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## **Positional enhancement in effort-based social comparisons**

Jeroen Nieboer\*

Salient social comparisons to peers are generally thought to increase people's productive effort. But social comparisons can also become ends in themselves, with individuals seeking to outrank others by costly, non-productive means. This paper explores the motives behind such tendencies in the context of a real-effort task. We offer subjects the option of enhancing their relative position, at a cost to themselves, in a social comparison based on points earned on the task. Despite receiving no tangible benefits from enhancing their position, at least half of our subjects sacrifice some of their experimental earnings to do so. We find that information conditions are crucial: expenditure on positional enhancement is twice as high when social comparisons only occur after enhancement. Surprisingly, this effect is not driven by the visibility of subjects' positional enhancement to peers; the increase is due to ex ante uncertainty over how one's (unenhanced) task output compares to peers. Since many professional settings are characterized by uncertainty over peer output, these findings can help organizations identify positional enhancement and reduce its costs.

**Keywords:** Social comparison; Status; Competition; Experiment.

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## 1. Introduction

When social comparisons on desirable dimensions loom large, people may seek to make some personal sacrifice to improve their position relative to peers. They may, for example, consume conspicuously to signal wealth (Amaldoss and Jain, 2005; Chao and Schor, 1998; Clingingsmith and Sheremeta, 2017; Duesenberry, 1949; Hopkins and Kornienko, 2004; Kuhn et al., 2011; Veblen, 1899), donate conspicuously to charity (Andreoni and Petrie, 2004; Ariely et al., 2009; DellaVigna et al., 2012) or accept lower salaries for jobs that enjoy a higher social status (Besley and Ghatak, 2008; Frank, 1985ab, 2005; Schaubroeck, 1996). Within their professional lives, people may devote time and other resources to activities deemed prestigious by their peers (Greenberg et al., 2007; Piazza and Castellucci, 2014). Principals and managers, aware of the power of such social incentives, may design or reinforce social comparisons to align with organizational objectives (Ashraf et al., 2014; Azmat and Iriberry, 2010; Bandiera et al., 2010; Blanes i Vidal and Nossol, 2011; Booij et al., 2017; Bothner et al., 2011; Cadsby et al., 2019; Ederer and Pataconi, 2010; Gee, 2019).

Individuals may not be aware of how social comparisons affect their preferences in everyday decisions, but evidence from lab experiments shows that many people will consciously incur a cost to ‘beat’ others in contests or rankings (Charness et al., 2014; Price and Sheremeta, 2011). Since victories in these experiments offer no extrinsic or tangible value, the question we ask in this paper is what motivates such sacrifices. Specifically, we investigate the drivers of sacrificing personal resources to boost one’s ranking in effort-based social comparisons – a phenomenon we refer to as *positional enhancement*. In our experimental study, subjects can enhance their position in a comparison with peers by using their experimental wages to artificially increase their publicized output from a real-effort task. We manipulate the information conditions of positional enhancement – specifically, whether subjects can observe what their peers’ output was prior to positional enhancement. Given that

field settings differ vastly in the visibility of inputs into individuals' productive output, we conjecture that information conditions are key to explaining differences between professional settings that lend themselves to social comparisons.

We hypothesize that positional enhancement is higher when it is done out of view of peers, privately and prior to any social comparison. We propose two channels for this increase and we experimentally control for the first channel: social visibility. The second channel is uncertainty about peers' unenhanced output at the time of deciding on enhancement, a factor that has received little attention in prior literature. It is, however, characteristic of many field settings that unobservability of peers' sacrifices (e.g. working overtime) goes hand-in-hand with unobservability of peers' unenhanced output (e.g. output within contractual working hours). This issue has arguably become more pertinent in recent times, as greater complexity and flexibility has made it harder to monitor productive effort in the modern workplace. Perhaps unsurprisingly, many firms now attempt to digitally monitor (proxies of) productivity.

This paper contributes to a growing experimental literature that identifies people's intrinsic valuations of favorable social comparisons by measuring experimental pay-offs forgone (Clingsmith and Sheremeta, 2017; Huberman et al., 2004; Pettit et al., 2010; Price and Sheremeta, 2011; Van den Bos et al., 2008; 2013). In these experiments, subjects that value their relative position end up leaving the lab with less money than their less comparison-sensitive peers, *ceteris paribus* (note this contrasts with the 'pay-off destruction' or 'money burning' literature, in which subjects also sacrifice earnings but with the specific purpose of leaving the lab with more money than their peers – see Sanjaya, 2021). In a seminal study, Huberman et al. (2004) find that subjects in a rent-seeking contest pay more if the contest winner is publicly recognized and applauded by other participants afterwards. Price and Sheremeta (2011) report positive bids in contests with a money price of zero, a result they attribute to a "joy of winning" utility. Van Den Bos et al. (2008; 2013) find that people bid

more in auctions against human than computer opponents. In Charness et al.'s (2014) real-effort task followed by a social comparison, the authors find that subjects are willing to use their task wages both to reduce others' task scores and enhance their own score. Subjects in this study could not see who had paid to reduce others' scores, but they could see who had paid to enhance their own score. The fact that social comparisons could be made on both enhanced *and* unenhanced scores may have reduced subjects' engagement in enhancement; this is the question with which our study concerns itself. And by controlling for social visibility experimentally, we can disentangle it from the effect of uncertainty about peer output.

A key motivation for studying social visibility is that an enhanced ranking may be seen by others as a signal of greater ability. Recent experimental evidence on social comparisons on cognitive and knowledge test scores suggests that we may find such ability signaling – Burks et al. (2013) and Ewers and Zimmermann (2015) find that people readily overstate their test score in public. A close analogue is found in the literature on conspicuous consumption, where spending can be a signal of wealth (Amaldoss and Jain, 2005; Chao and Schor, 1998; Duesenberry, 1949; Hopkins and Kornienko, 2004; Kuhn et al., 2011; Veblen, 1899). Concerns about projecting a positive social image have also been found in other domains, notably charitable giving and other pro-social acts (Andreoni and Bernheim, 2009; Andreoni and Petrie, 2004; Ariely et al., 2009; DellaVigna et al., 2012; Gächter and Fehr, 1999).

The control afforded by laboratory conditions means that experimental evidence can help disentangle different explanations. For example, studies of organizational behavior report that people boost their status in organizations through personal sacrifices for the collective good (Halevy et al., 2008, 2012; Willer, 2009) and helping peers (Bendersky and Shah, 2012; Flynn, 2003; Flynn et al., 2006), but these studies cannot disentangle status-seeking from other motives. In our lab experiment, we can rule out motives for personal sacrifices such as a greater good or (expected) material benefits from an enhanced social standing (Magee and Galinsky,

2008; Merton, 1968). Furthermore, the lab gives us control over social visibility of positional enhancement. This variable is not easily controlled in field settings as people talk to their colleagues and are curious about their relative performance (Alós-Ferrer et al., 2018).

