

Université de Montréal

When Those Asking for Help Also Help Us
The Role of Intergroup Reciprocity in Intergroup Helping Behaviors

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Résumé

Les comportements d'aide intergroupe sont une partie importante du fonctionnement social. Dans le présent article, nous suggérons que la littérature passée a trop focussé sur une perspective unidirectionnelle de l'aide intergroupe (c.-à-d., X aide Y), et que d'aller vers une perception réciproque des relations intergroupes (c.-à-d., X et Y s'entraident) pourrait contribuer à promouvoir l'aide intergroupe. La réciprocité intergroupe (c.-à-d., percevoir qu'un groupe dans le besoin aide aussi le groupe aidant) a ainsi été proposée comme un facteur qui pousse les gens à aider des exogroupes. De plus, nous avons également suggéré un mécanisme pour expliquer l'impact de la réciprocité intergroupe sur l'aide: la perception que les deux groupes sont intégrés dans un endogroupe commun. Une étude pancanadienne ($n = 1767$) a d'abord été conduite pour tester les deux hypothèses. Les résultats suggéraient qu'une plus grande réciprocité intergroupe augmente les comportements d'aide. Plus encore, la relation entre la réciprocité et l'aide était médiée par l'intégration dans un endogroupe commun. Une seconde étude ($n = 318$) a été conduite afin de répliquer et approfondir les résultats de la première étude. Les deux hypothèses ont partiellement été supportées. Cette étude suggèrait aussi que d'autres variables (c.-à-d., les attitudes, percevoir l'autre groupe comme utile, sentir une obligation morale, ressentir de l'empathie) pourraient expliquer la relation entre la réciprocité et l'aide dépendamment de la cible de l'aide.

Mots-clés : Réciprocité, Comportements d'aide, Aide intergroupe, Relations intergroupes, Intégration, Endogroupe commun.

Abstract

Intergroup helping behaviors are an important part of social functioning. In the current paper, we suggest that past literature has focused too much on a unidirectional perspective of intergroup help (i.e., X helping Y), and that switching towards a reciprocal perception of intergroup relationships (i.e., X and Y helping each other) could help promote intergroup helping. Intergroup reciprocity (i.e., perceiving that an in-need group also helps the helping group) was thus hypothesized as a factor that drives people to help outgroups. Moreover, we also suggested a mechanism to explain the impact of intergroup reciprocity on help: the perception that the two groups are integrated in a common ingroup. It was thus hypothesized that the impact of intergroup reciprocity on help is mediated by integration in a common ingroup. A Canada-wide study ($n = 1767$) was first conducted to test both hypotheses. Results suggested that higher intergroup reciprocity does increase help behaviors. Furthermore, the relationship between reciprocity and help was mediated by integration in a common ingroup. A second study ($n = 318$) was conducted to replicate and deepen the first study's results. Both hypotheses were partly supported. This study also suggested that other variables (i.e., attitudes, perceiving the group as useful, feeling a moral obligation, feeling empathy) may additionally explain the relationship between reciprocity and help depending on the target of help.

Keywords : Reciprocity, Helping behaviors, Intergroup help, Intergroup relations, Integration, Common ingroup.

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Liste des sigles et abréviations

CIIM : Common Ingroup Identity Model

T1 : Time 1

T2 : Time 2

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Introduction

La pandémie de COVID-19 a remis en question notre façon de concevoir plusieurs aspects de nos vies. Elle a notamment su remettre en question la conception traditionnelle des relations d'aide intergroupes, où un groupe aide et l'autre reçoit. Dans un contexte de crise où la contribution de chacun est nécessaire au bon déroulement de la société, force est de constater que les groupes dans le besoin ne sont pas que de simples récipients d'aide, mais également des membres actifs de la société qui contribuent à notre bien-être collectif. Autrement dit, l'asymétrie qui caractérise notre conception usuelle des relations d'aide (donneur / receveur) ne rend pas compte de la réciprocité qui existe dans les relations intergroupes. L'objectif de notre recherche était ainsi d'investiguer comment le fait de passer à une conception plus réciproque des relations d'aide intergroupes (où les deux groupes sont à la fois donneurs et receveurs) pourrait contribuer à encourager les comportements d'aide intergroupes.

L'article présenté dans cet essai doctoral est le résultat de deux études empiriques qui ont été produites entre 2020 et 2022. Celui-ci sera soumis à la revue *Group Processes & Intergroup Relations* (SAGE Journals) au courant de l'automne 2022. L'article contient un contexte théorique, puis une section méthodologie, résultats et discussion pour chacune des deux études, suivi d'une discussion générale et une conclusion.

Article

When Those Asking for Help Also Help Us: The Role of Intergroup Reciprocity in Intergroup Helping Behaviors

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Theoretical context

Helping people in need is an essential part of human functioning. It is easy to imagine how dysfunctional societies might become if people stopped helping each other. Many organizations rely on funds or volunteering time to help people in need. For example, such is the case for people dealing with poverty, homelessness, or illnesses and survivors of natural disasters. Research seeking to understand what motivates people to help others is essential to our ability to promote helping behaviors at large and, consequently, improve our societal functioning.

Existing research on factors that motivate helping behaviors mainly focused on why people help single individuals (e.g., Batson et al., 1988; Batson et al., 1989; Cialdini et al., 1987; Erlandsson et al., 2016), although many people in need of help are labeled as members of outgroups (e.g., the homeless, the poor). Only few studies to date examined intergroup help (van Leeuwen & Zagefka, 2017). So far, literature has mainly focused on a hierarchical and unidirectional view of intergroup relationships. The notion here is that in-need groups lack resources possessed by helping groups, which generates a fundamental imbalance of power in the intergroup helping relationship (van Leeuwen & Täuber, 2010). In our view, this is a reductionist conception of the intergroup relation, as in-need groups do not solely exist in the confines of the helping relationship. In-need groups are members of society and also engage in behaviors that benefit the helping group. In the current research, we were interested in what happens when the helping group perceives that the in-need group also provides help in other contexts. We propose that this form of intergroup *reciprocity* should foster further intergroup help.

Why do we help?

Helping behaviors have puzzled scientists for a long time, as they tend to challenge our understanding of human motivation (see Batson, 2017). Although helping behaviors are quite common, in a world where we generally understand human behaviors as driven by self-interest, it is hard to understand why people carry out behaviors aimed at the benefit of others (see Shaffer, 1985-1986). Broadly speaking, the literature has mainly focused on whether help was altruistically or selfishly motivated, with both interpretations having a substantial empirical support (e.g., Batson, 2017; Batson et al., 1988; Batson et al., 1989; De Clerq et al., 2019; Erlandsson et al., 2016; Telle & Pfister, 2016).

Scientists have so far been mostly interested in why individuals help other individuals in contexts that do not highlight group memberships (i.e., inter-individual helping behaviors). Little attention has been given to what motivates people to help outgroups (i.e., groups the person is not part of; van Leeuwen & Zagefka, 2017). This leaves a substantial gap in our understanding of helping behaviors, as many helping behaviors in society seem to be intergroup in nature.

Group boundaries are, in great part, created by group differences that are made salient in a given context (Levine & Crowther, 2008; Ryan et al., 2017), thus the very conditions that generate a need for help are likely to delineate group boundaries (van Leeuwen, 2007; van Leeuwen & Zagefka, 2017). For example, the homeless needs help due to homelessness, the disabled need help because of disability, and the poor need help because of poverty. Added to this, people are often asked to help outgroups as a whole rather than specific members of these groups. Organizations generally ask people to volunteer or give funds to “the poor” or “the homeless”, not to “Charles” or “Nancy”.

Research on intergroup helping behaviors (i.e., help directed towards outgroups; van Leeuwen & Zagefka, 2017) has focused mainly on the aspects of ingroup bias or favoritism (i.e., group-level “selfishness”) and an imbalance of power between the helping and the in-need group (e.g., Cunningham & Platow, 2007; Levine et al., 2005; Moran & Taylor, 2022; van Leeuwen & Täuber, 2010; Vezzali et al., 2015). Ingroup bias in intergroup helping has received substantial empirical support showing that people tend to favor their ingroup over outgroups (e.g., Cadenhead & Richman, 1996; Fiedler et al., 2018; Halabi et al., 2008; Hogg, 2016; James & Zagefka, 2017; Levine et al., 2005; Ryan et al., 2017; Vezzali et al., 2017). Furthermore, some have argued that intergroup helping relations are engrained in power dynamics, with disadvantaged groups needing resources held by privileged groups (van Leeuwen & Täuber, 2010). Because of that, intergroup help would sometimes be used as means to maintain a power imbalance between groups. That is, privileged groups would help disadvantaged groups as a way to maintain their privilege or to benefit their ingroup (e.g., Moran & Taylor, 2022; Nadler et al., 2009). One example of this is the fact that members of privileged groups are more likely to give “dependency-oriented” (e.g., doing a task for others) help towards disadvantaged outgroups, while being more likely to give “autonomy-oriented” (e.g., teaching others how to perform a task) help towards their own ingroup (Cunningham & Platow, 2007; Halabi et al., 2008; Moran & Taylor, 2022; Nadler et al., 2009).

Most of the literature on intergroup help thus assumes that intergroup helping relations are fundamentally unequal and *unidirectional*, with *helpers* giving to *recipients* of help (van Leeuwen & Täuber, 2010). Though that may be true when looking at the exchange occurring through a single act of helping, the view of helping relationship as unidirectional does not account for the fact that intergroup relations take place over longer periods of time and are not limited to the “transaction” of help from helper to recipient. Groups in need are part of society rather than mere recipients of help; that is to say, they can also be helpers in some contexts.

This was exemplified in recent events amidst the COVID-19 pandemic in Canada. In the spring of 2020, it was reported in Canadian media that numerous asylum seekers who were granted asylum by Canada took on frontline positions in healthcare services, helping Canadians to fight the virus. This realization was followed by demands from activists and politicians to help these asylum seekers in the legal processes to acquire a permanent residence status in Canada (Lévesque, 2020; Schué, 2020). The example of asylum seekers is a departure from the traditional unidirectional view of helping relationships. Asylum seekers did not only ask Canada for help (i.e., receiving asylum and permanent resident status), they also helped Canadians during the COVID-19 crisis. This switch to a bidirectional perception of the relationship with asylum seekers seemed to, in turn, have motivated Canadians to offer them more help. This example raises a novel question, that to the best of our knowledge what not yet addressed in the literature: what happens to helping behaviors when the intergroup helping relationship is not unidirectional, but *reciprocal*?

Intergroup reciprocity

Reciprocity has not yet received a lot of attention in research on helping behaviors, especially at the intergroup level. Previous psychology research has mainly tried to understand reciprocal patterns in human interactions. To do so, they tried to study reciprocity objectively using a behavioral definition and measurement of reciprocity: behaving similarly as another person as a response to that person’s behavior (e.g., Hugh-Jones et al., 2019). This means a first person gives, then a second person reciprocates or not, and reciprocity is deducted from whether or not (or how much) reciprocation occurred (e.g., Flynn & Yu, 2021; Hugh-Jones et al., 2019; Wilke & Lanzetta, 1970). Studies using that definition have studied reciprocity through game-like exchanges (e.g., Allocation Games), and found that prosocial behaviors are generally met with

reciprocation from others (e.g., Hugh-Jones et al., 2019; Wilke & Lanzetta, 1970). The main focus of the literature has thus been on what influences reciprocity in exchanges rather than how reciprocity motivates helping.

Additionally, the “objective” behavioral definition of reciprocity, while allowing tightly controlled experiments (e.g., Hugh-Jones et al., 2019; Li et al., 2017; Sellnow et al., 2021), does not capture the complexity of continuous relationships in real-life contexts. Namely, real-life relationships between individuals or groups take place over much longer time spans than game experiments, which makes it near impossible to objectively assess who gave and who reciprocated. For example, asylum seekers’ work in healthcare could either be interpreted as helping or as reciprocating previous help (i.e., being offered asylum).

To find real-life studies of reciprocity predicting outcomes, we generally have to look into the fields of organizational and developmental psychology (e.g., Buunk et al., 1993; Moliner et al., 2013; Perlow & Weeks, 2002; Smith et al., 2021). These fields have focussed on more perceptual definitions of reciprocity that describe it as a quality that is attributed to a relationship. In that context, reciprocity can be defined as a perception that a relationship is mutual, with a relatively fair balance of exchanges (i.e., giving as much as receiving) and a more equal than hierarchical nature (e.g., Moliner et al., 2013; Smith et al., 2021; Wintre et al., 1995). Studies in these fields generally found that reciprocity was associated with positive outcomes like less burnout, less negative affects and better relationship quality (Buunk et al., 1993; Moliner et al., 2013; Schwarz et al., 2005; Smith et al., 2021). Researchers also suggested that perception of low reciprocity in people’s relationship with their organization was related to more absenteeism (e.g., Geurts et al., 1995). In other words, perceived reciprocity with the organization determined how much time employees would give back to their employer.

