Collective choices under ambiguity

M. Vittoria Levati · Stefan Napel · Ivan Soraperra

Abstract We investigate experimentally whether collective choice environments matter for individual attitudes to ambiguity. In a simple two-urn Ellsberg experiment, one urn offers a 45% chance of winning a fixed monetary prize while the other offers an ambiguous chance. Participants choose either individually or in groups of three. Group decision rules vary in the level of individual responsibility for the others' payoffs: the collective choice is taken by majority, randomly delegated to two group members, or randomly delegated to a single group member. Although most participants display consistent ambiguity attitudes across their decisions, taking responsibility for the others tends to foster ambiguity aversion.

Keywords Ambiguity aversion · Majority voting · Random delegation · Experiment

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1 Introduction

Reliable probabilistic information about all possible consequences of a decision is the exception rather than the rule not only for individual but also for important collective decisions. The ambiguities associated with an individual's choice, for instance, to date a particular person, to accept a job offer, or to buy a house have their analogues in the decisions of company boards, international councils, government cabinets, or hiring committees to install new management, to take military action, to endorse a reform, to select a candidate, and so on. Such collective decisions often affect many individuals—sometimes very many—that either are not involved in the decision making or share responsibility for the outcome asymmetrically. Herein we investigate whether the particular type of responsibility required in a collective choice situation influences decision makers, leading them to change the attitudes to ambiguity they reveal in an individual choice situation.

It is well-known since the contribution of Ellsberg (1961) that individual decision making under ambiguity can substantially differ from that under risk. In particular, the analytically convenient assumption that people choose as if they maximized expected utility for some subjective probabilistic assessment (subjective expected utility theory) is understood to have clear limits. Many participants in experiments reveal a distaste for ambiguity that differs from traditional risk aversion; some also seek ambiguity. A recent survey of 39 experimental studies indicates that, on average, slightly more than 50% of subjects can be classified as ambiguity averse (Oechssler and Roomets 2014).[1] Several models of non-expected utility maximization have been developed in order to accommodate these findings (see, e.g., Gilboa and Schmeidler 1989, Klibanoff et al. 2005, Maccheroni et al. 2006, or Wakker 2010).

While much research has been conducted on the reaction of individual decision makers to ambiguity, surprisingly few experimental studies have investigated the reaction of groups. The existing results are mixed. Keller et al. (2007) asked for individuals' and dyads' hypothetical willingness to pay for ambiguous gambles and reported no conclusive differences between individual and group attitudes towards ambiguity. The robustness of ambiguity attitudes to social interaction has also been investigated by Charness et al. (2013), who found that the number of ambiguity neutral subjects increased after commu-

[1] See also Camerer and Weber (1992) for a comprehensive review of early empirical and theoretical work on ambiguity.

nication with another participant, though only in the presence of monetary incentives to persuade the other. This increase was obtained mainly at the expense of ambiguity seeking and ambiguity incoherent behavior and to a lesser extent at the expense of ambiguity averse behavior. Keck et al. (2014) distinguished between a treatment where participants had to make individual decisions, a treatment where participants had to decide individually after a group discussion, and a treatment where group members had to arrive at a joint decision after face-to-face interaction. They observed that groups as well as individuals after group discussion took more ambiguity neutral decisions than individuals who decided in isolation. Brunette et al. (2015) studied the impact of two collective decision rules—unanimity and majority—on individual attitudes to risk and ambiguity. They showed that groups, especially under the unanimity rule (implemented via an iterative process where group members had up to five trials to reach an agreement), were less risk averse than individuals, but did not detect any statistically significant change in ambiguity attitudes between individuals and groups (whatever the decision rule).

The present paper complements this literature by exploring if making decisions not only with but also *for* others influences individual ambiguity attitudes, independently of personal communication and coordination possibilities. We consider three collective decision rules in an environment with minimal interaction. The three collective decision rules vary in the level of individual responsibility for the group's choice: majority rule, random delegation to two group members, and random delegation to one group member.

While there is no theoretical reason why a decision maker's ambiguity attitude should differ between collective and individual choice contexts (the mentioned literature on non-expected utility is indeed silent on collective decisions), there exists some evidence that fear of negative evaluation by others enhances ambiguity aversion. In an early study, for instance, Curley et al. (1986) found an increase in ambiguity aversion when people knew that their decision was observed by a group of peers. The relevance of external judgments to ambiguity attitudes has been later confirmed by Muthukrishnan et al. (2009), who reported more ambiguity averse choices when participants knew that the true probability of winning a prize in an ambiguous gamble would be revealed in the presence of others, rather than in private. In an experiment in which the possibility of a negative evaluation by others was ruled out, Trautmann et al. (2008) showed that ambiguity aversion decreased significantly.[2] The reason put forward to justify the enhanced ambiguity aversion in social contexts is that the potential bad outcome after the risky choice, being attributable to bad luck, is more easily justifiable than the bad outcome after the ambiguous choice (which misses any probabilistic information). In our experiment, in the collective choice situations featuring one or two randomly selected delegates, the decision of the delegate(s) is not only revealed to the fellow members but also affects their payoffs. So fear of implicit blame may induce individuals who decide in a group and potentially impose their choice on the others to play it 'safe'. That is, they may rather go for a known chance instead of betting 'recklessly' on an event that can turn out to have been almost impossible to realize after the resolution of ambiguity. Our initial hypothesis is therefore that full or partial responsibility for the payoffs of others renders the fear of a negative evaluation salient and enhances ambiguity aversion.

