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Self-serving behavior of the rich causes contagion effects among the poor

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ABSTRACT

In a lab-in-the-field experiment, we study how the pro-social behavior of inhabitants of an impoverished neighborhood in Namibia is influenced after being informed about the pro-social or egoistic behavior of either a rich or a poor comparison group. We find that the poor behave significantly less prosocial when they learn about the egoistic behavior of the rich. Yet, neither the rich's pro-social behavior nor information on how other poor individuals behaved affects the poor's behavior. This contagion effect is not simply driven by imitating the behavior of the comparison group, or by social identity concerns or peer pressure. Instead, our data suggest that the poor's drop in pro-social behavior is caused by the violation of a social justice norm: The poor expect the rich to be pro-social and they are surprised if they act differently. Learning about the rich's egoism even makes participants' beliefs about what should be done (injunctive norms) significantly more egoistic. Hence, the self-serving behavior of the rich causes a double damage: Society not only suffers from their low pro-sociality but also from a spread of egoism among other members of society.

1. Introduction

In many societies, wealth and economic power are concentrated among a small group. As manifested in a wide range of philosophical, religious and political thought, social norms ask this group, the rich, to serve society and to share their wealth with the less fortunate.[1] While there is evidence that the rich often comply with this expectation (e.g., Korndörfer et al., 2015; Smeets et al., 2015; Andreoni et al., 2017), there is also evidence that the rich sometimes fail to comply. This is arguably a particular concern in many developing countries in which the rich are held less accountable and constrained by institutional checks and balances (e.g., Acemoglu and Robinson, 2012; Besley and Persson, 2014). This allows the rich to engage in multiple ways of self-serving behavior, for example in form of corruption or elite capture[2]

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[1] A moral obligation of the rich to share with society can be found in the writings of many great economists and philosophers, such as Aristotle, Bentham, Marx, Rawls and Smith, and in the widely used progressive income taxation. An early manifestation of this expectation can also be found in the bible "From everyone who has been given much, much will be demanded" (Luke 12:48), as well as in Zakat in the Quran.

[2] Elite capture describes the phenomenon whereby public resources are misappropriated by a political or economic elite, at the expense of the less powerful groups.

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(e.g., Bardhan, 2002; Bardhan and Mookherje, 2005, 2006) and tax evasion (e.g., "Paradise Papers").[3] With their disproportionate influence on economic and political decisions, the self-serving behavior of the rich has obvious direct consequences for economic outcomes. In this paper, we study whether there is an additional indirect effect when other members of society learn about it. In a widely cited paper, Traxler (2010) models behavioral contagion effects caused by norm violations of moral reference groups and conjectures that the rich are a particularly influential one. However, empirical evidence is hitherto limited to self-reported survey data (Hammar et al., 2009),[4] and we are unaware of any causal evidence on this hypothesis.

We report the results of a lab-in-the-field experiment in Namibia to measure potential contagion effects from the self-serving behavior of the rich to the behavior of the poor. By experimental design as well as through complementary qualitative evidence, we rule out mere conformity or imitation effects.[5] Ruling out such confounds requires varying both the reference group and the content of the information about the behavior of others, i.e., a 2×2 design plus control. Despite the large literature studying the effects of learning about the behavior of others, the existing empirical literature does not provide such variations and thus does not answer our research question. It either does not vary the content of the information provided, i.e., the rich only behaved either self-servingly or prosocially (Ebeling et al., 2017; Kumru and Vesterlund, 2010), or uses only reference groups of the same socioeconomic status (Bicchieri and Xiao, 2009; Cason and Mui, 1998; Frey and Meier, 2004; Iriberri and Rey-Biel, 2013; Krupka and Weber, 2009; Shang et al., 2008; Shang and Croson, 2009). To close this gap, we measure the behavior of participants from a poor neighborhood after they received information about the self-serving or prosocial behavior of residents of either a rich neighborhood or their own poor neighborhood. Thus, in a controlled way, we vary both the socioeconomic status of the comparison group (rich or poor) and their reported behavior (self-serving or prosocial).

Our lab-in-the-field experiment is conducted in Namibia, one of the countries with the highest income inequality worldwide (World Bank, 2014). Differences in wealth are therefore pronounced and naturally existing. Furthermore, Namibia's economic elite is expected to share their wealth with society. This becomes most evident in the national debate on the basic income grant, which started as a pilot project in 2008. Analyzing this public discourse, Ferguson (2015) identifies an asymmetric responsibility of the rich toward the poor, who in turn have a rightful entitlement to partake in the country's wealth. Thus, our experiment combines strict experimental control with a high degree of realism (Levitt and List, 2009).

We find that information about the egoistic behavior of the rich induces the poor to act significantly more egoistically. By contrast, neither the rich's prosocial behavior nor information on how other poor individuals behaved affects the poor. The spread of egoism is therefore not a simple conformity effect. Instead, participants seem to update their beliefs about the behavior of others, as they are surprised when they learn that the rich group did not share. Furthermore, it changes injunctive norms: the rich's failure to share leads other members of society to view their own egoistic behavior as more acceptable.[6] Thus, the rich's egoistic behavior causes a double damage: Society not only suffers from their low contributions, but also indirectly from contagion effects.

