Ballot Richness and Information Aggregation *

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September 18, 2026

Abstract

When voters differ in information quality, voting rules with richer ballot spaces can improve information aggregation by endogenously allocating more decision power to better-informed members. Using laboratory experiments, we compare two polar examples of voting rules in terms of ballot richness: majority voting (MV) and continuous voting (CV). CV outperforms MV on average, although the difference is smaller than predicted, and receives more support than MV in treatments where it is expected to perform better. Behaviour under CV is more dispersed than predicted, and voters with intermediate information assign more weight to their votes than their information warrants.

*We thank the editor and two anonymous referees for comments that substantially improved the paper. We also greatly benefited from comments and suggestions of Marina Agranov, Pazit Ben-Nun Bloom, Micael Castanheira, Guillaume Frechette, Alice Hallman, Tom Palfrey, Pellumb Reshidi, Santi Sanchez-Pages, Ro'i Zultan, and seminar participants at Aix-Marseille Université, the Berlin Behavioural Seminar, the Berlin Microeconomics Seminar, GATE Lyon, Universidad de Alicante, Universidad Carlos III de Madrid, Universidad de Malaga, Universitat Autonoma de Barcelona, Universität Hamburg, University of Bath, University of Haifa, University of Leicester, University of Pittsburgh, the Zurich Micro Theory Seminar, the ESA meetings in Bologna and Santa Barbara, the LSE Behavioural Political Economy Workshop, the PEPE workshop and the PETME Workshop. This project has received funding from the European Research Council (Laurent Bouton) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 637662) and from the Agence Nationale de la Recherche (Antonin Macé, grant ANR-17-EURE-001).

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1 Introduction

Voting is a widespread practice across our society: citizens vote to elect their representatives, shareholders to decide on managerial proposals, or countries to determine the international organizations' resolutions. In many of these institutions, voters are bound to either cast their vote to fully support one of the available options or abstain entirely. The dichotomous nature of the vote constrains the comprehensive expression of voter preferences and knowledge, leading to associated welfare costs. In the context of preference aggregation, it can lead to the so-called tyranny of the majority (i.e., a majority of voters impose a choice they care little about on a minority who cares intensely), while in the realm of information transmission, it results in information loss, and hence suboptimal decisions.

These issues have prompted the exploration of innovative voting systems designed to expand the range of choices available on the ballot. These include methods such as approval voting (Brams and Fishburn, 1978), storable votes (Casella, 2005), qualitative voting (Hortala-Vallve, 2012), and quadratic voting (Lalley and Weyl, 2018), among others. This literature has established that, in theory, broadening the options within the ballot space can effectively address certain inefficiencies associated with dichotomous voting.[1]

In this paper, we empirically examine the impact of ballot richness on collective decision-making, with a particular focus on information aggregation. In this context, traditional rules utilizing dichotomous votes, such as majority rule systems, exhibit two primary limitations: (i) they assign equal weight to all participating members, regardless of the quality of their information and (ii) entirely disregard the information of those who abstain.[2] On the opposite end of the spectrum in terms of ballot richness, is the system we call continuous voting (CV hereafter), where voters have the flexibility to endogenously allocate weights to their votes. Under such a mechanism, voters can adjust the weight they assign to their votes based on the quality of their information, placing more weight on better information. In equilibrium, voters have incentives to select optimal weights that implement efficient decisions for any given information structure.[3] The question is whether the desirable properties of this additional flexibility might be overturned by the additional complexity of the rule and the coordination issues due to the great multiplicity of efficient equilibria.[4]

[1] This body of literature has primarily focused on preference aggregation. Exceptions can be found in Ahn and Oliveros (2016) and Bouton et al. (2017).

[2] Abstention can arise in equilibrium as shown in Feddersen and Pesendorfer (1996). Such behaviour has been observed in the laboratory (see, e.g., Battaglini et al. (2010) and Herrera et al. (2019a)).

[3] Núñez and Laslier (2014) show that, in the case of private values, allocating a discrete amount of votes does not change the equilibrium outcomes. This implies that allowing voters to endogenously choose the weights assigned to their votes might lead to a Pareto improvement.

[4] Esponda and Vespa (2014) and Esponda and Vespa (2024) show that most subjects exhibit failures of contingent thinking even in simple voting decision problems.

We present the findings of a laboratory experiment in which groups decide with CV or with simple majority. First, we assess whether, as predicted by the theoretical model, CV yields superior welfare compared to majority rule. Second, we examine the voting behaviour under CV. A distinctive theoretical feature of this voting method lies in its robustness to variations in the number of voters, the introduction of new participant types, or changes in information technology. We designed experimental treatments to test for this robustness in the data. Third, we explore the impact of communication on behaviour and welfare. According to theoretical predictions, communication can mitigate welfare differences between systems. To test this prediction, we have treatments where subjects can communicate before voting. Finally, we investigate participants' preferences over these voting methods. In the absence of communication, they should prefer CV, and should be indifferent when communication is allowed.

Our key findings are as follows. First, in the absence of communication, CV leads to higher welfare than simple majority although the difference is not always significant and the magnitude of the difference is smaller than predicted. When communication is introduced, we do not observe significant differences in outcomes between the two voting rules. Second, under CV, subjects frequently use *partial abstention*–assigning higher weights to their vote when they possess better information. As predicted by the theory, voting behaviour is quite similar across treatments varying the number of voters, the introduction of new participant types, and changes in information technology. A notable deviation from our predictions is that participants with intermediate information assign disproportionately high weights compared to the most informed subjects. Third, in treatments without communication, subjects generally favor CV, with the extent of the support correlating with the difference in payoffs between the two mechanisms experienced in prior parts of the experiment. When communication is introduced, the preference overwhelmingly shifts toward simple majority.

Our paper contributes to the literature on information aggregation in binary elections.[5] Our main contribution with respect to this literature is to allow for divisible votes. By extending the ballot richness, we allow for the mechanism to fully aggregate information, even in small electorates. Within this broader literature, our study is particularly related to research on strategic abstention. A well-established finding in this area is that less informed voters abstain from voting in equilibrium (Feddersen and Pesendorfer, 1996; McMurray, 2013; Herrera et al., 2019b). Such behaviour has also been observed in the experimental data (Battaglini et al., 2010; Morton and Tyran, 2011; Mengel and Rivas, 2017; Herrera

[5]See, e.g., Austen-Smith and Banks (1996); Feddersen and Pesendorfer (1997, 1998); Myerson (1998); Bhattacharya (2013); Bouton et al. (2018); Barelli et al. (2019). Various predictions of these theories have been tested using laboratory experiments (see, for example, Ladha et al. (1996); Guarnaschelli et al. (2000); Bhattacharya et al. (2014); Bouton et al. (2017)).

et al., 2019a). Our experiment shows that when voters have access to a richer ballot space, full abstention declines significantly. Instead, voters express their views while making use of partial abstention, allowing them to assign greater voting power to better-informed participants without completely silencing the less informed ones.

We use continuous voting as the polar opposite to majority voting in terms of ballot richness.[6] Continuous voting is the binary-alternative specification of (continuous) range voting. Under range voting, voters assign to each alternative a score from an interval and scores are added; with two alternatives, only the difference between the two scores affects the outcome, yielding an action in an interval. Range voting has been advocated by Hillinger (2005) and studied axiomatically by Pivato (2014) and Macé (2018). The related theoretical contributions play distinct roles. Nitzan and Paroush (1982) derive information-optimal weights when a planner assigns voting power. Chakraborty and Ghosh (2003) show how divisible votes can endogenously implement efficient information weighting. Bouton et al. (2024) establish the broader ballot-richness result: expanding the feasible ballot weakly improves the best equilibrium, while the continuous ballot admits full-information-efficient equilibria. Our experiment asks whether this theoretical advantage survives the calibration and coordination demands faced by actual voters.

In practice, a few public elections do employ rich ballot spaces, where voters can indicate to some extent the intensity of their support for different candidates. Current examples include parliamentary elections in Latvia, Luxembourg and Switzerland (Renwick and Pilet, 2016), or some local elections in Germany (Bowler et al., 2018). Even a fully flexible system, equivalent to CV, was used in ancient Sparta to appoint new members to the Council of Elders, where the intensity of support was measured by the noise generated by the shouting electorate (Girard, 2010). Beyond public elections, a close real-world analogue of CV exists in corporate governance under proxy voting where mutual funds vote for multiple stakeholders and are not obliged to cast all the votes that they represent in the same direction. That is, they can engage in "split voting" which, in two-alternative elections, is equivalent to partial abstention. For instance, the voting-policy manual of BlackRock specifically "contemplates split voting, where BlackRock would vote differently for different funds" (see, Curtis et al. 2021). Of course, one-share-one-vote typically features asymmetric vote endowments, so it is not a literal implementation of CV with equal budgets, but it arguably captures the additional flexibility of not having to choose between exerting all your influence towards a

[6] With two alternatives and abstention, majority voting restricts each voter to the three actions $\{-1, 0, 1\}$; continuous voting convexifies this set to the full interval $[-1, 1]$. The comparison therefore changes the divisibility of voting power while holding fixed the alternatives, equal vote endowments, and additive aggregation. It isolates whether allowing voters to express how strongly their information points toward an alternative improves collective decisions once actual voters must calibrate and coordinate their weights.

certain outcome, or no influence at all.

A related way to concentrate decision power is vote delegation, as studied in Campbell et al. (2022) and Dhillon et al. (2026). Delegation can transfer an entire vote to a better-informed participant, but the delegator then ceases to transmit her own information. Continuous voting instead permits partial abstention: less-informed voters can reduce their influence without fully silencing their signals. The two mechanisms therefore share the ability of reallocating influence toward expertise but differ in whether information from less-informed participants is retained.

Our experiment also contributes to the growing literature on how groups select electoral rules to make decisions. Engelmann and Grüner (2017) examine the choice of voting thresholds for adopting reforms at the interim stage, where individual preferences are already realized. Their findings highlight that preferences are shaped by both self-interest and efficiency concerns.[7] Weber (2020) and Hoffmann and Renes (2022) analyse rule selection at both the ex-ante and interim stages, documenting significant divergences in preferences between these stages. Our study complements this literature in two ways. First, we focus on information aggregation in a common-value setting, where individual interests and efficiency objectives are aligned. This eliminates the tension between self-interest and group welfare, which in turn reduces the potential divergence in rule preferences across the ex-ante and interim stages. Second, we investigate individual preferences for voting mechanisms with differing levels of ballot richness, providing new insights into the implications of expanding voters' decision-making flexibility.

2 Theory

The primary goal of our theoretical analysis is to characterize the equilibria under Continuous Voting and compare their properties to those under Majority Voting. We adapt the framework from our companion paper Bouton et al. (2024) to reflect the experimental constraints, such as finite and symmetric signals. Proofs can be found in Appendix A.

2.1 Setup

A finite number of $n > 2$ voters must decide between two alternatives, A or B. Voters have common values and have uncertainty about which is the superior alternative. In particular, there are two possible states of the world, denoted by $\omega \in \{\alpha, \beta\}$. We denote the probability

[7] Engelmann et al. (2023) extend this analysis by investigating rule choices made under the veil of ignorance, where individual preferences are not yet known. See also Blais et al. (2015) and Bol et al. (2023) for related findings.

of state α by $\Pr(\alpha) \in (0,1)$. Voters' preferences are such that

$$u(A\,|\alpha) = u(B\,|\beta) = 1 \qquad (1)$$
$$u(B\,|\alpha) = u(A\,|\beta) = 0$$

where $u(x\,|\omega)$ denotes the utility if alternative x is chosen in state ω.

Information Structure. The state of the world cannot be observed directly, but each voter i receives an informative signal s_i. These signals are of heterogeneous quality, reflecting the fact that citizens differ in their expertise on the issue at hand. Specifically, each citizen is endowed with information quality $p_i \in P$, where P is a non-singleton finite set in $(\frac{1}{2}, 1)$. Precisions are drawn independently across players according to a common probability distribution F with support P. We denote the set of signals by $S = \{s^p_\omega \mid \omega \in \{\alpha, \beta\}, p \in P\}$. Conditional on the draw of p_i, the citizen draws her signal, either $s^{p_i}_\alpha$ or $s^{p_i}_\beta$, according to the following:

$$Pr(s_i = s^{p_i}_\omega \mid p_i, \omega) = p_i, \qquad Pr(s_i = s^{p_i}_{-\omega} \mid p_i, \omega) = 1 - p_i.$$

That is, each citizen knows her signal precision p_i, and receives a correct signal on the state with probability p_i. We denote by $t^p_\omega = \frac{\Pr(s^p_\omega|\alpha)}{\Pr(s^p_\omega|\beta)} = (\frac{p}{1-p})^{(\mathbb{1}(\omega=\alpha)-\mathbb{1}(\omega=\beta))}$ the type associated with signal s^p_ω. The type space is $T = \{\frac{p}{1-p} \mid p \in P\} \cup \{\frac{1-p}{p} \mid p \in P\}$ and we denote by t_i the type of voter i.

For any type profile $(t_1, \ldots, t_n)$, we say that a decision $d \in \{A, B\}$ is *optimal* if it maximizes voters' utility given $(t_1, \ldots, t_n)$. It follows that A is optimal if $Pr(\alpha \mid t_1, \ldots, t_n) \geq 1/2$ and that B is optimal if $Pr(\alpha \mid t_1, \ldots, t_n) \leq 1/2$. In order to make the problem interesting, we assume that the information structure is such that it is not always optimal to follow the prior. In other words, we assume that the maximum precision is high enough that there are realizations for which it is optimal for the group to go against the prior.[8]

The voting rules. We consider two voting rules. Under *Majority Voting* (MV, hereafter), agents must choose a vote $v_i \in V^{MV} = \{-1, 0, 1\}$, where $v_i = 1$ indicates a vote for A, $v_i = -1$ a vote for B, and $v_i = 0$ indicates abstention. Under *Continuous Voting* (CV, hereafter), each voter must choose a number $v_i \in V^{CV} = [-1, 1]$. Under both rules, A (B) is implemented if $\sum_i v_i > 0$ (< 0), with ties broken randomly.

Profiles and equilibrium. Each voter i's strategy is characterized by a mapping $\sigma_i : T \to \Delta(V)$, associating a (possibly random) vote from the ballot space V to each possible type. We say that a strategy profile $\sigma = (\sigma_i)_{1 \leq i \leq n}$ is *efficient* if it leads to the optimal decision for

[8] Formally, we assume that $\left(\frac{1-\max P}{\max P}\right)^n < \frac{Pr(\alpha)}{1-Pr(\alpha)} < \left(\frac{\max P}{1-\max P}\right)^n$.

any type realizations. Slightly abusing notation, σ is efficient if and only if

$$Pr(\alpha \mid t_1, \ldots, t_n) > \tfrac{1}{2} \quad \Rightarrow \quad \sum_{i=1}^{n} \sigma_i(t_i) > 0$$

$$Pr(\alpha \mid t_1, \ldots, t_n) < \tfrac{1}{2} \quad \Rightarrow \quad \sum_{i=1}^{n} \sigma_i(t_i) < 0.$$

Throughout, we focus on the notion of Bayes-Nash equilibrium (henceforth equilibrium). We say that an equilibrium σ is *non-trivial* if $\Pr_\sigma(A) \in (0,1)$. When the prior is even, it is often relevant to focus on *double-symmetric* equilibria, i.e. symmetric equilibria such that $\sigma(t) = -\sigma(1/t)$ for all t.

2.2 Equilibrium Analysis and Welfare

We start by a result underlining the efficiency of CV.

