information component and an associated degree of its precision; the former directly refers to factors underlying decisions (the fundamentals), while the latter determines how vague (ambiguous) the news is.[4] Even if in practice the two components often come indiscernibly together, politicians, regulators, service providers and corporations may opt to invest in improving the precision and reliability of news, or to present it as cheap talk, private opinions or rumors. This strategic choice requires understanding of the relative effects the

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[1] Janet L. Yellen, "The Outlook, Uncertainty, and Monetary Policy", Speech at the Economic Club of New York, March 29, 2016.

[2] Compared to the unambiguous announcement of a shift in monetary policy in December 2015, with a 1/4 per cent interest rise and expected two further rises during 2016, the March message looks vague. In particular, it stated that "gradual increases in the federal funds rate" were rather "a forecast for the trajectory of policy rates" than "a plan set in stone". yet most commentators would agree that it communicates a lower probability of future rate rises than did the policy signal three months earlier. In a short survey of randomly selected 389 US respondents just after the Fed press conference in March 2016 we obtained that from those who recently heard any news about the Fed, 48% reported that the news were the Fed would keep interest rates unchanged, while 17% believed they heard Fed would raise rates. For comparison, just before the announcement, in a comparable survey of 469 respondents, the proportions were 23% and 58% respectively.

[3] Data on lowest-unmatched price auctions reported in Eichberger and Vinogradov (2015) reveals that players' bidding behavior sharply changes after the moderator announces the winning bid is "below €20" or "below €300". In both cases the winning bid was just under €15, and 80% of participants anyway placed bids under €20 before the announcements, yet this fraction fell by some 30% after the second annoucement.

[4] In an investment context, Illeditsch (2011) refers to ambiguous information as "information that is difficult to link to fundamentals" or a "public signal with unknown precision."

content and the precision of news have on the public. We suggest an approach to disentangle the two and demonstrate ambiguity attitudes crucially shape responses to messages that vary in signaled probabilities of outcomes (risk) and ambiguity that surrounds them.

Most models of decisions in uncertainty would agree that a reduction in ambiguity aligns decisions toward ambiguity-neutrality. Less clarity is there with regards to the impact of imprecisely communicated probabilities of success. In the multiple priors framework (Gilboa and Schmeidler, 1989), a signal would change decisions of ambiguity-averse subjects only if it moves the lowest expected utility up or down. Therefore, a vague signal that changes the set of priors but leaves the worst expectation unchanged would lead to a response by ambiguity-neutral subjects but no change in the ambiguity-averse behavior. In the second-order models (e.g. Klibanoff et al., 2005; Nau, 2006; Neilson, 2010) response of ambiguity-averse subjects depends on how exactly signals affect the whole second-order distribution; theoretically, they can both underreact and overreact to news compared to the response of ambiguity-neutrals. Neo-additive capacities (Chateauneuf et al., 2007) explicitly weigh the probabilistic and the non-probabilistic components of the decision functional, suggesting a higher impact of probabilistic news on probabilistically-sophisticated subjects, as they assign a weight of unity to the probabilistic component.

To explicitly distinguish between the content of the news and its precision, we represent subjects' decision functional as a sum of a probabilistic component and an ambiguity premium. Existence and uniqueness of a probability measure within a single source of uncertainty follows from Chew and Sagi (2008). In the model of Abdellaoui et al. (2011), this measure equals a "matching probability" for some given probability value if it ensures the decision-maker is indifferent between betting on the source with this given probability, known to the decision-maker, and the source with an unknown probability. Our ambiguity premium is the difference between this given probability and the matching probability. Ambiguity-neutral subjects have zero ambiguity premium and therefore would only respond to signals that bear some probabilistic component. This allows us to empirically discriminate between probabilistic and non-probabilistic signals. Ambiguity-averse subjects may respond to non-probabilistic signals if these affect the ambiguity premium. Generally, the ambiguity premium depends on the level of probability, which implies differences in responses of ambiguity-averse and ambiguity-neutral subjects to different probabilistic signals, too. In particular, for high likelihoods of success one can expect the ambiguity premium decreases in probability (as success becomes more certain), and therefore decisions of ambiguity-averse subjects become closer to those of ambiguity-neutrals.

This framework allows us to formulate hypotheses not only with regards to probabilistic and non-probabilistic signals, but also with regards to differences between signals within each of these two groups. Signals in our paper resemble those frequently used by booking web-sites: "This hotel was booked 13 times on our site", "5 people are looking at this moment", or "Score based on 527 reviews: 7.9/10". Similarly, we tell our subjects that 12 [hypothetical] participants before them chose the ambiguous urn in the Ellsberg task, or 12 out of 20 actually won when drawing from that urn. As messages of this type are common in everyday life, they are easy to understand for subjects, yet allow experimenters to affect the perceived probability of success and ambiguity.

A number of studies have previously analyzed the impact of varying levels of ambiguity on decisions. Early studies by Curley and Yates (1985) and Bowen et al. (1994) represented ambiguity as an interval of possible values of probability, and varied both the length and the centerpiece of this interval with an objective to detect changes in the average ambiguity attitude of the sample. Budescu et al. (2002) focus on subjects' attitudes to the vagueness of probabilities and of outcomes; precision is also modeled by a range of possible values. Results indicate sensitivity to gain/losses framing, as well as to the domain of uncertainty (outcomes or probabilities). Du and Budescu (2005) use a similar approach to model imprecision and confirm an increase in ambiguity-avoidance in response to an increase in ambiguity in the gains domain. Kramer and Budescu (2005) make both urns in the Ellsberg task ambiguous, yet with different degrees of ambiguity: for better (imprecise) probabilities of success, they found less ambiguity avoidance, although when subjects choose between urns with imprecise and precise probability, ambiguity avoidance increases in the likelihood of success. The mechanics of this behavior is unclear. In our experiments subjects also face imprecise signals with varying probabilities, as well as signals with varying precision. Yet, we screen the cohort of ambiguity-neutral subjects and by comparing the behavior of the ambiguity-averse sub-group with them, explain choices by changes in the ambiguity premium. In particular, we obtain that subjects may neglect the ambiguity component of probabilistic but vague signals, unless the emphasis is explicitly on the difference in precision, which is exactly the case when subjects choose between imprecise and precise probabilities.

A different approach to vary ambiguity is used by Ahmed and Skogh (2006) who make subjects's payoffs dependent on a draw from an urn, the composition of which is either unknown, or described in a way that limits but does not fully reveal the likelihood of success, or described well enough to give a precise probability of it. This resembles our approach though we do not fully resolve ambiguity, and focus on the impact of communicated changes in probability and/or ambiguity. Ahmed and Skogh (2006) find that for high ambiguity subjects prefer to share losses, while along with a reduction in ambiguity subjects switch towards insurance. They attribute this change in behavior to the inability of participants to calculate a fair insurance premium when probabilities are not given. Equally, one could argue, if subjects form probabilistic beliefs as in Chew and Sagi (2008) and weigh them as in Abdellaoui et al. (2011), insurance premiums can be calculated, yet they would be different on the demand and supply sides. Therefore, due to ambiguity aversion, no insurance may be an equilibrium outcome, similarly to no trading in Dow and Werlang (1992) or no deposits in the banking equilibrium in Vinogradov (2012). However, no distinction between ambiguity-neutral or ambiguity-averse subjects is made in Ahmed and Skogh (2006), thus it is difficult to judge to which extent subjects' decisions are governed by ambiguity attitudes.

Sequential arrival of information is also used in a recent study by Baillon et al. (2015) who measure subjects' ambiguity aversion based on their decisions to trade in stock options, using real data. The main result is that as soon as more information about the dynamics of stock option performance becomes available, ambiguity-averse subjects form beliefs close to those of their ambiguity-neutral peers. This is similar to what we find here, except that we also explicitly distinguish between effects of a change in the level of the communicated likelihood of an event, and of a change in the precision of the news.

Some other papers extend the notion of varying ambiguity to several dimensions (sources). In Eichberger et al. (2015) subjects face a standard Ellsberg task, yet the level of ambiguity is varied by making both the probability and the payoff unknown, as compared to the traditional case of unknown probability. Although the theoretical prediction is that ambiguity-averse subjects should prefer the urn with a known composition, many participants in fact deviate in favor of the unknown urn when there is an additional source of ambiguity. Eliaz and Ortoleva (2015) generalize this result by showing that single-dimensional ambiguity is preferred to a multi-dimensional one but the correlated multidimensional ambiguity may be preferred to any ambiguity arising from a single dimension. In a way this implies that if ambiguity is unavoidable, people prefer "more ambiguity" if it arises from different correlated sources.

With a focus on the single-source ambiguity, it appears intuitively plausible that ambiguity-averse subjects prefer less ambiguity to more, which is confirmed by the rather limited number of experimental studies above. Our work extends this literature by investigating the impact of verbal signals, and especially the marginal contribution of each signal in a sequence, explicitly focusing on heterogeneity of participants. We collect data from five independent experiments, both lab-based and online, with and without monetary incentives, with the number of participants ranging from 109 to 892, giving us a total of 1182 valid (complete and non-duplicate) responses of ambiguity-averse and ambiguity-neutral subjects. For comparison, the number of valid responses is 64 in Du and Budescu (2005), 106 in Ahmed and Skogh (2006), 119 in Eichberger et al. (2015), 97 in Eliaz and Ortoleva (2015), and 64 in Baillon et al. (2015). The relationships between binary choices and determinants are then studied by estimating relevant probit regressions at the aggregate level for the pooled data, controlling for experiments, at the split level for the subsamples of lab versus online, and incentivized versus unincentivized experiments, and at the level of individual experiments, to ensure consistency of findings across them. All main results robustly hold in all our experiments.

In all exercises we detect a significant effect of all signals on subjects' choices; ambiguity-averse subjects are more likely to react to very vague news, which may be explained by the perceived reduction in ambiguity. The difference between ambiguity-averse and ambiguity-neutral subjects in their responses to signals becomes less significant once they face probabilistically informative signals; subjects with a better knowledge of mathematical statistics and probabilities are more likely to respond to them. The strongest response of subjects is observed for the signal that communicates the highest likelihood of success. Varying the precision of the signal also produces a significant albeit smaller effect on subjects' choices on average, all this coming through the ambiguity-averse cohort. Finally, varying probabilities only has a rather homogeneous effect on ambiguity-neutral and ambiguity-averse subjects. This latter result implies an ambiguity premium that is effectively flat in risk, although not necessarily equals to zero, as ambiguity is not fully resolved. A possible explanation is that when facing signals that bear a probabilistic component, subjects "edit" signals (in the sense close to that originally suggested by Kahneman and Tversky, 1979) and disregard the precision (ambiguity) component. This explains participants' choices in Ahmed and Skogh (2006) when they face imprecise yet probabilistic signals. In contrast, in Kramer and Budescu (2005) when participants choose between an imprecise and precise probability, the difference in ambiguity is explicitly emphasized, implying a strictly positive ambiguity premium. An increase in probability thus reduces the risk premium but not the ambiguity one, implying more subjects avoid the ambiguous prospect.

It is worth noting that Ahmed and Skogh (2006) report their subjects' responses are highly heterogeneous. Likewise, substantial heterogeneity of source functions (and thus ambiguity attitudes) is reported in Abdellaoui et al. (2011). On the one hand, with this in mind, consistency of our results across various experiments in different settings and cultures, is reassuring in terms of robustness. On the other hand, as more insight into individual decision-making is needed, we include subject-specific variables like gender, age, knowledge of probabilities and statistics, and a confidence measure in our analysis, to better understand who is more likely to respond to non-probabilistic messages. These factors do have occasional impact: proficiency in statistics affects subjects' reaction to probabilistic signals, which we attribute to the better understanding of the meaning of signals; high confidence makes subjects less likely to choose the ambiguous prospect. Still, ambiguity attitudes are the main variable that explains response to imprecise news. If ambiguity is high, even very vague news can move the market. For more precise news it is rather risk-aversion than ambiguity-aversion that matters.

2 Theoretical Framework

Our objective is to study the role of ambiguity attitudes in processing vague messages. We will do so in the context of a two-color Ellsberg experiment, where vague messages will potentially provide information about the probability of success for the ambiguous urn. Are ambiguity-averse or ambiguity-neutral subjects more likely to respond to this vague news? Several scenarios are conceivable, as highlighted in the Introduction. On the one hand, ambiguity-averse subjects may underestimate the validity of a vague message, and hence it is ambiguity-neutral subjects who should be more likely to react to it. On the other hand, messages may affect the very perception of ambiguity, thus reducing pessimism, and making ambiguity-averse subjects react stronger than ambiguity-neutrals. If a vague message communicates an increase in the probability of success, ambiguity-neutral subjects should respond to it, but would ambiguity-averse subjects do? If their pessimistic belief takes the lowest conceivable probability value, and this value is not affected by the news, then they would hardly react to probabilistic news. However, if probabilistic signals also affect the degree of pessimism, e.g. by moving that "lowest conceivable probability value" up, the answer may be opposite. To investigate this interaction of communicated probabilities and ambiguity [attitudes], we employ a framework based on Abdellaoui et al. (2011), which delivers testable predictions with regards to questions posed above.

2.1 Preliminaries

Consider the standard two-color Ellsberg task (hereinafter the Ellsberg task): payoff P is conditioned on event R which happens either with unknown probability if the source of uncertainty is A, or with probability $\frac{1}{2}$ if the source of uncertainty is B; the decision-maker chooses between the two sources. We denote $A \prec B$ if the decision-maker [strongly] prefers the source with the known probability, i.e. in the Ellsberg experiment he prefers to bet on drawing Red from urn B than on drawing Red from urn A (and $A \succ B$ if vice versa), i.e. if RB is the event of drawing Red from B, and RA is the event of drawing Red from A, then notation $A \prec B$ replaces $P_{RA}0 \prec P_{RB}0$. Preference relation $\succ$ is defined as converse to $\prec$; indifference $\sim$ occurs when neither $\prec$ nor $\succ$ holds. Assume subjects derive utility $u\left(P\right) > 0$ from P, and $u\left(0\right) = 0$ from getting nothing. Within each source of uncertainty subjects form probabilistic beliefs, as axiomatized by Chew and Sagi (2006). They are assumed to associate source B with the probability value $\frac{1}{2}$, as given to them; we will denote the probabilistic belief of subjects for source A as π.

