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Confidence and career choices: an experiment*

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Abstract

Confidence is often seen as an important determinant of success. However, empirical evidence regarding the causal effect of confidence on choices is sparse. Using a stylized laboratory experiment, we examine the effect of an increase in confidence on two important labor market choices: (i) the amount of ability-contingent earnings risk to take on, and (ii) the subsequent effort choice. We find that increased confidence leads subjects to take on more ability-contingent earnings risk. However, effort levels are unaffected. Overall, the upward shift in confidence is detrimental for low-ability workers as a result of high baseline levels of confidence.

Keywords: Beliefs; career choices; experiment; overconfidence; real effort

JEL classification: C91; D03; M50; J24

1. Introduction

Confidence in one's own abilities is commonly thought to be an important determinant of success. A large body of work has studied the channels through which holding overconfident beliefs might be beneficial. For example, Bénabou and Tirole (2002, 2003) provide a discussion of how a higher level of self-confidence can motivate individuals to work harder, overcome obstacles, and take beneficial risks. Brunnermeier and Parker

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(2005) argue that individuals might hold upward-biased beliefs in order to enjoy the consumption value of a rose-tinted view of the future, while Köszegi (2006) studies the behavior of individuals who derive ego-utility from overconfident beliefs. Complementing these theoretical discussions, a growing body of empirical literature has studied various mechanisms through which confidence can contribute to an individual's success. This literature presents evidence suggesting that more confident individuals are better at persuading others that they are of high ability (Burks et al., 2013; Schwarzmann and Van der Weele, 2019; Solda et al., 2020), work harder (Puri and Robinson, 2007; Pikulina et al., 2017; Chen and Schildberg-Hörisch, 2019), and that overconfidence is an adaptive evolutionary trait (Bernardo and Welch, 2001; Heifetz et al., 2007; Johnson and Fowler, 2011).

This view that being overconfident is beneficial represents a deviation from the standard Bayesian rational agent perspective; that is, for a standard economic agent, more accurate beliefs are typically better, particularly in non-strategic settings. Less accurate beliefs lead to more mistakes, which results in a loss of utility. A substantial body of theoretical and empirical evidence has documented examples of the costly mistakes that overconfident individuals can make, such as exposing themselves to excessive risk (relative to their risk preferences) in financial markets (Odean, 1998; Barber and Odean, 2001), poor managerial decisions (Malmendier and Tate, 2005), and over-entry into competition (Camerer and Lovallo, 1999; Niederle and Vesterlund, 2007; Dohmen and Falk, 2011).

What is relatively undisputed is that there exists a wealth of evidence documenting the widespread existence of overconfidence (see, e.g., Moore and Healy, 2008) and that overconfidence is one of the most commonly studied behavioral biases (see, e.g., Bénabou and Tirole, 2016). Despite this prevalence and prominence of overconfidence, causal evidence that assesses how an exogenous shift in confidence affects behavior is relatively scarce.¹ Such causal evidence is essential for evaluating whether increasing an individual's confidence is beneficial or harmful in a specific domain. Any observed correlation between individual heterogeneity in overconfidence and behavior or outcomes could simply be driven by other unobserved characteristics that generate the behavior or outcomes.

Furthermore, standard economic models typically postulate that behavior in the presence of dynamic uncertainty operates as follows: new information arrives \rightarrow_A beliefs are updated according to Bayes rule \rightarrow_B choices are

¹Some notable exceptions to this include papers that create exogenous variation in one's belief in oneself (relative to the truth) by varying the feedback that individuals receive, such as in Schwarzmann and Van der Weele (2019) and Chen and Schildberg-Hörisch (2019), or by varying the mechanism used for selection into the experiment (e.g., Camerer and Lovallo, 1999).

then prescribed by these posterior beliefs. There has been considerable effort invested recently in investigating the first relationship (\rightarrow_A), namely in testing the descriptive validity of the way belief formation has traditionally been modeled (see, e.g., Grether, 1980; Eil and Rao, 2011; Möbius et al., 2014; Heger and Papageorge, 2018; Benjamin, 2019; Coutts, 2019; Barron, 2021). However, less attention has been paid to the second relationship (\rightarrow_B), namely testing whether a shift in beliefs actually translates into a shift in choices in the way that traditional models predict. The literature that has studied this relationship empirically reveals a complex picture, documenting many instances in which beliefs do not affect behavior in the manner predicted by the standard model (see, e.g., Costa-Gomes and Weizsäcker, 2008; Duffy and Tavits, 2008; Ivanov et al., 2010; Chetty and Saez, 2013; Oster et al., 2013; Golman et al., 2017; Huck et al., 2018; Fischer and Sliwka, 2018; Lergetporer et al., 2018; Berkes et al., 2019; Bettinger et al., 2020; Haaland and Roth, 2019; Thaler, 2020). In Section 2, we provide a more detailed discussion of this literature. This body of work highlights the importance of empirically testing the causal relationship between beliefs and actions in different contexts, so that we can learn why a shift in (measured) beliefs has a large effect on behavior in some contexts and no effect in others. Additionally, these types of studies can serve as a litmus test in the evaluation of a particular model, since they provide a reduced-form test of a key comparative static (i.e., the causal effect of beliefs on actions).

In this paper, we contribute to this endeavor of studying how a shift in beliefs translates into changes in behavior. To do this, we use a stylized career choice task to study several channels through which an exogenous shift in beliefs can operate. We develop a simple theoretical framework and show empirically how an upward shift in confidence can causally influence decision-making regarding the subject's preferred payment scheme, effort provision, and resulting earnings.

The following illustrative example provides the intuition for how an increase in confidence can affect decision-making in a labor market setting. Imagine a computer programmer who is about to graduate from college (let us call her Taylor). Taylor is well educated. She has the choice between two types of jobs: (1) a job at a mid-sized company that will pay her a fixed wage, and (2) working at a start-up where her earnings will depend heavily on her performance. She will earn far more at the start-up if she is better than the average competing programmer who is also graduating from college, and far less if she is worse.

Assume that Taylor, like many other people, believes that she is better than average (see, amongst others Kruger, 1999; Burson et al., 2006; Healy and Moore, 2007; Moore and Healy, 2008; Benoît et al., 2015). This belief can be influential for two of her decisions. First, she needs to choose the

type of job that she thinks will be a good fit for her. Second, conditional on being in the job, she needs to decide how much effort to exert.

For the first choice of which job to take, most models would posit that the higher her confidence in her own ability, the more likely she is to choose to work at the start-up. Choosing to work at the start-up is the payoff-maximizing choice if her ability level is *actually* above average, but harmful, if she *wrongly believes* she is better than average.

With regards to her second choice of how much effort to exert within the job, however, an inflated level of confidence could motivate her to work harder in the start-up. The reason for this is that when an individual's payment depends directly on the output they produce, the returns to effort are proportional to their ability. An inflated view of their own ability implies an upward shift in their perceived returns to effort. However, if this confidence turns out to be misguided, the individual's motivation to exert a high effort level might also be misguided. It is of interest to evaluate empirically whether the standard theoretical relationship between beliefs and choices provides an accurate descriptive picture of actual behavior.

Ideally, one would study this question using survey data about real job and effort choices. However, this approach poses several challenges. First, it is non-trivial to gain access to accurate measurements of the beliefs, effort choices, and payment scheme preferences of job seekers, holding demand-side factors constant. Second, even if one does have access to high-quality data for these variables, it is not easy to find a natural experiment that provides an exogenous shock to these beliefs. We circumvent these issues by using a laboratory experiment, which allows full control over the environment. Doing this, we generate exogenous variation in beliefs to measure the causal effect of a shift in beliefs on (i) the selection into fixed [low-earnings risk] or ability-contingent [high-earnings risk] payment schemes, and (ii) effort exerted within a given incentive scheme. We derive our hypotheses for the experiment from a simple theoretical framework.

In the experiment, participants are divided into groups of ten. Each participant takes a test measuring their cognitive ability. This serves as our measurement of the participant's ability. They are then asked to estimate their belief about the probability that their IQ test performance was in the top half of their group of ten. We are interested in studying how a shift in the belief about their position in the ability distribution translates into payment scheme and effort choices made by the participants. We designed the experiment such that: (i) the ability distribution is held constant across treatments, and (ii) the influence of the individual's ability on their payoff is fixed prior to their payment scheme and effort choices. We therefore fix the participant's ability and belief about their ability (confidence) at the beginning of the experiment, and examine how it affects the decisions that follow.