A better understanding of the drivers of positional enhancement will hopefully shed light on real-world behaviors that closely resemble the costly sacrifices in our experiment: people going ‘above and beyond’ in professional settings, at a cost to themselves. A prominent example is presenteeism: working whilst ill, outside contracted hours either on e-mail (Becker et al., 2018) or by putting in ‘face time’ in the office (Elsbach et al., 2010). Recent evidence suggests these behaviors are widespread and costly to employees and, ultimately, employers (CIPD, 2018; Virgin Pulse, 2015). Survey respondents explicitly mention esteem from peers as a driving factor behind presenteeism (Dew et al., 2005; Johns, 2010; Löve et al., 2010; Stewart, 2003). Another example is employees ‘cutting corners’ by compromising their health or workplace safety (Frank, 1985b). Again, survey evidence suggests peer esteem is an important motivating factor (Choudry and Fang, 2008; Zhou et al., 2015). A better understanding of these behaviors will, we hope, lead to ways of mitigating such peer pressure.

We report three key findings. First, we replicate findings that substantial proportions of subjects engage in positional enhancement after an initial social comparison on unenhanced output – around half of our subjects do so. Second, positional enhancement is significantly and substantially higher when not preceded by a social comparison on unenhanced output. This increase is not due to social visibility of enhancement, as we find a similar increase when subjects’ enhancement is made public afterwards. Our third key finding is therefore that the documented difference is due to uncertainty about subjects’ task output relative to peers.

The rest of the paper is organized as follows. Section 2 describes the experiment; Section 3 presents our hypotheses; Section 4 contains results and Section 5 concludes with a discussion and suggestions for incorporating our findings into (organizational) policy.

## 2 Experimental design

The laboratory experiment consisted of a single (unpaid) practice period, followed by 10 paid periods. In each period, subjects were randomly matched with two peer subjects in the lab through a stranger matching protocol. They did not see these peers, however, as all subjects were seated in private cubicles and interacted only through our experimental software. Subjects accumulated earnings in Experimental Currency Units (ECU), which at the end of experiment were converted to Great British Pounds at the rate 1 ECU = £0.01.

### 2.1 Real-effort task stage

Every period started with a real-effort task, adapted from Charness et al. (2014). In this task, subjects sequentially decode randomly generated letters of the Latin alphabet (A-Z) by matching them to numbers 1-26 in a list of letter-number pairs on their computer screen.<sup>1</sup> Our subjects started each period with 25 points and received 5 more points for each correctly decoded letter. The points accumulated in a 1-minute task window are the subject's *task output* or *unenanced output*. Task output did not determine earnings; subjects were paid a flat wage of 100 ECU per period.

### 2.2 Enhancement stage

After completing the task, subjects were offered the opportunity to increase their output by buying points. Subjects could only buy in 5-point increments, with a 5-point output increase costing them 5 ECU (5% of their earnings for the period). The maximum increase was 100 points per period. This information was clearly shown on screen, such that it was clear to subjects that buying points reduced their earnings. The number of points bought in the

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<sup>1</sup> The experiment was conducted by computer using z-Tree (Fischbacher, 2007), version 3.3.8. The real-effort task part of the experiment was based on the original code by Charness et al. – we translated the on-screen text for the real-effort task from French to English but kept the screen design the same. The social comparison part of the experiment was programmed separately. All software used is available upon request.



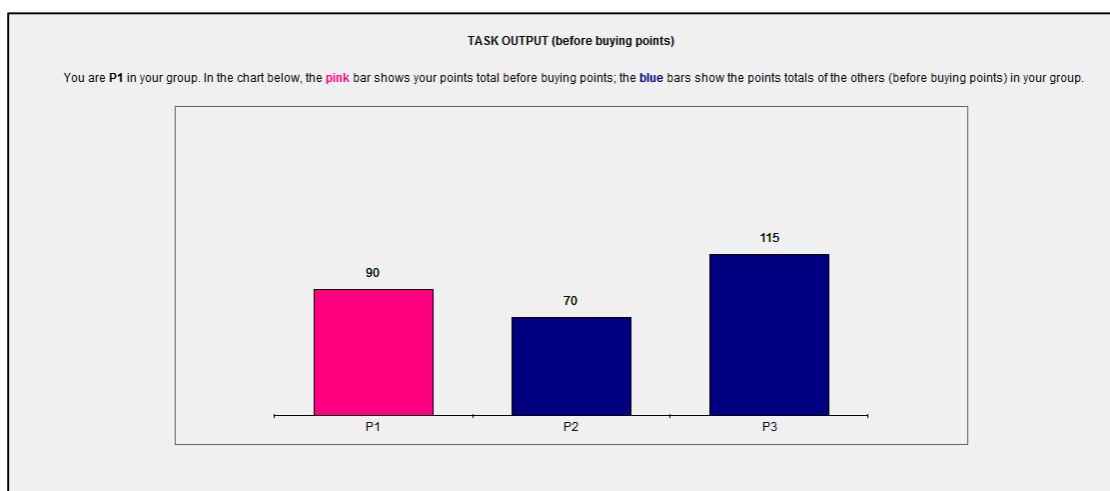
enhancement stage is the outcome variable of interest in our experiment: a monetary measure of personal sacrifice. Paying subjects a flat wage per period ensured that the budget for such sacrifices was the same across subjects, periods and treatments. We will henceforth refer to a subjects' points total after the enhancement stage as *final output* or *enhanced output*.

Subjects made their choices in the enhancement stage simultaneously and they knew that enhancement was available to everyone. Subjects always knew their own task output during the enhancement stage, as it was shown on screen. In some treatments, as explained below, subjects also saw the task output of the two peers in their group.

### 2.3 Social comparison stages

The social comparisons showed a subject's period output alongside that of the two matched peers. The design of the social comparison screen is shown in Figure 1: the brightly colored vertical bar shows the subject's own output; the dark bars represent the two peers. Corresponding output scores are shown above the bars. The vertical bar design makes salient the subject's ranking, thus facilitating a social comparison.

**Figure 1: Social comparison screen.**



Notes: The brightly coloured bar shows the subject's own output; the dark bars represent the two peer subjects. In the comparison shown in the figure (from treatment UNCERTAINTY), subjects are identified as P1, P2 and P3: this was the procedure used in the main treatment variants. In the ID variants, subjects were identified by their first name.

Neither the experiment’s instructions nor the social comparison screens contained any further reference to the social comparison. The experiment contained no lists of ranks within the group, no history on past output or ranks, no special mention or awards for high-ranking subjects and no references to ranking, position or comparison. Such content was purposefully omitted to avoid the impression that subjects’ ranks somehow mattered to the experimenter.

