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Shallow Meritocracy

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Abstract: Meritocracies aspire to reward hard work but promise not to judge individuals by the circumstances into which they were born. However, the choice to work hard is often shaped by circumstances. I show that people's merit judgments are insensitive to circumstances' effect on choice. In an experiment, US participants judge how much money workers deserve for the effort they exert. Unequal circumstances discourage some workers from working hard. Nonetheless, participants hold disadvantaged workers responsible for their choices. Participants reward the effort of disadvantaged and advantaged workers identically, regardless of the circumstances under which choices are made. Additional experiments identify an important underlying mechanism. Individuals understand that choices are influenced by circumstances. But, in light of an uncertain counterfactual state – what exactly would have happened on a level playing field – individuals base their merit judgments on the only reliable evidence they possess: observed effort levels. I confirm these patterns in a structural model of fairness views. Finally, a vignette study shows that merit judgments can be similarly "shallow" when choices are shaped by racism or poverty.

JEL-Codes: C91, D63, D91, H23.

Keywords: Meritocracy, fairness, responsibility, attitudes toward inequality, redistribution, social preferences, inference, uncertainty, counterfactual thinking.

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1 Introduction

The notion of meritocratic fairness is at the heart of Western political and economic culture. It shapes which inequalities are considered to be fair, which redistributive policies are implemented, and how welfare states are designed (Alesina and Glaeser, 2004; Alesina and Angeletos, 2005; Cappelen et al., 2020b; Sandel, 2020). In essence, meritocratic fairness means that people should be rewarded in proportion to their merit. Besides talent and skill, the choice to work hard and exert effort is considered a central determinant of merit. By contrast, external circumstances beyond the individual's control – such as parental background, race, or sex – are not viewed as legitimate sources of merit (Alesina and Angeletos, 2005; Almås et al., 2020; Cappelen et al., 2020b; Konow, 2000). Meritocratic fairness thus distinguishes between effort choices (relevant for merit) and external circumstances (irrelevant for merit).

However, the distinction between choices and circumstances is clouded by a ubiquitous feature of human behavior: Agents' choices are endogenous to and shaped by the circumstances, opportunities, and incentives they face. For instance, a person growing up with few opportunities and incentives to work hard might respond by exerting little effort. Likewise, minorities that experience discrimination might be discouraged from working hard. Indeed, empirical studies have linked effort, career, and schooling choices to gender norms, racial inequality, and the socioeconomic environment (e.g., Altmejd et al., 2021; Bursztyn et al., 2017; Carrell et al., 2010; Falk et al., 2020a; Glover et al., 2017; Parsons et al., 2011). Moreover, the fact that adverse environments often lead to detrimental decision-making is considered a key cause of poverty (e.g., Bertrand et al., 2004; Haushofer and Fehr, 2014). Thus, a fundamental issue in any meritocratic system is how to reward choices that are shaped by external circumstances. Are people held responsible for their choices when they are the product of external circumstances?[1]

This study explores the prevailing notion of meritocratic fairness in the US. It investigates whether people reward choices in the light of or irrespective of the surrounding circumstances. I build on a series of online experiments with a large, broadly representative US sample of about 4,000 respondents. The study proceeds in four steps.

First, I conduct a tightly controlled incentivized choice experiment to isolate and identify how people judge the merit of choices shaped by circumstances. In the experiment, each participant ("spectator") judges how much money two "workers" should earn for their effort in a piece-work job. The workers collect address data in a simple, standardized task that requires effort and diligence. Workers' effort choice is how many tasks

[1] As a side note, valuable talents, traits, and abilities such as cognitive skills are also commonly viewed as legitimate determinants of merit. Yet, these skills are also shaped by external circumstances (e.g., Alan and Ertac, 2018; Heckman, 2006; Kosse et al., 2019; Putnam, 2016), so a similar question arises for circumstances' effect on skills. This study focuses on circumstances' effect on choices because it is the simpler, more transparent, and relatable channel.

they complete. Their circumstances are the piece-rates they earn, which are randomly assigned and can be either high ($0.50) or low ($0.10), each with a 50% chance. By chance, one worker receives the high rate and the other the low rate. All workers know about the lottery, but – as described below – I vary across treatments whether workers know their assigned rate. The spectators learn about the workers' situation, then they decide which final payment each worker should earn. They can freely redistribute the earnings between the two workers, thereby judging which reward each worker deserves. These merit judgments are the central outcome variable of the study. I employ a contingent response method. Spectators make multiple merit judgments under different scenarios, each presenting different effort choices that the workers could make. To incentivize spectators' decisions, a random subset of their redistribution decisions is implemented.

To identify whether spectators' merit judgments are sensitive to circumstances' effect on workers' choices, the experiment exogenously varies the environment in which workers make their effort choices. In the *control* condition, the workers do not yet know their realized piece-rates. They only know their odds of obtaining a high or low piece-rate, which are identical for both workers. Hence, their effort choices are directly comparable because their choices are made in the same environment and subject to the same situational influence – a level playing field. By contrast, in the *treatment* condition, workers immediately learn about their realized piece-rates. Workers with the high piece-rate are encouraged and advantaged by these circumstances, whereas workers with the low piece-rate are discouraged and disadvantaged. Indeed, workers work much harder and complete roughly three times as many tasks for the higher piece-rate. Thus, the endogeneity of choices differentially (dis)advantages the workers in the treatment condition, but not in the control condition. I compare spectators' merit judgments across the two conditions and test whether spectators compensate the disadvantaged workers in the treatment condition for the fact that they are discouraged from working hard.

The results show that participants' merit judgments are insensitive to circumstances' effect on choices. While the spectators redistribute payments to reward workers for higher effort, they do so equally in both conditions. They do not respond to the fact that the disadvantaged worker is discouraged from working hard in the treatment condition but not in the control condition. The large sample size allows me to rule out even minor effect sizes. The results thus provide strong evidence for the absence of a meaningful effect of the endogeneity of choices on merit judgments. Spectators hold workers responsible for their choices, even if these choices are shaped by external circumstances over which the workers have no control.

In the second step, I ask *why* spectators do not factor in that circumstances influence workers' choices. I start by investigating whether spectators underestimate the power

of situational influence, that is, the effect of circumstances on effort choices, in line with the *fundamental attribution error* (Ross, 1977). If spectators underestimate situational influence, they have little reason to correct for it. In the main experiment, I measure incentivized beliefs about how strongly the piece-rates influence workers' effort choices. The results show, however, that spectators even slightly overestimate the piece-rate effect, so that its irrelevance for merit judgments cannot simply be attributed to biased beliefs.

Of course, this does not yet rule out that circumstances' effect on choices escapes spectators' *attention* while rewarding the workers. In a new experimental condition, I therefore implement an attention intervention in which I draw spectators' attention to the effect of situational influence just before their merit judgments. However, even then, their merit judgments remain insensitive to the endogeneity of choices.

Spectators thus seem to be aware of and accurately anticipate the average, expected piece-rate effect. However, they still do not know with certainty what the two specific workers for whom they are responsible would have done in equally advantaged circumstances. Would their disadvantaged worker have worked much harder for the high piece-rate, or would he still have exerted only little effort? This specific *counterfactual* remains unknown and uncertain, even when the expected counterfactual is known. In light of this, spectators might abstain from any conjecture and base their merit judgments on what they know with certainty: observed effort levels.

I test for the role of this uncertain counterfactual in an additional experiment in which I exogenously resolve the uncertainty of the counterfactual. I provide a subset of spectators with accurate and reliable information about what their specific disadvantaged worker would have done in the advantaged environment. I find that spectators' average merit judgments react strongly to this information. Spectators who learn that their disadvantaged worker would have worked harder in the advantaged environment take the endogeneity of choices into account and compensate their disadvantaged worker. By contrast, spectators who do not receive any information about the counterfactual remain unresponsive to the endogeneity of choices. Disadvantaged workers thus face a "burden of the doubt": When their counterfactual choices cannot be verified, only their observed choices count, and these observed choices are shaped by unequal circumstances. This suggests that uncertainty about what would have happened on a level playing field explains why merit judgments are insensitive to circumstances' effect on choices. Since this counterfactual is almost always unobservable in the real world, the insensitivity to the endogeneity of choices is likely to be a fundamental feature of merit judgments. Moreover, this effect is driven by about one-quarter of spectators. The remaining participants do not adjust their merit judgments even when the counterfactual is known.

In the third step, I shed light on the rich heterogeneity in merit judgments hidden behind the average results. I estimate a behavioral structural model of fairness views that organizes the reduced-form evidence and assesses the prevalence of different merit views in the population (DellaVigna, 2018). The model distinguishes between two meritocratic fairness views: "comparable choice meritocrats" and "actual choice meritocrats". Comparable choice meritocrats base their merit judgments on the counterfactual effort choices that workers would make in identical, comparable circumstances, but – in line with the reduced-form results – potentially discount this counterfactual when it is unknown and uncertain. Actual choice meritocrats reward workers proportionally to their actual effort choices, even if these choices are endogenous to external circumstances. The estimated model classifies 26% of participants as comparable choice meritocrats. In line with the reduced-form results, I estimate that they completely discount situational influence when the counterfactual is uncertain. Meanwhile, 37% of participants are classified as actual choice meritocrats. In addition, I estimate a share of 23% "libertarians" who accept any inequality and never redistribute and 14% "egalitarians" who think that the workers always deserve equal payment. The results show that people hold fundamentally different fairness views. Importantly, they also reveal that, even in a world where counterfactual choices are known, only about one-quarter of individuals would compensate for disadvantageous situational influences.

Although the controlled experimental environment comes with the crucial advantage that the effect of interest is credibly identified, it also comes at a cost: It differs from many real-life settings that characterize the debate about merit, choices, and circumstances. In the final, fourth step, I therefore run a vignette study showing that the insensitivity of merit judgments to circumstances' effect on choices can also be observed in real-world labor market and career choice scenarios. For instance, participants do not compensate a black employee who chooses not to work hard for a promotion but faces racial discrimination and has no chance of being promoted anyway. Likewise, they do not compensate a person who shows hardly any effort in his or her life but grew up in a discouraging environment with few opportunities and incentives to work hard. In both cases, the choice not to work hard legitimizes an unequal outcome, irrespective of the disadvantageous external situational influences.

Taken together, my findings suggest that the prevailing notion of meritocratic fairness is "*shallow*". Meritocratic fairness holds that individuals should not be judged by their external circumstances. Yet, people do not factor in that these external circumstances also influence the choices that agents make. They hold them responsible for these choices. Choices can thereby "launder" unequal circumstances and legitimize the ensuing inequality. These fairness views matter because they are likely to affect how people treat their co-workers and fellow citizens, which inequalities they accept, and which socioeconomic policies they support. One implication is that shallow meritoc-

racy could *doubly* disadvantage the disadvantaged. Not only do they face adverse and discouraging circumstances but they are also blamed and held responsible if they show less effort, dedication, and perseverance in these conditions. Moreover, affirmative action and redistributive policies, which aim to correct for this double disadvantage, are highly contentious and often opposed precisely because they are considered to be violating meritocratic fairness.

Related literature The study builds on and contributes to several strands of the literature. The fairness views of the general population have long been a focus of economic research because they are recognized as an important determinant of welfare systems and a defining feature of political culture (Alesina and Glaeser, 2004; Alesina and Angeletos, 2005; Alesina et al., 2018; Andreoni et al., 2020; Bonomi et al., 2021; Fisman et al., 2020; Gethin et al., 2021; Giuliano and Spilimbergo, 2013; Kuziemko et al., 2015; Stantcheva, 2021). Past research documents that the idea of merit is at the center of fairness and inequality acceptance. Merit is associated with choices such as to work hard or to take risks. Unequal rewards derived from unequally meritorious choices are typically considered fair and legitimate (Akbaş et al., 2019; Almås et al., 2020; Cappelen et al., 2007, 2010, 2013; Krawczyk, 2010; Mollerstrom et al., 2015). Small differences in merit sometimes justify large reward inequalities (Bartling et al., 2018; Cappelen et al., 2020a). Moreover, Cappelen et al. (2020c) show that even degenerate choices can have meritorious character. Participants in their study reward "choices" even when the agents have no real choice and can only decide between two identical alternatives. Thus, choices appear to be central for merit judgments. But choices are always the result of both internal causes – an agent's type, their personality, or taste for hard work – and external causes, namely circumstances' ubiquitous effect on choices. This study is the first to show that merit judgments do not noticeably differentiate between internal and external causes of choice, and it provides an in-depth analysis of the underlying behavioral mechanisms.

The finding that people are held responsible for their choices even if these choices are the product of external circumstances also relates to the literature on moral responsibility and moral luck (Baron and Hershey, 1988; Bartling and Fischbacher, 2012; Brownback and Kuhn, 2019; Cappelen et al., 2020c; Falk et al., 2021, 2020b; Gurdal et al., 2013; Nagel, 1979). Individuals are often not only judged by their choices but also the consequences of their choices, even if these are accidental, unintended, and the product of chance. Here, I show that individuals can be held responsible for external luck not only if it shapes the consequences of their decisions but also when it directly impacts their decisions.

This study also connects to a recent literature on inference in economics (e.g., Ben-

jamin, 2019; Bushong and Gagnon-Bartsch, 2021; Enke and Zimmermann, 2017; Graeber, 2021; Han et al., 2020; Liang, 2021). In particular, individuals often struggle with complex decisions in uncertain and contingent environments (Esponda and Vespa, 2014, 2019; Martínez-Marquina et al., 2019) – a key element of counterfactual thinking. However, counterfactual thinking itself remains relatively unexplored in economics, even though cognitive scientists have long since acknowledged its centrality to causal reasoning and inference (Byrne, 2016; Engl, 2018; Kahneman and Miller, 1986; Lagnado and Gerstenberg, 2017; Roese, 1997; Sloman, 2005). This study illustrates that the inherent uncertainty of the counterfactual strongly affects individuals' choices even though they accurately anticipate the expected counterfactual.

Finally, understanding the practice of meritocratic fairness informs the debate about the merits and myths of meritocracy led by social scientists and philosophers (Frank, 2016; Greenfield, 2011; Markovits, 2019; Sandel, 2020; Wooldridge, 2021; Young, 1958). This paper's contribution to the debate is to document people's prevailing notion of meritocratic fairness: Choices are a critical determinant of merit, even when external circumstances influence which choices are made.

The remainder of the paper is structured as follows. Section 2 sets the stage with a short conceptual discussion, Section 3 describes the main experimental design, and Section 4 presents the main results. Section 5 examines their behavioral foundations, Section 6 structurally estimates a model of fairness views, and Section 7 reports the vignette study. Finally, Section 8 concludes the paper.

2 Conceptual discussion

The goal of the paper is to explore whether people's merit judgments take into consideration that others' choices are often substantially shaped by circumstances. To fix ideas, this section discusses and compares two conflicting meritocratic fairness views that people could endorse.

As a motivating example, consider the following case of racial discrimination in the labor market: A white employee and an employee of color can choose whether to work hard for a promotion. However, their boss is notorious for being racist and has never promoted employees of color before. The white employee decides to work hard to win the promotion, the employee of color does not. In the end, the white employee is promoted and awarded an attractive bonus, the employee of color is not.

When judging whether the outcome of this illustrative story is fair, two intuitions collide. On the one hand, the white employee has worked harder, so he or she might deserve the promotion and the bonus. On the other hand, their effort choices have been shaped by the highly unequal and unfair circumstances of racial discrimination.

This simple story captures the essence of a fundamental question for meritocracy. If we want to reward others according to their effort choices but not their circumstances, do we hold them responsible for their choices when these choices are shaped by unequal circumstances?

More generally, consider a situation where two workers choose how much effort to exert, but unequal circumstances encourage one of the workers to work hard, while they discourage the other worker. I distinguish between two meritocratic views on how merit in such a setting should be evaluated, which I refer to as "actual choice meritocratism" and "comparable choice meritocratism".

Actual choice meritocrats hold people fully responsible for their choices, even if these choices are endogenous to external circumstances. Their merit judgments co-move with the effort choices that workers make. Whether these choices result from different environments, is considered irrelevant. This view often seems to underlie the public debate where the idea that people should be held responsible for their bad choices – be it in school (laziness, misdemeanor), health (nutrition, smoking), or at work (low career ambitions, low effort) – is paramount, often without regard to individuals' circumstances (see Greenfield, 2011, for a discussion).

By contrast, *comparable choice meritocrats* do not hold individuals responsible for external causes of choice but only for internal causes.[2] In economics, this view has been endorsed by Roemer (1993) (see Ramos and Van de gaer, 2016, for a discussion). Roemer argues that if individuals cannot be held responsible for their circumstances, they are also not responsible if these circumstances induce poor choices. Hence, when circumstances influence effort, merit and raw effort cannot be equated. Instead, merit judgments need to correct for external influence on choice. Comparable choice meritocrats, therefore, want to compensate workers for any discouraging situational influence. One option to account for the endogeneity of choices is to ask which choices the workers would have made in a fully comparable situation. For example, they could ask how hard the disadvantaged worker would work if his returns to effort would also be high. Then, they base their merit judgment on this counterfactual, comparable effort choice.[3] Of course, this requires an inference about counterfactual, comparable choices, which, if biased, could prevent comparable choice meritocrats from consistently applying their fairness view.

[2]These internal causes of choice, such as type or preference differences, can often be attributed to differential external circumstances as well – be it nature or nurture (Cesarini et al., 2009; Dohmen et al., 2012; Harden, 2021; Heckman, 2006; Kosse et al., 2019). While outside the scope of this paper, one could hence even ask whether these differences can justify merit differences.

[3]In principle, comparable choice meritocrats could also base their merit judgments on counterfactual effort choices in another environment, e.g., low returns to effort. Relatedly, Roemer (1993) takes an individual's relative ranking in the effort distribution conditional on circumstances as a comparable measure of merit. These details affect neither the qualitative argument here nor the interpretation of later treatment effects.

Conceptually, there are intriguing *normative* arguments for both actual choice and comparable choice meritocratism.[4] Here, however, the research question is of *positive* nature. The study investigates which merit judgments the general population makes. First, are they sensitive to circumstances' effect on choices? Second if not, are they insensitive because comparable choice meritocrats are absent from the population or because they incorrectly infer what would have happened under equal circumstances and fail to apply their merit view?

3 Experimental design

Studying how the endogeneity of effort choices shapes merit judgments requires a setting where choices are central to merit and merit judgments can be measured in an incentivized way. And it requires experimental conditions that exogenously vary circumstances' effect on choices. Below, I describe how I tailor the experimental design to meet both requirements.

3.1 Setting: Merit judgments

I create an experimentally controlled situation of inequality between *workers* (referred to as "he") and observe how study participants (*spectators*, referred to as "she") redistribute money between the workers, conditional on workers' effort choices. Spectators decide which reward each worker deserves and thereby judge which merit originates from the workers' choices.

Workers I hire US workers on Amazon's online labor market *Mechanical Turk* for a crowd-working job in which they collect email address data for another research project. In each task, a worker is given the name of a person, searches for the person's website, identifies their email address, and enters it in a data collection form. Typically, it takes about two minutes to complete one task. The crowd-working job requires no special qualification but demands effort and time, ensuring that hard work determines success rather than skill. Each worker k earns a piece-rate π_k (his returns to effort) and can freely choose how many tasks E_k to complete. Workers know that a lottery determines their piece-rate, which can either be high (\$0.50) or low (\$0.10). A worker's initial payment is $\pi_k E_k$. Workers know that someone else might influence their payment, but they neither know when, why, nor how this happens, nor who is involved in this process.

[4]For instance, incentives to behave well could deteriorate if individuals are not fully accountable for their actual choices. Moreover, workers already bore the costs of their working decisions. Why should a lazy worker be rewarded for the hard work he would have done (but did not do) in a counterfactual environment? On the other hand, it seems inconsistent to claim that external circumstances should not influence merit judgments, while their external influence on choice does.

This guarantees that workers cannot distort their effort decisions in anticipation of a later redistribution stage. Each worker additionally receives a fixed remuneration of \$1. The full instructions for the workers are available online (https://osf.io/xj7vc/).

