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By: **David L. Dickinson,** David Masclet, and Emmanuel Peterle

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Discrimination as favoritism: The private benefits and social costs of in-group favoritism in an experimental labor market

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A B S T R A C T

We examine both the private benefits and spillover costs of labor market favoritism in a unique laboratory experiment design. Our data show that both employment preference and wage offers favor in-group members. Workers positively reciprocate towards in-group employers by choosing higher effort in a gift-exchange game. Thus, favoritism can be privately rational for employers. However, unemployed subjects are allowed to burn resources (at a cost to themselves), and we document significantly increased resource destruction when unemployment can be attributed to favoritism towards others. This highlights a significant spillover and often ignored cost of favoritism, and it points to one possible microfoundation of some antisocial behavior.

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1. Introduction

Since the publication of Gary Becker's *The Economics of Discrimination* in 1957, the subject of discrimination has been of particular interest to labor economists. The literature on labor market discrimination is large and has benefitted from the complementary efforts of empirical, econometric, field experimental, and controlled laboratory studies. We aim to contribute to the existing literature by investigating a potential motive for taste-based discrimination in a unique experimental design that can help shed light on both the private incentives and spillover impacts of discriminatory practices. In other words, there may be instances where favoring one group of workers over another privately benefits the firm, yet generates spillover costs to society.

According to Becker, taste-based discrimination leads to suboptimal recruiting decisions. Thus, competitive markets should help eliminate this type of discrimination as prejudiced employers face higher production costs. In contrast, other models have considered that taste-based discrimination is driven by efficiency concerns such as reduced costs of communication (Lang, 1986; Athey et al., 2000; Efferson et al., 2008; Fu et al., 2012). Yet another alternative is that employers may benefit from in-group favoritism in terms of reciprocal effort. Specifically, in-group workers may provide high effort both in response to in-group employment favoritism as well as in-group wage favoritism. Either framework implies that not all

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taste-based discrimination is detrimental from the perspective of the firm's private cost/benefit analysis, and in-group favoritism may therefore be a potential source of taste-based discrimination. One of the main contributions of this paper is to show that in-group favoritism may affect hiring, wage, and effort choices. As has been noted recently, in-group favoritism (or, "endophilia") need not imply out-group discrimination or hostility ("exophobia") (Feld et al., 2016),[1] and so the motivation behind behaviors that produce similar outcomes may differ from what one typically considers discrimination. For expositional purposes, we use the terms discrimination and favoritism somewhat interchangeably in this paper.

In addition to providing evidence of favoritism and reciprocity between in-group employers and workers, another contribution of our paper is the generation of unique data on spillover effects of discrimination/favoritism. Specifically, our experimental design provides an avenue for spillover effects to manifest by allowing unemployed workers (who may or may not be able to attribute their unemployment status to in-group favoritism) to destroy others' resources (i.e., burn money). Thus, our data permit us to measure one type of discrimination spillover cost on society. There are limitations in using data from money-burning choices in a lab setting to understand true societal costs like fragmentation and increased conflict. Nevertheless, our data may be seen as providing at least some evidence of one micro-foundation of societal tensions, riots, or less extreme but still costly forms of antisocial behavior. We view our study as an important contribution to the literature because such costs of discrimination on society are typically ignored in existing research. A third contribution is that we provide a theoretical framework that can explain several of our findings as the result of preferences that incorporate moral motivations, fairness, and discrimination concerns. A final contribution of this study is the strategy method elicitation of decisions from subjects, which allows us to identify the impact of favoritism by examining each subject's full contingency choice set.

The potential for disgruntled societal groups to engage in costly antisocial behavior is real. For example, significant costs to society were incurred in France in 2006 when proposed labor laws upset young workers who viewed the laws as unfair or discriminatory towards new workers. In Paris alone, more than 500,000 protestors gathered, and the media coverage showed evidence of explicit societal costs as storefronts were vandalized and cars torched by a few dozen rioters.[2] Even recent civil unrest in the U.S. that is not specifically connected to labor market outcomes (e.g., the Baltimore riots of 2015) shows the potential societal costs of tension between identity groups (e.g., racial groups) and real or perceived in-group favoritism. Other examples of how discrimination may impact societal resources and wealth transfer in ways that are not necessarily antisocial might include the pursuit of ant-discrimination lawsuits.[3] Our design does not wealth transfers but does allow us to separate targeted resource destruction from indiscriminate resource destruction in a way that can be informative regarding our understanding of discrimination spillover costs.

To preview our main results, we document evidence of the private benefits to employers of in-group favoritism. However, unemployed workers are willing to engage in costly money burning in our setting, most notably when there is evidence of discriminatory employers. This highlights that society as a whole has an interest in addressing systematic favoritism.

2. Background

Labor market discrimination may exist for a variety of reasons. In Becker's model, discrimination in hiring or wages is caused by a 'taste for discrimination', which leads the employer to hire or pay higher wages to members of her/his own group (henceforth, "in-group"). Other models predict workplace segregation but consider that in-group biases are driven by efficiency considerations, such as reduced costs of communication (Lang, 1986; Athey et al., 2000). Communication costs, in general, are lower among individuals with a common group identity of some sort. This may be a contributing factor in understanding how in-group favoritism evolves (Efferson et al., 2008; Fu et al., 2012), which can then help explain segregation of informal networks (Marsden, 1987). In-group networks have been shown to impact hiring decisions (Granovetter 1995; Holzer, 1996; Bayer et al., 2008; Hensvik and Skans, 2016; Gee et al., 2017). For example, researchers have reported that workplaces with black supervisors or owners are significantly more likely to employ black workers (Bates, 1994; Carrington and Troske, 1998; Stoll et al., 2004; Giuliano et al., 2009). This suggests that in-group favoritism may be a key component of the discriminatory outcome.

An alternative to taste-based discrimination is statistical discrimination (Phelps, 1972; Arrow, 1972). According to this approach, employers have incomplete information about the worker's potential performance. Imperfect information arises either because minority groups emit noisier signals (Phelps, 1972; Aigner and Cain, 1977; Cornell, and Welch, 1996; Pinkston, 2003) or because negative prior beliefs about members of a particular group may become self-fulfilling in equilibrium (Lundberg and Startz, 1983). Due to the complexity of factors that may contribute to discrimination, we view this topic

[1] The Feld et al. (2016) study notes the lack of attention given to this distinction in the economics literature outside of a couple of older papers (Goldberg, 1982, and a survey article by Cain, 1986).

[2] In another example, costly riots took place in French city suburbs in October and November 2005—the environment of high youth unemployment was considered a reason for the social unrest. Cars and several public buildings were burned, and the riots resulted in three deaths of non-rioters, many police injuries, and nearly 3000 arrests. A state of emergency was declared on November 8, later extended for three weeks, and the government announced a crackdown on immigration. This event was an expression of frustration and real or perceived discrimination on the labor market from immigrant communities with Arab or Muslim background.

[3] We wish to thank an anonymous reviewer for suggesting the example of an anti-discrimination lawsuit as a type of resource cost due to discrimination that is not necessarily antisocial.

area as one where laboratory methods offer a particularly attractive approach for generating primary data that are relatively free from many of the confounds typically present in studies based on field data.

An exhaustive account of the different types of discrimination is beyond the scope of this paper, but it is worth noting the contributions of laboratory methods in this area. Examples of laboratory studies aimed at studying the determinants of discrimination include: Holm (2000), Anderson and Haupert (1999), Fershtman and Gneezy (2001), Fryer et al. (2005), Dickinson and Oaxaca (2009, 2014), Slonim and Guillen (2010), Castillo and Petrie (2010), Rödin and Özcan (2011) and Falk and Zehnder (2013); see also Anderson et al. (2006) and Lane (2016), for surveys. As noted above, laboratory methods are a methodological alternative intended to facilitate identification of the determinants of discrimination (Giuliano et al., 2009, is an exception) by isolating the key variables of interest, thus facilitating causal inference. Laboratory research has shown that statistical discrimination may result from risk aversion, mistaken stereotypes, incomplete information, or assessment bias (Anderson and Haupert, 1999; Davis, 1987; Fershtman and Gneezy, 2001; Dickinson and Oaxaca, 2009; Castillo and Petrie, 2010), and a another laboratory study found that discrimination may lead to hiring as well as wage-based discrimination (Dickinson and Oaxaca, 2014). Finally, a recent paper by Grosch and Rau (2017) focuses on discriminatory pay and its impact on antisocial behavior in a laboratory experiment study. Relative to Grosch and Rau (2017) we implement a stronger group identity manipulation, we use a decision task focused on reciprocity as a key mechanism behind in-group favoritism, and our money burning game is able to separate targeted from indiscriminate resource destruction among the unemployed.