Our paper is related to a recent and still emerging strand of experimental literature that studies risk taking on behalf of others in situations where there is one decision maker and the monetary payoffs of all group members are identical. The results from these studies are not clear-cut, although they were admittedly obtained under different experimental designs.[3] For example, Sutter (2009) used the investment task introduced by Gneezy and Potters (1997) and found that letting a member of a three-person group be responsible for the group payoffs increased investment levels into the risky asset compared to an individual baseline. No effect of social contexts on risk taking has been detected, however, by Bolton and Ockenfels (2010), investigating binary choice problems where the decision maker and another person were equally affected by the decisions, and by Humphrey and Renner (2011), who elicited risk preferences using a multiple price list design popularized by Holt and Laury (2002). Considering risky choices with both gains and losses, Pahlke et al. (2012) and Andersson et al. (2016) found that deciding for others reduced loss aversion in, respectively, a standard laboratory experiment with a student subject pool and an internet experiment with a large and heterogeneous sample of the general

[2] It has also been shown that ambiguity aversion is affected by the comparison with more familiar events or more knowledgeable individuals (e.g., Fox and Tversky 1995, Chow and Sarin 2002, Fox and Weber 2002).

[3] Experiments in which a person decided for one or more others with no consequence to himself have also been carried out. Findings are again not unanimous. Indeed, while Reynolds et al. (2009) and Eriksen and Kvaløy (2010) found individuals to be more risk averse when deciding for others than when deciding for themselves, Chakravarty et al. (2011) detected increased risk taking in decisions for others.

Danish population. Vieider et al.'s (2016) structural model analysis confirmed this finding, but also indicated that it held only for some specific definitions of loss aversion. According to both Pahlke et al. (2012) and Andersson et al. (2016), being responsible for the payoffs of others has a debiasing effect on loss aversion. In all cited studies, a single decision maker took decisions for himself and somebody else, and the various possible outcomes had known probabilities. We extend this previous work by investigating how not only full but also partial responsibility for the others' payoffs affects preferences in the case of ambiguous prospects.

We next describe the experiment, laying out our design and procedures. We present the results in Section 3, and conclude in Section 4.

2 Experimental design

2.1 Tasks and treatments

We use a particularly simple and tested experimental design which varies the standard Ellsberg two-urn experiment. There are two urns: urn K and urn U. Each urn contains 40 balls. Urn K (labelled 'A' in the experiment) is clear and, as such, is known to contain 18 yellow balls and 22 balls of various other colors. It will offer participants a known 45% chance of winning a monetary prize. Urn U (labelled 'B' in the experiment) is opaque and contains an unknown proportion of black and white balls. This urn will offer an ambiguous chance of winning the prize.

Participants are required to express their preferences for three prospects that are simple bets. In particular, one ball is drawn at random from each urn at the end of the experiment. Call U_B the event "black ball drawn from urn U", U_W the event "white ball drawn from urn U", and K_Y the event "yellow ball drawn from urn K". Each participant—whatever the experimental condition—must place three bets on these events in the following order:

(i) betting €20 on either U_B or K_Y,
(ii) betting €20 on either U_W or K_Y,
(iii) betting €20 on either U_B or U_W.

The first two binary choices elicit preferences over bets that relate to the ambiguous and risky urns, while the bet in the last binary choice concerns only the ambiguous urn. We view stating preferences over three different bets of

€20 as a comparatively easy task, which allows us to answer our main research question—do individual attitudes to ambiguity vary with different degrees of responsibility in collective decisions?—in a straightforward manner.[4]

The possible combinations of individual choices are indicated in column 1 of Table 1. Assuming a strict preference for winning one's bets, we can impute possible subjective beliefs about the number w of white balls from each binary choice. We classify a choice combination as consistent with subjective expected utility maximization (SEU) if the same w can rationalize all three choices. If no such w exists and cyclical preferences can be ruled out, the choices exhibit ambiguity aversion (AA). Remaining combinations are classified as intransitive (INTR).