After completing the IQ test, participants complete ten rounds of a mundane real-effort task that is chosen to capture pure effort and to have little dependence on ability. In each round, except the first, participants must choose to be compensated for their effort according to one of two available payment schemes. Subjects can choose to work either for an ability-contingent piece rate or for a fixed piece rate that does not depend on their ability. The ability-contingent piece rate pays a high wage if the subject is in the top half of their group in the IQ test at the beginning of the experiment and nothing if they are in the bottom half. The fixed risk-free piece rate is ratcheted up, increasing in each successive round, but always lies below the high piece rate of the ability-contingent piece rate. Thus, if a subject is highly confident of being in the top half of their group, choosing the ability-contingent piece rate maximizes their earnings.

The exogenous variation in beliefs about relative ability is generated in the experiment by exposing each entire group of ten participants to either a harder or an easier version of the IQ test (participants only interact with other participants who faced the same test). Subjects randomly assigned to the easy test condition are expected to assess their relative rank in the IQ test to be higher than subjects assigned to the difficult test condition. This is commonly referred to as the “hard–easy” effect (Kruger, 1999; Moore and Kim, 2003; Moore and Small, 2007; Healy and Moore, 2007; Dargnies et al., 2019). The underlying idea is that individuals fail to fully appreciate that when they find a test easy [difficult], the test is likely to also be easy [difficult] for all participants, not just for themselves. They therefore adjust their estimate of their own score more than they adjust their estimate of others’ scores, which leads to a predictable (and biased) shift in their estimate of their relative rank.

We find evidence that a small shift in the difficulty of the test leads to a large shift in the average belief that subjects hold regarding their relative placement. The effect is particularly strong for subjects who are in the bottom half of the group. These subjects report higher beliefs, on average, in the easy treatment than in the hard treatment. The beliefs of those in the top half are less affected on average.

Further, we find that this increase in confidence leads subjects to choose the ability-contingent piece rate more often. If randomly confronted with the hard test, subjects are more likely to choose the fixed piece rate. This shift in job choice occurs even though the incentives faced remain constant. This suggests that the way that knowledge is tested within an education system could have implications for the later choices made by individuals even if their ability level is unaffected. Regarding effort, we find that the level of intrinsic motivation of participants in our experiment is high, and largely insensitive to their beliefs, implying that we do not observe a shift in effort choices.

The shift in beliefs has important consequences for earnings of the bottom half of the group. This group earns only about a quarter of what the top half of the group earns on average, but their average earnings are reduced even further, by about 40 percent, when their confidence is exogenously increased. The reason for this is that overconfidence in relative ability is costly for below-average-ability individuals as it increases their probability of choosing an ability-contingent incentive scheme, which is a mistake for these individuals.

The remainder of the paper is structured as follows. Section 2 places our paper in the context of the related literature, Section 3 outlines the theoretical framework and hypotheses, and Section 4 describes the experimental design. Section 5 presents our results, Section 6 contains a discussion, and Section 7 concludes.

2. Related literature

In the discussion above, we noted that many economic models incorporate the following causal relationships: information \rightarrow_A (Bayesian) beliefs \rightarrow_B actions. The focus of this paper is on the second of these relationships, \rightarrow_B , which is important because the existing literature has demonstrated that the relationship between beliefs and actions frequently does not conform to the predictions of the standard model.

This divergence between predictions and observed behavior is often due to the model being incorrectly specified. There are several ways in which this can occur. First, an individual might have strong non-material tastes or motives that generate an insensitivity to movements in beliefs. Such non-material motives are often omitted from the utility function in standard economic models, implying that the insensitivity might not be predicted by the model. For example, motives such as a taste for, or an aversion to, either uncertainty or competition could generate such an insensitivity to shifts in beliefs about oneself when deciding between a competitive or fixed wage job.² In other contexts, non-monetary motives such as image concerns can generate a wedge between beliefs and behavior. For example, it is well documented that a social desirability bias or experimenter demand can shift behavior relative to an individual's underlying beliefs and preferences

²In the context of studying the gender-wage gap, Niederle and Vesterlund (2007) argue that a taste for competition is an important motive, with the components of the standard model (i.e., beliefs, monetary rewards, and risk preferences) proving insufficient to explain behavior. In a similar tournament experiment, Alnamlah and Gravert (2020) show that when failure in the tournament is attributed to bad luck, it does not have a significant effect on a woman's confidence (belief) while still generating a significant positive effect on her propensity to re-enter into competition (action).

(see, e.g., Nederhof, 1985; de Quidt et al., 2018, 2019).³ In relation to the current paper, a particularly important illustration of the beliefs–actions relationship that does not always conform to the predictions of the standard model is the example of effort choices. Effort choices in the lab and in the field have been found to not always respond to beliefs about expected monetary rewards in the manner predicted by the simple standard model, which omits non-monetary motives such as reciprocity and image concerns. Often the effort that individuals exert depends heavily on non-monetary factors as demonstrated by the gift exchange literature, which documents heterogeneous results (see, e.g., Kube et al., 2012, 2013; Esteves-Sorenson, 2018). Recent work has gone on to show that the factors motivating the provision of effort in employer–employee relationships are highly complex, with several different types of non-monetary (behavioral) motives playing a role (see, e.g., DellaVigna and Pope, 2018; DellaVigna et al., 2020). This serves to highlight why real-effort choices in the lab can often appear to be insensitive to (expected) monetary incentives, with the majority of subjects working for a piece rate of zero and also displaying a relatively low elasticity to large monetary incentive increases (see, e.g., Araujo et al., 2016; DellaVigna and Pope, 2018; Erkal et al., 2018; Goerg et al., 2019).⁴

Second, an individual's tastes and beliefs might not be independent (in contradiction to the standard model). A burgeoning recent literature has questioned the validity of this independence, presenting evidence of motivated belief formation, where beliefs are influenced by desires (see, e.g., Brunnermeier and Parker, 2005; Loewenstein and Molnar, 2018). Conversely, it is also plausible that beliefs shift tastes (e.g., my taste for competing in amateur running races might be increased by an upward shift in my belief about my own running ability). Such interactions between beliefs and tastes could also interfere with the predicted relationship between beliefs and actions. Third, many models assume that beliefs enter the individual's objective function linearly. However, theories of probability weighting (Kahneman and Tversky, 1979) and, more recently, cognitive uncertainty (Enke and Graeber, 2019) suggest a more nuanced role of beliefs, with less sensitivity at intermediate probabilities and higher sensitivity closer to the certainty extremes.

As a consequence of these considerations, reduced-form tests of the causal relationship between beliefs and actions are extremely important.

³ Individuals often try to hide their socially undesirable beliefs or preferences (e.g., discriminatory beliefs or preferences) by taking actions that obscure their true views but involve a material cost (see, e.g., Cunningham and de Quidt, 2016; Barron et al., 2020).

⁴ This insensitivity is also observed in the current paper, providing an illustration of the fact that shifting beliefs might shift the expected monetary costs and benefits of different actions, but not necessarily translate into a change in behavior.

Such tests allow us to understand whether observed correlations between beliefs and actions are truly causal and provide a test of the validity of the mechanics of the model along a dimension of central importance. Furthermore, they allow us to learn about the potential implications of interventions that target beliefs.⁵

The existing empirical evidence examining the causal role played by information provision and shifts in beliefs on actions is mixed. Often, the observed behavior deviates from the predictions of the standard rational model. This is highlighted by the fact that the provision of information frequently does not yield the expected behavior (see, e.g., Ivanov et al., 2010; Chetty and Saez, 2013; Oster et al., 2013; Kuziemko et al., 2015; Abramovsky et al., 2016; Golman et al., 2017; Fischer and Sliwka, 2018; Lergetporer et al., 2018; Berkes et al., 2019; Bettinger et al., 2020; Thaler, 2020; Haaland and Roth, 2019).⁶ In addition, while many studies assume that elicited beliefs have a simple linear causal mapping to decisions, those that have explicitly investigated this relationship document evidence across a range of domains, showing that the relationship is not so straightforward. Costa-Gomes and Weizsäcker (2008), for example, show that subjects who play a set of 3×3 normal-form games in their experiment fail to best-respond to their stated beliefs almost half of the time. The authors conclude that “subjects perceive the game differently when they (i) choose actions and (ii) state beliefs” (p. 729). However, in a subsequent paper, Costa-Gomes et al. (2014) introduce exogenous variation in beliefs within a trust game to demonstrate that elicited beliefs can have a causal impact on choices in that domain. In an information experiment on racial discrimination, Haaland and Roth (2019) show that information provision can shift individuals’ beliefs about the extent of racial discrimination, but still have no effect on the support for pro-black policies. In an experimental study of beliefs about pivotality and voting decisions, Duffy and Tavits

⁵Specifically, there are many scenarios where policymakers might diagnose incorrect beliefs as being one potential source of an undesirable outcome in society (e.g., lower confidence of women has been posited as one potential contributing factor to the gender-wage gap, and inaccurate beliefs about the returns to investing in the stock market is one potential explanation for low investment rates). Scenarios of this sort lend themselves to being addressed through information interventions. However, information interventions will only be effective if beliefs can be shifted in the anticipated manner, and this shift in beliefs translates into the anticipated change in behavior. Even in scenarios where beliefs will not be directly targeted, it might be important to understand the causal effect of a shift in beliefs on behavior because beliefs are often moved as a byproduct of other interventions.