2. Experimental design

2.1. Experimental set-up

Our experimental study took place in the poor Ombili neighborhood in Windhoek, Namibia's capital and largest city. The participants received information about the sharing behavior in either the Klein-Windhoek neighborhood or their own neighborhood. Both neighborhoods have pronounced differences in wealth, as displayed in Fig. 1 and expressed in the administrative statistics in Table A1. When we asked our experimental participants about their first association with the two neighborhoods, the majority names wealth differences (see Tables A2 and A3).

In order to conduct the experiment, we set up a new laboratory at the Ombili community center. Before running the experimental sessions, we informed the community leaders as well as the district counsellor about our experimental protocol without disclosing any research hypotheses. The Ministry of Home Affairs issued a research visa. We employed a team of nine Namibian research assistants, all fluent in English. Six were also fluent in Oshiwambo, the most common language in the Ombili neighborhood. The research assistants were ignorant about the hypotheses underlying our experimental study.

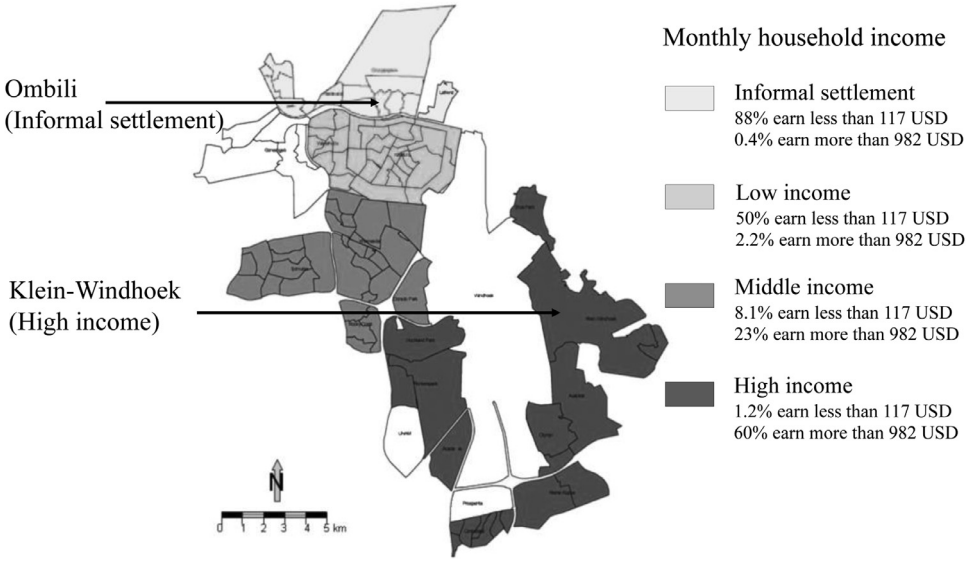
To recruit participants, the research assistants approached potential participants on the streets and asked them to participate in an experiment. If someone agreed, the recruiter would put her or his signature on a "ticket" allowing participation in the experiment within the next 40 min. The research assistants signed up 349 participants within a week. We did not allow persons without a ticket to take part in the experiment in order to avoid spillover effects between experimental sessions.

[3] https://www.icij.org/investigations/paradise-papers (last accessed on 21st of September 2020).

[4] Hammar et al. (2009) report that the most common argument among Swedes legitimizing tax evasion is that those in leading positions in society violate the social norms.

[5] Such conformity effects could stem from the poor's expectation that the rich possess better information (Vesterlund, 2003) or the poor's desire to imitate the rich in order to signal a higher status (Kumru and Vesterlund, 2010).

[6] Other self-serving exploitations of the decision context have been found in situations of uncertainty, for example regarding the consequences of one's decisions (e.g., Dana et al., 2007; Haisley and Weber, 2010; Exley, 2016), the responsibility for an outcome (Bartling and Fischbacher, 2012) or the relevant norms applying to a decision situation (Spiekermann and Weiss, 2016; Charness et al., 2019).



Source: Uhlendahl et al. (2010); Map modified

Fig. 1. Map of Windhoek by income level (Uhlendahl et al., 2010). The poor receive social information from either the impoverished Ombili neighborhood or the rich Klein-Windhoek neighborhood.

Furthermore, the research assistants never issued tickets to persons who asked for a ticket themselves. They also changed their recruiting locations within the Ombili neighborhood regularly. The experimental sessions were balanced by time of day.

To avoid security concerns regarding the handling of cash in the field laboratory, participants were paid in airtime for their mobile phones. The requirement that only owners of a cell phone could participate is not particularly restrictive, as most people own a cell phone even in poor neighborhoods (see Table A1). Airtime is a credit for pre-paid mobile phones, which can be used for texting, phone calls, internet data packages as well as small purchases. Airtime can easily be transferred for free from one user to the next. All our experimental participants knew and used airtime. We transferred all transactions in the afternoon of the same day that the experimental session took place. To strengthen the participants' trust in the transaction, they were informed that they could come the following day in case they did not receive their airtime. No participant complained about the payment process throughout the whole experiment.

We required participants to be able to read and write on their own. This allows us to run the experiment fully anonymously. Compared to experimental protocols in which subjects sit face-to-face with the experimenter while taking choices, this method should reduce the influence of experimenter demand effects and reputational motives.[7] The instructions and questions were pre-tested. We offered sessions in English and Oshiwambo, which were run simultaneously by our Namibian research assistants in two separate rooms. When preparing the instructions, the English version were translated into Oshiwambo and back to English several times to ensure that instructions were as identical as possible in both languages. We showed participants how to mark their decisions on an exemplary sheet to ensure understanding. Participants made their choices privately in cubicles made out of cardboard (see picture in A8) and left all completed paper sheets in urns by themselves. We never recorded any names of participants.