We remark that our asymmetric composition of risky urn K departs slightly from the canonical Ellsberg two-urn experiment. There, the risky urn contains a 50:50 proportion of winning and losing balls. An expected utility maximizer may—quite likely—apply the principle of insufficient reason to form subjective beliefs about ambiguous urn U. Consequently, in the canonical case, he will be indifferent between urns and may break the tie arbitrarily. The choice of K in one experimental condition and of U in another condition may then not reflect a shift in preferences, but just different tie-breaking. This creates noise in the data and requires a large sample size to be able to attribute observed differences in choices to treatment effects. Asymmetric odds for the risky urn, in contrast, induce strict preferences for expected utility maximizers who follow the principle of insufficient reason, thus reducing the amount of noise in the data.

Of course, design decisions need to be made about the direction and scope of asymmetry. Stacking urn K's odds in favor of winning would allow us to discriminate between SEU and ambiguity love, whereas stacking K's odds in favor of losing means distinguishing between SEU and AA. Still, either design option permits the detection of a preference shift: as long as the proportion of winning to losing balls in urn K is kept constant across conditions, we can attribute choice changes to treatment effects. The same argument applies to different options for the scope of asymmetry between winning and losing in K:

[4] An alternative would have been to ask subjects to evaluate a given ambiguous prospect against a list of risky prospects, with varying expected values. This multiple price list (MPL) procedure would have enabled us to quantify ambiguity attitudes, but it unnecessarily complicates the design of the conditions with shared responsibility. Additionally, as remarked by Andersen et al. (2006), a MPL allows for multiple switching points, thus leading to potentially inconsistent decisions, and may be susceptible to framing effects.

Table 1 Choices and implied preferences

Choices (i)–(iii)	Implied # of white balls w	INTR	AA	SEU
$K_Y \succsim U_B$ $U_W \succsim K_Y$ $U_W \succsim U_B$	$w \in [22, \ldots, 39]$			X
$U_B \succsim K_Y$ $K_Y \succsim U_W$ $U_B \succsim U_W$	$w \in [1, \ldots, 18]$			X
$U_B \succsim K_Y$ $U_W \succsim K_Y$ $U_W \succsim U_B$	$w \in [20, \ldots, 22]$			X
$U_B \succsim K_Y$ $U_W \succsim K_Y$ $U_B \succsim U_W$	$w \in [18, \ldots, 20]$			X
$K_Y \succsim U_B$ $K_Y \succsim U_W$ $U_W \succsim U_B$	$w \leq 18 \wedge w \geq 22$		X	
$K_Y \succsim U_B$ $K_Y \succsim U_W$ $U_B \succsim U_W$	$w \leq 18 \wedge w \geq 22$		X	
$K_Y \succsim U_B$ $U_W \succsim K_Y$ $U_B \succsim U_W$	$w \geq 22 \wedge w \leq 20$	X		
$U_B \succsim K_Y$ $K_Y \succsim U_W$ $U_W \succsim U_B$	$w \leq 18 \wedge w \geq 20$	X		

although subjects that are only mildly ambiguity averse may be (mis-)diagnosed as subjective expected utility maximizers if K's odds are 45:55 rather than 49:51, what matters is that probabilities do not vary across experimental conditions.

We strove to adopt odds for the risky urn which would identify clearly a suitably large group of non-SEU choices in a baseline condition. Given the relative prevalence of ambiguity aversion compared to ambiguity love documented in the literature (see Oechssler and Roomets 2014), this meant stacking urn K's odds in favor of losing.

Our research question will be addressed by comparing one individual and three group conditions: majority voting, random designation of two delegates, and random designation of a single delegate. In the *individual (I)* condition, each subject's possible winnings are determined only by the individual binary choices (i)–(iii) presented above. In each of the three group conditions, each subject is randomly assigned to a group of three people. This group places a single collective bet based on individual responses to problems (i)–(iii) and a collective decision rule, without scope for communication and coordination. In the *majority voting (MV)* condition, the collective decision is made by majority rule. In the *two-delegate (D2)* condition, it is determined by two randomly selected group members. In the *one-delegate (D1)* condition, it is determined by the individual choices of one randomly selected group member. The random selection of delegates in *D2* and *D1* takes place after each group member has responded to problems (i)–(iii). In all group conditions the monetary gains of the three members are *identical*, from an ex ante and an ex post perspective. The social risk and social ambiguity that subjects face in our study therefore do not provide a role for equity concerns in the tradition of Fehr and Schmidt (1999) or Bolton and Ockenfels (2000).

Given the binary choices (i)–(iii) faced by the participants, implementing condition *D2* requires us to follow a two-step sequential procedure in order to determine a group's relevant bet, i.e., the bet allowing each group member to win €20.[5] To ensure comparability across conditions, we use the same sequential procedure in all of them. More specifically, in each of the four conditions:

- first we look at choice (iii), i.e., at the bet on the color of the ball drawn from urn U;
- then we consider the participant's or group's preferences between the preferred bet on urn U and the bet on a yellow ball being drawn from urn K; that is, we compare the event individually or collectively chosen for urn U (either U_B or U_W) to K_Y.[6]

Although the described two-step procedure may render one of the three binary choices in I and *D1* not incentive compatible, only one misclassification of preferences could result from this. It concerns participants who want to bet on K_Y. In particular, there are four choice patterns that render K_Y the relevant bet in I and *D1*: two of these patterns are classified as AA ($K_Y \succsim U_W \succsim U_B$ and $K_Y \succsim U_B \succsim U_W$) and two as INTR ($U_W \succsim K_Y \succsim U_B \succsim U_W$ and $U_B \succsim K_Y \succsim U_W \succsim U_B$). It could therefore happen that participants

[5] The standard experimental economics practice of randomly selecting one of the three binary choices to determine the final outcome is not a feasible option when responsibility for the group payoffs must be shared between two group members.