In the current research, we decided to study intergroup reciprocity as a predictor of intergroup help in a real-life helping context. To our knowledge, this is the first time reciprocity is studied in the context of real-life intergroup help (cf., Hugh-Jones et al., 2019). Because, as stated previously, it is near-impossible to objectively assess behavioral reciprocity in real-life contexts, we decided to study how perceived intergroup reciprocity predicted intergroup help. For better operationalization, we define intergroup reciprocity as perceiving that an outgroup asking for help from one’s ingroup also engages in helping behaviors towards one’s ingroup (see

Walster et al., 1978). In other words, the demand for help is also accompanied by mutual help. We hypothesize that:

H1. *The more intergroup relations with an outgroup are perceived as reciprocal, the more people will be inclined to help that outgroup.*

Reciprocity and intergroup boundaries

The next question was to understand exactly why intergroup reciprocity would lead to help. As stated previously, intergroup helping is often interpreted as “selfish”, as it would be a way to maintain the power imbalance between privileged and in-need groups (e.g., Moran & Taylor, 2022; Nadler et al., 2009). In the same capacity, reciprocal behaviors are also often interpreted as selfishly motivated (e.g., Buunk & Schaufeli, 1999; Romano et al., 2022). The idea here is that people would engage in reciprocity because it is likely to result in receiving gain in the future (Romano et al., 2022). However, this tendency to reduce reciprocity to a cost-benefit calculation has been criticized by some authors (e.g., Ashworth, 2013; Minas et al., 2018) and may be a product of the way we operationalized and studied reciprocity through game-like exchanges in the past. Namely, it has been pointed out that reciprocity’s role in the development of relationships is often forgotten (Ashworth, 2013).

One alternative “non-selfish” way of interpreting the impact of reciprocity on help is through the *Common Ingroup Identity Model* (CIIM; Gaertner et al., 2016). The CIIM can be viewed as an extension of Sherif’s pioneering work (see 1988) and describes how intergroup prosocial cognitions and behaviors can be improved through the creation of a common ingroup identity (see Gaertner et al., 2016). In other words, people would be more likely to help outgroups when they feel that their ingroup and the outgroup are integrated in a common ingroup.

What is interesting here is that processes generally used to create common ingroup identities share conceptual similarities with reciprocity. The study of the CIIM has mainly focused on cooperation and common fate. Participants with differing group identities (e.g., Democrats / Republicans) would thus cooperate on a task or try to reach a common goal, which would elicit a new group identity through their interactions (e.g., Gaertner et al., 1990; Gaertner et al., 1999). Just like reciprocity, cooperating on a task or towards a common goal implies the

adoption of mutually beneficial behaviors between both parties and a relatively fair balance of exchange.

Therefore, despite cooperation being a more specific task-driven series of behaviors compared to perceived intergroup reciprocity, which characterizes how we perceive the relationship between groups, both are related to the idea that there is mutuality between parties involved. Perceived reciprocity may thus, in the same capacity as cooperation, strengthen the bonds between groups and contribute to a greater sense of “oneness”. Therefore, we propose that intergroup reciprocity not only fosters intergroup help, but also increases the perception that the in-need and the helping group are integrated in a common ingroup identity. That integration in a common ingroup identity would more precisely be the mechanism through which intergroup reciprocity leads to intergroup helping behaviors. Therefore, we hypothesize:

H2. The more intergroup relations with an outgroup are perceived as reciprocal, the more people will perceive that outgroup as integrated with their ingroup. That perceived integration in a common ingroup will mediate the relationship between perceived intergroup reciprocity and intergroup help.

The current research

The purpose of the current research was first to test the impact of intergroup reciprocity on intergroup help, and second to test a potential mechanism to explain that relationship: integration in a common ingroup. This was done in a real-life context by studying the relationship between the general Canadian population and asylum seekers during the COVID-19 pandemic. A first study was conducted to test our hypothesis across a large representative sample of Canadians (based on gender, age and province). Reciprocity was manipulated by presenting asylum seekers as both asking for help and engaging in help towards Canadians. The impact of that manipulation on integration and help was then measured. A second study was conducted on a smaller representative sample of the province of Quebec to both replicate our results and investigate alternative mediators.

Study 1

Methodology

Our first study was part of a broader online longitudinal research on the general Canadian population's response to the COVID-19 pandemic. Following the news coverage on asylum seekers working in healthcare, a quick experiment was added at the end of our questionnaire to test our hypothesis with a few items.

Participants

Participants were recruited with AskingCanadians, an online platform which recruits its users for a variety of surveys in exchange for a compensation. Our experiment was integrated at Time 6 of a broader longitudinal survey about the COVID-19 pandemic. The survey initially recruited 3617 Canadian participants over the age of 18, using quotas for age, gender and province to produce a representative sample of Canadians. At Time 6 (summer 2020), an initial sample of 2116 participants (41.5% attrition) participated to our experiment. Two "test items" were placed in the questionnaire to ensure participants were paying attention. A total of 158 participants (7.5% of the initial sample) failed at least one of the test items, making their answers unreliable. These participants were taken out of our final sample. As a part of our experiment was to present an asylum seeker with a specific nationality in a text, people who reported identifying with the same nationality as the presented asylum seeker were also excluded. A total of 191 participants (9.0% of the initial sample) were thus retracted from our final sample, which therefore consisted of 1767 participants (83.5% of the initial sample).

The mean age of our final sample was of $M = 51,15$ years old ($SD = 16,44$). Men and women were equally represented, with 49,86% of participants identifying as male and 50,08% identifying as female. Only one participant (0,06% of the sample) identified as another gender. Our sample also offered a relatively proportionate representation of all Canadian provinces by using quotas for each province during the recruitment.

Procedure

The experiment took place within an online survey on the AskingCanadians platform, and participants were instructed to answer questions relating to the COVID-19 pandemic. Participants first started the experiment by answering a question measuring initial attitudes towards asylum seekers before the experimental manipulation. They were then presented with a short text about a fictional asylum seeker who was a healthcare worker during the COVID-19 pandemic. We experimentally manipulated the level of reciprocity of the fictional asylum seeker¹, who was either working on the frontline (high reciprocity), working from home (moderate reciprocity) or had stopped working and relied on government aid (low reciprocity). Participants were randomly distributed into one of the three different experimental groups. Once they had read the text, participants had to answer to three final questions measuring integration and attitudinal help.

Measures

Initial attitudes. Initial attitudes were measured prior to the experimental manipulation with a single item. Participants would be asked “In general, how many new asylum seekers do you think we should accept in Canada?” and would answer using a Likert scale going from 1 (A lot less) to 10 (A lot More). Therefore, a higher score meant more favorable attitudes towards asylum seekers.

Integration. Integration of the presented asylum seeker in the common Canadian identity was measured with a single item. Participants were asked how much they agreed with the statement “I believe [Asylum seeker’s name] fits my definition of a true Canadian.” and would answer using a Likert scale going from 1 (Strongly disagree) to 10 (Strongly agree). Thus, a higher score meant a higher level of integration.

Help. Help was measured using two separate items. The first one, “specific” help, measured help towards the person presented in the text. Participants were asked “Should [Asylum seeker’s name] be eligible to a fast-track program to receive Canadian citizenship?” and would answer using a Likert scale going from 1 (Definitely no) to 10 (Definitely yes). The second item,

¹ There was also a nationality experimental manipulation (Chinese vs. Ukrainian vs. Haitian) that was meant to explore potential anti-Chinese biases in our results, making it a 3x3 experiment. Only a marginally significant effect showed that people were slightly less likely to help when the asylum seeker was Chinese. However, the strength of the effect was very small and likely marginally significant only because of the large sample size. Given that these results bore no implication for our conclusions, we decided to retract it from the article. Nonetheless, the manipulation was used as a covariate in our results to control its marginal effect.

“general” help, aimed at measuring help towards other asylum seekers. Participants would be asked how much they agreed with the statement “How many new asylum seekers like [Asylum seeker’s name] do you think we should accept in Canada?” and would answer using a Likert scale going from 1 (A lot less) to 10 (A lot more). On both questions, a higher score meant a higher disposition to help asylum seekers.

Independent variables for mediations

Since our mediation analyses consisted in linear regressions, we had to split our nominal independent variable (reciprocity) into binary variables. Rather than breaking it into “classic” dummy variables (where one group has a value of 1 and the others have a value of 0) we decided to break it into binary variables comparing only two groups at a time, thus creating three variables (“High vs. Low”, “Moderate vs. Low” and “High vs. Moderate”). Those variables were meant to offer more specific and interpretable results than having one dummy variables as the independent variable and one dummy as a control variable.

Results

Preliminary analyses

The distribution of our variables was checked to make sure they had a relatively normal distribution. As shown in Table 1, each variable respected criteria for normal distribution, with kurtosis and skewness scores falling between -1 and 1. We also conducted a single-factor ANOVA with experimental groups as the independent variable and initial attitudes as the dependent variable to make sure that our pre-experiment measure did not vary significantly across experimental groups ($F(2, 1765) = 1.90, p = .150, \eta_p^2 = .00$).

Effect of reciprocity on help

Our first hypothesis (H1) was that higher reciprocity would lead to more help. Single factor ANCOVAs were conducted to test H1. Experimental groups were the independent variable and initial attitudes were placed as covariate to predict specific and general help separately. Both specific help ($F(2, 1761) = 40.65, p < .001, \eta_p^2 = .04$) and general help ($F(2, 1764) = 39.93, p < .001, \eta_p^2 = .04$) significantly varied across experimental groups. Levels of help also varied

according to our hypothesis, with highest scores in the high reciprocity group, lowest scores in the low reciprocity group, and moderate scores in the moderate reciprocity group (see Table 2). As it is not possible to produce post-hoc results with ANCOVAs, we tested whether the differences between each group was significant using binary variables comparing two groups at a time in further ANCOVAs. Binary variables were the independent variables and initial attitudes were used as covariate. Results showed that there was significantly more help in the high reciprocity group compared to the low reciprocity group (Specific help: $F(1, 1189) = 80.88, p < .001, \eta_p^2 = .06$; General help: $F(1, 1189) = 79.90, p < .001, \eta_p^2 = .06$), in the high reciprocity group compared to the moderate reciprocity group (Specific help: $F(1, 1162) = 24.56, p < .001, \eta_p^2 = .02$; General help: $F(1, 1162) = 15.40, p < .001, \eta_p^2 = .01$) and in the moderate reciprocity group compared to the low reciprocity group (Specific help: $F(1, 1168) = 15.73, p < .001, \eta_p^2 = .01$; General help: $F(1, 1168) = 24.25, p < .001, \eta_p^2 = .02$). H1 was therefore supported by our results.

Mediation effect of integration

Our second hypothesis (H2) was that the effect of reciprocity on help would be mediated by integration. To test H2, six mediation analyses were conducted using the PROCESS macro (Hayes, 2013) to measure the mediation effect of integration on the relationship between the three reciprocity binary variables and both help measures. In all six analyses, initial attitudes were added as covariate. All analyses showed statistically significant indirect effects, thereby confirming the presence of a mediation (see Figure 1 and 2). H2 was therefore supported by our results, which showed that integration significantly mediated the relation between reciprocity and help. The models wielding the greatest effect sizes were those comparing the high reciprocity group with the low reciprocity group. Most models had a small, but statistically significant direct effect, suggesting that the mediation was generally partial. Models presenting a full mediation (i.e., without a significant direct effect) were those with smaller effects of reciprocity on help to begin with. Thus, in these models, the strength of the direct effect seemed to become non-significant as a result of the initial effect being smaller rather than the mediation being fuller (see Figure 1 and 2).

Discussion

The purpose of Study 1 was to examine the causal effect of intergroup reciprocity on intergroup help. It was hypothesized that perceived reciprocity would increase help (H1) and that this effect would be mediated by integration (H2). Results support our hypotheses. First, H1 was supported as perceived intergroup reciprocity did increase intergroup help. As anticipated, levels of help gradually increased with how much we portrayed the relation between Canadians and asylum seekers as reciprocal. This further supports previous literature showing an association between reciprocity and prosocial behaviors (e.g., Hugh-Jones et al., 2019; Wilke & Lanzetta, 1970). Second, H2 was also supported as integration in the larger ingroup mediated the relationship between reciprocity and help in every analysis. That mediation however was generally partial and seemed to sometimes be full only as a result of smaller effect sizes. This means that other factors may also explain the relationship between reciprocity and help in addition to integration. Nonetheless, results suggest that the CIIM (Gaertner et al., 2016) may provide a framework to better understand the association between perceived reciprocity and intergroup help.

One strength of our study is the use of an experimental manipulation, which allows us to suggest that there is a causal link between intergroup reciprocity and intergroup help. Thus, portraying asylum seekers as being more reciprocal with Canadians does lead to an increased predisposition to engage in help towards them. The same can be said about the impact of reciprocity on integration. This means that increasing perceived intergroup reciprocity by highlighting an outgroup's contribution to our ingroup's well-being may indeed be a proper strategy to enhance intergroup help through an increase in intergroup integration.

Nonetheless, this study contains some caveats that need to be addressed for future research. First, our help measure was more attitudinal in nature (i.e., attitudes towards helping) than behavioral. Future research should replicate our results with more behavioral measures to strengthen the conclusion that reciprocity leads to help behaviors. Secondly, the nature of the proposed help might also have influenced our results. The mediation of integration on the relation between reciprocity and help may actually be explained by the fact that help (accelerating the immigration process) was also related to the integration of asylum seekers in the broader Canadian ingroup. This type of help was chosen because it was what was proposed by the government to help asylum seekers working in healthcare, but future research should try to

replicate our results with types of help that are not related to integration. Thirdly, future research should also try to replicate our results with help targeting an outgroup as a whole (e.g., all asylum seekers) rather than only a subgroup engaging in more reciprocity (e.g., asylum seekers working in healthcare). This is especially important for our conclusions, as it would influence the amount of people that we can expect to help with potential interventions. Fourthly, it is worth noting that only single-item scales were used for this study. Using validated multiple-items scales in future research should help to strengthen the validity of our measures and our results. Finally, as we believe stronger support for our hypothesis can be drawn from also testing alternative hypotheses, we suggest that future research should also test alternative mediators for the relation between intergroup reciprocity and intergroup help. Other factors may better explain the impact of reciprocity on help than integration in the broader ingroup.