2.2. Treatments

Participants played a modified version of the dictator game (Forsythe et al., 1994), in which they could choose between two options to share an amount of 60N$ (~5US$) between themselves and the Disaster Management division of the widely known Namibian Red Cross (NRC). Option A gave participants 55N$ and 5N$ to the NRC. For readability purposes, we will label this option EGO ("egoistic") throughout the rest of the paper. Option B gave 30N$ to participants and 30N$ to the NRC. We will label this option PS ("prosocial"). The endowment of 60N$ corresponded to roughly four hours of earnings to participants (Namibian Statistics Agency, 2016). By choosing the Disaster Management division as a recipient, we guaranteed that neither subject group directly benefited from the contributions. Prior to the experiment, we signed a contract with the Namibian Red Cross (NRC) that stated our obligation to give money to the Namibian Red Cross Disaster Management Division

[7] A downside of our anonymous setting is that subjects struggled with understanding survey items using percentage shares. For this reason, we focus our analysis of beliefs that are measured through binary choices.

Table 1
Treatment overview ($N = 349$).

Reference Group	No Information	Majority EGO	Majority PS
No Reference	Baseline ($N = 81$)		
Rich Neighborhood (Klein-Windhoek)		Rich EGO Treatment ($N = 72$)	Rich PS Treatment ($N = 50$)
Poor Neighborhood (Ombili)		Poor EGO Treatment ($N = 86$)	Poor PS Treatment ($N = 60$)

Table 2
Demographics across treatments.

	Baseline	Rich EGO	Rich PS	Poor EGO	Poor PS
Age	25.987	26.764	25.388	25.329	23.897
	(8.517)	(7.280)	(9.411)	(7.259)	(6.566)
Male	0.400	0.347	0.540	0.384	0.458
	(0.493)	(0.479)	(0.503)	(0.489)	(0.502)
People in household	6.100	5.986	6.180	6.012	5.900
	(3.259)	(2.594)	(2.708)	(3.566)	(2.391)
Improved housing (brick wall)	0.325	0.319	0.265	0.267	0.350
	(0.471)	(0.470)	(0.446)	(0.445)	(0.481)
Report earning an income	0.383	0.389	0.245	0.349	0.322
	(0.489)	(0.491)	(0.434)	(0.479)	(0.471)
Employed or in training	0.272	0.458**	0.360	0.221	0.317
	(0.448)	(0.502)	(0.485)	(0.417)	(0.469)
Completed secondary school	0.827	0.847	0.900	0.906	0.883
	(0.380)	(0.362)	(0.303)	(0.294)	(0.324)
Session language Oshiwambo	0.543	0.472	0.580	0.512	0.350**
	(0.501)	(0.503)	(0.499)	(0.503)	(0.481)
Observations	81	72	50	86	60

Notes: We report the percentage share of the modus for the ordinally scaled variables. Interval variables show means with standard errors in parenthesis. For interval variables, we use t-tests (allowing for unequal variances). For binary categorical variables, we use the Chi-Square tests. For ordinally scaled variables, we use a Mann-Whitney-*U* test. All tests are two-sided. ** indicates a statistically significant difference ($p<0.05$) in comparison to the Baseline.

according to the decisions of the participants. This contract was shown to all participants before the experiment to reassure participants that their decisions were not hypothetical. No participant expressed unfamiliarity with the Red Cross.

We studied five treatments with a total of 349 participants (see Table 1) in a total of 44 sessions to identify the underlying mechanisms behind any changes in behavior. In the *Baseline* treatment, participants did not receive any information about the behavior of others. In the other four treatments, prior to making their own decision, participants received information about the majority behavior of participants either from the rich neighborhood (Klein-Windhoek) or from their own neighborhood (Ombili). In the Rich EGO treatment, we informed participants that in "a session we recently conducted [in Klein-Windhoek], most participants chose option A." In the Poor EGO treatment, the term in brackets was substituted by "here at Ombili Community Center". In the Rich PS treatment, we informed that in "a session we conducted recently [in Klein-Windhoek], the majority of participants decided for option B." In the Poor PS treatment, the term in brackets was substituted by "here at Ombili Community Center".

This 2 × 2 experimental design leads to the four information treatments Rich EGO, Rich PS, Poor EGO and Poor PS, plus a control treatment (Baseline). The social information was not deceptive. As in previous literature (e.g., Bicchieri and Xiao, 2009), it referred to a selectively chosen experimental session, which we conducted in Ombili and also in Klein-Windhoek for the purpose of gathering the social information for the experiment.

The experimental sessions ended with a questionnaire in which we elicited beliefs about social norms, participants' perceptions of both comparison groups (see Tables A2-A5), and demographics (Table 2). Most importantly, we measured injunctive norms (Cialdini et al., 1990) by asking "Which option do you think SHOULD be chosen by participants here?" Further, we asked whether and, if so, why participants were surprised by the information given to them. The questionnaire data allow us better to understand the motives guiding the behavior and to test whether participants indeed expected the rich to share with society. Further details can be found in the Appendix sections *Experimental Design* and *Instructions*.

