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Self-Control, Commitment and Peer Pressure: A Laboratory Experiment

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Abstract

This paper focuses on the relationship between individual self-control and peer pressure. To this end, we implement a laboratory experiment that proceeds in two parts. The first part involves an individual real-effort task in which subjects may commit themselves to achieve a certain level of performance while being tempted by an alternative recreational activity. The second part consists of bargaining in a power-to-take game in which previously earned revenues are at stake. Experimental treatments represent variations in the available information given to peers regarding previous individual behavior. The results show that many subjects commit themselves strongly and that future revelation of commitment decisions induces subjects to increase the credible components of commitment decisions. Past individual behaviors also play a role in bargaining behavior: (i) partners who have committed themselves benefit from both lower take and destruction rates, and (ii) partners who have succumbed to temptation suffer from both higher take and destruction rates.

Keywords: Self-control, temptation, commitment, willpower, laboratory experiment, peer pressure
JEL Classification: C91, C92, D63

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1 Introduction

Individuals in social contexts are subject to temptations, for example, socialization with colleagues during the work day, napping and drinking alcohol. Some evidence about the macroeconomic economic cost of giving in to temptation is well-known. For instance, obesity development throughout the world may be related to several causes, one being the decrease from the early 80s in relative prices of high-energy dense foods and drinks relative to fruits and vegetables (see Finkelstein, Ruhm, and Kosa, 2005) and another one being reductions in the time cost of food (Cutler, Glaeser, and Shapiro, 2003). These two causes combined with self control problems for the consumers may explain this "obesity pandemic" (Stutzer, 2006). As a consequence, medical costs for obesity have increased in a spectacular way. For instance, in the US, from 1998 to 2008, absolute and relative medical costs of obesity went from 42 billion US $, i.e. 6% of the total medical expenses, to 86 billion in 2006, namely 10% of total medical expenses (Finkelstein, Trogdon, Cohen, and Dietz, 2009). Such temptations present self-control problems, revealing a conflict between one's short-term interest and long-term interest (see e.g., Thaler and Shefrin, 1981; O'Donoghue and Rabin, 1999, 2001; Benabou and Tirole, 2006). To avoid giving in to temptation, individuals may choose or combine two different strategies: (1) commitment devices (Noor, 2007; Ozdenoren, Salant, and Silverman, 2006) or (2) the exertion of willpower, where the latter entails costs of self-control (Ariely and Wertenbroch, 2002; Gul and Pesendorfer, 2001). As noted by Nehring (2006), for a given individual, these two strategies essentially entail changing either individual's future choice sets or their future choice dispositions.

In real life, individuals are likely to use two main commitment devices. The first is to exclude opportunities from choice sets. Famous examples include Odysseus tying himself to the mast in Greek mythology (Elster, 1985) and Hernando Cortez burning his boats on arrival in Mexico *"in order to leave no cause of trouble behind"* (Diaz del Castillo, 1632). The second device is to self-impose costly commitments that may be monitored by a third party (see Ariely and Wertenbroch, 2002). For instance, people save for seasonal gifts in "Christmas club accounts", which pay no interest yet entail early withdrawal fees (Thaler and Shefrin, 1981). Such financially counter-normative behavior, which implies additional costs, deters people from withdrawing their savings for impulsive reasons (Fujita and Roberts, 2010), thus improving their motivation (see, for instance, DellaVigna and Malmendier, 2006). More generally, this type of commitment often aids savings goals, where an individual may try to avoid withdrawing money before a certain balance is reached to avoid financial penalties (see e.g., Ashraf, Karlan, and Yin, 2006).

Undertaking costly self commitments that are monitored by third parties to avoid temptation is very common (e.g., Weight Watchers programs, Alcoholic Anonymous, etc.). Such schemes at least entail that an individual states his commitment by giving

public information to other individuals such as friends, family members, and neighbors.[1] In addition, this procedure provides procrastinators not only with referees (i.e., people who verify that actual decisions are consistent with objective achievements) but also with support (i.e., people who encourage committed individuals).

An individual may thus have an interest in publicly communicating his private commitment or objectives. There may be several objectives for such behavior, but an important one is likely to relate to potential emotions arising from public observation. Indeed, by displaying private information, an individual may seek to avoid emotional costs and obtain potential emotional benefits, with both emotional dimensions helping him to overcome temptation. Indeed, failing to respect one's own commitment may induce personal shame that arises from others' appraisals of him (added to guilt, which is an internal emotion arising from one's inability to achieve one's goals, an emotional cost that is not publicly provided; see Kandel and Lazear, 1992). By the same token, succeeding in one's objective may occasion pride (Gintis, 2005; Corgnet, Gonzalez, and Rassenti, 2013), a positive self-image resulting from positive appraisals of others (added to the emotional benefit that arises from self-confidence). These positive and negative emotions are in fact related to peer pressure - the role played by awareness that one's own choices are observed by peers (see e.g., Falk and Ichino, 2006; Camerer and Malmendier, 2007) -, which is a key feature in this paper.

By way of a laboratory experiment, we investigate how peer pressure may influence strategies that individuals might use to overcome temptation, thus addressing our first question. However, another question immediately arises: if peer pressure can help people achieve better individual performance through righteous conduct, do peers also reward such efforts? We conjecture that peer pressure may be a device people employ in monitoring themselves and also that peers may be influenced in their decisions by the monitoring efforts made previously by their partner.

Our experiment aims to test these two conjectures through a sequence of individual decisions made in two parts. In the first part, subjects are asked to perform a real-effort task in which they have the opportunity to commit themselves in various ways to increase their motivation. However, as proposed by Ajzen's theory of perceived behavioral control (see Ajzen, 2002), people differ in the extent to which they view rewards, punishments, or other events in their lives as caused by their own actions and by factors beyond their control. Consequently, commitment and self-control behavior may explain how individuals perceived a given social control. To investigate this issue, in the second part of our experiment, participants are randomly paired in a bargaining game, the power-to-take game (Bosman and van Winden, 2002). In this game, the proposer may appropriate the responder's resources. The responder may then punish the proposer by destroying any share of his own resources. Therefore, the power-to-take game approximates social

[1]For instance, Dean Karlan's site Stickk.com asks people to determine a certain objective to reach and, among other things, the level of financial punishment to be levied on them in case of failure.

environments in which one party may appropriate the resources of another, such as taxation or monopoly power on consumer surplus (Reuben and van Winden, 2010). Through the use of this game, we seek to assess how self-control behavior may influence either appropriation behavior or punishing behavior.[2] Thus, the second part of the experiment is implemented through different treatments in which we vary the information that subjects obtain about their partner's behavior in the first part of the experiment (i.e., temptation, commitment level, etc.).

Our objectives are thus twofold. First, we aim to investigate how commitment levels and behaviors under temptation are affected by the revelation of information about such commitment levels and behaviors during the bargaining process. Second, we aim to observe whether such information, by triggering prosocial emotions (see Bowles and Gintis, 2005), such as shame and guilt, may influence collective bargaining. Finally, because the individual real-effort part results in heterogeneous endowments among partners at the beginning of the second part of the experiment, we elicit individual inequity aversion estimates by following a procedure outlined in Blanco, Engelmann, and Normann (2011).

To preview our results, we find that the revelation of an individual's past behavior to a future partner has strong effects. First, the revelation does not affect the objectives set *per se*, but it does increase significantly the penalties and the objective/penalty pairs, when information about these choices will be publicly shared. Second, revealing behavior under temptation has no effect on the behavior itself but increases the level of objectives set. Our findings also show that past individual behavior affects the bargaining process. Partners who have committed themselves benefit from both lower take and destruction rates, whereas partners who have given in to temptation suffer from both higher take and destruction rates.

The remainder of the paper is organized as follows. In section 2, we review the related literature. We provide details of our experimental design in section 3 and present the results of our experiment in section 4. Section 5 concludes.

2 Literature review: temptation and peer pressure

The theoretical and empirical literature on willpower (or self-control)[3] has grown significantly over the last decade.

From a theoretical point of view, the starting point in the literature is the seminal paper of Strotz (1956) which outlines the problem of time inconsistency due to (quasi-) hyperbolic time preferences (see e.g., Phelps and Pollak, 1968; Laibson, 1997; O'Donoghue

[2]The power-to-take game enables the observation of the appropriation behavior of individuals or punishing behavior in a continuous way, in contrast to traditional ultimatum games, which allow only for "yes or no" answers. In prior research, the power-to-take game has primarily been used to highlight the effects of emotions on decision-making, which is also a focus of our study.

[3]Self-control could be defined as a tendency to delay gratification; see Higgins and Makin (2004).

and Rabin, 1999). Within this framework, present-biased preferences may explain the desire for strong commitment. Temptation and dynamic choices have since been modeled on the assumption of fully time-consistent individuals (see in particular Gul and Pesendorfer, 2001, 2004, 2005; Miao, 2008).

From an empirical point of view, field experiments at first focused on the relationship between the commitment strategies of individuals and individuals' awareness of possible lack of self-control (e.g., Trope and Fishbach, 2000; Ariely and Wertenbroch, 2002; Ashraf, Karlan, and Yin, 2006; Ameriks, Caplin, Leahy, and Tyler, 2007; Burger, Charness, and Lynham, 2008). These studies show that people experience self-control problems, and as a result, they impose costly commitments on themselves to avoid self-control costs. Evidence on the positive impact of commitment devices on efficiency and performance is more mixed. For instance, Ashraf, Karlan, and Yin (2006) find that commitment limits overconsumption and thus encourages household savings, whereas Bucciol, Houser, and Piovesan (2011) report that resisting temptation negatively affects average worker productivity. The study most closely related to our work is that of Houser, Schunk, Winter, and Xiao (2010). The authors conduct a laboratory experiment in which subjects are repeatedly offered an option with an immediate benefit that also entails a reduction in their overall earnings. In their experimental design, participants could avoid temptation by bearing a cost that removes the tempting option, where this cost is exogenously chosen by the experimenter and subject to variation. The authors observe that (i) a substantial share of subjects commit themselves, but (ii) the commitment behavior tends to decrease with repetition, suggesting that willpower declines over time.[4]

All these works, both theoretical and empirical, highlight that individuals are willing to pay to impose on themselves costly commitments that limit costly self-control. Surprisingly, very few of these studies offer individuals the opportunity to monitor procrastination or self-control costs through social relationships. This omission is surprising, as many common daily situations highlight the importance of support groups. For example, organizations such as Alcoholics Anonymous, Narcotics Anonymous, Gamblers Anonymous, and Debtors Anonymous have branches in many countries and millions of members. In particular, to our knowledge, no laboratory experiment explores the relationship between self-control and peer pressure in a social context. Only Houser, Montinari, and Piovesan (2012) explore this issue through a field experiment. In their study, children are subjected to temptation in a manner similar to the famous "marshmallow test", with the

[4]One major difference between our approach and that of Houser, Schunk, Winter, and Xiao (2010) concerns the consequences of commitment. Indeed, Houser, Schunk, Winter, and Xiao (2010) use commitment devices to rule out temptation for the decision-maker: if an individual chooses to commit himself, the commitment cost is a sunk cost, as it completely eliminates the tempting option. By contrast, in our experiment, the commitment cost (or penalty) is imposed only if the subject does not reach his self-chosen objective, and more importantly, the commitment cost may be revealed to a subsequent partner. In this case, there is no possibility of withdrawing the tempting option. Our framework is more akin to situations of self-control linked to real or potential addictions that are monitored through an "anything anonymous" model. In this case, commitment to peers does not rule out temptation.

children's behavior potentially observed by peers. The authors find that self-control is stronger under public observation, with small children posing an exception, and that this effect is enhanced when the group size is larger.

Peer pressure has become an important issue in behavioral economics (e.g., Diamond and Vartiainen, 2007) since the seminal paper of Kandel and Lazear (1992). Peer pressure is often invoked to explain why group-based incentives are surprisingly successful despite the incentive to free-ride. Peer pressure appears to be effective because individuals wish to avoid the disapproval of their peers (e.g., Falk and Ichino, 2006), even if such disapproval does not involve a monetary cost (Masclet, Noussair, Tucker, and Villeval, 2003). In particular, people might be willing to pay to avoid guilt, induced by the awareness that one has violated social norms, or to avoid shame, caused by public disapproval of one's behavior.

