Motivated Risk Assessments

Islam, Marco

2021

Document Version:
Other version

Citation for published version (APA):
Islam, M. (2021). *Motivated Risk Assessments*. (Working Papers; No. 2021:12).

Total number of authors:
1

Working Paper 2021:12

Department of Economics
School of Economics and Management

Motivated Risk Assessments

Marco Islam

October 2021
Revised: July 2022

LUND UNIVERSITY

Motivated Risk Assessments

Marco Islam*

June 2022

Abstract

Do people form risk assessments to justify their actions? I investigate this question in a field experiment studying the dynamics of risk assessments for visiting a café during the COVID-19 pandemic. Randomly varying the incentive for a visit, I find that subjects with a high incentive visit cafés more often and downplay the risk compared with subjects with a low incentive. Importantly, the downplaying happens in anticipation of the visit and without new information, suggesting that the assessment update justifies engagement in risky behavior. This finding is inconsistent with Bayesian updating but consistent with the notion of motivated reasoning.

JEL codes: C93, D03, D91

Keywords: Risk Assessment; Motivated Reasoning; Self-Deception; Field Experiment

*Department of Economics, Lund University, email: marco.islam@nek.lu.se.

I thank Roland Bénabou, Pol Campos-Mercade, Matthew Collins, Benjamin Enke, Christine Exley, Thiemo Fetzer, Max Lobeck, George Loewenstein, Yeşim Orhun, Margaret Samahita, Florian Schneider, Peter Schwardmann, Frederik Schwerter, Marta Serra-Garcia, Petra Thiemann, Bertil Tungodden, Roel van Veldhuizen, Joël van der Weele and Erik Wengström for valuable comments. I am also thankful for comments from seminar participants at the Belief Based Utility Conference in Amsterdam, University of Cologne, Copenhagen Business School, Gothenburg University, Lund University and the global ESA conference. I acknowledge the financial support from the Institute of Economic Research at Lund University, the Jan Wallander and Tom Hedelius Foundation, and the Royal Swedish Academy of Sciences. The experiment was approved by the Swedish Ethical Review Authority (reference number: 2020-04270). It is pre-registered at AsPredicted: https://aspredicted.org/9wu9e.pdf.

1 Introduction

Despite exposure to accurate information, people systematically underestimate risks and engage in overly risky behavior in many situations. For example, people with high risk of a deadly disease often forego medical tests that would provide clarity about their condition and facilitate lifestyle adjustments for a longer and healthier life (Oster et al., 2013). Similarly, overconfident managers often underestimate risks, resulting in over-investments (e.g., Ben-David et al., 2013; Malmendier & Tate, 2005) and value-decreasing mergers (Malmendier & Tate, 2008). One important question in light of such erroneous assessments, is whether the risk assessments in these situations are outcomes of honest but biased reasoning (e.g., because people rely on heuristics or evaluate probabilities incorrectly) or whether they are subject to "wishful thinking" triggered by incentives. From a standard economic perspective, an uninformative incentive (material or immaterial) should not affect a person's evaluation of risk. However, research in psychology and behavioral economics suggests that people form judgments and beliefs based on not only facts and information but also individual goals and desires – a phenomenon referred to as motivated reasoning (Kunda, 1990).[1] There exists empirical evidence that people form so-called motivated beliefs and, to some extent, believe what they want to believe (Epley & Gilovich, 2016).[2] In fact, evidence suggests that motivated beliefs can justify socially undesirable behavior and cause such behavior to increase (e.g., Bosch-Rosa et al., 2021; Saccardo & Serra-Garcia, 2020).

To date, most evidence on motivated reasoning comes from the lab, while causal evidence from more natural field settings remains scarce. Only two studies document experimental field evidence. Schwardmann et al. (2019) uncover a motive for the development of motivated beliefs-—the desire to be persuasive. Huffman et al. (2019) examine motivated beliefs as an explanation for persistent overconfidence among managers. Little is known, however, about how motivated beliefs affect active risk-taking in natural settings, where risky actions can be consequential both for oneself and for others.

This paper studies motivated beliefs about risk, which I refer to as *motivated risk assessments*. Specifically, it documents novel field evidence on i) whether and why people form risk assessments by engaging in motivated reasoning, ii) whether risk assessments are motivated in response to or in anticipation of an incentivized risky activity and iii) how motivated risk assessments affect real-life behavior. I investigate whether young adults self-servingly downplay the risk of a hedonic but risky activity in response to an incentive-induced desire. In my preregistered experiment, young adults are incentivized to visit a local café during the Covid-19 pandemic. Each participant is assigned to one of two treatment groups. In the *LowIncentive* treatment, which serves as my control condition, participants receive a US$2 voucher; in the *HighIncentive* treatment, participants receive a US$12 voucher. The main preregistered hypothesis of the experiment is that subjects who receive the high incentive not only visit cafés more often but also assess the risk of Covid-19 as lower than subjects who receive the low incentive.

[1] Motivated reasoning describes the subconscious interplay of motivation and cognition during a thought process, which eventually leads to emotionally biased decisions.

[2] Bastardi et al. (2011) and Engelmann et al. (2019) provide two examples of belief formation that is not information-based. In these studies, beliefs are motivated by desire and anxiety, respectively.

To elicit each participant's risk assessment for visiting a café and study dynamics, I conduct surveys in three waves. The first survey (henceforth the Baseline survey) is conducted before the announcement of the voucher incentive to obtain an unbiased risk assessment. An additional survey (henceforth the Interim survey) is conducted after the introduction of the voucher but just before its redemption. Only participants who decide to use their voucher participate in the Interim survey. The last survey (henceforth the Endline survey) is conducted 15 days after the Baseline survey and the expiration of the voucher. All subjects participate in the Endline survey; if a subject uses her voucher, the risk assessment elicited in the Endline survey reflects her assessment after visiting the café.

To study if subjects motivate their risk assessments, my investigation relies on three key steps. First, I compare if subjects with a high incentive visit cafés more often than subjects with a low incentive. This is important to ensure the incentive treatment induced a behavioral response – a necessary condition for the formation of motivated risk assessments. Second, I conduct between- and within-subject comparisons of the risk assessments (both in incentivized and in non-incentivized forms) elicited during the Baseline and Endline surveys. This allows me to understand the dynamics evoked by the two incentives. If people motivate their risk assessment in response to an incentive, we would expect subjects with a high incentive to perceive a reduction in risk relative to subjects with a low incentive to justify visiting a café. Third, I identify the timing of this reduction in perceived risk by means of within-subject comparisons of the risk assessments elicited in the Interim and Endline surveys. This is important to understand if an assessment is updated in anticipation of the incentivized café visit or as a result of it. If people are Bayesian and update based on information, they should update their risk assessments after the voucher redemption in response to the experience of visiting the café. If they motivate their risk assessment to justify risky behavior, however, the updating should happen in anticipation of the visit and thus before it.

The results of my experiment suggest that many participants motivate their risk assessment for visiting a café. I find that subjects with a high incentive visit cafés significantly more often than subjects with a low incentive. Their propensity to use the voucher is 40 percentage points higher and their probability of visiting any café is 13 percentage points higher. Moreover, while subjects with a low incentive perceive that the danger associated with visiting a café increases over the duration of the study, which is consistent with the general increase in new Covid-19 infections during the study period, subjects with a high incentive perceive a drop in risk. The average treatment effect, measured during the Endline survey, corresponds to approximately 0.24 standard deviations (sd) and is driven by subjects who respond to the voucher incentive. For those participants who redeem their voucher and visit the café, the drop in perceived risk happens before visiting the café, as identified through the Interim survey This reduction implies that the updating is a direct result of the voucher incentive but not of the experience of visiting the café. Had the updating been experience-driven and consistent with Bayesian theory, it would have taken place after visiting the café. Instead, this finding suggests that the updating serves to justify a potentially risky activity. It is robust to alternative indicators of risk assessments (non-incentivized and incentivized) and cannot be explained by

experimenter demand effects or endogenous behavior changes such as café visits at quieter times. Further, the survey data allow me to exclude the possible explanation that treated subjects engage in a different type of information acquisition, as suggested by Ambuehl (2017). Over the 15 days between the Baseline and Endline surveys, they do not actively gather additional information about the danger of Covid-19 (neither the general danger nor the danger related to café visits), do not consume more news, and do not communicate more with other study participants. The results thus suggest that updating the risk assessments is motivated by the incentive-induced desire to visit a café.

Digging deeper into the survey data helps shed light on the nuances of motivated risk assessments as well as their underlying mechanisms. I distinguish between two components of risk (i.e., the risk perceived for oneself and risk imposed on others) and find that the assessment of risk subjects think they impose on others is mainly affected by the incentive and subject to motivated reasoning. As the risk imposed on others is construed more abstractly and more difficult to determine than the risk perceived for oneself, it provides the necessary "wiggle room" for a self-serving assessment.

Moreover, my data reveal that subjects who motivate their risk assessments (i.e., subjects with a high incentive) conveniently underestimate their prior assessments relative to those who have a low incentive to engage in motivated reasoning. They pretend (or believe) they always assessed the risk as sufficiently low to justify visiting a café when in fact they previously acknowledged that the risk was higher. This mechanism of conveniently biased recall has been shown to be effective in averting ego-threats for people to maintain a level of overconfidence (Huffman et al., 2019; Zimmermann, 2020). In the context of risk assessments, however, the misrecall serves a different purpose: it reduces the cost of a cognitive dissonance between hedonic desires and obstructive past beliefs and makes it easier for an individual to give in to an incentive.[3]

Lastly, I show that prosocial preferences are a potential driver of motivated risk assessments in the context of café visits. While for subjects high in altruism, the main treatment effect – downplaying risk to justify visiting the café – is large and statistically significant, for subjects low in altruism, it remains small and non-significant. Although prosocial subjects initially deem café visits as more dangerous than less prosocial subjects, provided with a voucher incentive, they are more likely to use that voucher to eat inside the café. Since café visits are social activities, prosocial people most likely suffer the most from restrictions and recommendations not to visit cafés and the costs of resisting a tempting voucher are higher than those for less social people.

In addition to the incentive treatment, my experiment includes an information treatment that runs orthogonal to the voucher incentive. Subjects assigned to that treatment are (truthfully) informed about the long-term health effects of Covid-19 and how the behavior of young adults during the pandemic jeopardizes older people. The treatment allows me to explore whether information about the source of the danger can

[3]Cognitive dissonance is a psychological concept that describes the tension between cognitive elements such as perceptions, desires, attitudes, and values. Since such a tension can trigger emotional discomfort, people strive to resolve dissonance and reach a state of consonance (Festinger, 1957, 1962). Today, the concept has found general approval in economics (e.g., Akerlof & Dickens, 1982; Gilad et al., 1987; Konow, 2000; Rabin, 1994). In particular, in the literature on motivated reasoning, the concept has been influential because a motivated distortion of cognitive elements can arise when resolving dissonance (Epley & Gilovich, 2016).

reduce the propensity to motivate risk assessments. Moreover, it enables me to study the direct effect of information on assessments and the propensity to visit a café. The provision of information about the source of the danger (i.e., Covid-19) appears to increase the perceived risk related to café visits. However, it does not seem to counteract the force of the voucher incentive. I find no evidence that subjects who receive the factual information are less likely to motivate their risk assessments for visiting cafés.

My study makes three important contributions. First, it offers novel and important experimental field evidence on motivated reasoning. To date, such evidence has been scarce. Most insights into the topic are either theoretical (e.g., Akerlof & Dickens, 1982; Bénabou & Tirole, 2002, 2004; Brunnermeier & Parker, 2005; Caplin & Leahy, 2001; Kőszegi, 2010; Schwardmann, 2019) or stem from lab or online experiments (e.g., Barrera et al., 2020; Di Tella et al., 2015; Exley & Kessler, 2019; Gneezy et al., 2020). To the best of my knowledge, the only insights into motivated reasoning or wishful thinking reported from field experiments pertain to overconfidence (Huffman et al., 2019) and self-persuasion to appear convincing during a debate (Schwardmann et al., 2019). My study adds to this literature in three ways: First, it provides insights from a different application allowing me to uncover a new motive for the formation of motivated beliefs – the justification of risky behavior. Second, my study focuses on both belief formation and a behavioral outcome (i.e., café visits), which enables me to study their interplay. Third, my experiment adopts a dynamic perspective, adding focus onto the updating of risk assessments. A notable related study investigating belief updating in a field setting is Di Tella et al. (2007). The authors show that a random allocation of land titles to Argentinian squatters supported the formation of self-serving beliefs favoring the working of free markets. However, since beliefs were elicited 20 years after the land reform, it is difficult to identify the cause of the belief updating. My study overcomes such identification issues by pinpointing the timing of the belief updating in response to the treatment intervention. Since I can observe that my treatment effect is driven by participants who update their assessment before the risky experience, I can exclude the explanation that the updating is experience-driven.

Thematically, my paper is most closely related to lab experimental work showing that the risk resulting from uncertainty can provide fertile ground for people to engage in motivated reasoning because it creates necessary wiggle room (Exley, 2016; Gneezy et al., 2020; Haisley & Weber, 2010; Saccardo & Serra-Garcia, 2020).[4] In the situations studied in those papers, uncertainty is deliberately inflated to justify self-serving behavior, which comes at the cost of harming others. In my setup, risk needs to be downplayed to resolve a cognitive dissonance between the hedonic desire to visit a café and the obstructive assessment that visiting a café could be dangerous. That is, risk assessments act as a lever to reduce potential psychological costs.[5]

[4]This paper focuses on the formation of motivated beliefs about *risk*. Although important, this topic has received little attention. To date, motivated reasoning has primarily been studied in the context of ability (Buser et al., 2018; Coutts, 2019; Eil & Rao, 2011; Zimmermann, 2020), overconfidence (Heidhues et al., 2018; Huffman et al., 2019; Schwardmann & Van der Weele, 2019), moral behavior (Carlson et al., 2020; Di Tella et al., 2015; Exley, 2016; Konow, 2000; Saucet & Villeval, 2019), and confirmation bias (Barrera et al., 2020; Charness & Dave, 2017; Charness et al., 2021; Schwardmann et al., 2019). For overviews, see Bénabou and Tirole (2016) and Epley and Gilovich (2016).

[5]In support of the notion of reducing cognitive dissonance, the first evidence was provided in the psychological literature (Bem, 1967; Festinger & Carlsmith, 1959). It showed that people forced to express an opinion opposed to their own were more likely to convince themselves of the opposite opinion if the force (or an external incentive) was moderate and additional

Second, my study raises our understanding of how people form and update risk assessments. Revealing that people can update their risk assessments even in the absence of new or changed information, I show that subjective risk assessments may be formed without the intent of accuracy. This insight extends prior research on risk assessments, which has never questioned such an intent. To date, the debate about the accuracy of assessments and perceptions has revolved around the notion of Bayesian updating and heuristics (e.g., Loewenstein & Mather, 1990; Slovic et al., 1980; Viscusi, 1985, 1990; Viscusi & O'Connor, 1984). That is, it has focused on the method of information processing and the question of the extent to which subjective assessments *could* be accurate given the information available. My work adds to this debate by documenting that assessment biases may not only arise when processing information. They can also arise in the presence of an uninformative incentive when the assessment formation is motivated by incentive-induced desires or hopes. My paper thus uncovers a new source of an assessment bias, which is substantially different from a heuristic.

Third, my study sheds light on the role of optimism when assessing risks and dangers. The results reveal that participants with a high incentive form optimistic beliefs to justify a pleasant but risky activity.[6] They downplay the risk of visiting a café, while doing so more frequently. This effect cannot be reconciled with changes in objective risk measures or the active acquisition of additional information. The result is consistent with the evidence reported by Oster et al. (2013), who document substantial differences in the willingness to test for Huntington's disease between individuals at high and low risk. The authors argue that information avoidance serves to maintain optimistic health beliefs and make decisions with long-term payoffs.[7] One important difference between my study and the analysis by Oster et al. (2013), however, is that my experiment exploits a random variation in the exposure to risk. This enables me to identify the causal relation between the incentive to be optimistic and belief about risk. Moreover, my paper does not study information avoidance. To form optimistic beliefs that justify behavior changes, my study participants need to actively update their prior assessments.

The remainder of the paper is structured as follows. In section 2, I present my experimental design and discuss how it allows me to identify whether risk assessments are motivated. Section 3 documents my main results. In section 4, I present evidence of the mechanisms and drivers underlying assessment updating and discuss how risk assessments associated with visiting a café affect those related to other activities. Section 5 concludes.

self-persuasion was necessary. My experimental setup is more closely related to the work of Brehm and Cohen (1962), where actions can be chosen freely and are not enforced by the experimenter.

[6]Another study that documents optimistic beliefs about Covid-19-related risks is Orhun et al. (2021). This study was developed parallel to mine and shows that the proximity of the date at which workers are called back to work affects their beliefs about Covid infection rates.

[7]Further empirical evidence for information avoidance related to medical testing is provided by Ganguly and Tasoff (2017) and Lerman et al. (1999). For theoretical evidence on the topic, see Schweizer and Szech (2018).

2 Experiment

2.1 Background: Covid-19 in Sweden

The Swedish approach to the Covid-19 pandemic was different than those in the US and most other European countries. Although various public institutions such as universities and libraries were closed temporarily (including at the time of this study), all restaurants, cafés, gyms, and private shops remained open. There was no obligation to wear face masks, neither outdoors nor inside shops and restaurants. People were merely encouraged to keep a distance from one another and avoid large crowds. Despite comparably high numbers of Covid cases, visiting cafés and restaurant was therefore not unusual for many people at that time.

In March 2021, when the experiment was conducted, the dominant SARS-CoV-2 variant in Skåne[8], the region in which the experiment was conducted, was Delta. At the time, Skåne's seven-day average of new Covid-19 infections per 100,000 inhabitants (henceforth the incidence rate) was around 214. By the end of the study in mid-April, the incidence rate had increased to approximately 300. By comparison, the incidence rate in April in Skåne was below the national incidence rate (385) but above the average incidence rates in the US (140) and European Union (221). Figure A1 in the Appendix depicts the trend of the incidence rate during the experimental period.

Like the US and other EU countries, Sweden started to roll out its vaccination campaign against Covid-19 in December 2020. At the start of my experiment, roughly 7.6% of Skåne's population over 18 years had received their first vaccination. However, due to the prioritization of older people and people working in the health care sector, none of the students recruited for this study had received a vaccination.

2.2 Experimental Design

My experiment was conducted in collaboration with a café in the city center of Lund, a midsize Swedish city. All study participants were offered a voucher for that café, which was valid for 14 days. In return, the café collected data whenever a voucher was redeemed. These data were matched with the responses from three experimental surveys:

1. **Baseline Survey.** Administered at the start of the experiment with all subjects.
2. **Interim Survey.** Administered within 14 days of the Baseline survey and just before visiting the café with subjects who redeemed their voucher.
3. **Endline Survey.** Administered 15 days after the Baseline survey, when the voucher had expired, with all subjects.

To understand how young adults assessed the Covid-19-related risk of visiting a café, the main survey questions, whose responses served as the outcome variables, were as follows:

[8]Skåne is one of the 25 administrative regions in Sweden and the second largest by population. Its population in 2020 was estimates to be 1,386,530.

- **Risk for Self.** If you visited a café or restaurant in Lund today, how worried would you be about your own health? (1 = not at all worried, 7 = very worried)
- **Risk for Others.** If you visited a café or restaurant in Lund today, how worried would you be that you could infect yourself during the visit and then pass the virus on to other people you interact with (e.g., friends, family or colleagues)? (1 = not at all worried, 7 = very worried)
- **Total Risk.** Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to visit a café or restaurant? (1 = not at all dangerous, 7 = very dangerous)

These three questions appeared in all three surveys, which allows me to investigate the dynamics of risk assessments as well as those assessments' updating patterns. Note however, that none of the questions were incentivized. I deliberately kept the main three questions simple and non-incentivized to make them intuitive to answer and maintain the notion that no objective truth for perceived risk exists. To check robustness and alleviate potential experimenter demand effects, however, I also employed an incentivized survey question in addition to the three non-incentivized questions. While it is monetarily cheap to satisfy experimenter demand for non-incentivized questions, deviating from the preferred answer, when correct responses are incentivized is costly because a student forgoes potential extra payments. The incentivized survey question appeared in the Baseline and Endline surveys:

- **Incentivized Risk Assessment.** What is the percentage share of new infections that is due to café and restaurant visits? (Please state a number between 0 and 100)

I verified the accuracy of the responses by comparing them with the estimate provided by Fetzer (2020). To ensure incentive compatibility, I used a quadratic scoring rule and paid two random students (one for the Baseline survey and one for the Endline survey) at the end of the study. Depending on the accuracy of their answers, the selected students could earn up to 500 Swedish kronor (SEK; about US$65) in addition to their regular payments. I restricted the response time for the incentivized question (and the other three questions on that page) to two minutes to ensure that participants could not search for the correct answer online.

To study the behavior of young adults in light of the risks of the pandemic, my analysis built on two other measures. First, I exploited the responses to an additional survey question posed in the Baseline and Endline surveys:

- **Self-Reported Café Visits.** How many times have you visited a café or restaurant during the last two weeks?

