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Social sampling shapes preferences for redistribution: Evidence from a national survey experiment

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A B S T R A C T

We offer experimental evidence for the effect of social sampling on redistributive preferences through a survey experiment using a probabilistic national sample in Germany. We primed respondents to think about different types of social contacts, in particular low- and high-income contacts. We find evidence for an indirect effect in which the priming task shapes preferences for redistribution through its effect on the respondents' estimates of their contacts' incomes. Respondents in the low-income (high-income) priming recalled social contacts with lower (higher) incomes, which in turn predict more (less) support for redistributive policies. The indirect effect of the low-income (high-income) priming is stronger among high-income (low-income) respondents, suggesting that our priming task elicited the social contacts whom the respondents, given their own incomes, are less likely to recall. We discuss the implications of these findings to our understanding of how social sampling shapes redistributive preferences as well as relates to social networks and ideology.

Support and opposition toward redistributive policies are one of the most studied topics in the social sciences. Scholars consequently have documented various factors that shape individuals' preferences for redistribution, such as the class composition of the society (Lupu & Pontusson, 2011), significant economic events (Ansell, 2014; Margalit, 2013), and psychological predispositions (Johnston, Lavine, & Federico, 2017). Objective economic circumstances also affect preferences, with wealthier people being less supportive of redistribution (Alesina & Giuliano, 2011).

Traditionally, such differences in redistributive preferences and, more generally, how people respond to economic inequality have been attributed to ideological and motivational factors, including self-interest. Complementary to those accounts, recent evidence suggests that at least some of this relation could stem from a very basic cognitive mechanism that allows people to construct a subjective representation of one's social environment and thereby influences social preferences: the process of social sampling.

The literature on social sampling shows that individuals' social contacts shape social judgments and political attitudes, including ones on income and wealth redistribution (Dawtry, Sutton, & Sibley, 2015; Galesic, Olsson, & Rieskamp, 2018). Higher income individuals tend to have social contacts who also have higher incomes. This overlap creates a perception that the population is richer than it actually is, which in turn is related with lower support for redistributive policies. At the same time, this literature assumes that social sampling processes are cognitive mechanisms that causally contribute to shaping political attitudes such as support for redistribution. Virtually all the studies on the topic have employed observational designs so far. Even though this assumption is plausible and many relevant other variables have been controlled for, the issue of causality remains.

Here, we provide evidence for the relationship between social contacts and redistributive preferences through an experiment embedded in a probabilistic nationally representative survey of German voters. We manipulated the salience of different categories of social contacts (control, low-income contacts and high-income contacts) and examine how it affects redistributive preferences through the perceived distribution of the contacts' incomes. We find statistically significant indirect effects from the treatment assignment to redistributive preferences via social contacts' mean incomes.

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1. Social sampling and support for redistribution

The social sciences are rich with studies on how the social environment shapes behaviors and attitudes on various issues. The study of economic inequality and redistribution is not an exception. Scholars have documented how social contexts (the localities in which an individual resides) and social networks (the personal connections that an individual has with others) shape preferences for redistribution (Condon & Wichowsky, 2019; Newman, 2013; Sands & de Kadt, 2020).

Two mechanisms often are invoked to explain this social influence. The first concerns the flow of information. Our social networks specifically and individuals around us more broadly influence the type of information and societal norms that we are exposed to (Bandura, 1976; Berelson, Lazarsfeld, & McPhee, 1954; Huckfeldt, Johnson, & Sprague, 2004). While diverse opinions do persist in social networks, in general social groups and networks tend to reinforce beliefs-affirming and group-conforming information and norms. Considering that high-income individuals tend to attribute success to hard work (Suhay, Klašnja, & Rivero, 2020) and that high-income individuals are also more likely to have high-income connections, this mechanism of information and values transmission partially can explain why high-income individuals are less supportive of redistribution, especially when framed as a hand-out negating the importance of hard work.

