Journal of Economic Behavior and Organization 183 (2021) 377–396

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Stress may increase choice of sooner outcomes, but not temporal discounting☆

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ARTICLE INFO

Article history:
Received 13 August 2020
Revised 24 November 2020
Accepted 19 December 2020
Available online 3 February 2021

JEL classification:
D03
C91
D9

Keywords:
Stress
Intertemporal choice
Temporal discounting
Present bias
Preferences
Laboratory experiment

ABSTRACT

Recent work in behavioral economics has shown that stress increases choices of smaller, sooner gains relative to larger, later monetary gains. The simplest model that explains these findings is one in which stress increases the discount rate or present bias. A sharp test of this model is provided by intertemporal choices in the losses or effort domain: this model predicts that stress should lead to increased choice of larger, later losses or effortful tasks relative to smaller, earlier ones. Here we show suggestive evidence for the opposite result: using a laboratory experiment with 578 participants from informal settlements in Nairobi, Kenya, we find that stress increases choices of smaller, sooner outcomes across domains. Specifically, we show that the effect is present in monetary gains and losses, and effortful tasks; and for both a psychosocial stressor (Trier Social Stress Test), and the pharmacological elevation of stress hormone levels using hydrocortisone. Importantly, the results are statistically robust only in the absence of clustering, and should thus be regarded as tentative. However, they are at least initially consistent with a model in which stress increases discounting in the gains domain but decreases it in the losses and effort domains; or with a model in which stress decreases the utility of any future outcome. Thus, stress may affect intertemporal choice, but may do so through mechanisms other than a simple increase in discount rates or present bias.

☆ This work was supported by grant NIH UH2 NR016378 from the National Institutes of Health and is part of the NIH Science of Behavior Change program. We are grateful to Tilman Graff, Daniel Mellow, Kristina Esopo, Amanda Starcev, Jennifer Adhiambo, Irene Gachungi, Faith Vosevwa, Chaning Jang, and David Clarence, for excellent research assistance, and the Busara Center for Behavioral Economics for data collection. This study was reviewed by Princeton University's IRB (0000007367), the Pharmacy & Poisons Board Kenya (PPB/ECCT/15/12/04), and the Kenya Medical Research Institute (#536). This study was registered in AEA RCT Registry (#2741).

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https://doi.org/10.1016/j.jebo.2020.12.024
0167-2681/

1. Introduction

People often make decisions which involve tradeoffs between the present and the future; for example, they decide between consuming today and saving for later. Recent research has suggested that such intertemporal choices are affected by stress: in particular, stress makes people more likely to choose smaller, sooner relative to larger, later monetary payments (Riis-Vestergaard et al., 2018; Delaney et al., 2014). A straightforward interpretation of these results is that stress increases the discount rate or present bias. This interpretation predicts that when intertemporal choices are made over losses or effort, stress should make people more likely to choose the larger, later losses or effort over the smaller, sooner one. Intuitively, the future losses or effort provision "hurts" less when it is discounted more, potentially making it the more attractive option relative to the smaller, sooner one.

In this paper, we test this prediction, and find that the effect of stress on intertemporal choice is not captured by this model. We report on a laboratory study with 578 participants from the informal settlements in Nairobi who were exposed to either a psychosocial stressor (the Trier Social Stress Test, TSST) or a corresponding control condition; or a pharmacological manipulation of the stress hormone cortisone with a pill containing 20 mg of hydrocortisone, or a placebo pill in a double-blind placebo-controlled study. We first show that both the psychosocial stressor and the administration of hydrocortisone strongly raise levels of the stress hormone cortisol. We then ask participants to make intertemporal choices between smaller, sooner and larger, later monetary gains and losses; and between completing fewer effortful tasks sooner or more effortful tasks later. We find that the stressors increase choices of the smaller, sooner outcome. Importantly, this happens both when the intertemporal choices are about monetary gains, and when they are about monetary losses or effort provision. The effects range between 3% and 9% in magnitude, and are robust to controlling for effects of stress on risk preferences (Koppel et al., 2017; Cahlíková and Cingl, 2017; Kandasamy et al., 2014; Delaney et al., 2014). However, the results are statistically robust only when standard errors are not clustered, and should thus be regarded as tentative.

The direction of the result is the opposite of what is predicted by a model in which stress increases the discount rate or present bias: in this case, delayed monetary losses and delayed effort provision would be perceived as less painful compared to sooner losses or effort, increasing the choice of larger, later losses and effort. Our results therefore suggest that the effect of stress on intertemporal choice may not be well-described by the simple model in which stress increases discounting or present bias. Instead, our findings, although tentative, are consistent with a model in which stress increases the discount rate or present bias in the gains domain, but decreases it in the losses domain; or alternatively, with a model in which stress decreases the utility of any future outcome, for example by subtracting a constant penalty term.

We report several additional results. First, the qualitative pattern of results holds both for the psychosocial stressor and for the pharmacological administration of stress hormones, despite the fact that the stressors differ in their effects on self-reported stress: While both the TSST and administration of hydrocortisone increase salivary cortisol, only the TSST increases self-reported stress. This finding has several implications. Because participants are unable to distinguish whether they received hydrocortisone or a placebo pill (as also shown in Riis-Vestergaard et al. (2018)), the impact of hydrocortisone on intertemporal choice is unlikely to result from an experimenter demand effect in which participants learn about their treatment status and alter their behavior in response. In addition, the similar effects of hydrocortisone and the TSST suggest that the effect of the TSST on intertemporal choice is likely to be mediated by its impact on cortisol levels.

Second, we find no difference in the effect of the stressors on intertemporal choice when they are administered once or repeatedly for seven consecutive days. We expose participants to the same stressor or control condition for seven consecutive days, and measure intertemporal choice on days one and seven. The similar findings on both days suggest that the impact of acutely administered stressors is similar to that of chronically administered stressors, contrasting with a previous study which found that chronic but not acute stress had an effect on risk preferences (Kandasamy et al., 2014).[1]

Finally, the impact of the stressors is similar for monetary losses and real effort, suggesting that intertemporal choice in the money and effort domains responds similarly to stress. This finding supports the similarity between money and effort discounting reported elsewhere; for example, using the same subject pool, Balakrishnan et al. (2020) find that monetary and effort discounting correlate strongly, and Andreoni et al. (2018) show that participants narrowly bracket when discounting money.

This study contributes to a growing literature on the effects of stress on economic choice. In particular, it extends existing work on stress and intertemporal choice, which has largely focused on the effects of acute stress on discounting of monetary gains (Delaney et al., 2014; Riis-Vestergaard et al., 2018; Haushofer et al., 2013; 2018). We extend this literature by also considering losses and effort discounting, comparing the effects of acute and chronic stress, and comparing the effects of hydrocortisone administration and psychosocial stress. Our finding that the effect of stress on discounting of monetary gains may not be well-described by the simple model in which stress increases the discount rate or present bias suggests that caution is warranted when interpreting the results of such studies, as the underlying effect may be more complex than a simple model suggests. More broadly, our study contributes to a larger literature that examines the effects of stress on risk aversion (Koppel et al., 2017; Cahlíková and Cingl, 2017; Kandasamy et al., 2014; Delaney et al., 2014; Porcelli and Delgado, 2009), social preferences (Ceccato et al., 2018; Taylor et al., 2000; Von Dawans et al., 2012; Buchanan and Preston, 2014),

[1] We hasten to add that we cannot hope to mimic the effects of truly chronic stress (over years) with this manipulation; however, it has been shown that the effects of stress differ, both behaviorally and neurobiologically, over short periods such as several hours (Joëls et al., 2011; Henckens et al., 2010; 2011) or approximately a week (Kandasamy et al., 2014).

and competitiveness (Buser et al., 2017; Halko and Sääksvuori, 2017; Buckert et al., 2017; Zhong et al., 2018; Cahlíková et al., 2019; Esopo et al., 2019; Goette et al., 2015).

This study also contributes to a literature that examines the effect of negative shocks, such as financial crises, conflict, and natural disasters, on preferences. Several recent papers have demonstrated increases in discounting or impatience after such naturally occurring shocks (Voors et al., 2012; Cassar et al., 2011; Bchir et al., 2013; Willinger et al., 2013). Together with existing work on stress and time preferences, our findings support the view that these effects may partly be mediated by the stress induced by these shocks.

The remainder of the paper is structured as follows. Section 2 describes the design, and Section 3 presents results. Section 4 concludes.

2. Design

2.1. Sampling strategy

Participants were recruited from the participant pool of the Busara Center for Behavioral Economics in Nairobi, Kenya. To be registered in the participant pool, respondents must be over the age of 18 years old, have access to a mobile phone, and have access to MPesa, a mobile money system used for payment of respondents. Busara's participant pool is broadly representative of Nairobi and Kenya (Haushofer et al., 2014b).[2] The study took place between February and December 2017.

Prior to the main study, we completed a number of pilots to perfect logistics, refine the relatively complicated protocols, and identify potential difficulties in the main study. Due to the resulting changes in protocol for the main study, no treatment effects were analyzed in the pilot studies. In addition, partial data from the 16 participants in the first session of the hydrocortisone study were discarded because the session was interrupted mid-way and could not be completed.

We recruited a total of 1149 participants to participate in four stress induction protocols: the TSST; hydrocortisone administration; the Cold Pressor task (Hines and Brown, 1936); and an economic stressor called the Centipede game (Rosenthal, 1981; Haushofer et al., 2018). In this paper, we focus on the 578 participants who participated in the TSST and hydrocortisone experiments. As we detail in Section 3.2, the Cold Pressor task and Centipede game did not lead to measurable increases in self-reported stress or stress hormones, and therefore we lack a first stage to study their effects on intertemporal choice.[3]

Participants were recruited by first generating call lists of potential participants from the Busara database who were between 18 and 40 years old and had completed at least eight years of schooling ("Standard 8" in Kenya). Potential participants were then called and screened, which further excluded participants who were currently pregnant or breastfeeding. In addition, we excluded participants who were recently ill, took medication, were over 190 pounds, or consumed stimulants for the hydrocortisone study to ensure that participants could safely receive the hydrocortisone.

After confirmation of eligibility, participants were scheduled for sessions. Each participant was told that they were expected to participate in the afternoons on seven consecutive days. To mitigate factors that might affect measurement of salivary cortisol, we asked participants not to smoke or drink alcohol or coffee on the day before the study began and on the days of the study; and not to eat, drink any liquids other than water, or engage in strenuous physical activity, including sexual activity, during the one hour before the study began on each day.[4]

The final sample for the present paper consists of 578 participants, of whom 278 were in the TSST condition (treatment: 135; control: 143), and 300 in the hydrocortisone condition (treatment: 153; control: 147). On day seven, the numbers of participants were somewhat lower due to attrition (TSST treatment: 103 [76% of day one participants]; TSST control: 130 [91%]; hydrocortisone treatment: 126 [82%]; hydrocortisone control: 124 [84%]). We discuss differential attrition in Section 3.1.