The responses to that question allow me to make general inferences about whether young adults visited cafés and restaurants. They capture visits at any café or restaurant in the two weeks before the respective survey. Again, since the question was asked twice over the 15 days, it allows me to study changes in behavior over time. However, because the responses to the question were self-reported, they were also subject to noise To

mitigate the influence of such noise, I also use the data collected at the studied café with which I collaborated. Since many of the subjects visited that café because of the voucher incentive, I can exploit the more nuanced data collected when they redeemed their voucher. In particular, I can study the time at which the café was visited, how busy the café was at that time, and whether the students ate inside or outside the café.

In addition to my main survey measures, I elicited a number of additional variables. First, similar to my main questions about visiting cafés and the risk involved, I elicited the perceived risk and behavior associated with the following activities: i) going to the gym, ii) meeting five or more people outside one's household, indoors, iii) meeting five or more people outside one's own household outdoors, iv) using public transportation, v) going to the supermarket, vi) attending a sports event or music concert as a spectator, vii) traveling by plane or train, and viii) attending an on-campus university lecture. Understanding participants' engagement in those activities and the perceived risk associated with them is useful to study spillover effects. Second, I conducted a norm elicitation and recall task in the Endline survey to explore two potential mechanisms underlying the dynamic updating of risk assessments. The norm elicitation task was similar to the task employed by Bursztyn et al. (2020). Parallel to the Endline survey, I asked a random student who did not participate in the experiment for her daily evaluation of the appropriateness of engaging in certain activities in light of the danger posed by Covid-19. Then, the subjects of this study were asked to guess how appropriate engagement in those activities was perceived by their external peer. I incentivized participants to report truthful guesses by providing an additional payment of SEK500 for one random subject whose responses matched the responses of the external student. In the recall task, subjects were incentivized to correctly recall their risk assessments reported in the Baseline survey. I randomly paid one subject an additional amount of SEK500 if her responses in the recall task matched her risk assessments from the Baseline survey. Finally, the Baseline survey contained a questionnaire to elicit risk, time, and social preferences (Falk et al., 2016) and sociodemographic variables. The Endline survey contained a questionnaire to elicit additional background information,[9] a cognitive reflection test (CRT) consisting of three questions (Frederick, 2005) and an open-ended question to explore whether students could guess the research purpose of this study. The latter can be used to analyze experimenter demand effects. Table A1 in the Appendix provides an overview of the three surveys.

2.3 Treatments

My experiment included two treatment manipulations resulting in a 2×2 between-subject design. Table 1 provides an overview of the four treatments. The main treatment manipulation was an exogenous variation in the voucher amount. I randomly distributed vouchers with values of SEK15 (about US$2) and SEK100 (about US$12). Providing a monetary incentive, which was earmarked for a hedonic yet (somewhat) risky activity, the voucher sought to test a subject's reported risk assessment and allows me to explore how the assessment translated into real behavior. Most importantly, however, I can study whether a subject adjusted

[9] As part of the background information, I asked students whether they had collected any additional Covid-19-related information during the two surveys or communicated with other participants.

Table 1: Experimental Treatments

Treatment	Voucher Amount	Information	Observation
LowIncentive-NoInfo	SEK15	No	109
LowIncentive-Info	SEK15	Yes	107
HighIncentive-NoInfo	SEK100	No	114
HighIncentive-Info	SEK100	Yes	104

her risk assessment in response to the incentive to justify her actions. I introduced the voucher and announced its value after participants had completed the Baseline survey. Thus, I can analyze the dynamics triggered by the incentive at a point at which the reported risk assessments were unaffected by the intervention.

One potential problem with the distribution of the vouchers was that the researcher, when handing them out, could implicitly send the signal that café visits during a pandemic are socially appropriate. To prevent that such a signal would only be sent in one treatment group, I decided to distribute the vouchers to all participants. That is, I chose a design with high and low incentives rather than a design in which only one treatment was incentivized. Moreover, to verify that the different voucher amounts would not send different signals about the appropriateness of visiting a café, I conducted a norm elicitation task during the Endline survey. The results of that task (see section 4.2) confirm that the voucher amount was not perceived as a signal for risk.

My main treatment intervention exploited the fact that physical interactions during a pandemic are dangerous. However, incentivizing participants to engage in a risky activity may raise ethical concerns. I acknowledged those concerns by taking a number of precautionary measures: First, I restricted my participant sample to a low risk group (i.e., students aged 18 to 30). Second, I preselected participants to ensure that none of them were part of the Covid-19 risk group. Third, I restricted café visits to groups of two and asked the café staff to enforce that rule. Fourth, I personalized the voucher to ensure that students would not pass it onto other people. Fifth, I asked students not to visit the café if they were showing any symptoms of an infection. Through all these measures, I minimized the risk of Covid exposure induced by my incentive intervention and reached the conclusion that the knowledge gain would outweigh the study's costs. I also asked the students at the end of the Endline survey whether they contracted Covid-19 during the study period but do not find any evidence that subjects in the HighIncentive treatment were more likely to be infected (see Table A2). This evidence provides support for the fact that my ex-ante ethical assessment of the feasibility of this study was correct. Finally, my ethical assessment and design of the study received approval from the the Swedish Ethics Review Authority.

My second experimental manipulation was an information treatment. At the start of the Baseline survey, I informed randomly chosen participants about both the long-term health effects of Covid-19 on young adults and how young adults jeopardize older people to study whether information can help alleviate self-deception and ultimately participation in risky activities. All the provided information was based on academic insights

and presented in the form of easy-to-understand summary statements (see section C.1 in the Appendix). I also included the academic source of the information next to the statements. Unlike the voucher incentive, the information was presented at the beginning of my experiment (i.e., before the start of the Baseline survey). It was not presented at any other point of the study.

2.4 Experimental Procedures

The experimental surveys included 434 students, aged 18 to 30, from different departments of Lund University. They were conducted online between March 2021 and April 2021 using the experimental software oTree (Chen et al., 2016). To include students with different backgrounds, I first contacted various lecturers and asked them to share an invitation message with a link to register for my study through their course communication platform. I also asked the lecturers to stress that both registration and participation was voluntary. Clicking on the link, all students interested in participating completed a registration form indicating their gender, age, and study field, whether they lived in Lund, whether they had previously been infected with Covid-19, and whether they were part of the Covid-19 risk group. In line with my preregistration and the requirements of the ethics review board, I used that information to exclude subjects who did not fulfill the age requirement, did not live in Lund, had previously been infected with Covid-19, or were part of the Covid-19 risk group. The remainder were contacted by email with the details for participation.

The participation email (see section C.4 in the Appendix) included a link to access the study and a unique participation ID that allowed me to track the subject and match her responses from the Baseline, Interim, and Endline surveys. Participation emails were sent randomly from March to April 2021 until the desired number of participants was reached. Aiming for the preregistered target of 400 subjects, I recruited a total of 500 students. The reason for this excess in recruiting was that I expected roughly 20% to withdraw from the study before the Baseline or Endline survey.

Every subject agreed to participate in two surveys and knew that the Endline survey followed 15 days after the Baseline survey. Prior to participation, every subject was informed that she would receive a flat participation fee of SEK150 (about US$18) for the two surveys but could also earn additional money. Information about the voucher incentive was not revealed until after the Baseline survey. Before the Baseline survey, all subjects had to undergo an additional prescreening to ensure (once more) that no participant was part of the Covid-19 risk group or had contracted Covid-19 before the experiment Moreover, I also ensured that participants were physically present in Lund during the experimental period.

Figure 1 shows a time line of the experimental procedure. For subjects assigned to the Information treatment, the experiment started with an information page and two control questions to ensure the information was understood. For all other subjects, it began with the Baseline survey (for subjects assigned to the Information treatment, the Baseline survey was part two). Upon the completion of the Baseline survey, each participant was told about the voucher incentive. I informed subjects about the voucher amount and announced that I would email them the activation link and terms of usage within 24 hours (see section C.5

in the Appendix). In the email, I further explained that the voucher needed to be activated on a phone or tablet and stressed that it should not be activated before its usage. Once activated, the voucher was valid for one hour and could not be reactivated. That is, any accidental activation would lead to the expiration of the voucher. I did not inform subjects that the three non-incentivized questions to elicit risk assessments (see page 6) had to be answered once again during the activation. I deliberately chose to surprise subjects with those three questions to ensure they would be answered when they had a clear intention to visit the café but before the actual visit took place. To distinguish whether the risk assessment is motivated to justify the visit or formed in response to the visit, the exact timing of the risk elicitation before visiting the café is crucial.[10]

Each voucher had a validity period of 14 days (i.e., it had to be activated and used between the Baseline and Endline surveys). After activation, the voucher had to be shown to a staff member, who verified the identity of the participant by checking her ID card. This procedure was necessary to ensure that vouchers were not passed onto other people and implies that the name of the participant had to be linked to the voucher. However, the participant's name was not saved with the data collected during the surveys.

Finally, each subject participated in the Endline survey, which lasted approximately 15 minutes. The Endline survey took place 15 days after the Baseline survey, which implies that if a subject decided to redeem her voucher, it was activated and used before she completed the Endline survey. This is important because it allowed me to capture the subject's risk assessment after visiting the café, thereby building the picture about the assessment's updating pattern. Provided a subject redeemed her voucher, I obtained three risk assessments: one before the treatment intervention, one after the treatment intervention but before visiting the café, and one after the treatment intervention and after visiting the café. This allowed me to identify the exact timing of a potential assessment update. Controlling for different alternative explanations, I interpret the reductions in risk assessments before visiting the café as support for a motivated risk assessment and those after the visit as instances of experience-based learning consistent with the notion of Bayesian updating.

2.5 Hypotheses

My experiment was designed to test the following preregistered hypotheses. First, I investigate how the voucher incentive affected behavior (i.e., the propensity to visit a café). Although café visits during a pandemic are more risky and less socially appropriate than those at other times, my first hypothesis is straightforward, as it rests on fundamental economic principles:

Hypothesis 1 (Incentives and Behavior)**.** *Subjects assigned to the SEK100 incentive treatment will visit cafés more often than subjects assigned to the SEK15 incentive treatment.*

My main point of interest in this study is to understand whether an incentive to engage in a risky activity

[10]It would have been possible to inspect the café before activating the voucher and hence form a risk assessment based on the number of guests currently in the café. However, the layout of the café alleviated such concerns to some extent. The café is built over two floors, where the first floor only includes the counter and the second floor includes all the tables and chairs. It would not have been possible to determine the occupancy of the café without passing the counter and inspecting the second floor.

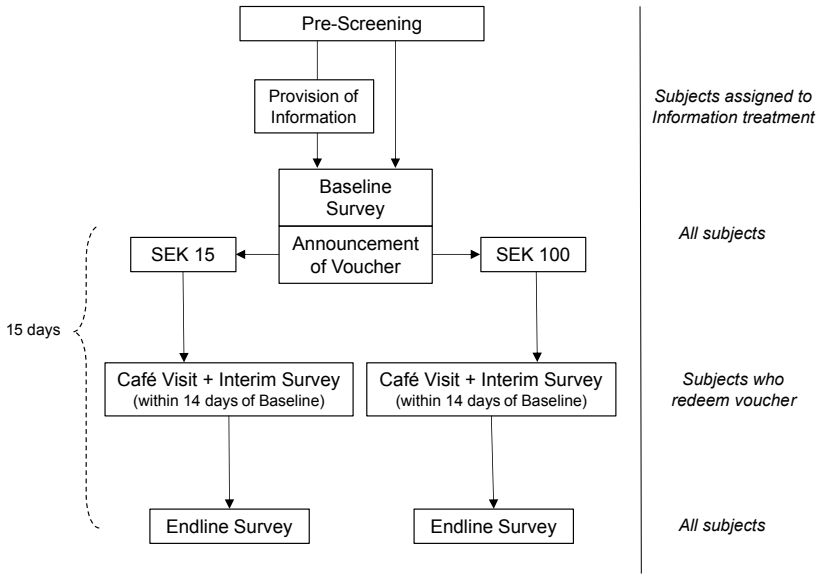


Figure 1: Timeline of the Experimental Procedure

causes a person to update her risk assessment such that the risky action becomes justifiable. While Bayesian theory predicts that assessments and beliefs should be updated *in response* to new or changed information, the literature on motivated reasoning suggests that beliefs may also be formed *in anticipation* of an event to gain anticipatory utility (e.g., Brunnermeier & Parker, 2005; Caplin & Leahy, 2001; Kőszegi, 2010) or to reduce a cognitive dissonance (e.g., Epley & Gilovich, 2016; Festinger, 1957). This implies that the risk assessment could also be updated in the absence of new information but in response to an incentive change. Building on this notion of motivated reasoning, I form my second and main hypothesis:

Hypothesis 2 (Incentives and Risk Assessments)**.** *Subjects who receive a SEK100 incentive to visit a café will perceive the danger of Covid-19 associated with a café visit as smaller (in the Endline survey) than subjects who receive a SEK15 incentive.*

In addition to the voucher treatment, my experiment included an Information treatment to study whether information about the source of the danger (i.e., Covid-19) can alleviate the propensity to form motivated risk assessments. Thus, I am particularly interested in the interaction of the two treatment interventions but consider this analysis as rather explorative. It is also possible, however, to study the direct effects of the Information treatment on both risk assessments and behavior. My third hypothesis addresses the link between information and risk assessments:

Hypothesis 3 (Information and Risk Assessments)**.** *Subjects who receive information about the health effects of Covid-19 perceive Covid-19 as more dangerous than subjects assigned to the* NoInformation *treatment. They will assess the general risk of the virus associated with a café visit (and the risk related to other activities) in the Baseline survey and in the Endline survey as greater than subjects in the* NoInformation *treatment.*[11]

[11]The preregistered hypothesis did not include the extra "and in the Endline survey".

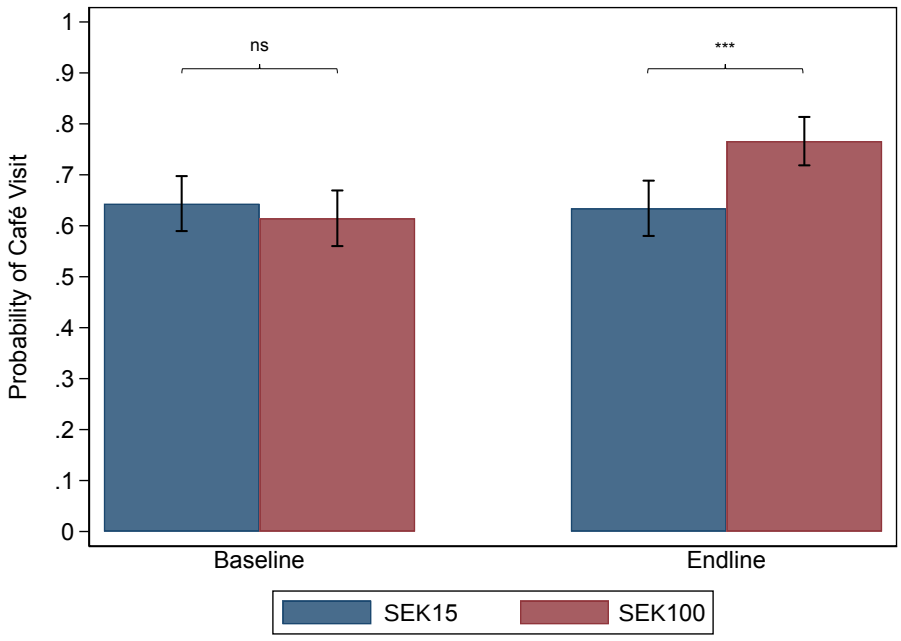


Figure 2: Probability of Visiting a Café

Notes: The figure shows the propensity to visit a café in the two weeks before the Baseline and Endline surveys by Incentive treatment. Error bars represent 90% confidence intervals. Test results stem from non-parametric Wilcoxon rank-sum tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Hypothesis 4 builds on Hypothesis 3 in that it predicts how an increase in perceived risk translates into real behavior:

Hypothesis 4 (Information and Behavior)**.** *Subjects assigned to the* Information *treatment will visit cafés and restaurants less often (or at less busy times) than subjects assigned to the* NoInformation *treatment.*

3 Results

The following section is divided into three parts. First, I show how the voucher treatment affected café visits between the Baseline and Endline surveys to understand whether the difference in the voucher incentive triggered a different desire to visit the café; this is a necessary condition to engage in motivated updating of risk assessments. Second, I turn to my key point of interest and focus on whether the incentive affected participants' subjective risk assessments. Third, I present the results of the Information treatment.

3.1 Risky Behavior

Of the 500 recruited students, 470 participated in the Baseline survey and 434 in the Endline survey. In line with my preregistration, my analysis only uses the data on the 434 subjects who completed the experiment. Of those, 126 decided to use their voucher. Table A2 in the Appendix shows the descriptive statistics, which

Table 2: Regression Results - Café Visits

	Cafe Visit					
	Used voucher at studied café		Ate inside at studied café		Any café before Endline survey	
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100 Voucher	0.402***	0.453***	0.270***	0.297***	0.131***	0.138**
	(0.039)	(0.053)	(0.034)	(0.048)	(0.044)	(0.060)
Information	-0.023	0.029	-0.027	0.001	-0.023	-0.016
	(0.039)	(0.039)	(0.034)	(0.026)	(0.044)	(0.066)
Information × SEK100		-0.104		-0.055		-0.015
		(0.078)		(0.068)		(0.088)
Constant	0.099***	0.073***	0.050**	0.037**	0.646***	0.642***
	(0.026)	(0.025)	(0.021)	(0.018)	(0.039)	(0.046)
Observations	434	434	434	434	434	434
R-squared	0.20	0.20	0.13	0.13	0.02	0.02

Notes: OLS regressions of café visits. Columns (1) to (4) show the coefficient estimates for the directly observable voucher data. For columns (1) and (2), the dependent variable is binary and indicates whether the voucher for the studied café was redeemed. For columns (3) and (4), the dependent variable is binary and indicates whether the voucher for the studied café was used to eat inside (as opposed to order takeaway). Columns (5) and (6) show the coefficients estimates for self-reported café visits. The dependent variable is binary and indicates whether any café was visited in the two weeks before the Endline survey. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

demonstrate that all the treatments were balanced in terms of the demographic variables and with respect to the elicited preferences. It also shows that no significant treatment difference in the number of café visits existed before the Baseline survey.

To assess how my treatment interventions affected a subject's propensity to visit a café, I exploit different types of data. I use both the direct measure of subjects who activated and redeemed their vouchers and subjects' self-reports of café visits in the two weeks before the Endline survey. The former allows me to document visits at one café only. However, it provides detailed information about that visit. The latter captures café and restaurant visits over two weeks, independent of the visited location. However, since it is a self-reported measure and relies on subjects remembering and reporting correctly, it is also more susceptible to noise. In combination, both measures provide a holistic picture of the subject's behavior.

Of the 126 students who redeemed their voucher, 107 came from the HighIncentive treatment and 19 from the LowIncentive treatment. Altogether, 75 subjects used their voucher to eat inside the café, while 51 ordered takeaway. Table 2 displays the treatment effects on café visits.[12] Columns (1) to (4) show the results of the directly observable data collected when vouchers were redeemed. They reveal that the voucher amount made an important difference. For participants who received the SEK100 voucher, the probability of using their voucher (eating inside the café) was approximately 40 (27) percentage points higher than for participants who received the SEK15 voucher. Both results are statistically significant. Relative to the LowIncentive treatment, this corresponds to an increase in voucher usage of more than 500% and

[12]Table A3 in the Appendix shows the results of the same regressions but including the control variables. The estimated treatment effects remain unchanged.

demonstrates that the SEK100 voucher incentive had a strong effect on the propensity to visit the studied café.

To investigate whether the voucher incentive in the HighIncentive treatment really triggered additional café visits or merely caused students to visit a different café, I turn to self-reported café visits. Figure 2 depicts the probability of visiting a café or restaurant in the two weeks before the Baseline and Endline surveys sorted by voucher amount. While I find no treatment difference before the incentive intervention, after the intervention, participants from the HighIncentive treatment were more likely to visit a café or restaurant than participants from the LowIncentive treatment. The treatment difference is statistically significant (Wilcoxon rank-sum test: $p = 0.003$). The regression results from the linear probability model presented in columns (5) and (6) of Table 2 confirm this visual impression. They indicate that the probability of visiting a café for subjects in the HighIncentive treatment was approximately 13 percentage points higher than for subjects assigned to the LowIncentive treatment. Relative to the propensity before the Baseline survey, the increase corresponds to approximately 25%. This result is robust to non-linear model specifications such as Probit or Logit (see Table A4 in the Appendix). The dependent variable in columns (5) and (6) is dummy coded indicating whether a person visited a café. The results thus provide insights into the voucher effect on the extensive margin. I binarized the dependent variable to avoid an inflated weighting of high numbers of café visits that result from working activities in cafés. Table A5 in the Appendix sheds light on the intensive margin effect. It shows the regression results for the original survey response (i.e., number of café visits). Excluding the 14 subjects who reported six or more café visits over the two-week period between the Baseline and Endline surveys, I find that subjects in the HighIncentive treatment reported 0.3 more café visits on average. This result is statistically significant and supports the evidence that the SEK100 voucher incentive not only induced a substitution effect but was also successful in encouraging additional café visits. My regression results are consistent with Hypothesis 1 and support the standard economic notion that incentives change behavior by changing relative prices. This basic mechanism also works when the incentivized activity is considered to be risky.