To describe subjects' decisions, we employ the source function approach of Abdellaoui et al. (2011) where prospects are evaluated with a weighted value $w_{So}\left(\pi\right)$ of probability π that a subject would conceive within a single source So of ambiguity: $w_{So}\left(\pi\right) u\left(P\right) + \left(1 - w_{So}\left(\pi\right)\right) u\left(0\right)$. The weighting function w_{So} maps probabilities into decision weights and is called a source function; we assume higher probabilities receive higher weights within the same source i.e. $w_{So}\left(\pi\right) > w_{So}\left(\pi'\right)$ for any $\pi > \pi'$. With $u\left(0\right) = 0$, and by dropping the subscript for the source with known probability, we can represent the choice between sources A and B as

$$A \prec B \Leftrightarrow w_A\left(\pi\right) < w\left(\frac{1}{2}\right). \tag{1}$$

A decision-maker is called *ambiguity-neutral* if he weighs probabilities in the ambiguous source the same way as in the unambiguous one, i.e. if $w_A\left(\pi\right) = w\left(\pi\right)$, for any probability value π; a decision-maker is called *ambiguity-averse*, if $w_A\left(\pi\right) < w\left(\pi\right), \forall\pi$, and *ambiguity-seeking*, if $w_A\left(\pi\right) > w\left(\pi\right), \forall\pi$. The latter two definitions reflect the fact that for any value of probability the decision-maker prefers the non-ambiguous, or the ambiguous, respectively, source, according to the preference relationship similar to (1).

Proposition 1 *In the Ellsberg task for ambiguity-neutral subjects holds* $A \prec (\succ) B \Leftrightarrow \pi < (>) \frac{1}{2}$, *and* $A \sim B \Leftrightarrow \pi = \frac{1}{2}$.

Abdellaoui et al. (2011) and Dimmock et al. (2016) show that in the Ellsberg task the subjective probability for the ambiguous urn is $\pi = \frac{1}{2}$; this is based on the symmetry consideration with regards to the colors of the balls. Dimmock et al. (2016) refer to this value as *ambiguity-neutral* probability, because this "would be the subjective probabilit[y] used by an ambiguity-neutral decision maker" (Dimmock et al., 2016, p. 1365). By definition, ambiguity-averse subjects assign weight $w_A\left(\frac{1}{2}\right) < w\left(\frac{1}{2}\right)$ to this probability and thus ought to prefer B to A in the Ellsberg experiment, while ambiguity-seeking subjects prefer A to B. With regards to ambiguity-neutral subjects, equality $w_A(\pi) = w(\pi)$ implies indifference between A and B. This allows one to use the two-color Ellsberg experiment as a simple test of ambiguity-neutrality: subjects who prefer B when the prize is conditioned on one color, and A when the prize is conditioned on another color, are deemed ambiguity-neutral, as this behavior is incompatible with ambiguity-averse or ambiguity-seeking. Some ambiguity-neutral subjects can, however choose the same urn for both colors, mimicking the behavior of, and thus erroneously classified as ambiguity-seeking or ambiguity-averse subjects. However, as we will show in a few steps, being able to identify a group of ambiguity-neutral subjects, and to separate away ambiguity-averse subjects from ambiguity-seeking, suffices for our purposes. This is exactly what the Ellsberg test delivers (see also Eichberger et al., 2015; Butler et al., 2014). We make a formal note of this fact in the following corollary to proposition 1.

Corollary 1 *In the Ellsberg task, ambiguity-averse subjects choose B and ambiguity-seeking subjects choose A independent of the color on which the prize is conditioned. Ambiguity-neutral subjects are indifferent between A and B: they choose A and B interchangeably.*

From now on, our focus will be on ambiguity-neutral and ambiguity-averse subjects only, although conclusions can be extended to ambiguity-seeking behavior, too. Our empirical analysis later on mainly deals with ambiguity-neutral and ambiguity-averse subjects, with an example how results change for ambiguity-seeking. The main idea is to exploit the difference between neutrality and non-neutrality to ambiguity. Theoretically, there is little conceptual difference between comparing ambiguity-neutral versus ambiguity averse or versus ambiguity-seeking subjects. Yet ambiguity-aversion usually dominates in the observed behavior, for which reason we concentrate on ambiguity-averse subjects in our discussion.

2.2 Ambiguity premium

Dimmock et al. (2016) define a *matching probability* as the value of probability in the risky prospect that makes the decision-maker indifferent between the risky and the ambiguous prospects with equal outcomes. They show that the matching probability is the ambiguity function $m_A(\pi) = w^{-1}w_A(\pi)$ that reflects both the ambiguity attitude of the decision-maker and the degree of ambiguity of the source of uncertainty. Using monotonicity of w, we can re-write the decision rule as

$$A \prec B \Leftrightarrow w^{-1}w_A(\pi) < \frac{1}{2},$$

or, by denoting $\theta_A(\pi) = \pi - m_A(\pi)$, as

$$A \prec B \Leftrightarrow \pi - \theta_A(\pi) < \frac{1}{2}. \tag{2}$$

Representation (2) distinguishes between the value of probability π that the decision-maker associates with the source, and an increment $\theta_A(\pi)$ that we call the *ambiguity premium*. In general, the latter depends on the probability measure π, on the level of ambiguity associated with source A, and on the ambiguity attitude of the decision-maker as reflected in his/her weighting functions w and w_A.[5] Note that $\theta_A = \theta_A(\pi)$ does not imply ambiguity attitudes and risk attitudes are correlated, as risk attitudes are governed by $u(\cdot)$, distinct from w and w_A.

Example 2.1 *Figure 1 gives an example of two source functions of the Prelec (1998) type* $w(p) = \exp(-\delta(-\ln p)^{\gamma})$, *as well as the resulting matching probability and ambiguity premium functions. The difference in the elevation (parameter* δ*) of the source functions is interpreted as ambiguity aversion due to* $w_A(\pi) < w(\pi)$ *(see also Abdellaoui et al., 2010, for the interpretation of the elevation as pessimism/optimism).*

Example 2.2 *Using data reported in Abdellaoui et al. (2011), Figure 2 shows matching probabilities and ambiguity premia: the best-fit Prelec type source function parameters are reported in the original paper; the inversion of the unambiguous source function* w^{-1} *is then applied to their reported values of decision weights* w_A *in the ambiguous source to produce matching probability* $m_A(\pi) = w^{-1}w_A(\pi)$ *for* $\pi = \frac{n}{8}$, $n = 1..7$; *end values are taken* $m_A(0) = 0$ *and* $m_A(1) = 1$; *the ambiguity premium is* $\theta_A(\pi) = \pi - m_A(\pi)$.

Why focus on the ambiguity premium instead of the matching probability? The premium captures the departure of $m_A(\pi)$ from the probability value π. This idea is implicit in

[5] In Kahn and Sarin (1988) the decision weight is also represented as a sum of the probabilistic component and an adjustment term, yet in their model probability of success is seen as a random variable, the probabilistic term is the mean probability value, and the adjustment reflects the ambiguity attitude of individuals and the dispersion of the probability of success. Although their approach is different from ours, it is worth noting that their adjustment term is independent of the "mean probability".

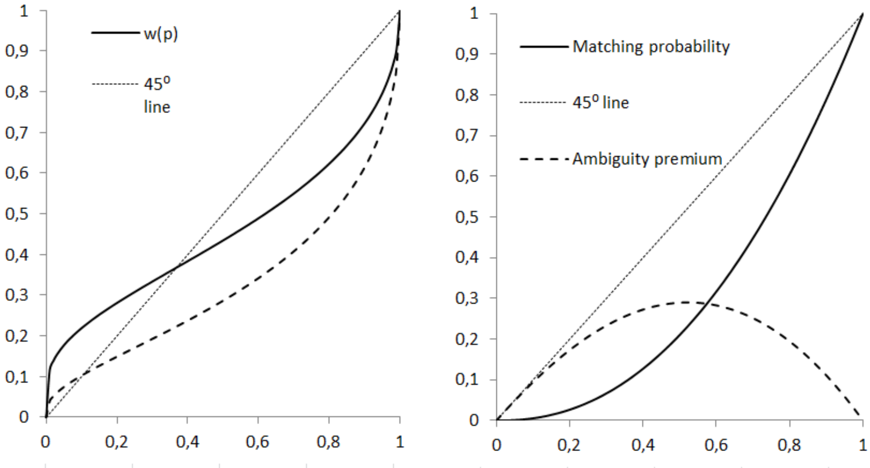

Figure 1. Source functions (left panel), matching probability and ambiguity premium for two-parametric Prelec-type probability weighting functions: $w(p) = \exp\left(-(-\ln p)^{\frac{1}{2}}\right)$, $w_A(p) = \exp\left(-\frac{3}{2}(-\ln p)^{\frac{1}{2}}\right)$.

the two ambiguity indices discussed by Abdellaoui et al. (2011) and Dimmock et al. (2016): if $\overline{m}_A(\pi) = c + s \cdot \pi$ is the trend line for the matching probability then $a = 1 - s$ represents subjects' likelihood insensitivity and $b = 1 - s - 2c$ is an ambiguity attitude index. The first one is explicitly the slope, and the second is the doubled mean of the corresponding trend for the ambiguity premium: $\overline{\theta}_A(\pi) = c + (1 - s) \cdot \pi$; $\left\langle \overline{\theta}_A(\pi) \right\rangle = (\overline{\theta}_A(0) + \overline{\theta}_A(1))/2 = b/2$. Both indices are independent of probability. However as shown in Figure 1 for the hypothetical matching probabilities and in Figure 2 for matching probabilities computed from data in Abdellaoui et al. (2011), the departure of the matching probability from π is not constant. Its variation, i.e. the dependence of the ambiguity premium on π, is crucial for the below analysis of the extent to which subjects underrect or overreact to communicated imprecise probabilities. This idea of a "departure" from a probability value, as in "under" and "over"-reaction, also embodied in the two indices above, leads us to prefer the parameter that is explicitly defined as such a departure.

For the following proposition preserve notation a for the likelihood-insensitivity and b for the ambiguity attitude index, as introduced in the previous paragraph.

Proposition 2 *Ambiguity-neutrality implies* $\theta_A(\pi) = a = b = 0$. *For ambiguity-aversion holds* $\theta_A(\pi) > 0$ *for any* $0 < \pi < 1$, *and* $b > 0$; *moreover,* $\theta'_A(\pi) < 1$ *for any* $0 < \pi < 1$, *and* $a < 1$.

In particular, the proposition implies ambiguity-aversion dictates at least *some* degree

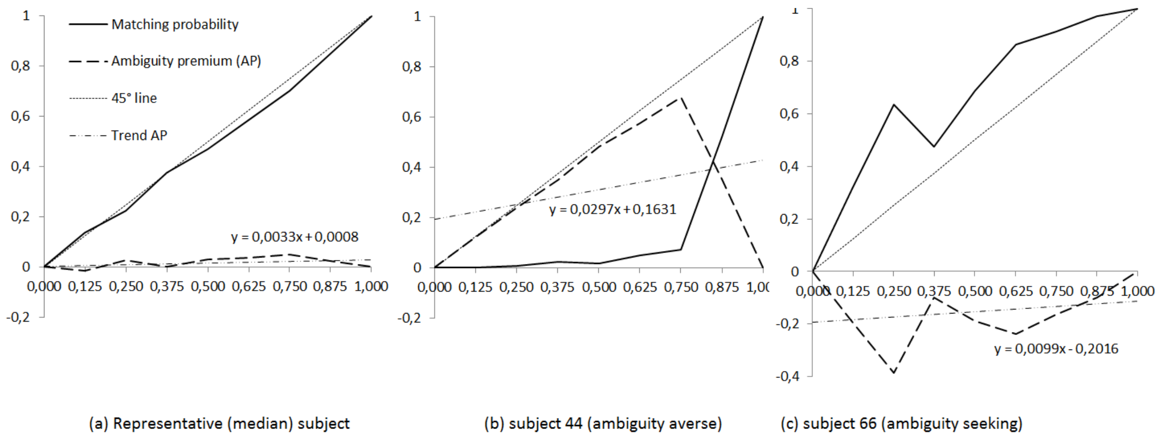

Figure 2. Matching probabilities and ambiguity premia computed from data in Abdellaoui et al. (2011).

of likelihood-insensitivity, $a < 1$. We do not require the proposition to hold for $\pi = 0$ and $\pi = 1$ for ambiguity-averse subjects as ambiguity premium may be zero for these values of probability, see Figure 1. One could argue that certainty, in the sense of $\pi = 0$ or $\pi = 1$, also implies no ambiguity. For example, Kahneman and Tversky (1979) stress that their probability weighting function "is not well-behaved near the end-points", explaining this by the limited ability of people to comprehend extreme probabilities, for which reason "highly unlikely events are either ignored or overweighted, and the difference between high probability and certainty is either neglected or exaggerated." (Kahneman and Tversky, 1979, p. 283).

Properties of the ambiguity premium determine the differential impact of an increase in π on ambiguity-averse and -neutral subjects.

Proposition 3 *Let $\theta_A(\pi)$ be monotonic on some $\left(\pi^L, \pi^H\right) \subset [0,1]$. For any $\pi_1, \pi_2 \in \left(\pi^L, \pi^H\right)$ such that $\pi_1 < \pi_2$ holds $m_A(\pi_2) - m_A(\pi_1) > (<)\, \pi_2 - \pi_1$ iff $\theta'_A(\pi) < (>)\, 0$.*

In the above proposition $m_A(\pi)$ is the decision functional of ambiguity-averse subjects, and π is that of ambiguity-neutrals. The proposition allows us to relate their behavior to the properties of the ambiguity premium: if and only if the latter decreases in probability, the decision functional of ambiguity-averse subjects changes more in response to a change in probability than the decision functional of ambiguity-neutrals. If $\theta_A(0) = \theta_A(1) = 0$ and $\theta_A(\pi)$ is continuous, then $\theta_A(\pi) > 0$ for $0 < \pi < 1$ implies existence of a global maximum of $\theta_A(\pi)$ on $[0,1]$. We will assume that the ambiguity premium has no local maxima other than the global maximum, as observed in Figure 1.