Participants' demographics in our treatments are displayed in Table 2. Only about one third lives in improved housing (brick walls instead of corrugated iron) and reports that they earn an income. In contrast, the monthly median income in the rich district is between 1100 and 2000 USD (Uhlendahl et al., 2010). Overall, the sample is well balanced between treatments. Checking for statistical balance between control and each of the treatment requires 32 statistical tests. Only two of the 32 tests exhibit statistically significant differences in comparison to the Baseline: the share of participants who participated in Oshiwambo in the Poor PS treatment and the share of participants employed or in training in the Rich EGO

Table 3
Average treatment effects.

	(1) EGO choice	(2) EGO choice	(3) EGO choice
Rich EGO Dummy	0.163**	0.182***	0.197***
	(0.069)	(0.067)	(0.073)
Rich PS Dummy	−0.012	−0.008	0.001
	(0.116)	(0.108)	(0.110)
Poor Ego Dummy	0.081	0.080	0.090
	(0.072)	(0.067)	(0.068)
Poor PS Dummy	0.023	0.037	0.049
	(0.098)	(0.096)	(0.096)
Employed or in training		−0.069	−0.096
		(0.060)	(0.084)
Session language Oshiwambo		0.061	0.070
		(0.056)	(0.065)
Male			0.010
			(0.055)
Reports earning an income			0.066
			(0.082)
Above median age (>23 years)			−0.027
			(0.057)
Above median people in household (>6)			0.042
			(0.063)
Above median house quality (brick wall)			0.022
			(0.063)
Above median schooling (>secondary)			−0.064
			(0.082)
Pseudo R^2	0.020	0.020	0.025
Observations	349	349	349
Number of Sessions	44	44	44

Notes: The table reports marginal effects at means from a probit regression on the probability of choosing the egoistic option in comparison to the baseline. Around 0.6% of covariate values are missing and were replaced with the mode (regression 3). Standard errors are clustered by experimental session. ** $p<0.05$; *** $p<0.01$.

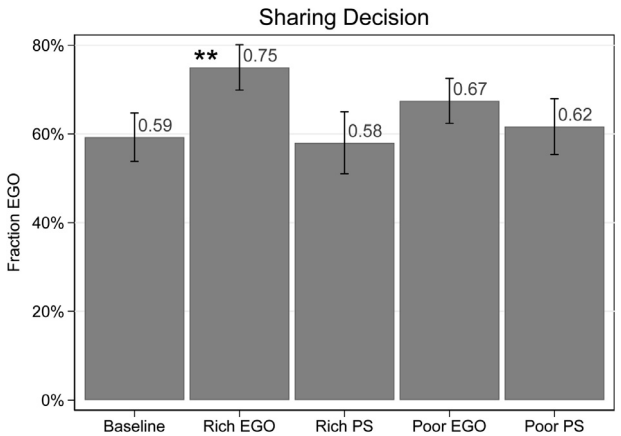


Fig. 2. Bars indicate the fraction of participants choosing the egoistic option for each of the treatments. Error bars show standard errors of the mean. ** indicates statistical significance at the 5% level in comparison to the Baseline.

treatment. When we control for these and other covariates in the regression analysis (Table 3), our main result remains unchanged.

3. Results

The participants' sharing decisions are displayed in Fig. 2. The bars show the fraction of egoistic choices for each of the treatments. In the baseline, 59% of the participants choose the egoistic option. When subjects are informed about the egoistic behavior of the rich, the share of egoistic choices significantly increases to 75% (Baseline vs. Rich EGO: $\chi^2(1,153)=4.25$, $p = 0.039$). When the poor receive the information that the rich predominantly behaved prosocially, their behavior does not change compared to the baseline treatment (Baseline vs. Rich PS: $\chi^2(1,131)=0.02$, $p = 0.887$). The share of prosocial

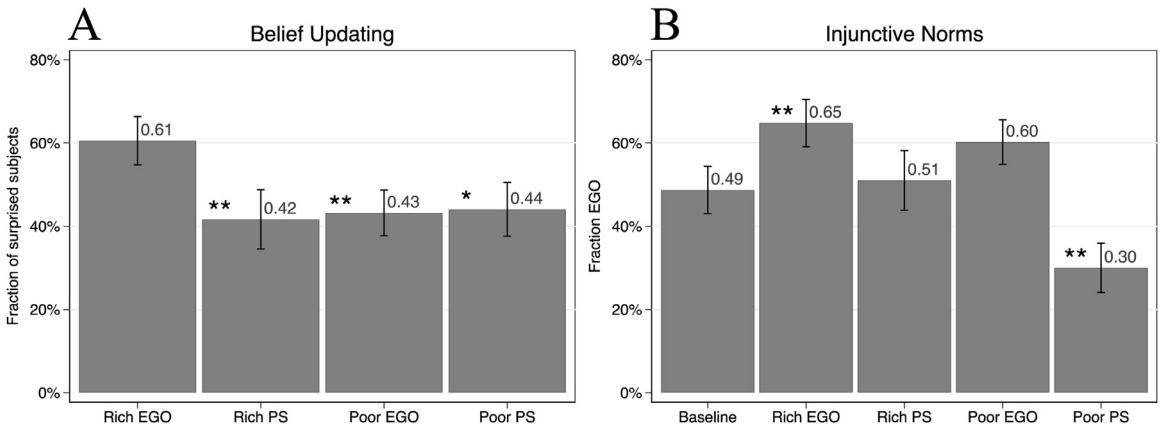


Fig. 3. Bars in **Panel A** show the share of participants being surprised by the social information across treatments. Bars in **Panel B** indicate the share of participants stating that the egoistic option should be chosen by participants (injunctive norms). Error bars show standard errors of the mean. ** indicates statistical significance at the 5% level and * at the 10% level. Differences are tested in comparison to the Rich EGO treatment in Panel A and in comparison to the Baseline in panel B.

choices is neither significantly different when subjects are informed that the poor behaved egoistically (Baseline vs. Poor EGO: $\chi^2(1,167)=1.20$, $p = 0.272$) nor when subjects receive information that the poor behaved prosocially (Baseline vs. Poor PS: $\chi^2(1,141)=0.08$, $p = 0.773$).

