Welfare Stigma in the Lab: Evidence of Social Signaling

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Abstract

Using a laboratory experiment, we present first evidence that stigmatization through public exposure causally reduces the take-up of an individually beneficial transfer. Our design exogenously varies the informativeness of the take-up decision by varying whether transfer eligibility is based on ability or luck, and how the transfer is financed. We find that subjects avoid the inference both of being low-skilled and of being willing to live off others. Using a placebo treatment we can exclude other explanations for the observed stigma effect. In the experiment, social stigmatization implies a reduction in the take-up rate of 30 percentage points.

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1 Introduction

Modern welfare states are characterized by large-scale welfare programs. However, many individuals surprisingly fail to take-up social benefits for which they are eligible (Currie, 2006). For instance, in the US about 37.7 percent of eligible individuals do not claim Supplemental Security Income (US Department of Health and Human Services, 2016), and for every three German welfare recipients it is estimated that two to three eligible individuals do not claim their transfers (Bruckmeier and Wiemers, 2012). Also nutritional assistance is traditionally characterized by incomplete take-up, be it in the form of food stamps (Haider *et al.*, 2003; Pinard *et al.*, 2017) or free school lunches (Bhatia *et al.*, 2011). From a neoclassical perspective, the incomplete take-up of welfare transfers is puzzling because individuals appear to leave money on the table and because non-take-up may have severe negative effects on long-term life outcomes (Hernanz *et al.*, 2004). Social scientists and policymakers regularly relate incomplete welfare take-up to the negative attitudes that society is believed to have about welfare claimants (stigma hypothesis). This idea is incorporated in many economic models, including prescriptive and normative analyses of public policy (Moffitt, 1983; Besley and Coate, 1992; Yaniv, 1997; Lindbeck *et al.*, 2003; Blumkin *et al.*, 2015). However, empirical evidence of the existence of welfare stigma effects on take-up behavior is surprisingly scarce (for a survey, see, e.g., Andrade, 2002; Currie, 2006).[1]

This paper is the first to provide causal evidence for the stigma hypothesis in a controlled laboratory experiment. We propose an empirical strategy that takes advantage of the link between welfare stigma and public inference in the controlled experimental setting. Specifically, we exogenously vary the degree of public exposure to a social transfer to identify the effect of stigmatization on transfer take-up. Further, we conduct a set of treatments that affect the informational content of claiming a transfer and can thereby disentangle two different sources of stigmatization. First, we vary whether eligibility is based on poor performance in a knowledge quiz or based on luck to show that stigma is caused by inferences about the claimant's inferior ability. Second, we vary whether the transfer is financed by contributions from other subjects or by the experimenter to show that stigma is also caused by inferences about the claimant's willingness to live off others. Finally, we analyze individuals' preferences about the transfer payment mode by letting individuals vote whether or not the transfer should be made publicly visible.

Our results reveal a significant and economically sizable stigma effect on transfer take-up: when transfer eligibility is based on quiz performance, subjects are 30 percentage points less likely to take a public, as compared to a private, redistributive transfer, even though the

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[1]Anecdotal evidence supports the practical relevance of welfare stigma. While attempts to reduce stigmatization have indeed contributed to soaring take-up rates of nutritional assistance in the US (The New York Times, 2009; Gray and Cunnyngham, 2016), stigma continues to be an issue at food banks in the UK (Garthwaite, 2016).

transfer amounts to a 50 percent increase in their payoff. When we reduce the informational content of the take-up decision by letting eligibility depend on luck instead of performance, we still observe a sizable and significant stigma effect. However, it is significantly lower than in the quiz treatment. In line with our theoretical framework, these results suggest that subjects avoid the inference both of being low-skilled (ability signaling) and of being willing to live off others (moral signaling). Using a set of treatments in which claiming a transfer does not reduce other participants' income, we show that the stigma effect and its two components are indeed driven by signaling motivations. In particular, transaction costs or meritocratic beliefs do not play a role. Results from a post-experimental questionnaire also confirm the social signaling interpretation as the treatments affect what subjects infer about a subject who claims a transfer in public.

The results from the voting stage reveal that more than half of those who pay for the transfer vote for the private transfer mode. Thus, they appear to have a positive willingness to pay to reduce welfare stigma as their own monetary payoff would be maximized by voting for the public mode.

Our paper contributes to the literature on program participation and welfare take-up by providing direct causal evidence for the existence of welfare stigma in a controlled laboratory experiment. While our experiment only captures the most important aspects of the welfare take-up decision, it allows us to identify the signaling mechanism that drives stigmatization and to distinguish the stigma effect from transaction costs. In previous economic studies, the relevance of stigma is often inferred from self-reports[2] or from an observed positive correlation between the amount of the benefit to which an individual is entitled to and the probability of welfare take-up.[3] Indeed, a higher benefit is more likely to compensate an individual for the experienced stigmatization and will result in a higher take-up rate. However, an increase in benefit size will increase take-up in any situation where transaction costs contribute to incomplete take-up and where the transaction cost is not increasing in the level of the benefit.[4]

By showing that social signaling concerns can be an important factor in an individual's decision about program participation, our paper relates to the growing interest in economics of how psychological factors affect program participation (see, e.g., Madrian and Shea, 2001; Bhargava and Manoli, 2015; Liebman and Luttmer, 2015),[5] which so far focuses on the role of

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[2]In survey studies, non-participants often state program stigma as one reason why they have not applied for the benefits that they are eligible for. For instance, see Bartlett and Burstein (2004) for self-reported stigma in the Food Stamp Program.

[3]See, e.g., Blundell *et al.* (1988) for housing benefits; Blank and Ruggles (1996), for family aid and nutritional assistance; Currie (2000), for public health care; and Whelan (2010), for social assistance payments.

[4]Examples of such transaction costs include the time needed to find the appropriate public authority or the hassle costs associated with filling out application forms.

[5]The field experiment by Bhargava and Manoli (2015) includes a stigma treatment in which the reminder letter sent to tax filers emphasized that the benefits of the EITC are an earned consequence of hard work rather than a welfare transfer. This attempts to affect the image associated with being an EITC claimant, in line with a notion of moral stigma. However, it is not clear whether the headline affects the signaling problem

behavioral deficiencies in incomplete take-up such as low program awareness, misperception of program features, and inattention. By design, these factors are excluded in our experiment in order to focus on the signaling aspect of stigmatization.[6] Our results suggest that individuals might be fully aware of certain program features, but that they deliberately refrain from participation in order to avoid public inferences about their type. This finding provides a rationale for partial take-up of welfare benefits even in programs that are well-known and well-established, such as Medicaid, Temporary Assistance for Needy Families (TANF), and the Supplemental Nutrition Assistance Program (SNAP) in the US (see, e.g., Currie, 2006). Furthermore, the finding that stigmatization contains a moral signaling element that does not relate to individual performance suggests that stigmatization may also contribute to low take-up rates in programs that are not means-tested. As these programs do not condition eligibility on individual economic performance and, thus, do not signal anything about claimants' earnings opportunities or wealth, the existing literature denies the existence of stigmatization for such schemes (Currie, 2006).

Our paper also relates to a growing experimental literature that studies how social signaling or image concerns affect economic decisions.[7] Our public exposure treatment is similar to a treatment manipulation used for instance in Ewers and Zimmermann (2015), who show that individuals desire signaling high ability by overstating performance,[8] and by McManus and Rao (2015), who find that individuals choose a more difficult task to signal higher intelligence when they have an audience. Our contribution to this literature is twofold. First, we show that signaling concerns also affect individuals' incentives in the welfare state, both when they decide about their own decision to take up a welfare transfer but also when they decide about the design of the take-up situation for others. Second, we show that signaling in the take-up situation involves two dimensions, ability and moral signaling, which we can separate from

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as the decision problem remains fundamentally unchanged. Furthermore, receiving support within the EITC is typically unobserved by the public and, therefore, associated with relatively little stigma – quite in contrast to more visible and stigmatized assistance such as food stamps or public housing. Thus, we find it unsurprising that this treatment, in contrast to interventions targeting informational challenges of potential claimants, was ineffective.

[6]Our experimental design also abstracts from peer effects because individuals do not know who is claiming a transfer when they decide about their own take-up. While positive peer effects may be driven by claimants feeling less stigmatized if more of their peers claim the same type of transfer, such effects can alternatively be explained by network information spillovers. Evidence suggests that peer effects indeed influence program participation. For instance, Bertrand *et al.* (2000) provide evidence that welfare take-up increases when the number of people in one's area speaking one's language is higher, and this effect is larger for individuals from high welfare-using language groups. Dahl *et al.* (2014) find that the paternity leave decision of fathers who face lower costs of paternity leave as a result of a policy reform will affect the paternity leave decision of their co-workers and brothers untreated by the reform.

[7]Individuals are found to signal pro-social behavior in charitable giving and public good situations in the lab (e.g. Andreoni and Bernheim, 2009; Ariely *et al.*, 2009), in the field (e.g. Rege and Telle, 2004; Soetevent, 2011), and to signal pro-social and sustainable attitudes in purchasing situations (Sexton and Sexton, 2014; Friedrichsen and Engelmann, 2017).

