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Would you lie about your mother's birthday? A new online dishonesty experiment☆

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ABSTRACT

We ask a representative sample of German household decision-makers to enter their mother's birthday, with potential payments depending on the month and the day they state. Thus, we create an incentive to lie. Compared to the die-under-the-cup experiment, our alternative has a lower probability that the income-maximizing outcome is true. Furthermore, it is better suited for online surveys and samples in which gambling is socially stigmatized. We conduct different variations of this game to crystalize design recommendations for researchers interested in our tool. Participants lied to receive higher payoffs, but only with real monetary incentives and only to a relatively small extent. Our results are largely insensitive to several design elements that we vary, such as the probability of being paid and the magnitude of the payoffs.

1. Introduction

There are two ways to investigate dishonesty in economic experiments. One is to allow participants to lie to other subjects like in the sender-receive game (Gneezy, 2005), and another is to allow them to misreport something to the experimenter, with "something" typically being the outcome of a die roll. The latter design, introduced by Fischbacher and Föllmi-Heusi (2013), is extremely simple and immediately became very popular; for example, it has been used to investigate the four-eyes principle (Beck et al., 2020), effects of gender on dishonesty (Muehlheusser et al., 2015), the impact of a loss-framing on lying (Schindler & Pfattheicher, 2017), lying under competition and in groups (Dannenberg & Khachatryan, 2020; Leib et al., 2021), and lying for the benefit of others (Buckle et al., 2021). Thielmann and Hilbig (2018) use the die-under-the-cup design to demonstrate that subjects' behavior is consistent with Becker's (1968) classical theoretical predictions: The willingness to lie strongly decreases with increasing probability of being caught and increasing severity of sanctions. Shalvi et al. (2011) modified the die-under-the cup paradigm allowing just one roll instead of three (of which only the first is supposed to count). In this treatment, they find less evidence for lying, possibly because it reduces the opportunities for self-justification and because of lying aversion (Gneezy et al., 2018).

However, dice experiments come with three drawbacks. One is that there is a probability of 1/6 of actually getting the payoff-maximizing outcome, thus one can never classify individual subjects as liars. Second, the method is unsuitable for online survey experiments as real dice are typically not available to subjects and they might not trust digital simulations of die rolling if these do not originate from external websites (Lilleholt et al., 2020). And third, gambling is restricted and regulated in some religions and cultures (Brešan, 2020). Rolling a die or flipping a coin to determine the payoff could be considered a "gamble".[1] Moreover, Alfonso-Costillo et al. (2022) demonstrate in their online die-under-the cup experiment that the alignment of dice numbers and monetary prizes seems to exaggerate cheating behavior.

This motivates us to create and try out an alternative experimental design. We ask a representative German household sample to enter their

☆ We preregistered our experiment in January 2021 at AsPredicted and obtained ethics approval from the Institutional Review Board of the German Association for Experimental Economic Research. Our dataset can be found here: https://doi.org/10.17632/r3mm7z6758.2.

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[1] Please note that the experiment per se might already be perceived as a form of "gamble". We owe this point to an anonymous reviewer.

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mother's birthday, with potential payoffs depending on the stated day and month. We assume that almost everybody knows the birthday of one's mother, but we create a clear incentive to lie with our payoff structure. Furthermore, we address all three problems mentioned above:

(1) The likelihood that payoff-maximizing answers are true is low, leaving us with a low share of false positives when we presume that a "31″ was a lie.
(2) Our method is suitable for online experiments.
(3) It does not violate any anti-gambling norms because it is based on a personal question.

With this experiment, we are trying to shed new light on lying behavior using a large representative sample of the general population and several treatment variants. In the treatments, we vary the probability of being paid, the magnitude of payoffs, the possibility to skip the question and to use the back button, and the date that respondents are supposed to enter. In a further treatment, we display the resulting payoffs that corresponds to the dates entered (live calculation). By testing these design options against each other, our study provides clear recommendations about how to implement and design online studies.

Our paper is organized as follows: Section 2 discusses how previous experiments offered subjects the opportunity to lie, some of them being unfeasible online, however. Section 3 introduces our novel preregistered design, treatments, research questions, and hypotheses. Section 4 presents the results, and Section 5 concludes.

2. Lying in online experiments

Fischbacher and Föllmi-Heusi (2013) let subjects, unobserved by anyone, roll a die and report the result to the experimenter – with the payoff depending on the reported result. The simplicity of this method to confront subjects with an incentive to lie instantly led to a large number of similar studies, one of the milestones being the study by Gächter and Schulz (2016) with 2284 subjects from 23 countries. The budget and manpower needed to perform their experiment in a lab naturally lead to the question of whether an online version might be possible. As shown in the overview of Garbarino et al. (2019), online dice experiments are rare compared to their lab versions before the pandemic. Two reasons for this come to mind: First, dice might be unavailable (or hard to find) in many private households, and second, "there is no way of controlling that subjects actually roll a die instead of just reporting some number" (Kroher et al., 2015, p.316).[2] The use of an online dice simulation (as in Suri et al., 2011) seems to solve these problems, but possibly creates a new one: Can we expect subjects to trust the promise that they will not be observed?[3] Can we expect that experimental demand effects are smaller in online compared to lab settings? At least the issue of online practicability has been dealt with by replacing dice with coins (e.g., Abeler et al., 2014). This is the dominant method in online versions. However, researchers can classify reported coin tosses as likely lies only if they bother their subjects with multiple repetitions. Furthermore, like dice rolling, coin tossing might violate a religious norm.

Garbarino et al. (2019) report no alternative to dice and coin experiments that has been used in online experiments. An original example of a design that is impossible to implement in an online or telephone survey is the one by Djawadi and Fahr (2015), whose subjects can decide to put a raffle ticket into a box despite not being entitled to do so. Another design depending on physical devices is a straightforward extension of dice throwing by Gneezy et al. (2018), who let subjects draw numbers written on paper from envelopes.[4]

Hugh-Jones (2016) used a quiz composed of six questions, such that it is highly unlikely that people know all the answers – just claiming to have known the answers was also possible, however.[5] A drawback is that in addition to the payoff and possibly lying aversion, a third motive enters: Appearing as a knowledgeable subject might be an objective in and of itself.