2.2. Session structure and treatment assignment

Sessions were conducted at the Busara Center for Behavioral Economics. All sessions took place in the afternoons to ensure low baseline cortisol levels (Haushofer et al., 2013). Sessions lasted on average 5.5 h for the TSST, and 5 h for the hydrocortisone study.

Participants were invited to the lab in groups of 10 for the TSST, and in groups of up to 20 for hydrocortisone administration. Assignment to experimental conditions was done as follows. For the TSST, treatment and control conditions alternated across days, and all participants on a given day were therefore exclusively in the treatment condition, or exclusively in the control condition. For hydrocortisone administration, individual participants were assigned to treatment or control within sessions, so that approximately half of participants in each session were in the treatment and the other half in the control condition.

[2] Data collected in informal settlements in Nairobi suggest that over 90% of residents have access to both a cell phone and M-PESA (Marx et al., 2019).

[3] Information regarding the other stress induction methods is available in the pre-analysis plan posted in AEA RCT Registry (#2741).

[4] Since we expected that participants would not always follow these requirements, we asked participants whether they did any of these activities and control for these reports in our analyses.

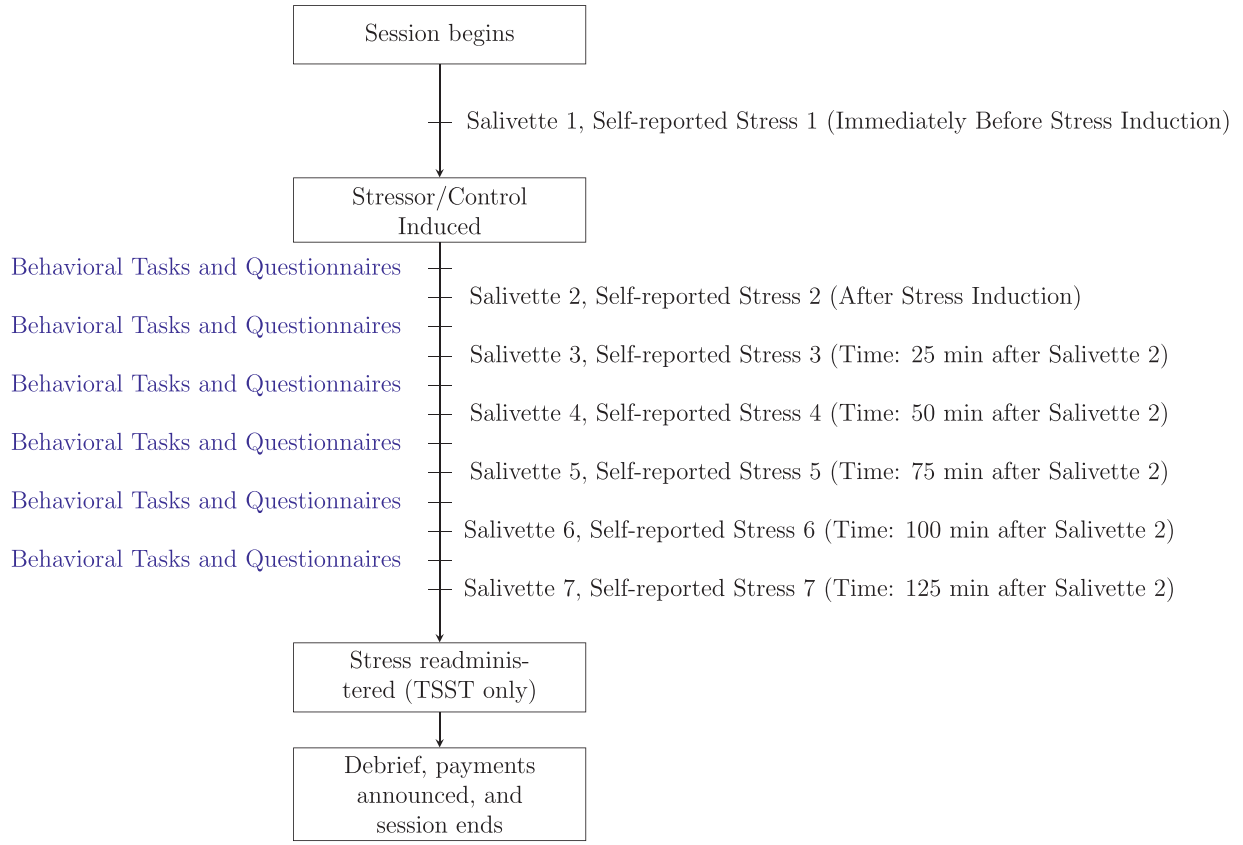

Fig. 1. Timeline for Sessions on Days 1 and 7.

Fig. 1 shows the overall structure of sessions across all stressors on days one and seven. Full session protocols are provided in a separate appendix. Each session began with a short welcome script in the waiting room. Informed consent was then obtained in the first of each set of seven sessions. The study was approved by Princeton University's IRB (0000007367), the Pharmacy & Poisons Board Kenya (PPB/ECCT/15/12/04), and the Kenya Medical Research Institute (#536).

In the hydrocortisone sample, we implemented an additional screening at the beginning of the session with nurses to exclude participants who might be pregnant or breastfeeding, who were taking any medications, or had conditions that could potentially interact with hydrocortisone.[5] In the TSST, we also implemented a brief series of questions to exclude participants who could be pregnant or breastfeeding. At this time, participants in the TSST (but not in the hydrocortisone study) also began wearing heart-rate monitors that they would wear for the duration of the session. Participants were then randomly assigned to specific seats in the lab.

Once in the lab, participants completed a baseline Positive and Negative Affect Schedule (PANAS, Watson et al., 1988) questionnaire on both days one and seven. All tasks and questionnaires were administered using touchscreen computers and the zTree experimental interface (Fischbacher, 2007) to enable computer-illiterate respondents to participate. In the hydrocortisone study, participants' baseline blood pressure and heart rate was then measured to allow the staff to monitor for adverse reactions to the hydrocortisone.

Participants then provided the first of seven saliva samples using a salivette sampling device, described in more detail below. At the same time as providing the saliva sample, participants rated their subjective stress; details are also given below.

Following this, the stressor was administered. For the TSST, the ten participants on each day were divided into two rooms with five participants each for stress induction; for the hydrocortisone sample, participants simply ingested the pill provided at their workstations. This took 60 min on average for the TSST and 1 min on average for the hydrocortisone study. On days one and seven, the stressor was followed by instructions and comprehension questions for the behavioral tasks. Enumerators read instructions and asked comprehension questions aloud to the respondents in Kiswahili to maximize comprehension. Participants answered these questions aloud; if participants did not understand, the enumerators would

[5] The consent form and detailed exclusion criteria are provided in a separate appendix.

re-explain the relevant information. They also completed practice rounds (not for payment) for each task to familiarize themselves with the interface.

The second salivette was taken approximately 25 min after the first one for the hydrocortisone study, after the instructions and comprehension checks for the behavioral tasks; since the TSST protocol took on average 60 min to administer, the second salivette was taken after the end of the TSST protocol, approximately 5 min after the end of the TSST protocol and 65 min after the first salivette. Saliva samples were thereafter collected in approximately 25 min intervals throughout the session, for a total of seven samples per participant.

Then, participants began to perform the behavioral tasks. Each participant completed a series of tasks and questionnaires on days one and seven related to three constructs: intertemporal choice, self-efficacy, and executive control. We first implemented the behavioral tasks, and then the questionnaires. Within each set of tasks and questionnaires, we randomized the order of tasks measuring each of the three constructs at the session level. Within the behavioral tasks related to intertemporal choice, we randomized the order of the monetary discounting tasks, the effort discounting task, and the risk preferences task. This paper focuses on the monetary and effort time preferences tasks, while controlling for responses in the risk preference task. The other tasks relating to self-efficacy and executive control were conducted for psychology studies with different hypotheses, and are therefore not reported here.[6] Salivettes and self-reported stress measurements were taken in approximately 25 min intervals following the second salivette, but so as to not interrupt tasks, which resulted in some variation in timing of measurements.

After the conclusion of the last behavioral task, participants completed a questionnaire which elicited information on their age, gender, marital status, educational attainment, number of siblings, disposable income, consumption in the past week, the frequency in which they usually receive salary, stability of employment, and whether they were currently in debt. In the hydrocortisone study, participants also answered a series of questions to assess whether they could guess their treatment status (hydrocortisone or placebo). In the TSST, participants answered a series of questions about how well they thought they performed relative to the other members of their group. Then, we revealed their final payments for their performance in the tasks; details are given below. In the hydrocortisone study, we again measured blood pressure and heart rate to ensure that there were no adverse reactions to the hydrocortisone. In the TSST study, participants underwent a second round of the stress induction, as describe below, in separate rooms, and then removed their heart-rate monitors. For all sessions, we ended by debriefing with the participants and reminding them of the procedure for the following day, if relevant.

On days two through six, participants completed the stress induction protocols in sessions that lasted between one to one and a half hours. In the hydrocortisone study, participants met at a location in Kibera, an informal settlement from which many of the participants were recruited. In the TSST study, participants met at the Busara Center. On day two, participants first completed a series of questionnaires on tablet computers regarding their exposure to chronic stress in both the TSST and the hydrocortisone studies. We then induced stress, using the relevant protocols. Participants again completed a PANAS questionnaire, a guessing module, and a brief questionnaire about their activities in the past day that could interfere with measurement of salivary cortisol. In addition, in the hydrocortisone study, participants' blood pressure and heart rate were measured at the beginning and end of these sessions to ensure that participants had no negative side-effects to the hydrocortisone.

2.3. Hydrocortisone administration

2.3.1. Description of stress induction

Hydrocortisone is the stable version of cortisol and is metabolized into cortisol upon ingestion. It is a standard approved drug used against rheumatoid and inflammatory diseases. Participants in the hydrocortisone arm of the study received either placebo (control) or 20 mg hydrocortisone (treatment) for up to seven consecutive days. This dose and treatment regimen are mild and therefore side effects were rare. Each dose was administered orally as a single pill, with a glass of water. The pills were custom-made by a compounding pharmacy such that placebo and hydrocortisone pills were indistinguishable. The design was double-blind, i.e. both the enumerators and participants were unaware of their treatment status. At the end of the sessions on days one and seven, we included a guessing module in which participants were asked which pill they thought they received, indicated how confident they were that they received that pill, and answered a series of questions about why they thought they received a particular pill.