As noted above, our focus in the present paper is on taste-based discrimination because our design leaves little room for statistical discrimination. This is the case because our experimental employers receive precise information regarding each worker's productivity (i.e., a cost function). Nevertheless, we acknowledge that employers may form beliefs that anticipate in-group reciprocity, and this may constitute a weak form of statistical discrimination. In our laboratory environment, group identity carries no relevant information content or cost advantage, thus removing common explanations from consideration in explaining our results. Taste-based preference for in-group members or the expectation of worker reciprocity would be the only identifiable reasons to show favoritism by group identity in such instances. Nevertheless, these two reasons highlight how both a preference and statistically based component may potentially contribute to in-group favoritism. One may simply prefer making favorable choices towards in-group members, or one may do so due to a belief that in-group reciprocity is stronger than out-group reciprocity—the line is somewhat blurred between taste-based and what is akin to statistical discrimination. We cannot and do not attempt to disentangle taste-based from this form of belief-based statistical discrimination, assuming it may exist, in our data.

Another relevant stream of literature is the laboratory research on group identity formation and its effects on behavior. Research has found that the more salient the in-group membership status, the larger the impact on behavior (Charness et al., 2007). Notably, a recent meta-analysis (Lane, 2016) focused on discrimination experiments with a group identity component and highlighted that both preference- and statistically-based discrimination have been found across laboratory studies using identify groups. One conclusion reached was that artificially induced group identities seem to generate relatively strong evidence of discrimination in laboratory studies.

Eckel and Grossman (2005) reported that group identification increased cooperation in a public goods game, while Chen and Li (2009) found that in-group members were more forgiving and more interested in maximizing welfare of their particular group.[4] Other recent research also reports significant in-group favoritism (e.g. Chen and Chen, 2011; Chen et al., 2014; Currarini and Mengel, 2016; see Hewstone et al., 2002 for a survey). Of particular relevance to our work is the Chen and Chen (2011) result that effort coordination increased to high levels when group identity was more salient. Due to our belief that reciprocity concerns may be at the heart of in-group favoritism, we use of a gift-exchange environment in our design. The Chen and Chen (2011) results suggest that positive reciprocity by in-group workers is likely in gift-exchange effort environments, which would imply that employers may rationally choose to favor in-group workers (i.e., discriminate against out-group workers).

Group identity is not without its drawbacks, as has recently been noted in the literature. Hostility towards out-group members may not always manifest (Goette et al., 2006), but there is an apparent dark side to group identity that may at times lead to vindictive or antisocial actions if certain triggers are present (e.g., intergroup competition, see Goette et al., 2012). Surprisingly, in-group favoritism may also increase norm violations by in-group members (Bernhard et al., 2006a). Our environment will allow us to examine and test distinct facets of the in-group favoritism hypothesis.

A key way in which our study contributes to this discussion of the dark side of group identity is by examining spillover effects of favoritism via a money burning game. Recent behavioural findings suggest that antisocial tendencies do exist in such games (see Zizzo and Oswald, 2001; Zizzo, 2003; Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011). These studies have provided strong evidence that people may be willing to harm others even in the absence of immediate or future expected monetary return. These papers have also shown that the desire to burn money is mainly rooted in inequality aversion and fairness concerns. In a seminal paper, Zizzo and Oswald (2001) designed a game where subjects could reduce (burn) other subject's payoffs at a personal cost.[5] Despite the personal cost, the majority of subjects chose to destroy some

[4] Others have shown that trust may increase within groups, which points to another potential benefit of showing preferences towards one's own group (Glaeser et al, 2000; Eckel and Wilson, 2004; Bernhard et al, 2006b; Goette et al, 2006; Falk and Zehnder, 2013; Buchan et al, 2008; Fiedler et al, 2011). This highlights that another rationale for discrimination may be to increase social capital within a group.

[5] There is a substantial literature on altruistic peer punishment building on Ostrom et al. (1992) and Fehr and Gächter (2002). The money burning component of our design differs from this literature in several important aspects. Altruistic peer punishment is usually the result of social norm violation,

portion of others' payoffs. Consistent with models of inequality aversion, the authors found that subjects burn money mainly to reduce inequalities: Higher payoff subjects experienced more money burned compared to lower payoff subjects. Such results suggest that those perceived to be in a favored position may have more money burned by others.

3. Experiment design

3.1. Preliminary phase—social preference elicitation and group identity induction

Before the main hiring and effort choice experiment, subjects participated in a 2-part preliminary phase. First, we used an existing procedure to generate common measures of social preferences (Blanco et al., 2011). Specifically, a measure of disadvantageous inequality aversion (i.e., "envy") is derived from ultimatum game responder choices, and a modified dictator game is used to generate a measure of advantageous inequality aversion (called "guilt" by Blanco et al., 2011, but can be considered a proxy for altruism. See instructions in Appendix A for further details).[6] Decisions in the social preference elicitation tasks were incentivized,[7] but participants were not informed of the preliminary task outcome until the end of the experiment in order to avoid wealth effects.[8] Furthermore, the lack of context in these preliminary tasks helped avoid the potential for behavioral spillovers into the main experiment.

During a second part of this preliminary phase, we induced group identities. These group identities are a relevant variable of interest in our design. Each experimental "society" consists of 8 subjects divided into two groups of 4 subjects each. Groups were formed based on similarity of choices in a movie preference task. More specifically, participants were presented with a set of choices between two movies – drama and comedy. Following those questions, we matched together the four participants who chose comedies (respectively, dramas) the most often. Participants were informed that their identity group members were similar in terms of movie preference, but they were not given any more specific information regarding the algorithm underlying the matching procedure. Then we increased the saliency of group identity by asking matched participants to choose a group name from among a predefined list of sea/ocean name options to represent the group's "identity" (e.g., group "Atlantic", group "Baltic").[9] Our choice to induce otherwise meaningless group identities rather than use subjects' natural identities (e.g. gender, ethnicity or social background) was intended to increase control over the group assignments and limit selection concerns in the data.[10] Thus, throughout the experiment subjects are assigned to a *society* that includes two different *identity-groups*.[11]

3.2. Main experiment phase

Our experiment consisted of four treatments that differed on two dimensions: worker employment assignments (random or based on rankings), and the effort cost of the workers (homogeneous or heterogeneous). The specific roles within each 8-subject experimental "society" are: 2 employers, 4 workers, and 2 unemployed. The baseline treatment, called *Homogeneous Ranking*, consisted of three stages that involve distinct decisions. In Stage 1, each subject made decisions in the role of an employer who must hire two workers who will each make an effort choice affecting the employer's monetary payoff (e.g. Sutter and Weck-Hannemann, 2003). As a potential employer, subjects ranked the other 7 members of his/her society from most (rank = 1) to least (rank = 7) preferred. Information on the group identity of each subject was common knowledge when making ranking decisions, and these rankings were then used to form firms within the society for some treatments.[12]

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typically within a public goods social dilemma. Norms in the money burning game are not comparable to contributions in a public goods environment, and the target of the money burning is not one's unemployed peers. Rather, the target of one's punishment in our experiment are either other workers and/or the employer, which represents punishment upstream in the relationship hierarchy.

[6] Inequality aversion stipulates that individuals care about the distribution of monetary payoffs in addition to their own payoff. Specifically, an inequality averse individual prefers equal monetary payoffs for all players, though some may have a differential aversion to inequality depending on whether it benefits (advantageous inequality) or harms oneself (disadvantageous inequality). Models of inequality aversion were first proposed by Bolton (1991) and refined by Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). Using the Blanco et al. (2011) tasks, we calculate the α and β parameters for each subject described in Fehr and Schmidt (1999) as measures of disadvantageous and advantageous inequality, respectively.

[7] More specifically, one row of the payoff matrix is randomly selected from each game for payoff.

[8] To avoid possible contamination of the main results due to this initial elicitation, we also note that wording of the social preference elicitation task was more neutral relative to the labor-context of the main experiment, and participants were not made aware of their payoffs regarding the social preference elicitation until the end of the experiment. It is also the case that what is really of interest to us is the main results comparison across treatments (and all subjects received the same elicitation task initially).

[9] Group name choice was accomplished using a 3-min chat feature in the computer interface such that participants could only interact with other members of their identity group. It was forbidden to reveal one's true identity. At the end of the 3-min chat, group name was selected by majority rule. If no agreement was reached, a random name was assigned to the group. However, agreement was reached by all groups without exception.

[10] One may expect that using movie preferences to match participants could lead to gender segregated groups. We do not observe unbalanced gender distribution among identity groups (i.e., movies tastes did not differ by gender).