Another alternative is the Mazar et al. (2008) real effort task, later also used in Kajackaite (2018), amongst others: From matrices of numbers, subjects have to detect those pairs that add up to exactly 10. Subjects can lie about the number of pairs found. The task is time-consuming, however, and saving time can be an additional motive for lying – especially in online experiments.

An interesting feature of our design is the true unobservability of the underlying information that subjects can choose to misreport on for financial gain. Variations of the dice experiment that focus on this feature of unobservability are referred to as "mind games" (e.g., Jiang, 2013; Kajackaite & Gneezy, 2017). Here, subjects are first asked to think of something (a dice side or a number), and only afterward roll a die. Payoff-maximizing rules differ from case to case, but the general design feature of "mind games" is that subjects can misreport the private information held in their mind about the die roll. Olsen et al. (2019) implement this design with an online dice feature.

We know of four other studies that have the similarity of asking for birthday information: Heinicke et al. (2019), Werthschulte et al. (2021), and Hermann and Brening (2022). Heinicke et al. (2019) introduce the "Even-Odd task" in their online experiment and let subjects calculate the sum of privately known numbers. A random device decides if even or odd sums are incentivized. In one of ten questions, participants should sum up the four digits of the day and month of a relative's or friend's birthday. Ninety-eight participants of their baseline treatment reported on average 6.7 (instead of 5) favorable outcomes. Similarly, Werthschulte et al. (2021) incentivize the reporting of even vs. odd months of the mother's birthday. With even months being incentivized, approximately 64 % of all participants in this experiment ($N = 596$) reported an even birth month, i.e., 14 percentage points more than expected. Likewise, Hermann and Brening (2022) incentivize the reporting of even years in which the participants' mothers are born, and 82 % ($N = 563$) report an even birth year. Charness and Rodriguez-Lara (2024) ask subjects to report whether their own year of birth is odd or even. We refrained from asking for *subjects'* years of birth as they might presume that we know the true birthday anyway, for example through past panel screenings.

Summing up, despite the considerable number of experimental designs for dishonesty research, there is still justification for modified designs. The next section describes our novel design and our hypotheses.

[2] However, also in lab experiments, subjects typically roll the die in isolation.

[3] Bereby-Meyer et al. (2020) try to avoid this problem by letting their online participants roll the die on an unaffiliated website (www.roll-dice-online.com). However, their subjects might suspect that this site is only seemingly unaffiliated. This was the case in the experiment of Pascual-Ezama et al. (2020), who observe die rolls that their subjects only thought to be private.

[4] Gneezy et al. (2018) also implement an online version of this experiment, letting subjects click on a box on the screen and letting them report the number that appeared, but that means that subjects know that their lies will be detected, like letting them throw dice while looking over their shoulder. The same holds for Innes (2022), who introduces an inventive (lab) variant of López-Pérez and Spiegelman's (2013) sender-receiver game that allows him to compare deceptive and dishonest lies.

[5] Alternatively, subjects could violate the rules by looking the answers up via google, crossing the line from lying (misreporting the state of the world) to cheating (manipulating the state of the world). Another example would be the task invented by Drupp et al. (2019): Producing as many paper shreds as possible from an A7-sized paper, some subjects added shreds from additional own paper.

3. Experimental design, research questions, and hypotheses

3.1. Experimental procedure

The experiment took place as part of a representative online survey of German household decision-makers in April and May 2021. In total, our sample comprises 2689 participants who are representative of German household decision-makers in terms of age, gender, place of residence, and high school graduation rate. Household decision-makers are defined in this context as individuals who are at least 18 years old and are primarily responsible for all types of decisions made in their household or equally responsible as another person.

The survey was conducted in cooperation with the professional market research company Psyma+Consultic GmbH (Psyma). The company was responsible for programming and hosting the survey and recruiting participants. To ensure representativeness for German household decision-makers, Psyma first recruited participants according to quotas for age, gender, place of residence at the federal-state level, and percentage of high school graduates in the overall German population. In the second step, participants were asked whether they were involved in decision-making processes in their households. Those who indicated that they were not involved were screened out. In addition, for quality reasons, participants were excluded from the sample if they failed any of the attention checks in the first parts of the survey before the experiment. These participants were replaced representatively, so this exclusion rule did not affect the sample size or the sample's representativeness. In addition to the dishonesty experiment, the survey consisted of several parts relating to economic preferences and individual attitudes, environmental and climate issues, and sociodemographic and socioeconomic characteristics. It also included a stated choice experiment on electricity tariffs (von Loessl, 2023), which is not considered in this paper. Since the other parts of the survey, including the choice experiment, were not incentivized and the participants did not know that they were part of more than one study, we do not expect any ordering effects from the survey parts that took place before the dishonesty experiment. The participants were assured that their responses would be evaluated anonymously and that the survey complied with data protection laws.

On average, respondents took 2:35 min to answer all experiment-related survey questions. These are 1) the question about the mother's birthday, 2) the question about whether the previous answer was deliberately false, 3) the questions about the expectations regarding the proportion of those who lied and those who would admit it, and 4) three survey items regarding the judgment of dishonest behavior. The average payoff for the dishonesty experiment across all subjects is €1.29, which translates into an hourly wage rate of €30.

3.2. Treatments

Table 1 gives an overview of the nine treatments implemented in the dishonesty experiment. The first treatment (T1) did not provide an incentive for lying and was stated as:

At this point, we ask you for the dates of selected days.
Please enter your mother's birthday here:

In all treatments, participants selected a day, a month, and a year from three drop-down menus to enter the date. Obvious misreporting, such as 'February 31st', was not possible. If participants did not want to answer the question, they could check a "Don't know" or "No answer" box.[6]

The treatments T2 to T9 were incentivized. In the second treatment (T2), all participants that entered a date received a payoff depending on the date entered. Specifically, the treatment was stated as:

At this point, you have the opportunity to receive a payoff: All participants will be paid out according to their entries. The amount of the payoff depends on your mother's birthday, which you please enter below, and is calculated according to the formula:

$$Payoff = day{*}50\ cents + month{*}10\ cents$$

To calculate your payoff, we multiply the given day by 50 cents and the given month by 10 cents and sum it up.
Please enter your mother's birthday here:

In the third treatment (T3), only 10 % of the participants that entered a date received a payoff. To obtain the same expected value for the payoff as in T2, the euro amounts in the payoff rule were multiplied by 10, resulting in the following instructions:

At this point you have the opportunity to receive a payoff: We will draw 10 % of the participants who will be paid out according to their entries. The amount of your possible payoff depends on your mother's birthday, which you please enter below, and is calculated according to the formula:

$$Payoff = day{*}€5 + month{*}€1$$

To calculate your payoff, we multiply the given day by 5 euros and the given month by 1 euro and sum it up.
Please enter your mother's birthday here:

Table 1
Overview of the treatments.