2.3.2. Experimenter demand effects: can participants tell if they received hydrocortisone?

Participants in the hydrocortisone treatment were asked to guess whether they received the hydrocortisone pill or a placebo pill at the end of the session on days one and seven. Participants were incentivized with an additional reward of KES 100 if they guessed their treatment status correctly. We also asked participants to indicate the confidence of their guess on a slider from 0 (very unsure) to 100 (very confident). Comparing participants' guesses with their actual treatment status, we observe that guesses are correct 48.3% of the time. Conducting a two-sided t-test, we cannot rule out that participants were guessing at random and getting the right answer 50% of the time (p-value 0.45). The share of participants guessing correctly

[6] Information regarding the measurement of other constructs are available in the pre-analysis plan posted in AEA RCT Registry (#2741).

also does not differ between the first and the last day of the experiment (p-value 0.46). If we weigh respondents' choices by regressing their guess on a constant using their indicated guessing confidence as analytical weights, 49.2% guess correctly, making it even harder to reject random guessing (p-value 0.694). We conclude that participants in the hydrocortisone study were not able to tell if they received the hydrocortisone pill or the placebo (as also shown in Riis-Vestergaard et al. (2018)). Thus, the impacts of hydrocortisone on intertemporal choice are unlikely to result from an experimenter demand effect in which participants learn about their treatment status and alter their behavior in response.

2.4. Trier social stress test

2.4.1. Description of stress induction

The Trier Social Stress Test (TSST) is designed to induce stress using two socially evaluative situations—a speech task and a mental arithmetic task, both performed in groups consisting of five participants. The protocol as detailed below includes minor changes to the standard TSST design (Kirschbaum et al., 1993) to adapt the design to the Kenyan setting, and to develop a "chronic" condition.

In the treatment group, participants completed a speech task and then a mental arithmetic task. The speech task was a simulated job interview that consisted of two parts. Participants were first given five minutes to prepare for a two-minute speech, in which they were supposed to describe why they were a good candidate for a fictitious job, loudly and clearly in English. After this preparation phase, two evaluators entered the room. The second phase consisted of a phase in which participants delivered their prepared speech to evaluators and answered a randomly chosen set of pre-determined questions asked by the evaluators about the fictitious job, e.g. their greatest professional strengths. While participants prepared the speech on each of the seven days and were told that they may have to give the speech on any given day, participants delivered their prepared speech only on days one and seven to the evaluators and the rest of the group. During the presentation, participants were not allowed to use their notes. Each participant was asked on average four to five questions per day. Throughout the session, evaluators maintained neutral to stern facial expressions, and provided little to no verbal and non-verbal feedback. To further increase stress, the evaluators wore white lab coats, and the entire session was recorded by video cameras. The evaluators then exited the room for several minutes to fill out an evaluation for each participant.

Following this, the evaluators returned to implement the mental arithmetic task. In this task, each participant was asked to count backwards in English from a given four-digit number (e.g. 4878, 4494, 3678) in steps of a specific number (e.g. 16). Both the start number and subtraction number varied each day; participants were not informed of these variations in advance. Each participant performed this task for two minutes. If a mistake was made, the participant was asked start again from the beginning. Performance in this task was also scored by the evaluators. Payment for the TSST was based on performance on the speech and arithmetic tasks, and this was known to participants; we discuss payment in Section 2.5.

In the control group, participants also completed a speech task and then a mental arithmetic task, but these were not recorded and not scored. The speech task consisted simply of an introduction, in which participants described themselves, activities they enjoy, and their usual daily routine. Participants were given five minutes to prepare for the two-minute speech. Two Busara staff members then entered the room. As in the treatment condition, participants prepared the speech on each of the seven days and were told that they may have to give the speech on any given day; in practice, participants delivered their prepared speech only on days one and seven. In contrast to the treatment condition, participants could use their notes while giving the prepared speech. This could be completed in whichever language the participants were comfortable speaking, and they did not have to speak clearly or loudly. Then the staff members conducted a question-and-answer phase using pre-determined questions. In contrast to the treatment conditions, these questions were easy and benign, e.g. asking people about what they think everyone should do at least once in their lives. Also in contrast to the treatment group, participants were told that if a question was difficult, they could use the time to talk about anything they like that would help the panel get to know them better. Again, each participant was asked on average four to five questions per day. The two staff members did not wear lab coats, maintained positive expressions and provided friendly non-verbal feedback throughout the session, and did not evaluate participants. The sessions were not recorded. The evaluators then implemented the mental arithmetic task, in which each participant was asked to count forward from a given number (e.g. 0, 40, 25) in steps of five for two minutes. The participants were neither stopped nor corrected for any mistakes. In contrast to the treatment condition, payments for the TSST control condition were based on mere participation in the tasks, rather than on performance. This was known to participants.

The design described above differs from the standard TSST design in several ways. First, participants prepared the speech on each of the seven days of the study, but were asked to deliver it on the first and seventh days. The duration of the speech was decreased from five to two minutes per participant. For the arithmetic task, we randomized the start number for each participant and, in the treatment condition, assigned different subtraction intervals across days to minimize learning effects. The duration of the arithmetic task was decreased from five to two minutes per participant. Finally, participants in both conditions also completed the arithmetic task a second time at the end of the session on days one and seven. We included a second repetition of the arithmetic task to induce anticipatory stress and maximize the effect of the treatment (stressor) condition.

At the end of each session in the treatment condition, we also included a guessing module in which we ask all participants about their rank on each task, overall rank, and their confidence about their rating.

2.4.2. Experimenter demand effects

While we cannot rule out the potential for experimenter demand effects, we minimize this concern by ensuring that all participants in each session experienced the same treatment status using a between-subject design. Therefore, it is unlikely that participants would be aware of other possible conditions of the TSST protocol.

2.5. Incentives

An overview of the payments that participants could receive is shown in Online Appendix Table A.5. In the hydrocortisone study, participants received compensation for attending the study of KES 650 on days one and seven, and KES 350 for each of days two through six; in addition, participants received a KES 50 on-time bonus for each day they arrived on time. Participants could additionally receive a bonus of KES 500 if they attended all seven days. At the time of the study, USD 1 was equivalent to KES 103.41 and KES 41.63 at purchasing power parity, using the World Bank official exchange rate and PPP conversion factor for private consumption for KES/USD in 2017.[7] In addition, participants guessed their treatment status on each day of the study; participants could earn an additional KES 100 for correctly guessing their treatment status on one of the seven days randomly chosen for payment. Finally, respondents had the opportunity to earn additional money from their choices in the behavioral tasks completed on the first and last day. The additional compensation for the behavioral tasks and guessing module was transferred to the respondents via MPesa within a few days of completion of the study, unless otherwise specified.

In the TSST study, participants received compensation of KES 650 on days one and seven for attending the study and KES 350 for days two through six for participation; in addition, participants received a KES 50 on-time bonus for each day they arrived on time. Participants could additionally receive a bonus of KES 300 if they attended all seven days. Participants could receive an additional bonus for performance in the speech and arithmetic components of the TSST, resulting in an expected payout of KES 250. The recipients of this bonus were determined as follows: In the treatment condition, each participant was given a score from 1 to 5 for their performance on each of the speech and arithmetic tasks for each day of participation. For any day a participant did not show up, they received a score of zero for both tasks for that day. Once data collection was complete, enumerators randomly chose one day for payment, and one of the two tasks to be paid out. The two participants with the highest performance scores in each session on the randomly chosen day and task received the bonus; the highest performer on the specific task and day received KES 1500, and the second highest performer received KES 1000. In the control condition, each participant was given a ticket for each task that was completed on each day of participation. For any day a participant did not show up, they did not receive a ticket for either task. Once data collection was complete, enumerators randomly chose one of the days and tasks to be paid out. The two persons in each session whose tickets were picked for the randomly chosen task received the bonus. The first person picked for a specific day and task received KES 1500 and the second person picked received KES 1000. This bonus was explained to all participants at the beginning of each day, and participants were reminded of it throughout the sessions. Finally, respondents had the opportunity to earn additional money from their choices in the behavioral tasks completed on the first and last day. The additional compensation was transferred to the respondents via MPesa within a few days of the study, unless otherwise specified.

An important overall goal of the payment scheme was to equalize the expected payoff across the TSST and hydrocortisone studies. In both studies, participants received KES 650 on days one and seven; KES 350 for days two through six for participation; and a KES 50 on-time bonus for each day they came on time. For complete on-time attendance, these payments sum to KES 3400. In the hydrocortisone study, participants could additionally receive a bonus of KES 500 if they attended all seven days, and KES 100 if they guessed their treatment status correctly on the day that was randomly chosen for payment, which corresponds to an expected payoff of KES 50 if participants guess at chance as expected. The expected total payout for hydrocortisone participants was thus KES 3950, which corresponds to USD 95 PPP. In the TSST study, participants received a bonus of KES 300 if they attended all seven days. In addition, two participants in each session comprised of 10 participants received KES 1500 and KES 1000 for task performance, respectively; i.e., an expected bonus of KES 250 across the 10 participants. Thus, the expected total payoff in the TSST study was also KES 3950.

2.6. Outcomes

We first discuss measures taken to validate whether treatment affected stress relative to the control group. We then discuss the intertemporal choice tasks.

2.6.1. Manipulation check: salivary cortisol and self reported stress

To assess whether our treatments successfully induced stress, we measured salivary cortisol and self-reported stress throughout the experiment. To measure salivary cortisol, we used the Salivette sampling device (Sarstedt, Germany). This method has been used extensively in psychological and medical research (Kirschbaum and Hellhammer, 1989), and more recently in developing countries in our own work and that of others (Fernald and Gunnar, 2009; Haushofer et al., 2018; Haushofer and Shapiro, 2016). The Salivette is a plastic tube containing a cotton swab, on which the respondent chews

[7] The average daily wage in the informal settlement is approximately KES 350 (Haushofer et al., 2014a).

lightly for one minute to fill it with saliva. Due to the non-invasive nature of this technique, we encountered no apprehension among respondents. The saliva samples were labeled with barcodes and stored in a freezer, and were later centrifuged and assayed for salivary free cortisol using a standard radioimmunoassay (RIA) on the cobas e411 platform at Lancet Labs in Nairobi. Lower detection limit for cortisol is 0.5 nmol/L, and the upper detection limit 1750 nmol/L. As described above, the first saliva sample was taken immediately before administration of the pills and the TSST. The second salivette was taken approximately 25 min after the first one for the hydrocortisone study, after the instructions and comprehension checks for the behavioral tasks; since the TSST protocol took on average 60 min to administer, the second salivette was taken after the end of the TSST protocol, approximately 5 min after the end of the TSST protocol and 65 min after the first salivette. Then measurements were taken an additional five times in approximately 25 min intervals. For our analysis, we first winsorize cortisol levels at the top 95% level, and then log-transform them, because cortisol levels in population samples are usually heavily skewed.[8] We report results for each salivette relative to baseline stress (before administration of the stressor on the same day) separately. As a summary measure, we report the average cortisol level of all samples collected after stress induction minus the pre-stressor measurement. We report results relative to baseline measures because we find no significant differences in baseline measures across days one and seven, and because this method minimizes individuals' natural daily variation in salivary cortisol and self-reported stress.[9]

Self-reported stress is measured as the response to the statement "In the present moment, I feel stressed" on a scale of 0 to 100 after each sample of salivary cortisol is taken. Similarly to salivary cortisol, we report the results for each self-report relative to baseline stress (before administration of the stressor on the same day) separately. We also report average stress across the session relative to baseline stress.