As noted by Battaglini, Benabou, and Tirole (2005), the small number of studies of the relationship between self-control and peer pressure may be explained by the separation of these two areas of inquiry within economics, in contrast to psychology or sociology. For example, Rachlin (2000) highlights the importance of patterning in monitoring self-control issues to enhance social cooperation. Brown and Rachlin (1999) explain the complementarity of these two issues very simply: *"The problem of self-control is a conflict between particular acts such as eating a caloric dessert, taking an alcoholic drink, or getting high on drugs, and abstract patterns of acts strung out in time such as living a healthy life, functioning in a family, or getting along with friends and relatives"* (Brown and Rachlin, 1999, p. 65). In economics, the only study that views these issues as complementary is Battaglini, Benabou, and Tirole (2005), who build a theoretical model that combines imperfect willpower, social learning and self-signaling. The authors emphasize the possibility of multiple equilibria and show that peer monitoring is useful from a welfare perspective if people are sufficiently confident not only in their own ability to resist temptation but also in that of their peers. Therefore, our experiment represents an important step in this line of research in enabling an empirical analysis of whether peer pressure may provide an effective means for individuals to overcome temptation.

3 Experimental design

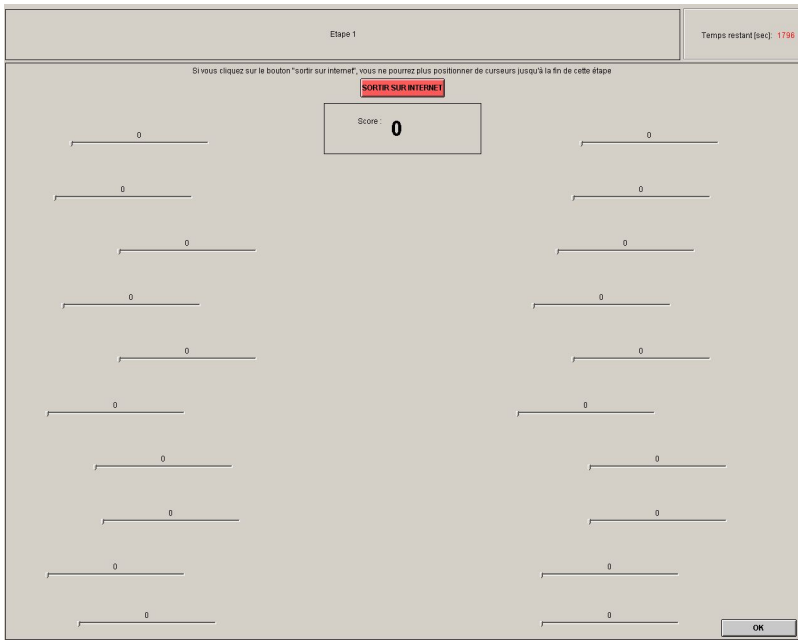
Our experiment consists of two consecutive parts. The first part involves an individual real-effort task. The second part involves a bargaining game in which the number of points at stake is determined by decisions made in the first part of the experiment. From instructions given at the beginning of the experiment,[5] subjects learn how participants will interact in the second part of the experiment, but they are not informed of what roles they will play in the bargaining game.

[5]See Appendix B for details.

3.1 Part 1. Individual real-effort task

In the first part of the experiment, subjects are involved in an individual real-effort task, based on Gill and Prowse (2012), that lasts one hour. Subjects face a computer screen that shows 20 sliders. For each slider, by scrolling with a mouse along the line, each subject must place the slider precisely in the middle of the line to accumulate points (see Fig. 1). Each correctly positioned slider gains the subject 5 experimental points. Each slider may be freely repositioned without a time limit until all sliders are correctly positioned. Next, the subject validates his screen by clicking a button, which causes a new screen to appear. The screen does not vary among subjects or among repetitions of the task. As Gill and Prowse (2012) stress, this particular real-effort task has the advantage of being very simple.[6] Moreover, the similarity of the task among repetitions implies little randomness, so the number of correctly positioned sliders closely tracks the amount of effort subjects undertake. This characteristic constitutes a major difference with other tasks used in experiments of this kind, such as mathematical problems or anagrams. Finally, given our focus on the issue of temptation, this task is intentionally very boring and physically exhausting for subjects.

Figure 1: Sliders task (with tempting option)



The first part of the experiment is divided into two consecutive and independent stages. In each of these stages, subjects must correctly position sliders for 30 minutes. In each stage, participants are told (i) that they have the option of committing themselves

[6]Of course, there are individual differences, as subjects are talented to varying degrees in the use of a computer and mouse (for instance, some often play video games) or may feel obliged to perform well. However, as subjects are randomly assigned to experimental sessions, we assume that such individual differences are cancelled out.

to achieving a certain number of tasks within the limited time they have and (ii), if they elect to make such a commitment, that they must choose a penalty to be applied should they fail to meet their chosen objective. The penalty rate consists of a reduction, from 0% to 100%, of the total number of points the subject has earned by the end of this stage. To help subjects set their objectives, a practice round lasting two minutes is implemented before the individual's real-effort task begins.

The two individual stages differ in whether a tempting option exists. In one of the two stages, subjects have the opportunity to stop their real-effort task at any moment to surf the Internet, indicated by a red button at the top of their computer screen (see Fig. 1). The opportunity to surf represents our tempting option, following Houser, Schunk, Winter, and Xiao (2010). If they surf the Internet, subjects are aware that they may not return to the real-effort task and accumulate experimental points until the end of the current stage. To avoid an order effect, in half of the experimental sessions, subjects engage in the real-effort task first with the tempting option and then without it. We reverse the order of the two stages in the second half of the experimental sessions.

3.2 Part 2. Bargaining part: the power-to-take game

3.2.1 The constituent game

After the individual part of the experiment, which lasts exactly one hour, is complete, the bargaining part begins. To begin this part of the experiment, the computer randomly allocates a role to each subject. The bargaining part consists of a one-shot two-player power-to-take game (Bosman and van Winden, 2002) in which each player (i.e., the proposer and the responder) starts with the experimental points he has earned during the individual real-effort task.

This game proceeds in two stages. In the first stage, the proposer chooses a take rate $t \in [0; 100\%]$, which is the part of the responder's endowment that will be transferred to the proposer after the second stage. In the second stage, the responder is informed of the proposer's choice, and the only action he can take in response is to determine the destruction rate $d \in [0; 100\%]$ of his own prior-to-take income. If we denote by E_P and E_R the endowments of the proposer and the responder, respectively, at the beginning of the power-to-take game, then the payoffs at the end of part 2 are as follows:

$$\begin{aligned} \pi_P &= E_P + t(1-d)E_R \quad \text{for proposer} \\ \pi_R &= (1-t)(1-d)E_R \quad \text{for responder} \end{aligned} \tag{1}$$

Because the amount at stake for each player at the outset of the game corresponds to the amount earned by each player in the first part of the experiment, the endowments of the proposer and the responder will differ. As a result, inequity aversion may influence the choices of the players at two distinct moments: first, at the beginning of the power-

to-take game, due to the difference between E_P and E_R, and second, after the proposer has chosen a take rate. To disentangle inequity aversion from the influence of the level of commitment and behavior under temptation as possible explanations of the take and destruction rates chosen, we implement a second experiment to elicit individual estimates of inequity aversion and thereby control for it. To this end, we follow the experimental design of Blanco, Engelmann, and Normann (2011).[7]

3.2.2 Experimental treatments

As part of our experimental design, the information provided to players in part 2 (i.e., bargaining) about their partner's behavior in part 1 (the individual part of the experiment) is varied.

Overall, five experimental treatments in a between-subject design are implemented. In our **Benchmark** treatment, players know only their partner's endowment and have no information about their partner's commitment decisions or his behavior under temptation. In the second treatment (**Commitment**), in addition to knowledge about their partner's endowment, subjects learn whether their partner committed himself in the first part of the experiment and, if so, the commitment level chosen (thenumber of tasks the partner pledged to successfully complete and the associated penalty set in case of failure). In the final three treatments, we add information regarding temptation. In the Asymmetric P treatment (**Asym. P**), only the proposer knows whether his partner succumbed to temptation. Conversely, in the Asymmetric R treatment (**Asym. R**), only the responder possesses this information. Finally, in the **Full** Information treatment, each player's behavior under temptation is common knowledge.

Because we have two different experimental orders for the first part (i.e., subjects are exposed to temptation in the first stage or in the second stage) and five treatments, overall we have ten different experimental conditions. Details are provided in Table 1.

Table 1: Number of subjects by experimental condition

Treatments	Benchmark	Commitment	Asym. P	Asym. R	Full	Total number of subjects
Temptation in second stage	20	20	20	16	20	96
Temptation in first stage	20	20	22	16	18	96
Total number of subjects	40	40	42	32	38	192

3.3 Behavioral Hypotheses

In this subsection, we state behavioral conjectures that enable us to make some predictions about the experimental results. We first present conjectures about ways in which, in our different treatments, information provided before the commencement of the second part of the experiment might influence an individual's behavior during the first part of

[7] See Appendix A for details.

the experiment, when commitment and effort decisions are made. Next, assumptions about the influence of this information (about first part decisions) on the bargaining part are set forth.

3.3.1 Commitment levels with the perspective of being observed

Experimental literature in economics and psychology has shown that social pressure might help an individual overcome temptation. In our experiment, such social pressure is ensured by i) the observability of past actions, depending on the experimental treatment, and ii) a bargaining game in which information given to participants should influence their decisions and thus the final outcome of the game. We assume that when individual commitments or behavior under temptation may be (or are) revealed, commitment levels increase.

- *Conjecture 1: A sense of being observed should increase commitment levels.*
- *Conjecture 2: The probability of giving in to temptation should be higher under the Benchmark and Commitment treatments than under treatments in which information about behavior under temptation is provided to the subsequent partner.*

3.3.2 Bargaining and observability of past behaviors

Because individuals are placed in a bargaining game under various degrees of information about past commitment behavior and self-control ability, the information may have mixed effects on (i) appropriation behavior (take rate) and (ii) punishing behavior (destruction rate) in the bargaining game:

- *Conjecture 3: An individual's own past behavior or past partner's behavior may affect take rates and destruction rates in the following ways:*
  - *(a) High (low) levels of commitment and self-control shown by oneself in the first part of the experiment should be associated with high (low) take rates or high destruction rates (depending on one's role) chosen in the second part of the experiment;*
  - *(b) High (low) levels of commitment and self-control shown by a partner in the first part of the experiment should be associated with low (high) take rates or destruction rates (depending on one's role) chosen in the second part of the experiment.*

3.4 Procedure

Our experiment was conducted at the LABEX-EM, University Rennes 1. The experiment was computerized, using the software Z-TREE (Fischbacher, 2007). Participants were recruited from a pool of undergraduate students, using the online Recruitment System

for Experimental Economics, ORSEE (Greiner, 2004). Overall, 192 subjects participated in 10 experimental sessions (see Table 1).

Before the game started, participants were told (i) that there would be two independent experiments,[8] (ii) that money earned in the experiments would depend on their decisions and the decisions of others in their experimental group, and (iii) that they would be paid the earnings they accrue in just one of the two experiments, randomly chosen by the computer, at the end of the session. To ensure equal average earnings regardless of the experiment chosen for payment, the conversion rate for the first experiment was 150 points = 1 Euro, whereas it was 1 point = 1 Euro for the second experiment. It was made very clear that information about earnings obtained in each experiment as well as the experiment chosen for payment would be given only at the very end of the experimental session. We set this condition to reduce the potential spillover effects of earnings from one experiment to the next.

To guarantee public knowledge, instructions regarding the first experiment were distributed and read aloud. All participants were required to answer several control questions to ensure that they understood the experimental procedures (see Appendix C). In particular, they were required to indicate the payoffs of each player under different outcomes of the game. Answers were privately checked and, if necessary, explained to the participants, and the experiment did not start until all participants had answered all questions correctly. Each session lasted up to two hours. Participants earned 25 Euros on average (including a show-up fee of 5 Euros).