[6] The sequence makes no difference in case of transitive preferences and sincere voting. If subjects—despite the lack of preference information in our communication-free design—seek to be strategic, there is scope for misrepresentation of preferences in *D2* and *MV*. This scope exists for *any* collective decision rule—sequential or not—if it is non-authoritarian and permits each bet to be selected (cf. the Gibbard-Satterthwaite theorem). Manipulation would, however, require very specific subjective beliefs and then affect only one's choice of U_B vs U_W.

who want to bet on K_Y are classified erroneously as intransitive. To control for this concern, we need to show (and we actually do) that the proportion of intransitive choices does not differ significantly across conditions. In particular, we shall compare the proportion of intransitive choices in conditions *MV* and *D2*—where intransitive preferences are truly intransitive—with the proportion of intransitive choices in *I* and *D1*. If, in *I* and *D1*, there were AA preferences mistakenly classified as INTR, we should observe fewer intransitive preference patterns in *MV* and *D2* than in *I* and *D1*. This is not the case.[7]

Additionally, to minimize the possibility of diagnosing a spurious intransitivity, we presented choices (i)–(iii) one after another on a clean computer screen. Hence, an ambiguity averse participant who wants to bet on K_Y would return an intransitive pattern such as "$K_Y \succsim U_B \succsim U_W \succsim K_Y$" only if he realized, when making choices (i) and (ii), that choice (iii) would render choice (ii) irrelevant.

Individual responsibility for the payoffs of other group members differs across the three group conditions. It is arguably maximal in *D1*: when a group member is randomly drawn as the delegate, his own choices (i)–(iii) determine the group's bet. In *D2*, responsibility is shared with one other randomly selected delegate. In *MV*, responsibility is shared with both other group members.[8]

To explore possible preference shifts, we employ a within-subjects design. That is, each participant must make decisions in condition *I* and in one of the three group conditions. To control for potential order effects, we run six treatments: in treatments *I-MV*, *I-D2* and *I-D1* subjects are first confronted with condition *I* and then with the respective group condition; treatments *MV-I*, *D2-I* and *D1-I* reverse this order. As we take into account order effects, we deem it unnecessary to implement a treatment *I-I*, where participants face condition *I* twice.

[7] Note that if a participant wants to bet on, e.g., U_W, so that the choice between U_B and K_Y becomes payoff irrelevant, this does not undermine the identification of the participant's preference type because the two choice patterns selecting U_W as the relevant bet (namely $U_W \succsim K_Y \succsim U_B$ and $U_W \succsim U_B \succsim K_Y$) are both classified as SEU.

[8] We refer readers to Braham and van Hees (2012) for a philosophical discussion of degrees of responsibility and the thorny issue of how individuals can be held responsible for a collective outcome for which their individual choices played no pivotal role.

2.2 Procedures

The experiment was programmed in Fischbacher's (2007) z-Tree software and conducted in the experimental laboratory of the Max-Planck Institute of Economics in Jena, Germany. The participants—undergraduate students from the Friedrich-Schiller University of Jena—were recruited using Greiner's (2004) ORSEE software.

The two urns—the clear urn K and the opaque urn U—were on display on the experimenter's desk, so that subjects could be certain that their contents could not be manipulated during the experiment. The opaque urn had been filled arbitrarily by an experimenter prior to each session. Participants were invited to check the contents of the urns after completion of the experiment and some did.

The full sequence of events, in all sessions and all treatments, unfolded as follows. Upon entering the laboratory, participants were randomly assigned to visually isolated computer terminals. Then all participants received written instructions informing them that the experiment included two parts, and that only one part would be randomly selected for payment at the end of each session. Paying out only one of the two parts allows minimizing portfolio effects (see, e.g., Cox et al. 2015). To mitigate potential demand effects, participants were immediately given only the instructions for Part 1, which were also read aloud in order to establish public knowledge. The instructions for Part 2 were distributed and read aloud after all participants completed Part 1.[9] Before making their choices in any of the two parts, participants had to go through a series of control questions. Only after the experimenter had ensured that everyone understood the instructions, the corresponding condition could start. Finally, participants were administered a post-experimental questionnaire, collecting socio-demographic characteristics such as age and gender.