Study 2

The purpose of our second study was to replicate our results while answering to the caveats of the first study. First, we changed the help measure to be a more behavioral measure. Second, new sub-hypotheses were added to test the previous study's blind spots. Like in Study 1, we hypothesize:

H1. The more intergroup relations with an outgroup are perceived as reciprocal, the more people will be inclined to help that outgroup.

H2. The more intergroup relations with an outgroup are perceived as reciprocal, the more people will perceive that outgroup as integrated with their ingroup. That perceived integration in a common ingroup will mediate the relationship between perceived intergroup reciprocity and intergroup help.

As specified in the previous discussion, we wanted to check if it was possible to increase help towards all asylum seekers and not just those working in healthcare. We thus added the sub-hypothesis:

H1a. *Perceived intergroup reciprocity will increase intergroup help even when the target of help is a group as a whole.*

We also suggested that results from Study 1 might have been influenced by the fact that our help measure was related to integration. However, if H2 is right, our hypothesized mediation should remain significant independently from the type of help. Thus, our second sub-hypothesis is:

H2a. *Integration in a common ingroup will mediate the relationship between perceived intergroup reciprocity and intergroup help even when the help behavior is not related to integration.*

Finally, we decided to add some potential alternate mediators to our study. The first one was intergroup attitudes, which consists in the general mental predisposition of a person towards an outgroup (see Colman, 2015). Essentially, the better intergroup attitudes are, the more the outgroup is perceived preferably, which can increase the likelihood of help (e.g., Taylor et al., 2020). Therefore, people may help an outgroup as a consequence of reciprocity because it makes them feel more positively about that group. The second alternate mediator is instrumentality, meaning the extent to which we perceive the outgroup as useful to our ingroup. Instrumentality is more associated with a “selfish” group motivation and fits with the idea that intergroup help may be a way to indirectly help our own ingroup (e.g., Nadler et al., 2009). In that case, reciprocity would lead to intergroup help because people would perceive positive interactions with an outgroup as beneficial for their ingroup. A third potential mediator would be moral obligations, or the sense of duty towards the outgroup. Previous literature has shown how intergroup help is sometimes used as a way to preserve a positive image of one’s ingroup (Hopkins et al., 2007). It is possible that higher perceived intergroup reciprocity would lead to a perception that one’s ingroup owes a debt to the outgroup, which would lead people to help them as a way of preserving a positive ingroup image. Lastly, a fourth potential mediator would be intergroup empathy. Empathy is perhaps the most heavily studied predictor of help (see Batson, 2017). Despite the absence of a clear theoretical association between reciprocity and empathy, we thought it would be important to add it to our study because of its relevance in the literature.

Our goal with alternate mediators is to test if they explain the association between reciprocity and help better than integration. If H2 is supported, the addition of alternate mediators to our mediation model should not null the indirect effect of integration. Thus, our last sub-hypothesis is:

H2b. Integration in a common ingroup will mediate the relationship between perceived intergroup reciprocity and intergroup help even when other mediators are added in our mediation analyses.

Methodology

Our second study was a two-time online experiment. To better assess the impact of the experimental manipulation, Time 1's purpose was to measure most of our variables to control for their distribution prior to the experiment. That way, we would measure how changes brought by the experiment would impact dependent variables. The experiment would be implemented at the beginning of Time 2, and variables would be measured a second time following the experiment in addition to the measure of help behaviors. Instead of the three levels of Study 1, only two levels of reciprocity (high vs. low) would be manipulated to simplify the results and increase the statistical power with a smaller sample. Study design and hypotheses were pre-registered on *AsPredicted*.

Participants

A first sample of 600 participants aged over 18 and living in the province of Quebec was constituted at time one with the help of a survey firm. Participants were targeted so that the sample would be representative of the Quebec population in terms of age, gender, region and education. The only category that we could not properly represent was people with only a primary school education. Of the 600, only 423 participants (70.5% of the initial sample) responded to time two. Of those 423, 44 (7.3% of the initial sample) retracted their consent at the end of the questionnaire. Participants whose response time was under four minutes or over two hours at Time 2 were also taken out of our final sample. Too short or too long response times suggested a lack of involvement in correctly completing the study, but too long response times

were additionally deemed to go beyond the time the manipulation still had an effect. An additional 61 participants (10.2% of the initial sample) were thus eliminated, making our final sample a total of 318 participants (53.0% of the initial sample). Generally speaking, there didn't seem to be differential attrition between the two times on our demographic variables. The only observable difference was that people aged between 18 and 24 were less likely to have completed the second measure time (34.9%) than the rest of the sample (55.2%). Mean age for our final sample was 49.90 years old ($SD = 16.91$), with 53.9% of participants identifying as men, and 46.1% as women. 31.9% of our sample had made at least undergraduate studies, and about 52.2% had an annual revenue of at least 50 000\$. It is also worth noting that 26.5% of participants were retired.

Procedure

Time 1. Participants first received an invitation to participate to a two-time study about attitudes towards asylum seekers on a survey firm's online platform. The true target of the study (i.e., helping behaviors) was hidden to avoid potential biases from the participants. If participants consented to participate, they would first be asked to answer a series of demographic variables, followed by a questionnaire measuring nearly all of our studied variables. The only variables left out were helping behaviors and helping efficacy because our measure could have revealed the true purpose of our study before Time 2. The completion of Time 1 took about a week.

Time 2. One week after the end of Time 1, invitations were sent to all participants who had completed the first questionnaire. The online survey started with the first experimental manipulation. Participants were randomly distributed between two experimental groups who had to read a fake journal article. The journal article dealt with asylum seekers' situation during the pandemic in three paragraphs that summed about 280 words. The first paragraph was the same across experimental groups and was a brief introduction stating that the number of asylum seekers has increased, making their immigration process longer (41 words). The middle paragraph described the situation of a specific asylum seeker and was the only part of the text that changed between the two groups (about 145 words). In the first group (low reciprocity), the asylum seeker in the text had to stop working due to COVID-19. In the second group (high reciprocity), the presented asylum seeker had been working in healthcare during the pandemic. The final paragraph was the same across experimental groups and stated that asylum seekers'

long immigration process caused mental distress and that organizations were looking for ways to help them (94 words). Following the article, participants were told that we were working with an organization helping asylum seekers that may be interested in recruiting them to sign a petition, but first wanted to investigate people's interest in such a petition. The target of the petition was our second experimental manipulation and was meant to test H2a. Participants were again randomly assigned to two different groups. In the first group, the petition asked for the acceleration of the immigration process for asylum seekers (integration-related). In the second group, the petition asked for an increase in funding for organizations helping asylum seekers (integration-unrelated). Their interest for the petition was measured and served as our help behavior measure. In both groups, participants had to rate how much they would want to sign the petition if it was to help all asylum seekers, and also if it was to help only asylum seekers working in healthcare. Participants were also asked if they wanted to receive a URL for the petition once it was published, which was both a measure and a way to convince the participants that the petition was real. Finally, participants completed the questionnaire measuring all studied variables. Participants completed Time 2 in about a week and a half.

Measures

Demographics. A few demographic variables were modified so they could serve as controls in our analyses. Age and gender already being continuous and dichotomic (no participants in the final sample reported identifying to a gender other than man or woman), they did not need modifications. We also asked participants their position on the political spectrum with a continuous scale from 1 (Left) to 7 (Right), 4 being the center value. Education and revenue, however, were transformed into dichotomic variables. Education was cut between people with at least undergraduate studies and people with lower diplomas. Revenue was cut between people with lower or higher revenues than 50 000\$. Finally, we had also asked the participants to give us the three first digits of their postal code, which we converted into population density data using the data from the website www.cybo.com. This gave us a population density index that however had a non-normal distribution and a too wide range of values for linear regressions (0.09 to 14 707 people / km²). It was thus transformed into a dichotomic variable with a median cut-off of 1036 people / km².

Perceived reciprocity. Perceived reciprocity was measured at both times by adapting the *Perception of Parental Reciprocity Scale* (Wintre et al., 1995) to our research context. Social psychologists rarely (if ever) measure perceived reciprocity, preferring the study of concrete reciprocal exchanges (e.g., Hugh-Jones et al., 2019). We thus had to search in other fields of study to find a perceived reciprocity measure, and consequently had to more substantially modify the scale to adapt it to our subject. The resulting scale contained 5 items to which participants answered how much they agreed with statements (e.g., “There is a collaboration between asylum seekers and Canadians / Quebeckers, even during times of hardship.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant higher perceived reciprocity. The scale demonstrated great internal consistency at both Time 1 ($\alpha = .83$) and Time 2 ($\alpha = .90$).

Integration. Integration was measured at both times by adapting the integration subscale of the *Multiple Identity Integration Scale* (Yampolsky et al., 2016) to our research context. Using a scale increased the validity of our measure compared to the single item of the first study. The resulting scale contained 5 items to which participants answered how much they agreed with statements (e.g., “The Canadian / Quebecker identity includes asylum seekers.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant higher integration. The scale demonstrated great internal consistency at both Time 1 ($\alpha = .90$) and Time 2 ($\alpha = .93$).

Attitudes. Attitudes towards asylum seekers was measured at both times by adapting items from the *Negative Attitudes Toward Immigrants Scale* (Varela et al., 2013) to our research context. The resulting scale contained 6 items to which participants answered how much they agreed with statements (e.g., “Asylum seekers are a burden to Canadian / Quebecker taxpayers.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant more positive attitudes. The scale demonstrated great internal consistency at both Time 1 ($\alpha = .90$) and Time 2 ($\alpha = .91$).

Instrumentality. Instrumentality of asylum seekers was measured at both times by adapting items from the instrumental subscale of the *Attitudes Toward Diverse Workgroups Scale* (Nakui et al., 2011) to our research context. The purpose was to measure how much people perceived asylum seekers as “useful” to healthcare services since it could potentially explain our results. The resulting scale contained 5 items to which participants answered how much they agreed with statements (e.g., “Asylum seekers are often efficient workers in healthcare services.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale

meant higher instrumentality. The scale demonstrated very high internal consistency at both Time 1 ($\alpha = .93$) and Time 2 ($\alpha = .96$).

Moral obligations. Moral obligations were measured at both times by adapting items from a scale made by Beugré (2012) to our research context. The purpose was to measure how much participants felt like Canadians / Quebeckers had a duty to help asylum seekers. The resulting scale contained 5 items to which participants answered how much they agreed with statements (e.g., “It is important that Canadians / Quebeckers make sure that asylum seekers are treated fairly.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant higher moral obligations. The scale demonstrated very high internal consistency at both Time 1 ($\alpha = .96$) and Time 2 ($\alpha = .96$).

Empathy. Empathy towards asylum seekers was measured at both times by adapting items from the affective subscale of Sirin et al.’s intergroup empathy scale (2016). The purpose was to measure participants’ empathy towards asylum seekers as a group. The resulting scale contained 5 items to which participants answered how much they agreed with statements (e.g., “I often feel worried for asylum seekers.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant higher empathy. The scale demonstrated good internal consistency at both Time 1 ($\alpha = .76$) and Time 2 ($\alpha = .80$).

Help behaviors. Help behaviors were measured by first pretending that we had a partnership with an organization that helped asylum seekers. Participants were informed that this organization wanted to create a petition to help asylum seekers, but first wanted to know how much people were interested in signing it. They were first asked, on a scale from 1 (Not interested at all) to 10 (Very interested) how much they were interested in signing the petition if it was aimed at helping all asylum seekers. This consisted in our first “General help” measure. Using the same scale, we then asked participants how much they would be interested in signing the petition if it was aimed at helping only the asylum seekers working in healthcare. This consisted in our second “Specific help” measure. It is worth noting that our general and specific help measures are not the same as in Study 1. In Study 1, specific help was towards the specific asylum seeker presented in the text and general help was towards asylum seekers similar to that one. Finally, participants were also asked if they would be interested in receiving the URL link for the petition once it was published. This consisted in a third “concrete” help measure as it was the closest to participants actually signing the petition.

Help efficacy. Finally, efficacy of the presented helping behaviors was measured at Time 2 by adapting a homemade scale that we used in a previous study (Laboissonnière & de la Sablonnière, 2018). Since perceived efficacy of the proposed help behavior (i.e., signing a petition) could influence our results (e.g., Choi & Moon, 2016), this was intended as a control variable. The resulting scale contained 4 items to which participants answered how much they agreed with statements (e.g., “I believe that signing a petition can help to change society.”) on a scale from 1 (Totally disagree) to 10 (Totally agree). Higher scores on the scale meant higher efficacy. The scale wielded great internal consistency ($\alpha = .85$).

Results

Preliminary analyses

The distribution of our variables was checked to make sure they had a relatively normal distribution. As shown in Table 3, all variables at both measurement times respected criteria for normal distribution, with kurtosis and skewness scores falling between -1 and 1. We also conducted single-factor ANOVAs with experimental groups as the independent variable and Time 1 variables as the dependent variables to make sure that our pre-experiment measures did not vary significantly across experimental groups. Not Time 1 variable varied significantly across experimental groups (Reciprocity: $F(1, 306) = 2.87, p = .091, \eta_p^2 = .01$; Integration: $F(1, 315) = 0.02, p = .892, \eta_p^2 = .00$; Attitudes: $F(1, 315) = 0.47, p = .493, \eta_p^2 = .00$; Instrumentality: $F(1, 313) = 0.00, p = .975, \eta_p^2 = .00$; Moral obligations: $F(1, 303) = 0.27, p = .606, \eta_p^2 = .00$; Empathy: $F(1, 304) = 0.53, p = .467, \eta_p^2 = .00$).