The second mechanism concerns the well-documented phenomenon of social comparison. In this perspective, the social environment shapes attitudes and behaviors not by passing certain information and filtering out the other, but by providing standards of comparison. Particularly among the less wealthy, exposure to inequality and a comparison to more socioeconomically advantaged individuals leads to a perception that one is having a lower status and thus a stronger support for redistribution (Brown-Iannuzzi, Lundberg, Kay, & Payne, 2015; García-Castro, Rodríguez-Bailón, & Willis, 2020; Sands & de Kadt, 2020). This status comparison can have an unintended effect. Homophily or the tendency to associate with similar others might induce in the minds of these individuals a perceived socioeconomic status that is higher than it actually is, which in turn weakens the support for redistribution among low-income citizens (Condon & Wichowsky, 2019; Jackson & Payne, 2020).

Recent studies have highlighted the influence of a third type of mechanism: social sampling (Dawtry et al., 2015; Galesic et al., 2018; Galesic, Olsson, & Rieskamp, 2012). The social sampling model assumes that people make social judgments based on sampling social instances from their memory, and these samples depend on their particular social environment. In this sense, an individual understands the broader social world by extrapolating from his or her own social circle.

The implications of the social sampling model to our understanding of redistributive preferences are intriguing. As individuals make inferences about the world based on their own social circles, individuals surrounded by high-income contacts are also more likely to think that the population is wealthier than it actually is. Similarly, those surrounded by low-income contacts are more likely to perceive the population as having a low level of income. These different perceptions of the population correspond to different preferences for redistribution (Dawtry et al., 2015). Those perceiving the population as prosperous are more content with the status quo, hence less supportive of redistributive measures. On the other hand, those perceiving the population as having low overall income would be more concerned about inequality and more supportive of redistributive policies.

While insightful, existing social sampling studies on redistribution almost exclusively rely on observational designs. These studies follow the same template. Respondents first indicate their own positions on a certain characteristic (say, for our purpose, income). Then, they estimate the income distributions of their social contacts as well as of the population at large. Lastly, they are asked to indicate the extent to which they agree or disagree with a set of statements on redistribution. An indirect effect model is then estimated that examines how the respondent's own income shapes his or her policy preferences via its effects on the estimated social circle's income distribution and the estimated population's income distribution. The researchers consider the social sampling explanation of redistributive preferences supported if the indirect effect is statistically significant (e.g., Dawtry et al., 2015).

This design leaves open the possibility to two threats. First, confounding might exist due to a common cause; in other words, spurious correlation. In this scenario, a third variable affects both the individual's own income, the individual's social circle, and the individual's preference for redistribution, creating an illusory relationship between the three. The second and more subtle one is due to conditioning on a variable that "is itself caused by two other variables, one that is (or is associated with) the treatment and another that is (or is associated with) the outcome." (Elwert & Winship, 2014, p. 31). The problem stems here from properly controlling for common outcomes.

These limitations do not negate the novelty and insights of the social sampling model in explaining variations in individuals' preferences for redistribution. Rather, they highlight the need for an approach that offers stronger internal validity by manipulating the respondents' estimates of their social circle's income distribution independently of their own income, and examining how this manipulation affects the respondents' preferences for redistribution through the estimated social circle's income distribution. Our study is specifically designed to provide this experimental evidence and thereby advance the literature.

2. Data

We fielded our study in December 2020 to January 2021 as part of a broader survey on inequality that contained multiple experiments. The survey was pre-registered on the OSF website [https://osf.io/zrycw/?view_only=5782b503633d4aa481be3399dfc38ab7]. Design, materials, and analysis plan are available from the link. We obtained the service of Infratest-Dimap as the sample provider. The company has about 120,000 panelists, who were recruited from members of Payback, Germany's largest reward program. Participation in the panel is by invitation only and there is no possibility of self-motivated registration to the panel. This minimizes the risk of panelists being professional survey takers.

Out of the total 120,000 panelists, our sample was drawn from around 72,000 panelists who in Spring 2019 were asked about their voting behavior. A random draw was applied to these panelists while still taking into account the cross-ratios for different demographic characteristics, such as gender, age, and education. This sampling method differentiates our study from those that utilize convenience samples and enables us to maximize the diversity of our respondents. In total, we received a comparatively high participation rate of 72.2% and collected responses from 4493 participants. The sample size was driven more by budget considerations and less on a specific power analysis. This is because, as mentioned above, the survey also included other studies that require their own preferred sample sizes. Maximizing sample size within the constraint of the overall research budget was therefore the optimal solution.