Corollary 2 *If there exists $0 < \pi^* < 1$ such $\theta'_A(\pi) < 0$ for any $\pi > \pi$, then ambiguity-averse subjects respond stronger than ambiguity-neutral subjects to an increase in probability π: $\Delta m_A(\pi) > \Delta\pi$ for $\pi > \pi^*$.*

We expect therefore a difference in responses of ambiguity-averse and neutral subjects to changes in communicated probability even if signals do not affect ambiguity.

2.3 Change in ambiguity versus change in risk

If new information becomes available about source A, it can either affect the probability π associated with this source, and through it, potentially, also the ambiguity premium $\theta_A(\pi)$, or the ambiguity premium solely, for example by changing subjects' perception of ambiguity associated with source A. Arguably, if subjects associate one source of uncertainty with a lower ambiguity premium than another, they regard the first source as less ambiguous. It is therefore a non-trivial task to distinguish between changes in risk (probability) and ambiguity. Extra information about the sources may affect subjects' perception of the "right" probability, and/or ambiguity of the source at the same time. Here we suggest a method to behaviorally test if extra information affects risk or ambiguity as perceived by subjects. By Propositions 1 and 2 the decision of ambiguity-neutral subjects is driven exclusively by the subjective probability value π; if the latter does not change, so does the choice between the two sources A and B. It follows that once we identify ambiguity-neutral subjects, their behavior can be used to interpret signals about the ambiguous source as affecting or not affecting the probabilistic component π.

The second part of Proposition 2 implies that an increase in π by $d\pi$ is never offset by the corresponding change in the ambiguity premium: $d\theta_A(\pi) = \theta'_A(\pi)\,d\pi < d\pi$, therefore change in risk always has an impact on decisions. We now focus on changes in decisions due to a change in ambiguity that is not associated with a change in risk.

Assume there exists a measure of ambiguity, $\delta \geq 0$, such that $\delta = 0$ corresponds to an unambiguous source, and a source with a higher level of ambiguity is associated with a higher value of δ. We now extend previous notation by denoting $w_{So(\delta)}(\pi)$ the source function for the source So associated with ambiguity measure δ, and explicitly including δ in the parameters of the ambiguity premium for source A: $\theta_A = \theta_A(\pi, \delta)$. As before, $w(\pi)$ is the source function for the unambiguous source: $w(\pi) = w_{So(0)}(\pi)$. Previously we employed condition $w_A(\pi) < w(\pi)$ for ambiguity-averse subjects. We now assume their preferences are also smooth, i.e. exhibit monotonicity in ambiguity: for any $0 \leq \delta_1 < \delta_2$

holds $w_{A(\delta_1)}(\pi) > w_{A(\delta_2)}(\pi)$ for any $0 < \pi < 1$, ambiguity-averse subjects strictly prefer less ambiguity to more ambiguity.

Proposition 4 *For any* $0 \leq \delta_1 < \delta_2$ *and for any* $0 < \pi < 1$ *ambiguity aversion implies* $\theta_A(\pi, 0) \leq \theta_A(\pi, \delta_1) < \theta_A(\pi, \delta_2)$.

By Proposition 4, a reduction in ambiguity without affecting risk reduces ambiguity premium of ambiguity-averse subjects. In the limit, if ambiguity is fully resolved, their ambiguity-premium becomes zero. As ambiguity-neutrality implies $\theta_A(\pi, \delta) = 0$ for any π and δ, only ambiguity-averse subjects would be likely to respond to a change in ambiguity that does not affect risk. However their response to ambiguity-reducing signals may be less pronounced for high values of probability if ambiguity premiums decline in probability as per Proposition 3.

2.4 Hypotheses

The above considerations lead us to five testable hypotheses. First, as **Hypothesis 1**, we expect ambiguity-averse subjects to be less likely to choose A, for any signal, as long as signals do not completely remove ambiguity. Note that in our set up subjects are classified into ambiguity-averse and -neutral by the standard Ellsberg task, and this binary classification does not change when more information is provided.[6] Hypothesis 1 therefore tests if this binary classification is robust to exposing subjects to signals of different precision. Besides, it tests if any form of communicating probabilities makes ambiguity considerations redundant (the answer is yes if or some signals we observe no difference between ambiguity-averse and -neutral subjects).

As per Proposition 2, ambiguity-neutral subjects would only react to signals that bear a probabilistic component. We can thus distinguish between signals that affect probabilities, and through them also ambiguity [premiums], and signals that affect only ambiguity premiums. Our **second Hypothesis** is that in the Ellsberg task an increase in π makes both ambiguity-neutral and amiguity-averse subjects more likely to choose A. Although for small probabilities an increase in π may be partly offset by a positive ambiguity premium,

[6] One may wish to extend the original interpretation of this classification by admitting subjects with low ambiguity premia may be classified as ambiguity-neutral, hence ambiguity-averse group includes subjects with high ambiguity premia as detected by the Ellsberg task. In this case our hypothesis implies that signals do not change the relationship between ambiguity premia of the two cohorts: if $\theta_{A,i} < E < \theta_{A,j}$ in the Ellsberg task, where E is the threshold that discriminates subjects into ambiguity-averse and ambiguity-neutral cohorts, then $\theta_{A(s),i} < \theta_{A(s),j}$ for any signal s. Notation $A(s)$ here recognizes that source A is formally seen as a different source under each signal s.

Proposition 2 implies there is still a positive impact on decisions of ambiguity-averse subjects. For high probabilities we have assumed ambiguity premium decreases in probability, therefore offsetting is not an issue.

If communicated probabilities of success are high enough, an increase in π is associated with a decrease in ambiguity premium θ_A, see Proposition 3, thus $\pi - \theta_A(\pi, \delta)$ increases faster than π alone. Our **third Hypothesis** is therefore that because of this effect of probabilities on ambiguity premium, more ambiguity-averse subjects, than ambiguity-neutrals, would change their behavior in favor of A in response to signals that communicate an increase in probabilities.

Assuming a decrease in $\theta_A(\pi, \delta)$ also reduces its elasticity in π and δ, we expect that for signals with higher values of probabilities and/or of greater precision, there will be less difference between decisions of ambiguity-averse and ambiguity-neutral subjects. This is our **Hypothesis 4**. Note that in Figure 1 the ambiguity premium $\theta_A(\pi, \delta)$ is less elastic in the mid-range probabilities, and more elastic for very high and very low values of π; this shape would imply a greater difference in responses of ambiguity-neutral and -averse subjects to signals that communicate high values of probability.

Moreover, since decisions of ambiguity-neutral subjects are unaffected by the level of ambiguity, our **Hypothesis 5** is that they equally react to different signals that communicate the same level of probability, while the behavior of ambiguity-averse subjects would be different.

3 Methodology and Data

Our data comes from both online and lab experiments. Jumping ahead, main results are identical across all independent online experiments reported in this paper, which adds validity to the online design. As a comparison benchmark, lab experiments confirm online findings. Despite this consistency, in this section we first justify the usage of the online setting, before detailing the design and the recruitment of subjects in all experiments.

3.1 Online versus lab experiments

The main reason for us to go online is the quality of data on ambiguity-neutralirty. First and foremost, we need a large enough sample to ensure the cohort of ambiguity-neutral subjects is of a reasonable size. As on average about 60% of subjects are found ambiguity-averse, with some studies reporting as many as above 70% (see Oechssler and Roomets, 2015), and the Ellsberg test can falsely classify ambiguity-neutral subjects as ambiguity-averse or -seeking, in a worst case scenario we can be left with about 10-15% of subjects deemed ambiguity-neutral. In a typical lab session this could mean as little as 3-5 participants in a cohort. To overcome this problem, lab results in our paper are based on several sessions. An online experiment is a rather inexpensive alternative to obtain the required large number of responses.

Second, the number of subjects classified as ambiguity-neutral may be artificially inflated in a lab. Fear of negative evaluation (FNE) by others is known to affect attitudes to ambiguity (Curley et al., 1986). Although one can design experiments to ensure preferences are not revealed to experimenters, thus avoiding FNE in experimental tasks (Trautmann et al., 2008), it continues to affect incentives in the lab: when the experiment becomes boring and the expected payoff does not suffice to keep subjects motivated to continue (Rubinstein, 2013), they might still do so, to avoid possible negative evaluation by other participants and the experimenters who would be able to observe subjects interrupting and leaving the lab. In addition, it has become a norm to offer subjects a show-up fee in a lab. It enters the total payoff together with an elaborate incentive scheme designed to reveal subjects' preferences and beliefs. The scheme itself may be quite complicated; in particular, with regards to lottery choices it involves a randomization device, which needs to be explained to the subjects. Suspicion is a known problem: participants may believe that experimenters manipulate the randomizing device in such a way as to minimize the payoffs (see, e.g., Frisch and Baron, 1988; Kühberger and Perner, 2003; Dimmock et al., 2016, for a discussion of the issue). Confusion about the payoff structure and suspicion may reduce the effectiveness of incentives, especially given the guaranteed show-up fee. Continuation despite the lack of incentives would result in random choices. In our setting this is a particular problem, as subjects randomizing between the two urns in the Ellsberg experiment are classified as ambiguity-neutral. Online experiments rule this out as subjects can leave the experiment at any moment; suspicion does not arise as all questions are hypothetical and randomization takes place in the minds of participants; and no show-up fee creates no incentives to continue despite lacking motivation.

Although evidence suggests online experiments are able to yield results similar to those obtained in a lab (see e.g. Krantz and Dalal, 2000), concerns may arise with regards to data validity; Birnbaum (2004) discusses methodology issues related to online surveys, Horton et al. (2011) summarize approaches that help validate the data. Vinogradov and Shadrina

(2013) argue that it is non-monetary intrinsic motivation of subjects (such as curiosity and willingness to help) that matters for the quality of data collected and its comparability with the lab. To control for this, in line with their results, we omit all incomplete responses. We also remove multiple submissions, i.e. all occasional duplicate entries as per the IP address.[7] The web platform[8] uses cookies to detect if the survey was taken previously from the same computer. To minimize the attrition effect, a large initial sample of responses was acquired. A sampling bias may occur with snowballing, to minimize which, the survey was introduced to subjects via different channels. Some authors suggest participants may wish to cheat in online experiments, submitting answers they believe experimenters would see as correct ones (Reips, 2000). To prevent this, the (preliminary) findings from the experiment, hypotheses and possible answers were not available to participants while the experiment was running. We also find it useful that the Ellsberg task does not impose a "right" or "wrong" answer thus removing incentives to cheat. Having a series of experiments under different arrangements allows us to reduce potential biases.

The clarity of questions was tested by trailing the experiments as face-to-face surveys to obtain feedback and ensure our instructions were clear. We piloted the experiment online in 2011 with 765 complete responses obtained via snowballing, of which 68.4% were classified as ambiguity-averse and 11.2% as ambiguity-neutral. Findings from this pilot are similar to what we report in the paper, yet due to the lack of data on gender and age we did not include it in the sample used here. Informal post-experiment feedback from the pilot, as well as from experiments reported below, confirmed subjects correctly understood the tasks.

Generally, unincentivized surveys are not uncommon and data from them is regarded reliable: for example, the Michigan Survey of Consumers is a major source of inflation expectations data for the U.S. (e.g., Thomas, 1999; Carroll, 2003; Dominitz and Manski, 2004). Although subjects are usually paid a fee to complete the [rather long] questionnaire, individual answers are not incentivized and payoff does not depend on the correctness of forecasts. McFadden et al. (2005) review possible biases in surveys and suggest remedies, in particular they note that hypothetical questions ("vignette surveys") and abstract questions (like "On a scale of 0 to 100, where 0 means no chance, and 100 means certainty, what would you say is the probability of ...?") yield answers highly predictive of actual subsequent behavior. Hollard et al. (2016) demonstrate that a simple non-incentivized rule of asking

[7] Unfortunately, this also removes all subjects that use the same access point (e.g. wireless router) to access internet.

[8] Online experiments were run on www.surveymonkey.com.

subjects about their subjective beliefs performs well in eliciting those beliefs, compared to incentivized rules.

An exhaustive discussion of pros and cons of online and lab experiments goes beyond the scope of this section (see, e.g. Reips, 2000, and Birnbaum, 2004, for an overview). There is an ongoing debate in the literature on the validity and generalizability of results from lab experiments (e.g. Rubinstein, 2001 and 2013, Levitt and List, 2007, Falk and Heckman, 2009 - just to mention a few). Our objective was to highlight why we chose the online setting as the main data collection tool, and to stress that all main results are confirmed in the lab. Consistency of results across individual experiments is reported in Section 5.2.

3.2 Questionnaire and variables

Subjects answer a questionnaire consisting of four parts, see Appendix A.2. In part I, they face the standard Ellsberg task and report whether they would bet on urn A or B if they need to pick a red (in question Q1) or a blue[9] (in question Q2) ball in order to win. By Corollary 1, this task is used as a simple test of ambiguity attitudes. For the major part of the analysis our focus is on ambiguity-aversion and ambiguity-neutrality.[10] We will code subject i's ambiguity aversion as a binary variable AA_i, which takes a value of 1 if the subject is classified as ambiguity-averse, and 0 if the subject is ambiguity-neutral according to this test. By Corollary 1, neither ambiguity-averse nor ambiguity-seeking subjects can be falsely classified as ambiguity-neutral, yet some ambiguity-neutral subjects may be falsely classified as ambiguity-averse. For this reason, any potential differences between cohorts with $AA_i = 1$ and $AA_i = 0$ are conservative estimates which would only become more pronounced if truly ambiguity-neutral subjects are removed from the cohort with $AA_i = 1$.

In Part II subjects are told the prize in all subsequent tasks is conditioned on drawing Red, as in Q1. Each question contains a signal that refers to choices and draws of hypothetical "other participants". For online participants, it is clear that questions and "other participants" are hypothetical. Although the word "hypothetical" does not appear in the questions, numbers were chosen so that subjects do not associate questions with the real participants in the lab. To avoid deception, in the lab sessions an effort was made to explain in the introduction that questions were hypothetical and outcomes would be computer-modelled. It was also made clear that hypothetical balls are returned to the urns after each

9 In some experiments we used black colour instead of blue.

10 As an alternative exercise, in Section 4.3 we will appropriately redefine the main variable to allow comparison between ambiguity-neutral and -seeking subjects, and will show the main results hold.

draw. In an informal post-experiment feedback, both in the lab and online[11] participants confirmed no confusion arose in this regard.