For the redistribution stage, workers are assigned to pairs. I will refer to the two workers in a pair as workers A and B. I focus on pairs where worker A receives a high piece-rate of \$0.50 and worker B receives a low piece-rate of \$0.10.[5] Inequality between the two workers is likely to prevail – either due to differences in effort E_k or the piece-rate π_k. Whereas effort E_k is a choice variable, the piece-rate π_k is outside the control of workers but is likely to shape the workers' effort choices. Indeed, workers complete, on average, more than three times as many tasks (mean: 16.8 tasks) for a high piece-rate of \$0.50 than for a low piece-rate of \$0.10 (mean: 5.0 tasks, see Appendix F), rendering the setting well-suited to study how merit judgments react to circumstances' effect on choices.

Spectators I invite adults from the general US population to participate in the online experiment. Each study participant ("spectator") is assigned to a pair of workers and informed about the workers' task, situation, choices, and earnings. In particular, spectators know that a lottery determines the workers' piece-rate. Spectators then determine the final earnings of both workers and judge which percentage share of the total performance-based earnings each worker deserves. That is, they can redistribute the earnings between both workers. Redistribution comes at no cost.[6] Spectators know that their decision is strictly anonymous and that workers are unaware of the redistribution stage. Appendix H provides the main instructions for spectators, and the full instructions are available online (https://osf.io/xj7vc/).

The redistribution decisions of spectators, neutral third-parties who have no monetary stake in the distribution of funds, commonly serve as a measure of fairness behavior and views (e.g., Almås et al., 2020; Andreoni et al., 2020; Cassar and Klein, 2019; Mollerstrom et al., 2015). They mirror the fact that society's fairness views are often implemented via redistributive schemes that intervene into naturally arising market outcomes – a feature that I want to capture in the experiment. I implement the merit judgments of 100 randomly selected spectators so that spectators' decisions are (probabilistically) incentivized. After all, their decisions can have real and meaningful

[5] In the experiment, I randomly vary whether worker A or worker B is the worker with the advantageous, high piece-rate. Here, I recode all responses as if worker A was the advantaged worker to ease analysis and exposition. Reassuringly, Table B.3 shows that spectators' redistributive behavior is insensitive to whether worker A or worker B is advantaged. Moreover, sometimes both workers of a pair receive a piece-rate of \$0.10 or both receive a piece-rate of \$0.50. These worker pairs are used in additional experimental conditions that I will introduce later.

[6] I abstract from the frequently studied fairness-efficiency trade-off. Existing research shows that fairness concerns often dominate efficiency concerns (Almås et al., 2020). Spectators cannot redistribute the fixed remuneration of \$1 but only the performance-based rewards.

consequences for the workers.[7]

To elicit spectators' merit judgments for various effort choices, I employ a contingent response method. Each spectator decides whether and how to redistribute the earnings in eight different effort scenarios. Each scenario describes how many tasks worker A and how many tasks worker B completed. The first seven scenarios are hypothetical, presented in random order, and selected to represent various effort shares of worker B (denoted by $e = \frac{E_B}{E_A+E_B}$). Panel A of Table 1 summarizes these effort scenarios. For example, in Scenario 1, worker A does all the work and completes 50 tasks, whereas worker B completes no task at all ($e = 0\%$). In Scenario 4, both workers complete 25 tasks ($e = 50\%$). Moreover, in Scenario 7, worker A completes 0 tasks and worker B completes 50 tasks ($e = 100\%$). The other scenarios present intermediate cases. The eighth scenario is real and describes how many tasks the two workers actually complete. Spectators' decisions in this scenario determine the workers' final payoff. However, spectators are not told which scenario is real and hence have to take each of their decisions seriously.[8,9] Effort choices in the real scenario vary across experimental conditions (introduced in the next subsection) due to the incentive effects of the conditions. Thus, the real scenario does not allow a consistent comparison across treatments. To circumvent this problem, I only analyze the merit judgments in the first seven scenarios. The contingent response method is central for the identification because it allows analyzing merit judgments for the same effort scenario and effort choices across the treatment and control conditions.

3.2 Conditions: Varying circumstances' effect on choices

In a between-subject design, I exogenously vary whether workers' effort choices are differentially affected by situational influence, that is, circumstances' effect on effort choices. For this purpose, I manipulate *when* the workers learn about the realized piece-rate of their lottery and inform spectators about this. Panel B of Table 1 provides an overview of both conditions.

[7] Charness et al. (2016) review the advantages and disadvantages of implementing the decisions of a subset of participants versus those of all participants. The literature documents little difference between both methods.

[8] Indeed, only a few spectators can distinguish the hypothetical scenarios from the real one, even after they saw all scenarios and made all of their redistribution decisions. When I ask them to guess which of the scenarios is real, 46% respond that they do not know. Among the others, only 16% guess correctly. Thus, the recognition rate is only slightly higher than what would be expected under random guessing (12.5%). Moreover, the experimental results are robust to excluding respondents who recognize the real scenario (see Appendix B.2).

[9] Methodological work has explored whether decisions elicited via a contingent response method or strategy method differ systematically from choices elicited via a direct-response method. In their review, Brandts and Charness (2011) conclude that most studies do not document such a difference. Moreover, none of the studies reviewed failed to replicate a treatment effect found with a contingent response method with the direct-response method.

Table 1 Overview of effort scenarios, experimental conditions, and studies

(A) Effort scenarios (presented in random order)

Effort share of worker B: e	(1) **0%**	(2) **10%**	(3) **30%**	(4) **50%**	(5) **70%**	(6) **90%**	(7) **100%**
Effort of worker A	50	45	35	25	15	5	0
Effort of worker B	0	5	15	25	35	45	50
Payment of worker A	\$25.00	\$22.50	\$17.50	\$12.50	\$7.50	\$2.50	\$0.00
(Share)	(100%)	(98%)	(92%)	(83%)	(68%)	(36%)	(0%)
Payment of worker B	\$0.00	\$0.50	\$1.50	\$2.50	\$3.50	\$4.50	\$5.00
(Share)	(0%)	(2%)	(8%)	(17%)	(32%)	(64%)	(100%)

Contingent response method: Each spectator faces eight effort scenarios. The seven scenarios above are hypothetical. An eighth effort scenario (not shown) is real. Spectators do not know which scenario is real and have to take each of their decisions seriously.

(B) Experimental conditions (between-subject)

	Control condition		***Treatment condition***	
Worker	**A**	**B**	**A**	**B**
Constant across conditions				
Realized π	\$0.50	\$0.10	\$0.50	\$0.10
Effort choices	*Depends on effort scenario*			
Payment	*Results from effort scenario and realized* π			
Varies across conditions				
Expected π	\$0.50 or \$0.10 each with 50%	\$0.50 or \$0.10 each with 50%	\$0.50	\$0.10

(C) Additional experimental conditions and studies (for later reference)

Study	**Section**	**Description**
Main study	3, 4	Varies whether endogeneity of choices (dis)advantages workers.
Conditions run in parallel to main study		
"Equal rates" conditions	4	Replicate main study, but workers receive same piece-rate.
Attention condition	5.2	Shifts attention towards endogeneity of choices.
"Equal rates" attention cond.	5.2	"Equal rates" version of the attention condition.
Additional experiments		
Disappointment study	4	Explores motive to compensate workers for disappointment.
Counterfactual study	5.3, 6	Reveals what would have happened in equal circumstances.
Vignette study	7	Explores merit judgments in real-world scenarios.

Notes: Panel A presents an overview of all effort scenarios. Panel B summarizes and compares the experimental conditions. Panel C lists all experimental conditions and studies that I present in this paper. Only the main study is introduced in this section. The details of all other experimental conditions and studies will be introduced in later sections.

Control: Both workers do not know their realized piece-rate while making their effort choices. They are aware that their piece-rates might either be \$0.50 or \$0.10 with equal chance. They learn about their realized piece-rate (\$0.50 for worker A and \$0.10 for worker B) only after completing their work.

Treatment: Both workers are informed about their realized piece-rate already before they decide how much effort they exert. Thus, worker A knows about his high rate of \$0.50 and worker B about his low rate of \$0.10 when they decide how many tasks they complete.

The experimental conditions vary whether the two workers in a pair optimize against identical or different piece-rate expectations. In the control condition, both workers face the same expected circumstances and respond to the same environment so that their effort choices are comparable. If one worker completes more tasks, this directly signals his higher taste for hard work. In the treatment condition, the workers face different circumstances and their effort choices are differentially affected by situational influence. The high piece-rate encourages worker A to work more, whereas the low piece-rate discourages worker B. Thus, if the advantaged worker A completes more tasks, this also reflects advantageous situational influence. By comparing spectators' redistributive behavior across treatment and control, I test whether and how the endogeneity of choices shapes merit judgments.

The contingent response method allows me to study merit judgments and their sensitivity to circumstances' effect on choices in seven different effort scenarios. Each scenario describes how much effort each worker exerts and how much money they initially earn. The scenarios are identical across the treatment and control conditions, but their interpretation changes. For instance, two workers who complete 25 tasks each (Scenario 4) show identical diligence in the control condition. However, in the treatment condition, working on 25 tasks for a \$0.50 piece-rate signals a much lower taste for hard work than working on 25 tasks for a \$0.10 piece-rate. As another example, if worker A completes 50 tasks and worker B does nothing (Scenario 7), worker A clearly signals higher diligence in the control condition. The situation is less clear in the treatment condition because the effort choices can be partially attributed to unequal circumstances.

For actual choice meritocrats, the difference between the treatment and control conditions is irrelevant. Their merit judgments depend solely on workers' actual effort choices which are identical across both conditions. But comparable choice meritocrats who recognize that worker B is disadvantaged by the endogeneity of choices and would work harder for a high piece-rate should compensate him with a higher reward share.

Table 2 Comparison of the sample to the American Community Survey

Variable	ACS (2019)	Sample
Gender		
Female	51%	51%
Age		
18-34	30%	30%
35-54	32%	33%
55+	38%	37%
Household net income		
Below 50k	37%	40%
50k-100k	31%	34%
Above 100k	31%	27%
Education		
Bachelor's degree or more	31%	43%
Region		
Northeast	17%	21%
Midwest	21%	21%
South	38%	36%
West	24%	22%
Sample size	2,059,945	653

Notes: Column 1 presents data from the American Community Survey (ACS) 2019. Column 2 presents data from the representative online sample.

3.3 Experimental procedures

Workers I recruited 336 workers on Amazon Mechanical Turk in May and June 2020 to participate in the crowd-working job. On average, the workers complete 12 tasks and earn about $5.40, but both figures vary across experimental conditions. I form 100 pairs with 200 of those workers and use them to incentivize spectators' redistribution decisions.[10]

Spectators I recruit a sample of 653 participants in collaboration with Lucid, an online panel provider which is frequently used in social science research (Coppock and McClellan, 2019, Haaland et al., forthcoming). The sample excludes participants who do not complete the first seven redistribution decisions or speed through the experimental instructions (see Appendix A). The sampling plan and the exclusion criteria were pre-registered (see Appendix G). The participants are broadly representative of the US adult population in terms of gender, age, region, income, and education. Table 2 displays summary statistics from the sample and compares them to the data obtained from the American Community Survey 2019. The sample follows the characteristics of the American population closely, except perhaps for education: 43% of the sample

[10] I ran the main experimental conditions together with additional robustness and mechanism conditions with a total of 1,855 participants. The additional conditions will be introduced later. The workers were recruited jointly for all experimental conditions. Appendix A provides an overview. Workers who were not selected for the redistribution stage received their original performance-based payments.

possess an undergraduate degree, compared to about 31% of the US population. Respondents were randomly assigned to either the treatment ($n = 329$) or the control ($n = 324$) condition. Appendix Table A.3 shows that the covariates are balanced across experimental conditions.

The experiment was conducted online in June 2020. Most participants spent 10 to 30 minutes to complete the experiment (15% and 85% percentile), with a median response duration of 16 minutes. The experiment is structured as follows. First, the participants answer a series of demographic questions, which monitor the sampling process. Inattentive participants are screened out in an attention check. Detailed instructions on the workers' situation and the redistribution decisions follow. The experimental treatment-control variation is introduced only at the end of the instructions. This guarantees that the instructions about the workers' task and the redistribution decisions are understood and interpreted identically across conditions. Then, a quiz tests whether participants understand the key aspects of the experiment and corrects them if necessary. Subsequently, participants make their redistribution decisions. Each redistribution decision screen also contains a tabular summary of the workers' situation, including their expected and realized piece-rates, to ensure that this information is salient in the moment of decision-making. Finally, I ask a series of follow-up questions to collect additional demographic variables and probe for possible mechanisms. Respondents also explain in an open-text format which thoughts and considerations shaped the merit judgments they made.

3.4 Additional experiments

I run a series of additional conditions and experiments to explore the robustness of the results and shed light on their behavioral mechanisms. The details will be introduced in later sections. For later reference, Panel C of Table 1 provides an overview and brief description of all conditions and studies.

4 Main result

I start by studying spectators' merit judgments in the control treatment. Here, workers' effort choices are comparable because they are made in an identical environment: Both workers expect either a \$0.50 or \$0.10 piece-rate (each with 50%). Only after completing their work, worker A learns that he randomly receives the high piece-rate of \$0.50, whereas worker B learns that he earns \$0.10 per completed task. Do spectators compensate worker B for the bad luck of a low piece-rate? Figure 1 visualizes the average share of the total earnings that spectators assign to the disadvantaged worker. Panel A

displays the mean share, averaged across all seven scenarios, and Panel B presents the results in each of the seven effort scenarios. The results show that spectators indeed counterbalance the bad luck of a low piece-rate. They strongly redistribute money from worker A (high piece-rate) to worker B (low piece-rate). Averaged across scenarios, worker B receives 44.1% of the total earnings (red bar), which is much higher than the share he would receive without redistribution (31.9%, gray line). In fact, many participants reward worker B proportionally to his effort share. They implement the payment shares that would have occurred if both workers had earned an identical rate (Appendix Figure B.1). Thus, in the control condition where both workers react to the same environment, merit derives mostly from effort choices.[11]

This sets the stage for my main research question. Do spectators take circumstances' effect on effort choices into account? In the treatment condition, workers learn about their realized piece-rates already before they make their effort choice. Consequently, worker B is disadvantaged as he endogenously reacts to a discouragingly low piece-rate of \$0.10. By contrast, worker A is encouraged by a high piece-rate of \$0.50. I test whether spectators assign a higher reward share to worker B in the treatment than in the control condition to compensate him for this disadvantageous situational influence.

The results show that merit judgments are fully insensitive to circumstances' effect on choices. Figure 1 shows that the payment shares are indistinguishable between the treatment and the control condition. Worker B receives on average 43.6% of the total earnings in the treatment condition and 44.1% in the control condition (Panel A). Hence, spectators do not compensate worker B for the disadvantageous situational influence in the treatment condition. They even assign an (insignificant) 0.49 pp lower share to him ($p = 0.464$; see Table 3). Panel B shows that this conclusion holds for all seven scenarios. Whether worker A or B completes more tasks, or both work equally hard, spectators do not counterbalance the effect of external situational influence. None of the seven treatment-control comparisons detects a significant difference, nor does a highly powered joint F-test that tests the null hypothesis that treatment differences are zero in all seven effort scenarios ($p = 0.668$).[12]

This null result does not reflect a noisy estimate but rather constitutes a precisely estimated null finding. Averaged across scenarios, the 95% confidence interval of the

[11] Deviations from effort-proportional rewards indicate traces of libertarian and egalitarian redistributive behavior. For instance, in effort Scenario 4 where worker B contributes exactly half of the tasks, worker B receives a mean payment share of 40.5% rather than an equal 50.0% share. This is due to "libertarian" spectators who never redistribute and always accept the pre-existing reward share of 17% (see Figure B.1). By contrast, in effort Scenario 1 where worker B completes no task at all, he still receives an average reward share of 7.8%. This is due to "egalitarian" spectators who always implement equal shares irrespective of the workers' effort decisions (see Figure B.1).

[12] The F-test is derived from a regression of worker B's payment share r_{is} on a treatment dummy interacted with a dummy for each scenario s and scenario fixed effects. It tests the null hypotheses that the treatment effects are zero in all seven effort scenarios. Standard errors are clustered at the participant level.

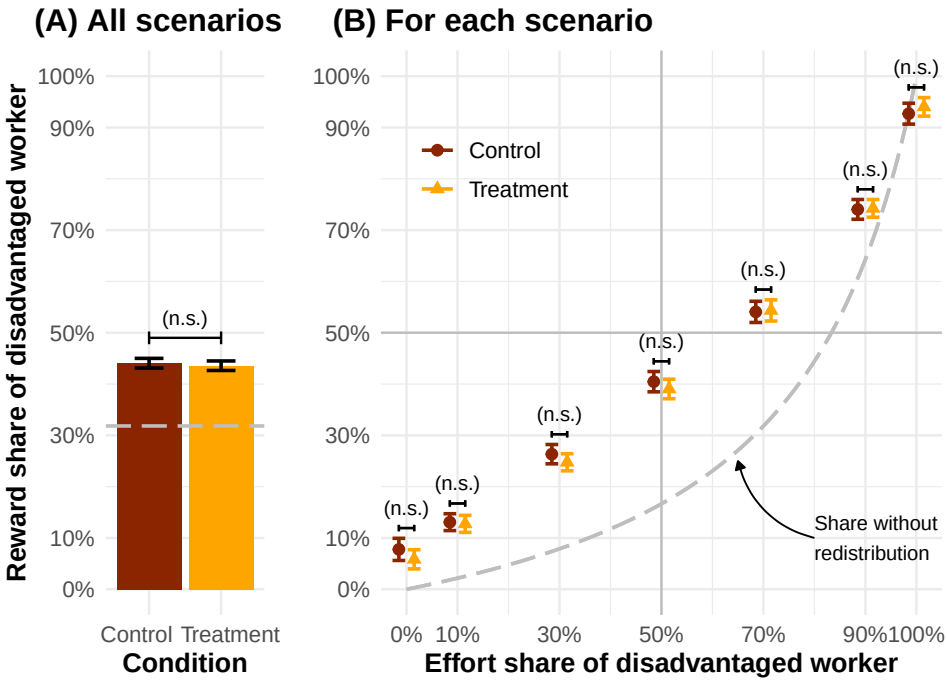

Figure 1 **Main experiment**: Mean reward share of disadvantaged worker (95% CI)

Notes: Results from the main study. Panel A displays the mean reward share assigned to the disadvantaged worker B in both experimental conditions, averaged across all seven effort scenarios, with 95% confidence intervals. Panel B plots the mean reward share in each effort scenario with 95% confidence intervals. The gray dashed line shows the default share, that is, which payment share worker B would receive if spectators do not redistribute. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, (n.s.) $p \geq 0.10$.

treatment effect ranges from -1.8 to 0.8 pp. This means that I can reject even tiny effect sizes with high statistical confidence, namely that workers who are disadvantaged by circumstances' effect on choices receive a compensation of more than 0.8 pp of the total payment. The results thus provide strong evidence for the absence of a meaningful effect.[13]

An average null effect might still conceal meaningful treatment effects for parts of the population. I therefore test for heterogeneous treatment effects. In the first step, I test for heterogeneity alongside six pre-registered covariates: gender, education, party affiliation, income, empathy, and internal locus of control. I assess empathy with four survey questions that measure perspective-taking and empathetic concern adopted from Davis (1983) and locus of control with a streamlined four-item scale developed in Kovaleva (2012). An internal locus of control measures whether a person attributes successes and failures to his or her own action and abilities instead of attributing them to luck, fate, or the actions of others. None of these variables significantly moderates the treatment effect (see Table B.2).[14] In the second step, I apply the model-free approach of

[13]Precisely estimated null results are very informative from a Bayesian learning perspective – often even more informative than rejections of a null hypothesis (Abadie, 2020).

[14]Moreover, none of the variables is significantly associated with merit judgments in the baseline control condition.