Manipulation check

Before testing our hypotheses, we checked if the reciprocity manipulation had its intended effect on self-reported reciprocity. The effect of our reciprocity manipulation on self-reported reciprocity was tested using a repeated measures ANOVA. Time 1 reciprocity and Time 2 reciprocity were used as repeated measures, and the reciprocity manipulation was used as the between-subjects predictor. There was no significant interaction between within-subject time variations in reciprocity and the reciprocity manipulation ($F(1, 302) = 0.72, p = .398, \eta_p^2 = .00$). This means that there were no significant differences between the two groups in how much

reciprocity changed between Time 1 and Time 2. However, the difference between Time 1 ($M = 6.14, SD = 1.69$) and Time 2 ($M = 6.54, SD = 1.81$) reciprocity was statistically significant ($F(1, 302) = 40.58, p < .001, \eta_p^2 = .12$), meaning that there was a significant increase in reciprocity at Time 2 across the full sample (see Table 3). Finally, there was no between-subjects effect of the reciprocity manipulation ($F(1, 302) = 2.15, p = .144, \eta_p^2 = .01$). These results suggest that our experimental manipulation failed to reach its intended effect of increasing reciprocity more in the high reciprocity group than in the low reciprocity group. Nonetheless, it was decided to still try to test H1 with the manipulation in case the absence of effect on self-reported reciprocity was the result of the scale not properly measuring reciprocity.

Effect of the reciprocity manipulation on help

Just like in Study 1, it was predicted that higher reciprocity would lead to more help (H1). ANCOVAs were first conducted to evaluate if the reciprocity manipulation had any effect on general and specific help. The reciprocity manipulation was the independent variable, with demographics, help efficacy and Time 1 reciprocity as covariates. There were no significant differences between high reciprocity ($M = 6.06, SD = 2.74$) and low reciprocity ($M = 5.81, SD = 2.89$) groups for general help ($F(1, 276) = 0.33, p = .568, \eta_p^2 = .00$). There were no significant differences between high reciprocity ($M = 7.34, SD = 2.70$) and low reciprocity ($M = 6.89, SD = 2.68$) groups for specific help ($F(1, 277) = 0.86, p = .355, \eta_p^2 = .00$). A logistic regression was similarly conducted to test the effect of the manipulation on concrete help, as it was a dichotomous variable. Demographics, help efficacy, Time 1 reciprocity and the reciprocity manipulation were used as predictors of concrete help. The reciprocity manipulation did not significantly predict concrete help ($b = 0.08, p = .798, OR = 1.08$), with 60% of participants helping in the high reciprocity group for 56% in the low reciprocity group. These results suggest that there was no significant differences between experimental groups in terms of help behaviors (see Table 4). H1 was thus not supported, but as suggested by results from the manipulation check, this may be explained by the intended manipulation simply failing to actually manipulate reciprocity.

Further analyses

In the absence of a significant effect of the manipulation on its measured construct and on any help variable, it was not reasonable to conduct our analyses as first intended. These results suggest that the manipulation failed to influence its related construct, which would lessen the validity of subsequent analyses. Consequently, we decided to conduct our analyses with self-reported reciprocity as the independent variable instead of the reciprocity manipulation (see Table 5 for correlations). This would still allow us to test our hypotheses but would not allow us to infer causality in the effect of reciprocity on help. Nonetheless, we decided to minimize this setback by using Time 1 reciprocity as the predictor of help at Time 2. By using a predictor at an anterior time from our outcome variables, we increase the likelihood of there being a possible causal link.

Effect of self-reported reciprocity on help

To test if higher reciprocity increased help (H1), and to test if higher reciprocity would lead to more help towards a whole group (H1a), linear regressions were first conducted to assess the effect of intergroup reciprocity measured at Time 1 on general and specific help at Time 2. Demographics were added as covariates as well as help efficacy (see Table 6). Time 1 reciprocity significantly predicted both general help ($\beta = .32, p < .001$) and specific help ($\beta = .22, p < .050$). Logistic regressions were conducted to measure the effect of reciprocity on concrete help. Just like with linear regressions, Time 1 reciprocity was the independent variable, and demographics and help efficacy were covariates. Concrete help was not predicted by Time 1 reciprocity ($b = 0.13, p = .17, OR = 1.14$), and was mainly related to help efficacy ($b = 0.62, p < .001, OR = 1.85$). Nonetheless, it is important to note that Time 1 reciprocity was originally correlated to concrete help ($r = .23, p < .001$; see Table 5). H1 was thus partly supported by our results as reciprocity was a significant predictor of two out of three help measures. H1a was also supported by the fact that reciprocity significantly predicted help towards all asylum seekers (i.e., general help). However, it is worth noting that general help was significantly lower than specific help ($t(314) = -9.70, p < .001$). Thus, despite reciprocity significantly predicting help towards all asylum seekers, participants helped less when the help was targeting all asylum seekers than when it was targeting only asylum seekers working in healthcare.

Mediation effect of integration on the relationship between self-reported reciprocity and help

It was also hypothesized that the relationship between reciprocity and help would be mediated by integration (H2). Mediation analyses were conducted to test H2 using PROCESS' model 4 (Hayes, 2013). Time 1 reciprocity was used as the independent variable, and Time 2 integration was used as the mediator in each analysis. Demographics and help efficacy were used as covariates (see Figure 3, 4, 5). Three mediation analyses were conducted to test the mediation on each help measure. It is worth noting that PROCESS uses logistic regressions when predicting binary outcome variables, which enables us to perform mediation analyses with concrete help.

Despite Time 1 reciprocity not predicting concrete help when previously testing H1, we still included it in our analyses. Because help efficacy was an important predictor of concrete help in the previous logistic regression (see Table 6) and because help efficacy also correlates with both Time 1 reciprocity and Time 2 integration (see Table 5), there could still be an indirect effect of reciprocity on concrete help that was previously clouded by help efficacy.

The relationship between reciprocity and general help (standardized indirect effect = .20, 95% CI = [.13, .28]), between reciprocity and specific help (standardized indirect effect = .13, 95% CI = [.06, .22]) and between reciprocity and concrete help (indirect effect = 0.17, 95% CI = [0.06, 0.31]) were all mediated by integration. The direct effect of reciprocity on general help remained significant in the mediation ($b = 0.20, p < .050$), while it was not the case when predicting specific help ($b = 0.12, p = .209$) and concrete help ($b = -0.05, p = .670$). These results suggest that the mediation for general help was partial, while the mediation for specific and concrete help were complete. Therefore, we found support for H2 as integration mediated the relationship between reciprocity and help on all three help measures.

Effect of the type of help. As a sub-hypothesis of H2, we also hypothesized (H2a) that integration would mediate the relationship between reciprocity and help whether the proposed help was related to integration (i.e., accelerating immigration) or not (i.e., giving funds to organizations). To test the potential impact of the type of help (accelerating immigration or giving funds) on how much the relationship between reciprocity and help was mediated by integration, we conducted moderated mediations in PROCESS' model 58 (Hayes, 2013). The covariates, independent and dependent variables were the same as in the previous mediation analyses. The type of help manipulation was added as the moderator in the model, and could

moderate both the link between reciprocity and integration and the link between integration and help. No significant moderated mediation was found for general help (index of moderated mediation = 0.10, 95% CI = [-0.09, 0.28]), specific help (index of moderated mediation = -0.11, 95% CI = [-0.30, 0.09]) or concrete help (index of moderated mediation = 0.06, 95% CI = [-0.15, 0.30]). These results suggest that the type of help did not impact how much integration mediated the relation between reciprocity and help. The H2a hypothesis was therefore supported by results.

Effect of alternative mediators. Finally, a second sub-hypothesis of H2 predicted that integration would remain a significant mediator of the relationship between reciprocity and help even when accounting for alternative mediators (H2b). Using once again PROCESS' model 4 (Hayes, 2013), we conducted parallel mediations to test H2b and to explore which mediators would have a significant indirect effect. Time 1 reciprocity was used as the independent variable, and all five Time 2 mediators (integration, attitudes, instrumentality, moral obligations and empathy) were used as the mediators in each analysis. Demographics and help efficacy were placed as covariates. Three analyses were conducted to test the mediation on each help measure (see Figure 6, 7, 8). The relationship between reciprocity and general help was still significantly mediated by integration (standardized indirect effect = .14, 95% CI = [.07, .23]), but also parallelly by attitudes (standardized indirect effect = .15, 95% CI = [.06, .25]) and empathy (standardized indirect effect = .05, 95% CI = [.01, .10]). However, the relationship between reciprocity and specific help was not significantly mediated by integration but was instead significantly mediated by instrumentality (standardized indirect effect = .08, 95% CI = [.01, .15]). Moral obligations also marginally mediated the relationship between reciprocity and specific help (standardized indirect effect = .07, 95% CI = [-.00, .15]) at $p < .07$ (93% CI = [.00, .15]). No mediator significantly mediated the relationship between reciprocity and concrete help. Thus, H2b was only supported with the help measure targeting all asylum seekers.

Supplementary alternative analyses

Previous analyses using self-reported reciprocity as the independent variable were also alternatively conducted using difference scores (Time 2 - Time 1) to measure how changes in reciprocity were related to help. The problem with that method, however, was that people who were initially high in reciprocity were more likely to help, but also less likely to have increases in reciprocity, while people low on reciprocity were less likely to help, but also more likely to

increase their perception of reciprocity. Essentially, there were significant negative correlations between all Time 1 variables and their subsequent change (see Supplementary Table 1). This appears logical, as there is less room to move upwards on a scale when a person is already close to the top and vice versa. However, it ultimately blurred results from difference scores, which is in good part why we opted for the previously presented analyses.

Nonetheless, this method showed relatively similar patterns of results. Regarding H1, change in reciprocity did significantly increase concrete help but did not significantly increase general and specific help. Thus, H1 was supported on one out of three help measures. Regarding H2, change in integration did mediate the relationship between change in reciprocity and general and specific help. Therefore, despite the absence of an initial direct effect of change in reciprocity on general and specific help, change in reciprocity had an indirect effect on help through change in integration. It is worth noting, however, that effect sizes were very small. H2a showed no significant results, but it is likely because mediation effects were too small to begin with. Finally, results regarding H2b showed that, when adding parallel mediators in the model, change in integration was still the only mediator in the relationship between change in reciprocity and general help, while change in instrumentality was the only mediator in the relationship between change in reciprocity and specific help. There were no mediators in the relationship between change in reciprocity and concrete help.

Discussion

The purpose of Study 2 was to replicate and examine previous results in more depth. This was done using more developed self-reported measures and behavioral help measures. In the absence of any manipulation effect, analyses were conducted only with self-reported measures, which prohibits us from drawing any causal effect from our results. It is worth noting, that we used Time 1 reciprocity to predict Time 2 measures, which at least means that reciprocity preceded mediator and outcome variables.

Once again, it was hypothesized that intergroup reciprocity would increase intergroup help (H1), but also that it would increase help when it targets a group as a whole (H1a). Reciprocity predicted every help measure, even when controlling for confounders. This means more reciprocity leads to more help, which supports H1. These results also support H1a since one

of our help measures targeted all asylum seekers as a group. This suggests that increasing intergroup reciprocity can effectively increase intergroup help on a broad intergroup scale and foster the types of behaviors that organizations try to promote (e.g., giving funds to help an outgroup). It is worth noting, however, that help towards a whole group (all asylum seekers) was significantly lower than help towards the specific subgroup whose reciprocal behaviors were highlighted (asylum seekers working in healthcare). In other words, reciprocity predicted help towards both targets, but people were more inclined to help those they perceived as directly engaging in described reciprocal behaviors.

Like in Study 1, it was also hypothesized that integration would mediate the relationship between reciprocity and help (H2). Additionally, it was hypothesized that this relationship would not be influenced by whether or not the proposed help was conceptually associated to integration (H2a) and would remain significant when accounting for other potential mediators (H2b). Support for H2 was found in the initial mediation analyses. The relationship between reciprocity and general help, reciprocity and specific help, and reciprocity and concrete help, were all mediated by integration.

Furthermore, H2a was also supported by the fact that the type of help did not moderate the mediation. Integration mediated the relationship between reciprocity and help even when the type of help was integration-unrelated (i.e., funding organizations helping asylum seekers). This suggests that results from Study 1 were not just a result of the proposed help being integration-related (i.e., accelerating asylum seekers' immigration process), but that integration is indeed a mechanism that promotes help following an increase in reciprocity. However, it is important to note that H2a was a null hypothesis, which warrants us to be cautious with that conclusion. Further research replicating our results with integration-unrelated help behaviors would be needed to strengthen our conclusions.

Furthermore, results from testing H2b suggest there may be more than integration to explain the effect of reciprocity on help. More precisely, what explains the effect of reciprocity on help seems to depend on who the target on help is. H2b was supported when the target of help was a whole group. The relationship between reciprocity and help was still significantly mediated by integration, but also additionally mediated by attitudes and empathy. This suggests that, when reciprocity increased, people were more likely to help a whole outgroup because they perceived this group as more integrated in a common ingroup, had more positive regard towards that group

and were more empathetic towards that group. However, H2b was not supported when the target of help was only the subgroup described to highlight intergroup reciprocity. Rather, the relationship between reciprocity and help was only significantly mediated by instrumentality, and marginally mediated moral obligations. This suggests that help towards specific subgroups known to be engaging in reciprocal behaviors may be better explained by the fact that people perceive these subgroups as more useful, and potentially by the fact that they believe they have a moral duty towards them. H2b was not supported when predicting whether or not people wanted to receive the link to the petition (i.e., concrete help). None of our mediators significantly mediated the relationship between reciprocity and concrete help. The lack of any mediation on the concrete help measure may be explained by the fact that the target of the petition was not specified. We told participants that we would choose whether the petition was aimed at all asylum seekers or only those working in healthcare depending on what stood out from their responses. Thus, target-specific motivations may have been mixed in our concrete help measure, leaving no variable as a significant mediator of the relationship between reciprocity and help when all mediators were taken into account.