In Q3 subjects learn that 12 other participants chose urn A, while in Q4 they learn that 12 out of 20 participants did so. Neither signal explicitly communicates anything about the distribution of balls in urn A, yet subjects may perceive them as such. By Proposition 2, responses of ambiguity-neutral subjects serve as a litmus test for the probabilistic component of signals. Question Q5 communicates that 12 out of 20 "other participants" drew a red ball from the ambiguous urn. This signal is designed to indicate the likely distribution of balls in urn A without removing ambiguity completely. Questions Q6 and Q7 differ from Q5 in either the communicated frequency of drawing Red from A (16/20 participants instead of 12/20) or the number of total observations on the basis of which this frequency was calculated (120/200 participants drew Red from A instead of 12/20). We associate these signals with a better probability of success in A ($16/20 > 12/20$), and a further reduction in ambiguity (increase in precision of the signal) respectively. All numbers are chosen with the intention to simplify calculations subjects might wish to perform.

Part III measures subjects' confidence by asking them whether they would draw from a different urn if they pick the ball of a non-winning color (question Q8).[12] We code variable $CONF = 1$ if in this question subjects report they would draw again from the same urn.

In Part IV, participants are asked to rate their proficiency in statistics and probabilities, indicate their age and gender. Along with revealed confidence, answers to these demographic questions serve as control variables. Variable $STATS$ captures subjects' proficiency in statistics: $STATS = 1$ for subjects who assess their knowledge of statistics and probabilities higher than 3, the median, at the five-point scale used in question Q10. $FEMALE$ takes value 1 for female subjects and 0 for males, as reported in answers to question Q11. This variable is available for all experiments except for experiment 1. $YOUNG$ distinguishes between younger (age reported in question Q12 is below 25, $YOUNG = 1$) and older ($YOUNG = 0$) cohorts of subjects. This split is dictated by the distribution of observations in the age

[11] In the pilot experiment, not reported in this paper, participants were recruited via a facebook account, and the feedback was collected via facebook, too. This does not violate anonymity of participants as their comments on facebook cannot be linked to their answers in the experiment. In other online experiments, participants had an option to leave feedback in the end of the experiment.

[12] In some experiments, an additional question in Part III asked if participants would change their choice if they see other participants doing so. This question measures susceptibility, complementing confidence. However, for data availability reasons, we do not include this question in the analysis. Where data was available, we estimated the models used in this paper by including susceptibility as control, with no changes for the results, and with susceptibility appearing insignificant in most instances.

groups, and is conveniently consistent with the definition of "youth" by the UN.[13]

Table 1 describes data from all five experiments. For comparison, Oechssler and Roomets (2015) summarize percentages of subjects that can be classified as ambiguity averse from 39 experimental studies: if extremes are omitted, on average 57.1% subjects are found ambiguity-averse, with the percentage ranging predominantly between 45% and 75%. Our observations lie comfortably within these limits.

Table 1. Summary of experiments

	Experiments				
	Web1	Lab1	Web2	Web3	Lab2
Dates	09/07/12 - 03/08/12	08/06/12	03/05/13 - 22/05/13	22/05/13 - 15/06/13	16/01/14 - 17/01/14
Total participants	615	109	892	686	119
Completion rate, %	86.0	100	92.6	89.0	100
Environment	online	lab	online	online	lab
Monetary incentives	yes	yes	no	no	yes
Recruitment	Snowball (social networks + emails)	On campus	Snowball (social networks + emails)	Snowball (social networks + emails)	Random selection from database
Ambiguity-averse, %	62.8	47.7	63.6	65.6	48.7
Ambiguity-neutral, %	11.5	29.9	8.4	7.0	34.5
High confidence, %	86.7	76.1	80.6	80.1	79.8
High proficiency in statistics, %	36.	19.6	40.2	41.6	31.9
Female, %	51.8	74.8	58.7	41.2	68.1
Age below 25, %	40.8	44.9	82.5	63.4	82.3
Valid responses [a)]	253	107	640	483	119

Notes:
[a)] Number of responses after dropping incomplete and duplicate (by IP-address) responses.

3.3 Recruitment and incentives

For the lab experiments, subjects are recruited on campus, representing a mix of students and staff. Experiment Lab1 took place at the Higher School of Economics in Perm (Russia), with recruitment through a newsletter and announcements in lectures. Due to space limitations, two sessions were held to collect answers, totalling 109 subjects; in each session, subjects were

[13] "Definition of Youth", United Nations fact sheet, http://www.un.org/esa/socdev/documents/youth/fact-sheets/youth-definition.pdf.

informed that once all answers are collected, three questions would be selected randomly, and for them urns A and B would be reconstructed[14] in front of the audience in a special prize-drawing session. Based on the actual draws and subjects' choices in the relevant questions, the participant with the highest number of correct guesses in these three questions, would receive the main prize (RUR 3000, about 70% of the official minimum monthly wage at that time), and the runner-up would receive the second prize (RUR 2000); any ties are resolved by randomization between participants with the highest number of guesses. The competition between the winner and the runner up is not a problem as it still creates incentives to provide the highest number of right guesses.

Experiment Lab2 took place at the ESSEXLab of the University of Essex (UK). The ESSEXLab maintains a database of students and staff who have pre-registered for participation in computerized lab experiments. Emails are sent to randomly selected subjects from this database to recruit subjects. The experiment was programmed with z-Tree.[15] After all answers are collected, the software emulates urns A and B by randomizing outcomes: the probability of drawing Red is set at 0.5 both for urn B in all questions and for urn A in questions Q1-Q4; at 0.6 for urn A in questions Q5 and Q7,; and at 0.8 for urn A in question Q6. Subjects receive £2 for each question where their answer matches the computer-modelled draw. A minimum payoff of £5 was guaranteed to participants; the average payoff was £17.

In all online experiments, subjects were recruited by snowball sampling, with an initial invitation sent by email within the professional network of the experimenters, as well as posted on social networks with a request to re-post. In experiment Web1, a prize (£100 cheque) was promised to the participant with the highest number of answers that match computer-generated draws; ties resolved by a random allocation, as in experiment Lab1. Subjects had an option to provide their email address to be contacted if they win; about two-thirds of them did. Other online experiments had no monetary incentives. We controlled for intrinsic non-monetary motivation (Vinogradov and Shadrina, 2013) by dropping observations from incomplete questionnaires. Adding experiments with no monetary incentives allows us to control for the effect of the random assignment of the prize on decisions in ambiguity - effectively, the randomization device embedded in such an incentive scheme, forms

[14] Identical machine-wrapped in blue and red foil chocolates were used as balls; non-transparent bags were used as urns. All chocolates were distributed in the audience after the experiment as a participation reward. Distribution of chocolates in Urn A was determined in front of the audience by the following mechanism: subjects submitted numbers 1 to 9, not knowing what would happen afterwards, then the fraction of subjects who submitted numbers 5..9 was calculated, multiplied by 100, and this value was taken as the number of red balls in urn A. Further details are in Vinogradov and Shadrina (2013).

[15] The software licence requires that we mention the use of it in our experiment and cite Fischbacher (2007).

a compound lottery together with the tasks subjects face in the experiment. Theoretically, this may distort subjects' choices. Having experiments with no monetary incentives removes this distortion.

3.4 Analysis

Each subject performs a series of tasks under different information conditions, which we call treatments. A question from the original Ellsberg task will be chosen as a basis treatment (control). Our objective is to measure the effect of a change in the information condition on subjects' choices. Denote subject i's choice in treatment $j = 1..J$ as $T_{i,j} \in \{A, B\}$. One observation is a response of one subject in one treatment (control).[16] Each observation can be assigned a number $n = J \cdot (i-1) + j$, establishing a one-to-one correspondence between subject-treatment tuples and observations. For each observation of subject i, we define J values of the response variable as

$$R_{J\cdot(i-1)+j} = \begin{cases} 1 & \text{if} \quad T_{i,j} = A, \\ 0 & \text{if} \quad T_{i,j} = B, \end{cases} \quad \text{for each } j = 1..J.$$

Knowing the values of control variables x_i for each subject i, we similarly define for each observation n the subject-specific control variable x_n as $x_{J\cdot(i-1)+j} = x_i$ for each $j = 1..J$. The same procedure is applied to ambiguity aversion AA_n.

Finally, we define J signal-specific indicators s_j (with $j = 1..J$) with the following values for each observation n:

$$s_{j,n} = \begin{cases} 1 & \text{if} \quad n = J \cdot (i-1) + j, \\ 0 & \qquad \text{otherwise.} \end{cases}$$

With this notation, each observation $n = J \cdot (i-1) + j$ consists of the response R_n of subject i to signal j (treatment j), subject i's ambiguity aversion $AA_n = AA_i$, other subject-specific factors x_n and a treatment indicator $s_{j,n}$, which takes a value of 1 if observation n corresponds to treatment j. This equips us with a tool to estimate the impact of signals, ambiguity aversion and behavioral factors on the response variable R_n. All variables are binary. All estimates will be obtained from probit regressions with standard errors clustered at the subject level, controlling for experiment-specific fixed effects.

It only remains to define the control condition. The Ellsberg task is the one with the least information on the ambiguous urn, and lends itself as a control, however it contains

[16] For methodological aspects of the within-subject design see, e.g. Charness et al. (2012).

two questions. Conveniently, ambiguity-averse subjects, the way we classify them, choose only urn B in both questions (similarly, ambiguity-seeking subjects choose urn A in both questions). Ambiguity-neutral subjects are expected to randomize 50-50 between A and B, yet in our data 66.2% of ambiguity-neutral subjects chose A in the first question, when asked to bet on Red, and 33.8% chose B in the second question, betting on Blue. This suggests either a color bias (subjects believe they are luckier when they bet on Red than on Blue), or the question order bias (subjects choose option A first as it comes first on the screen when reading from top to bottom and from left to the right, and then they choose their answer to the second question, so as to make it consistent with the first one, in line with ambiguity-neutrality; as the question with betting on Red comes first, it attracts more choices). All subsequent treatments, however, clearly specify that the prize would only be awarded for drawing Red, and come as a single question for each signal. The color bias can be ruled out as when asked about Red only, especially in questions Q3 and Q4, the fraction of ambiguity-neutral subjects choosing A becomes comparable to that in the Ellsberg task conditioned on Blue. To correct for the order bias, we choose the second question from the Ellsberg task as the control condition and denote it as s_{ctrl}. This approach provides a conservative estimate of differences between ambiguity-averse and -neutral subjects, as it makes the two groups closer to each other in their initial choices. As a robustness test, we will use two alternative specifications for the control condition. First, we will demonstrate how main results hold if the first ("Red") Ellsberg question is chosen for control. Second, we will designate Q3 ("12 other participants prefer urn A") as an alternative control condition. Moreover, we will redefine the control condition again, when comparing choices across "probabilistic" signals. This will allow us to contrast effects of signals in the *ambiguity domain*, i.e. compared to the original choices in conditions of high ambiguity, versus those in the *probability domain*, i.e. focusing on differences generated by signals that communicate probabilities.

In order to make notation more self-explanatory we denote signals as s_{12pref} for question Q3 ("12 other subjects prefer urn A"), $s_{12/20pref}$ for question Q4 ("12 out of 20 subjects prefer urn A"), and will use the communicated ratios of successes in A as subscripts in $s_{12/20}$, $s_{16/20}$ and $s_{120/200}$ for questions Q5-Q7. This corresponds to s_j used earlier.

4 Results

We first present results for the effects of signals on subjects' choices as compared to the original Ellsberg task, which we call the "total effect". Then we study "marginal effects"

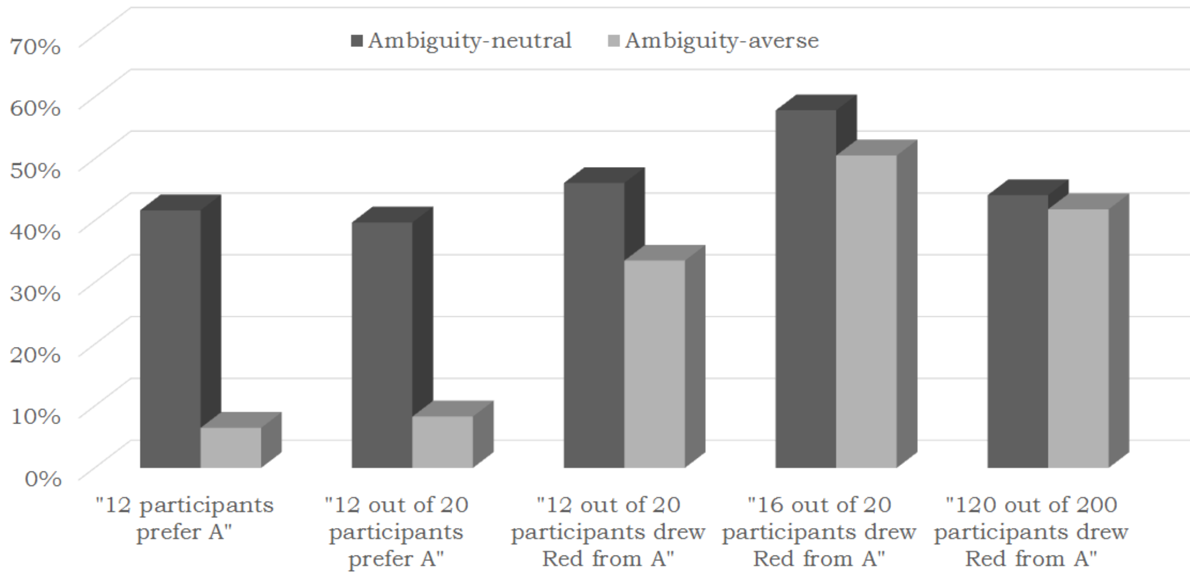

Figure 3. Fractions of ambiguity-averse and ambiguity-neutral subjects who choose the ambiguous prospect A after respective signals (messages on the x-axis).

of signals, i.e. changes in choices between different treatments, with the main focus on the probability domain. We further proceed with an analysis of behavioral factors that affect decisions.

4.1 Total effects

Figure 3 highlights a pattern in subjects' behavior with regards to vague news. First, in all treatments, less ambiguity-averse subjects than ambiguity-neutrals choose A, in line with our **first hypothesis**. The difference between the two fractions reduces, and its significance drops, from signal s_{12pref} to signal $s_{120/200}$, see also Table 2. In this sense, as the precision of signals increases and they bear more probabilistic information, choices of ambiguity-averse and ambiguity-neutral subjects become closer to each other, which confirms our **hypothesis four**.

