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Looking at the Bright Side: The Motivation Value of Overconfidence

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ABSTRACT

Looking at the Bright Side: The Motivation Value of Overconfidence

The motivation value of confidence postulates that individual effort provision is increasing in beliefs on one's own productivity. This relationship also holds for overconfident individuals who have exaggerated productivity beliefs (motivation value of overconfidence). We present first empirical evidence on the existence of a motivation value of absolute overconfidence that many microeconomic models build on. Moreover, we document that debiasing information increases the accuracy of productivity beliefs of overconfident individuals but comes at the cost of diminished effort provision – a result that is of obvious relevance for many contexts such as labor relations or learning at school. As a further conceptual contribution, we offer a novel strategy for identifying significant overconfidence at the individual level.

JEL Classification: C91, D91

Keywords: overconfidence, effort provision, laboratory experiment

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1. Introduction

Overconfidence is a widespread and well-documented phenomenon (Plous, 1993, Alicke and Govorun, 2005, Moore and Healy, 2008, Skala, 2008). It refers to an overestimate of one's own ability or productivity.[1] Empirical evidence on the consequences of overconfidence highlights diverse negative implications. For example, overconfident individuals tend to underestimate personal risks and hence are more likely to engage in poor financial planning, physically damaging behaviors, and risky driving (e.g. Davidson and Prkachin, 1997, Benartzi, 2001, Bhattacharya, Goldman, and Sood, 2004, Sandroni and Squintani, 2007). Overconfident managers are prone to corporate investment distortions (Malmendier and Tate, 2005), value-destroying mergers (Malmendier and Tate, 2008), and debt conservatism (Malmendier, Tate, and Yan, 2011). In experimental financial markets, overconfidence causes excess entry in competitive markets (Camerer and Lovallo, 1999) and reduces the trading performance (Biais et al., 2005). Overconfident individuals are also more likely to self-select into competition, even when their performance is poor so that they should not enter competition (Bartling et al., 2009, Dohmen and Falk, 2011, Danz, 2014). However, some empirical evidence stresses benefits of overconfidence as well. For example, overconfidence can lead to higher social status (Kennedy, Anderson, and Moore, 2013) and overconfident individuals are better able to persuade others (Schwardmann and van der Weele, 2016). Galasso and Simcoe (2011) and Hirshleifer, Low, and Teoh (2012) show that overconfident CEOs are more likely to pursue innovations.

In this paper, we empirically investigate a further possible consequence of overconfidence that has attracted particular attention by a growing number of microeconomic models (Bénabou and Tirole, 2002, Compte and Postlewaite, 2004, Gervais and Goldstein, 2007, Krähmer, 2007, Ludwig, Wichardt, and Wickhorst, 2011): the *motivation value of overconfidence*. The motivation value of overconfidence postulates that individuals with higher beliefs on their own ability, even the overconfident ones, will exert higher effort if effort and ability are complements. Intuitively, individuals with higher beliefs on their own ability anticipate greater return to effort and hence work harder. Whether this implication holds true empirically is not obvious though: the less confident might work harder in the hope of compensating their (perceived) lack of ability by extra effort, while the overconfident may become smug and slack. This intuition is formally captured by models that build on satisficing instead of optimizing behavior as introduced by Simon (1955): if individuals are satisficers who

[1] In this paper, we investigate the consequences of absolute overconfidence or overestimation, i.e. an overestimate of one's own productivity compared to an objective measure of it. In contrast, relative overconfidence or overplacement refers to an overestimate of oneself relative to others (Moore and Healy, 2008).

stop an activity as soon as their reach their individual, less-than-maximum target utility level, more confident individuals are predicted to exert less effort, ceteris paribus. Since the concept of satisficing was introduced by Simon (1955), it has been applied to a wide range of contexts such as behavioral theories of the firm (Cyert and March, 1992) and has stimulated models of incomplete preferences, models of behavior under ambiguity, theories of rational inattention and search theories (compare Hey, Permana, and Rochanahastin (2017) and the references cited therein). In psychology, satisficing is considered a heuristic that is ecologically rational, i.e. that, in particular decision environments, can outperform alternative, rational decision strategies (Gigerenzer and Goldstein, 1966). Empirical evidence in line with satisficing as opposed to optimizing behavior in work contexts and risky choice has been documented for students, cab drivers, farmers and managers alike (Camerer et al., 1997, Payne, Laughhunn, and Crum, 1980, 1981, Lopes, 1987). For example, cab drivers seem to set a loose daily income target and quit working once they have reached that target (Camerer et al., 1997).

The prominence of the motivation value of overconfidence as a basic ingredient of microeconomic models with overconfident agents contrasts a lack of empirical evidence in favor of it. As a first contribution, our paper aims at closing that gap by providing an empirical test of the motivation value of overconfidence, using a laboratory experiment. Moreover, we test an important implication of the motivation value of overconfidence, namely that informing overconfident individuals about their own productivity will reduce their effort provision. We hypothesize that such de-biasing information will lead to a downward adjustment in productivity beliefs, which in turn, will decrease overconfident individuals' effort provision. Finally, we move beyond existing research on overconfidence by offering a new empirical strategy to identify significant absolute overconfidence at the individual level. It is based on a definition of absolute overconfidence that allows for probabilistic beliefs on one's own productivity (as opposed to point beliefs) and acknowledges that observational measures of productivity are noisy albeit informative about the actual underlying productivity. Combining these two insights, we define an individual as overconfident if the median of her belief distribution exceeds the upper limit of the 95% confidence interval around her actual underlying productivity, which we construct based on noisily measured productivity. This definition implies that overconfident individuals assign more than 50% probability mass of the belief distribution to productivity levels higher than their actual productivity. In other words, they believe that it is more likely that they have a higher productivity than their true productivity than vice versa.

We first present a model that isolates the motivation value of confidence in the most simple, non-strategic setup. From the model, we derive two hypotheses: First, there is a motivation value

of confidence, i.e. individuals with a higher belief on their own productivity exert higher effort. This relationship also holds for overconfident individuals who have exaggerated productivity beliefs. Second, informing overconfident subjects about their own productivity will decrease their effort provision.

We then proceed by testing the two hypotheses in a laboratory experiment with 5 stages. Stage 1 measures individual productivity in a modified version of the slider task proposed by Gill and Prowse (2018). The task does not allow subjects to perfectly monitor their own productivity, which offers scope for over- or underconfidence. In stage 2, subjects' probabilistic beliefs on their own productivity in stage 1 are elicited using a visualized "ball allocation task". In the ball allocation task, subjects are asked to allocate 100 balls that each represent one percentage point probability into 11 bins that illustrate intervals of increasing productivity. Combining the data on observed productivity from stage 1 and median productivity beliefs from stage 2, we can identify overconfident subjects. In stage 3, subjects are randomly assigned to the treatment with information (INFO) or the treatment with no information (NOINFO) about their own productivity in stage 1. In stage 4, subjects work on the same real effort task as in stage 1. They can, however, choose individually how much effort to exert by stopping working on the task. Finally, in stage 5 we again use the ball allocation task to elicit subjects' belief distributions on their productivity in stage 4.

In line with the motivation value of confidence, we find that subjects with a higher belief on their own productivity exert higher levels of effort in stage 4. This relationship also holds and is particularly strong for overconfident subjects (motivation value of overconfidence). The exogenous variation in information provision across treatments INFO and NOINFO provides causal evidence that informing overconfident subjects about their own productivity results in a downward adjustment of their productivity beliefs and lower effort provision.

Our results provide an empirical backing for decision theoretic models in which the motivation value of overconfidence serves as a key mechanism through which enhanced effort provision can compensate the suboptimal individual decision making accompanying overconfidence. For example, Bénabou and Tirole (2002) emphasize that overconfidence in one's chance of success can mitigate less than optimal effort provision due to weak willpower and show that it can be optimal to maintain an upwardly-biased confidence level. Compte and Postlewaite (2004) model an agent whose probability of success in a sequence of tasks and the likelihood of undertaking a task are increasing functions of her confidence. They show that the agent is better off when she is moderately overconfident because the gain from enhanced performance offsets the loss of the suboptimal decision to undertake the task.

Other models rely on the motivation value of overconfidence as a basic ingredient when studying interactions of overconfident agents. Gervais and Goldstein (2007) focus on a firm with complementary effort among agents. The synergy between agents makes the existence of an overconfident agent who exerts more effort beneficial for the entire firm. In a similar vein, Ludwig, Wichardt, and Wickhorst (2011) model team work with two agents whose efforts are complements in a joint project. An overconfident agent engaging in excessive effort provision can improve the welfare of both agents by encouraging the unbiased agent to also exert higher effort. Analysing repeated contests, Krähmer (2007) shows that a contestant who is overly confident in her relative ability exerts more effort, and thus wins more often in contests, which reinforces the biased judgment. As a result, a worse but overconfident contestant and her overconfidence can prevail in contests in the long run.

Our findings also contribute to the empirical literature on the consequences of overconfidence and add insights on factors that motivate effort (DellaVigna and Pope, 2017). In particular, our results suggest boosting confidence as an effective and potentially cost-efficient way to enhance effort provision.[2] The negative impact of de-biasing information on the effort provision of overconfident individuals is of obvious relevance in diverse principal-agent contexts such as interactions between employers and employees or teachers and students. For example, employers could restrain from providing accurate feedback to an overconfident employee in order to continue benefiting from her exaggerated effort provision. Our findings also offer an explanation for why teachers are often reluctant or, for younger students, sometimes even prohibited to provide clear-cut, but possibly worse than expected feedback on students' skills, namely to avoid demotivating their students in future learning efforts. In a recent study, Fischer and Sliwka (forthcoming) show that students with higher beliefs in their learning ability indeed invest more in learning material when preparing for a test. Their experiment on the implications of *relative confidence* for learning dynamics focuses on the distinction between confidence in (the stock of) knowledge and confidence in the ability to learn. In contrast, we focus on the relation between *absolute overconfidence*, debiasing information and effort provision using a real effort task which has no scope for learning (increasing ability) over time – a setup that is closer to the theoretical models discussed above that rely on a motivation value of overconfidence.