[11] Identity-groups are not to be confused with a subject's individual randomly assigned ID number used to label a specific subject in the anonymous interaction environment. These ID numbers are randomly re-assigned in each stage of the experiment (i.e., it is not possible to track a specific subject across experiment stages).

[12] Specifically, subjects were told that those assigned a preferred rank are more likely to be recruited as a worker for that subject, should he/she be randomly assigned as an employer.

Subjects were fully aware that these rankings would be binding if randomly assigned as an employer in the following stage of the experiment.

Once all employers submitted their rankings, firms composed of an employer and two employees were formed using a two-step mechanism similar to the one suggested by Bogomolnaia and Jackson (2002) (see also Castillo and Petrie, 2010). In step one, the first employer (called A1) was randomly chosen by the computer and matched with her/his two preferred employees based on her/his ranking. Thus, a first firm was formed with this employer and her/his two best-ranked workers (called worker B1 and B2). In a second step, the second randomly chosen employer (called A2) was matched with her/his two most preferred employees (called B3 and B4) from among the remaining four participants who had not yet been assigned to the first employer in step one. The two participants not assigned to a firm were assigned the role of unemployed workers (unemployed worker C1 and unemployed worker C2, respectively). The two unemployed workers in each group did not take part in Stage 2 of the game and received a fixed payment of 5 EMU (Experimental Monetary Units), which was analogous to unemployment insurance.

The second experimental stage (Stage 2) consisted of a gift-exchange game between employers and their respective workers. Employers assigned (potentially unequal) wages to the workers of their respective firm. Wage options were $w = 16$ or $w = 32$, for the employer. We used the strategy method to elicit employers' decision for each potential identity-group composition of the firm. Workers then chose an integer level effort $e \in [1, 4]$ for each potential wage distribution within the firm and for both of the possible employer identity-groups.[13] Employed workers faced the same marginal cost of 5 EMU for each effort unit chosen, with the cost of effort function given by:

$$C(e) = 5e - 5 \tag{1}$$

Employer profits were a function of the two employee effort levels e_1, e_2 and the wages paid to each employee, w_1, w_2 according to the following function:

$$\prod_{\text{employer}} (e_1, e_2, w_1, w_2) = 32(e_1 + e_2) - w_1 - w_2 \tag{2}$$

Each worker i's payoff was given by:

$$\prod_{\text{worker}} (e_i, w_i,) = w_i - C(e_i) \tag{3}$$

Note that unemployed workers did not participate in Stage 2 and instead received a fixed amount of 5 ECU, which can be viewed as unemployment insurance.

In Stage 3, unemployed workers took part in a money burning game. At the time subjects made ranking, wage, and/or effort choices, no one was aware that there would be a third stage of the experiment. Here, unemployed workers can either target specific individuals in the society (at a relatively high cost — 1 EMU paid burns 5 EMU of a specific target individual) or burn money of both employers and workers without distinction (for a relatively low cost — 1 EMU paid burns 2 EMUs of other employers' and workers' payoff).[14] This Stage 3 money burning game allowed unemployed workers to express their negative emotions, such as anger, for not having been hired in a way that is costly to the society. We considered this to be important in evaluating the overall impact of discrimination or favoritism in the labor market.

A second treatment, *Homogeneous Random*, was similar to *Homogeneous Ranking* (described above) with the exception of Stage 1, which was replaced in *Homogeneous Random* by a simple random assignment of workers as either employed by a firm or unemployed. Specifically, in *Homogeneous Random* two participants were first selected at random by the computer to be employers, and then among the 6 remaining members of the experimental society, four subjects were randomly assigned as workers to employers A and B (two workers each). Thus, the remaining two workers were unemployed due to random selection.[15] It was apparent that in *Homogeneous Ranking* an unemployed worker might attribute this condition to the preference of employers, which allows us to examine how favoritism in hiring may impact decisions relative to the *Homogeneous Random* treatment.

Finally, the other two treatments, *Ranking Heterogeneous* and *Random Heterogeneous* replicated the first two treatments described with the exception of heterogeneity in the cost of effort functions (i.e., the productivity) of the workers. Specifically, in the *Heterogeneous* treatments, each society of 8 subjects was divided into 4 high productivity (low effort cost, $C(e) = 3e$-3) and 4 low productivity (high effort cost, $C(e) = 5e-5$) individuals distributed equally across the two identify groups. Once the employers were randomly selected from within the society, this effort cost information was not applicable to the employer subjects and the remaining 6 members of society always included 3 high and 3 low productivity workers.

By comparing decisions in *Homogeneous* and *Heterogeneous* treatments, we can investigate whether high productivity may alter the likelihood of employer in-group favoritism and/or whether unemployed workers consider the heterogeneity in

[13] Our choice to use strategy method elicitation (i.e., contingent choices) represents a trade-off between the amount of data generated versus the emotional arousal potential of the decision. Brandts and Charness (2011) reported in a recent survey that treatment effects found with contingent choices were always found also when using direct response elicitation. Because direct response elicitation likely involves a more hot emotional state than strategy method elicitation, our results may represent conservative estimates of the treatment effects if one believes that a more unconscious or emotional response partly generates in-group favoritism.

[14] A subject choosing the "burn with no distinction" option was not allowed to individually target subjects for additional money burning.

[15] Note that identity groups are irrelevant for the matching algorithm in the *Random* assignment treatments.

Table 1
Experimental sessions.

Sessions	Treatment	# Participants
1; 2	Homogeneous Ranking	48
3; 4	Homogeneous Random	48
5; 6	Heterogeneous Ranking	48
7; 8	Heterogeneous Random	48
	Total number of participants:	192

worker productivity when making money burning choices. Recall that unemployed workers earned a fixed payoff of 5 EMU, while the range of possible payoffs to employed workers was [1,32] EMU for high cost of effort workers, and [7,32] EMU for low cost of effort workers. The range of possible payoffs for the employers was [0,224] EMU.

3.3. Procedures and parameters

The experiment consisted of 8 sessions conducted at the CREM-CNRS (LABEX-EM) institute of the department of Economics of the University of Rennes 1 in France. Summary information about the 8 sessions is shown in Table 1.

A total of 192 undergraduate students in management, economics, law, medicine, arts and sciences were recruited via the ORSEE software (Greiner, 2004). Participants earned on average 15.52€, including a show-up fee of 5€. During the experiment, all payments were expressed in experimental currency units (EMU), and are converted to Euros at a predetermined conversion rate of 5 EMU = 1€. Some of the participants may have participated in experiments before but, to our knowledge, none had experience in any experiment similar to ours. No individual participated in more than one session of this study.[16] On average, sessions lasted about 75 min including instructions and payment of participants. The experiment was computerized using the Z-tree software package (Fischbacher, 2007).

3.4. Theoretical framework

Our identification of testable hypotheses will be facilitated by introduction of a simple theoretical framework. Absent the introduction of behavioral considerations, our game is one in which workers would choose minimal effort (since effort is costly), employers anticipate this and make minimal wage offers, and unemployed workers do not burn money because it is costly to do so. However, here we present a framework that incorporates moral motivations (see Figueres et al., 2013 for details on such models), reciprocity (see Rabin, 1993, for a formal model incorporating fairness into a utility function), and taste-based discrimination (introduced in Becker, 1957). Such considerations capture relevant features that may impact choices of workers and employers, as well as the unemployed, in our experimental environment. Our framework will consider the importance of group identity as well.

We present hypotheses in the next section in the chronology of the experiment sequence, but in order to understand some theoretical underpinnings of importance let us first consider the gift exchange game and proceed backwards through the game. As needed, we will index one's in-group as group A, and so group index B will refer to the out-group. In the final stage, worker i choose effort level e_i and has utility function given by:

$$U_i\left(e_i, w_{ij}\right) = w_{ij} - c(e_i) - \nu_i(e_i - \widehat{e_i}), \text{ with } \widehat{e_i} = \widehat{e_i}\left(\widetilde{e_i}, w_{ij}\right) \text{ and } i, j \in [A, B] \tag{3}$$

Here, w_{ij} is the wage employer j offers worker i, $c(e_i)$ is worker i's cost of effort function, and $\nu_i(e_i - \widehat{e_i})$ is one's "moral obligation" utility component. The moral obligation function generates disutility for effort deviations from one's moral ideal, $\widehat{e_i}$. Importantly, the moral ideal is a function of a moral imperative, $\widetilde{e_i}$, as well as the wage offered by the employer, and so the wage offer can be viewed as a signal of the kindness (or malice) of the employer towards the worker, in the spirit of Rabin (1993). Additional details are given in Appendix B, but under reasonable assumptions (e.g., one's moral obligation increases in w) then we show that there exists a positive relationship between wages and effort, i.e., $\frac{\partial e_i^*}{\partial w_{ij}} > 0$ (see Predictions 3 at the end of Appendix B).

