NBER WORKING PAPER SERIES

# INDIVIDUAL AND COLLECTIVE INFORMATION ACQUISITION: AN EXPERIMENTAL STUDY

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Working Paper 29557
http://www.nber.org/papers/w29557

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2021

We thank Marina Agranov, Roland Benabou, Tim Cason, Stephen Morris, Salvo Nunnari, and Wolfgang Pesendorfer for very helpful discussions and feedback. We gratefully acknowledge the support of NSF grants SES-1629613 and SES-1949381. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 29557
December 2021
JEL No. C91,C92,D7,D8

**ABSTRACT**

Many committees—juries, political task forces, etc.—spend time gathering costly information before reaching a decision. We report results from lab experiments focused on such information-collection processes. We consider decisions governed by individuals and groups and compare how voting rules affect outcomes. We also contrast static information collection, as in classical hypothesis testing, with dynamic collection, as in sequential hypothesis testing. Several insights emerge. Static information collection is excessive, and sequential information collection is non-stationary, producing declining decision accuracies over time. Furthermore, groups using majority rule yield especially hasty and inaccurate decisions. Nonetheless, sequential information collection is welfare enhancing relative to static collection, particularly when unanimous rules are used.

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An online appendix is available at http://www.nber.org/data-appendix/w29557

# 1 Introduction

## 1.1 Overview

Information acquisition precedes a variety of important decisions—juries attend to testimonies, boards of directors research investment prospects, government agencies such as the FDA or the EPA require evidence prior to the approval of new drugs or policies. In practice, information collection takes two broad forms. At times, it occurs in one shot: a volume of evidence is decided upon at the outset, after which a decision is taken. This method is often referred to as *classical hypothesis testing*. At other times, information is collected in increments and responsive to what has already been learned. This dynamic approach, pioneered by Wald (1947), is commonly termed *sequential hypothesis testing*. Although both methods are prevalent and heavily studied in the theoretical literature, there is a dearth of data on how each performs in practice. This paper reports results from an array of lab experiments inspecting both static and sequential information collection. We consider individuals as well as groups operating within various institutions.

There are several clear patterns in our data. Sequential information collection approximates the theoretically predicted decision accuracies, but these decline over deliberation time; static information collection is excessive; and groups exhibit markedly different behaviors than individuals. As theoretically predicted, welfare is higher when information is collected dynamically. However, institutions interact non-trivially with the information-collection protocol: Dynamic information collection with groups using unanimity rule generate the highest welfare, whereas groups using majority rule yield especially hasty and inaccurate decisions.

Our results have implications for institutional design when information collection is an important component of decision making. Using committees, rather than individuals, can be beneficial even if increasing the size of the decision body does not affect the overall information available. However, the rules governing how collective decisions are made and the information protocol in place—static or dynamic—need to be customized in tandem.

At the core of our experimental design is the following decision problem. There are two ex-ante equally likely states, A or B. The state can represent a guilty or an innocent defendant, a safe or unsafe drug, an investment that is profitable or not, etc. Ultimately, each participant must guess the state of the world and gets rewarded when correct. Each state is associated with a Brownian motion. The drift is $\mu$ when the state is A and $-\mu$ when the state is B. The Brownian motion's variance is state-independent. As time goes by, the realized sample path of the Brownian motion becomes increasingly informative about the underlying state. There is a flow cost of information collection. Whenever information collection terminates, participants know the posterior probability that the state is A and submit their guess. Naturally, the optimal guess corresponds to the more likely state. Our focus is on the trade-off pertaining to information collection: waiting longer before making a decision increases accuracy but comes at a cost.

We consider both static and dynamic information-collection procedures. The static setting emulates the classical hypothesis testing setup. Participants determine, at the outset, the time horizon during which they collect information by observing the Brownian path. They then see the path unravel for their desired time, get informed of the ultimate posterior over states, and make their guess. The dynamic setting implements the sequential sampling setup.[1] Participants track the evolution of the Brownian path and can stop at any time to submit their guess.

In our benchmark treatments, decisions are made by individuals, as in the canonical paradigms. Since many applications involve committees as decision-makers, in additional treatments, decisions are made in groups. When in a group, we consider two commonly-used institutions: majority and unanimity. In the static setting, group members all submit their desired information-collection horizon at the outset. Under majority rule, the median time is implemented for the group, whereas under unanimity, the maximal time is implemented. In the dynamic setting, group members decide whether to stop or continue information collection at each point in time. Under majority, whenever two members agree on a guess, information collection terminates for the group, and the majority guess is submitted. Analogously, under unanimity, whenever all members agree on the guess, information collection terminates, and that guess is implemented. In all our group treatments, information is public: group members are privy to the same information. Furthermore, group members receive the same payoff, derived from the common costs accrued during the group's information-collection period and the group's guess accuracy.

Our individual treatments offer a natural benchmark for the basic predictions emerging from the classical statistical information-collection procedures. In the static setting, our parameters are such that the optimal information-collection horizon is 30 seconds. In our experiments, on average,

[1] Specifically, our setup mimics that of Dvoretzky et al. (1953).

individuals choose 42 seconds, a choice that is 40% higher than is optimal. In the dynamic setting, it is optimal to stop whenever there is sufficient confidence in guessing the state, namely when the posterior belief exceeds a time-independent threshold—at that point, the costs of additional information exceed its benefits. Given our parameters, the optimal threshold posterior is 0.81. In our experimental treatments, individuals' mean posterior at decision time is relatively close to that predicted by theory, standing at 0.77. However, participants do not use time-independent thresholds. In fact, we see decreasing thresholds over time, with participants becoming more lenient as time passes.[2] In particular, contrary to theoretical predictions, quicker decisions tend to be more accurate. As we discuss in more detail in our literature review, this observation is consistent with a wide neuroscience literature documenting a similar pattern using perception tasks. However, our data are unique since, by design, we directly observe the posterior probabilities participants see over time. This allows us to show that participants respond to local features of information paths, including recent slope and variation.

To identify group effects, disjoint from preference-aggregation effects, our groups are by design homogeneous. Theoretically, group outcomes should coincide with individual outcomes (in the efficient equilibrium). Specifically, threshold posteriors and waiting times should be independent of the decision rule. Nonetheless, we find that participants in groups behave differently from participants making decisions in isolation, and that this behavior depends on both the information-collection protocol and the voting rule in place.

Majority and unanimity generate different behaviors and outcomes. Groups governed by majority lead to the quickest decisions.[3] We suggest a "demand for agency" channel that may explain this hastiness by groups using majority rule. In our static treatments, hastiness is an improvement, as groups using majority move closer to the optimal outcome. In contrast, in our dynamic treatments, this hastiness is harmful as it leads to excessively low levels of information collection. The consequences of unanimity rule depend on the information-collection protocol. In static treatments, the amount of information collected by groups using unanimity lies between the amount collected by individuals and that collected by groups governed by majority rule. In our dynamic treatments,

[2] While we see participants making decisions at lower posteriors as time goes by within rounds, this pattern does not extend across rounds: participants' mean stopping posteriors are higher in the second half of our sessions.

[3] This is not a purely mechanical artifact of aggregation, which we show by simulating outcomes from hypothetical groups generated by using the data in our individual treatments.

groups using unanimity collect the most information and come close to the theoretical benchmark of decision accuracy. As a result, the highest welfare is observed in the case of groups that collect information sequentially and use unanimity rule.

Sequential information collection always outperforms static information collection in terms of expected welfare. An alternative way to evaluate outcomes is through decision accuracies, abstracting from costs. In our data, decisions made by groups using majority are more accurate when information is collected statically rather than dynamically. This is a consequence of the excessive information collection in our static treatments combined with the hastiness observed in our dynamic majority treatments. Thus, when democratic decision bodies affect a large population, so that information costs are inconsequential to society, static information collection may be beneficial.

### 1.2 Related Literature

The problem of testing statistical hypotheses is an old one. Its origin can be traced back to Thomas Bayes, who provided the well-known formulation of posterior probabilities of event "causes" in the 18th century. Classical hypothesis testing has been used, formally or informally, for centuries, see Stephan (1948). It came of age with the development of statistical hypothesis tests by Neyman and Pearson (1933), who showed that the likelihood ratio test is the most powerful hypothesis test for a given data set. Examples abound for its uses. See, for example, Greene (2018).

Sequential sampling, proposed by Wald (1945, 1947), introduced the idea of collecting data dynamically. With each piece of data, a likelihood ratio test is performed to determine whether more observations are needed to accomplish a desired level of statistical confidence. When data come at a cost, Wald's method offers efficiency gains over its static counterpart—when data is collected in increments, a researcher can condition additional data collection on what had already been observed. Sequential sampling has been used widely to describe how individuals collect information, more on that below, and to guide researchers in the creation of databases, see Dominitz and Manski (2017) and references therein.

Recent theoretical work has investigated how groups approach the deliberative process, linking information acquisition with ultimate decisions. Persico (2004), Martinelli (2006), and Gerardi and Yariv (2007, 2008) investigate environments in which information collection by a committee is "static," reminiscent of classical hypothesis testing. In those models, each individual can acquire a costly signal about a payoff-relevant state. The aggregation process then introduces free-riding motives. This contrasts with our setting, where any information collected by the group is public, with its costs equally shared.

Strulovici (2010), Chan et al. (2018), and Henry and Ottaviani (2019) consider environments in which information collection is sequential: the committee decides at each date whether to continue acquiring costly information, or whether to stop and choose an alternative. In particular, Chan et al. (2018), which our dynamic group treatments mimic, as well as Henry and Ottaviani (2019) and McClellan (2021), build on the literature on sequential hypothesis testing that started with Wald (1947).

In terms of experimental work, there is a large literature that studies how individuals collect and process information statically. Many papers consider the collection of information when agents have non-instrumental motives, for example seeking confirmatory information as in Fischer et al. (2005) or ego-promoting information as in Eil and Rao (2011). Relatively few papers study experimentally how individuals trade off precision of payoff-relevant information and its costs, which is at the heart of the classic hypothesis testing paradigm. Ambuehl and Li (2018) elicit valuations of payoff-relevant information structures. They show that valuation of information under-reacts to increased informativeness, but that individuals value information that may yield certainty particularly highly. Hoffman (2016) uses a field experiment in which business experts are compensated for their guess of the price and quality of actual websites. Participants can acquire a costly signal before deciding. He also finds that participants underpay for strong signals and overpay for weak signals. Our static treatments add to this literature by illustrating how both individuals and groups resolve the accuracy-cost trade-off.[4] To our knowledge, there is little experimental work that speaks directly to the sequential sampling setup.[5] Several papers inspect individual dynamic search behavior experimentally, see Gabaix et al. (2006), Brown et al. (2011), Caplin et al. (2011), and references therein. In these experiments, participants also spend resources over time in the hopes of identifying a good alternative. However, the underlying optimization problem is quite different from ours. Chen

[4]Several studies inspect information collection in strategic settings different from ours. Elbittar et al. (2020) and Bhattacharya et al. (2017) consider information aggregation settings in which individuals acquire private information, Szkup and Trevino (2021) explore information collection in the context of global games, while Gretschko and Rajko (2015) focus on auctions.

[5]Canen (2017) provides some field evidence on voters sequentially collecting information prior to elections. Interestingly, the idea of using sequential experimental *designs* has been suggested in various contexts, see El-Gamal and Palfrey (1996), Chapman et al. (2019), Imai and Camerer (2018), and references therein.

and Heese (2021)'s experiment resembles our individual dynamic treatment. However, their focus is on the ethical valence of the alternatives.

The neuroscience literature has produced a rich body of work that inspects binary perceptual tasks. Response times are often interpreted as costly, turning the problem into a sequential sampling one, often termed the drift-diffusion model. Much of the focus of this literature concerns the association between correct choice rates and response times, see for instance Swensson (1972), Luce et al. (1986), Ratcliff and Smith (2004), and Ratcliff and McKoon (2008). The main finding emerging from this literature is that quick decisions tend to be more accurate. This insight is in line with our observation of declining thresholds in the dynamic treatments: as time passes, our participants stop information collection with less certainty on the correct choice. An important contrast with these studies is that we observe—in fact, provide—the posterior probability that any choice is correct over time. This allows us to speak directly to new theories of dynamic choice that have emerged recently, see Baldassi et al. (2020) and Fudenberg et al. (2018).