In terms of research methods, we offer a clean conceptual definition of absolute overconfidence and a new empirical strategy to identify significant absolute overconfidence at the individual

[2] Santos-Pinto (2008) show theoretically that principals benefit from agents' overconfidence and the resulting over-provision of effort if self-image and effort are complements.

level. Previous papers on absolute overconfidence have directly compared point beliefs on absolute performance to measured performance to identify overconfidence (e.g. Blavastkyy, 2009, Urbig, Stauf, and Weitzel, 2009, Clark and Friesen, 2009, Ludwig and Nafziger, 2011, Sautmann, 2013, Hollard, Massoni, and Vergnau, 2016). Only Ludwig and Nafziger (2011) discuss the risk of misclassifying individuals due to measurement error and compare average point beliefs on absolute performance to average performance to identify overconfidence at the group level only. The other papers use that approach to identify absolute overconfidence at the individual level, ignoring possible misclassification due to measurement error. Moreover, Malmendier and Tate (2005, 2008), Malmendier, Tate, and Yan (2011), Galasso and Simcoe (2011), Hirshleifer, Low, and Teoh (2012) use indirect approaches to categorize CEOs as overconfident based on their options exercise behavior or their portrayal in the press.[3] In contrast, the key conceptual contribution of our paper is to propose a precise definition of absolute overconfidence that regards *observed productivity as a noisy measure of actual productivity* and accounts more generally for individual *probabilistic productivity beliefs*. That means, we allow for the possibility that individuals are not 100% sure about their productivity as a point belief suggests. Based on that definition, we offer a novel strategy for identifying significant overconfidence at the individual level that can be applied more broadly in future work.

The remainder of the paper is structured as follows. In section 2, we provide a definition of absolute overconfidence and show how we can build on that definition in order to empirically identify absolute overconfidence at the individual level. In section 3, we model how beliefs on own productivity translate into effort provision. Based on this model, we derive hypotheses concerning the motivation value of (over)confidence as well as on the effect of information about actual productivity on the effort provision of overconfident subjects. The experimental design that we use to test the two hypotheses is described in section 4. Section 5 presents results. We discuss our findings and conclude in section 6.

[3] In particular, Malmendier and Tate (2005) and Galasso and Simcoe (2011) classify CEOs as overconfident if they, e.g., hold nontradeable in-the-money executive stock options until expiration rather than exercising them after the vesting period or if they exercise options of their own company later than suggested by a rational benchmark, since such behaviors suggest overconfidence in the own ability to keep the company's stock price rising. Malmendier and Tate (2008), Malmendier, Tate, and Yan (2011), Hirshleifer, Low, and Teoh (2012) additionally rely on a CEO's characterization as "confident" or "optimistic" in the press.

2. Definition and Identification of Absolute Overconfidence

In this paper, we study absolute overconfidence, i.e. overestimation of one's own productivity in a given task with respect to an objective measure of own productivity.[4] Consequently, identifying absolute overconfidence requires two elements: an individual's belief on her own productivity and information on her actual underlying productivity.

We move beyond previous approaches to identify overconfident individuals in two respects: we propose a definition of absolute overconfidence that takes individual *probabilistic* productivity beliefs (as opposed to point beliefs) into account and regards *observed productivity as a noisy measure of actual productivity*. Based on that definition, we offer a novel strategy for identifying significant overconfidence at the individual level.

To our judgment, assuming probabilistic productivity beliefs as opposed to point beliefs is more plausible and robust for several reasons. First, point beliefs imply that individuals are absolutely certain in their beliefs, which is often not the case. Second, when individuals are asked to reveal a point belief, it is often not clear what is actually elicited: mean, median, or mode of their belief distribution. Even when the moment to be measured is explicitly specified, it can be too complicated to understand and measurement may eventually fail to elicit the moment accurately. Finally, a framework building on belief distributions is more general and contains point beliefs as a special case.[5] Despite these advantages, we are not aware of other papers that use probabilistic beliefs to identify overconfident individuals.

Based on the assumption of probabilistic beliefs, we define an individual as overconfident if the median of her belief distribution exceeds her actual underlying productivity. The intuition behind this definition is that an individual is overconfident if she assigns more than 50% probability mass of the belief distribution to productivity levels higher than her true productivity, i.e. if she believes that

[4] In principle, overconfidence can result from overestimating own productivity for a realistic assessment of task difficulty and/or from underestimating exogenous task difficulty (compare Heidhues, Koszegi, and Strack (forthcoming) who portray an agent who simultaneously holds beliefs on her ability and an external fundamental, which, together with effort, determine her performance). Our use of the term overconfidence covers both possible sources of absolute overconfidence.

[5] For those reasons, probabilistic beliefs have become widely used. For over a century, researchers have elicited beliefs in the form of probability distributions and it has become a common practice in surveys since the early 1990s (Manski and Neri, 2013). Experimental economists often favor probabilistic belief elicitation for its superior predictive power of choice behavior, e.g. Nyarko and Schotter (2002). Experimental economic research using probabilistic beliefs is growing (e.g. Manski and Neri, 2013, Neri, 2015, Gee and Schreck, 2018).

it is more likely that her productivity exceeds her true productivity than vice versa. As a further advantage, like any quantile, the median is more robust than the mean when it comes to outliers in the belief distribution. In Section 4, we will describe a ball allocation task that we use to elicit belief distributions from which we infer median beliefs.

We now turn to the difference between observed and actual underlying productivity. Any measurement of productivity is subject to noise, and therefore only partially represents the actual productivity underlying the measurement. Due to temporary variation in unobserved factors such as luck, concentration, or distraction measuring productivity repeatedly may reveal different observed values for the same individual given the same actual underlying productivity. This measurement error could result in misclassifying individuals in terms of overconfidence. As a consequence, observed productivity alone is not a reliable benchmark to compare the productivity beliefs with.

In order to avoid misclassification due to measurement error, we consider observed productivity as a random draw from a productivity distribution that is shaped by the actual underlying productivity. Assume that for an individual i, the outcome of a task is binary: either a success with probability p_i or a failure with probability $1 - p_i$. p_i is the actual underlying productivity of individual i. The number of realized successes then obeys the binomial distribution $B(p_i, n)$, where n corresponds to the number of observed task outcomes. By the Central Limit Theorem, the observed success rate (observed productivity) is asymptotically normal,

$$q_i \sim N(p_i, \frac{p_i(1-p_i)}{n}),$$

where q_i is the observed productivity. With 95% probability, the actual productivity falls into the confidence interval around the observed productivity $[q_i - 1.96\sigma_i, q_i + 1.96\sigma_i]$, where $\sigma_i = \sqrt{\frac{q_i(1-q_i)}{n}}$. The boundaries of the confidence interval can be computed using the observed productivity q_i and the number of observed outcomes n.

We identify an individual as significantly overconfident if the median of her belief distribution m_i exceeds the upper limit of this confidence interval, i.e. $m_i > q_i + 1.96\sigma_i$. Analogously, an individual is classified as underconfident if $m_i < q_i - 1.96\sigma$. Individuals with $q_i - 1.96\sigma_i \leq m_i \leq q_i + 1.96\sigma_i$ are well-calibrated since their productivity beliefs are either accurate or very similar to their actual productivity. We summarize our definition of significant absolute overconfidence at the individual level in definition 1.

Definition 1 In tasks with a binary outcome, an individual i is significantly overconfident if

$m_i > q_i + 1.96\sigma_i$, where m_i is the median of her belief distribution, q_i is observed productivity, $\sigma_i = \sqrt{\frac{q_i(1-q_i)}{n}}$, and n is the number of observed task outcomes.

3. Model and Hypotheses

In this section, we first specify a theoretical model on how productivity beliefs translate into effort provision. We then derive hypotheses concerning the motivation value of (over)confidence as well as the effect of information provision about actual productivity on the effort provision of overconfident subjects.

Model

A single agent has to decide on the effort level $e \in [\underline{e}, \bar{e}]$ to exert in a task with production function $Q(e,p)$, where p denotes her *a priori* unknown productivity, $p \in [0,1]$. The agent's utility function takes the following form[6]:

$$U(e,p) = rQ(e,p) - L(e). \tag{1}$$

For each unit produced, the agent gains a utility increment of $r > 0$, e.g. a piece rate payment. Effort provision induces a cost represented by the loss function $L(e)$. Suppose $Q(e,p)$ and $L(e)$ are continuous and twice differentiable. We introduce the following assumptions:

Assumption 1 (i) $Q_e > 0$, $Q_{ee} \leq 0$, (ii) $Q_p > 0$, (iii) $L_e > 0$, $L_{ee} \geq 0$ (iv) $Q_{ep} > 0, \forall e \in [\underline{e}, \bar{e}], \forall p \in [0,1]$.

Part (i) implies that the marginal return to effort is positive and monotonically decreasing. Part (ii) assumes that production is strictly monotonically increasing in productivity for given effort level. Part (iii) guarantees that the marginal utility loss is positive and monotonically increasing in effort. Part (iv) formalizes complementarity between effort and productivity.[7] Under these assumptions, the following proposition holds:

Proposition 1 *Holding a higher belief on own productivity leads to higher effort provision.*

Proof: Let p_l and p_h denote two productivity beliefs with $p_l < p_h$, e_l and e_h the respective utility maximizing effort levels. The following first order conditions must hold: $rQ_e(e_h)_{|p_h} = L_e(e_h)$ and $rQ_e(e_l)_{|p_l} = L_e(e_l)$. By $Q_{ep} > 0$, $rQ_e(e_h)_{|p_l} < rQ_e(e_h)_{|p_h}$, which implies $rQ_e(e_h)_{|p_l} < L_e(e_h)$. Suppose $e_l \geq e_h$. Since $Q_{ee} \leq 0$ and $L_{ee} \geq 0$, we then have $rQ_e(e_l)_{|p_l} \leq rQ_e(e_h)_{|p_l} < L_e(e_h) \leq L_e(e_l)$,

[6] For the sake of simplicity, the utility function refers to a risk-neutral individual. Results remain qualitatively the same if we introduce risk aversion or risk proclivity.

