Inconsistently consistent: Rationality is not reliable

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Abstract

Contemporarily, experimental investigations of revealed preference choice consistency utilize different tasks interchangeably. However, the reliability of choice consistency measurements among (inter-method) and within tasks (test-retest) has not been determined so far. Hence, it is unclear whether estimations of choice consistency fulfill a basic requirement of valid psychometric measures. Further, it is unclear how far results from different studies using different methodologies are comparable. In the study described here, we investigated the reliability of two established and one novel choice consistency tasks in an online-experiment under non-incentivized conditions in the choice domain of social decisions. Our results confidently indicate generally poor inter-method reliability and at best moderate test-retest reliability for the two indices, the Critical Cost Efficiency Index (CCEI) and the Houtman-Maks-Index (HMI), with the CCEI being the tentatively more reliable measure. This is especially concerning, since the full experiment (including test and retest measurement) lasted on average less than 45 minutes. Hence, it appears that estimations of choice consistency do not fulfill a basic requirement of valid psychometric measures. Further, results from different studies using different methodologies should not be compared without caution. Future work should investigate the impact of incentivization as well as the choice domain generality of our results.

Keywords: GARP, revealed preference, choice consistency, rationality, reliability

The Reliability of Choice Consistency

Revealed preference theory (Houthakker, 1950; Samuelson, 1938; Varian, 1982) assumes that any collection of choices by a rational decision maker can be reconciled with a definite structure of wants, cost efficiency and transitivity (Afriat, 1973). Specifically, for the typically considered two-dimensional choice objects (e.g. bundles of two different goods) the Generalized Axiom of Revealed Preference requires that if a decision maker accepts costs to choose a choice object over another (strict direct revealed preference), they may, *ceteris paribus*, in fact never choose the latter over the former choice object (no direct revealed preference) as long as it is not associated with higher costs (Banerjee & Murphy, 2006).

The most prominent way to quantify revealed preference choice consistency is Afriat's Critical Cost Efficiency Index (CCEI; Afriat, 1972; Varian, 1991): "The critical cost efficiency index (CCEI) is inspired by the fact that inconsistent choice behavior is not cost efficient. With given prices of choice options, the budget determines all affordable choice options. A revealed preference violation occurs when the decision maker does not choose the most preferred alternative that was affordable given the budget, but selects another less preferred option. This can be interpreted as a waste of money, as the decision maker did not obtain the maximum subjective value for [their] money. The critical cost efficiency index denotes the minimal hypothetical reduction of the budget of the decision maker necessary, so that all more preferred but not chosen options become unaffordable when the revealed preference violation occurs. A critical cost efficiency index of 1 denotes perfect consistency: The budget does not need to be reduced. The index approaches zero as the behavior becomes more inconsistent and the budget needs to be reduced starkly to eliminate inconsistency." (Nitsch & Kalenscher, 2020a). A

drawback of the CCEI and conceptually similar indices is that it is entirely determined by the least cost-efficient choice and, thus, inherently is quite susceptible to outliers.

The Houtman-Maks-Index (HMI; Heufer & Hjertstrand, 2015; Houtman & Maks, 1985) on the other hand does not consider the severity of choice consistency violations but instead determines the size of the largest subset of choices consistent with GARP (or any other choice consistency axiom). Hence, the measure is possibly more robust to single outliers but also more sensitive to multiple but practically negligible violations.

Contemporarily, experimental investigations of revealed preference choice consistency utilize different tasks interchangeably. One line of research uses a task introduced by Choi et al. (2007). In their elaborate and widely used paradigm (henceforth “Diagram task”), participants must allocate a budget between two dimensions (e.g. two investment accounts, oneself and a co-player, apples and oranges) using a cartesian coordinate display. The task is mostly applied in the investigation of choices under risk (e.g. Castillo et al., 2017; Cettolin et al., 2019; Choi et al., 2014; Kurtz-David et al., 2019) but also intertemporal choices (Chakraborty et al., 2017; Kim et al., 2018). It has the appeal that it transparently depicts all economic parameters (budget, prices, budget line etc.) and even allows for a visual identification of inconsistent choices. A potential drawback is that the task can be hard to understand for people without experience in the interpretation of diagrams and the theoretically large number of potential choice options.