Table 3 Treatment effects on average reward share of disadvantaged worker

	Mean reward share of disadvantaged worker (in %)				
	Main	Robust: No quiz mistakes	Robust: Decisions 1-3	Robust: High duration	Robust: With controls
	(1)	(2)	(3)	(4)	(5)
Treatment	−0.493	−1.002	−0.135	0.160	−0.353
	(0.673)	(0.827)	(1.335)	(0.785)	(0.684)
Constant	44.068***	44.792***	43.652***	43.479***	47.264***
	(0.480)	(0.573)	(0.915)	(0.553)	(4.569)
Controls	–	–	–	–	✓
Observations	653	395	653	471	634
R^2	0.001	0.004	0.000	0.000	0.004

Notes: Results from the main study, ordinary least squares (OLS) regressions, robust standard errors in parentheses. The outcome variable is the reward share (in %) a spectator assigns to the disadvantaged worker B, averaged across all seven effort scenarios. The independent variable is a treatment indicator. Column 1 presents the main specification. Columns 2-5 present different robustness specifications: Column (2) excludes respondents who initially answer at least one quiz question incorrectly, Column (3) considers only the first three decisions of each participant, Column (4) excludes the 25% respondents with the lowest response duration, and Column (5) includes controls (indicators for female gender, college degree, and being Republican, as well as log income, and age). *** $p < 01$, ** $p < 0.05$, * $p < 0.10$.

Ding et al. (2016) that tests whether *any* significant treatment heterogeneity exists. The method relies on randomization inference and basically tests whether the treatment distribution of the outcome variable is identical to the control distribution shifted by the average treatment effect. No significant heterogeneity in treatment effects is detected ($p = 0.446$), which corroborates my main result.

Result 1: Individual merit judgments do not factor in circumstances' effect on choices. People reward others for their effort, even if effort choices are shaped by external circumstances.

Robustness

I replicate the results in multiple robustness checks. In the first set of robustness tests, I ensure that the findings are not driven by a misunderstanding of the instructions, survey-taking fatigue, or inattentive participants – all of which would increase survey noise and thus could potentially conceal treatment effects. In Column 2 of Table 3, I exclude participants who initially answer one of the control questions incorrectly which could indicate a lack of understanding. In Column 3, I restrict the analysis to the first three redistribution decisions each participant makes, which would arguably be less affected by survey fatigue. In Column 4, I exclude the 25% of participants with the lowest response duration to drop participants who might "speed through" the survey.

All three specifications replicate the main results. Moreover, I obtain virtually identical results if I control for respondents' demographic background (Column 5).

Second, one might be concerned that the direct effect of the piece-rates on earnings is too salient and crowds out attention to circumstances' effect on choices. For example, a disadvantaged worker who completes 15 tasks and earns only \$1.50 would have earned \$7.50 with a high piece-rate. Spectators might primarily think about this difference and thereby overlook that the worker would also have worked much harder (e.g., complete 35 tasks for a payment of \$17.50). However, evidence from two additional experimental conditions that I ran in parallel to the main study does not support this explanation ("equal rates" conditions, $n = 661$, Appendix B.3 provides further details). The two conditions keep the realized piece-rate of both workers constant. In the control "equal rates" condition – analogously to the main experiment – both workers have identical expectations about their piece-rate (\$0.10 or \$0.50 with an equal chance). In the treatment "equal rates" condition, worker A expects to earn either \$0.50 or \$0.90, whereas worker B expects to earn only \$0.10 or \$0.50. Thus, worker A is advantaged by situational influence and encouraged to work hard, whereas worker B is disadvantaged and discouraged from working hard. However, in both conditions, chance determines that both workers earn the same rate of \$0.50, so that their initial earnings are fully proportional to their effort. Consequently, there is no direct piece-rate effect on payments that could distract spectators. Still, this independent robustness experiment fully replicates the main results. I detect no significant difference in merit judgments across the two conditions. Again, the null result is obtained with high precision (Table B.4).

A third potential concern is that a compensation for disappointment confounds the null effect. Worker B receives bad news upon learning that he only earns a low piece-rate, and the timing of bad news could matter. In the control condition, worker B receives this information only after he stopped working which could lead to larger disappointment. If spectators share this concern, they might want to assign a higher payment share to worker B in the *control* condition to compensate him for the higher disappointment. Any such effect would run opposite to the main treatment effect and could therefore conceal its existence if, by chance, the two effects offset each other in all seven effort scenarios. To be on the safe side, I design an additional experiment that rules out this confounding channel (disappointment study, $n = 606$, run in February 2021 with a US convenience sample, see Appendix B.4 for further details). I replicate the main design with one crucial exception: Workers do not make a choice. Instead, all workers have to complete exactly ten tasks. Since no choice is involved, choices are not endogenous, situational influence on effort choices does not exist, and there is no reason to compensate for it. However, the motive to compensate for the timing of bad news is still present. If it matters, spectators should compensate worker B with a higher payment share in the control condition. The results reveal a negligible and insignificant

difference that could not even conceal a minor treatment effect (Table B.5).

Lastly, I corroborate my main findings by analyzing the open-text responses in which the participants of the main study explain how and why they made their merit judgments. The open-text data are rich in detail and capture the thoughts that were on participants' minds while they were making their merit judgments. For the analysis, each response is manually classified into different fairness arguments (see Appendix B.5). Hardly any participant in the treatment condition (1%) mentions that they considered that the workers' choices were strongly shaped by circumstances. By contrast, most participants (59%) argue that workers' effort choice should determine the final payments. Moreover, the explanations that respondents offer do not differ significantly across the treatment and control condition (see Table B.7). The open-text data thus replicate the main findings. Further, reassuringly, virtually none of the open-text responses mention any motive, thought, or consideration that could potentially confound the treatment effect.[15,16]

5 Mechanism

This section investigates why individuals' merit judgments are insensitive to circumstances' effect on effort choices. The conceptual framework of Section 2 suggests two explanations. On the one hand, circumstances' effect on choices could simply be irrelevant for merit views. Spectators' fairness preferences might hold that merit should be solely grounded on actual effort choices ("actual choice meritocratism"). On the other hand, spectators might actually prefer to correct for circumstances' effect on choices ("comparable choice meritocratism"), but they struggle to do so because they fail to infer what would have happened in identical, comparable circumstances. Here, I explore three behavioral obstacles that could impair spectators' inference: the fundamental attribution error, a lack of attention, and the uncertainty of the counterfactual.[17]

[15] 10% of the respondents refer to the idea to equalize payments ("egalitarians"). 13% refer to the argument that both workers deserve their initial earnings because they accepted the conditions of the crowd-working job ("libertarians"). These motives are orthogonal to the experimental variation, but account for the finer features of the observed merit judgments (see Section 6 and Footnote 11). Only 22% of the responses cannot be classified, mostly because the responses are too short or vague. This fraction is small in light of the inherent noise in open-text data. Table B.8 shows that the fairness motives that spectators mention in their open-text responses are strongly correlated with their merit judgments.

[16] One example of a motive that could confound the treatment comparison is the motive to compensate for disappointment, which I discussed and ruled out above. An additional example is that the spectators attempt to draw inferences about the workers' life situations outside the experiment, for example, their opportunity costs. I discuss in Appendix B.6 why such considerations are not consistent with the results. More importantly, virtually none of the open-text responses mention this or a similar line of thought.

[17] Cappelen et al. (2021, 2019) study fairness views in an uncertain environment but their mechanisms can only play a negligible role in my setting (see Appendix C.4 for a discussion).

5.1 Fundamental attribution error

Spectators might overly attribute choices to the decision-maker and underestimate the role of circumstances, that is, that workers' effort strongly react to the piece-rate workers earn. Such an inferential error would be in line with the so-called fundamental attribution error, namely the notion that individuals underestimate situational influences on human decisions (Ross, 1977). If spectators underestimate the effect of circumstances on workers' choices, they have little reason to correct for it. To shed light on this mechanism, the main study elicits participants' beliefs about how workers' effort choices react to the piece-rate. Spectators learn that workers complete on average five tasks for a \$0.10 piece-rate and estimate how many tasks workers complete on average for a \$0.50 piece-rate. Their responses are incentivized: One out of ten participants earns a \$5 Amazon gift card if their response is at most one task away from the true value.

The findings do not support that a fundamental attribution error explains why individuals do not factor in circumstances' effect on choices. On average, participants believe that workers complete 3.46 times as many tasks for a rate of \$0.50 than for a rate of \$0.10. Thus, the perceived incentive effect is even slightly larger (though not significantly so) than the observed effect of 3.33 ($p = 0.749$, t-test, Figure C.1).

5.2 Attention

Spectators could be unaware of circumstances' effect on effort choices while making their merit judgments. Once asked explicitly about it, participants acknowledge that situational influence exists, but it might still escape their attention while they make their merit judgments. Attention (or a lack thereof) is a powerful explanation of behavior in many other domains (e.g., Andre et al., forthcoming; Chetty et al., 2009; Gabaix, 2019; Taubinsky and Rees-Jones, 2018). To test for this mechanism, I ran an additional experimental condition in parallel to the main study that draws participants' attention to the endogeneity of effort choices just before their merit judgments ($n = 274$).[18]

> **Attention**: I explicitly inform spectators that "the piece-rates strongly influence the number of tasks a worker completes." Spectators learn how large this incentive effect is on average and read two typical comments by workers that explain why this is the case. For example, the comment of a typical disadvantaged worker with a \$0.10 rate is: "For the amount of time that goes into these tasks, the compensation is simply just not sufficient." Participants have to spend at least 20 seconds on this

[18] The study protocol closely follows the main experiment. As before, the sample broadly represents the US population, and treatment assignment is balanced across covariates (see Appendix A).

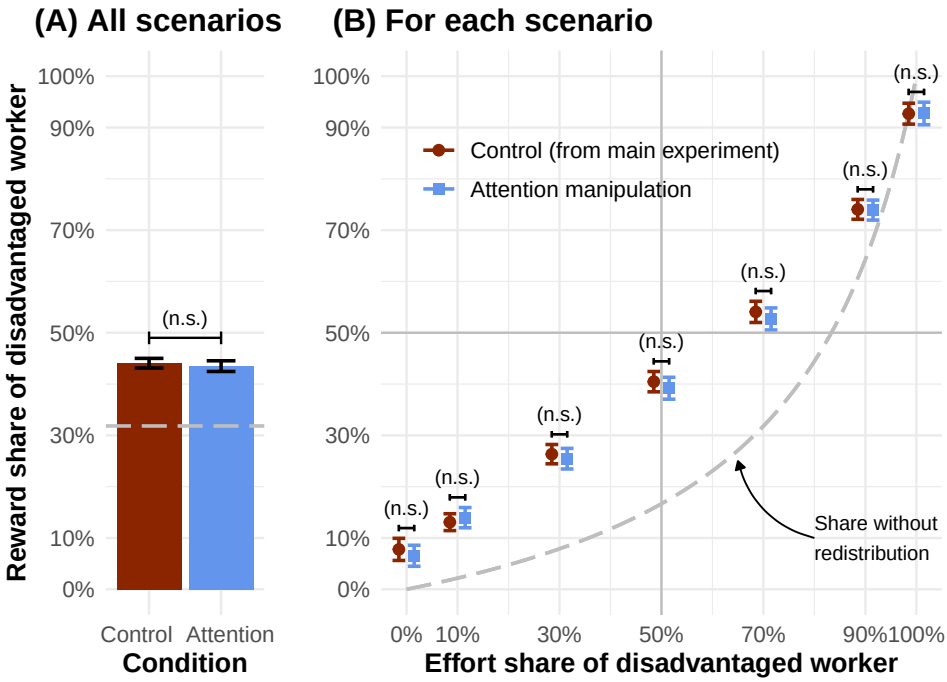

Figure 2 Attention manipulation: Mean reward share of disadv. worker (95% CI)

Notes: Results from the attention condition and the control condition of the main study. Panel A displays the mean reward share assigned to the disadvantaged worker B in both experimental conditions, averaged across all seven effort scenarios, with 95% confidence intervals. Panel B plots the mean reward share in each effort scenario with 95% confidence intervals. The gray dashed line shows the default share, that is, which payment share worker B would receive if spectators do not redistribute. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, (n.s.) $p \geq 0.10$.

information page, whose key message is repeated on the next page and tested for in the subsequent quiz.

Combining a qualitative statement, quantitative information, and workers' first-hand comments on their own experiences ensures that situational influence is salient to spectators while making their merit judgments. If a lack of attention to situational influence explains the main result, spectators should compensate the disadvantaged worker with a higher reward share in the attention condition compared to the baseline control condition.

However, participants who are informed about and focused on situational influence still do not compensate the disadvantaged workers. Figure 2 visualizes this result (following the format of Figure 1). As before, the null effect is precisely estimated and present in each of the seven effort scenarios (Panel B). Aggregated across scenarios, the mean payment share of worker B is 43.5% in the attention condition versus 44.1% in the control condition (Panel A). The 95% interval of their difference allows me to rule out even tiny treatment effects of 0.8 pp (see also Table C.1.A).[19] I also find virtually

[19]The results are robust to excluding potentially inattentive responses (misunderstanding of the instructions, survey-taking fatigue, "speeders"; see Appendix B.2). I also replicate the results in an analo-

no difference between the attention condition and the treatment condition of the main experiment (mean payment share: 43.6%, see Table C.1.B).[20] Hence, a lack of attention to the endogeneity of effort choices also does not explain the results.

5.3 Uncertainty of the counterfactual

Compensating worker B for the disadvantageous situational influence he is exposed to does not only require an understanding and an awareness of the average piece-rate effect. It also raises the question of what the two specific workers to whom a spectator has been assigned would have done in identical circumstances. How many tasks would worker B have completed had he also earned a high piece-rate of \$0.50? Such a counterfactual benchmark would underlie the reward decision of a comparable choice meritocrat, who believes that external situational influences cannot justify merit and hence would want to correct for it.[21] However, this counterfactual is unknown and uncertain even for spectators who accurately anticipate the average piece-rate effect. Recent research shows that people struggle with complex decisions in uncertain and contingent environments, rendering this a promising explanation for why spectators' merit judgments are insensitive to the endogeneity of choices (Esponda and Vespa, 2019; Martínez-Marquina et al., 2019). Spectators might abstain from any conjecture and base their merit judgments on what they know with certainty: observed effort levels.[22]

I devise a new mechanism experiment in which some spectators are explicitly informed about worker B's counterfactual effort choice, thereby removing any uncertainty about the counterfactual state (counterfactual study, $n = 945$).[23] For this purpose, I recruit new workers and elicit their effort choice for both the high and the low piece-rate. Workers commit to how many tasks they would complete for both piece-rates, are then randomly assigned to one piece-rate, and subsequently have to follow-up on their commitment. Importantly, this technique measures worker's counterfactual effort choice in an incentivized way. Thus, I know how many tasks the workers (would) complete for

gous comparison of the "equal rates" control condition with an additional "equal rates" attention condition ($n = 267$, see Panel C of Table C.1 and Appendix C.2).

[20] In the main text, I compare the attention condition with the control condition because this increases my chances to find an effect and, thereby, renders the null result even stronger.

[21] As discussed in Section 2, this benchmark is not unique. For instance, a comparable choice meritocrat might also ask what both workers would have done for a low piece-rate of \$0.10 or in another common piece-rate environment.

[22] Here, the line between cognition-based and preference-based explanations becomes blurred. Spectators might discount the uncertain counterfactual because doing so is cognitively less demanding or because they have a preference to base their merit judgments on verifiable evidence rather than mere conjectures.

[23] I ran this experiment in January 2021. The study protocol closely follows the main experiment. As before, the sample broadly represents the US population, and treatment assignment is balanced across covariates (see Appendix A). The results are robust to excluding potentially inattentive responses (misunderstanding of the instructions, survey-taking fatigue, "speeders"; see Appendix B.2).

both piece-rates. Spectators are informed about this procedure.

As before, spectators make merit judgments in eight scenarios of which the first seven are hypothetical (contingent response method). Spectators do not know which of the eight scenarios is real so that all of their decisions are probabilistically incentivized. The first three scenarios are taken from the main experiment and are presented in random order. Here, the advantaged worker A completes more tasks than the disadvantaged worker B, that is, 50 to 0 tasks ($e = 0\%$), 45 to 5 tasks ($e = 10\%$), or 35 to 15 tasks ($e = 30\%$).[24] The next four scenarios are randomly generated and will be used in Section 6.

Spectators are randomized into one of three experimental conditions. The conditions vary whether and what spectators learn about what the disadvantaged worker would have done in the advantaged environment. Table 4 provides an overview of all effort scenarios and experimental conditions.

No information (short: None): No information about worker B's counterfactual effort choice is provided. The condition thus replicates the main treatment condition and serves as a baseline condition in this experiment.

Low counterfactual (short: Low): Spectators are informed about worker B's counterfactual effort choice for a high piece-rate. In the "low counterfactual" condition, worker B would not change his effort provision and thus would not exert more effort for a higher piece-rate. This also means that worker B's effort choice is not shaped by his circumstances.

High counterfactual (short: High): This condition provides information about worker B's counterfactual effort choice, too. Here, however, worker B would complete as many tasks as worker A for a high piece-rate. Situational influence thus exists and strongly affects worker B's choice. Workers A and B (would) make the same choices in the advantaged environment; hence, this information also implies that they share the same taste for hard work.

Figure 3 presents the results, using the same format as earlier figures (see also Table C.2). First, it reveals that the average reward for worker B is very similar in the "no information" condition and the "low counterfactual" condition.[25] Thus, in the baseline condition with unknown counterfactual, spectators reward worker B as if they knew that his counterfactual effort choice would be no different. This suggests that spectators in the baseline condition base their merit judgments on the assumption that choices have

[24] In the other scenarios of the main experiment, the disadvantaged worker completes the same or a larger number of tasks than the advantaged worker. These scenarios are not compatible with the "high counterfactual" condition and therefore not included.

[25] If at all, spectators are even slightly more generous toward worker B in the "low counterfactual" condition. This difference is significant in the scenario where worker B has an effort share of 30%.

Table 4 Experimental conditions in the counterfactual study

	(1)	(2)	(3)	(4)-(7)
	Actual effort share of worker B			
Effort scenario	0%	10%	30%	Random*
	Counterfactual effort share of worker B, by experimental condition			
No information	–	–	–	–
Low counterfactual	0%	10%	30%	Random*
High counterfactual	50%	50%	50%	Random*

*Effort choices: E_A is uniformly randomly drawn from the integers between 0 and 50. E_B ranges from 0 to 25. Counterfactual effort choice of worker B: C_B equals $E_B + X$ where X ranges from 0 to 25.
Notes: This table presents an overview of all seven effort scenarios and the experimental conditions in the counterfactual study. A contingent response method is used: Each spectator faces eight effort scenarios. The seven scenarios above are hypothetical. An eighth effort scenario (not shown) is real. Spectators do not know which scenario is real and have to take each of their decisions seriously. Scenarios (1) to (3) provide the reduced-form evidence analyzed in this section. They are presented in random order to spectators. Data from scenarios (4) to (7) are used in Section 6 to structurally estimate a model of merit views.

not been shaped by circumstances. They focus on observable effort choices, the only reliable evidence they have.

Second, a comparison of the "low counterfactual" and "high counterfactual" conditions exposes that, once known, the counterfactual choice of worker B matters substantially for spectators' merit judgments. Spectators distribute on average a 9.7 pp higher payment share to worker B when they know that he would have worked as hard as worker A, had he earned a high piece-rate (Panel A of Figure 3). The effect occurs in all three effort scenarios (Panel B). It is driven by a subset of spectators who distribute the payment equally once they know that both workers would have worked equally hard for a high piece-rate. About 32% of spectators implement equality in the "high counterfactual" condition, whereas only 7% do so in the "low counterfactual" and "no information" condition respectively (see also Figure C.2).[26]

In short, some spectators care about the counterfactual effort choice of worker B. Once known, their merit judgments take circumstances' effect on choices into account and compensate workers who are disadvantaged by external circumstances. This effect is driven by about one-quarter of participants, whereas the remaining participants do not adjust their reward behavior to the counterfactual information. However, when no information on the counterfactual choice is provided, *all* participants do not factor in the effect of situational influence. This suggests that, in the presence of an unknown, uncertain counterfactual, spectators base their merit judgments on the only clear and

[26]Could the large effect of the "high counterfactual" treatment be partially driven by an experimenter demand effect? The null result in the attention experiment renders such an explanation unlikely. Here, the scope for demand effects seems to be higher. Respondents receive two pages of information which strongly emphasize the endogeneity of choices. Nonetheless, I do not find a treatment effect, suggesting that demand effects are not an empirically important factor in the experimental context of this study.