The sample is evenly split on gender (50.5% males and 49.4% females). The mean age is 54.04 with standard deviation 15.85. The youngest participant was 19 years old and the oldest was 92 years old. We utilize all of these respondents as long as they have data on the analyzed variables.

2.1. Design and procedure

Given that it is impossible to randomly assign individuals to different social circles, our experimental design leveraged the oft-used method of psychological priming. Our priming task was designed to increase the salience and accessibility of certain types of social contacts that the respondents already have (Sherman, Mackie, & Driscoll, 1990). We randomly assigned respondents into one of three experimental groups: a control group ($N = 1509$), a High-Estimation group ($N = 1524$), and a Low-Estimation group ($N = 1460$).

Respondents in the control group were instructed to think of all adults with whom they had been in personal contact at least twice in the last 12 months and give an estimate about how many of such people they had been in contact with. This operationalization of social contacts directly follows the ones used in Galesic et al. (2012) and Dawtry et al. (2015). We additionally asked respondents to list up to four names of their contacts to increase the likelihood that the respondents really thought about their contacts, hence increase their salience.

Respondents in the High-Estimation and the Low-Estimation groups followed the same procedure, with one important difference. For respondents in the High-Estimation group, rather than thinking about"all adults" with whom they had been in personal contact, they were requested to think about (and list up to four names of) adults with whom they had been in personal contact and"have a high level of education (at least a university or college degree)". Respondents in the Low-Estimation group instead were asked to focus on adults who"have a low level of education (at most a secondary school diploma)". We used education as it is positively related to income, thus enables us to obscure the objective of our experimental treatment of priming high or low income social contacts. By asking respondents in the High- and the Low-Estimation groups to focus on different subsets of social contacts, we therefore increased the salience of these particular types of social contacts and their likelihood of shaping subsequent social judgments (Galesic et al., 2018).

Following the priming part, we elicited from the respondents information about their social contacts. It should be noted that, while we asked respondents to list up to four names of their contacts, our focus is not limited to these up-to-four listed contacts. We treated the listed contacts as a subset of a broader set of contacts. The listing of the contacts, therefore, was intended merely to increase the salience of contacts that fit the priming criteria, not to direct respondents to think only about these listed contacts. As we are concerned with how the income distribution of one's social contacts shapes one's preference for redistribution, we consequently asked respondents to estimate what percentages of their social contacts fall into each of six net income categories (less than 1000 €, between 1001 and 2000 €, between 2001 and 3000 €, between 3001 and 4000 €, between 4001 and 5000 €, and more than 5000 €). Our treatment specified that"net income" means income after taxation. The income categories match the categories we used for the question on respondents' own incomes and are a simplified version of the 500 €—increment categories used in Galesic et al. (2012).

We provided a real-time calculation of the total percentages that the respondent entered and encouraged them to enter the percentages such that they all summed up to 100%. However, as allocating the percentages under such a constraint can be cognitively taxing, to minimize attrition we decided against forcing respondents to do so. In the end, about 72% of respondents gave estimates that summed up to 100%. For the other 28% whose answers did not sum up to 100%, we assumed proportionality and rescaled their answers to make them sum up to 100%.

2.2. Dependent variable

After estimating the income distributions of their social contacts, we assessed preferences for redistribution with four questions from Dawtry et al. (2015). Example questions include"The government should redistribute wealth through high taxes on the rich" and"The fact that some people in Germany are rich and others are poor is an acceptable part of our economic system." The scale has a good reliability (Cronbach's alpha = 0.71) and a unidimensional structure, with the first factor explaining about 68% of the variance. Theoretical values of the scores range from 1 to 6 with higher scores representing a stronger preference for redistribution.

2.3. Analysis

As a manipulation check, we examine whether our treatment instructions achieved the intended effects of eliciting different types of social contacts among the respondents. Did respondents in the Low Estimation group think of social contacts who have lower incomes than those provided by respondents in the High Estimation group?

Answering this question necessitates calculating the mean income of each respondent's social contacts. Here we closely followed the approach used by Dawtry et al. (2015). First, for each middle income category (i.e., 1001 € - 2000 €; 2001 € - 3000 €; 3001 € - 4000 €), we multiplied the respondent's estimate of the proportion of his/her social contacts who fell into that income category with the category's midpoint. For the lowest income category, we set the incomes at 80% of the upper bound (i.e., 80% x 1000 € = 800 €) and for the highest income category we set the incomes at 30% above the lower bound (i.e, 130% x 5001 € = 6501 €). Second, we took the sum of the multiplication result from each income category and took the log of the value.