A strictly positive and significant fraction of ambiguity-averse subjects choose the ambiguous prospect after signals s_{12pref} and $s_{12/20pref}$ (recall, all ambiguity-averse subjects, by our classification, choose B in the control treatment), although these signals do not explicitly hint towards any particular value of the probability of success in A. The non-probabilistic nature of these signals, as explained in the theoretical framework, is confirmed by no significant change in choices of ambiguity-neutral subjects, see differences $s_{12pref} - s_{ctrl}$ and $s_{12/20pref} - s_{ctrl}$ in Table 2. The magnitude of changes in the fractions of ambiguity-averse

Table 2. Signals and choices

	Ambiguity-neutral, AN	Ambiguity-averse, AA	Difference $AN - AA$
% of subjects choosing A in treatments:			
s_{12pref}: 12 prefer A	41.67	6.47	35.19***
$s_{12/20pref}$: 12 out of 20 prefer A	39.71	8.31	31.40***
$s_{12/20}$: 12 out of 20 drew Red from A	46.08	33.52	12.552***
$s_{16/20}$: 16 out of 20 drew Red from A	57.84	50.53	7.31*
$s_{120/200}$: 120 out of 200 drew Red from A	44.11	41.84	2.28
Differences			
$s_{12pref} - s_{ctrl}$	7.84	6.47***	1.37
$s_{12/20pref} - s_{ctrl}$	5.88	8.31***	-2.43
$s_{12/20} - s_{ctrl}$	12.25**	33.52***	-21.27***
$s_{16/20} - s_{ctrl}$	24.02***	50.53***	-26.51***
$s_{120/200} - s_{ctrl}$	10.29**	41.84***	-31.54***
$s_{12/20pref} - s_{12pref}$	-1.96	1.84***	-3.80
$s_{12/20} - s_{12pref}$	4.41	27.05***	-22.760***
$s_{16/20} - s_{12pref}$	16.18***	44.06***	-27.88***
$s_{120/200} - s_{12pref}$	2.45	35.366***	-32.91***
$s_{12/20} - s_{12/20pref}$	6.37	25.22***	-18.84***
$s_{16/20} - s_{12/20pref}$	18.14***	42.22***	-24.08***
$s_{120/200} - s_{12/20pref}$	4.41	33.52***	-29.11***
$s_{16/20} - s_{12/20}$	11.76***	17.00***	-5.24
$s_{120/200} - s_{12/20}$	-1.96	8.30***	-10.27***
$s_{120/200} - s_{16/20}$	-13.73***	-8.706***	-5.03

Note: T-test for differences in means; *** $p < .01$, ** $p < .05$, * $p < .1$.

and -neutral subjects looks similar, yet note that initially a large fraction of ambiguity-neutral subjects chose A, while zero ambiguity-averse subjects did, this explains the difference in significance (for ambiguity-neutral subjects observed changes are in line with their randomizing behavior at this stage).

When, however, a signal hints towards a particular value of the probability of success, both ambiguity-averse and ambiguity-neutral subjects react to such news, even though the news is still vague from a frequentist perspective. This confirms our **second hypothesis**. Differences in Table 2 demonstrate a stronger response of ambiguity-averse, than ambiguity-neutral subjects to probabilistic signals $s_{12/20}$, $s_{16/20}$ and $s_{120/200}$, which is in line with our **hypothesis three**: probabilistic signals affect decisions of ambiguity-neutral subjects to a lesser degree than those of ambiguity-averse. For ambiguity-neutral subjects, improving the precision of the signal (difference $s_{120/200} - s_{12/20}$ in Table 2) has no significant effect, as predicted by **hypothesis five**; note that the change in precision, as formulated in the questions, was sufficient to ensure reaction of ambiguity-averse subjects.

Importantly, the difference between the effects of the two non-probabilistic signals, $s_{12/20pref} - s_{12pref}$, is also significant for ambiguity-averse subjects although it is rather small, while remains insignificant for ambiguity-neutrals. Even a small change in the formulation of the message, that makes it look somewhat more plausible, affects behavior of ambiguity-averse participants. Generally, comparison of signals $s_{12/20pref}$ - $s_{120/200}$ with signal s_{12pref} instead of s_{ctrl} yields very similar results to the above, confirming our findings are not biased by the choice of the control condition. Remarkably though, this comparison for ambiguity-neutral subjects reveals little change generated by signals $s_{12/20}$ and $s_{120/200}$, indicating the communicated probability of success is not high enough to generate a significant change in choices. Yet note that here the results are also internally consistent: ambiguity-neutral subjects equally respond to signals with different precision but the same probability, even when the basis for comparison is changed (s_{12pref} instead of s_{ctrl}).

This simple analysis of differences does not account for heterogeneity of subjects. To address this, we estimate the impact of signals on subjects' choices, controlling for subject-specific parameters, see Table 3. Signals notation is now used to refer both to regressions constructed for the corresponding treatments, and to the dummies used in those regressions. Each regression contrasts the relevant treatment with a control condition s_{ctrl}, hence each dummy takes a value of 0 if the relevant observation comes from the control condition. All signals appear to significantly affect choices. The weakest, albeit still significant impact comes through s_{12pref} and $s_{12/20pref}$, which are least informative by design. Signaling a better probability (as in $s_{16/20}$) has the strongest impact on subjects. To compare the relative strength of the impact, note that coefficients for s_{12pref} and $s_{12/20pref}$ are not statistically different, while $s_{12/20}$, $s_{16/20}$ and $s_{120/200}$ significantly differ from them and between each other (Wald test, $p < .01$ for all pairs except $s_{12/20}$ and $s_{120/200}$ where $p < .05$). In all treatments, ambiguity-averse subjects are less likely to choose A. Good knowledge of statistics makes subjects more likely to respond to probabilistic signals $s_{12/20}$ - $s_{120/200}$. Strikingly, in all treatments more confident subjects are less likely to go for the ambiguous prospect. The role of gender is not consistently visible although there is a weak tendency for female subjects to more frequently choose A. We will return to the role of behavioral factors later.

Table 3. Impact of signals (pooled data)

	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AA	-0.174	-0.162	-0.186	-0.186	-0.163
	(-17.59)***	(-15.68)***	(-10.54)***	(-8.99)***	(-8.48)***
STAT	-0.009	-0.008	0.043	0.048	0.061
	(-0.82)	(-0.78)	(2.90)***	(3.15)***	(4.02)***
YOUNG	0.016	0.016	0.003	0.015	0.028
	(1.28)	(1.21)	(0.17)	(0.83)	(1.60)
CONF	-0.034	-0.047	-0.059	-0.056	-0.068
	(-3.04)***	(-4.15)***	(-3.50)***	(-3.03)***	(-3.87)***
FEMALE	0.011	0.028	0.019	0.031	-0.000
	(1.10)	(2.56)**	(1.28)	(2.00)**	(-0.01)
s_{12pref}	0.079				
	(8.08)***				
$s_{12/20pref}$		0.088			
		(8.79)***			
$s_{12/20}$			0.313		
			(20.21)***		
$s_{16/20}$				0.433	
				(26.33)***	
$s_{120/200}$					0.366
					(22.34)***
Observations	2364	2364	2364	2364	2364

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

In Table 3, ambiguity attitude has a universal effect on subjects' choices across treatments: being ambiguity-averse makes subjects less likely to choose A. The question is however if they are also more or less likely to respond to signals than ambiguity-neutral subjects. To investigate, we include the interaction term between ambiguity aversion, AA, and signal dummies, using the same controls as above. According to the decision rule (2) and Proposition 2, the common component in the decisions of ambiguity-neutral and ambiguity-averse subjects is driven by a communicated change in the ambiguity-neutral probability π, while the difference between these two cohorts is explained by the ambiguity-premium $\theta_A(\pi)$. For this reason, signal terms without interaction reflect the impact of signals on decisions through a change in π, while the interaction term captures the ambiguity premium effect on ambiguity-averse subjects. Results in Table 4 confirm the first two signals affect subjects' decisions through ambiguity premiums solely, while s_{12} - $s_{120/200}$ are clearly perceived as changes in probability. Ambiguity averse subjects are more affected by probabilistic signals: all interaction coefficients are positive, working against the separate effect

Table 4. Impact of signals and ambiguity-aversion (pooled data)

	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AA	-0.614	-0.633	-1.163	-1.239	-1.221
	(-49.08)***	(-48.78)***	(-50.37)***	(-53.14)***	(-49.90)***
STAT	-0.009	-0.009	0.046	0.051	0.066
	(-0.85)	(-0.80)	(2.92)***	(3.14)***	(4.07)***
YOUNG	0.017	0.017	0.003	0.016	0.030
	(1.27)	(1.21)	(0.14)	(0.85)	(1.59)
CONF	-0.035	-0.049	-0.061	-0.057	-0.070
	(-2.97)***	(-4.04)***	(-3.36)***	(-2.92)***	(-3.73)***
FEMALE	0.011	0.030	0.020	0.032	-0.001
	(1.08)	(2.61)***	(1.26)	(1.96)**	(-0.07)
s_{12pref}	0.027				
	(1.72)*				
$s_{12pref} \times AA$	0.477				
	(25.78)***				
s_{12pref}		0.018			
		(1.06)			
$s_{12pref} \times AA$		0.521			
		(26.34)***			
$s_{12/20}$			0.080		
			(2.71)***		
$s_{12/20} \times AA$			1.109		
			(33.20)***		
$s_{16/20}$				0.155	
				(5.09)***	
$s_{16/20} \times AA$				1.209	
				(36.04)***	
$s_{120/200}$					0.074
					(2.44)**
$s_{120/200} \times AA$					1.218
					(35.32)***
Observations	2364	2364	2364	2364	2364

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

of ambiguity aversion, yet the response is stronger to probabilistic signals (e.g. coefficients for $s_{12/20pref} \times AA$ and $s_{12/20} \times AA$ are different at $p < .01$, Wald test). Although all interaction terms are positive and significant, they counteract the separate effect of AA, so that responses of ambiguity-averse subjects to probabilistic signals are more aligned with those of ambiguity-neutrals, consistent with Table 2.[17]

4.2 Marginal Effects

The core of our model is the ambiguity premium, $\theta_A(\pi, \delta)$, which is assumed to change in response to changes in probability. Table 4 highlights differences in responses of ambiguity-averse and -neutral subjects to probabilistic signals, in line with Hypotheses 3 and 4, based on assumptions made about $\theta_A(\pi, \delta)$. To investigate the issue deeper, we now take a closer look at the impact communicated changes in probability have on ambiguity-averse subjects, i.e. on marginal effects of signals. For this purpose, we consider pairs of treatments, in which one is designated to be the new control condition, and the other one is the stimulus, as follows:

1. For the pair of treatments $s_{12/20}$ and $s_{16/20}$ designate $s'_{ctrl} = s_{12/20}$;
2. For the pair $s_{12/20}$ and $s_{120/200}$ designate $s'_{ctrl} = s_{12/20}$;
3. For the pair $s_{16/20}$ and $s_{120/200}$ designate $s'_{ctrl} = s_{16/20}$;

and finally to assess the difference between non-probabilistic signals:

4. For the pair s_{12pref} and $s_{12/20pref}$ designate $s'_{ctrl} = s_{12pref}$.

Table 5 presents the results. As for the non-probabilistic signal $s_{12/20pref}$, an attempt to make the news sound "more plausible" by indicating a ratio of subjects who prefer urn A brings little difference compared to s_{12pref}. Although significance is only at $p < 0.1$ level, it is ambiguity-averse subjects, again, who respond to this "improvement" in the signal. Effectively, a very vague message contained in s_{12pref} already suffices to reduce the ambiguity premium, and any further reduction can hardly be achieved by providing additional signals of the same type.

With regards to probabilistic signals, Table 5 further confirms Hypothesis 5: only ambiguity-averse subjects change their choices in response to an improvement in the precision of the signal, see column $s_{120/200}$ vs. $s_{12/20}$. Strikingly however, the improvement in probability ($s_{16/20}$ vs. $s_{12/20}$) has a homogeneous effect on ambiguity-averse and ambiguity-

[17] To confirm this, we estimated the same regression with an omitted AA-term, which yielded small and insignificant coefficients for $s_{16/20} \times AA$ and $s_{120/200} \times AA$.

Table 5. Marginal effects of signals (pooled data)

	$s_{12/20pref}$ vs. s_{12pref}	$s_{16/20}$ vs. $s_{12/20}$	$s_{120/200}$ vs. $s_{12/20}$	$s_{120/200}$ vs. $s_{16/20}$
AA	-0.206	-0.097	-0.098	-0.053
	(-10.05)***	(-2.42)**	(-2.46)**	(-1.29)
STAT	-0.005	0.107	0.121	0.127
	(-0.32)	(4.08)***	(4.62)***	(4.79)***
YOUNG	0.030	0.019	0.032	0.046
	(1.56)	(0.62)	(1.03)	(1.48)
CONF	-0.055	-0.085	-0.098	-0.094
	(-3.06)***	(-2.62)***	(-3.10)***	(-2.88)***
FEMALE	0.036	0.049	0.017	0.029
	(2.29)**	(1.88)*	(0.66)	(1.12)
$s_{12/20pref}$	-0.017			
	(-0.92)			
$s_{12/20pref} \times AA$	0.039			
	(1.82)*			
$s_{16/20}$		0.118		
		(3.15)***		
$s_{16/20} \times AA$		0.049		
		(1.24)		
$s_{120/200}$			-0.015	-0.137
			(-0.42)	(-3.36)***
$s_{120/200} \times AA$			0.098	0.051
			(2.51)**	(1.18)
Observations	2364	2364	2364	2364

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Control condition is the second one in each pair in the column head. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

neutral subjects, which is against both our assumption that the ambiguity premium decreases in π, and the associated Hypothesis 3. Neither there is any significant difference between ambiguity-averse and ambiguity-neutral subjects in the comparison of $s_{120/200}$ vs. $s_{16/20}$, which suggests the ambiguity premium is effectively flat. If the ambiguity premium is flat indeed, we have to conclude that there ought to be equally no difference between $s_{120/200}$ and $s_{12/20}$, which is against the data.