The social comparison screens showed either the task output or final output of subjects, depending on treatment and treatment stage. To avoid any confusion between the two types of social comparison screen, an on-screen header stated whether subjects were looking at task output (“before buying points”) or final output (“after buying points”). We next explain the purpose of the two types of social comparisons in our experimental treatments.

## 2.4 Treatments

**Table 1: Treatment stages/timeline.**

Treatment/Stage	Decoding task	Social Comparison (task output)	Enhancement	Social Comparison (task output)	Social Comparison (final output)
BENCHMARK	X	X			
OBSERVABLE-1X	X	X	X		X
OBSERVABLE-2X	X	X	X	X	X
UNCERTAINTY	X		X	X	X
UNOBSERVABLE	X		X		X

Notes: This table shows the different stages of a period for all five main treatments, in chronological order from left to right. Subjects in a treatment only enter the stages marked with an X. Each treatment has a main and an ID variant (in which identity is made salient).

Our between-subjects design consists of five treatments, with subjects randomly allocated to a treatment at the session level. Table 1 shows the stages in each treatment.

The first treatment, BENCHMARK, has no enhancement stage; it simply provides us with a measure of task output in absence of enhancement. After the effort task, subjects in BENCHMARK see the task output social comparison screen, after which they move on to the next period. The other treatments all have an enhancement stage and a final output social comparison stage, but differ in whether subjects are shown the task output of the two group peers and whether this happens before or after the enhancement stage. Task output of peers is therefore either observable or unobservable, and when observable it may be uncertain at the point of deciding on enhancement (treatment UNCERTAINTY).

In the OBSERVABLE-1X and OBSERVABLE-2X treatments, group members see each other's task output before the enhancement stage. Enhancement was therefore transparent in both treatments – comparing a peer's output before and after the enhancement stage revealed how many points the peer had bought. As Table 1 shows, the only difference between these two treatments is that the latter treatment makes it easier for subjects to infer enhancement by peers since task output is shown a second time, right before the final output social comparison. The OBSERVABLE-2X treatment was designed for direct comparison with the UNCERTAINTY treatment, in which subjects did not observe the task output of peers before positional enhancement but did so right before the final output social comparison. Finally, subjects in the UNOBSERVABLE treatment could not observe peers' task output and, by extension, enhancement. The UNOBSERVABLE and UNCERTAINTY treatments thus allow us to disentangle the effect of the social visibility of positional enhancement (UNCERTAINTY only) from the effect of uncertainty about the task output of peers (both treatments). Note that subjects knew which social comparisons they would see at the various stages, as this was explained in the instructions and demonstrated in the practice period.

Subjects interacted anonymously – they were referred to as P1, P2 and P3 in the social comparison screens. To ensure that social comparisons were sufficiently salient, we additionally conducted an ‘ID’ variant of each treatment in which subjects were identified by their first names. Henceforth, we use the <sup>MAIN</sup> and <sup>ID</sup> superscripts to indicate specific variants of treatments. Subjects in ID treatments consented beforehand to having their names displayed on screen and were asked afterwards whether they knew anyone else in the session.<sup>2</sup> Since none of our findings are sensitive to the exclusion of subjects that said they knew someone, it is highly unlikely that subjects’ behavior was influenced by relationships outside the laboratory. It is worth pointing out that the ID variants of the OBSERVABLE-\*X treatments do not control for the possibility that subjects recognize names of those they were paired with in earlier periods – we check for this in our analysis of enhancement over time (Section 4.3).

## 2.5 Procedure

All sessions were run in 2014-2015 at the London School of Economics Behavioral Research Lab in London, United Kingdom. In total, 477 subjects participated in 35 sessions (9-18 subjects per session). Upon arrival, subjects were randomly assigned to a computer cubicle and completed a demographics questionnaire. They then received written instructions (see Appendix), which were also read out loud by an experimenter. The experimenter then took clarification questions in private and repeated answers to questions about the experimental design out loud. After all questions were answered, subjects completed one unpaid practice period and were given another opportunity to ask questions. Afterwards, subjects started with the 10 paid periods. After the final period, subjects were shown a screen with their total earnings. They were then individually called to the front of the room and paid in private. Average earnings per subject were £9.55 and average session time was 40 minutes.

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<sup>2</sup> Subjects were reassured that their first names would only be used for the on-screen display and that the information would not be stored anywhere or used to link their responses in the experiment to their identity.

### 3. Hypotheses

Social comparisons are closely linked to the concept of social status, which derives from the rank one occupies in a social comparison. A vast literature in anthropology, psychology and sociology supports the idea that people desire high social status (Chen et al., 2012; Heffetz and Frank, 2003).<sup>3</sup> Status hierarchies are innate to human social interaction; they have even been shown to arise spontaneously in newly formed groups of strangers (Anderson et al., 2015; Bales et al., 1951; Barkow et al., 1975; Maslow, 1943).

Related to the desirability of status, central to the present study is the notion that social comparisons induce people to make monetary sacrifices to better their position relative to others. As in previous experimental work (Price and Sheremeta, 2011; Van Den Bos, 2008, 2013; Charness et al., 2014), subjects interact anonymously with strangers through computer terminals and any benefits from outranking others must be strictly intrinsic. Although standard expected utility theory predicts no positional enhancement in any of our treatments, several theoretical approaches allow for people to intrinsically value their relative position (Amaldoss and Jain, 2005; Bénabou and Tirole, 2016; Duesenberry, 1949; Frank, 1985ab; Hopkins and Kornienko, 2004). These theories posit that the cost of higher status utility (rank in a social comparison) falls on the individual, as in our experiment. Hence our first hypothesis:

*H1: Subjects will engage in costly positional enhancement.*

Based on the abovementioned experimental findings (esp. Charness et al., 2014), we expected non-zero positional enhancement even when subjects' engagement in enhancement is publicly visible – i.e. in our OBSERVABLE-\*X and UNCERTAINTY treatments. But we

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<sup>3</sup> Of course, ranking highly in some social comparison does not automatically confer high social status, nor do people always agree on the metric of ranking (Bederson et al., 2018; Feltovich et al., 2002; Ridgeway, 2007).

expected the amounts spent on positional enhancement to be even higher in the UNOBSERVABLE treatment, when there is no such visibility:

*H2: Positional enhancement will be higher when it is not publicly visible.*

We hypothesized this may happen for two reasons: (i) social signaling of ability and (ii) concerns about appearing unethical. Social signaling of ability, as described in Section 1, is enabled by people believing that peers credit them with greater (absolute or relative) output than their task performance merits (Bendersky and Shah, 2013). Since previous evidence shows that people readily overstate their unobserved intellectual performance to peers (Burks et al., 2013; Ewers and Zimmermann, 2015), we may expect people to engage in such signaling in our experimental setting too. Note it is not necessary for peers to believe that someone's enhanced position in the social comparison signals greater ability, only for the person doing the enhancing to believe that it will. Alternatively, or additionally, people may be concerned that their peers view buying points as unethical or 'cheating' (Anderson et al., 2008; Charness et al., 2014). They will therefore be less inclined to enhance their output when this is visible to peers, in line with the directional hypothesis H2. However, one can never exclude the possibility that some contrarian subjects engage in 'conspicuous positional enhancement'.