Proposition 1. *(Bouton et al., 2024) Under CV, when the prior is even, there are double-symmetric, efficient equilibria σ, such that:*

$$\sigma(t) = \kappa \log(t) \quad \textit{with} \quad \kappa \in \left(0, \left(\log\left(\tfrac{\max P}{1-\max P}\right)\right)^{-1}\right].$$

Under the equilibrium described in Proposition 1, voters pick optimal weights for information aggregation given their signal precisions, as described in the seminal result of Nitzan and Paroush (1982). As this strategy profile is efficient in a common value game, it is an equilibrium by the standard argument of McLennan (1998).[9]

Unlike typical findings in the literature on strategic participation, the equilibria described in Proposition 1 do not feature full abstention. All voters partially abstain unless they have maximum precision, and weights increase with signal precision. Notably, this equilibrium is robust to changes in the information structure: equilibrium strategies are independent of the precision distribution, the number of voters and the addition of new precisions to the initial set P.[10]

Proposition 1 highlights that there is a great multiplicity of efficient equilibria under CV. There is actually a continuum of such equilibria, for all the different values of κ. The efficiency of CV thus relies on the ability of voters to coordinate on the same efficient equilibrium strategy. As discussed in the Introduction, there is evidence that such coordination often proves quite challenging in practice.

[9]The proof of Proposition 1 is analogous to Proposition 2 in Bouton et al. (2024), and is therefore omitted.

[10]Provided that $\max P$ does not change.

There might also exist other efficient equilibria beyond those outlined in Proposition 1. For a small electorate, minor deviations from the described strategies can still constitute an equilibrium (for a fixed perturbation size ε, this happens if for any set of signal realizations, the posterior is sufficiently far from 1/2). Yet, there are at least two cases in which every symmetric efficient equilibrium must belong to the set characterised in Proposition 1: when the electoral size n becomes large or when (in an extended model) F has full support on $(\frac{1}{2}, \max P]$.

Finally, note that Proposition 1 only deals with the case of an even prior. Yet, the result extends to the case of an uneven prior: CV remains efficient, and the following symmetric strategy profiles constitute efficient equilibria: $\sigma(t) = \kappa\,(c + \log(t))$, where $c = \frac{1}{n}\log\left(\frac{\Pr(\alpha)}{1-\Pr(\alpha)}\right)$ and $\kappa \in \left(0, \left(\log\left(\frac{\max P}{1-\max P}\right) + |c|\right)^{-1}\right]$.

We now turn our attention to equilibria under MV. We say that a strategy $\sigma_i : T \to \Delta(\{-1, 0, 1\})$ is a *monotone cutoff strategy* if the support of $\sigma_i(t_i)$ is a (weakly) increasing correspondence of t_i and if $\sigma_i(t_i)$ is a pure action in $\{-1, 0, 1\}$ for all but at most two types (i.e. the cutoffs).

Proposition 2. *Under MV, a non-trivial equilibrium always exists. In any such equilibrium, each player employs a monotone cutoff strategy.*

Proposition 2 indicates that (non-trivial) equilibrium strategies under MV can be described by two cutoffs. Each voter votes for B (i.e. -1) when her type falls below the lowest cutoff, and for A (i.e. 1) if it falls above the highest cutoff, while she abstains if it falls in between the two cutoffs. Mixing is possible if a voter's type coincides with one of the two cutoffs.

We now focus on the comparisons of CV and MV under their best equilibria. We say that a strategy profile σ (strictly) dominates another one τ if the ex-ante probability of implementing the correct outcome (A in α and B in β) is (strictly) higher in σ than in τ.[11]

Proposition 3. *The best equilibrium under CV dominates the one under MV. When the prior is even, for any non-singleton finite set of signal precisions $P \subset (1/2, 1)$, there exists $n(P)$ such that if n is even or if $n \geq n(P)$, then the dominance is strict.*

The intuition for Proposition 3 is that extending the strategy space of players in a common value game can only allow voters to reach a better equilibrium.[12] More precisely, we know that (i) there are efficient equilibria under CV that are not feasible under MV since

[11] Note that this dominance relation is consistent with our efficiency notion since any efficient profile dominates any non-efficient one.

[12] This result is reminiscent of the finding of Ahn and Oliveros (2016) that Approval Voting dominates other voting rules in multi-candidate elections because of its richer ballot space.

they involve partial abstention, and that (ii) these efficient equilibria dominate any feasible strategy profile under MV, i.e., any profile for which each voter either votes "fully" in favor of one alternative, or abstains. The precise condition for (ii) to hold strictly is that the number of voters n is even, or higher than a constant $n(P)$, which is typically quite low. For instance, the condition always holds strictly in committees of at least 5 voters if the highest and lowest precisions differ by at least 0.1. The dominance of CV over MV is thus strict for all our experimental parameters, as described in Section 3 below.[13]

There are three caveats to this result. First, by expanding the strategy space (going from MV to CV), it could be that CV also produces worse equilibria than MV. However, in Bouton et al. (2024) we show that the worst equilibrium under CV is no worse than under MV. Second, the result remains silent on the relative complexity of reaching the most efficient equilibrium under any of the two rules. On the one hand, the efficient equilibrium of CV requires (at least approximately) fine-tuning one's weight as a function of the signal precision, while the most efficient equilibrium under MV simply consists of abstaining below a certain level of precision . On the other hand, when the environment (parameters of the game) varies, equilibrium strategies remain the same under CV, while the abstention cutoffs may be complex to determine under MV. Finally, there is a great multiplicity of efficient equilibria under CV, and voters need to coordinate on one of them in order for CV to feature its desirable properties. By contrast, coordination issues seem less prevalent under MV. For example, for the experimental parameters, there is a unique pure-strategy double-symmetric payoff dominant equilibrium under MV. Testing the two rules in the lab allows us to study how players address these challenges.

Finally, we consider an extended game where the voting stage is preceded by a communication stage. This communication stage is assumed to be a simultaneous, cheap talk game. For simplicity, we assume that the set of messages that each player can send coincides with the set of signals.

Proposition 4. *Under communication, there are efficient equilibria under both voting rules.*

The intuition for Proposition 4 stems from the simple logic described in Gerardi and Yariv (2007): under common values, there is an equilibrium of the communication game where all voters truthfully report their signal and then they all vote for the alternative most likely to be correct given the set of signal realizations. Note that there exists a multiplicity of efficient equilibria and that individual votes are not pinned down by the requirement to play an efficient equilibrium.

[13]The strict dominance result in Proposition 3 is stated for an even prior. Yet, as we show in Appendix A, CV also strictly dominates MV for our Asymmetric Prior treatment.

3 The Experiment

3.1 Experimental Design

We designed various treatments to test the theoretical predictions from Section 2. In the beginning of the experiment, subjects were randomly divided into groups of 5 or 9, and these groups were fixed for the rest of the experiment. Except for the voting rule, the set of parameters, which we denote as the *environment*, was fixed throughout the session. The experiment consisted of *three parts*. In the first two parts, groups had to vote for 20 rounds using either MV or CV, with different rules in the two parts.[14] At the beginning of part three, each group had to decide which rule to use. Each subject chose one of the rules, and one of the decisions was randomly picked and implemented (i.e. we implemented random dictatorship) for 10 additional periods (with the same parameters as in parts 1 and 2).

At the beginning of each round, the colour of a triangle was chosen randomly to be either blue or red with probabilities $\Pr(\alpha)$ and $1 - \Pr(\alpha)$ respectively. Subjects were not told the colour of the triangle, but were told that their goal would be to work together as a group to guess the colour of the triangle. Independently, each subject would observe one ball (a *signal*) drawn randomly from an urn with 100 blue and red balls. The exact composition would depend on their *precision*. The precision of each voter was private information and randomly drawn in each round from a commonly known distribution. After observing their signals, each subject had to vote. The group decision was the colour that received more votes, with ties broken randomly. Subjects' payoff was 100 points if the colour chosen by the group was the same as the colour of the triangle and 0 points otherwise.

			Probability of Each Precision				
Environment	**n**	**Prior**	55%	60%	75%	95%	**Chat**
Baseline	5	0.5	0.15	0.70	–	0.15	–
Distribution (D)	5	0.5	<u>0.25</u>	<u>0.50</u>	–	<u>0.25</u>	–
Size (S)	<u>9</u>	0.5	0.15	0.70	–	0.15	–
More Types (MT)	5	0.5	0.15	<u>0.50</u>	<u>0.20</u>	0.15	–
Asymmetric Prior (A)	5	<u>0.3</u>	0.15	0.70	–	0.15	–
Communication (C)	5	0.5	0.15	0.70	–	0.15	<u>Yes</u>

Table 1: List of all environments and their corresponding parameters. n denotes the group size. Prior corresponds to the ex-ante probability of state α. Columns 55%–95% indicate the probability associated with each signal precision. *Chat* specifies whether participants were allowed to communicate via chat before voting. Underlined parameters highlight deviations from the baseline environment.

The environments implemented in the experiment are summarized in Table 1. In the envi-

[14] We balanced the two sequences: half of the groups use MV in part 1 and CV in part 2, while the remaining half followed the opposite order.

ronment called *Baseline*, $n = 5$, $\Pr(\alpha) = 0.5$, the set of different precisions was $\{55\%, 60\%, 95\%\}$, and the likelihood of each precision was 0.15, 0.7 and 0.15 respectively in both states (symmetric signal structure). All other environments are variations of this one. Under *Distribution* (D), we change the likelihood of the precisions to 0.25, 0.5 and 0.25 respectively. Under *Size* (S), we change $n = 9$. Under More Types (MT), we increase the set of different precisions to $\{55\%, 60\%, 75\%, 95\%\}$, and the likelihood of each precision was 0.15, 0.5, 0.2 and 0.15 respectively. Under *Asymmetric Priors* (A), we change the prior to $\Pr(\alpha) = 0.3$. Under *Communication* (C), we allow subjects to freely communicate before they vote. Table 2 summarizes the payoff dominant equilibria and the respective payoffs under both mechanisms.[15]

		Equilibrium MV				**Equilibrium CV**				**Payoffs**	
Parameters	**Colour**	55%	60%	75%	95%	55%	60%	75%	95%	π^{MV}	π^{CV}
Baseline	Both	20	20	–	20	1.36	2.75	–	20	76	83
Distrib (D)	Both	0	0	–	20	1.36	2.75	–	20	85	89
Size (S)	Both	0	0	–	20	1.36	2.75	–	20	85	91
More Types (MT)	Both	0	0	20	20	1.36	2.75	7.46	20	82	87
Asym Prior (A)	Blue	0	0	–	20	0.20	1.52	–	17.82	79	86
	Red	0	20	–	20	2.38	3.69	–	20		

Table 2: Equilibrium actions and expected payoffs under Majority (MV) and Continuous Voting (CV) for each ball colour. For comparability, under MV we assign 20 points to a vote and 0 points to abstention. For MV, we report the payoff-dominant equilibrium. For CV, we report the payoff-dominant equilibrium described in Proposition 1, in which the highest-weight voter assigns the maximum possible weight.

The specific parameters underlying these environments were chosen through a systematic grid. We considered signal precisions drawn from $\{0.55, 0.60, \ldots, 0.95\}$, with probabilities restricted to multiples of 5%, and searched for a Baseline combination satisfying three requirements: an economically meaningful welfare gap between CV and MV, a small enough number of subjects per group to be feasible on a laboratory budget, and a small enough number of precisions so that the informational environment is tractable for participants. The resulting Baseline combines weak (55%), intermediate (60%), and highly informative (95%) signals, with the intermediate type most frequent, producing substantially different efficient CV weights while keeping the task transparent; ex post, this mix also crystallizes the central tradeoff under majority voting, where low-precision voters risk either overturning a better-informed voter's signal by voting, or discarding their own information and risking a draw by abstaining. Starting from this Baseline, *Distribution*, *Size*, and *More Types* each perturb one primitive in turn to test the invariance of CV's equilibrium weights established in Proposition 1, against which the payoff-dominant MV cutoff, by contrast, does change.

[15]There is multiplicity of equilibria under majority. Table D1 in the appendix summarizes all the pure-strategy double-symmetric equilibria under majority in symmetric treatments.

Asymmetric Prior instead tests a complementary prediction — that voters should adjust their weights to compensate for a more likely state — while *Communication* removes CV's theoretical advantage altogether, since truthful information sharing makes efficient decisions attainable under both rules. Taken together, the treatments span environments in which the equilibrium welfare advantage of CV over MV ranges from strictly positive to zero, as, within our common-value framework, the best equilibrium under MV can never outperform the best equilibrium under CV.

3.2 Experimental Procedures

Experiments were conducted at the Experimental Economics Laboratory at the University of Valencia (LINEEX) between December 2017 and February 2018. We had 12 independent groups for each treatment, making a total of 408 participants. Students interacted through computer terminals, and the experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)). All experimental sessions were organized along the same procedure: subjects received detailed written instructions (see Appendix G), which an instructor read aloud. Before starting the experiment, students were asked to answer a questionnaire to check their full understanding of the experimental design. At the end of the experiment, the computer randomly selected 2 rounds from part 1, two from part two and one from part 3, and participants earned the total of the amount earned in these rounds. Points were converted to euros at the rate of 0.025€per point. In total, subjects earned an average of 14.54€, including a show-up fee of 5€. Each experimental session lasted approximately an hour.

3.3 Hypotheses

We draw a number of theoretical hypotheses from the equilibrium analysis in section 2 that we outline below. The first hypothesis focuses on welfare comparison across systems, which follows from Proposition 3 and Proposition 4. The next two hypotheses focus on voter behaviour in symmetric treatments without communication. They are direct corollaries of Proposition 1 and Table 2. The last hypothesis focuses on the Asymmetric treatment. It follows from an extension of Proposition 1 to asymmetric priors and from Table 2.

Hypothesis 1. *Welfare is strictly higher under CV than MV without communication, but no welfare differences exist under communication.*

Hypothesis 2. *In symmetric treatments without communication, weights increase with signal precision (strictly under CV).*

Hypothesis 3. *In symmetric treatments without communication, behaviour under CV is independent of the environment.*

Hypothesis 4. *In the Asymmetric Prior environment, voters using CV assign higher weights to the outcome that is more likely based on the prior.*

4 Experimental Results

Our analysis focuses on data from the first two parts of the experiment to avoid selection issues. Unless otherwise specified, comparisons between the two voting mechanisms are within-subjects, since every group of participants used both systems (in Parts 1 and 2 of the experiment, with the order balanced across groups); comparisons across environments, by contrast, are between-subjects, since each group was assigned to a single environment. In all regression analyses, we account for correlation within matching groups by clustering standard errors at the group level. Welfare regressions use the group as the unit of observation, while analyses of behaviour and mechanism choice are conducted at the individual level. Unless stated otherwise, statistical significance is assessed at the 10% level.

4.1 Welfare

We examine the welfare properties of CV and MV by analysing two metrics: the percentage of optimal decisions (as defined in Section 2.1) and the average payoffs, summarized in Table 3.