[8]Reporting performance truthfully is the choice that maximizes monetary payoffs in Ewers and Zimmermann (2015).

each other by systematically varying the informational content of the decision to take up the transfer.[9]

Furthermore, we add to the literature that uses incentivized laboratory experiments to investigate preferences for redistribution and taxation with its potential interplay with social motives (see, e.g., Tyran and Sausgruber, 2006; Durante *et al.*, 2014; Agranov and Palfrey, 2015). In contrast to these papers, we do not investigate what determines the preference for (the level of) redistribution *per se* but the circumstances under which transfers are paid.[10] We find that a large fraction of those subjects who effectively pay for the transfer, reveal a preference for keeping welfare claiming anonymous.

Our experimental findings on the importance of social stigmatization for welfare take-up can serve as an input for the political discussion about the design and organization of welfare programs. If the policy maker aims to achieve complete take-up within the target group, our results suggest that social welfare services should be both claimable and paid out discretely.[11] For instance, authorities may prefer waiting rooms in which individuals with different types of requests (e.g., claiming welfare benefits, getting a new passport, registering a newborn) are pooled such that the mere presence in the room does not allow for an inference about the individual's request. The results also provide an argument for cash rather than in-kind transfers because the former are typically less visible and therefore less prone to welfare stigma. If in contrast, the policy goal is to reduce the take-up rate of a program, our analysis implies that social transfers should be made less anonymous.[12]

The paper proceeds as follows. We develop a theoretical framework for welfare take-up in the presence of social signaling concerns in section 2. Based on this framework, we discuss our experimental design in section 3 and develop several testable predictions. We present our results with respect to individual take-up decisions in section 4 and discuss individual voting behavior in section 5. Section 6 concludes.

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[9]Experimental studies on two-dimensional signaling are extremely rare. An exception is Bracha and Vesterlund (2017), who analyze how individual donations are affected by the desire to the signal income and generosity, showing that both are important. Bursztyn and Jensen (2017) show that students desire to signal both high ability and low effort.

[10]Our participants cannot influence transfer size or eligibility but they indirectly decide about the realized level of redistribution because the take-up rate differs systematically between private and public.

[11]For many policymakers, improving the take-up of social programs is an unequivocal objective. E.g., the European Commission (2015) refers to the need to reduce non-take-up in its proposed Council Decision on guidelines for the employment policies of the Member States: "Social protection systems should be designed in a way that facilitate take up of all persons entitled, support investment in human capital, and help prevent, reduce and protect against poverty."

[12]Blumkin *et al.* (2015) analyze theoretically how welfare stigma can be used as an ordeal mechanism that may improve the target efficiency of a welfare system.

2 A theoretical framework of welfare take-up and social stigma

In this section, we develop a framework for the individual decision to take up a welfare benefit under signaling concerns that allows us to derive testable hypotheses.

Suppose an individuals' utility is given by

$$u(c, t, s, \delta) = c + \delta t - \delta(s + \alpha)$$

where c is the level of consumption without a transfer, t is the transfer, $\delta \in \{0, 1\}$ denotes the decision whether or not to take up a transfer if eligible, s denotes the stigma costs associated with taking up the transfer, and α is the moral disutility associated with receiving a transfer. This disutility may reflect attitudes with respect to earned entitlements and redistribution.

Assumption 1. *Assume that α is distributed according to a distribution function F, which is continuous, differentiable, and strictly increasing over its support $[0, A]$. Denote the associated density by $f(\cdot)$.*

The stigma costs, $s = aR_A + bR_M$, are increasing to the extent that taking up the transfer is associated with ability stigma, $R_A \geq 0$, or moral stigma, $R_M \geq 0$,[13] and the parameters $a > 0$ and $b > 0$ are the marginal disutilities associated with ability stigma and moral stigma, respectively.[14]

The ability stigma term R_A captures the idea that individuals may feel stigmatized because taking up a transfer reveals that they are less able; i.e., the decision to take the transfer may signal inferior ability because only less able individuals are eligible for the transfer. The moral stigma term R_M accounts for the fact that individuals may feel stigmatized because taking up the transfer reveals that they are willing to live off others; i.e., the take-up decision reveals something about an individual's moral attitude toward receiving money from others as measured by α.[15] An extended model can include the role of laziness or effort signaling for the take-up decision in a similar way.[16] As the take-up decision in our experiment is only informative about ability, we abstract from effort in the model.

We specify these stigma terms as depending on the expected deviation from the unconditional expectation of an individual's ability, θ, after observing the take-up decision in case of ability stigma, and as a function of the difference between the unconditional expectation of α

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[13]By definition, the stigma terms are always positive if some, but not all, individuals decide to take up the transfer. They are null if nobody or everyone takes the transfer.

[14]Our modeling of social stigma follows the same logic as models of social image concerns in the context of pro-social behavior: see, for instance, Bénabou and Tirole (2006).

[15]Our notion of ability signaling relates to statistical stigma in Besley and Coate (1992) and our notion of moral signaling bears similarity with their concept of taxpayer resentment. Relatedly, Stuber and Schlesinger (2006) discuss identity-related stigma as opposed to treatment stigma. See also Rainwater (1982).

[16]Since effort is a choice variable itself, the model would then have to account for the trade-off between the disutilities of effort and laziness signaling. Therefore, the model would need to incorporate the effort choice explicitly.

and the expectation of an individual's moral attitude α conditional on the take-up decision in case of moral stigma. Both stigmata also depend on the degree of public exposure λ. We assume there are two functions $h_1(\cdot)$ and $h_2(\cdot)$, increasing in both arguments, such that

$$R_A(\delta, \lambda) = h_1(E[\theta] - E[\theta|\delta = 1], \lambda) \quad \text{and} \quad R_M(\delta, \lambda) = h_2(E[\alpha] - E[\alpha|\delta = 1], \lambda)$$

We assume that an individual will not experience stigma if she decides not to take up the transfer or if the take-up decision remains private ($\lambda = 0$), such that $R_A(0, \cdot) = R_A(\cdot, 0) = R_M(0, \cdot) = R_M(\cdot, 0) = 0$. Note that her private moral concerns α may still lead her to not take up a transfer of size t in a private situation if $t < \alpha$. On the other hand, take-up behavior is informative of an individual's ability θ and her moral attitude α if the take-up decisions are public such that stigma exists in a public situation. As R_A and R_M are increasing in λ, higher public scrutiny intensifies the feeling of being stigmatized.[17]

The decision of an individual with moral attitude α to take up the transfer ($\delta = 1$) or not ($\delta = 0$) depends on the trade-off in utilities and the individual will take the transfer if doing so yields at least the same utility as not taking it. Denote consumption without the transfer by c and the transfer by t. Then, for everyone claiming the transfer it must be true that $u(c + t, s, \delta = 1) \geq u(c, s, \delta = 0)$. This expression is equivalent to

(1) $$\alpha \leq t - aR_A(\lambda) - bR_M(\lambda)$$

If equation 1 is fulfilled for all α, all individuals claim the transfer independent of their moral attitude. Similarly, if there is no α for which 1 holds, no individual will claim the transfer.[18] To focus on the interesting cases, we assume that the tradeoff is negative for some α and positive for others.[19] Define $G(\alpha) = t - aR_A(\lambda) - bR_M(\lambda) - \alpha$. We make two technical assumptions.

Assumption 2. *Assume that there exist* $\alpha, \alpha' \in [0, A]$ *such that* $G(\alpha) < 0 < G(\alpha')$.

Assumption 3. *Assume that the distribution of moral attitudes fulfills* $f(\alpha) < (b\alpha)^{-1}$ *for all* $\alpha \in [0, A]$.[20]

Assumption 2 implies that an individual with moral attitude $\tilde{\alpha}$ exists who is just indifferent between taking up the transfer and not taking it. Assumption 3, ensures that the threshold

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[17]At this point, we do not take a stance on whether this increase in stigmatization depends on the signal about an individual's type from the take-up decision becoming more precise or because stigmatization is felt more intensely. In our experimental design, we control the informativeness of the take-up decision so that publicity works exclusively through the way individuals feel stigmatized for a given signal.

[18]Alternatively, one could, for each individual attitude α, find the smallest cutoff level of the transfer that this individual would be willing to take depending on visibility.

[19]This assumption is also consistent with our experimental results showing that take-up is neither zero nor complete. See the results section below.

[20]This assumption is, for instance, fulfilled if moral attitudes are uniformly distributed on $[0, 1]$ and the marginal utility from moral stigma is less than one, $b < 1$.

value determined by equation 1 is unique. Then, the trade-off in equation 1 defines a unique cutoff value $\tilde{\alpha}$ such that all individuals with $\alpha \leq \tilde{\alpha}$ claim the transfer and those with $\alpha > \tilde{\alpha}$ do not claim the transfer.[21] The implied take-up rate is given by the fraction of individuals with a moral attitude below the threshold, i.e., $F(\tilde{\alpha})$.