In each condition, participants were informed about their task and the sequential procedure used to determine the bet relevant to them (in condition *I*) or to their group (in conditions *MV*, *D2*, and *D1*).

– In condition *I*, participants knew that their possible winnings would be determined by considering first their choice between U_B and U_W and then either their choice between U_B and K_Y (if they preferred U_B to U_W) or their choice between U_W and K_Y (if they preferred U_W to U_B).

[9] A translated version of the instructions can be found in the supplement.

- In condition MV, group members were informed that the two relevant choices (U_B versus U_W, and then either U_B versus K_Y or U_W versus K_Y) would be made by majority rule.
- In condition *D2*, group members knew that, after each one of them had responded to problems (i)–(iii), the computer software would randomly select two group members to be the group's delegates and proceed as follows: calling the delegates member X and member Y, as in the instructions, the software would first check member X's choice between U_B and U_W. If X preferred U_B to U_W, the software would place a bet according to member Y's choice between U_B and K_Y; otherwise, if X preferred U_W to U_B, it would implement member Y's choice between U_W and K_Y. So Y's preferences between K_Y and the color from urn U which was selected by X would determine the bet relevant to the group.
- In condition *D1*, group members were aware that, after each one of them had made the three binary choices, the computer software would randomly select one group member as the delegate. The delegate's individual choices would directly determine the bet relevant to the group.

At the end of each part, participants were given feedback on the bet relevant to them or to their group. In condition MV, the three group members received information about the color from urn U selected by the majority and about the majority preference between the selected color from U and K_Y. In the random delegate conditions *D2* and *D1*, group members were first informed about their role (i.e., whether they had been been selected as delegate X, or as Y, or not at all); then they were revealed the relevant choices of the two delegates in *D2* and of the unique delegate in *D1*. Except for earning the same payoffs, there was no interaction among group members; they remained anonymous to each other.

At the end of each session, a randomly selected participant determined the part that was paid out by drawing one of two cards numbered "1" and "2" from an opaque bag; then another randomly selected participant drew a ball from each urn. Subjects were paid €20 if the color of the ball drawn from the (individually or collectively) chosen urn matched that of their bets, and nothing otherwise.

Sessions lasted about 50 minutes. 447 students (182 males and 265 females) participated in the experiment in total. Participants were 23.5 years old on average (s.d. 3.8) and they were enrolled in different fields of study with the most common majors being Engineering (34%), Science/Medicine (19%),

Table 2 Number of participants for each type of preference

Choices	Pref.	Freq. I				Freq. $D2$		Freq. MV		Freq. $D1$	
		I-XX[†]	MV-I	$D2$-I	$D1$-I	$D2$-I	I-$D2$	MV-I	I-MV	$D1$-I	I-$D1$
$U_W \succsim K_Y \succsim U_B$	SEU	21	13	11	9	7	3	12	7	7	6
$U_B \succsim K_Y \succsim U_W$	SEU	31	11	13	14	12	7	16	5	12	9
$U_W \succsim U_B \succsim K_Y$	SEU	25	12	13	8	7	6	10	11	8	5
$U_B \succsim U_W \succsim K_Y$	SEU	20	7	14	16	12	7	7	8	12	7
$K_Y \succsim U_W \succsim U_B$	AA	41	26	20	26	20	16	22	15	27	14
$K_Y \succsim U_B \succsim U_W$	AA	32	16	17	13	28	20	19	13	18	18
$U_W \succsim K_Y \succsim U_B \succsim U_W$	INTR	6	3	1	1	2	0	2	1	3	1
$U_B \succsim K_Y \succsim U_W \succsim U_B$	INTR	4	2	1	0	2	1	2	0	0	0
	Σ	180	90	90	87	90	60	90	60	87	60

[†] This column reports the results from treatments *I-MV*, *I-D2*, and *I-D1* (60 subjects each).

and Economics and Business Administration (18%). Average earnings were approximately €15 (inclusive of a €4 show-up fee).

3 Results

Table 2 shows the number of participants who chose in accordance with SEU, AA and INTR preferences (as listed in Table 1), separately for each treatment.

We start the analysis by allaying possible concerns about spuriously diagnosing an intransitivity rather than ambiguity aversion in conditions I and $D1$. We recall that the only misclassification that can occur involves bets on K_Y (which may be classified as intransitive instead of ambiguity averse) and that the proportions of intransitive choices should be higher in I and $D1$ than in MV and $D2$ if misclassifications are present.

Looking at Table 2, two things stand out. First, INTR choices are rare in Part 1 and become very rare in Part 2, whatever the treatment. Second, the proportion of INTR choices in conditions I and $D1$ is not different from that in conditions MV and $D2$. Considering Part 1 choices, intransitive preference patterns are diagnosed for 5.56% (10/180) of the subjects in condition I, 4.44% (4/90) in $D2$ and MV, and 3.45% (3/87) in $D1$. Fisher's exact tests do not reject the null hypothesis that these ratios are the same across conditions (p-values ≥ 0.367 uniformly).