Table 3 reports the average treatment effects using parametric tests. We run probit regressions with dummy variables for the four social information treatments on the probability of taking the EGO choice. The regression table shows marginal effects at means. In Model 2, we only control for the two variables where we found statistically significant differences between the Baseline and one of the social information treatments. In Model 3, we include a full set of sociodemographics. We cluster standard errors at the session level to account for possible session effects (Frechette, 2012).

In Model 1, without controlling for any co-variates, the probability of choosing the egoistic option is 16.3 percentage points higher ($p = 0.019$) in the Rich EGO treatment than in the baseline treatment. This difference increases to 18.2 percentage points in Model 2 ($p = 0.007$). The specification with a full set of controls yields a point estimate of 19.7 percentage points ($p = 0.006$). Adjusting this p-value for multiple hypothesis testing using the conservative Bonferroni correction for four treatment arms (Savin, 1980) still yields a statistically significant effects of the Rich EGO treatment in Model 2 ($p^{adj.}$=0.028) and Model 3 ($p^{adj.}$=0.024). The coefficients for the other treatments remain insignificantly different from zero. In contrast to the non-parametric test (Fig. 2), we can reject equality of coefficients between Rich EGO and Poor EGO when controlling for covariates in Model 2 at the 10% level ($p = 0.068$, Wald test) and at the 5% level in Model 3 at the ($p = 0.049$, Wald test). This shows that the participants are more likely to choose the egoistic option when the information about egoistic behavior comes from the rich instead of the poor reference group.

We analyze questionnaire data in order to understand the channel behind the main results. First, participants overwhelmingly associate people in Klein-Windhoek with wealth (Table A2). Second and more importantly, subjects are surprised, and therefore update their beliefs, when they learn that the rich behaved egoistically, which can be seen in Panel A of Fig. 3.

The majority of the poor participants expect the rich to decide prosocially: 61% of participants in the Rich EGO treatment are surprised when they learn that the rich group did not share. The share of surprised participants is significantly higher in Rich EGO than in the other social information treatments, where the level of surprised participants is between 42% and 44%.[8] Note that we cannot analyze whether being surprised moderates behavior on an individual level, since we cannot identify those who would have been surprised had they received certain information from a different treatment.

Additionally, a qualitative content analysis (Mayring, 2000) of participant statements about the social information they received show more direct – qualitative – evidence that participants expected the rich to share. Three independent raters classified the participants' explanations on why they were or were not surprised about the social information. Krippendorff's Alpha (Hayes and Krippendorf, 2007) indicates satisfactory inter-coder reliability. Roughly 25% of the statements of those participants who were surprised in Rich EGO were classified as "The rich should share." Another 18% of participants explicitly relate their surprise to the wealth of the people in Klein-Windhoek (e.g., "yes, because they are rich"). Another 23% of participants merely reiterate that they were surprised (Krippendorff's Alpha: 0.74). The statements of participants in the Rich PS treatment who were not surprised paint a similar picture: 50% state that the rich have enough money to donate and 19% declare that the rich have a responsibility to donate. 19% state that PS is the right option (Krippendorff's Alpha: 0.8). In contrast, statements from the Poor EGO and Poor PS treatments do not indicate a role-specific norm for the poor to

[8] Rich EGO vs. Rich PS: $\chi^2(1,119)=4.10$, $p = 0.043$; Rich EGO vs. Poor EGO: χ^2 (1,152)=4.56, $p = 0.033$; Rich EGO vs. Poor PS: χ^2 (1,130)=3.52, $p = 0.061$.

share. Our questionnaire data therefore provide both quantitative as well as qualitative evidence that the effect of the Rich EGO treatment is driven by an – unexpected – social norm violation.

The social information also changes the participants' beliefs about what *should* be done. The bars in panel B of Fig. 3 correspond to the participants' personal injunctive norms. In the baseline treatment, 49% participants state that EGO should be chosen. When being informed that the rich behaved egoistically (Rich EGO treatment), a statistically significantly larger share of participants (65%) state that the egoistic option should be chosen ($\chi^2(1,149)=3.90$, $p = 0.048$). The injunctive norm is unaffected in the Rich PS treatment ($\chi^2(1,127)=0.063$, $p = 0.801$). Note that these results are consistent with the existence of a social norm that asks the rich to share with society. Learning that the rich share merely confirms what the poor expect of this group anyway. By contrast, learning about the egoistic behavior of the rich allows the poor to justify their own egoistic behavior: "If even the rich do not share, why should I?" Interestingly, in the Poor PS treatment, injunctive norms also change ($\chi^2(1,138)=4.93$, $p = 0.026$), but, as noted above, the actual behavior is unaffected.