2.6.2. Intertemporal choice

We measure intertemporal choice using Multiple Price List (MPL) tasks over three domains: monetary gains, monetary losses, and with a real-effort task.

To measure intertemporal choice in the gains and losses domains, participants were asked to make 48 choices between payments at earlier or later dates. We employed three payment date combinations: today vs. two weeks from today; today vs. four weeks from today; and two weeks from today vs. four weeks from today. In both the gains and the losses domains, participants were endowed with KES 1600 at both earlier and later dates, to equalize reference points across domains and time, and to avoid negative payment outcomes. The order of the three time frames within each task was randomized at the session level. In the gains domain, the payment at the early date was always equal to KES 400, while the option at the later date changed in either ascending or descending order (randomized across individuals within a session) between eight values (KES 340, 400, 440, 600, 700, 800, 1200 and 1600). For example, participants were asked if they preferred a gain of KES 400 today or a gain of KES 800 two weeks from today. The choices in the losses domain were symmetric to the gains domains, with only the signs of payments reversed. For example, participants were asked if they preferred a loss of KES 400 today or a loss of KES 800 two weeks from today. The full set of choices for the monetary gains and losses tasks are presented in Online Appendix Tables A.1 and A.2. If one of the monetary MPL tasks was chosen for payment, participants received an earlier or later payment based on one of the randomly chosen decisions made during the task.

A common criticism of monetary discounting tasks is that money is fungible and therefore these tasks may not capture time preferences over consumption in the presence of functioning credit markets (Augenblick et al., 2015). We therefore also implemented a task of choices over time-dated effort. In a framework similar to the monetary MPL, participants had to choose between an earlier and later level of effort, in the form of a specific number of phone calls to the Busara Center in ten minute intervals at particular hours in the evening. The number of calls on the earlier date was fixed at either two or six, and the number of calls on the later date varied between two and twelve. For example, participants were asked if they preferred having to make two calls today or six calls two weeks from today. For a subset of participants, the number of calls on the earlier date was always two, and the number of calls on the later date varied only from one to six (rather than one to twelve). We increased the number of choices in the effort discounting task midway through the study because we wanted to better match interest rates across the different intertemporal choice tasks. All participants in the hydrocortisone study played the variation in which they chose between two phone calls on the earlier date, or a number between one and six calls at the later date. 20% of participants in the TSST played the first variation, and the remainder played the second variation of the effort discounting task.

Respondents were told they would be paid a fixed amount of KES 500 one month from the date of the session if they successfully completed their effort task. The full set of choices for the effort MPL task is presented in Online Appendix Table A.3.

[8] We deviate from the pre-analysis plan, in which we specified that we would winsorize cortisol at the 95% level, but not that we would log-transform them, because the use of log cortisol is standard in the literature (Haushofer and Shapiro, 2016; Riis-Vestergaard et al., 2018; Haushofer et al., 2013). The results with winsorized cortisol (not log-transformed) are qualitatively similar and are available in Online Appendix A.9.

[9] In doing so, we deviate from the pre-analysis plan, in which we specified that we would use the absolute level of cortisol and self-reported stress, rather than the level relative to baseline measures, and that we would report area under the curve (AUC), which is the area under the curve in a plot of each measurement against time. The results are qualitatively similar and are available in Online Appendix Tables A.10 and A.11. We prefer the results relative to the baseline measures since they minimize individuals' natural daily variation in salivary cortisol and self-reported stress. Also note that AUC is very nearly a linear transformation of the average.

In estimating the effects of stress on intertemporal choice, we control for possible effects on risk preferences.[10] We adapted the theoretical framework and experimental design from Tanaka et al. (2016) to focus on loss aversion and risk aversion. Participants made 27 decisions between two 50/50 lotteries, represented by two balls each in two jars. The first series of ten lotteries included only gains, and participants received no endowment. The first series was followed by the lotteries of only losses, in which the endowment was KES 1000. Finally, the third series had an endowment of KES 350 and included six lotteries with mixed gains and losses. A detailed summary of the choices for the risk preference task is provided in Online Appendix Table A.4. If the task was chosen for payment, one lottery was chosen at random at the session level for payment. Online Appendix B.4 discusses how we use this data to estimate individual parameters of loss and risk aversion using maximum likelihood. These parameters enter our analysis as control variables, as described below.

Our main outcome of interest is a dummy variable indicating for each choice situation of the intertemporal choice tasks whether individuals chose the sooner amount of money or effort provision. Note that we deviate from the pre-analysis plan by using this outcome; we had pre-specified a structural model of time preferences, but our maximum likelihood estimation was highly dependent on starting values, and we therefore have no confidence in the estimated parameters (for details, see Online Appendix B.4). The advantage of using the likelihood of the earlier choice from the tasks is that it allows us to use all individual choices, does not require assumptions regarding (or exclusion of) participants who switch their choices from earlier to later multiple times within a frame, and participants who never switch.

Thus, we have three main outcomes: whether the choice is for the earlier, (typically) smaller gain of money in the gains domain; whether the choice is for the earlier, (typically) smaller losses of money in the losses domain; and whether the choice is for the earlier, (typically) fewer number of calls in the real-effort task.

Participants were provided with task-specific training on the interface and encouraged to ask questions before they began the tasks. To assess whether participants understood the tasks, we test adherence to the law of demand by examining whether participants make consistent choices in each possible pair of choices within a time frame: Specifically, participants are inconsistent with the law of demand if they switch from choosing the larger, later outcome to the smaller, sooner outcome when the amount of the larger, later outcome increases (analogously for losses and effort). Randomly responding would lead to inconsistency on 25% of possible pairs. In the same subject pool, Balakrishnan et al. (2020) found inconsistency levels of 7%. In this study, we find that only 5.2% of choice pairs within a frame violate the law of demand. Thus, we believe that our participants understood the task well.

3. Results

Our analysis largely follows our pre-analysis plan (AEA RCT Registry #2741, https://www.socialscienceregistry.org/trials/2741); as before, we note when and why we deviate from the pre-analysis plan.

3.1. Baseline balance and attrition

We first examine whether treatment and control participants are similar in baseline characteristics in Table 1. We use an OLS regressions of the following form:

$$y_{it} = \beta_0 + \beta_1 T_i \cdot Day1_t + \beta_2 Day7_t + \beta_3 T_i \cdot Day7 + \varepsilon_{it} \quad (1)$$

Here, y_{it} is the outcome of interest for respondent i on day t. T_i is a treatment indicator that takes value of 1 for respondents that received treatment (stress induction) and 0 for respondents that received the control protocol. $Day7_t$ indicates whether the observation is from day 7 of the study; similarly, $Day1_t$ indicates whether the observation is from day 1 of the study. Thus, β_1 estimates the treatment effect of stress on day 1 (compared to the control group on day 1), β_2 measures the day 7 fixed effect, and β_3 estimates the treatment effect of stress on day 7 (compared to control group on day 7). We report β_1 ("Treatment effect Day 1") and β_3 ("Treatment effect Day 7") in the table since these are our coefficients of interest. We also test whether the effect of treatment is different on days one and seven, i.e. whether $\beta_1 = \beta_3$, and report the corresponding p-value. Standard errors are clustered at the level of randomization: at the individual-day level for the hydrocortisone study, and at the session level for the TSST.[11] We implement the main specification separately for each stressor.

Table 1 examines whether treatment and control groups are similar in gender, age, marital status, number of siblings, weekly spending (USD PPP), disposable monthly income (USD PPP), body mass index, number of dependents, whether the respondent is financially dependent on someone else, whether the respondent does any work for which they are paid money, weekly income (USD PPP), whether in debt, and level of education. The coefficients in the row on "Treatment effect Day 1" are largely statistically insignificant, with the exception that treated participants on Day 1 are less likely to be in debt, less likely to be employed, and have a higher number of dependents in the hydrocortisone subsample and that treated

[10] Time preferences are usually studied through choices between smaller, sooner and larger, later payments. This task only identifies time preference parameters when the utility function is linear (Andersen et al., 2008). It is therefore important to account of possible non-linearity of the utility function, which can be done by measuring risk preferences.

[11] We cluster standard errors at the level of randomization by creating and clustering on a variable that takes distinct values for each individual-day in the Hydrocortisone study, and each session-day in the TSST.

Table 1
Baseline Balance.

	(1) Female	(2) Age	(3) Married	(4) Number of Siblings	(5) Weekly spending (USD PPP)	(6) Disposable monthly income (USD PPP)	(7) Body mass index	(8) Number of dependents	(9) Financially depends on someone else	(10) Work for money	(11) Income (USD PPP)	(12) In debt	(13) Level of Education	(14) Joint p-value
Hydrocortisone														
Treatment effect Day 1	−0.005	0.144	0.101	−0.017	0.794	6.956	-0.485	0.415	0.016	-0.107	2.448	-0.107	0.104	0.61
	(0.056)	(0.531)	(0.110)	(0.251)	(2.368)	(5.378)	(0.508)	(0.236)*	(0.057)	(0.058)*	(12.003)	(0.043)**	(0.067)	
Treatment effect Day 7	0.021	0.384	0.072	−0.025	7.646	−1.780	-0.151	0.315	-0.049	-0.104	6.417	-0.020	0.114	0.80
	(0.060)	(0.562)	(0.119)	(0.263)	(4.643)	(6.044)	(0.623)	(0.268)	(0.063)	(0.063)*	(13.467)	(0.053)	(0.071)	
Difference *p*-value	0.752	0.757	0.860	0.982	0.189	0.281	0.678	0.779	0.439	0.969	0.826	0.203	0.919	
Control mean Day 1	0.619	27.320	1.932	4.422	20.761	34.734	22.092	2.578	1.585	1.571	90.009	1.224	1.340	
	(0.487)	(4.483)	(0.948)	(2.132)	(22.322)	(44.991)	(4.288)	(1.923)	(0.494)	(0.497)	(106.937)	(0.419)	(0.555)	
Control mean Day 7	0.653	27.323	1.960	4.573	18.173	41.352	22.408	2.645	1.605	1.556	95.969	1.234	1.298	
	(0.478)	(4.335)	(0.923)	(1.984)	(15.356)	(54.897)	(4.833)	(1.976)	(0.491)	(0.499)	(119.210)	(0.425)	(0.525)	
Participant-Day Observations	550	550	550	550	550	550	550	550	550	550	550	550	550	
Participants (Day 1)	300	300	300	300	300	300	300	300	300	300	300	300	300	
Trier Social Stress Test														
Treatment effect Day 1	0.055	0.049	−0.006	0.247	−6.537	13.818	-0.338	-0.122	-0.035	0.040	0.009	-0.033	-0.346	0.04**
	(0.045)	(1.460)	(0.140)	(0.416)	(2.660)**	(14.886)	(0.592)	(0.429)	(0.054)	(0.044)	(24.285)	(0.052)	(0.148)**	
Treatment effect Day 7	0.038	−0.230	−0.009	0.291	−1.332	3.846	0.474	-0.146	-0.046	0.022	-8.605	-0.015	-0.197	0.70
	(0.062)	(1.577)	(0.162)	(0.380)	(2.625)	(10.331)	(0.880)	(0.466)	(0.051)	(0.067)	(25.326)	(0.061)	(0.141)	
Difference *p*-value	0.830	0.897	0.987	0.938	0.169	0.584	0.447	0.970	0.878	0.822	0.807	0.825	0.469	
Control mean Day 1	0.552	28.951	2.028	3.790	27.618	64.993	24.865	3.315	1.664	1.420	137.782	1.196	1.916	
	(0.499)	(6.421)	(0.956)	(2.261)	(32.065)	(72.651)	(4.817)	(2.485)	(0.474)	(0.495)	(165.643)	(0.398)	(0.736)	
Control mean Day 7	0.554	29.123	2.038	3.923	24.150	56.616	24.440	3.185	1.638	1.415	154.185	1.238	1.915	
	(0.499)	(6.409)	(0.968)	(2.236)	(20.155)	(58.612)	(4.407)	(2.237)	(0.482)	(0.495)	(179.506)	(0.428)	(0.726)	
Participant-Day Observations	511	511	511	511	511	511	511	511	511	511	511	511	511	
Participants (Day 1)	278	278	278	278	278	278	278	278	278	278	278	278	278	

