Revealed Incomplete Preferences: Supplemental Appendix

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I. Theories of Incompleteness

Here we overview the theoretical framework underlying our experiment. A model of decision making under subjective uncertainty is presented in Bewley (2002). It shows that a strict preference relation that is not necessarily complete, but satisfies all other axioms of the standard Anscombe-Aumann framework, can be represented by a unique utility index and a *set* of probability distributions. In this model, lack of completeness is thus reflected in multiplicity of beliefs: The unique subjective probability distribution of the standard expected utility framework is replaced by a set of probability distributions.

Since we restrict attention to monetary outcomes that depend on a binary event, we can denote the state as $\{nv, v\}$ (corresponding to "not verb" and "verb") and describe the set of all probability distributions over this state space using the interval $[0,1]$ with generic element π. Let $\succsim$ denote an individual's preference relation over elements of $\boldsymbol{R^2}$ (these are pairs of monetary outcomes in each state). Bewley's Knightian Decision Theory can be summarized by saying that for any $p, q \in \boldsymbol{R^2}$

$$p \succsim q \qquad \text{if and only if} \qquad E_\pi[u(p)] \geq E_\pi[u(q)] \text{ for all } \pi \in \Pi \tag{1}$$

where we define $E_\pi[u(p)] \equiv \pi u(p_{nv}) + (1-\pi)u(p_v)$, and let $u(x)$ denote the utility yielded by the monetary amount x, and let $\Pi \subseteq [0,1]$. That is, p is preferred to q if

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it has higher expected utility for all probability distributions corresponding to the interval Π. If the inequality in (1) changes direction for different elements of Π, the two alternatives are not comparable. In this model, alternatives are evaluated one probability distribution at a time, and they can be ranked only when all those evaluations agree. In this theory, the incompleteness is reflected by multiple subjective probabilities, and if Π reduces to a singleton the preferences are complete and this is standard subjective expected utility.[1]

Bewley's Knightian Decision Theory is extended in Galaabaatar and Karni (2013) by admitting not only multiple probabilities but also multiple utility functions. This model can be described by the following representation: for any $p, q \in \boldsymbol{R^2}$

$$p \gtrsim q \qquad \text{if and only if} \qquad E_\pi[u(p)] \geq E_\pi[u(q)] \text{ for all } u \in \mathscr{U} \text{ and } \pi \in \Pi \tag{2}$$

where $\mathscr{U}$ is a set of utility functions for monetary amounts. That is, p is preferred to q if it has higher expected utility for all probability distributions and all utility functions. Alternatives are evaluated one probability and utility function pair at a time, and they can be ranked only when all those evaluations agree. Notice that even when the individual's subjective probability is unique incompleteness is reflected by many utility functions.

In a world where probabilities are objective Dubra et al. (2004) presents a model of incompleteness that weakens the original von-Neumann & Morgenstern axioms by dropping completeness. Preferences are described by a single *objective* probability distribution and many utility functions: for any $p, q \in \boldsymbol{R^2}$

$$p \gtrsim q \qquad \text{if and only if} \qquad E_\pi[u(p)] \geq E_\pi[u(q)] \text{ for all } u \in \mathscr{U} \tag{3}$$

where, again, $\mathscr{U}$ is a set of utility functions for monetary amounts.[2] Here, p is preferred to q if it has higher expected utility for all utility functions. If the inequality in (3) changes direction for different elements of $\mathscr{U}$, the two alternatives are not comparable. In this model, alternatives are evaluated one utility function at a time, and they can be ranked only when all those evaluations agree. Whenever $\mathscr{U}$ contains a single utility function, this model reduces to von-Neumann & Morgenstern expected utility with objective probabilities.

[1] See Rigotti and Shannon (2005) for a precise statement of this result.

[2] For other models inspired by Aumann (1962) see Ok, 2002; Eliaz and Ok, 2006; Ok et al., 2012.