The left panel of Fig. 1 presents a comparison of respondents' estimates of the distribution of their social contacts' incomes and the actual population distribution from the 2018 survey of the high quality German Socio-Economic Panel (SOEP). We can see that the distribution provided by respondents in the control group approximates the actual SOEP distribution quite well, especially in the mid- and upper-income categories. To the contrary, the distribution provided by respondents in the Low Estimation group tends to underestimate, and the distribution provided by the High Estimation group tends to overestimate the actual population distribution.

The right panel of Fig. 1 presents the predicted value of logged mean income for each treatment group obtained from a regression of social contacts' incomes on the treatment assignment. The predicted values clearly show that the mean income of the social contacts of respondents in the Low Estimation group is lower than the mean income of the social contacts of respondents in the control group. The opposite is true with respondents in the High Estimation group. These respondents provided estimates of their social contacts' incomes that are higher than the estimates provided by respondents in the control group.

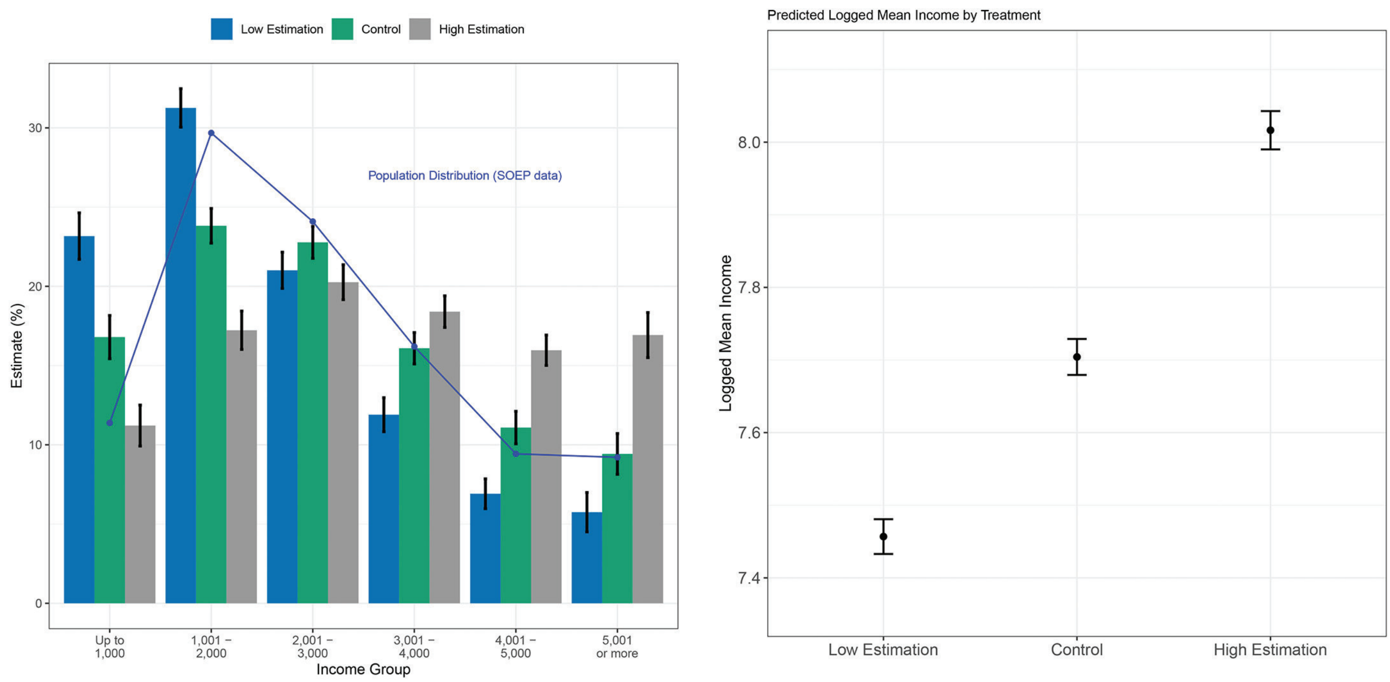
2.4. Unmediated model (pre-registered and confirmatory)

Our next step is to examine whether our experimental treatment has a direct effect on redistributive preferences. This test is pre-registered in our pre-analysis plan and thus confirmatory in nature. In the pre-

Table 1
Standardized Path Coefficients of the Mediation Models.