Theoretically, CV should fully aggregate information, allowing voters to replicate the decisions of a benevolent dictator with access to all the signals. In contrast, MV is optimal only when communication is allowed. The upper part of Table 3 presents the frequency of reaching the optimal decision under each mechanism and environment. The table shows three clear patterns. First, the frequency of adopting the optimal decision is consistently lower than the theoretical predictions. The deviation is substantial: under CV, the difference from the theoretical prediction is 17.67%, while for majority, it is 11.03%. Second, consistent with theoretical predictions, CV yields a higher frequency of optimal decisions than MV in all treatments without communication, except for the *Prior* environment.[16] When pooling all treatments without communication, this difference is statistically significant ($p = 0.064$),

[16]The probability of making the optimal decision aggregates mistakes of very different natures. Appendix E disaggregates optimal decisions by group posterior and demonstrates that this pattern holds consistently across different group posterior beliefs.

providing support for Hypothesis 1.[17,18] No significant differences between mechanisms are observed when communication is allowed. Third, communication significantly increases the probability of making the optimal decision under *both* voting mechanisms ($p < 0.01$). While this is in line with the prediction under MV, it is not with CV. The magnitude of the effect of communication is also economically meaningful: it increases the probability of making the optimal decision by more than 10%.

We reach similar conclusions when we use the complementary measure of welfare of voters' average payoffs, summarised in the lower part of Table 3. This measure aggregates mistakes weighted by their cost. Consistent with the previous results, average payoffs are strictly higher under CV than under MV in all treatments without communication. When pooling all these treatments, the difference is statistically significant ($p = 0.053$).[19] The

[17]Comparisons between mechanisms in this section are based on linear regressions of the relevant outcome (an indicator for whether the group reached the optimal decision, or the group's payoff) on the full set of environment-mechanism indicator variables (omitting the constant), with standard errors clustered at the group level. The coefficients from this regression are simply the average value of the outcome in each environment-mechanism cell (the "Data" columns of Table 3); tests reported in the rest of the section are Wald tests based on this regression. The pooled comparisons re-estimate the same regression restricting the sample to the five environments without communication and testing equality of the (pooled) CV and MV coefficients. Based on the standard deviation of the group-level CV–MV differences (15.7 payoff points; 15.4 percentage points for optimal decisions), this pooled comparison has 82% power at the 10% level to detect the theoretically predicted payoff difference of 5.21 points, and essentially full power to detect the predicted difference of 11.5 percentage points in the frequency of optimal decisions; the minimum detectable effect at 80% power is about 5 points on either measure. Because the design is balanced across environments and mechanisms, adding environment fixed effects to this pooled regression leaves the CV–MV point estimate unchanged by construction; the associated p-values are also unchanged ($p = 0.064$ for decisions, $p = 0.053$ for payoffs). A joint (Chow-type) test of complete poolability — equality of both the environment intercepts and the mechanism effect across all five environments — cannot be rejected for either measure ($F(8, 59) = 1.14$, $p = 0.349$ for decisions; $F(8, 59) = 0.13$, $p = 0.998$ for payoffs).

[18]A joint test of equality of the five environment-specific CV–MV differences reported in Table 3 cannot reject homogeneity ($F(4, 59) = 0.31$, $p = 0.872$), so we do not find evidence that this difference varies systematically across environments. When analysing environments independently, the difference is statistically significant only in the *More Types* environment. As a robustness check on inference with a limited number of clusters, we recompute both p-values using a wild cluster bootstrap-t (Cameron et al., 2008) (clustering and resampling by matching group, null imposed, Rademacher weights; Stata's `boottest`): the pooled difference remains significant at the 10% level used throughout the paper ($p = 0.067$, vs. $p = 0.064$ above), while the *More Types* difference does not survive this more conservative test ($p = 0.158$). Estimating the same environment-by-mechanism comparisons with a probit model instead of a linear probability model leads to the same conclusions: the *More Types* difference again falls just short of the 10% level ($p = 0.104$, vs. $p = 0.097$ in the linear model), while no other conclusion changes. A probit with environment fixed effects and a common mechanism effect, which is not saturated and so is not mechanically tied to the linear estimates, gives a mechanism coefficient significant at the 10% level ($z = 1.89$, $p = 0.059$) and an average marginal effect of 3.78 percentage points ($p = 0.057$), close to the linear estimate of 3.75; none of the environment fixed effects is individually significant ($p > 0.34$).

[19]As with the optimal-decision measure, a joint test of equality of the five environment-specific CV–MV differences cannot reject homogeneity ($F(4, 59) = 0.13$, $p = 0.971$). When analysing treatments separately, the difference is statistically significant only in the *Size* environment. Under the wild cluster bootstrap-t procedure, the pooled payoff difference remains significant ($p = 0.054$, vs. $p = 0.053$ above), while the *Size* environment difference does not survive this stricter test ($p = 0.113$).

		CV		MV			
	Environment	Data	Eq.	Data	Eq.	Diff.	p-value
Percentage	All without comm.	80.58	100.00	76.83	88.46	3.75	0.064
Optimal	Baseline	80.42	100.00	77.08	91.25	3.33	0.278
Decisions	Distribution	82.92	100.00	77.92	85.21	5.00	0.292
	Size	80.00	100.00	75.00	87.29	5.00	0.251
	More Types	84.17	100.00	77.92	87.71	6.25	0.097
	Prior	75.42	100.00	76.25	90.83	-0.83	0.883
	Communication	92.92	100.00	92.92	100.00	0.00	1.000
Payoffs	All without comm.	76.83	85.58	72.83	80.38	4.00	0.053
	Baseline	76.25	83.33	72.92	76.25	3.33	0.444
	Distribution	75.42	88.33	73.33	83.13	2.08	0.705
	Size	78.75	87.08	72.92	85.63	5.83	0.091
	More Types	77.50	88.33	72.08	79.38	5.42	0.224
	Prior	76.25	80.83	72.92	77.50	3.33	0.476
	Communication	80.42	83.33	81.67	83.75	-1.25	0.669

Table 3: Welfare measures under continuous voting (CV) and majority voting (MV) across environments. Rows labelled 'All without comm.' pool all treatments without communication. *Data* reports observed outcomes, while *Eq.* reports equilibrium predictions evaluated at the realised values. *Diff.* reports the difference between CV and MV. *p-values* reports the p-value from a Wald test of equality between the CV and MV coefficients.

magnitude of the effect is slightly smaller than predicted: the theoretical payoff difference across mechanisms in non-communication environments is 5.21, while the observed difference is 4.00.[20]

In order to investigate further the differences in payoffs across mechanisms, and to separate the effect of voting behaviour from that of the particular signal realisations drawn in the experiment, we simulated 1000 experimental sessions for each independent group based on observed behaviour for each group.[21] Figure 1 displays the cumulative distribution function of the average payoffs in these sessions as well as the average payoff in these simulations. Except for the *Distrib* environment, the distribution of payoffs under CV first-order stochastically dominates the one for MV (or is very close to it).[22] As a result, the average payoffs

[20]Both welfare measures show an increasing trend over the periods played in the first two parts of the experiment. This improvement is common to both mechanisms rather than specific to CV: adding to the pooled regression a dummy for Part 2 and its interaction with the mechanism indicator yields a small and statistically insignificant interaction for both payoffs ($\beta = 2.33$, $p = 0.51$) and optimal decisions ($\beta = 0.17$, $p = 0.96$), so we cannot reject that the CV–MV difference is the same in Parts 1 and 2. The improvement over time itself is significant for optimal decisions (7.0 percentage points between Parts 1 and 2, $p = 0.01$) and, while smaller and not statistically significant, points in the same direction for payoffs ($p = 0.18$). Our results also remain robust when controlling for a common linear time trend across periods.

[21]In total, we conducted 12,000 simulated sessions for each environment-mechanism combination. Each session comprised 20 independent simulations to reflect the structure of an actual experimental session. For each simulation, a state was randomly selected, signals were randomly assigned to participants, and voting followed the distribution of votes observed in the corresponding group.

[22]The distribution of payoffs under CV first-order stochastically strictly dominates the one under majority for environments *Baseline*, *Prior* and *Size*. The same happens for *More Types* except for the two lowest

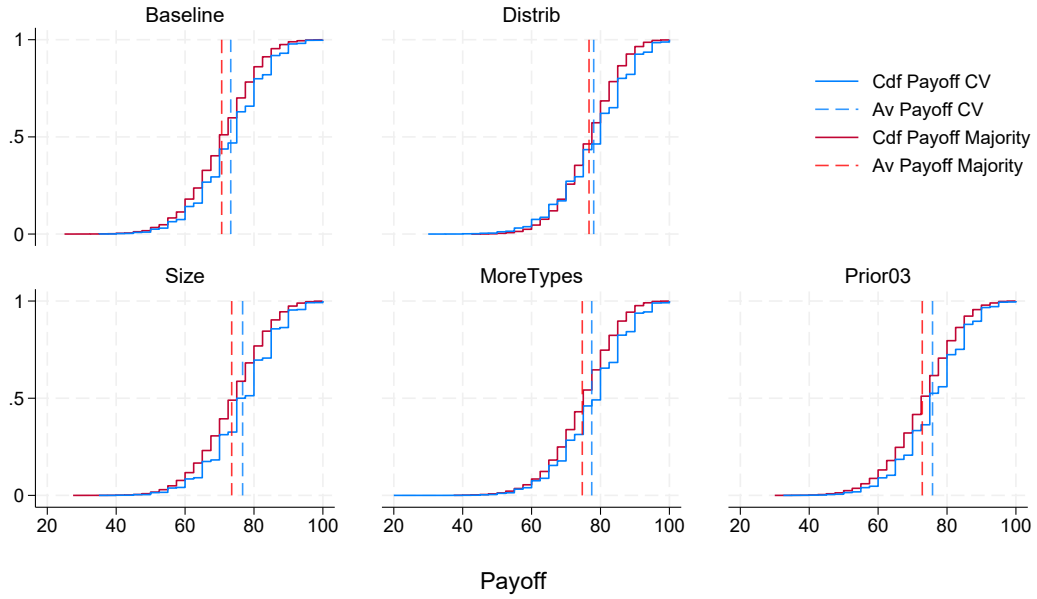


Figure 1: Cumulative distributions of (average) payoffs from 1,000 simulated experimental sessions, each consisting of 20 independent simulations based on observed behaviour (see details in footnote 21). Vertical dashed lines indicate the average payoffs for each environment and mechanism.

under CV are strictly higher than average payoffs under MV.[23] However, the magnitude of the difference is not large. Finally, in line with the theoretical predictions, we find no significant payoff differences across mechanisms in the presence of communication.

Two main factors explain the relatively small payoff differences across mechanisms whenever there is no communication. First, deviations from theoretical predictions in terms of voting behaviour (see next Section) reduce the expected payoff difference. In the simulations, which average over signal realisations, the payoff difference is 5.40 when voters follow equilibrium behaviour but only 2.56 when they follow observed behaviour, a reduction of about a half. The latter is smaller than the difference of 4.00 observed in the data; the gap between the two reflects the particular signal draws of the experiment, which the simulations average out. We therefore regard 2.56 as the more conservative estimate of the effect

payoffs out of 12000. For *Distrib*, this happens for payoffs higher than 70.

[23]The simulations do not add independent observations: each of the 60 groups remains one cluster. What they do is remove the noise that the 20 particular state and signal draws faced by a group introduce into its realised outcomes, while holding the group's observed voting behaviour fixed. As a consequence, the standard deviation of the group-level CV–MV difference falls from 15.7 payoff points in the data to 4.1 in the simulations, and a paired test over the same 60 groups yields $t = 4.78$ ($p < 0.001$; Wilcoxon signed-rank $p < 0.001$; CV exceeds MV in 48 of the 60 groups) for the simulated payoff difference of 2.56, and $t = 4.82$ ($p < 0.001$) for the simulated difference of 3.37 percentage points in the frequency of optimal decisions. A joint test of equality of the simulated differences across the five environments does not reject homogeneity ($F(4, 55) = 0.38$, $p = 0.82$ for payoffs; $F(4, 55) = 0.14$, $p = 0.97$ for optimal decisions).

of ballot richness given observed behaviour, and 4.00 as its realised counterpart. Second, the study focuses on symmetric signal distributions, where signals occur with the same collection of probabilities in the two states, and for which MV performs relatively well. Allowing for highly skewed signal distributions could significantly widen the payoff gap between the mechanisms. For example, using the parameters from Bouton et al. (2017) would yield a predicted payoff difference of 17.9 (treatment M2),[24] roughly two and a half times the largest predicted payoff difference across our environments.

Although this design choice is potentially conservative, we deliberately did not introduce an asymmetric signal structure. Under such a structure, subjects would have to understand that the probability with which a particular signal is generated may depend on the direction of the evidence, so that signals favouring different states need not have symmetric informational content. This would add a further layer of inference to an already unfamiliar voting task. It would therefore become difficult to distinguish mistakes arising from the voting rule from mistakes arising from subjects' understanding of the signal-generating process. We instead retained symmetric signal structures in order to isolate more cleanly the effect of ballot richness.

4.2 Voting Behaviour

This section begins by examining voting behaviour in the symmetric treatments without communication. We then turn to the treatments with an asymmetric prior and communication in subsections 4.2.1 and 4.2.2. For clarity, data from blue and red signals are pooled.[25]

For CV to strictly outperform MV, it is essential that voters make use of partial abstention. In our experiment, subjects opt for partial abstention 73.94% of the time under CV. Both full abstention and full vote are significantly lower than under MV: 4.49% vs 24.25% and 21.56% vs 75.75% respectively.[26] Figure 2 highlights how voting behaviour varies with information quality. Under both systems, full abstention decreases significantly as informa-

[24] In that treatment, the prior is even and there are two signals a and b such that $Pr(a \mid \alpha) = 0.99$ and $Pr(b \mid \beta) = 0.3$, yielding the asymmetric type space $T = \{1/30, 99/70\}$. Here a signal b is much more informative about the state than a signal a.

[25] Table D2 in the appendix displays the average weights under both voting systems for each environment, signal precision, and signal realization. It also includes tests of differences across signal realizations, based on linear regressions. We find no significant differences in 20 out of 26 comparisons.

[26] This pattern is consistent across environments and precisions. In the aggregate, the p-value< 0.01 for all environments. When looking at each environment and precision separately, all p-values< 0.1 except for the precision of 95% in the environment Distribution.

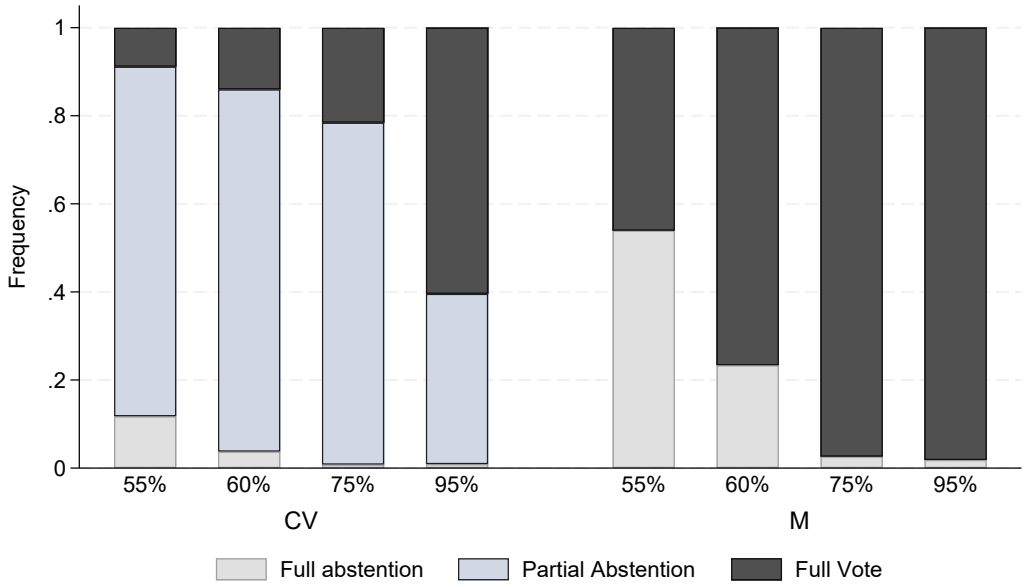


Figure 2: Basic aggregate behaviour by voting rule and signal precision. Each stacked bar shows the composition of votes by type: full abstention (bottom, grey) corresponds to a weight of 0; partial abstention (middle, blue) indicates a weight with absolute value strictly between 0 and 20; and full vote (top, black) corresponds to a weight with absolute value of 20.

tion quality improves, while the frequency of casting a full vote increases.[27,28] Under CV, partial abstention decreases with the quality of information.[29]

Figure 3 gives a more detailed picture of voting behaviour under CV by showing the cumulative distribution of weights for different signal precisions. The data reveals a clear and consistent pattern: voters tend to assign higher weights to more precise signals. With only a few exceptions in the negative domain, the weight distributions for high-precision signals stochastically dominate those for lower precision signals.[30] The average weights for each signal and treatment displayed in Figure 4 further illustrate this pattern by showing the average observed behaviour for each signal in each symmetric treatment. Overall, the evidence supports Hypothesis 2: on average, under CV, participants place greater weight on

[27]Figure C3 in the Appendix reports the same breakdown of full abstention, partial abstention, and full vote separately for each of the four symmetric environments without communication. As the figure shows, behaviour is remarkably similar across environments.