We can also rule out the possibility that a high fraction of subjects were indifferent to winning their bets and chose at random. Under indifference, we should expect a distribution close to 50% SEU, 25% AA and 25% INTR—which we don't. Exact goodness-of-fit tests indeed reject the null hypothesis that the data come from a multinomial distribution with parameters 0.5, 0.25, and 0.25 in all conditions (p-values < 0.001). We may thus safely assume that the binary choices represent actual preferences over the prospects, and that these are predominantly strict. As an additional control, we performed the analysis reported in this section using a more conservative classification of ambiguity averse preferences, namely we classify the subjects showing intransitive choices in conditions *I* and *D1* as ambiguity averse. This different classification strategy does not alter qualitatively any of the results reported below.

Turning to our main research interest, rows 5 and 6 of Table 2 indicate that the percentage of ambiguity averse choices is non-negligible in all conditions: it ranges from 40.6% (73 out of 180) in condition *I* of treatments *I-MV*, *I-D2* and *I-D1* to 60.0% (36 out of 60) in condition *D2* of treatment *I-D2*. Individual ambiguity aversion *is* an important issue also in collective choice.

Figure 1 illustrates the relative proportions of ambiguity averse choices observed in the six treatments, considering only the 418 out of 447 participants with transitive choice patterns in both parts of the experiment. The figure consists of six blocks of two bars each. Regardless of the order in which the conditions were encountered, the first bar of each block represents the proportion of AA choices in the individual condition, and the second bar shows the proportion of AA choices in the group condition indicated at the bottom of the bar.

The figure suggests that the frequencies of ambiguity averse choices in *I*, *MV*, *D2* and *D1* have not been affected by the order in which the condition was faced. A series of χ^2-tests confirm that there are no significant order effects ($\chi^2(5) = 3.159$ with p-value $= 0.675$ for *I*; $\chi^2(1) = 0.010$ with p-value $= 0.919$ for *MV*; $\chi^2(1) = 0.470$ with p-value $= 0.493$ for *D2*; and $\chi^2(1) < 0.001$ with p-value $= 0.987$ for *D1*). Thus, in investigating how often subjects prefer betting on the known urn in each condition compared to betting on either color from the unknown urn, we will pool the data from different sequences.

Comparing the frequency of ambiguity averse choices in the individual and majority voting conditions (blocks *I-MV* and *MV-I*), we observe virtually no difference in AA choices when participants choose individually and when

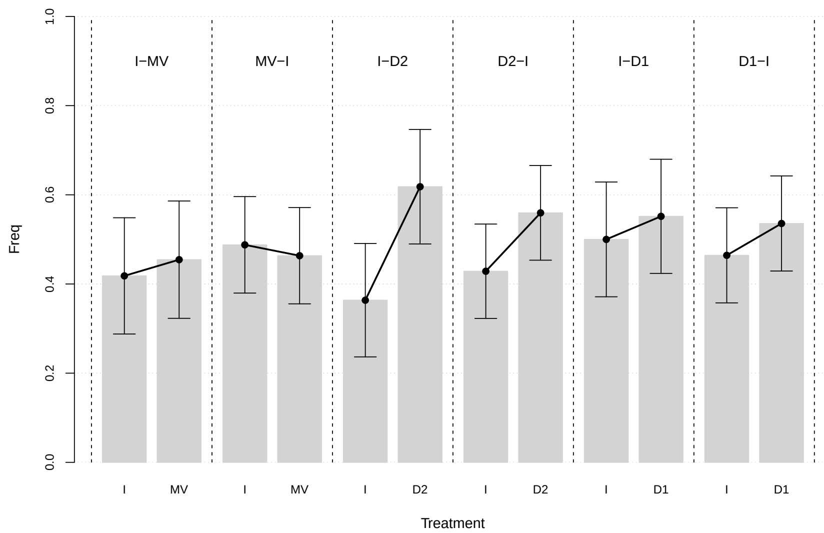

Fig. 1 Frequency of ambiguity averse choices by treatment (transitive subjects only)

they choose under majority rule (McNemar's $\chi^2(1) = 0.000$, p-value=1.000). This result is consistent with that of Brunette et al. (2015), differences in preference elicitation procedures notwithstanding. As to the treatments with random delegation to one group member (blocks *I-D1* and *D1-I*) or to two group members (blocks *I-D2* and *D2-I*), Figure 1 shows that the frequency of ambiguity averse choices is higher when one or two group members impose their preferences on the others than when subjects choose individually. McNemar's chi-square tests on within-subject data indicate that the result is strongly significant for *D2* ($\chi^2(1) = 17.857$, p-value<0.001), and close to significance at the 10% level for *D1* ($\chi^2(1) = 2.454$, p-value=0.117). Using between-subject data, namely choices made in the first part, yields similar results ($\chi^2(1) = 0.271$ with p-value=0.602 for *I* vs *MV*; $\chi^2(1) = 3.853$ with p-value=0.050 for *I* vs *D2*; $\chi^2(1) = 2.585$ with p-value=0.108 for *I* vs *D1*).