In the game-stage that precedes effort choices, the firm chooses wages. Firm a will set wages w_a and w_b to maximize:

$$\begin{aligned} &U_a(w_a, w_b, e_a, e_b, L_a, L_b) \\ &\quad = Q(e_a(w_a)L_a, e_b(w_b)L_b) - w_aL_a - w_bL_b - \nu_a(w_a - \widehat{w_a}) - \nu_b(w_b - \widehat{w_b}) - dL_b \end{aligned} \tag{4}$$

Here, $Q(.)$ is the production function, which depends on worker effort and size of the labor force, L, for both in-group (a) and out-group (b) workers. Moral motivation is now separated based on the group identity of the worker, where we assume moral motivation towards in-group workers is greater, $\nu_a > \nu_b$. Finally, the parameter d represents Becker's (1957) coefficient

[16] The ORSEE recruitment software allows us to clearly identify students who have already participated to a similar game. However, we acknowledge that we cannot totally rule out the fact that they might have played a similar game in another university/institution, though we consider this very unlikely.

of discrimination. Thus, for each additional worker the employer hires into L_b will be treated as if that worker's wage is $w_b + d$. The constraint is derived from the workers' maximization first-order conditions. Under reasonable assumptions (see Appendix B for details) the firm will show in-group wage favoritism and set wages such that $w_a^* > w_b^*$. However, this framework leads to the result that the discrimination coefficient does *not* impact wage choices but rather employment choice through L, which is the first stage of the gift exchange game. When employers maximize (4) with respect to L_a and L_b, this dictates the Nash equilibrium optimal hiring (or non-hiring) choices, h, such that $L_i^* = h_i^*(L_i, d)$ for $i \in [a, b]$, and $h_a^* > h_b^*$ may result from $d > 0$ (see Appendix B for details). In our environment, hiring preferences are only captured in the ranking choices made in *Ranking* treatments.

Finally, the money burning game can be considered in isolation in our environment since participants were not aware of it at the time of making initial strategy method choices over wages and effort levels.[17] Let utility for unemployed workers be defined as:

$$U_i^{\text{unemployed}} = z - c(b_{AA}) - c(b_{AB}) - \nu\left(b_{AA} - \widehat{b_{AA}}\right) - \gamma\left(b_{AB} - \widehat{b_{AB}}\right)$$
$$\text{with } \widehat{b_{ij}} = \widehat{b_{ij}}\left(\widetilde{b}_i, h_i\right) \text{ for } i, j \in [A, B] \tag{5}$$

where z represents unemployment insurance (payoff), $c(b_{ij})$ is the cost to unemployed worker i to burn money of employer j, the functions ν and γ represents a disutility to any deviation from one's moral obligation to burn money of in-group and out-group individuals, respectively, and $\nu > \gamma$. Appendix B derives the sufficient conditions for money burning, which is assumed to be a function of both the moral imperative to burn—this may be set to zero, for example—and the hiring (or non-hiring) choice, h. Note that this framework would not predict money burning towards employers in the *Random* treatments, nor would it predict the burning of other employee's money (such may result if payoff inequality concerns were more formally modeled, which we do not do here).

3.5. Hypotheses

Here, we establish hypotheses regarding outcomes in each stage of our gift exchange game, as well as for money burning. We appeal both to predictions from our theoretical framework as well as from established empirical results in the literature, and we connect each hypothesis in this section to the specific "prediction" identified in the Appendix B derivations. Note that due to the 2×2 design we employ, each hypothesis below is conditioned on holding the other factor constant.

Consider first the hiring decisions in the *Ranking* treatments. If participants do not have discriminatory or favoritism based preferences, they should view group identities as irrelevant when assigning ranks — they should assign ranks randomly. However, one may conjecture that employers may have distaste for hiring people not belonging to their own group (Becker, 1957; Lang, 1986; Athey et al., 2000), or a preference for hiring in-group members as research has documented (Bouckaert and Dhaene, 2004).[18] Our first prediction (Appendix B) states that employers may only hire in-group workers given a sufficiently high discrimination coefficient, d. In addition, one may also reasonably expect that individuals may be more likely to hire in-group individuals if they anticipate reciprocal higher effort in the gift exchange game (i.e., higher effort than out-group workers). However, in-group favoritism may be offset in the *Heterogeneous Ranking* treatment if employers care more about high productivity (i.e., low effort cost) than group identity. Our hypotheses are summarized below in H1:

H1a. In *Ranking* treatments, preferred ranks will be assigned to in-group subjects.

H1b. In-group favoritism in rankings will be lower in *Ranking Heterogeneous* compared to *Ranking Homogeneous* due to worker productivity differences.

Our second set of hypotheses describe the expected impact of in-group favoritism (or out-group discrimination) on wage choices. These hypotheses are derived in Appendix B and result from a stronger moral obligation to in-group workers. We also conjecture that there may be a trade-off between hiring and wage discrimination, which may imply increased wage favoritism in *Random* treatments where employers cannot engage in hiring discrimination. This trade-off between hiring and wage discrimination is quite intuitive and was found in Dickinson and Oaxaca (2014), and Prediction 2b (Appendix B) identifies higher in-group wages as a result of stronger moral obligations to in-group workers.

H2a. Wages offered to in-group subjects will be higher than wages offered to out-group subjects (i.e., in-group wage favoritism).

H2b. In-group wage favoritism will be higher in *Random* treatments where there is not an additional opportunity to show favoritism in hiring.

These first two sets of hypotheses focus on in-group favoritism impact on the dimensions of subject rankings and wage offerings. The remaining two sets of hypotheses focus on reciprocity effects of the workers (hired and unemployed).

[17] This incomplete information may seem unreasonable, but one can consider it a reasonable approximation of a hiring and wage choice decision environment in which the employers do not take into account actions by unemployed workers when making their choices.

[18] In-group favoritism and out-group discrimination have been very robust findings in the social psychology literature (Tajfel et al., 1971; Billig and Tajfel, 1973; Turner and Brown, 1978; Vaughan et al., 1981; Diehl, 1988; Pratto and Shih, 2000).

A large body of research documents positive reciprocity in numerous settings (including gift exchange experiments, see Fehr et al., 1997). Recent research also finds that individuals display more positive reciprocity towards in-group members (Chen and Li, 2009). Our theoretical framework predicts a positive wage-effort relationship that results from moral obligations and wages being interpreted as acts of kindness that increase one's moral ideal level of effort (Prediction 3a in Appendix B). This leads to hypotheses H3:

H3a. In-group workers will choose higher effort levels than out-group workers.

H3b. The will be positive wage-effort reciprocity, and this relationship will be stronger between in-group employer-workers.

Finally, our last hypothesis concerns money-burning decisions. Because burning money is costly, selfish and rational players should never burn money. However, individuals may have distributional concerns that influence their decisions in the money burning stage. Specifically, being unemployed may create dissatisfaction, which would be heightened when unemployment results from the intentional ranking choices. Thus, the unemployed may be more willing to sacrifice a part of their payoffs in order to burn employers' money in *Ranking* treatments, even when there are no monetary gains from doing so (Rabin, 1993; Falk and Fischbacher, 2006). The theoretical framework predicts differential money burning in the *Random* treatments (Predictions 4b and 4c in Appendix B). We also conjecture that, in both *Random and Ranking* treatments, income comparisons may affect the decision to burn money. If the unemployed suffer from disadvantageous inequality aversion then they may be willing to burn money in order to reduce income differences (Fehr and Schmidt, 1999). The intentionality behind rankings that lead to unemployment, however, would still imply a disproportionate burning of employers' money, relative to other workers' money, in *Ranking* treatments. We also hypothesize that money burning may be higher in societies with heterogeneous productivity across workers. In this case, employed workers who are low productivity (i.e., high cost of effort) may be a particular target for money burning given that employers would not reasonably hire a low productivity worker unless favoring one's group identity more strongly than one's productivity (and profit) potential. These hypotheses are summarized as follows:

H4a. Money burning will be higher in *Ranking* compared to *Random* treatments.

H4b. Money burning in *Ranking* treatments will more often target employers than individuals in general.

H4c. Money burning will be higher in *Heterogeneous* treatments and will more often target low productivity (employed) workers, rather than high productivity workers.

4. Results

We first investigate whether participants show favoritism towards in-group members (i.e., discriminate) in hiring, wage and effort decisions. These outcome measures inform our understanding of the private incentives firms may have to show favoritism. After analysis of the employer and worker decisions, we then investigate the social costs of favoritism in the form of unemployed worker money-burning choices.