[28]See Table D3 in the Appendix, which reports linear regressions on the probability of "full abstention" and "full vote" and shows that full abstention significantly decreases with the precision, while full vote increases with it (under both mechanisms). This result is robust to the inclusion of covariates from the questionnaire.

[29]The regression reported in Table D3 shows that the frequency of a partial vote significantly decreases with the precision of the signal.

[30]Figure C4 in the Appendix confirms that this pattern is consistent across all symmetric treatments. Similarly, Figure C5 shows that the pattern persists across both the first and second half of the block.

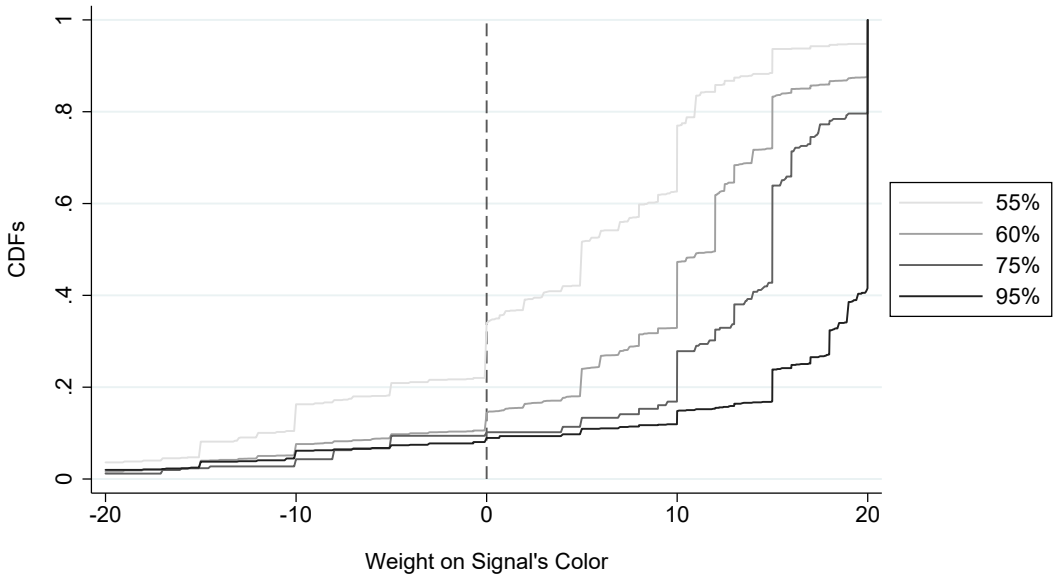


Figure 3: Cumulative distribution of weights by signal precision in the main treatments under CV.

more precise signals.[31]

A striking prediction under CV is that behaviour should not vary across symmetric environments. Figure 4 (and Figure C4 in the Appendix) shows that participants' behaviour is indeed remarkably similar across treatments. This consistency is particularly noteworthy given the between-subject design of the experiment: no participant experienced more than one treatment.[32] These findings provide support for Hypothesis 3.

While voting patterns appear largely consistent across treatments, we identify three clear deviations from the theoretical predictions outlined in Section 2 in terms of voting behaviour under CV. First, some participants vote for the colour opposite to their signal (see Figure 3).[33] The probability with which they do it decreases with the precision of the signal: on average, they vote against their signal with probabilities 22%, 11%, 9% and 8% for the precisions of 55%, 60%, 75% and 95% respectively. Second, we observe substantially greater variance than predicted. If all participants coordinated on the same efficient equilib-

[31] All pairwise comparisons are significantly different ($p < 0.01$).

[32] We cannot reject the null hypothesis that weights are consistent across the four main treatments. However, when we include control variables from the questionnaire, we reject the null hypothesis of consistency across treatments for signals with 55% precision, primarily due to treatment *Distrib*, where the average weight is significantly higher than in the other treatments (4.95 vs 1.94, 2.2, and 1.74). When excluding treatment *Distrib*, we find no significant difference in weights ($p > 0.80$). The results are robust to considering relative rather than absolute weights. Detailed results are provided in Tables D4 and D5 in Appendix D.

[33] Voting against the signal is frequently observed in majority voting experiments on information aggregation. See, for example, Guarnaschelli et al. (2000); Bouton et al. (2017); Mengel and Rivas (2017); Mattozzi and Nakaguma (2023).

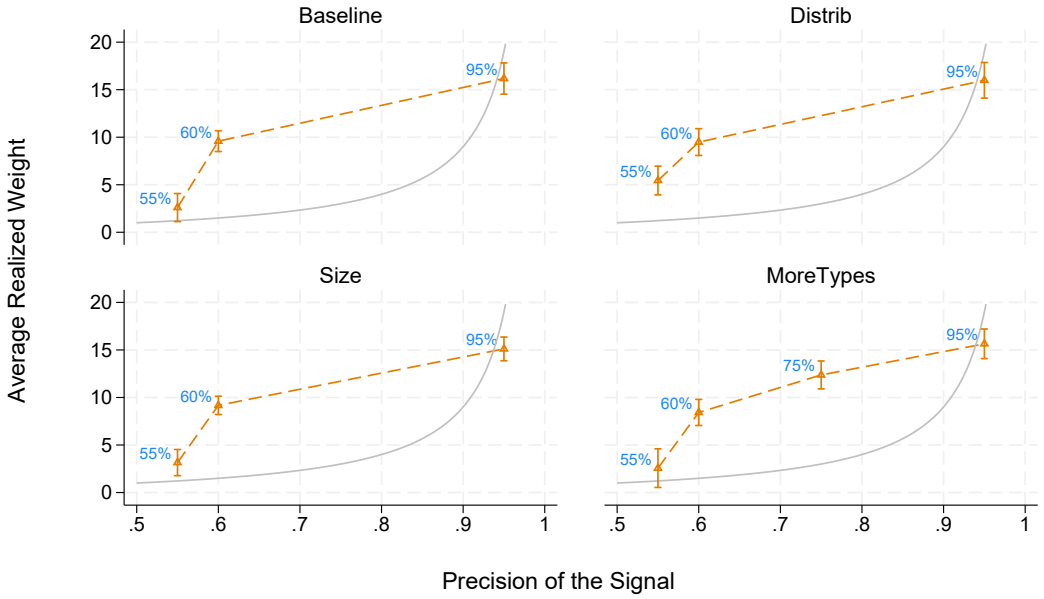


Figure 4: Average realized weight for the colour of the signal in the symmetric treatments under CV, disaggregated by treatment and by signal precision. The efficient equilibrium that assigns the maximum weight on the most informative signal is indicated by solid gray line.

rium, following the formula laid out in Proposition 1 with the same scale parameter κ, we would expect step functions in the distribution of voting weights. Instead, Figure 3 reveals significant dispersion.[34] As a consequence, low-quality signals may carry disproportionately large weights, potentially having more impact on the decision than high-quality signals. For instance, participants with a precision of 60% choose the maximum weight of 20 for 12.44% of the cases, while participants with a precision of 95% choose a lower weight 41.55% of the time. In our equilibrium predictions reported in Table 2, we focused on the focal efficient equilibrium in which voters with the highest precision assign the maximum voting weight of 20. Even though voting data presents more variation than coordination on this focal equilibrium would suggest, we do see support for focusing on that particular equilibrium as a benchmark. On Figure 3, we indeed observe that subjects with the highest precision vote for their signal's colour with a weight of 20 approximately 60% of the time, and we further see on Figure C4 in Appendix C that this pattern remains broadly stable across environments.

The third and perhaps most remarkable deviation from the theory is that participants attach significantly higher weights than predicted for signals with intermediate precisions (see Figure 4).[35] This indicates that signals of intermediate precision are overweighted in group

[34]This is not driven by different groups converging to different equilibria. While the results exhibit some heterogeneity across groups, the qualitative pattern is consistent in all of them.

[35]A joint test of coefficients from a linear regression rejects that average weights are proportional to the

decisions, weakening the influence of more informative signals. One possible explanation is that individuals misjudge the informativeness of different signals. This aligns with recent findings by Agranov and Reshidi (2023), which show that participants often fail to fully account for the non-linearities required for accurate Bayesian updating. Instead, they tend to rely on an updating rule that only partially reflects these complexities, resulting in behaviour that falls between Bayesian and linear updating. In our data, we reject a linear relation of weights with respect to the precisions ($p < 0.01$). In fact, we find that weights are concave with respect to the precision. Relatedly, Augenblick et al. (2021) finds that individuals tend to overestimate the value of weak signals and underestimate the strength of more reliable ones. However, a key departure from their findings in our data is that overinference occurs primarily among subjects with intermediate-level signals (60% precision), whereas it is absent among those with the least informative signals (55% precision).[36]

Under MV, there is a multiplicity of pure-strategy equilibria in all environments, but there is a unique double-symmetric payoff-dominant equilibrium for each environment (see Table D1 in the Appendix). Comparing these payoff dominant equilibria across environments, we should observe stark differences in behaviour. In the baseline treatment, all voters should cast their vote according to their signal, with no one abstaining. In the other main treatments, however, only voters with a precision of 75% or higher should vote, while the rest should abstain. Contrary to these predictions, the data reveal little variation in behaviour across treatments (see Table D6 in the Appendix). This discrepancy may be attributed to the relatively small utility differences between equilibria. As shown in Table D1 in the Appendix, the probability of selecting the correct alternative differs by less than 0.05 between the Pareto-dominant and Pareto-inferior equilibria across treatments. Similarly to what we observed under CV, and in line with Hypothesis 2, weights under MV tend to increase with signal precision. All pairwise comparisons are statistically significant, except between signals of 75% and 95% precision.[37]

4.2.1 Compensating for Asymmetries

When the prior is uneven, as in the *Asymmetric Prior* environment, voters need to compensate for the differing likelihoods of the two states, requiring stronger evidence to select the less likely state. In this section, we assess the extent to which voters were able to compen-

log-likelihood ratios ($F_{3,47} = 41.64$, $p < 0.01$). All pairwise comparisons are significant with $p < 0.1$. If we add the covariates from the questionnaire, the general result holds but the pairwise comparisons of average weights with precisions 60%, 75% and 95% are no longer significant.

[36]See Benjamin (2019) for a comprehensive survey on biases in belief updating.

[37]The equality of behaviour between precisions of 75% and 95% is consistent with the equilibrium predictions reported in Table D1, where both types vote in the same manner across all equilibria.

sate under both mechanisms. Table D2 in Appendix D displays the average weights for each mechanism, environment, precision and signal colour.

Consistent with Hypothesis 4, we find evidence of compensation under both mechanisms. Under CV, we find some evidence of over-compensation by voters in comparison to the theoretical predictions. On average, voters should compensate by adjusting their weights by 2.18 when voting for blue versus red. In the data, the average weight difference is 4.83. The difference in weights between blue and red signals for precisions of 55% and 60% in *Asymmetric Prior* are 3.43 and 4.17, respectively, which are larger than any other differences observed. However, these differences are not consistently significant. For the 55% precision, there is no significant difference in weight across colours in treatments without communication, and while the difference in the *Asymmetric Prior* is significantly larger than in the Baseline, it is not significantly different from other environments. With precision 60%, all pairwise comparisons are highly significant ($p < 0.01$), and we reject the null hypothesis that the differences are equal across treatments. At the highest precision, we observe no significant differences compared to other environments.

Under MV, we should also observe compensation in favour of red.[38] Table D6 in the Appendix shows that, similar to the main treatments, voters mix at the aggregate level across all types. While the reaction is less pronounced than predicted, we observe differences across signal colours. As shown in Table D2 in Appendix D, the average weight for red signals is significantly higher with precisions 55% and 60% (but not for 95%). The magnitude of this effect is also sizeable: while the differences across signals in other environments and precisions do not exceed 2.72, the differences in this case are 5.03 and 5.62 for 55% and 60% respectively.[39]

4.2.2 The Effect of Communication

In this section, we examine the effect of communication on voting behaviour. As discussed in Section 2, there exist equilibria under both mechanisms in which voters share their private information with the group and vote according to the group posterior. We begin by examining whether participants reveal relevant information during the communication treatments and then assess the extent to which private information influences voting decisions after

[38] According to the theoretical predictions (see Table 2), voters with a blue signal should only vote for the color of their signal if their precision is 95%, while those with a red signal should vote for red if their precision is 60% or higher, and abstain otherwise.

[39] We reject the null hypothesis that weight differences across colours are consistent across majority treatments without communication. For precisions of 55% and 60%, there are no significant differences between the *Asymmetric Prior* and *Baseline* environments. However, significant differences emerge when compared to other symmetric treatments without communication. At the highest precision, no significant differences are observed between the *Asymmetric Prior* and other environments.

accounting for the group posterior.

Do participants reveal relevant information before the vote in the communication treatments? The answer is unequivocally yes. On average, groups exchange 10.64 messages per round. In 45.05% of these messages, participants discuss both the precision and the colour of their signals, while an additional 27.37% focus solely on the colour. Since CV has efficient equilibria even without communication, one might expect weakly higher levels of communication under MV. However, we find no economically significant differences between the mechanisms in terms of communication volume or types of messages.[40]

Next, we investigate whether the average voting weights reflect participants' private information or whether, conditional on the group posterior, these weights remain largely invariant across signals. Figure 5 presents the average weights assigned to the colour supported by the group posterior for all signals, separated by the strength of the posterior, under both mechanisms.[41] The most likely outcome is normalised to blue, indicating that blue signals align with the group posterior, whereas red signals do not. The figure reveals a clear pattern: conditional on a group posterior, there is little variation in average weights across different signals. Even when a signal contradicts the group posterior, its corresponding average weight is similar to that of a signal that aligns with the posterior.

To further quantify this relationship, we estimate the weight as a function of the log-likelihood of the signal and the log-likelihood of the group posterior. The results show that the coefficient for the log-likelihood of the signal is relatively small in magnitude and statistically insignificant under both mechanisms. This suggests that, once the group posterior is accounted for, private information has little to no influence on voting behaviour.

Our findings contrast with those in Guarnaschelli et al. (2000), who study communication through straw polls before voting in an information aggregation experiment with binary states and signals. In their setting, despite more than 90% of participants truthfully revealing their signals, their private information still significantly influences the final vote, leading to substantial differences in outcomes across mechanisms. In our experiment, however, private information does not significantly affect voting behaviour under either mechanism after controlling for the group posterior.[42]

[40] See Table D7 for a summary of message statistics across mechanisms.