Comparing the proportion of ambiguity averse choices across group conditions, we see that this proportion is about the same for *D2* and *D1* ($\chi^2(1) = 0.4677$, p-value=0.494), whereas it is higher for *D2* and *D1* compared to *MV*. The difference is significant for *D2* vs *MV* ($\chi^2(1) = 4.175$, p-value=0.041), but not for *D1* vs *MV* ($\chi^2(1) = 1.894$, p-value=0.169).

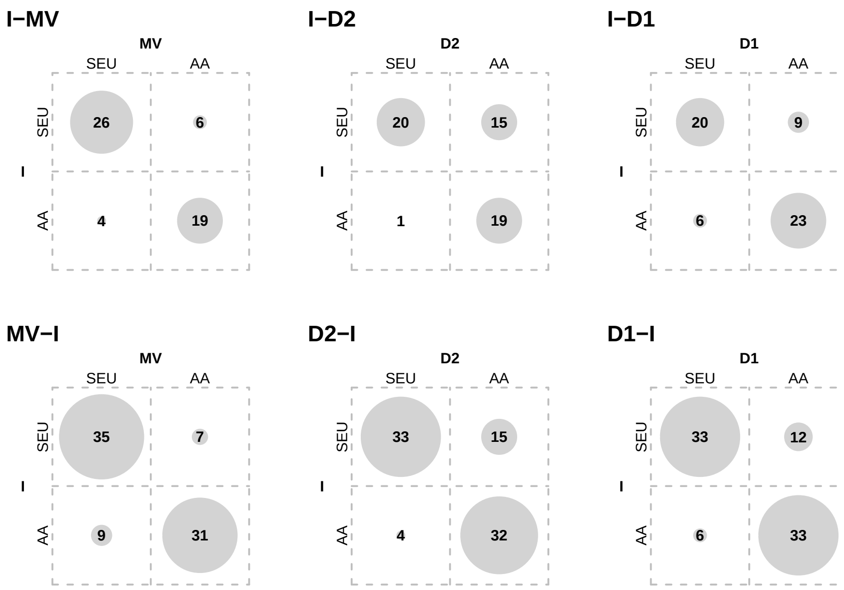

Fig. 2 Frequency counts of ambiguity averse (AA) and subjective expected utility (SEU) preferences. The diameter of the gray disk in each cell is proportional to the number of subjects in that cell

These results suggest that responsibility for the others' payoffs affects attitudes to ambiguity, although this effect is not monotonically increasing with the degree of responsibility. In particular, it appears that ambiguity aversion is enhanced just due to the presence of one group member who has no say in the collective decision.

Figure 2 reports a detailed within-subject classification of all transitive choices. It enables us to assess the potential presence of a shift in ambiguity aversion when we move from individual to collective choices and vice versa in a less aggregated form. The six panels correspond to our six treatments. Each panel classifies participants in the corresponding treatment as ambiguity averse (AA) or subjective expected utility maximizers (SEU). In panel/treatment I-MV, for instance, 26 subjects are consistently SEU in both experimental parts, 19 subjects are consistently AA, 4 subjects switch from being AA in Part 1 (condition I) to being SEU in Part 2 (condition MV), and 6 subjects switch from being SEU in Part 1 to being AA in Part 2. To facilitate visual comparisons, each cell reports a gray disk whose diameter is proportional to the number of subjects in that cell.

We can see that the great majority of observations lie on the main diagonal. That is, subjects reveal consistent ambiguity attitudes across both parts (the lowest level of consistency, 70.9%, is observed in treatment *I-D2*). However, while the number of subjects switching in the two directions is about the same in *I-MV* and *MV-I*, there is noticeably more switching in one direction than in the other in *I-D2* and *D2-I*. More specifically, in *I-D2*, 15 subjects with *SEU* preferences in Part 1 (condition *I*) become *AA* in Part 2 (condition *D2*), while only 1 subject changes from *AA* in Part 1 to *SEU* in Part 2. Conversely, in *D2-I*, subjects more often switch from *AA* in Part 1 to *SEU* in Part 2 than vice versa (15 vs. 4). The direction of the shifts in *I-D1* and *D1-I* resembles that observed in the treatments with *D2*, but the magnitudes are smaller. These observations corroborate the finding already derived from the aggregate data: the random delegate conditions *D1* and *D2* foster ambiguity aversion, as suggested by our initial hypothesis grounded in previous studies of fear of negative evaluation (Curley et al. 1986, Trautmann et al. 2008, and Muthukrishnan et al. 2009).