3.2 Risk Assessments

3.2.1 Non-Incentivized Risk Assessments

To address the study's key point of interest, in this subsection, I analyze the dynamics of risk assessments for visiting a café induced by the different voucher amounts. Figure 3 depicts the standardized reported risk associated with visiting a café by survey and voucher treatment. The variable Total Risk captures the total perceived risk of contracting Covid-19 and jeopardizing both oneself and others during and after the visit. As expected, in the Baseline survey, before the introduction of the voucher, the average risk assessments across the two voucher treatments were similar. This indicates that no meaningful differences in prior beliefs about the risk associated with visiting a café existed. Shifting attention to the Endline survey, however, the average risk assessments induced by the different voucher amounts change. While subjects offered the

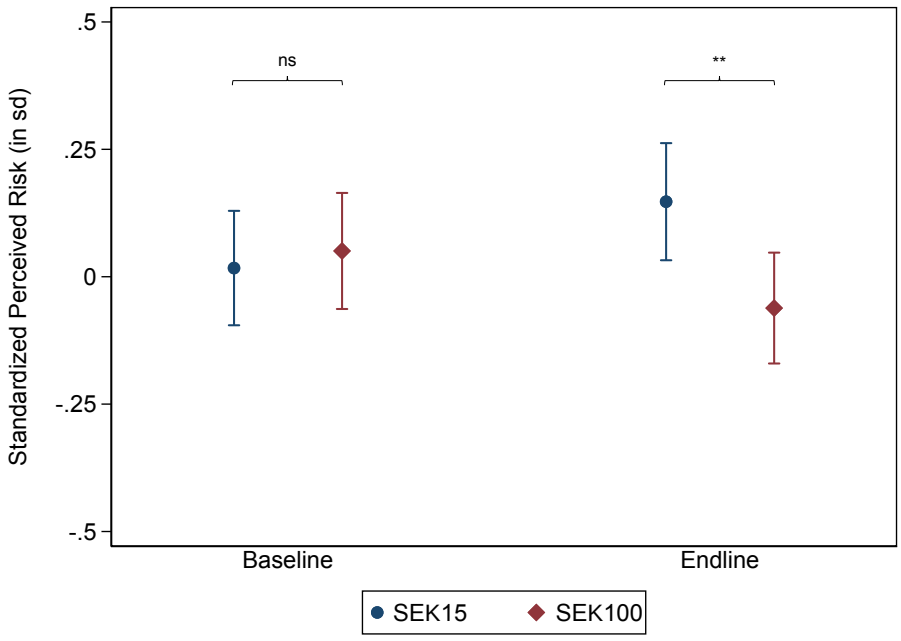


Figure 3: Perceived Risk Associated with Visiting a Café

Notes: The figure shows the standardized total risk associated with visiting a café for the two voucher treatments in the Baseline and Endline surveys. It captures the response to question 3 (Total Risk). Error bars represent 90% confidence intervals. Test results stem from non-parametric Wilcoxon rank-sum tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

SEK15 incentive perceived visiting a café as more risky than in the Baseline survey, participants assigned to the HighIncentive treatment assessed the risk as lower. The treatment difference is statistically significant (Wilcoxon rank-sum test: $p = 0.024$).

To confirm this visual intuition, I conduct multiple OLS regressions using the responses to my three main survey questions as the dependent variables. Table 3 presents the regression results. All the assessment measures of risk are standardized (i.e., the estimates are in units of sd). Starting with the assessment of Total Risk and focusing on the effect of the voucher incentive, the data collected during the Baseline survey support the impression that no treatment differences existed before the voucher intervention. Since the incentive was only introduced at the end of the Baseline survey, this result is expected. However, in the regression results from the Endline survey, the effect of the variation in the voucher amount is pronounced. Providing a subject with the SEK100 incentive caused a drop in the average perceived risk by approximately 0.21 sd relative to the risk perceived in the LowIncentive treatment. This result is robust to adding various controls.

The evidence presented thus far has exploited the cross-sectional treatment variation. However, since each subject participated in the Baseline and Endline surveys, I can also analyze the within-subject variation induced by the voucher incentive. Columns (4) and (5) of Table 3 document the results of the panel analysis in which the treatment effect is captured by the difference-in-difference estimator *Endline* × *SEK100*. Independent of the model specification, both the regressions reveal that the difference in risk assessments between

Table 3: Regression Results - Risk Assessment

	Total Risk of Cafe Visit					Risk for Self	Risk for Others
	Baseline	Endline		Panel			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SEK100 Voucher	0.037	-0.205**	-0.230**	0.037	0.021	-0.033	-0.037
	(0.096)	(0.095)	(0.093)	(0.096)	(0.095)	(0.098)	(0.096)
Information	0.206**	0.212**	0.208**	0.209**	0.206**	0.218**	0.182**
	(0.097)	(0.096)	(0.095)	(0.089)	(0.088)	(0.090)	(0.087)
Endline				0.130**	0.130**	0.137***	0.155***
				(0.052)	(0.052)	(0.045)	(0.051)
Endline × SEK100				-0.242***	-0.242***	-0.099	-0.282***
				(0.072)	(0.072)	(0.068)	(0.075)
Constant	-0.085	0.042	0.057	-0.086	-0.077	-0.125	-0.035
	(0.082)	(0.083)	(0.082)	(0.080)	(0.079)	(0.083)	(0.082)
Observations	434	434	434	868	868	868	868
R-squared	0.01	0.02	0.07	0.02	0.06	0.02	0.02
Controls			✓		✓		

Notes: OLS regressions of standardized risk assessments (z-scores). Columns (1) to (5) show the results for the total perceived risk (Total Risk) associated with visiting a café. Column (6) shows the results for the risk perceived for oneself (Risk for Self). Column (7) shows the results for the risk the subjects believed to impose on others (Risk for Others). In columns (1) to (3), I exploit the cross-sectional data from the Baseline and Endline surveys, respectively. Columns (4) to (7) present the regression results of the entire panel with standard errors clustered at the individual level. Controls include age, gender, health status, and CRT results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the two voucher treatments changed by approximately 0.24 sd between the Baseline and Endline surveys. The wedge driven between the two treatments is a result of the different dynamics induced by the different voucher amounts. Subjects assigned to the LowIncentive treatment experienced a small risk increase of 0.13 sd over the 15 days between the two surveys, consistent with the general time trend of increasing Covid cases during the study period (see Figure A1 in the Appendix). By contrast, subjects assigned to the HighIncentive treatment experienced a drop in perceived risk. This result is robust to adding control variables and individual fixed effects (see Table A6 in the Appendix). More importantly, it is driven by subjects who redeemed their vouchers to eat inside the café (see Table A7 in the Appendix), providing support for Hypothesis 2 and demonstrating the causal relation between risk assessments and café visits. The analysis in the Appendix compares subjects who reacted to the incentive with subjects who did not, which does not allow for conclusive insights because the behavioral response is not exogenous. As in Oster et al. (2013), however, the evidence is suggestive for the notion that changes in incentive-induced assessments can be causal for behavior changes.

Focusing on the responses to survey questions 1 (Risk for Self) and 2 (Risk for Others), my data further allow me to distinguish between different risk components; the risks subjects perceive for themselves and the risk they believe to impose on others. Models (6) and (7) of Table 3 show the regression results for these two types of risk using the data from the Baseline and Endline surveys. The estimates of the interaction term in both models are negative, thus supporting the result that the difference in risk assessments induced by

the different voucher amounts changed between the two surveys. However, only the estimate in model (7) is statistically significant. The size of the voucher treatment effect on Risk for Others (approximately 0.28 sd) is similar to the effect on Total Risk but is considerably larger than the effect on Risk for Self (approximately 0.1 sd). This result suggests that it was the belief about the risk imposed on others that was mainly affected by the incentive and subject to motivated reasoning. As it is more difficult to determine the risk imposed on others than the risk perceived for oneself, the assessment of Risk for Others provides the necessary wiggle room for a motivated risk assessment.

The results presented in this subsection are robust to experimenter demand effects. In Appendix B, I provide an additional analysis that exploits the open-ended text question from my Endline survey to infer how well students could anticipate the research purpose of this study. It reveals that only a small proportion of students inferred the correct study purpose or understood that the voucher was part of study rather than another reward for participation. Correct anticipation did not correlate with treatment assignment and excluding those subjects who did anticipate the study purpose does not change the results.

3.2.2 Incentivized Risk Assessments

As pointed out before, all of the three survey measures of risk assessment presented in Table 3 were intentionally kept non-incentivized. To provide an alternative measure of risk assessments that complements the analysis, both the Baseline and the Endline surveys included an incentivized question that asked students to report their belief about the proportion of Covid-19 new infections due to café and restaurant visits. Table 4 reports the regression results for the incentivized risk assessments.

All the regression estimates for the analysis of the incentivized measure that are indicative of the treatment effect of the HighIncentive treatment show the expected negative sign. However, only the results from the panel analysis, which exploit the power from both the between-subject and the within-subject variation, are statistically significant. They suggest that subjects provided with the SEK100 incentive believe that the proportion decreased over the two-week period and the expected drop (captured by the difference-in-difference estimator $SEK100 \times Endline$) is on average 3.3 percentage points greater than the drop for subjects in the LowIncentive treatment. While the decrease in the LowIncentive treatment is approximately 9.2% relative to the average expected proportion at Baseline, the decrease in the HighIncentive treatment corresponds to 22.8%. I interpret the expected proportion of new infections due to café visits to be instrumental for the perceived risk of visiting a café. Hence, the results for the incentivized measure complement those for the non-incentivized measure above. The results in Table 4 also provide evidence mitigating the potential concerns of the experimenter demand effects. Since correct responses were rewarded with a probabilistic payment of up to SEK500, I believe the incentive was sufficiently large to counteract the experimenter demand.

Table 4: Risk Assessment - Incentivized Measure

	Expected proportion of new infections due to café visits (in %)					
	Endline		Full Panel		Reduced Panel	
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100 Voucher	-1.462	-1.142	1.803	1.919	1.517	1.663
	(1.277)	(1.262)	(1.482)	(1.489)	(1.159)	(1.149)
Information	1.334	1.321	1.707	1.664	0.692	0.618
	(1.282)	(1.278)	(1.208)	(1.206)	(0.985)	(0.966)
Endline			-1.912**	-1.912**	-0.636	-0.636
			(0.914)	(0.917)	(0.766)	(0.769)
Endline × SEK100			-3.258**	-3.258**	-3.021***	-3.021***
			(1.359)	(1.364)	(1.108)	(1.112)
Constant	18.344***	18.190***	20.071***	20.034***	16.867***	16.854***
	(1.061)	(1.051)	(1.167)	(1.164)	(0.926)	(0.916)
Observations	434	434	868	868	742	742
R-squared	0.01	0.04	0.02	0.04	0.02	0.05
Controls		✓		✓		✓

Notes: OLS regressions of expected proportion of new Covid infections due to café and restaurant visits (Incentivized Risk Assessment). Columns (1) and (2) show the results from the between-subject comparison. It exploits the cross-sectional variation using the data from the Endline survey. Columns (3) and (4) present the regression results of the entire panel. Columns (5) and (6) present the regression results of the reduced panel. The reduced panel excludes subjects who reported that the proportion of new infections due to café or gym visits is greater than 50%. Standard errors are clustered at the individual level. Controls include age, gender, health status, and CRT results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2.3 Timing of Updating

The evidence provided in the previous subsections suggests a clear treatment effect of the SEK100 voucher incentive on subjects' perceived risk. The larger the incentive to visit a café, the stronger the propensity for participants to update their risk assessment downward (i.e., assess the risk as lower). However, one unanswered question is whether the updating induced by the voucher incentive took place before (i.e., in anticipation) or after (i.e., as a result of) visiting the café. Answering that question is important to distinguish a motivated updating of risk assessments from supposedly standard Bayesian updating. To answer it, I exploit the data from my Interim survey collected for those participants who redeemed their voucher. These data include another set of survey responses to my three main questions eliciting the risk related to visiting a café. Since the data were collected just before participants visited the café and participants were not informed about it, the data contain useful information that – in combination with the data from the Baseline and Endline surveys – can identify when the assessment was updated.

The left panel of Figure 4 depicts the dynamic of the average total risk of visiting a café. It demonstrates that subjects who visited the café reduced their risk assessment just before the activation of the voucher in anticipation of their visit. Relative to the Baseline survey, the previsit risk assessment elicited through the Interim survey dropped by approximately 0.33 sd (t-test: $p = 0.007$), and then remained at a similar level until the Endline survey (t-test: $p = 0.725$). Had the updating taken place in response to visiting the café

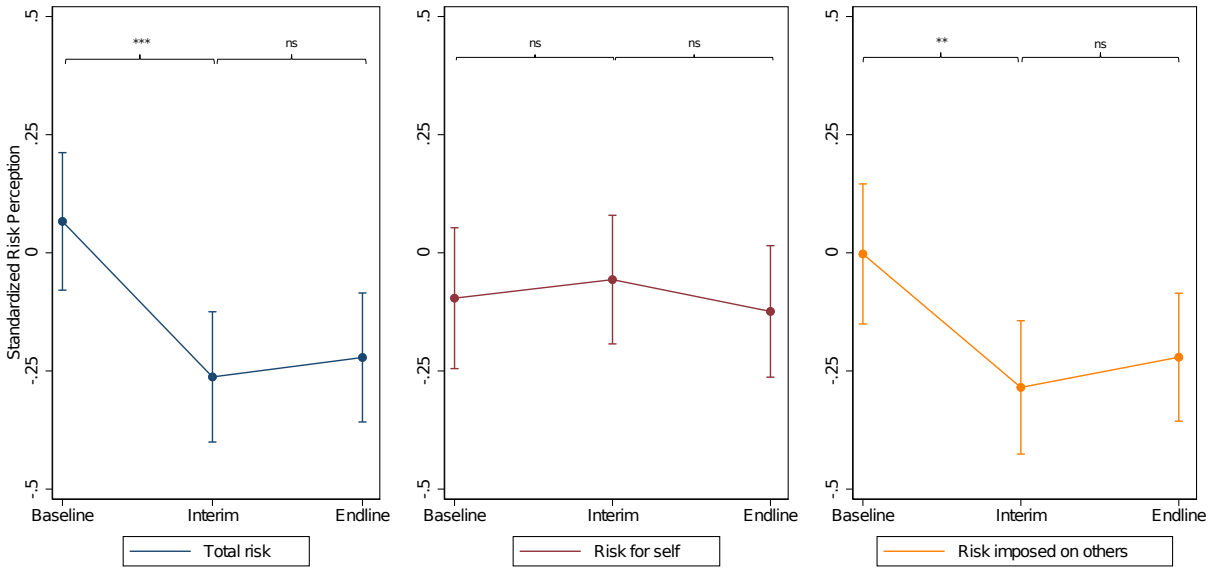


Figure 4: Timing of the Updating

Notes: The figure shows the dynamics of average risk assessments for subjects who redeemed and used their voucher. The left panel shows the dynamic pattern for the Total Risk of visiting a café. The middle panel shows the dynamic for the reported risk perceived for oneself Risk for Self. The right panel depicts the dynamic for the risk of visiting a café, which is believed to be imposed on others (Risk for Others). The variables are standardized (z-scores). Error bars represent 90% confidence interval. Test results stem from two-sided t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(rather than in anticipation of it), we would have expected a drop in risk assessments after the visit (i.e., between the Interim and Endline surveys). Clearly, the data do not support that inference, suggesting that the assessment update was not experience-driven and took place in the absence of new information. The dynamic is driven by subjects who used their voucher to sit inside the café rather than those who order takeaway.[13] This indicates that it is not the 100SEK voucher that causes people to think more carefully about the risk of a visit but the voucher-induced desire that calls for the assessment updating. Moreover, the evidence on the dynamic lends support to a causal effect of the risk assessments. Only an assessment of low risk that precedes risky behavior can provide an additional reason for visiting a café. As such, the updating pattern in Figure 4 constitutes a necessary condition for causality.

To provide insights into the heterogeneity of the updating timings, Figures A5 and A6 in the Appendix show the dynamics for all 126 subjects who activated and used their voucher sorted by whether they sat inside the café or ordered takeaway. Focusing on students who ate inside the café (i.e., the main driver of the average dynamic), I find that almost 50% downplay the risk of Covid before visiting the café. Only 29% exhibit an updating pattern, which is consistent with the standard notion of Bayesian updating. These results concur with the evidence presented in the previous subsections and provide further support for a causal relation between risk assessments and risky behavior.

[13] Figure A4 in the Appendix illustrates the dynamic for voucher users divided into those who sat inside the café and those who ate outside.

The middle and right panels of Figure 4 show the dynamics for the risk assessments of Risk for Self and Risk for Others, respectively. Interestingly, while the dynamics for the risk imposed on others follow the same trend as the total risk, there is little variation in the risk perceived for oneself over time. This confirms the evidence in Table 3 that the assessment of Risk for Others is the main driver of the treatment effect and the component that can be motivated the easiest.

Although the data collected during the activation of the voucher were gathered before the subject visited the café, does it really allow me to rule out that risk assessments might be updated based on the occupancy of the café? After all, a subject could have evaluated the occupancy level before she decided to use the voucher. To alleviate this concern, I divide subjects who visited the café into those who visited the café under high (above median) occupancy and low (below median) occupancy. Recall that the occupancy level of the café was determined by the café staff each time a voucher was redeemed based on the number of customers. The results for the assessment of Total Risk are presented in Figure 5. Unless the average prior beliefs about the occupancy of the café were incorrect (i.e., most subjects systematically overestimated the occupancy before their visit), Bayesian updating theory predicts that perceived risk should increase when the café is busier and decrease when the café is quieter. Regardless of the actual occupancy, however, my data suggest that after their update subjects always assessed the risk as lower. Again, this evidence is inconsistent with Bayesian updating but in line with the notion of motivated reasoning. There is no difference in the assessment updating across the two groups. Risk assessments before visiting the café dropped by approximately 0.32 sd (in the low occupancy group) and 0.33 sd (in the high occupancy group) relative to the Baseline survey. Both assessment updates are statistically significant. In combination with the fact that subjects could not evaluate the occupancy level from outside the café because its seating area is on the second floor, the presented evidence lends support to the interpretation that the occupancy of the café did not influence the assessment updating before visiting the café.

Another theory that could explain the updating of risk assessments is that participants who visited the café actively searched for additional information to understand the risks of visiting a café (Ambuehl, 2017). Since updating in response to information acquisition could also take place before visiting the café, the dynamics in Figures 4 and 5 are unsuitable for alleviating such a concern. However, in the Endline survey, I asked subjects whether they had searched for additional information about Covid-19 (e.g., information about infection rates in cafés and gyms) and specify the information if they had. I also elicited each subject's news consumption in the two weeks between the two main surveys and asked about communication with other participants to explore potential spillover effects.[14] Table A11 in the Appendix presents the treatment effects for the three measures. The regression results show no significant differences in behavior for the two voucher treatments, suggesting that subjects who received the SEK100 incentive were not better informed about the risks of Covid-19 than subjects with the SEK15 voucher.

[14] A coding error for the survey responses to the communication measure existed. Subjects were asked to distinguish among no communication, communication before the Baseline survey, and communication before the Endline survey. Unfortunately, the error prevented me from separating the two former responses. The final measure can only distinguish whether communication between the two main surveys took place.

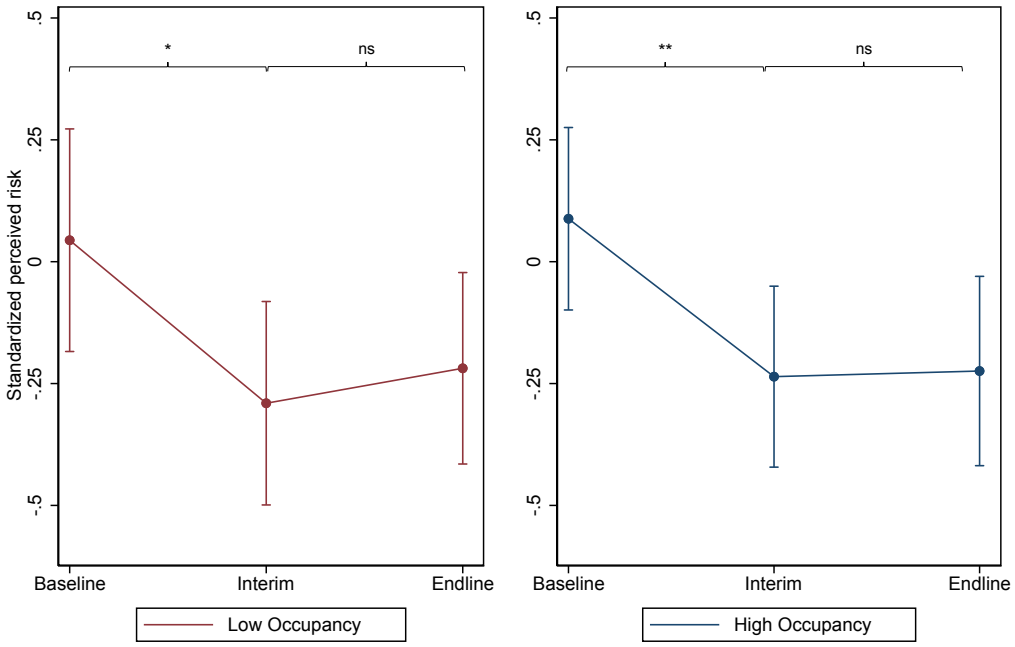


Figure 5: Timing of the Updating for High and Low Occupancy

Notes: The figure shows the dynamics for assessments of total risk associated with visiting a café by occupancy. The left panel depicts the dynamic for subjects who visited the café at low occupancy. The right panel shows the dynamic for subjects who visited the café at high occupancy. The measure of *perceived risk* is standardized (z-scores). Error bars represent 90% confidence interval. Test results stem from two-sided t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.3 Role of Information

Lastly, I turn to the effects of my INFORMATION treatment. Recall that participants assigned to that treatment received two types of information: i) information about the possible long-term health effects of Covid-19 for young adults and ii) information about how infected young adults jeopardize older people, who are more vulnerable to the virus. All this information was provided before the Baseline survey and not repeated during the Endline survey. Although the main purpose of this treatment was to study whether the provision of information can alleviate the formation of motivated risk assessments, it is also interesting to analyze the treatment's direct effect on risk assessments.