⁶In terms of information interventions, we are limited to considering those papers that have survived the premature culling of projects that yield a null effect. This publication bias is likely to reduce the set of papers that document evidence of information interventions that failed to demonstrate a treatment effect.

(2008) provide evidence at odds with the theory by documenting a weak relationship between perceived decisiveness and turnout – subjects whose perceived pivotality probability was higher than the cost of voting frequently did not vote, while many of those with a perceived pivotality probability considerably below the cost of voting still decided to participate.

In addition to contributing to the broader body of work examining how beliefs translate into actions, our paper relates most closely to the literature that studies the relationship between beliefs about relative ability and stylized labor market choices, such as career choices, effort provision, willingness to compete, and risk-taking behavior (e.g. Camerer and Lovo, 1999; Niederle and Vesterlund, 2007; Dohmen and Falk, 2011; Bruhin et al., 2018; Cheung and Johnstone, 2017; Pikulina et al., 2017, 2018; Chen and Schildberg-Hörisch, 2019). Dohmen and Falk (2011), for example, also study the choice between a variable and fixed-payment wage scheme. They show that individual characteristics, such as relative self-assessment and risk aversion, are important predictors of how individuals sort into the different incentive schemes. Our paper differs from this body of work in two important ways. First, we focus attention solely on the causal effect of a shift in beliefs, while holding the true ability distribution constant. Second, our design separates the measurement of ability and effort, allowing us to fix the individual's ability measurement and its influence on their payoff prior to facing the different incentive schemes (i.e., we hold the ability component of the production function fixed).

Our experimental design is related in spirit to Camerer and Lovo (1999), who study entry into competition due to underestimating one's competitors. In their experiment, individuals are either informed or not informed during recruitment into the experiment that they will be competing against other subjects in a skill-based task. This information leads to more self-selection into the experiment in the informed treatment. High-skilled, highly competitive individuals select into the experiment, and then select into competing within the experiment. They fail to recognize that others are behaving in the same way and so they are not competing against a random draw from the population.⁷ Importantly, the results they observe are not necessarily caused by overconfidence, since overconfidence might co-vary with other individual characteristics that contribute to

⁷This “reference group neglect” is related in essence to the hard–easy effect. Both concepts involve individuals neglecting the fact that there is a shift in the background distribution against which they are being compared, and both fall into the broader class of biases, where individuals neglect some feature of the data-generating process, leading to biased inference and systematic mistakes in decision-making. Notable examples include correlation neglect and selection neglect (see, e.g., Jehiel, 2018; Barron et al., 2019; Enke and Zimmermann, 2019; Enke, 2020). Reference group neglect could be viewed as a form of selection neglect.

the self-selection both into the experiment and into competing.⁸ This observation does not diminish the contribution of the study, since in many real-world settings, overconfidence is likely to co-vary with these unobserved factors in the same way. We view our paper as providing complementary evidence by focusing on the narrower question of isolating the role played purely by confidence. Our paper also demonstrates a method that can be used to examine the causal role played by a shift in confidence on actions in other domains.

3. Theoretical framework

In this section, we motivate the experimental design and hypotheses through means of a simple model where a worker chooses her payment scheme and then effort level. The objective of the theoretical framework is to provide some discipline and precision to the ensuing discussion. To do this, we augment the model used by DellaVigna and Pope (2018) in their large-scale study of real effort and motivation.

3.1. A simple model

Consider an individual i , who can earn money by performing a task that requires costly effort, e . She is either a high-ability or low-ability individual, $a \in \{a_L, a_H\}$. Prior to performing the task, the individual chooses between two incentive schemes: (i) one where high-ability individuals will earn a high wage, $w(a_H) = w_H$, and low-ability individuals will earn a low wage, $w(a_L) = w_L$; (ii) one that pays a fixed wage to everyone, \bar{w} , where $w_H > \bar{w} > w_L$. After choosing her incentive scheme, she chooses the level of effort, e , that she would like to exert. Following DellaVigna and Pope (2018), we allow the subject to derive some intrinsic utility from effort, denoted by s .⁹

A risk-neutral individual would choose her incentive scheme, $w \in \{\bar{w}, w(a)\}$, and effort level, e , by solving

$$\max_{w \in \{\bar{w}, w(a)\}} \max_{e \geq 0} \mathbf{E}_a[(s + w) \cdot e - c(e)], \quad (1)$$

⁸This is supported by the fact that a replication by Dankova and Servatka (2019), which extends Camerer and Lovo (1999) by studying both men and women (since the original study focuses on men), finds that the results are highly sensitive to the participant's gender.

⁹As in DellaVigna and Pope (2018), we view this intrinsic motivation term as capturing, in reduced form, any non-monetary reward the workers derive from working on the task. In terms of the laboratory experiment described below, this is taken to include any sense of duty to, or gratitude towards, the experimenter for the fixed show-up fee. DellaVigna and Pope (2018) argue that this non-monetary reward term is important for explaining the commonly observed non-zero effort in fixed-wage laboratory experiments.

where $c(e)$ is the cost of exerting effort, and is assumed to satisfy $c'(e) > 0$ and $c''(e) > 0$. The expectation operator, \mathbf{E}_a , denotes the expectation with respect to the individual's ability, and s represents the individual's intrinsic motivation for completing the task. Because uncertainty about one's own ability is only directly relevant for the wage schedule, we can rewrite equation (1):

$$\max_{w \in \{\bar{w}, w(a)\}} \max_{e \geq 0} (s + \mathbf{E}_a[w]) \cdot e - c(e). \tag{2}$$

Equation (2) shows that the individual's subjective belief regarding the likelihood that she is high-ability, $\hat{\pi} = \hat{\mathbf{P}}(a = a_H)$, is important for her decision about both which incentive scheme to take, and how much effort to exert if she chooses the ability-contingent incentives. Essentially, the choice of an incentive scheme involves a choice between being paid a certain piece rate of $\mathbf{E}_a[\bar{w}] = \bar{w}$, or an expected piece rate of $\mathbf{E}_a[w(a)] = \hat{\pi} \cdot w_H$ for each unit of effort. We normalize $w_H = 1$.

3.1.1. The effort choice. Conditional on the choice of an incentive scheme, w , the individual chooses effort, e^* , according to the condition $c'(e^*) = s + \mathbf{E}_a[w]$. Under the certain piece rate (PR) incentive scheme, she chooses $e_{PR}^* = c'^{-1}(s + \bar{w})$. Under the ability-contingent (AC) incentive scheme, she chooses $e_{AC}^* = c'^{-1}(s + \hat{\pi} \cdot w_H)$. If her confidence is sufficiently low – that is, she believes that the probability that she is high-ability is below a certain threshold (i.e., $\hat{\pi} < \bar{w}/w_H$) – then the individual exerts more effort under the certain piece rate incentives. However, if she is sufficiently confident in her own ability, such that $\hat{\pi} > \bar{w}/w_H$, then she expects a higher piece rate under ability-contingent incentives and would work harder under these incentives. We therefore define a threshold belief, namely $\pi^e := \bar{w}/w_H$, such that

- for low beliefs (i.e., if $\hat{\pi} \in [0, \pi^e]$), the individual exerts more effort under certain piece rate incentives than under ability-contingent incentives;
- for high beliefs (i.e., if $\hat{\pi} \in [\pi^e, 1]$), the individual exerts more effort under ability-contingent incentives than under certain piece rate incentives.