One possible explanation of this apparent puzzle is in the sensitivity of the ambiguity-premium to changes in π and δ: changing communicated probability from $\pi = \frac{12}{20}$ to $\frac{16}{20}$ produces a weak decrease in the ambiguity premium, statistically undetectable in our sample, and similarly the change from $\pi = \frac{16}{20}$ to $\frac{120}{200}$ leads to an insignificant decrease in θ_A, yet jointly the two effects add up and the overall impact of a change from $\pi = \frac{12}{20}$ to $\frac{120}{200}$ is noticeable and statistically significant, thus the difference between ambiguity-averse and ambiguity-neutral subjects in the comparison $s_{12/20}$ versus $s_{120/200}$. We return to this issue in Section 6.

4.3 Ambiguity-seeking

Our analysis focuses on ambiguity-averse subjects in comparison with ambiguity-neutrals. All results extend to the ambiguity-seeking behavior, with the only remark that the ambiguity premium $\theta_A(\pi, \delta)$ is negative, and its absolute value decreases in δ and in π (for large enough probabilities). This produces hypotheses symmetrical to those for ambiguity-aversion. In particular, we expect that less ambiguity-seeking subjects choose A after signals; they are more likely to respond to non-probabilistic signals; and they differently respond to probabilistic signals than ambiguity-neutral decision-makers. As an illustration, Table 6 presents the ambiguity-seeking counterpart of Table 4. Variable AS is defined similarly to AA: it takes value 1 if the Ellsberg test classifies subject as ambiguity-seeking, and 0 if as ambiguity-neutral. The interpretation of results is as above. Remarkably, confidence plays no role in the comparison of ambiguity-neutral and ambiguity-seeking subjects.

4.4 Behavioral Factors

The four behavioral variables are proficiency in statistics, confidence, gender and age. Their role in determining responses to signals is summarized in Table 7. Again, all signals have a significant impact on decisions, which robustifies the result obtained above. On average, subjects with a better knowledge of probabilities and statistics, are more likely to choose A, yet this effect comes mainly due to their response to probabilistic signals, see interaction terms in column "STAT".

As in Tables 3 and 4, confidence has a universally negative effect on subjects' responses, yet this time we can confirm this effect does not depend on the signal, apart from a weak positive interaction with the strongest probabilistic signal $s_{16/20}$ (the overall effect is still negative though). This suggests that confidence itself is a very strong decision factor: if subjects are convinced their view of the world is correct, they stick to it, whatever the news. Interestingly, confidence is insignificant for ambiguity-seekers, as in Table 6: all our signals communicate "good" probabilities (above $\frac{1}{2}$), which does not contradict the optimistic view of the world, while goes against the pessimistic view, for which reason confidence pulls ambiguity-averse subjects back. It is well possible that the role of confidence would be opposite if "bad" probabilities are communicated (below $\frac{1}{2}$), investigation of which is left to future research.

Table 6. Impact of signals and ambiguity-seeking (pooled data)

	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AS	1.482	1.485	1.480	1.469	1.444
	(54.31)***	(57.23)***	(56.49)***	(56.25)***	(53.76)***
STAT	-0.016	0.006	0.046	0.063	0.045
	(-0.70)	(0.27)	(1.95)*	(2.69)***	(1.95)*
YOUNG	0.028	0.026	0.038	0.012	0.044
	(1.04)	(0.92)	(1.37)	(0.42)	(1.70)*
CONF	-0.007	0.001	-0.041	-0.040	-0.036
	(-0.28)	(0.05)	(-1.56)	(-1.50)	(-1.41)
FEMALE	0.001	0.030	-0.027	0.002	-0.002
	(0.04)	(1.26)	(-1.17)	(0.08)	(-0.09)
s_{12pref}	0.058				
	(1.72)*				
$s_{12pref} \times AS$	-1.336				
	(34.89)***				
s_{12pref}		0.035			
		(1.05)			
$s_{12pref} \times AS$		-1.329			
		(-35.67)***			
$s_{12/20}$			0.090		
			(2.72)***		
$s_{12/20} \times AS$			-1.362		
			(-35.38)***		
$s_{16/20}$				0.163	
				(5.10)***	
$s_{16/20} \times AS$				-1.411	
				(38.29)***	
$s_{120/200}$					0.078
					(2.44)**
$s_{120/200} \times AS$					-1.283
					(-34.01)***
Observations	1220	1220	1220	1220	1220

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

Female subjects tend to choose A more frequently than males, except for the most precise signal $s_{120/200}$, to which male subjects react stronger. Young age does not seem to matter for decisions.

Table 7. Effects of behavioral factors on choices (pooled data)

	F = STAT	F = YOUNG	F = CONF	F = FEMALE
AA	-0.164	-0.167	-0.167	-0.166
	(-9.09)***	(-9.14)***	(-9.13)***	(-9.13)***
STAT	-0.074	0.053	0.053	0.053
	(-2.10)**	(3.80)***	(3.79)***	(3.79)***
YOUNG	0.025	0.028	0.025	0.025
	(1.51)	(0.72)	(1.50)	(1.51)
CONF	-0.070	-0.070	-0.128	-0.070
	(-4.31)***	(-4.30)***	(-3.73)***	(-4.30)***
FEMALE	0.029	0.028	0.028	0.050
	(1.99)**	(1.96)**	(1.96)*	(1.52)
s_{12pref}	0.125	0.133	0.103	0.137
	(5.44)***	(3.40)***	(2.79)***	(4.75)***
$s_{12/20pref}$	0.136	0.140	0.136	0.123
	(5.72)***	(3.45)***	(3.45)***	(3.93)***
$s_{12/20}$	0.312	0.384	0.312	0.388
	(13.14)***	(9.72)***	(7.87)***	(12.75)***
$s_{16/20}$	0.422	0.482	0.416	0.493
	(18.18)***	(12.32)***	(10.35)***	(16.39)***
$s_{120/200}$	0.346	0.414	0.363	0.452
	(14.54)***	(10.05)***	(8.75)***	(14.62)***
$s_{12pref} \times F$	0.046	0.007	0.050	0.003
	(1.04)	(0.15)	(1.14)	(0.08)
$s_{12/20pref} \times F$	0.052	0.015	0.025	0.046
	(1.23)	(0.32)	(0.55)	(1.15)
$s_{12/20} \times F$	0.155	-0.023	0.074	-0.036
	(3.78)***	(-0.52)	(1.63)	(-0.92)
$s_{16/20} \times F$	0.161	-0.002	0.085	-0.022
	(3.94)***	(-0.04)	(1.87)*	(-0.57)
$s_{120/200} \times F$	0.188	-0.001	0.070	-0.066
	(4.55)***	(-0.02)	(1.50)	(-1.66)*
Observations	7092	7092	7092	7092

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

To complement this view, we investigate if these factors have different effects on ambiguity-averse and ambiguity-neutral subjects' decisions. Table 8 presents results for these subsamples jointly and separately. Confirming our findings in Tables 3 – 7, subjects with better knowledge of statistics are more likely to choose A when exposed to probabilistic signals. This holds for both ambiguity-averse and –neutral subgroups. Apart from some effect of age in the treatment with the strongest signal $s_{120/200}$, and of gender in the treatment with signal $s_{16/20}$, behavioral factors do not matter for ambiguity-neutral subjects. In contrast, the effect of confidence in the above tables is attributable to its prominence in the ambiguity-averse cohort. Female subjects are also more likely to choose A in treatments with very vague news s_{12pref} and $s_{12/20pref}$.

Finally, we have also tested whether behavioral factors explain marginal effects of signals, similarly to Table 5, yet no significant effects were detected except for gender, confirming female subjects are less likely to respond to $s_{120/200}$. All other conclusions remain unaffected, therefore we do not report these results here.[18]

4.5 Understanding probabilities

Above we assumed subjects understand signals $s_{12/20} - s_{120/200}$ as information about the probability of drawing Red from the ambiguous urn. Since our messages are coded in a frequentist way, subjects with high proficiency in statistics should be better equipped to recognize this, which explains why $STATS$ raises the likelihood of choosing A in treatments with probabilistic signals (see Tables 7 and 8). This holds both for ambiguity-averse and ambiguity-neutral subjects. This observation allows us to assure that our signals have had a desired impact: in the cohort of subjects with high proficiency in statistics all probabilistic signals lead to an increase in the fraction of ambiguity-neutrals who choose A, see terms without interaction in Table 9. Low proficiency in statistics does not imply subjects do not understand probabilistic signals, as they demonstrate a significant response to $s_{16/20}$, yet it appears that signals $s_{12/20}$ and $s_{120/200}$ do not communicate a high enough probability for the ambiguity-neutral participants to significantly revise their π and switch to A.

Ambiguity-neutral subjects in the cohort with a high proficiency in statistics react equally to $s_{12/20}$ and $s_{120/200}$ (respective coefficients in Table 9 are equal: Wald test does not reject equality, $p = .552$). However the ambiguity premium effect on ambiguity-averse subjects (the interaction terms) is stronger with $s_{120/200}$ than with $s_{12/20}$ (Wald test $p = .047$). The ambiguity-premium effect confirms that $s_{120/200}$ induces a lower degree of ambiguity (higher precision) than $s_{12/20}$, and knowledge of probability and statistics helps subjects recognize this. Although in the cohort with low proficiency in statistics we also observe significant differences between ambiguity-neutral and ambiguity-averse subjects (significant

[18] Estimates are available from authors on request.

Table 8. Determinants of choices (pooled data)

	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
	Ambiguity-averse and ambiguity-neutral subjects				
STAT	-0.027	-0.016	0.096	0.110	0.140
	(-1.36)	(-0.79)	(3.34)***	(3.67)***	(4.76)***
YOUNG	0.034	0.039	0.007	0.034	0.059
	(1.40)	(1.57)	(0.20)	(0.97)	(1.68)*
CONF	-0.065	-0.084	-0.097	-0.084	-0.107
	(-2.87)***	(-3.74)***	(-2.74)***	(-2.22)**	(-2.93)***
FEMALE	0.025	0.062	0.040	0.063	-0.003
	(1.25)	(3.01)***	(1.38)	(2.09)**	(-0.09)
Observations	1182	1182	1182	1182	1182
	Ambiguity-averse only				
STAT	0.013	-0.018	0.083	0.103	0.133
	(0.85)	(-1.01)	(2.72)***	(3.15)***	(4.20)***
YOUNG	0.041	0.037	0.024	0.044	0.033
	(1.81)*	(1.62)	(0.64)	(1.13)	(0.86)
CONF	-0.051	-0.077	-0.110	-0.099	-0.147
	(-2.89)***	(-3.90)***	(-2.78)***	(-2.29)**	(-3.52)***
FEMALE	0.039	0.041	0.031	0.048	-0.012
	(2.41)**	(2.28)**	(1.00)	(1.45)	(-0.37)
Observations	992	992	992	992	992
	Ambiguity-neutral only				
STAT	-0.086	0.117	0.240	0.177	0.162
	(-1.04)	(1.46)	(2.84)***	(2.07)**	(1.99)**
YOUNG	-0.025	0.014	-0.116	-0.073	0.201
	(-0.27)	(0.15)	(-1.27)	(-0.81)	(2.19)**
CONF	0.046	-0.012	0.014	0.025	0.014
	(0.57)	(-0.15)	(0.17)	(0.31)	(0.18)
FEMALE	-0.103	0.088	0.043	0.131	0.030
	(-1.34)	(1.12)	(0.56)	(1.79)*	(0.40)
Observations	190	190	190	190	190

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. T-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

interaction terms), responses of ambiguity-averse participants to $s_{12/20}$ and $s_{120/200}$ do not differ much ($p = .404$). It appears that although participants recognize ambiguity "in general" and respond to it, knowledge of probabilities and statistics helps them recognize changes in ambiguity.

Table 9. Signals and proficiency in statistics.

	High proficiency in statistics, $STATS = 1$					Low proficiency in statistics, $STATS = 0$				
	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AA	-0.486	-0.497	-1.053	-1.054	-1.093	-0.652	-0.712	-1.089	-1.201	-1.140
	(-25.46)***	(-23.15)***	(-25.52)***	(-25.51)***	(-25.58)***	(-41.40)***	(-40.21)***	(-42.68)***	(-45.82)***	(-45.15)***
YOUNG	0.014	-0.006	0.010	0.028	0.028	0.021	0.026	-0.002	0.011	0.031
	(0.69)	(-0.27)	(0.32)	(0.87)	(0.90)	(1.25)	(1.43)	(-0.07)	(0.45)	(1.34)
CONF	-0.023	-0.041	-0.061	-0.045	-0.066	-0.043	-0.056	-0.057	-0.062	-0.072
	(-1.28)	(-2.34)**	(-1.97)**	(-1.39)	(-2.05)**	(-2.68)***	(-3.39)***	(-2.54)**	(-2.49)**	(-3.08)***
FEMALE	0.021	0.023	0.020	0.025	-0.010	0.007	0.030	0.012	0.036	0.005
	(1.40)	(1.50)	(0.80)	(0.98)	(-0.40)	(0.50)	(1.94)*	(0.60)	(1.70)*	(0.23)
s_{12pref}	0.017					0.032				
	(0.60)					(1.65)*				
$s_{12pref} \times AA$	0.397					0.490				
	(12.83)***					(21.65)***				
$s_{12/20pref}$		0.051					0.001			
		(1.96)**					(0.03)			
$s_{12/20pref} \times AA$		0.379					0.603			
		(12.58)***					(22.85)***			
$s_{12/20}$			0.197					0.037		
			(3.78)***					(1.04)		
$s_{12/20} \times AA$			0.924					1.067		
			(17.01)***					(26.41)***		
$s_{16/20}$				0.247					0.121	
				(4.57)***					(3.30)***	
$s_{16/20} \times AA$				0.986					1.184	
				(17.55)***					(29.24)***	
$s_{120/200}$					0.181					0.034
					(3.47)***					(0.94)
$s_{120/200} \times AA$					1.050					1.152
					(19.08)***					(28.33)***
Observations	896	896	896	896	896	1468	1468	1468	1468	1468

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

5 Robustness

In section 4.2 we altered the control condition in order to measure marginal effects of signals. Hypotheses 1 (ambiguity-averse subjects are less likely to choose A independent of signals), 2 (both types of subjects react to an increase in π), and 5 (ambiguity-neutral subjects equally react to signals of different precision) are confirmed. Hypothesis 4 (when subjects face more ambiguity, there is more difference between ambiguity-averse and -neutral subjects) is confirmed by the difference between the two types when ambiguity is high ($s_{12/20pref}$ versus s_{12pref}). With regards to Hypothesis 3, observed behavior suggests a puzzle that we are going to address later. The analysis of marginal effects thus robustifies the main findings from Table 4 by varying the control condition. Main results are also robust to re-focusing the analysis on ambiguity-seeking behavior, see section 4.3. In this section we provide two additional variations of the control condition, and then demonstrate consistency throughout the variety of experimental settings in our sample.