Proposition 1. *Individual behavior is characterized by a cutoff strategy. For a cutoff value $\tilde{\alpha}$ implicitly defined by $G(\tilde{\alpha}) = 0$, individuals with $\alpha < \tilde{\alpha}$ take the transfer, and individuals with $\alpha > \tilde{\alpha}$ do not take the transfer. The take-up rate is given by $F(\tilde{\alpha})$.*

Using implicit differentiation, we analyze how the threshold value and, thus, the take-up rate changes in response to changes in the economic trade-off. These comparative statics are the basis for our design and the following three corollaries are the foundation for the testable predictions presented later on. Denote the threshold value of moral attitude for a given environment by $\tilde{\alpha}(\lambda, \gamma, t)$.

Corollary 1. *Take-up of the transfer is lower if the decision to take up the transfer is more visible, $F(\tilde{\alpha}(\lambda', \gamma, t)) < F(\tilde{\alpha}(\lambda, \gamma, t))$ for $\lambda' > \lambda$.*

Denote the difference in take-up due to increased visibility by
$\Delta = F(\tilde{\alpha}(R_A(\lambda), R_M(\lambda), t)) - F(\tilde{\alpha}(R_A(\lambda'), R_M(\lambda', t)))$. This is the "stigma effect", and it depends on the size of both ability and moral stigma.

Corollary 2. *The stigma effect is larger if take-up is more informative about ability, i.e., if ability stigma is larger, $\Delta(R_A', R_M) > \Delta(R_A, R_M)$ for $R_A' > R_A$.*

Finally, the moral stigma component captures that individuals may be reluctant to claim a transfer in public because they do not want to appear to be taking other people's money. This moral stigma, denoted by R_M in the model, depends on how morally appropriate individuals think it is that an individual who is formally entitled to claim a transfer actually does so. We argue that the moral appropriateness of a transfer may change with its type of financing (in particular, the degree of redistribution involved) and with perceptions of entitlement that will differ if income differences are based on different performance or are random. Changes in the appropriateness of taking up a transfer are reflected in a changing distribution of α so that the take-up rate changes for a given $\tilde{\alpha}$. Denote by γ the degree of redistribution involved, a higher γ meaning a more redistributive transfer. We make the following assumption:

Assumption 4. *Assume that $F_{\gamma'}(\alpha) \leq F_\gamma(\alpha)$ for all α whenever $\gamma' \geq \gamma$.*

Corollary 3. *The stigma effect is larger if the transfer is more redistributive, i.e., if moral stigma is larger, $\Delta(R_A, R_M(\gamma')) > \Delta(R_A, R_M(\gamma))$ for $\gamma' > \gamma$.*

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[21] We assume that the transfer is taken up in case of indifference, but, as we assume a continuous distribution of types, this assumption is immaterial to our results.

If the financing becomes more redistributive, individuals become, on average, more morally concerned and, therefore, fewer individuals will claim the transfer. This change in take-up behavior also affects the stigma effect because individuals do not want to be seen as redistributing money to their own advantage.

3 Experimental design and hypotheses

In light of our theoretical model, we develop a 2 x 2 x 2 design to cleanly test whether stigma contributes to low take-up rates and to disentangle possible sources of stigma in the laboratory. The treatment variations are the following. First, we implement possible stigmatization within-subject by varying whether the take-up of a transfer is public or private. Second, we vary between-subject the informational content of take-up by letting eligibility depend on quiz performance or randomness. Third, we vary between-subject whether the transfer is redistributive or a subsidy by the experimenter. This design allows us to cleanly test several hypotheses that we develop below and that are based on the theoretical framework developed in section 2.

3.1 General setup

The experiment consists of three stages: (1) a general knowledge quiz; (2) the decision whether to claim a transfer for both a private and a public scheme; (3) a vote about whether the transfer should be paid out in private or in public.

In stage (1), all participants take part in a multiple choice quiz with 18 general knowledge questions.[22] Participants have six minutes to decide upon their answers. Each correctly answered question is rewarded with one point, while wrongly answered questions or those unanswered receive zero points. Depending upon the treatment introduced below, the instructions for stage (1) contain information on whether quiz performance will determine ranks or if they are allocated randomly and that payout is based on payout schedule A or B (cf. Table 1) conditional on decisions in later stages. We elicit beliefs about each participant's performance in the quiz directly after the quiz, and participants receive no feedback at this stage.

rank	A	B redistribution	subsidy
1	16 Euro	14 Euro	16 Euro
2	11 Euro	10 Euro	11 Euro
3	6 Euro	9 Euro	9 Euro

Table 1: Payoffs schedule

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[22]The translated quiz is provided as a screenshot in the appendix.

In stage (2), we use the strategy method and ask each participant to decide whether or not to take up a transfer if he would rank third. A participant's decision is implemented if he ends up being ranked third. Taking up the transfer implies that in the respective group, payment schedule B is used instead of payment schedule A. When taking their decisions, participants do not know whether they would be eligible but feedback about the ranking is given to participants only after they have made their take-up decisions.

In stage (3), after the rank has been revealed, each subject decides whether the private or the public treatment should become payoff-relevant. In each group, the decision of one group member is drawn at random and is implemented for the respective group (random dictator voting).

Note that the instructions for stages (2) and (3) are only provided to the subjects after stage (1) is over. None of the instructions include information about the public or private transfer regime but these details are only given on-screen.[23]

3.2 Treatments

Public vs. private We vary the visibility of the transfer by making transfer take-up public or private. In the public treatment, claiming a transfer requires the participant to walk through the lab and pick up a slip of paper at the experimenter's desk. In the private treatment, claiming a transfer only requires to indicate the decision on the screen to have it included in the experimental payout. We use the strategy method to elicit the take-up decision from each participant for both the private and the public condition. All participants decide sequentially, on two separate screens, whether they want to claim the transfer if it was paid out in private and if it had to be claimed publicly. The order of decisions is randomized at the group level to control for possible order effects.[24] The instructions emphasize that the take-up decision is binding, i.e., in case the participant is actually ranked third, the previously made decision will be executed. We ensure that all subjects adhere to their public take-up decision by asking them to enter the number written on the slip of paper in the computer program. The session can only continue once all public transfer claimants have entered this number.

Quiz vs. random We vary the informational content of the take-up decision by allocating ranks either according to quiz performance or randomly. In the quiz treatment, participants are informed in the first part of the instructions that their ranks will be determined by the

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[23]We do not reveal the private/public difference in the instructions in order to mitigate a possible demand effect. Therefore, we also randomized the order of decisions, see discussion below.

[24]We do not find evidence of order effects in the take-up rates, thus refuting the hypothesis that our results are driven by a demand effect. We discuss the robustness of our results in more detail in section 4.4.

number of points achieved in the quiz.[25] In the random treatment, they are informed that ranks are determined randomly and not affected by quiz performance.[26] We chose the general knowledge quiz as it generates an informative signal about the ability of the participant without being very susceptible to differences in effort as the time budget was very generous.[27]

Redistribution vs. subsidy While the transfer is redistributive in the majority of sessions, we also ran a set of sessions where the transfer is paid by the experimenter without affecting the payoffs of better ranked group members. We discuss below that this treatment variation allows us to cleanly test for moral signaling and to control for confounding explanations. In both treatments, participants are informed that claiming a transfer leads to payment schedule B in Table 1 but which schedule B is shown alternates between treatments. The instructions for stage (1) and the control questions are adjusted accordingly but everything else remains unchanged.

3.3 Hypotheses

Based on the previous literature and our theoretical framework, we expect that public exposure influences take-up decisions because individuals feel stigmatized if they have to publicly reveal that they intend to claim a redistributive transfer (cf. corollary 1). Public exposure has an effect if at least one of the marginal disutilities a and b in our model is positive.

Prediction 1 (Stigma effect). *If social signaling matters, take-up rates in the quiz-redistribution treatment are higher in the private than in the public setting.*

According to our theoretical framework, the stigma effect persists in the random treatment where the ability signal is muted because public take-up of a redistributive transfer is associated with a negative inference on moral attitudes (R_M); i.e. individuals are reluctant to be perceived as taking money from others ($b > 0$).

Prediction 2 (Moral signaling I). *If individuals care about signaling of moral attitudes, a stigma effect persists in the random-redistribution treatment.*

However, unlike in the random treatment, in the quiz treatment take-up is informative about an individual's ability. Due to this scope for negative inferences about a claimant's ability (R_A) we predict that the quiz treatment induces more severe stigmatization (cf. corollary 2) because individuals do not want to be perceived as less able ($a > 0$).

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[25]In each group, the participant with the highest number of points is ranked first, the one with the second highest number of points second, and the one with the lowest number third. Ties are broken randomly.

[26]Participants work on the knowledge quiz irrespective of being in the quiz and random treatment. Thus, all sessions last the same amount of time and potential outcome differences are not driven by differences in opportunity costs of time.

[27]A discussion about the ability signal induced by the quiz is provided in Section 4.4.