[7] These assumptions are satisfied by our experimental design and the data we obtain.

which contradicts equation $rQ_e(e_l)_{|p_l} = L_e(e_l)$. Therefore, for $rQ_e(e_l)_{|p_l} = L_e(e_l)$ to hold, it must be that $e_l < e_h$. *qed*

To introduce belief updating, we consider the following three-period model. At the center of our interest is an overconfident agent, whose prior belief on her own productivity is unrealistically high. In period 1, let her belief on her productivity be $\widehat{p}_0$, $\widehat{p}_0 > p$, where p is her actual productivity.[8] The agent exerts her utility maximizing effort e_0 based on her productivity belief $\widehat{p}_0$, which results in output $q = Q(e_0, p)$, while the agent has anticipated to produce $\widehat{q}_0 = Q(e_0, p_0)$. Since Q is monotonically increasing in p, $q < \widehat{q}_0$. In period 2, the agent is informed about the real output q and, as a response, adjusts her productivity belief to $\widehat{p}_1$, which satisfies $Q(e_0, \widehat{p}_1) = q$. Given $Q_p > 0$, it must hold that $\widehat{p}_1 < \widehat{p}_0$. That is, faced with adverse feedback, the agent adjusts her belief on her productivity downwards. In period 3, the agent exerts her utility maximizing effort e_1 regarding $\widehat{p}_1$. As $\widehat{p}_1 < \widehat{p}_0$, it follows directly from Proposition 1 that $e_1 < e_0$.

Corollary 1 *An unexpectedly low productivity feedback causes a decrease in effort provision.*

Median Utility Maximization

When probabilistic beliefs are taken into account, $\widehat{p}_0$ and $\widehat{p}_1$ refer to the medians of individual belief distributions. Following the monotonicity of the production function in argument p, inserting a median belief into the utility function gives the median of the agent's ex-ante probabilistic belief on her ex-post utility. This effectively implies that the agent exerts the effort level that maximizes the median of her utility distribution. The study of quantile maximization dates back to Manski (1988) who points out that "if actions are characterized by probability measures of outcomes, then we should consider rational any pattern of behavior consistent with the existence of a preference ordering on the space of these probability measures." More recently, quantile maximization was axiomatized by Rostek (2010).

In our set-up, the intuition for assuming median utility maximization is the following: Let the optimal effort corresponding to median belief on productivity be e^*. An agent with probabilistic productivity belief believes that it is unlikely (less probable than 50%) that her productivity is lower than her median belief. Due to the positive monotonicity of optimal effort in productivity belief (Proposition 1), she would not exert lower effort than e^*. At the same time, she believes that it is

[8] For expositional clarity, we set aside probabilistic beliefs for now, allowing us to proceed without imposing any assumption on the belief probability distribution. Later, probabilistic belief will be introduced by using the median of the belief distribution as p in the model.

also unlikely (less likely than 50%) that her productivity is higher than the median belief. Hence, the effort she exerts will not exceed e^*. Combining the arguments above, her optimal effort level is e^*, which means the agent chooses her effort provision to maximize median utility.

Hypotheses

Based on the model, we derive the following two hypotheses.

Hypothesis 1 (motivation value of confidence) A higher belief on own productivity leads to higher effort provision.

When faced with an effort-intensive task, individuals have to decide how much effort to exert. Without knowledge about their true productivity, they need to rely on their belief on their productivity to make this decision. Whether diligence is induced by higher or lower confidence in one's own productivity constitutes the first research question that we aim to answer.

Our model assumes that individuals choose their effort level by balancing expected marginal benefits and marginal costs of effort provision in order to maximize their utility. As a consequence, individuals with higher productivity beliefs will exert higher effort since they expect to obtain higher marginal benefits from effort provision. This motivation value of confidence is particularly relevant for individuals who overestimate their own productivity, due to its potential to offset the suboptimal decision making caused by overconfidence through exaggerated effort provision (e.g. Bénabou and Tirole, 2002, Compte and Postlewaite, 2004).

In contrast, if individuals are satisficers (Simon, 1955) instead of utility maximizers, more confident individuals are predicted to exert less effort. In general, satisficers will stop an activity (such as information search or, in our case, effort provision) as soon as they reach their individually accepted target outcome level (so-called "aspiration level") – despite the fact that they could obtain an even better outcome by continuing the activity. In our case, the aspiration level corresponds to an individual target utility level that is less than the maximal attainable utility. If effort and productivity are complements, more productive individuals can reach a given aspiration level by exerting less effort than less productive individuals. As a consequence, satisficing individuals with higher productivity beliefs are predicted to exert less effort.

Hypothesis 2 Informing overconfident individuals on their own productivity reduces their effort provision.

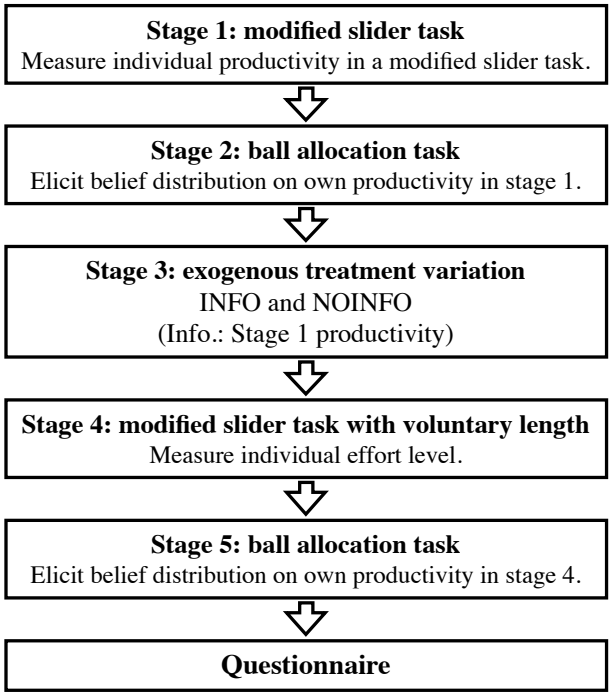
When overconfident individuals receive feedback on their actual productivity, they will adjust their productivity beliefs downwards. As predicted by Hypothesis 1, this downward adjustment of

productivity beliefs due to de-biasing information will reduce effort provision.

4. Experimental Design and Implementation

We designed a laboratory experiment to test Hypotheses 1 and 2. Figure 1 presents an overview of the experimental design that consists of five stages.

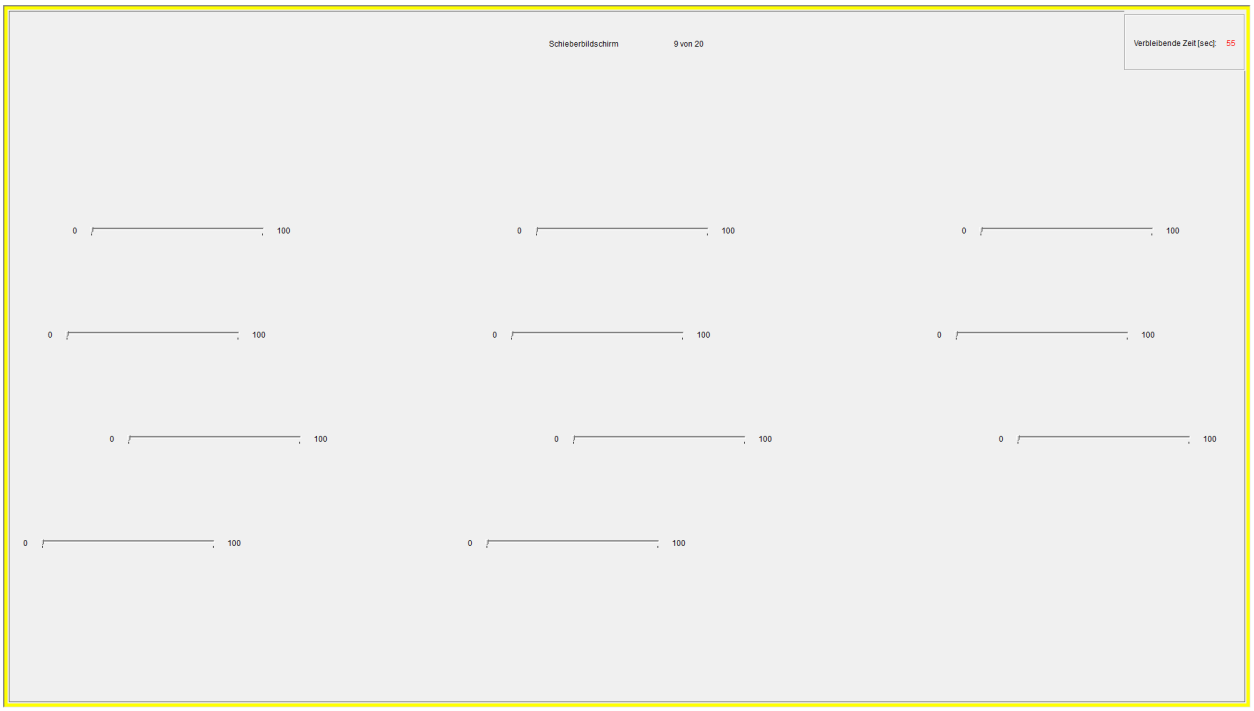
Figure 1: *Overview of Experimental Design*



Stage 1: In stage 1, we measure individual productivity using an adapted version of the well-established slider task (Gill and Prowse, 2018). Subjects worked on 20 slider screens that each displayed 11 sliders (see Figure 2).[9] The subjects' task was to drag each slider with the mouse and to position it into the small interval $[49.5, 50.5]$ at the middle of a scale that ranged from 0 to 100. For each subject, the proportion of correctly positioned sliders among all 220 sliders serves as the measure of individual productivity. Subjects earned a piece rate of 1 point for each successfully

[9] We used 11 sliders per screen to eliminate obvious focal points in the later belief elicitation. Slider screens and "choice list screens" were shown alternately, each 20 times. The choice list screens elicited subjects' median beliefs on their own productivity in an incentive compatible way. A detailed description of the choice list screens, the reasons why we do not use them in the main analysis, and related robustness checks are provided in Appendix B.