II. TASTES VS. BELIEFS

One theory is that incompleteness reflects imprecision in beliefs, while an alternative hypothesis is that incompleteness reflects imprecision in tastes. It is also possible that both beliefs and tastes are imprecise and both contribute to incompleteness. We designed Experiment 2 partially to investigate these two sources of incompleteness. Our main environment is one of subjective uncertainty, allowing for incompleteness to result from imprecise beliefs and/or imprecise tastes. We attempt to disentangle these two channels in two ways. First, recall that, after we elicited subjects' beliefs that the word-of-the-day would be a verb, we asked them whether they were sure of this belief. We classify subjects as having precise or imprecise beliefs based on this self-reported measure of belief precision. We say that, for subjects with precise beliefs, incompleteness can only result from imprecise tastes while for subjects with imprecise beliefs, incompleteness can result from imprecise beliefs, imprecise tastes, or both.

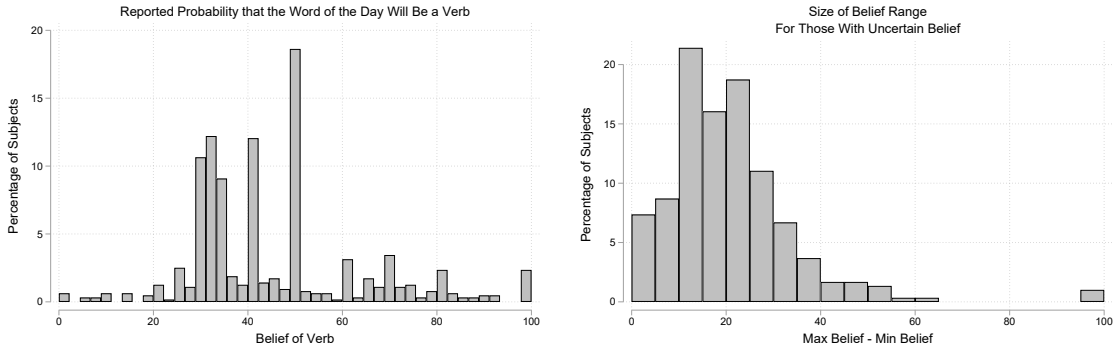


Figure I: Distribution of Reported Beliefs of the Likelihood that the word-of-the-day will be a Verb

Note: The left panel presents the distribution of reported beliefs that the word-of-the-day would be a verb. The right panel presents the distribution of the size of belief ranges for the 47% of subjects who reported having an uncertain belief.

The left panel of Figure I shows the distribution of reported beliefs that the word-of-the-day will be a verb. Most subjects are clustered around a belief of one-third, with another group clustered around one-half. 47% of subjects indicate uncertainty about their reported belief.[3] Subjects who report uncertainty about their belief have

[3]This is on par with the 49% of subjects who report uncertainty about developing late-onset

the opportunity to report a range of beliefs, and on average, they report ranges that span 19 percentage points. The full distribution of the size of the belief ranges for those with uncertain beliefs can be found in the right panel of Figure I.

If incompleteness stems mainly from uncertainty in beliefs, then we would expect the subjects who report incompleteness to be the subjects who indicate uncertainty about their beliefs. Table I shows the joint distribution between incompleteness and belief uncertainty. Of those with incomplete preferences, only half have uncertain beliefs. Among subjects with belief uncertainty, 43% report incomplete preferences, directionally but only marginally significantly higher than the 36% reporting incompleteness among those with certain beliefs (Fisher's exact, $p = 0.075$). Furthermore, among the subsample of subjects with imprecise beliefs, we find no significant correlation between tendency to report incompleteness and the size of the belief range (correlation: 0.0447, $p = 0.441$).

Thus, despite evidence that many subjects have imprecise beliefs—and it might be the case that this belief imprecision contributes to incompleteness to some extent—it does not appear to be the case that belief imprecision is the main contributor to incompleteness in preferences in our experiment. Note that, because we ask a finite number of questions in a particular portion of the lottery space, it's possible, and indeed expected, that individuals with uncertain beliefs would have revealed incompleteness in other questions even if they did not report incompleteness in our study. We can only detect incompleteness as a function of our specific questions, and we would expect belief imprecision to manifest as incompleteness at least somewhere in lottery space.

	Preferences	
	Complete	Incomplete
Certain Belief (53% of subjects)	34.0%	19.2%
Uncertain Belief (47% of subjects)	26.6%	20.2%

Table I: Relationship Between Incompleteness and Imprecise Beliefs
Note: The table presents the aggregate percentage of subjects broken down by whether they have certain or uncertain beliefs and whether they have complete or incomplete preferences. Reported percentages are unconditional.

dementia—a dramatically different event—in Giustinelli et al. (2019).