Another line of research uses a more simplistic choice bundles task that was first prominently used by Harbaugh and colleagues (2001) and many others since (e.g. Bedi & Burghart, 2018; Burghart et al., 2013; Chung et al., 2017; Nitsch et al., 2021). In the choice bundles task (henceforth “Bundles task”), the budget line is divided into (equidistant) discrete points which are subsequently presented as a discrete set of choice options to the participant.

Conveniently, in this task participants can ignore the underlying economic parameters and must only choose the most liked choice bundle. This significantly reduces the cognitive demand of the task and is desirable for indivisible goods (e.g. food items), specific participant groups (e.g. children) and research questions (e.g. decisions under stress). A drawback of the task is that discrete choice options can only approximate optimal choices from a continuous budget line, which might introduce inconsistency by itself (see Dzielwski, 2018, for a psychophysical interpretation of choice consistency; see Nitsch & Kalenscher, 2020b for an application). A compromise (which so far has not been applied, however) would be to present participants with a slider (henceforth “Slider task”; see Methods), that allows for the continuous allocation of the budget while concealing economic parameters to a degree that allows for an intuitive approach to solving the task.

Problematically, however, the reliability of choice consistency measurements among (inter-method) and within tasks (test-retest) has not been determined so far. Hence, it is unclear how far estimations of choice consistency fulfill a basic requirement of valid psychometric measures. Further, it is unclear how far results from different studies using different methodologies are comparable. Lastly, it is an open question which type of consistency quantification, CCEI or HMI, is more desirable in terms of reliability. In the study described here, we investigated the reliability of social choice consistency measured via the three above described choice consistency tasks in an online-experiment under non-incentivized conditions. Specifically, we tested the following hypotheses:

H1: There is a significant, large correlation between the CCEI estimates of the Bundles, Slider and Diagram task. This means that the tasks are reliably measuring the same psychological construct.

H2: There is a significant, large test-retest reliability for CCEI estimates of the Bundles, Slider and Diagram task. This means that the tasks do reliably measure choice consistency for two subsequent measurements.

H3: There is a significant, large correlation between the HMI estimates of the Bundles, Slider and Diagram task. This means that the tasks are reliably measuring the same psychological construct.

H4: There is a significant, large test-retest reliability for HMI estimates of the Bundles, Slider and Diagram task. This means that the tasks do reliably measure choice consistency for two subsequent measurements.

H5: The CCEI and HMI show significantly different test-retest reliability.

Statistical and econometric power analyses confirmed sufficient power of our design. Analyses of the distribution of consistency indices showed that our participants, despite non-incentivization, behaved on average highly consistent (in line with previous reports in the literature) and significantly more consistent than bootstrapped random deciders. Given this, our results confidently indicate generally poor inter-method reliability and at best fair test-retest reliability for CCEI (H1 and H2) and HMI (H3 and H4), with the CCEI being the tentatively more reliable measure (H5). This is especially concerning, since the full experiment (including test and retest measurement) lasted on average less than 45 minutes. Hence, it appears that estimations of choice consistency do not fulfill a basic requirement of valid psychometric measures. Further, results from different studies using different methodologies should not be compared without caution.

Methods

Participants

101 adult, English-speaking participants completed our experiment. 48 were randomly assigned to an experimental manipulation group that is irrelevant to the presented research question and were, thus, discarded for all analyses. No other participants were excluded resulting in a final sample size of $N = 53$ participants. Table 1 gives an overview of the demographics.

Procedure & Design

Participants were recruited via Prolific receiving a compensation of 4.50 pounds. The experiment was programmed in jsPsych (de Leeuw & Motz, 2016) and hosted on Pavlovia. Before the start of the experiment, all participants were fully debriefed about the content and aim of the research project and provided informed consent via a check box. After providing consent, we asked for their demographic information. Next, participants underwent the first measurement of all three experimental tasks in randomized order. For the first measurement, each task entailed a detailed description and 5 practice trials. After completion of the first measurement participants solved a filler task that consisted of reading three informational texts about unrelated topics and answering three quiz questions on the content of the texts (see Appendix B). Then participants underwent the second measurement of all three experimental tasks, again in randomized order. At the end of the experiment, participants answered several questions regarding their decision strategies and experiences solving the tasks. Then they were redirected back to Prolific to receive their compensation.

