Violent Conflict and Parochial Trust: Lab-in-the-Field and Survey Evidence[1]

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Abstract

How does conflict exposure affect trust? We hypothesize that direct (first-hand) experience with conflict induces *parochialism*: trust towards out-groups worsens, but trust towards in-groups, owing to positive experiences of kin solidarity, may improve. Indirect exposure to conflict through third-party accounts, on the other hand, reduces trust toward everyone, owing to negativity bias. We find consistent support for our hypotheses in a lab-in-the-field experiment in Maluku, Indonesia, which witnessed a salient Christian-Muslim conflict during 1999-2002, as well as in three cross-country datasets exploiting temporal and spatial variation in exposure to violence. Our results help resolve a seeming contradiction in the literature and inform policies on resolving conflicts.

Keywords: Trust; conflict; direct exposure; indirect exposure; religion; discrimination.
JEL Classification: C93, D74, Z12, Z13

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1 Introduction

Trust is an important precursor of key socially desirable outcomes, from economic growth to social capital and political stability (Knack and Keefer 1997; Olken 2009; Bjørnskov 2012; Nunn, Qian and Wen 2023), among many others. Mistrust, once it takes root, is extremely persistent (Nunn and Wantchekon 2011; Nikolova, Popova and Otrachshenko 2022): understanding trust is therefore critically important. But choosing who to trust is an ominous decision in human interactions, even in ordinary times. Deciding who to trust is more difficult, still, in post-conflict societies, where tensions are high and further conflict often looms. Thus, understanding how conflict affects trust is crucial if we are to work towards building more peaceful societies in the wake of violence.

Despite the critical importance of the trust – conflict nexus, the literature has so far failed to reach a consensus as to how conflict affects trust. The range of results in the literature is wide, including negative effects on trust towards everyone (Posen 1993; Colletta and Cullen 2000; De Luca and Verpoorten 2015b), negative effects towards out-groups (Whitt 2010; Rohner et al. 2012, 2013; Korovkin and Makarin 2023), and positive effects (Lyons et al. 1998; Bellows and Miguel 2009; Blattman 2009; Voors et al. 2012; Gilligan et al. 2014; De Luca and Verpoorten 2015a; see Bauer et al. 2016 for a meta-analysis).[2] These strands of literature stand in apparent contradiction, and the question of whether conflict damages trust remains without a conclusive answer.

In this paper, we attempt to reach such an answer by tackling two main problems that have affected existing work: *conflict exposure* and *generalizability*. First, we propose a conceptual framework relating post-conflict individual-level trust outcomes to one's degree of exposure to the violence. Our approach relies on asking *how* each individual learns about the violence. Specifically:

(i) Witnessing violence first-hand is doubtless devastating and should reduce trust in out-group members. *But* it also offers bonding opportunities with in-group members (e.g. mutual defense, offering of shelter, collective coping). We thus expect *direct* conflict exposure to lead to reduced out-group trust and increased in-group trust.

(ii) Learning about the violence exclusively through second-hand accounts (being *indirectly* exposed) means one was comparatively much less in need of the helping

[2] The latter is consistent with psychological phenomena labelled "altruism born of suffering" (Staub and Vollhardt 2008) or "posttraumatic growth" (Tedeschi and Calhoun 2004).

hands of others. Still, indirectly exposed individuals heard about other people's negative experiences and frightening stories. Under the (in our view) reasonable assumption of negativity bias, which we elaborate on below, indirectly exposed people have heard mainly about the bad aspects of the conflict and very little, if at all, about positive experiences of solidarity (Baumeister et al. 2001. p. 331-332). Thus, we expect *indirect* conflict exposure to reduce trust in all people.

Germany's *Kriegsenkel* generation (grand-children of war) provides corroborating evidence for our framework. The *Kriegsenkel* is a generation of people born after World War II, who carry a large emotional burden from the war, including often describing themselves as "not trusting people"[3]. These grandchildren of war have learned about the war *exclusively* from the reports of others, and meet in dedicated forums and self-help groups to discuss their ideas, problems and experiences, both on the internet and in many German cities. There has been a large amount of writing on the topic, mainly in German, including magazine articles (e.g. Janker 2015; Halser 2016), novels written from the perspective of grandchildren of war, as well as non-fiction books written by psychologists, sociologists, and *Kriegsenkel*, to give guidelines and advice to other affected people (e.g. Jakob 2020[4]). The *Kriegsenkel* are generally seen as less trusting and deemed to have a high share of people with attachment disorders. The experience of the grandchildren of war is not unique to Germany: Aarts (1998) interviewed psychotherapists about their experiences providing care for the children of World War II survivors from the former Dutch East Indies. Aarts found that the children "were perceived to lack basic trust and to have difficulties in regulating or expressing strong emotions" (Jakob 2020, p. 187).