Objective Lotteries To confirm the previous finding, we run two additional treatments, each with about 150 new subjects recruited through Prolific. These treatments are exactly the same as our original experiment, except that we provide subjects with an objective probability. Specifically, in one treatment, we tell subjects that there is a $\frac{1}{3}$ chance of the event realizing—analogous to a $\frac{1}{3}$ chance that the word-of-the-day would be a verb—and in the other, we tell subjects that there is a $\frac{1}{2}$ chance of the event realizing.[4] These objective probabilities reflect the modal beliefs from our subjective version of the experiment, allowing us to make comparisons about the likelihood of incompleteness across objective and subjective uncertainty.[5]

At an individual level, we find no significant reduction in the tendency to report incompleteness when lotteries are defined over events with objective probabilities. Recall that, in our original data, 39% of subjects reported incompleteness at least once. We find a similar result in our new objective treatments: 37% of subjects report incompleteness at least once when probabilities are objectively given (Fisher's exact, $p = 0.615$).

To look at incompleteness on the level of an individual question, we compare individuals with beliefs near one-third or one-half in the subjective treatment who are *uncertain* about this belief to individuals with the same beliefs in the objective treatment who are *certain* about their belief. The number of subjects who fit these criteria are referenced in Table II. For this analysis, we call reported beliefs in $[30, 35]$ "near one-third" and beliefs in $[48, 52]$ "near one-half" in order to allow for small deviations in reporting.

Table II presents the aggregate choice data. For beliefs near one-third, we find extremely similar rates of incompleteness. For beliefs near one-half, we find slightly more incompleteness reported by individuals with uncertain beliefs in our subjective

[4]Since we want to truthfully tell subjects the objective probabilities while keeping everything as similar as possible to our subjective treatments, we show subjects pictures of cards that have "verb" or "not a verb" written on them. For example, in the treatment where we induce an objective prior of $\frac{1}{3}$, we show subjects three cards, one of which says "verb" and the other two say "not a verb." We tell subjects that we will randomly select one of these cards and the label of the card will determine their payment from a given lottery. We tell subjects that they can watch us live-streaming the card draw on Twitch in three days to further instill trust in the randomization process. We did live-stream the card draws, but did not have any viewers.

[5]We still elicit subjects' beliefs at the end of the experiment and we ask them whether they are certain about this belief. Aggregating across all subjects in both objective treatments, only 11% indicated that they were unsure about their reported belief. This is significantly lower than the 47% who indicated belief uncertainty in the subjective treatment (Fisher's exact, $p < 0.001$).

		Strict Preference	Indifferent	Incomplete
Near One-Third	Uncertain (N=92)	93.3%	3.8%	2.9%
	Certain (N=117)	93.5%	3.4%	3.1%
Near One-Half	Uncertain (N=50)	91.9%	5.1%	3.0%
	Certain (N=126)	94.5%	3.5%	2.0%

Table II: Aggregate Choice Data

Note: The table presents the percentage of subjects who had a strict preference, were indifferent, and were unable to compare the gambles, aggregated across subjects and reference lotteries. Excluded here are the comparisons in which we asked subjects to compare the reference lottery to itself. Uncertain $\frac{1}{3}$ are subjects who reported beliefs in $[30,35]$ in the subjective treatment and reported that they were not sure of this belief; Certain $\frac{1}{3}$ are subjects who reported beliefs in $[30,35]$ in the objective treatment and reported that they were sure of this belief. The $\frac{1}{2}$ are defined analogously in the range $[48,52]$. Sample sizes, N, are reported as number of subjects who satisfy the given belief restrictions.

treatment compared to individuals with certain beliefs in our objective treatment (rank-sum on subject-level rates of incompleteness, $p = 0.430$ for beliefs near one-third and $p = 0.443$ for beliefs near one-half). Nevertheless, most of the incompleteness remains even with objective probabilities that individuals are certain about. Thus, we conclude that belief uncertainty is not the main contributor to observed incompleteness in our study.