Our experimental design was completely within-subject. Participants solved all three decision tasks for two measurements (3 x 2 within-subject design).

Experimental Tasks

Generally, all decision tasks consisted of $I = 20$ decisions per measurement where participants, hypothetically, had to allocate a budget m_i between them and their best friend, resulting in a final monetary split of $x_i = (x_i^{Self}, x_i^{Friend})$. Importantly, the monetary endowment m_i and the “prices” of keeping and giving money $p_i = (p_i^{Self}, p_i^{Friend})$ varied per decision. Hence,

$$x_i^{Self} = \frac{share_i^{Self} m_i}{p_i^{Self}} \text{ and } x_i^{Friend} = \frac{share_i^{Friend} m_i}{p_i^{Friend}}, \text{ with the share indicating the relative fraction of the}$$

budget (0 to 1) allocated to each account. Budgets and prices were randomly sampled per trial:

$m_i \in [2, 3, 4, 5, 6, 7, 8, 9, 10]$ and $p_i^{Self}, p_i^{Friend} \in [1, 2, 3]$. Note, that for our analysis we

normalized prices and budgets so that $\sum p_i = 1$ and $m_i = x_i^{Self} p_i^{Self} + x_i^{Friend} p_i^{Friend}$.

For each task and measurement, we further included two attention check trials where participants were instructed to allocate the full budget either to themselves or their best friend. Those trials were not included in the analysis. If participants failed an attention check for a given measurement of a task, we excluded that measurement of the task from our analysis specifically.

A screenshot of a trial from each task is available in Appendix A.

Diagram Task

For each decision participants had to choose a point on a diagonal line in a coordinate system. The points on the diagonal line represented the possible money allocations between them and their best friend that they might choose. In each coordinate system, the vertical axis corresponded to the money chosen for themselves (“You”), and the horizontal axis corresponded to the money chosen for their best friend (“Friend”). While they were making their decision, they could see which amount of money they had chosen for themselves and for their best friend in the

upper right corner of the coordinate system. The flatter the lines, the more money their best friend could receive as a maximum compared to them. The steeper the lines are, the more money they could receive as a maximum compared to their best friend.

Bundles Task

For each decision participants had a choice of 5 different money allocations and were instructed to simply choose the allocation that they thought was best.

Slider Task

For each decision participants had to choose a point on a horizontal slider, which represented the possible allocations of money amounts between them and their best friend. While making their decision, they could see which amount of money they had chosen for themselves and their best friend in two boxes above the slider. The labeling of the endpoints and spatial presentation was randomized from round to round.

Task Questionnaires

At the end of the experiment participants were asked to answer how they reached their decisions (“How did you reach your decisions?”) and what they considered particularly important in their decisions (“What was particularly important to you in your decisions?”) in open text format. Further they were asked multiple questions regarding their experiences with the specific task formats that will be reported elsewhere.

Analysis

Revealed Preference Analysis

Let N be the number of different commodity types in a commodity bundle. Let X be the non-negative, N -dimensional space of commodity bundles. Let P be the non-negative, N -dimensional space of prices of commodities. Let M be the non-negative, one-dimensional space

of budgets. Let $I = i, j, \dots, n$. denote observations of choice. Let x_i be the chosen commodity bundle of an observation i . Each bundle x_i is a N -dimensional vector of the shape

$x_i = (x_i^1, x_i^2, \dots, x_i^n)$, with each scalar component x_i^n representing the quantity of commodity type

n within bundle x_i . Let p_i be the given prices of commodities of an observation i . Each prices p

are a N -dimensional vector of the shape $p_i = (p_i^1, p_i^2, \dots, p_i^n)$, with each scalar component p_i^n

representing the price of commodity type n per unit size. Then the scalar product $x_i \bullet p_i$

represents the total price of a commodity bundle x_i at some prices p_i . Let m_i be the given budget

of an observation i . We assume, that a decision maker spends all their budget so that $x_i \bullet p_i = m_i$

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Definition 1 (Direct Revealed Preference). A bundle x_i is directly revealed preferred to another bundle x_j if and only if $x_j \bullet p_i \leq m_i$. Then we denote $x_i R_D x_j$.