4. Discussion

We conduct a lab-in-the-field experiment in an impoverished neighborhood in Namibia and use a novel 2 × 2 design to study the sharing behavior of the poor after being informed about the sharing behavior of either a rich or a poor comparison group. We find that the poor behave significantly less prosocially when they learn about the egoistic behavior of the rich. Our quantitative and qualitative data suggest that this contagion effect is caused by the violation of a social justice norm, which asks the rich to share their wealth with society. The unexpected norm violation then allows the poor to justify their own egoistic behavior. In the following, we discuss different mechanisms and alternative interpretations of our findings.

A rich literature shows that social information can influence behavior in several ways (Cialdini and Goldstein, 2004; Bicchieri et al., 2018). Our experimental design and the results narrow down the set of possible channels. We argue that the contagion effect is unlikely to be explained by theories based on peer pressure, social-identity concerns or social learning.

Because participants decide under full anonymity, we can rule out peer pressure or reputational motives (e.g., Bursztyn and Jensen, 2015; Dellavigna et al., 2017). Identity or status concerns are also unlikely to be the driving motivation behind our results. The former predict conformity to the ingroup behavior, based on the assumption that conforming behavior is driven by a shared identity (Akerlof and Kranton, 2000). Participants aim to maintain a distinct positive social identity from an outgroup: "Whatever they are, we are not" (Tajfel and Turner, 1986; Hogg, 2006).[9] A second strand of literature, by contrast, argues for conformity to the rich group because of status-seeking and upward assimilation: conforming to the rich allows the poor to conclude that they are among the "better ones" (Collins, 2000).[10] Importantly, theories based on identity and status-seeking both predict conformity to one of the two groups, regardless of the content of the social information. However, this is not what we observe. We only find conformity to the rich in the Rich EGO treatment. Identity or status concerns therefore seem unlikely to cause our treatment effects.

Another potential mechanism relates to social learning with informational asymmetry (e.g., Bikhchandani et al., 1998; Vesterlund, 2003). The poor may initially consider the Namibian Red Cross (NRC) a good charity and view the rich as better informed about the quality of charities. Therefore, the poor decide to be less generous after learning that the majority of the rich did not donate to the NRC. We tried to minimize uncertainty about the quality of the charity by choosing a well-known charity and showing subjects a contract between the University of Cologne and the NRC.[11] In addition, we do not find evidence for such a quality-signaling channel when analyzing the qualitative statements on why subjects were surprised (see the qualitative content analysis above). We thus think that informational asymmetry is unlikely to drive our results.

Would our interpretation of our main results change if participants viewed the experimental game as a public-good game? In our study, subjects split their endowment between themselves and the NRC Disaster Management, which has the basic structure of a dictator game (as in Eckel and Grossman, 1996). We chose the Disaster Management division of the NRC as a recipient to guarantee that neither subject group directly benefited from the contributions. Furthermore, there is no strategic interaction between the participants. It is nevertheless conceivable that subjects perceived the game as a prisoner's dilemma, as charitable giving can be seen as a public good and choices in our experiment are binary. In this case, the experimental results could be rephrased in the language of a prisoner's dilemma, with the prosocial choice being replaced by "cooperation" and the egoistic choice by "defection". In such situations, reciprocal altruism is a strong motivation that can lead to conditional cooperation (Fischbacher et al., 2001; Fehr and Fischbacher, 2003). Conditional cooperators only cooperate if they believe others to do so as well. In the social information treatments, we manipulate this belief. Our questionnaire results suggest that the poor are significantly more surprised when they learn about the egoism of the rich compared to other social information. This suggests that the rich are expected to be more cooperative. Such group-specific expectations have also been documented in the context of public-goods games with heterogeneous endowments: Reuben and

[9] A stronger conformity to groups that are similar has been documented by Fatas et al. (2018), Gino et al. (2009), Jetten et al. (1996) and Shang et al. (2008).

[10] Empirical evidence for such effects is provided by Kumru and Vesterlund (2010) and Ebeling et al. (2017).

[11] The contract also showed our obligation to donate according to the subjects' choices.

Riedl (2013) show that players with higher endowments are expected to contribute more in public-goods games. After updating this prior belief (the higher cooperativeness of the rich) due to the experimental manipulation, the participants should still expect the rich to be more cooperative than the poor given the same social information. As a consequence, conditionally cooperative participants should cooperate more in Rich EGO than in Poor EGO. If at all, we find the opposite. Thus, conditional cooperation alone, understood as matching the expected contribution of others, does not explain the treatment effects. However, subjects might additionally condition their contributions on the perceived fairness of the contributions of others. In contrast to the poor, defection of the rich is also a violation of moral expectations. Participants may respond to this norm violation of the rich through their own low contributions. Thus, the interpretation of the situation as a public-goods game does not change our major takeaway: the unexpected violation of a group-specific norm leads to contagion effects because participants behave egoistically themselves.