4 Experimental results

Because decisions made in the individual real-effort task may be affected by the prospect that these decisions will be revealed to the partner in the bargaining game, we first analyze how such information affects an individual's commitment and behavior under temptation. We then examine the effects of the individual's past behaviors on bargaining.[9]

4.1 Self-control and peer pressure

4.1.1 Commitment decisions

Our first research goal was to analyze whether subjects commit themselves, and if so, whether the future provision of information about one's commitment to peers affects such behavior. Because no significant differences were found between the two stages of

[8] Experiment 1 refers to the real-effort task followed by the power-to-take game, whereas experiment 2 refers to the experiment dedicated to the elicitation of inequity aversion estimates.

[9] Because the main purpose of our experiment concerns (i) commitment decisions, (ii) behavior under temptation, and (iii) the effects of their revelation on bargaining, the results regarding inequity aversion are reported, for exposition purposes, in Appendix A2.

the real-effort task (i.e., with and without the tempting option), a summary of commitment decisions is provided in Table 2. While subjects *a priori* have no incentive to commit themselves, we note that, on average, a non-negligible portion of the subjects committed themselves at least once, with significant objective levels and high penalties in case of failure. An examination of Table 2 suggests a significant effect: when information about commitment is subsequently provided to peers, there appears to be an increase in the level of penalties. Another finding is the higher performance of committed subjects when commitment patterns and behaviors under temptation are revealed in the second part of the experiment (all Mann-Whitney U tests are significant at the 5% level), a point to which we return at the end of this subsection.

Table 2: Descriptive statistics

Treatments	Committed subjects (in %)	Objectives (in sliders)	Penalties (in %)	Performance committed subjects[a] (in sliders)	Performance non committed subjects[a] (in sliders)
Benchmark	33.75	201.00	18.25	454.89	428.58
		(143.43)	(26.14)	(93.79)	(61.97)
Commit.	45.00	155.81	29.50	458.22	446.20
		(125.93)	(36.61)	(72.72)	(64.47)
Asym. P	39.28	229.18	27.12	524.00	476.19
		(149.16)	(32.78)	(159.97)	(103.33)
Asym. R	39.06	211.00	27.68	468.84	408.89
		(122.34)	(31.60)	(70.05)	(67.04)
Full	46.06	221.43	28.65	498.88	423.75
		(137.91)	(30.68)	(101.09)	(95.62)
Aggregate data					
With commitment info.	42.43	203.07	28.31	488.14	441.37
		(136.62)	(32.76)	(110.30)	(88.60)
With temptation info.	41.52	221.37	27.84	439.75	499.72
		(136.83)	(31.35)	(95.51)	(120.12)

Notes: Standard deviation in parentheses.[a] For comparison purpose, they correctly posit, on average, 21 sliders in the 2 minute practice round.

To obtain stronger statistical evidence supporting these observations while controlling for (i) the various experimental conditions implemented and (ii) the socio-demographic characteristics of the participants, we conduct econometric analyses. Because participants make sequential decisions (a commitment decision followed by subsequent objective level and penalty level decisions), we deem the Heckman two-stage model to be the most suitable model for our purposes (see Heckman, 1979). In the first stage of the Heckman model, called the selection equation, we estimate a probit regression to model the decision to commit. The dependent variable is the probability of commitment, and the specification includes the socio-demographic characteristics of subjects (ω_i), their performance during the practice round, the presence of temptation and fixed effects for experimental treatments to highlight whether any significant differences with respect to the Benchmark treatment exist. In particular, this approach allows for observations of differences associated with information regarding either commitment or both commitment and temptation.

In the second stage, we estimate the determinants of the chosen level of objectives

Table 3: Heckman two-stage model results

Dependent variable	Prob. commitment	Objectives	Penalties	Objectives × Penalties
Commitment treatment	-0.180	-63.376	53.302**	6159.271*
	(0.461)	(47.963)	(20.522)	(3747.301)
Asym. P treatment	-0.121	59.501	35.110*	6089.919*
	(0.428)	(67.376)	(17.676)	(3226.818)
Asym. R treatment	-0.277	19.141	41.855**	10015.977**
	(0.521)	(61.760)	(16.948)	(4984.054)
Full inf. treatment	-0.810	5.099	52.739**	11137.154*
	(0.525)	(47.660)	(20.655)	(5814.698)
Tempting option exists= 1 if yes	-0.517	48.983	16.241	1350.511
	(0.444)	(56.937)	(15.158)	(2534.363)
Perf. during practice round	0.079***	7.668*	-2.173*	-208.331
	(0.022)	(4.026)	(1.232)	(316.991)
Constant	-8.449***	64.360	244.335**	39381.636
	(1.807)	(170.240)	(119.284)	(26603.164)
Inverse mills ratio	-	-	-46.695**	-8663.509**
			(19.861)	(4239.114)
Tempting option order × treatment F.E	Yes	Yes	Yes	Yes
Socio-demographic controls	Yes	Yes	Yes	Yes
F-test ($\omega_i = 0$)	17.23	2.23	3.13	4.22
p-value	(0.000)	(0.091)	(0.049)	(0.018)
Statistics				
N	192	87	87	87
Left-censored observations	-	-	5	-
Right-censored observations	-	-	14	-
R-square	0.285	0.218	0.232	0.243

Notes: ***, **, *: denote statistical significance at the 1%, 5% or 10% level, respectively. Robust standard errors in parentheses. Benchmark treatment used as reference. Socio-demographic controls include: age, gender (Female = 1) and field of study (Non economics=1) except for the second stage of the Heckman two-stage model.

using an OLS regression, whereas a double-censored Tobit specification is used for the chosen level of penalties to account for the lower and upper penalty limits.[10]

Finally, it is worth noting that the probability of commitment during the second stage of the real-effort task may depend on what happened in the first stage: the commitment decision, the achieved performance or whether the subject has succumbed to temptation. The results of Chi-square tests support the intuition that the probability of commitment in the second stage depends significantly (at the 1% level) on the commitment decision made in the first stage. Moreover, as noted by Houser, Schunk, Winter, and Xiao (2010), if an individual prefers to commit, commitment occurs at the first opportunity. To focus on the potential effect of the subsequent revelation of information about commitment decisions, the parametric analysis that follows considers commitment decisions made only during the first stage of the real-effort task. Table 3 reports the results.

We begin by examining estimates of the selection equation (Column 1). As suggested in Table 2, subsequent revelation of one's commitment decision does not affect one's

[10]To account for self-selection bias, from the first stage, we compute the expected error; then, we include the estimated error (i.e., the Inverse Mills' Ratio) as a regressor in the equation in the second stage. Because identification in the Heckman two-stage model requires removing at least one variable in the second stage that is included in the first stage, the dummy variable that accounts for the field of study is dropped from the second stage (Amemiya, 1985; Maddala, 1983).

propensity to commit. Furthermore, the presence of the tempting option does not induce more subjects to commit to be less tempted to surf the Internet. Unsurprisingly, committed subjects are the highest-performing subjects during the practice round.

We now turn to the second stage of the estimation procedure. We first analyze the factors that predict the level of objectives chosen (Column 2). The results obtained from the Heckman two-stage model highlight that the inverse Mill's Ratio is not significant, revealing that the self-selection bias is not a relevant econometric issue in this regression. To obtain more efficient parameters, we conduct a simple OLS regression on the objectives set by committed subjects.[11] The results confirm that there is no significant increase or decrease in the level of objectives set compared to the Benchmark treatment. It would be therefore possible to conclude that the revelation of commitment decisions during the bargaining part of the experiment does not impact willingness to commit or the level of objectives set during the individual part of the experiment.

However, this conclusion no longer holds when penalties chosen in the individual part are analyzed (Column 3).[12] We first note that the inverse Mill's Ratio is strongly significant, indicating that self-selection bias is a relevant econometric issue in this case and must be controlled for by modeling the propensity to commit in the first stage. As might be expected, the penalties are significantly higher when subjects know that information about their commitment behavior will be provided to their partner.

At first glance, it may seem surprising that the future revelation of commitment behavior during bargaining impacts only the level of penalties. However, this outcome is reasonable if we consider the relevant patterns of commitment. The willingness to commit or the objectives set have no impact when subjects establish low-level or null penalties: a commitment is credible only if participants match it with sufficiently high penalties. This is even truer when high penalties are associated with high objective levels, as is the case here (Spearman rank correlation coefficient: $\rho = 0.3767$, $p < 0.001$ when commitment decisions are displayed). Thus, one might expect that subjects would set higher objective/penalty pairs, given future public observation of their commitment decisions. The estimation of the Heckman two-stage model, where the interaction term between objectives and penalties is the dependent variable, confirms that revealing commitment decisions has a strong and positive effect on the objective/penalty pairs chosen (Column 4).[13]

Result 1. *Conjecture 1 is partially verified: the future revelation of commitment decisions increases the penalties that subjects are willing to bear in the case of failure. It also increases the objective/penalty pair chosen, an aspect of commitment associated with credibility.*

[11] Results from Heckman two-stage model are qualitatively similar and available from the authors upon request.

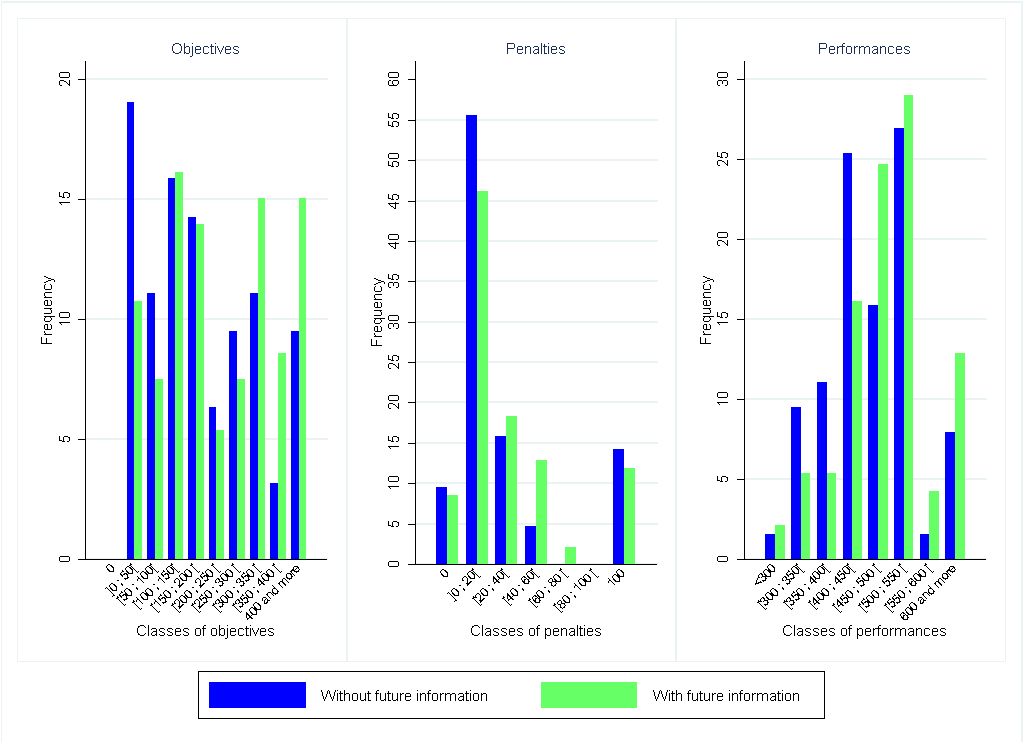
[12] It is worth noting that, regardless of the experimental treatment, all subjects' performances exceed the objectives they set.

[13] Because only 5 out of the 87 observations may be left-censored, we use OLS regression for the second stage. The results are the same if we use a left-censored Tobit model in the regression.