4.1. Discrimination and its rationale

4.1.1. In-group favoritism in hiring

Our data show that participants tend to rank in-group members more favorably (i.e., lower ranks). In the *Homogeneous Ranking* treatment, in-group members are assigned an average rank of 2.35 whereas out-group members are assigned an average rank of 5.24 (Wilcoxon signed-rank, $p < 0.01$).[19] The additional information on worker costs of effort in the *Heterogeneous Ranking* treatment lowers this gap—the average in-group ranking is 2.98 compared to 4.77 for out-group members, which is a significantly reduced gap compared to *Homogeneous Ranking* gap (Wilcoxon Mann-Whitney, $p < 0.01$). However, preferential ranking of in-group members remains significant (Wilcoxon signed-rank, $p < 0.01$).

Table 2 reports the results of rank-ordered logit models (Beggs et al., 1981) on the determinants of the employer's ranking decision. The dependent variable $Rank_{ij}$ corresponds to the rank employer i assigns to each potential worker j. The independent variables include the candidate's identity group, as well as a control for candidate's productivity in the heterogeneous ranking treatment (column 3).[20] Coefficients reported indicate the marginal change in the log-odds of choosing one rank higher, holding all other variables constant, but our focus is on the direction of the estimated effects. All models indicate that in-group members are ranked significantly better than out-group members (i.e., *lower* rank values). Models (2) and (3) of Table 2 report estimates in the *Homogeneous* and *Heterogeneous* treatments respectively. The significant negative coefficient on *Player j In-Group* ($p < 0.01$) confirms the existence of in-group favoritism at the hiring stage. Although in-group members are ranked better than out-group members in both treatments (models (2) and (3), $p < 0.01$ in both instances),

[19] All non-parametric tests are run on independent observations or they take into account the matched pairs nature of the data, depending on the comparison examined. We report two-sided p-values throughout.

[20] Each subject had to assign the same set of ranks (1–7) to the other subjects, and so subject-fixed variables (e.g., gender, altruism, etc) are omitted from these regressions.

Table 2
Ranking decisions.

Rank attributed by player *i* to player *j* – Rank-Ordered Logit estimates				
	(1) Pooled regression	(2) Homogeneous Treatment	(3) Heterogeneous Treatment	(4) Heterogeneous Treatment
Player *j* In-Group	−1.725*** (0.126)	−2.604*** (0.244)	−1.180*** (0.152)	−1.210*** (0.153)
Player *j* High Effort Cost	–	–	–	0.394** (0.154)
Total observations	672	336	336	336
Number of subjects	96	48	48	48

Notes: Standard errors are displayed in parentheses. *Lower ranks are preferred ranks.*
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (2-tailed).

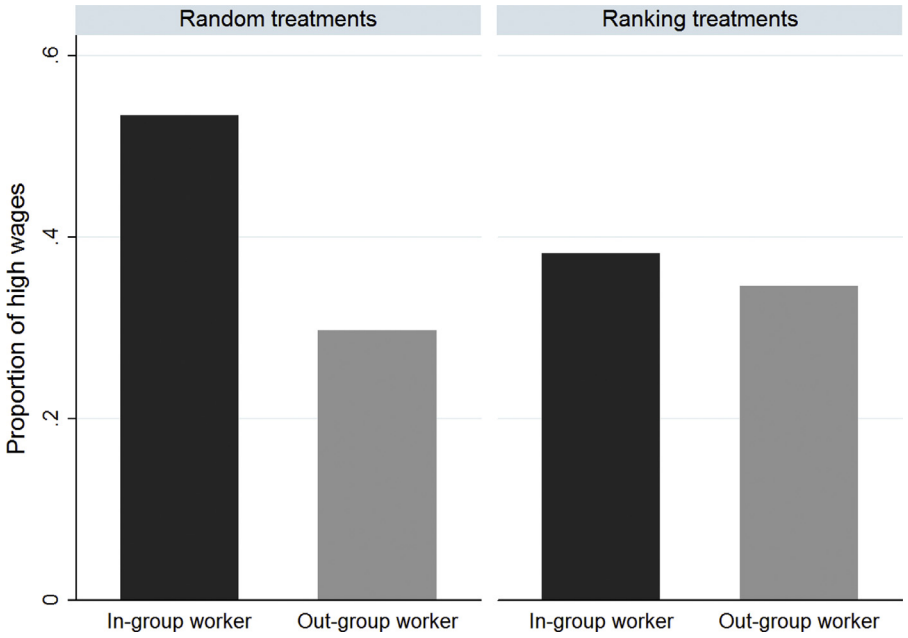

Fig. 1. Wage discrimination.

favoritism is lower in the presence of heterogeneous productivities as indicated by the positive and significant ($p < 0.05$) coefficient estimate on *Player j High Effort Cost* in model (4).[21] This supports both Hypotheses 1a and 1b. Model (4) indicates that low productivity (i.e., high effort-cost) workers are ranked worse than high productivity workers, holding group identity constant ($p < 0.05$). If one includes an interaction term between high effort cost and in-group status of player *j* in model (3), results are unchanged and the interaction term is statistically insignificant ($p > 0.10$, estimates available on request).

Our interpretation of these first results is that employers anchor on group identity, with a preference for in-group status, when no other information is available. Additional information, such as productivity differences, give employers other characteristics of the worker on which to base a ranking (i.e., an employment preference). These findings support both hypotheses H1a and H1b.

Result 1a. In *Ranking* treatments, in-group members are ranked significantly better, on average, than out-group members.

Result 1b. The extent of in-group favoritism is lower—but still significant—in the *Heterogeneous* treatments. This is consistent with the premise of Hypothesis H1b, which stated that high productivity of an out-group worker would mitigate some of the in-group favoritism.

4.1.2. In-group favoritism in wage-setting

Fig. 1 reports the proportion of high wages offered to in-group versus out-group members for both *Random* and *Ranking* treatments. Consistent with hypothesis H2a, we observe that in-group workers receive high wages significantly more often than out-group workers (X^2 test, $p < 0.01$). This wage gap is significantly positive in the *Random* treatments (X^2 test, $p < 0.01$), but not in *Ranking* treatments (X^2 test, $p = 0.509$), which supports hypothesis H2b.

[21] Also supporting this statement is the fact that the coefficient associated with "*Player j In-Group*" in Table 2 is significantly higher (i.e., less negative, which indicates less favoritism) in the *Heterogeneous* treatment (model 2) compared to the *Homogeneous* treatment in model (3) ($z = -4.95$, $p < 0.01$).

Table 3
Wage-setting decision.

Probability that i offers a high wage to j – Random-effects Logit estimates			
	(1) Pooled	(2) Homogeneous Treatment	(3) Heterogeneous Treatment
Player j In-Group	2.074*** (0.389)	10.6347** (4.628)	1.178*** (0.405)
Ranking Treatment	−0.486 (1.326)	1.758 (4.229)	1.104 (1.272)
In-Group × Ranking Treatment	−1.042* (0.533)	−8.546* (4.548)	−0.236 (0.574)
Player j High Effort Cost	–	–	0.062 (0.1331)
Heterogeneous Treatment	−1.692 (1.237)	–	–
Heterogeneous × Ranking Treatment	2.041 (1.818)	–	–
Altruism	4.763*** (1.747)	11.787** (4.333)	4.536** (2.200)
Constant	−2.302** (1.018)	−8.125* (4.437)	−3.416*** (1.173)
Total observations	624	144	480
Number of subjects	48	24	24

Notes: Standard errors are displayed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (2-tailed).
The higher number of observation for a set number of subjects in *Heterogeneous* treatments is due to the increase in decision options using the strategy method when introducing another differentiating factor (i.e., cost of effort combinations possible among an employer's two workers when making wage choices).

Table 3 reports random-effect logit estimates on the probability of offering a high wage.[22] In all treatments, we observe in-group favoritism in wage offerings (H2a). In-group workers have a significantly increased probability of receiving a high wage offer compared to an out-group worker (Table 3, Model (1), $p < 0.01$). Models (2) and (3) display separated estimates of the impact of *Player j In-Group* in the *Homogeneous* and *Heterogeneous* productivity treatments, which are statistically significant at the $p < 0.05$ (*Homogeneous*) and $p < 0.01$ (*Heterogeneous*) levels. The coefficient on the interaction variable *In-Group × Ranking Treatment* in model (2) suggests that preferential in-group wages are marginally less likely in *Homogeneous Ranking* compared to *Homogeneous Random* ($p < 0.10$; marginal support for hypothesis H2b). This is intuitive given the *Ranking* treatments allow for favoritism or discrimination in rankings, and therefore employment likelihood, prior to a wage decision. Together with previous evidence from Fig. 1, these findings may point to a trade-off between hiring discrimination and wage discrimination in the absence of productivity information on workers. Dickinson and Oaxaca (2014) also find evidence of a trade-off between hiring and wage discrimination in a laboratory setting designed to examine statistical discrimination. Results in Table 3 also show that more advantageous-inequality averse subjects, based on the Blanco et al. (2011) measure they call "altruism", offer higher wages (see footnote 6).