To which extent these findings are specific to our experimental context is another important question. An expectation of the rich to share can be found across many societies and is manifested the writings of great economists and philosophers, such as Aristotle, Bentham, Marx, Rawls, and Smith, and in the widely used progressive income taxation. Among Swedish taxpayers, Hammar et al. (2009) provide survey evidence showing that social norm violations of those in leading positions are the most common argument to legitimize tax evasion. Reuben and Riedl (2013) as well as Brekke et al. (2017) provide evidence from the Netherlands and Norway that student subjects with higher endowments are expected to contribute more in public-goods games. We thus believe that our findings might generalize to other populations in which the rich are expected to share. It is an interesting question for future research whether our results might generalize to a situation where the rich are not expected to share and subjects are surprised by the generosity of the rich. In this case, following the rich's behavior would be monetarily costly for the poor, which would be an important difference to the contagion effect found in this study.

More empirical work is also needed in order to understand better how moral reference groups are selected and under which conditions contagion effects can be expected. The rich may be a particularly influential reference group because their norm violations allow others to feel legitimized in their own egoistic behavior (Traxler, 2010). In our case, the responsibility of the rich thus goes beyond material contributions: they set moral benchmarks. Future work seems particularly important in light of the potential relevance of our findings to a wide range of settings, such as tax compliance, environmental protection or performing civic duties. In times when information travels fast and the behavior of the rich and powerful is at the center of (social) media attention, the detrimental consequences of self-serving behavior can quickly amplify.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2020.12.032.

Appendix

Table A1
Wealth and infrastructure differences between the comparison groups.

Household owns / has access to	Improvised Housing Unit (Shack)	Cement Block/ Brick Houses	Internet at Home	Piped Water Inside	Electricity for Lighting	Mobile Telephone
Ombili (Tobias Hainyeko)	65.8%	14.8%	2.4%	13.2%	27%	68.8%
Klein-Windhoek (Windhoek East)	0.4%	97.1%	57.6%	97.5%	99%	87.2%

Source: Namibian Statistics Agency (2011).

Table A2
First associations with Klein-Windhoek (rich neighborhood).

Category Classification	Percentage Share
People in Klein-Windhoek are rich and have high living standards	55.9%
Positive Characteristics (good, helpful, respect)	15.7%
Expressing wealth-unrelated differences	8.3%
Lacking knowledge about Klein-Windhoek	9.3%
Other types of association	7.9%
Incomprehensible statement	4.2%

Notes: We classify first associations with Klein-Windhoek given by participants in the baseline treatments, who do not receive social information.

Table A3
First associations with own neighborhood.

Category Classification	Percentage Share
People live in poverty. Bad standard of living. Help/change is needed	53.7%
Good people that respect each other	22.4%
Some people misbehave	9%
Other reasons	9%
No idea / Not understandable	6%

Notes: We classify first associations with Ombili given by participants in the baseline treatments, who do not receive social information.

Table A4
Amount of "good people" in Klein-Windhoek vs Ombili.

Amount of "good people" ($N = 81$)	Own Neighborhood	Klein-Windhoek
Most	17	28
Many	17	12
Some	29	29
Few	16	9
None	2	3

Notes: We find no difference in the share of "good people" in Klein-Windhoek in comparison to their own neighborhood according to the views of participant in the Baseline (two-sided Friedman test, $p = 0.579$).

Table A5
Perceived similarity with people from either neighborhood.

Perceived Similarity ($N = 81$, Baseline sample)	Own Neighborhood	Klein-Windhoek
Very Similar	17	6
Similar	23	14
Different	13	15
Very Different	22	25
None of the above	6	21

Notes: Participants perceive themselves to be more similar to people from own neighborhood (two-sided Friedman test, $p = 0.003$).

Details of the experimental design

At the beginning of the experiment, we informed participants in all treatments that the sessions would be conducted in both Klein-Windhoek (the rich neighborhood) and in the Ombili community center (the poor neighborhood). We then asked participants to write down whether they were taking part in Ombili or in Klein-Windhoek. This prepared the participants for the forthcoming social information. The fact that 98% of participants correctly answered that they were taking part in Ombili also serves as a successful check that participants were able to follow the instructions and could communicate in writing.

In the information treatments (Rich EGO, Poor Ego, Rich PS, and Poor PS), participants then received information about the behavior of participants from a previous session from their own poor neighborhood (Ombili) or of participants from the rich neighborhood (Klein-Windhoek). In the Poor EGO treatment, we informed participants that, in "a session we recently conducted [here at Ombili Community Center], most participants chose option A." In the Rich EGO treatment, the term in brackets was substituted by "in Klein-Windhoek." In the Poor PS treatment, we informed them that, in "a session we conducted recently [here at Ombili Community Center], the majority of participants decided for option B." In the Rich PS treatment, the term in brackets was substituted by "in Klein-Windhoek." To provide the social information without deception, we ran sessions consisting of ten participants on the street in Klein-Windhoek and selected those in which the majority behaved either prosocially or egoistically. The procedure of varying social information by disclosing choices from

Table A6
Probit regression only for "strong" beliefs.

	EGO Choice (1)	EGO Choice (2)
Rich EGO Dummy	0.165**	0.195***
	(0.070)	(0.071)
Rich PS Dummy	−0.038	−0.017
	(0.108)	(0.095)
Poor Ego Dummy	0.082	0.085
	(0.073)	(0.068)
Poor PS Dummy	−0.088	−0.070
	(0.125)	(0.128)
Employed or in training		−0.137*
		(0.077)
Session language		0.057
		(0.059)
Male		0.004
		(0.057)
Age		0.004
		(0.057)
People in household		−0.003
		(0.011)
Reports earning an income		0.086
		(0.078)
Improved housing (brick wall)		0.018
		(0.068)
Pseudo R^2	0.023	0.036
Observations	313	313
Number of Session	43	43

Notes: The table reports marginal effects at means from a probit regression on the probability of choosing the egoistic option in comparison to the baseline. Around 0.6% of covariate values are missing and were replaced with the mode (regression 3). Standard errors are clustered by experimental session. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

selected samples is common in the literature (e.g., Frey and Meyer, 2004; Bicchieri and Xiao, 2009). For the Poor EGO and Poor PS treatments, we used selected sessions from the baseline treatment.