Path	Basic Model	Conditional Model
Redistribution on		
Treat: Low	0.040 (0.02)*	0.027 (0.02)
Treat: High	0.023 (0.02)	0.004 (0.02)
Contacts Mean Income	0.172 (0.02)***	0.099 (0.02)***
Own Income Group		0.174 (0.02) ***
Contacts Mean Income on:		
Treat: Low	0.235 (0.02)***	0.071 (0.04)
Treat: High	0.297 (0.02)***	0.385 (0.05)***
Own Income Group		0.410 (0.02)***
Treat Low x Own Income Group		0.179 (0.05)***
Treat High x Own Income Group		0.102 (0.05)*
Indirect: Low Estimation	0.040 (0.01)***	
Indirect: High Estimation	0.051 (0.01)***	
Total: Low Estimation	0.000 (0.02)	
Total: High Estimation	0.028 (0.02)	
χ 2 (df)	0.00(0)	3.84(2)
Scaled χ 2 (df)	0.00(0)	2.97(2)
R-squared: Redistribution	0.024	0.051
R-squared: Contacts Mean Income	0.212	0.335
N	4104	3789

*$p < .05$ **$p < .01$ ***$p < .001$.



(a) Distributions of Social Contacts by Treatment Group

(b) Predicted Logged Mean Income of Social Contacts

Fig. 1. Manipulation Check. Respondents in the Low Estimation Group listed social contacts with lower incomes than those listed by respondents in the control group. To the contrary, respondents in the Higher Estimation Group listed contacts with higher incomes than those listed by respondents in the control group.

analysis plan, we refer to this step as the basic model that involves regressing "the dependent variable on the corresponding treatment assignment, taking into account sampling weight." This analytical plan was intentionally broad because it also captured the strategy we employ in analyzing other experiments in the survey (see the section on "Data" above).

In practice, we translated this broad description into specific analytical choices. We used the R package lavaan 0.6–8 (Rosseel, 2012), setting the default maximum likelihood option as the estimation method. Lavaan enables us to conduct our analysis in a structural equation modeling framework, hence giving us the ability to estimate both direct and indirect effects (detailed below in the exploratory section). We simply regressed the dependent variable on the treatment assignment, taking into account the provided sampling weights and including all responses containing all relevant variables (that is, employing a listwise deletion). As our dependent variable is an average of four questions on redistribution, this means that our unmediated model included 4465 respondents (out of 4493 total respondents) who answered at least one of the redistributive preferences questions.