Prediction 3 (Ability signaling I). *If individuals care about ability signaling, the stigma effect is larger in the quiz treatment than in the random treatment.*

Finally, we can use the difference in the income-generating process between the quiz and the random treatment to assess whether individuals have a *meritocratic attitude*. We say that an individual has a meritocratic attitude if she respects performance based income, i.e., if income is based on luck instead of performance then she finds it more morally acceptable to claim a transfer.[28] In terms of our theoretical framework, meritocratic attitudes at the population level are reflected by the distribution of α shifting to lower values when income is random instead of performance-based, so that $F_{\text{random}}(\alpha) \geq F_{\text{quiz}}(\alpha)$ for every α.

Prediction 4 (Meritocratic attitudes). *If individuals have meritocratic attitudes, the take-up rate of a redistributive transfer in private is higher in the random treatment than in the quiz treatment.*[29]

The subsidized treatments, in which claiming the transfer does not affect the payout of others, allow us to control for transaction costs and the signaling of meritocratic attitudes in the take-up decision.

First, if some individuals dislike public exposure or do not want to stand up and walk through the lab (summarized as transaction costs), take-up will be lower in public than in private irrespective of any signaling concern. Consequently, we would expect a public-private gap, even if ranks are drawn randomly and the transfer is a subsidy. However, according to our theory, neither ability nor moral signaling should have bite in this case, such that we predict no stigma effect.

Prediction 5 (Placebo test). *If transaction costs do not play a role, the stigma effect disappears when the transfer is a subsidy and ranks are random.*

Second, as described above, take-up in the redistribution treatments carries a moral signal in both the quiz and the random treatment. We use the stigma effect in the random treatment as a measure for moral signaling since ability signaling cannot play a role. But when we change from quiz to random, we not only change what can be inferred about an individual's ability from taking the transfer. In addition, the income-generating mechanism is different, which may affect the moral appropriateness of claiming a transfer. We argued above that

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[28]We chose the term "meritocratic attitude" because quiz performance reflects ability or skills and resulting income differences may be regarded as justified. See, for instance, Alesina and Angeletos (2005) who point out that whether inequality is based on luck or ability determines demand for redistribution. Experimental evidence also suggests that many people favor redistribution when inequality is due to luck but much less so when inequality results from individual choices (Cappelen *et al.*, 2013).

[29]Note, that meritocratic attitudes would also decrease public take-up in the quiz treatment but the take-up decision in public may also be affected by the signaling of meritocratic attitudes as discussed below. To test for meritocratic attitudes *per se*, we therefore focus on the treatment difference between quiz and random in the private transfer regime.

	redistribution	subsidy	Description
quiz	7 sessions, 165 subjects	3 sessions, 69 subjects	rank based on quiz performance
random	7 sessions, 159 subjects	2 sessions, 48 subjects	rank determined randomly
Description	transfer to rank 3 is paid by better rank subjects	transfer to rank 3 is paid by experimenter	

Table 2: Numbers of sessions and subjects per treatment. Each subject takes part in both the private and the public treatment.

such meritocratic attitudes would lead to a shift in the take-up levels in the private setting (see prediction 4). In the public setting, this same level effect should be present but in addition individuals may want to signal that they honor entitlements that have been earned in a competitive environment (*signaling of meritocratic attitudes*). In this case, our previous estimate of the ability-related stigma effect would be biased upwards. By comparing the observed stigma effects in the two redistribution treatments with those in the two subsidy treatments, we can test whether individuals indeed try to signal a meritocratic attitude.

Prediction 6 (Meritocratic signaling)**.** *If individuals desire to signal a meritocratic attitude, the difference in the stigma effects between quiz and random is smaller in the subsidized treatments than in the redistribution treatments.*

3.4 Procedures

The experiments were carried out at Technical University Berlin between November 2015 and June 2016. The experimental software was programmed using z-Tree (Fischbacher, 2007) and subjects were recruited using ORSEE (Greiner, 2015). In total, 441 subjects took part in 14 sessions of 24 subjects and five sessions of 21 subjects.[30] The number of sessions and subjects per treatment is summarized in Table 2. Sessions lasted 45 to 60 minutes each and participants earned, on average, 11.24 Euros.

Upon entering the laboratory, subjects were randomly allocated a cubicle and asked to carefully read the experimental instructions for stage (1) and work on the quiz task. After the quiz had ended, participants received instructions for stages (2) and (3) of the experiment, including a set of control questions. The experiment only started once everyone had correctly

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[30]The approximate number of necessary subjects for the redistributive treatments was determined by a power analysis. The effect size used for the calculation is based on a pilot study with a similar design that one of the co-authors conducted for his Master thesis (see Schmacker, 2015).

answered all questions. After the end of the experiment, we administered a post-experimental questionnaire while preparing for payment. Payments were made individually in a separate room.

4 Experimental results

Subjects answered an average of 9.57 questions in the quiz correctly where the minimal score was 3, i.e., all subjects worked on the task. The quiz task was able to differentiate well between the ranks: in the quiz treatment subjects in rank 1 answered with 11.82 significantly more questions correctly than those in rank 2 with 9.52 questions ($t(154) = 8.375, p < 0.001$) who themselves answered more correctly than those in rank 3 with 7.44 questions ($t(154) = 7.859, p < 0.001$). There are no statistical differences to the random treatment with respect to the number of questions answered correctly, although in this treatment subjects were told that ranks did not depend on quiz performance. Moreover, the participants did not differ across treatments with respect to any demographic characteristic that we elicited.[31]

To test our hypotheses, we investigate the take-up behavior as elicited by the strategy method, meaning that the actual number of claimants is not relevant for the analysis.[32] We first show that public exposure significantly reduces the take-up rate of a redistributive transfer when eligibility is based on quiz performance. Having established the existence of a stigma effect, we decompose it into effects related to ability signaling and to moral signaling by looking at a treatment in which eligibility is based on a randomly drawn rank. Then, we present results from the subsidized treatments that allow us to separate ability and moral signaling from other possible explanations like meritocratic considerations and transaction costs.

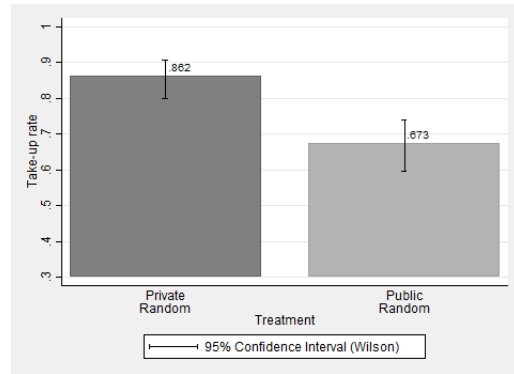## 4.1 Evidence of welfare stigma

The *quiz redistribution treatment*, in which income is based on quiz performance and transfers come at a cost to other participants in the same experiment, mirrors two crucial features of the decision to participate in a welfare program. First, eligibility for social benefits is typically based on criteria that are informative about the claimant's performance in a competitive environment. Second, welfare benefits must be financed: they reduce consumption possibilities of other society members and often involve redistributing money from upper and middle class

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[31]Table 6 in the Appendix displays descriptive statistics of the sample. Fifty-nine percent of subjects are male, subjects are an average of 24 years old, nearly all are students, and almost thirty percent are also working. Twenty-four percent are enrolled in a subject related to economics (economics, business, industrial engineering) and none had ever participated in more than three experiments.

[32]In 126 out of 147 groups a transfer was claimed, where 35 transfers were given out under the stigma regime and 91 under the private regime. In all but two sessions there was at least one claimant who received a public transfer.
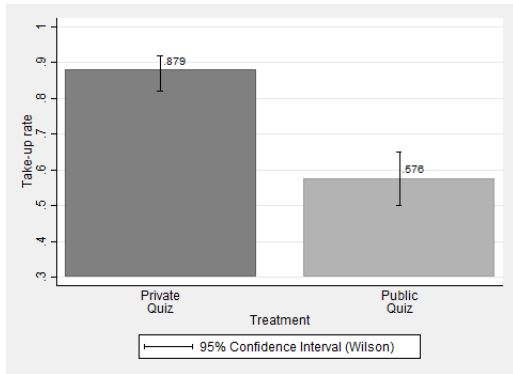
(a) quiz treatment (n=165). (b) random treatment (n=159)

Figure 1: Take-up rate by transfer regime (redistribution treatments)

individuals (taxpayers) to poorer ones (benefit recipients). This treatment acts as the benchmark setting in which stigma may run through different channels, and subsequent treatments will address these channels one by one.

Figure 1a illustrates the take-up rates under the private and public transfer regime in the quiz redistribution treatment. The left bar shows that 87.9 percent of subjects decide to take up the transfer if it was private, whereas only 57.6 percent would do so in the public treatment (right bar). The resulting public-private gap of 30.3 percentage points is statistically significant ($t(164) = 7.998, p < 0.001$) and relevant in magnitude; the take-up rate goes down by a third. This effect is remarkable because forgoing the transfer is costly: not taking the transfer means passing up a 50 percent increase in the experimental earnings when ranked third, i.e. 6 Euros instead of 9 Euros. In line with our theoretical model, we interpret the decrease in the take-up rate due to public exposure as a *stigma effect*.