Table 3 in subsection 3.2 reports the regression results for the treatment variable Total Risk.[15] It shows that information exposure can alter subjects' assessment of the risk associated with a café visit. Subjects assigned to the INFORMATION treatment assessed the risk of visiting a café as significantly higher than subjects who did not receive the information. The treatment effect corresponds to approximately 0.21 sd and is stable over the two-week study period, supporting Hypothesis 3. The long-term effect, however, may appear surprising since the information treatment did not seem to affect the propensity to visit a café.

[15]Table A9 in the Appendix shows the regression results using the measures Risk for Self and Risk for Others as the dependent variables.

Table 5: Regression Results – Information Effect

	Perceived Risk of Cafe Visit			
	Endline		Panel	
	(1)	(2)	(3)	(4)
Information	0.236*	0.154		0.210
	(0.139)	(0.135)		(0.135)
SEK100 Voucher	-0.181	-0.226*		0.028
	(0.130)	(0.123)		(0.128)
Info × SEK100	-0.049	0.025		-0.013
	(0.191)	(0.181)		(0.189)
Endline × Info			-0.011	-0.011
			(0.147)	(0.104)
Endline × SEK100			-0.259*	-0.259***
			(0.135)	(0.096)
Endline × Info × SEK100			0.035	0.035
			(0.205)	(0.145)
Constant	0.030	0.075	0.034	-0.079
	(0.096)	(0.093)	(0.025)	(0.092)
Observations	434	434	868	868
R-squared	0.02	0.14	0.86	0.06
Controls		✓		✓
Individual FE			✓	

Notes: The table reports the results of four OLS regressions. The dependent variable is Total Risk. Columns (1) and (2) show the results from the cross-sectional data from the Endline survey. Columns (3) and (4) show the results from the panel data using observations from the Baseline and Endline surveys. Standard errors are clustered at the individual level. Controls include age, gender, health status, and CRT results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

None of the seven estimates in Table 2 (subsection 3.1), neither from self-reported café visits nor from the voucher data, suggest that participants who received information were more reluctant to visit cafés than those who did not. I also find little evidence that the provided information reduced the propensity to visit a café for subjects with the SEK100 voucher. None of the interaction terms in models (2), (4), and (6) of Table 2 are statistically significant, which implies that subjects who received the SEK100 voucher as well as the information treatment did not behave differently in terms of voucher usage and indoor visits. One explanation for these findings could be that subjects did not perceive the information as salient enough to remember it when they decided to visit a café but managed to recall it during the surveys.

The results of my INFORMATION treatment are consistent with the evidence provided by Getik et al. (2021) and Haaland and Roth (2021), who document that information-induced shifts in beliefs may not necessarily lead to behavior changes. However, the results may raise doubts about the causal effect of risk assessments on behavior. Clearly, the factual information provided in the INFORMATION treatment did not cause any behavior change; however, it is difficult to transfer this inference to my incentive treatment. Information and incentive-induced desires are two sources of shifts of risk assessments, and their effects may differ both in

magnitude and in duration. Moreover, whether information and incentive-induced desires affected the risk assessments of similar subjects is unclear. Hence, the results of the information treatment should not be interpreted as contradicting to the evidence provided in support of causality in subsection 3.2.

Finally, I focus on the interaction of the two treatment interventions to investigate whether information exposure can mitigate the propensity to motivate risk assessments. The regression results in Table 5 do not show a significant effect of the interaction independent of the model specification. Neither the data from the Endline survey nor the panel data suggest that the information treatment shifted the risk assessments of subjects assigned to the HighIncentive treatment. Considering the inability of the Information treatment to prevent subjects from visiting cafés, this result is unsurprising.

4 Mechanisms and Scope of the Effect

My experimental results concur with the notion of motivated reasoning and show that people can strategically adjust their risk assessments to convince themselves that engagement in a risky activity is justifiable. In this section, I explore the mechanisms underlying the updating of risk assessments, sort my findings in light of the theoretical literature, and study the heterogeneity effects. I also investigate the scope of the formation of self-serving assessments. *How* did people convince themselves that the risk of visiting a café was reasonably low? *Which* students used their voucher and updated their risk assessments self-servingly? And *how* did updating the risk assessment of visiting a café affect the perceived risk associated with behavior in other domains? To answer these questions, I exploit the additional data collected in the Baseline and Endline surveys.

4.1 Endogenous Memory

Bénabou and Tirole (2002) theorize and Zimmermann (2020) documents empirical evidence that selective recall can help a person deal with negative feedback, which constitutes a potential ego-threat or contradicts one's desired self-view. In my study, subjects did not receive ego-threatening feedback. Instead, they were incentivized toward a behavior that conflicted with their prior risk assessments resulting in a cognitive dissonance. This dissonance between the incentive-induced desire to visit a café and one's prior risk assessment incurred a psychological cost. Similar to the case of negative feedback, however, this cost could reduce if the recall of previous risk assessments was systematically biased. I therefore explore if convenient memory lapses also served as an effective aid to suppress obstructive past assessment.

I conducted an incentivized recall task in the Endline survey in which subjects were asked to recall their responses from the Baseline survey. Among others, each subject had to recall her answers to the three questions that elicited the perceived risk associated with visiting a café (i.e., Total Risk, Risk for Self and Risk for Others). I construct a new recall variable *Recall* (= *Recalled Risk Assessment from the Baseline Survey* − *True Risk Assessment from the Baseline Survey*) for each risk component by calculating the difference

Table 6: Results from the Recall and Norm Task

	Recall of Baseline Response						Second-OrderBelief	
	Total Risk		Risk for Self		Risk for Others		Appropriateness of Cafe Visit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SEK100 Voucher	-0.339***	-0.357***	-0.148	-0.166*	-0.343***	-0.353***	0.132	0.137
	(0.094)	(0.094)	(0.093)	(0.093)	(0.118)	(0.117)	(0.096)	(0.097)
Information	0.107	0.107	0.040	0.048	-0.086	-0.081	-0.175*	-0.186*
	(0.094)	(0.095)	(0.093)	(0.094)	(0.118)	(0.119)	(0.096)	(0.095)
Constant	0.160**	0.169**	0.161**	0.166**	0.270***	0.272***	0.019	0.022
	(0.081)	(0.081)	(0.075)	(0.075)	(0.097)	(0.097)	(0.084)	(0.085)
Observations	434	434	434	434	434	434	434	434
R-squared	0.03	0.04	0.01	0.02	0.02	0.03	0.01	0.03
Controls		✓		✓		✓		✓

Notes: The table shows the regression results for the responses from two tasks. Columns (1) to (6) show the results from the recall task indicating the treatment effects on the discrepancy between reported risk from the Baseline survey and memorized risk. The dependent variables are Total Risk, Risk for Self and Risk for Others, respectively. Columns (7) and (8) show the results of the norm task. The dependent variable is the reported (and incentivized) second-order belief about the appropriateness of visiting a café. All the dependent variables are standardized (z-scores). Controls include age, gender, health status, and CRT results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

between the subject's recalled response from the Baseline survey and her true response from the Baseline survey and use it as dependent variable in my regression analysis. Columns (1) to (6) of Table 6 present the regression results for the recall differences.

For all three measures of risk in Table 6 there exists a treatment effect of the voucher amount. It appears that subjects with the SEK100 voucher underestimated their responses from the Baseline survey relative to subjects in the LowIncentive treatment. That is, the difference captured by the variable *Recall* was smaller for subjects in the HighIncentive treatment than for subjects in the LowIncentive treatment, particularly for the assessments Total Risk and Risk for Others. Although, on average, the risk assessments reported in the Baseline survey were similar across the two voucher treatments (see the result in section 3.2), the assessments recalled 15 days later were significantly smaller. Specifically, the recalled Total Risk in the HighIncentive treatment was 8% lower than that in the LowIncentive treatment, while the recalled Risk for Others was 10% lower. The Information treatment, by contrast, did not significantly affect the ability to recall one's previous responses.

Digging deeper into these findings to understand what drives the differences between the HighIncentive and LowIncentive treatments, it becomes clear that the direction of the recall error was consistent with the updating of risk assessments over the two-week study period in both treatments. As the number of positive Covid cases increased, so did the perceived risk for subjects in the LowIncentive treatment. As a result of this (possibly subconscious) trend, some subjects in the LowIncentive treatment may have forgotten their original responses in the Baseline survey and defaulted to their reported risk assessment in the Endline survey. Indeed, for all measures of risk assessments (i.e., Total Risk, Risk for Others and Risk for Self), the correlations between the recalled assessments and reported assessments in the Endline survey were

larger than the correlations between the recalled assessments and original assessments in the Baseline survey ($p = 0.056$, $p = 0.048$, and $p = 0.023$, respectively). In the HighIncentive treatment, similar behavior can be observed. However, since the average risk assessment in this treatment decreased over time in response to the voucher incentive, the recalled assessment was lower than the original assessment reported in the Baseline survey. Considering that the benchmark trend for memory lapses observed in the LowIncentive treatment developed in the opposite direction, I interpret the biased recall in the HighIncentive treatment as strategic ignorance. Relating these findings to Zimmermann (2020), the evidence supports the notion that not only negative feedback but also obstructive past assessments can be suppressed to reduce cognitive dissonance.

Finally, the results of the memory task further mitigate concerns of experimenter demand effects for the main treatment effect – downplaying risk to justify a risky café visit. Of the subjects who used their voucher to eat inside the café, more than 37% underestimated their non-incentivized Baseline assessment. Among those who did not eat inside or did not redeem their voucher, that share was 23%. Had the risk assessment been a result of experimenter demand effects, more café visitors should have remembered their true Baseline assessments because their motive for misrecall – strategic ignorance – which implies an additional loss of money, would have been reduced. This would have resulted in two similar distributions of recall responses for students who did and who did not visit the café.

4.2 Norm Perception

Another potential mechanism that could underlie the strategic updating of risk assessments is a specific, treatment-induced norm perception. In a recent paper, Bicchieri et al. (2020) suggest that people can motivate their beliefs about norms to justify immoral but personally beneficial behavior. They state that if people can credibly convince themselves that a social norm does not apply in a particular context, seemingly immoral behavior becomes more viable because it is not perceived as a transgression. I exploit the data collected in an incentivized norm task to test whether my treatment interventions induced different norm perceptions. In particular, I examine whether subjects in the HighIncentive treatment develop a norm perception in a direction that allows them to think of visiting a café as socially appropriate behavior.

Recall from section 2 that subjects' goal in the norm task was to guess how socially appropriate students foreign to my experiment rated visiting a café. I interpret the second-order belief measure obtained in response as a proxy for the perceived norm. This interpretation is similar to, for example, the measures in Bursztyn et al. (2020) and Krupka and Weber (2013). The regression results, which allow for inferences about the treatment effects on norm perceptions are shown in columns (7) and (8) of Table 6. Since the belief variable is standardized, the effects can be interpreted in standard deviations. The estimates of the voucher treatment have the expected positive sign, implying that subjects with the larger incentive might have perceived it as more appropriate to visit a café. However, the estimates are non-significant, inferring that a self-serving norm interpretation did not play an important role in subjects' justification of visiting a café. Table A8 in the Appendix provides robustness for this result. It shows that even those subjects who

visited the studied café did not perceive a café visit as more appropriate. The finding that no treatment differences in norm perceptions exist also suggests that subjects did not interpret the voucher amount as a signal of the social suitability of visiting a café. Had the voucher amount been implicit information about the danger of visiting a café, its effect should have been captured in different norm perceptions, similar to the information provided in the INFORMATION treatment.

The INFORMATION treatment is the only treatment intervention that caused a (marginally) significant effect in the perception of norms. Although the respective information was provided before the start of the Baseline survey and second-order belief was elicited two weeks later, information exposure seems to have shifted the belief about the appropriateness of a visiting a café downward. The size of the effect is approximately 0.18 sd. Had the effect of the Information treatment mainly been driven by experimenter demand effects, we would have expected to see no differences in the second-order belief elicitation between the INFORMATION and the NOINFORMATION treatments. Since the norm task was incentivized, the observed discrepancy between the two treatments implies that a significant proportion of subjects in the INFORMATION treatment were willing to forgo a substantial additional payment to satisfy the experimenter. I deem such a scenario as unlikely.

4.3 Theoretical Explanations

4.3.1 Anticipatory Anxiety

One potential explanation for my main finding of strategic assessment updating could be the the notion of manageable beliefs as a means to derive anticipatory utility (or avert anticipatory "disutility"). In the context of health risk, this has been modeled by Oster et al. (2013) and Schwardmann (2019).[16] In essence, the downplaying of risks could have served as a coping strategy to reduce anticipatory anxiety around Covid-19, making redeeming the voucher psychologically easier. Schwardmann (2019, p.1) himself, however, notes that "health risk denial is more attractive to the agent if her ability to act is limited", that is, if a person has limited scope to adjust her behavior to the threat. In my experiment, this ability was not constrained. Preventive measures to contract Covid-19 were not affected by the voucher incentive. All subjects were free to choose whether they wanted to visit the café, order takeaway, or stay at home. In addition, I find no evidence that students downplayed the risk they perceived for themselves; they only downplayed the risk they thought they imposed on others. Had the students really intended to reduce anticipatory anxiety, playing down the dangers for themselves would have been an obvious strategy. Thus, although the theoretical explanation of anticipatory anxiety has merit and has been shown to be a potential driver for the formation of motivated beliefs (e.g., Engelmann et al., 2019), I only find limited support for it in my data.

[16] The theoretical models presented by Brunnermeier and Parker (2005) and Bénabou (2013) also account for anticipatory utility by allowing agents to choose their beliefs but capture applications unrelated to the setup of health risk.

4.3.2 Cognitive Dissonance

A second explanation for motivated belief formation, one that appears more consistent with my data, is cognitive dissonance reduction. The theory of cognitive dissonance is rooted in psychology (Brehm & Cohen, 1962; Festinger, 1957; Festinger & Carlsmith, 1959) but has also found approval in economics since the seminal work of Akerlof and Dickens (1982). It posits that dissonances (i.e., tensions or inconsistencies) between people's beliefs, decisions or values pose a psychological cost, which people strive to reduce by reaching a more pleasant state of internal consonance. One way to reach a state of consonance is through motivated reasoning (Golman et al., 2016). Rabin (1994) models cognitive dissonance as a cost factor, $D(X - Y)$, that increases in the discrepancy between the desired level of action X (here: café visits), and the appropriate level of that action, Y (here: the appropriate level of café visits following from the perceived risk of Covid-19). While the agent can directly control the level of X, she can only form a belief about the appropriate level, Y but can actively manipulate (or motivate) those beliefs at the cost $C(Y)$. Under standard assumptions of a concave utility and convex cost functions, this simple framework is sufficient to derive the prediction that for low costs of belief manipulation and a sufficiently large distaste for cognitive dissonance, the agent will reduce the dissonance by motivating her beliefs about Y.

In my experiment, the high voucher incentive may have induced a cognitive dissonance for many subjects. In particular, it evoked a desire for a pleasant café visit that conflicted with the subject's obstructive past assessment that such an activity is risky. Reducing this dissonance through a formation of motivated risk assessments would therefore imply a reduction of psychological costs, further reinforcing the propensity to visit a café. The evidence of my data provides support for the explanation of cognitive dissonance reduction as the main effect of motivated assessment updating is driven by those subjects who sat inside the café but not those who ordered takeaway (see Figure A4). Moreover, as implied by Rabin's 1994 model, the subjects only motivated the belief which was least costly to manipulate. In my experiment, this was the assessment of risk they thought to impose on others as it was more ambiguous than the risk for oneself. Lastly, I find support for the notion of cognitive dissonance reduction in the findings of my recall task. Subjects of the HighIncentive treatment were more likely to conveniently underestimate their Baseline assessments in order to appear consistent with their updated assessment and convince themselves that no dissonance ever existed. This mechanism of selective recall likely enhanced the self-efficacy of the motivated risk assessments (Bénabou & Tirole, 2016).

4.4 Heterogeneity Analysis

Which types of students were the most susceptible to motivate their risk assessments and visit cafés? The analysis of risk assessments in subsection 3.2 revealed no information to answer this question. In my preregistered analysis, I planned to analyze gender effects as well as the heterogeneity that may result from different cognitive abilities. In addition, I explore heterogeneity effects that stem from differences in social preferences. Although the following analysis is primarily explorative, I deem its insights useful since it reveals interesting

Table 7: Heterogeneity Analysis

	Panel A. Observed Behavior					
	Dep. Var.: Used Voucher			Dep. Var.: Ate Inside		
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100 Voucher	0.426***	0.476***	0.405***	0.330***	0.355***	0.273***
	(0.059)	(0.080)	(0.039)	(0.051)	(0.068)	(0.034)
Male	-0.078**			-0.006		
	(0.040)			(0.026)		
Male × SEK100	-0.050			-0.119*		
	(0.078)			(0.067)		
CRT		0.022			0.017**	
		(0.017)			(0.008)	
CTR × SEK100		-0.038			-0.044	
		(0.037)			(0.030)	
Altruism			0.001			0.001
			(0.018)			(0.011)
Altruism × SEK100			0.057			0.076***
			(0.036)			(0.030)
Constant	0.130***	0.049	0.088***	0.040**	0.005	0.037***
	(0.034)	(0.031)	(0.019)	(0.020)	(0.010)	(0.013)
Observations	434	434	434	434	434	434
R-squared	0.21	0.20	0.21	0.14	0.13	0.15
	Panel B. Risk Assessment - Dependent Variable: Total Risk					
	Male		CRT Score		Altruism	
	Yes	No	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100 Voucher	0.085	-0.018	-0.001	0.090	0.019	0.055
	(0.142)	(0.134)	(0.126)	(0.151)	(0.140)	(0.132)
Endline	0.133*	0.128*	0.080	0.221**	0.155**	0.109
	(0.076)	(0.071)	(0.062)	(0.093)	(0.074)	(0.073)
SEK100 × Endline	-0.243**	-0.242**	-0.241***	-0.263**	-0.155	-0.323***
	(0.102)	(0.102)	(0.087)	(0.125)	(0.105)	(0.098)
Constant	0.032	0.004	0.034	-0.013	-0.139	0.151*
	(0.105)	(0.089)	(0.087)	(0.109)	(0.101)	(0.091)
Observations	416	452	536	332	408	460
R-squared	0.00	0.01	0.01	0.01	0.00	0.01

Notes: The table consists of two panels. Panel A includes the six OLS regressions used to explore the heterogeneity in café visits. The regressions exploit the cross-sectional variation. Columns (1) to (3) show the results for subjects who used their voucher. Columns (4) to (6) show results for subjects who ate inside the café. Panel B includes the six OLS regressions for the dependent variable Total Risk. The regressions exploit the panel structure of the data. In columns (1) and (2), I divide the sample based on gender. In columns (3) and (4), I divide the sample at the median of the CRT score. In columns (5) and (6), I distinguish between subjects low in altruism (below median) and subjects high in altruism (above median). Standard errors are clustered at the individual level. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

correlations.

The results of the heterogeneity analysis are in Table 7. Panel A shows the heterogeneity observed for

café visits and panel B shows that for the risk assessment Total Risk. Moreover, panel A exploits the cross-sectional variation and panel B exploits the panel structure of the data. The variables of interest are *Male*, *CRT*, and *Altruism*. *Male* is a binary variable indicating whether a subject is male.[17] *CRT* represents the score of the correctly answered CRT questions. Recall that every subject had to answer three CRT questions; thus, the score ranges from 0 (zero correct answers) to 3 (three correct answers).[18] *Altruism* represents the subject's self-evaluation of her social preferences and ranges from 0 (not prosocial) to 10 (very prosocial). In my regressions, this variable is treated as continuous.

Focusing on café visits (see panel A), I find no heterogeneity effect for cognitive abilities, neither for voucher usage nor for indoor visits. The data show that male subjects were approximately 12 percentage points less likely to sit inside the café than non-male participants but not less likely to use their voucher. Most importantly, however, subjects who reported to be more prosocial had a significantly greater propensity to sit inside the café than less prosocial subjects. An increase of 1 sd in the self-assessment of altruism corresponds to a 7.6 percentage point increase in the probability of eating inside. This result is robust when controlling for risk assessments (see Table A12 in the Appendix), suggesting that altruistic people had an inherent urge to visit cafés, which was larger than that of less altruistic people, and were willing to take more risks to do so. Nonetheless, the finding is surprising because the visits took place despite the fact that altruistic students perceived the risk associated with visiting a café as larger (see Table A13 in the Appendix). It also contrasts with the findings of Campos-Mercade et al. (2021) and Fang et al. (2021) that prosocial people are more likely to adhere to social distancing rules to prevent the spread of Covid-19.