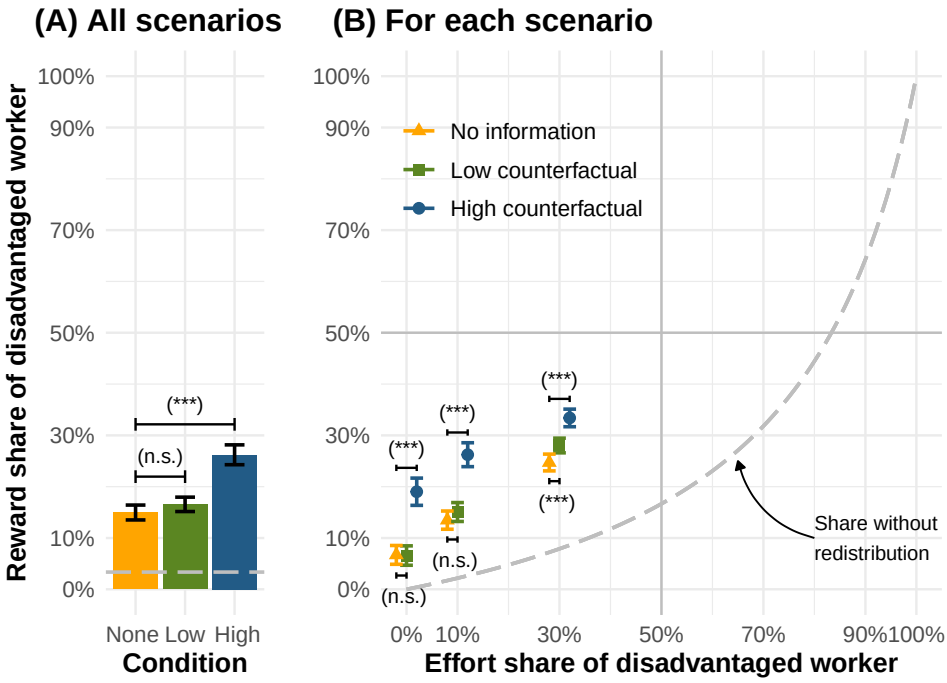

Figure 3 **Counterfactual study**: Mean reward share of disadv. worker (95% CI)

Notes: Results from the counterfactual study, decisions 1-3. Panel A displays the mean reward share assigned to the disadvantaged worker B in each experimental condition, averaged across all three effort scenarios, with 95% confidence intervals. Panel B plots the mean reward share in each effort scenario with 95% confidence intervals. The gray dashed line shows the default share, that is, which payment share worker B would receive if spectators do not redistribute. I test for differences between the "High counterfactual" and the "No information" condition (upper test) and between the "Low counterfactual" and the "No information" condition (lower test). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, (n.s.) $p \geq 0.10$.

reliable evidence they have, namely observed effort choices. Disadvantaged workers thus face a "burden of the doubt": When their counterfactual choices cannot be verified, only their observed choices count, and these are shaped by unequal circumstances.

Result 2: Once the counterfactual is revealed, spectators on average compensate workers for disadvantageous situational influence. The uncertainty of the counterfactual state can thus explain why merit judgments do not factor in circumstances' effect on effort choices.

In light of the framework discussed in Section 2, this means that comparable choice meritocrats exist but do not apply their merit view when the counterfactual effort choice under equal circumstances is uncertain and unknown. At the same time, the results suggest that only about one-quarter of individuals are comparable choice meritocrats. Spectators, thus, seem to endorse fundamentally different fairness views. The next section organizes these reduced-form findings in a structured framework and sheds light on the heterogeneity of fairness views.

6 A structural model of heterogeneous fairness views

In all experiments, the distribution of merit judgments exhibits discrete spikes that coincide with distinct fairness views (see Figures B.1 and C.2). Some respondents reward both workers equally once they know that the workers would have made the same choice in a counterfactual, identical environment, while other spectators disregard this information and reward workers according to their actual choices. Some spectators completely abstain from redistribution, while yet another group always implements full equality. These decisions correspond to different fairness types. Here, I present a simple behavioral structural model that estimates the prevalence of these fairness views in the US population (DellaVigna, 2018). Two of these types – actual choice meritocrats and comparable choice meritocrats – have already been introduced in Section 2. Two additional ones, libertarians and egalitarians, will be introduced in this section.

6.1 Model and estimation

In line with Almås et al. (2020), I assume that spectator i selects a reward share r_i for the disadvantaged worker to maximize the utility function

$$U(r_i) = -\left[r_i - m_i(e,s)\right]^2$$

where $m_i(e,s)$ denotes i's merit view, that is, her view about which reward the disadvantaged worker deserves for providing the effort share e in situation s. Thus, the spectator wants to implement the reward share r_i that she thinks is merited by worker B: $r_i^* = m_i(e,s)$. However, spectators' decisions are noisy and deviate from their merit views by a normally distributed response error $\varepsilon_{is} \sim_{iid} N(0,\sigma^2)$.

$$\hat{r}_i^* = m_i(e,s) + \varepsilon_{is}$$

The model assumes that the population is separated into four distinct fairness types. *Actual choice meritocrats* hold that the disadvantaged worker B deserves a payment share equal to his effort share, irrespective of whether effort choices are endogenous to external circumstances. For instance, he deserves 25% of the payment if he completed 25% of the tasks, and he deserves 75% of the payment if he completed 75% of the tasks. In short,

$$m_i^{\text{Actual}}(e,s) = e$$

By contrast, *comparable choice meritocrats* compensate for circumstances' effect on choices. They think that the disadvantaged worker B deserves a payment share equal to the counterfactual effort share c that he would have provided had he been in the same advantaged circumstances as worker A. This requires an inference about the counter-

factual effort shares under equally advantaged, comparable circumstances, $\hat{E}_i c(e,s)$. When the counterfactual $c(e,s)$ is known and revealed to the spectators, we have $\hat{E}_i c(e,s) = c(e,s)$. When the counterfactual is uncertain, I assume that comparable choice meritocrats accurately anticipate the expected counterfactual effort share $Ec(e,s)$ but "discount" it and put more weight on the observed effort share e. These assumptions are in line with the reduced-form results.

$$m_i^{\text{Comparable}}(e,s) = \begin{cases} \rho Ec(e,s) + (1-\rho)e & \text{if counterfactual is uncertain} \\ c(e,s) & \text{if counterfactual is known} \end{cases}$$

The discounting of the expected counterfactual could be interpreted as a probabilistic failure to engage in counterfactual reasoning (with probability $1-\rho$) or as a preference to base merit judgments on verifiable information (with weighting factor $1-\rho$).[27]

Egalitarians hold that the workers always deserve equal payment shares and, thus, implement equality:

$$m_i^{\text{Egalitarian}}(e,s) = 50\%$$

Libertarians regard any pre-existing earning share p as legitimate and, thus, fully accept any pre-existing inequality:

$$m_i^{\text{Libertarian}}(e,s) = p(e,s)$$

I use the merit judgments made in Scenarios 4 to 7 of the counterfactual study to estimate the model. These scenarios randomly vary the effort share of both workers and, in the counterfactual conditions, the counterfactual effort share of worker B (see Table 4, Scenarios 4-7). They cover a rich variety of cases and are hence ideally suited to estimate how common the different fairness views are. Moreover, this procedure allows me to explore the replicability of my reduced-form findings, which do not depend on data from Scenarios 4 to 7. I estimate six parameters, namely the population shares of each preference type together with the discount parameter ρ and the standard deviation of the response error σ.

The parameters are identified by the within-subject variation in effort scenarios and the between-subject variation in experimental conditions. For example, the share of egalitarians is reflected in the number of individuals who equalize payments in all effort scenarios. Likewise, the share of comparable choice meritocrats becomes evident in the conditions where the counterfactual is known. Here, the share influences how many respondents are willing to redistribute payments according to counterfactual effort shares. In turn, the discount parameter ρ can be identified in the condition where

[27] I calibrate $Ec(e,s)$ to the worker data. Appendix D.2 shows that the results of the model are insensitive to two different calibration approaches.

the counterfactual is uncertain and the merit judgments of comparable choice meritocrats crucially hinge on the discounting of the expected counterfactual.

I employ a constrained maximum likelihood procedure. Appendix D presents the technical details of the estimation procedure and shows that the results are robust to a series of sensitivity checks, such as a specification with trembling-hand response error or an exclusion of participants who initially failed a control question. I also confirm the numerical stability of the maximum likelihood estimator in Monte Carlo experiments.

6.2 Results

The model estimates that 37% of the population are actual choice meritocrats, while 26% are comparable choice meritocrats. Libertarians and egalitarians have a population share of 23% and 14%, respectively (see Table 5). Thus, a large majority of participants, namely 63%, endorse a meritocratic fairness ideal.[28] However, many meritocrats are actual choice meritocrats (about 60% of all meritocrats). For them, it is irrelevant that workers' choices are shaped by unequal circumstances, even if they know what would have happened under equal circumstances. Only about one-quarter of individuals are comparable choice meritocrats and prefer to take the endogeneity of choices into account. But I estimate a ρ of 0.00 which means that even they fully discount counterfactual choices if the counterfactual is uncertain.[29,30]

The estimated model mirrors and replicates the reduced-form results. For instance, a ρ of 0.00 explains why merit judgments are entirely insensitive to circumstances' effect on choices in the conditions where the counterfactual is unknown. Likewise, the model estimates a share of comparable choice meritocrats of 26% which aligns with the observation that one-quarter of respondents is responsible for the treatment effect in the counterfactual experiment (see Section 5.3). To give another example, the estimated libertarian share of 23% is consistent with the fact that, depending on the effort scenario, 18% to 29% of respondents accept the pre-existing inequality (see Figures B.1 and C.2).

Next, I test whether the composition of fairness types or the uncertainty discount parameter ρ vary across different parts of the population. To answer this question, I

[28]The estimated share of meritocrats is much higher than in Almås et al. (2020) who classify 37.5% of the US population as meritocrats. In their setting, spectators receive only coarse, binary information about effort choices, namely which of two workers is more productive. Merit presumably plays an even larger role in my setting because the piece-rate task provides a clear and fine-grained measure of effort.

[29]The estimate for ρ is on the boundary. Standard inference in constrained maximum likelihood models can become unreliable if one of the parameters is on or near the boundary (Schoenberg, 1997). In Appendix D.1, I run simulation experiments to show that the inference is nevertheless reliable.

[30]Appendix D.3 discusses an extension of the model that allows for heterogeneity in ρ, that is, multiple sub-types of comparable choice meritocrats with different degrees of counterfactual discounting. However, the model does not detect such heterogeneity. Virtually all comparable choice meritocrats fully discount the counterfactual.

Table 5 Results of the structural estimation

	Estimate	95% confidence interval
Population shares		
Actual choice meritocrats	36.7%	[33.0% – 40.3%]
Comparable choice meritocrats	26.2%	[22.8% – 29.6%]
Libertarians	23.0%	[20.2% – 25.7%]
Egalitarians	14.2%	–
Counterfactual discount parameter		
ρ	0.00	[0.00 – 0.09]
Error term and sample		
σ *noise*	9.27	[9.06 – 9.49]
Respondents	945	
Decisions	3777	

Notes: Results from the counterfactual study, decisions 4-7, maximum likelihood estimation of the structural model of fairness views. The estimates indicate the population shares of different fairness views and the uncertainty discount parameter ρ. No confidence interval is reported for the share of egalitarians because their share is deduced from the other estimates. See Appendix D.1 for further details.

re-estimate the model and allow its parameters to vary across two separate groups of the population (see Appendix D.4). In separate analyses, I compare female versus male respondents, above-median income versus below-median income respondents, respondents with versus without a college degree, and Republicans versus Democrats. I detect only one significant difference across these groups: College-educated spectators are 7pp less likely to be libertarians. Instead, they are more likely to be classified as comparable choice meritocrats or egalitarians (but these differences do not reach significance). Given the lack of statistical significance, all other patterns require a cautious interpretation. For example, I find more actual choice meritocrats and less egalitarians among Republicans. Moreover, I estimate a ρ of 0 in each group, indicating that all groups fully discount uncertain counterfactual states (see Table D.2).

Taken together, the results show that people endorse fundamentally different fairness views. Crucially, even if the counterfactual choice were known (arguably a very rare situation in the real world), only about 26% of individuals would compensate for disadvantageous situational influence.

Result 3: A structural model of merit views classifies only 26% of individuals as comparable choice meritocrats who want to correct for circumstances' effect on choices. Replicating earlier results, the model also estimates that even comparable choice meritocrats discount circumstances' effect on choices when the counterfactual is uncertain.

7 Vignette study with real-world scenarios

The controlled set-up of the online experiment has many advantages. In particular, it measures merit judgments in situations with real consequences, and it allows for an exogenous variation of external situational influence. However, its stylized environment – two crowd-workers, working for a randomly assigned piece-rate, earning up to $25 – also comes at a cost: It differs from many real-life settings that characterize the debate about meritocracy.

In this section, I therefore explore whether merit judgments are also insensitive to the endogeneity of choices in three real-world scenarios. I report results from an additional vignette study ($n = 1{,}222$) which sheds light on the following three questions, chosen as common and important practical examples of merit judgments. First, revisiting the example of racial discrimination in the labor market discussed in Section 2, are minorities compensated for the detrimental choices they might make because they are discriminated? Second, is a person growing up with few opportunities and incentives to exert effort blamed for being idle? And, third, is an entrepreneur rewarded for taking the risk of founding a company if he inherited a fortune so generous that it made founding easy and substantially reduced any risk involved? The study was run in February 2021 in collaboration with the survey company Lucid. Respondents were recruited from the general US population.[31]

7.1 Vignettes

Each vignette describes a simple hypothetical scenario with two people that are exposed to unequal situational influences. The person disadvantaged by situational influence earns much less money due to the detrimental choice he makes. Below, I outline each vignette.[32]

> **Discrimination vignette**: A white and a black employee compete for a promotion. However, their boss is notorious for being racist and has never promoted employees of color before. The white employee decides to work hard to win the

[31] The study was conducted in two waves. Wave 1 was collected together with the disappointment study. Here, every respondent faced two randomly selected vignettes. Wave 2 was launched shortly thereafter, and respondents faced all vignettes in random order. I exclude respondents who speed through the survey and complete the vignettes with an average response time of less than one minute. The results are robust to both stricter and more lenient exclusion criteria (see Table E.1). Table A.2 shows that the sample does not fully match the characteristics of the general population. Among others, the sample contains more females, more older respondents, and more respondents with a low income. However, the results are robust to the use of survey weights that correct for these imbalances (see Appendix E).

[32] The full wording of the vignettes is presented in Appendix I. The vignette survey also contained a fourth vignette on criminal behavior which requires a tailored analysis and discussion and is not reported here for brevity (but see Appendix E).

promotion, the black person does not. In the end, the white employee is promoted and receives an attractive one-time bonus of $10,000.

Poverty vignette: In this vignette, the advantaged person grew up in a rich family, went to good schools, and was taught that "you can go as far as your hard work takes you." The disadvantaged person grew up in a poor family, went to poor-quality schools, and was always told that "the poor stay poor, and the rich get richer." Whereas the advantaged person always worked hard in his life and, as a consequence, earns $125,000 a year, the disadvantaged person never worked hard and earns only $25,000 a year.

Start-up vignette: The vignette portrays two passionate software developers who always dreamed of founding a software start-up. The advantaged person inherited a considerable fortune that provided him with enough money to found and fail several times without any risk of financial ruin. By contrast, the disadvantaged person would have struggled to gather enough money to launch even a first start-up and would have been broke if his first attempt had failed. The advantaged person decided to take the risk and founded his own software start-up. He earns $200,000 a year today. The disadvantaged person decided to work as a software developer for a local company. He earns $50,000 a year today.

Analogous to the main experiment, respondents can specify how much money each person deserves by hypothetically redistributing the income (bonus) between the two people. If their merit judgments are sensitive to circumstances' effect on choices, they should compensate the disadvantaged person for the adverse situational influence that shaped his choice. Redistribution toward the disadvantaged person could, however, also be explained by other fairness motives. In particular, respondents might assign more money to the disadvantaged person simply because they prefer a more equal outcome. Or they want to compensate the disadvantaged person for living in worse circumstances, for example, for not inheriting any money in the start-up vignette.

To identify the sensitivity of merit judgments to circumstances' effect on choices, I introduce a between-subject variation that is analogous to the counterfactual study of Section 5.3. Respondents are randomized into one of three treatments. The treatments vary whether and what spectators learn about what the disadvantaged person would have done in the advantaged environment.

Baseline: The vignettes describe only the actual decisions of both persons.

Low counterfactual: Each vignette states that the disadvantaged person would not have made a different choice if he had been in the advantaged situation. Hence, his choice was not shaped by his circumstances.

High counterfactual: Here, the disadvantaged person would have made the same choice as the advantaged person if he had been in the advantaged situation. Hence, his choice was strongly shaped by his circumstances.

7.2 Results

Table 6 summarizes the results. Once more, I find that merit judgments are insensitive to the endogeneity of effort choices. First, I observe only little redistribution toward the disadvantaged person in the baseline condition. For instance, in the discrimination vignette, only 42% of respondents assign a positive reward share to the discriminated black employee (Column 1, Panel A), and, on average, he receives only 14% of the total pay-off (Column 2, Panel B). Most respondents accept that he comes away empty-handed. His choice not to work hard legitimizes the highly unequal outcome. In the poverty vignette, 55% of respondents are willing to compensate the person who grew up in poverty, but he is still assigned only 24% of the total earnings (only 7 pp more than he would receive without redistribution).

Next, I study the difference in merit judgments between the baseline and the "low counterfactual" condition. In the baseline condition, situational influence is present (though uncertain), whereas it is verifiably absent in the "low counterfactual" condition. If, as in the main experiment, baseline merit judgments are insensitive to situational influence, they should be similar across the baseline and the "low counterfactual" condition. Indeed, the reward decisions are virtually identical in both conditions. Pooled across vignettes, only 0.4 pp more respondents redistribute money toward the disadvantaged person in baseline than in "low counterfactual" (Column 4, Panel A). Likewise, the average reward share of the disadvantaged person is only 1.5 pp higher in the baseline condition (Column 4, Panel B). Both effects are statistically insignificant.

In stark contrast, the "high counterfactual" condition increases the share of respondents who redistribute money toward the disadvantaged person by 12.6 pp and raises his mean reward share by 6.8 pp across vignettes. The results are mainly driven by the discrimination and the poverty vignette, whereas they are more muted in the start-up vignette. For instance, in the discrimination vignette, 23 pp more respondents are willing to assign a positive reward share to the black employee once they know that he would have worked equally hard had his boss given him a fair chance. Likewise, the fraction of respondents who compensate the disadvantaged person increases by 9 pp in the poverty vignette. Respondents thus only integrate circumstances' effect on choices in their merit judgments once the counterfactual is known but do not take it into consideration if the counterfactual is uncertain.