Definition 2 (Revealed Preference). A bundle x_i is revealed preferred to another bundle x_k if there exists a transitive preference relation $x_i R_D x_j R_D x_k$ between both bundles. We denote $x_i R x_k$.

Definition 3 (Strict Direct Revealed Preference). A bundle x_i is strictly directly revealed preferred to another bundle x_j if and only if $x_j \bullet p_i < m_i$. Then we denote $x_i P_D x_j$.

Axiom 1 (Generalized Axiom of Revealed Preference). $x_i R x_j \Leftrightarrow \neg x_j P_D x_i$.

In this framework, the CCEI presents a relaxation of Defintion 3, so that only

$x_i P_D x_j \leftarrow x_j \bullet p_i < CCEI * m_i$ is required. The CCEI is then the highest possible value so that

Axiom 1 holds for all observations. The HMI on the other hand denotes the largest number of observations $HMI \leq I$ for which Axiom 1 holds.

Statistical Analysis

To quantify the reliability of choice consistency we calculated the Pearson correlation coefficient between the variables of interest. Reliability indicates how much of the total variance in the variable of interest is not caused by measurement error. A reliability of 1 indicates a perfect measurement: all variance in the measurement is caused by true differences in the variable of interest. As reliability approaches zero, the measurement is becoming less precise and a higher fraction of variance is due to measurement error (Kimberlin & Winterstein, 2008). In accordance with our hypotheses, we tested for a positive correlation using one-sided t-tests. Specifically, for hypothesis 1 and 3 we calculated the correlation of consistency measures among tasks for each measurement ($3 \times 2 = 6$ comparisons). For hypothesis 2 and 4 we calculated the correlation of consistency measures within tasks across measurement (3 comparisons). To test whether the HMI is a more reliable measure than the CCEI (hypothesis 5), we pairwise tested for differences in the test-retest reliability of CCEI compared to HMI per task (3 comparisons; Diedenhofen & Musch, 2015). The level of statistical significance was set *a priori* to $\alpha = .05$. However, since we conducted multiple comparisons for each hypothesis, we applied a Bonferroni correction resulting in a significance-threshold of $\alpha = .05/6 \approx .008$ for hypotheses 1 and 3, and $\alpha = .05/3 \approx .017$ for hypotheses 2, 4 and 5. Further, for qualitative interpretation of reliability we will adhere to the following standards: a reliability coefficient of below .40 is considered poor; when it is between .40 and .59 it can be considered fair; when it is between .60 and .74, it can be considered good; and when it is between .75 and 1.00 it can be considered excellent (Cicchetti, 1994).

Power Analysis

We *a priori* defined the required sample size of our experiment using a statistical power analysis in G*Power (Erdfelder et al., 1996). As mentioned above, we would only deem a correlation between and within tasks large enough, if the effect size is $r \geq 0.5$. Additionally, we aimed for a statistical power of $1 - \text{Beta} = .90$ and considered the lowest Bonferroni-corrected significance level of $\alpha = .05/6 \approx .008$. Results indicated that we would need at least 44 participants to reach our power goal.

Bronars' Power

To determine the statistical power of our GARP test we bootstrapped 1000 virtual participants from our dataset (Bronars, 1987). Results showed that Bronars Power = 91,8% bootstrapped participants did not pass GARP.

Qualitative Content Analysis

In order to gain a further understanding of the decision-making process and to further validate our data we conducted an inductive, qualitative content analysis (Mayring, 2004) using the two free-text responses about the decision strategy of our participants. For our analysis, we concatenated the answers of our participants to both questions. Using an inductive approach, 5 exhaustive and mutually-exclusive categories were generated. Lastly, we compared the relative frequencies of categories to identify key decision-making strategies.

Results

Reality Check

To ensure that our collected choice data can be meaningfully interpreted we tested for a difference in choice consistency of our participants to a benchmark of 53 bootstrapped

participants using a non-parametric Mann-Whitney-U test. Results indicated that our participants were significantly more consistent in their choices for all tasks and measurements according to both CCEI and HMI (see table 2 for descriptive statistics and significance tests, and figure 1 and figure 2 for the full distribution of the data).

H1: Inter-method Reliability of CCEI

Our results indicate poor inter-method reliability, with 1 out of 6 comparisons showing a significant correlation of the measured CCEI values (see table 3) and a mean correlation of $r=0.237$.