[41] For comparability, weights under MV are normalised to $\{-20, 0, 20\}$.

[42] Relatedly, Goeree and Yariv (2011) and Le Quement and Marcin (2020) also examine the effect of communication on voting behaviour and outcomes. In contrast to us, they focus on a setting with heterogeneous preferences.

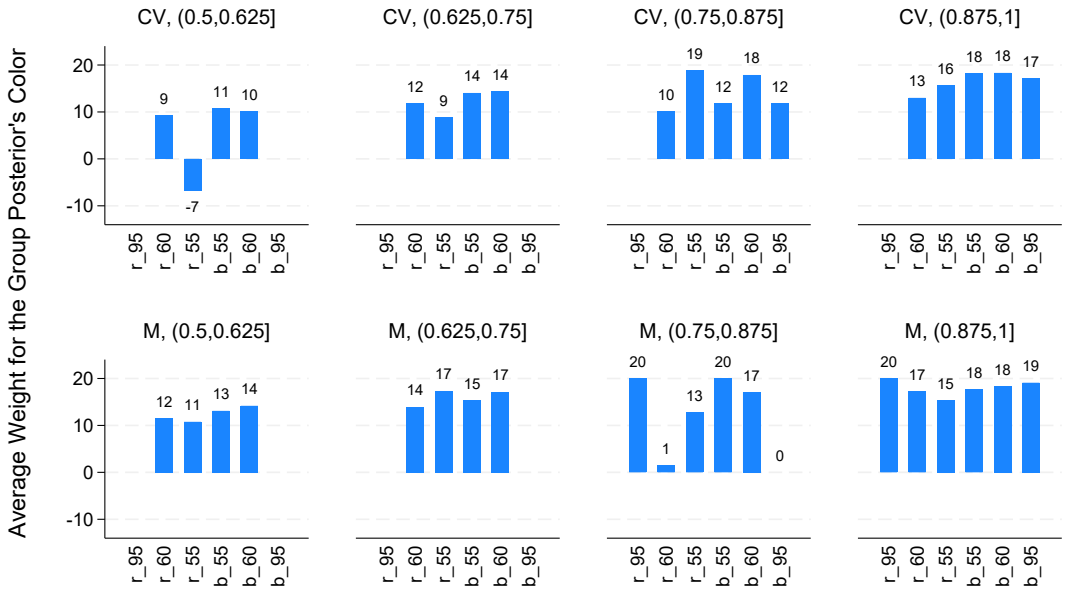


Figure 5: Average weights for each signal by mechanism and posterior. The most likely outcome is normalized to blue, meaning blue signals align with the group posterior, while red signals do not.

4.3 Choice of mechanisms

In part 3 of the experiment, subjects were asked to select a voting mechanism that would be used for 10 additional rounds. In a frictionless setting—where mechanisms impose no differences in cognitive effort or time, and preferences depend solely on expected payoffs—participants should unanimously opt for CV in the absence of communication. However, in sessions without communication, only 58.33% of subjects preferred CV over MV. As Figure 6 shows, support for CV varied across treatments but consistently exceeded that for MV in environments without communication. By contrast, under communication, where there are no clear theoretical predictions, around 88% of participants opted for MV.

What drives these preferences? To explore this question, we conduct an exploratory analysis using a linear probability model to predict the likelihood of selecting CV in part 3, as summarised in Table 4. Our findings indicate that realised payoffs play a significant role: higher realised payoffs under CV increase the likelihood of choosing it, while higher realised payoffs under MV have the opposite effect, with a similar magnitude. The effect is economically meaningful: a payoff difference of 10 (out of 100) corresponds to a 15 percentage point increase in the probability of selecting CV.[43]

[43]We obtain similar results when using simulated payoffs instead of realised payoffs in treatments without communication. This alternative measure mitigates the impact of random variation due to limited observations. However, it is not observed by subjects, and constructing simulated payoffs under communication is not straightforward.

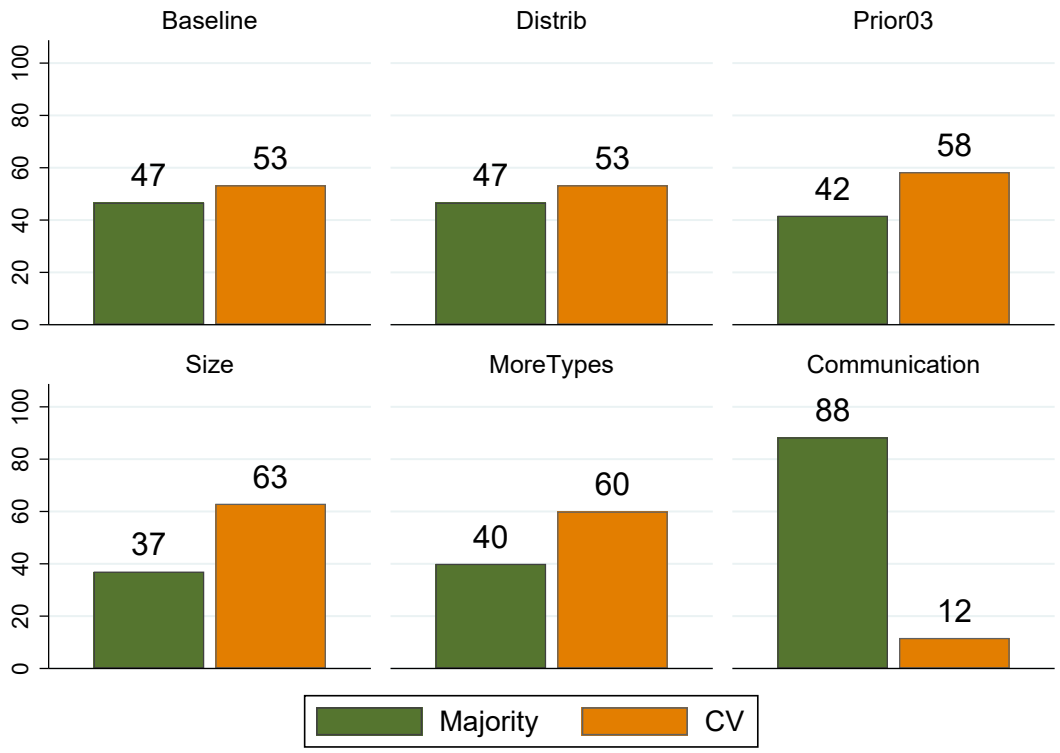


Figure 6: Support for each mechanism in part 3 of the experiment.

By contrast, we find no robust or economically significant effects for other potential determinants, including group decision time, cognitive complexity, and mechanism order. While decisions under CV took 28% longer on average,[44] this had no systematic impact on preferences. Likewise, cognitive ability—proxied by control question completion time—shows no clear effect, nor does the order in which participants experienced the mechanisms. The only other strong predictor is communication: when participants can discuss before voting, support for CV drops significantly.

[44]Figure C6 in Appendix C plots the average time to reach a decision by mechanism and period. The gap is present from the first period and narrows with experience under both mechanisms.

Dep. Var.: System Choice	(1)	(2)	(3)	(4)	(5)	(6)
Payoff CV	0.015***	0.015***	0.016***	0.016***	0.016***	0.017***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Payoff MV	-0.018***	-0.018***	-0.016***	-0.016***	-0.016***	-0.014***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Time CV			-0.004	-0.001	-0.002	0.000
			(0.004)	(0.008)	(0.009)	(0.007)
Time MV			-0.014**	-0.017*	-0.016	0.006
			(0.006)	(0.010)	(0.012)	(0.010)
Time Control Quest. CV				-0.084	-0.089	-0.121*
				(0.070)	(0.073)	(0.068)
Time Control Quest. MV				-0.018	-0.011	-0.039
				(0.060)	(0.062)	(0.061)
Order (CV First)					0.021	0.107
					(0.126)	(0.106)
Communication						-0.450***
						(0.086)
Constant	0.662**	0.715*	0.695*	0.773*	0.767*	0.388
	(0.274)	(0.386)	(0.391)	(0.391)	(0.390)	(0.310)
Questionnaire Controls		✓	✓	✓	✓	✓
Observations	408	408	408	408	408	408
Clusters	72	72	72	72	72	72
R-squared	0.234	0.247	0.270	0.275	0.275	0.324

Table 4: Linear regression of the probability of voting for CV in part 3. Standard errors are clustered at the matching group level.

5 Conclusions

In this paper, we present a laboratory experiment in which participants endogenously determine the weight of their votes. Our findings reveal that welfare under CV is generally higher than under MV, though the observed difference is smaller than theoretical predictions suggest. When free communication is allowed, the performance gap between the two mechanisms disappears. In terms of voting behaviour we find that, while the main comparative statics of the model are broadly supported by the data, there are notable deviations. The most significant is that voters with intermediately informative signals tend to assign disproportionately high weights to their votes, diluting the influence of highly informative signals.

This study opens several avenues for future research. One potential direction is to examine CV in asymmetric settings, where it is expected to outperform majority voting even without differences in voter precision. Another promising area is testing the mechanisms in more natural settings where participants may be uncertain about their own precision or the information technology of others. In our controlled experiment, confidence and performance were necessarily linked, but in more natural environments, confidence may not accurately reflect actual performance. This discrepancy could have important implications for the information-aggregating properties of both mechanisms (see, e.g., Kartal and Tyran 2022; Enke et al. 2023). Investigating how CV and MV perform under these conditions would offer valuable insights into the robustness and real-world applicability of both systems.

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A Proofs

A.1 Proof of Proposition 2

We start by establishing the following key lemma, where we use the notation $\hat{\sigma}_i(t_i)$ to denote a possible realization of the random variable $\sigma_i(t_i)$.

Lemma 1. *For any profile σ, interim utilities associated to each action are such that:*

- *$U_i(1 \mid t_i) - U_i(0 \mid t_i)$ is increasing in t_i, and strictly so whenever $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) \in \{-1, 0\}) > 0$.*
- *$U_i(0 \mid t_i) - U_i(-1 \mid t_i)$ is increasing in t_i, and strictly so whenever $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) \in \{0, 1\}) > 0$.*

Proof. We write the proof for the first bullet point (the remaining one follows a similar argument). Noting $\Delta_i(t_i) := U_i(1 \mid t_i) - U_i(0 \mid t_i)$ for the difference of (expected) interim utilities, we have:

$$
\begin{aligned}
\Delta_i(t_i) = \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = 0 \mid t_i)\mathbb{E}\left[\frac{u_i(A) - u_i(B)}{2} \;\middle|\; t_i, \sum_{j\neq i} \hat{\sigma}_j(t_j) = 0\right] + \\
\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = -1 \mid t_i)\mathbb{E}\left[\frac{u_i(A) - u_i(B)}{2} \;\middle|\; t_i, \sum_{j\neq i} \hat{\sigma}_j(t_j) = -1\right] \\
= \sum_{k=-1}^{0} \frac{1}{2} \sum_{t_{-i}\in T^{n-1}} \Pr(t_{-i} \mid t_i) \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = k)(Pr(\alpha \mid t_i, t_{-i}) - Pr(\beta \mid t_i, t_{-i})) \\
= \sum_{k=-1}^{0} \frac{1}{2} \sum_{t_{-i}\in T^{n-1}} \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = k) \frac{\Pr(t_i, t_{-i})}{\Pr(t_i)} \left(\frac{Pr(\alpha, t_i, t_{-i})}{\Pr(t_i, t_{-i})} - \frac{Pr(\beta, t_i, t_{-i})}{\Pr(t_i, t_{-i})}\right) \\
= \sum_{k=-1}^{0} \frac{1}{2} \sum_{t_{-i}\in T^{n-1}} \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = k) \left(Pr(\alpha) \frac{Pr(t_i, t_{-i} \mid \alpha)}{\Pr(t_i)} - Pr(\beta) \frac{Pr(t_i, t_{-i} \mid \beta)}{\Pr(t_i)}\right) \\
= \sum_{k=-1}^{0} \frac{1}{2} \sum_{t_{-i}\in T^{n-1}} \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = k) \left(Pr(t_{-i}, \alpha) \frac{Pr(t_i \mid \alpha)}{\Pr(t_i)} - Pr(t_{-i}, \beta) \frac{Pr(t_i \mid \beta)}{\Pr(t_i)}\right) \\
= \sum_{k=-1}^{0} \frac{1}{2} \sum_{t_{-i}\in T^{n-1}} \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = k) \left(Pr(t_{-i} \mid \alpha) \frac{t_i}{\frac{Pr(\beta)}{Pr(\alpha)} + t_i} - Pr(t_{-i} \mid \beta) \frac{1}{1 + \frac{Pr(\alpha)}{Pr(\beta)} t_i}\right).
\end{aligned}
$$

This concludes the proof of the lemma. □

To conclude the proof of Proposition 2, let σ be a non-trivial equilibrium. If i is a voter such that $\sigma_i(t_i)$ is a singleton independent from t_i, then she follows a monotone cutoff strategy.

Consider now i such that there exists t_i, t_i' with $\Pr(\hat{\sigma}_i(t_i) \neq \hat{\sigma}_i(t_i')) > 0$, we say that i is a switcher. As σ is a non-trivial equilibrium such that (i) votes are conditionally independent across voters, and (ii) votes cannot differ by more than 2 for any given voter, we have two cases to consider:

- if $\Pr(\sum_{j=1}^{n} \hat{\sigma}_j(t_j) = 0) > 0$, as i is a switcher, it must be the case that either $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = 0) > 0$ (this happens if i plays 0 with positive probability) or $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = -1) \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = 1) > 0$ (this happens if i only plays -1 and 1 with positive probability). In both cases, we have that $U_i(1 \mid t_i) - U_i(0 \mid t_i)$ and $U_i(0 \mid t_i) - U_i(-1 \mid t_i)$ are strictly increasing in t_i by Lemma 1. As σ is an equilibrium, σ_i must be a monotone cutoff strategy.

- if $\Pr(\sum_{j=1}^{n} \hat{\sigma}_j(t_j) = 0) = 0$, then $\Pr(\sum_{j=1}^{n} \hat{\sigma}_j(t_j) = -1) \Pr(\sum_{j=1}^{n} \hat{\sigma}_j(t_j) = 1) > 0$. As i is a switcher, we have that either $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = -1) \Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = 1) > 0$ (this happens if i plays 0 with positive probability) or $\Pr(\sum_{j\neq i} \hat{\sigma}_j(t_j) = 0) > 0$ (this happens if i only plays -1 and 1 with positive probability). As for the previous case, it follows that σ_i must be a monotone cutoff strategy.

We have thus established that any non-trivial equilibrium must be in monotone cutoff strategies.

To conclude, we show that a non-trivial equilibrium exists. Assume without loss of generality that $Pr(\alpha) \geq Pr(\beta)$, so that A is the ex ante optimal decision. Let $t^* = t_\beta^{\max P}$ be the strongest possible type in favor of B. Consider the symmetric strategy profile σ, where each player i abstains when she gets t^* and votes for A otherwise, i.e. $\sigma_i(t^*) = 0$ and $\sigma_i(t) = 1$ for any $t \neq t^*$. The only event in which the final decision is B (with positive probability) arises when all voters have the type t^*, in which case β is strictly more likely than α (by the assumption made in footnote 8). It follows that $U(\sigma) > U(A) \geq U(B)$. By the argument of McLennan (1998), the best strategy profile τ must be an equilibrium and it must be such that $U(\tau) \geq U(\sigma) > U(A)$. Hence it must be that τ is a non-trivial equilibrium (otherwise the expected utility would not be higher than $U(A)$, a contradiction). We conclude that a non-trivial equilibrium always exists.