Notes: Balance on demographic variables by treatment group. The dependent variables are listed in the top row. The "Treatment effect Day 1" and "Treatment effect Day 7" rows show the difference in the dependent variable between treatment and control groups on the respective day, and its standard error in parentheses, estimated using OLS. Standard errors are clustered at the session level for the TSST study, and at the individual level for the hydrocortisone study. "Female" indicates whether the respondent is female. "Age" is the respondent's age in years. "Married" indicates whether the respondent is married rather than single or living with partner. "Number of siblings" refers to the respondent's number of siblings (between 0 and 20). "Weekly spending" refers to the amount of money the respondent reports typically spending in a week in USD PPP. "Disposable monthly income" refers to the income the respondent reports having after rent, taxes, bills, etc. in USD PPP. "Body mass index" is equal to weight in kg divided by the square of height in m. "Number of dependents" is the number of people that depend entirely on the respondent's income. "Financially depends on someone else" indicates whether the respondent is entirely supported by someone else financially, without earning their own money. "Work for money" indicates whether the respondent reports doing any work for which they are paid money. "Income" refers to money earned per week in USD PPP. "In debt" indicates whether the respondent reports that they are currently in debt. "Level of education" refers to the highest level of education the respondent completed, in years. This analysis drops cohorts one and three of the hydrocortisone study; we did not collect demographic data for day one participants of cohort one and due to an issue in the lab, we cancelled day seven of the study for cohort three. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

Table 2
Differential attrition.

	(1) *WillAttritHydrocortisone*	(2) *WillAttritTSST*
By Treatment Status		
Treatment	0.042	0.133
	(0.044)	(0.055)**
By Demographics		
Female	−0.191	−0.018
	(0.058)***	(0.059)
Age	0.010	−0.007
	(0.006)	(0.004)
Married	−0.064	0.013
	(0.027)**	(0.031)
Number of siblings	−0.023	0.006
	(0.011)**	(0.009)
Weekly spending (USD PPP)	0.002	−0.001
	(0.001)*	(0.001)
Disposable income per month (USD PPP)	−0.000	0.001
	(0.001)	(0.000)***
Body mass index	−0.000	−0.003
	(0.004)	(0.008)
Number of dependents	0.011	−0.001
	(0.012)	(0.010)
Financially depends on someone else	−0.102	0.006
	(0.049)**	(0.057)
Any work for money	−0.012	0.005
	(0.054)	(0.044)
Weekly income (USD PPP)	0.001	−0.000
	(0.000)	(0.000)
Is in debt	0.038	0.075
	(0.059)	(0.077)
Level of Education	−0.118	−0.071
	(0.046)**	(0.039)*

Notes: Attrition by study from Day 1 to Day 7. The dependent variable is an indicator for whether a participant in the hydrocortisone study (Column 1) or TSST study (column 2) leaves the study between Day 1 and Day 7. The top panel ("By Treatment Status") is an OLS regression of attrition on a treatment dummy. The bottom panel ("By Demographics") is a separate OLS regression of attrition on a vector of demographic controls. In both panels, standard errors are clustered at the session level for the TSST study and at the individual level for the hydrocortisone study, reflecting the respective levels of randomization. "Treatment" refers to treatment status (stressor or control) within the study. "Female" indicates whether the respondent is female. "Age" is the respondent's age in years. "Married" indicates whether the respondent is married rather than single or living with partner. "Number of siblings" refers to the respondent's number of siblings (between 0 and 20). "Weekly spending" refers to the amount of money the respondent reports typically spending in a week in USD PPP. "Disposable monthly income" refers to the income the respondent reports having after rent, taxes, bills, etc. in USD PPP. "Body mass index" is equal to weight in kg divided by the square of height in m. "Number of dependents" is the number of people that depend entirely on the respondent's income. "Financially depends on someone else" indicates whether the respondent is entirely supported by someone else financially, without earning their own money. "Work for money" indicates whether the respondent reports doing any work for which they are paid money. "Income" refers to money earned per week in USD PPP. "In debt" indicates whether the respondent reports that they are currently in debt. "Level of education" refers to the highest level of education the respondent completed, in years. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

participants in the TSST study are less educated and have lower weekly spending than control participants. The joint test in Column 14 indicates that treatment and control groups are similar in observable characteristics at baseline ("Treatment effect Day 1") for the hydrocortisone subsample ($p = .61$), but different in the TSST subsample ($p = .04$).

Next, we examine whether there is differential attrition in Table 2. The top panel regresses attrition on treatment status. We find evidence that treated participants in the TSST are significantly more likely to attrit. The bottom panel examines whether demographic characteristics, measured on day 1, predict attrition on day 7. We find evidence that gender, marital status, number of siblings, weekly spending, whether the respondent financially depends on someone else, and the respondent's level of education significantly predict attrition in the hydrocortisone subsample, and that monthly disposable income and level of education predicts attrition in the TSST subsample. Given that there are some concerns with attrition, we present the results separately for days 1 and 7.

Given the concerns with attrition and baseline balance, we show both specifications in which we control for the observable characteristics that predict attrition and are different at baseline across treatment and control groups, and basic specifications in which these controls are omitted. Since the results from day 1 are not affected by attrition, we place greater emphasis on these results.

3.2. Manipulation check

In this section we examine whether each stressor increased respondents' levels of self-reported stress and salivary cortisol. Fig. 2 plots estimated treatment effects, i.e. the coefficients in a regression of these outcomes on treatment (stress),

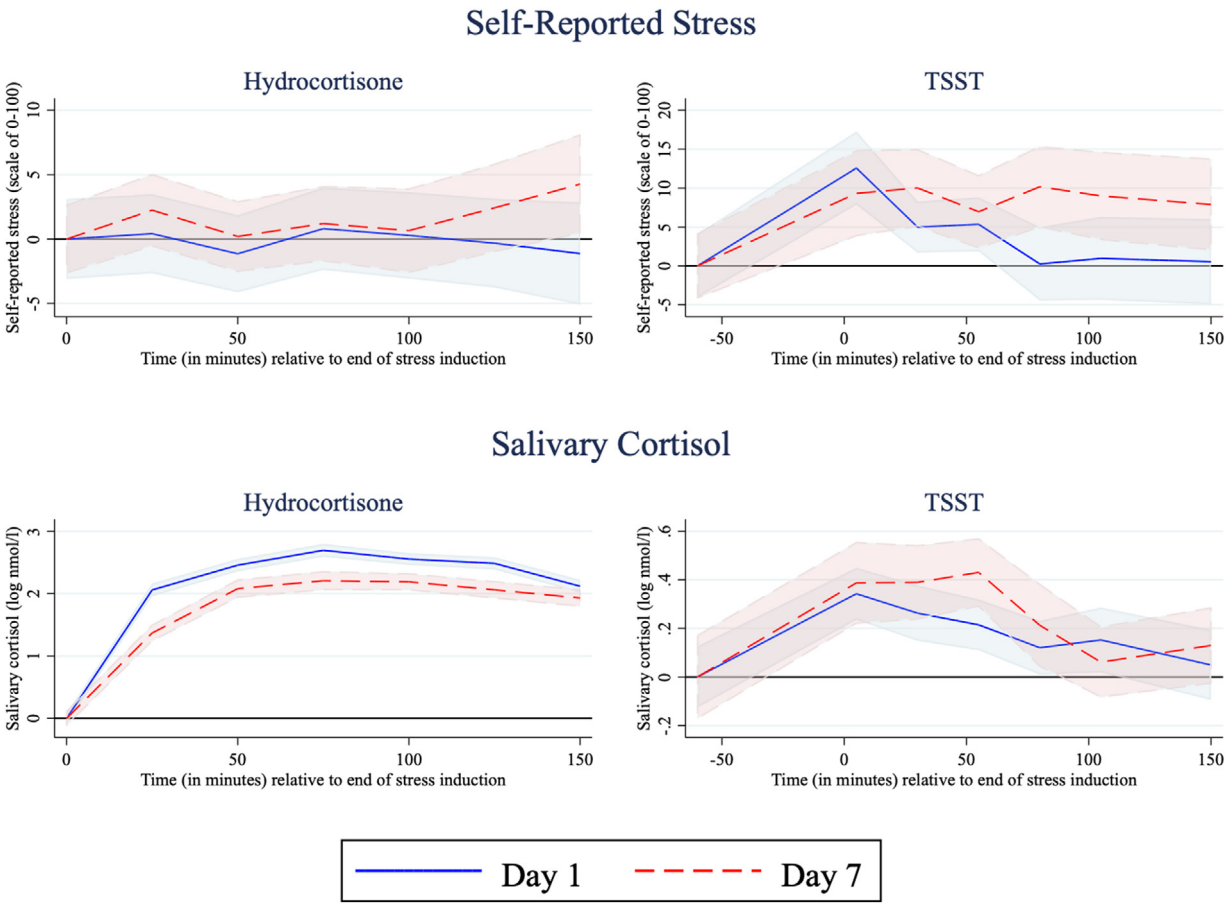

Fig. 2. Effect of Stressors on Self-Reported Stress and Salivary Cortisol: Treatment Effects. *Notes:* The lines represent estimated OLS coefficients of self-reported stress (units of 0–100, top row) and salivary cortisol (log nmol/l, bottom row) on treatment assignment for each measurement timepoint, normalized by subtracting pre-stressor baseline levels (before initiation of the stressor), separately for hydrocortisone (left column) and TSST (right column) and for Day 1 (solid line) and Day 7 (dashed line). Bands indicate 1 standard error.

adjusted for baseline (pre-stressor) measures on each day. Table 3 shows estimation results for self-reported stress, and Table 4 for salivary cortisol, similarly adjusting for baseline values, using the specification presented in Eq. 1.