Another difference between the OBSERVABLE-\*X and UNOBSERVABLE treatments is due to the simultaneous nature of positional enhancement: subjects in the latter treatment do not know the task output of others when deciding on positional enhancement. To measure the effect of this uncertainty, we conducted the UNCERTAINTY treatment in which positional enhancement was revealed to one's peers at the end of the period. We expected that making positional enhancement publicly observable in this way would make it less attractive to subjects, for the reasons discussed above. But if subjects are simply driven by a desire to

beat peers in the final output comparison, not knowing peers' task output may lead them to spend more resources on positional enhancement. The intuition behind such uncertainty driving greater enhancement is that, if we assume a loss aversion-like utility with respect to one's relative position, subjects' sensitivity to being outranked increases when they do not know the output of their peers.<sup>4</sup> A simple desire to outrank others could have many origins – a “joy of winning” utility (Kuhnen and Tymula, 2012; Price and Sheremeta, 2011), last-place aversion (Kuziemko et al., 2014; Martinangeli and Windsteiger, 2020), a fundamental desire for status (Anderson et al., 2015), management of one's self-image or self-confidence (Bénabou and Tirole, 2011; 2016; Villeval, 2020) or a more complex motive of deceiving the self (Mijović-Prelec and Prelec, 2010, Solda et al., 2019; see also Bénabou and Tirole, 2016).

Previous studies find that the salience of social comparisons moderates their influence on behavior (Ashraf et al., 2014; Bogaerts, and Pandelaere, 2013; Shepherd et al., 1995). Since the anonymous interactions between subjects in our main treatments may not produce the same social incentives as naturally occurring social comparisons, we additionally conducted all treatments in an ID variant where subjects were identified by their first name. This gives rise to our final hypothesis:

*H3: Enhancement visibility (H2) will have a stronger effect when identities are salient.*

This may be due to average positional enhancement being lower when publicly observable (due to more salient concerns about appearing unethical) or average positional enhancement being higher when unobservable (due to ability signaling being more salient).

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<sup>4</sup> We may assume, for example, that subjects have a utility function  $U_i = w - p_i + \Pr(\pi_i \geq \pi_j) [\beta(\pi_i - \pi_j)] + \Pr(\pi_i < \pi_j) [\alpha(\pi_i - \pi_j)]$  where final output  $\pi = e + p$  consists of task output (effort)  $e$  and positional enhancement  $p$ . It can be shown that, if subjects care sufficiently about their relative output and experience more disutility from comparing negatively than utility from comparing favorably ( $\alpha \geq \beta$ ), they will increase positional enhancement in response to uncertainty about peer output. This is similar to the “behindness aversion” modelled by Ederer and Pataconi (2010). See also the discussion on reference points in Ludwig and Lünser (2012).

## 4 Results

### 4.1 Task output

Before we analyze between-treatment differences in positional enhancement, we consider whether treatments affected subjects' effort on the real-effort task. Table 2 shows the mean and standard deviation of task output across treatments. By comparing the BENCHMARK treatment to others, it does not appear that the introduction of positional enhancement has affected the effort subjects exerted on the task.

In fact, we detect no systematic or substantial differences in task output levels between any treatments. Although mean performance across all periods does vary somewhat, neither two-tailed Mann-Whitney U tests (all  $p > 0.1$ ) nor regression models (Table A1 in the Appendix) show statistically significant differences between treatments. Wald equality tests on the coefficients reported in Table A1 cannot reject the null of equality, nor do F-tests indicate joint significance of the treatment coefficients. We also find no systematic differences between treatments in terms of task output variance, using subjects as individual observations for two-tailed equality of variances tests between pairs of treatments (all  $p < 0.1$ ). Table 2 also shows only a handful of subjects exerting low levels of effort, as defined by a threshold of 70 points, and no correlation with treatment. Subjects across treatments exerted similar levels of effort; potentially close to maximum levels of effort. We investigated this latter conjecture by conducting an additional treatment ( $n=30$ ) without a social comparison stage in which subjects were strictly paid for performance: each point earned translated into 1 ECU. Subjects' mean task output in this treatment was 108.1 points. This suggests that subjects across our other treatments were performing close to their maximum and not substituting task effort with positional enhancement. It is also worth noting that, with a minimum score of 25 points and treatment mean task output in the 99-106 points range, the 100 points 'budget' for positional enhancement offered substantial scope to enhance one's output relative to peers.



**Table 2: Task output, by treatment.**

	BENCHMARK	OBSERVABLE- 1X	OBSERVABLE- 2X	UNCERTAINTY	UNOBSERVABLE
Main treatments	<i>n</i> =45	<i>n</i> =48	<i>n</i> =51	<i>n</i> =45	<i>n</i> =51
Mean points earned (SD)	99.5 (16.9)	104.1 (13.0)	105.5 (12.9)	101.3 (10.5)	101.4 (12.9)
% of subjects <70 points	6.7%	0%	0%	0%	2.0%
ID treatments	<i>n</i> =48	<i>n</i> =45	<i>n</i> =51	<i>n</i> =48	<i>n</i> =45
Mean points earned (SD)	103.8 (13.1)	98.6 (13.2)	102.8 (18.6)	102.6 (15.6)	102.8 (12.9)
% of subjects <70 points	0%	0%	0%	2.1%	0%

Notes: Each subject started with 25 points, earning 5 points for each correctly decoded letter. Subjects with fewer than 70 points (mean across all 10 periods) decoded at less than half the average work rate.

## 4.2 Positional enhancement

Table 3 shows aggregate positional enhancement statistics across treatments. There is clear support for H1 with subjects engaging in enhancement in all treatments. There are also patterns in the differences between treatments, with the UNCERTAINTY and UNOBSERVABLE treatments showing higher levels of positional enhancement. The table also shows that treatment differences on mean points bought are partly driven by the external margin – that is to say, greater percentages of subjects engaging in enhancement. But the internal margin is a greater contributor – subjects buy two to three times as many points per period in the UNOBSERVABLE and UNCERTAINTY treatments. This holds true for both the main and ID treatment variants.