5.1 Alternative specification of the control condition

First, we re-define s_{ctrl} to be the first question in the Ellsberg task, and measure effects of all signals on subjects' choices as compared to this benchmark. Results in Table 10 confirm main findings (see coefficients for AA and the interaction terms), except that we observe now a significant negative effect of all signals on choices, which is due to the order bias, as discussed in Section 3.4.

Finally, we re-define s_{ctrl} as s_{12pref}, which, by design, is the least informative of all signals, and re-estimate the same model. This gives us a further conservative estimate, as some ambiguity-averse subjects have already responded to signal s_{12pref}: the difference between $s_{12/20pref}$ and s_{12pref} has already been reported in Table 6 as a marginal effect. Yet effects of all other signals confirm main findings, see Table 11. Note that, consistent with Table 2, results demonstrate the communicated probability in $s_{12/20}$ and $s_{120/200}$ is not high enough to generate any significant change in the behavior of ambiguity-neutral subjects, this time also controlled for their gender, age, confidence and knowledge of statistics. The equality of coefficients also demonstrates ambiguity-averse subjects equally [non-] respond to these two signals. Yet the significant interaction terms confirm stronger response of ambiguity-averse subjects, both to these two signals and to $s_{16/20}$.

5.2 Individual experiments

Above, we pooled data from several experiments, controlling for experiment-specific fixed

Table 10. Impact of signals and ambiguity-aversion (pooled data, alternative control condition)

	s_{12pref}	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AA	-0.687	-0.743	-1.293	-1.446	-1.433
	(-56.66)***	(-57.64)***	(-60.40)***	(-61.43)***	(-59.63)***
STAT	0.001	0.002	0.057	0.062	0.077
	(0.12)	(0.14)	(3.61)***	(3.84)***	(4.80)***
YOUNG	0.012	0.013	0.000	0.014	0.027
	(0.90)	(1.00)	(0.00)	(0.74)	(1.41)
CONF	-0.007	-0.020	-0.023	-0.017	-0.031
	(-0.49)	(-1.43)	(-1.16)	(-0.83)	(-1.55)
FEMALE	0.008	0.026	0.016	0.028	-0.004
	(0.78)	(2.28)**	(1.03)	(1.77)*	(-0.27)
s_{12pref}	-0.072				
	(-5.11)***				
$s_{12pref} \times AA$	0.543				
	(32.88)***				
$s_{12/20pref}$		-0.089			
		(-5.66)***			
$s_{12/20pref} \times AA$		0.624			
		(33.14)***			
$s_{12/20}$			-0.107		
			(-3.99)***		
$s_{12/20} \times AA$			1.230		
			(42.11)***		
$s_{16/20}$				-0.043	
				(-1.46)	
$s_{16/20} \times AA$				1.406	
				(42.68)***	
$s_{120/200}$					-0.121
					(-4.24)***
$s_{120/200} \times AA$					1.420
					(44.11)***
Observations	2364	2364	2364	2364	2364

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

Table 11. Impact of signals and ambiguity-aversion (pooled data)

	$s_{12/20pref}$	$s_{12/20}$	$s_{16/20}$	$s_{120/200}$
AA	-0.206	-0.327	-0.345	-0.343
	(-10.05)***	(-10.54)***	(-10.63)***	(-10.67)***
STAT	-0.005	0.046	0.051	0.065
	(-0.32)	(2.59)***	(2.86)***	(3.59)***
YOUNG	0.030	0.017	0.030	0.043
	(1.56)	(0.77)	(1.44)	(1.99)**
CONF	-0.055	-0.065	-0.061	-0.074
	(-3.06)***	(-3.04)***	(-2.78)***	(-3.41)***
FEMALE	0.036	0.028	0.040	0.009
	(2.29)**	(1.52)	(2.18)**	(0.49)
$s_{12/20pref}$	-0.017			
	(-0.92)			
$s_{12/20pref} \times AA$	0.039			
	(1.82)*			
$s_{12/20}$		0.036		
		(1.12)		
$s_{12/20} \times AA$		0.259		
		(7.06)***		
$s_{16/20}$			0.124	
			(3.61)***	
$s_{16/20} \times AA$			0.309	
			(7.80)***	
$s_{120/200}$				0.026
				(0.83)
$s_{120/200} \times AA$				0.341
				(9.28)***
Observations	2364	2364	2364	2364

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. ***$p < .01$, **$p < .05$, *$p < .1$.

effects. Table 12 presents main results for individual experiments, as well as for incentivized versus non-incentivized, and online versus lab-based experiments. All main results hold: ambiguity-aversion is robustly associated with a stronger reaction to all signals, ambiguity-neutral subjects robustly react to $s_{16/20}$; in experiment Web1 they also equally react to $s_{12/20}$ and $s_{120/200}$, while in all other experiments, except for Lab1, we are unable to detect any significant response of them to these two signals. The only exception here is Lab1, where ambiguity-neutral subjects seem to react to $s_{120/200}$ but not to $s_{12/20}$. This may be due to insufficient incentives and subsequently randomizing behavior of otherwise ambiguity-averse subjects, see discussion in Section 3.1.

All control variables also demonstrate consistent effects in individual experiments, except for gender: female subjects are more likely to choose A in online experiments, while less likely to do so in the lab setting (although the latter effect is insignificant, most likely due to the low number of observations). Potentially this can be associated with different impacts of online (typically accessed from home, at convenient time, relaxed and comfortable) and lab (formal, scheduled, less comfortable) environments on female and male subjects. As gender is used as a control variable, this difference does not affect our main results, yet raises a note of caution for experimental investigations of gender gaps in decisions, as results may be environment-sensitive.

Table 12. Effects of signals (individual experiments).

	Experiments					All	All	With	No
	Web1	Lab1	Web2	Web3	Lab2	online	lab	incentives	incentives
AA	-1.266	-1.518	-1.325	-1.152	-1.636	-1.356	-1.590	-1.559	-1.255
	(-16.77)***	(-19.92)***	(-28.86)***	(-21.05)***	(-23.98)***	(-44.79)***	(-31.72)***	(-40.03)***	(-35.85)***
STATS	0.013	0.031	0.076	0.071	-0.049	0.061	-0.015	-0.002	0.074
	(0.33)	(0.54)	(3.36)***	(2.93)***	(-0.96)	(3.98)***	(-0.39)	(-0.07)	(4.49)***
YOUNG	0.044	0.087	-0.002	0.018	0.012	0.020	0.051	0.040	0.019
	(1.10)	(1.64)	(-0.06)	(0.66)	(0.17)	(1.11)	(1.24)	(1.39)	(0.90)
CONF	-0.029	-0.075	-0.085	-0.081	-0.044	-0.077	-0.061	-0.057	-0.085
	(-0.57)	(-1.21)	(-3.34)***	(-2.51)**	(-0.89)	(-4.18)***	(-1.58)	(-1.84)*	(-4.28)***
FEMALE	0.069	-0.055	0.026	0.049	-0.070	0.044	-0.059	0.025	0.038
	(1.81)*	(-0.89)	(1.15)	(1.88)*	(-1.25)	(2.82)***	(-1.45)	(0.87)	(2.20)**
s_{12pref}	0.165	0.184	0.082	0.018	-0.081	0.080	0.034	0.072	0.057
	(1.54)	(1.54)	(1.31)	(0.26)	(-0.90)	(1.87)*	(0.46)	(1.22)	(1.24)
$s_{12/20pref}$	0.164	0.109	0.012	0.038	-0.058	0.052	0.012	0.056	0.022
	(1.97)**	(1.16)	(0.16)	(0.51)	(-0.68)	(1.16)	(0.18)	(1.10)	(0.42)
$s_{12/20}$	0.189	0.135	0.071	0.073	0.097	0.096	0.111	0.131	0.071
	(1.85)*	(1.31)	(1.03)	(1.06)	(1.05)	(2.16)**	(1.63)	(2.37)**	(1.44)
$s_{16/20}$	0.163	0.364	0.164	0.125	0.175	0.150	0.257	0.221	0.147
	(1.76)*	(3.94)***	(2.29)**	(2.00)**	(1.90)*	(3.47)***	(3.85)***	(4.24)***	(2.99)***
$s_{120/200}$	0.189	0.234	0.059	0.055	0.001	0.085	0.100	0.123	0.057
	(1.85)*	(2.25)**	(0.90)	(0.77)	(0.01)	(1.93)*	(1.49)	(2.25)**	(1.17)
$s_{12pref} \times AA$	0.970	1.224	0.955	0.827	1.320	1.014	1.292	1.261	0.904
	(8.39)***	(8.87)***	(14.07)***	(10.58)***	(10.96)***	(22.27)***	(14.45)***	(18.82)***	(17.83)***
$s_{12/20pref} \times AA$	1.008	1.346	1.027	0.833	1.520	1.058	1.446	1.356	0.949
	(11.02)***	(11.66)***	(13.46)***	(10.25)***	(14.37)***	(22.13)***	(18.43)***	(23.24)***	(16.99)***
$s_{12/20} \times AA$	1.207	1.587	1.216	1.086	1.602	1.269	1.599	1.518	1.165
	(11.28)***	(13.25)***	(16.98)***	(15.12)***	(14.66)***	(27.71)***	(20.00)***	(25.10)***	(22.61)***
$s_{16/20} \times AA$	1.325	1.421	1.243	1.146	1.663	1.329	1.557	1.527	1.205
	(13.55)***	(13.60)***	(16.76)***	(17.39)***	(15.38)***	(29.89)***	(20.06)***	(26.67)***	(23.54)***
$s_{120/200} \times AA$	1.261	1.551	1.286	1.158	1.752	1.336	1.670	1.582	1.235
	(11.79)***	(12.80)***	(18.68)***	(15.40)***	(16.71)***	(29.18)***	(20.98)***	(26.18)***	(24.28)***
N	1128	498	2766	2106	594	6000	1092	2220	4872

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

Finally, we report marginal effects of signals clustered for lab / web and incentivized / non-incentivized subsamples, see estimates for the signal effect and its interaction with ambiguity aversion in Table 13. All estimates are constructed the same way as in Table 5 but controls are not reported now, to save space. Consistently in all settings behavior of ambiguity-neutral subjects is affected neither by $s_{12/20pref}$ as compared to $s_{12/20}$ nor by $s_{120/200}$ as compared to $s_{12/20}$, confirming they do not respond to a change in ambiguity (precision). In online experiments (of which non-incentivized are a sub-sample) the difference between ambiguity-averse and -neutral subjects in response to $s_{120/200}$ as compared to $s_{12/20}$ is significant; it is positive but lack significance in lab experiments. In all settings, $s_{16/20}$ is a strong enough probabilistic signal to make all subjects to change choices, yet we only observe the difference between ambiguity-neutral and -averse subjects in the online sample, and only for the comparison between $s_{16/20}$ and $s_{12/20}$. Where effects are significant, they conform with our findings above.

6 Discussion

Our experiments confirm the hypotheses from Section 2, except for Hypothesis 3, for which we detected a violation in Section 4.2. First, subjects crudely classified as ambiguity-averse by the Ellsberg test, are persistently less likely to choose the ambiguous prospect in all treatments; similarly, ambiguity-seekers remain more likely to choose A whatever signal they receive. This lends support to using the Ellsberg test as a simple detector of ambiguity attitudes.

Second, positive news about "the fundamentals" (increase in π) makes all subjects more likely to choose the ambiguous prospect, yet ambiguity-averse subjects react stronger. Third, ambiguity-neutral subjects do not respond to non-probabilistic signals and respond equally to probabilistic signals of different precision. It is non-neutrality to ambiguity that makes subjects respond to news that bear little about the fundamentals; the effect appears to be through a perceived reduction in ambiguity; it works both for ambiguity-aversion and ambiguity-seeking. Although it is conceivable that subjects update their subjective probabilities based on the reported behavior of other subjects (e.g. if they believe that others possess information that they do not, which is quite plausible if no information is available), the non-response of ambiguity-neutral decision-makers rules this possibility out. Knowing more does not necessarily mean being better informed (especially so if information does not add knowledge about the fundamentals), yet people might feel confirmed in their

Table 13. Marginal effects of signals (by type of experiment)

	$s_{12/20pref}$ vs. s_{12pref}	$s_{16/20}$ vs. $s_{12/20}$	$s_{120/200}$ vs. $s_{12/20}$	$s_{120/200}$ vs. $s_{16/20}$
	S = $s_{12/20pref}$	S = $s_{16/20}$	S = $s_{120/200}$	S = $s_{120/200}$
		All Lab		
S	-0.020	0.177	-0.014	-0.189
	(-0.36)	(2.45)**	(-0.19)	(-2.51)**
S $\times AA$	0.148	-0.051	0.088	0.135
	(1.91)*	(-0.59)	(1.03)	(1.49)
Observations	364	364	364	364
		All online		
S	-0.016	0.082	-0.016	-0.102
	(-0.90)	(1.99)**	(-0.41)	(-2.23)**
S $\times AA$	0.026	0.091	0.100	0.011
	(1.23)	(2.09)**	(2.39)**	(0.24)
Observations	2000	2000	2000	2000
		All incentivized		
S	-0.013	0.117	-0.010	-0.127
	(-0.34)	(2.02)**	(-0.17)	(-2.04)**
S $\times AA$	0.082	0.012	0.085	0.072
	(1.69)*	(0.19)	(1.33)	(1.06)
Observations	740	740	740	740
		All non-incentivized		
S	-0.019	0.119	-0.021	-0.146
	(-1.04)	(2.60)***	(-0.47)	(-2.95)***
S $\times AA$	0.025	0.063	0.107	0.048
	(1.15)	(1.30)	(2.25)**	(0.92)
Observations	1624	1624	1624	1624

Note: The dependent variable is dummy equal to 1 if subject chose A in the relevant treatment. Control condition is the second one in each pair in the column head. Probit estimates. Marginal effects (evaluated at the mean of independent variables) reported. All regressions include variables AA, STAT, CONF, Young, Female and the constant term (not reported). Robust standard errors clustered at participant level. Z-statistics in parentheses. $^{***}p < .01$, $^{**}p < .05$, $^{*}p < .1$.