# 2 Experimental Design

A description of the interface and sample instructions are available in the Online Appendix. At the core of our experimental design is the choice of the amount of information to acquire prior to making a binary decision. There are two possible states: A and B. Although labeled neutrally in the lab, these can stand for a guilty or innocent defendant in the jury context, a good or bad policy in the political context, a profitable or unprofitable investment in a finance context, etc. At the start of each period, one of the states is chosen at random with probability 1/2. Participants ultimately need to guess the state and are paid according to the correctness of their guess. In the lab, participants receive $2 for a correct guess and nothing otherwise.

Before making their guess, participants have access to information that evolves according to a continuous-time Weiner process. The process has state-independent instantaneous variance $\sigma^2$, but state-dependent drift. When the state is $A$, the drift is $\mu$; When the state is $B$, the drift is $-\mu$. To produce reasonable expected round durations, throughout our treatments, $\mu = 0.84$ and $\sigma^2 = 1$. Naturally, our experimental software provides an approximation of the continuous setup, where the interface is updated five times a second.

There are two dimensions that we vary across our treatments: whether information acquisition decisions are static or sequential and whether choices are made by individuals, groups using majority rule, or groups using unanimity rule.

In what follows, we start by describing our sequential treatments. The design of these treatments guided our design of the static treatments, which are described next.

**Sequential Sampling** Our dynamic treatments mimic the sequential-sampling environment of Dvoretzky et al. (1953). In these treatments, participants observe information evolve over time and, at each instant, can guess $A$, $B$, or wait for further information by choosing $W$. Information comes at a flow cost of 40 cents a minute.

In the treatment in which individuals make decisions on their own—the *individual dynamic* treatment—a round ends as soon as a participant makes an $A$ or $B$ guess.

In our group treatments, participants are randomly matched to form groups of three in each round. Information is public: all individuals in the group observe the same information. A round ends as soon as a quorum of $q$ individuals agrees on an $A$ or $B$ guess. In the *majority dynamic* treatment, $q = 2$. In the *unanimity dynamic* treatment, $q = 3$. As long as a quorum has not been reached, participants can change their decisions between $A$, $B$, and $W$ at any time. Throughout, participants can see the choices of other group members.

**Static Sampling** Our static treatments mimic the setting of the *classical hypothesis testing* environment. At the beginning of each round, participants decide on the amount of time they want to spend collecting information. As in the dynamic treatments, information costs are fixed at 40 cents a minute.

When individuals make decisions independently—the *individual static* treatment—they observe the information evolve for the amount of time that they chose.[6] Their guess is then automated to reflect the state that is more likely given the information collected: either $A$ or $B$.[7]

Our static-sampling group treatments are analogous to those corresponding to the dynamic treatments. In each round, participants are randomly re-matched into groups of 3. At the outset of

[6] This design was chosen for two reasons. First, we wanted to maximize comparability with the sequential-sampling treatments. Second, we wanted to offer participants sufficient learning opportunities.

[7] The guess is automated in order to reduce noise in our data. Because participants' guesses in the individual dynamic treatment best respond to the information 98% of the time, it is unlikely this restriction impacts our qualitative results.

each round, participants submit simultaneously their desired waiting time. In the *majority static* treatment, the resulting group waiting time is the median desired waiting time of group members: this is the minimal time at which a majority of group members would agree to stop information collection. It is the maximal desired waiting time of group members in the *unanimity static* treatment: this is the minimal time at which all group members would agree to stop information collection. As in the individual treatment, participants observe the information evolve for the amount of time chosen by the group. The group guess, $A$ or $B$, is again automated.

**Feedback and Payments** In all treatments, the feedback at the end of each round contains participants' payoffs and other group members' choices whenever relevant. In groups, all members are paid the same amount, incorporating the accuracy of the group's guess and information-acquisition costs.

Each treatment was preceded by two practice rounds, followed by 30 payoff-relevant rounds. Participants were ultimately paid for 20 randomly-selected rounds out of these 30.

**Information Processes** The 30 information processes participants experienced in the experimental rounds were identical across treatments. To select these processes, we randomly generated 15 sample paths with the parameters specified above. These processes are "representative" in that the mean, median, and five quintiles of the theoretically-optimal sequential stopping times match those of the underlying distribution (see the following section for a description of the theoretical predictions). These processes correspond to the first 15 real rounds in each treatment. The last 15 processes in each treatment were derived by generating the reflected "mirror images" of the first 15 processes. Namely, whenever the realized state in the original process is $A$ (or $B$), it is $B$ (or $A$) in the reflected process. Furthermore, at any time $t$, if the original process indicates a probability $p$ that the state is $A$, the reflected process indicates a probability $1-p$ that the state is $A$. The reflected processes were used in the same order as the original processes. In that way, participants effectively faced the same 15 decision problems twice during a session. This design element allows us to evaluate learning in a highly controlled fashion.[8]

The evolution of a Weiner process provides continuous information on the likelihood of either

[8] As we soon describe, the evolution of the process was depicted through a uni-dimensional scale capturing posterior probabilities updated over time. Identifying repetitions is extremely unlikely: it would require the memorization of many ordered values and the realization that they are mirrored.

state. Nonetheless, the Bayesian calculus necessary to deduce this likelihood is non-trivial. The difficulty this calculus introduces is orthogonal to our investigation.[9] To mitigate the impacts of participants' limitations in statistical analysis, our design directly displays the evolution of the *probability* that the state is $A$ (or $B$).

**Auxiliary Elicitations** At the end of each session, participants completed two risk-elicitation tasks as in Gneezy and Potters (1997). Namely, participants were provided with 200 tokens that they had to allocate between a safe investment, returning token for token, and a risky investment with a mean higher than 1 and a non-trivial variance (e.g., one paying 2.5 the amount invested with probability 50%). In addition, participants took part in two dictator-games, one in which the amount of tokens transferred was translated $1:1$ and one in which the amount of tokens transferred was doubled for the recipient. Participants were paid for one randomly-chosen risk-elicitation task and one randomly-chosen dictator game.[10]

**Summary** The experiments were run at the Princeton Experimental Laboratory for the Social Sciences (PExL) with 254 participants. We conducted at least four sessions for each group treatment, with at least 12 participants in each. Table 1 summarizes our treatments and the corresponding volume of participants.[11] The experimental software was programmed using oTree (Chen et al., 2016).

Table 1: Participants and Rounds

| | Dynamic | | Static | |
|---|---|---|---|---|
| | Participants | Rounds | Participants | Rounds |
| Individual | 34 | 1,020 | 31 | 930 |
| Majority | 48 | 480 | 48 | 480 |
| Unanimity | 48 | 480 | 45 | 450 |

## 3 Theoretical Predictions

We now outline the theoretical predictions for our various treatments. For details, see Dvoretzky et al. (1953) or Chan et al. (2018).

[9] It is well known that lab participants are frequently challenged by statistical updating, see for instance the survey of Benjamin (2019).

[10] We elicited duplicate responses to allow for measurement-error correction as suggested in Gillen et al. (2019).

[11] Given our grouping protocol, the number of rounds per participant in our group treatments is three times lower than in the individual treatment.

We consider the setting described in our experimental design. An agent assesses which one of two ex-ante equally likely states, $A$ or $B$, are realized. Information follows a Weiner process with a variance of 1. When the state is $A$, the process has drift $\mu = 0.84$; When the state is $B$, the process has drift $-\mu = -0.84$. Tracking this information comes at a flow cost of $c$. The agent guesses the state that is more likely once information collection terminates. For ease of exposition, we normalize the reward for an ultimately correct guess of the state to be 1. With this normalization, the flow cost corresponding to that used in our experiments is $c = 0.2$.

It is convenient to define $\mu' \equiv 2\mu^2$. The agent's posterior belief is then given by a Wiener process, with drift $\mu'$ and instantaneous variance $2\mu'$ in state $A$, and drift $-\mu'$ and instantaneous variance $2\mu'$ in state $B$. A higher value of $\mu'$ indicates a more informative process. For our parameters, $\mu' = 1.4$.

### 3.1 Static Treatments

In order to obtain the optimal wait time in the static setting, we need to compute the probability of guessing the true state correctly for any chosen time $t$. This probability can be shown to be given by the following expression.[12]

$$\int_0^\infty \frac{1}{\sqrt{4\pi\mu' t}} e^{-\frac{(x-\mu' t)^2}{4\mu' t}}\, dx = \frac{1}{2}\left(\operatorname{erf}\left(\frac{\sqrt{\mu' t}}{2}\right) + 1\right).$$

In the static setting, a risk-neutral agent maximizes:

$$\max_t \frac{1}{2}\left(\operatorname{erf}\left(\frac{\sqrt{\mu' t}}{2}\right) + 1\right) - c\,t.$$

The optimal wait time is then:

$$t^* = \frac{2\,W\left(\frac{(\mu')^2}{32\pi c^2}\right)}{\mu'},$$

where $W(\cdot)$ is the Lambert W function (i.e., $W(x) = w$ if and only if $x = we^w$).

With our parameter values $t^* = 0.49$. Since one unit of time in the lab is one minute, this

[12] If $\Phi(\cdot)$ denotes the cumulative distribution function of the standard normal, the error function $\operatorname{erf}(\cdot)$ is defined so that $\operatorname{erf}(x) = 2\Phi(\sqrt{2}x) - 1$.

optimal wait time translates to 29.58 seconds.[13]

Consider now a group of $n > 1$ homogeneous agents who choose their desired search times simultaneously. The group then collects information for a duration corresponding to either the median or the maximal specified time. As before, the group guess corresponds to the more likely state given the posterior that is generated by the collected information. Group members are (identically) rewarded as in the one-agent setting.

The utilitarian efficient equilibrium for the group corresponds to the optimal search time described above, namely 29.58 seconds. Furthermore, this choice is weakly dominant for any agent, regardless of the strategies other agents in the group utilize.

### 3.2 Sequential Treatments

One of the main contributions of Wald (1945) and the continuous-time counterpart of Dvoretzky et al. (1953) is to demonstrate that, in the sequential-sampling setting, an optimizing agent uses a simple threshold policy. Namely, at any time $t$, the agent calculates the log-likelihood ratio $\theta_t = \log\left(\frac{\Pr[A]}{\Pr[B]}\right)$. The optimal policy specifies a pair of cutoffs $(g, G)$, with $G \geq g$, such that the agent stops information collection and guesses the state is $A$ whenever $\theta_t \geq G$. Similarly, the agent stops information collection and guesses the state is $B$ whenever $\theta_t \leq g$.

For $\theta \in [g, G]$, let $u(\theta|g, G)$ represent the expected payoff from the deliberation process. A similar derivation to that of Chan et al. (2018) yields:[14]

$$\begin{aligned} u(\theta|g,G) = & \frac{e^G(e^\theta - e^g) + (e^G - e^\theta)}{(1+e^\theta)(e^G - e^g)} \\ & - \frac{c}{\mu'} \frac{(G-\theta)(e^{G+\theta} + e^g) + (\theta - g)(e^{g+\theta} + e^G) - (G-g)(e^\theta + e^{G+g})}{(1+e^\theta)(e^G - e^g)}. \end{aligned}$$

The corresponding first-order condition with respect to the lower boundary is then:[15]

$$\frac{\partial u(\theta|g,G)}{\partial g} = \frac{-(e^G - e^\theta)}{(1+e^\theta)(e^G - e^g)^2}\left[e^g(e^G - 1) - \frac{c}{\mu'}\left((G-g)e^g(e^G-1) + (e^G - e^g)(1-e^g)\right)\right] = 0.$$

[13] A discussion of this setting in the presence of risk aversion is presented in the Online Appendix.