Turning to the regression results for the reported total risk in panel B, I find no heterogeneity effects in the perceived risk associated with visiting a café for *Male* or *CRT*. For males and non-males as well as for subjects with low (below median test score) and high (above median test score) cognitive abilities, the main treatment effect – downplaying risk to justify visiting a café – captured by the interaction variable *SEK100* $\times$ *Endline* is similar and statistically significant. Thus, the motivated updating of risk assessments did not seem to be gender-specific or driven by subjects with lower cognitive abilities. For *Altruism*, however, the case is different. The formation of motivated risk assessments was small and non-significant for subjects low (below median) in altruism, while it was considerably larger and highly statistically significant for subjects high (above median) in altruism. In fact, the treatment effect for subjects high in altruism (0.323 sd) was more than twice as large as that for subjects low in altruism (0.155 sd). Table A14 in the Appendix provides additional insights into the role of altruism for the variables Risk for Self and Risk for Others. As for the total risk, students high in altruism were more likely to downplay the risk they thought they imposed on others, but the discrepancy between the two altruism types was smaller. For the assessment Risk for Self, the treatment effects for the two altruism types were virtually identical.

The results documented in this section provide suggestive evidence that prosocial motives played an

[17] The study included two non-binary subjects and two subjects who preferred not to reveal their gender. Those subjects are categorized as non-males. Excluding them from the sample does not change the results.

[18] Since no subject was registered for experimental participation in Lund before this study, I assume that a majority had not seen the CRT questions before.

important role in the motivation of risk assessments in my context. Considering the result that the formation of motivated risk assessments is mainly driven by the belief about risk imposed on others, this finding appears intuitive because prosocial people should have more scope to downplay the component of risk for others. On the other hand, it is also plausible that prosocial people simply enjoy social activities such as visiting a café more than less prosocial people. Therefore, their temptation to give in to the incentive and justify the visit could have been particularly strong. This finding appears consistent with the explanation cognitive dissonance reduction. Since prosocial people care more about others, they may experience stronger dissonance between the desire to visit a café and the dangers of such a visit. To reduce the dissonance, they motivate their risk assessments and downplay the risk.

4.5 Spillover Analysis

Besides the question of how a subject managed to justify her risky behavior, one policy-relevant point of interest is to understand whether (and if so to what extent) the incentive to engage in one risky activity caused an updating of risk assessments related to behavior in other domains. In other words, did the motivation of one risk assessment spill over to the assessments of other risks? To answer that question, I elicited a number of additional risk assessments associated with other activities: i) going to the gym, ii) meeting five or more people outside one's own household indoors, iii) meeting five or more people outside one's own household outdoors, iv) using public transportation, v) going to the supermarket, vi) attending a sports event or music concert as a spectator, vii) traveling by plane or train, and viii) attending an on-campus university lecture.

For each of these variables, I conduct an OLS regression similar to that for the risk associated with visiting a café. The results of the regressions are in Table A15 in the Appendix. For six of the eight activities (i, ii, v, vi, vii, and viii) the interaction estimate of interest (*Endline* × *SEK100*) has a negative sign. This implies that relative to the Baseline survey, subjects with the SEK100 café voucher also experienced a stronger drop in risk assessments related to those other activities. However, only three of the assessment estimates (i, ii, and vii) are statistically significant and only two (i and vii) remain significant after applying the Romano–Wolf multiple hypothesis correction (Romano & Wolf, 2005).[19] These two activities can be considered to be hedonic activities, which lends room for the interpretation that spillover effects were more likely in domains in which self-persuasion was easier because the cognitive dissonance of hedonic desires and obstructive past beliefs was greater. It is, however, difficult to control for outside factors that could have affected the risk assessment related to the other activities and as such may have contributed to the spillover effect of the café voucher. For example, for many subjects, the participation date of the Endline survey was just before the Easter weekend. This holiday could itself have been an incentive for students to travel and thus motivate the associated risk.

To test the robustness of this result in relation to gym visits, I exploit data from an incentivized question

[19] The Romano-Wolf p-values of the interaction term *Endline* × *SEK100* are in Table A16 in the Appendix.

on perceived risk. Just as for café visits, before the Baseline and Endline surveys, I asked each participant to estimate the proportion of new Covid-19 infections expected to be due to gym visits. I used the estimates from Helsingen et al. (2020) to determine the target responses and employed a quadratic scoring rule to ensure incentive compatibility. The spillover effects of the voucher treatment are shown in columns (1) and (2) of Table A17 in the Appendix. I find that in the Endline survey, subjects in the HighIncentive treatment expected the infection rate in gyms to be significantly lower than subjects in the LowIncentive treatment. The difference corresponds to approximately 3.7 percentage points and is similar to that observed for café visits (see Table 4).

Finally, I compare self-reported gym visits before the Baseline and Endline surveys to shed light on whether treatment spillovers affected actual behavior. The regression results presented in columns (3) and (4) of Table A17 suggest that no such behavioral effects exist. None of the estimates are statistically significant. In fact, the coefficient of interest, *Endline* $\times$ *SEK100*, even has a negative sign, implying that gym visits in the HighIncentive treatment decreased over time. I therefore interpret the findings in this section that the spillovers of the voucher treatment may have affected the risk assessments related to other hedonic activities but did not translate into more risky behavior.

5 Conclusion

This paper provides novel experimental field evidence that young adults motivate their risk assessments when provided with an incentive to engage in a risky activity. Exploiting the conditions of the Covid-19 pandemic, which made it more dangerous to engage in social activities, I show that relative to students with a SEK15 voucher, students with a SEK100 voucher i) visited cafés more often and ii) strategically updated their assessment by downplaying the risk of such a visit. While subjects with a low incentive perceived an increase in risk over the two-week study period consistent with the time trend of increasing Covid-19 cases, subjects with a high incentive reported a drop in perceived risk. Importantly, this treatment effect was driven by subjects who visited cafés but did not result from the experience of those visits. My results suggests that risk assessments were updated before the visits and in the absence of new information. Subjects with the SEK100 voucher neither actively collected additional information about the risks of Covid nor exhibited endogenous behavior changes before the redemption of the voucher that could explain the updating or its timing. This finding is inconsistent with the notion of Bayesian belief updating and suggests that the risk assessment served to justify the risky activity.

The evidence provided by this study implies that trusting individuals to responsibly assess the risk of a hazard – even if information about the source of the danger is available – can be risky. Exposed to a sufficiently large incentive, many people seem able to convince (or deceive) themselves that hedonic activities are harmless when, in fact, they have previously acknowledged that those activities entail substantial risks. My data suggest that in particular the risk thought to be imposed on others seems easy to downplay. This

is an important insight with respect to public goods policies, as it suggests that individuals do not fail to account for the social dimension of their actions per se but situationally underestimate the repercussions of their behavior in their own favor.

My analysis also points out an important mechanism of motivated risk assessments that can help the formulation of policy advice. The experimental results suggest that individuals conveniently misrecalled previous risk assessments to convince themselves of their assessment updates. That is, they intended to maintain an image of consistency and pretended (or believed) that risk assessments were never updated. For subjects in the HighIncentive treatment, this systematic misrecall cannot be explained by mere memory lapses. Instead, it seems to result from strategic ignorance, which alleviated cognitive dissonance. The mechanism of endogenous memory has previously been shown to play a crucial role for people to maintain a positive self-image and avert ego-threats (e.g., Huffman et al., 2019; Zimmermann, 2020). This study extends this body of evidence by showing that the mechanism is also successful in dealing with obstructive past beliefs. Thus, rather than providing information about the source of a threat, a more promising strategy for practitioners to alleviate the formation of motivated risk assessments could be to stimulate a person's memory while stressing how the environmental conditions have changed over time.

Finally, my data suggest that subjects who reported to be more prosocial were more likely to visit a café and downplayed the respective risk. On the one hand, this result appears surprising because, initially, prosocial students deemed café visits as more risky than less prosocial students. On the other hand, considering that café visits are social activities, prosocial students most likely obtained the greatest benefit from redeeming their voucher. Thus, the cognitive dissonance between avoiding risk and indulging in a hedonic activity may have been particularly pronounced for those individuals. This finding highlights the importance of understanding the risk context. To design effective policy interventions, it is thus crucial to identify vulnerable groups.

References

Akerlof, G. A., & Dickens, W. T. (1982). The economic consequences of cognitive dissonance. *The American economic review*, *72*(3), 307–319.

Ambuehl, S. (2017). An offer you can't refuse? Incentives change how we inform ourselves and what we believe. *CESifo Working Papers*, (6296).

Barrera, O., Guriev, S., Henry, E., & Zhuravskaya, E. (2020). Facts, alternative facts, and fact checking in times of post-truth politics. *Journal of Public Economics*, *182*, 104123.

Bastardi, A., Uhlmann, E. L., & Ross, L. (2011). Wishful thinking: Belief, desire, and the motivated evaluation of scientific evidence.

Bem, D. J. (1967). Self-perception: An alternative interpretation of cognitive dissonance phenomena. *Psychological review*, *74*(3), 183.

Bénabou, R. (2013). Groupthink: Collective delusions in organizations and markets. *Review of Economic Studies*, *80*(2), 429–462.

Bénabou, R., & Tirole, J. (2002). Self-confidence and personal motivation. *The quarterly journal of economics*, *117*(3), 871–915.

Bénabou, R., & Tirole, J. (2004). Willpower and personal rules. *Journal of Political Economy*, *112*(4), 848–886.

Bénabou, R., & Tirole, J. (2016). Mindful economics: The production, consumption, and value of beliefs. *Journal of Economic Perspectives*, *30*(3), 141–64.

Ben-David, I., Graham, J. R., & Harvey, C. R. (2013). Managerial miscalibration. *The Quarterly journal of economics*, *128*(4), 1547–1584.

Bicchieri, C., Dimant, E., & Sonderegger, S. (2020). It's not a lie if you believe the norm does not apply: Conditional norm-following with strategic beliefs. *Available at SSRN 3326146*.

Bosch-Rosa, C., Gietl, D., & Heinemann, F. (2021). Risk-taking under limited liability: Quantifying the role of motivated beliefs. *Available at SSRN 3985775*.

Brehm, J. W., & Cohen, A. R. (1962). Explorations in cognitive dissonance.

Brunnermeier, M. K., & Parker, J. A. (2005). Optimal expectations. *American Economic Review*, *95*(4), 1092–1118.

Bursztyn, L., González, A. L., & Yanagizawa-Drott, D. (2020). Misperceived social norms: Women working outside the home in saudi arabia. *American economic review*, *110*(10), 2997–3029.

Buser, T., Gerhards, L., & Van Der Weele, J. (2018). Responsiveness to feedback as a personal trait. *Journal of Risk and Uncertainty*, *56*(2), 165–192.

Campos-Mercade, P., Meier, A. N., Schneider, F. H., & Wengström, E. (2021). Prosociality predicts health behaviors during the covid-19 pandemic. *Journal of Public Economics*, *195*, 104367.

Caplin, A., & Leahy, J. (2001). Psychological expected utility theory and anticipatory feelings. *The Quarterly Journal of Economics*, *116*(1), 55–79.

Carlson, R. W., Maréchal, M. A., Oud, B., Fehr, E., & Crockett, M. J. (2020). Motivated misremembering of selfish decisions. *Nature communications*, *11*(1), 1–11.

Charness, G., & Dave, C. (2017). Confirmation bias with motivated beliefs. *Games and Economic Behavior*, *104*, 1–23.

Charness, G., Oprea, R., & Yuksel, S. (2021). How do people choose between biased information sources? Evidence from a laboratory experiment. *Journal of the European Economic Association*, *19*(3), 1656–1691.

Chen, D. L., Schonger, M., & Wickens, C. (2016). Otree—an open-source platform for laboratory, online, and field experiments. *Journal of Behavioral and Experimental Finance*, *9*, 88–97.

Coutts, A. (2019). Good news and bad news are still news: Experimental evidence on belief updating. *Experimental Economics*, *22*(2), 369–395.

Di Tella, R., Galiani, S., & Schargrodsky, E. (2007). The formation of beliefs: Evidence from the allocation of land titles to squatters. *The Quarterly Journal of Economics*, *122*(1), 209–241.

Di Tella, R., Perez-Truglia, R., Babino, A., & Sigman, M. (2015). Conveniently upset: Avoiding altruism by distorting beliefs about others' altruism. *American Economic Review*, *105*(11), 3416–42.

Eil, D., & Rao, J. M. (2011). The good news-bad news effect: Asymmetric processing of objective information about yourself. *American Economic Journal: Microeconomics*, *3*(2), 114–38.

Engelmann, J., Lebreton, M., Schwardmann, P., van der Weele, J. J., & Chang, L.-A. (2019). Anticipatory anxiety and wishful thinking.

Epley, N., & Gilovich, T. (2016). The mechanics of motivated reasoning. *Journal of Economic perspectives*, *30*(3), 133–40.

Exley, C. L. (2016). Excusing selfishness in charitable giving: The role of risk. *The Review of Economic Studies*, *83*(2), 587–628.

Exley, C. L., & Kessler, J. B. (2019). *Motivated errors* (tech. rep.). National Bureau of Economic Research.

Falk, A., Becker, A., Dohmen, T. J., Huffman, D., & Sunde, U. (2016). The preference survey module: A validated instrument for measuring risk, time, and social preferences.

Fang, X., Freyer, T., Ho, C. Y., Chen, Z., Goette, L., et al. (2021). *Prosociality predicts individual behavior and collective outcomes in the covid-19 pandemic* (tech. rep.). University of Bonn and University of Mannheim, Germany.

Festinger, L. (1957). *A theory of cognitive dissonance* (Vol. 2). Stanford university press.

Festinger, L. (1962). Cognitive dissonance. *Scientific American*, *207*(4), 93–106.

Festinger, L., & Carlsmith, J. M. (1959). Cognitive consequences of forced compliance. *The journal of abnormal and social psychology*, *58*(2), 203.

Fetzer, T. (2020). *Subsidizing the spread of covid19: Evidence from the UK's eat-out to-help-out scheme*. University of Warwick, Department of Economics.

Frederick, S. (2005). Cognitive reflection and decision making. *Journal of Economic perspectives*, *19*(4), 25–42.

Ganguly, A., & Tasoff, J. (2017). Fantasy and dread: The demand for information and the consumption utility of the future. *Management Science*, *63*(12), 4037–4060.

Getik, D., Islam, M., & Samahita, M. (2021). *The inelastic demand for affirmative action* (tech. rep.). Working Paper Series.

Gilad, B., Kaish, S., & Loeb, P. D. (1987). Cognitive dissonance and utility maximization: A general framework. *Journal of Economic Behavior & Organization*, *8*(1), 61–73.

Gneezy, U., Saccardo, S., Serra-Garcia, M., & van Veldhuizen, R. (2020). Bribing the self. *Games and Economic Behavior*, *120*, 311–324.

Golman, R., Loewenstein, G., Moene, K. O., & Zarri, L. (2016). The preference for belief consonance. *Journal of Economic Perspectives*, *30*(3), 165–88.

Haaland, I., & Roth, C. (2021). Beliefs about racial discrimination and support for pro-black policies. *The Review of Economics and Statistics*, 1–38.

Haisley, E. C., & Weber, R. A. (2010). Self-serving interpretations of ambiguity in other-regarding behavior. *Games and economic behavior*, *68*(2), 614–625.

Heidhues, P., Kőszegi, B., & Strack, P. (2018). Unrealistic expectations and misguided learning. *Econometrica*, *86*(4), 1159–1214.

Helsingen, L. M., Løberg, M., Refsum, E., Gjøstein, D. K., Wieszczy, P., Olsvik, Ø., Juul, F. E., Barua, I., Jodal, H. C., Herfindal, M., et al. (2020). A randomised trial of covid-19 transmission in training facilities. *medRxiv*.

Huffman, D., Raymond, C., & Shvets, J. (2019). Persistent overconfidence and biased memory: Evidence from managers. *Pittsburgh: University of Pittsburgh*.

Konow, J. (2000). Fair shares: Accountability and cognitive dissonance in allocation decisions. *American economic review*, *90*(4), 1072–1091.

Kőszegi, B. (2010). Utility from anticipation and personal equilibrium. *Economic Theory*, *44*(3), 415–444.

Krupka, E. L., & Weber, R. A. (2013). Identifying social norms using coordination games: Why does dictator game sharing vary? *Journal of the European Economic Association*, *11*(3), 495–524.

Kunda, Z. (1990). The case for motivated reasoning. *Psychological bulletin*, *108*(3), 480.

Lerman, C., Hughes, C., Trock, B. J., Myers, R. E., Main, D., Bonney, A., Abbaszadegan, M. R., Harty, A. E., Franklin, B. A., Lynch, J. F., et al. (1999). Genetic testing in families with hereditary nonpolyposis colon cancer. *Jama*, *281*(17), 1618–1622.

Loewenstein, G., & Mather, J. (1990). Dynamic processes in risk perception. *Journal of Risk and Uncertainty*, *3*(2), 155–175.

Malmendier, U., & Tate, G. (2005). Ceo overconfidence and corporate investment. *The journal of finance*, *60*(6), 2661–2700.

Malmendier, U., & Tate, G. (2008). Who makes acquisitions? ceo overconfidence and the market's reaction. *Journal of financial Economics*, *89*(1), 20–43.

Orhun, A. Y., Cohn, A., & Raymond, C. (2021). Motivated optimism and workplace risk. *Available at SSRN*.

Oster, E., Shoulson, I., & Dorsey, E. (2013). Optimal expectations and limited medical testing: Evidence from huntington disease. *American Economic Review*, *103*(2), 804–30.

Rabin, M. (1994). Cognitive dissonance and social change. *Journal of Economic Behavior & Organization*, *23*(2), 177–194.

Romano, J. P., & Wolf, M. (2005). Stepwise multiple testing as formalized data snooping. *Econometrica*, *73*(4), 1237–1282.

Saccardo, S., & Serra-Garcia, M. (2020). Cognitive flexibility or moral commitment? Evidence of anticipated belief distortion.

Saucet, C., & Villeval, M. C. (2019). Motivated memory in dictator games. *Games and Economic Behavior*, *117*, 250–275.

Schwardmann, P. (2019). Motivated health risk denial and preventative health care investments. *Journal of health economics*, *65*, 78–92.

Schwardmann, P., Tripodi, E., & Van der Weele, J. J. (2019). Self-persuasion: Evidence from field experiments at two international debating competitions.

Schwardmann, P., & Van der Weele, J. (2019). Deception and self-deception. *Nature human behaviour*, *3*(10), 1055–1061.

Schweizer, N., & Szech, N. (2018). Optimal revelation of life-changing information. *Management Science*, *64*(11), 5250–5262.

Slovic, P., Fischhoff, B., & Lichtenstein, S. (1980). Facts and fears: Understanding perceived risk. *Societal risk assessment* (pp. 181–216). Springer.

Viscusi, W. K. (1985). A bayesian perspective on biases in risk perception. *Economics Letters*, *17*(1-2), 59–62.

Viscusi, W. K. (1990). Do smokers underestimate risks? *Journal of political Economy*, *98*(6), 1253–1269.

Viscusi, W. K., & O'Connor, C. J. (1984). Adaptive responses to chemical labeling: Are workers bayesian decision makers? *The American Economic Review*, *74*(5), 942–956.

Zimmermann, F. (2020). The dynamics of motivated beliefs. *American Economic Review*, *110*(2), 337–61.