Figure 2: *A Slider Screen*



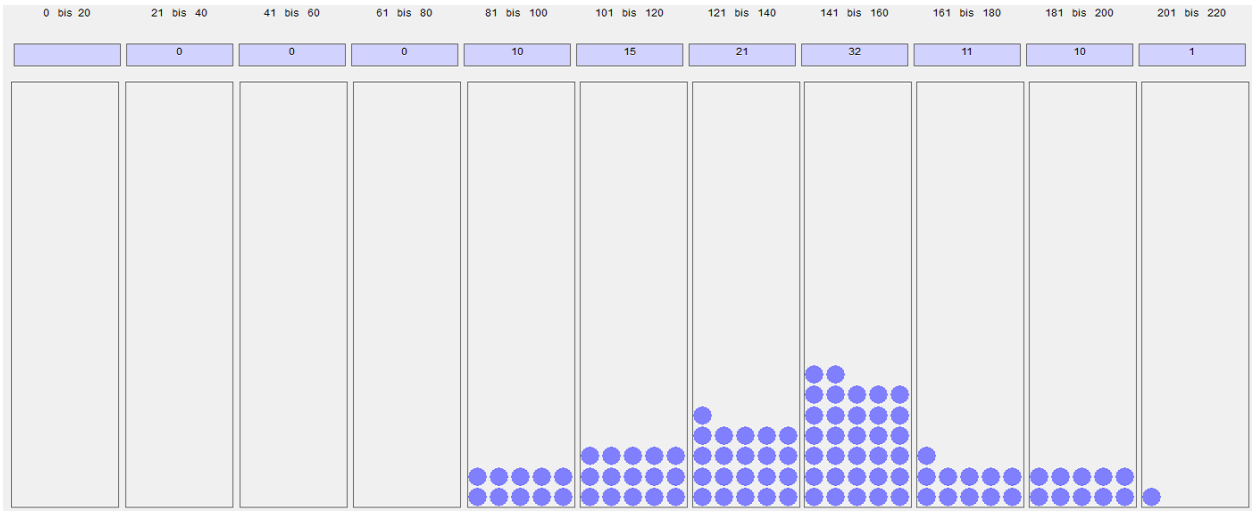
positioned slider. At the end of the experiment, each point was exchanged into 0.05 Euro.

In contrast to the original version of the slider task (Gill and Prowse, 2018), the numerical position of each slider was not displayed on screen and subjects could only guess the slider's position by eye-balling. Thus, subjects could not perfectly monitor their productivity, offering scope for over- or underconfidence. Further advantages of the slider task are that it does not require prior knowledge and does not exhibit a time trend in performance, in line with the assumption that underlying true productivity is constant over time.[10]

Subjects had 55 seconds to work on each slider screen. Fixing an upper time limit ensures that our measure of individual productivity, the number of correctly positioned sliders, is comparable across subjects. Only after 55 seconds, subjects could proceed to the next screen that appeared automatically. Our data suggest that 55 seconds were sufficient for subjects to work on all 11 sliders: on average, subjects left only 5 out of 220 sliders untouched in stage 1. After 20 slider screens, the experiment moved on to stage 2 automatically.

[10] In a Tobit panel regression of the number of correctly positioned sliders per screen on a screen sequence number and an additional dummy for the last screen, the coefficient of the screen sequence number is not significant (-0.006, $p = 0.13$) and the dummy for the last screen is marginally significant only (-0.19, $p = 0.07$). The absence of a trend for productivity implies $Q_{ee} = 0$ so that Part (ii) of Assumption 1 of our model is met.

Figure 3: *Ball Allocation Task*



Stage 2: In Stage 2, we elicit each subject's belief distribution on the overall number of correctly set sliders in Stage 1 using a ball allocation task, adapted from Delavande and Rohwedder (2008) (see Figure 3). In the ball allocation task, each subject had 100 balls. Each ball represents one percentage point of probabilistic belief and had to be allocated to one of 11 bins representing the intervals [0, 20], [21, 40],..., [201, 220]. The number of balls a subject allocated into each bin indicates the probability, with which the subject believed that the actual number of correct sliders falls in the bin's interval. The allocation of balls therefore approximates a subject's belief distribution. The ball allocation task was incentivized using the randomized Quadratic Scoring Rule (rQSR) adapted from Drerup, Enke, and Von Gaudecker (2017) and Schlag and van der Weele (2013). For each subject i, we first computed a number Y_i following the formula below:

$$Y_i = \sum_{j=1}^{11} (b_i^j - 100 \times 1_j)^2,$$

where $j \in 1, 2, 3, ..., 11$ denotes the respective bins, b_i^j denotes the number of balls subject i assigned to bin j, indicator 1_j equals 1 for the bin that contains the actual number of correctly positioned sliders and 0 otherwise. Y_i is increasing in the number of balls a subject allocated into the *wrong* bins. Y_i has a minimum of 0 and a maximum of 20,000. Subject i obtained 30 points if and only if $Y_i < X_i$, where X_i is a random number drawn from the uniform distribution $U[0, 20000]$. This payment scheme ensures that the probability of winning the lottery increases in the number of balls allocated to the correct bin, while the magnitude of reward remains fixed. As a consequence, this procedure incentivizes subjects to reveal their beliefs truthfully regardless of their risk preference,

since risk-averse, risk-neutral and risk-seeking subjects alike will strive to maximize the winning probability of the lottery by allocating the balls in accordance with their true belief distribution.

Stage 3: Subjects were randomly assigned to one of two treatments (INFO or NOINFO), in which subjects either received feedback on their own productivity or not. In the INFO treatment, the computer screen of each subject displayed her own actual number of correctly set sliders on each slider screen in stage 1 and their aggregate, along with the corresponding beliefs. Subjects could read this private information for up to 2 minutes and could proceed to Stage 4 by clicking a "continue" button. After 2 minutes, they would receive a reminder urging them to click "continue". In the NOINFO treatment, subjects did not receive any information. To keep the treatments similar, they were given a break of up to 2 minutes, which was announced on the screen.

Stage 4: The set-up of Stage 4 was similar to the one in Stage 1: At most twenty slider and choice list screens were shown alternately and subjects earned a piece rate of 1 point for each successfully positioned slider. Piece rate payment ensures that effort and productivity are complementary in Stage 4 and that measurement of overconfidence is consistent in Stage 1 and 4.

In contrast to Stage 1, after completing each screen in Stage 4 subjects could choose between continuing to the next slider screen and terminating the slider task. Subjects knew beforehand that they could work on up to 20 slider screens. Subjects could also omit the slider task in Stage 4 altogether and enter Stage 5 directly by clicking a "terminate" button on the instruction screen at the beginning of the stage. The number of slider screens a subject worked on serves as the measure of her effort level, with a minimum of 0 and a maximum of 20. Subjects could leave the laboratory once they had individually gone through all stages of the experiment. Therefore, exerting less effort made the experiment shorter. In order to avoid potential spillovers of one departure on the leave or stay decision of the remaining subjects, we invited the subjects to come to the laboratory any time within a 3-hour time range (either from 9 am to 12 am or from 2 pm to 5 pm). Observing a departure of a fellow subject did not provide information on how long she had worked.

Stage 5: We used the same ball allocation task as in Stage 2 to elicit each subject's belief distribution on the total number of correctly positioned sliders in Stage 4. Again, each subject had 100 balls, symbolizing 100 percentage points. Unlike in Stage 2, the length of intervals represented by each bin was determined by the number of screens a subject had worked on in Stage 4. For example, after working on 4 screens, a subject saw bins representing the intervals $[0, 4], [5, 8], ..., [41, 44]$. The individual-specific upper bound was the total number of sliders a subject had worked on. Subjects were incentivized in the same way as in Stage 2 such that the allocation of balls approximated the

belief distribution on the number of correctly positioned sliders in Stage 4. Subjects who had skipped Stage 4 skipped also Stage 5.[11]

Final questionnaire: After Stage 5, subjects answered a questionnaire on, among other things, socio-demographics, risk and ambiguity preferences, personality traits and survey measures of absolute overconfidence, relative overconfidence, and over-precision.

Payments: Subject were informed about their level of earnings and paid in cash right after they had finished the experiment. Total earnings were the sum of the following components: the amount earned in the slider task and choice lists in Stages 1 and 4, and in the ball allocation task in Stages 2 and 5; a random payoff of either 0, 1, or 2.5 Euro for revealing risk preferences in a Holt and Laury table (Holt and Laury, 2002) and a random payoff of either 0 or 2 Euro for revealing ambiguity aversion in the questionnaire; a 1 Euro reward for answering the questionnaire and a 2 Euro show-up fee. On average, subjects earned 11.6 Euro.

Instructions and control questions: Detailed paper instructions were handed out before Stage 1 and Stage 2. Subjects kept and could refer to the instructions until the end of the experiment. In addition, subjects answered two control questions designed to test and improve their understanding of the corresponding tasks before each of Stage 1 and Stage 2. The correct answer to each control question consisted of more than one element. Only when all correct elements were ticked, the answered was considered correct. When a correct answer was submitted, the experiment proceeded. If an answer was wrong on the first try, a subject learned that the answer was wrong and was encouraged to try again. If subjects failed again on the second try, the correct answer was shown along with an explanation. The recorded answers to the control questions show that the vast majority subjects understood the tasks well before carrying them out.

Implementation: We run six sessions in the BonnEconLab in Bonn, Germany in October and November 2016. 180 participants aged 17 to 61 took part in the experiment (average age of 23, with 19 and 28 being the 10% and 90% quantiles, respectively). 73 of them were male and 107 were female. The subject pool consisted mainly of students from various majors in University of Bonn (89%). 89 subjects were randomly assigned to the INFO treatment and 91 to the NOINFO treatment. Treatments were randomized within sessions to balance the data with respect to time of the day, weekday, and weather etc. The experiment lasted about one hour on average. We used

[11] One subject anticipated that we would ask for beliefs in Stage 5 again, sat strategically idly in Stage 4 and allocated all 100 balls to bin 1 in Stage 5 in order to earn the reward in Stage 5 with certainty. This subject earned less (9.05 Euro) than the average payoff of 11.60 Euro.

z-tree (Fischbacher, 2007) to implement the experiment and hroot (Bock, Baetge, and Nicklisch, 2014) for inviting subjects and recording their participation. Instructions and interfaces on the client computers were written in German, as subjects were either German natives or German speaking. Appendix A contains an English translation of the instructions.

5. Results

In this section, we first summarize key features of our data and identify overconfident subjects. We then address Hypotheses 1 and 2, before we provide further results and robustness checks.