H2: Test-Retest Reliability of CCEI

2 out of 3 comparisons showed a significant correlation of the measured CCEI values (see table 3). Further, results indicated a mean correlation of $r=0.459$ ($r=0.537$ excluding the novel Slider task), which can be considered fair. The average absolute difference between first and second measurement amounted to $M_delta_CCEI = 0.084$ for the Diagram task, $M_delta_CCEI = 0.111$ for the Bundles task and $M_delta_CCEI = 0.113$ for the Slider task (see Appendix C for visualization).

H3: Inter-method Reliability of HMI

Our results indicate poor inter-method reliability, with 1 out of 6 comparisons showing a significant correlation of the measured CCEI values (see table 4) and a mean correlation of $r=0.254$.

H4: Test-Retest Reliability of HMI

1 out of 3 comparisons showed a significant correlation of the measured HMI values (see table 4). Further, results indicated a mean correlation of $r=0.428$ ($r=0.471$ excluding the novel Slider task), which can be considered fair. The average absolute difference between first and

second measurement amounted to $M_delta_HMI = 0.791$ for the Diagram task, $M_delta_HMI = 1.042$ for the Bundles task and $M_delta_CCEI = 0.837$ for the Slider task (see Appendix C for visualization).

H5: Test-Retest Reliability Comparison of CCEI and HMI

Our results indicate a significantly higher test-retest reliability of the CCEI compared to the HMI for the Diagram task ($z = -2.283$, $p = .022$), but neither for the Bundles task ($z = 0.858$, $p = .391$) nor the Slider task ($z = 0.502$, $p = .616$).

Qualitative content analysis of reported decision strategies

Our results indicated that the vast majority of participants either tried to fairly share the payout (23 participants, 43.396%) or maximize the total payout (20 participants, 37.736%). Few participants decided with an egotistical (6 participants, 11.321%) or prosocial bias (2 participants, 3.774%). For 2 participants (3.774%) we could not determine a strategy from their response. For an overview see figure 3.

Discussion

So far the reliability of revealed preference choice consistency measurements among (inter-method) and within tasks (test-retest) has remained an untouched topic. Any interpretations of rationality as a valid psychometric trait variable, as well as comparisons between studies of different methodology, therefore, relied on a leap of faith in this aspect. In the study described here, we investigated the reliability of social choice consistency measured via three choice consistency tasks in an online-experiment under non-incentivized conditions. We operationalized choice consistency via two popular and conceptually dissimilar quantitative indices, namely Afriat's critical cost efficiency index (CCEI) and the Houtman-Maks-Index (HMI).

Statistical and econometric power analysis confirmed sufficient power of our design. Analyses of the distribution of consistency indices showed that our participants, despite non-incentivization, behaved on average highly consistent and significantly more consistent than bootstrapped random deciders. Given this, our results confidently indicated generally poor inter-method reliability and at best fair test-retest reliability for CCEI (H1 and H2) and HMI (H3 and H4), with the CCEI being the tentatively more reliable measure (H5). This is especially concerning, since the full experiment (including test and retest measurement) lasted on average less than 45 minutes. Hence, it appears that estimations of choice consistency do not fulfill a basic requirement of valid psychometric measures. Further, results from different studies using different methodologies should not be compared without caution. However, there are three important limitations to our study.

First and foremost, neither of our choice tasks was incentive-compatible, which is an important feature in economic decision-making. While we cannot rule out that our participants might have behaved differently under full incentivization, we can robustly show that our participants behaved significantly more consistently than random bootstrapped deciders. Further, participants' introspections on their decision strategies do not raise concerns regarding artificial or non-interpretable response behaviour (see figure 3). Lastly, we imposed higher standards of valid choices than previous studies by implementing multiple attention checks. Therefore, overall, our data does not provide evidence for random or artificial behaviour.

The second limitation is that we only used tasks in the social domain of choice, similar to Andreoni et al. (2002). Hence, we cannot speak to the domain generality of our results, which is an important question for future research as we recently formulated elsewhere (Nitsch & Kalenscher, 2020a).