A.2 Proof of Proposition 3

Proof. Consider first the case where n is even, that is $n = 2q$. Assume that an efficient equilibrium σ exists under MV. Let $p, p' \in P$ be such that $p < p'$. Let $\mathbf{t_{-i}}$ be a type profile such that q voters receive t_β^p, while $(q-1)$ voters receive t_α^p. As the prior is even, by efficiency of σ, we must have $\Pr(\hat{\sigma}_i(t_\alpha^{p'}) + \sum_{j\neq i} \hat{\sigma}_j(t_j) > 0) = 1$, as A must be chosen if i is of type $t_\alpha^{p'}$,

and $\Pr(\hat{\sigma}_i(t_\beta^p) + \sum_{j\neq i} \hat{\sigma}_j(t_j) < 0) = 1$, as B must be chosen if i is of type t_β^p. Hence, it must be that $\sigma_i(t_\alpha^{p'}) = 1$ and $\sigma_i(t_\beta^p) = -1$, and this conclusion holds for each i. Consequently, for a type profile $\mathbf{t}$ such that q voters receive t_β^p, while q voters receive $t_\alpha^{p'}$, we have $\sum_{i=1}^n \sigma_i(t_i) = 0$, while efficiency requires $\sum_{i=1}^n \sigma_i(t_i) > 0$. We thus obtain a contradiction.

Consider now the case where n is odd, that is $n = 2q + 1$. Assume that an efficient equilibrium σ exists under MV. Let $p \in P$ and let $\mathbf{t}_{-\mathbf{i}}$ be a type profile such that q voters receive t_α^p, while q voters receive t_β^p. As the prior is even, by efficiency of σ, we must have $\Pr(\hat{\sigma}_i(t_\alpha^p) + \sum_{j\neq i} \hat{\sigma}_j(t_j) > 0) = 1$ and $\Pr(\hat{\sigma}_i(t_\beta^p) + \sum_{j\neq i} \hat{\sigma}_j(t_j) < 0) = 1$. Hence, it must be that $\sigma_i(t_\alpha^p) = 1$ and $\sigma_i(t_\beta^p) = -1$, and this conclusion holds for each i and each $p \in P$.

Let $p_m := \min P$ and $p_M := \max P > p_m$. Consider a type profile $\mathbf{t}$ such that q voters receive $t_\alpha^{p_M}$ while $(q+1)$ voters receive $t_\beta^{p_m}$. Observe that $\Pr(\alpha \mid \mathbf{t}) > 1/2$ if and only if

$$\left(\frac{p_M}{1-p_M}\right)^q > \left(\frac{p_m}{1-p_m}\right)^{q+1} \quad \Leftrightarrow \quad q > \frac{\ln\left(\frac{p_m}{1-p_m}\right)}{\ln\left(\frac{p_M(1-p_m)}{(1-p_M)p_m}\right)} := q(P).$$

Let $n(P) := 2\lfloor q(P) \rfloor + 3$ and assume $n \geq n(P)$, so that $q > q(P)$. We have $\Pr(\alpha \mid \mathbf{t}) > 1/2$ but $\sum_{i=1}^n \sigma_i(t_i) = q - (q+1) = -1 < 0$, hence a contradiction with the efficiency of σ.

To conclude, if n is even, or if n is odd with $n \geq n(P)$, there is no efficient equilibrium under MV. □

To complement the proof, we first plot the value of $n(P)$ as a function of the minimal and maximal precisions of P.

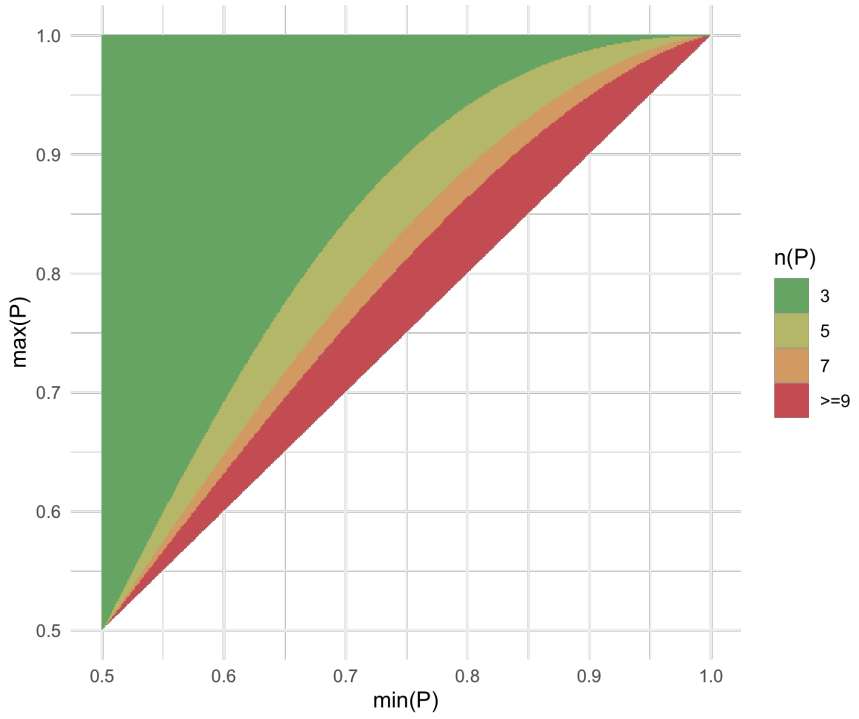


Figure A1: Bound $n(P)$ as a function of minimal and maximal precisions.

We observe that $n(P)$ is typically quite low. When the prior is even, CV strictly dominates MV for committees of at least 5 voters provided that the highest and lowest precisions differ by at least 0.1.

Finally, for the Asymmetric Prior treatment (A), we can follow a reasoning similar to that in the proof of Proposition 3. Suppose that an efficient equilibrium σ exists under MV. Observe that

$$\Pr(\alpha \mid t_\alpha^{0.55}, t_\alpha^{0.55}, t_\alpha^{0.55}, t_\beta^{0.55}, t_\beta^{0.55}) \approx 0.34 < 1/2 < \Pr(\alpha \mid t_\alpha^{0.95}, t_\alpha^{0.55}, t_\alpha^{0.55}, t_\beta^{0.55}, t_\beta^{0.55}) \approx 0.89,$$

from which it follows (by efficiency of σ) that $\sigma_i(t_\alpha^{0.55}) = -1$ and $\sigma_i(t_\alpha^{0.95}) = 1$. Then, observe that

$$\Pr(\alpha \mid t_\alpha^{0.55}, t_\alpha^{0.55}, t_\alpha^{0.55}, t_\alpha^{0.55}, t_\alpha^{0.55}) \approx 0.54 > 1/2,$$

from which it follows (by efficiency of σ) that $\sigma_i(t_\alpha^{0.55}) = 1$. We thus obtain a contradiction. We conclude that no efficient equilibrium exists under MV for the Asymmetric Prior treatment (A).

B Questionnaire Data

In this section we describe the data collected in the questionnaire at the end of the experiment (see Table B1) and show how these vary across treatments (see Table B2). Variables Party and Religion were not included in the regressions in Appendix D.

Variable	Description
Gender	Female $= 1$; Male $= 0$
Age	Age in years
Economics	$= 1$ if the major is Economics. Originally, this was a categorical variable with the options "Law" (9.07%), "Economics" (14.95%), "Philology / Literature" (2.45%), "Physics/Chemistry/Biology" (2.70%), "Engineering" (17.40%), "History" (1.23%), "Politics" (1.72%), "Mathematics" (1.23%), "Others" (49.26%).
Year	Years of studies.
Religiosity	Degree of religiosity. Likert scale from 0 to 3.
Religion	Categorical variable: Christian (53.19%), Hinduist (0), Muslim (1.23), No religion (37.99), Other Religion (1.47), Prefer not to answer (6.13). Not included in the regressions.
Politics	Interest in Politics. Likert scale from 1 to 4.
Party	Categorical variable: Podemos (16.67%), PP (14.71%), PSOE (12.01%), EUPV-EV (2.94%), UPvD (1.23%), Primavera (0.98%), Others (31.86%), Dk/Na (19.61%) Not included in the regressions.
Risk	Tendency to take risks. Likert scale from 1 to 5.
Trust	Tendency to trust people. Likert scale from 1 to 5.
Experienced	$= 1$ if the subject has participated in 4 or more experiments. Originally, this was a categorical variable about participation in previous experiments: "Never", "1-3", "4-6", and "More than 6".
Siblings	Number of siblings.

Table B1: Description of variables in the questionnaire data.

	Baseline	Distrib	Prior	Size	MoreTyp	Comm.	p-value
Gender	0.62	0.67	0.62	0.54	0.48	0.62	0.31
Age	20.90	20.58	21.07	20.80	19.93	20.78	0.01
Economics	0.13	0.17	0.18	0.13	0.18	0.12	0.83
Year	3.05	2.87	3.28	2.95	2.52	3.00	0.11
Religiosity	0.65	0.68	0.68	0.70	0.67	0.68	1.00
Politics	1.50	1.48	1.47	1.52	1.58	1.78	0.17
Left wing	0.37	0.37	0.33	0.31	0.27	0.40	0.67
Risk	2.67	2.57	2.63	2.65	2.72	2.48	0.66
Trust	2.40	2.52	2.28	2.57	2.38	2.43	0.58
Experience	0.50	0.28	0.38	0.41	0.37	0.33	0.22
Siblings	1.40	1.28	1.27	1.33	1.18	1.13	0.68

Table B2: Summary statistics by environment groups. The last column reports the p-value of an F-test of equality across treatments.

C Additional Figures

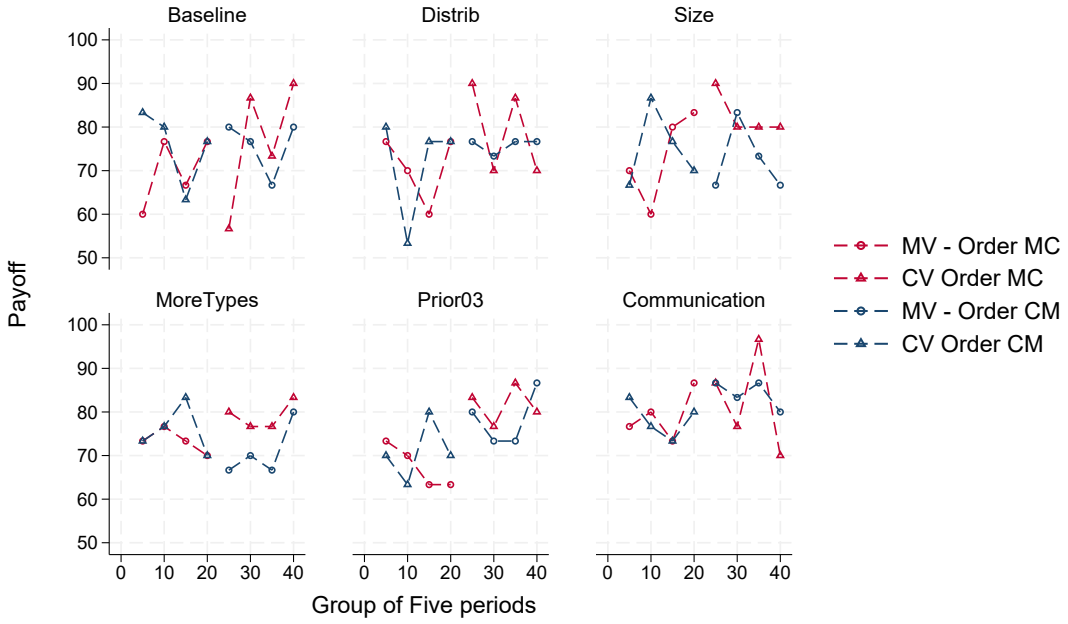


Figure C1: Temporal evolution of average payoffs. 'MC' denotes groups that used MV in Part 1 and CV in Part 2, while 'CM' refers to groups that experienced the reverse order.

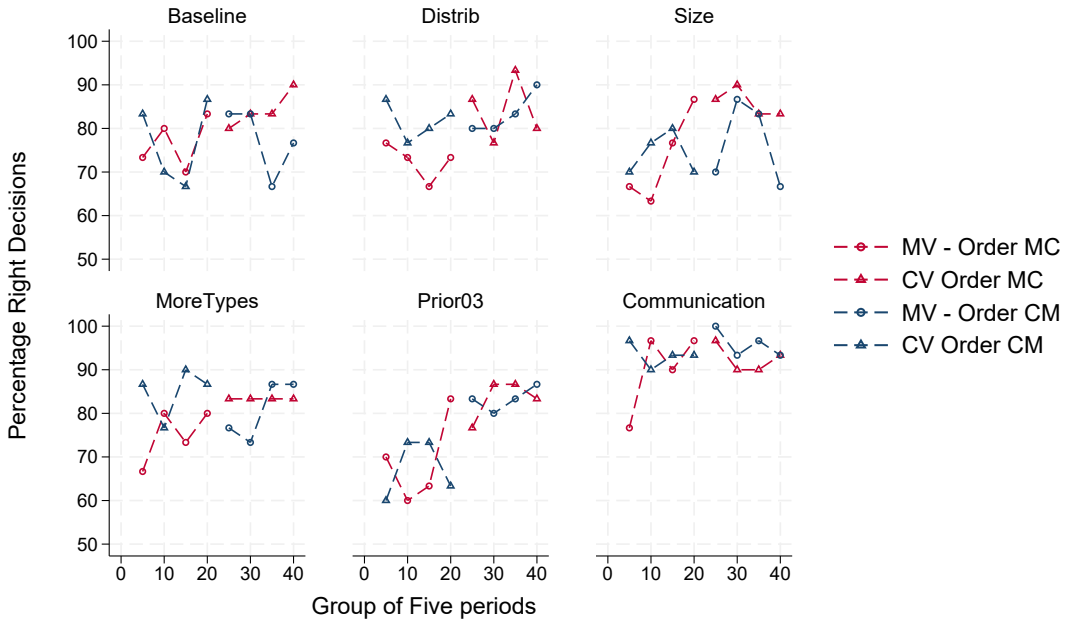


Figure C2: Temporal evolution of the percentage of optimal decisions at the group level. 'MC' denotes groups that used MV in Part 1 and CV in Part 2, while 'CM' refers to groups that experienced the reverse order.