Treatment	Maximum payoff (Probability of payoff)	Observations (incl. those who skipped)	Brief description
T1	€0 (100 %)	802 (923)	Participants receive no incentive.
T2	€16.70 (100 %)	119 (128)	Participants have a 100 % probability of payoff depending on the specified birthday of their mother.
T3	€167.00 (10 %)	119 (128)	Participants have only a 10 % probability of payoff, but an identical expected value as in T2.
T4	€16.70 (10 %)	221 (251)	Participants have a 10 % probability of payoff, with the same payoff rule as in T2.
T5	€16.70 (10 %)	233 (252)	Same as T4, with the additional feature of a live calculation for the possible payoff.
T6	€16.70 (10 %)	252 (252)	Same as T4, with the participants unable to skip the question.
T7	€16.70 (10 %)	229 (251)	Participants are presented with a randomly generated date and are asked on the following screen to enter that date. Payoff rules are the same as in T4.
T8	€16.70 (10 %)	241 (252)	Same as T7, with the additional feature that participants are given the possibility to view the randomly generated date again.
T9	€16.70 (10 %)	237 (252)	Participants are asked to enter the date of a given public holiday. Payoff rules are the same as in T4.

[6] Screenshots of all treatments in the original German version can be found in the Online Appendix.

The fourth treatment (T4) is a mixture of T2 and T3. The payoff probability is the same as in T3 (10 %) and the payoff rule is the same as in T2. As a result, the expected value for the payoff in T4 is lower than in T2 and T3. Accordingly, the instructions were given as:

At this point you have the opportunity to receive a payoff: We will draw 10 % of the participants who will be paid out according to their entries. The amount of your possible payoff depends on your mother's birthday, which you please enter below, and is calculated according to the formula:

$$Payoff = day*50\ cents = month*10\ cents$$

To calculate your payoff, we multiply the given day by 50 cents and the given month by 10 cents and sum it up.
Please enter your mother's birthday here:

The last two treatments that refer to the mother's birthday are T5 and T6. The instructions are identical to T4, except that in T5 a live calculation of the possible payoff was added directly below the drop-down menus for entering the mother's birthday, and in T6 no "Don't know" or "No answer" box was offered so that the question could not be skipped. The reason for including T5 is that a live payoff calculation is a standard option of experimental software, hence evidence whether it has an impact on behavior is worthwhile. T6 is a typical design option as well, already since the time of pen-and-pencil studies.

In the seventh treatment (T7), we did not ask participants to enter their mother's birthday but a randomly generated date that we showed them directly below the instructions. The payoff probability and payoff rule were the same as in T4. The treatment was stated as:

At this point you have the opportunity to receive a payoff: We will draw 10 % of the participants who will be paid out according to their entries. Below you will be shown a randomly selected date. The amount of your possible payoff depends on this date, which you please enter in the following, and is calculated according to the formula:

$$Payoff = day*50\ cents + month*10\ cents$$

To calculate your payoff, we multiply the given day by 50 cents and the given month by 10 cents and sum it up.
Please enter in the following the date shown here for your payoff: [randomly generated date]
[New screen] Please enter the date just shown to you here:

The eighth treatment (T8) had the same instructions as T7. The only difference was that participants could return from the date input screen to the instructions screen and view the randomly generated date again.

Finally, in the ninth and last treatment (T9), we did not ask the participants to enter their mother's birthday but the date of the holiday New Year's Day. The payoff probability and payoff rule were again the same as in T4. Specifically, the treatment was stated as:

At this point you have the opportunity to receive a payoff: We will draw 10 % of the participants who will be paid out according to their entries. Below you will be shown a randomly selected holiday. The amount of your possible payoff depends on the date of this holiday, which you please enter in the following, and is calculated according to the formula:

$$Payoff = day*50\ cents + month*10\ cents$$

To calculate your payoff, we multiply the given day by 50 cents and the given month by 10 cents and sum it up.
Please enter the date of the New Year's Day holiday here:

Table 1 gives an overview of the nine experimental treatments. In total, 2453 of the 2689 survey participants took part in the experiment. The remaining 236 skipped the mother's birthday question and therefore did not receive any payoff. Survey participants were randomly assigned to treatments, with about 30 % scheduled to go to the no-incentive treatment (T1), about 5 % to the higher-payoff treatments (T2 and T3), and about 10 % to the lower-payoff treatments (T4 to T9). The summary statistics by treatment, provided in Table A2 in the Online Appendix, show that our treatment groups are well-balanced concerning the elicited characteristics, providing evidence of successful randomization.

3.3. Research questions and hypotheses

The treatments in our experimental design were designed to address six research questions. A graphical representation of the relationship between treatments and research questions is shown in Fig. 1.

Research question 1 (RQ1): Do financial incentives alter the inclination to lie?

Experimental economics uses real monetary payoffs to incentivize subjects' decisions. Seldom, hypothetical incentives are used as a treatment, for the sole purpose of learning what difference it makes to use them. Lee (2007) compares 24 such studies, finding that in half of the cases, "financial incentives lead subjects to an improved performance over hypothetical incentives" (p. 648), while only two studies found the opposite effect. Lee (2007) defines performance to be improved if it is closer to a normative prediction. This prediction is unclear in our particular experiment, however, as honesty and profit maximization are mutually antagonistic. Hence, our investigation of the effects of incentives is worthwhile, even though the issue has already received considerable attention. For example, the Gerlach et al. (2019) meta-study on dishonest behavior illuminates that "Increasing the incentive was associated with higher standardized reports in sender–receiver games but not in the other experimental paradigms" (p.19).

To address RQ1, we compare T1 with T2. We expect that a financial incentive to lie might increase the inclination to lie.