4.1.2 Temptation decisions

Our second research objective was to test whether providing information about behavior under temptation during bargaining would help subjects overcome temptation during the real-effort task. The small number of subjects who give in to temptation (7.81%) leads us to use non-parametric tests to examine this question. Clearly, there is no support for the view that the subsequent revelation of behavior under temptation affects the propensity of subjects to give in to temptation (Chi-square test: $\chi^2 = 0.0186$, $p = 0.892$). More precisely, 7.5% of subjects choose to surf the Internet when such behavior will remain hidden (i.e., in the Benchmark and Commitment treatments), and 8.03% on average choose to surf the Internet when this behavior may or will be revealed.[14] Conjecture 2 is thus rejected.

Figure 2: Objectives, penalties and performances for committed subjects depending on future information about behavior under temptation



A rather surprising result is that future revelation of whether a subject has given in to temptation affects the behavior of committed subjects. Fig. 2 indicates the objectives and penalties set by subjects as well as their achieved performance, sorted by whether behavior under temptation is revealed. As suggested by Fig. 2 and Table 2, future revelation of this information increases the level of objectives that individuals set (Mann-

[14] Note that this information is provided with certainty in the Full Information treatment, whereas in the Asym. P and Asym. R treatments, this revelation depends on the role of the player in the power-to-take game.

Whitney U test: $z = -2.150$, $p = 0.0315$) and the performance they achieve (Mann-Whitney U test: $z = -2.362$, $p = 0.0182$). Consequently, in these cases, subjects who commit themselves perform on average better than those who do not (Mann-Whitney U test: $z = -4.239, p < 0.001$),[15] a finding that is more strongly driven by an increase in the performance of committed subjects than by a decrease in the performance of non-committed subjects. The performance achieved by committed subjects may be regarded as an alternative means for subjects to demonstrate that they were able not to give in to temptation or at least that they exerted strong performance before succumbing to it.

Finally, one may conjecture that committed subjects who do not give in to temptation perform better than subjects who succumb to it. Taking into account all experimental treatments, this conjecture is confirmed by a Mann-Whitney U test ($z = 2.238$, $p = 0.0252$). Representing a final interesting aspect of our data are the behavioral differences concerning commitment patterns between those who succumb to temptation and those who do not: subjects who succumb to temptation set lower objectives ($z = 1.666$, $p = 0.0958$) and higher penalties in case of failure ($z = -2.006$, $p = 0.0449$).

Result 2. *Few subjects succumb to temptation. Furthermore, the revelation of behavior under temptation does not affect the propensities of subjects to succumb to temptation, but it does increase (i) the level of objectives set and (ii) the achieved performance of committed subjects.*

4.2 How do individual commitment and temptation impact bargaining?

The power-to-take game implemented in the second part of our experiment approximates social environments in which one party (the proposer) may appropriate the resources of another (the responder) through the take rate chosen. This decision may be influenced by the commitment decision of the partner and by his behavior under temptation. In response to the take rate, the responder may punish the proposer by choosing a destruction rate applied to his own endowment. Further, his response may be influenced by the past behavior of his partner. Therefore, our final research question concerns how past individual behavior affects decisions made during bargaining.

4.2.1 Take rates

Averaging over all treatments, the mean take rate observed in our experiment is 68.69%, which is in line with previous findings (see Bosman, Sutter, and van Winden, 2005, Reuben and van Winden, 2010, for instance). Nonetheless, unlike in previous studies, heterogeneity of performance in the real-effort task leads to different endowments among participants at the beginning of the bargaining game. These differences in endowment suggest the need to control for inequity aversion when examining the effect of past individual behavior on chosen take rates. More precisely, using a Mann-Whitney-U test, the

[15] This finding strengthens the results of Ariely and Wertenbroch (2002), while it contradicts the results of Bucciol, Houser, and Piovesan (2011).

hypothesis that both players have the same endowment is rejected in treatments where proposers have no information about their partner's behavior under temptation (Benchmark: $z = -4.023$, $p < 0.001$; Commitment info: $z = -4.813$, $p < 0.001$; Asym. R: $z = -3.546$, $p < 0.001$). Assuming inequity aversion, one may expect higher take rates in these experimental treatments to narrow this gap. However, as observed in Table 4,mean take rates are quite similar and are not significantly different across all treatments; even if Table 4 shows clear evidence of heterogeneity in the proposers' behavior. However, similarities in mean take rates across treatments arise in two distinct situations: first, treatments with significant differences in endowments but no information about the partner's behavior under temptation, and second, treatments with no significant differences in endowments but with revelation of the partner's behavior under temptation. In light of this finding, we conclude that the similarity in the mean take rates may be explained by inequity aversion, but it may also reflect the revelation of information about the partner's past behavior.

Table 4: Descriptive statistics of take and destruction rates

Treatments	Benchmark	Commitment	Asym. P	Asym. R	Full
Take rate	66.5	71.20	62.66	65.87	77.42
	(25.08)	(25.10)	(26.78)	(29.32)	(26.91)
Destruction rate	21.00	21.90	21.47	27.19	36.10
	(39.86)	(39.74)	(39.82)	(39.19)	(41.90)
% Destruction rate = 0	70	70	76.19	56.25	47.37
% Destruction rate = 100	20	20	19.05	18.75	21.05

Notes: Standard deviation in parentheses.

To investigate the possible effect of past individual behavior on take rate decisions, we estimate a right-censored Tobit regression model to account for the upper limit of observed take rates (i.e., 100%). The dependent variable is the chosen take rate, and the independent variables are socio-demographic characteristics of the subjects (ω_i= age, gender, field of studies), the individual parameters of inequity aversion (to control for sensitivity to differences in endowments at the beginning of the game), the proposer's endowment, differences in endowment between the proposer and the responder, variables for commitment and temptation (i.e., the frequency of commitment, behavior of temptation, whether commitment has been made under temptation for both players) and the fixed effects of the experimental treatments.[16]

The results are reported in Table 5. Column 1 shows the regression results estimated for the whole sample for the two treatments considered. We observe first that proposers who have committed themselves a high number of times[17] choose low take rates, a finding contrary to Conjecture 3.a. One possible explanation is that in treatments Asym. P and Full Information, committed subjects perform the best (see Table 2). Consequently,

[16]Only the Asym. P and Full Information treatments are used, as information about the past behavior of responders is only provided to proposers in these treatments.

[17]They may not commit themselves at all, they may commit themselves only in one of the two stages, or they may commit themselves in both stages.

in these treatments, committed subjects have high endowments and may therefore not be tempted to try to capture a large share of their partner's endowment. This conjecture is supported by the strong and negative effect of the proposer's endowment on the chosen take rate. Second, we observe that the partner's past behavior matters: proposers apply a lower take rate to a committed partner but a higher take rate to a partner who has succumbed to temptation. Conjecture 3.b. is thus confirmed.

Table 5: Tobit estimates for take rate decisions

Dep. variable	Take rate		
	Full sample (1)	Subsample (2)	Subsample (3)
Number of proposer's commitment	-34.160***	-49.558***	
	(8.692)	(4.641)	
Proposer's commitment under temptation=1	26.405	82.746***	
	(15.771)	(8.241)	
Objectives set by proposer			-0.074**
			(0.033)
Penalties set by proposer			-0.512**
			(0.210)
Proposer gives in to temptation=1	-2.333	44.110***	
	(11.782)	(4.699)	
Number of responder's commitment	-18.178**	23.583***	
	(7.324)	(3.735)	
Responder's commitment under temptation=1	12.532	-153.395***	
	(15.801)	(12.759)	
Objectives set by responder			0.002
			(0.028)
Penalties set by responder			-0.208*
			(0.112)
Responder gives in to temptation=1	29.013*	39.825***	
	(14.588)	(10.931)	
Proposer's endowment	-0.013**	-0.014***	-0.024
	(0.006)	(0.004)	(0.023)
Endowments difference	0.001	0.004	0.023
	(0.004)	(0.003)	(0.018)
Alpha[a]	11.202**	19.899***	-9.699
	(4.063)	(1.306)	(6.454)
Beta[b]	-20.098*	-146.705***	-50.313*
	(11.181)	(9.644)	(24.737)
Constant	-40.315	432.843***	208.132
	(87.570)	(38.818)	(125.123)
Socio-demographic controls	Yes	Yes	No
F-test ($\omega_i = 0$)	5.13	41.68	
p-value	(0.006)	(0.000)	
Exp. treatments fixed-effects (Ref= Full inf.)	Yes	Yes	Yes
Statistics			
N	40	22	19
Right censored observations	13	5	4
Pseudo R-square	0.162	0.467	0.098

Notes: ***, **, * denote statistical significance at the 1%, 5% or 10% level, respectively. Robust standard errors in parentheses. Socio-demographic controls include: age, gender (Female = 1) and field of study (Non economics=1). [a]: individual parameter of disadvantageous inequity.[b]: individual parameter of advantageous inequity. See Appendix A.

We observe that the influence of the responder's past behavior is even stronger when we focus on take rates applied by the proposer when he has committed himself. Specification 2 pertains to committed proposers and includes all aforementioned variables. From the results reported in Column 2, we note that partners who have committed them-

selves suffer from higher take rates, except when a commitment was made under the tempting option. In this latter case, the take rate applied is significantly lower. This finding suggests that when partners have exerted an effort by committing themselves under the tempting option, then proposers claim smaller portions of their partners' endowments. Conversely, proposers who have exerted an effort by committing themselves under temptation claim significantly larger portions of their partners' endowments. A new result regarding behavior under temptation emerges: proposers who have succumbed to temptation are more eager to take from responders, whereas responders who have succumbed to temptation always suffer from a higher take rate.

We now deepen the analysis by exploring the effects of commitment patterns (i.e., the level of objectives and the level of penalties set) of both players. Consequently, the sample is reduced to committed proposers associated with committed responders. The results are reported in Column 3. We first note that proposers who have strongly committed themselves with high objectives and high penalties claim lower shares of their partners' endowments. This result corroborates the negative impact of the frequency of the proposer's commitment (Columns 1 and 2). However, this result weakens our previous conclusions, according to which proposers who have expanded their effort by committing themselves under temptation claim significantly larger portions of their partners' endowments. Regarding partners' behavior, those who have set high penalties benefit from lower take rates. An explanation relates to the fact that proposers consider only the credible component of commitment (i.e., penalties) as well as the effort exerted by committing under temptation.

Overall, individual parameter estimates of inequity aversion appear to be predictors of the take rate applied: proposers who express a strong sensitivity to disadvantageous inequity choose high take rates to potentially reduce the gap in payoffs, whereas those who are averse to advantageous inequity apply low take rates to avoid increasing the gap.