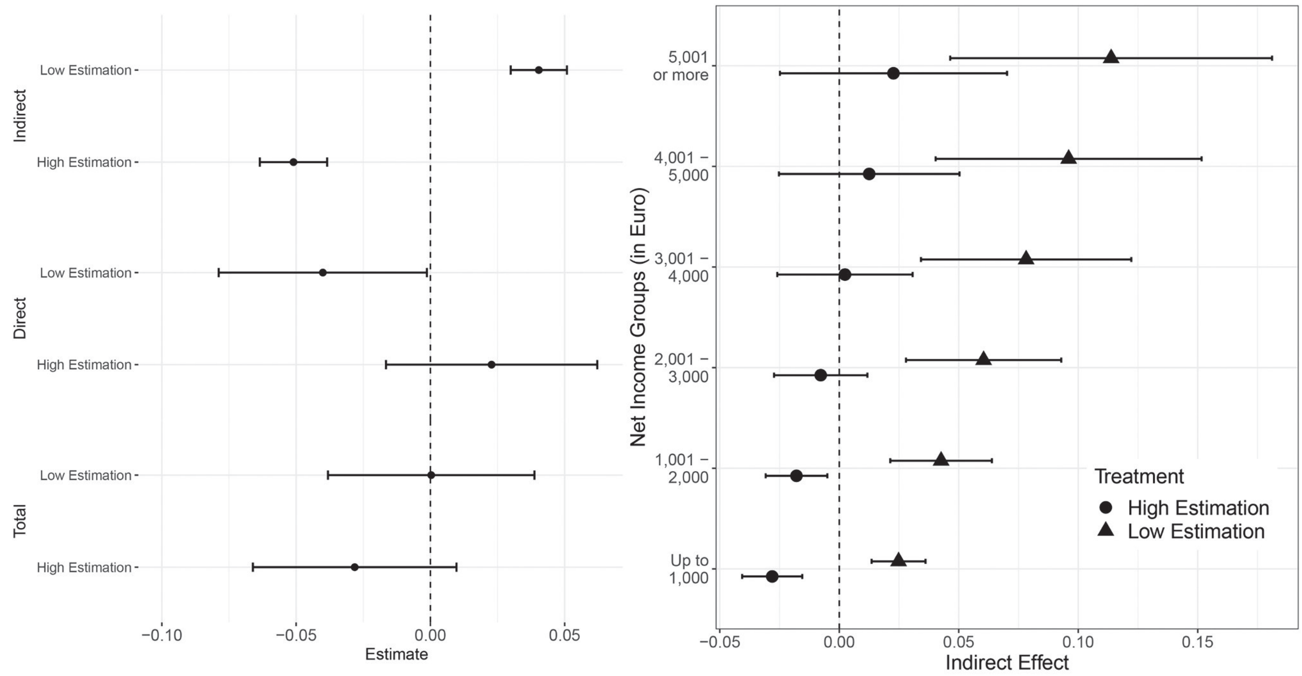
We find no statistically significant effects of the treatment assignment. For the Low Estimation treatment the standardized path coefficient is $B =$ .004 ($S.\ E. = .02$) whereas for the High Estimation treatment the standardized coefficient is $B =$ .024 ($S.\ E. = .02$). The lack of statistical significance of the treatments, compounded with the low R-Squared $R^2 = .001$ suggests that the experimental treatments alone—priming respondents to think about social contacts with certain characteristics—may not be sufficient to exert direct, immediate effects on redistributive preferences.

2.5. *Mediation models (exploratory)*

To follow up on the direct effects' lack of statistical significance, we embarked on an exploratory mediation analysis. We specifically focus on the indirect effects of the treatments on redistributive preferences via social contacts' mean income. Several recent studies (Rucker, Preacher, Tormala, & Petty, 2011; Zhao, Lynch, & Chen, 2010) have highlighted the potential insights on examining indirect effects. Furthermore, a calculation of indirect effects more explicitly takes into account the tenet of the social sampling model that activations of certain social contacts are not sufficient. In order to make inferences and judgments about the broader social world, these social contacts have to be evaluated in the context of a characteristic of interest (Galesic et al., 2018). In our design, social contacts were activated in the priming stage and their relevant characteristic (income, in this case) was made salient by asking respondents to estimate the distribution of their contacts' incomes.

We estimated two mediation models. In the basic mediation model, we model an indirect effect from the treatment assignment to preference for redistribution via social contacts' mean income. The second model, the conditional mediation, expands the basic model by testing for a conditional indirect effect. As one of the strongest predictors of an individual's social circle's income is the individual's own income (Dawtry et al., 2015), it is possible that the indirect effect of the treatment assignment on redistributive preferences is conditioned by the respondent's own income. Specifically, our treatment should prompt the respondents to think about contacts whom they otherwise, given their incomes, are less likely to recall. This would mean that the indirect effect of the Low-Estimation group should be particularly strong for high-income respondents and the indirect effect of the High-Estimation group should be particularly strong for low-income respondents. As is customary in the estimation of indirect effects, in both models we also included direct effects from the treatment assignment to the dependent variable.

Fig. 2 presents standardized path coefficients from the two (basic and conditional) mediation models. In the basic mediation model (Panel A), we find statistically significant indirect effects from the treatment groups to preferences for redistribution via social contacts' mean income. The effect is positive for the Low Estimation group, suggesting that prompting the respondents to recall their social contacts who have low education leads to stronger preferences for redistribution by first priming them with their broader low income social contacts.