Research question 2 (RQ2): Does the probability of receiving a payoff (10 % vs. 100 %) alter the inclination to lie?

Randomly picking the subset of subjects who actually will be paid is an established experimental practice. A well-known study by Bolle (1990) has shown that subjects paid randomly do not behave markedly differently from those receiving certain payoffs in the ultimatum game. Celse et al. (2019) find similarly in a modified die-under-the-cup experiment that dishonesty seems to be independent of payoff uncertainty. However, for some types of experiments, Charness et al. (2016, p.148) have put forward a plausible concern. If subjects have to incur moral costs to obtain a monetary payoff (e.g., lying to get more money), then paying randomly would reduce the benefits of norm-violating decisions, while the moral costs remain the same. Of course, this concern almost completely hinges on random pay also reducing the *expected* payoff. In our experiment, we try both: Random pay with reduced and random pay with preserved monetary incentives, allowing us to also answer the next research question.

To address RQ2, we compare T2 with T3. In these groups, respondents have identical expected payoff values. However, they have different probabilities of receiving a payoff. We have no set expectations for this comparison.

Research question 3 (RQ3): Does the expected payoff value alter the inclination to lie?

It might be intuitive to presume that the inclination to lie increases when the stake does. However, at a second glance, it is not only the benefits of a lie but also its costs that might increase when financial incentives get higher. This argument has been put forward by experimenters who found even less lying for higher stakes (Cohn et al., 2019, p.71; Le Maux et al., 2021, p.25). Across all studies covered in the meta-analysis by Abeler et al. (2019), stake size does very little to explain differences in lying, but this might not hold under all circumstances. Kajackaite and Gneezy (2017) report that lying increases when subjects can be sure they cannot be caught: they win if they roll a die and

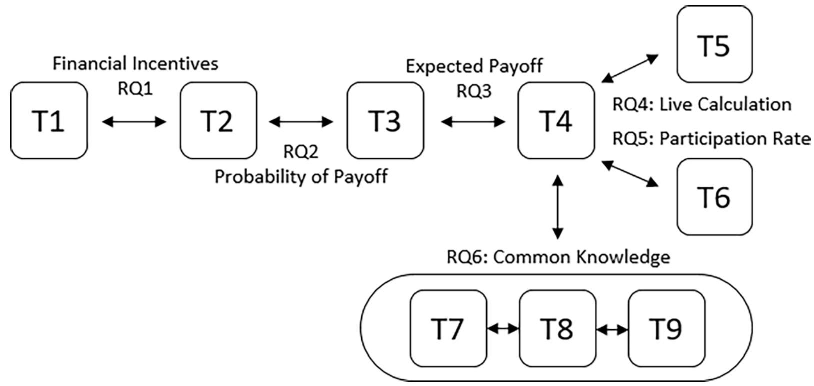

Fig. 1. Treatments and research questions.

the outcome is identical to the number they thought of before in private. In this treatment, but not in a control treatment where rolling "5″ wins, the inclination to lie increases with stake size. Chua et al. (2022) do not vary the stake sizes but the percentage of stakes being shared with charity – either 0, 10, 50, 90, or 100 %. They find similar and significant lying behavior in all treatment groups, except for the one that shares 100 % with charity and keeps nothing of the payoff.

To address RQ3, we compare T3 with T4. It is intuitive to expect that the ten times higher expected value might increase the inclination to lie, despite the result of the meta-analysis by Abeler et al. (2019).

The following three research questions are not motivated by previous experiments' results but instead, try to answer practical questions concerning design options in online experiments. While we use the options as treatments, others might have only one of these options due to technical restrictions, or they might wish to know whether preferring one over the other could lead to different results. An indication that this might happen is a finding by Bryan et al. (2013): A small change in a design detail – replacing the verb "cheating" with "being a cheater" in the instructions – made a difference in the relative frequency of lying. Our own differences between treatments are of different size, so to say, though in other domains: We vary the following design elements: no live calculation versus live calculation of the payoff, voluntary versus mandatory participation in the experiment, and private versus common knowledge. The following research questions arise from these variations:

Research question 4 (RQ4): Does the live calculation and visualization of the possible payoff alter the inclination to lie?

To address RQ4, we compare T4 with T5. We expect the visualization of the potential payoff, as well as the live calculation while respondents enter (and potentially modify) the date, might increase their inclination to lie. Furthermore, people may have difficulties in understanding the consequences of their decisions and thus might be reluctant to lie. The live calculation option may dismiss the possibility that confusion might play a role in the observed decisions.[7]

Research question 5 (RQ5): Does the skip rate depend on the treatment conditions and does the inability to skip the question alter the inclination to lie (or the underlying sample structure, which might alter the inclination to lie)?

We address the first part of RQ5 (RQ5.1) in our secondary analysis, where we compare the proportion of people who skip the question across the first five treatments. We expect equal skip rates across all treatments. To address the second part of RQ5 (RQ5.2), does the inability to skip the question alter the inclination to lie, we compare T4 with T6 in our main analysis. We do not expect many people to forgo an additional potential payoff and refuse to answer the main question in the other treatments. Thus, T6 was designed as a control treatment just in case many participants were to skip the question.

Research question 6 (RQ6): Does common knowledge that the researchers might know the correct answer to the question alters the inclination to lie?

Hermann and Brening (2022) use a variant of the "Even-Odd task" of Heinicke et al. (2019). Participants in their *observable* treatment report if the last digit of their randomly generated experiment completion code is even or odd and participants in their *unobservable* treatment if the year of their mother's birthday is even or odd. With even numbers being incentivized, 82 % of the participants report that their mother's birth year is even, and 72 % that their completion code ends with an even number.

To address RQ6, we compare T4 with T7. We expect that people are more likely to lie about a date when it is private information (i.e., their mother's birthday in T4) versus information that is possibly available to the researchers (randomly generated date shown to participants in T7). In T8, with the additional offer to see this date again, the inclination to report a wrong date might further decrease compared to T7. Furthermore, we analyze the frequency of misreporting in T7-T9, i.e. how often participants wrongly report the randomly shown date (T7-T8) as well as the date of the public holiday (T9).