We find that hydrocortisone administration significantly increases salivary cortisol, but not self-reported stress. Specifically, Table 3 shows that hydrocortisone administration does not have significant effects on self-reported stress, both overall (Column 8), and immediately after hydrocortisone administration (Columns 2–4). In addition, the treatment effects are quantitatively small, with a 0.238 decrease in self-reported stress on day 1 and a 1.675 increase in self-reported stress on day 7 (Column 8); compared to the control day 1 pre-stressor mean of 20.830 (see Column 1 of Online Appendix Table A.10), this corresponds to a -1.1% effect on day 1 and a $+8.0\%$ effect on day 7. In contrast, hydrocortisone administration has large and statistically significant effect (at the 1% level) on salivary cortisol (Column 8 of Table 4) on both days 1 and day 7. The treatment effects are large, corresponding to a 2.38 log point (238%) increase in salivary cortisol on day 1, and a 1.98 log point (198%) increase in salivary cortisol on day 7 (Column 8). As can be seen in Figure 2 and Column 3 of Table 4, salivary cortisol peaks in the fourth measurement, which is approximately 75 min after administration of hydrocortisone. We also find evidence that the overall effect of hydrocortisone administration on salivary cortisol on day 7 is smaller in magnitude than on day 1 ($p = .025$, Table 4, Column 8). Thus, the effect of hydrocortisone administration on salivary cortisol is larger in the acute condition than in the chronic condition, consistent with Kandasamy et al. (2014). The fact that hydrocortisone administration increases salivary cortisol but not self-reported stress is also consistent with previous work (Riis-Vestergaard et al., 2018).

The TSST has statistically significant effects on both salivary cortisol and self-reported stress. As can be seen in Fig. 2, the effects are largest for both self-reported stress and salivary cortisol approximately 5 (corresponding to the second measurement) to 55 min (corresponding to the fourth measurement) after the end of administration of the TSST protocols on both days 1 and 7, and then declines over the course of the sessions. In Table 3 Column 8, we show that the TSST has a significant effect on self-reported stress immediately after stressor administration on both day 1 and day 7. Specifically, we find 4.257 increase in self-reported stress on day 1, and a 8.903 increase in self-reported stress on day 7 (Column 8); compared to

Table 3
Effects of Stressors on Self-Reported Stress.

	(1) Self-report 1 (pre-stressor)	(2) Self-report 2	(3) Self-report 3	(4) Self-report 4	(5) Self-report 5	(6) Self-report 6	(7) Self-report 7	(8) Average Self-report 2 to 7 minus Self-report 1 (pre-stressor)
Hydrocortisone								
Treatment effect Day 1	0.000	−0.466	−1.458	0.965	−0.226	0.098	−1.428	-0.238
	(0.000)	(2.778)	(3.249)	(3.531)	(3.618)	(3.867)	(4.251)	(3.091)
Treatment effect Day 7	0.000	2.245	0.200	1.204	0.658	2.426	3.378	1.675
	(0.000)	(2.404)	(2.605)	(2.869)	(3.423)	(3.611)	(4.357)	(2.752)
Difference *p*-value		0.461	0.691	0.958	0.859	0.660	0.430	0.644
Control mean Day 1	0.000	−1.776	−1.156	0.048	2.776	3.510	5.869	1.401
	(0.000)	(25.699)	(27.185)	(29.860)	(31.570)	(33.894)	(35.723)	(27.343)
Control mean Day 7	0.000	0.565	2.895	2.677	6.032	6.137	6.088	4.070
	(0.000)	(21.157)	(24.758)	(28.031)	(30.807)	(31.499)	(36.185)	(25.451)
Participant-Day Observations	550	550	550	550	550	550	512	550
Participants (Day 1)	300	300	300	300	300	300	280	300
Trier Social Stress Test								
Treatment effect Day 1	0.000	12.577	4.994	5.351	0.252	0.970	0.099	4.257
	(0.000)	(5.373)**	(4.796)	(4.535)	(5.856)	(5.818)	(6.434)	(4.759)
Treatment effect Day 7	0.000	9.331	10.024	6.971	10.184	8.997	7.909	8.903
	(0.000)	(5.045)*	(4.851)**	(4.323)	(4.629)**	(4.721)*	(5.552)	(4.278)**
Difference *p*-value		0.661	0.464	0.797	0.189	0.289	0.362	0.471
Control mean Day 1	0.000	0.734	2.517	1.545	5.371	6.874	10.805	4.425
	(0.000)	(34.160)	(30.167)	(31.225)	(38.201)	(39.880)	(43.232)	(29.868)
Control mean Day 7	0.000	3.892	3.869	4.185	3.923	4.877	8.654	4.900
	(0.000)	(31.548)	(32.471)	(30.774)	(32.503)	(32.481)	(36.288)	(29.303)
Participant-Day Observations	511	511	511	511	511	511	501	511
Participants (Day 1)	278	278	278	278	278	278	268	278

Notes: Effects of stressors on self-reported stress. The dependent variables, listed in the top row, are the seven self-reports of stress obtained during the session, and their average. Self-reported stress is measured by the response to the statement "In the present moment, I feel stressed" on a scale of 0 to 100. All measures of self-reported stress are normalized by subtracting their baseline levels (before initiation of the stressor, Self-report 1) on the same day. The "Treatment effect Day 1" and "Treatment effect Day 7" rows show the difference in the dependent variable between treatment and control groups on the respective day, and its standard error in parentheses, estimated using OLS. Standard errors are clustered at the session level for the TSST study, and at the individual level for the hydrocortisone study. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

Table 4
Effects of Stressors on (Log) Salivary Cortisol.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log Cortisol 1	Log Cortisol 2	Log Cortisol 3	Log Cortisol 4	Log Cortisol 5	Log Cortisol 6	Log Cortisol 7	Average Log Cortisol 2 to 7 minus Log Cortisol 1 (pre-stressor)
Hydrocortisone								
Treatment effect Day 1	0.000	2.044	2.401	2.708	2.546	2.462	2.101	2.382
	(0.000)	(0.119)***	(0.123)***	(0.124)***	(0.116)***	(0.122)***	(0.122)***	(0.102)***
Treatment effect Day 7	0.000	1.392	2.096	2.242	2.215	2.058	1.920	1.979
	(0.000)	(0.151)***	(0.174)***	(0.182)***	(0.168)***	(0.165)***	(0.169)***	(0.147)***
Difference *p*-value		0.001***	0.154	0.035**	0.105	0.050**	0.387	0.025**
Control mean Day 1	0.000	−0.163	−0.120	−0.350	−0.344	−0.440	−0.429	-0.310
	(0.000)	(0.760)	(0.791)	(0.813)	(0.730)	(0.845)	(0.843)	(0.618)
Control mean Day 7	0.000	−0.072	−0.229	−0.317	−0.303	−0.352	−0.427	-0.286
	(0.000)	(0.810)	(0.899)	(0.914)	(0.871)	(0.854)	(0.819)	(0.674)
Participant-Day Observations	546	534	540	537	544	543	537	546
Participants (Day 1)	300	295	299	298	299	299	298	300
Trier Social Stress Test								
Treatment effect Day 1	0.000	0.343	0.263	0.214	0.121	0.160	0.057	0.190
	(0.000)	(0.104)***	(0.060)***	(0.104)**	(0.124)	(0.090)*	(0.108)	(0.071)***
Treatment effect Day 7	0.000	0.387	0.394	0.430	0.213	0.061	0.130	0.268
	(0.000)	(0.120)***	(0.157)**	(0.128)***	(0.129)	(0.101)	(0.134)	(0.099)***
Difference *p*-value		0.779	0.439	0.195	0.611	0.468	0.673	0.527
Control mean Day 1	0.000	0.103	0.029	−0.106	−0.250	−0.277	−0.211	-0.116
	(0.000)	(0.893)	(0.819)	(0.936)	(0.933)	(0.822)	(0.914)	(0.676)
Control mean Day 7	0.000	−0.154	−0.278	−0.347	−0.337	−0.297	−0.422	-0.305
	(0.000)	(0.930)	(0.839)	(0.820)	(0.943)	(1.013)	(0.847)	(0.696)
Participant-Day Observations	498	498	496	496	496	495	495	498
Participants (Day 1)	269	269	268	268	267	266	266	269

Notes: Effects of stressors on salivary cortisol. The dependent variables, listed in the top row, are the seven measures of salivary cortisol obtained during the session, in log nmol/l, and their average. All cortisol measures are normalized by subtracting their baseline levels (before initiation of the stressor, Log Cortisol 1) on the same day. The "Treatment effect Day 1" and "Treatment effect Day 7" rows show the difference in the dependent variable between treatment and control groups on the respective day, and its standard error in parentheses, estimated using OLS. Standard errors are clustered at the session level for the TSST study, and at the individual level for the hydrocortisone study. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

the control day 1 pre-stressor mean of 28.063 (see Column 1 of Online Appendix Table A.10), this corresponds to a +15.2% effect on day 1 and a +31.7% effect on day 7. The elevated stress levels persist somewhat longer on day 7, with the result that the average increase across all timepoints is significantly different from baseline on day 7 but not day 1 (Column 8). Table 4 shows that TSST also significantly affects salivary cortisol immediately following administration of treatment on both days 1 and 7 (Columns 2–4), and on average throughout the session (Column 8). The treatment effects amount to a 0.19 log points (19%) increase in salivary cortisol on day 1 and a 0.27 log points (27%) increase in salivary cortisol on day 7 (Column 8). As with self-reported stress, the effects on salivary cortisol are largest immediately after stress induction (measures 2–4). We do not find evidence that the average effect of the TSST on salivary cortisol is significantly different in the acute and chronic condition ($p = .527$, Column 8).