III. Additional Tables and Figures

Payment if Not Verb ($)	Payment if Verb ($)
5	19
5	16
6	18
7	10
7	16
8	17
7	19
8	13
6	12
9	14
9	9
9	11
10	8
10	13
10	6
13	3
16	3
11	5
12	1
11	10
12	6
14	2
14	4
17	1
12	8

Table III: List of All Lotteries

Note: This presents the state-dependent payment values of all gambles as depicted in Figure II. (9, 11) and (14, 2) were the two reference lotteries.

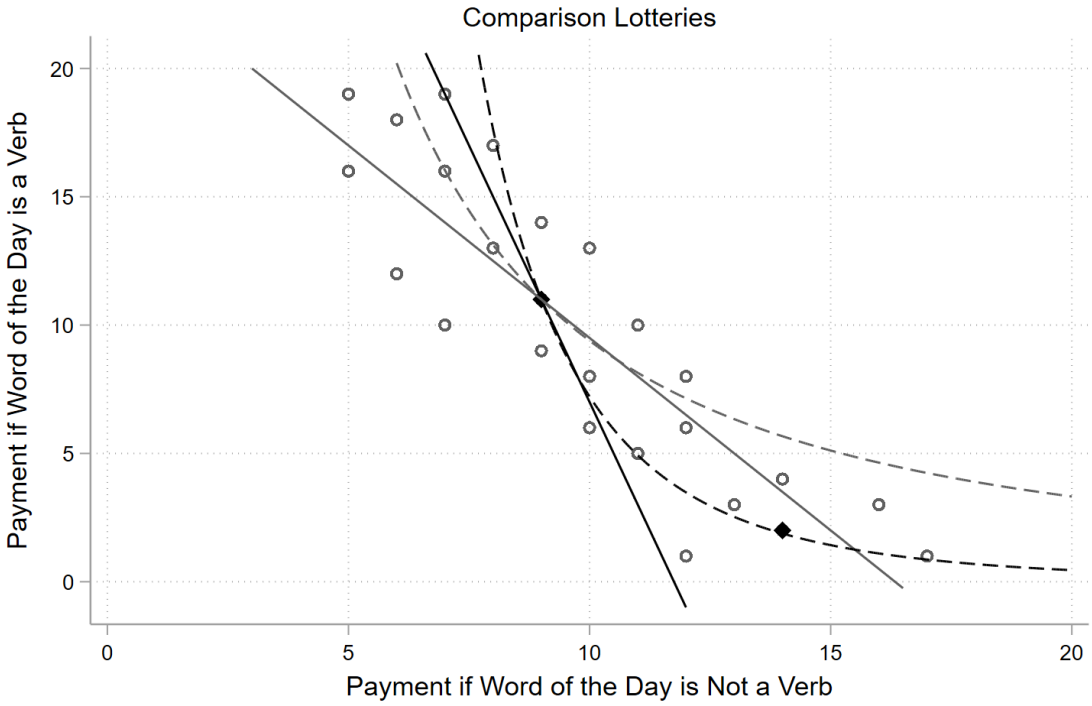


Figure II: Log Utility Functions Through Reference Lottery (9,11)
Note: Solid lines show linear indifference curves while dashed lines show log-utility. The black lines show $pr(verb) = 0.2$ while grey lines show $pr(verb) = 0.4$.