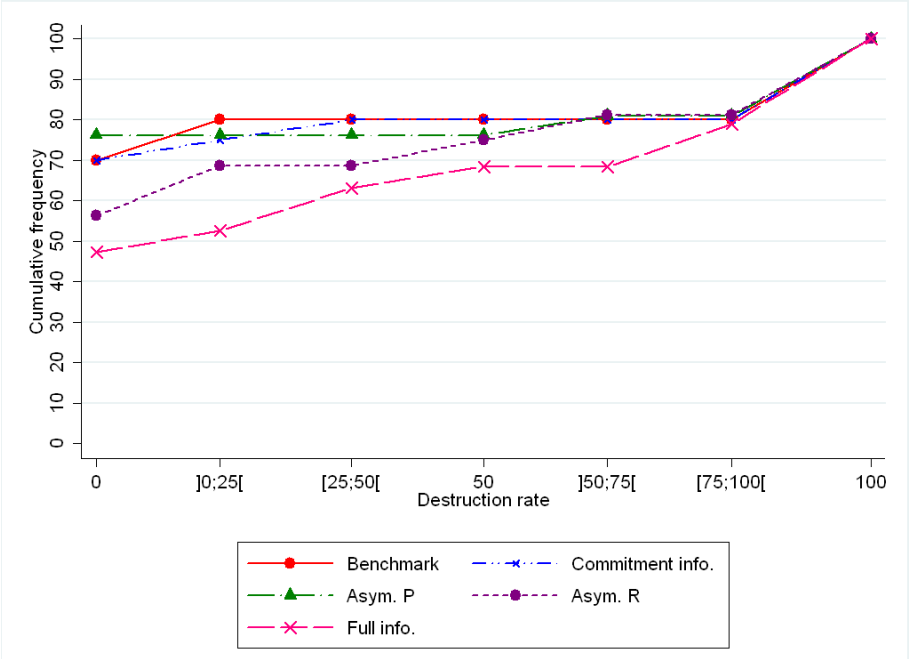
Result 3. *Committed proposers choose lower take rates, except those who have also given in to temptation and those who have committed themselves under temptation. The past behavior of responders matters: those who have given in to temptation are punished by high take rates, whereas those paired with committed proposers benefit from lower take rates when they have committed themselves under temptation or when they have set high penalties.*

4.2.2 Destruction rates

A final important question is whether supplying information about a partner's past behavior affects decisions regarding destruction rates. Our results are in line with those obtained in the previous literature in two ways. First, responders on average destroy 25.31% of their endowments. Second, a clear behavioral dichotomy is observed in the de-

struction rates of responders: 64.58% of responders destroy nothing and 19.79% destroy everything. But an important difference between our findings and those of the existing literature is evident: we find lower null destruction rates than those reported by Bosman, Sutter, and van Winden (2005) in their effort condition (64.58% vs. 79%). An explanation for this finding may relate to the commitment decisions and behavior under temptation exhibited by both players. As seen in Table 4, full knowledge by both players of each other's behavior leads to the highest destruction rates. This finding is even more striking in Fig.3, which depicts the cumulative frequency of destruction rates per experimental treatment. We observe that until destruction rates are lower than 100%, the cumulative distribution in the Full Information treatment is below that of all other distributions. This finding suggests that in the Full Information treatment, when both players have complete information about the commitment decisions and behaviors under temptation of the other player, a smaller share of responders destroy small parts of their endowment than in other experimental treatments, and conversely, a larger share destroy their full endowment. This difference in the distribution of destruction rates is corroborated by a Kolmogorov-Smirnov test ($D = 0.7143$, $p = 0.031$). This finding offers a first insight into our research question.

Figure 3: Cumulative frequency of destruction rates by experimental treatment



To explore the effects of past individual behaviors in greater depth, we estimate a double-censored Tobit regression model to consider both lower (i.e., 0%) and upper (i.e., 100%) limits of the destruction rate. The regressors are the same as those used in the econometric analysis of the take rate, but we now add the take rate chosen by proposer.

We pool data from experimental treatments in which full information about partner's past behavior is provided (i.e., the Asym. R and Full Information treatments). The results, reported in Column 1 of Table 6, pertain to the entire sample of the two treatments considered. As usually observed in the literature, we note a positive and significant influence of take rates on destruction rates. Our findings strongly suggest that, after controlling for this factor, both the commitment decision and behavior under temptation affect destruction rates. Responders who have committed themselves (indicating high effort) choose significantly higher destruction rates, whereas those who have succumbed to temptation choose lower destruction rates. This finding accords with Conjecture 3.a. The following reasoning may explain this result: by committing frequently, responders may feel they merit to keep their endowment. As a consequence, they may experience negative emotions following a proposer's choice of a take rate and thus may be more likely to destroy a large part of their endowment to prevent the proposer from taking a large share. Such negative emotions disappear when the responder himself has given in to temptation.

Table 6: Tobit estimates for destruction rate

Dep. variable	Destruction rate		
	Full sample (1)	Subsample (2)	Subsample (3)
Take rate	3.296***	2.371	1.125*
	(0.680)	(1.440)	(0.505)
Number of proposer's commitment	-32.231**	-108.216**	
	(14.632)	(38.786)	
Objectives set by proposer			0.099
			(0.068)
Penalties set by proposer			-3.457*
			(1.631)
Proposer gives in to temptation=1	58.464***	55.372	
	(19.038)	(41.247)	
Number of responder's commitment	43.235**	165.465**	
	(18.637)	(55.437)	
Objectives set by responder			0.287
			(0.149)
Penalties set by responder			-1.399**
			(0.516)
Responder gives in to temptation=1	-168.708*	-155.990*	
	(87.170)	(77.475)	
Responder's endowment	0.028**	-0.026	-0.087*
	(0.013)	(0.031)	(0.039)
Endowments difference	0.010	0.043	0.020
	(0.011)	(0.023)	(0.016)
Alpha[a]	17.462	-28.424	77.102*
	(11.773)	(29.551)	(31.721)
Beta[b]	57.041	-4.769	498.262*
	(42.337)	(140.719)	(204.008)
Constant	-544.600**	-163.556	52.265
	(213.928)	(132.980)	(92.341)
Socio-demographic controls	Yes	No	No
F-test ($\omega_i = 0$)	3.61		
p-value	(0.030)		
Exp. treatments fixed-effects (Ref= Full inf.)	Yes	Yes	Yes
Statistics			
N	35	17	14
Left censored observations	18	8	7
Right censored observations	7	3	2
Pseudo R-square	0.268	0.229	0.283

Notes: ***, **, * denote statistical significance at the 1%, 5% or 10% level, respectively. Robust standard errors in parentheses. Socio-demographic controls include: age, gender (Female = 1) and field of study (Non economics=1). [a]: individual parameter of disadvantageous inequity.[b]: individual parameter of advantageous inequity. See Appendix A.

As expected, the opposite result for the partner's behavior is observed: responders appear to be more forgiving towards committed partners, whereas they apply higher destruction rates when their partners have succumbed to temptation. Thus, responders prefer to destroy large portions of their endowment to prevent those proposers who have given in to temptation from taking significant shares of the endowments they feel they have earned through their individual efforts. This result accords with Conjecture 3.b.

Most of these findings are robust to whether we consider the entire sample or only committed players B (i.e., players B who have committed themselves in one or two stages of the real-effort task; see Column 2, Table 6). To conclude, we examine the relationship between partners' commitment decisions and the destruction rates applied by committed responders (Column 3). We observe that only penalties set by each player significantly

affect the destruction rates applied: responders who have strongly committed themselves are more forgiving, and the destruction rates chosen are also lower when partners have set high penalties for failure to perform the real-effort task.

These results suggest that responders are less severe towards partners who have committed themselves a high number of times and those who have set large penalties for themselves.

Result 4. *The partner's past behavior impacts the destruction rate decision: responders destroy a smaller share of their endowments when partners have committed themselves a high number of times, especially when they have set high penalties for themselves. Conversely, responders are more likely to destroy large shares of their endowments when their partners have succumbed to temptation. Consequently, the full information setting leads to the highest destruction rates.*

5 Discussion

Our study provides innovative experimental evidence regarding the relationship between individual self-control and peer pressure. Specifically, we conducted a laboratory experiment in which participants first undertook a boring real-effort task under condition of temptation and then took part in a power-to-take game in which available information about the behaviors in the first part of the experiment of opposing players varied across experimental treatments. Because endowments at stake in the power-to-take game were heterogeneous (as they were derived from individuals' performances in the real-effort task), subjects also participated in a second experiment, enabling us to control for individual inequity aversion.

One objective of the paper has been to examine whether individuals, in the face of temptation, are willing to make costly commitments if they know that such commitments will become known to peers. More precisely, we sought to explore the effect of future revelations of current behavior on commitment and temptation. A second objective has been to analyze whether past individual behaviors play a role in bargaining decisions.

The results show that revelation of present behavior to a future partner only increases the penalty individuals are willing to assess themselves for failure to achieve a stated objective and an aspect of commitment associated with credibility, i.e., the interaction between established objectives and the penalty for failure to meet the objectives set. In this case, commitment is credible only if participants match their chosen objectives with sufficiently high self-penalties. Conversely, providing information about commitment behavior does not affect either a subject's propensity to commit or the level of objectives that the individual sets for himself. Second, future revelation of present behavior under temptation does not help subjects avoid succumbing to temptation but does affect their commitment behavior: the level of objectives set and the performance achieved by committed subjects increase. Achieved performance may be regarded as a means for subjects

to demonstrate to their partner that they have exerted sufficiently high effort, even if they succumb to the temptation to surf the Internet.

Our findings also highlight that past individual behavior plays a role in bargaining. Examining the determinants of the choice made by the proposer, we observe that their partner's past behavior comes into play in the take rate decision: (i) proposers are less aggressive towards responders who have committed themselves to a high number of times, and (ii) proposers who committed themselves in the first part of the experiment apply low take rates when partners have committed themselves under temptation or have set high penalties for themselves. Conversely, proposers tend to take more from their partners when the latter have given in to temptation. The take rate in the latter case appears to be a means of punishing responders who have succumbed to temptation. Similar results are found for the responder, as information about both commitment and temptation plays a role: the destruction rate applied is lower towards partners who have committed themselves, especially towards those who have set high penalties for themselves. On the other hand, responders are more punitive towards partners who have given in to temptation and punish them by applying high destruction rates.

From our results, it is evident that individuals might view their own past commitment and effort behavior as relevant in subsequent bargaining processes, revealing fairness concerns. Individuals who choose high levels of commitment tend to choose high destruction rates, likely finding it unfair that they would be not rewarded for their own commitment and effort. Emotional responses are likely to be observed in this scenario, as participants may experience frustration in this situation. Such an interpretation is consistent with Loewenstein (1996), who suggests that selfish behavior may arise from visceral urges or drive-states - i.e., "hot" preferences - and conflict with prosocial behavior, which is related to more abstract and "cold" preferences (see e.g., Loewenstein and O'Donoghue, 2007).

Moreover, recent neurological evidence demonstrates that brain regions pertaining to our rational self (prefrontal cortex) are more active when we consider issues of fairness, cooperation and trust (i.e., Lieberman, 2010).

More generally, as shown by Battaglini, Benabou, and Tirole (2005), informational spillovers are an important aspect of peer interactions, particularly when individuals face self-control issues. Our experiment attempts to shed light on the social aspect of compulsive behavior, showing clearly that individuals use peer pressure to address issues of temptation. However, living in plain sight of others enhances a sense of fairness, as individuals feel that strong commitment should be rewarded by peers. Such feelings may even exacerbate conflicts over resource appropriation. The question is therefore whether the positive effect of peer pressure as a tool for monitoring oneself overwhelms the negative effect of stronger conflicts among peers who are being observed by others.

As a result, our experiment provides a first insight into how peer pressure may affect

self-control. Nonetheless, due to the relevance of the relationship between peer pressure and self-control, more experimental work is needed to explore this issue in greater depth.

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Appendix

A. Experiment 2: Inequity aversion

A.1. Elicitation

After the first experiment, we conducted a second experiment aimed at estimating the individual parameters of inequity aversion, following Fehr and Schmidt (1999)'s model. This model assumes that the utility of player i may be written as:

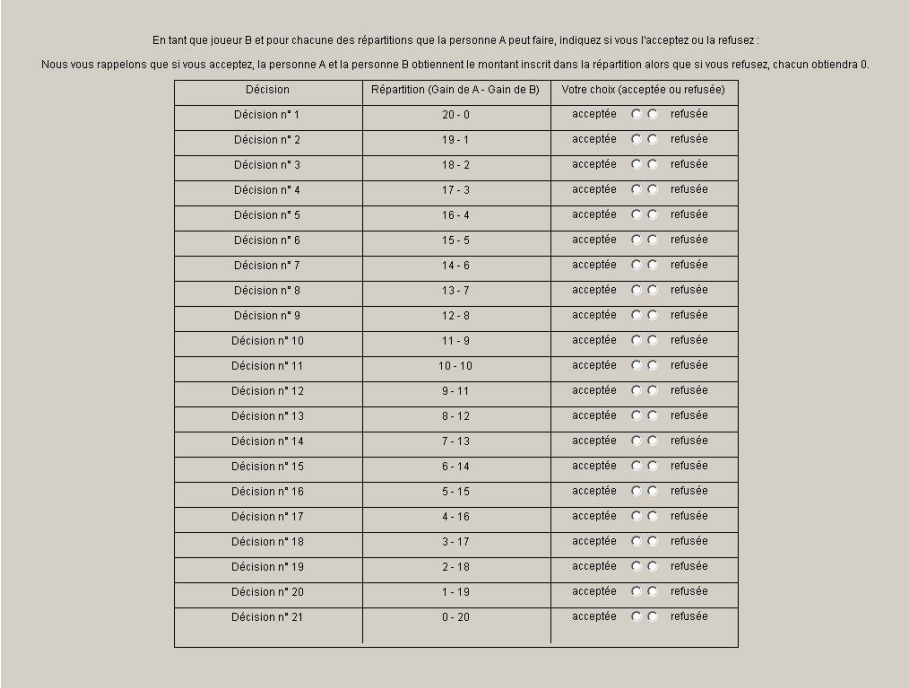
$$U_i = x_i - \alpha_i \max\left(x_j - x_i, 0\right) - \beta_i \max\left(x_i - x_j, 0\right) \tag{2}$$

where x_i is the monetary payoff of player i, x_j is the monetary payoff of player j, α_i is the parameter for disadvantageous inequity of player i and β_i is the parameter for advantageous inequity of player i. It is assumed that $\alpha_i \geq \beta_i$.