Online Appendix Tables A.10 and A.11 show that the results are similar when we do not adjust for baseline levels, but rather use the absolute levels of self-reported stress and log cortisol in the analysis. The effects of both stressors on salivary cortisol are also similar if we control for factors that are known to affect salivary cortisol, specifically whether the respondent reported smoking that day, drinking alcohol, tea or coffee that day, ate or drank anything beside water in the 2 h before the session, and performed intense exercise in the 2 h before the session. The results are shown in Online Appendix Table A.12.[12]

To benchmark these effect sizes, we next compare the effect sizes found here to those of interventions outside the laboratory on salivary cortisol and perceived stress. Haushofer and Shapiro (2016) find that USD 1500 unconditional cash transfers decrease salivary cortisol by 0.16 log points. Similarly, Haushofer et al. (2020) find that health insurance reduces salivary cortisol by 0.14 log points.[13] These effect sizes are in line with the estimated effects of the TSST on salivary cortisol; if anything, the effects of the TSST are slightly larger in magnitude since the TSST produces an increase in salivary cortisol by 0.19 log points on day 1 and 0.268 log points on day 7. Unsurprisingly, the effects of hydrocortisone on salivary cortisol are far larger than the naturally occurring effects found in Haushofer and Shapiro (2016) and Haushofer et al. (2020). With regard to self-reported stress, Haushofer et al. (2020) and Haushofer and Shapiro (2016) measure self-reported stress using Cohen's Perceived Stress Scale (Cohen et al., 1983), which differs from the measure used in this paper. Haushofer et al. (2020) finds that health insurance decreases self-reported stress by 0.29 standard deviations; Haushofer and Shapiro (2016) find that unconditional cash transfers decrease self-reported stress by 0.26 standard deviations. Taking the estimated effects of stressors on self-reported stress from Column 8 of Table 3 and converting to standard deviation units (control day 1 standard deviation of 26.517 for hydrocortisone and 30.210 for TSST; c.f. Online Appendix Table A.10 Column 1), we find that the TSST increases self-reported stress by 0.14 standard deviations on day 1 and 0.30 standard deviations on day 7. Thus, the effect of the TSST on self-reported stress, on day 7 especially, is also similar in magnitude to the effects of real-world interventions.

To summarize, our results indicate that the stressors had different effects on self-reported stress and on salivary cortisol: hydrocortisone had large and significant effects on salivary cortisol, but not on self-reported stress. The TSST had effects on both self-reported stress and salivary cortisol. The magnitude of the TSST on both self-reported stress and salivary cortisol are similar in magnitude to the effects of large-scale interventions outside of the laboratory, specifically the provision of health insurance and large unconditional cash transfers.

The effects of the other stressors, i.e. the cold-pressor task and the Centipede game, on self-reported stress and salivary cortisol are shown in Online Appendix Tables A.6 and A.7. We find that neither the physical stressor, the cold-pressor task, nor the economic stressor, the Centipede game, have significant effects on self-reported stress and salivary cortisol (cf. Column 8). Therefore, we do not discuss them further in this paper.

3.3. Intertemporal choice

In this section we explore the effects of treatment on intertemporal choice. We had pre-specified a parametric analysis in the pre-analysis plan, based on the quasi-hyperbolic model by Laibson (1997). However, as pointed out above and detailed in Online Appendix B.4, our maximum likelihood estimation was highly dependent on starting values, and we therefore have no confidence in the estimated parameters. We therefore rely on individual choice data for our main estimation instead: our main outcome is a dummy variable indicating for each individual decision situation whether the participant chose the sooner option over the later option. We use the following Probit model to examine whether treatment increases the likelihood that individuals choose the sooner outcome:

$$\Pr(y_{itj}) = \Phi(\beta_0 + \beta_1 T_i + \beta_3 Set_j + \beta_4 Order_j + \varepsilon_{itj}) \tag{2}$$

Here, y_{itj} indicates whether respondent i on day t chooses the earlier choice for decision j. T_i is defined as before. Set_j and $Order_j$ are fixed-effects for payment-date combinations (today vs. two weeks from today, today vs. four weeks from today, and two weeks from today vs. four weeks from today) and question order of the intertemporal choices. Thus, β_1 ("Treatment

[12] We also collected heart rate data throughout the session only for the TSST subsample. As shown in Online Appendix A.8, we find that stress in the TSST study significantly decreases the average heart rate (relative to baseline stress) on day 1 but does not significantly affect heart rate variability.

[13] These studies were both randomized controlled trials, so these effects can be interpreted as causal. Like this study, they also took place in Kenya; Haushofer and Shapiro (2016) study a rural sample, while the sample in Haushofer et al. (2020) is urban and very similar to the one studied here. The interventions in both studies ended several months before measurement took place, and the effects are therefore not immediate reactions to receiving the interventions (although they are still short-run).

effect") is our coefficient of interest; it estimates the treatment effect of stress compared to the control group. We report marginal effects for ease of interpretation.

We implement the main specification separately for each day and each stressor. In addition, because we do not find statistically significant differences in self-reported stress and salivary cortisol between day 1 and day 7 treatment effects, we also implement a specification in which we pool observations across days and include day fixed effects. Note that we do not report the difference between treatment effects on days 1 and 7 because Probit models do not deal well with interaction terms (Ai and Norton, 2003). We no longer cluster our standard errors because doing so would violate the identifying assumption in Probit regressions that the standard errors are iid-normal. As a robustness check, we also implement OLS regressions in which standard errors are clustered at the level of randomization: at the individual-day level for the hydrocortisone study, and at the session level for the TSST study.

We test the robustness of the results with several checks. First, to account for possible non-linearities of individuals' utility function, we add controls for loss aversion and curvature of the utility function obtained from the risk preference task. Online Appendix B.4 discusses how we estimate individual parameters of loss and risk aversion using data from the risk preferences task with maximum likelihood. In addition, we control for baseline characteristics that predict attrition, as well as baseline characteristics that are imbalanced. Second, to confirm that the results are not dependent on the payment-date combination, we repeat these regressions separately by frame (i.e. today vs. two weeks from today; today vs. four weeks from today; and two weeks from today vs. four weeks from today). Third, we ensure that the results are not driven by individuals who switch from earlier to later choices (or vice versa) multiple times within a frame by repeating the main analysis while excluding these individuals.

Our main results are shown in Table 5. Recall that in the gains domain, an increase in the likelihood that individuals choose the sooner outcome implies that they choose a smaller, sooner monetary gain over a larger, later monetary gain. In the losses domain, choosing the sooner outcome means choosing a smaller but sooner monetary loss over a larger, later monetary loss (coming out of the endowment). In the effort domain, choosing the sooner outcome means choosing a smaller, sooner number of phone calls to be completed over a larger, later number of calls, for a fixed amount of money.

In the first row of Table 5, we pool the all the data from the study—both stressors, and days 1 and 7—to provide the most highly-powered test of the question whether the stressors affect intertemporal choice. We find that the treatments increase the likelihood that individuals choose the sooner, smaller amount of money in the monetary gains domain by 4.0 percentage points; the effect is significant at the 1% level and corresponds to a roughly 9% increase compared to the control group mean in both the acute and chronic conditions. Similarly, treatment increases earlier choices in the monetary losses domain by 2.5 percentage points (4%) on day 1, and by 1.7 percentage points (3%) in the effort domain. Because both the losses and effort domains involve a smaller sooner cost compared to a larger later cost, we average across monetary and effort choices in Column 4, finding an average increase of 2.0 percentage points. All effects are statistically significant at the 1% level.

The next two rows of the top panel of Table 5 show that the results are qualitatively similar for days 1 and 7. Note in this context that we place greater emphasis on the day 1 results because they do not suffer from differential attrition in the TSST subsample. The treatment effect coefficient for monetary gains on day 1 is very similar to the pooled specification. The coefficient on monetary losses is positive, but does not reach statistical significance. However, the coefficient for effort is positive and statistically significant, and the averaged losses/effort coefficient in Column 4 is positive and statistically significant. The treatment effect results for day 7 are broadly similar. Thus, when pooling across both stressors, we find robust evidence that the treatments increased choices of the sooner option, in both the monetary gains, monetary losses, and effort domains.

In the next two panels of Table 5, we examine the results separately by stressors (TSST and hydrocortisone). We observe a similar pattern for the hydrocortisone subsample as in the first panel, with the exception that the treatment coefficient in the monetary losses domain (Column 2, Hydrocortisone) is smaller in magnitude and not statistically significant compared to the specification in the first panel. Similarly, the results for the TSST are broadly similar to the first panel, with the exception that the coefficient in the pooled specification is smaller in magnitude and not statistically significant in the effort domain (Column 3, TSST). On the whole, the main results are remarkably consistent across days and induction methods. Thus, across all three domains in which we measure intertemporal choice (monetary gains, monetary losses, and effort), we find consistent evidence that individuals are more likely to choose the sooner choice as a result of stress.

We repeat these estimations using an OLS linear probability model instead of Probit in Table 6. There is a tradeoff between using OLS and Probit: recall that in Probit, we cannot cluster standard errors, but it has the advantage that the outcome is restricted to the unit interval. The opposite is true for OLS: we can cluster standard errors, but the model is not constrained to the unit interval and therefore less appropriate for binary outcomes. Neither model is thus perfectly adequate for our purposes, and we regard them as equally informative.

In the OLS analysis, we cluster standard errors at the level of randomization: at the session level for the TSST study, and at the individual level for the hydrocortisone study. Table 6 reports the results of the OLS version of our main specification, clustering the standard errors at the level of randomization. Unsurprisingly, the standard errors are larger in magnitude as a result. Consistent with the Probit results, we find that treatment increases the likelihood of the early choice across domains. The effects are similar in magnitude, but due the larger standard errors, are significant only at the 10% level in the monetary gains domains. We confirm that the difference in precision relative to the Probit model is due to clustering by re-estimating

Table 5
Effect of Stressors on Likelihood of Early Choice (Probit marginal effects).

	Probit			
	(1) Gains	(2) Losses	(3) Effort	(4) Avg (2) and (3)
All Studies				
Treatment effect Pooled	0.040	0.025	0.017	0.020
	(0.007)***	(0.005)***	(0.005)***	(0.004)***
Treatment effect Day 1	0.040	0.010	0.039	0.025
	(0.008)***	(0.008)	(0.008)***	(0.006)***
Treatment effect Day 7	0.040	0.042	−0.006	0.015
	(0.008)***	(0.009)***	(0.008)	(0.006)**
Mean Day 1	0.45	0.66	0.67	0.66
Mean Day 7	0.48	0.70	0.67	0.69
N	25,460	25,460	30,343	55,803
Participant-Day Observations	1,061	1,061	1,061	1,061
Participants (Day 1)	578	578	578	578
Hydrocortisone				
Treatment effect Pooled	0.044	0.007	0.036	0.019
	(0.009)***	(0.009)	(0.011)***	(0.006)***
Treatment effect Day 1	0.047	0.000	0.027	0.012
	(0.013)***	(0.009)	(0.016)*	(0.009)
Treatment effect Day 7	0.040	0.015	0.047	0.028
	(0.013)***	(0.012)	(0.013)***	(0.008)***
Mean Day 1	0.45	0.65	0.68	0.66
Mean Day 7	0.49	0.71	0.65	0.69
N	13,196	13,196	9466	22,662
Participant-Day Observations	550	550	550	550
Participants (Day 1)	300	300	300	300
Trier Social Stress Test				
Treatment effect Pooled	0.035	0.043	0.009	0.021
	(0.009)***	(0.008)***	(0.006)	(0.005)***
Treatment effect Day 1	0.033	0.022	0.045	0.035
	(0.012)***	(0.011)*	(0.009)***	(0.007)***
Treatment effect Day 7	0.037	0.069	−0.027	0.006
	(0.013)***	(0.014)***	(0.008)***	(0.006)
Mean Day 1	0.45	0.67	0.66	0.66
Mean Day 7	0.46	0.69	0.68	0.69
N	12,264	12,264	20,877	33,141
Participant-Day Observations	511	511	511	511
Participants (Day 1)	278	278	278	278

Notes: Effects of treatment (stress induction) on likelihood of early choice, marginal effects from Probit model. The dependent variable in all columns is a dummy for choosing the "early" option in decision situations in the gains domain (Column 1), losses domain (Column 2), and effort domain (Column 3). Column (4) averages the effects in the losses and effort domains. The "Treatment effect Day 1" and "Treatment effect Day 7" rows show the difference in the dependent variable between treatment and control groups on the respective day using separate regressions, and their standard error in parentheses; similarly the "Treatment effect Pooled" row shows the estimated treatment effect from a separate regression in which we pool observations across days and include day fixed effects. The probit regressions are carried at the level of individual choices and include day (when relevant), set, and question order fixed effects. Standard errors are bootstrapped. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

the OLS model *without* clustering. Indeed, as shown in Online Appendix Table A.13, when we do not cluster standard errors, the precision of the OLS model is much greater, and the levels of statistical significance are similar to those of the Probit model. Note that this means the Probit results themselves should be regarded as suggestive because of the lack of clustering.