[14] Our formulation here differs from that of Chan et al. (2018) in that they consider exponentially discounted utilities, whereas we consider flow costs of time spent on information collection. This modification simplifies the experimental interface.

[15] The first-order approach is indeed valid, we omit details for the sake of brevity.

This condition shows that the cutoffs satisfying the first-order condition do not depend on the current log-likelihood ratio $\theta$. Thus, solutions are stationary.

Because the problem is symmetric, the solution satisfies $g = -G$. The optimal value of $G$ can then be determined by the following implicit function:

$$c\left(2e^{G}G + e^{2G} - 1\right) - e^{G}\mu' = 0$$

With $\mu' = 1.4$ and $c = 0.2$, the numerical solution for the optimal boundary is $G^* = 1.46$. Translated into probabilities, this value becomes $\frac{e^{1.46}}{1+e^{1.46}} = 0.81$. Thus, in the dynamic version, a risk-neutral agent should wait until the probability of the most likely state is 81%.

Consider now a group of $n > 1$ homogeneous agents. At each point in time, each agent decides whether she would like to stop and guess $A$, stop and guess $B$, or wait. The group continues information collection until either a majority or a unanimity of agents in the group choose to guess the same state.

The utilitarian efficient equilibrium for the group corresponds to the optimal search policy described above, namely utilizing a threshold of 81%. Furthermore, as long as agents use symmetric cutoff policies, this choice is a best response for any agent, regardless of the (potentially different) cutoffs chosen by other agents in the group.

# 4 Approach to Data Analysis

As may be expected, participants' behavior changes during early experimental rounds as they learn about the problem. We see no evidence for substantial learning in later rounds. For details, see the Appendix. Throughout the paper, we present figures aggregated across all experimental rounds as those displayed appear virtually identical when we use either the full data or the last half of our sessions. Regression results are presented for data corresponding to all rounds in the text, and for the last 15 rounds in the Online Appendix. The qualitative messages remain the same.[16] We also discuss individual- and session-level heterogeneity in the Appendix.

Risk attitudes and altruism proclivities do not appear to play an important role in explaining

[16]Recall that, in our design, the processes participants encounter in the first and second half of each session are equivalent.

patterns in our data, even after measurement-error correction. We therefore do not include data from these elicitations in our main specifications. See the Online Appendix for related analyses, which also allow for various levels of clustering.

## 5 Broad Patterns of Behavior

Table 2 displays an aggregate overview of some of our results. It displays the estimated mean of the posteriors with which the pivotal vote has been cast, and the estimated mean time taken to cast the pivotal vote. As can be seen, our individual and majority dynamic treatments lead to less accurate decisions than theoretically predicted, whereas the unanimity dynamic treatment yields outcomes that are extremely close to those theory predicts.[17] Furthermore, the majority dynamic treatment corresponds to the least amount of waiting, an observation we shall return to.

Differences between threshold posteriors in the data and those predicted by theory may, at first blush, appear small. Nonetheless, these differences translate to fairly large differences in wait times. For instance, the unanimity dynamic treatment leads to double the wait time in majority dynamic treatment. This is a direct consequence of the convexity of precision costs— the marginal time required to attain a given increase in precision is increasing in the level of the precision.

Static treatments yield excessive waiting relative to that predicted by theory. Again, the majority-rule treatment generates the hastiest decisions, though differences are not significant.

Contrary to theory, mean decision times are longer in the static treatments than in the dynamic treatments for both individuals and groups using majority. Moreover, the differences between mean posteriors at decision times in static and dynamic treatments are not as large as theory predicts.

Figure 1 depicts the evolution of posteriors and the choices made in each of our 15 processes in the individual treatments, both static and dynamic. Our use of identical processes across treatments allows for such a direct comparison. In order to simplify the presentation, each panel aggregates observations from two reflected processes (for example, panel 1 corresponds to the first and sixteenth process, panel 2 to the second and seventeenth process, etc.). The Figure illustrates the point at which individuals "pulled the trigger" and the pivotal vote was cast.

[17] Since the path of posteriors intersects the stopping threshold from below, noise would lead to an underestimate of the "true" threshold in the mind of participants. In the Appendix, we allow for such noise and show that the resulting bias is very small in our data.

Table 2: Aggregate Behavior

| | Dynamic Treatment | | | | Static Treatment | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean Posterior | | Mean Time Waited | | Mean Posterior | | Mean Time Waited | |
| | All Rounds | Last 15 | All Rounds | Last 15 | All Rounds | Last 15 | All Rounds | Last 15 |
| Individual | 0.77 | 0.78 | 33.56 | 37.55 | 0.75 | 0.75 | 41.69 | 40.45 |
| | (0.003) | (0.005) | (0.687) | (1.12) | (0.004) | (0.006) | (0.561) | (0.824) |
| Majority | 0.73 | 0.73 | 23.07 | 24.38 | 0.74 | 0.74 | 36.25 | 34.48 |
| | (0.002) | (0.003) | (0.335) | (0.51) | (0.003) | (0.005) | (0.326) | (0.515) |
| Unanimity | 0.82 | 0.84 | 46.71 | 53.68 | 0.76 | 0.75 | 40.46 | 37.77 |
| | (0.002) | (0.003) | (0.724) | (1.11) | (0.004) | (0.005) | (0.343) | (0.547) |
| Theory | 0.81 | | 39.03 | | 0.72 | | 29.58 | |

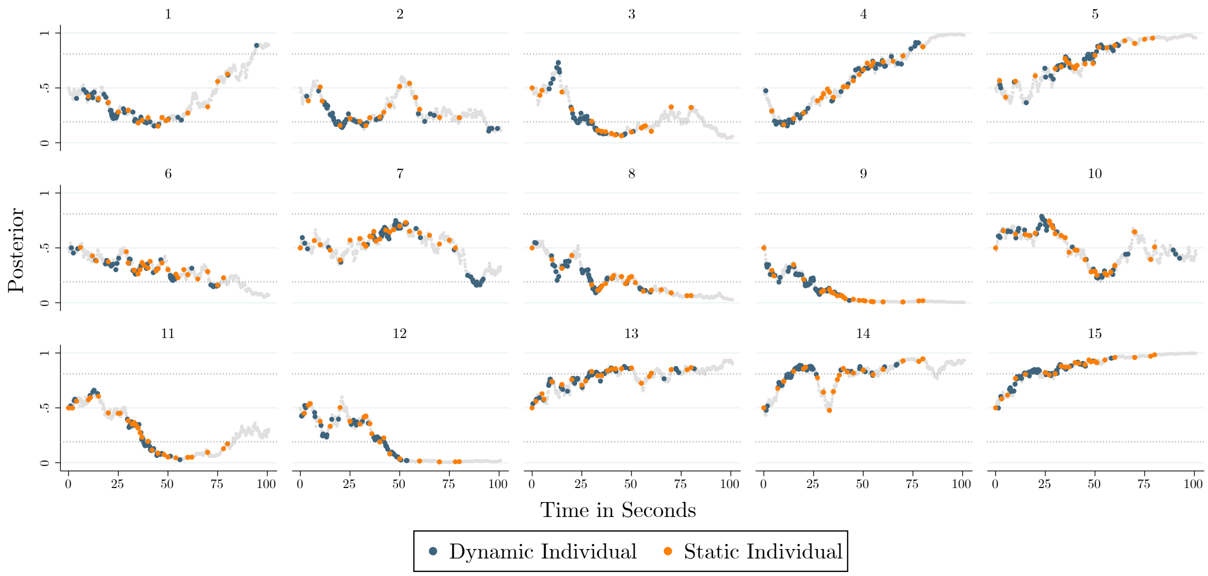
Standard errors in parentheses

The Figure suggests some important themes that appear in our more detailed analysis below. First, it is apparent that decisions are heterogeneous, corresponding to a spectrum of stopping posteriors. Second, many observations are close to optimal. In fact, many participants stop information collection at the theoretically-predicted posterior accuracy (corresponding to the horizontal dashed lines within each panel). In the dynamic setting, participants clearly respond to information in that decisions are more clustered around higher posteriors. Third, individuals in the dynamic treatment become more lenient, requiring less accuracy to stop, the longer they wait: they display decreasing thresholds. Consider, for example, process 10. Several individuals decide late in the process, when posteriors are close to 50%, despite choosing not to stop at earlier points, when posteriors were close to 80%. Last, because in the static treatment individuals cannot condition their choices on the history, the resulting decision posteriors are far more dispersed.[18] For instance, in processes 9 and 12, some static choices take place at extreme posteriors (close to zero) that had already stabilized for some time. Earlier stopping would have been preferable if agents had been able to condition their behavior on the history. In contrast, in processes 2 (around 50 seconds) and 14 (around 35 seconds), some static decisions terminate at posteriors of around 50% in regions where no dynamic decisions terminate.

The analogous figure for our majority and unanimity treatments appears in the Appendix. Results are similar: we see more leniency over time in the dynamic treatments, and more decisions at extreme posteriors—either low or high—in the static treatments.

[18] We return to a discussion of posterior dispersion in Section 8.

Figure 1: Pulling the Trigger: Individual Treatments



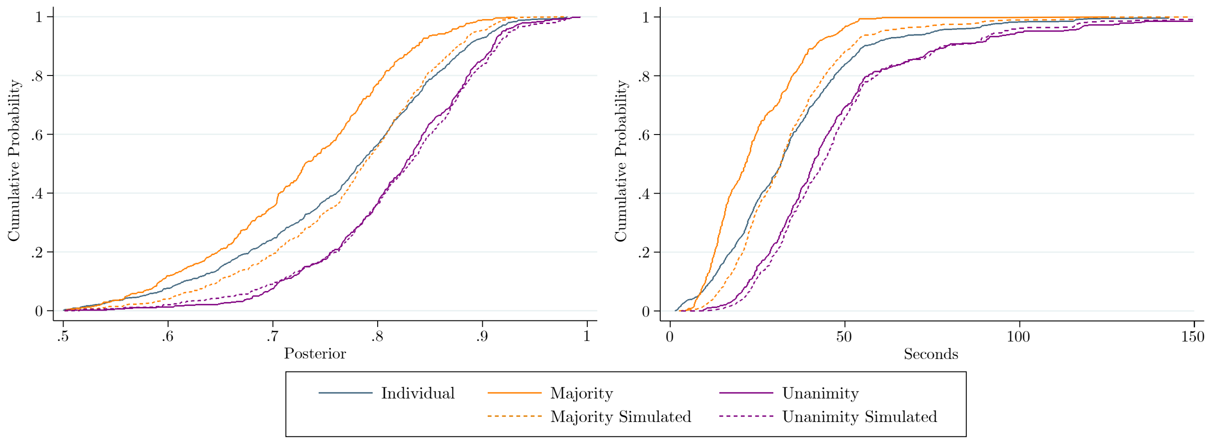

In what follows, we analyze the behavior that underlies these initial observations. The next section describes behavior in our dynamic treatments. The section that follows offers a comparison with their static counterparts.

# 6 Sequential Information Collection

## 6.1 The Impacts of Decision Procedures

For each of our dynamic treatments, Figure 2 displays the cumulative distribution functions of the posteriors when the decision was made (the left panel) as well as the associated decision times (the right panel). We see a substantial impact of the governing decision rule. Distributions can be ordered via first order stochastic dominance, with the unanimity dynamic treatment yielding the highest-accuracy decisions and taking the longest to conclude, and the majority dynamic treatment yielding the least-accurate and hastiest decisions. In particular, the averages presented in Table 2 are not principally driven by outliers.