Importantly, for high levels of intrinsic motivation, s , effort provision becomes less sensitive to variation in the monetary incentives. The presence of such strong intrinsic motives has been well documented in laboratory real-effort tasks (see, e.g., Araujo et al., 2016; DellaVigna and Pope, 2018; Erkal et al., 2018; Goerg et al., 2019). This literature has shown that when there is a time constraint in place for exerting effort on the

task (as there typically is in the laboratory), these intrinsic motives can generate a “ceiling effect” on effort provision (even for a piece rate of zero). In terms of the model described in equation (2), this insensitivity to (expected) monetary rewards can occur in the following way. If the time limit for working on the task constrains effort to be below a certain effort level, $e \leq \bar{e}$, and intrinsic motivation, s , is sufficiently strong, this can result in effort being determined by the time limit binding rather than by the marginal cost equaling the marginal benefit of effort (i.e., $\bar{e} \leq e_{PR}^*$ and $\bar{e} \leq e_{AC}^*$). This occurs, for example, when the intrinsic incentives alone are sufficient to induce maximum effort for the time limit available for the task. If this is the case in the scenarios that we consider, then the observed effort level chosen under both sets of incentives will be equal.

3.1.2. The incentive scheme choice. When choosing between incentive schemes, the individual chooses the ability-contingent incentives whenever she expects to earn more per unit of effort under them than she would under the certain piece rate per unit of effort:

$$(s + \hat{\pi} \cdot w_H) \cdot e_{AC}^* - c(e_{AC}^*) \geq (s + \bar{w}) \cdot e_{PR}^* - c(e_{PR}^*). \quad (3)$$

This inequality holds whenever $\hat{\pi} \cdot w_H \geq \bar{w}$.¹⁰ It holds even if the effort level chosen under both incentives schemes is the same (i.e., if $e^* = \bar{e}$). Under risk neutrality, the threshold for the choice of incentives, and the threshold for effort choices are equal (i.e., $\pi^i = \pi^e = \bar{w}/w_H$). We can therefore summarize the influence of beliefs on choices as follows:

- a *low-confidence* individual (i.e., one with a belief $\hat{\pi} \in [0, \pi^i]$) will choose the certain piece rate incentives, and exert (weakly) lower effort under ability-contingent incentives than under certain piece rate incentives;
- a *high-confidence* individual (i.e., one with a belief $\hat{\pi} \in (\pi^i, 1]$) will choose the ability-contingent incentives, and exert (weakly) higher effort under ability-contingent incentives than under certain piece rate incentives.

¹⁰To see this, notice that if $\hat{\pi} \cdot w_H > \bar{w}$, then the individual could simply choose the ability-contingent incentives and set effort equal to the optimal effort level under certain piece rate incentives, $e = e_{PR}^*$, and receive a higher expected payoff than under the certain piece rate incentives, i.e.

$$(s + \hat{\pi} \cdot w_H) \cdot e_{PR}^* - c(e_{PR}^*) \geq (s + \bar{w}) \cdot e_{PR}^* - c(e_{PR}^*).$$

Because $e = e_{AC}^*$ maximizes the left-hand side of this inequality, equation (3) must also hold.

In Online Appendix A.2, we relax the risk-neutrality assumption and show that the threshold belief at which individuals will switch incentive choice differs from the one at which effort is affected by incentives.

3.2. Hypotheses

The theoretical framework above provides us with a set of hypotheses that we test in the experiment. The central objective is to ask how a shift in confidence about one's own ability affects incentive scheme choices and effort choices. To do this, we use the well-established hard–easy effect (see Moore and Healy, 2008) to induce exogenous variation in subjects' beliefs about their own ability, $\hat{\pi}$, keeping everything else constant (e.g., actual ability, a).

In the experiment, it is reasonable to expect heterogeneous beliefs. Therefore, we consider a continuum of agents who hold beliefs, distributed on the unit interval, $\hat{\pi} \sim F(\hat{\pi})$, such that $f(\hat{\pi})$ is everywhere positive on $\hat{\pi} \in [0, 1]$. Because the evaluation of our main hypotheses relies on our experimental design generating exogenous variation in beliefs across treatments, our first hypothesis tests whether we observe a shift in beliefs due to the hard–easy effect in our experiment:

Hypothesis 1 (Shift in Beliefs). *Beliefs about one's own relative ability in the easy treatment will be higher, on average, than beliefs in the hard treatment.*

Our second hypothesis tests whether incentive scheme choices are affected by the hypothesized shift in beliefs between treatments. The logic behind this hypothesis is that individuals who hold a higher belief in their own ability are more likely to choose the ability-contingent incentives; that is, an upward shift in $\hat{\pi}$ for all individuals implies that $\hat{\pi} \geq \bar{w}/w_H$ will hold for a greater fraction of individuals.

Hypothesis 2 (Incentive Choices). *An exogenous increase in confidence will lead to a higher fraction of individuals choosing the ability-contingent incentives.*

Third, we ask how a shift in beliefs affects effort choices. In terms of the model discussed above, there are two scenarios: one in which the intrinsic motive to exert effort is low and the elasticity with respect to extrinsic monetary incentives is high, and one in which the intrinsic motive is high and therefore the elasticity with respect to extrinsic monetary incentives is low.

When designing the experiment, we had in mind the first scenario in which the intrinsic motivation to exert effort does not crowd out the influence of extrinsic monetary incentives. This ex-ante hypothesis is

captured by Hypothesis 3(a). However, in view of the fast-growing recent literature showing how important intrinsic incentives are in determining effort provision (and in view of our results), we find it useful to also explicitly write down an alternative hypothesis in a form of Hypothesis 3(b), which captures such high intrinsic motivation scenarios.¹¹ This alternative hypothesis was formulated ex post.

Hypothesis 3 (Effort Choices). *We will observe one of the following two patterns of behavior for effort choices. (a) Low intrinsic motivation: for high-confidence individuals, effort choices will be higher under the ability-contingent incentives than under the certain piece rate incentives. In this scenario, an upward shift in confidence will increase overall average effort. (b) High intrinsic motivation: effort choices will not be influenced by the incentive scheme. In this scenario, an exogenous shift in confidence will not affect effort choices.*

Ultimately, we also want to examine the effect that an upwards shift in confidence has on earnings. Within each treatment in our experiment, individuals are classified into two groups: high and low ability. An upward shift in beliefs is likely to lead to a very different effect on outcomes for individuals in these two groups. Essentially, individuals who are actually of high ability benefit from an upward shift in confidence as this leads to them switching towards the ability-contingent incentive scheme more often. The reverse is true for low-ability individuals. The boost in confidence can be harmful for them, as they may switch to the ability-contingent incentive scheme even though it results in a loss of earnings. Online Appendix A.1 contains a more detailed discussion of this intuition, and Hypothesis 4 summarizes the main testable implications.

Hypothesis 4 (Earnings). *While an increase in confidence will have an ambiguous effect on average earnings, the framework suggests that: (a) it will lead to weakly lower average earnings for low-ability individuals, (b) it*

¹¹One reason for including this ex post alternative hypothesis (as opposed to only outlining our ex-ante hypothesis) is to note that the literature has progressed to the point where the role of intrinsic motives in generating a ceiling effect is well established, and also to point out that several papers have proposed and tested potential experimental design solutions to this issue (see, e.g., Araujo et al., 2016; DellaVigna and Pope, 2018; Erkal et al., 2018; Chen and Schildberg-Hörisch, 2019; Goerg et al., 2019; DellaVigna et al., 2020). Most of these solutions involve increasing the value of the outside option (e.g., by allowing subjects to leave the lab, browse the internet, or earn money by doing something else). As we will discuss below, while we did attempt to address this issue by telling subjects they were permitted to use their phone instead of working on the task, the results suggest that in our experiment this proved insufficient to overcome subjects' intrinsic incentives to exert effort. We acknowledge that this can be viewed as a limitation of our experimental design.

will lead to weakly higher average earnings for high-ability individuals, and (c) it will increase earnings inequality overall.