We next subject the results to several robustness checks. First, to account for possible non-linearities of individuals' utility function, in Online Appendix Table A.14 we add controls for loss aversion and curvature of the utility function obtained from the risk preference task. In addition, these specifications control for baseline characteristics that predict attrition, as well as baseline characteristics that are imbalanced. We find qualitatively similar results; specifically, the signs and levels of statistical significance are similar to those found in Table 5.

Next, in Online Appendix Table A.15, we repeat these regressions separately by frame (i.e. today vs. two weeks from today; today vs. four weeks from today; and two weeks from today vs. four weeks from today). Qualitatively, the results do not appear to be driven by any particular frame.

We also repeat the main regressions while excluding individuals who "switch" from earlier to later choices (or vice versa) multiple times within a frame. Online Appendix Table A.16 shows that the treatment effects are similar in significance and, if anything, larger in magnitude compared to the main specification. We confirm in Online Appendix Table A.17 that stress induction does not influence whether participants switch multiple times (or never) within a frame, though there is evidence that participants in the hydrocortisone treatment are more likely to switch multiple times within a frame in the effort task.

Table 6
Effect of Stressors on Likelihood of Early Choice (OLS).

	OLS			
	(1) Gains	(2) Losses	(3) Effort	(4) Avg (2) and (3)
All Studies				
Treatment effect Pooled	0.039	0.024	0.017	0.020
	(0.021)*	(0.019)	(0.023)	(0.018)
Treatment effect Day 1	0.039	0.010	0.038	0.025
	(0.028)	(0.027)	(0.029)	(0.024)
Treatment effect Day 7	0.040	0.041	−0.006	0.015
	(0.031)	(0.026)	(0.037)	(0.026)
Mean Day 1	0.45	0.66	0.67	0.66
Mean Day 7	0.48	0.70	0.67	0.69
N	25,460	25,460	30,343	55,803
Participant-Day Observations	1061	1061	1061	1061
Participants (Day 1)	578	578	578	578
Hydrocortisone				
Treatment effect Pooled	0.043	0.007	0.036	0.019
	(0.028)	(0.026)	(0.031)	(0.023)
Treatment effect Day 1	0.046	0.001	0.027	0.012
	(0.037)	(0.036)	(0.041)	(0.030)
Treatment effect Day 7	0.039	0.015	0.047	0.029
	(0.043)	(0.038)	(0.046)	(0.034)
Mean Day 1	0.45	0.65	0.68	0.66
Mean Day 7	0.49	0.71	0.65	0.69
N	13,196	13,196	9466	22,662
Participant-Day Observations	550	550	550	550
Participants (Day 1)	300	300	300	300
Trier Social Stress Test				
Treatment effect Pooled	0.034	0.042	0.009	0.021
	(0.031)	(0.028)	(0.031)	(0.026)
Treatment effect Day 1	0.032	0.021	0.045	0.035
	(0.044)	(0.041)	(0.038)	(0.035)
Treatment effect Day 7	0.037	0.067	−0.026	0.006
	(0.046)	(0.037)*	(0.049)	(0.039)
Mean Day 1	0.45	0.67	0.66	0.66
Mean Day 7	0.46	0.69	0.68	0.69
N	12,264	12,264	20,877	33,141
Participant-Day Observations	511	511	511	511
Participants (Day 1)	278	278	278	278

Notes: Effects of treatment (stress induction) on likelihood of early choice, OLS model. The dependent variable in all columns is a dummy for choosing the "early" option in decision situations in the gains domain (Column 1), losses domain (Column 2), and effort domain (Column 3). Column (4) averages the effects in the losses and effort domains. The "Treatment effect Day 1" and "Treatment effect Day 7" rows show the difference in the dependent variable between treatment and control groups on the respective day using separate regressions, and their standard error in parentheses; similarly the "Treatment effect Pooled" row shows the estimated treatment effect from a separate regression in which we pool observations across days and include day fixed effects. The OLS regressions are carried out at the level of individual decisiosn and include day (when relevant), set, and question order fixed effects. Standard errors are clustered at the session level for the TSST study, and at the individual level for the hydrocortisone study. Levels of statistical significance: $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$.

To relate the magnitude of the effects of the stressors on intertemporal choice to the magnitude of their effects on cortisol levels, notice that dividing their effect on intertemporal choice by their effect on cortisol levels corresponds to an instrumental variables estimate. We illustrate this approach for the gains domain. The cortisol increase induced by hydrocortisone, averaged across days 1 and 7, is 2.18 log points. The cortisol increase induced by the TSST, averaged across days 1 and 7, is 0.23 log points. Hydrocortisone increases the likelihood of choosing the early outcome in the gains domain by 4 percentage points, and the TSST increases it by 3.5 percentage points. This implies that a 1 log point (100 percent) increase in cortisol levels induced by hydrocortisone leads to a $4/2.18 = 1.83$ percentage point increase in the likelihood of choosing the early outcome. A 1 log point increase in cortisol levels induced by the TSST is predicted to lead to a $3.5/0.23 = 15$ percentage point increase in the likelihood of choosing the early outcome, although we stress that it is unlikely that a social stressor would be able to induce such a strong increase in cortisol.

Finally, as described above, we estimated the β (present bias) and δ (impatience) parameters of the quasi-hyperbolic discounting model (Laibson, 1997) using a two-step maximum likelihood method described in the Online Appendix B.4, taking utility function curvature (σ) and loss aversion (λ) into account. As discussed above, we do not have confidence in our parameter estimates because they are highly dependent on starting values in the maximum likelihood estimation. This results mainly from participants who always choose earlier or later outcomes within a frame, and the multiple switches within a frame. Nevertheless, because we pre-specified these outcomes, we show results in Online Appendix Table A.18.

As described, the estimates are noisy, and neither hydrocortisone administration nor the TSST have a consistent pattern of significant effects on δ and β.[14]

4. Conclusion

In this study, we show suggestive evidence that both hydrocortisone administration and the TSST increase the likelihood that individuals choose the earlier option across all domains of intertemporal choice tested here. These findings suggest that stress may increase the propensity to choose sooner outcomes, irrespective of whether this leads to a smaller monetary gain, or a smaller monetary loss or level of effort. This tentative result is the opposite of what is predicted by a model in which stress increases the discount rate or present bias, as would perhaps seem plausible given previous evidence on the effects of stress on discounting of monetary gains (Riis-Vestergaard et al., 2018; Delaney et al., 2014): in this case, delayed monetary losses and delayed effort provision would be perceived as less painful, increasing the choice of larger, later losses and effort. Our results therefore suggest that the effect of stress on intertemporal choice may not be well-described by the simple model. Instead, it is consistent with a model in which stress increases the discount rate or present bias in the gains domain, but decreases it in the losses domain. Alternatively, it is also consistent with a model in which stress decreases the utility of any future outcome, for example by subtracting a constant penalty term. In the losses domain, this might be caused by an increase in negative anticipatory utility of losses or effort, leading participants to wanting to get losses and effort "out of the way". It is important to note, however, that our findings reach conventional levels of statistical significance only in the absence of clustered standard errors, and our results should therefore be regarded as preliminary.

Our finding that the effect of stress on discounting of monetary gains may not be well-described by the simple model in which stress increases the discount rate or present bias suggests that caution is warranted when interpreting the results of studies that measure temporal discounting or present bias using choices solely in the domain of monetary gains, as the underlying effect may be more complex than a simple model suggests.

Due to the tentative nature of our results, we hesitate to speculate about policy implications. However, if the preliminary effects we report were confirmed, they would suggest that individuals become less willing to wait for positive outcomes when under stress; but that stress in fact leads them to prepone tasks or payments in the loss domain. Policy-makers could then consider selectively intervening to reduce stress in situations where they want to encourage future-oriented behavior related to gains; e.g., in stressful investment decisions. Conversely, they may be less inclined to reduce stress in situations where they want to encourage early action related to costs or effort; e.g., in stressful decisions about the adoption of productive technology.

Our study leaves a number of open questions. Most importantly, our Probit results, without clustering, are robust, but the OLS results, with clustering, are statistically weak. A highly-powered replication of our study would therefore be desirable. Second, while our results tentatively argue against a simple model in which stress increases temporal discounting, it leaves open which alternative model is correct; future work might attempt to establish this. Third, because our parametric estimation was unsuccessful, future work should also address whether our results are best described by a change in an exponential or a hyperbolic parameter. Finally, because we failed to induce stress using the cold-pressor task and the Centipede game, the question whether stress has similar effects on intertemporal choice in domains of stress other than those tested here remains open.

Declaration of Competing Interest

The authors declare that we have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper. However, in the interest of full disclosure, we address potential concerns below: Johannes Haushofer is the founder and Scientific Director of the Busara Center for Behavioral Economics and Prachi Jain is a Research Affiliate of the Busara Center for Behavioral Economics; the affiliation does not provide financial/personal interests that affect the objectivity of this work in any way. There are no other relevant declarations of interest for this work.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2020.12.024.

[14] We also administered the "Consideration of Future Consequences" (CFC) scale (Strathman et al., 1994). Participants were asked to indicate how much the behavior described in a statement is characteristic of them, from "not at all like me" (0) to "very much like me" (5). There are nine statements representative of forward thinking (e.g. "I am ready to sacrifice my current happiness or wellbeing in order to achieve future results") and five reverse statements (e.g. "I only act to satisfy immediate needs, thinking the future will take care of itself"), which are scored accordingly. We use an index generated from the CFC, reverse-scoring relevant items such that they are all positive or negative and weighing each question equally, as our primary outcome measure. Column 7 of Online Appendix Table A.17 shows that we find small and largely insignificant effects on the CFC. This is perhaps not surprising since the CFC is not designed to separately identify preference for earlier choices as opposed to time discounting.

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