After receiving information about the majority behavior of another group, participants were asked to guess how many participants in the mentioned session behaved prosocially or egoistically. Participants in the EGO treatments answered the question "What share of participants in that session in Klein-Windhoek [Ombili Community Center] decided for Option A?" by marking their guess on a scale from 8 out of 10 to 10 out of 10. In the PS treatments, participants were asked to guess "What share of participants in that session in Klein-Windhoek [Ombili Community Center] decided for Option B?" on a scale from 6 out of 10 to 10 out of 10. The belief range elicitation was kept equal between comparison groups, the crucial element in the social comparison theories, but varied slightly between the PS and EGO treatments. We performed a robustness check of our main results by running the regression of Table 3 only with participants who had similarly strong beliefs, i.e., by dropping those participants in the PS treatments that believed that less than 80% decided for option B. Table A6 shows that the results stay the same. The coefficient for Rich Ego is statistically different from zero ($p = 0.017$ and $p<0.01$ when adding controls). In the baseline treatment, participants were neither provided with social information nor asked for their own estimates about the behavior of another group.

All participants then played a modified pen-and-paper version of the dictator game. They could choose between two options that determined how a total of 60N$ (~5US$) would be split between themselves and the Namibian Red Cross Disaster Management, which is an established charity and well known among participants. Option A gave participants 55N$ and 5N$ to the Namibian Red Cross Disaster Management. Option B gave 30N$ to themselves and 30N$ to the Namibian Red Cross Disaster Management. The Disaster Management division of the Red Cross was chosen to avoid participants possibly directly benefiting themselves when giving to the charity.

The second part of the experiment measured beliefs on social norms. In line with the experimental manipulation, we elicited expectations in the two EGO treatments by asking what share of participants decided for option A and in the two PS treatments by asking for the share who decided for option B. In order to compare expectation in the social information treatments to the baseline treatment, we ran two different versions of the latter. One randomly selected half of the participants in the baseline treatment (baseline A) were asked what share decided for option A, and the remaining half (baseline B) were asked for the share that decided for option B. The belief elicitations were incentivized by paying an additional 1 N$ in airtime for every correct guess.

We measured empirical expectations by asking "Please guess: What share of participants here in this session decided for Option A?" We then inquired about personal injunctive norms by asking "Which option do you think SHOULD be chosen by participants here?" Further, we measure second-order normative expectations (Bicchieri, 2005) by asking "Please guess: What share of participants here in this session thinks that option A SHOULD be chosen?" Afterwards, we measured empirical

expectations for Klein-Windhoek by asking "Please guess: What share of participants in Klein-Windhoek decided for option A?" and second-order normative expectations by asking "Please guess: What share of participants in Klein-Windhoek in the mentioned session thinks that option A SHOULD be chosen?"

Participants in pilot sessions struggled with the belief elicitation by percentage shares. A debriefing revealed that several participants gave answers that were inconsistent with what they wanted to express. Many had difficulties in understanding the concept of percentage shares. Before each of the main sessions, we tried to enhance understanding of percentage shares by giving brief visual examples with apples and tomatoes. We are nevertheless still skeptical about the quality of the belief elicitation data by percentage shares and therefore focus our analysis on expectations and norms that required only binary answers. Note that difficulties of study participants in developing countries with complex psychological scales have already been thoroughly documented elsewhere (see Laajaj and Marcours, 2017). Such difficulties in understanding can possibly be overcome by sitting down face-to-face with participants and helping them mark answers. We deliberately opted against such a design, since this could have induced undesirable demand and signaling effects.

The third part of the experiment asked participants in which neighborhood they lived, as well as for their first associations with people from their own neighborhood and from Klein-Windhoek. Furthermore, we measured the perceived similarity with both neighborhoods and the amount of "good people" in either neighborhood. In the information treatments, we additionally asked whether and why participants were surprised by the information given to them (e.g., "Did it surprise you that in the mentioned session the majority of people in Klein-Windhoek decided for option A?")

The questionnaire ended with demographics containing questions on the materials from which their house is built, the type of toilet, their household income per month, the languages spoken, their age, gender, occupation, and education, how many people lived in their household, and whether they knew details about the experiment prior to participating. Lastly, we asked whether they could be contacted for future studies and for their phone number for airtime transfers.

Below are the instructions for all treatment variations. Part one and two of the instructions vary by treatment. Part three is identical in all treatments, except that, in the information treatments, we additionally asked whether and why participants were surprised by the information given to them (e.g., "Did it surprise you that in the mentioned session the majority of people in Klein-Windhoek decided for option A?")

As explained before, there exist two variations for the baseline for the second part of the instructions. In the Baseline A version, we elicit beliefs about norms by asking "What share of participants […] decided for option A?", whereas in the Baseline B version we ask "What share of participants […] decided for option B?"

Picture of laboratory session

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