The third limitation is that we only investigated reliability for two out of many (however, conceptually closely related) consistency indices, e.g. the Money Pump Index ([Echenique et al., 2011](#)), Varian's Index ([Varian, 1991](#)) or the Minimum Cost Index ([Dean & Martin, 2016](#)). While an exhaustive evaluation of all existent consistency indices is beyond the scope of this paper, our openly shared dataset (<https://doi.org/10.17605/OSF.IO/QNFTU>) allows for the conceptual replication of our results via other consistency measures.

In conclusion, despite the above mentioned limitations, our results indicate that rationality in the interpretation of revealed preference consistency does not meet a fundamental requirement of valid psychometric measures, i.e. reliability. This suggests that choice consistency might rather be considered a characteristic of a specific collection of choices than an underlying trait variable of the decision-maker. This calls for caution in the interpretation of previous results in the field.

CRedit author statement

Felix J. Nitsch: Conceptualization, Methodology, Software, Formal analysis, Investigation, Data Curation, Writing - Original Draft, Visualization, Supervision, Project Administration. **Luca M. Lüpken:** Conceptualization, Software, Investigation, Writing - Review & Editing. **Nils Lüscho:** Software, Writing - Review & Editing. **Tobias Kalenscher:** Conceptualization, Writing - Review & Editing, Supervision, Funding acquisition.

Open Practices

Anonymized raw and processed data and analysis scripts are available at: <https://doi.org/10.17605/OSF.IO/QNFTU>. The experiment was not pre-registered.

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Tables

Table 1

Demographics

Variable	Frequency	Percent
Total	53	
Gender		
<i>Male</i>	23	56.604%
<i>Female</i>	20	43.396%
Education		
<i>No high school diploma</i>	1	1.887%
<i>High school diploma</i>	22	41.509%
<i>Jr./Comm. College degree / Undergraduate degree</i>	23	43.396%
<i>Masters Degree / Professional schools degree (Law, Medicine, etc.)</i>	7	13.208%
Monthly Net Income (in Pounds)		
<i>0-499</i>	16	30.189%
<i>500-999</i>	13	24.528%
<i>1000-1499</i>	8	15.094%
<i>1500-1999</i>	3	5.660%
<i>2000-2999</i>	5	9.434%
<i>>3000</i>	3	5.660%
<i>Omitted</i>	5	9.434%

Table 2

Choice consistency of participants compared to bootstrapped virtual deciders

Task 1	Measurement	Index	M (SD)	W	p
Diagram	1	CCEI	0.904 (0.183)	2309	<.001*
Bundles	1	CCEI	0.858 (0.157)	2162	<.001*
Slider	1	CEEI	0.891 (0.166)	2289	<.001*
Diagram	2	CCEI	0.929 (0.169)	2457	<.001*
Bundles	2	CCEI	0.867 (0.151)	2135	<.001*
Slider	2	CEEI	0.935 (0.148)	2488	<.001*
Diagram	1	HMI	19.065 (1.357)	2366	<.001*
Bundles	1	HMI	18.388 (1.483)	1979	<.001*
Slider	1	HMI	19.250 (1.014)	2476.5	<.001*
Diagram	2	HMI	19.458 (0.922)	2574	<.001*
Bundles	2	HMI	18.692 (1.322)	2098.5	<.001*
Slider	2	HMI	19.365 (1.172)	2504	<.001*
Bootstrap		CCEI	0.738 (0.151)		
Bootstrap		HMI	17.906 (1.197)		

Note: The asterisk denotes significance at $p < .05/12 = .004$ (Bonferroni corrected for 12 parallel tests).

Table 3

Reliability of the Critical Cost Efficiency Index

Task 1	Task 2	Measurement	t(df)	r	p
Diagram	Bundles	1	t(40)=0.043	0.007	.483
Diagram	Slider	1	t(37)=1.250	0.201	.110
Bundles	Slider	1	t(39)=-0.284	-0.045	.611
Diagram	Bundles	2	t(46)=0.752	0.110	.228
Diagram	Slider	2	t(45)=7.866	0.761	<.001*
Bundles	Slider	2	t(49)=1.248	0.176	.109
Diagram	Diagram	1 & 2	t(41)=5.088	0.622	<.001*
Bundles	Bundles	1 & 2	t(46)=3.324	0.440	<.001*
Slider	Slider	1 & 2	t(41)=1.864	0.280	.035

Note: The asterisk denotes significance according to the predefined threshold per hypothesis.