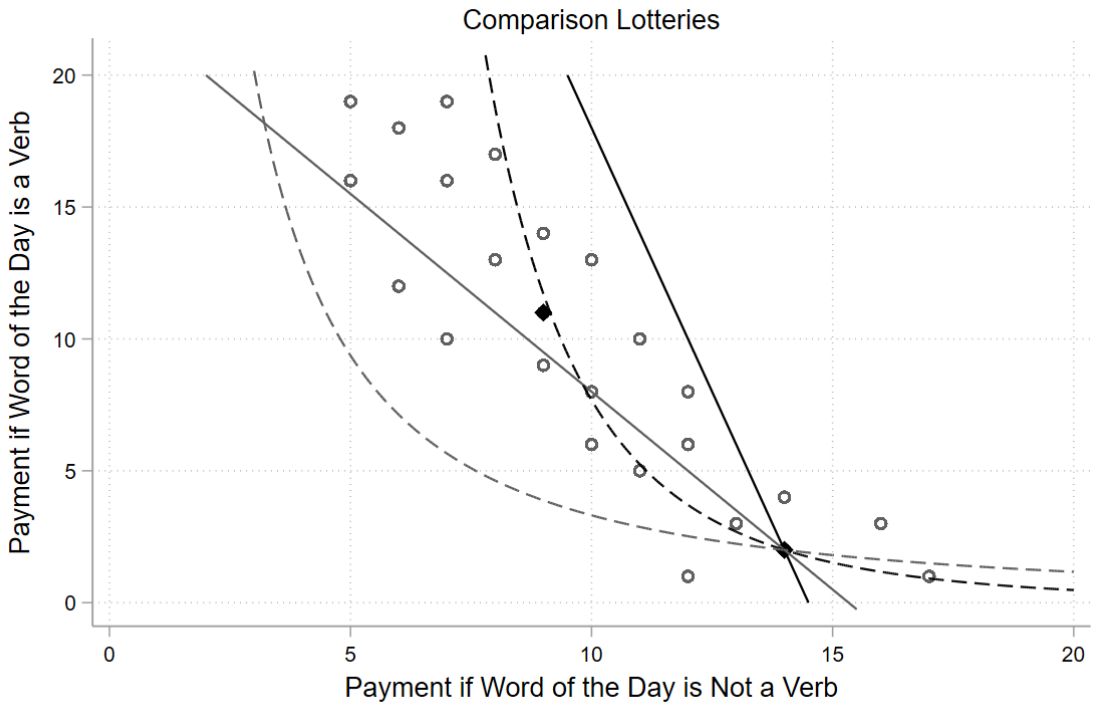


Figure III: Log Utility Functions Through Reference Lottery (14, 2)
Note: Solid lines show linear indifference curves while dashed lines show log-utility. The black lines show $pr(verb) = 0.2$ while grey lines show $pr(verb) = 0.4$.

Decision 1 of 25:

Gamble 1	Gamble 2
$9 if the word-of-the-day is not a verb	$16 if the word-of-the-day is not a verb
$11 if the word-of-the-day is a verb	$3 if the word-of-the-day is a verb

I rank **Gamble 1** above Gamble 2

I rank Gambles 1 and 2 **exactly the same**

I **don't know** how I rank Gambles 1 and 2

I rank **Gamble 2** above Gamble 1

Figure IV: Screenshot of Decision Screen in Non-Forced Treatment
Note: Decisions in the Forced treatment looked exactly the same, without the indifferent and incomplete answer options.

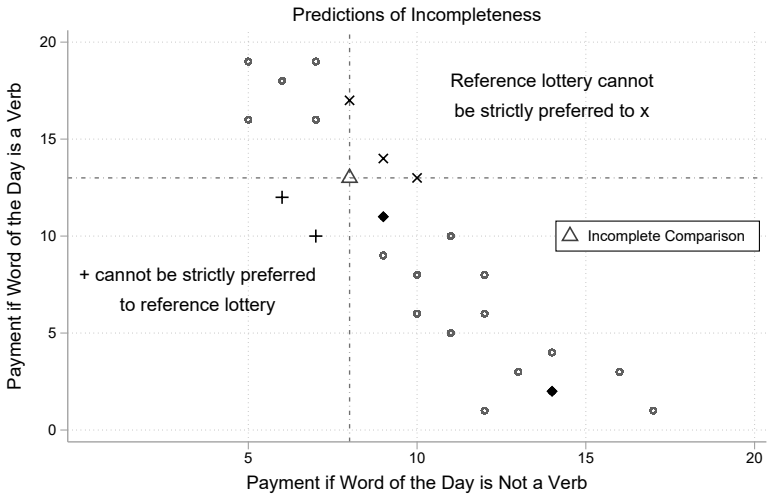


Figure V: Predictions on Preferences Given Observed Incompleteness

Note: Points show the 25 lotteries used in the experiment. Given a reported incomparability—here the example shown is (8,13)—models of incompleteness constrain the reports for dominant and dominated lotteries. The reference lottery should not be strictly preferred to lotteries that dominate (8,13)—lotteries marked with an (x)—and lotteries that are dominated by (8,13)—marked with a (+)—should not be strictly preferred to the reference lottery.