4. Experimental design

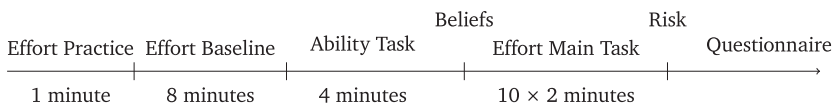
4.1. Overview of the experimental timeline

Figure 1 outlines the timeline of the experiment. The first part of the experiment obtains a baseline measure of the participants' willingness to exert effort for a fixed wage. This stage is implemented prior to treatment, and therefore provides a control measure of effort that is unaffected by the hard–easy task assignment. Next, the ability task is the stage in which the treatment is introduced, with participants exposed to either a hard or easy version of the ability task. In each session, subjects are randomized into two groups of ten. One of the groups completes the hard task, and the other completes the easy task. The reason for this is to have within-session randomization as it is important to control for session fixed effects, but participants never interact with the participants in the other group. After the ability task, we elicit participants' beliefs about their relative placement in the group. Thereafter, they proceed to the main effort task. In this task, participants face ten rounds of the completing the effort task under either a fixed wage or an ability-contingent wage. In each round, except the first, participants first choose one of the two wage structures, and then exert effort. The ability-contingent wage remains constant in every round, while the fixed wage is ratcheted up in order to provide a fine-grained measure of the participant's indifference point. The experiment concludes with a measurement of subject's risk preferences and the questionnaire. We provide further details on each of the components of the experiment in the following sections.

4.2. The ability and the effort measurement tasks

The main components of the experiment are the “ability task”, used to measure a , and the “effort main task”, used to measure e . One challenge for an experiment of this nature is that it is non-trivial to measure ability

Figure 1. Sequence of experimental parts



and effort separately.¹² We explicitly try to address this issue by using two separate tasks – one that we view as depending more on the individual's ability, and less on the effort they exert; and one that depends more on effort, and less on their ability. We contend that this choice of tasks provides us with a reasonably clean measure of these two variables of interest.

4.2.1. The ability task. The ability task consists of a test that is often used to measure IQ, namely Raven Progressive Matrices. Subjects have four minutes to solve as many matrices as they can. Subjects can go back and forth between the 12 matrices and can change their answers until the time is up. Every correct answer yields one point, and there are no negative points for wrong answers. We chose not to directly incentivize the task for two reasons. First, IQ tests tend to induce an intrinsic motive to perform well. Second, we wanted to limit the role of hedging in the belief elicitation. Importantly, the motive to perform well does not differ between the two treatments. In addition, when subjects complete the IQ task, they do not know that they will later be incentivized for accurate beliefs. This is to prevent them from intentionally performing poorly.

4.2.2. The effort task. While the ability task was chosen such that the limiting factor in participants' performance is their ability, the effort task was chosen to be a task where ability plays a minor role and all participants have a lot of control over their performance (i.e., performance depends predominantly on how motivated the individual is in exerting effort). For this purpose, we adopted the slider task by Gill and Prowse (2012). Using the mouse, participants move sliders on the screen from position zero to position 50. Sliders are shown in sets of 20. When all 20 sliders are set to 50, the subject can click the submit button and the sliders are reset to zero for a new round. In Section 4.5, we discuss the incentives that subjects face.

We also measure each individual's baseline effort level under fixed piece rate incentives. This is done prior to the introduction of the treatment variation and it serves two purposes. First, it allows us to check for balance of effort in the slider task between treatment groups, prior to the treatment manipulation. Second, it allows us to control for baseline effort levels when assessing the effect of the treatment, thereby reducing unobserved individual level heterogeneity.

¹²We view ability, a , as being a fixed characteristic of the individual that they cannot change during the time frame of the experiment. In contrast, we view effort, e , as being a malleable object that the participant has full control over.

Table 1. Treatment variation in ability task

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|----------------|----|-----|----|----|----|----|----|----|----|-----|----|----|
| High (Easy) | C1 | C3 | D7 | C6 | D8 | C2 | D6 | C5 | C7 | C12 | C4 | D4 |
| Low (Hard) | E4 | E12 | D7 | C6 | D8 | E6 | D6 | C5 | C7 | E1 | C4 | D4 |
| Same/different | ≠ | ≠ | = | = | = | ≠ | = | = | = | ≠ | = | = |

4.3. Treatment variation

The objective of the treatment variation is to exogenously shift confidence using a minimal intervention. Therefore, the two treatment conditions are completely identical except for a slight difference in the difficulty of the ability task. Within each session, subjects are randomly assigned to one of two groups. One entire group is exposed to a harder version, and the other group to an easier version of the Raven Progressive Matrices. Eight of the twelve puzzles are identical in both treatments. The remaining four are either slightly easier or slightly harder than the rest. Table 1 shows the precise sequence of Raven matrices faced in each of the treatment groups. In each of the four matrices that differ, switching a C-matrix for an E-matrix represents an increase in difficulty. Moving backwards and forwards between puzzles is allowed in the task. Subjects only interact with other participants who completed exactly the same test as them (i.e., in their treatment group of ten subjects), and they know this.¹³

This approach draws on the finding in the psychology literature that when the difficulty of a task increases, this causes a downward shift in an individual's confidence regarding their relative position in the ability distribution. Conversely, facing an easier task makes individuals more confident regarding their position in the distribution (Burson et al., 2006; Healy and Moore, 2007; Larrick et al., 2007; Moore and Healy, 2008; Bordley et al., 2016; Benoît et al., 2015). Importantly, these results assume a constant group composition, so there is no reason for the individual's actual rank to change when the difficulty of the test is shifted. An explanation for this result is that when the difficulty of a test is reduced, individuals find the test easier and adjust their assessment of their own performance upwards. However, they do not adjust their belief of the distribution of others' scores up as much. This results in a higher relative assessment of their own performance. Kruger (1999) shows that this miscalibration can lead to the majority of subjects evaluating themselves as worse-than-average in difficult tasks and better-than-average in easy tasks.

¹³Our experiment uses within-session assignment to treatment in order to avoid the numerous potential issues associated with between-session randomization.

In our experiment, therefore, we name the treatment in which subjects face the hard test, the “Low-confidence” treatment, and the treatment in which subjects face the easier test, the “High-confidence” treatment.

4.4. The belief elicitation task

After the ability task, we elicit subjects’ beliefs about their relative performance in comparison to the nine other participants in their group who faced the same task as them. More specifically, we asked subjects the following question. “What do you think is the probability that you scored among the top five participants in the IQ picture task?” To give some guidance in thinking about probabilities, we provided the participants with a scale of possible answers ranging from “0 – I am certain that I scored in the bottom half” to “100 – I am certain that I scored in the top half”. Participants were free to state any number from 0 to 100. Their guess is incentivized using the quadratic scoring rule (Selten, 1998). The quadratic scoring rule is explained in detail to them in the instructions provided, both on screen and on paper. The scoring rule is designed to provide the highest expected payoff when subjects state their true beliefs. Maximum earnings are €2 for the belief elicitation task. The belief elicitation came as a surprise at this point in the experiment to prevent subjects artificially manipulating their earlier scores in the ability task in anticipation of the belief elicitation.

In addition, we asked participants to report their best guess of how many points they scored in the task and what they believe the fifth highest score in their group is (unincentivized, in order to avoid hedging).

4.5. The wage scheme choice

To obtain a fine-grained measurement of subjects’ precise relative valuation of the two payment schemes, we constructed a task where subjects make the choice ten times, but the fixed wage is gradually ratcheted up in each successive choice. Therefore, subjects face ten rounds of two-minute real effort tasks. In each of these rounds (except the first), subjects can choose whether they would prefer a fixed piece rate, or whether they want to work under the ability-contingent piece rate. Table 2 summarizes the two wage schemes available in each round. A subject’s switching point reveals the round in which they are indifferent between the two wage schemes.

Another important feature of the experimental design is that the ability component of the production function is held fixed, and the distribution is the same in the two treatments (i.e., 50 per cent in the top half, and 50 percent in the bottom half). This is done by using the ability task to obtain a

Table 2. Payment scheme in main effort task

| Period number (2 min each) | Option A payment (per 20 sliders) | Option B payment (per 20 sliders) |
|-------------------------------|--------------------------------------|--------------------------------------|
| 1 | Can't choose Option A | €1 if top half; €0 if bottom half |
| 2 | €0.10 | €1 if top half; €0 if bottom half |
| 3 | €0.30 | €1 if top half; €0 if bottom half |
| 4 | €0.40 | €1 if top half; €0 if bottom half |
| 5 | €0.50 | €1 if top half; €0 if bottom half |
| 6 | €0.55 | €1 if top half; €0 if bottom half |
| 7 | €0.60 | €1 if top half; €0 if bottom half |
| 8 | €0.65 | €1 if top half; €0 if bottom half |
| 9 | €0.70 | €1 if top half; €0 if bottom half |
| 10 | €0.80 | €1 if top half; €0 if bottom half |

fixed measurement of ability before the participant reports their belief, and before they choose between payment schemes. This implies that differences in beliefs, choice of incentive scheme, and effort can be causally attributed to the shift in beliefs due to the treatment. It rules out potential issues that can arise if one were to allow participants to first make an incentive scheme choice, and then produce output that depends on both ability and effort. In this case, one would not be able to distinguish a high-ability individual who exerts low effort from a low-ability individual who exerts high effort.