We follow the procedure of Blanco, Engelmann, and Normann (2011), whereby subjects make decisions in two different games: an ultimatum game using the strategy method and a modified dictator game. In each game, subjects do not learn their role (for example, proposer or responder in the ultimatum game) until the end of the game.

More precisely, the ultimatum game is used to elicit the individual parameter of disadvantageous inequity, α_i. In this game, the proposer must divide 20 points between himself and the responder. Next, the responder must decide whether to accept or reject the proposition. In our experiment, all subjects decide first as a proposer and second as a responder. To avoid any feedback and to elicit the complete strategy of responders, we use the strategy method; that is, responders must decide whether to accept or reject any of the 21 possible distributions (ranging from $(20, 0)$ to $(0, 20)$; see Fig. 4). The estimation of α_i is obtained through the decisions of the responder i and corresponds to the switch point between rejecting and accepting the distribution.

Figure 4: Table for responder's choices in the ultimatum game



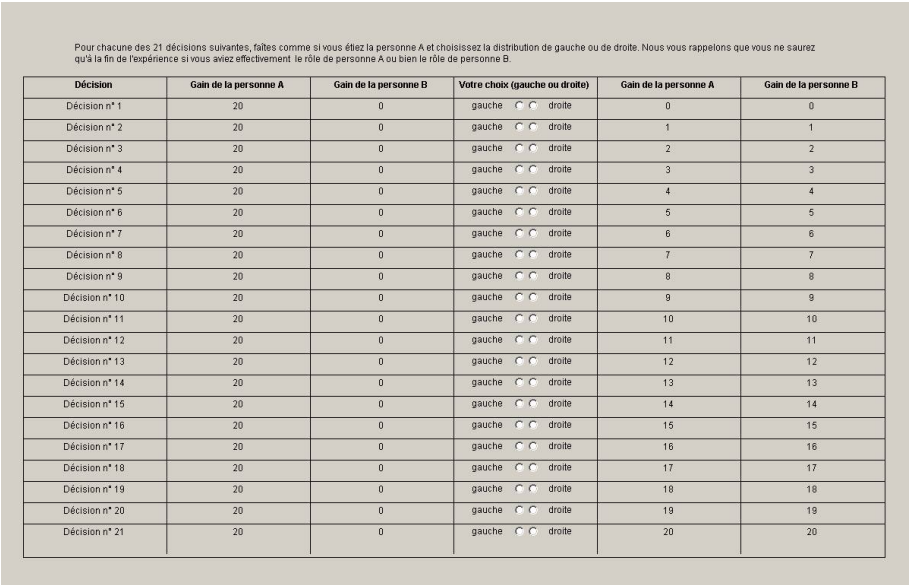
En tant que joueur B et pour chacune des répartitions que la personne A peut faire, indiquez si vous l'acceptez ou la refusez :

Nous vous rappelons que si vous acceptez, la personne A et la personne B obtiennent le montant inscrit dans la répartition alors que si vous refusez, chacun obtiendra 0.

Décision	Répartition (Gain de A - Gain de B)	Votre choix (acceptée ou refusée)
Décision n° 1	20 - 0	acceptée ○ ○ refusée
Décision n° 2	19 - 1	acceptée ○ ○ refusée
Décision n° 3	18 - 2	acceptée ○ ○ refusée
Décision n° 4	17 - 3	acceptée ○ ○ refusée
Décision n° 5	16 - 4	acceptée ○ ○ refusée
Décision n° 6	15 - 5	acceptée ○ ○ refusée
Décision n° 7	14 - 6	acceptée ○ ○ refusée
Décision n° 8	13 - 7	acceptée ○ ○ refusée
Décision n° 9	12 - 8	acceptée ○ ○ refusée
Décision n° 10	11 - 9	acceptée ○ ○ refusée
Décision n° 11	10 - 10	acceptée ○ ○ refusée
Décision n° 12	9 - 11	acceptée ○ ○ refusée
Décision n° 13	8 - 12	acceptée ○ ○ refusée
Décision n° 14	7 - 13	acceptée ○ ○ refusée
Décision n° 15	6 - 14	acceptée ○ ○ refusée
Décision n° 16	5 - 15	acceptée ○ ○ refusée
Décision n° 17	4 - 16	acceptée ○ ○ refusée
Décision n° 18	3 - 17	acceptée ○ ○ refusée
Décision n° 19	2 - 18	acceptée ○ ○ refusée
Décision n° 20	1 - 19	acceptée ○ ○ refusée
Décision n° 21	0 - 20	acceptée ○ ○ refusée

Regarding advantageous inequity, we use the modified version of the dictator game in which subjects must make decisions as a proposer by choosing between two distributions - a non-egalitarian one $(20, 0)$ and an egalitarian one (x_i, x_i), for 21 possibilities (ranging from $(0, 0)$ to $(20, 20)$; see Fig. 5). The estimate of the advantageous inequity parameter, β_i, corresponds to the switch point from the unfair distribution $(20, 0)$ to the egalitarian one (x_i, x_i).

Figure 5: Table for the modified dictator game



Pour chacune des 21 décisions suivantes, faites comme si vous étiez la personne A et choisissez la distribution de gauche ou de droite. Nous vous rappelons que vous ne saurez qu'à la fin de l'expérience si vous aviez effectivement le rôle de personne A ou bien le rôle de personne B.

Décision	Gain de la personne A	Gain de la personne B	Votre choix (gauche ou droite)	Gain de la personne A	Gain de la personne B
Décision n° 1	20	0	gauche ○ ○ droite	0	0
Décision n° 2	20	0	gauche ○ ○ droite	1	1
Décision n° 3	20	0	gauche ○ ○ droite	2	2
Décision n° 4	20	0	gauche ○ ○ droite	3	3
Décision n° 5	20	0	gauche ○ ○ droite	4	4
Décision n° 6	20	0	gauche ○ ○ droite	5	5
Décision n° 7	20	0	gauche ○ ○ droite	6	6
Décision n° 8	20	0	gauche ○ ○ droite	7	7
Décision n° 9	20	0	gauche ○ ○ droite	8	8
Décision n° 10	20	0	gauche ○ ○ droite	9	9
Décision n° 11	20	0	gauche ○ ○ droite	10	10
Décision n° 12	20	0	gauche ○ ○ droite	11	11
Décision n° 13	20	0	gauche ○ ○ droite	12	12
Décision n° 14	20	0	gauche ○ ○ droite	13	13
Décision n° 15	20	0	gauche ○ ○ droite	14	14
Décision n° 16	20	0	gauche ○ ○ droite	15	15
Décision n° 17	20	0	gauche ○ ○ droite	16	16
Décision n° 18	20	0	gauche ○ ○ droite	17	17
Décision n° 19	20	0	gauche ○ ○ droite	18	18
Décision n° 20	20	0	gauche ○ ○ droite	19	19
Décision n° 21	20	0	gauche ○ ○ droite	20	20

To avoid any order effects, in half of the experimental sessions, the ultimatum game is played before the modified dictator game, and we reverse the order in the other half. We apply this setting to each experimental treatment. Moreover, subjects know that they will be paired with a different participant in these two games, a participant who is also different from their partner in the power-to-take game, to rule out reputation and retaliation (or acknowledgment) effects.

A.2. Results

Decisions made in the two games enable us to select subjects with consistent preferences; that is, subjects at some point switch (if they switch at all) from choosing the left column to choosing the right column, but they do not switch back. Overall, out of 192 participants, 158 (82.29%) behaved consistently in the two scenarios. This result is in line with those of Blanco, Engelmann, and Normann (2011) who find values of 84.72%. In Table 7, we summarize the distribution of the advantageous and disadvantageous inequity parameters.

Table 7: Distribution of alpha and beta paramaters

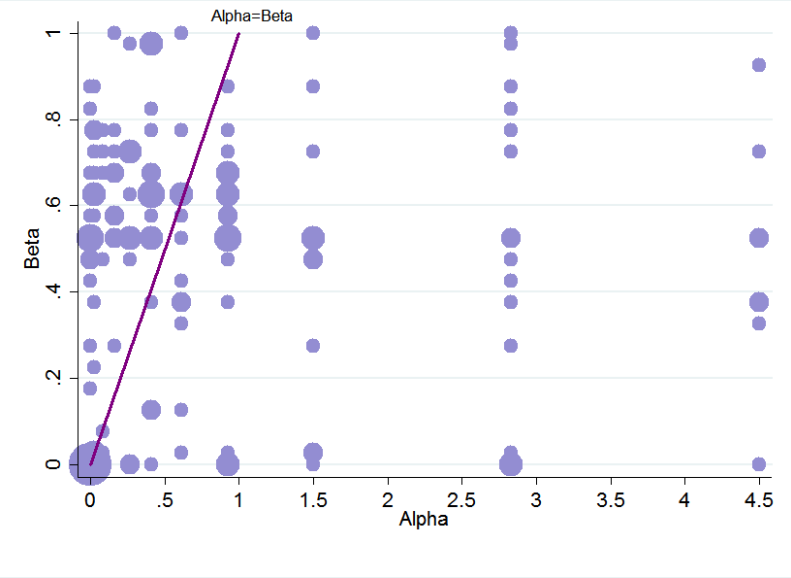
α	F&S	Blanco et al.	Data	β	F&S	Blanco et al.	Data
$\alpha < 0.4$	30%	31%	43.67%	$\beta < 0.235$	30%	29%	24.05%
$0.4 \leq \alpha < 0.92$	30%	33%	20.26%	$0.235 \leq \beta < 0.5$	30%	15%	16.46%
$0.92 \leq \alpha < 4.5$	30%	23%	31.01%	$0.5 \leq \beta$	40%	56%	59.49%
$4.5 \leq \alpha$	10%	13%	5.06%				

As our experiment replicates the one of Blanco, Engelmann, and Normann (2011) in eliciting individual parameters of inequity aversion, it is interesting to compare our results with those obtained by Blanco, Engelmann, and Normann (2011) and with the theoretical distribution assumed by Fehr and Schmidt (1999). Regarding disadvantageous inequity aversion, a Chi-square goodness-of-fit test indicates significant differences between our distribution and those of Fehr and Schmidt (1999) ($\chi^2 = 18.751, p = 0.000$) and Blanco, Engelmann, and Normann (2011) ($\chi^2 = 28.029, p = 0.000$). More precisely, we observe a greater proportion of subjects with a weak aversion toward disadvantageous inequity and a lower proportion of highly inequity-averse subjects. Regarding advantageous inequity, our distribution differs significantly from that of Fehr and Schmidt (1999) ($\chi^2 = 26.536, p = 0.000$), but the difference between our results and those of Blanco, Engelmann, and Normann (2011) is not statistically significant ($\chi^2 = 1.902, p = 0.386$).

Finally, the implementation of the two scenarios allows us to determine the joint distribution of the α and β parameters. Fig. 6 depicts both individual parameters, which are found to be widely distributed in our subject pool, a finding indicative of the highly heterogeneous subject pool used in our experiment. Similarly to Blanco, Engelmann, and Normann (2011), we reject the assumption of a positive correlation between α_i and β_i

(Spearman rank correlation coefficient, $\rho = 0.1002$, $p = 0.210$ and Blanco, Engelmann, and Normann (2011) find $\rho = -0.03$, $p = 0.820$). Moreover, only 49.36% of subjects' decisions are consistent with the hypothesis that $\alpha_i \geq \beta_i$, while there are 62.29% of subjects in the study of Blanco, Engelmann, and Normann (2011). The corresponding data points lie above the $\alpha = \beta$ line in Fig. 6.