**Table 3: Positional enhancement, by treatment.**

	OBSERVABLE-1X	OBSERVABLE-2X	UNCERTAINTY	UNOBSERVABLE
Main treatments	<i>n</i> =48	<i>n</i> =51	<i>n</i> =45	<i>n</i> =51
Mean points bought (SD)	2.5 (5.9)	3.6 (7.1)	10.5 (19.9)	8.3 (14.1)
% of subjects who buy points	50.0%	64.7%	71.1%	56.9%
Conditional mean points bought (SD)	5.0 (13.9)	5.5 (13.6)	14.8 (28.0)	14.7 (24.2)
ID treatments	<i>n</i> =45	<i>n</i> =51	<i>n</i> =48	<i>n</i> =45
Mean points bought (SD)	4.0 (6.5)	4.5 (9.5)	9.4 (14.0)	8.8 (21.4)
% of subjects who buy points	51.1%	39.2%	66.7%	60.0%
Conditional mean points bought (SD)	7.8 (20.0)	11.4 (22.7)	14.1 (24.4)	14.7 (29.2)

Notes: Positional enhancement standard deviation measures differences in mean enhancement between subjects. Positional enhancement points are bought with period earnings of 100 ECU, meaning that the positional enhancement amounts can also be interpreted as percentages of earnings spent on enhancement. Note that to ‘buy points’ should be interpreted as having bought points at least once, i.e. in one period.

The ID treatment variant manipulation does not appear to have affected levels of positional enhancement. One treatment worth pointing out is OBSERVABLE-2X<sup>ID</sup>, with fewer subjects buying points but more points bought by those who do. A potential explanation is that two counterbalancing mechanisms are at work, driven by the greater transparency of enhancement in this treatment combined with the salience of individual identity. This makes

the average subject becomes more reluctant to engage in positional enhancement, but those that do feel emboldened to enhance their position when peers are also doing so openly.

**Table 4: Regression of positional enhancement on treatments, random-effects GLS.**

	Pooled data (1)	Main treatments (2)	ID treatments (3)
OBSERVABLE-1X	Ref.	Ref.	Ref.
OBSERVABLE-2X	0.756 (1.093)	1.038 (1.454)	0.440 (1.631)
UNCERTAINTY	6.701*** (1.426)	8.012*** (1.143)	5.406* (2.525)
UNOBSERVABLE	5.333** (1.884)	5.822* (2.414)	4.811 (3.033)
ID variant	0.447 (1.225)		
Constant	3.025*** (0.899)	2.521* (1.006)	4.011*** (0.716)
Observations	3840	1950	1890

Notes: Dependent variable is points added to output per period (positional enhancement). Coefficients in uppercase are treatment variables, Period and Subject random effects not shown in table. Standard errors (clustered at session level) are reported in parentheses. Significance levels: \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

To formally test for differences between treatments, we conducted a regression analysis with three random-effects GLS specifications reported in Table 4. First, positional enhancement does not differ between the OBSERVABLE-1X and OBSERVABLE-2X treatments. The slightly higher transparency of task output in the latter treatments thus had no effect on levels of positional enhancement.

Second, positional enhancement is higher in treatment UNOBSERVABLE than in OBSERVABLE-1X (although we do not find a statistically significant difference for ID

treatments, we do find a strongly significant effect for the pooled data) and OBSERVABLE-2X (Wald coefficient tests on Table 4, pooled data  $p=0.0215$ , main variant  $p=0.0492$ ; ID variant  $p=0.1841$ ). Although this pattern is in line with our second hypothesis (H2), the second implication of this hypothesis is not supported by the data. As the reader may recall, we conducted the UNCERTAINTY treatment in which task output was revealed *after* enhancement. We thus control for social visibility of positional enhancement. We find that positional enhancement in the UNCERTAINTY treatment is higher than in OBSERVABLE-1X and OBSERVABLE-2X (for the latter, Wald coefficient tests on all Table 4 models: all  $p<0.05$ ). Furthermore, we cannot reject the null that positional enhancement in UNCERTAINTY is the same as UNOBSERVABLE (Wald coefficient tests, all Table 4 models  $p>0.1$ ). We therefore find no evidence that ability signaling or concerns about appearing unethical to peers drive different levels of positional enhancement. It is not public observability of enhancement, but rather uncertainty (the lack of feedback on peers' task output) that drives greater positional enhancement. This uncertainty about the performance of peers on the task has a substantial economic effect on positional enhancement: it effectively doubles the amount of earnings sacrificed by subjects (from around 5% to 10%).

Given we find no support for H2, it should come as no surprise that H3 is not supported by the data either. In fact, we find no evidence that the ID treatment manipulation affected levels of positional enhancement. Apart from the subtle difference in enhancement across the OBSERVABLE-\*X treatments mentioned above, it appears that greater salience of identity does not affect subjects' valuation of their relative position in our experiment.

### **4.3 Enhancement over time**

Figure 2 shows how positional enhancement evolved over the ten periods of the experiment. Enhancement was relatively high in the first two periods, before dropping and

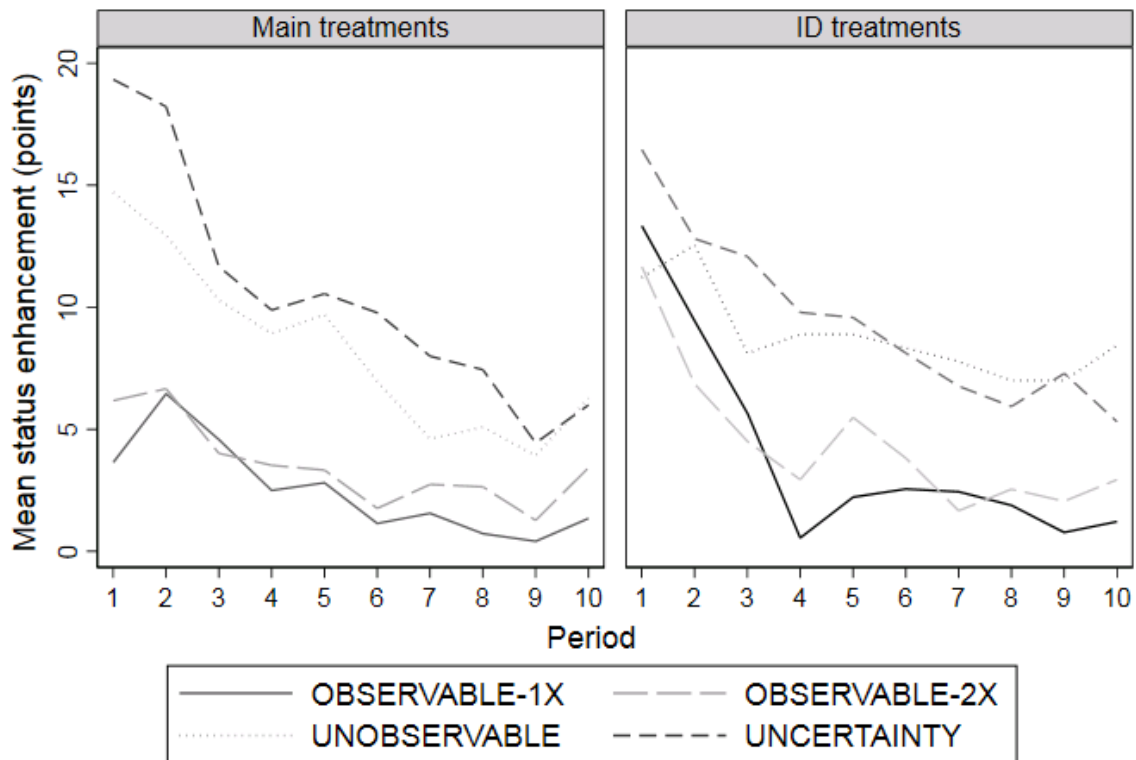
stabilizing. Across all treatments and variants, an average of 32.3% of subjects engaged in some enhancement in periods one and two, a percentage that dropped to 26.3% in period three and 21.1% in period four, then stabilizing in the 15-20% range for the remaining periods. Within-subject Wilcoxon signed-rank tests of average enhancement confirm that it was higher in the first five periods than in the final five periods (all treatments  $p < 0.1$ ). Although the experiment started with a practice period, the decrease in the early periods might reflect some learning on the relationship between enhancement and earnings. Alternatively, subjects may have found that their spending on positional enhancement did not result in the desired outcome, due to others enhancing their outputs too. Another explanation is that subjects felt less of a need to enhance their output after learning about the distribution of peers' abilities in earlier periods (similar to the mechanism identified by Bendersky and Shah, 2013).