Table 4

Reliability of the Houtman-Maks-Index

Task 1	Task 2	Measurement	t(df)	r	p
Diagram	Bundles	1	t(40)=0.438	0.069	.332
Diagram	Slider	1	t(37)=0.690	0.113	.247
Bundles	Slider	1	t(39)=0.422	0.067	.338
Diagram	Bundles	2	t(46)=1.577	0.226	.061
Diagram	Slider	2	t(45)=7.584	0.749	<.001*
Bundles	Slider	2	t(49)=0.743	0.106	.230
Diagram	Diagram	1 & 2	t(41)=2.651	0.383	0.006*
Bundles	Bundles	1 & 2	t(46)=4.468	0.550	<.001*
Slider	Slider	1 & 2	t(41)=2.296	0.338	.013*

Note: The asterisk denotes significance according to the predefined threshold per hypothesis.

Figures

Figure 1
Distribution of CCEI in participants and bootstrapped virtual participants

Histograms of the Critical Cost Efficiency Index (CCEI) distribution of our participants (dark grey) in comparison to bootstrapped random deciders (light grey) for the 3 different tasks and 2 measurements. In all cases, real participants behaved significantly more consistently than the bootstrapped random deciders. Furthermore, the absolute levels of consistency are in alignment with consistency levels reported in the literature.

Figure 2
Distribution of HMI in participants and bootstrapped virtual participants

Histograms of the Houtman-Maks-Index (HMI) distribution of our participants (dark grey) in comparison to bootstrapped random deciders (light grey) for the 3 different tasks and 2 measurements. In all cases, real participants behaved significantly more consistently than the bootstrapped random deciders. Furthermore, the absolute levels of consistency are in alignment with consistency levels reported in the literature.

Figure 3
Qualitative content analysis of self-reported decision strategies

Relative frequency of self-reported decision strategy categories (N = 53). Results do not raise concerns regarding artificial or non-interpretable response behaviour.

Appendix A - Screenshots from Diagram, Bundles and Slider task

Depicted are the Diagram task (upper left), the Bundles task (upper right) and the Slider task (lower left).

Appendix B - Filler Task

Text 1: I start in Munich. I want to go to the Gasmanns. Alexandra Gasmann was nice on the phone, but also direct. She has nine children and she's a CDU (German political party) woman. She will tell me off, just think of Seehofer with Röttgen (German politicians) back then. They know how to do that. I'm here because sometimes I would like to have an even bigger family, but my wife doesn't really, and our life is also not exactly what I would call zen. I will test this, being a "dad for a day", because the real dad is at work until noon. Let's do it! Mrs. Gasmann gets up at 5 am every day because she also just wants to take a breath for five minutes in the morning without hearing "Mommy" right away and having to be there. It's her chill area in the kitchen while making sandwiches from 5-6 am in the morning. I get that. Samantha has to go to school, Ferdinand too, Jakob is still hanging around before daycare starts, and I'm about to take Gwendoline to the school bus. The older ones are already on their way. I'll say it right away, this is not a posh family, wrong channel. I have to drag the toddler to daycare, he doesn't want to walk at all. His peers and his friends don't have as many siblings as he does, but he thinks it's "cool", and he wouldn't want to be alone with mom and dad under any circumstances. Next, I go grocery shopping with Mrs. Gasmann and ask her if she sometimes gets stupid remarks. "Don't you have a TV at home? Did you really need that now, too?". Remarks like "Are you too stupid to use contraception" are part of the daily routine. Mrs. Gasmann has days on which such remarks bounce off of her and days on which she gets really grumpy. However, when asked if she wants to have more children, she replies that she would be very happy if that worked out. Alexandra buys huge bags of groceries every day. How do they manage with the money? They have a fantastic rental contract, then child support, Alexandra is paid a little for her work in the district parliament and her husband Arthur is a master butcher. It is enough for private schools, but food is mostly from the discounter. Alexandra buys 22 pounds of potatoes and about 125-134 cups of milk a week. The Gasmanns have two apartments: Downstairs the youngest live with the parents, upstairs the older kids, and there is also the laundry room. They have three washing machines and Alexandra washes 18 loads a week and also irons everything. She always has the best ideas, she tells me.