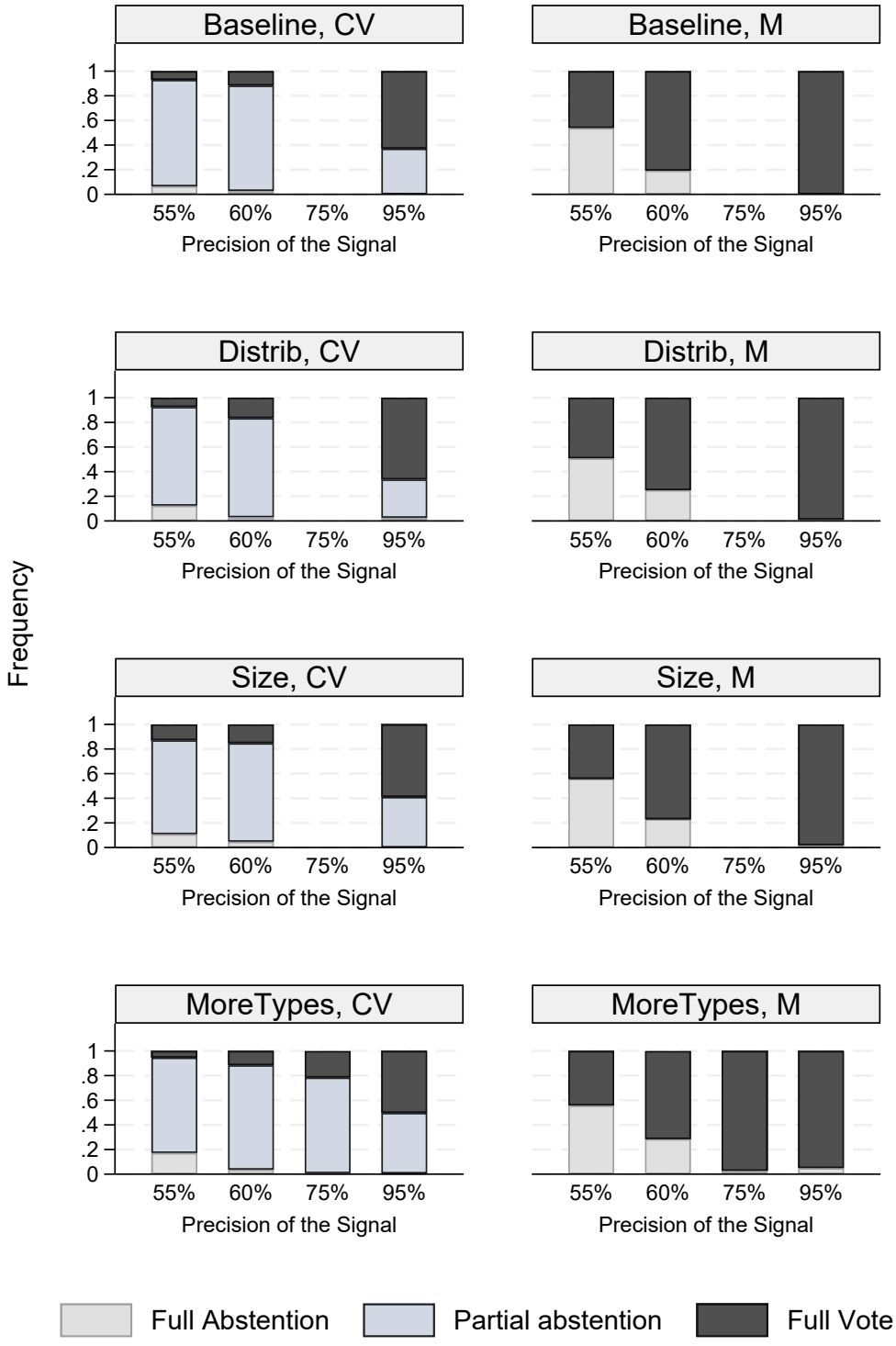


Figure C3: Basic aggregate behaviour by voting rule, signal precision and environment. Each stacked bar shows the composition of votes by type: full abstention (bottom, grey) corresponds to a weight of 0; partial abstention (middle, blue) indicates a weight with absolute value strictly between 0 and 20; and full vote (top, black) corresponds to a weight with absolute value of 20.

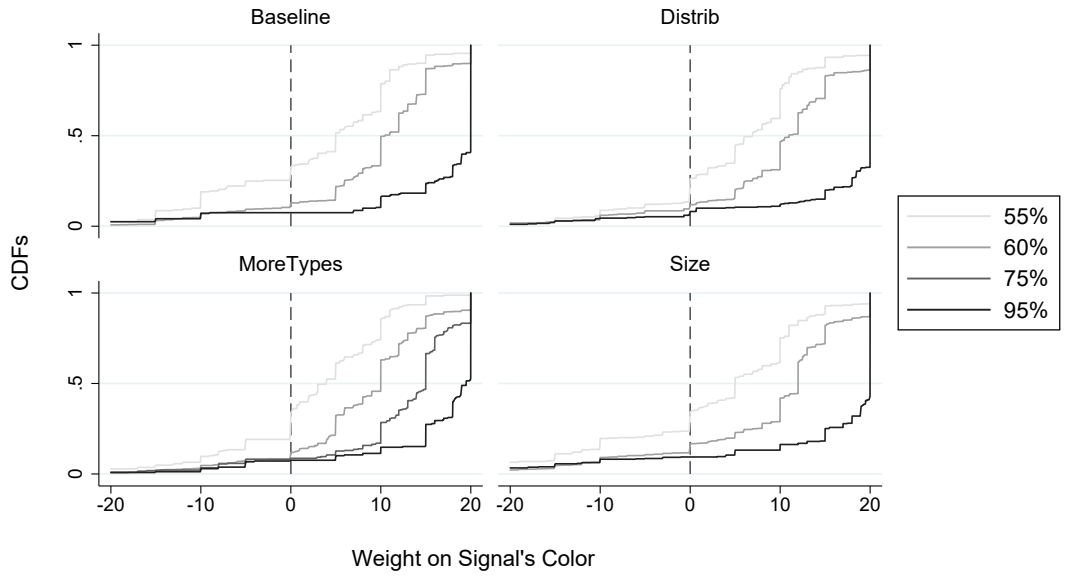


Figure C4: Cumulative distribution of weights by signal precision in the symmetric treatments.

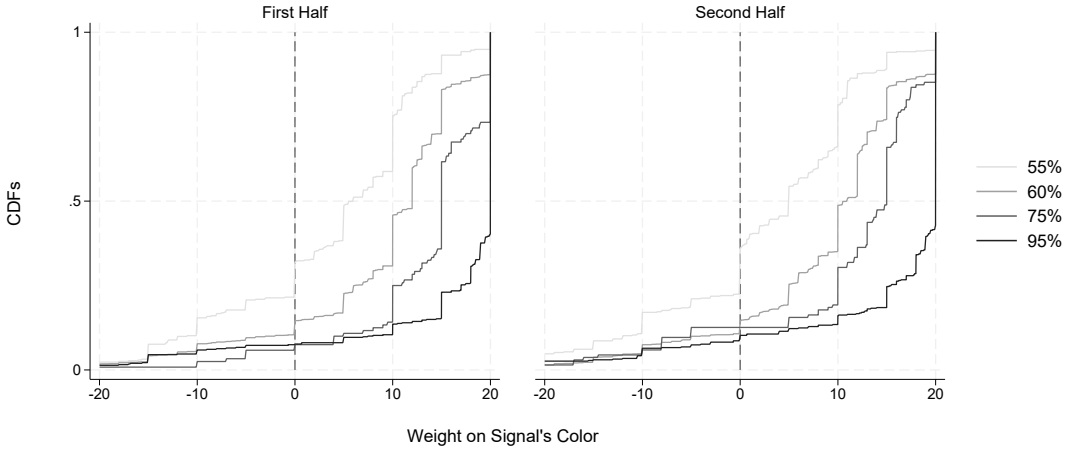


Figure C5: Cumulative distribution of weights by signal precision in the symmetric treatments, separated by the first half of the block and the second one.

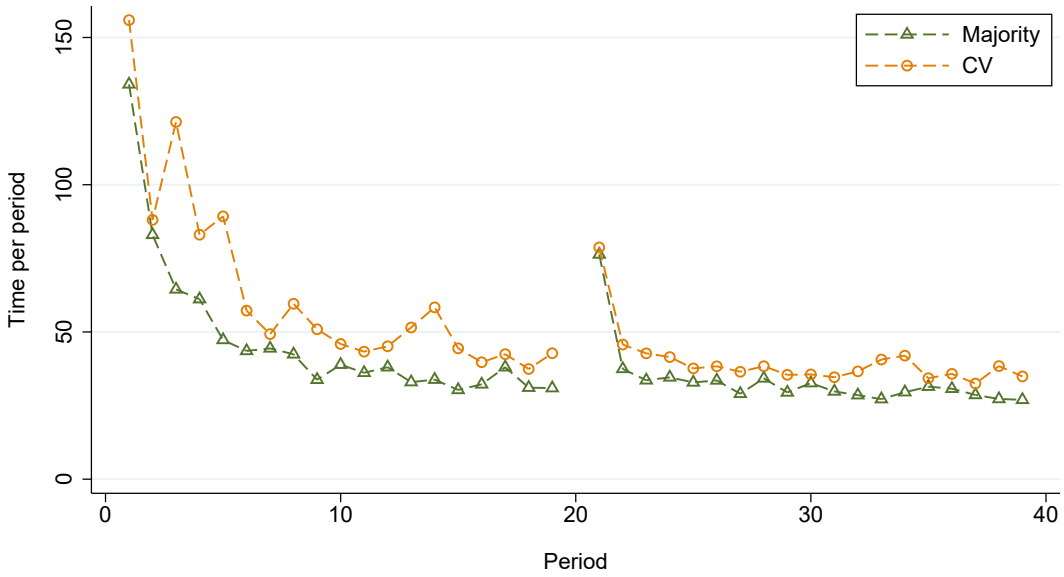


Figure C6: Time to finish each period for each mechanism in parts 1 and 2.

D Additional Tables

	Precision cutoff			
	55%	60%	75%	95%
Baseline	**0.757**	0.754	–	0.751
Distribution	0.802	No eq.	–	**0.848**
Size	0.820	0.820	–	**0.852**
More Types	0.802	0.801	**0.819**	No eq.

Table D1: Pure-strategy double-symmetric equilibria in the symmetric environments under majority rule, along with their expected payoffs. *Precision cutoff* denotes the minimum precision level at which subjects vote, with those having lower precision choosing to abstain. The equilibrium that is Pareto-efficient is highlighted in bold. For instance, we read on the first line that there are three pure-strategy double-symmetric equilibria in the Baseline environment. The first cell indicates that the equilibrium for which the precision cutoff is 55%, i.e. all voters vote, yields an expected payoff of 0.757.

		Av Weight CV				Av Weight MV			
Environment	Prec.	s_b	s_r	dif	p-value	s_b	s_r	dif	p-value
Baseline	55	3.32	1.55	1.77	0.319	-0.27	2.45	2.72	0.341
	60	9.87	9.31	0.56	0.246	11.52	13.26	1.74	0.220
	95	16.53	15.82	0.72	0.503	18.20	19.18	0.97	0.485
Distrib	55	5.75	5.20	0.55	0.665	5.52	3.50	2.02	0.210
	60	9.40	9.57	0.16	0.818	11.90	11.59	0.31	0.767
	95	15.78	16.23	0.45	0.709	19.43	18.19	1.24	0.025
Size	55	2.66	3.69	1.03	0.524	1.66	0.94	0.72	0.668
	60	9.39	8.94	0.45	0.554	11.24	11.74	0.50	0.384
	95	14.12	16.04	1.92	0.054	13.82	15.89	2.07	0.043
MoreTypes	55	1.58	3.74	2.16	0.058	1.84	0.19	1.65	0.403
	60	9.11	7.80	1.31	0.098	10.36	10.87	0.51	0.425
	75	12.33	12.40	0.07	0.945	17.32	16.75	0.57	0.724
	95	14.20	17.01	2.82	0.049	16.67	16.63	0.03	0.975
Prior	55	2.02	5.44	3.43	0.200	1.28	6.32	5.03	0.037
	60	7.27	11.45	4.17	0.011	11.32	16.94	5.62	0.018
	95	15.40	17.10	1.70	0.407	14.74	16.25	1.51	0.595
Comm.	55	3.60	2.78	0.81	0.784	4.76	4.05	0.71	0.769
	60	5.95	6.89	0.94	0.536	6.46	7.55	1.09	0.670
	95	16.69	17.40	0.72	0.563	19.04	17.18	1.86	0.185

Table D2: Average weight for each mechanism, environment, precision and signal color. *dif* displays the absolute difference and *p-value* displays the statistical significant of the difference based on linear regressions.

	Full Vote				Full Abstention				Partial Abstention	
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)
Precision	1.286***	1.296***	0.910***	0.918***	-0.159***	-0.158***	-0.910***	-0.918***	-1.127***	-1.138***
	(0.078)	(0.079)	(0.057)	(0.058)	(0.037)	(0.038)	(0.057)	(0.058)	(0.069)	(0.068)
Gender		-0.018		0.118***		-0.033**		-0.118***		0.051
		(0.030)		(0.026)		(0.015)		(0.026)		(0.033)
Age - 18		0.015		0.010		-0.007**		-0.010		-0.008
		(0.010)		(0.010)		(0.004)		(0.010)		(0.011)
Years Stud.		-0.018		-0.002		0.004		0.002		0.014
		(0.015)		(0.015)		(0.005)		(0.015)		(0.016)
Risk		-0.019		-0.027		0.008		0.027		0.011
		(0.015)		(0.016)		(0.008)		(0.016)		(0.017)
Trust		0.006		-0.041***		0.003		0.041***		-0.009
		(0.013)		(0.015)		(0.009)		(0.015)		(0.014)
Experiments		0.022		-0.036**		0.008		0.036**		-0.030
		(0.016)		(0.013)		(0.012)		(0.013)		(0.022)
Politics		-0.008		-0.004		-0.004		0.004		0.012
		(0.016)		(0.019)		(0.012)		(0.019)		(0.017)
Siblings		0.010		0.001		0.005		-0.001		-0.015
		(0.016)		(0.015)		(0.007)		(0.015)		(0.018)
religiosity		0.009		0.021		0.008		-0.021		-0.017
		(0.018)		(0.015)		(0.016)		(0.015)		(0.024)
Constant	-0.632***	-0.616***	0.159***	0.280***	0.150***	0.131**	0.841***	0.720***	1.482***	1.485***
	(0.055)	(0.092)	(0.051)	(0.090)	(0.033)	(0.064)	(0.051)	(0.090)	(0.049)	(0.109)
Mechanism	CV	CV	MV	MV	CV	CV	MV	MV	CV	CV
Observations	5,760	5,760	5,760	5,760	5,760	5,760	5,760	5,760	5,760	5,760
Clusters	48	48	48	48	48	48	48	48	48	48
R-squared	0.189	0.199	0.086	0.128	0.011	0.025	0.086	0.128	0.127	0.140

Table D3: Linear regression of the probability of full abstention, full vote and partial vote in symmetric environments. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$,* $p < 0.1$.

	Without Covariates		Including Covariates	
	All	All but	All	All but
Precisions	Environ.	*Distrib*	Environ.	*Distrib*
55%	0.104	0.874	0.072	0.819
60%	0.716		0.923	
95%	0.826		0.781	

Table D4: Test of equality of average weights under CV in symmetric treatments without communication based on linear regressions.

	All Symm. CV Treats			All Without Distrib		
Without		60%	55%		60%	55%
Covariates	95%	0.909	0.108	95%	0.766	0.780
	60%		0.029	60%		0.849
	Joint test		0.047	Joint test		0.981
With		60%	55%		60%	55%
Covariates	95%	0.936	0.131	95%	0.812	0.771
	60%		0.123	60%		0.786
	Joint test		0.085	Joint test		0.985

Table D5: Test of equality of ratio of weights of signals of different precisions among symmetric treatments without communication. Based on linear regressions.