Recall that by comparing public and private, we net out all potential determinants that might affect the take-up behavior but that are invariant to the visibility of the welfare take-up decision. Other determinants may include, for instance, self-signaling concerns or internalized shame. Similarly, other psychological frictions (e.g., decision errors) that may explain why the take-up rate deviates from 100 percent, which would be the prediction of a neoclassical model of welfare take-up behavior, cannot explain the stigma effect. Under the assumption that transaction costs are not relevant, the only thing that varies is the visibility of the take-up decision.[33] Hence, the treatment effect can only be attributed to a participant's anticipation of the inferences the public (i.e., the other participants in the laboratory) will make upon observing her taking up the transfer. This finding confirms Prediction 1.

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[33]Using the *subsidized treatments* that we discuss in section 4.3, we show that indeed transaction costs related to the public setting do not explain or even contribute to the observed effect. See also Prediction 5 and Result 5.

Result 1 (Stigma effect). *Welfare stigma matters. The take-up rate in the quiz-redistribution treatment is significantly lower in public than in private (both statistically and economically).*

4.2 Ability and moral signaling as determinants of welfare stigma

Our theoretical framework distinguishes between ability and moral signaling. To disentangle which of these motives drives the observed stigma effect in take-up rates, we look at the *random redistribution* treatment, where a subject's rank and, therefore, also eligibility to claim a transfer is determined randomly. If rank is based on luck alone, then taking up the transfer does not allow the public to draw any inference about the claimant's ability or skills. But, as transfers are redistributive, the decision to publicly claim a transfer is informative about the claimant's moral attitude. If the stigma effect goes to zero when transfer eligibility is random, the stigma effect described above must be driven by ability signaling alone. If we find a stigma effect of similar size, we would conclude that moral signaling is the only relevant factor. If it decreases but remains significantly positive, both ability and moral signaling are at play (cf. Predictions 2 and 3).

Figure 1b depicts the take-up rate in the random redistribution treatment, again divided into public and private. We also observe a difference in the take-up rates when rank is assigned by chance: here, the stigma effect amounts to 18.9 percentage points. Making the take-up of the transfer public reduces the take-up rate by roughly 22 percent from the take-up rate in private, leading to the conclusion that a concern for moral signaling affects the take-up decision, which confirms Prediction 2.

Result 2 (Moral signaling I). *Moral signaling matters. The take-up rate in the random-redistribution treatment is significantly lower in public than in private.*

However, the stigma effect is smaller if income is determined randomly (0.189) than if it is based on the quiz (0.303). To statistically test for differences between random and quiz, we run the following difference-in-differences estimation:

$$(2) \qquad \text{Take-up}_i = \alpha + \beta_1 \text{Quiz}_i + \beta_2 \text{Public}_i + \beta_3 (\text{Quiz x Public})_i + \epsilon_i,$$

using ordinary least squares with standard errors clustered on the subject level. The estimated interaction effect β_3 and its standard error are reported in the bottom-right cell in Table 3. As can be seen, the diff-in-diff of 0.114 is significant at the 5 percent level, suggesting that ability signaling does indeed matter. This finding lends support to our Prediction 3.

Result 3 (Ability signaling I). *Ability signaling matters. The stigma effect is significantly higher in the quiz-redistribution treatment than in the random-redistribution treatment.*

Table 3 further summarizes the take-up rates of the treatments discussed so far and the estimated differences between them. The horizontal within-subject differences are calculated

Table 3: Take-up rates in respective treatment (redistribution)

Task	Private transfer	Public transfer	Difference (paired)
Quiz	0.879	0.576	0.303***
	(0.025)	(0.039)	(0.038)
	[165]	[165]	[165]
Random	0.862	0.673	0.189***
	(0.027)	(0.037)	(0.032)
	[159]	[159]	[159]
Difference	0.017	-0.097*	DiD=0.114**
(unpaired)	(0.037)	(0.054)	(0.050)
	[324]	[324]	[324]

Notes: Standard errors in parentheses, number of observations in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ according to paired/unpaired t-tests.

using paired t-tests, and the vertical between-subject differences are assessed using unpaired t-tests.[34]

When the transfer is private, we find virtually no difference in the take-up rates between quiz and random (first column of Table 3). This result is consistent with the notion that there is no room for social signaling in the private treatment.[35] Furthermore, it indicates that individuals do not act as if they perceive it to be more morally appropriate to claim a transfer if income is based on luck than if it is based on performance (cf. Prediction 4).[36]

Result 4 (Meritocratic attitudes). *Take-up rates in private are inconsistent with meritocratic attitudes. Private take-up is not significantly lower when income is based on ability than when it is based on luck.*

We conclude that both ability and moral signaling are relevant for the observed public-private gap but meritocratic attitudes can be ignored. The public-private gap of 30.3 percentage points in the main quiz treatment is reduced by 11.4 percentage points when shutting down the ability signaling channel.

4.3   Excluding transactions costs and meritocratic signaling as confounds

We argue that our experimental design can identify and disentangle moral and ability signaling as components of the total stigma effect. Our argument relies on two assumptions that are

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[34]The results are qualitatively similar when using McNemar's test and Fisher's exact test to account for the categorical nature of the dependent variable.

[35]In contrast, the take-up rate of the public transfer is 9.7 percentage points lower when ranks are based on quiz rather than on luck (a decrease by 14.4 percent in the take-up rate), implying that the estimated diff-in-diff effect is indeed largely driven by behavioral adjustments under the visibility of welfare transfers.

[36]Behavior appears to differ from stated attitudes. In our post-experimental questionnaire, we find some indication for meritocratic attitudes. Subjects in the quiz treatment agree to the statement "The subject in rank three is entitled to receive a transfer" to a significantly lower degree than in the random treatment according to a Wilcoxon rank-sum test ($z = 4.435, p < 0.001$). The difference in perceived entitlement may just not be large enough to affect behavior.

tested in this section. (1) We assume that transactions costs are not relevant, which implies that the stigma effect in the random redistribution treatment identifies moral signaling. (2) We assume that moral signaling concerns do not vary between random and quiz treatment, which ensures that the interaction effect between quiz treatment and public take-up measures ability signaling. The first assumption is violated if participants experience transaction costs in the public setting, for instance because they dislike having to stand up and walk to the experimenter desk. The second assumption is violated if individuals want to signal a meritocratic attitude that finds it more objectionable to take money from other group members when income is based on performance than when it is based on luck.[37] The desire to signal a meritocratic attitude could also result in the non-zero diff-in-diff that we have ascribed to ability signaling above, and it would bias our estimate of the stigma effect upwards.

We test these two assumptions using the subsidized treatment, where taking up the transfer does not affect the earned incomes of others. First, we test whether transaction costs are relevant (cf. Prediction 5) by comparing take-up rates between private and public when ranks are random so that both ability and moral signaling are ineffective. Second, we test whether meritocratic signaling plays a role (cf. Prediction 6) by comparing the diff-in-diffs between redistribution and subsidy treatments. To fix ideas, consider the resulting treatment structure:

$$\text{DiDiD} = \underbrace{[\overbrace{(t_{\text{priv,quiz}} - t_{\text{pub,quiz}})}^{\text{ability+moral+meritocratic}} - \overbrace{(t_{\text{priv,rand}} - t_{\text{pub,rand}})}^{\text{moral}}]}_{\text{redistribution}} - \underbrace{[\overbrace{(t_{\text{priv,quiz}} - t_{\text{pub,quiz}})}^{\text{ability}} - (t_{\text{priv,rand}} - t_{\text{pub,rand}})]}_{\text{subsidized}}$$

where t denotes the take-up rate in the respective treatment combination. Until now, we have considered the first diff-in-diff with redistribution. Now, consider the second diff-in-diff. The public-private-gap in the subsidized quiz treatment captures ability signaling without moral signaling and, thus, also without the meritocratic component. Moreover, the public-private-gap in the subsidized random treatment isolates other explanations, like transaction costs, since there is no scope for signaling motives.