Figure 2: Dynamic Treatment CDFs




As discussed in Section 3, theory suggests we should not observe any differences in outcomes among voting rules. However, theory also predicts a single threshold posterior, whereas we observe substantial heterogeneity in behavior in the individual treatment. Given this heterogeneity, it is natural to ask whether the differences we observe across our dynamic treatments are simply a mechanical consequence of the grouping of three random individuals that respond heterogeneously to the task at hand. Specifically, groups governed by majority rule decide according to the second order statistic, whereas groups governed by unanimity rule decide according to the third order statistic. To assess whether the differences we see among these treatments are purely mechanical, we simulate hypothetical groups of three participants by drawing data from our individual treatment.[19]

Figure 2 presents the resulting cumulative distribution functions from these simulated groups, alongside the distributions we observe in our data. The additional accuracy granted by groups using unanimity appears to be a purely mechanical phenomenon.[20] In contrast, groups using majority rule yield substantially less accurate and hastier decisions than those of simulated groups using majority, suggesting that hasty majority choices are not the pure consequence of a mechanical aggregation effect.[21] We will discuss this phenomenon in more detail in Section 6.4 below.

[19]Specifically, for each round, we randomly group the 34 participants in our individual treatment into 11 groups of 3 participants, randomly discarding one. We do so $1,000$ times. Across all 30 rounds, $330,000$ groups are therefore simulated.

[20]A two-sided Kolmogorov-Smirnov test fails to reject the hypothesis that these distributions, the simulated and observed unanimity group decisions, are identical. One possible concern is that observations generating these figures are correlated. This raises questions about the validity of standard statistical tests for comparing these distributions; see additional analysis in the Online Appendix. We soon use regression analysis, with adequate error clustering, to statistically determine what affects decisions.

[21]A two-sided Kolmogorov-Smirnov test rejects the hypothesis that these distributions are identical, though the

## 6.2 Non-stationary Behavior

We now assess the determinants of when participants decide to terminate information collection. Theoretically, the probability of voting should be 1 when the posterior reaches its theoretical threshold value of 0.81, and 0 for any lower posterior. In particular, the probability of voting should respond only to observed posterior probabilities, not to the time that has passed, to features of the sample path, or to choices of other group members.

We describe the behavior of our participants by presenting results from a probit regression. The left-hand side variable captures whether a participant has voted, and the main explanatory variable is the posterior. In the left panel, as an additional explanatory variable we include the time (in minutes) to allow for time dependence in voting outcomes. To allow for the possibility of path dependence, in the right panel, we also include features of the sample paths. Specifically, we divide each round into (non-overlapping) 5-second time intervals. Within each 5-second window, we record our left-hand side variable—whether a vote was cast; our explanatory variables—the posterior and time at the end of the window; as well as the slope and standard deviation of the sample path.[22] We utilize data up until participants cast their individual votes. Table 3 reports the corresponding coefficient estimates.

Table 3: Probit Regression

| | $P(Vote)$ | | | | | |
|---|---|---|---|---|---|---|
| | Individual | Majority | Unanimity | Individual | Majority | Unanimity |
| *Posterior* | 5.357*** | 5.149*** | 5.690*** | 5.071*** | 3.787*** | 5.463*** |
| | (0.400) | (0.426) | (0.406) | (0.402) | (0.478) | (0.427) |
| *Time* | 0.242** | 0.798*** | 0.333*** | 0.313*** | 0.673*** | 0.328*** |
| | (0.120) | (0.179) | (0.111) | (0.109) | (0.189) | (0.110) |
| *Slope* | | | | 0.137*** | 0.132*** | 0.0475* |
| | | | | (0.0360) | (0.0338) | (0.0253) |
| *Standard Dev* | | | | -0.142 | 0.626** | 0.350* |
| | | | | (0.212) | (0.275) | (0.203) |
| *Constant* | -4.980*** | -4.626*** | -5.263*** | -4.891*** | -3.880*** | -5.192*** |
| | (0.344) | (0.291) | (0.312) | (0.332) | (0.367) | (0.335) |
| *N* | 7865 | 6772 | 11113 | 6824 | 5301 | 9660 |

Standard errors in parentheses
Individual-level clustering
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The left panel of Table 3 indicates that decisions to cast a vote are responsive to posteriors,

caveat regarding such tests still holds. We use regression analysis below to make a stronger statistical case for the difference between group treatments.

[22] The slope corresponds to the average posterior gain per minute calculated using the 5-second window. To make the ranges of posteriors and standard deviations comparable across 5-second windows, we normalize the standard deviation through a multiplication by 5. Different time windows, of 3,...,7 seconds, yield similar results. So do regressions focusing on the last 15 rounds of each session. See the Online Appendix for details.

with higher posteriors naturally leading to increased voting probabilities. In addition, *stopping decisions are not stationary*: controlling for posteriors, the more time passes, the more likely agents are to make a decision. For example, in the individual treatment, waiting for one additional minute is equivalent to an approximate increase of 4.5 percentage points in the observed posteriors ($0.045 \times 5.357 \sim 0.242$): the probability an individual casts a vote with a hypothetical posterior of 0.80 at the outset approximately coincides with the probability an individual casts a vote with a posterior of 0.76 after one minute has passed. Furthermore, time appears to have a stronger impact on the likelihood of making a decision when groups use majority rule.

In the right panel, we also include features of the sample paths. The coefficients corresponding to posteriors and time passed change only slightly. In addition, the coefficient corresponding to *Slope* is positive and significant. This implies that after a brief period in which posteriors increase rapidly, a decision is more likely, particularly for individuals and groups using majority rule. The coefficient corresponding to *Standard Dev* is barely significant, however. That is, recent variation in posteriors has a limited effect.

For an alternative approach in which we analyze observed stopping posteriors directly, see the Appendix. Similar conclusions emerge. Importantly, although within a round later decisions are made at lower posteriors, this is not the case between rounds. In fact, the average stopping posteriors, as well as durations, are higher in the second half of sessions.[23]

Our finding that the probability of casting a vote is increasing in time, even while controlling for the posterior, is connected to the drift-diffusion model (DDM)—see, e.g., Swensson (1972), Luce et al. (1986), Ratcliff and Smith (2004), and Ratcliff and McKoon (2008). As mentioned above, this literature finds that quick decisions tend to be more accurate. An important contrast with these studies is that we observe—in fact, provide—the posterior probability that any choice is correct at each point in time. This allows us to speak directly to new theories of dynamic choice that have emerged recently, see Baldassi et al. (2020) and Fudenberg et al. (2018). The explanation provided by Fudenberg et al. (2018) for the relationship between speed and accuracy relies on decision-makers being uncertain about their payoffs, which translates into uncertainty about the process and leads to optimal non-stationary behavior. In our setting, the problem is inherently

[23]Recall that the second 15 rounds utilize the same processes as the first 15 rounds, only mirrored. Thus, the increase in the observed stopping posteriors cannot be an artifact of features of the processes themselves.

stationary, and the only uncertainty is about which of two drifts governs the process. In our data, experience does not significantly reduce the degree to which thresholds are decreasing, suggesting that it is unlikely that "subjective uncertainty" about the process is what drives this behavior. Furthermore, as mentioned, stopping behavior responds to the sample path itself, behavior that cannot be explained with a pre-determined (potentially time-variant) threshold, as in Fudenberg et al. (2018).[24]

Our non-stationarity results are also related to the results of Brown et al. (2011). They experimentally study a stationary job-search problem with a known distribution of wage offers. Reported reservation wages decrease over time. They consider two potential explanations for this phenomenon: non-stationary time discounting, and a "sunk-cost fallacy" whereby agents set reservation wages in response to cumulative costs. Both effects are present in their treatments, although the first is more pronounced. As mentioned above, our participants also react to features of the process itself, a phenomenon that could not be examined in Brown et al. (2011). Furthermore, one of our findings is incompatible with agents placing exaggerated weight on cumulative costs: as we documented in Section 5, agents wait excessively in our static treatments.

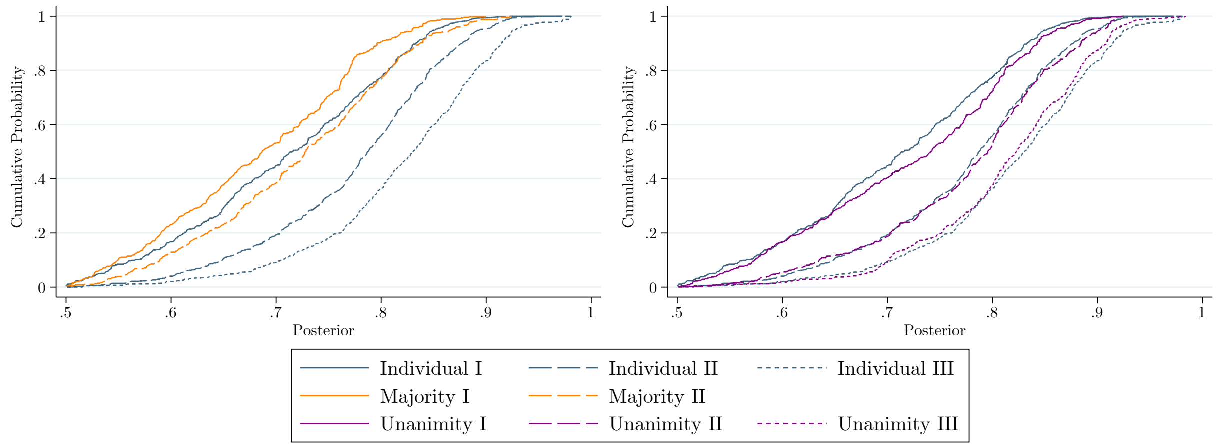
### 6.3 Voting First, Second, and Third

We now discuss how the patterns of behavior compare between our treatments. In Figure 3 we present the distributions of posteriors corresponding to the first and second votes in the majority treatment (in the left panel), and the distributions of posteriors corresponding to the first, second, and third votes in the unanimity treatment (in the right panel).[25] Alongside these distributions, we present analogous distributions for simulated groups of three generated from the individual treatment via the procedure described in Section 6.1.

[24] McClellan (2021) derives non-stationary threshold posteriors as the consequence of agency frictions. Strack and Viefers (2021) report on path dependence in a related search setting. See also references therein.

[25] In a group setting, as long as a pivotal vote has not been cast, participants can change their vote, from say A, to W, or to B. However, in both group treatments, roughly 85% of rounds end with each participant casting at most one vote. Therefore, the first votes in a group yield a good approximation of order statistics for the group treatments. They also offer the theoretically valid way to compare group and individual treatments—in the individual treatment, the first vote terminates information collection.

Figure 3: Dynamic Treatment CDFs by Vote Order




An implication of our discussion in Section 6.1 is that the third-order statistic from the individual simulated treatment is very close to the distribution of the third and pivotal voter from the unanimity treatment. The right panel of Figure 3 confirms this finding and reveals that this similarity also holds for the first and second voter. Therefore, this figure reinforces the idea that individual voter behavior under unanimity is very similar to behavior of individuals deciding in isolation, and that the differences in outcomes under unanimity are exclusively due to the aggregation rule acting on heterogeneous individuals. For the majority treatment, the left panel of Figure 3 demonstrates that hasty behavior is not only a characteristic of the second (and pivotal) voter; the first voter appears to be hasty as well. Both the first- and second-order statistics from the simulated individual treatment stochastically dominate the observed distributions corresponding to the first and second voters from the majority treatment. Interestingly, the distribution of second voters under majority is very similar to the distribution of first voters in the individual simulated treatment, a point we soon return to.

As we show in the Appendix, analysis of individual behaviors suggests no clear cluster of "types," although individual choices exhibit substantial heterogeneity in both their means and their variability across rounds.[26]

[26]We do not see substantial persistence in vote orders of individuals: there are very few participants who are always first, always second, or always third to vote. We also provide more detailed analysis of the individual vote patterns. We illustrate that, in our group majority treatments, participants vote for W, which corresponds to waiting for further information, far less often than in groups using unanimity.

## 6.4 Hasty Majority Decisions and a Demand for Agency

Why are decisions under majority so hasty while unanimity decisions are not? We explore one possible mechanism generating hasty majority decisions: a demand for agency. Prior work suggests that individuals have a taste for agency, the ability to influence outcomes. See, for instance, Fehr et al. (2013), Bartling et al. (2014), and Pikulina and Tergiman (2020). When operating alone, or in a group using unanimity, agency is guaranteed—in both cases, a decision can only be made after each participant has cast a vote. In contrast, under majority rule, the group decision is made by two out of three group members, those who are first to vote. Thus, agency eludes a participant who pursues a more demanding threshold. There is, then, a non-trivial trade-off between desired accuracy and hastiness for the sake of agency.