Figure 2 shows that higher enhancement in the UNOBSERVABLE and UNCERTAINTY treatments persists in later periods. This is supported by our re-estimates of the regression models reported in Table 4 whilst excluding the first two or first five periods. These restricted estimation exercises, reported in the Appendix (Tables A2 and A3), replicate the unrestricted models' treatment effects qualitatively. The findings reported earlier are thus robust to the exclusion of decisions in earlier periods. As before, we also find no differences in the level of positional enhancement due to the ID treatment variant manipulation. We therefore find no evidence that positional enhancement in the OBSERVABLE- $X^{ID}$  treatments is different due to subjects recognizing names of peers they were randomly matched with in earlier periods (in the UNOBSERVABLE<sup>ID</sup> and UNCERTAINTY<sup>ID</sup> treatments this was not possible, as subjects only saw the names of peers at the end of each period).

The similarity of positional enhancement in treatments UNOBSERVABLE and UNCERTAINTY in later periods of the experiment is striking. After having completed at least half of the experiment, subjects in both treatments have arguably formed some empirically

informed beliefs on the distribution of final output, which they could use to decide on a suitable level of positional enhancement. The persistent gap between these two treatments and the other treatments, then, likely represents the greater resource requirements of positional enhancement when subjects cannot observe the task output of those they are paired with for the period. Because, even when subjects have accumulated significant experience in the enhancement setting, they still face the uncertainty of not knowing who they will be compared to in a given period. This explains why, in the short term as well as the long term, uncertainty about the task output of peers yields higher levels of positional enhancement.

**Figure 2: Positional enhancement over time, treatment means.**



#### 4.4 Enhancement frequency

Table 5 presents the data on subjects' frequency of positional enhancement in four categories: never, 1-2 periods, 3-5 periods and more than half of periods.

**Table 5: Frequency of positional enhancement per subject, by treatment.**

	OBSERVABLE-1X	OBSERVABLE-2X	UNCERTAINTY	UNOBSERVABLE
Main treatments	<i>n</i> =48	<i>n</i> =51	<i>n</i> =51	<i>n</i> =45
0 periods (never)	50.0%	35.3%	28.9%	43.1%
1-2 periods	35.4%	35.3%	28.9%	23.5%
3-5 periods	12.5%	17.7%	17.8%	7.8%
>5 periods	2.1%	11.8%	24.4%	25.5%
ID treatments	<i>n</i> =45	<i>n</i> =51	<i>n</i> =45	<i>n</i> =48
0 periods (never)	48.9%	60.8%	33.3%	40.0%
1-2 periods	33.3%	17.7%	18.8%	31.1%
3-5 periods	8.9%	13.7%	31.3%	4.4%
>5 periods	8.9%	7.8%	17.1%	24.4%

Notes: Out of a total of 10 periods.

Focusing on the treatments in which positional enhancement was significantly higher, UNOBSERVABLE and UNCERTAINTY, note the strikingly high percentage of subjects that engaged in enhancement more than five out of ten periods. These subjects also spent considerable amounts of their earnings: average spending per enhancement instance for these subjects was around 35% of their period earnings, higher than those who enhanced their outputs less frequently.<sup>5</sup> These spending patterns suggest something particular about people's

<sup>5</sup> Recall that subjects could buy up to 100 points to enhance their output in a given period. In the UNOBSERVABLE treatments, those with an enhancement frequency of 1-2 periods bought on average 25.1 points per enhancement instance, those with frequency 3-5 periods 28.9 points and those with >5 periods 36.8 points; in the UNCERTAINTY treatment, those with a frequency of 1-2 periods bought 21.0 points, those with frequency 3-5 periods 18.3 points and those with >5 periods 34.2 points.

motives to engage in a costly jostling for position. First, despite the simultaneous nature of positional enhancement, subjects in the OBSERVABLE-\*X treatments may have looked for more promising opportunities for enhancement based on their ‘starting point’ (task output) relative to peers. Second, when subjects do not know how their starting point compares to peers, many of them (around a quarter of our subject pool) are keen to ensure that positional enhancement pays off and therefore increase their spending. These findings suggest that subjects are not just generally boosting their output for the final social comparison, but they specifically aim to outrank their peers.

#### **4.5 Dynamic determinants of enhancement**

Although behavior from round to round is endogenous to treatment, it may nevertheless be instructive to investigate correlates of individual instances of positional enhancement. We conduct a random-effects GLS regression analysis on pooled data (both treatment variants), reported in Table 6. Controlling for the subject’s task output in period  $t$ , we regress enhancement spending in period  $t$  on task output rank in period  $t$  (OBSERVABLE-\*X treatments only), enhanced output rank in period  $t-1$  and the difference between the subject’s final output and the peer group mean in period  $t-1$ . We also estimate separate models with an interaction term for a positional enhancement indicator and final rank in period  $t-1$ .

The coefficient on task output in the current period is negative and significant in all treatments: the higher subjects’ task output, the fewer points they bought to enhance their output. But even controlling for subjects’ task output, we find that subjects engaged in more enhancement when they rank lower on task output in the current period, at least for the OBSERVABLE-\*X treatments in which this rank was visible. Compared to top-ranked subjects, those ranked second bought more points and bottom-ranked subjects bought even more points (Wald coefficient test, model 1:  $p=0.000$ ; model 2:  $p=0.000$ ). These results reinforce the idea that subjects spend money with the purpose of outranking others.