Equipped with our conceptual framework and corroborating evidence, we first conduct a lab-in-the-field experiment in Indonesia testing how participants' exposure to ethnic tensions and a violent conflict between Muslims and Christians influences their propensity to trust members of their religious or ethnic in-group and out-group. Using a standard trust game, we find that indirect and direct exposure indeed have different effects: directly exposed participants are strikingly parochial, trusting in-groups much more than out-groups, while indirectly exposed participants exhibit lower levels of trust towards everyone, in-groups and out-groups alike. These patterns are observed both for Muslim and Christian respondents, which we tentatively interpret as an

[3] See for example: http://www.forumkriegsenkel.de/Studie.htm
[4] See http://www.uni-forst.gwdg.de/~wkurth/psh/k_lit_en.htm for an overview of some books on the topic.

indication that the rules governing human parochialism are fairly universal, as suggested by Henrich (2020, p. 328: "our species (has) a genetically evolved response to war and other shocks").

Still, Henrich's point notwithstanding, the *generalizability* problem, as alluded to above, persists. Existing studies in the literature on trust and conflict have conducted surveys and/or experiments in particular times and places. While doubtless valuable, one cannot learn from such contributions whether the results might be specific to a particular conflict, polity, or time period. External validity is thus worrisome, which may indeed explain some of the heterogeneity in the results documented in the existing literature. In this regard, our trust game in Maluku is no exception. We therefore examine whether the results we find in Maluku also hold in other parts of the world. Specifically, we analyze three secondary datasets: (i) the Social Well-Being Survey (SWBS) in Asia, which covers 1,111 respondents from seven Asian countries; (ii) the World Values Survey (28,073 respondents from 12 countries); and (iii) the Afrobarometer (15,723 respondents from 11 countries).[5] Doing so allows us to empirically establish that the patterns found in Maluku do have external validity and are not an artefact of the experimental set-up, or specific to the Maluku conflict.

Across the lab-in-the-field experiment and the three survey datasets, we find that indirect conflict exposure significantly reduces trust towards all groups – congruent with the anecdotal evidence from Germany's grandchildren of war. Direct exposure, on the other hand, induces discriminatory trust by enhancing trust within groups and reducing trust in out-group members. The latter finding is consistent with the parochial altruism (that is, in-group bias) phenomenon documented in Bauer et al. (2016, p. 271), who stress that "The most important next step will be for researchers to focus on establishing the reach and generality of (parochial altruism)", described by Blattman (2022, p. 77) and Haushofer et al (2023) as a key behavioral tendency. Here, we document a *parochial trust* phenomenon: trust is systematically biased towards in-groups and against out-groups. The fact that we find these patterns in such a wide variety of contexts supports Henrich's (2020, p. 328) universality claim, i.e. that humans share a genetically evolved psychology to deal with war. Our findings are therefore likely applicable to a wide range of potential conflicts.

We employ a battery of robustness checks to support our findings. In particular, we document the phenomenon of parochial trust at both the extensive margin (direct exposure relative to indirect exposure) as well as at the intensive margin of conflict, as we find that varying degrees

[5] See Figure A.8 for a map of countries covered.

of direct exposure to conflict are also associated with more parochial trust. Moreover, we run various tests to make sure that our findings are not driven by self-selection into conflict exposure. Randomization inference estimates also corroborate the findings from the lab-in-the-field experiment under all possible alternate treatment assignments.

2 Contributions and Related Literature

In the literature, trust is commonly defined as the expectation a person has about another person's cooperative behavior or willingness to reciprocate a favor. Trust is a key element of social capital and believed to foster several normatively desirable outcomes, including cooperation, economic growth, civic engagement, and democracy (Uslaner 2002; Bjørnskov 2009, 2012; Fukuyama 1995). Trust is also the basis for peace negotiations: only with sufficient trust can people cooperate and come together to resolve disputes. But trust involves vulnerability and the risk of being worse off if the counterpart defects. A history of conflict and fear may increase the perceived risk that the counterpart defects, which may hinder the levels of trust and dialogue required for reconciliation (Posen 1993; Shayo and Zussman 2017; Casey and Glennerster 2016) and cause a vicious cycle of violence (Weingast 1998; Rohner et al. 2013). Trust in a post-conflict environment is thus an important indicator of the potential for peace reconciliation.