Figure 6: Distribution of inequity aversion parameters for consistent choices



Result. *Subjects exhibit various degrees of inequity aversion and disprove two assumptions of Fehr and Schmidt (1999): the positive relationship between disadvantageous and advantageous inequities aversion and* $\alpha_i \geq \beta_i$.

B. Experimental instructions for experiment 1 with the Full Information treatment and tempting option in the first stage (translated from French)

Welcome. You are participating in an experiment financed by the National Agency for Research. If you read these instructions carefully, you may earn a significant sum of money. The amount of your earnings depends not only on your decisions but also on the decisions of other participants with whom you will interact. It is important that you do not talk to any of the other participants until the experiment is over. If you have a question at any time, please raise your hand and a monitor will come to your desk to answer it in private. If you do not respect this rule, we will be forced to terminate the experiment, and you will be not paid.

This experiment consists of two distinct experiments. Here are the instructions for the first experiment. This first experiment is composed of three stages:

- In the first stage, you will be left alone to undertake an effortful task. Each task performed will allow you to accumulate experimental points. A higher number

of accomplished tasks will result in your having a higher number of experimental points. You will be notified of the number of points you have earned at the end of this stage of the experiment.

- The second stage will be identical to the first one, except for one difference, which is explained in the next part of the instructions.
- The third stage follows stages 1 and 2. Groups of two people will be randomly formed. You will then collectively determine the payoffs that each member of the group will receive. This amount will be based on the experimental points that each member of the group has accumulated in the first two stages.

The second experiment is independent of the first one. You will be paired with another participant who is different from your partner in the first experiment. You will have to indicate, between two distributions of experimental points between you and your partner, the one that you prefer. This second experiment will consist of two scenarios.

Your earnings at the end of the experiment will be equal to one of the following:

- Your earnings at the end of the experiment 1
- Your earnings at the end of scenario 1 of experiment 2
- Your earnings at the end of scenario 2 of experiment 2

to which a show-up fee of 5 Euros will be added. The experiment (and scenario if it is experiment 2) remunerated will be randomly chosen by the computer at the end of the experiment and will be the same for all participants in this session.

From now, we will provide you details of experiment 1

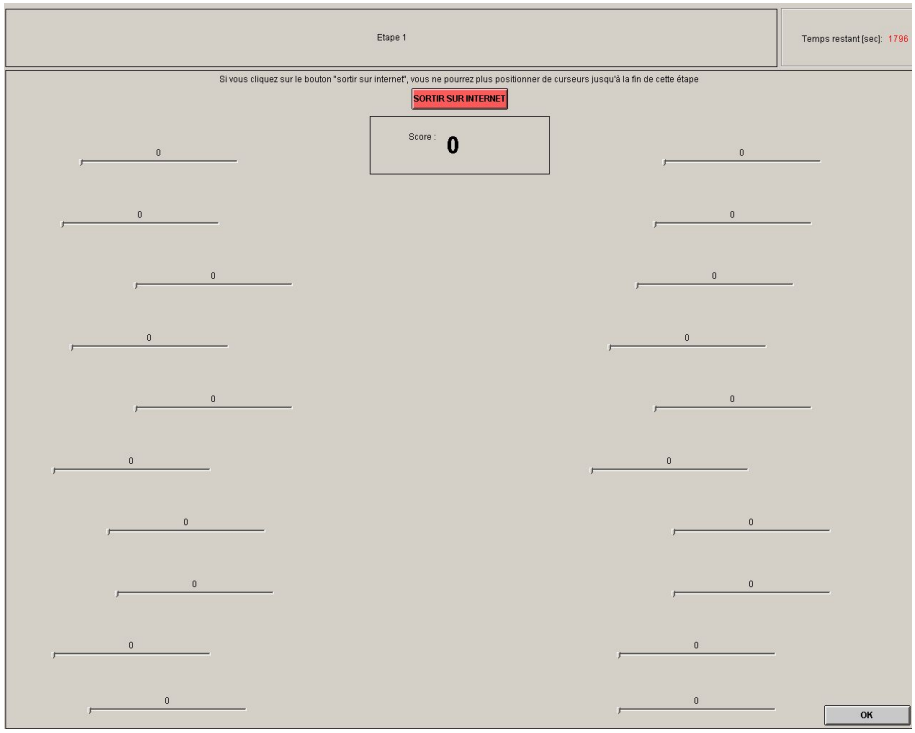
If experiment 1 is randomly selected for payment, the experimental points you have earned in this experiment will be paid to you in Euros according to the following exchange rate: 150 points = 1 Euro.

In the first stage, you will undertake the same task for 30 minutes. A higher number of accomplished tasks will result in a higher number of accumulated experimental points. In this stage, you will be presented with a computer screen with 20 sliders. Each slider is initially positioned at 0 and may be moved as far as 100. Each slider has a number above the line showing its current position. You may readjust the position of each slider as many times as you want. Your total number experimental points accumulated during this task will be the number of sliders positioned at exactly 50 at the end of 30 minutes.

The following figure (Fig. 7) represents a screen with 20 sliders. Once all sliders have been correctly positioned, you must validate the screen by clicking the "ok" button at the bottom right. Once the screen has been validated, the computer will indicate to you the

number of correctly positioned sliders, and an identical screen will appear. In addition, at the middle top of the screen, a red button is displayed. This button allows you to exit the screen with sliders and surf the Internet.

Figure 7: Computer screen for the first stage with temptation



You can choose to surf the Internet at any moment and visit any website you wish, but if you leave the screen with the sliders, you will be unable to return to this screen until the end of the 30 minute session. Your earnings for this first stage, in experimental points, will be the number of sliders you have correctly positioned before exiting to surf the Internet.

- For each correctly positioned slider, you will earn 5 points.
- For each incorrectly positioned slider, you will earn 0 points.

Overall, you will have 30 minutes to complete as many tasks as possible. If, at the end of the 30 minutes allocated to you, you have correctly positioned only a fraction of the 20 sliders on the screen, the correctly positioned sliders will automatically be validated and will be counted in your performance.

In addition, at the beginning of the first stage, you will have an opportunity to indicate the number of sliders you commit yourself to correctly positioning. You can also choose not to commit yourself to meeting any specific objective.

If you decide to commit yourself, you indicate the following:

- The number of sliders you intend to position correctly (this is called your commitment).
- The reduction rate of your earnings, which will be applied in case of failure. This reduction rate must be an integer from 0% to 100% inclusive.

The screen will be as follows (Fig. 8):

Figure 8: Computer screen for commitment decisions

To help you in your assessment of the number of sliders you can correctly position in 30 minutes, there will be a practice round lasting two minutes before the start of the experiment. This will allow you to learn the number of sliders you can correctly position in two minutes.

Examples

Assume that you commit yourself to correctly positioning 10 sliders and choose a reduction rate of 10%.

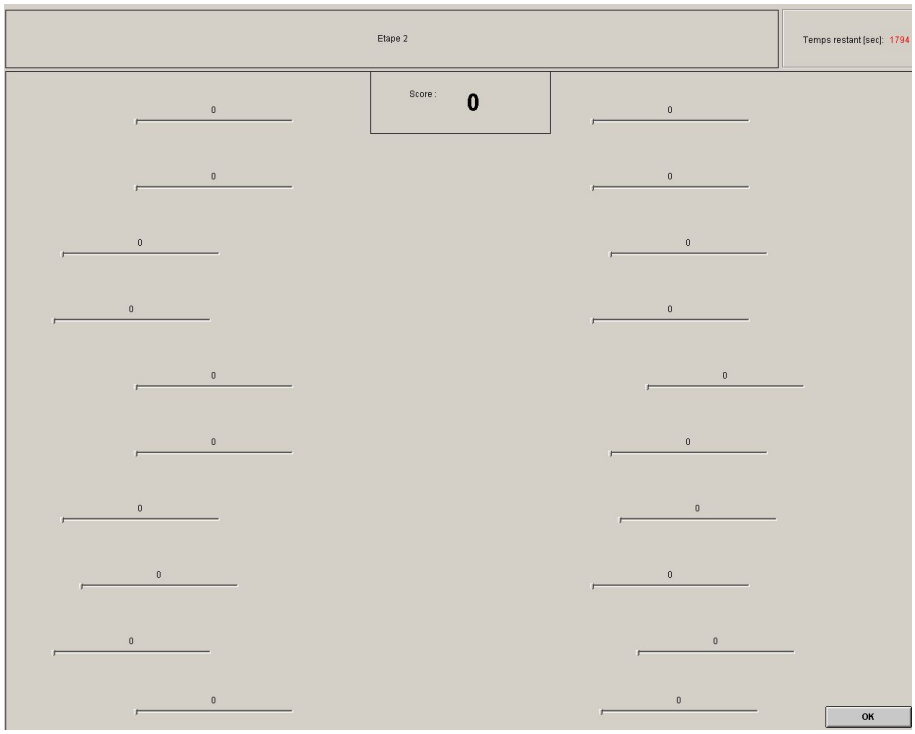
- If you have correctly positioned only 9 sliders, your objective is not met at the end of the first stage, and the reduction rate is applied. The 9 sliders provide you $9 \times 5 = 45$ points. However, because your objective has not been reached, the reduction rate of 10% is applied to the 45 points, leading to a reduction of 4.5 points. Therefore, at the end of the first stage, you will have earned $45 - 4.5 = 40.5$ points.

- Conversely, if you have exceeded your objectives by correctly positioning 15 sliders, you have reached your objectives, and the reduction rate will not apply. Your earnings at the end of the first stage will be equal to $15 \times 5 = 75$ points.

At the end of the first stage, you will learn the number of sliders that you have correctly positioned, whether you have met your objectives (in the case of commitment) and the number of experimental points obtained. Next, the second stage will begin.

The second stage is nearly identical to the first (length 30 minutes, opportunity of commitment) but with one difference. As the figure below indicates (Fig. 9), you will not have the opportunity to surf the Internet.

Figure 9: Computer screen for the second stage without temptation



At the end of this second stage, you will learn the number of sliders you have correctly positioned, whether you have met your objectives (in the case of commitment) and the number of experimental points obtained in this stage. Next, the third stage will begin. At the beginning of the third stage, the experimental points earned by each subject in the first two stages will be added up.

Each participant now has at his disposal the number of experimental points he has accumulated in the first two stages. The third stage consists of two phases. In phase 1, only player A makes a decision, whereas in phase 2, only player B makes a decision. Therefore, each participant makes one decision. You will learn your role through a message displayed on your computer screen. If you are player A, a screen with some information

will appear, whereas if you are player B, a message will appear asking you to wait while your partner makes his decision.

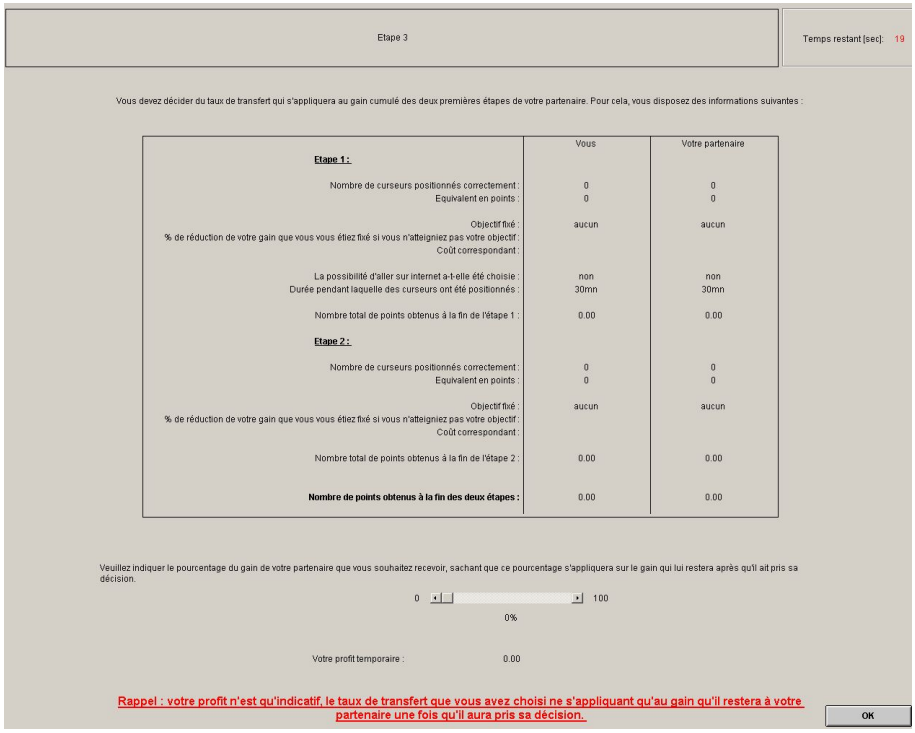
Before both players (A and B) make their decisions, the computer screen will provide the following information:

- The number of experimental points each player in the group earned in stage 1 and stage 2 and the total sum of points earned
- The commitments, if any, made by each player in the group, and if commitments were made, the level of objectives and penalties set and whether players were successful in reaching their objectives
- The moment at which either player, if any, stopped working on the task to surf the Internet.