Taken together, the results suggest that merit judgments are insensitive to situational

Table 6 Merit judgments in the vignette study

(A) Share of respondents redistributing towards the disadvantaged worker

	Binary indicator for compensation			
	Discrimination	Poverty	Start-up	Pooled
	(1)	(2)	(3)	(4)
Low counterfactual	0.015	−0.001	−0.026	−0.004
	(0.041)	(0.041)	(0.040)	(0.029)
High counterfactual	0.230***	0.090**	0.059	0.126***
	(0.040)	(0.040)	(0.039)	(0.029)
Constant	0.424***	0.547***	0.630***	
	(0.028)	(0.028)	(0.028)	
Vignette FE	–	–	–	✓
Observations	889	887	888	2,664
R^2	0.044	0.008	0.005	0.587

(B) Mean reward share of disadvantaged person

	Reward share of disadv. person (in %)			
	Discrimination	Poverty	Start-up	Pooled
	(1)	(2)	(3)	(4)
Low counterfactual	0.133	−2.387*	−2.391	−1.539
	(1.658)	(1.197)	(1.413)	(1.085)
High counterfactual	13.590***	4.003***	2.867*	6.795***
	(1.797)	(1.277)	(1.463)	(1.177)
Constant	13.994***	24.208***	33.497***	
	(1.182)	(0.874)	(1.044)	
Initial reward share	0.00	17.00	20.00	
Vignette FE	–	–	–	✓
Observations	889	887	888	2,664
R^2	0.082	0.029	0.015	0.683

Notes: Results from the vignette study, OLS regressions, robust standards (Columns 1-3) and standard errors clustered at the respondent level (Column 4) in parentheses. The dependent variable in Panel A is a binary indicator for whether a respondent compensates the disadvantaged person by redistributing money toward him. The dependent variable in Panel B is the reward share assigned to the disadvantaged person. The independent variables are treatment dummies. Columns 1-3 report results from different vignettes, and Column 4 displays the pooled results. In each panel, p-values of the coefficients in Columns 1-3 are adjusted for multiple hypothesis, using the Benjamini-Hochberg adjustment. *** $p < 01$, ** $p < 0.05$, * $p < 0.10$.

influence not only in the controlled experimental setting but that the same phenomenon is to be expected in many important real-life domains of a meritocracy.

Result 4: Merit judgments do not factor in circumstances' effect on choices also in important real-world scenarios.

8 Conclusion

The idea of meritocracy has become central in Western politics, where it has shaped the public debate, the political and economic culture, and social reforms. Meritocracy promises that the family, neighborhood, and other circumstances into which one is born should not matter. This promise is popular and closely connects to the prominent ideas of equal opportunity and the American dream.

However, the findings from a series of experiments with about 4,000 US participants suggest that the prevailing notion of meritocratic fairness is *shallow*. Circumstances often shape which choices agents make, and people's merit judgments are insensitive to this effect. Thus, individuals hold others responsible for their choices even when these choices are strongly shaped by external circumstances. Once unequal opportunities led to unequally meritorious choices, these choices thereby "launder" the unequal circumstances and legitimize the resulting inequality.

Evidence on the mechanism behind this phenomenon suggests that it is likely to be a fundamental feature of merit judgments. About one-quarter of participants would – in principle – prefer to compensate agents for disadvantageous effects of circumstances on choice. Yet, they abstain from doing so unless they verifiably know what would have happened otherwise, on a level playing field. Such uncertainty about the counterfactual state is usually inevitable in the real world and will thus form a binding constraint for merit judgments. The results also show that many individuals do not factor in circumstances' effect on choices even when they are fully informed about the counterfactual.

These results refine our understanding of the popular notion of meritocratic fairness and have important implications for the debate about equal opportunity. First, in a shallow meritocracy, the disadvantaged can be doubly disadvantaged. When unequal circumstances impede accumulating merit because they discourage hard work or stifle ambitions, disadvantaged agents not only face adverse and discouraging circumstances, but they are also blamed and regarded as undeserving if they show less dedication and perseverance in these circumstances. Their choices and achievements are measured with the same yardstick as those of advantaged groups, even though their starting position is different. Second, affirmative action policies, which aim to correct for unequal opportunities that agents face in producing merit, undermine this popular notion of

meritocratic fairness. This could explain why they belong to the most controversial policy issues (Harrison et al., 2006). Third, for shallow meritocrats, predistributive and redistributive policies differ in a critical respect: Predistribution equates circumstances ex-ante. It thereby prevents that a differential effect of circumstances on choices occurs, and shallow meritocrats will endorse the accompanying increase in equal opportunities. By contrast, redistribution – even if targeted to compensate for unequal opportunities – only intervenes after unequal circumstances led to unequal choices. In so far as this clashes with the principle of responsibility for choices, it is likely to meet resistance among shallow meritocrats. A policymaker would therefore face wider support for predistributive rather than redistributive policies.

In light of these consequences, an important avenue for future research is to identify when and how unequal socioeconomic circumstances shape important life choices, such as working hard, taking risks, holding ambitious career aspirations, or observing the law (Altmejd et al., 2021; Bursztyn et al., 2017; Carrell et al., 2010; Glover et al., 2017). Such research will reveal the contexts in which shallow meritocracy matters most, and where merit judgments are susceptible to ignore sizable effects of circumstances on choices. Moreover, not only choices but also valued abilities such as cognitive skills are considered important determinants of merit. Future research is needed to explore whether people's evaluations of skills are similarly blind to the circumstances that foster or impede their development. Likewise, it would be fruitful to investigate how common the neglect of circumstances' effect on choices is outside the US, in countries with different cultures or welfare state regimes.

The pros and cons of meritocracy have been subject to a heated public debate (Frank, 2016; Greenfield, 2011; Markovits, 2019; Sandel, 2020; Wooldridge, 2021; Young, 1958). In view of this debate, it seems warranted to ask to what extent we should actually be concerned about meritocracy being shallow. This is a normative question open for debate, but I briefly sketch two possible perspectives. On the one hand, one could think of shallow meritocracy as a problematic flaw in merit judgments. It arguably appears inconsistent to acquit people from their circumstances but at the same time hold them responsible for the choices that these circumstances promote and produce. On the other hand, this behavior might constitute a second-best response to a world of limited information. After all, neither the ultimate cause of each decision nor the decisions that would have been made in counterfactual states of the world are known. Holding others responsible for their choices could be an adaptive, simple shortcut, a societal rule-of-thumb. It provides clear incentives to agents and clear guidance to spectators. Ultimately, shallow meritocracy and responsibility for one's choices may simply be a practical necessity of living together. Either way, those opposed to shallow meritocracy might have a strong argument for advancing equal opportunities: Equal opportunities also level circumstances' effect on choices, defusing the problem of shallow meritocracy.

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Appendices

Table of Contents

A Samples

Overview Table A.1 provides an overview of all spectator samples used in this study. It lists all experimental conditions and studies and describes when and how they were conducted.

Sample characteristics Table A.2 summarizes the demographic characteristics of each sample.

Exclusion criteria in online experiments Exclusion criteria are preregistered (see Appendix G). The samples do not contain the following responses:

1. Respondents who do not complete the first seven redistribution decisions.[33]
2. Respondents who spend less than 30 seconds on the instructions until the first treatment variation is introduced.
3. Duplicate respondents (very rare cases).

Balanced assignment of experimental conditions Table A.3, Table A.4, and Table A.5 show that the demographic covariates are balanced across experimental conditions in all studies. I test for balanced treatment assignment by regressing the demographic variables on a treatment indicator. Across all studies, the coefficient estimates are mostly small, indicating that the demographic covariates are balanced across treatments. For each study, I also test the joint null hypothesis that *all* treatment differences are zero. None of the highly-powered F-tests rejects this hypothesis. For the vignette study, the joint effect is marginally significant ($p = 0.083$), but the effect sizes are relatively minor.

[33] There is only one redistribution decision in the disappointment study. Here, I exclude all respondents who do not complete the study.

Table A.1 Overview of all samples

Sample	When	How	Population	Recruitment	n
Main study					
Main treatment and control condition	June 2020	Online experiment	US adults (targeted*)	Via survey company Lucid	653
Conditions run in parallel to main study					
"Equal rates" conditions	June 2020	Online experiment	US adults (targeted*)	Via survey company Lucid	661
Attention condition	June 2020	Online experiment	US adults (targeted*)	Via survey company Lucid	274
"Equal rates" attention condition	June 2020	Online experiment	US adults (targeted*)	Via survey company Lucid	267
Additional experiments					
Disappointment study	February 2021	Online experiment	US adults	Via survey company Lucid	606
Counterfactual study	January 2021	Online experiment	US adults (targeted*)	Via survey company Lucid	945
Vignette study	February 2021	Online survey	US adults	Via survey company Lucid	1,222**
				Total n	4,033

*The sampling process targeted a sample that represents the general population in terms of gender, age (3 groups), region (4 groups), income (3 groups), and education (2 groups). The counterfactual study did not target education.

**Wave 1 of the vignette study was attached to the disappointment study. 595 respondents of the disappointment study also participated in the vignette study. The total n does not double-count these respondents.

Notes: This table provides an overview of all spectator samples used in this study. It lists all experimental conditions and studies and describes when and how they were conducted.

Table A.2 Comparison of all samples to the American Community Survey (ACS)

Variable	ACS (2019)	Main study	Equal rates	Atten-tion	Attention equal rates	Disap-point-ment	Counter-factual	Vig-nettes
Gender								
Female	51%	51%	52%	52%	48%	63%	53%	61%
Age								
18-34	30%	30%	28%	32%	33%	11%	23%	15%
35-54	32%	33%	32%	32%	29%	30%	35%	33%
55+	38%	37%	41%	36%	38%	59%	42%	52%
Household net income								
Below 50k	37%	40%	43%	39%	44%	47%	39%	45%
50k-100k	31%	34%	32%	34%	33%	34%	32%	33%
Above 100k	31%	27%	26%	26%	23%	19%	30%	22%
Education								
Bachelor's degree or more	31%	43%	40%	38%	36%	48%	56%	47%
Region								
Northeast	17%	21%	16%	16%	16%	25%	17%	25%
Midwest	21%	21%	22%	18%	21%	25%	21%	23%
South	38%	36%	39%	44%	38%	35%	38%	36%
West	24%	22%	23%	23%	25%	15%	24%	16%
Sample size	2,059,945	653	661	274	267	606	945	1,222

Notes: Column 1 presents data from the American Community Survey (ACS) 2019. The other columns describe the different experimental samples.

Table A.3 Test for balanced treatment assignment – part 1

Main study (treatment vs. control)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	−0.001	0.150	0.754	0.000	−0.012	−0.022	0.031
	(0.039)	(1.339)	(4.488)	(0.039)	(0.032)	(0.038)	(0.032)
Constant	0.511***	47.116***	76.831***	0.426***	0.213***	0.374***	0.204***
	(0.028)	(0.935)	(3.144)	(0.027)	(0.023)	(0.027)	(0.022)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.992$							
Observations	653	653	653	653	653	653	653
R^2	0.000	0.000	0.000	0.000	0.000	0.001	0.001

"Equal rates" conditions ("equal rates" treatment vs. "equal rates" control)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.022	−1.782	0.715	−0.048	0.022	0.049	−0.063*
	(0.039)	(1.387)	(4.426)	(0.038)	(0.032)	(0.038)	(0.033)
Constant	0.509***	49.357***	74.720***	0.429***	0.208***	0.366***	0.264***
	(0.028)	(1.026)	(3.109)	(0.028)	(0.023)	(0.027)	(0.025)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.306$							
Observations	661	661	661	661	661	661	661
R^2	0.000	0.003	0.000	0.002	0.001	0.003	0.006

Notes: OLS regressions, robust standard errors in parentheses. Within each panel, each column regresses a demographic variable on a treatment dummy to test for imbalanced treatment assignment. In each panel, a joint F-test, estimated in a SUR model, tests the hypothesis that all treatment differences are zero. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.4 Test for balanced treatment assignment – part 2

Attention condition (compared to control condition of main study)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Attention	0.011	−1.356	−0.225	−0.042	−0.034	0.064	0.023
	(0.041)	(1.383)	(4.655)	(0.040)	(0.032)	(0.040)	(0.034)
Constant	0.511***	47.116***	76.831***	0.426***	0.213***	0.374***	0.204***
	(0.028)	(0.935)	(3.145)	(0.027)	(0.023)	(0.027)	(0.022)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.400$							
Observations	603	603	603	603	603	603	603
R^2	0.000	0.002	0.000	0.002	0.002	0.004	0.001

Attention "equal rates" condition (compared to "equal rates" control condition)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Attention	−0.026	−2.743*	−3.466	−0.069*	0.002	0.012	−0.009
	(0.041)	(1.472)	(4.485)	(0.040)	(0.034)	(0.040)	(0.036)
Constant	0.509***	49.357***	74.720***	0.429***	0.208***	0.366***	0.264***
	(0.028)	(1.026)	(3.109)	(0.028)	(0.023)	(0.027)	(0.025)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.400$							
Observations	589	589	589	589	589	589	589
R^2	0.001	0.006	0.001	0.005	0.000	0.000	0.000

Disappointment study (treatment vs. control)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	−0.021	0.610	7.267*	0.033	0.008	0.011	−0.064**
	(0.039)	(1.297)	(4.005)	(0.041)	(0.035)	(0.039)	(0.029)
Constant	0.636***	55.844***	62.980***	0.464***	0.245***	0.341***	0.185***
	(0.028)	(0.916)	(2.716)	(0.029)	(0.025)	(0.027)	(0.022)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.214$							
Observations	606	606	606	606	606	606	606
R^2	0.000	0.000	0.005	0.001	0.000	0.000	0.008

Notes: OLS regressions, robust standard errors in parentheses. Within each panel, each column regresses a demographic variable on a treatment dummy to test for imbalanced treatment assignment. In each panel, a joint F-test, estimated in a SUR model, tests the hypothesis that all treatment differences are zero. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A.5 Test for balanced treatment assignment – part 3

Counterfactual study (low/high counterfactual condition vs. control)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low count.	−0.018	−1.322	3.011	0.017	0.019	−0.019	0.018
	(0.040)	(1.686)	(4.615)	(0.040)	(0.033)	(0.039)	(0.034)
High count.	−0.046	2.631	0.041	0.059	−0.013	−0.014	0.009
	(0.040)	(2.682)	(4.635)	(0.040)	(0.032)	(0.039)	(0.034)
Constant	0.556***	50.869***	79.513***	0.534***	0.211***	0.393***	0.227***
	(0.028)	(1.389)	(3.218)	(0.028)	(0.023)	(0.028)	(0.024)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.717$							
Observations	945	945	945	945	945	945	945
R^2	0.001	0.003	0.001	0.002	0.001	0.000	0.000

Vignette study (low/high counterfactual condition vs. control)

	Female	Age	Income (in $1k)	Bachelor's degree	Region: Mid-west	Region: South	Region: West
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low count.	0.009	−0.080	3.792	0.054	−0.039	0.084**	0.011
	(0.034)	(1.209)	(3.637)	(0.035)	(0.030)	(0.034)	(0.026)
High count.	−0.016	−0.976	5.773	0.026	−0.020	0.018	−0.031
	(0.034)	(1.161)	(3.590)	(0.035)	(0.030)	(0.033)	(0.025)
Constant	0.612***	53.918***	66.715***	0.448***	0.249***	0.331***	0.163***
	(0.024)	(0.849)	(2.522)	(0.024)	(0.021)	(0.023)	(0.018)
Joint F-test (H_0: all differences between conditions are zero). $p = 0.083$							
Observations	1,222	1,222	1,222	1,222	1,222	1,222	1,222
R^2	0.000	0.001	0.002	0.002	0.001	0.006	0.002

Notes: OLS regressions, robust standard errors in parentheses. Each column regresses a demographic variable on treatment dummies to test for imbalanced treatment assignment. A joint F-test, estimated in a SUR model, tests the hypothesis that all treatment differences are zero. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Main experiment and robustness

B.1 Treatment effect

Average treatment effect Table B.1 tests for differences in merit judgments across the treatment and control conditions of the main study.

Histogram Figure B.1 plots the full distribution of reward shares assigned to the disadvantaged worker B in the main treatment and control condition. It shows histograms for each experimental condition and each effort scenario.

Heterogeneous treatment effects in main study Table B.2 tests for heterogeneous treatment effects.

Table B.1 Mean treatment effects in main study

Main study: Treatment − Control

Effort scenario e	0%	10%	30%	50%	70%	90%	100%	Average
Reward diff.	-1.93	-0.33	-1.58	-1.42	0.29	0.20	1.33	-0.49
Standard error	1.46	1.19	1.28	1.40	1.49	1.32	1.39	0.67
CI, 95%	[-4.8, 0.9]	[-2.7, 2]	[-4.1, 0.9]	[-4.2, 1.3]	[-2.6, 3.2]	[-2.4, 2.8]	[-1.4, 4.1]	[-1.8, 0.8]
p-values, t-tests	0.184	0.781	0.218	0.310	0.848	0.879	0.339	0.464
p-value, F-test	0.668							

Notes: Results from OLS regressions. Columns "0%" to "100%" present results for each of the seven effort scenarios, and Column "Average" presents results averaged across all scenarios. The outcome variable is the reward share assigned to the disadvantaged worker B. "Reward diff." denotes the estimated treatment effect (share in treatment condition versus share in control condition). Robust standard errors, 95% confidence intervals, and p-values are reported. The last row, "p-value, F-test", presents the p-value from an F-test that tests the joint null hypothesis that the differences are zero in each effort scenario. It is estimated in a SUR model with standard errors that are clustered at the respondent level.

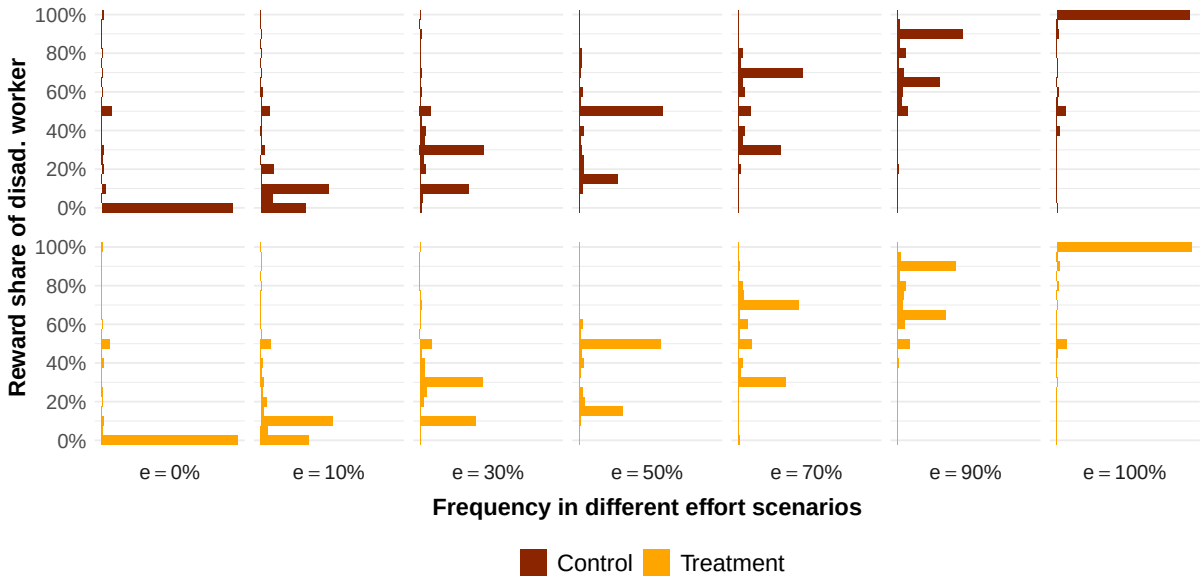


Figure B.1 Histogram of reward share of disadvantaged worker in main study

Notes: Histogram of the reward share assigned to the disadvantaged worker B in the treatment and control condition of the main study.