Appendices

A Additional Figures and Tables

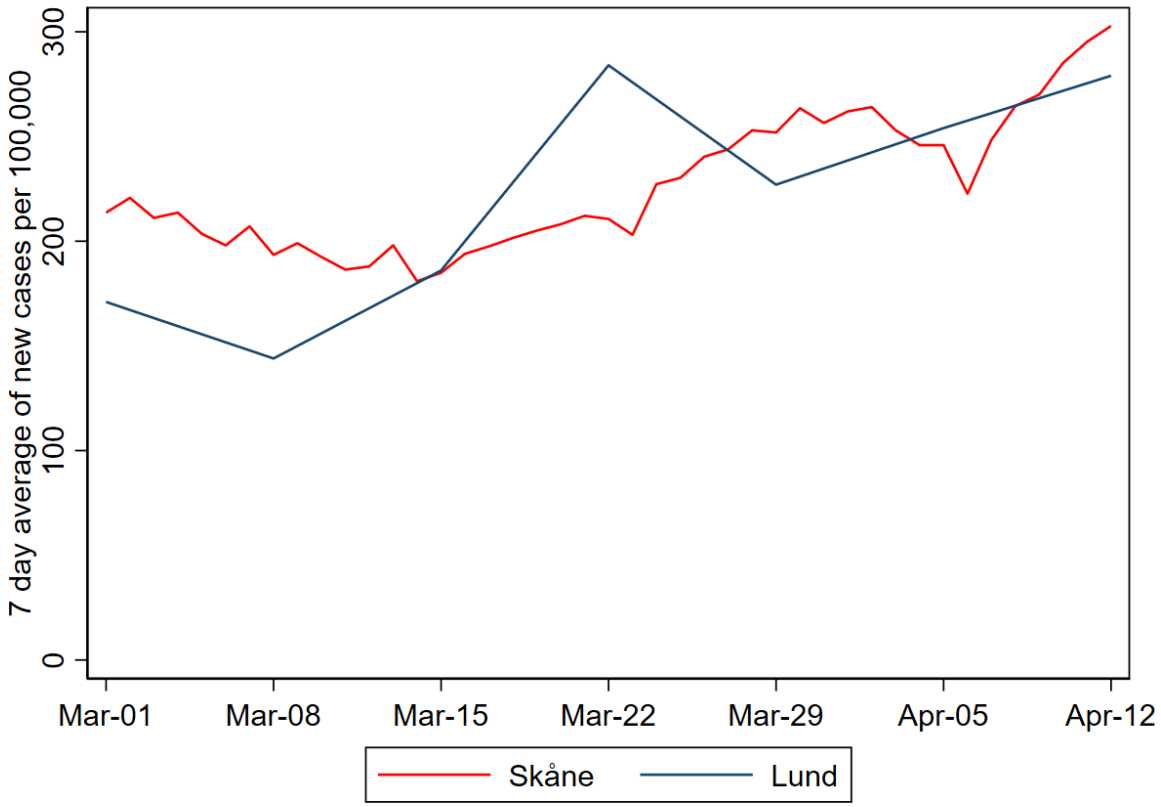


Figure A1: Trend of Covid-19 New Infections in Lund and Skåne

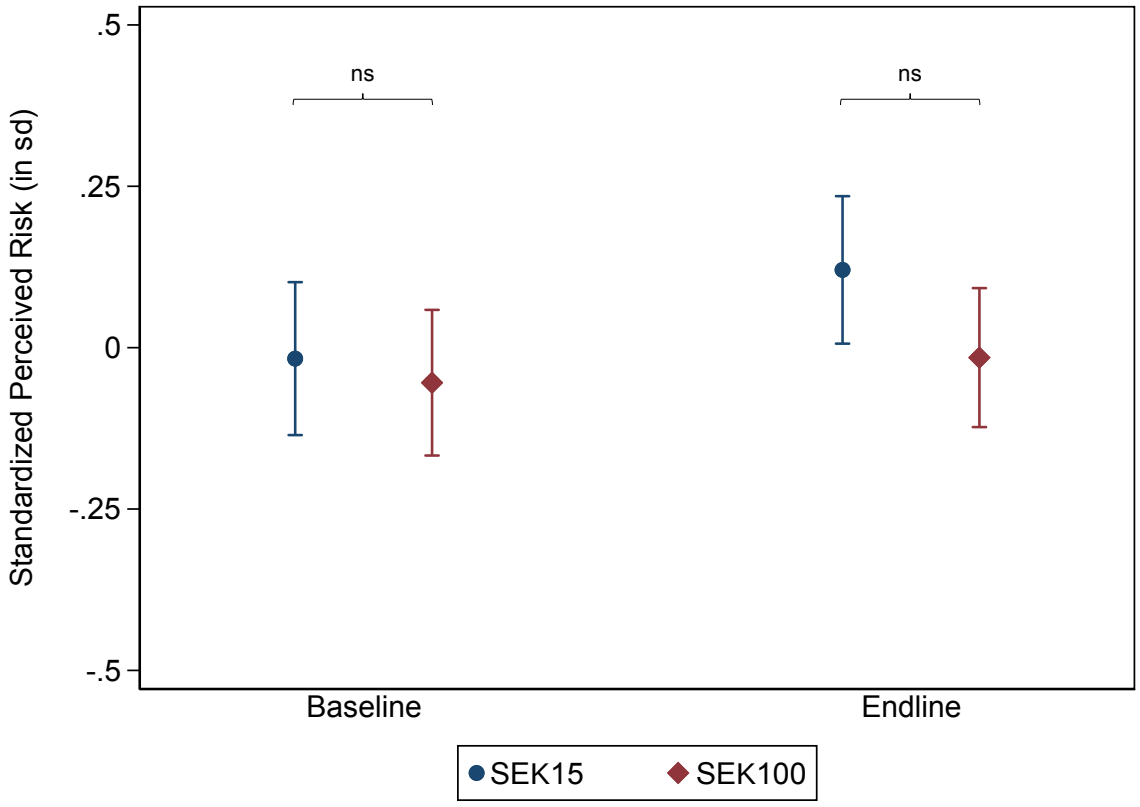


Figure A2: Perceived risk for self associated with café visit

Notes: The figure shows the standardized perceived risk for oneself associated with a café visit for the two voucher treatments in the Baseline and Endline survey. It captures the response to question 1 (Risk for Self). Error bars represent 90% confidence intervals. Test results stem from non parametric Wilcoxon rank-sum tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

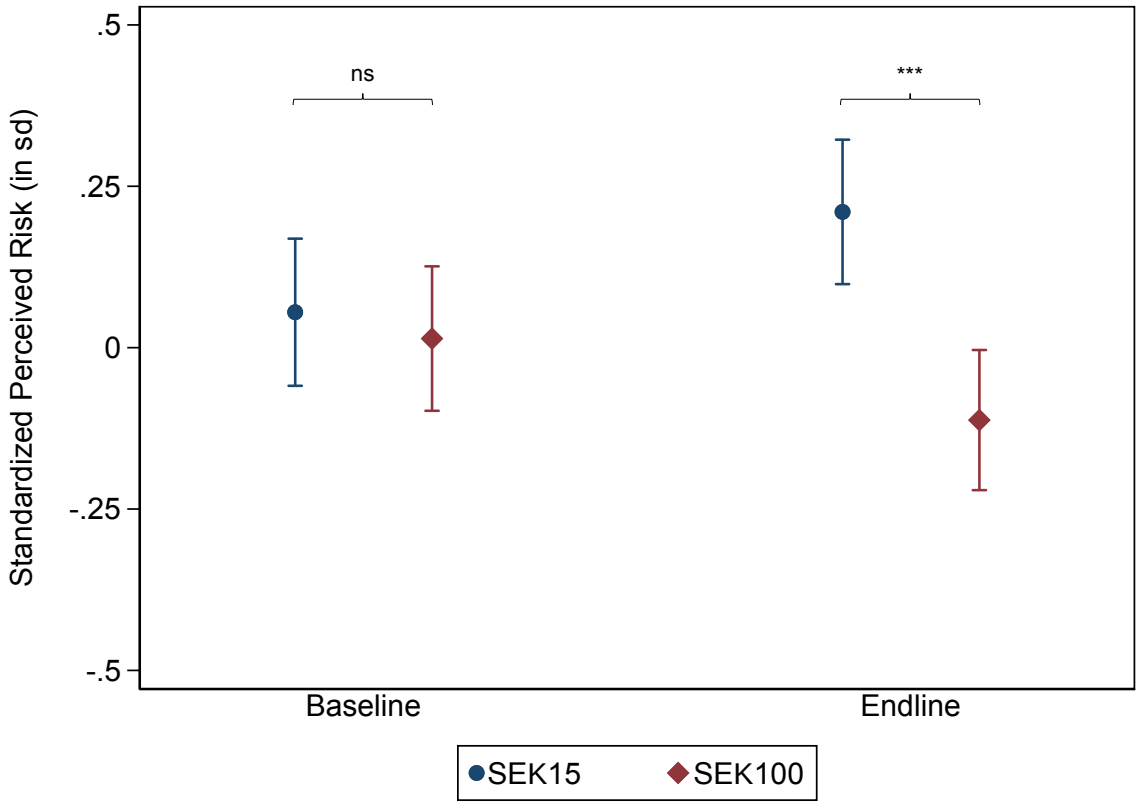


Figure A3: Perceived risk for others associated with café visit

Notes: The figure shows the standardized perceived risk for others associated with a café visit for the two voucher treatments in the Baseline and Endline survey. It captures the response to question 2 (Risk for Others). Error bars represent 90% confidence intervals. Test results stem from non parametric Wilcoxon rank-sum tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

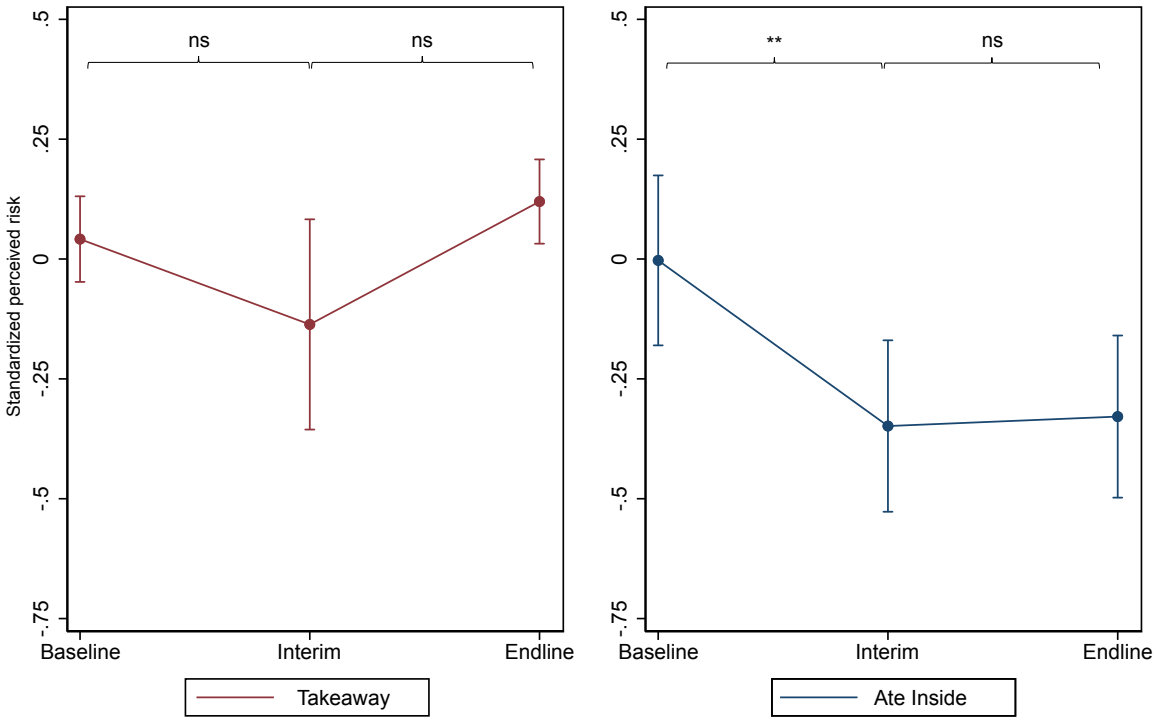
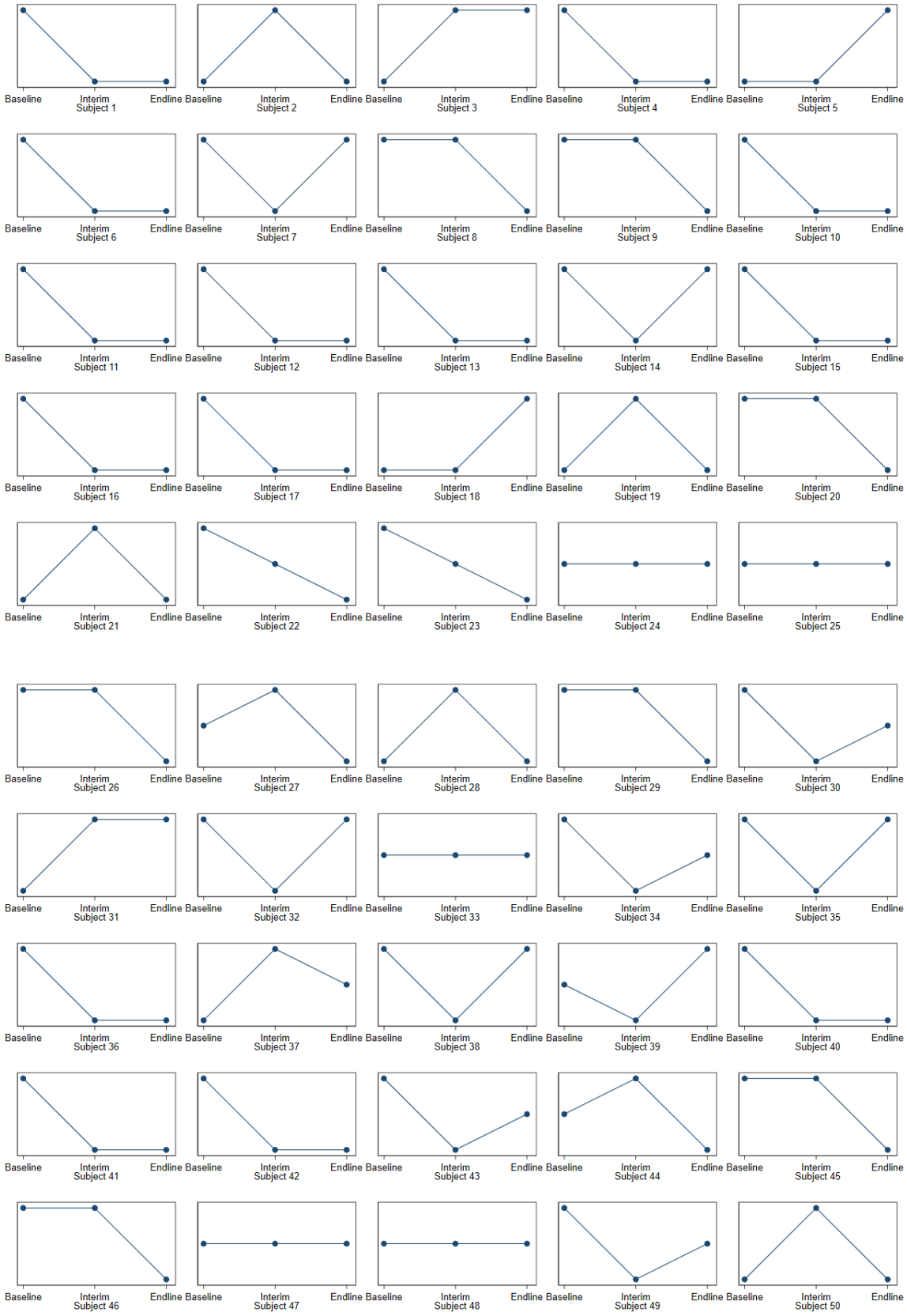


Figure A4: Timing of Updating for Subjects who Eat Inside and Order Take Away

Notes: The figure shows dynamics of average risk assessments for subjects who redeemed and used their voucher. The left panel shows the dynamic pattern for the Total Risk of a café visit for subjects who order take away. The right panel depicts dynamic for subjects who order eat inside the café. The variables are standardized (z-scores). Error bars represent 90% confidence interval. Test results stem from two-sided t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.


Baseline
Interim
Endline
Subject 1
Subject 2
Subject 3
Subject 4
Subject 5
Subject 6
Subject 7
Subject 8
Subject 9
Subject 10
Subject 11
Subject 12
Subject 13
Subject 14
Subject 15
Subject 16
Subject 17
Subject 18
Subject 19
Subject 20
Subject 21
Subject 22
Subject 23
Subject 24
Subject 25
Subject 26
Subject 27
Subject 28
Subject 29
Subject 30
Subject 31
Subject 32
Subject 33
Subject 34
Subject 35
Subject 36
Subject 37
Subject 38
Subject 39
Subject 40
Subject 41
Subject 42
Subject 43
Subject 44
Subject 45
Subject 46
Subject 47
Subject 48
Subject 49
Subject 50

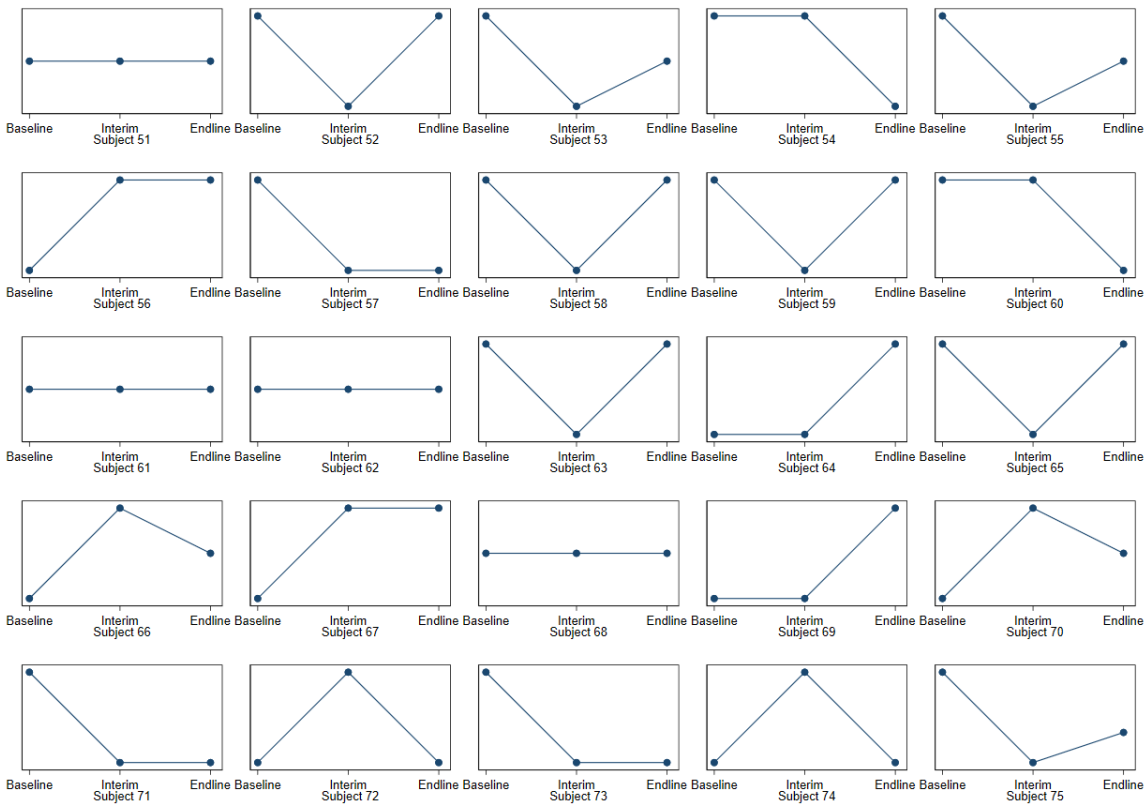


Figure A5: Heterogeneity in Dynamics of Risk Assessments (Subjects who Eat Inside)

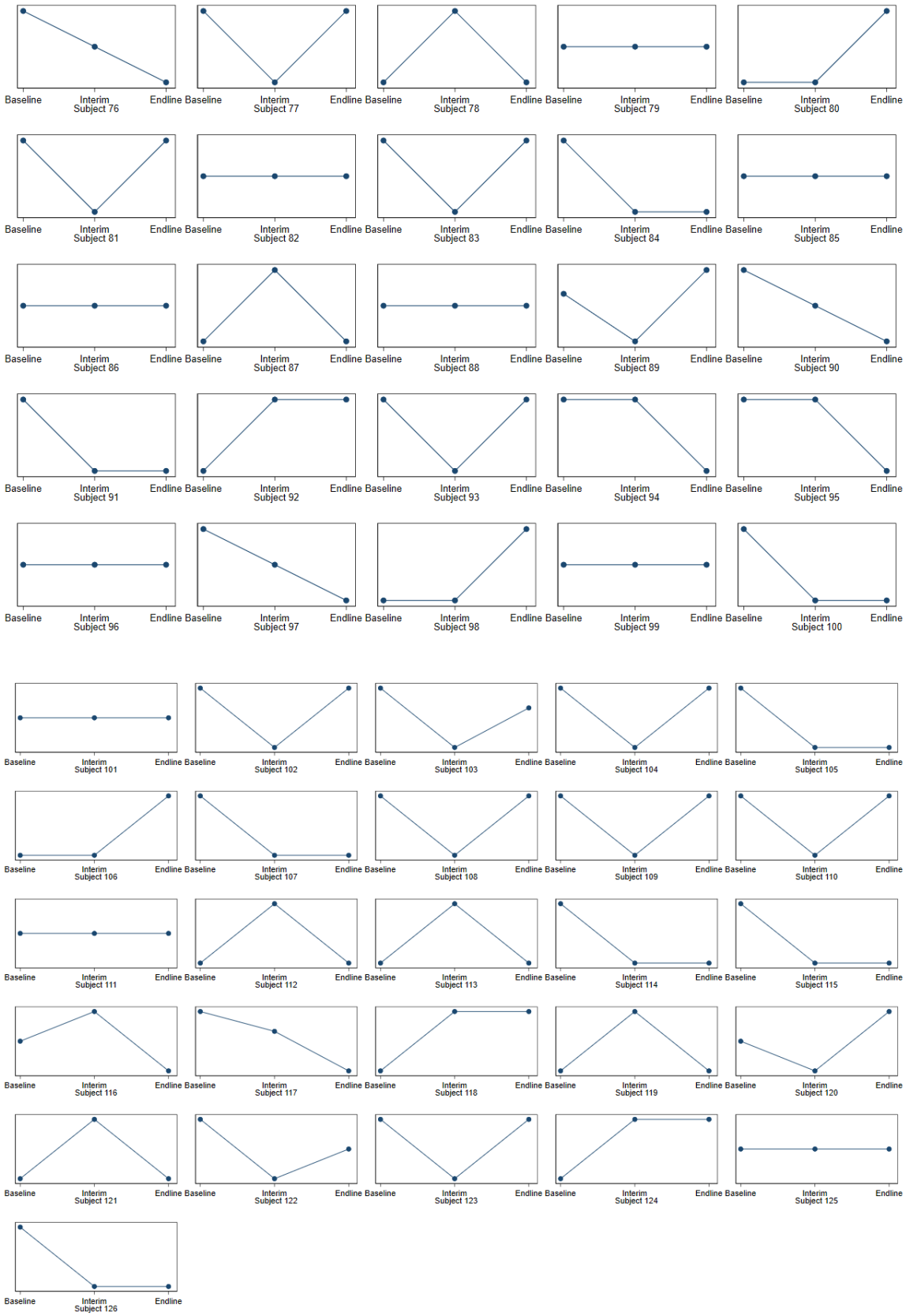


Figure A6: Heterogeneity in Dynamics of Risk Assessments (Subjects who Eat Outside)