Figure 2 and Table 4 summarize the results of the subsidized treatments. The random treatment reported in the second row of Table 4 captures any other channel that would produce a positive public-private gap but shuts down the ability signaling channel. As can be seen, the public-private gap vanishes almost completely. While the point estimate of 0.021 is still positive, it is not statistically different from zero at conventional statistical levels, suggesting that transaction costs in the public setting are not of major concern in our

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[37]Note that we did not find evidence in the private take-up rates that would suggest that individuals actually find it more objectionable to claim a transfer when income is based on performance than when it is based on luck (cf. result 6)
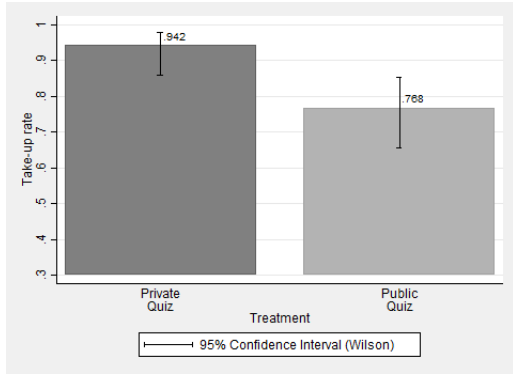
(a) quiz treatment (n=69)
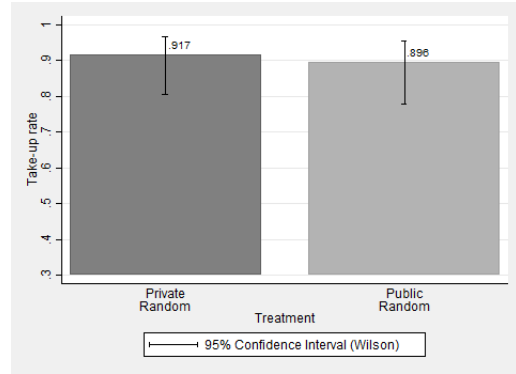
(b) random treatment (n=48)

Figure 2: Take-up rate by transfer regime (subsidized treatments)

experiment (cf. Prediction 5). This observation is reassuring as it suggests that the observed stigma effect is in fact driven by social signaling concerns.

Result 5 (Placebo test). *Transaction costs do not play a role. In the subsidized random treatment public take-up is not significantly different from private take-up.*

We also find that the desire to signal a meritocratic attitude does not have a significant effect on take-up rates. If individuals wanted to signal a meritocratic attitude, the effect of stigma related to ability signaling would be overestimated in the redistribution treatments (cf. Prediction 6). However, when we difference the two public-private gaps of the subsidized treatments, we obtain an estimate of the stigma effect due to ability signaling of 0.153, which is slightly higher than the estimate of 0.114 obtained from the redistribution treatments in the previous section. To test for statistical differences between the two diff-in-diffs, we estimate

Table 4: Take-up rates in respective treatment (subsidized)

Task	Private transfer	Public transfer	Difference (paired)
Quiz	0.942	0.768	0.174***
	(0.028)	(0.051)	(0.054)
	[69]	[69]	[69]
Random	0.917	0.896	0.021
	(0.040)	(0.045)	(0.047)
	[48]	[48]	[48]
Difference	0.025	-0.128*	DiD_s=0.153**
(unpaired)	(0.048)	(0.071)	(0.076)
	[117]	[117]	[117]

Notes: Standard errors in parentheses, number of observations in square brackets.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ according to paired/unpaired t-tests.

Table 5: Regression of the public-private gap on treatment characteristics (model 3)

	public-private gap
Quiz	0.153**
	(0.072)
Redistribution	0.168***
	(0.057)
Quiz x Redistribution	-0.039
	(0.087)
Constant	0.021
	(0.047)
Adj. R^2	0.031
N	441

Notes: Robust standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the DiDiD from above by pooling the observations from all treatments presented so far:

$$(3) \quad \text{public-private gap}_i = \alpha + \beta_1 \text{Quiz}_i + \beta_2 \text{Redistribution}_i + \beta_3 (\text{Quiz x Redistribution})_i + \epsilon_i$$

We regress the public-private gap, i.e., the within-subject difference $(t_{\text{pub}} - t_{\text{priv}})$ between the public and the private transfer regime, on treatment dummies for quiz and redistribution and their interaction. Formally, β_3 is a triple difference estimator (DiDiD) that identifies the meritocratic component.

As can be seen from Table 5, the interaction effect β_3 is insignificant, suggesting that there is not a significant difference between the two differences. Therefore, we reject the hypothesis underlying Prediction 6 that individuals want to appear as if they honored earned income more than random income.

Result 6 (Meritocratic signaling). *The desire to signal meritocratic attitudes does not affect the take-up behavior. The difference in stigma effects between quiz and random are not significantly different between subsidized and redistribution treatment (i.e. the DiDiD estimator is not significant).*

This result implies that the ability effect estimated in our previous section was not driven by signaling meritocratic considerations. In fact, β_1 estimates an ability signaling effect of 0.153 net of potential interactions with moral signaling as well as transaction costs. Moreover, β_2, estimated to 0.168, identifies the moral signaling effect, net of potential transaction costs.

4.4 Robustness: Experimenter demand effect and strategy method

First, we show that the results are unlikely to be driven by an experimenter demand effect, which one could be concerned about due to the within-subject design using the strategy method. As subjects are asked to make their take-up decision for both the public and the
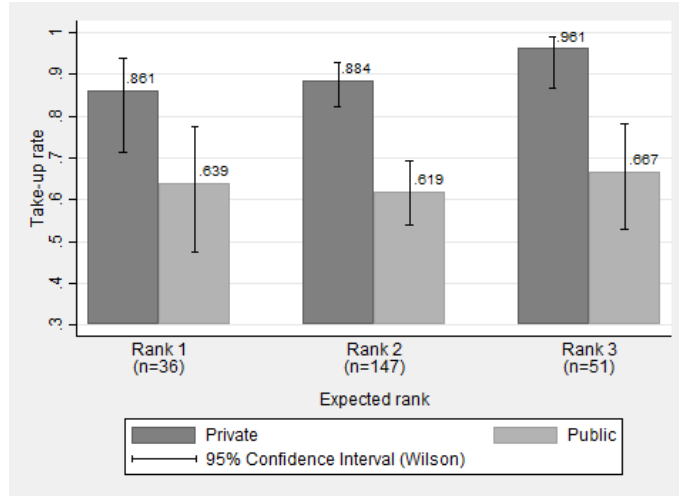
Figure 3: Take-up rate by expected rank (n=234)

private transfer regime, they might feel inclined to give systematically different responses across settings, thereby producing an artificial public-private gap in take-up rates.[38] We mitigate such tendencies by presenting both transfer regimes not at the same time but in randomized order on separate screens. When subjects are asked about their take-up decision for the private transfer, they do not know that there will also be a decision for a public transfer, and vice versa. If there was an experimenter-demand effect, we would expect that subjects who learn on the second screen that there is both a public and a private transfer have lower public take-up rates (when private is presented first) and higher private take-up rates (when public is presented first). Instead, we observe that pooled over all treatments the mean take-up rate of the public (private) transfer is 0.675 (0.886) if the private transfer is presented first and 0.677 (0.887) if the public transfer is presented first. Thus, there are no systematic differences in line with an experimenter demand effect. Similarly, there are no significant differences in the take-up rates when comparing order effects by treatment.

Second, we check whether the take-up decision is associated with the expectation about one's own rank when income is based on quiz performance. If the stigma effect is different between those who are confident being in rank 1 and those who suspect they are in rank 3, the strategy method might not be appropriate. It is therefore reassuring that there are no significant differences in the take-up by expected rank as Figure 3 illustrates.

Third, we provide evidence that our treatments actually result in measurable variations in the perceived extent of ability and moral signaling. A Wilcoxon rank-sum test reveals that there is greater approval to the statement "The participant in rank 3 has poor knowledge" in the quiz treatment than in the random treatment ($z = -8.706, p < 0.001$). Moreover,

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[38]A demand effect in the sense that individuals respond feeling pressured to answer consistently across public and private conditions would only work against finding a stigma effect.

significantly more subjects in the random than in the quiz treatment believe that the person in rank 3 had bad luck ($z = 5.292, p < 0.001$). Thus, the survey answers support our identification of ability signaling, which rests on the assumption that subjects perceive taking up a transfer in the quiz treatment as sending a negative signal about their ability. However, there are still many subjects who consider quiz performance to be a matter of luck (see Table 7 in the appendix). Thus, we conclude that our experimental measure of ability-related welfare stigma represents a lower bound of this effect. The survey answers suggest that an income source that is more strongly associated with ability (e.g., IQ test, school grades) might produce an even larger stigma effect.

5   Preferences for transfer regime

So far, we analyze how stigmatization affects individual decisions to take up a welfare transfer. In this section, we present results from the second part of our experiment, where we use a random dictator decision rule to elicit individuals' preferences for the public or private transfer mode.

Figure 4 illustrates the fraction of participants who vote in favor of the public transfer regime, divided by rank and income source. As subjects already know their rank at this stage, we differentiate between those who presumably benefit from the public transfer regime (ranks 1 and 2) and those who are harmed by it due to stigmatization (rank 3). As expected, few subjects in rank 3 vote in favor of the public transfer regimes, and in all treatments they are less likely to vote for the public regime than those with ranks 1 and 2. Moreover, Figure 4 shows that there are more subjects in rank 1 who vote for the public transfer regime in the quiz treatment, but none of the treatment differences are significant.[39]

However, keeping in mind that the take-up rate is much lower under the public transfer regime, it is striking that there are relatively few rank 1 and rank 2 subjects voting for it. If we assume that subjects take the stigma effect into account – and we find a strong indication that they do in the post-experimental questionnaire[40] – we would expect all payoff-maximizing agents in rank 1 and 2 to vote in favor of the public transfer.