(a) Basic Mediation Model

(b) Conditional Mediation Model

Fig. 2. Panel (a) shows the indirect, direct, and total effects of the treatments from the basic mediation model. The Low (High) Estimation Group has a statistically significant positive (negative) indirect effect on redistributive preferences. Panel (b) shows how the strengths of the indirect effects are conditional on the respondent's own income. The positive indirect effect of the Low Estimation group is stronger for high income respondents while the negative indirect effect of the High Estimation group is stronger for low income respondents. The complete path coefficients are available in Table 1.

Conversely, the negative effect of the High Estimation group suggests that the priming task brings these respondents to first recall their contacts who have higher incomes, who then lead these respondents to have weaker preferences for redistribution.

There are at least two explanations for why the indirect effects in the mediation model are statistically significant but the direct effects in the unmediated model (the pre-registered, confirmatory analysis) are not. First, it could be that the relationships between our treatment, social contacts' mean income, and redistributive preferences are that of *indirect-only mediation* (Zhao et al., 2010). In this type of mediation, the mediation model's indirect effect is statistically significant but the direct effect is not—a description that matches the pattern of the indirect- and the direct-effects of our High Estimation treatment.

Such a pattern offers evidence for the hypothesized mediator and suggests that an omitted mediator is unlikely (Zhao et al., 2010, p. 210). While initial studies on mediation focus on direct effects and total effects, or even treat them as prerequisites of a mediation analysis, recent studies argue that focusing on indirect effects can help theory development (Rucker et al., 2011). In that light, we believe that our indirect effects, even without statistically significant direct effects, already carry theoretical significance by highlighting how social contacts' mean income mediates the effects of social sampling on redistributive preferences.

The second explanation that may reconcile the statistically significant indirect effects with the non-statistically significant direct effects concerns *competitive mediation* (Zhao et al., 2010, p. 210). In this scenario, an unmeasured mediator counteracts the effect of the other, measured mediator.[1] In our case, a potential unmeasured mediator that can explain the null direct effect is perceived or subjective income.

Brown-Iannuzzi et al. (2015) show that perceived socioeconomic status is negatively related to redistributive preferences. It is possible that our treatment affects not only social contacts' mean income but also the respondent's perception of his or her socioeconomic status. An individual who is primed with higher (lower) income social contacts might feel that he or she has low (high) socioeconomic status, which then relates to high (low) preferences for redistribution. If that was the case, the indirect effect of the High Estimation treatment would be positive and the indirect effect of the Low Estimation treatment would be negative—exact opposites of the results we have with social contacts' mean income as mediator. The opposite mechanisms might cancel each other out, resulting in a null effect in our unmediated model.

In our case, this scenario is a plausible one as we consider the signs of the direct effects in our basic mediation model. Here, we see that the Low Estimation treatment has a negative and statistically significant effect on redistributive preferences whereas the High Estimation treatment has a positive effect (albeit not statistically significant). These direct effects represent the effects of the treatments on redistributive preferences after social contacts' mean income are taken into account. In other words, holding social contacts' mean income constant, respondents who were primed with low-income contacts became less supportive of redistribution and respondents who were primed with high-income contacts became more supportive. This pattern is consistent with the possibility of perceived socioeconomic status as an unmeasured mediator. Unfortunately, we are unable to test this possibility in a more direct way as we did not include subjective socioeconomic status as mediator in the experiment. We nonetheless encourage future studies to more explicitly examine how social contacts shape redistributive preferences through either social sampling or social comparison.

The conditional mediation model (Panel B) examines the extent to which the indirect effects are conditioned by the respondent's own income (measured in the six categories as the social contacts' income). We find that the Low Estimation treatment is particularly strong among respondents who have a high income. To the contrary, the effect of the High Estimation treatment is particularly strong among respondents with a low income. This pattern offers two insights on social sampling

[1] We thank Reviewer 1 for offering this explanation.

and redistributive preferences.

First, since low (high) income individuals generally are more likely to have low (high) income social contacts who then shape the individuals' preferences for more (less) redistribution, the crossover pattern we observe suggests that our priming tasks likely make salient in the minds of the respondents social contacts whom they otherwise, given their own incomes, are less likely to think about. Recalling these"known, but forgotten" contacts, then, significantly shifts how these respondents view redistribution.