**Table 6: Regression of positional enhancement (PE) on output, rank, previous round's behavior and demographics, all treatments random-effects GLS.**

	OBSERVABLE-X		UNCERTAINTY		UNOBSERVABLE	
	treatments		treatments		treatments	
	(1)	(2)	(3)	(4)	(5)	(6)
Task output <sub>t</sub>	-0.266*** (0.038)	-0.259*** (0.039)	-0.472*** (0.089)	-0.501*** (0.084)	-0.420*** (0.039)	-0.421*** (0.031)
Task Rank <sub>t</sub> = 1	Ref.	Ref.				
= 2	6.695** (0.668)	6.718** (0.630)				
= 3	12.207*** (0.864)	12.244*** (0.873)				
Final Rank <sub>t-1</sub> = 1	Ref.		Ref.		Ref.	
= 2	-1.043** (0.496)		-1.008** (0.429)		0.441 (0.768)	
= 3	-1.262*** (0.325)		-2.699*** (0.767)		-0.524 (1.057)	
PE <sub>t-1</sub> =No*Final Rank <sub>t-1</sub> = 1		Ref.		Ref.		Ref.
= 2		-0.453 (0.496)		-0.141 (0.815)		1.720** (0.543)
= 3		-0.736 (0.534)		-1.137 (0.758)		2.000** (0.504)
PE <sub>t-1</sub> =Yes*Final Rank <sub>t-1</sub> = 1		4.515** (1.622)		4.448*** (1.122)		8.362** (3.476)
= 2		2.284 (1.404)		2.352 (1.643)		7.447*** (1.989)
= 3		4.806** (2.004)		-0.577 (2.094)		3.732 (2.650)
Final Distance from $\mu_{t-1}$	-0.709*** (0.025)	-0.716*** (0.025)	-0.706*** (0.080)	-0.729*** (0.074)	-0.631*** (0.051)	-0.649*** (0.046)
Gender (1=Female)	-0.075 (0.795)	-0.125 (0.676)	1.068 (1.494)	0.977 (1.394)	-0.853 (3.479)	-0.258 (3.178)
Age	0.097** (0.049)	0.087* (0.041)	0.565* (0.231)	0.505** (0.219)	-0.040 (0.147)	-0.073 (0.133)
Period 6-10	-1.800** (0.649)	-1.450** (0.602)	-3.168** (1.074)	-2.578* (1.057)	-3.073* (1.343)	-2.430 (1.316)
Constant	24.462*** (3.440)	22.837*** (3.649)	45.267*** (13.931)	47.668*** (13.311)	54.007*** (6.282)	51.455*** (5.234)
Observations	1611	1611	819	819	819	819

Notes: Dependent variable is number of points bought (positional enhancement) in period  $t$ ; Task Rank indicates subject rank in group based on task output; Final Rank indicates subject rank in group based on final (enhanced) output; PE<sub>t-1</sub> indicates whether subject engaged in positional enhancement (points buying) in the previous period; Final distance from  $\mu_{t-1}$  is the group's mean final output minus own final output in the previous period; Period 6-10 is a dummy variable. Data from main and ID treatment variants were pooled for all models. Standard errors (clustered at session level) are reported in parentheses. Significance levels: \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

We see positive and significant coefficients on the interaction of previous period final rank and previous period enhancement in the OBSERVABLE-\*X treatments (model 2). Wald tests for equality of these interaction coefficients do not allow us to reject the hypothesis that coefficients are the same for all ranks (all  $p > 0.1$ ), suggesting tendency to enhance one's output was persistent across periods. A similar result holds for the UNCERTAINTY treatments, but for the UNOBSERVABLE treatments (model 6) we see higher spending by those who did not enhance their output in the previous period and ranked second or third. There is also more general evidence that positional enhancement was influenced by previous experiences of performing poorly in a social comparison: the previous period's difference between the subject's own final output and the mean of the group (*Final Distance from  $\mu_{t-1}$* ) is negatively correlated with points buying in all model specifications.

## 5 Discussion

Our findings show that uncertainty about the productive output of peers increases the material sacrifices that people will make to out-rank said peers on an output-based social comparison. Strikingly, and in contrast to studies where subjects can publicly overstate their performance on intellectual tasks (Burks et al., 2013; Ewers and Zimmermann, 2015), we find no evidence of social visibility effects, such as ability signaling. It could be that effort-based social comparisons are perceived differently than comparisons on intellectual tasks, or perhaps the act of sacrificing earnings for points is driven by different motivations than the 'costless' act of overstating one's performance. Of course, it is possible that other types of effortful tasks (in the lab or in the field) are more likely to trigger social visibility effects, for example due to ethical concerns about enhancement leading to social pressure, or because skill on these task is held in higher esteem by those participating. This is an intriguing question for potential follow-up work, which may also speak to the external validity of the reported findings.

Our experimental task seems to have triggered a rather pure desire to outrank others on output, with around half our subjects willing to incur a material cost to do so. As in prior experimental work (esp. Charness et al., 2014), it seems that people desire a high relative position so much that they do not mind others knowing that they gave themselves a leg up. As detailed in Section 3, there are several explanations for such a desire, which may be conceptualized as a fundamental tendency (Anderson et al., 2015; Kuhnen and Tymula, 2012; Kuziemko et al., 2014; Martinangeli and Windsteiger, 2020; Price and Sheremeta, 2011) or related to managing one's self-image (Bénabou and Tirole, 2011; 2016; Mijović-Prelec and Prelec, 2010, Solda et al., 2019; Villeval, 2020). As the focus of our experiment was the role of information conditions, we cannot attempt to distinguish between these explanations. Furthermore, to understand the mechanism by which uncertainty over peer output drives higher positional enhancement, we need to better understand the beliefs (first and second order) of subjects. To avoid experimenter demand effects and to not stray too far from the design of Charness et al. (2014), we opted for not eliciting beliefs in the present experiment. Future studies may focus on innovative ways of eliciting beliefs, perhaps in one-shot designs. An intriguing line of belief-related research are the organizational studies in which people are credited with some extra ability even when they are known to have inflated their credentials (Anderson et al., 2012; Kennedy et al., 2013). But given the simple and deliberately non-field-like context of our experimental design, it may not be the right tool for such investigations.

If we accept that there are field settings in which a pure desire to outrank others is at least one of the motivating factors, then it does not seem much of a stretch to acknowledge that uncertainty over peer output may play the role it does in our experiment. That is, we can easily imagine settings in which uncertainty over the output of relevant peers makes one more likely to sacrifice some leisure time or other resource. Consider modern bureaucracies, where working hours and locations are more fluid than before and employees may experience an

‘always on’ culture (Becker et al., 2018; Von Bergen and Bressler, 2019). Other examples are presenteeism and unsafe workplace behaviors. Employees may feel under greater pressure to make personal sacrifices because they overestimate the output of their peers relative to their own. If true, then management may want to mitigate positional enhancement by creating more transparency around ‘unenhanced’ output and/or by singling out the valuable contributions of those employees or managers who stay within the bounds of contracted hours and procedures (Bendersky and Pai, 2008; Schoenberg and Haruvy, 2012). The most effective solution may depend on whether employees’ beliefs on their peers’ activity are out or within-equilibrium. Perhaps there is merit in doing away with certain enhancement opportunities altogether, such as those companies and jurisdictions seeking to stop the practice of e-mailing out of hours. A notable feature of some of these efforts is that they explicitly recognize the positional externalities that positional enhancement can create – if all your colleagues start working an hour of overtime every day, the pressure is on for you to do the same. If you do, then everyone’s relative positions remain unchanged. It is not clear that such outcomes are desirable for the organization’s management, since employees consistently working overtime or making other types of sacrifices can eventually take its toll in high employee turnover, low morale or more mistakes (CIPD, 2018; Stewart et al., 2003). But these are empirical questions.