Table A1: Survey Content

Survey	Who participated?	Elicitation of Risk Perceptions		Café visits	Socio-demographics	Preference elicitation	Norm elicitation	Recall task	Cognitive Reflection Test	Background information
		Non-incentivized	Incentivized							
Baseline	All subjects	✓	✓	✓	✓	✓				
Interim	Subjects who redeem voucher	✓								
Endline	All subjects	✓	✓	✓			✓	✓	✓	✓

Table A2: Descriptive Statistics and Balance Tests

	SEK15 Voucher					SEK100 Voucher						
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	Mean Diff	p-value
Male	216	.54	.50	0	1	218	.50	.50	0	1	.04	.500
Age	216	22.50	2.42	19	30	218	22.25	2.23	19	30	.25	.259
Study econ	216	.28	.45	0	1	218	.33	.47	0	1	−0.05	.281
Housemates	216	3.56	3.85	0	22	218	3.98	4.87	0	30	−0.42	.318
Health	216	8.26	1.49	2	10	218	8.29	1.47	3	10	−0.03	.860
Patience	216	7.29	1.85	1	10	218	7.36	1.81	1	10	−0.07	.707
Altruism	216	6.46	1.95	1	10	218	6.38	2.23	1	10	.08	.666
Risk	216	5.38	2.00	1	10	218	5.54	1.85	1	10	−0.16	.396
Wear mask	216	.76	.43	0	1	218	.79	.41	0	1	−0.03	.531
Café visits before Baseline	216	1.63	1.98	0	10	218	1.78	2.13	0	12	−0.15	.449
Information treatment	216	.50	.50	0	1	218	.48	.50	0	1	.02	.704
Contracted Covid during study	216	.06	.02	0	1	218	.03	.01	0	1	.03	.097

Table A3: Regression Results – Café Visits (with Controls)

	Cafe Visit					
	Used voucher at studied café		Ate inside at studied café		Any café before Endline survey	
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100 Voucher	0.396***	0.461***	0.268***	0.306***	0.133***	0.145**
	(0.039)	(0.053)	(0.034)	(0.048)	(0.044)	(0.060)
Information	-0.020	0.048	-0.024	0.016	-0.020	-0.008
	(0.039)	(0.039)	(0.034)	(0.027)	(0.044)	(0.067)
Information × SEK100		-0.134*		-0.080		-0.024
		(0.078)		(0.068)		(0.088)
Constant	0.101***	0.068***	0.050**	0.030	0.643***	0.637***
	(0.027)	(0.025)	(0.022)	(0.019)	(0.039)	(0.046)
Observations	434	434	434	434	434	434
R-squared	0.22	0.23	0.15	0.15	0.03	0.03
Controls	✓	✓	✓	✓	✓	✓

Notes: OLS regressions of café visits. Columns (1) to (4) show coefficient estimates for the directly observable voucher data. For columns (1) and (2), the dependent variable is binary and indicates whether the voucher for the studied café was redeemed. For columns (3) and (4), the dependent variable is binary and indicates whether the voucher for the studied café was used to eat inside (as opposed to order takeaway). Columns (5) and (6) show the coefficients estimates for self-reported café visits. The dependent variable is binary and indicates whether any café was visited in the two weeks before the Endline survey. Controls include age, gender, health status, results from the cognitive reflection test and preference measures. Robust standard errors are reported in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Regression Results – Café Visits (Probit & Logit)

	Cafe Visits at any Café			
	Probit		Logit	
	(1)	(2)	(3)	(4)
SEK100 Voucher	0.394***	0.437**	0.649***	0.723**
	(0.128)	(0.181)	(0.214)	(0.303)
Information	-0.060	-0.019	-0.093	-0.027
	(0.129)	(0.177)	(0.214)	(0.288)
Information × SEK100		-0.087		-0.149
		(0.258)		(0.432)
Constant	0.372***	0.352***	0.598***	0.565***
	(0.109)	(0.124)	(0.178)	(0.201)
Observations	434	434	434	434
Controls	✓	✓	✓	✓

Notes: Probit and Logit regressions for self-reported café visits. Columns (1) and (2) show coefficients estimates for self-reported café visits from Probit estimations. Columns (3) and (4) show coefficients estimates for self-reported café visits from Logit estimations. The dependent variable is binary and indicates whether any café was visited in the two weeks prior to the Endline survey. Robust standard errors are reported in parentheses. Controls include age, gender, health status, results from the cognitive reflection test and preference measures. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Regression Results – Reported Number of Café Visits

	Number of café visits before Endline survey (any café)			
	Full Sample		Restricted Sample	
	(1)	(2)	(3)	(4)
SEK100 Voucher	0.190	0.194	0.289**	0.295**
	(0.238)	(0.238)	(0.141)	(0.143)
Information	-0.332	-0.339	-0.111	-0.110
	(0.235)	(0.240)	(0.141)	(0.142)
Constant	1.845***	1.847***	1.387***	1.384***
	(0.254)	(0.257)	(0.120)	(0.121)
Observations	434	434	420	420
R-squared	0.01	0.02	0.01	0.02
Controls		✓		✓

Notes: OLS regressions for reported number of café visits at any cafe in the two weeks prior to the Endline survey (Self Reported Café Visits). Columns (1) and (2) show the results from the full sample. Columns (3) and (4) depict results from a restricted sample. The restricted sample excludes 14 subjects who reported 6 or more café visits over a two week period in part, as a result of their work at a café. Controls include age, gender, health status and results from the cognitive reflection test. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Regression Results – Risk Assessment using Fixed Effects

	Non-Incentivized Assessment			Incentivized Assessment	
	Total Risk	Risk for Self	Risk for Others	Share of New Infections	
	(1)	(2)	(3)	(4)	(5)
Endline	0.130*	0.137**	0.155**	-1.912	-0.636
	(0.073)	(0.064)	(0.073)	(1.292)	(1.082)
Endline × SEK100	-0.242**	-0.099	-0.282***	-3.258*	-3.021*
	(0.102)	(0.096)	(0.106)	(1.920)	(1.566)
Constant	0.034	-0.036	0.034	21.806***	17.949***
	(0.025)	(0.024)	(0.027)	(0.480)	(0.391)
Observations	868	868	868	868	742
R-squared	0.86	0.88	0.85	0.76	0.76
Individual FE	✓	✓	✓	✓	✓

Notes: OLS regressions for non-incentivized and incentivized measures of risk assessments. Columns (1) to (3) show the results for the three non-incentivized measures Total Risk, Risk for Self and Risk for Others. The dependent variables are standardized (z-scores). Column (4) and (5) show the results for the incentivized measure of perceived risk. The dependent variable captures the expected share of new infections, which is due café and restaurant visits. Column (4) presents results of the entire panel. Column (5) presents results of a reduced panel. In the reduced panel, subjects who reported a share greater than 50% either for café visits or gym visits were excluded. Standard errors are clustered at the individual level. Controls include age, gender, health status and results from the cognitive reflection test. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Risk Assessment by Café Visit

	Dep Var: Total Risk						Dep Var: Risk for Others		
	SEK100			SEK15/SEK100					
	Baseline	Endline	Panel	Baseline	Endline	Panel	Baseline	Endline	Panel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Visited studied café	-0.057	-0.374***	-0.057	-0.044	-0.449***	-0.044	-0.039	-0.369***	-0.039
	(0.142)	(0.135)	(0.143)	(0.119)	(0.114)	(0.119)	(0.127)	(0.115)	(0.127)
Endline			-0.015			0.078**			0.071*
			(0.059)			(0.039)			(0.041)
Visited studied café × Endline			-0.317***			-0.404***			-0.331***
			(0.106)			(0.094)			(0.103)
Constant	0.068	0.053	0.068	0.042	0.120**	0.042	0.041	0.112**	0.041
	(0.086)	(0.081)	(0.086)	(0.054)	(0.053)	(0.054)	(0.053)	(0.053)	(0.053)
Observations	218	218	436	434	434	868	434	434	868
R-squared	0.00	0.03	0.02	0.00	0.03	0.01	0.00	0.02	0.01

Notes: OLS regressions for risk assessments. There are two independent variables both of which are binary. *Visited specific café* indicates whether the voucher was used to sit inside the specific café. *Endline* captures the natural time trend of risk assessments over the study period. In columns (1) to (6) the dependent variable is the perceived Total Risk. In columns (7) to (9) the dependent variable is the perceived Risk for Others. Robust standard errors are reported in parentheses. For panel data, the standard errors are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Norm Perception – Robustness

	Second-Order-Belief	
	Appropriateness of Cafe Visit	
	(1)	(2)
Visited studied café	0.111	0.100
	(0.103)	(0.104)
Constant	-0.032	-0.029
	(0.058)	(0.058)
Observations	434	434
R-squared	0.00	0.02
Controls		✓

Notes: The table shows the regression results for the responses of the norm task. The dependent variable is the reported (and incentivized) second-order belief about the appropriateness of visiting a café. The dependent variables is standardized (z-score). Controls include age, gender, health status, and CRT results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Perceived Risk for Self and Others

	Risk for Self				Risk for Others			
	Baseline		Endline		Baseline		Endline	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SEK100 Voucher	-0.033	-0.030	-0.132	-0.130	-0.037	-0.055	-0.320***	-0.334***
	(0.098)	(0.096)	(0.094)	(0.093)	(0.096)	(0.094)	(0.094)	(0.092)
Information	0.223**	0.220**	0.213**	0.211**	0.232**	0.226**	0.132	0.128
	(0.099)	(0.097)	(0.094)	(0.093)	(0.096)	(0.095)	(0.094)	(0.093)
Constant	-0.128	-0.128	0.015	0.015	-0.060	-0.048	0.145*	0.154*
	(0.085)	(0.082)	(0.082)	(0.082)	(0.084)	(0.082)	(0.083)	(0.081)
Observations	434	434	434	434	434	434	434	434
R-squared	0.01	0.07	0.02	0.07	0.01	0.06	0.03	0.08
Controls		✓		✓		✓		✓

Notes: OLS regressions for Risk for Self and Risk for Others. Columns (1) to (2) and (5) to (6) show the results using data from the Baseline survey. Columns (3) to (4) and (7) to (8) present results from the Endline survey. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effect of Risk Perception on Behavior

	Self-reported café visit before Endline survey (any café)								Ate inside studied café	
	Voucher value									
	SEK15	SEK100	SEK15/SEK100	SEK15	SEK100	SEK15/SEK100	SEK15/SEK100		SEK15/SEK100	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Risk for Self (Lag)	-0.117***	-0.063**	-0.117***				-0.070**	-0.075*	-0.056**	-0.021
	(0.031)	(0.030)	(0.031)				(0.030)	(0.045)	(0.024)	(0.017)
Risk for Self (Lag) × SEK100			0.054					0.011		-0.068
			(0.043)					(0.060)		(0.043)
Risk for Others (Lag)				-0.115***	-0.041	-0.115***	-0.031	-0.064	0.032	0.018
				(0.032)	(0.032)	(0.032)	(0.031)	(0.046)	(0.026)	(0.023)
Risk for Others (Lag) × SEK100						0.074		0.064		0.032
						(0.045)		(0.062)		(0.048)
Constant	0.636***	0.765***	0.636***	0.637***	0.765***	0.637***	0.637***	0.636***	0.173***	0.037***
	(0.032)	(0.029)	(0.032)	(0.032)	(0.029)	(0.032)	(0.038)	(0.038)	(0.018)	(0.013)
Observations	216	218	434	216	218	434	434	434	434	434
R-squared	0.06	0.02	0.06	0.06	0.01	0.06	0.06	0.07	0.01	0.14

Notes: OLS regressions for café visits before the Endline survey. The independent variables are Risk for Self and Risk for Others elicited in the course of the Baseline survey. In columns (1) to (8) the dependent variable is the self-reported measure of whether any café was visited in the two weeks prior to the Endline survey. Columns (1) and (4) show results for subjects who received the SEK15 voucher. Columns (2) and (5) show the results for subjects who received the SEK100 voucher. Columns (3) and (6) pool both incentive treatments. Columns (9) and (10) show the result for subjects who used their voucher to sit inside the specific café. Note that among the subjects who used their voucher to sit inside the café the vast majority had a SEK100 voucher. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Alternative Updating Explanations

	Additional Info	News Consumption	Communication
	(1)	(2)	(3)
SEK100 Voucher	0.037	-0.010	0.009
	(0.031)	(0.847)	(0.025)
Information	0.062**	-1.110	0.050**
	(0.031)	(0.847)	(0.025)
Constant	0.066**	7.915***	0.044**
	(0.026)	(0.732)	(0.022)
Observations	434	434	434
R-squared	0.01	0.00	0.01

Notes: The table shows the results of 3 OLS regressions. Column (1) shows the effects of the *voucher* and the INFORMATION treatments on subjects to collect additional information about Covid-19. The dependent variable is binary and indicates whether a subject actively searched for more information (e.g. the infection rate in cafés). Column (2) reports the treatment effect on news consumption (i.e. how often a subject consumed news during the two weeks prior to the Endline survey). Column (3) shows the treatment effects on communication among study participants before the Endline survey. The dependent variable is binary. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Robustness Analysis for Role of Altuism

	Dep Var: Ate Inside		
	(1)	(2)	(3)
SEK100	0.262***	0.267***	0.261***
	(0.033)	(0.033)	(0.034)
Altruism	0.007	0.006	0.004
	(0.012)	(0.012)	(0.012)
Altruism × SEK100	0.075**	0.074**	0.077***
	(0.029)	(0.029)	(0.030)
Total Risk	-0.055***		
	(0.016)		
Risk for Self		-0.046***	
		(0.015)	
Risk for Others			-0.036**
			(0.016)
Constant	0.045***	0.043***	0.045***
	(0.014)	(0.013)	(0.014)
Observations	434	434	434
R-squared	0.17	0.17	0.16

Notes: OLS regression results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Preferences

	Total Risk	Risk for Others	Ate Inside	Norm Perception
	(1)	(2)	(3)	(4)
Altruism	0.136***	0.124***	0.042**	0.060*
	(0.033)	(0.033)	(0.018)	(0.032)
Risk-Seeking	-0.262***	-0.260***	-0.027	0.048
	(0.033)	(0.033)	(0.018)	(0.031)
Patience	0.158***	0.159***	0.011	-0.023
	(0.033)	(0.033)	(0.018)	(0.028)
Constant	0.043	0.046	0.174***	2.436***
	(0.032)	(0.032)	(0.018)	(0.029)
Observations	868	868	434	434
R-squared	0.10	0.10	0.02	0.02

Notes: OLS regression results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Role of Altruism for Risk for Self and Risk for Others

	Dep Var: Risk for Self		Dep Var: Risk for Others	
	Altruism			
	Low	High	Low	High
	(1)	(2)	(3)	(4)
SEK100	-0.051	-0.020	-0.043	-0.032
	(0.146)	(0.135)	(0.141)	(0.131)
Endline	0.156**	0.122**	0.201**	0.116*
	(0.068)	(0.060)	(0.081)	(0.066)
SEK100 × Endline	-0.094	-0.103	-0.253**	-0.310***
	(0.103)	(0.090)	(0.113)	(0.100)
Constant	-0.105	0.059	-0.096	0.185**
	(0.109)	(0.095)	(0.105)	(0.090)
Observations	408	460	408	460
R-squared	0.01	0.00	0.01	0.02

Notes: OLS regression results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Spillover Effects

	Perceived Risk Associated with Activity							
	Gym	Meet Indoors	Meet Outdoors	Publ. Transport	Supermarket	Sport Event	Travelling	Uni Lecture
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information	0.171*	0.241***	0.149*	0.176**	0.213**	0.161*	0.226**	0.147*
	(0.091)	(0.087)	(0.086)	(0.088)	(0.088)	(0.088)	(0.088)	(0.089)
SEK100 Voucher	0.022	-0.051	-0.062	-0.035	0.042	0.046	0.066	-0.054
	(0.098)	(0.097)	(0.094)	(0.097)	(0.099)	(0.095)	(0.097)	(0.097)
Endline × SEK100	-0.152**	-0.127*	0.008	0.015	-0.031	-0.106	-0.181**	-0.082
	(0.062)	(0.077)	(0.086)	(0.078)	(0.076)	(0.073)	(0.073)	(0.069)
Constant	-0.069	-0.065	-0.066	-0.042	-0.115	-0.011	-0.096	0.006
	(0.082)	(0.082)	(0.083)	(0.082)	(0.081)	(0.080)	(0.081)	(0.085)
Observations	868	868	868	868	868	868	868	868
R-squared	0.01	0.02	0.01	0.01	0.01	0.02	0.02	0.01

Notes: OLS regression results. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Romano-Wolf corrected p-values for Spillover Effects

	Endline x SEK100		Information	
	Model p-value	Romano-Wolf p-value	Model p-value	Romano-Wolf p-value
Gym	.0146	.0865	.0600	.1898
Meet Indoors	.0990	.4313	.0060	.0460
Meet Outdoor	.9292	.9760	.0832	.1898
Publ. Transport	.8460	.9760	.0454	.1758
Supermarket	.6770	.9600	.0161	.0759
Sport Event	.1470	.5097	.0693	.1898
Travelling	.0137	.0865	.0106	.0639
Uni Lecture	.2383	.6457	.0995	.1898

Notes: The table presents Romano-Wolf corrected values for the independent variables *Survey 2* × *SEK100* and Information of the OLS regressions presented in table A15.

Table A17: Spillover Effects Robustness

	Perceived Risk of Gym Visit		Self-Reported Gym Visits	
	(1)	(2)	(3)	(4)
Information	-0.157	-0.392	-0.032	-0.033
	(1.265)	(1.221)	(0.044)	(0.043)
SEK100 Voucher	1.919	2.080	0.000	0.001
	(1.468)	(1.433)	(0.047)	(0.047)
Endline × SEK100	-3.715***	-3.715***	-0.014	-0.014
	(1.388)	(1.393)	(0.031)	(0.031)
Constant	18.573***	18.607***	0.405***	0.405***
	(1.189)	(1.177)	(0.039)	(0.039)
Observations	868	868	868	868
R-squared	0.01	0.06	0.00	0.05
Controls		✓		✓

Notes: The table shows the results of OLS regressions for alternative indicators for the risk of visiting a gym. Columns (1) and (2) depict the results for an incentivized measure of perceived risk - the expected share of Covid-19 new infections that are due to gm visits. Columns (3) and (4) show the results for the self-reported measure of gym visits. The dependent variable in columns (3) and (4) is binary and indicates whether a gym was visited during the period 2 weeks prior to the respective survey. All regressions exploit data from the Baseline and Endline survey. Standard errors are clustered at the individual level. Controls include age, gender, health status and results from the cognitive reflection test. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Experimenter Demand

To reduce concerns of experimenter demand effects, my primary measure was to exploit a number of incentivized questions, which made it costly to deviate from one's preferred response. In addition to that, I conducted an analysis, similar to Schwardmann et al. (2019), to investigate whether the recruited subjects in my sample were able to anticipate the research hypothesis of this study. To satisfy experimenter demand, anticipation the research hypothesis is a necessary condition. At the end of the Endline survey (page 8), all subjects were asked to fill in an open text field to state their guesses of the research intent. I asked an independent person to read all of those entries and manually categorize them. Table B1 provides an overview of the categorization.

Table B1: Summary Statistics by Treatment

Category Number	Category	Voucher		Total	Share (in %)
		SEK15	SEK100		
7	Study whether risk is assessed to justify behavior	0	1	1	0.23
6	Study risk assessment or behavior related to voucher	13	12	25	5.76
5	Study if risk assessment and behavior are linked	15	16	31	7.14
4	Study change in risk assessment or behavior over time	26	22	48	11.06
3	Study risk assessment or behavior under COVID	105	111	216	49.77
2	Study COVID in general (e.g. spread of virus)	34	45	79	18.20
1	Other research question	16	5	21	4.84
0	Left field blank or stated "No Idea"	7	6	13	3.00

Notes: The table displays the categorization of responses from the open text field to indicate the study intent. The eight categories were defined by the researcher but the categorization was made by an independent person.

The responses indicating the study intent are categorized in eight groups and ranked by accuracy in descending order. Category 0 includes empty responses or responses without a valid guess. Only 3% of the subjects fall into that category. Category 1 includes guesses, which were entirely unrelated to the purpose of study (e.g., responses that indicated that this study tested their memory). Category 2 includes guesses, which indicated that the study purpose was to investigate Covid-19 in general (e.g., how the virus would spread in Sweden). Category 3 includes guesses, which indicated that the study purpose was to investigate students' risk assessment or their behavior during the pandemic. This is in line with the general study purpose stated in the consent form. It is therefore not surprising that almost 50% of the responses repeated that intent. However, the responses do not reveal any deeper understanding of the study purpose. Category 4 includes guesses, which indicate that the longitudinal structure of the study had the purpose to identify changes in assessments and/or behavior over time. Category 5 includes guesses, which reveal that the students understood that I sought to explore the relation between risk assessments and behavior. However, the responses do not suggest that the participants understood that the voucher was part of the study. Only approximately 6% of the responses reveal that the students understood that the voucher was part of the study (rather than just an

additional payment). Those responses indicate that the risk assessments or the behavior elicited during the surveys was linked to the voucher. With one exception, all of those responses implied that my main purpose was to study whether students would respond to the voucher (category 6) suggesting that the main point of interest was to study risky behavior rather than assessments. Only one student (category 7) correctly anticipated the main study purpose, that is, whether the risk assessment served to justify risky behavior.