Second, the interaction pattern shows that the positive effect of the Low Estimation treatment is more consistent than the negative effect of the High Estimation treatment as the former is statistically significant across all levels of respondent income. This hints to the possibility that individuals, regardless of income, respond more strongly and consistently (and in ways that favor redistribution) when reminded about lower-income individuals in the society than when reminded about higher-income individuals. If true, this offers a policy opportunity to employ social sampling approaches to shape public preferences for redistribution.

3. Discussion

How does social sampling affect redistribution? Using an experimental design within a nationally representative survey, we demonstrate that social sampling is a cognitive mechanism that plays an important role in the construction of redistributive preferences. More specifically, we successfully manipulated how strongly people considered different sections of the distribution of their social contacts (control, low-income contacts, and high-income contacts). This, in turn, indirectly affected redistributive preferences through the perceived distribution of the social contacts' incomes. This indirect effect is additionally moderated by participants' own incomes in interesting ways: the impact of our manipulation was stronger for those participants who were made to think about social contacts who were on the opposite ends of the distribution compared to the participants themselves; that is, those whom they might otherwise be less likely to think about.

Due to the homophilic nature of humans' social networks (McPherson, Smith-Lovin, & Cook, 2001), individuals are mostly surrounded by and think about people who are similar to them with regard to important features, such as income (Alesina & Giuliano, 2011). These social contacts serve as the individuals' microcosms, which influence their perceptions of the world. A consequence of social networks' role as an information source is that, even under a condition of no motivated cognition, individuals would still have a distorted picture of the world because their social networks are hardly representative of the world in the first place. This phenomena, in turn, highlights that social sampling may be a cognitive mechanism that shapes redistributive preferences in a way that complements other, more motivational mechanisms such as self-interest or ideology.

An important point for future work concerns the extent to which social sampling may be independent or related to more motivational and egocentric mechanisms. Our conditional model shows that social sampling can induce individuals to endorse policy positions incongruent with their material self-interests. The Low Estimation treatment persuaded high income individuals to support redistributive policies and the High Estimation treatment persuaded low income individuals to oppose redistributive policies (Panel B in Fig. 2). In a supplementary analysis available in the Online Appendix, we also find that our findings hold up even after we control for an extensive set of common covariates, namely age, sex, respondent income, general preference for welfare policies, ideology, and political interest. These suggest that the effects of social sampling indeed go beyond the effects of ideology and existing attitudes toward welfare policies.

At the same time, for two reasons, it is unreasonable to argue that the effects of social sampling are totally unrelated to mechanisms attributed to motivated cognition such as ideology. First, the formation of individuals' social networks itself is to an extent influenced by existing preferences. There is ample evidence of how individuals prefer to befriend or be surrounded by attitudinally or behaviorally similar others (Brown & Enos, 2021; Lewis, Gonzalez, & Kaufman, 2012; McPherson et al., 2001; Wimmer & Lewis, 2010). This coincidence means that the social network that an individual samples from is itself already shaped by the individual's existing sociopolitical preferences.

Second, social sampling might be related to ideology in that individuals with a certain ideological predilection might be more or less likely to recall certain types of contacts. For example, a leftist might be more likely to recall individuals who are poor when expressing their attitudes on redistribution. In this case, selective recall driven by strong ideological views interacts with network homophily to provide an even more distorted representation of the world. Both of these rationales suggest that the underlying processes that shape the construction of preferences—in our case, social sampling and more motivational cognitive processes such as ideological thinking—might be complementary as opposed to oppositional, and that understanding these complements is a major task ahead.

As inequality in income and wealth is on the rise around the globe (Atkinson, Piketty, & Saez, 2011; Piketty, 2017), social scientists as well as policy-makers are asking,"what can be done?". This question becomes all the more pressing as many Western democracies also witness a rise in political polarization along new societal cleavages (Kriesi et al., 2012). Our study suggests that a simple intervention—encouraging individuals to think about social contacts who were on the opposite of income distribution—has the potential to invoke a change in redistributive preferences. Inducing a shift in who are recalled and considered representations of the society is arguably less intrusive and more open to intervention than many other forms of inducements, such as changing ideological positions or perceptions of self-interest. In this light, our results highlight the possibility of an intervention in which individuals are encouraged to look beyond their immediate social environments and consider the interests of others and of society as a whole.

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