To evaluate the plausibility of a demand for agency, we start by inspecting remaining voters' responses to the first vote being cast. Our interest is in examining how behavior compares across treatments. Under majority rule, a demand for agency would introduce a race between the remaining two group members and thereby reduce the posterior at which the second vote is cast. Table 4 displays the results of a regression in which, within each combination of treatment, group, and round, we calculate the difference between the posterior at which the second vote was cast, and the posterior at which the first vote was cast.

Table 4: Difference in Posterior: Second vs First Voter

| | $(p_2 - p_1)$ |
|---|---|
| *Constant* | 0.193*** |
| | (0.0176) |
| $d_M$ | -0.154*** |
| | (0.0338) |
| $d_U$ | -0.00695 |
| | (0.0205) |
| $p_1$ | -0.607*** |
| | (0.0639) |
| $p_1 \times d_M$ | 0.164*** |
| | (0.0548) |
| $p_1 \times d_U$ | 0.00585 |
| | (0.0288) |
| $N$ | 330960 |

Standard errors in parentheses
Process-level clustering
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

The variables $d_M$ and $d_U$ are dummy variables corresponding to the majority and unanimity treatments, respectively. The variable $p_1$ stands for the posterior associated with the first vote cast in the group. Since $p_1$ can take values between 0.5 and 1, we re-normalize the values of $p_1$ by subtracting 0.5. Thus, the intercept corresponds to the *additional* accuracy required by the second voter when the first voter casts a vote with a posterior of 0.5. The variables $p_1 \times d_M$ and $p_1 \times d_U$ correspond to the interactions between $p_1$ and the corresponding treatment dummies, allowing for different slopes across treatments.[27] To calculate the difference between the posteriors with which votes are cast, we rely on choices across different individuals. Thus, we cluster errors at the process level. We once more rely on simulating the first, second, and third votes from the individual treatment based on the procedure described in Section 6.

As can be seen, $d_M$ and $p_1 \times d_M$ are both statistically significant at the 1% level, indicating a different slope and intercept for the majority treatment: in that treatment, the second voter places a lower "premium" on top of the posterior with which the first vote is cast. In other words, second voters are hastier under majority than they are under unanimity, or in the simulated groups based on the individual treatment. In contrast, there is no statistically significant difference between either the intercepts or the slopes of the unanimity and (simulated) individual treatments.[28]

The results of Table 4 are consistent with a demand for agency exhibited by later voters under majority rule. Furthermore, given the observed responsiveness of second voters to first-voters' choices under majority, there is a strategic reason for first voters to expedite their choices as well. Relatively impatient agents, who are likely to be first voters, can manipulate the pivotal threshold posteriors to be more in line with their preferences. If agents did not exhibit a demand for agency, a lenient group member, associated with a low threshold posterior, would have to accept the higher, median threshold posterior utilized in the group. Instead, with a demand for agency, by expediting her choice, the more lenient member induces a hastier second vote—ideally, she would tailor the posterior at which she votes so that the pivotal vote would occur at precisely her desired threshold posterior. This is consistent with our observations: Figure 3 indicates that the distribution of posteriors when the second, pivotal votes are cast under majority closely approximate with that of the first, most lenient votes under both the unanimity and the (simulated) individual treatments.

[27]Results remain virtually identical when controlling for learning and the time it takes to reach the theoretical stopping threshold in each sample path.

[28]In the Online Appendix, we compare the difference between the posteriors of the third and second vote in the unanimity treatment with that of the simulated individual treatment. There appears to be no statistically significant difference between the intercepts, whereas the slope of the unanimity treatment appears different only at the 10% significance level.

The evidence also allows us to distinguish between the demand for agency and an alternative demand for pivotality. If individuals displayed a demand for pivotality, one would expect second voters in unanimity to delay their votes in order to be more likely to be the pivotal voter. However, as shown in Figure 3, behavior by second voters in unanimity does not significantly differ from behavior by second voters in simulated groups. The same evidence also suggests that participant behavior does not seem consistent with a desire for a diffusion of responsibility. In this case, we would expect the second and third voters in unanimity to vote faster in order to avoid being the pivotal voter.

# 7 Static Information Collection

## 7.1 Group Level Distributions

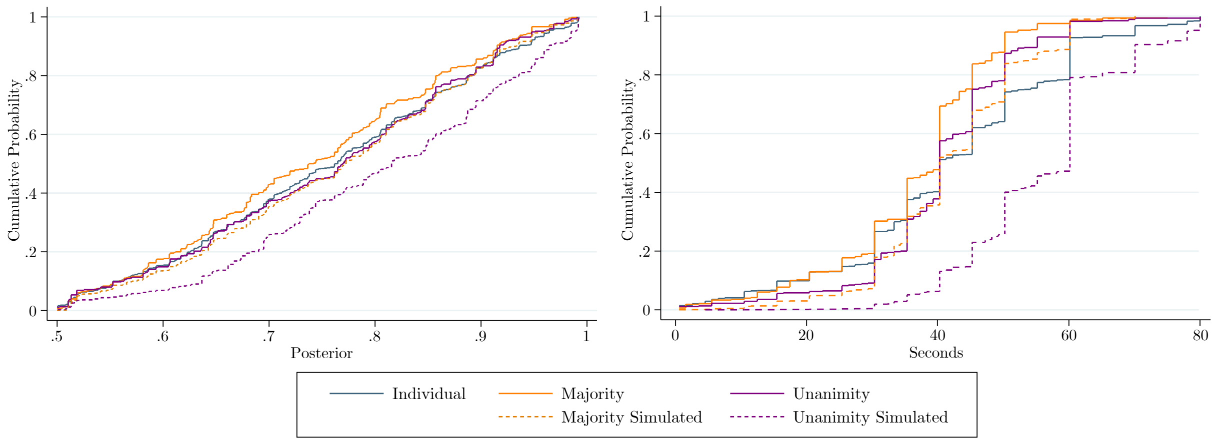
In the dynamic treatments, we focused on the posterior probabilities associated with votes. In the static treatments, participants choose the duration of information acquisition at the outset. Thus, our focus shifts to the time chosen for information collection.

Figure 4 presents the cumulative distribution functions of chosen times across our static treatments, as well as the corresponding realized posterior probabilities.[29] In contrast to observations from the dynamic treatments, the distributions of chosen times across our static treatments appear similar to one another, although chosen times in the majority treatment are roughly first order stochastically dominated by those in both the individual and unanimity treatments: similar to our dynamic settings, groups using majority rule are hastier. These observations are in line with the average times chosen across treatments reported in Table 2.[30] The chosen-time distribution corresponding to the unanimity treatment second order stochastically dominates that of the individual treatment. That is, times chosen by individuals are more dispersed than times chosen by groups using unanimity rule.

[29] Participants predominantly specified an integer number of seconds, with some attraction to round numbers, corresponding to the apparent jumps in the distributions.

[30] In the Online Appendix, we use regression analysis with various levels of clustering to evaluate the differences across treatments.

Figure 4: Static Treatment CDFs




The similarity between the distributions should be interpreted with caution. As in the dynamic case, the heterogeneity in individual choices implies that, were group members mimicking their choices as individuals, there would be differences across treatments; chosen times in our majority and unanimity treatments would correspond to the median and maximal preferred times within the groups. Therefore, to understand behavior in the group treatments, we simulate the distributions of choices in groups following the procedure described in Section 6. Namely, for each round, we form random groups of three individuals from our individual treatment and consider the median (majority simulated) and maximum (unanimity simulated) times within that group.

Under unanimity, it is the "most patient" group member who governs a group's decisions. It is then unsurprising that the distribution of resulting simulated wait times under unanimity differs substantially from that corresponding to individual decisions. It also differs from our observed unanimity treatment, implying a non-mechanical difference between individual treatments and groups using unanimity rule. There is a similar non-trivial effect on groups using majority rule that is not mechanical: the simulated distribution does not coincide with those generated by observed group behavior.[31] Regression analysis in the Online Appendix confirms that the effects of both majority and unanimity rule are not purely a mechanical artifact. Furthermore, while we see some learning leading participants to select shorter times in the second half of our sessions, this learning is limited in scope and duration; see the Appendix for further details. In particular, throughout

[31]The two-sided Kolmogorov-Smirnov test rejects the hypothesis that the distributions associated with the simulated and observed unanimity decisions, as well as the simulated and observed majority decisions, are identical.

our experiments, both types of group treatments lead to *hastier* decisions than those generated by a purely mechanical aggregation effect.

The distribution of induced stopping posteriors are similar across our three treatments, although simulated groups using unanimity first-order stochastically dominate all other distributions. Why do we see differences in chosen times, but little differences in induced posteriors? In our static treatments, participants choose excessively long information-collection durations. Since expected induced posteriors are concave in these durations, the differences in chosen times between the treatments translate into smaller differences in the corresponding posteriors.

## 7.2 Individual-Level Static Choices

Figure 5 presents the distribution of the shortest (denoted by I, in analogy to Figure 3), median (denoted by II), and longest (denoted by III) chosen times for the (simulated) individual, majority, and unanimity static treatments.[32]

Figure 5: Static Treatment CDFs by Vote Order

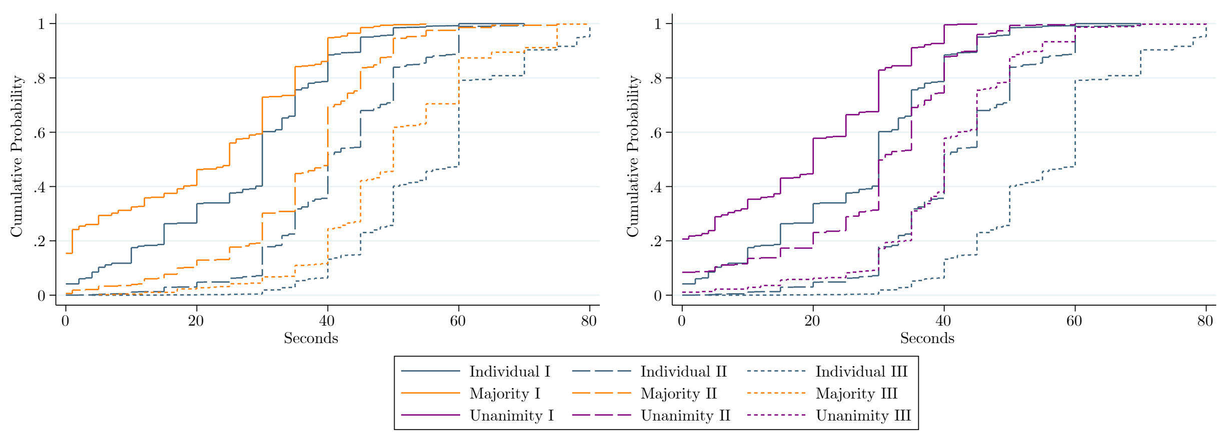



Figure 5 reveals a clear first order stochastic dominance relationship for the shortest, median, and longest times chosen across our treatments. The distributions of all three times corresponding to the unanimity treatment are dominated by those corresponding to the majority treatment, which are dominated by those corresponding to the simulated groups based on the individual treatment. In particular, behavior under both majority and unanimity differs from that in simulated groups.

[32] As for the dynamic treatments, when considering our individual treatment, we group participants into random groups of three and consider the shortest, median, and longest chosen times.

This echoes our conclusion that group effects are present and go beyond the pure mechanical effects driven by heterogeneity in our sample. In contrast to our dynamic treatments, all individual group members' votes are hastier under unanimity.

Figure 5 also suggests different degrees of heterogeneity among group members across our treatments. The times chosen within groups are closest in our unanimity treatment and furthest apart in our individual treatment. For instance, the distance between the median shortest and median longest chosen times under unanimity is approximately 20 seconds; the median distance is 30 seconds in simulated groups based on our individual treatment. Thus, more inclusive rules appear to generate a pressure for conformity.