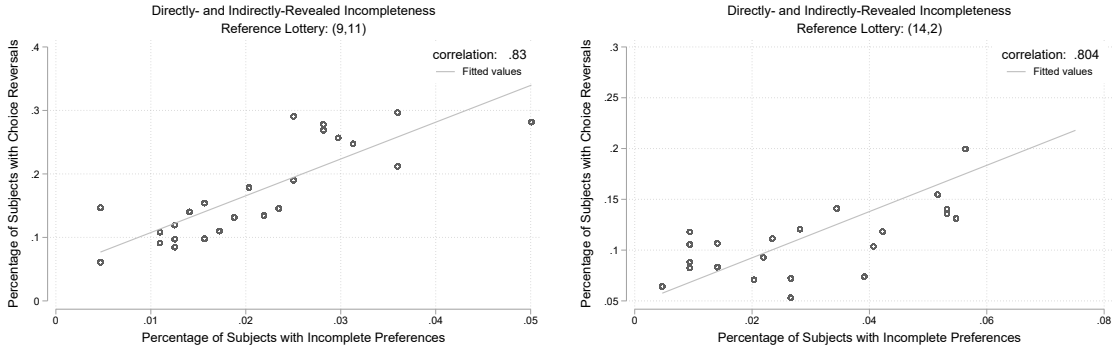


Figure VI: Preference Reversals as a Potential Indicator of Underlying Incompleteness

Note: Each point on a graph represents one of our 24 comparison lotteries; we exclude questions in which subjects compare the reference lottery to itself. The two panels separate the two reference lotteries.

IV. Instructions and Details on Algorithms

Here we present the exact language used for the instructions relating to our elicitation methodology. We first present the instructions for Experiments 1 and 2, respectively. Then we discuss the details of the algorithms in Experiment 2.

IV.A. Experiment 1 Instructions

> *If you are selected to receive a bonus payment, and if you are selected to be paid for this part of the study, then we will use your answers to guess what you would prefer in another question and we will pay you based on what we think you would prefer in that question. We have a tested algorithm that uses your actual answers to these questions to understand your preferences between the two urns and guesses which urn you'd rather bet on. Importantly, your answers cannot affect which question you are paid for, but can only affect which Urn we draw a ball from in the unknown question. Therefore, it's in your best interest to answer what you really think.*
>
> - *If you choose to bet on one of the urns, then our algorithm will use this question to understand your relative preferences between the two urns.*
> - *If you say that you are indifferent between the two urns, then our algorithm will know when you think the urns are exactly the same. Because of this, you can only say that you are indifferent in at most one question, since this is the question that the algorithm will use to figure out when you are exactly indifferent.*
> - *If you say that you don't know which urn you prefer, then our algorithm will not use this question to understand your preferences. You can say that you don't know as many or as few times as you wish.*

We also included understanding questions to ensure that individuals understood what would happen in each of those three scenarios.

IV.B. Experiment 2 Instructions

> *We will not actually pay you directly for the gambles in these question groups. Instead, we will pay you based on what your responses imply about what gambles you prefer in some other decision that you will not*

face. We will use your choices in this question group to understand what types of gambles you like or dislike.

At the end of the experiment, we will pick two gambles. We will pay you the gamble that we think you prefer out of those two randomly selected gambles. We will use your earlier choices to decide which gamble we think you prefer in this decision.

How do we use your choices to understand which gamble you prefer?

We have an algorithm, based on previous research, that learns how you rank gambles.

For example, if you tell us that you like $5 more than $4, the algorithm learns that you like more money to less money. If instead you had said that you like $4 more than $5, the algorithm would learn that you like less money!

Your choices teach the algorithm what type of gambles you like.

- *If you know which gamble you like better:*
  If you say that you rank one gamble over the other, then we will use this information to help our algorithm understand which gambles you would rather have.
- *If you like the two gambles equally:*
  If you say that you rank the two gambles equally, then we will use this information to help our algorithm understand when having either of two gambles is the same to you.
- *If you are not sure which gamble you prefer:*
  If you say that you do not know how to rank the two gambles, then we will not use that question in our algorithm.