The analysis relies on observations from 176 subjects, 88 in the INFO and 88 in the NOINFO treatment. We exclude one subject who stated in the final questionnaire that she exited stage 4 accidentally by pressing the wrong button and three subjects who gave wrong answers to all four control questions. As a result of random assignment, observed productivity and median productivity beliefs in Stage 1 do not differ significantly across treatments (Mann-Whitney-U test, $p = 0.85$ and $p = 0.48$, respectively).[12] Out of a total of 220 sliders, the mean number of correctly set sliders in Stage 1 is 40 (std. dev. 17.38) in INFO and 38 (std. dev. 14.14) in NOINFO, while the average median productivity belief is 108 (std. dev. 32.65) in INFO and 107 (std. dev. 33.71) in NOINFO.

5.1. Identification of Overconfident Subjects

We first compute each subject's median belief on Stage 1 productivity using the corresponding histogram of the probabilistic belief distribution from the allocation of balls in Stage 2. The bins of the histograms are the same as the bins in the ball allocation task. We compute the median of this belief distribution m_i as

$$m_i = \beta_i - d_i \times \frac{\sum_{j=1}^{k_i} b_i^j - 50}{b_i^{k_i}},$$

where i indicates the subject, j denotes the serial number of the bins. b_i^j is the number of balls subject i allocates to bin j. d_i represents the length of the intervals. k_i denotes the serial number of the bin that contains the median and thus satisfies $\sum_{j=1}^{k_i-1} b_i^j < 50 \leq \sum_{j=1}^{k_i} b_i^j$. $b_i^{k_i}$ is the number of balls that subject i allocates to the bin that contains the median. β_i is the upper bound of subject i's k_ith interval.

[12] Throughout the paper, we report p-values for two-sided tests. The next paragraph describes in detail how we infer median productivity beliefs from the ball allocation task.

Following the identification strategy outlined in Section 2, we classify 166 subjects as overconfident (83 in INFO and 83 in NOINFO): the median of their belief distribution exceeds the upper limit of the 95% confidence interval around their observed productivity. 5 subjects are classified as underconfident (3 in INFO and 2 in NOINFO) and 5 as well-calibrated (2 in INFO and 3 in NOINFO). The high share of overconfident subjects is compatible with a large body of research, which documents that overconfidence is a prevailing phenomenon, see e.g. the studies cited in Plous (1993) or Malmendier and Tate (2005) for a more recent study on absolute overconfidence.[13]

5.2. Result 1: The Motivation Value of (Over)Confidence

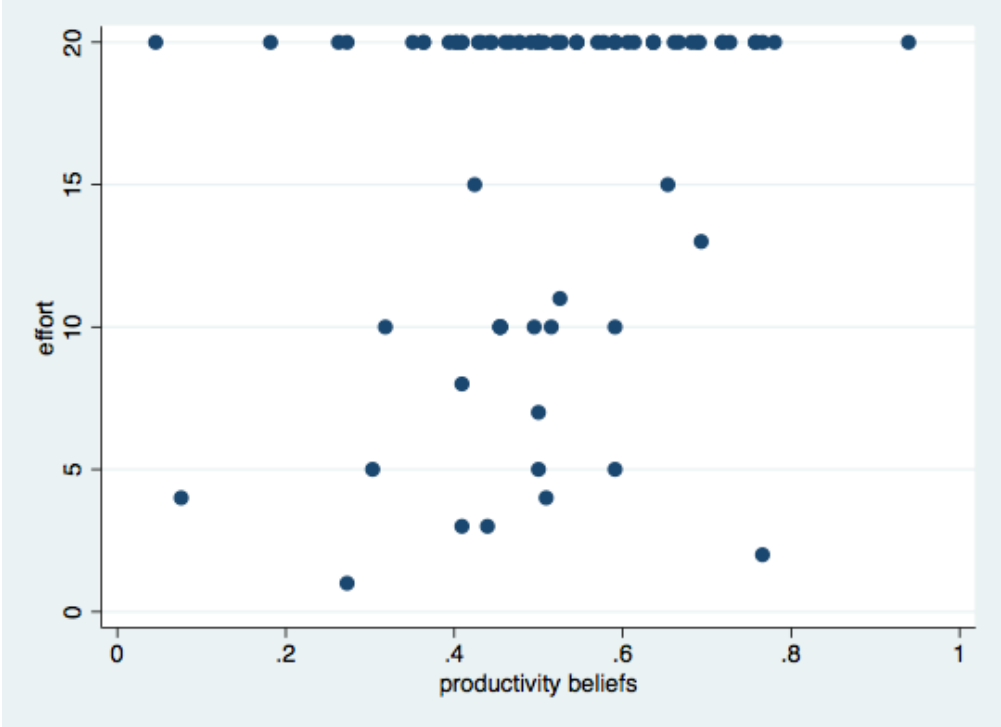
We now turn to Hypothesis 1, which states that higher beliefs in one's own productivity lead to higher effort provision (motivation value of confidence). Figure 4 depicts individual effort choices and productivity beliefs in treatment NOINFO. The vertical axis displays chosen effort in the slider task in Stage 4, measured by the number of screens worked on. The horizontal axis represents subjects' median productivity belief on the share of correctly positioned sliders that is elicited by the ball allocation task in Stage 5, i.e. m_i/(number of screens worked on in stage 4 * 11 sliders). As hypothesized, productivity beliefs and effort provision are significantly positively correlated (NOINFO treatment, Pearson correlation, $r = 0.25$, $p = 0.02$). Considering overconfident subjects only, the Pearson correlation between productivity beliefs and effort choices increases to $r = 0.32$, $p < 0.01$ (NOINFO treatment).

Figure 4 reveals that a significant fraction of subjects exerted maximum effort by working on all 20 slider screens (59 subjects), while others stopped working earlier.[14] It seems likely that many of the maximum effort subjects would have worked on more than 20 screens if that option would have been available. In line with that intuition and the motivation value of confidence, the maximum effort subjects have higher productivity beliefs than the remaining subjects (median beliefs are 0.56 and 0.49, respectively, Mann-Whitney-U test, $p = 0.06$). To take censoring into account, we display results of a Tobit regression of effort choice on productivity beliefs in Table 1. Results in Table 1 confirm that higher productivity beliefs predict higher effort provision. On average, subjects with a 10 percentage points higher productivity belief work on 2.6 additional screens, i.e. increase their effort choice by 13 percentage points (2.6/20, i.e. the maximum number of screens).

[13] Dated back to the 1770s, Adam Smith already pointed out that "every man" tends to be overconfident about his chance of gain (Smith 1776).

[14] Both kinds of behavior are compatible with Part (iii) of Assumption 1 of our model. In particular, exerting

Figure 4: *The relation between beliefs on productivity and effort provision*



Notes: The variable *productivity beliefs* represents subjects' median beliefs on their success rate in Stage 4, inferred from the ball allocation task in Stage 5. The variable *effort* corresponds to the number of slider screens worked on in Stage 4. Only observations from the NOINFO treatment are displayed.

To sum up, in line with the motivation value of confidence, we find that individuals with higher productivity beliefs exert more effort.

5.3. Result 2: Information Reduces Overconfident Subjects' Effort Provision

Hypothesis 2 postulates that overconfident subjects who are informed about their own actual productivity will adjust their productivity beliefs downwards and lower their effort provision.

In line with Hypothesis 2, we find that information provision decreases overconfident subjects' exaggerated productivity beliefs. In Stage 5, the productivity beliefs of overconfident subjects in the INFO treatment are significantly lower than in treatment NOINFO (see column (4) of Table 2, Mann-Whitney-U test, $p < 0.01$). Also within the INFO treatment, overconfident subjects significantly reduce their productivity beliefs after receiving information on their own productivity

positive, but less than maximum effort implies $L_{ee} > 0$.

Table 1: *Tobit regression of effort choice in Stage 4 on productivity beliefs in Stage 5*

Dependent variable: effort level in Stage 4	
Productivity belief in Stage 5	26.64**
	(12.91)
Sigma	15.71
	(2.55)
Constant	13.80**
	(6.25)
N	86
Pseudo R^2	0.015

The variable *effort level* is measured by the number of screens worked on in Stage 4. The independent variable *productivity belief* refers to the median belief in percentages (expressed between 0 and 1) elicited by the ball allocation task in Stage 5. The productivity beliefs in Stage 4 are missing for 2 subjects who chose to skip Stage 4. N=59 observations are censored from above at 20.

(comparison of columns (2) and (4), last row of Table 2, Wilcoxon signed-ranks test, $p < 0.01$). While the updating of beliefs is substantial, it is not perfect. In contrast, in the NOINFO treatment overconfident subjects' beliefs remain stable over time (comparison of columns (2) and (4), second to last row of Table 2, Wilcoxon signed-ranks test, $p = 0.95$).

Moreover, informed overconfident subjects substantially reduce their effort provision. On average, they work on 13 instead of 16 screens in Stage 4 (see Figure 5, Mann-Whitney-U test, $p = 0.04$). Thus, overconfident subjects in treatment INFO exert about 19% lower effort than those in NOINFO treatment. In line with Hypothesis 2, debiasing overconfident individuals' beliefs on their own productivity leads to lower effort provision. Due to the random assignment of subjects to the INFO and NOINFO treatment, our data provide causal evidence that informing overconfident subjects about their own productivity reduces their effort provision.