Strengths of Study 2 mainly lie in our use of alternative mediators and two measurement times. By accounting for other factors that may have explained the relationship between reciprocity and help, we have been better able to isolate the effect of integration. It has also helped to nuance our conclusions and open future perspectives. The use of two measurement times has also increased the likelihood of a potential causal link despite the failure of the manipulation. Nonetheless, there are still limitations. Namely, despite our efforts to produce a representative sample, participants were recruited via an online platform, which means they may still belong to a specific demographic of people seeking to respond to online surveys. Additionally, all of our measuring scales were adjusted to our research context. Despite most adjustments being minor, they may still hinder the validity of the scales. However, the main limitation seems to be the failure of the experimental manipulation.

Consequently, it is important to address the lack of effect of our reciprocity experimental manipulation. Despite it not being possible to properly identify the reason why the manipulation failed, we believe the text used for the manipulation was too long. Participants on paid online platforms are likely motivated to answer surveys as fast as possible to receive their monetary compensation (e.g., Barge & Gehlbach, 2012). Being presented with a text that is more time

consuming may have resulted in participants simply not reading the text, or otherwise not giving it proper attention. This would also explain why Study 1's manipulation worked, as it was only a brief statement. Future research may want to use manipulations that require less efforts from participants or settings where the manipulation cannot be overlooked.

General discussion

Many groups in our society need the help of the general population to improve their quality of life. Understanding what leads people to help outgroups has the potential to shed a light on the ways we can encourage individuals to help groups in need. In the current research, we were interested in how perceiving an in-need outgroup as not just recipients of help, but also givers of help, may increase help behaviors towards them. Two studies examined the effect of intergroup reciprocity on intergroup help. First, it was hypothesized that perceiving more intergroup reciprocity would lead to more help. Second, it was also hypothesized that integration in a common ingroup would mediate the relationship between reciprocity and help. Using an experimental design, Study 1 supported a causal effect of reciprocity on help. It also showed that integration mediated that relationship. Study 2 replicated those results with more robust measures and strengthened Study 1's conclusions by showing that the results were applicable with different targets of help and different types of help behaviors. Study 2 also nuanced our conclusions by showing that integration in a common ingroup was only one of the mechanisms through which reciprocity increased help. However, no causality could be drawn with certainty from Study 2, as reciprocity was self-reported rather than manipulated in an experiment.

Perhaps the strongest conclusion that can be drawn from the current research is that intergroup reciprocity influences intergroup help. This was supported whether reciprocity was elicited through information given to participants (Study 1) or self-reported (Study 2) and whether the target of help was a single outgroup member engaging in the presented reciprocal behavior (Study 1), the subgroup of outgroup members engaging in the presented reciprocal behavior (Study 1 and 2) or the whole outgroup (Study 2). This bears implications for both research and practice. First, research on intergroup relations should take a greater interest in how perceiving intergroup reciprocity shapes intergroup behaviors, affects and cognitions. Past research has focused too much on demonstrating the presence of "tit-for-tat" reciprocal patterns

in human interactions (e.g., Haushofer et al., 2010; Hugh-Jones et al., 2019), and not enough on the outcomes of perceiving reciprocity. Authors from many fields have argued that reciprocity is a fundamental aspect of human morality and relationship-building (see Buunk & Schaufeli, 1999). It may thus be central to the development of positive intergroup relationships. Second, in practice, organizations whose goal is to help groups in need may also want to portray these groups' relationship with the majority group as more reciprocal. Namely, their contribution to the common well-being could be highlighted. For example, presenting people with disabilities as also workers in important fields could be a way of fostering others' help towards them.

Regarding the specific mechanisms that explain why people help outgroups as a result of perceiving reciprocity, we found substantial evidence for the role of integration in a common ingroup. Thus, perceiving an outgroup as engaging in reciprocity increases the perception that this group is integrated with our ingroup, which in turn leads to more help. Not only does this further support the CIIM (see Gaertner et al., 2016), but it also shows that more perceptual processes could also be studied in the model. Previous studies testing the CIIM have focused more on behavioral processes (i.e., cooperation, common fate) from which common group identities tend to emerge (e.g., Gaertner et al., 1990; Gaertner et al., 1999), but it seems harder to draw large-scale practical recommendations from these processes. Perceptual processes like perceived reciprocity can be implemented in the ways we portray or speak of intergroup relations, which could lead to more easily applicable recommendations from future research on the CIIM.

However, Study 2 showed that integration should not be the only process studied to understand the role of intergroup reciprocity on intergroup help. Attitudes, perceived instrumentality, moral obligations and empathy also explained why people engaged in help. This seems to point out that many different motivations to help may rise from perceived intergroup reciprocity. It is likely that reciprocity is a multi-faceted construct that may appeal to different motives. On one hand, reciprocity has an instrumental dimension, as positively engaging with people likely to reciprocate will likely lead to further gain. On the other hand, reciprocity has a relational dimension, as it is a display of mutual appreciation and trustworthiness. In the middle, reciprocity also has a moral dimension, as it relates to fairness (i.e., receiving as much as giving). What our research suggests is that the motivations leading to help behaviors as a result of reciprocity will depend on who is precisely the recipient of help. Motivations that are more "outgroup-focused" (i.e., feeling more positive about the outgroup, seeing the outgroup as

sharing a common ingroup, having empathy for the outgroup), or “altruistic” in a way, seem to better explain why we help whole groups. Meanwhile, motivations that are more “ingroup-focused” (i.e., perceiving the outgroup as useful to our ingroup, feeling that our ingroup has responsibilities towards the outgroup), or “selfish”, seem to better explain why people help subgroups that are specifically portrayed as engaging in reciprocal behaviors. This may be because people directly engaging in known reciprocal behaviors are seen as more likely to offer gains and more deserving of help, while anticipated gain and moral concerns are more diffused with whole groups. In the latter case, behaviors seem more determined by how people relate to the other group.

The fact that empathy and attitudes parallelly mediated the relationship between reciprocity and help along with integration is also in line with past research showing that empathy and attitudes conjointly predict help behaviors towards whole groups. The *Empathy-Attitudes-Action Model* (Batson et al., 2002; Taylor et al., 2020) has previously supported that the impact of empathy on intergroup help was mediated by more positive attitudes. Our research adds to the literature showing more empathy and better attitudes towards an outgroup translate into help behaviors towards that outgroup. Just like with integration in a common ingroup, fostering intergroup reciprocity could be a new intervention aimed at increasing empathy and positive intergroup attitudes.

The differential motivations depending on the specificity of the target of help bears important implications for research. Most of the literature on reciprocity has focused on individual targets, even in intergroup contexts (e.g., Hugh-Jones et al., 2019), which may explain part of why selfish motives take a predominant place in our understanding of reciprocity. The current research is in line with previous articles suggesting that past research on reciprocity has adopted a reductionist view of the concept (e.g., Minas et al., 2018), leaving behind its role in developing relationships. Future research on intergroup reciprocity should investigate behaviors that target whole groups (e.g., giving funds, engaging in activism, supporting laws) to further understand the relational motives that predict intergroup behaviors. It is worth noting that reciprocity was more strongly correlated to help towards whole groups than towards the specific subgroup in Study 2, which may suggest that perceived intergroup reciprocity is more important to broader intergroup relationships.

Finally, it is also worth noting that our results may not be limited to intergroup relationships but also extend to professional helping relationships. Professionals such as nurses, psychotherapists and social workers develop long-term relationships with their patients, which may be impacted by reciprocity. The narrow conception of reciprocity may only view this relationship through the lens of the economic exchange occurring (i.e., providing help, then receiving money), but some authors suggest that reciprocity in patients demonstrating care for their care provider may also have a positive impact on both parties (e.g., Hem & Pettersen, 2011; Pettersen & Hem, 2011; Reid et al., 2005; Slowiaczek, 2021). As our results would suggest, reducing the traditional asymmetry (i.e., increasing reciprocity) in these relationships could improve professionals' involvement in helping relationships. In that instance, the effect of reciprocity may also be explained by an increased feeling that both patient and professional form a dyad (i.e., proxy for integration in a common ingroup). Clinical research should further investigate how reciprocity shapes care providers' behaviors, and possibly the patients' behavior.

Future directions

Future research studying reciprocity's impact on intergroup help may first want to explore instances where intergroup reciprocity is more based on reported intentions than reported behaviors. Reciprocity in our research was based on presenting behaviors from an in-need group that contributed to our ingroup's well-being, but the nature of some in-need groups' difficulties sometimes puts barriers to their potential contributions. For example, homeless people face many challenges that make it harder to reach employment, intellectual disabilities may similarly limit people's work prospects. However, presenting these groups as *desiring* to contribute may also increase the perception that there is a form of intergroup reciprocal mutuality. This could be a more realistic way of increasing perceived reciprocity with groups that have less resources.

Furthermore, helping behaviors we studied had a relatively low cost (i.e., signing a petition). This is generally standard in the literature, because people are unlikely to engage in costly behaviors (i.e., giving money or time) within the context of a study. However, people may be motivated differently when engaging in costly help behaviors than when engaging in low-cost help. This is especially true with intergroup help. The cost-benefit ratio of helping is generally seen as lower with intergroup help, as helpers are less likely to receive gain afterwards than when helping an ingroup individual (e.g., Buunk & Schaufeli, 1999; Romano et al., 2022). Therefore,

increasing the cost of the behavior further decreases the cost-benefit ratio, making it likely to reduce the selfish motives to help. Future research may want to explore whether or not intergroup reciprocity increases more costly forms of outgroup help.

Future research may also want to replicate our results in different contexts. The COVID-19 pandemic presented a particular context that might have made people behave in different ways. The pandemic presented every aspect of a dramatic social change (de la Sablonnière, 2017), which implies an instability in social and normative functioning. Helping groups engaging in useful reciprocal behaviors may be an attempt to re-establish social functioning by rewarding valuable behaviors. Consequently, people may have been motivated differently if there had been no crisis or if the presented reciprocal behavior of the outgroup had no bearing with the pandemic.

Finally, it is worth noting that asylum seekers also represent a special class of people who live in a country without knowing if they will end up official residents. They are not officially “Canadian”, but they strive to be, which increases their potential of being either rejected or accepted by the rest of the population. This might make integration a more relevant construct to understand our behaviors towards asylum seekers than towards other groups, and may thus be the reason why integration mediated the relationship between reciprocity and help. Therefore, further research with other groups (especially non-immigrant) is needed to strengthen our conclusions.

Conclusion

Past research on intergroup helping has focused on a unidirectional conception of the helping relationship. Our research suggests that moving towards a reciprocal conception of that relationship may present new avenues to promoting intergroup help. Intergroup reciprocity seems to appeal to many aspects of human motivation, which might make it a flexible drive to help that can encourage intergroup prosocial behaviors across different contexts. Helping those who also help us is not just a strategic action, it is also a way of building better intergroup relationships.

Most important to remember is that asylum seekers’ work in healthcare preceded the COVID-19 pandemic. What we consider to be reciprocal behaviors was already present; the pandemic only shed a light on it. Like asylum seekers, most groups in need of help also engage in daily behaviors that are essential to the proper functioning of society. It is not so much a question

of whether or not there is intergroup reciprocity, but a question of acknowledging its presence. As suggested decades ago by Walster et al. (1978), reciprocity only exists in the eyes of the beholder.