3.4. Additional questions for the secondary analysis

On the screen that follows the main question (incentivized in all treatments except for T1), we asked respondents whether they purposefully entered a false date. At this point, they also had the option to go back to the screen before and alter their entries. We recorded whether respondents chose this "back button" and the adjustments they made. We expected some respondents to take up this option to modify the entered date. Two scenarios are plausible: Either subjects change a dishonest to the honest answer to feel less guilty, or they change the honest to a dishonest answer to increase their payoffs. We hypothesize that respondents change the date in a payoff-increasing (and likely maximizing) way.

Furthermore, we assessed the respondents' beliefs about the share of participants who would admit to having lied. We incentivized the best 20 guesses with €10 each. Afterward, we assessed the belief about the share of participants who actually lied.[8]

Alongside respondents' socio-demographic characteristics, their economic preferences (i.e., risk, trust, patience, and altruism based on Falk et al., 2016, 2018) and their political orientation (i.e., conservative,

[7] We owe this point to an anonymous reviewer.

[8] Since we do not know the proportion of participants who actually lied, we did not incentivize this question.

social, liberal, and ecological based on Ziegler, 2017),[9] we additionally ask three survey items referring to the personal judgment of dishonest behavior and include those items in our regression analysis. These items ask how people judge a household's tax fraud of €500, taking public transport without a valid ticket, and shoplifting items worth €25, on a 5-point scale from "not bad at all" to "very bad".[10] We expect people who have a greater aversion to these actions to be less likely to lie (i.e., less likely to state a high day or month) compared to people who consider the three described behaviors as fairly unproblematic.

In summary, we consider the following aspects relevant for our secondary analysis: 1) whether respondents admit to lying as well as their beliefs about the share of liars and those who admit their lie, 2) the skip rate across treatments (i.e., RQ5.1), 3) whether respondents use the back-button and modify their initial entry, 4) participants' response time, and 5) further determinants of lying, such as socio-demographic characteristics as well as political and economic preferences.

4. Results

4.1. Mother's birthday treatment effects

In this subsection, we concentrate on our first six treatments, representing our mothers' birthday experiment. Table 2 presents the average birthdays and birth months stated by participants across treatments. Furthermore, *Percent >=25th* and *Percent >=30th* indicate the share of participants who stated birthdays greater or equal to the 25th or 30th day of any month, respectively.[11] Finally, ***r*** corresponds to the standardized measure developed by Abeler et al. (2019, p.1120) for the difference between reported outcomes and the statistical expectation discussed below. Figs. 2 and 3 show the histograms of the reported birthdays and birth months. Figures A1 and A2 in the Online Appendix show the respective violin plots.

Ex ante we see no reason for parents to prefer May 31st over June 1st as their child's birthday. Nevertheless, we rule out the possibility that real birthdays are more likely to occur on the last days of a month by showing (in Table 2 as well as Figs. 2 and 3) results for a representative sample of birthdays retrieved from the Socio-Economic Panel (SOEP) wave 2020 (Richter & Schupp, 2015).

The average birth months do not differ significantly by treatment (Fig. 3) and from the expected value of around 6.5.[12] As we incentivize the reported days much higher than the months (€0.50 vs. €0.10), we concentrate our analyses on mothers' birthdays in what follows. On average, we observe that participants in T1 reported lower days than expected.[13] We can see from Fig. 2 that not incentivized respondents (in T1) choose days from 1 to 7 especially often. In contrast, mothers' birthdays after the 24^{th} and especially after the 29^{th} are overrepresented in T5.

The standardized measure of lying developed by Abeler et al. (2019) considers the difference between reported outcomes and statistical expectations. Adapting the notation of their measure ***r*** (Abeler et al., 2019, p.1120) to our specific case,

$$r = \frac{d - E[d^{true}]}{d^{max} - E[d^{true}]} \ \ if \ d \geq E[d^{true}]$$

and

$$r = \frac{d - E[d^{true}]}{E[d^{true}] - d^{min}} \ \ if \ d < E[d^{true}].$$

$E[d^{true}]$ is the expected true day on which mothers are born, d is the reported day, d^{min} is the lowest possible d (thus, 1), and d^{max} is the highest possible one (31). $E[d^{true}]$ for birthdays is 15.72964.

In their meta-study covering 90 studies (most of them dice experiments), Abeler et al. (2019) find an average r of 0.234. We observe much smaller values of r, in T1, T3, and T4 even negative values in Table 2. The highest r in our sample is 0.0793 in T2.[14]

To analyze our research questions RQ1 to RQ5.2, we use Mann-Whitney U tests for the mothers' birthdays and two-sided Fisher exact tests to check if the reported birthdays are higher than or equal to 25 or not and higher than or equal to 30 or not, respectively. Table 3 presents the corresponding p-values for each research question corrected for multiple hypotheses testing (Benjamini et al., 2006; Anderson, 2008). According to the non-parametric tests, participants in T2 – with sure payoffs depending on the stated birthday – do not lie significantly more than in T1 without payoffs (RQ1). Furthermore, we see that the probability to receive a payoff in T2 vs. T3 (RQ2) and the expected value of the payoffs in T3 vs. T4 (RQ3) do not affect the answers. Live calculations of the payoff in T5 do not significantly increase the probability of entering higher birthdays compared to T4 (RQ4). Not allowing the question about the mother's birthday to be skipped in T6 does not change the responses compared to T4 either (RQ5.2).

Table 4 presents a summary of OLS and Tobit regressions for the (nearly) continuous dependent variable 'day'. Furthermore, we use marginal and discrete effects of Probit regressions to determine the main factors for the inclination to lie concerning the dependent variables '>=25′ and '>=30′. In contrast to the analyses in Table 3, the results of the OLS and Tobit regressions suggest that participants report significantly higher birthdays in T2 compared to T1 (RQ1). However, this result is driven by lower birthday reports than expected in T1 and not by higher reports than expected in T2 (see Table 2). Moreover, in line with Table 3, the marginal effects of the Probit regressions are not significant. The only significant effect in the Probit regressions indicates that live calculations increase the probability of entering the two highest birthdays possible by 5.3 percentage points (RQ4). However, this tendency is not confirmed by the OLS and Tobit regressions.

4.2. Additional analyses

In this subsection, we first discuss RQ6: lies in disguise vs. open lies and misreporting in T7-T9. Furthermore, we present the results of additional analyses concerning participants' beliefs, skip rate, use of the

[9] We elicit respondents' economic preferences as well as their political orientation via 5-point scales. The English translations of the questions used are shown in the Online Appendix.