There are a few additional features of the payment scheme choice that are worth noting. First, to rule out learning effects (with respect to own ability), subjects receive no feedback about their relative ability score or about their performance in any components of the effort task until the very end of the experiment. Second, in the initial period, all participants must work under the ability-contingent piece rate. This feature allows us to assess how the shift in confidence affects effort provision when all subjects are forced to work under the ability-contingent piece rate, thereby avoiding endogenous selection effects – in all other rounds, the incentive scheme that a subject faces is endogenous. Third, the fixed piece rate increases in each period from €0.10 per 20 sliders in the second period to €0.80 per 20 sliders in the last period. Once the expected earnings from the ability-contingent piece rate are lower than the fixed piece rate in that period, individuals should switch to the fixed piece rate and choose it for the remainder of the experiment, assuming risk-neutral preferences.¹⁴

¹⁴At the end of the experiment, five of the ten rounds are randomly chosen for payment. Subjects had to answer four control questions before starting the task to ensure comprehension of the payment scheme. To elicit the baseline motivation of moving sliders, subjects complete nine

4.6. The risk elicitation

Finally, we elicit risk preferences by adapting the preferences module on risk taking by Falk et al. (2016) to our setting. The staircase procedure is essentially equivalent to a standard multiple price list, presenting multiple choices between a sure payoff and a gamble, but it requires fewer decisions on the part of the subject in comparison to a traditional price list by avoiding redundant choices. The staircase we use has four choices between a sure payment and a risky gamble. The outcomes of the risky gamble are always €0 or €1, each associated with a 50 percent chance of occurring. The sure payment value was varied across decisions to allow us to elicit the subject's point of indifference. One of the decisions was randomly chosen for payment.

At the end of the experiment, we administered a comprehensive questionnaire.

4.7. The procedure

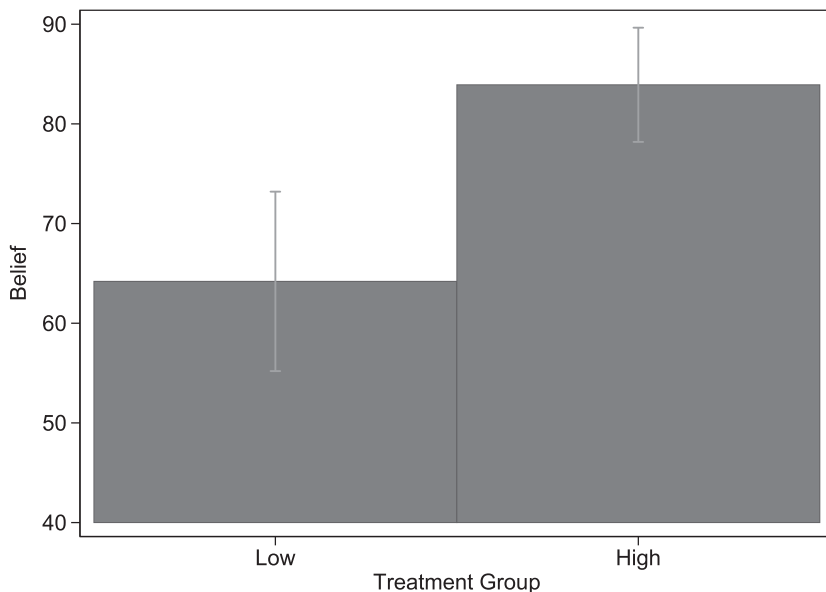
The experiment was programmed in zTREE (Fischbacher, 2007) and conducted at the WZB-TU experimental laboratory in 2017. Participants were solicited through an online database using ORSEE (Greiner, 2015) from a subject pool of mostly undergraduate students from all faculties. In total, 100 subjects participated in five sessions, with 20 in each: 47 of them were female, 49 male, and four chose not to self-report their gender. Subjects received a show-up fee of €5 plus their earnings from the tasks. Mean earnings for the 60-minute sessions amounted to €13.30. The relevant instructions were handed out to participants at the beginning of each stage and read out loud. Complete instructions as they appeared to participants are provided in the Online Appendix.

5. Results

5.1. Does the hard-easy treatment shift beliefs?

The main objective of our treatment manipulation is to exogenously shift participants' beliefs about their relative performance in the IQ test. In line with Hypothesis 1, Figure 2 shows that we find a significant difference

minutes of the effort task at the start of the experiment, the first minute being an unincentivized practice round. In the baseline round, we pay €0.30 per 20 sliders, and all completed sets are paid out. The objectives of the baseline round were the following. First, it allows subjects to familiarize themselves with the slider task, thereby ameliorating learning effects during the main effort task. Second, it allowed us to obtain a baseline measure of subjects' effort choices prior to the treatment variation under fixed incentives. This allows us to check for baseline balance in effort, and also to control for subjects' baseline effort at the individual level.

Figure 2. Average stated beliefs by treatment

Notes: Vertical lines denote 95 percent confidence intervals around the mean.

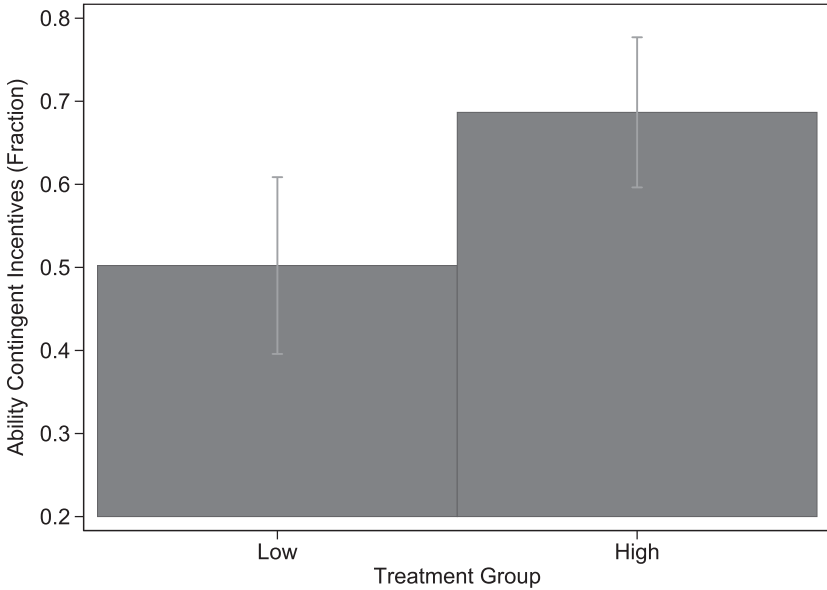
in the participants' level of confidence in their own ability between the two treatment groups, where confidence refers to the individual's stated probability of being in the top half of their group (t -test, $p < 0.01$).¹⁵ We refer to the easy task treatment as the High-confidence treatment, and the difficult task treatment as the Low-confidence treatment.

Result 1. *In line with the previous hard–easy effect literature, reducing the difficulty level of the ability task increases the average confidence that participants have in their own relative performance.*

5.2. The influence of beliefs on wage scheme choices

Next, we present evidence on whether the increase in confidence translates into actions by increasing the proportion of individuals choosing a high-earnings-risk wage scheme (i.e., the ability-contingent wage). This is a test

¹⁵The top panel of Table E1 in Online Appendix E shows that our treatment groups are balanced on observable characteristics (as would be expected given the within-session randomization). The second panel confirms that, by design, the ability score differs, but the relative ability distribution is identical between treatments.

Figure 3. Propensity to choose ability-contingent incentives

Notes: Vertical lines denote 95 percent confidence intervals around the mean.

of Hypothesis 2. Figure 3 shows that the ability-contingent wage is chosen significantly more often in the High treatment condition than in the Low treatment condition (diff. = 18pp; t -test, $p < 0.01$).¹⁶ The first two columns of Table 3 show that this result is unaffected by session fixed effects.