Therefore, you have an incentive to tell us which gamble you rank higher, or that you rank them equally. If you do that consistently, our algorithm will use your answers to get a better idea of your preferred gamble at the end of the experiment.

You also have an incentive to tell us when you do not know so that we do not enter your choice into our algorithm. If you told us that you rank one above the other when in reality you were unsure, our algorithm might get the "wrong idea" about the types of gambles you prefer.

We also included understanding questions to ensure that individuals understood what would happen in each of those three scenarios.

IV.C. Experiment 2 Algorithm Details

Our main data contains responses from 639 participants recruited using two different elicitation algorithms. These two algorithms potentially offer different reporting incentives and have different theoretical properties, but we show in Table IV that subjects' responses were the same regardless of the exact incentive mechanism.

We designed our first algorithm, that we call the "Set Construction Algorithm," to map out the theoretical objects of better-than and worse-than sets. We start with a "better-than set" and a "worse-than set" that each contain 10 randomly-selected lotteries from the space of all possible lotteries (x, y), with $x, y \in [0, 20]$. When a subject reports that (x, y) is strictly preferred to a reference lottery, we replace one of the lotteries in the better-than set with $(x+i, y+i), i \in [1, 5]$.[6] When a subject reports that the reference lottery is strictly preferred to (x, y), we replace one of the lotteries in the worse-than set with $(x-i, y-i)$. When a subject reports indifference between a reference lottery and (x, y), we replace one of the lotteries in the better-than set *and* one in the worse-than set as described above. When a subject reports incompleteness, we do not change the better than and worse than sets.

If a subject were paid from this procedure, then we randomly selected to pay them from the better-than set or worse-than set with equal chance. If we randomly selected the worse-than set, then the subject would receive the reference lottery as payment (since they preferred the reference lottery to any lottery in the worse-than set).[7] If we randomly selected the better-than set, then we would randomly select one of the lotteries in the better-than set, and the subject would receive this lottery as payment.

In theory, this procedure is desirable for a few reasons. First, given that subjects are never paid for any of the exact choices p vs. q that they make throughout the experiment, we are never forced to complete their preference between p and q com-

[6] In most of our data, $i = 1$. However, technically there are two comparison lottery pairs for which $(x_1 + 1, y_1 + 1) = (x_2, y_2)$, so one of the lotteries that could be put into the better-than or worse-than set was itself a comparison lottery. Because of this, in later treatments, we randomly select i and put a restriction to prevent this. We do not believe this makes a difference in practice.

[7] Technically, this means that subjects do not have a strict preference to report when a lottery is worse than the reference lottery, since they will be paid the reference lottery anyway. We could have introduced strict incentive by replacing a lottery from the worse-than set and *also* changing the better-than set, for example, by removing any lotteries in the better-than set that were worse than the current lottery. We avoided this for the additional complication, and so that there was no risk of the better-than set becoming empty. Instead, we included lotteries that were dominated by the reference lottery so that we could identify whether subjects report when the reference lottery is strictly better. As we discuss in Section IV.E, 90% of subjects do.

parisons in which they report incompleteness. Second, the procedure described above allows for estimated preferences to be incomplete. We are simply constructing better-than and worse-than sets, and the union of these sets need not (and in fact does not) span the entire space of lotteries. Third, in this simple set construction procedure, we make no functional form or other parametric assumptions, so choices only inform preferences through dominance. Finally, the question we ultimately pay subjects is one where we are "sure," under dominance, that they have complete preferences.

The one major drawback of this type of procedure is that the questions subjects answer influence the possible lottery choices they could be paid, exactly because of this last feature where we only pay subjects for a choice where they have complete preferences. In particular, the lotteries that are replaced into the better-than and worse-than sets are a function (via dominance) of the questions that a subject answers with strict preference or indifference. As a result, this algorithm is not incentive compatible for all possible beliefs and preferences. For example, if a subject thought that the better than set contained "very good" lotteries to start with, then they would not want to indicate that any comparison lotteries were preferred to the reference lottery, since this would replace a "very good" lottery with an inferior one. There is no reason for subjects to hold this belief and it is not accurate—the better-than and worse-than sets start out with randomly-generated lotteries, and we state this in the additional information about the algorithm—but we cannot entirely rule out such beliefs.[8]