In the Appendix, we provide additional individual-level analysis. As in our dynamic treatments, we see no evidence of clear clustering of "types," although there is substantial heterogeneity in mean times chosen and their variability across participants.[33]

Certainly, there are several features that differ across our dynamic and static treatments. Most notably, in dynamic treatments, participants observe processes evolve as they make their decisions, and can monitor other group members' choices over time. Instead, all decisions are made ex-ante in our static treatments. Both information-collection protocols lead to hasty majority decisions. However, in the static treatments, there is excessive information collection, even after many rounds of experience, and *both* majority and unanimity rules hasten participants' decisions significantly. In what follows, we assess the welfare implications of these differences.

# 8 Performance

In this section, we compare the performance of individuals and groups to shed light on the impact of procedures and decision rules on ultimate outcomes, accounting for both decision quality and information costs.

**Decision Accuracy** In many settings, decisions made by a small group of individuals affect a large population—political decisions, jury verdicts, determinations of agencies such as the FDA or EPA, and so on. In such environments, a natural welfare criterion pertains to the accuracy

[33] As in our dynamic treatments, we also do not see substantial persistence in terms of "roles," with very few individuals nearly always stating the lowest, nearly always stating the median, or nearly always stating the longest desired duration in their group.

of decisions: information-collection costs are born by only a minuscule fraction of the population. Theoretically, individuals and groups make the same choices regardless of the voting rule, and the only distinction is between static and dynamic information collection. The predicted accuracy is 0.81 in the dynamic setting, higher than the predicted accuracy of 0.72 in the static setting.

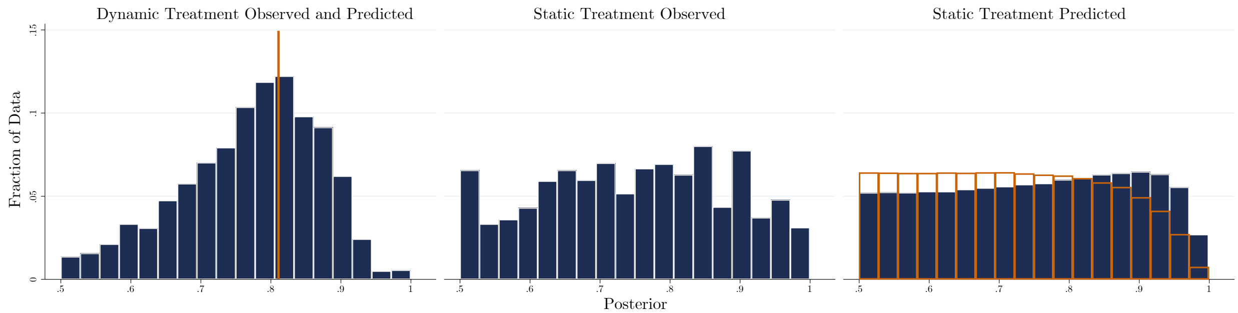
As Table 2 indicates, in our data, the accuracy differences between static and dynamic information collection are far smaller than theoretically predicted. Nonetheless, groups using unanimity and collecting information sequentially yield the most accurate decisions.

In contrast with theoretical predictions, under majority rule, static information collection yields more accurate decisions than its dynamic counterpart. This is a consequence of participants in our static treatments overshooting the theoretical benchmark. Although excessively costly for decision-makers, this overshooting generates greater accuracy. At the same time, in the dynamic setting, majority rule leads to particularly hasty decisions, which are therefore less accurate than theoretically predicted. There is a caveat to this comparison, however. By their nature, static decisions cannot directly target the posterior at which information collection stops. Thus, stopping posteriors tend to be more disperse than when targeting an explicit accuracy threshold.

Figure 6 displays the distributions of posteriors in our dynamic and static treatments.[34] The left and middle panels display realized posteriors under the dynamic and static treatments, respectively (the vertical line on the left panel denotes the theoretically predicted stopping posterior of 0.81). The solid histogram on the right panel displays expected posteriors for the static treatments, conditional on their average observed waiting times (40 seconds). The hollow histogram on the right panel displays the theoretically expected posteriors for the static treatments with optimal wait time (29.6 seconds). As can be seen, the posteriors generated in our dynamic treatments (left panel) are far less dispersed than those of our static treatments (middle panel). This is in line with theoretical predictions. Thus, under majority, although static information collection may outperform the theoretical benchmark in terms of expected decision accuracy, it does run the risk of decisions being made with inconsistent precision levels.

[34] Separate figures for each of our decisions rules yield qualitatively identical patterns.

Figure 6: Posterior Histograms




**Decision Accuracy and Costs** We now turn to the evaluation of welfare accounting for costs; that is, welfare from the point of view of the committee making the decision. Indeed, in many environments—firms' hiring processes, publication review protocols, managerial investment choices—information costs are internalized by those affected by the ultimate decision.

We normalize the payoff for a correct guess to 1, the cost to 0.2, and divide the time waited in seconds by 60. Utilizing the posterior and time of the pivotal vote, we calculate the following performance measure, corresponding to average welfare:

$$\lambda_{i,g}^{benchmark} = p_{i,g} - 0.2 \cdot t_{i,g}$$

where $i$ represents a treatment, and $g$ represents a particular group in a particular round within the treatment. We report the estimated mean of these performance measures under $\lambda^{benchmark}$ and *Performance Level* in Table 5 (first column).

In line with theoretical predictions, dynamic treatments generate higher performance than static treatments. These differences are statistically significant at any conventional significance level. Furthermore, there is a non-trivial interaction between the decision rule and information-collection protocols, albeit not statistically pronounced. In our dynamic treatments, the average performance of groups voting under unanimity exceeds the performance of individuals and groups using majority. In our static treatments, the average performance of groups using either majority or unanimity exceeds that of individuals. These comparisons remain the same, if slightly less pronounced, when focusing on the last 15 session rounds, see the Online Appendix for details.

Table 5: Performance

| | $\lambda^{benchmark}$ | | $\lambda^{expected}$ | |
|---|---|---|---|---|
| | Performance Level | Relative Performance | Performance Level | Relative Performance |
| *Individual Dynamic* | 0.655 | 0.863 | 0.651 | 0.841 |
| | (0.00344) | (0.0191) | (0.00292) | (0.0162) |
| *Majority Dynamic* | 0.650 | 0.832 | 0.648 | 0.820 |
| | (0.00498) | (0.0277) | (0.00273) | (0.0152) |
| *Unanimity Dynamic* | 0.662 | 0.902 | 0.660 | 0.889 |
| | (0.00560) | (0.0311) | (0.00216) | (0.0120) |
| *Individual Static* | 0.615 | 0.936 | 0.609 | 0.883 |
| | (0.00325) | (0.0265) | (0.00213) | (0.0173) |
| *Majority Static* | 0.617 | 0.952 | 0.615 | 0.935 |
| | (0.00513) | (0.0417) | (0.00115) | (0.00933) |
| *Unanimity Static* | 0.620 | 0.978 | 0.614 | 0.924 |
| | (0.00444) | (0.0361) | (0.00123) | (0.00999) |
| *N* | 3840 | 3840 | 3840 | 3840 |

Standard errors in parentheses
Individual-level clustering

The performance measure assessed above necessarily inherits the randomness induced by the particular information processes participants face. Consider, for example, our static treatments. Given the choice of time spent on information collection, the resulting posterior depends on the realized sample path. This inherent randomness introduces noise in our assessments, which could render comparisons between treatments insignificant. Instead, one could consider *expected* welfare, accounting for the *expected* posterior implied by each choice of stopping times. Similarly, in our dynamic treatments, it is natural to consider the *expected* time induced by any choice of decision accuracy and assess performance accordingly.

In the dynamic case, for any threshold posterior $p$, the expected stopping time is $\mathbb{E}[t|p] = \frac{(2p-1)\log\left(\frac{p}{1-p}\right)}{\mu}$. Thus, we define the expected performance for the dynamic treatments as[35]

$$\lambda_{i,g}^{expected} = p_{i,g} - 0.2 \cdot \mathbb{E}[t|p_{i,g}]$$

In the static case, for any chosen duration $t$, the expected posterior is $\mathbb{E}\left[p|t\right] = \frac{1}{2}\left(\text{erf}\left(\frac{\sqrt{1.4t}}{2}\right) + 1\right)$. Accordingly, we define the expected performance for the static treatments as

$$\lambda_{i,g}^{expected} = \mathbb{E}[p|t_{i,g}] - 0.2 \cdot t_{i,g}$$

[35] As a caveat, in our dynamic treatments, we effectively assume time-independent thresholds for these estimates. This approximation simplifies assessments dramatically and yields results that are in line with those from our alternative performance measures.

We report the estimated mean of these performance measures under $\lambda^{expected}$ and *Performance Level* in Table 5 (third column). As was the case with our benchmark performance measures, all dynamic treatments outperform all static treatments at any conventional significance level. Now, however, the noise reduction brings forth differences within the dynamic and static treatments. The dynamic unanimity treatment outperforms the dynamic individual and majority treatments ($p < 0.01$), while the static individual treatment is outperformed by static majority ($p < 0.01$) as well as static unanimity ($p < 0.05$). Thus, after accounting for noise inherited by our processes, we find statistically significant differences between and within the dynamic and static treatments (which remain when considering the last 15 rounds). Importantly, the efficacy of rules governing decision making depends crucially on the information-collection format in place.

**Relative Performance** We now assess how close participants came to the theoretically optimal performance. To do so, we calculate the theoretically optimal performance by utilizing the optimal posterior $p^* = 0.81$ for the dynamic case and the optimal wait time $t^* = 29.58$ for the static case.

$$\lambda^*_{dynamic} = p^* - 0.2 \cdot \mathbb{E}[t|p^*] = 0.68 \qquad \lambda^*_{static} = \mathbb{E}[p|t^*] - 0.2 \cdot t^* = 0.62.$$

In contrast, an immediate decision would yield an accuracy of 0.5 at no cost. This constitutes a plausible lower bound on performance and results in an expected payoff of $\underline{\lambda} = 0.5$.[36] A measure capturing the *relative* performance is then:

$$\tilde{\lambda}_{i,g} = \frac{\lambda_{i,g} - \underline{\lambda}}{\lambda^*_j - \underline{\lambda}} \qquad\qquad j \in \{dynamic,\ static\}.$$

An estimated relative performance of 0 indicates that, on average, the treatment performs no better than an immediate decision that incorporates no information. In contrast, an estimated relative performance of 1 indicates that the treatment exhibits optimal performance.[37] We report these estimated relative performances in Table 5 under *Relative Performance* (second and fourth columns).

[36]It is certainly possible to achieve lower performance. For example, an excessively long wait can yield negative expected payoffs. We do not observe such behavior in our data.

[37]By definition, the comparison of decision-making rules within either our static or dynamic treatments coincides with the comparison generated by our expected welfare measure.

As can be seen, static treatments exceed the dynamic treatments when it comes to information utilization. For example, focusing on our arguably less noisy measure of expected performance (fourth column), the dynamic treatments reach scores between 0.82 and 0.89, whereas the static treatments reach scores between 0.88 and 0.94 (comparisons that, again, remain virtually identical when focusing on the last 15 rounds alone).

Substantively, while information is utilized more efficiently in our static treatments, the dynamic information-collection protocol is sufficiently more effective that it still yields greater expected welfare.

## 9 Conclusions

This paper reports results from a set of experimental treatments testing static and sequential sampling, in individuals and groups. Sequential sampling yields superior outcomes to static sampling, with groups under unanimity delivering the best outcomes. Contrasting theoretical predictions, sequential sampling yields time-decreasing thresholds and static sampling yield excessive information collection. Furthermore, groups behave differently than individuals, beyond mechanical aggregation effects. Majority rule yields the quickest decisions, particularly in sequential sampling.