In order to investigate the reasons for this voting pattern, Table 8 in the appendix takes a closer look at the voting motives that were stated by the subjects in the post-experimental questionnaire. We see that a majority of subjects in ranks 1 and 2 who voted for the public transfer agreed to the statement "I want to reduce the take-up probability to raise my payout."

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[39]One reason why the support for the public transfer mode may be higher in the quiz than in the random treatment could be that participants are curious about who performed poorly in the quiz and are not ashamed of admitting so and of asking for redistribution. If this was the case, it would only add to the neoclassical motives pushing for a high share voting in favor of the public transfer and cannot explain why support for the public regime is so low.

[40]75.3 percent of subjects on rank 1 and 2 agree to the statement "It is discomforting for the claimant when the transfer is public" and 70.8 percent agree to the statement "Public transfers reduce take-up probability."
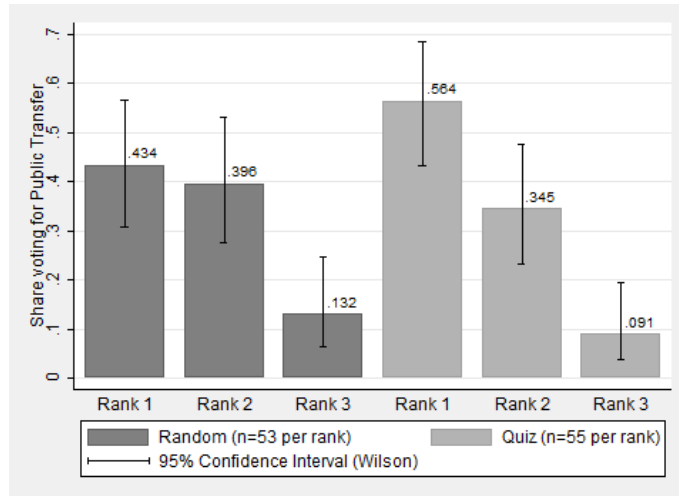
Figure 4: Share voting for public transfer by rank and task, only redistribution (n=324)

However, they are rather indifferent to the statement "Free-riders should be identified as such." In contrast, 82.0 percent of those who voted against the public transfer agreed to the statement "I don't want the claimant to be ashamed." Thus, there are, on the one hand, many subjects who anticipate the existence of a stigma effect and vote for the public transfer to reduce the take-up probability. On the other hand, many subjects acknowledge the existence of the welfare stigma but have social preferences toward the subject in rank 3. They vote against the public transfer to spare them the shame of stigmatization.[41]

In summary, the voting patterns from the second part of the experiment provide further evidence of the existence of welfare stigma in line with our theoretical framework (e.g., Prediction 1). These findings indicate that we should not only investigate preferences for redistribution but we need to take into account that individuals have preferences with respect to the way that redistributive payments are paid out. Specifically, our data suggests that even many of those individuals who are net payers dislike stigmatization.

6 Conclusion

Economists typically assume that individuals' welfare take-up decision is exclusively driven by the mere trade-off between the material benefits and costs of a social benefit. Our paper suggests that social considerations in the form of social signaling concerns are also important. Using a laboratory experiment, we present causal evidence that social stigmatization significantly reduces the take-up of a welfare benefit even though the benefit would constitute a considerable increase in payoffs.

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[41]Nevertheless, there are a few subjects who appear not to be responsive to stigma. Among those who are in rank 3 and vote in favor of the public transfer regime 6 out of 11 disagree with the statement "It is discomforting for the claimant when the transfer is public."

The design is closely linked to a theoretical framework in which stigmatization depends on the inferences about a claimant's type, and it separately identifies the effects of ability stigma (take-up signals inferior ability) and moral stigma (take-up signals the willingness to live off others) on an individual's decision to claim a welfare benefit. There are three key results: First, making take-up public reduces the take-up rate significantly by approximately 30 percentage points. Second, in our experiment we estimate that ability signaling reduces take-up by 15.3 percentage points and moral signaling by 16.8 percentage points, that is, both ability and moral signaling matter. Third, when subjects are asked to vote on one of the two transfer regimes, more than half of the net payers (i.e., individuals ranked 1 or 2) vote against the public transfer regime even though doing so implies that they have to pay the transfer with higher probability.

Our design deliberately abstracts from several interesting aspects that are also relevant for program participation. For instance, individuals typically choose how much to work or shirk, and eligibility not only depends on ability but also on endogenously chosen effort levels. The desire to avoid stigmatization associated with being on welfare may lead individuals to choose higher effort levels in the first place. Furthermore, individuals may falsely claim a transfer they are not eligible for and stigmatization may deter some of these unjustified claims. Both aspects, as well as the interaction between social signaling concerns and other barriers to program participation, like unawareness and program complexity, are interesting topics for further research.

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A   Proofs

Proof of Proposition 1. Suppose assumptions 2 and 3 hold. As we have also assumed that the distribution of α is continuous, we know that the moral stigma expression is continuous and, therefore, $G(\alpha)$ is continuous. By Assumption 2, we find $\alpha, \alpha' \in [0, A]$ such that $G(\alpha) < 0 < G(\alpha')$. As $G(\alpha)$ is defined on the closed interval $[0, A]$, the intermediate value theorem tells us that a value $\tilde{\alpha}$ exists for which $G(\tilde{\alpha}) = 0$. By Assumption 3, we have $\frac{d}{d\alpha}(t - aR_A(\lambda) - bR_M(\lambda) - \alpha) = -b\partial R_M(\lambda)/\partial \alpha - 1 = b\alpha f(\alpha) - 1 < 0$. Thus, the trade-off defined in equation 1 only holds for equality at most once and $\tilde{\alpha}$ with $G(\tilde{\alpha}) = 0$ is unique. □

Proof of Corollary 1. The threshold from equation 1 decreases if the take-up decision is more publicly exposed. The denominator is positive by Assumption 3.

$$\frac{d\tilde{\alpha}}{d\lambda} = -\frac{aR'_A(\lambda) + bR'_M(\lambda)}{b\partial R_M(\lambda)/\partial \tilde{\alpha} + 1} < 0$$

Intuitively, if claiming the transfer is more exposed, the disutility from the associated stigma weighs more heavily such that only individuals with low moral concern claim the transfer. This threshold translates directly into the rate of take-up so that we obtain the following result with respect to the effect of making the take-up decision public. □

Proof of Corollary 2. Conditional on the take-up decision being public, the threshold from equation 1 decreases if the decision to take-up the transfer leads to a more negative signal about the individual's ability, i.e., if $R_A(\lambda)$ (which is weakly positive) increases. Intuitively, with a higher ability stigma, only individuals with low moral concern take the transfer so that the moral stigma associated with take-up becomes larger.

$$\frac{d\tilde{\alpha}}{dR_A(\lambda)} = -\frac{a}{b\partial R_M(\lambda)/\partial \tilde{\alpha} + 1} < 0$$

This derivative is unambiguously negative because the moral stigma associated with taking up the transfer is decreasing with $\tilde{\alpha}$. The more individuals that take the transfer, the higher the conditional expectation of their moral concern, thus, on average, the welfare claimant is more moral and less stigmatized. If all take the transfer, the claimant does not differ in his or her moral concern from the average population and the moral stigma is zero. Thus, we find that the stigma effect of publicity is larger if the take-up eligibility is related to ability. □

Corollary 3. Consider $\gamma' > \gamma$. Then for a given threshold $\tilde{\alpha}$, it holds that $F_{\gamma'}(\tilde{\alpha}) \leq F_{\gamma}(\tilde{\alpha})$ because of the assumed first-order stochastic dominance shift in the distribution of moral concerns. □

B Additional tables

Table 6: Descriptive statistics

	n	Mean	Std. Dev.	Minimum	Maximum
Male	441	0.592	0.492	0	1
Age	441	23.900	4.234	16	48
Studying	441	0.952	0.213	0	1
Working	441	0.297	0.457	0	1
Subject related to Econ	441	0.243	0.429	0	1
Experimental Experience	441	1.889	0.995	0	3
Correct answers in quiz	441	9.574	2.591	3	17

Table 7: Post-experimental questionnaire responses regarding signaling

Statement		Rel. Frequency	
		Quiz	Random
"The participant in rank 3 has poor knowledge"			
Strongly Disagree	0	28.17	75.47
	1	29.58	10.69
	2	19.72	10.06
	3	19.25	3.14
Strongly Agree	4	3.29	0.63
N		213	159
"The participant in rank 3 had bad luck"			
Strongly Disagree	0	7.69	14.49
	1	14.96	5.80
	2	26.50	13.53
	3	37.18	14.98
Strongly Agree	4	13.68	51.21
N		234	207

Notes: The number of observation differs since the first question was not included in the first three sessions.