In addition to documenting the treatment effect on choices, it is informative to provide more direct evidence on whether this treatment effect operated via beliefs. To do this, Column 3 of Table 3 shows that the subjects' reported beliefs about their likelihood of being in the top half are highly predictive of their incentive scheme choices – a 1 percentage point (pp) increase in a participants' belief is associated with choosing the ability-contingent incentives 0.86 pp more often. However, this relationship may be endogenous. A nice feature of the experimental design is that we can use the treatment variation as an instrument for beliefs. Columns 4 and 5 report the results from this exercise, showing that the exogenous shift in beliefs does indeed translate directly into a change in wage choices. This cleanly demonstrates a causal relationship between beliefs and action choices in this context, showing that this result is not driven by other

¹⁶Wondering what Taylor would do if she were a man? In Online Appendix D, we analyze the gender heterogeneity in the stylized career choices we observe.

Table 3. Propensity to choose the ability-contingent incentives

| | OLS (1) | OLS (2) | OLS (3) | IV (4) | IV (5) |
|-----------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| Treatment (High = 1) | 0.18*** (0.07) | 0.18*** (0.07) | | | |
| Subjective belief | | | 0.86*** (0.10) | 0.94*** (0.26) | 0.93*** (0.26) |
| Risk, CE ($p = 50$) | | | | | 0.21 (0.19) |
| Constant | 0.50*** (0.05) | 0.55*** (0.08) | -0.01 (0.09) | -0.07 (0.20) | -0.17 (0.22) |
| Session fixed effects | | ✓ | ✓ | ✓ | ✓ |
| Observations | 100 | 100 | 100 | 100 | 100 |
| Adjusted R^2 | 0.058 | 0.068 | 0.462 | | |
| First-stage F | | | | 13.91 | 13.88 |

Notes: In the instrumental variables (IV) regressions, subjective beliefs are instrumented using the treatment dummy. Standard errors are given in parentheses. The dependent variable is the fraction of AC choices in rounds 2–10. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

unobserved differences between individuals who hold high beliefs and low beliefs.¹⁷

Result 2. *An increase in confidence results in a higher propensity to choose the ability-contingent wage scheme.*

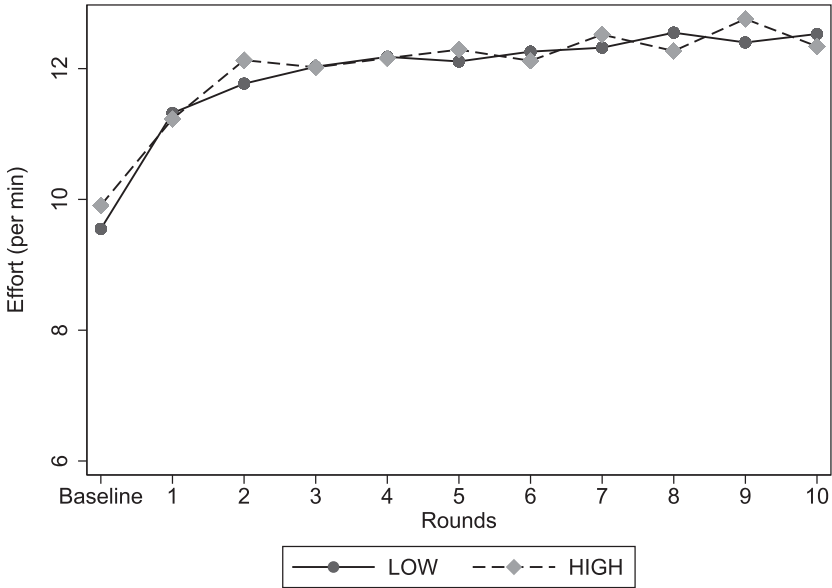
5.3. Influence of beliefs and incentive choice on effort

Once an individual has chosen their incentive scheme, the second choice that must be made is the choice of how much effort to exert. For this effort choice, our simple theoretical framework described two sets of mutually exclusive predictions: one for scenarios where intrinsic motivation is low, and one for scenarios where intrinsic motivation is high.

Taken together, the data collected in our experiment are more consistent with the second scenario. We measure effort using the variable “effort per minute”, which reflects the number of sliders completed during each minute within a particular round. We find no significant difference in average effort exerted between treatment groups (see Figure F1 and Table E3 in the Online Appendix). Further, we present two additional pieces of evidence in favor of the explanation that intrinsic motivation is high in the experiment.

First, we show that effort is not strongly associated with the participants’ expected wage rate. Figure 4 plots the average per minute effort exerted in

¹⁷In Online Appendix B, we provide evidence on wage scheme choice heterogeneity across rounds.

Figure 4. Effort choices across rounds, by treatment

the baseline round, as well as in every subsequent round. While we do see some initial learning, after the baseline round, there is very little change in effort exerted even though the value of the fixed piece rate increases from €0.10 to €0.80, and the fraction of individuals choosing this fixed piece rate incentive increases substantially in both treatment groups between round 2 and round 10.

Second, we show that effort does not appear to be associated with the participants' beliefs even when they are exogenously assigned to the ability-contingent wage scheme. Figure F2 in the Online Appendix focuses on the first round in which all participants faced the ability-contingent incentive scheme. This figure shows that, in both treatments, effort is highly unresponsive to beliefs. While our treatment successfully shifted the beliefs of participants in the two treatments, it did not affect the relationship between beliefs and effort, which is rather flat. This finding is not completely surprising in view of the recent literature that documents an unresponsiveness of effort to monetary incentives under certain conditions (see, e.g., Corgnet et al., 2015; Araujo et al., 2016; DellaVigna and Pope, 2018; Erkal et al., 2018; Goerg et al., 2019; DellaVigna et al., 2020). These studies provide strong evidence of a complex and important role played by different intrinsic motives in determining the effort levels. Several studies have also proposed solutions to the low elasticity of effort to material

Table 4. Effort choice (per minute) under ability-contingent incentives (Round 1)

| | OLS (1) | OLS (2) | OLS (3) | IV (4) | IV (5) | IV (6) |
|-----------------------|--------------------|--------------------|--------------------|--------------------|-------------------|-------------------|
| Treatment (High = 1) | -0.09 (0.45) | -0.09 (0.44) | | | | |
| Subjective beliefs | | | 1.11 (0.79) | -0.46 (2.19) | -2.29 (1.79) | -2.30 (1.79) |
| Baseline effort | | | | | 1.00*** (0.16) | 1.00*** (0.16) |
| Risk, CE ($p = 50$) | | | | | | 0.50 (1.37) |
| Constant | 11.32*** (0.31) | 11.97*** (0.54) | 11.09*** (0.77) | 12.27*** (1.76) | 3.91*** (1.10) | 3.68** (1.27) |
| Session fixed effects | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 100 | 100 | 100 | 100 | 100 | 100 |
| Adjusted R^2 | -0.010 | 0.013 | 0.033 | | | |
| First-stage F | | | | 13.92 | 13.09 | 13.03 |

Notes: The dependent variable is Round 1 effort per minute. Higher values of risk variable (i.e., certainty equivalent for 50–50 gamble) imply risk loving. Standard errors are given in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

incentives issue by increasing the opportunity cost of working on the task (see, e.g., DellaVigna and Pope, 2018; Erkal et al., 2018; Goerg et al., 2019; Chen and Schildberg-Hörisch, 2019).¹⁸

Both of these results are interesting in light of the fact that we have already seen that there was a strong causal response of the wage scheme choice to a shift in beliefs. This causal effect on wages means we can rule out the hypothesis that beliefs are generally not meaningful for action choices. Overall, this evidence is in line with the explanation that the intrinsic motive to exert effort in the task crowds out the extrinsic motive, resulting in a very low elasticity of effort to changes in expected monetary rewards.

Table 4 reiterates these findings by examining the correlates of effort in Round 1 (i.e., under the ability-contingent wage). Columns 1 and 2 confirm that there is no treatment difference in effort choices; Columns 3 and 4 provide further evidence that there is no significant relationship

¹⁸While these solutions are very useful, it is perhaps worth noting that a high elasticity to monetary incentives is not an a priori desirable feature of an experimental design. There are many real-world jobs in which intrinsic incentives are more important and do dominate the extrinsic incentives. Therefore, the optimal experimental design depends on the characteristics of the context that one wishes to capture. For example, allowing subjects to leave the lab early might reflect jobs with flexible working hours, but not reflect those with rigid working hours. However, in the context of our experiment with the focus on the role of beliefs, the insensitivity of effort to monetary incentives is a limitation of the design.

between individuals' beliefs and their effort choices. In Columns 5 and 6, we include baseline effort, which was measured prior to the treatment variation being introduced. We again observe no significant relationship between effort and beliefs.