Table B.2 Heterogeneous treatment effects in the main study

	Mean reward share of disadv. worker (in %)
Treatment	9.953
	(8.966)
Female (bin.)	0.024
	(0.993)
College (bin.)	0.570
	(1.092)
Republican (bin.)	−0.852
	(1.002)
Income (log)	0.180
	(0.621)
Empathy (std.)	0.668
	(0.513)
Internal LOC (std.)	0.467
	(0.458)
Treatment × Female (bin.)	0.448
	(1.389)
Treatment × College (bin.)	−0.336
	(1.495)
Treatment × Republican (bin.)	0.764
	(1.394)
Treatment × Income (log)	−0.993
	(0.832)
Treatment × Empathy (std.)	−0.496
	(0.719)
Treatment × Internal LOC (std.)	−1.571
	(0.656)
Constant	42.098
	(6.663)
Observations	634
R^2	0.019

Notes: Results from the main study, OLS regressions, robust standard errors in parentheses. The outcome variable is the reward share assigned to the disadvantaged worker B, averaged across the seven effort scenarios. The independent variables include interaction terms of the treatment dummy with six respondent characteristics: a dummy for female gender, having a Bachelor's degree, and being Republican, logarithmic income, a standardized empathy score, and a standardized internal locus of control score. p-values of the interaction effects (printed in bold) are adjusted for multiple hypotheses testing with the help of the Benjamini-Hochberg procedure. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.2 Robustness checks

Robustness of treatment effects Figure B.2 explores the robustness of the treatment effects in the main study. For later reference, it also reports the same sensitivity analyses for the attention manipulation, the "equal rates" robustness conditions, the "equal rates" attention manipulation, and the counterfactual study. The following robustness specifications are estimated.

1. **Full sample**: Full sample, replicates main results.
2. **Exclude speeders**: I exclude the 25% participants with the lowest response duration.
3. **Exclude quiz mistakes**: I exclude participants who answer at least 1 question of the quiz wrongly.
4. **Exclude decisions 4-7**: I consider only the first three redistribution decisions of each participant. (Note: Not applicable in the counterfactual study, as I always focus on the first three redistribution decisions here.)
5. **Exclude recognition of real scenario**: I drop all respondents who are able to distinguish the hypothetical scenarios from the real one, after they saw all scenarios.
6. **Exclude $0.10**: Only applicable to the "equal rates" conditions and the "equal rates" attention manipulation. The "equal rates" control condition comes in two variants. Either both workers receive a piece-rate of $0.10 or both workers receive a piece-rate of $0.50 (see Appendix B.3). One concern is that only the latter variant can be cleanly compared to the treatment condition in which both workers end up with a piece-rate of $0.50. This would be the case if the *level* of the piece-rates affects *relative* reward shares. This robustness check therefore excludes spectators in the "equal rates" control condition with a piece-rate of $0.10.

The estimated treatment effects are robust in all studies.

Robustness to the order of workers In the experiment, I randomize whether worker A or worker B is advantaged or disadvantaged. The main analysis recodes all responses as if A was the advantaged worker to ease analysis and exposition. Here, I test whether a reverse order of workers, that is a worker pair in which worker A is disadvantaged and worker B is advantaged, affects merit judgments. I regress the average reward share respondents assign to the disadvantaged worker on a dummy for reversely ordered worker pairs. Table B.3 shows the results. The random variation in the order of workers does not affect merit judgments.

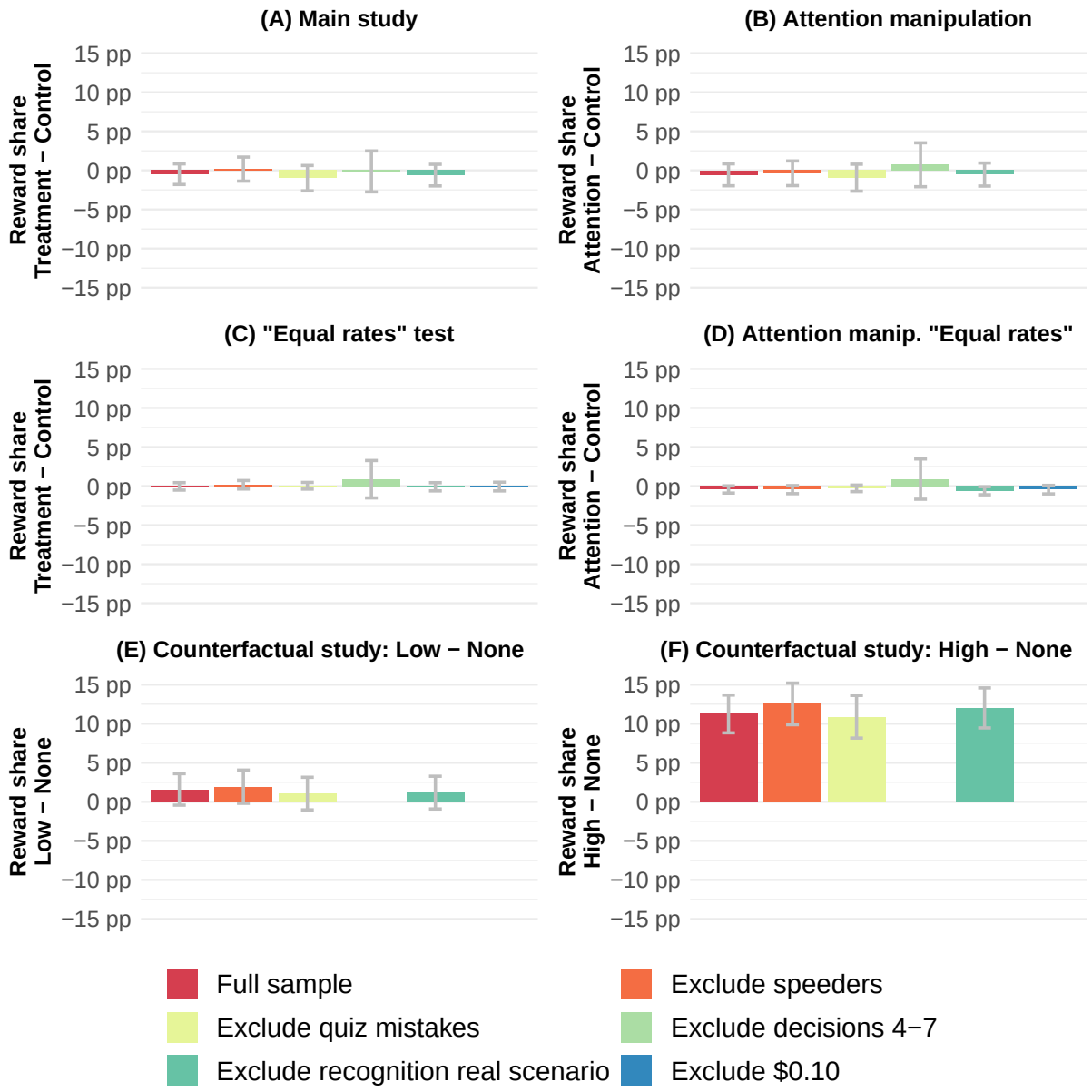


Figure B.2 Robustness of average treatment effects (with 95% CI)

Notes: Results from the main study, the attention condition, the "equal rates" conditions, the "equal rates" attention condition, and the counterfactual study. Each panel presents the results from a different treatment comparison. Each panel plots the treatment effect on the reward share assigned to the disadvantaged worker B (averaged across the effort scenarios) in different robustness specifications. See above for a description. The gray errorbars are 95% confidence intervals.

Table B.3 Robustness of merit judgments to the order of workers

	Mean reward share of disadv. worker (in %)				
	Main study	Robustness study "Equal rates"	Attention study	Attention study "Equal rates"	All
	(1)	(2)	(3)	(4)	(5)
Reverse order	−0.327	0.133	−0.058	0.274	−0.037
	(0.674)	(0.243)	(1.064)	(0.344)	(0.302)
Condition FE	✓	✓	✓	✓	✓
Observations	653	661	274	267	1,855
R^2	0.001	0.000	0.000	0.002	0.194

Notes: Results of the main study, the "equal rates" conditions, the attention condition, and the "equal rates" attention condition. OLS regressions, robust standard errors in parentheses. The outcome variable is the reward share assigned to the disadvantaged worker, averaged across all seven effort scenarios. The independent variable is a dummy that takes value 1 if worker A is disadvantaged and worker B is advantaged and value 0 for the opposite case. (Note: In the remainder of the paper, I recode all responses as if A was the advantaged worker to ease analysis and exposition.) Columns 1-4 present results from different conditions. Column 5 presents a pooled estimate. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.3 "Equal rates" conditions

More details on the experimental conditions

"Equal rates" control condition ($0.50 version) Both workers do not know their realized piece-rate while making their effort choices. They are aware that their piece-rates might either be $0.50 or $0.10 with equal chance. They learn about their realized piece-rate ($0.50 for worker A and $0.50 for worker B) only after completing their work.

"Equal rates" treatment condition Both workers do not know their realized piece-rate while making their effort choices. Worker A is aware that his piece-rate might either be $0.90 or $0.50 with equal chance. Worker B is aware that his piece-rate might either be $0.50 or $0.10 with equal chance. They learn about their realized piece-rate ($0.50 for worker A and $0.50 for worker B) only after completing their work.

The experiment relies on a between-subject treatment-control variation which is analogous to the main study but keeps the realized piece-rate of both workers constant. In the treatment condition, worker A is encouraged to work hard by the lucrative prospect of earning either $0.90 or $0.50 per task. In comparison, in the control condition, worker B is disadvantaged and discouraged from working hard by the mediocre prospect of earning either $0.50 or $0.10 per task. However, in both conditions, chance determines that both workers earn the same rate of $0.50. Workers initial earnings are thus fully

proportional to their effort, and there is no direct piece-rate effect on payments that could distract spectators.

I ran the "equal rates" conditions together with the main study in June 2020. The study protocol is identical. As before, the sample broadly represents the US population, and treatment assignment is balanced across covariates (see Appendix A).

The instructions are available online (https://osf.io/xj7vc/).

Qualifying note: Workers who receive a \$0.90 piece-rate receive their payments without a redistribution stage. Workers with a \$0.10 piece-rate are used in a second variant of the "equal rates" control condition in which both workers earn \$0.10. To maximize statistical power, I present results in which I pool the \$0.50 and the \$0.10 control conditions, but the results are virtually identical if I only use the \$0.50 control condition described above (see Appendix B.2).

Average treatment effect Table B.4 tests for differences in merit judgments across the treatment and control conditions of the "equal rates" manipulation.

Robustness The results are robust to excluding potentially inattentive responses (misunderstanding of the instructions, survey-taking fatigue, and "speeders"; see Appendix B.2).

Table B.4 Mean treatment effects in "equal rates" conditions

"Equal rates": Treatment − Control								
Effort scenario e	0%	10%	30%	50%	70%	90%	100%	Average
Reward diff.	1.51	0.55	0.64	-0.14	-0.69	-1.70	-0.44	-0.04
Standard error	1.43	1.09	0.67	0.18	0.63	1.15	1.19	0.24
CI, 95%	[-1.3, 4.3]	[-1.6, 2.7]	[-0.7, 1.9]	[-0.5, 0.2]	[-1.9, 0.6]	[-4, 0.6]	[-2.8, 1.9]	[-0.5, 0.4]
p-values, t-tests	0.292	0.613	0.336	0.423	0.277	0.140	0.711	0.872
p-value, F-test	0.747							

Notes: Results from OLS regressions. Columns "0%" to "100%" present results for each of the seven effort scenarios, and Column "Average" presents results averaged across all scenarios. The outcome variable is the reward share assigned to the disadvantaged worker B. "Reward diff." denotes the estimated treatment effect (share in treatment condition versus share in control condition). Robust standard errors, 95% confidence intervals, and p-values are reported. The last row, "p-value, F-test", presents the p-value from an F-test that tests the joint null hypothesis that the differences are zero in each effort scenario. It is estimated in a SUR model with standard errors that are clustered at the respondent level.

B.4 Disappointment study

More details on the experimental conditions

Disappointment control condition Both workers have to complete 10 tasks. They do not know their realized piece-rate while making their effort choices. They are aware that their piece-rates might either be \$0.50 or \$0.10 with equal chance. They learn about their realized piece-rate (\$0.50 for worker A and \$0.10 for worker B) only after completing their work.

Disappointment treatment condition Both workers have to complete 10 tasks. They are informed about their realized piece-rate already before they decide how much effort they exert. Thus, worker A knows about his high rate of \$0.50 and worker B about his low rate of \$0.10 when they decide how many tasks they complete.

I ran the "disappointment" experiment in February 2021 with a convenience sample of US adults recruited with the help of the survey company Lucid. Treatment assignment is balanced across covariates (see Appendix A). The results are robust to the use of post-stratification weights (see Table B.5).

The instructions are available online (https://osf.io/xj7vc/).

Average treatment effect Table B.5 tests for differences in merit judgments across the experimental conditions of the disappointment study.

Table B.5 Treatment effects in the disappointment study

	Reward share of disadvantaged worker (in %)	
	(1)	(2)
Treatment	−2.202	−0.763
	(1.422)	(2.122)
Constant	36.695***	35.863***
	(0.973)	(1.387)
Weights	–	✓
Observations	606	606
R^2	0.004	0.000

Notes: Results from the disappointment study, OLS regressions, robust standard errors in parentheses. The outcome variable is the reward share assigned to worker B (low piece-rate). The independent variable is a treatment indicator. Column 1 reports the unweighted main specification. Column 2 applies post-stratification weights. The weights render the sample representative for the US general population in terms of gender, age, income, education, and census region. I use a raking algorithm (R package `anesrake`) and follow the guidelines of the American National Election Study to calculate the survey weights (Pasek et al., 2014). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.5 Open-text data

Coding scheme and examples At the end of the survey, respondents explain in an open-text format which thoughts and considerations shaped the merit judgments they made. The open-text data are rich in detail and allow to capture the thoughts that were on participants' minds while they made their merit judgments. Below, I report three example responses.

> "The reasoning behind the decisions I made was based on how much each worker performed. I had it so that each worker would get the percentage of money according to the percentage of work he completed."
>
> "I believed that each worker should be paid based on their amount of work. The person who completed 45 tasks should make more money than the person who completed 10 tasks, even if they knew they were making less from the beginning."
>
> "The workers agreed to particular rates and amount of work required to be performed. They knowingly made the decisions regarding how much money they earned. Fair or not what you agree to work for is what they should be paid."

Most responses refer to one (sometimes two, rarely more) distinct fairness view. I develop a coding scheme that captures these views. Each response is assigned to the fairness views it refers to. Table B.6 provides an overview of all codes.

Which fairness views are mentioned? Table B.7 reports the share of responses that are assigned to each fairness code in the control and treatment conditions of the main study. The last column test whether these shares differ across the conditions and detects no significant differences.

Correlation with merit judgments Table B.8 shows that the fairness motives that spectators mention in their open-text responses are strongly correlated with their merit judgments.

Table B.6 Classification of open-ended responses

Code	Explanation	Example
Fairness codes		
Effort	Reward based on work, effort, task completion.	"People should get paid based on their work quality and effort [...]"
Initial outcome fair	Reward based on initial payments. Workers accepted their work conditions, hence no need for redistribution.	"I based my decisions on the 'ground rules' the workers signed up for before they did the tasks. They each knew that the chance of being paid either $0.10 or $0.50 per piece we 50% up front and agreed to do the work."
Equality	Equal rewards irrespective of effort and circumstances.	"I felt that they were equally deserving."
Endogeneity	Acknowledgment that workers' effort choices are shaped by their circumstances.	"[...] The lower rate does not promote someone to work that hard."
Need	Decision shaped by concern that workers need a sufficient income.	"They both need money to survive in this planet"
Residual codes		
Misunderstanding	Explanation clearly reveals misunderstanding of the instructions.	"I'm not quite sure I understood if I was supposed to change the amount paid."
Other	Explanation too vague or nonsensical to assign a fairness code.	"Based on my idealogy of fairness, worker's wages and/or ability."

Notes: This table provides an overview of the different categories in the coding scheme, an explanation for each code, and example extracts from open-text responses that belong to the corresponding category.

Table B.7 Frequency of fairness motives in open-text data

Code	Control	Treatment	p-value
Fairness codes			
Effort	58.3%	59.2%	0.915
Initial outcome fair	8.2%	12.9%	0.375
Equality	10.0%	10.3%	0.915
Endogeneity	0.6%	1.0%	0.915
Need	0.3%	0.6%	0.915
Residual codes			
Misunderstanding	1.6%	1.3%	0.915
Other	27.0%	22.2%	0.575
Sample size	319	311	

Notes: This table shows which share of treatment respondents and control respondents mention different fairness motives in their open-text response. See Table B.6 for details on the coding scheme. The last column contains p-values from χ^2-tests which test for the equality of proportions in each row and are adjusted for multiple hypotheses testing, using the Benjamini-Hochberg procedure.

Table B.8 Correlations between merit judgments and open-text explanations

	Effort-proportional shares	No redistribution	Equal shares
	(1)	(2)	(3)
Explanation: Effort	0.270***	−0.110***	−0.040**
	(0.025)	(0.028)	(0.018)
Explanation: Initial outcome fair	−0.135***	0.342***	−0.122***
	(0.029)	(0.041)	(0.015)
Explanation: Equality	−0.059	−0.097**	0.075*
	(0.040)	(0.049)	(0.038)
Constant	0.411***	0.500***	0.158***
	(0.021)	(0.027)	(0.018)
Observations	4,410	4,410	4,410
R^2	0.091	0.067	0.022

Notes: Results from the main study, OLS regressions, standard errors (in parentheses) clustered at respondent level. I pool respondents from the treatment and control conditions. The outcome variables are three dummy variables that indicate different redistribution patterns: (1) The reward share p assigned to worker B (low piece-rate) in an effort scenario is at most 2 percentage points away from the effort-proportional share e; (2) spectators do not intervene (redistribute at most 2% of the total income); (3) spectators implement equality (share at most 2% away from equal split). The regressors are dummy variables that indicate whether respondents mention different fairness motives in their open-text explanations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.6 Opportunity costs as confounder

One could argue that the spectators attempt to draw inferences about the workers' life situations outside the experiment. A worker who completes 25 tasks for a \$0.10 piece-rate (treatment) might not only be more diligent than a worker who completes the same amount of tasks for better piece-rate prospects of either \$0.10 or \$0.50 (control). He might also assign a higher marginal value to money or have lower marginal opportunity costs of time. Spectators could interpret this as a sign of neediness and assign a higher payment share to the disadvantaged worker B in treatment than control. Any such argument predicts the existence of a treatment effect and is thus firmly rejected by the null result in the main experiment. Moreover, none of the open-text responses mentions this line of reasoning or similar considerations that could confound the treatment effect.

C Mechanism evidence

C.1 Beliefs about situational influence in the main study

Beliefs about situational influence in the main study Figure C.1 shows the average beliefs about the number of tasks workers complete on average for a high rate of $0.50 and compares it to workers' real choices. Spectators have, on average, very accurate beliefs about the incentive effect in the study environment.

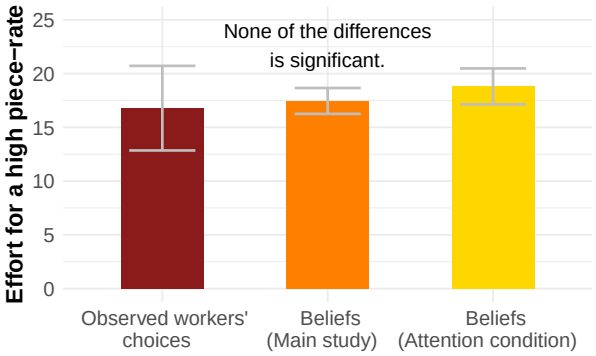


Figure C.1 Average beliefs about the piece-rate effect (with 95% CI)

Notes: Results from the main study and the attention condition. The figure presents the average observed and average perceived effort choices of workers for a high piece-rate of $0.50. The average number of completed tasks for a low piece-rate is 5.04. Red bar: Actual effort decisions of workers. Orange bar: Effort choice that spectators expect in the main study. Yellow bar: Effort choice that spectators expect in attention condition. The gray errorbars are 95% confidence intervals. t-tests are used to evaluate the significance of the differences.