Table 8: Post-experimental questionnaire responses regarding voting

Statement		Rel. Frequency	
		Pro Public	Contra Public
"Free-riders should be identified as such"			
Strongly Disagree	0	10.64	34.43
	1	23.40	27.87
	2	29.79	24.59
	3	21.28	8.20
Strongly Agree	4	14.89	4.92
N		94	122
"I want to reduce the take-up probability to raise my payout"			
Strongly Disagree	0	9.57	27.87
	1	11.70	20.49
	2	23.40	31.15
	3	28.72	13.11
Strongly Agree	4	26.60	7.38
N		94	122
"I don't want the claimant to be ashamed"			
Strongly Disagree	0	18.09	2.46
	1	24.47	4.92
	2	39.36	10.66
	3	8.51	22.13
Strongly Agree	4	9.57	59.84
N		94	122

Notes: Only subjects in ranks 1 and 2 are considered.

C   Instructions (translated from German)

Welcome to our experiment!

During the experiment you are not allowed to use electronic devices or communicate with other participants. Please do only use the programs and functions provided for the experiment. Please do not talk to other participants. If you have a question, please raise your hand. We will come to you and will quietly answer your question. Please never ask your questions aloud. If the question is relevant for all participants, we will repeat it and answer it loudly. If you do violate these rules, we must exclude you from the experiment and the payoff.

The following instructions describe the process of the experiment and are equal for all participants. You can earn money in this experiment. The level of your payout depends on your decisions, on the decisions of other participants and on chance. Please carefully read the instructions. You can leave the experiment at any time. If you want to do so, please raise your hand. You will only be paid off if you stay until the end of the experiment.

Quiz

In this experiment, you and two other participants will form a group. The group will remain the same for the entire experiment. All participants will first answer a quiz. You receive 18 questions on different domains of general knowledge. There are four possible answers to each question, of which exactly one is correct. You will obtain one point for each question answered correctly. You do not receive any point for questions that were not answered or incorrectly answered. You have six minutes to work on the quiz. After that, all given responses will be submitted.

[Quiz: Your pay-off depends on how well you solve your tasks in comparison to the other two group members. The member of the group who has collected the biggest amount of points after six minutes receives the first rank, the member of the group with the second biggest amount of points receives the second rank and the member of the group with the third biggest amount of points receives the third rank. If two or three members of the group have the same amount of points, it will be determined randomly who gets the higher rank.]

[Random: Independently of the amount of collected points, each group member will be randomly assigned to a rank, which is relevant for the rest of the experiment. Your rank does in no way depend on your performance in the quiz.]

Pay-Off

The payoff to one participant depends on her rank. There are two possible modes of payoff: payoff mode A and payoff mode B.

[Redistribution:]

Rank	Mode A	Mode B
1	16 Euro	14 Euro
2	11 Euro	10 Euro
3	6 Euro	9 Euro

[Subsidized:]

Rank	Mode A	Mode B
1	16 Euro	16 Euro
2	11 Euro	11 Euro
3	6 Euro	9 Euro

[Redistribution: Mode A differs from mode B in that the participant on the third rank receives a transfer from the participants on the first and second ranks.]

[Subsidized: Mode A differs from mode B in that the participant on the third rank receives a transfer.]

Which of these payoff modes will be applied depends on your decisions and the decisions of the other participants in the second part of the experiment. You will receive the instructions for the second part upon completing the first part.

Second part of the experiment

[to be handed out after the first part has ended]

The second part of the experiment will decide which of the two payoff modes will be applied. In each group, this depends on the decisions of the group member on the third rank. If the group member on the third rank decides to take the transfer, payoff mode B will be applied. If the group member on the third rank decides not to take the transfer, payoff mode A will be applied.

Stage 1

You now must make a binding decision on whether you would like to take the transfer in the case of being placed on the third rank. You will only find out about your rank in stage 2. If you end up on the third rank, the decision that you now make will be applied. On two consecutive screens, you will now receive information on the conditions under which you can receive the transfer. On each of these screens, you have to decide on whether you would take the transfer under the given conditions and in case you end up on the third rank. Which of these conditions will be applied is determined in stage 2. Please consider that your decision is binding and irreversible for the rest of the experiment.

Stage 2

[Quiz: After making your decision in stage 1, you will find out about the rank you achieved in your group.] [Random: After taking your decision in stage 1, you will find out about the rank you were randomly assigned to.] For each group it will now be decided which of the conditions shown in stage 1 will be applied. Out of the two possible conditions, each member of the group will now pick the one that should be applied according to his/her opinion. In each group, one participant will be randomly picked and her decision will be applied in her group.

Payoff

Remember: the pay-off depends on the rank. Whether payoff mode A or B will be applied depends on the group member on the third rank. If that member decided to take the transfer, mode B will be applied. If that member decided against the transfer, mode A will be applied to the pay-off. It is now relevant how the group member on the third rank decided under the conditions picked by the group.

[Redistribution:]

Rank	Mode A	Mode B
1	16 Euro	14 Euro
2	11 Euro	10 Euro
3	6 Euro	9 Euro

[Subsidized:]

Rank	Mode A	Mode B
1	16 Euro	16 Euro
2	11 Euro	11 Euro
3	6 Euro	9 Euro

Please answer the attached control questions and raise your hand when you are done. An experimenter will then come to you to check your answers. If you have any questions, please raise your hand.

Questionnaire

1. Does your rank depend on your performance in the quiz? Yes/No

2. [Quiz: Assume you scored the second highest number of points in your group.]
   [Random: You were randomly assigned to the second rank.] In the first stage, the two other group members decided to take the transfer under both conditions.

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(a) What is your payoff?

(b) What is the payoff of the group member on rank 1?

3. Assume that you decided for a certain condition in stage 2. The two other group members decided for the other condition. Which condition is relevant for payoff?

- The condition that I chose.
- The condition that the other two group members chose.
- Both conditions are possible.

D Screenshots

Please decide whether you would like to take up the transfer under the following condition if you are on rank 3.

Private Transfer:
In order to receive the transfer, no further action is required. It will automatically be added to your experimental payout.

Remember that your decision is binding and irreversible.

Would you like to take up the transfer under this condition? ○ Yes
○ No

OK

Please decide whether you would like to take up the transfer under the following condition if you are on rank 3.

Public Transfer:
In order to receive the transfer, you have to stand up and collect a yellow slip of paper at the experimenter's desk.

Remember that your decision is binding and irreversible.
In case of the public transfer the experiment can only go through if all transfer claimants have collected their yellow slip of paper and have entered the number on the screen.

Would you like to take up the transfer under this condition? ○ Yes
○ No

OK

Figure 5: Take-up conditions presented on-screen in randomized order (translated from German)

Who was the first Federal President of the Federal Republic of Germany?
- Ludwig Erhard
- Konrad Adenauer
- Theodor Heuss
- Heinrich Lübke

When was the Berlin Wall built?
- 1962
- 1958
- 1961
- 1960

What role do chromosomes play?
- Carrying genetic information
- Cell respiration
- Storage of vitamins
- Photosynthesis

"The Nutcracker", "Romeo and Juliet" and "Swan Lake" are works by...
- Franz Schubert
- Wolfgang Amadeus Mozart
- Pyotr Ilyich Tchaikovsky
- Ludwig van Beethoven

Which organ of the body produces the hormone insulin?
- Thyroid
- Adrenal gland
- Spleen
- Pancreas

Where do the 2016 Olympics take place?
- Rio de Janeiro
- Rome
- London
- Tokyo

Which country does NOT share the Alps?
- France
- Germany
- Italy
- Hungary

What is the capital of Ethiopia?
- Addis Abeba
- Kigali
- Mogadishu
- Abu Dhabi

Who is incumbent head of government of Spain?
- Rodriguez Zapatero
- Pedro Passos Coelho
- Matteo Renzi
- Mariano Rajoy

Who won the Nobel Prize in Literature in 2015?
- Alice Munro
- Svetlana Alexievich
- Mario Vargas Llosa
- Garcia Marquez

Which style are the groups of artists "Brücke" and "Blauer Reiter" associated with?
- Expressionism
- Impressionism
- Cubism
- Surrealism

Where is the European Central Bank (ECB) headquartered?
- Brussels
- Strasbourg
- Frankfurt am Main
- London

Which character was invented by James Joyce?
- Leopold Bloom
- Jarvis Lorry
- Oliver Twist
- Samuel Pickwick

"The Sorrows of Young Werther" is a work by...
- Friedrich Schiller
- Johann Wolfgang Goethe
- Johann Gottfried Herder
- Christoph Martin Wieland

The Peace of Westphalia in 1648 ended the....
- Seven Years' War
- Franco-Prussian War
- Hundred Years' War
- Thirty Years' War

Newton's laws are about...
- Electricity
- Motion
- Radiation
- Magnetism

Who is the incumbent head of the German Ministry of Health?
- Peter Altmaier
- Hermann Gröhe
- Karl Lauterbach
- Daniel Bahr

Who is President of the German Federal Constitutional Court?
- Peter Müller
- Udo di Fabio
- Hans-Jürgen Papier
- Andreas Voßkuhle

Figure 6: Screenshot of quiz (translated from German)