To gain further insights into the relationship between individual ambiguity aversion and the applicable collective decision rule, Table 3 reports the results of two random effect logit regressions with ambiguity averse choices as dependent variable. Model (1) is a basic specification, which includes only treatment dummies (*MV*, *D2*, and *D1*). It confirms the impression from Table 2 and our non-parametric analysis that the probability of ambiguity averse choices is significantly higher in conditions *D1* and *D2* than in condition *I*.

Model (2) adds controls for the participants' age ("age"), their gender (the "female" dummy equals 1 for females), the number of experiments they participated in (the "Exp$\geq$8" dummy takes value 1 if the subject participated in more than 7 experiments, where 7 was the median participation in previous experiments), the time the participants needed to make a choice ("chtime"), and their perception of the difficulty of this experiment compared to others they had taken part in (the "More difficult" and "Less difficult" dummies). The coefficients of *D1* and *D2* are still positive and significant, implying that the results of enhanced ambiguity aversion in the single- and dual-delegate conditions are robust to the inclusion of control variables. None of the control variables are significant at the 5% level; "female" and "chtime" are both significant at the 10% level. The coefficient of "female" is positive, meaning that females in our sample are more likely to select ambiguous prospects compared to males. The coefficient of "chtime" is negative, implying that the longer a

Table 3 Random effect logit regressions on ambiguity aversion choices

	Model (1) (n=418 × 2)			Model (2) (n=418 × 2)		
	Coef.	S.E.		Coef.	S.E.	
(Intercept)	-0.514	0.244		0.266	1.588	
MV	0.048	0.352		0.127	0.361	
D2	1.575	0.392	***	1.674	0.405	***
D1	0.732	0.348	*	0.754	0.352	*
age	—	—		−0.012	0.060	
female	—	—		0.823	0.465	°
Exp≥8	—	—		0.191	0.441	
chtime	—	—		−0.016	0.009	°
More difficult	—	—		−0.896	0.699	
Less difficult	—	—		−0.772	0.539	
mixing distr. par.	3.481	0.441	***	3.525	0.462	***
LogLik	−501.0			−495.3		

Significance codes: *** 0.001 ** 0.01 * 0.05 ° 0.10

subject takes to make a choice the lower the likelihood to show ambiguity averse preferences.

To verify whether decision making time differs across conditions, we ran a linear random effect model regressing the variable "chtime" on treatment dummies and on a dummy variable indicating that the choices were made in Part 2. The results of the regression fail to reject the hypothesis that the time spent to make a decision is the same across conditions. Controlling for age, gender, participation in previous experiments, and perceived difficulty of the task does not qualitatively change this result.

4 Concluding remarks

Our study contributes to the small existing literature on collective choices under ambiguity by studying how individuals react to different collective decision rules that assign different responsibility for the others' payoffs. Our data confirm that ambiguity aversion is a significant issue not only in individual but also in collective decision making environments. As a matter of fact, we detected high proportions of ambiguity averse choices both in our individual condition (on average 43%) and in our three group conditions, where ambiguity averse choices range from 46% in the *MV* condition of treatment *MV-I* to 60% in the *D2* condition of treatment *I-D2*.

Comparing choices in the individual and group conditions reveals that (*a*) between 70% and 82% of the subjects do not exhibit differences in their individual and collective ambiguity attitudes, and (*b*) those with differences are predominantly *more ambiguity averse* when they bear full or partial responsibility for the others' payoffs. The latter result supports our initial hypothesis that collective decision rules requiring the decision maker to impose one or all of his choices on the others foster ambiguity aversion. Based on the few papers who studied individual attitudes to ambiguity in a group environment without opportunities for communication and coordination (Curley et al. 1986, Trautmann et al. 2008, and Muthukrishnan et al. 2009), we speculated that fear of implicit blame would encourage group members whose choice could be imposed on the others to play it 'safer' and to go for a known chance of winning €20.

We do not observe a change in individual ambiguity attitudes for every group condition. In line with Brunette et al.'s (2015) experiment, we find that preferences under majority rule (*MV*) do not differ significantly from those under individual choice. Additionally, rule differences between the single (*D1*) and dual (*D2*) delegate conditions do not generally translate into significant differences in ambiguity attitudes. The mere presence of a passive member of the group, who has no influence on the final outcome, may increase the salience of being judged for making a 'wrong' decision and, as a result, increase the fear of implicit blame.

Our result of enhanced ambiguity aversion in the random delegate conditions does not parallel the findings of Pahlke et al. (2012) and Andersson et al. (2016) on loss aversion. Both of these studies detected a decrease in loss aversion when subjects made decisions for others. The most likely explanation suggested by the authors is that loss aversion is a type of emotional bias that may be mitigated by being responsible for someone else's payoff. The fact that we do not observe such a decrease in the delegate conditions may mean that either ambiguity aversion and loss aversion are driven by different psychological mechanisms or, if ambiguity aversion—like loss aversion—is perceived as an emotional bias in the group conditions, the resulting decrease in the bias is completely offset by the increase in the fear of implicit blame.

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