Text 2: "You are what you eat", goes the popular saying. It means what we eat is the cornerstone of our life. It determines our health, our mobility and also our education. But you can't teach an old dog new tricks. In other words, if you don't learn to eat healthy as a child, you won't learn later, and then you won't teach your children either. In Germany it is particularly serious. There is hardly any other European country with so many malnourished adults and children. Fortunately, there is school, where you learn for life. In the best case scenario, nutrition would be a compulsory subject and the students would go to the cafeteria with their teachers after class. There, the food would be prepared by trained chefs, and the students could immediately apply what they have just learned. But the reality is different. In Germany, it has to be practical - and cheap. More than a third of the schools we surveyed complained about a budget that was too tight. On average, a meal may only cost students €2.50, but a healthy meal costs at least €4 per serving. In many schools, good quality is, therefore, not possible. Most of the time, the food is cooked elsewhere and then delivered hot, so it is kept warm for several hours. This is also the case in about half of the schools we surveyed, including two-thirds of the elementary schools. What finally ends up on the plate is overcooked with no trace of vitamins. There is hardly any variety. If several dishes are offered at all, the students often have to decide a week in advance what they would like to eat. By far not every school, where students eat, has its own cafeteria, and if one exists, it is often cold, cramped or uncomfortable. Churches are no different, to save money, the food is often prepared and served by unlearned and poorly paid helpers. Hygiene regulations are rarely known, or adequate washing or sanitizing facilities are lacking. As a result, no one likes to go there. If they can, students prefer to go to the kiosk instead and eat there. According to experts, it would cost the state about 500 million euros a year to provide all students with a healthy lunch. That is only a fraction of what is spent on military weaponry every year. Why are investments made for this, but not for the children? In fact, responsibility is shifted from the state to the communities and often from the communities to the schools. They, in turn, transfer much of the responsibility to caterers who are supposed to deliver cheaply. No one wants to be responsible. Yet, good school catering would have many advantages. The children would not only be healthier and perform better, but they would also know more about healthy eating. In the long run, this would even save money, because, in the future, there would be fewer cases of illness society would have to pay for, and there would be more educated people who also earn more. The bottom line is a healthier and richer society.

Text 3: This is me, Tobias. Normally I see the world from 6 feet. For seven days, I want to leave this perspective and explore the world of the little ones. This daycare here in Otterndorf on the North Sea is perfect for that. 130 children come and go here. What do they do all day? What does life look like from a height of 3'6"? What do children think of our adult world? What can I perhaps learn from them? - That's what this text is about. Food determines the time reckoning here. There are three times of day: Before the meal, after the meal and of course the meal itself. The entire process is accompanied by an impressive ritual: "Beep, beep, little mouse, come out of your little house. Little mouse says "beep, bon appétit!". On my first day, the little ones eat large amounts of rice and a strange brown sausage gravy. The rest of the week, the menu includes woodruff jello and whole wheat pasta with custard. All that

high-energy food must of course be broken down, preferably by “fighting”. Imagination is the best recipe. Everyone used to have it. The children say they have coffee here, but that it doesn't taste good and that you get a stomach ache if you drink too much of it. Rationality always comes at the expense of fun. This sobering principle becomes clear when adult voices interfere with the little ones' plans and routines: The kids have to help clean the bathroom if they don't behave well. During sports, it quickly becomes evident that with the first leap over the box, the disappointment of earlier is forgotten: Life is always lived in the here and now. The moment counts. All the things that we adults have to painstakingly relearn in yoga-zen-mindfulness seminars, they can all do here, just like that. When did I lose that? A daycare day lasts about three adult days, but at some point, it is still closing time, usually around 5 pm.

Memory test:

1. What time does Mrs. Gassmann get up every morning?
 1. 5 am
 2. 6 am
 3. 7 am
2. How much would it cost the German government to provide a healthy lunch?
 1. 200 million pounds
 2. 300 million pounds
 3. 500 million pounds
3. According to the children, which drink causes a stomach ache?
 1. Coffee
 2. Coke
 3. Beer

Appendix C - Test-Retest Reliability

Depicted are the test-retest reliability of each task for the CCEI and HMI (in black). For comparison, in grey, 53 bootstrapped deciders are depicted. The dashed line indicates optimal test-retest reliability ($r=1$).