Our key findings in evaluating Hypotheses 2 are summarized in Results 2a and 2b.

Result 2a. In-group members receive higher wage offers than out-group members.

Result 2b. In-group wage favoritism is marginally greater with random employment assignment and homogeneous worker productivity (*Homogeneous Random*).

4.1.3. In-group worker reciprocity effects

We first observe that higher wages are reciprocated with higher effort levels, as is common in the literature (Fehr et al., 1997). Workers offered a low wage of 16 ECU provide average effort of 1.22, whereas workers offered a high wage of 32 ECU provide average effort of 1.79. This difference is statistically significant (Wilcoxon signed-rank test, $p < 0.01$) and holds in all treatments. Fig. 2 reports the distribution of effort chosen at both wage levels.

The data in Fig. 2 show evidence that workers choose higher effort levels at the higher wage rate, which support hypothesis H3b. The highest effort choices, independent of wages, are made by in-group workers, which supports hypothesis H3a. In *Ranking* treatments, the average effort exerted by in-group workers is 1.67, whereas it is 1.54 for out-group workers (Wilcoxon Mann-Whitney, $p = 0.026$).

One could expect that discrimination would be particularly costly for a prejudiced employer who hires a low productivity in-group member, rather than a high productivity out-group worker. Our initial nonparametric analysis of the data show, however, that there is no difference in effort choices in that comparison (Wilcoxon Mann-Whitney, $p = 0.726$). We also fail to find any evidence that an out-group worker who is offered a lower wage than an in-group worker chooses lower effort. Indeed, an out-group member offered a wage of 16 while the in-group coworker receives 32 does not choose lower effort compared to if both were paid 16 (Wilcoxon signed-rank, $p = 0.9847$).[23]

Table 4 reports multivariate analysis of the determinants of effort choices. We estimate random effects Tobit models to explore the influence of wage comparisons and group identity on effort. The use of Tobit models is justified by the high number of left-censored observations in the sample. Fig. 2 and Table 4 indicate a significant positive relationship between

[22] Employers always earn more in our design, and so we do not include the *envy* parameter that is used in some of the other regression analysis.

[23] Recall that the strategy-method implies each subject make choices for all contingencies, which includes choosing effort for each possible group identity and wage offer to the other worker hired by the same employer.

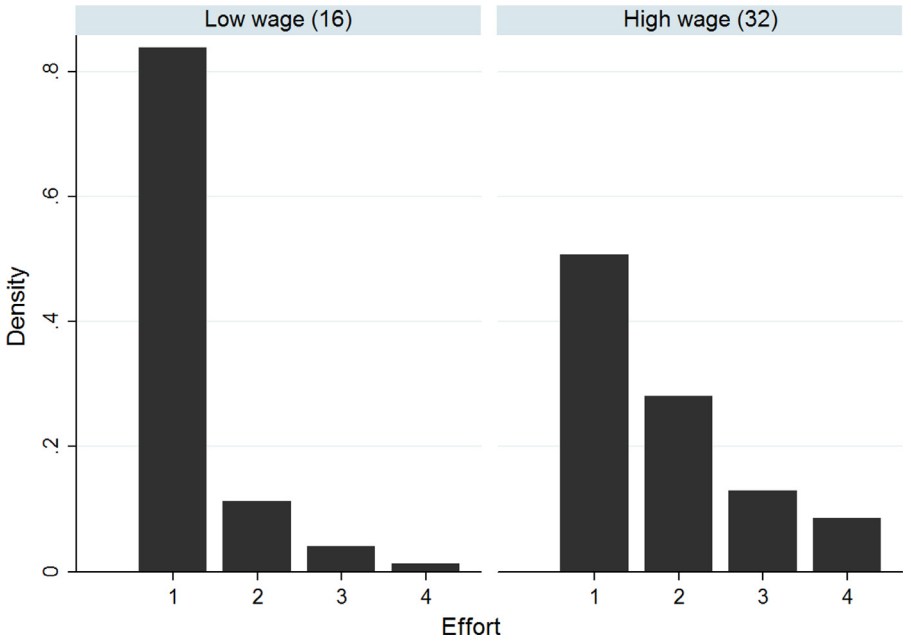

Fig. 2. Reciprocity by wage level.

Table 4
Worker effort choices.

Dependent variable = Effort choice of worker – Random-effect Tobit estimates				
	(1)	(2)	(3)	(4)
Wage	0.105*** (0.005)	0.103*** (0.007)	0.101*** (0.008)	0.101*** (0.008)
In-Group employer	0.355*** (0.071)	0.355*** (0.071)	0.325*** (0.119)	0.324*** (0.119)
In-Group coworker	0.308*** (0.071)	0.308*** (0.071)	0.308*** (0.071)	0.308*** (0.071)
Ranking Treatment	0.796* (0.4419)	0.792* (0.446)	0.796* (0.447)	0.391 (0.364)
Wage > coworker wage	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Wage = coworker wage	–	−0.017 (0.088)	−0.017 (0.088)	−0.017 (0.088)
Wage < coworker wage	–	−0.100 (0.148)	−0.100 (0.148)	−0.101 (0.148)
Low cost of effort (high productivity)	–	0.033 (0.513)	0.006 (0.515)	0.165 (0.666)
In-Group employer × Wage	–	–	0.048 (0.148)	0.048 (0.148)
Altruism	–	–	–	4.190*** (0.666)
Envy	–	–	–	0.224 (0.159)
Constant	−3.453*** (0.373)	−3.352*** (0.423)	−3.309*** (0.556)	−4.561*** (0.561)
Total observations	1,536	1,536	1,536	1,536
Number of subjects	96	96	96	96

Notes: Standard errors are displayed in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (2-tailed).

wage and effort ($p < 0.01$ in all models). This positive reciprocity finding is typical in the gift-exchange game (Fehr et al., 1997) and indicates social motivations driving reciprocity. Table 4 also identifies a marginal positive reciprocity effect of being employed in the *Ranking* treatment (*Ranking* coefficient in models (1)–(3) of Table 4, $p < 0.10$ in each case). This is consistent with positive reciprocity because only in *Ranking* treatments the employer ranking choices are responsible for a worker not being unemployed, but the effect is not very precisely measured and disappears when additional controls are added in model (4).

The estimation results indicate that effort choice is not influenced by coworker wage in any of our models. However, effort choice is significantly positively related to both the employer and coworker in-group status ($p < 0.01$ in all instances). All models in Table 4 point to group identity as an important determinant of effort choice in our experimental setting, which supports hypothesis H3a, although our theoretical framework does not predict that in-group co-workers will influence effort choices similar to having an in-group employer. Models (3) and (4) of Table 4 include an interaction variable *In-Group Employer* × *Wage* aimed at capturing any differential response from in-group workers to higher wages. We do not observe a significance difference in the positive reciprocity effect between in-group and out-group members. Our estimation results therefore only partially support hypothesis H3b. One might interpret this result to say that workers are always open to positively reciprocate higher wages, but the presence of more in-group individuals in one's work group (employers and other

Table 5
Money burning – descriptive statistics.

	Total money burnt (EMUs)	% of participants who burn	% of participants who "target burn"	% of participants who "burn all"
Random Homogeneous	17	16.66	8.33	8.33
Ranking Homogeneous	37	33.33	25	8.33
Random Heterogeneous	87	58.33	50	8.33
Ranking Heterogeneous	112	75	25	50

Notes: participants who "target burn" designate participants in the role of unemployed that decided to target at least one individual, i.e. to spend 1 EMU to burn 5 EMU from a chosen participant. Recall that participants who decide to do so do not have the opportunity to "burn all", i.e. to destroy 2 EMU to all workers and employers at the cost of 1 EMU.

workers) is sufficient to stimulate another component of altruism not captured in the standard measure that also predicts higher effort choices (see Table 4, model 4). Our findings regarding worker effort choices are summarized in results 3.

Result 3a. In-group workers choose significantly higher effort levels compared to out-group workers.

Result 3b. There is a significant positive wage-effort effect, but the effect is not moderated by group identity matches—we have only partial support for H3b.