		Blue Signal			Red Signal		
	Precision	Signal	Against	Abstent.	Signal	Against	Abstent.
Baseline	55%	51.45	26.59	21.97	43.71	23.95	32.34
	60%	79.34	10.05	10.62	82.02	9.48	8.50
	95%	94.48	5.52	0.00	95.81	4.19	0.00
Distrib	55%	60.42	11.66	27.92	58.66	18.24	23.10
	60%	78.02	9.55	12.43	77.27	9.56	13.17
	95%	93.82	5.82	0.36	95.00	4.38	0.63
Size	55%	49.12	23.10	27.78	48.48	23.03	28.48
	60%	78.19	10.58	11.24	77.18	11.10	11.71
	95%	86.65	12.46	0.89	89.72	9.35	0.93
MoreTypes	55%	48.74	23.62	27.64	48.95	20.53	30.53
	60%	75.81	8.75	15.44	76.72	10.17	13.10
	75%	90.18	8.48	1.34	91.15	7.69	1.15
	95%	88.89	8.77	2.34	91.71	5.70	2.59
Prior	55%	51.59	29.94	18.47	59.02	21.86	19.13
	60%	74.71	16.52	8.77	91.07	4.84	4.09
	95%	90.18	8.93	0.89	91.81	5.60	2.59
Communication	55%	61.15	38.85	0.00	57.67	40.49	1.84
	60%	65.60	33.60	0.80	68.44	30.64	0.92
	95%	96.67	3.33	0.00	93.87	5.52	0.61

Table D6: Frequencies of voting the signal, voting against or abstaining under Majority for each signal precision and signal realization in each treatment.

	MV	CV	Overall
Average # of Messages per round	10.88	10.40	10.64
% Precision is mentioned	48.85	51.51	50.18
% Color is mentioned	70.90	73.94	72.42
% Both Precision and Color are mentioned	42.40	47.71	45.05

Table D7: Summary statistics of the chats in the communication treatments.

E Optimal Decisions By Group Posterior

The probability of making the optimal decision reported in section 4.1 aggregates mistakes of very different natures. Some mistakes are more costly than others: mistakes when there is massive evidence in favour of one of the states are more costly than when the posterior is very close to 0.5. Figure E1 disaggregates the probability of adopting an optimal decision by posteriors. In order to circumvent the limitation of the small number of observations for some of the posteriors, we provide simulation results based on observed behaviour for all treatments without communication.[45] We separate posteriors in five regions: $[0, 0.1]$, $(0.1, 0.4)$, $[0.4, 0.6]$, $(0.6, 0.9)$, and $[0.9, 1]$. The figure shows two regularities. First, CV tends to perform better across most posterior ranges. Second, under both mechanisms, the probability of an optimal decision increases when posteriors are more extreme.

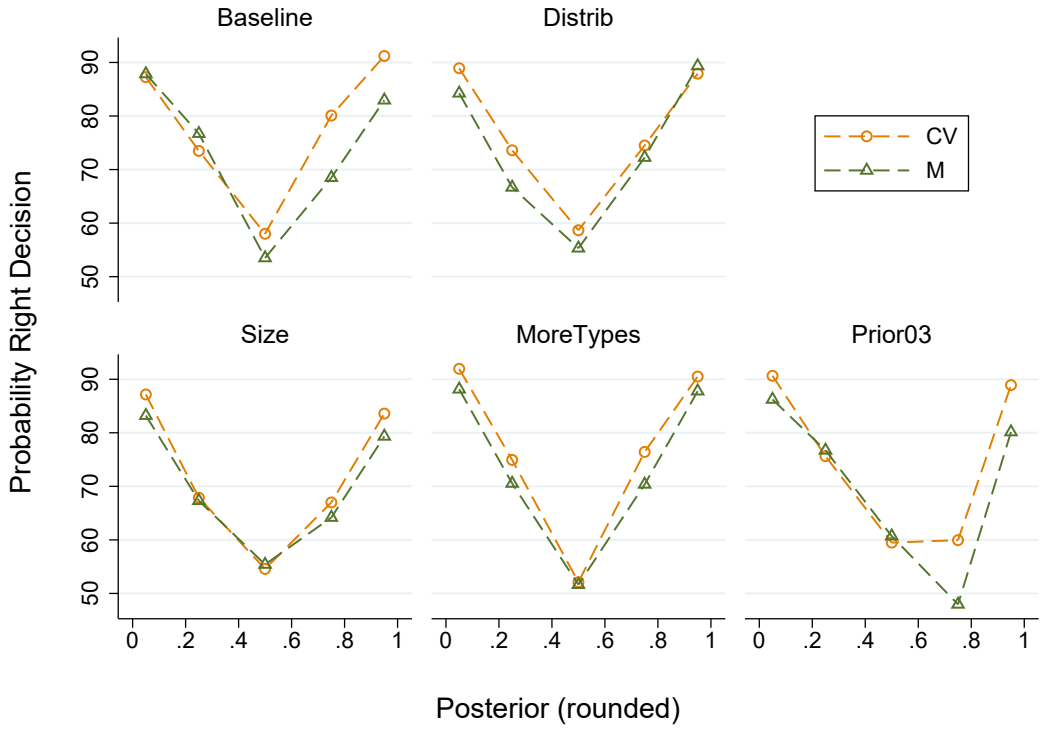


Figure E1: Probability of taking the optimal decision by posteriors. Posteriors (obtained with all the signals received in the group) are separated into 5 categories: $[0, 0.1]$, $(0.1, 0.4)$, $[0.4, 0.6]$, $(0.6, 0.9)$, and $[0.9, 1]$.

[45] In particular, we ran 10,000 simulations for each independent group and mechanism. In each of these simulations, a state is randomly chosen, the signals are randomly assigned to each of the subjects, and then a vote is cast following the distribution of votes observed in that group.

F Balancedness of Signals

	CV				M			
Treatment	55	60	75	95	55	60	75	95
Baseline	14.08	70.42	–	15.50	14.25	70.25	–	15.50
Distrib	25.58	48.42	–	26.00	25.42	51.00	–	23.58
Size	15.42	69.35	–	15.23	15.69	69.07	–	15.23
MoreTypes	15.50	48.33	21.25	14.92	16.92	48.58	19.08	15.42
Prior	13.92	72.83	–	13.25	14.42	70.17	–	15.42
Communication	12.67	72.17	–	15.17	14.00	72.58	–	13.42

Table F1: Percentage of signal precisions across mechanisms and environments. The sample is restricted to the first two parts of the experiment to avoid selection effects. Kolmogorov–Smirnov tests do not reject the null of equal distributions across mechanisms in any environment.

	CV				M			
Treatment	55	60	75	95	55	60	75	95
Baseline	51.48	59.05	–	95.16	50.29	60.14	–	95.16
Distrib	60.59	59.90	–	94.23	57.38	59.64	–	95.05
Size	52.25	60.55	–	95.44	56.05	59.99	–	95.74
MoreTypes	52.69	61.72	74.51	96.09	51.23	58.66	71.18	95.14
Prior	50.30	60.98	–	93.71	54.34	60.33	–	92.43
Communication	57.89	59.24	–	97.25	57.74	61.31	–	96.89

Table F2: Percentage of correct signals by environment, precision level, and mechanism. The sample is restricted to the first two parts of the experiment to avoid selection effects. For each environment–precision cell, we test equality of proportions between mechanisms using chi-squared tests. None of the comparisons are statistically significant.

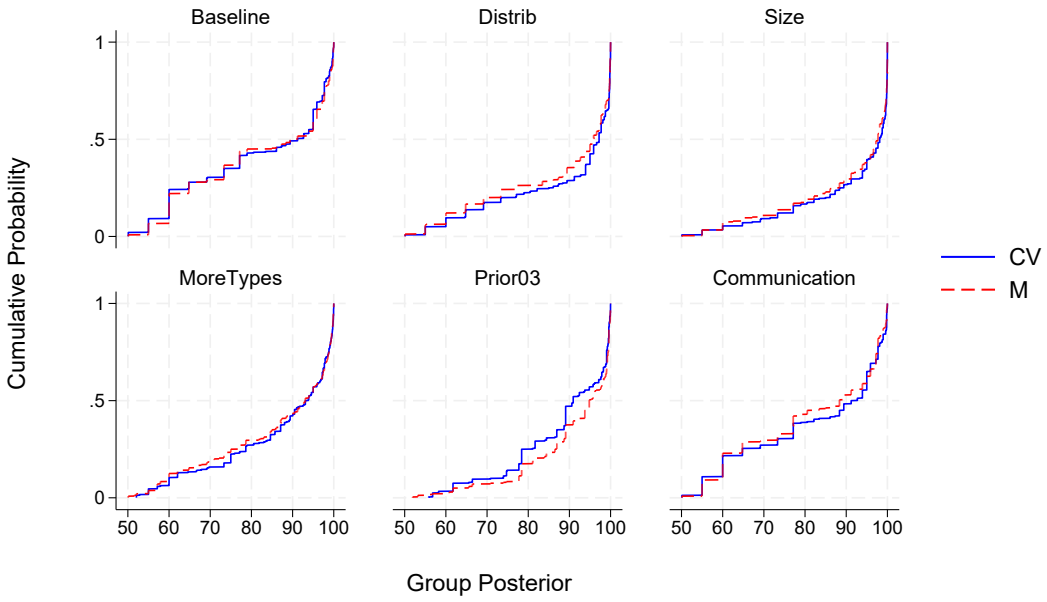


Figure F1: Cumulative distributions of group posteriors for the most likely state, across different environments and mechanisms. Group posteriors are calculated using all signals received within each group. Kolmogorov–Smirnov tests reveal no significant differences between mechanisms in most environments. The only exception is *Asymmetric Prior* ($p = 0.021$), where the average posterior is marginally higher under the majority rule. Note that this imbalance works against Hypothesis 1: the signal draws in that environment were somewhat more informative under MV, which is consistent with it being the only environment in which CV does not outperform MV on the optimal-decision measure (Table 3).

G Experimental Instructions

Thank you for taking part in this experiment. Please read these instructions very carefully. It is important that you do not talk to other participants during the entire experiment. In case you do not understand some parts of the experiment, please read through these instructions again. If you have further questions after reading and hearing the instructions, please raise your hand out of your cubicle: we will then approach you in order to answer your questions personally. Please do not ask anything aloud. The rules are the same for all participants.

During the experiment all sums of money are listed in ECU (for Experimental Currency Unit). How much you earn depends partly on your own decisions, partly on the decisions of other participants, and partly on chance. Your earnings during the experiment will be converted to euros at the end and paid to you in cash. The exchange rate is 40 ECU = 1€. The earnings will be added to a participation payment of 5€. Your personal earnings will be paid to you in cash as soon as the experiment is over.

After the experiment, we will ask you to complete a short questionnaire, which we need for the statistical analysis of the experimental data. The data of the questionnaire, as well as all your decisions during the experiments will be anonymous.

The experiment you are participating in is a group decision-making experiment. At the beginning of this experiment, participants will be randomly and anonymously divided into groups of 5 participants. These groups remain unaltered for the entire experiment, but you will never be told with whom you were interacting in your group.

The experiment consists of three parts. Your earnings and decisions in each part do not depend on earnings and decisions in other parts. The instructions for each part of the experiment will be given at the beginning of that part. Now, we proceed to Part 1 of the experiment.

INSTRUCTIONS PART 1 / 2

These are the instructions for part 1/2. This part is composed of 20 rounds. The instructions are going to be the same for all rounds. [Only for part 2: The instructions for this part will be the same as the instructions of part 1, except for the voting system.]

The Triangle Color

At the beginning of each round, the computer will choose the color of a triangle randomly. Each time, the triangle will be blue ▲ with **50%** probability, and with **50%** probability it will be red ▲. You will not know the color of the triangle, but each member of your group will receive a hint. Your objective **as a group** will be to guess the color of the triangle.

Hint

As a hint of the color of the triangle, each member of your group will observe the color of one ball, drawn from an urn filled with 100 red and blue balls. The composition of blue and red balls will depend on the **color of the triangle** and on **your precision** (which is

either 55, 60, or 95). In particular, if your precision is X, the urn from which you will draw a ball will contain a X number of balls of the color of the triangle, and 100 – X balls of the opposite color. That is,

- If your precision is 55 and…
  - the triangle is blue ▲, then the urn will contain 55 blue balls and 45 red balls.
  - the triangle is red ▲, then the urn will contain 45 blue balls and 55 red balls.
- If your precision is 60 and…
  - the triangle is blue ▲, then the urn will contain 60 blue balls and 40 red balls.
  - the triangle is red ▲, then the urn will contain 40 blue balls and 60 red balls.
- If your precision is 95 and…
  - the triangle is blue ▲, then the urn will contain 95 blue balls and 5 red balls.
  - the triangle is red ▲, then the urn will contain 5 blue balls and 95 red balls.

You will observe the color of one ball, drawn randomly from this urn. A higher precision means that it is more likely that the ball you receive has the same color as the triangle.

At the beginning of each round, you and each of your fellow group members will be assigned a given level of **precision**. In particular, each member will be assigned
- a precision of **55** with a probability of **15%**
- a precision of **60** with a probability of **70%**
- a precision of **95** with a probability of **15%**

Precisions will be assigned independently for each member of the group, so you and the other members of your group might have different precisions. You will learn your own precision, but will not know the precision of the other members of the group.

Together, the color of the triangle and your level of precision determine the number of blue and red balls in your urn. The computer will then draw one of these balls at random, and show it only to you. The computer follows the same procedure for each of the other four members of your group. Each member has his or her own precision, and might observe the same or another ball than the one you observe. But, in each round, the color of the triangle is the same for all members of the group.

Your Voting Decision

[M treatments:] Your voting decision is one of three options: (1) vote Blue, (2) vote Red, or (3) Abstain from voting. Each actual vote counts as one point for that color.

[CV treatments:] Your voting decision is one to (1) vote Blue, or (2) vote Red, and indicate the number of points you allocate to the color you vote for. You can allocate any number between 0 and 20 to the color you vote for, including decimals. You can insert decimals using a point. You can abstain by allocating zero points.

The other members of your group will cast votes in the same fashion.

Voting Rule and Group Decision

The **group decision** will be determined by majority / cumulative voting. In particular, the group decision will be the color which receives the highest number of votes/points. That is,

- If the number of votes/points for blue is **higher** than the number of votes for red, the group decision is blue.
- If the number of votes/points for red is **higher** than the number of votes/points for blue, the group decision is red.

Finally, if the number of votes/points for blue and red is the same, one of them is going to be selected randomly. With 50% probability the group decision will blue, and with 50% probability the group decision will be red.

Your Payoff

If your **group decision** is

- **equal** to the **color of the triangle**, each member of your group earns 100 ECUS.
- **not equal** to the **color of the triangle**, each member of your group earns 0 ECUS.

Information at the end of each Round

Once you and all the other participants have made your choices, the round will be over. At the end of each round, you will receive the following information about the round: the color of the triangle, the total numbers of votes/points for Blue and the total numbers of votes/points for Red [[M:] and abstentions in your group].

[Only in Communication treatments:] Communication before Voting

After receiving your hint, and before voting, you will be able to communicate with the members of your group through a chat window during 30 seconds. In this chat, it is important that (i) you do not identify yourself or (ii) you do not use improper language towards other members of the group. Besides that, you can communicate anything you wish. You can write messages by typing a message in the blue bar in the inferior part of the screen and send it by hitting enter. All messages will be seen by all members of the group and each member will have an anonymous identifier (Player 1, Player 2, …). If you want to leave the chat earlier, please press the button "I'm done" in the bottom right corner. If all participants push this button before the 30 seconds deadline, the communication stage will finish.

Final Earnings in this Part

After the 20 rounds are over, the computer will randomly select 2 of the 20 rounds and you will receive the rewards that you had earned in each of those rounds. Each of the 20 rounds has the same chance of being selected.

Control Questions

Before starting, you will have to fill in some control questions in the computer terminal. Click the button OK after you have answered a question to move to the next question. In case you answer wrongly, an error message will pop out and you will have to answer it again. Once you and all the other participants have filled all the questions the experiment will start.