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Table 1

Descriptive statistics of all studied variables – Study 1

	<i>M</i>	<i>SD</i>	Skewness	Kurtosis
Initial attitudes	4,54	2,69	0,21	-0,82
Integration	6,09	2,74	-0,25	-0,79
Specific help	6,29	2,96	-0,39	-0,95
General help	5,58	2,73	-0,11	-0,79

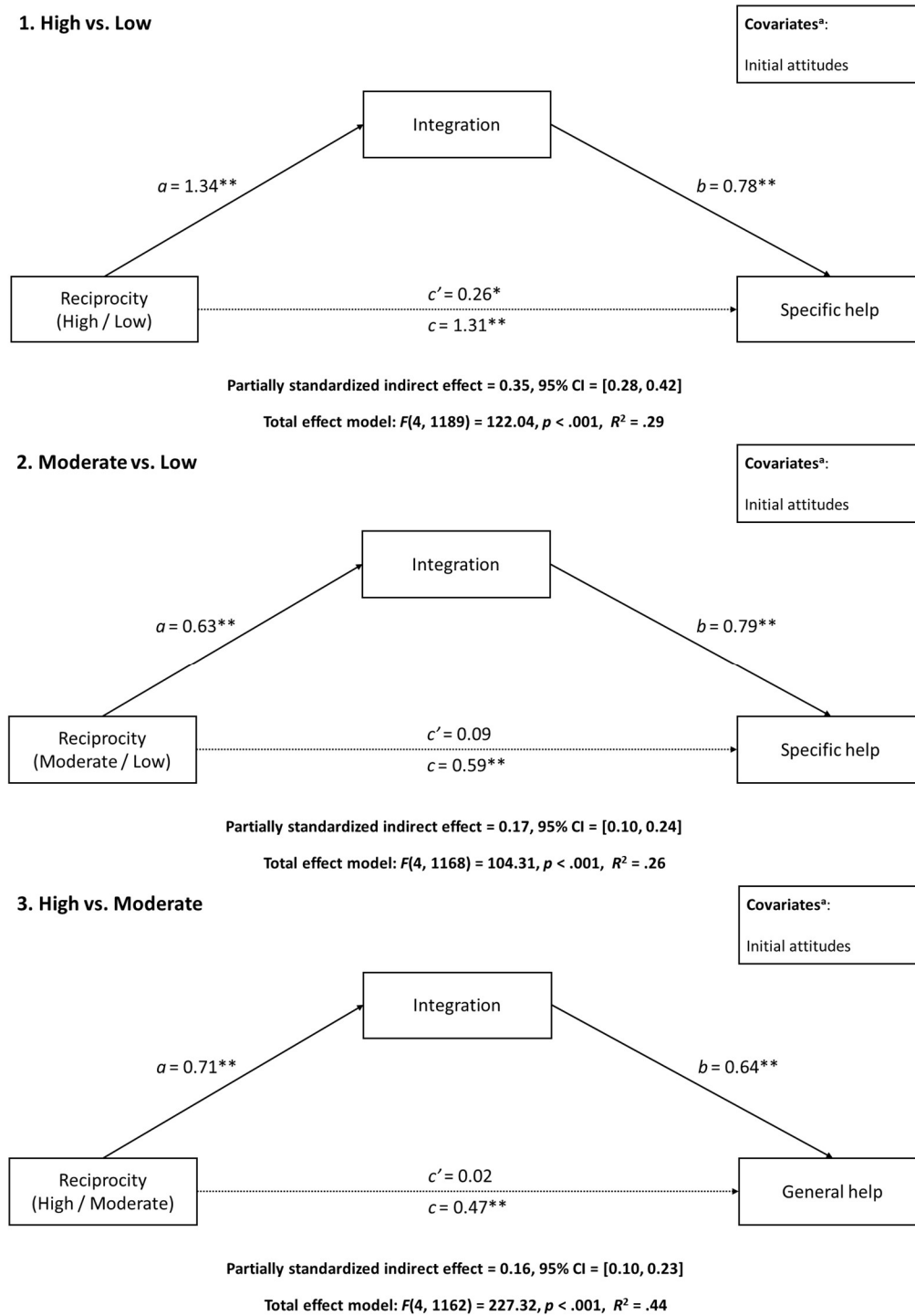
Table 2

Means of all variables by experimental group – Study 1

Experimental groups	Initial attitudes		Integration		Specific help		General help	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
High reciprocity (<i>n</i> = 594)	4.38	2.71	6.69	2.62	6.87	2.89	5.97	2.74
Moderate reciprocity (<i>n</i> = 573)	4.58	2.74	6.08	2.79	6.26	2.89	5.63	2.75
Low reciprocity (<i>n</i> = 600)	4.68	2.61	5.50	2.70	5.73	2.99	5.14	2.64

Figure 1

Mediations of integration on the relationship between reciprocity and specific help – Study 1



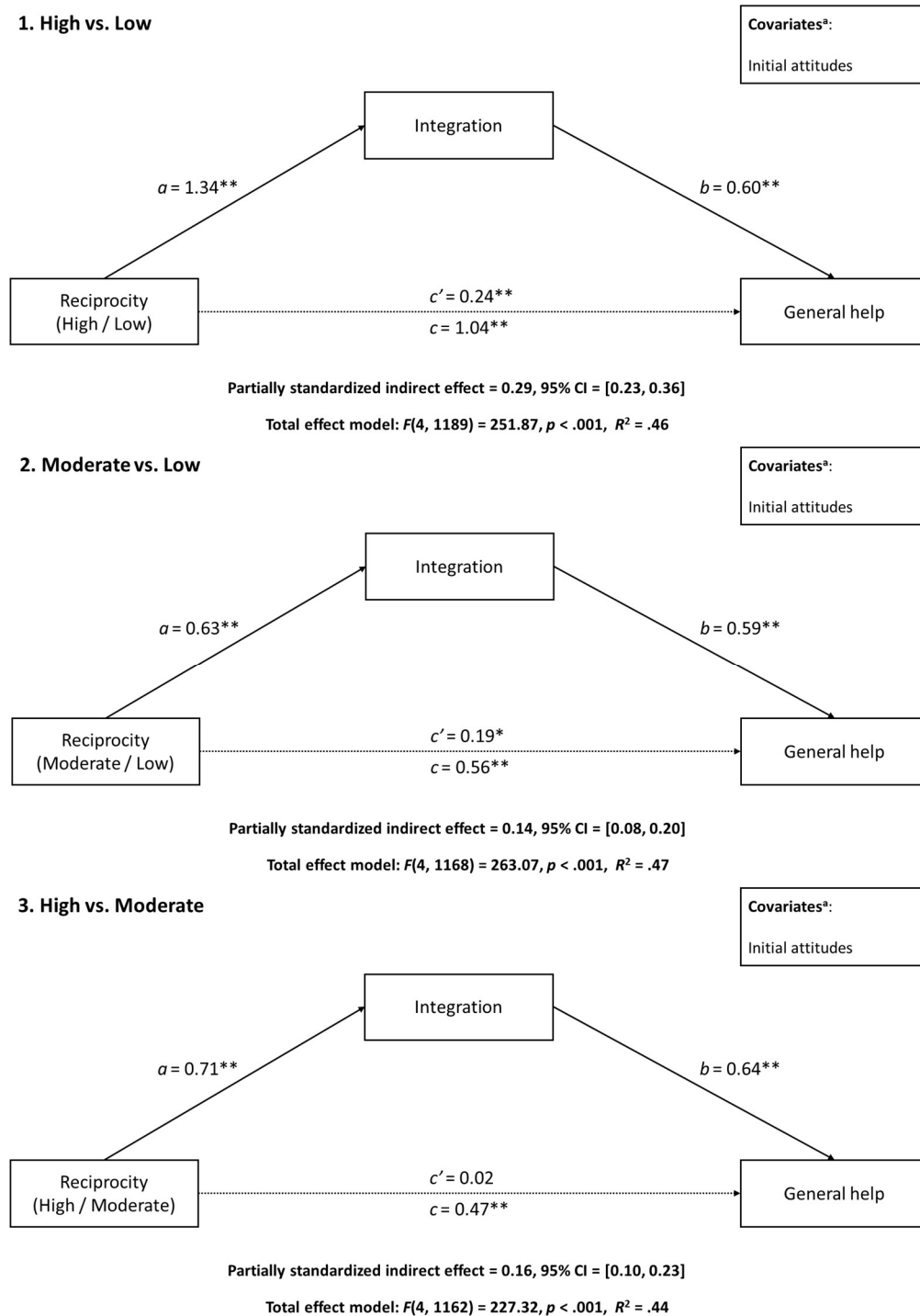
Note. Regression coefficients a , b , c and c' are unstandardized betas. Coefficient c is the total effect, while coefficient c' is the direct effect. Indirect effects are partially standardized because the independent variable is dichotomous.

^a The undiscussed nationality manipulation was also added as a covariate as it had a small marginal effect on variables.

* $p < .05$, ** $p < .01$

Figure 2

Mediations of integration on the relationship between reciprocity and general help – Study 1



Note. Regression coefficients a , b , c and c' are unstandardized betas. Coefficient c is the total effect, while coefficient c' is the direct effect. Indirect effects are partially standardized because the independent variable is dichotomous.

^a The undiscussed nationality manipulation was also added as a covariate as it had a small marginal effect on variables.

* $p < .05$, ** $p < .01$

Table 3

Descriptive statistics for all studied variable – Study 2

	Time 1				Time 2			
	<i>M</i>	<i>SD</i>	Skewness	Kurtosis	<i>M</i>	<i>SD</i>	Skewness	Kurtosis
Reciprocity	6.14	1.69	-0.13	-0.25	6.54	1.81	-0.22	-0.26
Integration	5.24	1.99	-0.19	-0.30	5.63	2.13	-0.23	-0.26
Attitudes	5.95	2.21	-0.34	-0.51	6.17	2.17	-0.42	-0.36
Instrumentality	6.15	2.08	-0.26	-0.11	7.02	1.97	-0.35	-0.28
Moral obligations	7.70	1.89	-0.42	-0.80	7.74	1.90	-0.46	-0.76
Empathy	5.97	1.54	0.08	0.08	5.89	1.66	-0.20	-0.03
General help	-	-	-	-	5.94	2.81	-0.36	-0.86
Specific help	-	-	-	-	7.12	2.69	-0.92	-0.09
Concrete help	-	-	-	-	0.58	-	-	-
Help efficacy	-	-	-	-	5.90	2.04	-0.11	-0.21

Table 4

Means and standard deviations of every measure by reciprocity experimental groups and measurement time – Study 2

	Low reciprocity (<i>n</i> = 156)				High reciprocity (<i>n</i> = 162)			
	T1		T2		T1		T2	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Reciprocity	5.97	1.75	6.46	1.84	6.30	1.61	6.61	1.78
Integration	5.23	2.01	5.76	2.14	5.26	1.98	5.51	2.12
Attitudes	5.86	2.17	6.07	2.21	6.03	2.26	6.26	2.13
Instrumentality	6.15	2.15	6.82	2.12	6.16	2.02	7.21	1.79
Moral obligations	7.65	1.92	7.66	1.84	7.76	1.87	7.82	1.95
Empathy	5.91	1.43	5.86	1.64	6.03	1.65	5.93	1.68
General help	-	-	5.81	2.89	-	-	6.06	2.74
Specific help	-	-	6.89	2.68	-	-	7.34	2.70
Concrete help	-	-	0.56	-	-	-	0.60	-
Help efficacy	-	-	5.88	2.01	-	-	5.91	2.07

Table 5

Correlations between all studied variables – Study 2

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
1. Reciprocity (T1)	-															
2. Integration (T1)	.58**	-														
3. Attitudes (T1)	.76**	.58**	-													
4. Instrumentality (T1)	.53**	.58**	.53**	-												
5. Moral Obligations (T1)	.54**	.45**	.61**	.51**	-											
6. Empathy (T1)	.44**	.51**	.50**	.46**	.50**	-										
7. Reciprocity (T2)	.73**	.54**	.65**	.50**	.51**	.42**	-									
8. Integration (T2)	.56**	.65**	.61**	.60**	.47**	.49**	.65**	-								
9. Attitudes (T2)	.67**	.53**	.84**	.52**	.59**	.40**	.75**	.67**	-							
10. Instrumentality (T2)	.49**	.45**	.49**	.71**	.47**	.36**	.65**	.58**	.59**	-						
11. Moral Obligations (T2)	.45**	.40**	.56**	.43**	.69**	.37**	.58**	.56**	.64**	.59**	-					
12. Empathy (T2)	.42**	.45**	.48**	.47**	.41**	.70**	.52**	.51**	.49**	.48**	.42**	-				
13. General Help	.47**	.48**	.54**	.46**	.42**	.47**	.60**	.66**	.65**	.56**	.52**	.54**	-			
14. Specific Help	.35**	.40**	.40**	.38**	.35**	.38**	.46**	.51**	.47**	.51**	.44**	.33**	.70**	-		
15. Concrete Help	.23**	.27**	.31**	.30**	.28**	.33**	.41**	.39**	.38**	.38**	.36**	.35**	.57**	.55**	-	
16. Help Efficacy	.34**	.37**	.38**	.32**	.32**	.33**	.50**	.42**	.47**	.47**	.44**	.49**	.52**	.47**	.50**	-

Note. Coefficients are two-tails standardized correlation coefficients.

* $p < .05$, ** $p < .01$

Table 6

Linear regressions predicting help measures with measured reciprocity – Study 2

	Help measures								
	General help			Specific help			Concrete help		
	β	<i>SE</i>	<i>p</i>	β	<i>SE</i>	<i>p</i>	<i>b</i>	<i>SE</i>	<i>p</i>
Demographics									
Age	.07	0.01	.124	.04	0.01	.507	0.02	0.01	.047
Gender	.09	0.26	.081	.02	0.28	.777	0.10	0.29	.736
Education	-.07	0.29	.184	-.04	0.30	.471	-0.11	0.32	.736
Revenue	-.04	0.27	.424	-.02	0.28	.879	-0.35	0.29	.770
Population density	-.05	0.26	.289	-.05	0.27	.381	0.09	0.30	.245
Political orientation	-.17	0.12	<.001	-.13	0.13	.021	-0.12	0.14	.413
Controls									
Help efficacy	.34	0.07	<.001	.33	0.07	<.001	0.62	0.10	<.001
Predictor									
Reciprocity (T1)	.32	0.08	<.001	.22	0.09	<.001	0.13	0.10	.170
<i>R</i> ²	.40		<.001	.25		<.001	.25 ^a		