Our experimental paradigm and our results point to several possible future directions of inquiry. In our study of individuals, it would be interesting to consider behavior under a richer set of parameters. In our study of groups, we have focused on a baseline case in which the model predicts no group effects. This is intended as an initial benchmark on which to build a richer understanding of heterogeneous committees, as in the model studied in Chan et al. (2018). Specifically, similar experiments could be designed with group members experiencing heterogeneous preferences over alternatives and heterogeneous information costs. It would also be interesting to vary other features of groups: their size, the monitoring available to group members, etc.[38]

[38]Naturally, the study of larger groups would entail some non-trivial logistical hurdles as most physical laboratories are limited in size.

# 10 Appendix

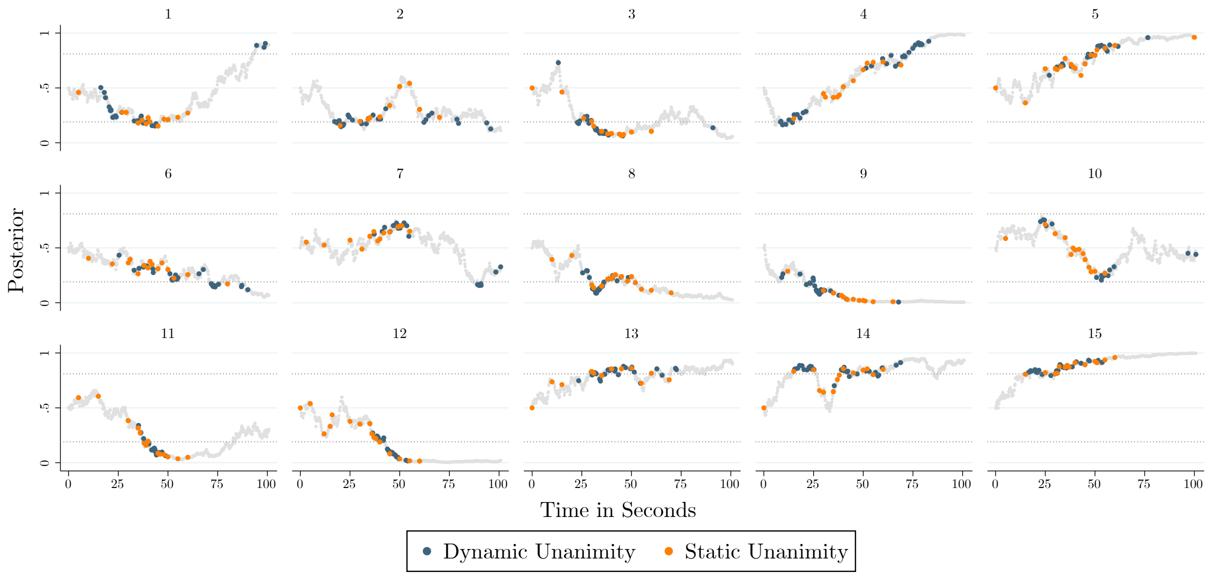
## 10.1 Pulling the Trigger: Majority and Unanimity

Figure 1 in the text depicts the evolution of posteriors and the corresponding choices in our individual treatments, both static and dynamic. Figure 7 and Figure 8 below provide analogous graphs for our majority and unanimity treatments, which depict decision posteriors corresponding to pivotal votes.

The general patterns observed for our individual treatments remain. For example, later decisions in our dynamic treatments often correspond to lower accuracies in our dynamic treatments and posteriors generated in our static treatments are more dispersed than those emerging from our dynamic treatments. However, there are some differences. In particular, groups using majority pull the trigger far quicker than groups using unanimity, in line with results described in the text.

Figure 7: Pulling the Trigger: Majority Treatments

Figure 8: Pulling the Trigger: Unanimity Treatments




## 10.2 Non-stationary Threshold Posteriors

In the text, we consider individuals' voting probabilities in each of our treatments. We now take a different approach, analyzing the realized decision posteriors when the pivotal vote is cast. Table 6 displays regression analysis pertaining to individual and group choices—the stopping posterior—in our dynamic treatments. We use the shorthand of $I$, $M$, and $U$ for the individual, majority, and unanimity treatments, respectively. The variables $d_M$ and $d_U$ are dummy variables for the majority and unanimity treatments. To allow for learning, we include dummy variables of the form *Last 15 X*, with $X$ denoting the treatment; these indicate whether observations are taken from the last 15 rounds of our sessions. Last, we consider the impacts of time spent collecting information to potentially account for non-stationary thresholds. We do so in two ways. First, we classify the processes as "Slow" or "Quick". For this classification, we calculate the time it takes to reach the theoretically optimal threshold of 0.81 in each process. If a process takes more time than the median process to pass the 0.81 threshold (i.e., 29.8 seconds) we label it "Slow"; otherwise, the process is labeled "Quick". The resulting variable *Slow X* is a dummy variable indicating whether a process is slow in each treatment X. We also consider the time spent collecting information in each treatment X, denoted by *Time X*. The last three specifications allow for fixed effects corresponding to the individuals casting the pivotal votes. Errors are clustered at the individual level.

The first column of Table 6 echoes our observations in the text. We see significant differences between treatments, with less precise, or hasty, majority decisions and more precise, or slower, unanimous decisions. Compared to the individual treatment, the mean posterior with which the pivotal majority vote is cast is about 4 percentage points lower, whereas the mean posterior with which the pivotal vote is cast is about 4 percentage points higher.

Throughout, we see a significant effect of learning over the first 15 rounds, with participants becoming more patient, casting their vote with a significantly higher decision posterior. Because both the individual and majority treatments yield, on average, posteriors well below the theoretically optimal, the increase in decision posteriors in later rounds is a move towards the optimal choice. In the unanimity treatment, however, learning leads to overshooting, with an average decision posterior of 0.84 in the last 15 rounds. As mentioned at the outset, and elaborated on below, we do not see evidence of substantial learning beyond the first 15 rounds.

The second and third columns consider the impacts of the underlying process, i.e., whether it is slow or quick. Slow processes are associated with significantly lower decision posteriors across all our dynamic treatments. This association is present and similar in both magnitude and significance, even when restricting attention only to the last 15 rounds of each session. It is most pronounced for groups deciding through majority rule, and least pronounced in groups using unanimity. Lower decision posteriors in slow processes indicate a non-stationary threshold for halting information collection. The last two columns of Table 6 illustrate a declining-threshold pattern more directly, and echo the results presented in the text. Namely, we introduce an explicit dependence on the time at which a pivotal vote is cast.[39] The estimated coefficients corresponding to decision times are negative and statistically significant: the longer it takes for the pivotal vote to be cast, the lower is the threshold posterior. As before, the least affected treatment is unanimity and the most affected treatment is majority. In particular, in the majority treatment, in the last 15 rounds, for each 5 seconds that the group decision is delayed, the average threshold posterior decreases by almost one percentage point.

[39]The fixed-effects specification is appropriate since, without it, we could in principle identify a misleading positive association between decision times and decision posteriors. Indeed, mechanically, since we consider a diffusion with drift, posteriors exhibit an increasing trend. Group fixed effects cannot be used due to the random matching protocol we utilize. We therefore use pivotal-voter fixed effects to adequately capture the response to time passed.

Table 6: Decreasing Thresholds

| | Posterior | | | | |
|---|---|---|---|---|---|
| | Ordinary Regression | | | Fixed Effects Regression | |
| | All Rounds | | Last 15 Rounds | All Rounds | Last 15 Rounds |
| *Constant* | 0.755*** | 0.785*** | 0.806*** | | |
| | (0.00846) | (0.00738) | (0.0109) | | |
| $d_M$ | -0.0362*** | -0.0303*** | -0.0372*** | | |
| | (0.0112) | (0.0107) | (0.0128) | | |
| $d_U$ | 0.0444*** | 0.0347*** | 0.0431*** | | |
| | (0.0103) | (0.00885) | (0.0124) | | |
| *Last 15 I* | 0.0247*** | 0.0247*** | | 0.0299*** | |
| | (0.00647) | (0.00647) | | (0.00790) | |
| *Last 15 M* | 0.0162*** | 0.0162*** | | 0.0224*** | |
| | (0.00613) | (0.00611) | | (0.00653) | |
| *Last 15 U* | 0.0376*** | 0.0376*** | | 0.0430*** | |
| | (0.00717) | (0.00688) | | (0.00726) | |
| *Slow I* | | -0.0648*** | -0.0576*** | | |
| | | (0.00557) | (0.00625) | | |
| *Slow M* | | -0.0774*** | -0.0736*** | | |
| | | (0.00717) | (0.0101) | | |
| *Slow U* | | -0.0440*** | -0.0271*** | | |
| | | (0.00652) | (0.00989) | | |
| *Time I* | | | | -0.000651*** | -0.00110*** |
| | | | | (0.000209) | (0.000238) |
| *Time M* | | | | -0.00130*** | -0.00165*** |
| | | | | (0.000340) | (0.000523) |
| *Time U* | | | | -0.000524*** | -0.000723*** |
| | | | | (0.000132) | (0.000218) |
| $N$ | 1980 | 1980 | 990 | 1980 | 990 |

Standard errors in parentheses
Individual-level clustering
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## 10.3 Estimation with Implementation Trembles

We now consider the possibility that participants implement their optimal threshold with trembles.

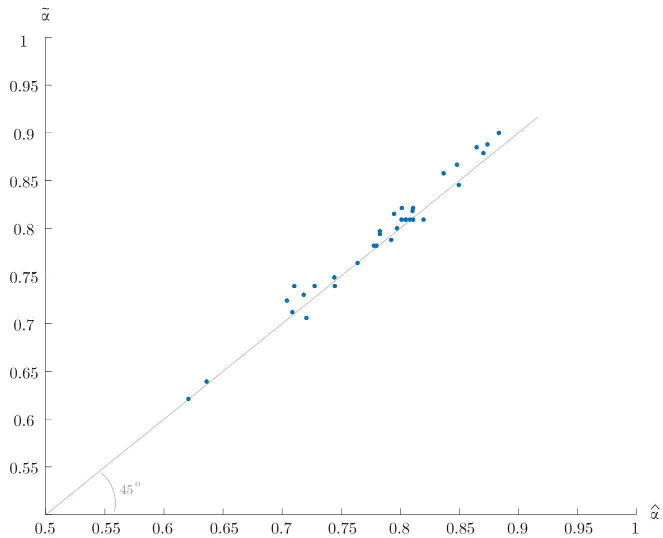
Suppose that instead of casting their vote based on their preferred threshold $f(t)$, participants cast their vote based on $f(t) + \varepsilon_t$, where $\varepsilon_t$ is drawn from a normal distribution with mean 0 and standard deviation $\sigma_\varepsilon$.

In order to estimate the true $f(t)$, we first calculate, in our data, the average time participants take to cast a vote $\bar{t}$, and the observed estimated path of stopping posteriors—identified by an intercept $\hat{\alpha}$ and slope $\hat{\beta}$— derived from running an individual-level fixed-effects linear regression on the individual dynamic treatment data. In our estimation exercise, we match these three "moments" of our data: $\bar{t}$, $\hat{\alpha}$, and $\hat{\beta}$.

Specifically, we simulate Brownian paths with the parameters utilized in the experiment. We also simulate potential thresholds with different intercepts $\tilde{\alpha}$ and slopes $\tilde{\beta}$. Afterwards, we implement the decisions with different noise levels $\tilde{\sigma}_f$. For each conjectured $\{\tilde{\alpha}, \tilde{\beta}, \tilde{\sigma}_f\}$, we calculate the square distance from the the three moments we match, namely $\bar{t}$, $\hat{\alpha}$, and $\hat{\beta}$. Technically, we estimate our parameters of interest via the method of simulated moments (MSM). Our estimated parameter values are then $\tilde{\alpha}$, $\tilde{\beta}$, and $\tilde{\sigma}_\varepsilon$ that minimize the sum of squared errors.[40]

Figure 9 plots the individual level intercepts estimated with and without trembles.