At this point, our results show evidence of in-group favoritism by employers, even when group identity carries no practical information content or cost savings to employers. And, while the gift exchange reciprocity effect estimated is unconditional (i.e., not stronger among in-group employers and workers), we also estimate a general tendency for workers to choose higher effort levels for in-group employers. Additionally, the data indicate higher effort choices when the anonymous coworker is also an in-group member, and the higher effort levels in the *Ranking* treatments indicate that workers reciprocate the intentional rankings that led to their being employed. Put together, these results point to a rational reason for employers to show favoritism towards in-group applicants and workers. However, our last set of results looks at the important spillover effects of such favoritism in our experimental society.

4.1.4. The social costs of discrimination

Our last set of findings concern money burning decisions of the unemployed subjects in each experimental society (a total of $n = 48$ unemployed subjects in our data set). Recall that this component of our design attempts to explore a micro foundation of resource destruction that may help our understanding of how societal tensions can result from labor market favoritism.[24] Table 5 shows descriptive statistics that highlight key treatment differences. Specifically, money burning is always higher in *Ranking* treatments compared to the analogous *Random* treatment. In the *Random* treatments combined, only 8.33% of unemployed participants decide to indiscriminately burn money from employers and workers (i.e., no specific target subject). This proportion is marginally higher in the combined *Ranking* treatments, where 29.17% of unemployed participants engaged in "burn all" decisions (X^2, $p = 0.064$). This suggests that employment that results from intentional ranking choices is more likely to lead to money burning. An interesting difference seen in Table 5, however, is that the additional money burning in *Ranking Heterogeneous* compared to *Random Heterogeneous* seems due to an increase in nonspecific "burn all" decisions. In the *Homogeneous* treatments, the additional money burning due to employment by rankings seems more focused on burning money of targeted subjects. We explore these observations econometrically in Table 6.

Table 6 shows results from estimations aimed at identifying the factors that influence the probability that an unemployed subjects *i* burns money of a subject *j* (who could be an employed worker or an employer).[25] In Table 6, the dichotomous dependent variable in model (1) equals one if subject *j*'s payoff decreases due to *i*'s money burning decision. In this case, *j*'s money may have been burnt due to either a targeted money burning choice or as part of a general "burn all" decision by the unemployed subject *i*. In model (2), we focus only on whether *j*'s payoff was reduced due to a targeted money burning decision (i.e., we ignore "burn all" decisions in model (2)). Other regressors control for envy (i.e., disadvantageous inequality aversion), earnings of the target, role (employer or worker) of the target, and treatment variables.

Our results in Table 6 indicate that employers ("*j is an employer*" variable) are marginally more likely than workers to be targeted for money burning ($p < 0.10$).[26] This finding is consistent with hypothesis H4b. Money burning is marginally more frequent in *Heterogeneous* treatments, and more so when the target, *j*, has a high cost of effort, compared to money burning in the *Homogeneous* treatment. For this comparison, we test the combined coefficients on *Heterogeneous* + (*Heterogeneous*

[24] We noted in the Introduction that not all responses to discrimination are antisocial, but our specific experimental environment explores actual resource destruction as opposed to wealth transfers. As such, our lab money burning stage is more akin to antisocial vandalism or rioting, as opposed to potentially prosocial acts intended to transfer wealth (e.g., an anti-discrimination lawsuit).

[25] The standard errors presented in Table 6 are clustered at the independent observation level, which in this case is at the level of the *experimental society*. We apply this restriction in the analysis of the unemployed actions only, because unlike employers of workers, unemployed participants receive information on outcomes within the experimental society when making their money-burning decision.

[26] As we will see, the results in Table 6 are all marginal at the $p<0.10$ level (for the 2-tailed test), but it should be noted that the analysis of money burning decisions presents us with the smallest number of observations for our analysis.

Table 6
Determinants of money burning.

Probability that unemployed i harms participant j – Random-Effects Logit estimates		
	(1) Direct + indirect target	(2) Direct target only
Random Treatment	*Ref.*	*Ref.*
Ranking Treatment	1.711*	2.657*
	(1.012)	(1.565)
Heterogeneous Treatment	3.368*	2.153
	(1.780)	(1.548)
Heterogeneous × Ranking Treatment	3.298*	−4.182**
	(1.971)	(1.957)
j is an Employer	2.156**	2.333*
	(0.918)	(1.233)
Earnings of j	−0.010	0.006
	(0.052)	(0.048)
Het. Treat. × Worker j High Cost	2.032*	2.353*
	(1.083)	(1.218)
Het. Treat. × i has High Cost	0.476	2.372
	(2.106)	(1.469)
Het. Treat. × i has Low Cost & Worker j High Cost	−0.036	−0.028
	(1.050)	(1.332)
i and j are NOT from same group	0.441	0.394
	(0.280)	(0.545)
i's Envy	−0.038	−0.862
	(0.714)	(0.635)
Constant	−8.083***	−6.552***
	(1.571)	(1.716)
Total observations	288	288
Number of subjects	48	48

Notes: Standard errors clustered at the experimental society level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (2-tailed).

× *Worker j High Cost*) and find the difference significant in model 1 ($X^2 = 6.34$, $p < 0.042$) and marginal in model 2 ($X^2 = 5.74$, $p = 0.057$).

This may indicate that unemployed workers view low productivity workers who were employed as illegitimate. Money burning is also marginally more frequent in *Ranking* treatments, where participants were assigned the role of unemployed following employers' intentional hiring decisions ($p < 0.10$). The regressions also underline particular behaviors in the *Heterogeneous Ranking* treatment. When including targeted money burning and decisions to burn money indiscriminately in model (1) we find that this sort of money burning is marginally stronger in *Heterogeneous Ranking* compared to *Heterogeneous Random* ($p < 0.10$). However, when focusing only on targeted money burning in model (2), the unemployed burn marginally more in *Heterogeneous Random* compared to *Heterogeneous Ranking* ($p < 0.10$).[27]

To further address the determinants of money burning even absent hiring discrimination, we report additional estimations of the determinants of money burning in Appendix C on the separate subsamples of *Random* treatments and the *Ranking* treatments. Only in *Ranking* treatments are the employers marginally more targeted than workers ($p < 0.10$). In contrast, in the *Random* treatments when effort costs are *Heterogeneous*, there is a marginal increase in the probability of money burning, in general, and of being targeted if one is a high effort cost worker ($p < 0.10$).[28] It may be the case that heterogeneity in productivity represents an inequality that is viewed as unfair and therefore promotes antisocial behaviors even when unemployment does not result from employer preferences. Together, these results are consistent with the hypothesis that the unemployed spread out their resource destruction across employers and other workers when intentionality led to one's unemployed status. Nevertheless, while the results in Table 6 are somewhat supportive of hypotheses H4a-H4c, our design choice that allows for two types of money burning (targeted versus general) inherently complicates the analysis and interpretation of these money-burning results.

In addition to examining the determinants of individual money burning decisions, we also examine the monetary cost to society of the money burning that occurs. Indeed, because our main goal with respect to the money-burning stage was to identify the extent of any behavioral spillovers, a more direct approach to answering this question is an econometric

[27] Because unemployed are constrained in our experiment to either "burn all" or burn specific targets' money, it is somewhat intuitive that there is more indiscriminate burning in *Heterogeneous Ranking*. This is because the unemployed may feel anger towards *both* employers (due to ranking choices that led to being unemployed) as well as other employees who were favored in hiring, and "burn all" offers the most amount burned per dollar spent in our experimental design (see again Section 3.2).

[28] Testing the linear combination of the coefficients of *Heterogeneous Treatment* and *Het. Treat.* × *Worker j High Cost* in Table 6, we find that high cost workers in the *Heterogeneous* treatment are significantly more targeted than workers in *Homogeneous* treatments for both *Random* ($p < 0.01$) and *Ranking* ($p < 0.01$) treatments.

Table 7
The cost for society of money burning.

Monetary loss incurred from money burning – OLS estimates				
	(1) Pooled	(2) *Random*	(3) *Ranking*	(4) *Ranking*
Ranking Treatment	0.556* (0.344)	–	–	–
Heterogeneous Treatment	1.944** (0.716)	1.944*** (0.465)	2.083*** (0.509)	2.696*** (0.494)
Heterogeneous × Ranking Treatment	0.139 (1.037)	–	–	–
Worker	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Employer	0.677* (0.425)	0.208 (0.494)	1.146** (0.540)	1.146** (0.494)
Level of hiring discrimination	–	–	–	1.225*** (0.325)
Constant	0.247 (0.361)	0.403 (0.277)	0.646 (0.402)	−0.988* (0.569)
Total observations (Subjects)	144	144	144	144
R^2	0.2264	0.2035	0.2357	0.3677

Notes: Standard errors clustered at the *experimental society* level in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (2-tailed).
"Level of hiring discrimination" (0, 1, or 2) reports the number of employers in the experimental society who discriminate by ranking only in-group workers at the three top positions during the hiring stage.

evaluation of the costs to society, which we present in Table 7. For this, we measure the cost incurred from money burning upon each of the 144 participants in the role of employer or employed worker. The outcome measure we use is the difference between earnings before and after unemployed participants decided to burn (or not) money within the experimental society. In the following, we refer to this measure as the *cost incurred from money burning*.