Based on the responses, if anything, very few students correctly anticipated the main research hypothesis of this study. A Wilcoxon rank-sum test further rejects the hypothesis that students of the HighIncentive treatment were better in assessing the study purpose than students of the LowIncentive treatment ($p = 0.860$). Nevertheless, it could be possible that subjects whose responses revealed a deeper understanding of the study purpose, biased my regression results. I therefore replicated my main regression analysis for risk assessments excluding subjects of category 5, 6 and 7. The results are displayed in Table B2.

Table B2: Risk Assessments Excluding Subjects whoose Guesses Revealed a Depper Unerstanding of the Study Purpose

	Non-Incentiveized Measure				Incentivized Measure	
	Total Risk		Risk for Others		Share of new infections	
	Cat 0-5	Cat 0-4	Cat 0-5	Cat 0-4	Cat 0-5	Cat 0-4
	(1)	(2)	(3)	(4)	(5)	(6)
SEK100	0.106	0.083	0.050	0.010	2.126	2.018
	(0.098)	(0.102)	(0.098)	(0.101)	(1.517)	(1.568)
Information	0.200**	0.198**	0.168*	0.157*	1.975	1.513
	(0.090)	(0.095)	(0.089)	(0.091)	(1.234)	(1.266)
Endline	0.120**	0.110**	0.155***	0.157***	-1.823*	-1.670*
	(0.053)	(0.055)	(0.054)	(0.057)	(0.958)	(0.977)
Endline × SEK100	-0.225***	-0.228***	-0.293***	-0.296***	-3.177**	-3.182**
	(0.074)	(0.077)	(0.078)	(0.083)	(1.419)	(1.469)
Constant	-0.135*	-0.118	-0.093	-0.064	19.579***	19.746***
	(0.080)	(0.084)	(0.082)	(0.087)	(1.162)	(1.213)
Observations	816	754	816	754	816	754
R-squared	0.01	0.01	0.02	0.02	0.02	0.02

Notes: The table displays the regression results of 6 panels excluding subjects of either category 6 and 7 (odd columns))or category 5,6 and 7 (even columns). The information above the column row indicates the included category numbers. The dependent variables are the three non-incentivized measures of perceived risk, Total Risk, Risk for Self and Risk for Others. Standard errors are clustered at the individual level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The regression results replicate the main findings displayed in Table 3 and Table 4. All the regression estimates and their significance levels, for both non-incentivized and incentivized risk assessments, remain qualitatively unchanged. The interaction term (*Endline* × *SEK100*) still reveals a considerable treatment effect suggesting that subjects of the HighIncentive treatment motivate their risk assessments by downplaying the risk related to a café visit. Even after excluding subjects who merely notice a relation between the elicited risk assessments and the elicited behavior (category 5), this main result remains robust. I interpret the regression results as evidence that anticipation of the research hypothesis did not drive the effect of

motivated risk assessments.

C Surveys

C.1 Baseline Survey

Welcome to our study and thank you for participating!

Background
I am a researcher from Lund University conducting a research study about COVID-19. By participating in this study you contribute to the important research in this area. As you know, the COVID-19 pandemic is currently threatening millions of people around the world. For that reason, research that allows us to understand how people behave during this pandemic is essential. I therefore like to ask you to take this study seriously and give truthful answers.

The Study
This study is targeted at students who currently live in Lund. **Please do not participate if you will not be in Lund for the next 2 weeks.**
The study will consist of two parts. Part 1 will take place today. You will be asked to read a brief information note about COVID-19 and then participate in a survey that will last around 10-15 minutes. Upon completion of the first part, you will receive a compensation of 50kr. The second part of this study will take place in 15 days. It will consist of a second survey, which will also be about 10-15 minutes long. For the second survey, you will receive a compensation of 100kr. During both surveys you will also have the chance to earn additional money. At the end of the survey today you will be asked to state your preferred payment method. You can choose between bank transfer and Swish.
You will receive a reminder to participate in survey 2. However, if you already know that you will not be able to participate in both surveys, please do not participate today.

Management of Data and Confidentiality
Both surveys will collect data about your choices but all information will be confidential and anonymous. While the results of this study will be presented in a research paper, I will only report general, descriptive data. None of your choices will be linked to your personal information, which means that the reported data cannot be traced back to you as an individual. Lastly, all responses will be protected so that unauthorized persons will not be able to access them.

Participation
Your participation in this study is voluntary and you can choose to cancel the participation at any time by pressing a button that cancels the questionnaire. If you choose not to participate or want to cancel your participation, you do not need to state why.

No special knowledge is required for the two parts of this study. I am mostly interested in your personal opinions and judgments. However, please do not participate in this study if you are currently infected with COVID-19, if you have been infected previously or if you are part of the COVID-19 risk group.

Responsible Researcher
Marco Islam
PhD Student
marco.islam@nek.lu.se
Department of Economics
Lund University, Sweden

☐ I am not infected with COVID-19 or have been infected previously.

☐ I am not part of the COVID-19 risk group.

☐ I commit to participate in both parts of the study.

Continue

Information about COVID-19

On this page, I would like to provide you with some information about the danger of COVID-19 for people in your age group. Even though you may have heard about some of the information before, I would like to ask you to take some time and read this page carefully. At the end of the page, you will have to answer two questions in order to proceed. Thanks for taking the time!

Health effects for young adults

1. Even among young adults without underlying chronic medical conditions, COVID-19 can result in prolonged illness: Among young adults infected with COVID-19, 35% had not returned to their usual state of health 2–3 weeks after detecting the virus. (Source)
2. A study from the US reveals an association between COVID-19 and an increased risk for a stroke for young populations without a severe risk for a stroke. Compared to the seasonal influenca, the chance of experiencing a stroke increases by factor 7.6. (Source)
3. The WHO reports that COVID-19 increases the risk of heart failure and damage to lung tissue. Moreover, it may affect a person's mental health as COVID-19 can cause anxieties and depressions. (Source)

Young adults and their impact on others

1. A study from the Stockholm Region has shown that in the early phase of COVID-19 (March-June) disproportionally many older people were infected who lived in areas with young people. (Source)
2. Young adults seem to have a longer incubation time (8 days) than middle-aged or elderly patients, which means they can infect others over a longer time period. (Source)
3. On average, young adults infect 50% of their family members within 1.4 days. (Source)
4. Different sources from the US report that increases in COVID-19 cases among young adults are most likely to affect people aged >60 with a delay. This delay is estimated to be around 14 days. (Source1, Source2)

☑ I have read the information and would now like to answer the two questions.

1. By which factor does the chance of receiving a stroke increase after being infected with COVID-19 compared to the seasonal influenca?

2. How many days does it take approximately (in the US) until the virus passes from young adults to people who are older than 60 years?

Continue

Page 1

Please answer the following questions.

How dangerous do you think is COVID-19 for you personally?

1 - Not at all dangerous

How dangerous do you think is COVID-19 for the people you interact with regularly (e.g. friends, family or colleagues)?

4

How dangerous do you think is COVID-19 for a person in the age group 18 to 40?

2

How dangerous do you think is COVID-19 for a person who is 65 years or older?

7 - Very dangerous

How dangerous do you think is COVID-19 for a person who is part of the risk group? (People in the risk group are for example people who are older than 65, people who have a weak immune system or people with a long-term medical condition):

7 - Very dangerous

How many people you interact with regularly (e.g. friends, family or colleagues) are part of the COVID-19 risk group?

10

How likely do you think is it that you would catch COVID-19 if a person you interact with (e.g. friends, family or colleagues) was infected?

81%-90%

How many non-infected people do you think will catch the virus from you, if you were infected?

20

Next

Page 2

The following questions revolve around the risks of visiting a café or a restaurant in times of COVID-19. With café or restaurant visits, we mean classic "in-house visits" where you sit down at a table to eat your food or drink your drinks. We do not mean short visits to pick up food or drinks, which are then consumed outside of the cafe or the restaurant.

If you visited a cafe or restaurant in Lund today, how worried would you be about your own health?

3

If you visited a cafe or restaurant in Lund today, how worried would you be that you could infect other guests or personnel with COVID-19?

5

How likely do you think is it that during your visit of the cafe or restaurant at least one other guest would be part of the COVID-19 risk group?

21%-30%

If you visited a café or restaurant in Lund today, how worried would you be that you could infect yourself during the visit and then pass the virus on to other people you interact with (e.g. friends, family or colleagues)?

7 - Very worried

Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to visit a café or restaurant?

6

Next

Page 3

Considering the risk for yourself and the risk you impose on others (both during and after the activity), how dangerous do you think is it currently ...

... to go to the gym?

6

... to use means of public transportation?

4

... to meet 5 or more people who do not live with you indoors?

5

... to meet 5 or more people who do not live with you outdoors?

2

... to go to the supermarket?

3

... to go to a concert or a sport event (assuming it is allowed to do so)?

5

... to travel by plane or train?

5

... to attend a university lecture?

5

Next

Page 4

Time left to complete this page: 1:22

The following four questions allow you do earn an extra payment of up to 500 kr. You have exactly 120 seconds to answer them. At the end of the study, we will draw one question and select 1 participant at random to win the payment. If you are chosen for payment, we will evaluate how close your answer for the selected question is to the correct answer. The closer it is to the correct answer, the more money you will win. The table below details your payments in this task correspond with the accuracy of your answer.

	Payments according to the deviation of your answer and the correct answer					
Deviation	0% Your answer exactly matches the correct answer	+/− 5%	+/− 10%	+/− 15%	+/− 20%	> 20%
Payment	500 SEK	475 SEK	400 SEK	275 SEK	100 SEK	0 SEK

Please answer the following four questions:

What is the percentage share of new infections that is due to gym visits? (Please state a number between 0 and 100):

5

What is the percentage share of new infections that is due to café and restaurant visits? (Please state a number between 0 and 100):

2

How many people were registered as newly infected with COVID-19 in Skåne yesterday?

53

On average, how many people did one contagious person in Sweden infect yesterday?

1.1

Next

Page 5

How many times have you visited a café or restaurant during the last 2 weeks?

1

How many times have you gone to the gym during the last 2 weeks?

0

How many rides in means of public transportation have you made during the last 2 weeks?

4

How many times have you followed news about COVID-19 in the media during the last 2 weeks?

5

Next

Page 6

How many people you interact with regularly (e.g. friends, family or colleagues) are or have been infected with COVID-19?

3

Do you think it should be enforced to wear a face mask in public buildings or means of public transportation?

Yes

Do you wear a face mask in public spaces?

Sometimes

Next

Page 7

How old are you in years?

24

What is your highest academic degree (e.g. Bachelor)?

Master

How many persons live in your household?

2

How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?

9

In general, how willing are you to take risks?

6

How willing are you to give to good causes without expecting anything in return?

9

In your opinion, how is your health condition?

10 - very good

Next

Thank you! You have now completed the first survey!

This survey has been conducted in collaboration with the café Incognito, which is located in Lund, Lilla Fiskaregatan 23. Among all participants, some have been randomly selected to receive a 100kr gift card in addition to their guaranteed payments for participating in the surveys. You are one of the lucky persons! During the next 24 hours, you will receive an email, with a link that allows you to activate the gift card. The gift card is valid for 14 days and is not transferable to another person. Additional information about the conditions to use the gift card can be found in the mail.

As promised, you will also receive your main compensation of 50kr for your participation. You can choose whether you want to receive this payment by Swish or by ordinary bank transfer. Please fill in the necessary information below so that we can initiate the payment as soon as possible.

Remember: The second survey will take place in 15 days. You will receive a reminder by email one day before. Please also check your spam folder. It is very important that you are able to participate.
For the second survey, you will receive a compensation of 100kr. All potential bonus payments will be determined after the second survey. Note however that you are only eligible to receive those payments if you participate in both surveys.

Payment Information

Which payment method do you prefer?

- ○ Bank Transfer
- ◉ Swish

Telephone number:

123456789

Continue

C.2 Voucher

In order to activate your gift card, you will have to enter your participation ID. You will find your ID in the e-mail, which included the link to this gift card gift card.
Important: Once you activate the gift card, it will only be valid for 60 minutes, so please do not activate it before you really want to use it.

Participation ID:

I want to activate my gift card now

Your gift card has been successfully activated! Before it will be displayed, however, we would kindly like to ask you to answer the three questions below.

The following 3 questions revolve around the risks of visiting a café or a restaurant in times of COVID-19. With café or restaurant visits, we mean classic "in-house visits" where you sit down at a table to eat your food or drink your drinks. We do not mean short visits to pick up food or drinks, which are then consumed outside of the cafe or the restaurant.

1. How worried are you about your own health when you visit a café or restaurant?

----

2. How worried are you that you could infect yourself during the café visit and then pass the virus on to other people you interact with (e.g. friends, family or colleagues)?

----

3. Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to visit a café or restaurant??

Display the gift card

Your gift card is now valid!
Please show it to the personnel together with your ID.

Name: John Doe
Value: SEK 100
Expiration Date: 05.04.2021-18:00
Participation code: H18UfV

C.3 Endline Survey

Welcome to the second survey of our study and thanks for participating again! Today's survey will take about 10-15minutes. For its completion you will receive a compensation of 100kr. Before you start to fill out or second survey, please answer the following question:

Are you currently infected with COVID-19 or have you been infected previously?

○ Yes ○ No

Start

Page 1

The following questions revolve around the risks of visiting a café or a restaurant in times of COVID-19. With café or restaurant visits, we mean classic "in-house visits" where you sit down at a table to eat your food or drink your drinks. We do not mean short visits to pick up food or drinks, which are then consumed outside of the cafe or the restaurant.

If you visited a cafe or restaurant in Lund today, how worried would you be about your own health?

1 - Not at all worried

If you visited a cafe or restaurant in Lund today, how worried would you be that you could infect other guests or personnel with COVID-19?

1 - Not at all worried

How likely do you think is it that during your visit of the cafe or restaurant at least one other guest would be part of the COVID-19 risk group?

0%-10%

If you visited a café or restaurant in Lund today, how worried would you be that you could infect yourself during the visit and then pass the virus on to other people you interact with (e.g. friends, family or colleagues)?

1 - Not at all worried

Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to visit a café or restaurant?

1 - Not at all dangerous

Next

Page 2

Considering the risk for yourself and the risk you impose on others (both during and after the activity), how dangerous do you think is it currently ...

... to go to the gym?

2

... to use means of public transportation?

4

... to meet 5 or more people who do not live with you indoors?

4

... to meet 5 or more people who do not live with you outdoors?

3

... to go to the supermarket?

4

... to go to a concert or a sport event (assuming it is allowed to do so)?

5

... to travel by plane or train?

5

... to attend a university lecture?

5

Next

Page 3

Now we would like to know how *socially appropriate* you think other students find it to behave in certain ways.

Every morning, we ask one random student who does not participate in this study how *socially appropriate* he/she thinks it is to engage in the activities listed below. It is your task to guess the answer of the random student we asked today.

Your response on this page allows you to earn additional 500kr. At the end of the study we will select one of your four answers on this page at random and verify if the selected answer matches with the answer of the random student. If your answer matches with his/her answer, you will enter a pool of participants who are eligible for the extra payment. Of all eligible participants, we will select 1 participant at random at the end of the study.

Note that in this task it is important for you to imagine what other people think is socially appropriate behavior. This can but does not have to be the same behavior that you deem appropriate.

How *socially appropriate* does the randomly chosen student think is it currently ...

.. to visit a café in Lund?

... to go to the gym?

... to meet 5 or more people who do not live with you indoors?

... to engage in risky activities if those support local businesses (e.g. cafés, boutiques or bookstores)?

Next

Page 4

Time left to complete this page: 1:10

The following four questions allow you do earn an extra payment of up to 500 kr. You have exactly 120 seconds to answer them. At the end of the study, we will draw one question and select 1 participant at random to win the payment. If you are chosen for payment, we will evaluate how close your answer for the selected question is to the correct answer. The closer it is to the correct answer, the more money you will win. The table below details your payments in this task correspond with the accuracy of your answer.

	Payments according to the deviation of your answer and the correct answer					
Deviation	0% Your answer exactly matches the correct answer	+/− 5%	+/− 10%	+/− 15%	+/− 20%	> 20%
Payment	500 SEK	475 SEK	400 SEK	275 SEK	100 SEK	0 SEK

Please answer the following four questions:

What is the percentage share of new infections that is due to gym visits? (Please state a number between 0 and 100):

4

What is the percentage share of new infections that is due to café and restaurant visits? (Please state a number between 0 and 100):

1

How many people were registered as newly infected with COVID-19 in Skåne yesterday?

48

On average, how many people did one contagious person in Sweden infect yesterday?

0.9

Next

Page 5

How many times have you visited a café or restaurant during the last 2 weeks?

2

How many times have you been to the gym during the last 2 weeks?

0

How many rides in means of public transportation have you made during the last 2 weeks?

6

How many times have you followed news about COVID-19 in the media during the last 2 weeks?

6

Next

Page 6

On this page, we would like to show you 7 question, which we have asked you in survey 1 already. We would like to understand how well you can remember the answers you gave in survey 1. Please think about how you answered the 7 questions two weeks ago.

Your answers on this page allow you to earn additional 500kr. At the end of the study we will select one of your 7 answers on this page at random. We will then verify if the selected answer matches with your answer from survey 1. If your answer matches with your answer from survey 1, you will enter a pool of participants who are eligible for the extra payment. Of all eligible participants, we will select 1 participant at random at the end of the study.

If you visited a café or restaurant in Lund today, how worried would you be about your own health?

3

If you visited a café or restaurant in Lund today, how worried would you be that you could infect other guests or personnel with COVID-19?

3

How likely do you think is it that during your visit of the café or the restaurant at least one other guest would be part of the COVID-19 risk group?

11%-20%

If you visited a café or restaurant in Lund today, how worried would you be that you could infect other people you interact with (e.g. friends, family or colleagues) after your visit?

5

Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to visit a café or restaurant?

6

Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to go to the gym?

6

Considering the risk for yourself and the risk you impose on others (both during and after your visit), how dangerous do you think is it currently to use means of public transportation?

6

Next

Page 7

Please answer the following three questions.

A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball.How much does the ball cost (in cents)?

If it takes 5 machines 5 minutes to make 5 widgets, how many minutes would it take 100 machines to make 100 widgets?

In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake,how many days would it take for the patch to cover half of the lake?

Next

Page 8

Please answer the following questions.

Before you participated in the two surveys of this study, did you talk to other participants about them?

During the two weeks in between the two surveys, did you actively search for additional information about COVID-19 (for example, information about the infection rate at cafes or gyms)?
If so, please specify the information you collected.

Please guess what the two surveys intend to study:

Next

C.4 Participation Mail

Subject: Covid-19 Survey Study

Dear [Name],

Some weeks ago, you registered to participate in a survey study, which seeks to study how students perceive the threat of Covid-19 and how the pandemic affects their lives. Now, I would kindly like to invite you to participate in this study. Your participation involves filling out two surveys. The first survey can be accessed by clicking on the link below. The second survey will take place 15 days after you completed the first. For your participation in the two surveys, you are guaranteed to receive a compensation of 150kr. However, you can also earn significantly more money depending on your answers and a bit of luck.

You can participate by using a computer, smartphone or tablet. All your answers in the surveys will be anonymous. All data will be treated in accordance with the general data protection regulations (GDPR) of the EU and will only be used for research purposes. Your participation is entirely voluntary and is highly appreciated.

For any questions, please contact me at marco.islam@nek.lu.se or by responding to this e-mail.

Thank you very much for supporting my research!
Marco

Your participation ID: XXXXX
Link to Survey 1: https://covsurv1.herokuapp.com/room/Covid/

C.5 Voucher Mail

Subject: Covid-19 Survey Study - Café voucher

Hej XXX

Thank you for participating in the first survey. As promised at the end of the survey, I would like to send you your digital voucher for café [name of café] in Lund. Your voucher has a value of 100 SEK. Before you activate it, please read the following information carefully.

Terms

To redeem and use your voucher, you need to activate it through a phone or tablet until [Date two weeks after Baseline Survey]. Please note that once you activate the voucher, you must redeem and use it within 60 minutes. You can activate it by clicking on the link on the bottom of this e-mail and entering your participation ID. Make sure not to activate your voucher before you really want to use it.

Your voucher is personalized. This implies that only you can use it. You are not allowed to give it to another person. When you want to redeem it, please show the activated voucher on your phone or tablet together with an ID to any staff member of the café. You do not need to print it.

Due to the current circumstances, please do not visit the café if you experience any cold symptoms. I would also like to ask you not to visit the café with more than one other person.

About café [name of café]

The café is located at [address of café]. It is open on weekdays from 11:00 – 18:00 and on Saturdays from 11:00 – 17:00. You can find more information about the café here: [link to webpage of café]

Participation ID and Activation Link

Participation ID: XXXXX

Activation Link: https://covsurvgift.herokuapp.com/InitializeParticipant/ecrzldjp

For any questions, please contact me at marco.islam@nek.lu.se or by responding to this e-mail.

Kind regards,

Marco