Figure 9: Individual Intercepts With and Without Trembles




*The graph above depicts the fixed-effects estimates of individual-level intercepts $\hat{\alpha}$ displayed on the horizontal axis and corrected estimates of individual level intercepts $\tilde{\alpha}$ on the vertical axis.*

From our observed data, we estimate the average intercept as $\hat{\alpha} = 0.782$, while the estimated slope is $\hat{\beta} = -0.000457$. After accounting for potential implementation trembles, we estimate the variance of the implementation error to be $\sigma_\varepsilon = 0.01$, $\alpha = 0.789$, and $\beta = -0.000486$. Thus, with this approach, and with these utilized moments, we do not find an economically significant difference from the estimations in which we simply ignore potential implementation trembles.

## 10.4 Learning

**Dynamic Treatment Learning** In order to assess learning in our dynamic treatments, we examine whether there is a trend in participants' stopping posteriors over the course of our sessions. In Table 7 we regress participants' stopping posteriors on *Round*, which stands for the session round; *Slow*, which identifies the process occurring during the round as a slow or a quick process (see

[40]We use Monte Carlo simulations to show that this method is indeed reliable in our setting, consistently estimating the true parameter values.

Section 10.2 above); and an interaction between *Round* and *Slow*, allowing for a different learning trend depending on the process.[41] We run an individual-level fixed-effects regression, allowing for a different intercept for each participant. By running the regression separately for each dynamic treatment, we allow for learning to affect these treatments differently. To see whether there were enough rounds for learning to converge, we run additional regressions separately for the first and the last 15 rounds. In addition, we control for $Correct_{t-1}$ that equals 1 if the previous period's individual decision, or group decision in the majority and unanimity treatment, was correct, and equals 0 if the previous period's decision was incorrect. Finally, we control for $Difference_{t-1}$, which equals the difference between participants' last-period choice from the mean stopping posterior of other members of their group in the last period (for our majority and unanimity treatments only).

Table 7: Dynamic Treatment Learning

| | Posterior | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Individual Treatment | | | Majority Treatment | | | Unanimity Treatment | | |
| | All Rounds | First 15 | Last 15 | All Rounds | First 15 | Last 15 | All Rounds | First 15 | Last 15 |
| *Round* | 0.00154*** | 0.00511*** | -0.000277 | 0.00150*** | 0.00177 | 0.00271*** | 0.00192*** | 0.00267*** | 0.00360*** |
| | (0.000555) | (0.00114) | (0.00118) | (0.000420) | (0.00150) | (0.000742) | (0.000276) | (0.000788) | (0.000764) |
| *Round* × *Slow* | 0.000619 | 0.00223 | 0.00190 | -0.000186 | 0.0110*** | 0.00184 | 0.000912* | 0.00904*** | -0.00246* |
| | (0.000445) | (0.00249) | (0.00163) | (0.000701) | (0.00253) | (0.00175) | (0.000515) | (0.00182) | (0.00134) |
| *Slow* | -0.0705*** | -0.0860*** | -0.101** | -0.0712*** | -0.168*** | -0.119*** | -0.0782*** | -0.147*** | -0.000178 |
| | (0.00882) | (0.0220) | (0.0372) | (0.0133) | (0.0251) | (0.0405) | (0.00966) | (0.0169) | (0.0296) |
| $Correct_{t-1}$ | -0.0218*** | -0.0402*** | -0.00913 | -0.0256*** | -0.0333*** | -0.0183* | -0.0287*** | -0.0247*** | -0.0364*** |
| | (0.00655) | (0.00910) | (0.00997) | (0.00687) | (0.00912) | (0.00969) | (0.00588) | (0.00643) | (0.00968) |
| $Difference_{t-1}$ | | | | 0.0367 | 0.0290 | -0.0172 | 0.0417 | 0.0319 | -0.0348 |
| | | | | (0.0371) | (0.0610) | (0.0427) | (0.0282) | (0.0338) | (0.0387) |
| Individual Level FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $N$ | 986 | 476 | 510 | 728 | 339 | 389 | 1392 | 672 | 720 |

Standard errors in parentheses
Individual-level clustering
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

From the estimated coefficients of $Correct_{t-1}$, we see that, on average, participants cast their individual votes with a lower posterior in round $t$ if their or their group's guess in round $t-1$ was correct. In contrast, the coefficients of $Difference_{t-1}$ is never statistically significant, implying that group effects operate more forcefully through the outcomes they generate.

Importantly, when it comes to learning, the regressions in the second and third columns reveal that both the magnitude and statistical significance of *Round* and *Round* × *Slow* drop in the last 15 rounds in the individual treatment. A similar decrease is observed for our majority and unanimity treatments. Even in cases where statistical significance persists, the magnitude is much lower in

[41] We showed that participants tend to vote with a lower posterior when faced with a slow process, which is why we allow for different slopes and intercepts depending on the features of the process. Otherwise, if earlier rounds entail quicker processes, for example, we could erroneously infer a declining stopping posterior.

the last 15 round. The finding that the magnitude of learning is substantially lower in the last 15 rounds compared to the first 15 rounds, as well as the decrease in statistical significance, leads us to believe that 30 rounds afforded sufficient learning opportunities.

**Static Treatment Learning** We now perform a similar analysis for the static treatment. The specification of the regressions presented in Table 8 is as described in Section 10.4. However, the dependent variable, corresponding to participants' choice, is now desired duration rather than stopping posterior. Furthermore, in the static case, participants cannot react differently to slow and quick sample paths, since those evolve only after decisions have been made. Thus, we do not include *Slow* and *Round* × *Slow* in the regressions below.

Table 8: Static Treatment Learning

| | Time Waited | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Individual Treatment | | | Majority Treatment | | | Unanimity Treatment | | |
| | All Rounds | First 15 | Last 15 | All Rounds | First 15 | Last 15 | All Rounds | First 15 | Last 15 |
| *Round* | -0.223* | -0.758*** | -0.140 | -0.260*** | -0.628*** | -0.0220 | -0.444*** | -0.767*** | -0.241* |
| | (0.117) | (0.232) | (0.138) | (0.0931) | (0.211) | (0.130) | (0.0900) | (0.190) | (0.128) |
| $Correct_{t-1}$ | -1.964* | -2.228* | -0.0829 | -1.458** | -1.856 | -0.747 | 0.0774 | -0.574 | 0.619 |
| | (1.020) | (1.093) | (1.437) | (0.623) | (1.369) | (0.716) | (0.813) | (0.901) | (0.930) |
| $Difference_{t-1}$ | | | | 0.384** | 0.381** | 0.0656* | 0.140*** | 0.0669 | 0.0685 |
| | | | | (0.155) | (0.165) | (0.0366) | (0.0456) | (0.0596) | (0.0562) |
| Individual Level FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $N$ | 899 | 434 | 465 | 1392 | 672 | 720 | 1305 | 630 | 675 |

Standard errors in parentheses
Individual-level clustering
$^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

The estimated coefficients of $Correct_{t-1}$ and $Difference_{t-1}$ are typically insignificant: participants do not greatly react to whether or not the last-period decision was correct, or to the difference between their last-period decision and other group members' last-period decision. Once more, the magnitude and statistical significance of *Round* greatly diminishes in the last 15 rounds. This leads us to believe that participants had sufficient rounds to learn and adjust their decisions.

### 10.5 Individual-Level Choice Heterogeneity

Figure 10 reports the average and the standard deviation of the stopping posterior for every participant in our three dynamic treatments. We do not see clear "types" in our data. There is considerable heterogeneity in the average stopping posterior. There is also substantial variability within each participants' choices. Nonetheless, we see no clear relationship between mean stopping posteriors and the variability of participants' choices across rounds in the individual and majority treatments. There is a mild negative relationship in the unanimity treatment. This negative association under unanimity is, at least in part, mechanical: very high average posteriors leave little room for variability.

Figure 10: Dynamic Treatments: Individual-level Choice Heterogeneity

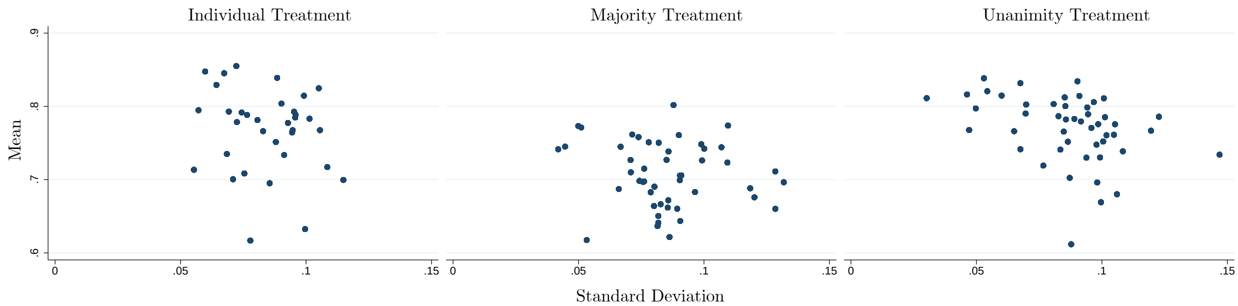
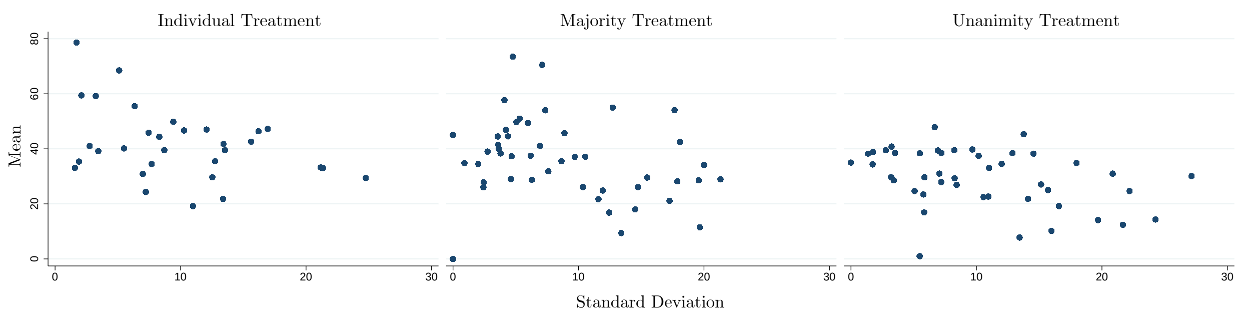



Figure 11 displays similar patterns in our static treatments. [42] Again, we do not see clear "types" in our data, and participants behavior exhibits substantial heterogeneity in both mean desired durations and their variability.

Figure 11: Static Treatments: Individual-level Choice Heterogeneity




These patterns remain qualitatively similar as participants gain experience, although we do see a reduction of choice variability in the last 15 rounds. In particular, average standard deviations decrease from 0.084, 0.085, and 0.086 to 0.078, 0.081, and 0.083, for the individual, majority, and unanimity dynamic treatments, respectively. For the static treatment, the average standard deviations fall more substantially, from 9.82, 10.80, and 10.33 to 7.33, 5.56, and 7.31 for the individual, majority, and unanimity treatments, respectively.

[42] One outlier is excluded from figure Figure 11 in the majority treatment. This individual experimented between the max time (300 seconds) and a choice of 1 second, leading to a standard deviation larger than 100.

Given the observed individual-level heterogeneity, unsurprisingly, we also observe differences in behavior across sessions. Results remain similar after dropping any particular session. It is worth highlighting the following consequences of dropping the most extreme sessions. Specifically, in dynamic treatments, in both the majority and individual treatments, we observe a session with particularly patient behavior. Excluding the extreme session from our majority treatment only reinforces the hastiness that we discussed in Section 6.4. If we exclude the extreme session from our individual treatment, our discussion comparing treatments would require a slight modification: differences would emerge between the individual and unanimity treatments, but they would still be substantially smaller than the differences between the individual and majority treatments. In addition, we observe a session with particularly hasty behavior under static majority. Excluding this session from our analysis only strengthens our finding that the majority static treatment yields more accurate decisions than the majority dynamic treatment.

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