5.4. Information Reduces Overconfident Subjects' Quality of Work

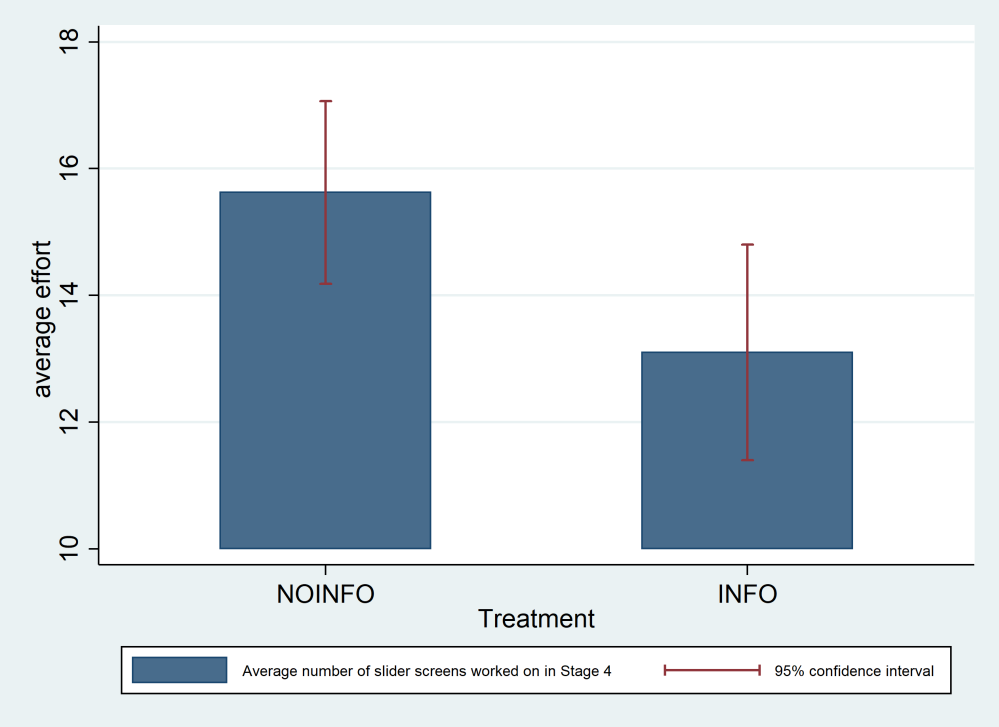
So far, we have focused on the effect of information on overconfident subjects' effort provision at the extensive margin, measured by the number of slider screens subjects work on. However, not only does the amount of work the informed overconfident subjects do decline significantly, but also the

Table 2: *Productivity and beliefs on productivity of overconfident subjects in Stage 1 and Stage 4*

Treatment	(1) Productivity in Stage 1	(2) Belief on productivity in Stage 2	(3) Productivity in Stage 4	(4) Belief on productivity in Stage 5
NOINFO	18%	50%	16%	50%
INFO	18%	51%	15%	23%

Productivity refers to the average % of correctly positioned sliders and beliefs reflect the corresponding average belief on productivity. Beliefs on productivity in Stage 1 and 4 are measured in Stages 2 and 5, respectively.

Figure 5: *Effort provision in Stage 4*



quality of their work (effort at the intensive margin). We measure quality by the number of correctly positioned sliders per screen that subjects have worked on. In particular, for informed overconfident subjects, the percentage of correctly positioned sliders per screen drops significantly from 18% in Stage 1 to 15% in Stage 4 (Wilcoxon signed-ranks test, $p < 0.01$). In contrast, uninformed overconfident subjects' number of correctly positioned sliders per screen does not change significantly

from Stage 1 to Stage 4 (18% in Stage 1 and 16% in Stage 4, Wilcoxon signed-ranks test, $p = 0.20$). This finding and Hypothesis 2 share the same intuition that informing overconfident subjects about their exaggerated beliefs on their own productivity hurts their performance.

5.5. Robustness Checks

Before discussing the implications of our findings, we exclude several alternative explanations of our data.

A first concern might be that receiving information about own Stage 1 productivity enables subjects to infer their payment from Stage 1, which, despite the small stakes, might raise the salience of wealth effects. 83 out of 88 of subjects in treatment INFO receive information that induces a downward adjustment of expected earnings. Thus, in the presence of wealth effects information should induce higher levels of effort provision in Stage 4 if the marginal utility of money is decreasing. Such an increased effort provision would counteract the hypothesized negative effect of information on effort provision in Stage 4. However, we find a strongly negative effect of information on effort provision. Moreover, the correlation between productivity in Stage 1 and effort provision in Stage 4 is low and not significant in treatment INFO (Pearson correlation, $r = 0.03$, $p = 0.76$). Consequently, wealth effects cannot have a major impact on effort provision in Stage 4.

A second concern could be that information affects beliefs and, as a consequence, effort provision not only through its content but also through further channels such as emotions like disappointment. While we consider emotion effects a possible inherent and integral part of any information provision, our paper focuses solely on the instrumental value of information content, since we aim at providing empirical evidence on the motivation value of overconfidence as it is used in microeconomic modeling, e.g. in Krähmer (2007) or Bénabou and Tirole (2002). Results in Table 3 suggest that information provision influences effort exertion by affecting productivity beliefs, while emotions play at most a subordinate role. Table 3 displays results of a Tobit regression of Stage 4 effort provision on the corresponding productivity beliefs and a dummy variable *information* that takes the value 1 for subjects in the INFO treatment and 0 otherwise. Productivity beliefs are a highly significant predictor of effort provision, while the information dummy is not (column (1)). This result is qualitatively the same when restricting the sample to overconfident subjects only, for whom information conveys bad news (column (2)).

Finally, on top of beliefs on own productivity individual personality traits such as locus of control and conscientiousness are considered to be predictors of individual effort provision and performance,

Table 3: *Tobit regression*

Dependent variable: effort level in Stage 4		
	All	OC only
Productivity belief in Stage 5	19.55**	22.89**
	(9.01)	(9.34)
Information	-0.17	1.71
	(3.54)	(3.67)
Sigma	15.34	15.26
Constant	16.83***	14.33***
	(4.70)	(4.90)
N	169	160
Pseudo R^2	0.012	0.013

The variable *effort level* is measured by the number of screens worked on in Stage 4. The independent variable *information* takes a value of 1 in the INFO treatment and 0 in the NOINFO treatment, *productivity belief* refers to the median belief in percentages, elicited by the ball allocation task in Stage 5. The productivity beliefs in Stage 4 are missing for 7 subjects who chose to skip Stage 4.

see e.g. Almlund et al. (2011). If these traits are correlated with productivity beliefs, they could possibly cause a spurious correlation between productivity beliefs and effort provision or bias our estimates. Since we obtained independent measurements of locus of control and conscientiousness in the final questionnaire of the experiment[15], we can rule that out: neither locus of control nor conscientiousness are significantly correlated with a subject's belief in own productivity (Spearman correlations are 0.001 and 0.021, respectively, both $p > 0.78$).

6. Conclusion

Our results provide first empirical evidence on the existence of a motivation value of absolute overconfidence that numerous microeconomic models incorporate as a basic ingredient of their modeling approach (e.g. Bénabou and Tirole, 2002, Compte and Postlewaite, 2004, Gervais and Goldstein, 2007, Ludwig, Wichardt, and Wickhorst, 2011) and contrast predictions based on satisficing (Simon, 1955): Ceteris paribus, individuals with a higher belief on their own productivity exert higher levels of effort and this relationship also holds for overconfident individuals.

Moreover, we show that de-biasing overconfident individuals by informing them about their true productivity reduces their productivity beliefs and hurts their effort provision. According to our basic model, the over-provision of effort by overconfident agents is to their detriment since marginal costs of effort provision will exceed marginal benefits. In richer settings, however, overconfident individuals (Bénabou and Tirole, 2002, Compte and Postlewaite, 2004) or other parties such as their employers, team members or partners (Gervais and Goldstein, 2007, Ludwig, Wichardt, and Wickhorst, 2011) may well benefit from their exaggerated effort provision. For example, employers could restrain from providing accurate performance feedback to an overconfident employee in order to continue benefiting from her exaggerated effort provision in the future.[16] Thus, in contrast to the general notion that more accurate beliefs enhance decisions, our results imply that not providing overconfident individuals with information on their own productivity can sometimes be beneficial.

More generally, our findings contribute to the empirical literature on the consequences of overconfidence and add insights on factors that motivate effort, compare DellaVigna and Pope (2017).

[15] Our measure of locus of control comprises 10 items adapted from Rotter (1966) that are used in the 2005 wave of the German Socio-Economic Panel. To measure conscientiousness, we use the two items proposed by Rammstedt and John (2007).

[16] This implication is in line with plenty empirical evidence that subjective performance evaluations in firms often tend to be too lenient (Prendergast, 1999).

In particular, our results suggest boosting confidence as an effective and potentially cost-efficient way to enhance effort provision.

In terms of research methods, the contribution of our paper is twofold. First, we add to the literature addressing ceiling effects in real effort tasks in general and the slider task in particular, i.e. participants' tendency to exert (close to) maximum effort in experimental real effort tasks independent of the incentives (e.g. Corgnet, Hernán-González, and Schniter, 2015, Eckartz, 2014, Gächter, Huang, and Sefton, 2016, Araujo et al., 2016, Goerg, Kube, and Radbruch, 2017). We show that offering subjects flexibility on when to start and end working in an experiment at least mitigates ceiling effects in the slider task. More importantly, we propose a definition of overconfidence which takes into account two important and yet often ignored facts, namely measurement error in productivity and that individuals typically hold probabilistic beliefs on own productivity. Based on this definition, we develop an innovative method for empirically identifying significant absolute overconfidence *at the individual level* that can be applied more broadly in future work.

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Appendix (for online publication)

Appendix A – Experimental Instructions

This section contains an English translation of the experimental instructions that were originally written in German. Subjects received a printed version of the instructions for part 1 once they sat down in the laboratory.

The experiment

General explanations

Welcome to this economic experiment.

In the course of this experiment you can earn a nonnegligible amount of money. The exact amount strongly depends on your decisions. So please read the following instructions carefully! If you have any questions, please raise your hand and we will come to your seat.

During the whole experiment, it is not allowed to talk to the other participants, to use cell phones, or to launch any programs on the computer. Disregarding any of these rules will lead to your exclusion from the experiment and from all payments.

The earnings resulting from your decisions will be paid out to you in cash at the end of the experiment. During the experiment we do not talk about Euro but points. Consequently, our total payment will be calculated in points first. At the end of the experiment, your total points will be converted into Euro, using the following rule:

1 point = 5 cents

Additionally, you will receive 40 points for showing up on time that you will be paid at the end of the experiment independent from your other decisions in the experiment. On the following pages, we will describe the exact experimental procedure. The experiment consists of 5 consecutive parts.