Given these pros and cons, we re-ran our experiment using a different—and potentially more standard—incentive mechanism, that we call the "MLE Algorithm." Here, we fixed a single payment question for all participants, $(14,2)$ vs. $(9,5)$, which was not one of the questions that subjects faced in the experiment. Using a standard maximum likelihood approach, we took all of the questions in which a subject reported strict preference or indifference to estimate a CRRA utility function for each subject who received a bonus payment. Incomparabilities simply did not enter into this estimation. We then calculated whether the subject would prefer $(14,2)$ or $(9,5)$ given their estimated utility function, and we paid them based on this prediction.

Here, the incentives are, loosely, that reporting strict preferences or indifference

[8]We needed the sets to start with randomly-selected lotteries to give subjects an incentive to answer questions where they had a strict preference. Otherwise, a subject could, for example, answer one question and be guaranteed that lottery for payment.

can help the estimation procedure form a more precise estimate of the utility function. Since subjects do not know what question is being paid, if their response could change the estimated utility function, then there are questions where this change in estimate could lead to different payment lotteries. When subjects are not sure of their preference in a given comparison, reporting incompleteness prevents this comparison from potentially biasing the estimation.

The drawbacks of this type of procedure are, in some sense, the opposite of the Set Construction Algorithm. Here, we fix a payment question ex-ante and we force a complete preference in this comparison. It could be the case that subjects actually have incomplete preferences in this question, but this procedure does not allow for that. Furthermore, we also have to use a specific functional form for the estimation. In theory, it could be that subjects have complete but non-CRRA preferences, which could lead to biases in reporting.

Reference Lottery	Prefer Reference	Prefer Comparison	Indifferent	Incomplete
Set Construction	34.2%	55.0%	8.2%	2.6%
MLE	33.4%	56.2%	7.7%	2.7%

Table IV: Aggregate Choice Data
Note: Subjects made 25 comparisons for each reference lottery. The table presents the percentage of subjects who preferred the reference lottery, preferred the comparison lottery, were indifferent between the two, and were unable to compare the two, aggregated across subjects.

Table IV shows that subjects' responses are statistically indistinguishable across these algorithms (Fisher's exact $p = 0.249$). The same percentage of subjects clicked to learn more about the algorithm ($p = 0.829$), and the choices among those who read the algorithm details are also statistically indistinguishable ($p = 0.098$)). Given that these two algorithms are very different from one another, and that the theoretical pros and cons are quite opposite, we find this compelling evidence that the details of the underlying algorithm do not affect subjects' responses.

In running our MLE Algorithm treatment, we also recruited 166 additional subjects and incentivized them under the same algorithm but exogenously provided them with the information about the algorithm. That is, rather than choosing to learn the information or not by clicking a button, we showed this information to all participants. Specifically, we tell subjects the following:

> *We will use maximum-likelihood estimation to estimate a constant relative risk aversion utility function. Maximum likelihood estimation will find*

> *the constant relative risk aversion parameter such that your choices are most probable under this model. Then, we will use that model to predict what you would choose in another question, and will pay you based on this prediction. The choices that you make will help this estimation procedure to choose the parameter that best fits your preferences. If you say that you "do not know" which gamble you prefer in a question, we will not use this question in our maximum likelihood estimation. We will use only the questions where you know which gamble you prefer.*

We link Wikipedia pages for maximum likelihood estimation, constant relative risk aversion, and utility functions.

We find a slight reduction in the percentage of subjects who report incompleteness in this treatment (31% vs. 41%, $p = 0.071$). We find it implausible that this reflects subjects reading and optimally responding to the MLE and CRRA detailed incentives. Instead, this seems in line with Danz et al. (2022) who find that providing detailed information on incentives can lead to subjects distorting their responses. In support of this, we find that, among subjects who ever report incompleteness, they report the same percentage of incomplete comparisons at a question level ($p = 0.414$) regardless of whether information was exogenously provided. Thus, it appears that some subjects are "scared off" or confused by the elicitation details, so too much information can cause a reduction in incompleteness on the extensive margin but not on the intensive margin.

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