C.2 Attention manipulation

Average treatment effect Table C.1 tests for differences in merit judgments across the experimental conditions. Panel (A) compares the attention condition with the control condition of the main experiment, Panel (B) compares it to the treatment condition. All three conditions were collected in parallel.

"Equal rates" attention condition Panel (C) of Table C.1 builds on an analogous attention manipulation that extends the "equal rates" conditions. I explicitly inform spectators that "the piece-rates strongly influence the number of tasks a worker completes." Spectators learn how large this incentive effect is on average (in the equal rates conditions, see Appendix F) and read two typical comments by workers that explain

why this is the case. Participants have to spend at least 20 seconds on this information page, whose key message is repeated on the next page and tested for in the subsequent quiz. Data for this condition was collected together with the main study, the "equal rates" conditions, and the main attention manipulation discussed in the main text.

Robustness For robustness analyses, see Section B.2.

Table C.1 Mean treatment effects of attention manipulation

(A) Attention manipulation: Attention − Control (compared to main control condition)

Effort scenario e	0%	10%	30%	50%	70%	90%	100%	Average
Reward diff.	-1.24	0.88	-0.88	-1.28	-1.38	-0.14	0.04	-0.57
Standard error	1.52	1.31	1.40	1.48	1.52	1.40	1.53	0.72
CI, 95%	[-4.2, 1.7]	[-1.7, 3.4]	[-3.6, 1.9]	[-4.2, 1.6]	[-4.4, 1.6]	[-2.9, 2.6]	[-3, 3]	[-2, 0.8]
p-values, t-tests	0.412	0.504	0.529	0.388	0.366	0.921	0.980	0.423
p-value, F-test	0.583							

(B) Attention manipulation: Attention − Treatment (compared to main treatment condition)

Effort scenario e	0%	10%	30%	50%	70%	90%	100%	Average
Reward diff.	0.69	1.21	0.70	0.14	-1.66	-0.34	-1.29	-0.08
Standard error	1.41	1.32	1.33	1.46	1.52	1.33	1.45	0.71
CI, 95%	[-2.1, 3.5]	[-1.4, 3.8]	[-1.9, 3.3]	[-2.7, 3]	[-4.6, 1.3]	[-2.9, 2.3]	[-4.1, 1.6]	[-1.5, 1.3]
p-values, t-tests	0.626	0.360	0.601	0.923	0.275	0.799	0.374	0.910
p-value, F-test	0.768							

(C) "Equal rates" attention man.: Attention − Control (compared to "equal rates" control condition)

Effort scenario e	0%	10%	30%	50%	70%	90%	100%	Average
Reward diff.	-1.73	-0.03	0.03	0.16	-0.38	-0.47	-0.60	-0.43
Standard error	1.29	1.14	0.75	0.21	0.73	1.20	1.27	0.23
CI, 95%	[-4.3, 0.8]	[-2.3, 2.2]	[-1.4, 1.5]	[-0.2, 0.6]	[-1.8, 1]	[-2.8, 1.9]	[-3.1, 1.9]	[-0.9, 0]
p-values, t-tests	0.178	0.979	0.968	0.452	0.601	0.698	0.638	0.066
p-value, F-test	0.208							

Notes: Results from OLS regressions. Each panel presents the results from a different comparison of experimental conditions. The title of each panel describes which experimental conditions are compared. Columns "0%" to "100%" present results for each of the seven effort scenarios, and Column "Average" presents results averaged across all scenarios. The outcome variable is the reward share assigned to the disadvantaged worker B. "Reward diff." denotes the estimated treatment effect. Robust standard errors, 95% confidence intervals, and p-values are reported. The last row, "p-value, F-test", presents the p-value from an F-test that tests the joint null hypothesis that the differences are zero in each effort scenario. It is estimated in a SUR model with standard errors that are clustered at the respondent level.

C.3 Counterfactual study

Average treatment effect Table C.2 tests for differences in merit judgments across the experimental conditions of the counterfactual study.

Histogram Figure C.2 plots the full distribution of reward shares assigned to the disadvantaged worker B in the conditions of the counterfactual study. They show histograms for each experimental condition and each effort scenario.

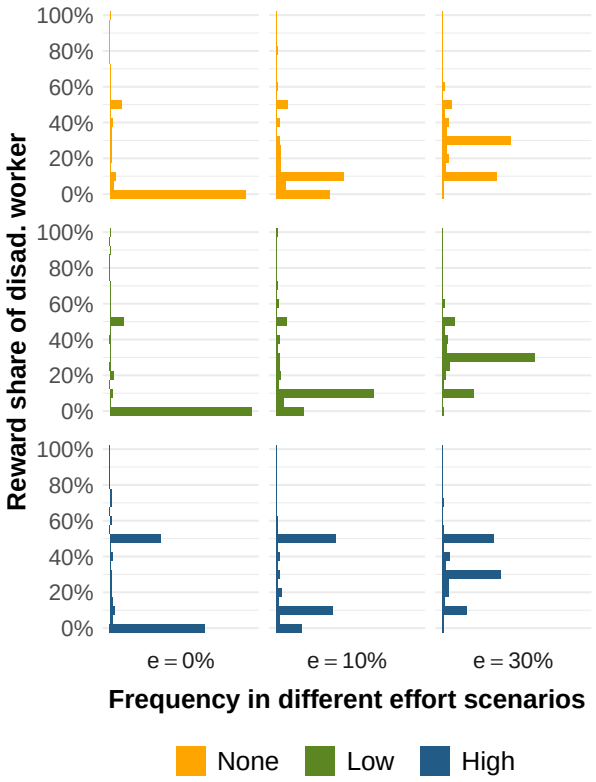


Figure C.2 Counterfactual study: Histograms of reward share of disadv. worker

Notes: Histograms of the reward share assigned to the disadvantaged worker B for each experimental condition and each effort scenario in the counterfactual study.

Table C.2 Mean treatment effects in counterfactual study

(A) Low counterfactual − No information

Effort scenario e	0%	10%	30%	Average
Reward diff.	-0.13	1.58	3.32	1.59
Standard error	1.34	1.31	1.11	1.03
CI, 95%	[-2.8, 2.5]	[-1, 4.1]	[1.1, 5.5]	[-0.4, 3.6]
p-values, t-tests	0.923	0.227	0.003	0.123
p-value, F-test	0.011			

(B) High counterfactual − No information

Effort scenario e	0%	10%	30%	Average
Reward diff.	12.31	12.75	8.69	11.25
Standard error	1.65	1.49	1.21	1.23
CI, 95%	[9.1, 15.5]	[9.8, 15.7]	[6.3, 11.1]	[8.8, 13.7]
p-values, t-tests	<0.001	<0.001	<0.001	<0.001
p-value, F-test	<0.001			

Notes: Counterfactual study, results from OLS regressions. Panel A compares the *Low counterfactual* with the *No information* condition. Panel B compares the *High counterfactual* with the *No information* condition. Columns "0%" to "30%" present results for each of the three effort scenarios, and Column "Average" presents results averaged across all three scenarios. The outcome variable is the reward share assigned to the disadvantaged worker B. "Reward diff." denotes the estimated treatment effect. Robust standard errors, 95% confidence intervals, and p-values are reported. The last row, "p-value, F-test", presents the p-value from an F-test that test the joint null hypothesis that the differences are zero in each effort scenario. It is estimated in a SUR model with standard errors that are clustered at the respondent level.

C.4 Cappelen et al. (2021, 2019)

Cappelen et al. (2021, 2019) study fairness views in an uncertain environment but their mechanisms can only play a negligible role in my setting.

Cappelen et al. (2019) show that individuals do not want to risk rewarding the wrong person and hence prefer more equal rewards when it is unclear who merits the higher reward. However, in my setting, it is clear for comparable choice meritocrats that worker B merits a (weakly) higher reward in the treatment than in the control condition. It remains only unclear how much higher the reward should be. "Risk-averse" comparable choice meritocrats would still want to compensate the disadvantaged worker when the counterfactual is uncertain to ensure their reward decision is close to the expected fair merit judgment.

Relatedly, Cappelen et al. (2021) show that individuals are more concerned with false negatives (do not reward a deserving worker) than with false positives (reward an undeserving worker). At first glance, this effect should translate into a larger tendency to compensate the discouraged worker, opposite to what I find. However, in the redistribu-

tion setting of my experiment, false negatives for worker B (not rewarding a relatively deserving disadvantaged worker) also imply a false positive for worker A (rewarding a relatively undeserving advantaged worker) (and vice versa), clouding the distinction between false negatives and false positives.

D Structural model of fairness views

D.1 Maximum likelihood estimation

Data Counterfactual study, decisions 4-7, 945 respondents. In decisions 4-7, respondents face a randomly generated effort scenario.[34] The effort share of worker B and his counterfactual effort share (had he earned a high piece-rate) are drawn as follows.

- Effort of worker A: Uniformly randomly drawn from the set $\{0, 1, ..., 49, 50\}$.
- Effort of worker B: Uniformly randomly drawn from the set $\{0, 1, ..., 24, 25\}$.
- Counterfactual effort of worker B for a high piece-rate: The difference between the counterfactual and observed effort is uniformly randomly drawn from the set $\{0, 1, ..., 24, 25\}$.
- The effort and initial payment shares of both workers follow from the above variables.

In the baseline condition, no information about the counterfactual effort choice of the disadvantaged worker is provided. In the "low counterfactual" and the "high counterfactual" conditions, spectators are informed about what the disadvantaged worker would have made in advantaged circumstances.

Model Each individual endorses one of the four merit views that are discussed in Section 6 of the main text. A respondent i of type t rewards the workers according to her merit view $m_i^t(e, s)$ in scenario s and a normally distributed response error $\varepsilon_{is} \sim_{iid} N(0, \sigma^2)$. That is, $r_{is} = m_i^t(e, s) + \varepsilon_{is}$.

As discussed in Section 6, I parametrize the fairness view of comparable choice meritocrats as follows:

$$m_i^{\text{Comparable}}(e, s) = \begin{cases} \rho Ec(e, s) + (1 - \rho)e & \text{if counterfactual is uncertain} \\ c(e, s) & \text{if counterfactual is known} \end{cases}$$

This means that comparable choice meritocrats tend to discount the expected counterfactual effort choice if it is uncertain. The discount parameter is ρ.

I also need to estimate spectators' expectation of the counterfactual effort share, $Ec(e, s)$, when the counterfactual is unknown. In line with the evidence of Section

[34] The contingent response method allows me to freely vary the effort choices of workers in the hypothetical scenarios without being deceptive.

5, I assume that spectators correctly anticipate the average effect of the piece-rate. In the data, I observe that workers are willing to complete about 12.5 tasks more for a high piece-rate (see Table F.1, Column 3). I use this estimate to derive spectator's expected counterfactual effort choice of worker B ($EC_B = E_B + 12.5$) for each effort scenario in the baseline condition where the counterfactual effort choice is unknown.[35] This allows me to derive $Ec(e,s) \approx \frac{EC_B}{E_A+EC_B}$. Below, I show that I obtain virtually identical results with an alternative specification of $Ec(e,s)$. The results are insensitive to the calibration $Ec(e,s)$ because the spectators fully discount it anyway.

The model has six parameters: the population shares $\boldsymbol{\theta}$ of the four merit views ($\sum_t \theta_t = 1$), the discount parameter ρ, and the standard deviation of the response error σ. I impose $0 \leq \theta_t \leq 1 \; \forall t$, $0 \leq \rho \leq 1$, and $\sigma > 0$.

Log-likelihood

$$\log F(\boldsymbol{r} \mid \boldsymbol{\theta}, \rho, \sigma) = \sum_i \log f_i(\boldsymbol{r}_i \mid \boldsymbol{\theta}, \rho, \sigma) \tag{1}$$

$$f_i(\boldsymbol{r}_i|\boldsymbol{\theta}, \rho, \sigma) = \sum_t \theta_t f_i^t(\boldsymbol{r}_i \mid \theta_t, \rho, \sigma) \tag{2}$$

$$f_i^t(\boldsymbol{r}_i \mid \theta_t, \rho, \sigma) = \prod_s \varphi(r_{is} - m_i^t(s,e,\rho), \sigma^2) \tag{3}$$

where φ denotes the normal density function.

Estimation I estimate the model in R with the help of the `maxLik` package (Henningsen and Toomet, 2011). The BFGS algorithm is used to solve the constrained optimization problem. I estimate ρ, σ, and the share of actual choice meritocrats, comparable choice meritocrats, and libertarians. The share of egalitarians follows via $\sum_t \theta_t = 1$.

Computational robustness I confirm the numerical stability of the maximum likelihood estimator in three steps. First, I replicate the results in 100 estimations with random start parameters. Second, I generate 100 simulated data sets from the model with randomly drawn parameters and confirm that the estimates recover the parameters of the models. Third, I replicate the results with the Nelder-Mead optimization algorithm.

Inference for constrained maximum likelihood Standard inference in constrained maximum likelihood models can become unreliable if one of the parameters is on or near the boundary (Schoenberg, 1997). Since I estimate a ρ of 0.00 which is on the

[35] Workers can complete at most 50 tasks, so I cap the counterfactual effort choices at 50.

boundary, caution seems to be warranted. The discussion below indicates, however, that the inference is nevertheless reliable.

First, I obtain virtually identical estimates and standard errors for $\boldsymbol{\theta}$ and σ if I estimate the model without constraints (results available upon request).

Moreover, I assess the coverage of the confidence intervals in an independent simulation experiment. To this end, I generate 1,000 simulated data sets from the model, assuming that the main estimates in Table 5 are the true parameter values. In particular, I impose $\rho = 0$. For each simulated data set, I derive the maximum likelihood estimates and their associated 95% confidence intervals. Then, I assess whether the confidence intervals cover the "true" parameters in about 95% of cases. This is indeed the case. The estimated coverage frequency ranges from 93.8% to 97.4%. I obtain similar results if I randomly perturb $\boldsymbol{\theta}$ and σ in each simulation to explore the coverage in the neighborhood of the estimated parameters (here, the coverage ranges from 94.4% to 98.0%).

D.2 Robustness of estimates

Table D.1 shows that the results of the maximum likelihood are robust across several different specifications.

- Main: Main specification
- Duration: Excludes respondents with a response duration that is lower than the 25% percentile.
- Quiz: Excludes respondents who answer at least one quiz question wrongly.
- Guess correct: Excludes respondents who are able to distinguish the real scenario from the hypothetical ones.
- Multipl. effort: Here, I calibrate spectators' expectations of worker B's counterfactual effort as $EC_B = 3.3 * E_B$, assuming that the effect of the higher piece-rate is multiplicative. In the data, I observe that workers are willing to complete about 3.3 as many tasks for a high piece-rate than for a low piece-rate (see Table F.1, Column 3).
- Bounds adjust: Because the support of normal noise is unbounded, the likelihood function assigns positive probability to reward shares below 0% or above 100% that cannot occur in practice. Here, I limit the support to values that can occur in practice. I rescale each error density by the inverse cumulative density that lies outside the interval [0%-100%].

- Trembling: I explore an alternative error specification. Respondents have a "trembling hand" and their response r_{is} is fully random (uniform over [0%-100%]) with probability α. With probability $1 - \alpha$, their response is very close to their merit view (normal error with a standard deviation of 2 percentage points).

D.3 Heterogeneity in discount parameter ρ

The main model allows for only one type of comparable choice meritocrats, whereas, in principle, multiple types with a differential degree of counterfactual discounting ρ could exist. To explore this possibility in a tractable, simple procedure, I estimate a model that allows for six different, pre-defined types of comparable choice meritocrats, each with a fixed discount parameter $\rho \in \{0.0, 0.2, 0.4, 0.6, 0.8, 1.0\}$. How much probability mass does the model assign to comparable choice meritocrats with $\rho \neq 0.0$? The test is demanding because it is easy for the model to fit various discounting types if they exist in the data.

Do types with a higher discount parameter exist? The results indicate that this is not the case. The estimated model virtually reproduces the estimates of the main model (Table 5). 25.5% of respondents are comparable choice meritocrats with a ρ of 0. Only 0.9% of individuals are estimated to be comparable choice meritocrats with $\rho > 0$, and none of the underlying shares is significant. Therefore, I conclude that no sizeable heterogeneity in ρ exists.

D.4 Heterogeneity by demographics

The model allows to estimate whether its parameters differ for subgroups of respondents. Consider two groups of respondents, group A and group B. I assume that the population shares of different fairness types and the counterfactual discount parameter are $(\boldsymbol{\theta},\ \rho)$ in group A. In group B, the population shares are $(\boldsymbol{\theta},\ \rho) + \boldsymbol{\lambda}$. That is, I allow each parameter p to differ by λ_p between both groups.

I estimate this model separately for the following group comparisons: male versus female respondents, respondents with below-median versus above-median income, respondents without versus with college degree, Democrats versus Republicans. Table D.2 displays the resulting estimates of $\boldsymbol{\lambda}$.

Table D.1 Robustness of structural estimation

	(1) **Main**	(2) **Duration**	(3) **Quiz**	(4) **Guess correct**	(5) **Multipl. effort**	(6) **Bounds adjust**	(7) **Tremb-ling**
Population shares							
Actual choice meritocrats	36.7%	35.5%	39.9%	36.8%	36.7%	35.9%	34.7%
	(1.9%)	(2.1%)	(2.3%)	(2.0%)	(1.9%)	(2.1%)	(1.9%)
Comparable choice meritocrats	26.2%	28.9%	27.3%	26.2%	26.2%	26.2%	29.2%
	(1.7%)	(2.0%)	(2.1%)	(1.9%)	(1.8%)	(2.1%)	(1.8%)
Libertarians	23.0%	23.7%	22.5%	23.4%	23.0%	23.8%	24.8%
	(1.4%)	(1.6%)	(1.7%)	(1.5%)	(1.4%)	(1.4%)	(1.5%)
Egalitarians	14.2%	11.9%	10.4%	13.6%	14.1%	14.1%	11.3%
	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Counterfactual discount parameter							
ρ	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)	(0.11)	(0.01)
Error term and sample							
σ *noise*	9.27	9.16	8.60	9.32	9.27	9.72	
	(0.11)	(0.13)	(0.12)	(0.12)	(0.11)	(0.13)	
α *noise*							0.23
							(0.01)
Respondents	945	708	656	834	945	945	945
Decisions	3777	2831	2621	3333	3777	3777	3777

Notes: Results from counterfactual study, decisions 4-7. Maximum likelihood estimation of the structural model of fairness views. Standard errors in parentheses. The estimates indicate the population shares of different fairness views and the discounting parameter ρ. The columns estimate the model for different specifications. See text above. No standard errors are reported for the share of egalitarians because their share is deduced from the other estimates.

Table D.2 Differences of model parameters ($\boldsymbol{\lambda}$) by group

	(1) **Female** (vs. male)	(2) **Income >median** (vs. ≤median)	(3) **College degree** (vs. none)	(4) **Republican** (vs. Democrats)
Differences in shares				
Actual choice meritocrats	6.0%	2.2%	-2.2%	4.7%
	(3.7%)	(3.8%)	(3.8%)	(3.8%)
Comparable choice meritocrats	-1.6%	-3.5%	5.3%	-0.8%
	(3.5%)	(3.5%)	(3.5%)	(3.5%)
Libertarians	-2.4%	-2.6%	-6.7%**	-0.5%
	(2.8%)	(2.8%)	(2.9%)	(2.9%)
Egalitarians	-1.9%	3.9%	3.6%	-3.4%
	(–)	(–)	(–)	(–)
Differences in counterfactual reasoning				
ρ	0.00	0.01	0.00	0.00
	(0.09)	(0.10)	(0.09)	(0.09)
Sample				
Respondents	916	916	916	916
Decisions	3661	3661	3661	3661

Notes: Results from counterfactual study, decisions 4-7. Maximum likelihood estimation of the structural model of fairness views which allows for different parameters across two groups of individuals. Standard errors in parentheses. The table reports the estimated differences in parameters ($\boldsymbol{\lambda}$). For the sake of brevity, the baseline estimates ($\boldsymbol{\theta}$ and ρ) as well as the normal error (σ, constant across groups) are not reported. The columns report results from separate estimations. The column labels indicate which two demographic groups are compared. See text above. No standard errors are reported for the share difference of egalitarians because their share is deduced from the other estimates. *** $p < 01$, ** $p < 0.05$, * $p < 0.10$.