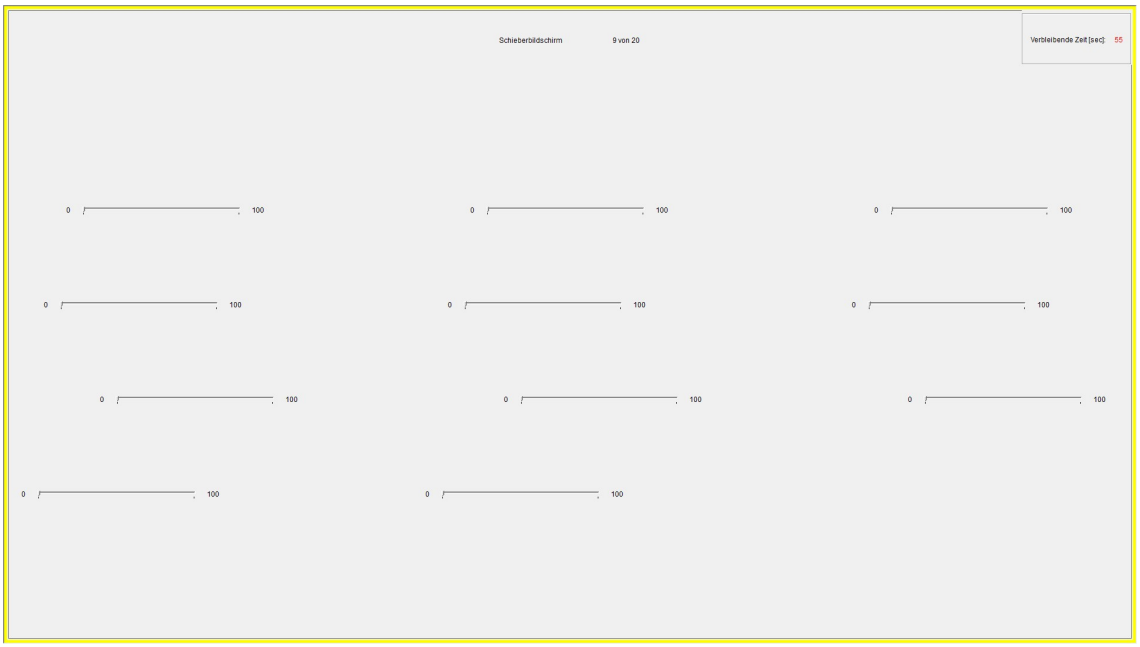
Your decision in part 1

In **part 1** you will see a screen with 11 sliders ("slider screen") and a "table screen" alternately, each screen 20 times.

You have 55 seconds of time to work on each slider screen. The remaining time will be shown in the upper right corner of the screen. When the time is over, the next table screen will be shown

automatically.

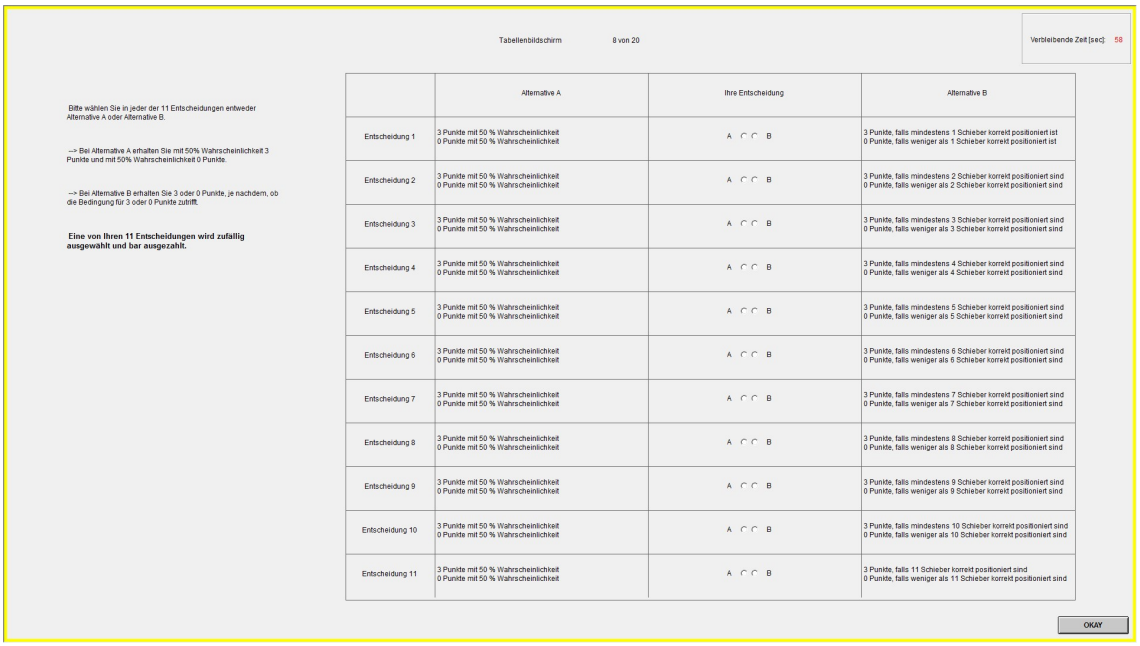
Your task on each **slider screen** is to **position as many of the 11 sliders in the centre of the respective scale as possible**.



Initially, each slider is at the very left end of the scale (position 0) and can be moved on the scale with the help of the mouse, maximally to the very right end of the scale (position 100). To move the slider you have to press the left mouse button. However, the current position of the slider is not shown, so that you have to estimate on your own where the middle of the scale is. The arrows on the keyboard and the mouse wheel are deactivated. When the working time for a slider screen is over the number of sliders you positioned correctly will be counted and saved by the computer automatically. At this point in time you do not yet get feedback on how many sliders you positioned correctly.

A slider will count as correctly positioned at the middle position if you have positioned it between positions 49.5 and 50.5 (so either exactly at the middle position 50 or very nearby position 50). **For each correctly positioned slider you will get a point.**

Every slider screen is followed by a **table screen** which looks like this:



Tabellenbildschirm 8 von 20

Verbleibende Zeit [sec]: 58

Bitte wählen Sie in jeder der 11 Entscheidungen entweder Alternative A oder Alternative B.

–> Bei Alternative A erhalten Sie mit 50% Wahrscheinlichkeit 3 Punkte und mit 50% Wahrscheinlichkeit 0 Punkte.

–> Bei Alternative B erhalten Sie 3 oder 0 Punkte, je nachdem, ob die Bedingung für 3 oder 0 Punkte zutrifft.

Eine von Ihren 11 Entscheidungen wird zufällig ausgewählt und bar ausgezahlt.

	Alternative A	Ihre Entscheidung	Alternative B
Entscheidung 1	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 1 Schieber korrekt positioniert ist 0 Punkte, falls weniger als 1 Schieber korrekt positioniert ist
Entscheidung 2	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 2 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 2 Schieber korrekt positioniert sind
Entscheidung 3	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 3 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 3 Schieber korrekt positioniert sind
Entscheidung 4	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 4 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 4 Schieber korrekt positioniert sind
Entscheidung 5	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 5 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 5 Schieber korrekt positioniert sind
Entscheidung 6	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 6 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 6 Schieber korrekt positioniert sind
Entscheidung 7	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 7 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 7 Schieber korrekt positioniert sind
Entscheidung 8	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 8 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 8 Schieber korrekt positioniert sind
Entscheidung 9	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 9 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 9 Schieber korrekt positioniert sind
Entscheidung 10	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls mindestens 10 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 10 Schieber korrekt positioniert sind
Entscheidung 11	3 Punkte mit 50 % Wahrscheinlichkeit 0 Punkte mit 50 % Wahrscheinlichkeit	A B	3 Punkte, falls 11 Schieber korrekt positioniert sind 0 Punkte, falls weniger als 11 Schieber korrekt positioniert sind

OKAY

For each of the 11 lines (decision 1 to decision 11), please click on either alternative A or B with your mouse to decide which alternative you prefer. Both alternatives are lotteries. Lottery A is the same for every decision: With a probability of 50%, you will earn 3 points and with a probability of 50%, you will win 0 points.

For alternative B, you will win either 3 points or 0 points as well. **For alternative B, the number of sliders that were correctly positioned at the middle position on the previous slider screen determines whether you will get 3 points or 0 points.** The conditions that need to be met to earn 3 points become more and more demanding with each decision: If you choose alternative B in the first decision, you will earn 3 points if you positioned at least one slider correctly on the previous slider screen; in case that you did not position any sliders correctly, you will earn 0 points. If you choose alternative B in the second (third etc.) decision, you will earn 3 points if you positioned at least two (three etc.) sliders correctly; in case you positioned less than two (three etc.) sliders correctly, you will earn 0 points. If you choose alternative B in the last decision, you will earn 3 points if you positioned all sliders correctly; in case you positioned less than all 11 sliders correctly, you will earn 0 points. **Thus, it depends on your personal estimate how many sliders you have positioned correctly whether alternative A or B is more attractive for you.**

An example: You think that the probability that you positioned 6 sliders correctly is higher than

50% and the probability that you positioned 7 sliders correctly is lower than 50%. In that case, your earnings will be highest if you choose alternative B in decision 1 to 6 and alternative A in decision 7 to 11. In decision 1 to 6, you will then earn 3 points with a probability higher than 50%. If you had chosen alternative A in decisions 1 to 6, you would have earned 3 points only with a probability of 50%. If you choose alternative A in decisions 7 to 11, you will earn 3 points with a probability of 50%. If you had chosen alternative B in decisions 7 to 11, you would have earned 3 points with less than 50%.

As soon as you have changed from alternative B to A, the lower rows of a table will fill automatically, because alternative B's attractiveness decreases top-down. As long as you do not click the "OKAY" button you can still change your decision. As soon as you have made all eleven decisions of a given table to your entire satisfaction, please click the "OKAY" button downright. Then, the next slider screen will be displayed.

Your payoff from part 1 of the experiment is composed of the following parts:

Slider screens: For each correctly positioned slider (out of altogether 20*11=220 sliders) you will receive one point - thus, a maximum of 220 points!

Table screens: For each table screen, only <u>one</u> decision will be paid. So you will receive a maximum of 3 points for answering a table screen. First, one of the 11 decisions is drawn randomly for each table screen. All decisions are selected with the same probability (i.e. 1/11). Only the selected decision determines your payment. **This implies that you should make your decision in every line of each table as if this was your only decision.** In the next step, it is checked whether you have chosen alternative A or B in the selected decision. If you have chosen alternative A, the 50-50 lottery will be played and will determine your payoff. If you have chosen alternative B, you will either get 3 points or 0 points, depending on the number of sliders you positioned correctly on the respective slider screen.