The costs incurred from money burning are marginally higher in the *Ranking* treatments (Table 7, model (1), *Ranking treatment* dummy, $p < 0.10$), but significantly higher when accounting for the degree of hiring favoritism observed in the group (Table 7, model (4), *Level of hiring discrimination* variable, $p < 0.01$). The degree of hiring favoritism/discrimination is calculated within a society as the number of employers (zero, one or two) who rank only in-group workers at the three top positions during the hiring stage—we could refer to these as prejudiced employers. On average, the per-subject cost incurred from money burning is 0.886 EMU in absence of discrimination. The cost increases to an average of 2.72 EMU when the society is composed of one prejudiced employer and 2.62 EMU when there are two prejudiced employers.

Our findings confirm that the monetary loss to the experimental society from money burning is marginally higher in the *Ranking* treatments (H4a). They also suggest that the presence of initial inequality in productivity (i.e., *Heterogeneous* treatments) significantly increases these money-burning spillover costs (H4c) as evidenced by the positive an significant coefficient on the *Heterogeneous* treatment dummy in all models of Table 7 ($p < 0.01$ in each case). Interestingly, employers are harmed significantly more than workers in the *Ranking* treatments in models (3) and (4) of Table 7 ($p < 0.05$), where unemployment is the result of the intentional ranking choices of employers. This is consistent with unemployed subjects using targeted money burning as an explicit punishment mechanism in addition to the general money burning that can be exercised at lower marginal cost per EMU burned. It is likely the case that targeted money burning is favored when only the employers are the object of one's discontent. However, when productivity differences among workers are also present, unemployed workers may be frustrated or angry with both employers and other workers, and it is more cost effective to burn additional resources with an indiscriminate "burn all" decision in our design. Model (4) in Table 7 also estimates that money-burning costs are a direct and increasing function of the level of favoritism identified in the experimental society.

Overall, we find support for hypotheses H4a–H4c and, importantly, these results highlight how labor market discrimination or favoritism may lead to undesirable spillover effects).[29] The evaluation of Hypotheses 4a–4c are summarized as follows:

Result 4a. In *Ranking* treatments, money burning is marginally higher. Indiscriminate money burning occurs more frequently in *Heterogeneous Ranking* than *Homogeneous Ranking*.

Result 4b. Employers are targeted marginally more than workers for money burning in general, and experience significantly higher money burning losses in the *Ranking* treatments.

Result 4c. Money burning losses are significantly higher in *Heterogeneous* productivity treatments.

5. Discussion

A unique experimental design permitted us to investigate not only the private incentives of favoritism (or discrimination), but it also allowed us to evaluate a type of spillover costs on society. Consistent with previous findings, we found that the

[29] We acknowledge that there are multiple reasons why individuals may riot. It is because we find evidence that the *Ranking* treatment leads to more employer-targeted money burning that we can claim that our results identify a spillover effect directly linked to favoritism (as opposed to the general discontent money burning that may result from labor productivity inequality we induce in *Heterogeneous* treatments).

conditions for the occurrence of discrimination are rather weak (Holm, 2000). Our lab design generated discriminatory treatment of individuals not associated with one's group under conditions of a contrived laboratory-induced group identity. An alternative perspective is to view this differential treatment as favoritism towards in-group members. Either way, a key objective of this paper is to highlight that favoritism based purely on group identity (i.e., no impact on costs or information from showing such favoritism) can be privately beneficial to an employer, and yet produce spillover costs to society from discontent among excluded workers. Importantly, this discontent will express itself in one way or another (as it always does), and we focused on one particular channel for expressing discontent that yields insights into issues relevant to society. Our extension of an existing theoretical framework (Figueres et al., 2013) generated testable hypotheses that were generally supported in our data and derived from the assumption that preferences incorporate concerns for moral ideals, fairness, as well as taste-based discrimination.

We opted for primary data generation with controlled laboratory experiments to more cleanly identify favoritism based purely on group identity preferences, as well as to channel any discontent into a limited set of consequential options for our subjects. In field data, hiring based on social networks likely allows for reduced communication costs or provides employers with valuable information on a candidate, both of which present a confound in assessing favoritism for the pure reason of group identity preference. Unemployed worker discontent in naturally occurring settings can also be expressed in numerous ways that are often not quantifiable and not always even identifiable. As such, for our particular research question the laboratory offers an advantage over field data and makes possible the identification of causal effects of favoritism that are difficult to identify in naturally occurring field data.

Our three key findings are: First, we found evidence that even a weakly constructed group identity can fuel favoritism or "endophilia" (Feld et al., 2016) both at the hiring stage and in wage offers. This is consistent with previous findings of in-group favoritism, such as in Chen and Chen (2011). While this documents multiple dimensions on which discrimination may operate, even in a laboratory environment (see Dickinson and Oaxaca, 2014), the potential for positive reciprocity towards employers by those hired and/or offered high wages implies that such in-group favoritism may be in the monetary payoff interest of the employer. Secondly, subjects generally reciprocated high wage offers with higher effort choices (consistent with Fehr et al., 1997), although the effect was not magnified by in-group interactions as we hypothesized. Rather, our evidence in support of stronger reciprocity among in-group members (Chen and Li, 2009) was with respect to effort responses to hiring choices. Our data confirmed that workers made higher effort choices for in-group employers, and chose marginally higher effort when hiring was due to intentional rankings. This finding suggests that employers should rationally prefer in-group workers over out-group workers, all else equal. Finally, while there may be some evidence that in-group favoritism benefits employers, we considered the spillover costs on society of unemployed workers burning resources. We found evidence of significant money burning when productivity inequality existed in society (consistent with Zizzo and Oswald, 2001), as well as when employment discrimination was more present. Thus, labor market favoritism that plausibly produces discontent is likely an important micro-foundation of more significant societal costs such as fraud or social services abuse, or more severe forms of antisocial behavior such as rioting, vandalism, or looting.

To be fair, such discrimination may also lead to presumably less antisocial forms of resource use, such as would be the case with increased antidiscrimination lawsuits. However, our design is not able to speak directly to these types of unemployed worker actions because our environment does not allow the unemployed to recapture additional resources because of the money burning choices. Doing so would also require a formal and centralized institution as opposed to the informal and decentralized nature of the money burning choices we examined. These design options represent possible future extensions of this study. One clear example of a policy intervention that may help reduce more wasteful forms of antisocial resource destruction would be laws aimed at equal opportunity protections, such as the Equal Opportunity Act in the U.S, or Article 13 of the Treaty of Amsterdam in the European Union. While the actual enforcement of such laws is another matter, our results highlight that some antisocial money burning activities may not even have a clear avenue of mitigation. Recall that our additional estimations in Appendix C reported marginal increases in money burning even when hiring discrimination was *not* possible (i.e., *Random* treatments) as long as it was known that not all were endowed with the same cost of effort (perhaps a proxy of ability endowments one may offer to the labor market).

This study is not without a few other limitations, which may suggest additional avenues to extend this research. For example, our use the strategy method was a design choice intended to maximize the data generation from a fixed set of subjects. One might argue that data from contingency decisions may differ from choice data elicited from a single decision scenario. A recent survey by Brandts and Charness (2011) indicated that most studies comparing the strategy method with direct response elicitation showed replicated results across methods. Moreover, treatment effects found using the strategy method were always observed using direct response elicitation as well. One view of the strategy method of elicitation is that is places subjects in a more "cold" emotional state compared to direct elicitation, and so behaviors like money burning may be less prevalent than if making the same decision in a "hot" emotional state, such as with direct elicitation. At the very least one can argue, given results from this recent survey (Brandts and Charness, 2011) that we could expect similar results if using direct elicitation methods.

Finally, we highlight the small number of observations on money burning decisions due to the somewhat limited ($n = 48$) set of unemployed subjects in our design. A study more focused on the money burning costs may choose an experimental design with a larger set of unemployed relative to employed subjects, as well as the introduction of multiple periods to perhaps introduce endogenous interventions and allow others to learn from money burning choices. The sensitivity of our results to elements such as these are ultimately empirical questions that can be explored in future research. Nevertheless, we

feel that our evidence for favoritism spillover costs, even in a stylized laboratory environment, highlights their importance in understanding the full impact of differential treatment in labor markets. It also highlights an important argument for why discrimination or favoritism are undesirable for society as a whole.

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Supplementary materials

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