Result 3. *Effort choices are largely unresponsive to shifts in beliefs, and to the participant's choice of incentive scheme. The low elasticity of effort to change in the expected monetary rewards is indicative of a scenario where intrinsic motivation is high.*

5.4. Earnings

We now turn to the effect of increased confidence on earnings. While the earnings structure was chosen by the experimenter and the absolute levels are thus not externally valid to earnings structures with different characteristics, it is nevertheless interesting to investigate how participants make choices in a given earnings environment. Hypothesis 4 states that increased average confidence will lead to weakly lower earnings for low-ability individuals and weakly higher earnings for high-ability individuals, and will result in a higher earnings inequality overall. To evaluate this hypothesis, we split the sample into the top half and bottom half ability groups and look at the effect of the treatment on each group. Figure F3 in the Online Appendix shows that earnings for individuals in the bottom half reduced by 40 percent from €3.47 to €2.11, and earnings for the top half were almost unchanged by the treatment, at just above €11. Table 5 shows the same pattern of results, indicating a significant drop in the earnings of the bottom half group ($p < 0.05$). The average effect when pooling ability types has a negative sign but is not significant.

Table 5. Change in earnings due to exogenous belief shift

| | All | | Bottom | | Top | |
|-----------------------|-------------------|--------------------|-------------------|-------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Treatment (High = 1) | -0.59 (0.99) | -0.57 (1.01) | -1.36** (0.63) | -1.35** (0.64) | 0.19 (0.82) | 0.20 (0.81) |
| Constant | 7.27*** (0.70) | 10.39*** (2.32) | 3.47*** (0.45) | 5.81*** (1.55) | 11.07*** (0.58) | 11.69*** (1.87) |
| Baseline effort | | ✓ | | ✓ | | ✓ |
| Risk, CE ($p=0.5$) | | ✓ | | ✓ | | ✓ |
| Session fixed effects | | ✓ | | ✓ | | ✓ |
| Observations | 100 | 100 | 50 | 50 | 50 | 50 |
| Adjusted R^2 | -0.007 | -0.029 | 0.070 | 0.062 | -0.020 | -0.001 |

Notes: The dependent variable is main task earnings. Standard errors are given in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

This evidence is consistent with the predictions discussed in the theoretical framework section, showing that an increase in confidence leads to a drop in earnings for the low-ability individuals who are already earning far less, and thereby moving in the direction of higher overall earnings inequality.¹⁹

Result 4. *An increase in confidence leads to low-ability individuals earning even less than their already low earnings, while high-ability individuals are unaffected. This is suggestive of an increase in inequality with higher confidence, but our data do not permit us to estimate a significant change in inequality.*

One outstanding question is why we observe a relatively large decrease in earnings for the low-ability individuals, but hardly any change in earnings for the high-ability individuals. We discuss this question in more detail in Online Appendix C. In short, the effect of treatment on the earnings of the bottom half, but not the top half, is driven by the fact that individuals in the top half are already highly confident in their ability, and frequently choose the ability-contingent wage, while individuals in the bottom half appear to hold more malleable beliefs and are willing to be convinced that they are in the top half when taking an easier test.

6. Discussion

One should always exercise caution when extrapolating findings from a laboratory experiment to the real world. However, our findings can be used to provide insight into the way that confidence influences the types of choices that are relevant for one's career. In many professions, talent or ability are essential for success. Thus, holding overconfident beliefs about one's ability may prove extremely costly if it leads to individuals mistakenly entering into these talent-intensive careers. For example, for artists, musicians, football players, and arguably for academic researchers, the reward schedule is highly ability-contingent. While exerting a high degree of effort is necessary in these professions, one can only compensate for a lack of ability through increased effort to a limited extent. Thus, it can be a mistake for low-ability individuals to choose

¹⁹While the Gini coefficient increases from 0.275 in Low to 0.293 in High, and Figure F4 in the Online Appendix provides suggestive evidence of higher inequality in the High treatment by plotting the earnings histograms of both treatments, a Mann–Whitney rank-sum test indicates that there is no significant difference between the earnings distributions in the two treatments. Furthermore, a difference-in-difference estimate of the change in the earnings between the top half and bottom half within each group has a negative point estimate of -1.56 but is not significant at the 10 percent level. We are not able to detect the effect of treatment on inequality.

to enter these professions. Similarly, entrepreneurs, where ability can be considered a combination of the business idea and the talent of the entrepreneur, have a high risk of toiling for years without any success.

Given how costly it is to choose the wrong profession, and how this choice seems to hinge on one's beliefs about oneself, it is worrying that the results of our experiment, as well as those in the previous hard–easy effect literature, demonstrate that one's beliefs about one's own abilities are highly malleable – particularly those of individuals of lower ability.

In our experiment, participants' inference about their placement in the ability distribution is influenced by the difficulty of the task. In a real-world setting, this could mean that exposing children or students systematically to challenges that are “too easy” (e.g., “spoon-feeding” them) might result in them holding an artificially high sense of confidence in their own abilities. These inaccurate beliefs may be reinforced and intensified through other societal channels, for example, by the well-meaning rose-tinted feedback of family and friends, and the education system. It is therefore concerning that both of these channels seem increasingly tailored towards providing positively skewed feedback.²⁰

The overarching policy lesson of this paper is that while there might certainly be benefits to building up confidence, when it comes to professions or tasks that rely heavily on talent that cannot be easily compensated for by increased effort, interventions aimed at increasing confidence might hurt exactly those people they are intended to help. It might be better to construct interventions that help individuals develop their abilities, but also provide them with accurate feedback.

7. Conclusion

In this paper, we have shown how an individual's confidence can be easily shifted by a small change in their environment and how that confidence

²⁰Evidence of such positively skewed social feedback structures includes the following examples. First, in a controlled laboratory experiment, Gneezy et al. (2017) show that even strangers are usually unwilling to give negative feedback to another person face-to-face, even when it is costly to withhold this feedback. Second, looking at observational data, the past two decades have seen an enormous grade inflation at the university level, both in the US and in many European countries (Rosovsky and Hartley, 2002). According to the Higher Education Statistics Agency (HESA, 2021), in 2006/07, only 13 percent of all students received a first-class degree in the UK. In 2019/20, 35 percent did. Nordin et al. (2019) argue that grade inflation is prevalent around the world and, using data from Sweden, provide evidence that it can affect important long-run life outcomes.

shift can causally influence the choices they make. The discussion above regarding the way beliefs influence actions raises the question of how exactly this causal shift operates. According to standard models, a shift in beliefs would leave tastes unchanged and influence action choices by altering the perceived returns to effort under the ability-contingent incentives. However, it is also plausible that a shift in confidence affects choices by influencing the individual's tastes (e.g., for taking on risk). While we cannot rule out the possibility that the shift in confidence in our experiment influenced participants' tastes, we can ask whether the treatment affected one key preference parameter, since we elicited the participants' risk preferences after they were treated. It turns out that these elicited risk preferences are nearly identical across the two treatments.²¹ This indicates that risk preferences remained unchanged by the confidence shift, and points towards a shift in the perceived returns to effort as a more likely channel.

This study contributes towards the task of developing a better descriptive understanding of the way people absorb new information, form their beliefs, and adjust their decisions on the basis of those beliefs. While there is already a large and rapidly expanding literature that focuses on the relationship between information and subjective beliefs (information \rightarrow_A beliefs), this paper contributes to the less advanced project of empirically examining the causal relationship between these subjective beliefs and action choices (beliefs \rightarrow_B actions). A clear understanding of both of these components is essential if we wish to accurately describe how people interact with the uncertainty in their environment. Our results demonstrate that small changes in the environment, even uninformative ones, can lead to substantial shifts in confidence and translate into large adjustments to decision-making. We also show that while the belief shift affected the incentive scheme choice, it did not influence effort choices. This illustrates that domain-specific factors can be important in determining the precise relationship between beliefs and actions, with non-material motives often exerting an important influence. While the stylized nature of our experiment implies that the results should not be extrapolated to settings in the real world with different characteristics, our study points towards the importance of conducting more empirical research that explicitly examines the causal relationship between beliefs and actions.²²

²¹The average certainty equivalent for a 50–50 gamble between €1 and €0 in the Low treatment is 0.483, while it is 0.489 in the High treatment (t -test, $p = 0.84$).

²²While the control afforded by laboratory experiments provides one appealing avenue for this line of enquiry, a recent wave of online information provision experiments offers another fruitful path for conducting research of this nature (see Haaland et al., 2020, for a recent review).

Supporting information

Additional supporting information may be found online in the supporting information section at the end of the article.

Online Appendix Replication Files

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