[10] The questions originate from the "co: MTMM Experiments 2" presented in GESIS Panel Study Description (August 2020).

[11] We preregistered the analysis of these variables to get a better intuition of the distribution of the outcome variable *day* and the potential extent of lying by treatment.

[12] The probability of observing January, March, May, July, August, October, and December is 8.4873 % (31/365.25) each, April, June, September, and November 8.2136 % (30/365.25) each, and February 7.7344 % (28.25/365.25). The expected value for *month* is (1 + 3 + 5 + 7 + 8 + 10+12)*0.084873+(4 + 6 + 9 + 11)*0.082136+2*0.07344=6.5151. We do not find evidence that participants who report a high number for *day* indicate a low number for *month*. Our expectation was that respondents who lied about the *day* to receive more money would indicate a low number for *month*, which might be a cheap way to make them feel less guilty. We rather observe the opposite, but only a very weak positive correlation between days and months (Spearman r = 0.06, p = 0.01). This stands in contrast to the findings of Barron (2019), where subjects exaggeratedly try to appear honest in a situation with little incentive to legitimate their lies in high-incentivized decisions.

[13] The expected value for a *day* is $\frac{(\sum_{i=1}^{28} i*12) + 29*11.25 + 30*11 + 31*7}{365.25} = 15.72964$.

[14] We conducted a pen & paper version of our experiment with staff and students from the University of Kassel (N = 533) and paid 10 % of the participants. The results were similar to the online version, but r was slightly higher: 0.1108.

Table 2
Overview of the reported days and months by treatment.

Treatment	Obs.	Avg. day	Std. dev. day	Percent >=25th	Percent >=30th	r	Avg. month	Std. dev. month
T1	802	14.94	8.89	18.85	4.11	−0.0538	6.23	3.50
T2	119	16.94	8.68	26.05	8.40	0.0793	6.59	3.54
T3	119	15.60	8.72	16.95	6.72	−0.0018	6.89	3.67
T4	221	15.67	9.02	21.56	5.43	−0.0059	6.50	3.57
T5	233	16.49	9.30	26.41	10.73	0.0480	6.80	3.56
T6	252	16.85	8.57	25.50	6.75	0.0767	6.44	3.59
SOEP	5337	15.62	8.87	21.10	5.08	–	6.43	3.39

Notes: Kruskal-Wallis test for *day:* $p = 0.0135$ and *month:* $p = 0.1875$. See Table 1 for a brief description of the treatments. See the p-values of tests with pairwise comparisons of the treatments versus the SOEP sample (wave 2020) and the expected value of *day* or *month* in the Online Appendix (Table A8).

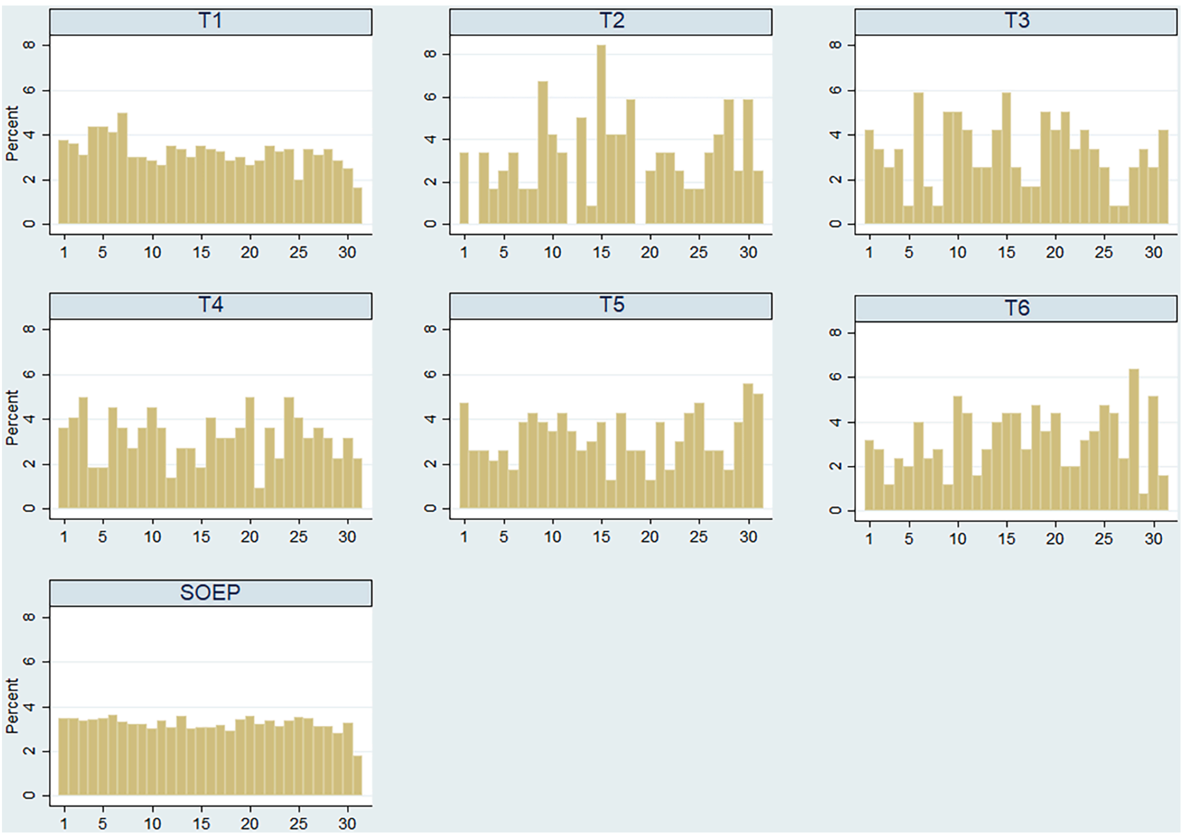

Fig. 2. Histogram of birthdays by treatment.

back button, response time, and other determinants of lying.