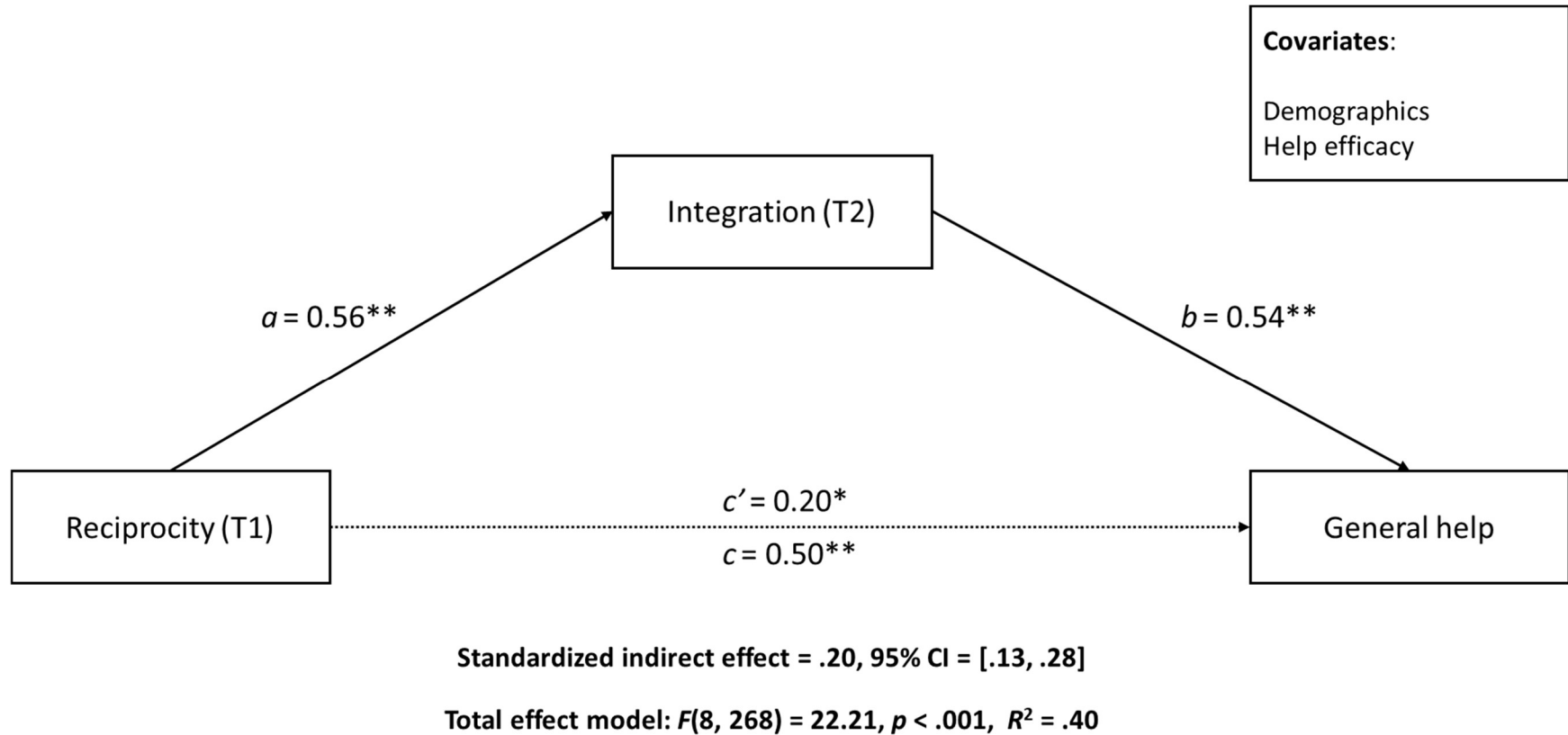
Note. Aside from the *R*² row, presented regression coefficients are standardized betas for general and specific help (linear regressions) and unstandardized betas for concrete help (logistic regression).

^a *R*² for concrete help was calculated by SPSS with the Cox & Snell method for logistic regressions. SPSS does not provide a *p* value for Cox & Snell *R*².

* *p* < .05, ** *p* < .01

Figure 3

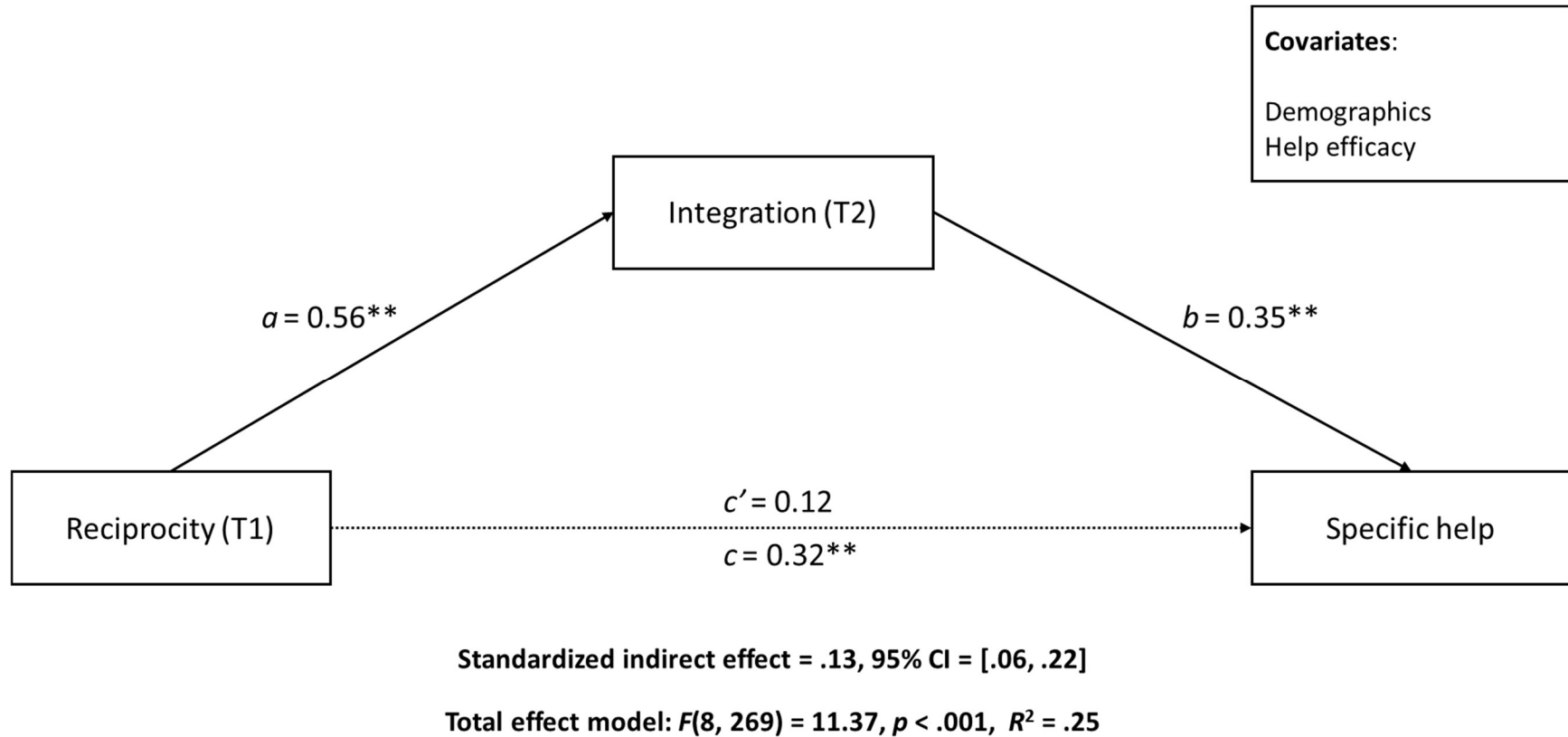
Mediation of integration on the relationship between reciprocity and general help – Study 2



Note. Regression coefficients a , b , c and c' are unstandardized betas. Coefficient c is the total effect, while coefficient c' is the direct effect.
* $p < .05$, ** $p < .01$

Figure 4

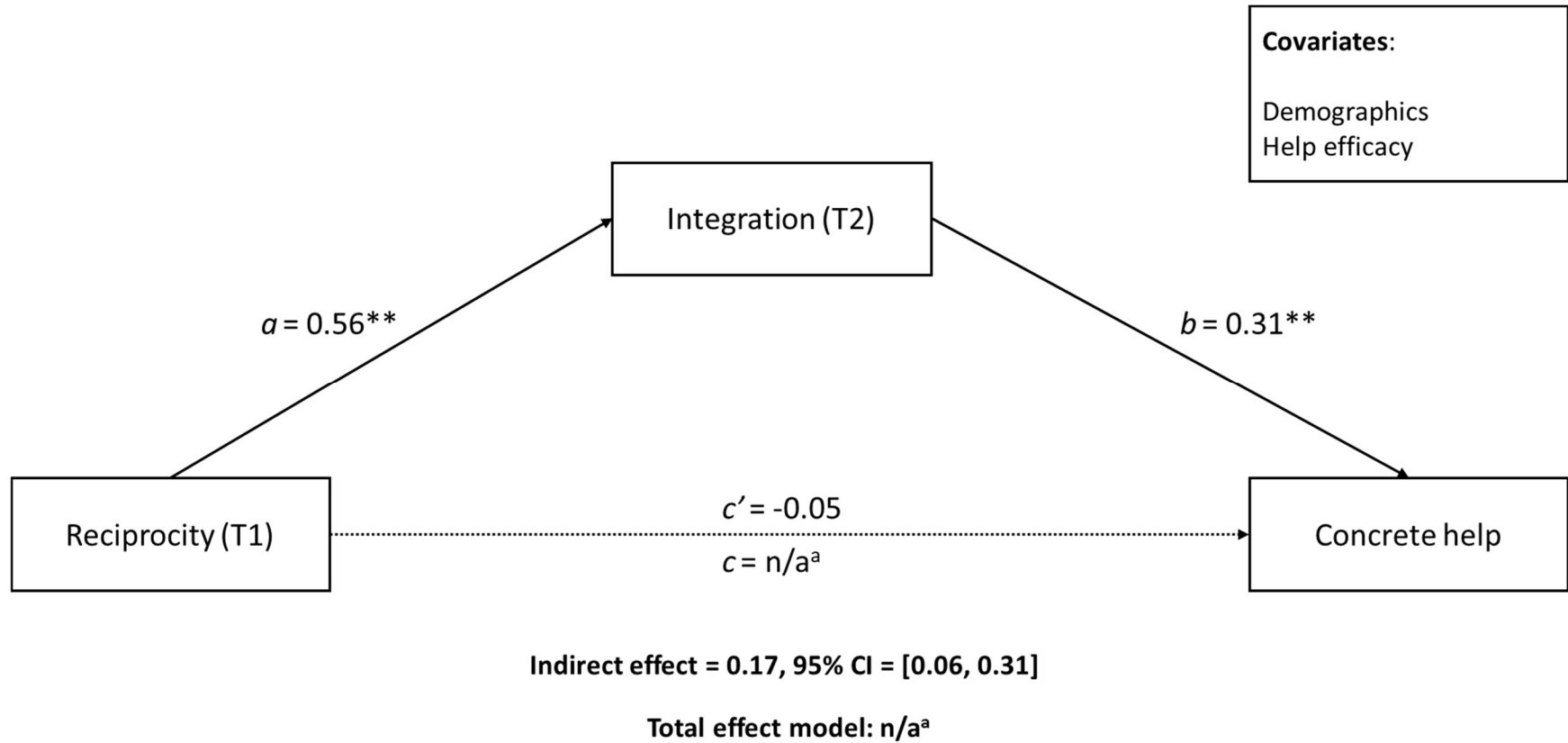
Mediation of integration on the relationship between reciprocity and specific help – Study 2



Note. Regression coefficients a , b , c and c' are unstandardized betas. Coefficient c is the total effect, while coefficient c' is the direct effect.
* $p < .05$, ** $p < .01$

Figure 5

Mediation of integration on the relationship between reciprocity and general help – Study 2



Note. Regression coefficients a , b and c' are unstandardized betas. Coefficient a is from a linear regression, coefficients b and c' are from logistic regressions. Coefficient c is the total effect, while coefficient c' is the direct effect. The indirect effect is unstandardized because the dependent variable is dichotomous.

^a PROCESS does not provide total effects (c) or total effect models with dichotomous dependent variables.

* $p < .05$, ** $p < .01$

Figure 6

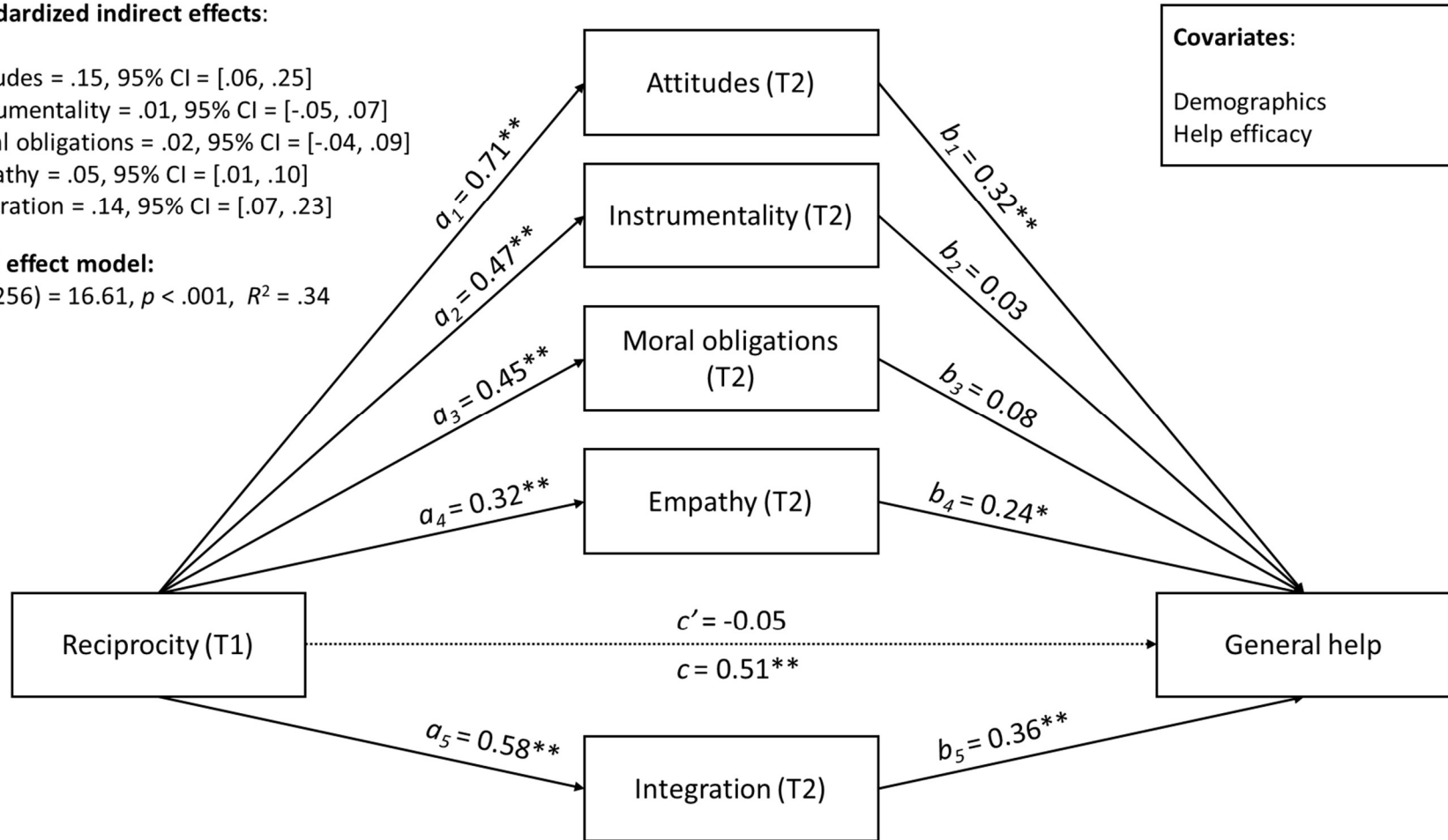
Parallel mediations of all mediators on the relationship between reciprocity and general help – Study 2