<u>An example:</u> Suppose that in the 1st step the 4th decision of a table screen is drawn randomly. In decision 4, alternative A was chosen. In the 2nd step, it will be determined randomly whether you earn 3 points or 0 points. Both payoffs are equally likely (both have a probability of 50%). As soon as you have worked on all 20 slider and table screens, the next parts of the experiments will follow.

Details on **parts 2 to 5** will be provided in the course of the experiment.

Training tasks and control questions

Before part 1 of the experiment begins, we would like to kindly ask you to answers some questions concerning your understanding of the tasks and decisions. Answering those questions will help to get acquainted with the situation, so that you can make good decisions later on.

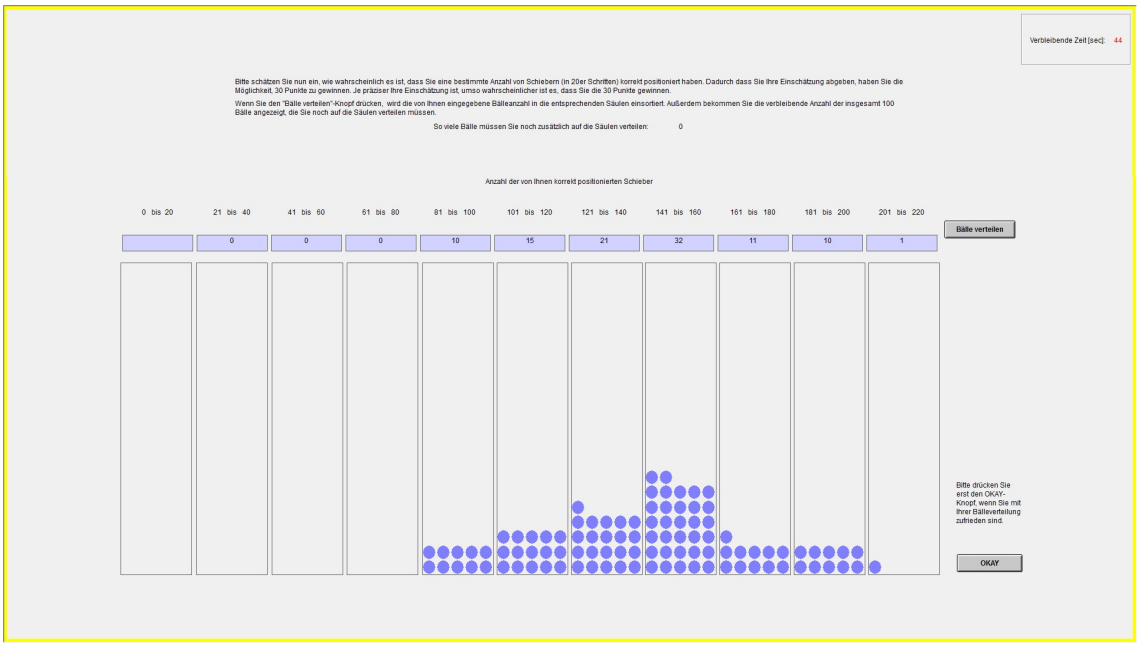
At the end of today's experiment - right after part 5 - some screens with questions and the like will follow before you will receive your earnings.

If you have any questions right now or during the time you work on the control questions, or if you would now like to start with the control questions and the experiment, please raise your hand. We will then come to your seat to answer your questions and to start the experiment. Please do not pose your questions loudly! **Please do not press the START button on your own!**

Your decisions in part 2

In part 2 of the experiment, we would like you to provide an estimate concerning the probability that you positioned a certain number of sliders correctly (in steps of 20). By providing that estimate, you have the possibility to earn 30 points. **The more precise your estimate is, the more likely it is that you will earn the 30 points.**

The screen which we use for asking for your estimate looks like this:



There are **11 pillars which each represent a certain quantity (in steps of 20) of correctly positioned sliders in part 1.** Reminder: In part 1 of the experiment you worked on 220 sliders in total. Thus, the first pillar stands for 0-20, the second pillar for 21-40 and the last pillar for 201-220 correctly positioned sliders. **Your task is to distribute 100 balls across these pillars. Each ball represents one percentage point.** If you place, e.g., 50 balls in the second pillar, that implies that you assume that with 50% probability you positioned 21-40 sliders of all 220 sliders correctly. If you place, e.g., 23 balls in the ninth pillar, that implies that with 23% probability you assume that you positioned between 161 and 180 sliders of all 220 sliders correctly. **The more likely you deem a certain pillar to contain the number of sliders you positioned correctly, the more balls should be put into this pillar.** The task will only be finished when you have distributed exactly 100 balls into the 11 pillars and you feel confident about the resulting probability distribution because it is a good fit to your estimate concerning the correctly positioned sliders. In that case, please press the OKAY button downright to continue with part 3 of the experiment.

To put balls into a pillar please fill in the respective number into the input field above the pillar. When you press the "Distribute balls" button, the number of balls you filled in will be put into the respective pillars. Furthermore, the remaining number of the 100 balls which you still have to distribute among the pillars will be shown. You can change the number of balls in a pillar until you press the OKAY button.

To sum up: The more precise your estimate - i.e. the more balls you placed in the correct pillar and the less balls you placed in the wrong pillars - the more likely it is that you will earn 30 points.

(Only) for those who are interested in the exact payoff scheme: After you have distributed all 100 balls into the 11 pillars a number A is calculated in the following way:

$$A = \sum_{i=1}^{11} (balls\ in\ pillar\ i - 100 * I_i)^2 \tag{2}$$

where i=1, ..., 11 refers to the different pillars and I_i equals 1 for the pillar which contains the number of the correctly positioned sliders and 0 otherwise. The more your estimate deviates from the actual number of correctly positioned sliders, the higher is A. Then, a number X is drawn randomly from the interval [0, 20000]. If $A < X$, you will win the additional 30 points. If $A > X$, you will not win further points.

Appendix B – Choice List Screens

In the experiment, we elicited the medians of subjects' belief distributions using two tools, the choice lists in Stage 1 and 4 and the ball allocation task in Stage 2 and 5. The choice list screens were designed to directly elicit the median of a subject's belief distribution on the number of correctly positioned sliders on the preceding slider screen. In each choice list (see Table 4), subjects faced two payment alternatives. Alternative A was a two-outcome lottery with possible payments of 0 or 3 points, each with 50% probability. It remained constant in all rows of a choice list table. Choosing alternative B, a subject earned 3 points if she had positioned at least a given number of sliders correctly on the previous slider screen and 0 points otherwise. Starting from 1, the required number of correctly positioned sliders increased by 1 in each row of the choice list when moving from top to bottom. In any given row h, alternative B yielded a higher expected payoff than alternative A, if and only if a subject's performance on the previous slider screen exceeded h with more than 50% probability. In line with this reasoning, a subject should choose alternative B when she believed that it was more likely than 50% that she had h sliders positioned correctly. When choosing alternative B in row h, a subject should also choose alternative B in all the rows above row h (single switching

Table 4: *A Choice List*

	Alternative A	Alternative B
Decision 1	3 points with 50% probability 0 points with 50% probability	3 points if you positioned at least 1 slider correctly 0 points if you positioned less than 1 slider correctly
Decision 2	3 points with 50% probability 0 points with 50% probability	3 points if you positioned at least 2 sliders correctly 0 points if you positioned less than 2 sliders correctly
Decision 3	3 points with 50% probability 0 points with 50% probability	3 points if you positioned at least 3 sliders correctly 0 points if you positioned less than 3 sliders correctly
⋮	⋮	⋮
Decision 11	3 points with 50% probability 0 points with 50% probability	3 points if you positioned 11 sliders correctly 0 points if you positioned less than 11 sliders correctly

point).[17] Since subjects choose between two risky alternatives and always prefer a higher winning probability, the optimal switching point is independent of a subject's risk attitudes. To incentivize the truthful revelation of beliefs, one row of each choice list was randomly selected for payment at the end of the experiment. Subjects had to click a "finish" button to exit a choice list screen. After leaving a choice list screen, the next slider screen was shown automatically. In Stage 1, each type of screen appeared alternately 20 times, before the experiment moved on to Stage 2.

In our main analysis, we focus on identifying overconfident subjects based on medians elicited by the ball allocation task instead of the choice lists for several reasons: Most importantly, we cannot use the median beliefs elicited by the choice lists to identify significant overconfidence at the individual level as outlined in Section 2, which requires the median belief on a large number of sliders in order to apply the Central Limit Theorem. While the ball allocation task elicits the belief distribution regarding the overall number of correctly positioned sliders on all 20 screens, the choice lists elicit the median of subjects' belief distribution with regard to the number on each screen. Due to the discontinuity of the support space, beliefs elicited by the choice lists are not addable, i.e. the sum of each subject's 20 medians elicited by the choice lists in Stage 1 does not necessarily equal the

[17] In order to reduce the number of clicks subjects had to make during the experiment, alternative A would automatically be selected in all lower rows after a subject had selected alternative A in one row. Subjects still had the opportunity to revise the resulting switching point.

median belief on the overall number of correctly positioned sliders on all 20 screens. As a result, the median belief on the overall number of correctly positioned sliders cannot be inferred from the screen-wise median beliefs elicited by the choice lists.

Moreover, measuring median beliefs via choice lists seems to be less intuitive for subjects than using the ball allocation task. In the questionnaire, 56 subjects indicated that the choice lists are more intuitive, while 124 subjects chose the ball allocation task. In a similar vein, subjects' answers to the control questions show that the ball allocation task was easier to understand. Regarding the choice lists, 48 out of 180 participants gave a wrong answer to control question 1, 15 to control question 2. When it comes to the ball allocation task, 7 subjects gave wrong answers to control question 3, and 9 subjects failed on control question 4. 14 subjects failed both control questions for the choice lists, while only 4 gave wrong answers to both control questions concerning the ball allocation task.

Average choice list elicited median beliefs in Stages 1 and 4 are 4.91 (std. dev. 1.70) and 3.92 (std. dev. 2.70), respectively, compared to 5.37 (std. dev. 1.65) and 3.97 (std. dev. 2.25) in the ball allocation task, where numbers are obtained by dividing the overall median belief by 20. It is reassuring that the correlation of within-subject median beliefs across the two elicitation tools is high and highly significant (Pearson correlation $r = 0.70$, $p < 0.001$ in Stage 1 and $r = 0.54$, $p < 0.001$ in Stage 4).