Lies in disguise vs. *open lies*

To address RQ6, we compare the average reported day of the mothers' unknown birthdays in T4 (15.67) with the average reported day of the known random date in T7 (16.37).[15] These two treatments share the same incentive structure. According to a two-sided Mann-Whitney U test and a two-sample Kolmogorov-Smirnov test, the reported days do not differ between T4 and T7 ($p = 0.419$ and $p = 0.634$, respectively). This is in contrast to Hermann and Brenig (2022), who find individuals to lie more frequently in unobservable settings. Yet there might be two opposing effects leading to this result. On the one hand, a lie about the birthday of one's mother cannot be tracked in our setting. On the other hand, the moral costs of lying about the mother's birthday might be higher than the costs of lying about a random day.

"Wrong": Misreporting in T7-T9

In T7, the percentage of participants who reported a different date than the randomly selected date they were supposed to enter is 10.0 % (considering the day and month). When we allow participants to see the date once again (in T8) – i.e., when the excuse of not being able to remember is no longer valid, only 5.0 % misreported the date (two-sided Fisher exact test: $p = 0.052$). 3.4 % of the participants in T9 stated that New Year's Day is on December 31st instead of January 1st, which 85.7 % answered.[16] Table A7 in the Online Appendix analyzes treatment effects and the impact of our control variables on the inclination to

[15] The random day shown in T7 averaged 16.23, which is slightly larger than $E[d^{true}]$ (15.73) for birthdays, but not statistically significantly different (t-test: p = 0.258, Wilcoxon signed rank test: p = 0.272).

[16] In the unincentivized counterpart to this question (asked respondents from T1 subsequently to entering their mother's birthday), 95.7 % reported January 1st as the date of New Year's Day (two-sided Fisher exact tests: p < 0.001). However, similarly to T9, 3.4 % of respondents in T1 stated that New Year's Day is on December 31st (two-sided Fisher exact tests: p = 1.000). Hence, while we find evidence that a significant share of respondents lies in a payoff-increasing manner, the payoff-maximizing answer could actually be due to an 'honest mistake', where respondents confuse New Year's Day and New Year's Eve.

Fig. 3. Histogram of birth months by treatment.

Table 3
Non-parametric tests by research question.

Non-parametric test: Dependent variable Research question		Two-sided Mann-Whitney U Mother's birthday (1,…,31)	Two-sided Fisher exact “>=25″ (0/1)	Two-sided Fisher exact “>=30″ (0/1)
RQ1 (T1 vs. T2)	$N =$ 921	$p = 0.069$	$p = 0.160$	0.127
RQ2 (T2 vs. T3)	$N =$ 238	$p = 0.318$	$p = 0.186$	0.615
RQ3 (T3 vs. T4)	$N =$ 340	$p = 0.642$	$p = 0.330$	0.539
RQ4 (T4 vs. T5)	$N =$ 454	$p = 0.333$	$p = 0.319$	0.107
RQ5.2 (T4 vs. T6)	$N =$ 473	$p = 0.202$	$p = 0.368$	0.490

Notes: P-values corrected for multiple hypotheses testing.

Table 4
Summary of regression coefficients by research question.

Estimation model: Dependent variable Research question	OLS Mother's birthday (1, …,31)	Tobit Mother's birthday (1, …,31)	Probit “>=25″ (0/1)	Probit “>=30″ (0/1)
RQ1 (T1 vs. T2)	β=2.004 $p =$ 0.019	β=2.071 $p =$ 0.022	n.s.	n.s.
RQ2 (T2 vs. T3)	n.s.	n.s.	n.s.	n.s.
RQ3 (T3 vs. T4)	n.s.	n.s.	n.s.	n.s.
RQ4 (T4 vs. T5)	n.s.	n.s.	n.s.	dy/dx=0.053 $p = 0.037$
RQ5.2 (T4 vs. T6)	n.s.	n.s.	n.s.	n.s.

Note: Coefficients for RQ1 (row 1) are taken from Table A3 in the Online Appendix. All other coefficients refer to the same models, but with different baseline categories for the treatment indicators to address the research questions. Tables A4 – A6 show the estimations with additional control variables.

report a wrong date. The probability of a wrong date is lower in T8 (when respondents can see the date again) compared to T7. It is slightly lower for younger and more risk-averse respondents with a university degree, higher income, and social policy identification.

Admissions and beliefs

Only 2.2 % of our participants admitted that they were lying about their mother's birthday. However, on average, our participants believed that 31.4 % would have lied and 20.1 % would have admitted it. For those who actually admitted lying, these beliefs were 54.9 % and 35.4 %, respectively. The finding that people overestimate the extent of lying by others is in line with previous results by Abeler et al. (2014).

There are two noteworthy determinants of the share of liars that our participants are guessing. First, participants who skipped the question about their mother's birthday believed that 37.5 % would lie and that 27.9 % admit their lies; both of these numbers are markedly higher than the respective average. Second, Abeler et al. (2014) find that older respondents' guess of the extent of others' lies is lower. In our experiment, we find strong support for this result. Table 5 displays the mean participants' beliefs about the share of participants who lie about their mother's birthday by age group. The older the age group, the lower the belief about others' lies.[17]

Skip rate

To address the first part of RQ5 (RQ5.1), we compare the share of survey participants who skipped the experiment across the first five treatment groups. Table 6 shows that we observe no significant differences in skip rates across the treatment groups.

Back button

All our treatments have the back-button feature, and we observe that 5.6 % of our participants used it. While nobody pushed the back button in T3, 16 % did so in T9. Of those who used the back button, 83.1 % did

[17] This result is robust to a Probit model estimation including all control variables (Extension 3 in Table A7 in the Online Appendix).

Table 5
Participants' beliefs about the share of lies about mother's birthday (in T1-T6) by age group.

Age group	N	Mean		18–24	25–29	30–39	40–49	50–64	65+
18–24	104	48.14 %	18–24	–	–	–	–	–	–
25–29	157	41.97 %	25–29	$p = 0.127$	–	–	–	–	–
30–39	252	39.28 %	30–39	$p = 0.014$	$p = 0.324$	–	–	–	–
40–49	273	33.76 %	40–49	$p = 0.001$	$p = 0.010$	$p = 0.059$	–	–	–
50–64	520	27.46 %	50–64	$p = 0.001$	$p = 0.001$	$p = 0.001$	$p = 0.004$	–	–
65 +	440	22.33 %	65 +	$p = 0.001$	$p = 0.001$	$p = 0.001$	$p = 0.001$	$p = 0.003$	–

Note: P-values are corrected for multiple hypotheses testing and refer to two-sided *t*-tests.