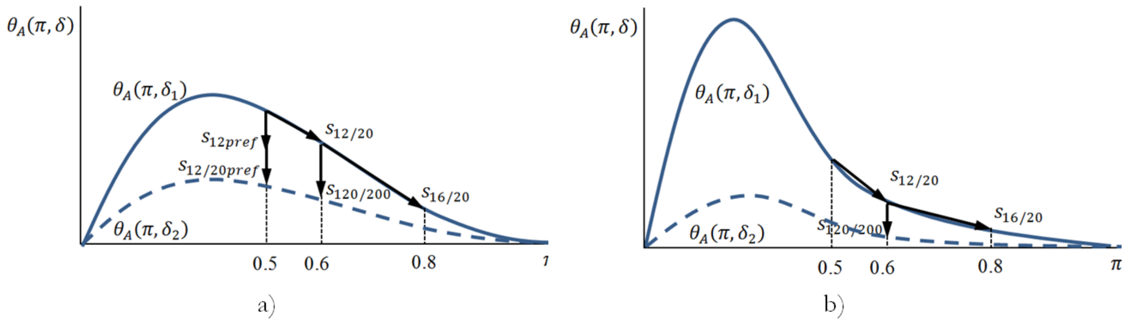

Figure 4. Hypothetical ambiguity premium and signals.

view of the world, for which reason they feel like they face less ambiguity.

We derived the hypotheses from a decomposition of the decision functional in probability and an ambiguity premium. It is the properties of the latter that explain the differences in responses of ambiguity-neutral and non-neutral subjects. The ambiguity premium depends not only on the level of ambiguity but also on risk (probability). Figure 4 (a) schematically places all our signals in the ambiguity premium framework. We depict signals s_{12} and $s_{12/20}$ in association with $\pi = 0.5$ as symmetry considerations dictate this level of probability for urn A (as in Dimmock et al., 2016), yet signals themselves cannot be linked to a change in this probability value (as ambiguity-neutral subjects do not respond). The communicated probability in other signals appears to be high enough in our experiments to ensure ambiguity-averse subjects react to them stronger than ambiguity-neutrals, which is only possible if the ambiguity premium declines in probability. Depending on the curvature of the ambiguity premium, signaling high probabilities may have a stronger effect than improving precision of low probabilities (panel (a) in Figure 4). Marginal effects in Table 5 demonstrate no difference in the reaction of ambiguity-averse and ambiguity-neutral subjects to a change from $s_{12/20}$ to $s_{16/20}$ and from $s_{16/20}$ to $s_{120/200}$, yet a significant difference for the move from $s_{12/20}$ to $s_{120/200}$. One explanation to this is that ambiguity premium becomes close to flat for high probabilities (panel (b) in Figure 4), even if it remains strictly positive.

Properties of the ambiguity premium have implications for the source functions of non-neutral to ambiguity subjects. For each individual, his source function for the ambiguous source $w_A(\pi)$ may be seen as a transformation of the source function $w(\pi)$ he would reveal for the unambiguous risky source. As demonstrated by Abdellaoui et al. (2011), this "transformation" implies a change in both parameters of the Prelec-type approximation. While a change in the ambiguity index, as demonstrated in Figure 1, is expected, as the level of ambiguity changes, a change in the likelihood insensitivity is not that obvious. The mechanism, by which ambiguity generates a change in the likelihood-insensitivity is in the ambiguity premium: $w_A(\pi) = w(m_A(\pi)) = w(\pi - \theta_A(\pi))$. An increase in the ambiguity premium for mid-range probabilities counterbalances an increase in the probability of success, thus adding likelihood insensitivity to $w_A(\pi)$, on top of that already contained in $w(\pi)$. Dimmock et al. (2016) call it "ambiguity-generated likelihood insensitivity" and derive the corresponding index a directly from $m_A(\pi)$, yet do not investigate the local properties of $m_A(\pi)$. As our analysis shows, local properties matter. One of those is the insensitivity of the ambiguity premium to high probabities, as demonstrated in Figure 4.

An alternative explanation may resort to the differential processing of decisions in ambiguity and risk by human brain (Hsu et al., 2005) and potential inability of subjects to process the two dimensions of uncertainty simultaneously. For example, in psychology, multiple visual stimuli may suppress recognition of each other, unless attention is directed to one of them, allowing filtering out irrelevant information (e.g. Kastner et al., 1998). Similarly, in the "editing" phase of decision-making, suggested by Kahneman and Tversky (1979), subjects simplify the decision task and may filter out information about ambiguity when comparing two probabilities, as in the pairs $s_{12/20}$ vs. $s_{16/20}$ and $s_{16/20}$ vs. $s_{120/200}$, for which reason we obtain no difference in responses of ambiguity-averse and ambiguity-neutral subjects. This corresponds to the flatter segment of the ambiguity premium curve. In contrast, directing attention to the ambiguity dimension, by comparing signals $s_{12/20}$ and $s_{120/200}$ that communicate the same value of probability, reveals the impact of a change in the ambiguity premium. In our experiments all questions came sequentially, in the following order: $s_{12/20}$, then $s_{16/20}$ and then $s_{120/200}$. While one could speculate that each of the two former signals "frames" the answer to the subsequent one, subjects never faced directly the comparison of $s_{12/20}$ and $s_{120/200}$. As in some of our experiments the difference in responses to the latter two was not statistically significant (see Table 13), the previous explanation, i.e. the relatively flat ambiguity premium, also appears plausible.

7 Conclusion

Decisions in uncertainty depend on the probabilistic and the non-probabilistic components of information. We have presented results of an Ellsberg-type experiment with signals that vary either the level of ambiguity, or probability, or both. On the theoretical side, we have isolated an ambiguity premium in the decision functional. The ambiguity premium determines the difference in responses of ambiguity-neutral and non-neutral subjects to vague news. For ambiguity-averse subjects it is strictly positive (unless uncertainty is fully resolved), decreases in the perceived level of ambiguity and, at least for high enough values, in the probability of success. On the empirical side, these properties explain the main differences we observe in experiments: when ambiguity is high, ambiguity-averse subjects respond to very vague news that bear no probabilistic component, respond stronger than ambiguity-neutral subjects to probabilistic news, and, unlike ambiguity-neutrals, differently respond to probabilistic news of different precision.

Reading and understanding news requires skills: subjects with higher proficiency in statistics respond stronger to news with a frequentist description of the probability of success. The difference in responses of ambiguity-averse and ambiguity-neutral subjects does not completely disappear when they face less ambiguous (more precise) signals, although their choices become more aligned. At the same time, when facing probabilistic news, subjects tend to disregard the ambiguity component, demonstrating insensitivity of the ambiguity premium to probabilities. However once equal probabilities are communicated and attention is drawn to precision, the ambiguity component plays a role, confirming the ambiguity premium is non-zero. Ambiguity premium (and the conjunct matching probability) thus explains much of the observed responses to vague news; it is easy to elicit experimentally, and understanding its local properties such as curvature and behavior at end points, on top of the global likelihood-insensitivity and the global ambiguity aversion, is important to predict choices of non-neutral to ambiguity subjects when the probability of success changes but remains imprecise.

Disentangling the content of news from its precision (ambiguity) is not always possible. Although we designed signals with an intention to have some that do and some that do not communicate probabilities, it was important to empirically establish this difference. The non-response of ambiguity-neutral subjects serves as a test of the non-probabilistic nature of signals in our experiments. Including an Ellsberg-type test as a routine question in surveys, such as those on inflation expectations and consumer behavior, may prove useful for such an identification in future research: in our analysis classifying subjects into ambiguity attitudes by the two-color Ellsberg test produces highly consistent results in all treatments and experimental settings.

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Appendix A. Appendix

A.1 Proofs

Proposition 1:

Proof. Substitute for $w_A(\pi) = w(\pi)$ in (1) to obtain $A \prec B \Leftrightarrow w(\pi) < w\left(\frac{1}{2}\right) \Leftrightarrow \pi < \frac{1}{2}$. Similarly $A \succ B \Leftrightarrow w(\pi) > w\left(\frac{1}{2}\right) \Leftrightarrow \pi > \frac{1}{2}$. For $A \sim B$ both $\pi < \frac{1}{2}$ and $\pi > \frac{1}{2}$ are ruled out by the above, hence $A \sim B \Leftrightarrow \pi = \frac{1}{2}$. ■

Proposition 2:

Proof. Ambiguity-neutrality: $w_A(\pi) = w(\pi) \Leftrightarrow w^{-1}w_A(\pi) = \pi \Leftrightarrow \pi - m_A(\pi) = 0$. For ambiguity-aversion, by monotonicity of w condition $w_A(\pi) < w(\pi)$ implies $w^{-1}w_A(\pi) < \pi$ and hence $\pi - m_A(\pi) > 0$, which translates into $\theta_A(\pi) > 0$. Taking the first derivative, we obtain $\theta'_A(\pi) = 1 - m'_A(\pi)$. To prove $\theta'_A(\pi) < 1$, we need to show $m'_A(\pi) > 0$. This follows from the monotonicity of the source functions: $m'_A(\pi) = \frac{\partial w^{-1}(w_A)}{\partial w_A}\frac{\partial w_A(\pi)}{\partial \pi} > 0$ as $\frac{\partial w_A}{\partial \pi} > 0$ and $\frac{\partial w^{-1}(w_A)}{\partial w_A} = \left(\frac{\partial w_A(w)}{\partial w}\right)^{-1} > 0$.

Let $\overline{\theta}_A(\pi) = c + (1-s)\pi$ be the least-squares regression line. By statistical properties, the regression line passes through the mean of the independent values, which is $\pi = \frac{1}{2}$, and through the mean of the dependent values, which is strictly positive due to $\theta_A(\pi) > 0$ for all π, hence $\overline{\theta}_A\left(\frac{1}{2}\right) > 0$. Together with linearity of the trend it implies $\frac{\overline{\theta}_A(0)+\overline{\theta}_A(1)}{2} > 0$, hence $b > 0$. Slope $\theta'_A(\pi) < 1$ for all π implies $a = 1 - s < 1$. ■

Proposition 3:

Proof. With monotonicity, $\theta'_A(\pi) < 0$ is equivalent to $\theta_A(\pi_2) < \theta_A(\pi_1)$ and $\theta'_A(\pi) > 0$ is equivalent to $\theta_A(\pi_2) > \theta_A(\pi_1)$. Consider $m_A(\pi_2) - m_A(\pi_1) = \pi_2 - \theta_A(\pi_2) - (\pi_1 - \theta_A(\pi_1)) = \pi_2 - \pi_1 - (\theta_A(\pi_2) - \theta_A(\pi_1))$. It follows that $m_A(\pi_2) - m_A(\pi_1) > \pi_2 - \pi_1$ iff $\theta_A(\pi_2) - \theta_A(\pi_1) < 0$, which holds iff $\theta'_A(\pi) < 0$. Similarly, $m_A(\pi_2) - m_A(\pi_1) < \pi_2 - \pi_1$ iff $\theta'_A(\pi) > 0$. ■

Proposition 4:

Proof. From $w_{A(0)}(\pi) = w(\pi)$ obtain $w^{-1}\left(w_{A(0)}(\pi)\right) = w^{-1}(w(\pi)) = \pi \Leftrightarrow \theta_A(\pi, 0) = 0$. Consider $w_{A(\delta_1)}(\pi) > w_{A(\delta_2)}(\pi)$. By monotonicity of $w(\cdot)$, obtain $w^{-1}w_{A(\delta_1)}(\pi) > w^{-1}w_{A(\delta_2)}(\pi) \Leftrightarrow \pi - \theta_A(\pi, \delta_1) > \pi - \theta_A(\pi, \delta_2) \Leftrightarrow \theta_A(\pi, \delta_1) < \theta_A(\pi, \delta_2)$. ■

A.2 Questionnaire

The following questionnaire was used without major alterations in all experiments reported in this paper. Minor alterations concerned the availability of the "Indifferent" option, on top of the options to choose urn A or urn B in experiments Web1 and Web2. This does not effect the classification of subjects as ambiguity-averse or ambiguity-neutral, as all experiments included a version of questions Q1 and Q2 without the indifference option. In the analysis of choices this indifference option might underestimate the fraction of subjects who choose A, thus providing us with a conservative estimate for our results.

The questionnaire:

Consider two identical urns each of which has 100 balls colored red and black. One of the urns has an unknown number of balls of each color. The other one has exactly 50 red and 50 black balls.

Balls are returned to the urns after each draw.

Part I

Q1. If a red ball is drawn you will get the prize. Would you prefer to draw the ball from Urn A or Urn B?

Q2. If a black ball is drawn you will get the prize. Would you prefer to draw the ball from Urn A or Urn B?

Part II

From now on you can get the prize only if the red ball is drawn.

Q3. 12 people before you preferred urn A to urn B when asked to draw a red ball. Which urn would you prefer now?

Q4. 12 out of 20 people before you preferred urn A to urn B when asked to pick a red ball. Which urn would you prefer now?

Q5. 12 people out of 20 picked a red ball from urn A. Which urn would you prefer now?

Q6. 16 people out of 20 picked a red ball from urn A. Which urn would you prefer now?

Q7. 120 out of 200 people picked a red ball from urn A. Which urn would you prefer now?

Part III

Q8. If you pick the wrong color you can return the ball to the urn and instead pick again. Will you pick again from the same urn?

Q9. (not used in this study) 8 out of 10 people who used their second chance before you, have changed the urn to pick the ball. Would you prefer now the same urn for your second chance?

Part IV

Q10. Please rate your knowledge of probability and statistics on the scale of 1 to 5, 1 being unfamiliar with probability and statistics (basic knowledge or no knowledge) and 5 being solid in these subjects (have taken a course on them, studied them somewhere else etc.).

Q11. Sex (M = male, F = female)

Q12. Age (subjects choose on the following scale: below 25, 25 – 35, 36 – 45, 46 – 55, 56 – 65, above 65)

END OF THE QUESTIONNAIRE