An important question of external validity for the present study is whether similar results would obtain when subjects know each other and have repeat interactions, as they do in the typical workplace. It may also be more realistic to represent information on peer output as a noisy signal. Another possible extension is to introduce additional motives for making personal sacrifices – promotion prospects, bonuses, etcetera – that are common to field settings. In closing, we express the hope that follow-up work will explore the phenomenon of positional enhancement in a wide variety of settings.

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## Appendices

### Appendix I: Further statistical analysis.

**Table A1: Task output by treatment, random-effects GLS.**

	All treatments (1)	Main treatments (2)	ID treatments (3)
BENCHMARK	Ref.	Ref.	Ref.
OBSERVABLE-1X	-2.424 (2.385)	-0.035 (2.890)	-5.212 (3.146)
OBSERVABLE-2X	0.325 (2.584)	1.410 (3.084)	-1.019 (4.113)
UNCERTAINTY	-1.861 (1.828)	-2.829 (2.798)	-1.229 (1.809)
UNOBSERVABLE	-1.845 (2.188)	-2.766 (3.020)	-1.023 (2.911)
ID variant	-1.116 (1.482)		
Constant	99.873*** (1.883)	100.035*** (2.433)	98.759*** (1.708)
Observations	5250	2880	2370
F-test Treatment coefficients	0.4958	0.5991	0.8489

Notes: Dependent variable is task output (in points) per period. Coefficients in uppercase are treatment variables, Period and Subject random effects not shown in table. Standard errors (clustered at session level) are reported in parentheses. Significance levels: \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .



**Table A2: Regression of positional enhancement on treatments, random-effects GLS  
(periods 3-10).**

	All treatments (1)	Main treatments (2)	ID treatments (3)
OBSERVABLE-1X	Ref.	Ref.	Ref.
OBSERVABLE-2X	1.017 (0.921)	0.955 (1.030)	1.081 (1.571)
UNCERTAINTY	6.252*** (1.293)	6.584*** (0.983)	5.945* (2.378)
UNOBSERVABLE	5.463** (1.774)	5.085* (2.156)	5.889* (2.986)
ID variant	0.358 (1.171)		
Constant	1.850** (0.664)	1.888*** (0.495)	2.167*** (0.487)
Observations	3072	1560	1512

Notes: Dependent variable is points added to the subject's output per period (positional enhancement). Coefficients in uppercase are treatment variables, Period and Subject random effects not shown in table. Demographic control variables used were gender and age. Standard errors (clustered at session level) are reported in parentheses. Significance levels: \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

**Table A3: Regression of positional enhancement on treatments, random-effects GLS (periods 6-10).**

	All treatments (1)	Main treatments (2)	ID treatments (3)
OBSERVABLE-1X	Ref.	Ref.	Ref.
OBSERVABLE-2X	1.081 (1.002)	1.331 (0.988)	0.830 (1.823)
UNCERTAINTY	5.482*** (1.202)	6.092*** (1.108)	4.910* (2.097)
UNOBSERVABLE	5.082** (1.898)	4.331* (1.760)	5.933 (3.556)
ID variant	0.716 (1.205)		
Constant	1.051 (0.703)	1.042** (0.383)	1.778 (0.928)
Observations	1920	975	945

Notes: Dependent variable is points added to the subject's output per period (positional enhancement). Coefficients in uppercase are treatment variables, Period and Subject random effects not shown in table. Demographic control variables used were gender and age. Standard errors (clustered at session level) are reported in parentheses. Significance levels: \* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

### Appendix II: Experimental instructions

**NB: instructions that differ between treatments are shown below in *italics* (followed by treatment or treatment variant name in [brackets]). Reference treatment is UNOBSERVABLE.**

Welcome to the London School of Economics Behavioural Research Lab. Today, you will be taking part in an experimental study. If you have a question at any point please do not hesitate to contact the experimenter. The experimenter will be at the desk in the front of the room for the duration of the study.

The whole session will last about an hour. For your participation in this experiment, you will earn money depending on the decisions that you take during the session. This amount of money will be paid to you, privately and in cash, at the end of this session.

It is absolutely essential that, during the whole session, you avoid speaking to, or communicating in whatever form with, the other participants in the lab.

### Earnings and ECUs

The experimental study features 10 rounds of an experimental task. You will be paid for each of these rounds in a currency called ECUs, which can easily be translated into pounds. Before these 10 ‘paid’ rounds, there will be a practice round so you can get used to the task.

For every round of this experimental task, you will earn a ‘wage’ of 100 ECUs. Note that you will receive your wage of 100 ECUs at the start of each round and this amount does not depend on your performance on the task. At the end of the experiment, ECUs will be converted to cash at the rate: 1 ECU = £0.01.

### The Task

The task involves matching letters with numbers from a list. You will be awarded points (not ECUs) for matching letters to numbers. The more letters you match with the correct number, the more points you get.

You will get 1 minute per round to match as many letters to numbers as possible. A counter on your screen will show you how many points you have scored so far. You will start with 25 points. For each correct answer, you will receive 5 additional points.

Before the end of the round, you will get the opportunity to increase your points total by exchanging ECUs from your 100 ECU ‘wage’ for points. Each point added to your points total will reduce your current round earnings by 1 ECU.

In each round, you will be put in a group with two other participants in this session. The members of each group are identified as *P1, P2 and P3* [baseline treatments] / *by their first name and initial of their last name* [ID treatments]. Groups are formed randomly and change every round. The two other members of your group will perform the same task as you. *You will see their points total on your screen immediately after the task, before you have the opportunity to increase your points total by buying points* [treatments OBSERVABLE-1X, OBSERVABLE-2X] / *You will (also) see their points total on your screen after you have had the opportunity to increase your points total by buying points*

[treatments OBSERVABLE-2X, UNCERTAINTY]. You will also see their final points total on the results screen at the end of each round.

If you have any question about the experiment, please feel free to ask us now. If you have any questions at any stage of the experiment, please do not hesitate in calling the experimenter.

You will first play one practice round of the task. There will be another opportunity to ask questions after the practice round. Click the button on your screen if you are ready to start the practice round.