E Vignette study

Robustness of treatment effects Table E.1 shows that the results of the vignette study are largely insensitive to the exclusion criterion and to survey weights that render the sample representative for the US general population in terms of gender, age, income, education, and census region. I use a raking algorithm (R package `anesrake`) and follow the guidelines of the American National Election Study to calculate the survey weights (Pasek et al., 2014).

- **Main**: Main specification
- **Keep 45s+**: Exclude respondents who complete the vignettes with an average response time of less than 45 seconds (instead of 60s).
- **Keep 75s+**: Exclude respondents who complete the vignettes with an average response time of less than 75 seconds (instead of 60s).
- **Weighted**: Weighted OLS regression.

Results of additional crime vignette The vignette survey also contained a fourth vignette on criminal behavior (see Appendix I for the full vignette wording).

> **Crime vignette**: In this vignette, the advantaged person grew up in a rich neighborhood with low crime rates. He went to good schools, and his parents made sure he grew up in a loving, nurturing environment. The disadvantaged person grew up in a poor neighborhood with very high crime rates. His parents often neglected him, and both his family and peers committed crimes. While the advantaged person started studying business and works as a salesman, the disadvantaged person started selling drugs and frequently violates the law. Both earn $50,000 a year today.

In contrast to the other vignette, the crime vignette revolves around legal versus illegal behavior instead of hard work or entrepreneurial risk-taking, and both persons earn equal instead of unequal incomes. As a consequence, respondents redistribute money *away* from the disadvantaged, criminal person in the baseline condition, likely because they reject the illegal source of his income. Only 41% accept the initial income equality between both persons (Column 1, Table E.2). This fraction is virtually identical in the low counterfactual treatment, but 12.3 percentage points higher in the high counterfactual treatment, replicating the findings in the other vignettes.

Still, Column 2 suggests that the average reward share of the unlawful person might be slightly lower when respondents know that the person would violate the law even if

he had grown up in privileged circumstances. This effect is driven by a slightly larger share of respondents who take all money away from the unlawful person (Column 3). Both effects are however only marginally significant.

Table E.1 Robustness of the results from the vignette study

(A) Share of respondents redistributing towards the disadvantaged worker

	Binary indicator for compensation			
	Main	Keep 45s+	Keep 75s+	Weighted
	(1)	(2)	(3)	(4)
Low counterfactual	−0.004	−0.016	0.002	−0.000
	(0.029)	(0.029)	(0.031)	(0.038)
High counterfactual	0.126***	0.122***	0.122***	0.135***
	(0.029)	(0.029)	(0.031)	(0.037)
Vignette FE	✓	✓	✓	✓
Observations	2,664	2,789	2,390	2,664
R^2	0.028	0.028	0.027	0.024

(B) Mean reward share of disadvantaged person

	Reward share of disadv. person (in %)			
	Main	Keep 45s+	Keep 75s+	Weighted
	(1)	(2)	(3)	(4)
Low counterfactual	−1.539	−1.828*	−0.974	−1.332
	(1.085)	(1.075)	(1.121)	(1.495)
High counterfactual	6.795***	6.921***	6.861***	6.847***
	(1.177)	(1.175)	(1.224)	(1.447)
Vignette FE	✓	✓	✓	✓
Observations	2,664	2,789	2,390	2,664
R^2	0.135	0.133	0.139	0.116

Notes: Results from the vignette study. OLS regressions with standard errors clustered at the respondent level. The dependent variable in Panel A is a binary indicator for whether a respondent compensates the disadvantaged person by redistributing money towards him. The dependent variable in Panel B is the reward share assigned the disadvantaged person. The independent variables are treatment dummies. Columns 1 shows the main specification. Column 2-4 report different robustness checks that are explained above. *** $p < 01$, ** $p < 0.05$, * $p < 0.10$.

Table E.2 Vignette study: Results from the crime vignette

	Binary indicator for equal shares	Reward share of disadv. person (in %)	Binary indicator for giving 0% to the disadv. person
	(1)	(2)	(3)
Low counterfactual	−0.031	−3.066*	0.056*
	(0.040)	(1.649)	(0.029)
High counterfactual	0.123***	3.347**	−0.004
	(0.040)	(1.571)	(0.027)
Constant	0.412***	34.111***	0.124***
	(0.028)	(1.114)	(0.019)
Observations	894	894	894
R^2	0.018	0.017	0.006

Notes: Results from the vignette study. OLS regressions with robust standard errors. Column 1 regresses a binary indicator for whether a respondent accepts the reward equality between both persons on treatment dummies. The dependent variable in Column 2 is the reward share assigned the disadvantaged person. In Column 3, the dependent variable is a binary indicator for taking all money away from the unlawful person. *** $p < 01$, ** $p < 0.05$, * $p < 0.10$.

F Endogenous effort choices in the worker setting

This appendix documents that the piece-rates strongly influence how much effort a worker exerts. I study the effort choices of 548 workers who were recruited for the study. 336 workers were recruited for the main study and the additional "equal rates" and attention conditions (Amazon Mechanical Turk, US, May and June 2020). 212 were recruited for the *counterfactual* study (Amazon Mechanical Turk, US, January 2021).[36]

Table F.1 regresses the number of completed tasks on an indicator for a high piece-rate. Specifically,

- Column 1, Main: "High rate" means a piece-rate of \$0.50 instead of \$0.10.
- Column 2, Robustness "equal rates": "High rate" means (uncertain) piece-rate prospects of \$0.50 or \$0.90 (with equal chance) instead of \$0.10 or \$0.50 (with equal chance).
- Column 3, Counterfactual: "High rate" means a piece-rate of \$0.50 instead of \$0.10. The counterfactual study uses a within-subject design. Each worker decides how much effort he would exert for a high piece-rate and for a low piece-rate.

The higher piece-rate leads to a 333% higher effort in the main condition, a 155% higher effort in the "equal rates" condition, and a 335% higher effort in the counterfactual condition. Thus, the external piece-rate strongly affects how much effort the workers exert.

Table F.1 The effect of a high piece-rate on workers' effort

	Effort (number of completed tasks)		
	Main	Robustness "Equal rates"	Counterfactual
	(1)	(2)	(3)
High rate	11.744***	5.553**	12.547***
	(2.308)	(2.357)	(1.540)
Constant	5.040***	10.044***	5.349***
	(1.135)	(1.226)	(1.043)
Observations	124	212	212
R^2	0.142	0.029	0.149

Notes: OLS regressions, robust standard errors in Columns 1 and 2, standard errors clustered at the worker level in Column 3. The dependent variable is the number of tasks a worker completes. "High rate" is an indicator for high piece-rate (prospects).

[36] In addition, I recruited 56 workers for the *robustness "disappointment"* study (Amazon Mechanical Turk, US, February 2021). Workers in this condition do not make an effort choice. They have to complete exactly ten tasks.

G Research transparency

Preregistration The main study, the "equal rates" conditions, the attention condition, the "equal rates" attention condition, and the counterfactual study were preregistered as project #AEARCTR-0005811 at the AEA RCT Registry. The preregistration includes details on the experimental design, the full experimental instructions, thus the full list of measured variables, the sampling process and planned sample size, exclusion criteria, hypotheses, and the main analyses. The following notes document where I deviate from the preregistration.

- The preregistration uses a different title and different treatment labels.
- Non-preregistered analyses include the comparison of worker B's reward share, averaged across effort scenarios (a straight-forward summary of the scenario-by-scenario differences), and the structural estimation.
- Wherever I explicitly deviate from the analysis plan, I choose a more conservative approach. For instance, I do not adjust the treatment comparisons in each effort scenario for multiple hypothesis testing. This renders their non-significance even more conservative. The highly significant effects in the counterfactual study survive even conservative adjustments for multiple hypotheses testing.
- The sample size differs slightly from the pre-registered size of about 300 per condition due to the logistics of the sampling process.
- The preregistration defines the difference in payment shares $\Delta p = \frac{P_A}{P_A+P_B} - \frac{P_B}{P_A+P_B}$ as main outcome variable. In contrast, I use worker B's payment share $p = \frac{P_B}{P_A+P_B}$ as main outcome variable. Since both are linearly dependent ($p = \frac{1-\Delta p}{2}$), this difference does not affect the results but eases their interpretation.

The vignette study was not pre-registered.

Ethics approval The study obtained ethics approval from the German Association for Experimental Economic Research (#HyegJqzx, 12/11/2019).

Data and code availability All data and code will be made available online.

Competing interests I declare that I have no competing interests.

H Extract from the main study's instructions

This appendix shows the central experimental instructions from the main study. The full experimental instructions for all studies are available at https://osf.io/xj7vc/.

Part 1

In what follows, we will ask you to make a series of decisions that might have **consequences for a real-life situation**.

Please read the following pages very carefully. A **quiz** will test your understanding. You can proceed with the study only if you answer all quiz questions correctly.

→

– PAGE BREAK –

The context of your decision

Our institute currently hires adults from the US general public on an online job portal to work on an important task for one of our projects.

Task
These workers search for publicly available email addresses of academic economists. In each task, a worker is given the name of one economist, searches for the economist's personal or university webpage, identifies his or her email address and sends it to us.

The task requires no special qualification or ability, but demands concentration and effort. Typically, it takes about 2 minutes to complete one task.

Workers can freely choose how long they work and how many tasks they want to complete. At most, they can complete 50 tasks.

← →

The context of your decision

Payment
Each worker receives a fixed reward of $1.00 for completing the job as well as a variable payment. The variable payment depends on the number of completed tasks, a piece-rate, and your decisions in this survey. From now on, when we say "payment", we are only referring to this variable payment. It is calculated in two steps:

(1) A worker initially earns a fixed amount for each solved task. We refer to this amount per task as a piece-rate.

variable payment = number of tasks x piece-rate

For example, a worker who has a piece-rate of $0.20 and solves 10 tasks receives a variable payment of $2 (namely $0.20 x 10).

(2) Afterwards, someone else determines the final payments. Workers are informed about this, although they do not know how and why this happens.

This is where you come into play ...

←

→

Your decisions

In the last weeks, we hired 200 workers and matched them into 100 pairs. The decisions that you and others make in this study determine their final earnings. We randomly select one study respondent for each pair of workers.

If you are one of the selected respondents, **your decisions determine the final earnings of a pair of workers**. Let us call them *worker A* and *worker B*.

You can redistribute the payments between worker A and worker B. That is, you decide which share of the total payment amount each worker receives.

Example: Worker A receives a payment of $10 and worker B of $5 so that the sum of their payments is $15. You can freely choose how to distribute the total amount of $15 between both workers.

Completely anonymous: Please note that your decisions are completely anonymous. The workers will receive the shares that you choose with no further information. In particular, they will not learn anything about you or the nature of your decisions.

Multiple decisions - each might matter

We ask you to consider **8 different scenarios** corresponding to different possible work outcomes for worker A and worker B. 7 of those scenarios are hypothetical. 1 scenario is real and describes what actually happened when worker A and worker B worked on this task.

You will make **one distribution decision for each scenario**. If you are among the selected respondents, your decision in the real scenario is implemented and determines how much each worker earns. However, you will not be told which scenario really happened, so all of your decisions are important.

Therefore, please take each decision seriously. It might matter a lot to two real workers from the US.

← →

– PAGE BREAK –

The piece-rates

Recall that the piece-rates of the workers determine how much they initially earn for each task. In what follows, we explain how these piece-rates are determined.

← →

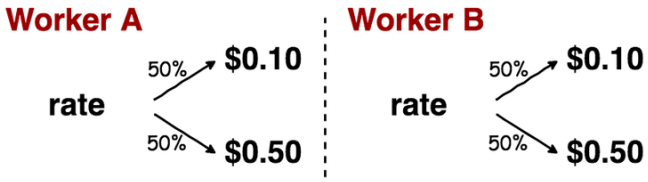
– INFORMATION FOR *CONTROL* GROUP –

The piece-rates

Please read the following information very carefully.

The piece-rate of each worker was determined randomly by a virtual coin flip. Each worker had a 50% chance to get a piece-rate of $0.10 and a 50% chance to get a piece-rate of $0.50. One coin flip determined the rate of worker A, and another coin flip determined the rate of worker B.

Thus, the workers had equal prospects to work for the low or the high rate.



Importantly, workers did not know during their work which piece-rate they would get. Only the chances of getting the rates were known. The coin was flipped only after a worker completed and submitted the job. Only then, a worker was informed about his or her definite piece-rate.

In the end, the coin flip determined the following definite rates:

- **Worker A** had a rate of **$0.50**.
- **Worker B** had a rate of **$0.10**.

Thus, they worked for a different rate, but they were informed about their rate only after they completed the job.

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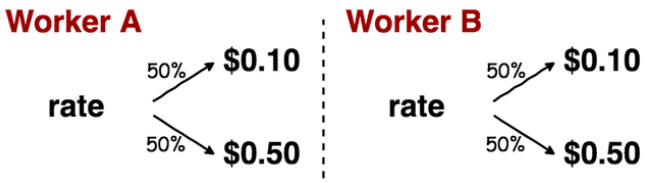
– INFORMATION FOR *TREATMENT* GROUP –

The piece-rates

Please read the following information very carefully.

The piece-rate of each worker was determined randomly by a virtual coin flip. Each worker had a 50% chance to get a piece-rate of $0.10 and a 50% chance to get a piece-rate of $0.50. One coin flip determined the rate of worker A, and another coin flip determined the rate of worker B.

Thus, the workers had equal prospects to work for the low or the high rate.



Importantly, workers knew which piece-rate they would get before starting their work. The coin was flipped before the workers started working and workers were informed about the result directly.

The coin flip determined the following definite rates:

- **Worker A** had a rate of **$0.50**.
- **Worker B** had a rate of **$0.10**.

Thus, they worked for a different rate.

←

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– EXAMPLE: REDISTRIBUTION DECISION FOR *CONTROL* GROUP –

Scenario 1

	Rate prospects (known to worker)	Final rate (unknown to worker)	Completed tasks	Initial payment
Worker A	$0.10 or $0.50 50% chance for each	$0.50	**45 tasks** 90% of total work	**$22.50** 98% of total payment
Worker B	$0.10 or $0.50 50% chance for each	$0.10	**5 tasks** 10% of total work	**$0.50** 2% of total payment
			Total payment:	$23.00

Please split the total payment between both workers.
To do so, please specify which share of the total payment each worker gets. The shares need to add up to 100%.

Share of **worker A**	0 %
Share of **worker B**	0 %
Total	0 %

– EXAMPLE: REDISTRIBUTION DECISION FOR *TREATMENT* GROUP –

Scenario 1

	Rate (known to worker)	Completed tasks	Initial payment
Worker A	$0.50	**45 tasks** 90% of total work	**$22.50** 98% of total payment
Worker B	$0.10	**5 tasks** 10% of total work	**$0.50** 2% of total payment
		Total payment:	$23.00

Please split the total payment between both workers.
To do so, please specify which share of the total payment each worker gets. The shares need to add up to 100%.

Share of **worker A**	0 %
Share of **worker B**	0 %
Total	0 %

I Extract from the vignette study's instructions

This appendix shows the scenario descriptions from the vignette study. The full instructions for the vignette study are available at https://osf.io/xj7vc/.

I.1 Scenario "discrimination"

Richard and Oliver work for the same company. In the last months, they competed for a promotion that came with an attractive one-time bonus of $10,000.

However, their boss is notorious for favoring white employees. In fact, he has never promoted a black person before, although he has had plenty of opportunities to do so.

Richard is white. He worked hard to win the promotion.

Oliver is black. He did not work hard to win the promotion.

Who got promoted?

As a consequence of their choices, Richard is promoted and receives the bonus of $10,000. Oliver is not promoted and receives no bonus.

[*Addendum, High counterfactual condition*]

What if the boss did not favor white employees?

Assume that if the boss did not favor white employees, Oliver would have made the same choice as Richard. **Oliver would have worked as hard as Richard did.**

[*Addendum, Low counterfactual condition*]

What if the boss did not favor white employees?

Assume that if his boss did not favor white employees, Oliver would still have made the same choice. **Oliver would not have worked hard.**

I.2 Scenario “poverty”

Mike

Mike grew up in a rich family. He was always told, “In this country, you can go as far as your hard work takes you.” His family expected him to work hard. Mike went to good, engaging schools that challenged him. He knew he would be popular among his peers if he achieved good grades and worked hard.

Mike has always worked hard in his life.

Paul

Paul grew up in a poor family. He was always told, “In this country, the poor stay poor, and the rich get richer.” His family did not expect him to work hard. Paul went to poor-quality schools where he was bored and never challenged. He knew he would be popular among his peers if he was lazy, rebelled against authority, and violated rules.

Paul has never worked hard in his life.

Income today

As a consequence of their choices, Mike earns $125,000 a year, and Paul earns $25,000 a year.

[*Addendum, High counterfactual condition*]

What if Paul had grown up in Mike’s environment?

Assume that if Paul had grown up in the same environment as Mike, he would have made the same choices as Mike. **Paul would always have worked as hard as Mike did.**

[*Addendum, Low counterfactual condition*]

What if Paul had grown up in Mike’s environment?

Assume that if Paul had grown up in the same environment as Mike, he would still have made the same choices. **Paul would never have worked hard in his life.**

I.3 Scenario “start-up”

Frank

Frank always dreamed of founding his own software start-up. He knew that he would **inherit a considerable fortune**. Therefore, he knew that he had enough money to launch his start-up, and that even if his first attempts failed, he would have enough money left to try again and pursue a new business idea.

Frank decided to take the risk and founded his own software start-up.

Ray

Ray always dreamed of founding his own software start-up, too. However, Ray’s parents were poor and he had **very little money**. Therefore, he knew that it would be difficult to find enough money to launch a start-up, and he knew that if his first attempt failed, he would be broke.

Ray decided not to take the risk. Instead, he works as a software developer for a local company.

Income today

As a consequence of their choices, Frank earns $200,000 a year, and Ray earns $50,000 a year.

[*Addendum, High counterfactual condition*]

What if Ray had had as much money as Frank?

Assume that if Ray had had as much money as Frank, he would have made the same choices as Frank. **Ray would have taken the risk and founded his own software start-up.**

[*Addendum, Low counterfactual condition*]

What if Ray had had as much money as Frank?

Assume that if Ray had had as much money as Frank, he would still have made the same choices. **Ray would have decided not to take the risk. Instead, he would work as a software developer for a local company.**

I.4 Scenario "crime"

Robert

Robert grew up in a rich neighborhood with very low crime rates. His parents made sure he grew up in a loving, nurturing environment. Robert has always been told, "In this country, you can rise as far as you want if you play by the rules." Robert went to good, engaging schools that challenged him. Many of his peers planned to study at a university.

Robert started studying business at the age of 20. Today, he works as salesman. He never does anything illegal.

John

John grew up in a poor neighborhood with very high crime rates. His parents often neglected him. Once his father was caught selling drugs and had to spend several years in jail. John has always been told, "Playing by the rules means nothing when the rules are stacked against you." He went to poor-quality schools where he was bored and never challenged. Many of his peers had already committed crimes by the time they reached their teenage years.

John committed his first crime at the age of 20. Today, he sells drugs. He frequently violates the law.

Income today

As a consequence of their choices, Robert earns $50,000 a year, and John earns $50,000 a year.

[*Addendum, High counterfactual condition*]

What if John had grown up in Robert's environment?

Assume that if John had grown up in the same environment as Robert, he would have made the same choices as Robert. **John would never do anything illegal.**

[*Addendum, Low counterfactual condition*]

What if John had grown up in Robert's environment?

Assume that if John had grown up in the same environment as Robert, he would still have made the same choices. **John would sell drugs and frequently violate the law.**