Table 6
Skip rate by treatment.

Treatment	N	Skip rate		T1	T2	T3	T4	T5
T1	923	13.11 %	T1	–	–	–	–	–
T2	128	7.03 %	T2	$p = 0.127$	–	–	–	–
T3	127	7.09 %	T3	$p = 0.127$	$p = 0.642$	–	–	–
T4	251	11.95 %	T4	$p = 0.561$	$p = 0.202$	$p = 0.202$	–	–
T5	252	7.54 %	T5	$p = 0.059$	$p = 0.642$	$p = 0.642$	$p = 0.186$	–

Note: P-values are corrected for multiple hypotheses testing and refer to two-sided Fisher exact tests.

not modify their entry. Roughly 8.1 % increased the entered date, but only 1.47 % (two participants in T9) in a payoff-maximizing way. Surprisingly, 8.8 % (3.68 %) of those who used the back button adjusted the indicated date in a payoff-decreasing (minimizing) way. Contrary to our assumption that participants will use the back button only to increase (maximize) their payoff, it seems that it is just as likely that they cannot sustain their lie in the presence of a back button as they may feel caught by the experimenter or are plagued by their bad conscience, and thus retract their lie.

Response time

On average, participants need about one minute to enter the requested date (1 min and 5 s, standard deviation: 2 min and 14 s). The response time varies considerably by treatment – it is lowest without incentives (T1: 33 s) and highest in T8, where people can see the date again (1 min and 37 s, see Tables A1 and A2 in the Online Appendix). In line with Shalvi et al. (2012), our estimation results (see Tables A5 and A6 in the Online Appendix) suggest a negative correlation between decision time and the inclination to lie[18] (see also the discussion of Foerster et al., 2013, who found delaying responses in a die-under-cup task to increase lying behavior before a short break but no longer in the task repetition afterward, and Shalvi et al., 2013, who considers the latter finding as confirming indication that honesty requires time in tempting situations). This is in contrast to Beck (2021), who finds honesty to be intuitive, i.e., honest behavior requires less response time, if (and only if) subjects swear an honesty oath before the decision, while otherwise decision time and the inclination to lie do not correlate. The regression coefficients in Tables A5 and A6 of response time on the reported birthday are negative across all model specifications. However, only one coefficient (for the dependent variable '>=25′ is statistically significant. Hence, we do not find conclusive evidence concerning response time and dishonest behavior.

Further determinants of lying

Besides the treatment variables, beliefs, and further experimental variables (such as the back button and response time), we included socio-demographics, political and economic preferences, as well as the judgment of dishonest behavior as control variables (see Table A6 in the Online Appendix) in our endeavor to explain the value of the reported birthday. Most of our control variables are unrelated to the reported day. "Aversion to theft", however, is associated with a significantly lower likelihood to report a 30th or 31st as the mother's birthday. Whereas the student dummy is not significant in all our specifications, having a university degree has a small negative impact on reporting birthdays later than the 24th. See the meta-study of Peterson (2001) for mixed results comparing student and non-student samples in social science research; these are also found specifically for economic experiments (e.g., Bortolotti et al., 2015, versus Exadaktylos et al., 2013).

5. Conclusion

The main purpose of this study is to provide researchers with a tool comparable to Fischerbacher and Föllmi-Heusi's (2013) dice experiment that is more suitable for online or telephone surveys and has a lower likelihood that the true answer is identical to the payoff-maximizing one. Our experiment has also the advantage of avoiding a "gamble", which might be important for research in some cultures.

Analyzing a representative sample of 2453 German household decision-makers, we find only little correlation between dishonesty and respondents' socio-demographic characteristics as well as their political and economic preferences. In particular, we find no evidence that students behave differently from the general population, which generally supports the validity of (lab) experimental studies that rely on student samples.

Our results indicate that the absence of incentives entices participants to respond sloppily. In our case, this means that a considerable share of respondents chooses the first numbers from the drop-down menu and, thus, reports significantly lower birthdays than expected. There is no indication that this kind of sloppiness also happens with incentives. This result is similar to Baillon et al. (2022), who find that respondents of a subjective well-being survey less often stick to default answers when the survey is incentivized. However, the authors find no effect of incentives on the quality of answers in a survey on health.

The vast majority of subjects do not exploit the opportunity to lie in our experiment. One possible reason is that people hesitate to lie when this in a sense involves relatives. However, in Heinicke et al. (2019), the one of the ten questions that is related to ours, namely "… recall the day and month of the birthday of a relative or friend. Next, please sum up the 4 digits of the birthday…", a considerable amount of lying did occur.[19] A likely mechanism leading to lying for this task is subjects' regret not to have chosen another friend or relative, or a kind of moral licensing arguing that the lie is no lie for another preceding choice – something that is not available for mother's birthday. The low prevalence of lying in our study could be explained by a lack of attention of the survey

[18] We excluded eight participants whose decision time exceeded 5 minutes from the econometric analysis, to avoid these outliers dominating the corresponding estimated parameter.

[19] The average share of answers generating a payoff for this question was 0.632, compared to the statistical expectation of 0.5 when all answers are honest. The prevalence of lying was higher for four other of the ten questions, for which the overall average was 0.646.

participants, which would be a limitation. As mentioned above, we find some evidence for this in the treatment without incentives. However, we find very robust results in a variety of settings with incentives: sure payoff or a 10 % chance to receive a payoff, high or low incentives, with or without live calculation of the payoffs, and voluntary or mandatory response. Depending on their specific experimental design, researchers may want to choose one of our incentivized treatments for their (online) experiment, knowing that these different features will not significantly impact experimental outcomes. Future research can check our results' generalizability further and test other design elements – e.g., open text boxes instead of drop-down menus. Moreover, future experiments could analyze in more detail whether the question's sensitivity and the answer's observability influence our results.

CRediT authorship contribution statement

Victor von Loessl: Writing – review & editing, Writing – original draft, Visualization, Validation, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Christoph Bühren:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Björn Frank:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Investigation, Formal analysis, Conceptualization. **Heike Wetzel:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization. **Elina Wiederhold:** Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Our dataset can be found here: https://doi.org/10.17632/r3mm7z6758.2

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2024.102191.

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