Standardized indirect effects:

Attitudes = .15, 95% CI = [.06, .25]
 Instrumentality = .01, 95% CI = [-.05, .07]
 Moral obligations = .02, 95% CI = [-.04, .09]
 Empathy = .05, 95% CI = [.01, .10]
 Integration = .14, 95% CI = [.07, .23]

Total effect model:

$F(8, 256) = 16.61, p < .001, R^2 = .34$



Note. Regression coefficients a , b , c and c' are unstandardized betas. Coefficient c is the total effect, while coefficient c' is the direct effect.

* $p < .05$, ** $p < .01$

Figure 7

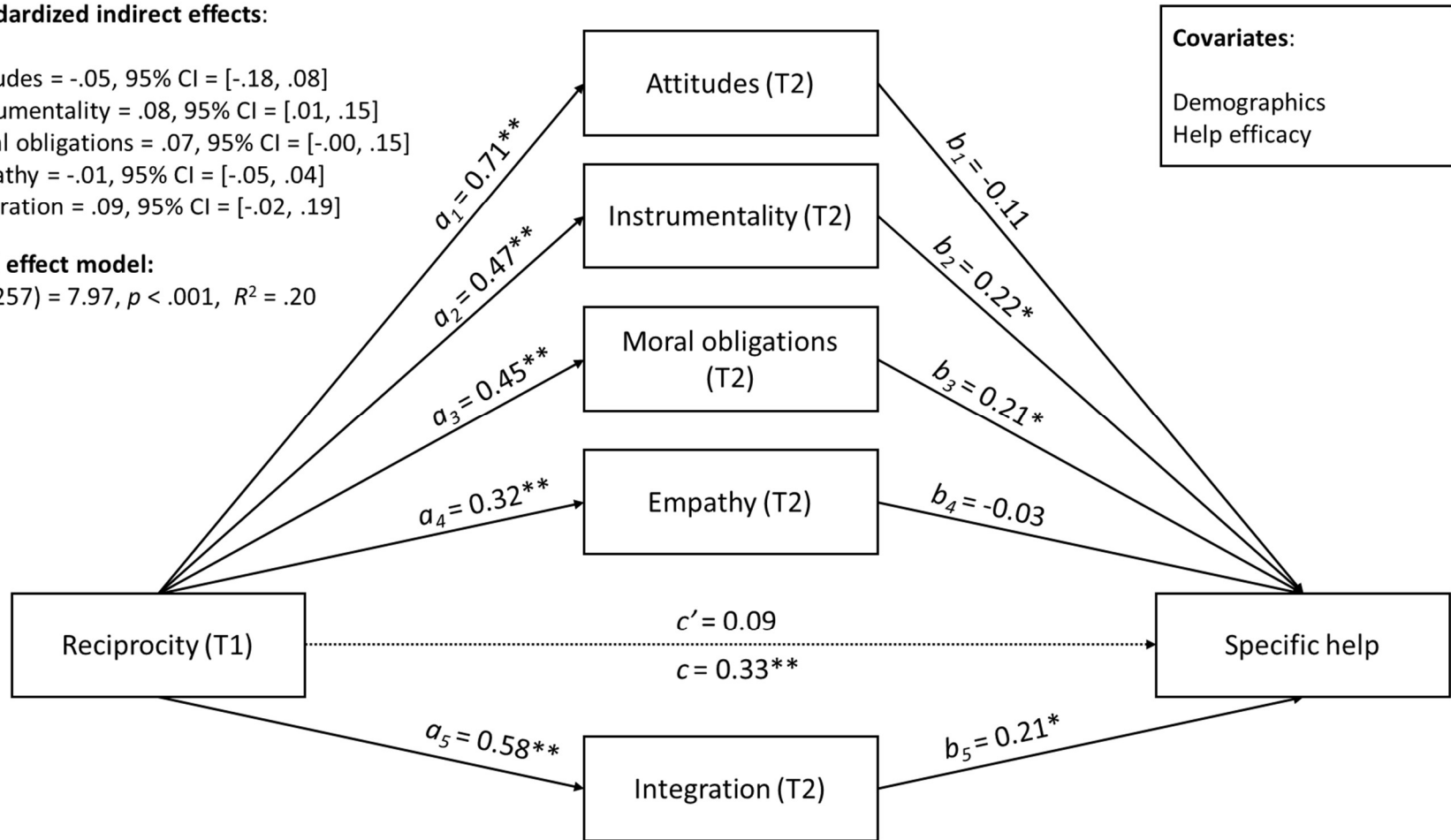
Parallel mediations of all mediators on the relationship between reciprocity and specific help – Study 2

Standardized indirect effects:

Attitudes = -.05, 95% CI = [-.18, .08]
 Instrumentality = .08, 95% CI = [.01, .15]
 Moral obligations = .07, 95% CI = [-.00, .15]
 Empathy = -.01, 95% CI = [-.05, .04]
 Integration = .09, 95% CI = [-.02, .19]

Total effect model:

$F(8, 257) = 7.97, p < .001, R^2 = .20$



Note. Regression coefficients *a*, *b*, *c* and *c'* are unstandardized betas. Coefficient *c* is the total effect, while coefficient *c'* is the direct effect.
 * $p < .05$, ** $p < .01$

Figure 8

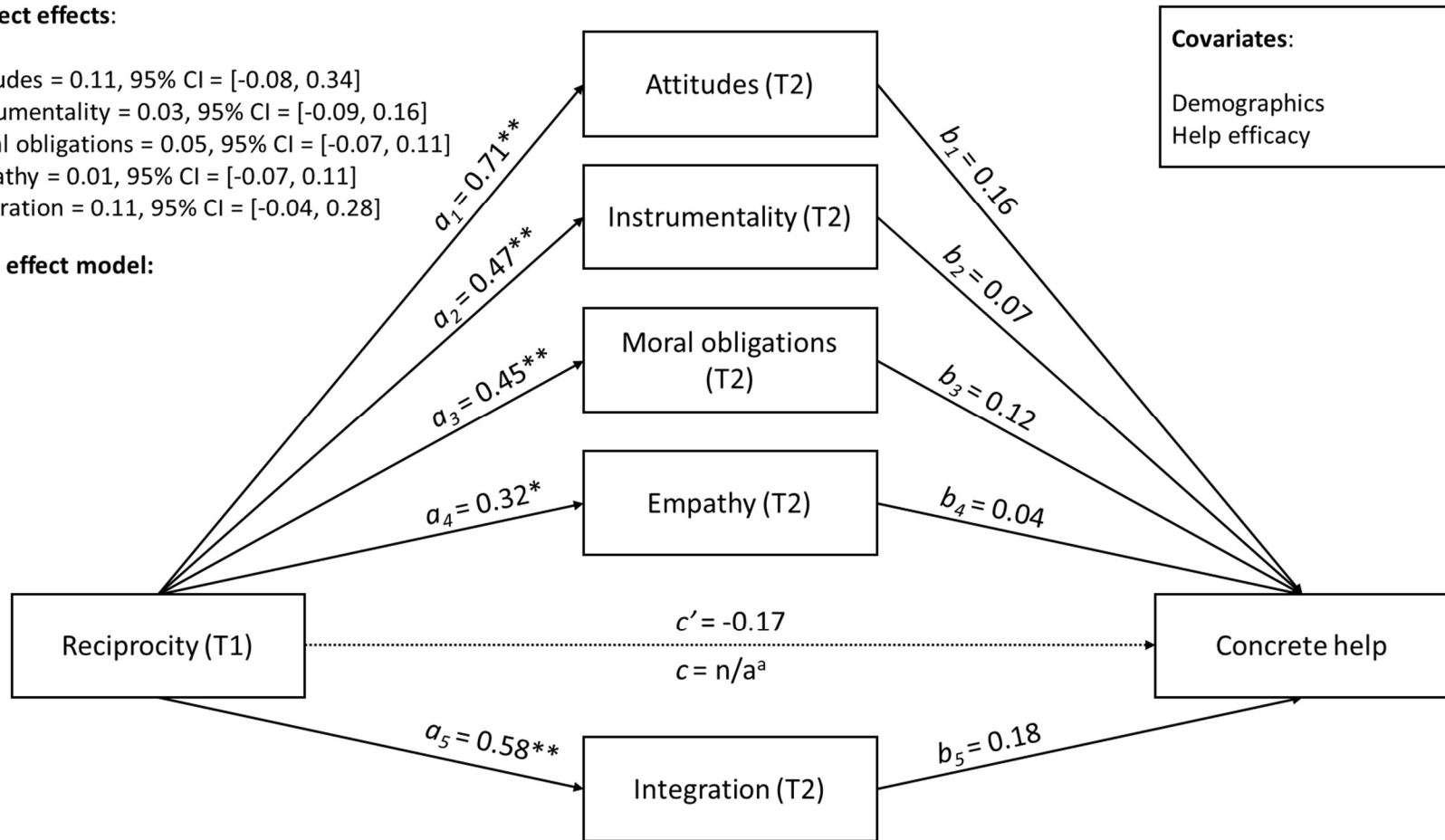
Parallel mediations of all mediators on the relationship between reciprocity and concrete help – Study 2

Indirect effects:

Attitudes = 0.11, 95% CI = [-0.08, 0.34]
 Instrumentality = 0.03, 95% CI = [-0.09, 0.16]
 Moral obligations = 0.05, 95% CI = [-0.07, 0.11]
 Empathy = 0.01, 95% CI = [-0.07, 0.11]
 Integration = 0.11, 95% CI = [-0.04, 0.28]

Total effect model:

n/a^a



Note. Regression coefficients a , b and c' are unstandardized betas. Coefficient a is from a linear regression, coefficients b and c' are from logistic regressions. Coefficient c is the total effect, while coefficient c' is the direct effect. The indirect effect is unstandardized because the dependent variable is dichotomous.

^a PROCESS does not provide total effects (c) or total effect models with dichotomous dependent variables.

* $p < .05$, ** $p < .01$

Supplementary Table 1

Correlations between Time 1 variables, change in variables at Time 2 and help measures – Study 2

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
1. Reciprocity (T1)	-															
2. Integration (T1)	.58**	-														
3. Attitudes (T1)	.76**	.58**	-													
4. Instrumentality (T1)	.53**	.58**	.53**	-												
5. Moral obligations (T1)	.54**	.45**	.61**	.51**	-											
6. Empathy (T1)	.44**	.51**	.50**	.46**	.50**	-										
7. Reciprocity (Δ)	-.25**	-.03	-.12*	-.05	-.02	-.04	-									
8. Integration (Δ)	.04	-.28**	.11*	.09	.04	-.05	.23**	-								
9. Attitudes (Δ)	-.13*	-.05	-.28**	-.05	-.07	-.18**	.41**	.15*	-							
10. Instrumentality (Δ)	-.04	-.04	.02	-.28**	.01	-.02	.28**	.18**	.19**	-						
11. Moral obligations (Δ)	-.07	-.07	-.10	-.11	-.33**	-.13*	.31**	.21**	.21**	.24**	-					
12. Empathy (Δ)	.01	-.06	-.05	.00	-.06	-.30**	.18**	.20**	.16**	.20**	.18**	-				
13. General help	.47**	.48**	.54**	.46**	.42**	.47**	.18**	.29**	.14*	.23**	.10	.14*	-			
14. Specific help	.35**	.40**	.40**	.38**	.35**	.38**	.18**	.21**	.11*	.33**	.12*	.07	.70**	-		
15. Concrete help	.23**	.27**	.31**	.30**	.28**	.33**	.25**	.17**	.10	.21**	.09	.12*	.57**	.55**	-	
16. Help efficacy	.34**	.37**	.38**	.32**	.32**	.33**	.22**	.13*	.13*	.27**	.12*	.21**	.52**	.47**	.50**	-

Note. Coefficients are two-tails standardized correlation coefficients.

* $p < .05$, ** $p < .01$