This information is shown throughout the third stage regardless of the role (A or B) of the player. The decisions to be made are as follows:

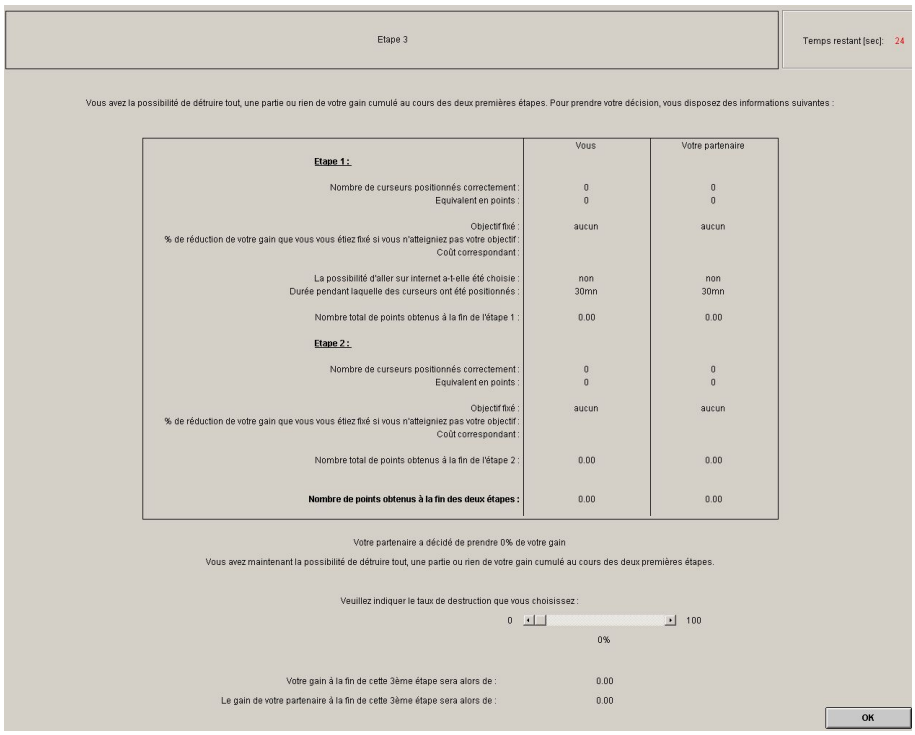
In phase 1, each player A is paired with a player B. Each player A must choose a percentage, called the take rate. This percentage determines how much of player B's endowment, accumulated by player B in the first two stages, will be transferred to player A. The percentage chosen by player A must be an integer in the interval $[0, 100]$. Player A will indicate the chosen percentage on the following computer screen (Fig. 10):

Figure 10: Computer screen for Player A (proposer)'s decisions in the power-to-take game



In phase 2, player B of each group makes a decision. Player B learns the decision that player A has made. Next, player B chooses the percentage of his own endowment that will be destroyed called the destruction rate. The percentage chosen by player B must be an integer in the interval $[0, 100]$. The transfer from player B to player A will be based on the endowment of player B that is left. When player B chooses his destruction rate, the number of points that he keeps for himself appears immediately on the screen before validation along with the current number of points player A will earn, as shown in the figure below (Fig. 11). Therefore, player B may change his destruction rate as many times as desired before validating the screen.

Figure 11: Computer screen for Player B (responder)'s decisions in the power-to-take game



Examples:

Example 1: Assume that player A has earned 3,000 points at the end of the first two stages and that player B has earned 2,000 points.

- Phase 1: Player A chooses a take rate of 60%
- Player B may destroy none, all or a part of his endowment. Assume that he chooses a destruction rate of 0%. Then:
  - The transfer from player B to player A is equal to 60% of 2,000 points, yielding 1,200 points

- By the end of the third stage, player A obtains: 3,000 points (his earnings at the end of the first two stages) + 1,200 points (from the transfer) = 4,200 points
- By the end of the third stage, player B obtains: 2,000 points (his earnings at the end of the first two stages) - 1,200 points (transfer to player A) = 800 points

Example 2: Assume that player A has accumulated 3,000 points by the end of the first two stages and that player B has accumulated 2,000 points.

- Phase 1: Player A chooses a take rate of 60%
- Phase 2: Player B may destroy none, all or a part of his endowment. Assume that he chooses a destruction rate of 50%. Then:
  - After destruction, player B has 1,000 points $(2,000 - (50\% \times 2,000) = 1,000)$
  - The take rate that player A has chosen will be applied to the 1,000 points of player B
  - With a take rate equal to 60%, 60% $\times$ 1,000 = 600 points will be transferred to player A
  - At the end of the third stage, player A has 3,000 points (his earnings at the end of the first two stages) + 600 points (from the transfer) = 3,600 points
  - At the end of the third stage, player B has 2,000 points (his earnings at the end of the first two stages) - 1,000 points (destruction) - 600 points (transfer to player A) = 400 points

To help you in the computation, you have at your disposal a calculator, and the computer will indicate the number of experimental points that each player of your group will earn once all decisions have been made.

Instructions regarding experiment 2 will be displayed on the computer screen at the end of this first experiment. Before the experiment starts, you must answer a short questionnaire to verify your understanding of the instructions.

C. Questionnaire for experiment 1 (translated from French)

1. Your earnings at the end of the first two stages depends on the following:
   (a) the performance of another participant only
   (b) your best performance only, either in the first stage or in the second stage
   (c) the sum of your accumulated points totals in the first two stages
2. Assume that in the second stage you have set a commitment equal to 100 correctly positioned sliders and a penalty equal to 10% if you do not reach your commitment. What have you earned at the end of the second stage if you have correctly positioned 200 sliders, knowing that 1 correctly positioned slider earns you 5 points?

(a) 1000 points

(b) 200 points

(c) 900 points

3. Assume that in the second stage you have set a commitment equal to 100 correctly positioned sliders and a penalty equal to 10% if you do not reach your commitment. What have you earned at the end of the second stage if you have correctly positioned 50 sliders, knowing that 1 correctly positioned slider earns you 5 points?

   (a) 250 points

   (b) 225 points

   (c) 0 point

4. In the third stage, assume that player A has an endowment of 4,000 points and that player B has 6,000 points

   - Player A decides that 50% of player B's endowment will be transferred to him (thus, player A chooses a take rate of 50%)
   - Player B chooses a destruction rate of 10%

   What are the earnings (in points) of players A and B at the end of the third stage?

   (a) 4,000 for player A and 6,000 for player B

   (b) 6,700 for player A and 2,700 for player B

   (c) 7,000 for player A and 5,400 for player B

5. In the third stage, assume that player A has 100 points and player B has 50 points

   - Player A decides that 10% of player B's endowment will be transferred to him (thus, player A chooses a take rate of 10%)
   - Player B chooses a destruction rate of 100%

   What are the earnings (in points) of players A and B at the end of the third stage?

   (a) 100 for player A and 0 for player B

   (b) 100 for player A and 50 for player B

   (c) 0 for player A and 0 for player B

6. In the third stage, assume that player A has 1,000 points and player B has 1,000 points

   - Player A decides that 100% of player B's endowment will be transferred to him (thus, player A chooses a take rate of 100%)

- Player B chooses a destruction rate of 0%

What are the earnings (in points) of players A and B at the end of the third stage?

(a) 0 for player A and 0 for player B

(b) 1,000 for player A and 1,000 for player B

(c) 2,000 for player A and 0 for player B

Thank you for your answers. When you are ready, raise your hand to attract our attention.

D. Experimental instructions for experiment 2 (translated from French)

You will take part in a second experiment that is independent of the first experiment. Your decisions in this experiment are fully independent of your previous choices.

This experiment involves two different and independent scenarios. In each scenario, you must make one or several decisions without knowing the decisions that other participants make. The other participants will not know what decisions you make.

Only one of the two scenarios will be used to determine your payment, if this experiment is randomly selected for payment by the computer. Each scenario has the same chance of being randomly selected at the end of the experiment, and the same scenario will be chosen for all participants. For this second experiment, the exchange rate used is 5 points = 1 Euro.

We now describe the first scenario.

SCENARIO 1

Groups of two individuals (player A and player B) are randomly formed. These groups are different from those formed during experiment 1. Player A must choose between two displayed distributions of payoffs to player A and player B. A total of 21 situations are presented. The roles of player A and player B are randomly assigned at the end of the experiment. If you are selected as player A, you must choose one of the two distributions presented for each of 21 situations (these situations will be displayed on your screen).

Example:

Player A's payoff	Player B's payoff	Decision	Player A's payoff	Player B's payoff
20	0	Left Right	5	5

Assume that during the experiment, you have had to choose between the left-hand distribution and the right-hand distribution:

- If you choose the left-hand distribution, player A will receive 20 points, and player B will receive 0 points.
- Alternatively, if you choose the right-hand distribution, both player A and player B will receive 5 points.

If this scenario is selected at the end of experiment 2 for payment, one of the 21 decisions will be randomly selected for payment. The chosen distribution will dictate the payoffs to player A and player B. Moreover, the computer will assign your role randomly, but you will learn your role only at the end of the scenario. Consequently, you will make your choices as player A, but it is possible that the computer will assign you the role of player B at the end of the scenario. In this case, you will receive the payoff bound to player B.

Subjects take part in scenario 1 before the instructions pertaining to scenario 2 are displayed on their screen. See Fig. 4 in Appendix A1 for computer screen of decisions in the ultimatum game.

SCENARIO 2

Groups of two individuals (player A and player B) are randomly formed. These groups are different from those formed in experiment 1 or the previous scenario. In this scenario:

- Player A must divide his endowment of 20 points between himself and player B.
- Player B can choose whether to accept or reject the division:
  - If player B accepts the division, both individuals receive the proposed amount.
  - If player B rejects the division, both individuals receive a null payoff.

Be careful: the computer will randomly assign you the role of player A or player B at the end of the scenario. Consequently, you will make your decisions both as player A (to choose a division) and as player B (to decide for each of the 21 possible situations whether to accept or reject the proposed division).

If this scenario is randomly selected for payment, the computer will select the offer that player A has made and will determine whether player B has accepted or rejected the division.

- If you learn that you are player A, you will receive the amount you have chosen, if player B has accepted the division. Otherwise, you will receive nothing.
- If you learn that you are player B, you will receive the amount chosen by player A if you have accepted the division; if you have rejected it, you will receive nothing.

Example:

- As player A, from your 20 points, how many points will you offer to player B? Assume that player A proposes 9 points to player B and keeps 11 points for himself.
- As player B, indicate whether you accept or reject each of the following divisions.

If we take the division (11,9) :

Division		Your choice	
A	B	Accept	Reject
11	9		

- If player B checks "Accept": player A obtains 11 points, and player B obtains 9 points.
- If player B checks "Reject": both player A and player B obtain 0 points.

See Fig. 5 in Appendix A1 for computer screen of decisions in the modified dictator game.