Understanding the contradictory results in the literature can only be approached by capturing the varying types of exposure to conflict (direct vs. indirect) *and* with a clear distinction between in-group and out-group. In a recent meta-analysis, Bauer et al. (2016) point out that too few studies clearly distinguish between in-group and out-group behavior to derive clear results on discriminatory trust. In doing so, we improve upon two strands of literature. The first studies patterns of behavior in conflict-affected areas, but without accounting for the nature of the conflict exposure of participants. For example, Schubert and Lambsdorff (2014) show that Palestinian recipients in ultimatum games in the West Bank display negative reciprocity towards ultimatums written in Hebrew. Similarly, Gneezy and Fessler (2011) exploit group-level, but not individual-level variation in conflict across time (prior to, during and after the 2006 war between Israel and Hezbollah; see also Whitt and Wilson 2007; Whitt 2012). A second strand of literature examines the degree of conflict exposure, without distinguishing between trust towards in-group members and towards out-group members. For example, Voors et al. (2012) document effects of conflict exposure on prosociality, risk, and time preferences in Burundi, while Gilligan et al. (2014) show that conflict-affected villages in Nepal display higher levels of collective action. Using surveys

with Turkish conscripts, Kibris and Gerling (2022) show that conflict exposure increases trust, but subjects who have experienced traumatic incidents exhibit lower levels of trust. Islam et al. (2023) find that conflict exposure affects men and women differently, but do not study differences in behavior towards particular in-groups or out-groups.[6]

Our work contributes to an as-yet small strand of the literature investigating both subjects' degree of conflict exposure and in-group / out-group effects. The pattern emerging from these studies is that discrimination increases with exposure to conflict. In different games with children in the Republic of Georgia and Sierra Leone, Bauer et al. (2014) find children to favor in-group over out-group members. This discrimination is more pronounced for highly exposed subjects. Similarly, Cecchi et al. (2016) show that street soccer players in Sierra Leone who were more strongly exposed to violence during the conflict are more altruistic toward in-group members in a dictator game and, at the same time, reveal more aggressive behavior toward their opponents during soccer games. Mironova and Whitt (2016a) find that high exposure to violence of Albanians and Serbs in Kosovo has no effect on trust in people from their local community, i.e. clear in-group members. However, they also point to a negative effect on trust in people from elsewhere in Kosovo, whose group affiliation is more uncertain. In addition, they find higher pro-sociality toward in-group than toward out-group members, with the gap between in-group and out-group further increasing with exposure to both physical violence and destruction of property. Mironova and Whitt (2016b) show that discrimination 18 years after the conflict in Bosnia was even higher than 8 years after the conflict, but that victims of violence exhibited more pro-social behavior toward out-group members than non-victims.

We depart from these studies by examining the effects on trust of *direct* and *indirect* conflict exposure. Thus, we focus on two meaningfully different types of conflict exposure (having made first-hand experiences vs. having heard about others' experiences). This distinction is theoretically relevant (Section 3), because people with first-hand experience of conflict may be affected quite differently from people with only word-of-mouth knowledge of conflict. Practically, this distinction is also highly relevant because typically indirectly affected individuals make up a much larger share of the population than directly affected ones (Korovkin and Makarin 2023). We do so in four different studies in which we operationalize direct and indirect exposure in consistent ways.

[6] See also Gangadharan et al. (2022) for Cambodia and Booth et al. (2022) for China, who distinguish between direct and indirect exposure. A summary table of the experimental literature can be found in Table A.1 in the appendix.

3 Conceptual Framework: Conflict Exposure and Hypotheses

Spyer (2002, p. 33) argues that war reports, which obfuscate the initiators of incidents, produce "a sense of phantom danger, which lurks both nowhere in particular and therefore potentially everywhere in general", which creates opportunities for destructive (and potentially inaccurate) stereotypes (Bai et al. 2022, Cuddy et al 2007). Thus, we expect people who only learn about the violence through second-hand reports to distrust others indiscriminately, without focus on any particular groups.

Under the assumption of negativity bias, indirectly exposed people primarily learn negative information from their in-groups, but not positive information (or a negligible amount of the latter). This assumption, while not directly testable, strikes us as appropriate for three reasons. First, the literature, at large, emphasizes the asymmetric effects of positive and negative events. In Kahnemann and Tversky's (1979) prospect theory, the value function that maps monetary gains and losses to utility delivers larger utility *losses* for, e.g., a loss of $100, than utility *gains* for a gain of $100. If the propensity to communicate information about a given event is proportional to the utility changes from said event, then it follows that negative events should be communicated about more than positive ones. Thus, even an agent who communicates in an unbiased fashion, will broadcast more negative stories than positive ones. Second, but not necessarily consequently, people put more emphasis on bad events in their interpersonal communication (Baumeister et al. 2001). Words which describe negative events are more commonly found in natural language than their opposites (i.e. would-be descriptors of positive events; Rozin et al. 2010). Third, even if we set aside the question of whether the sender of information emits biased signals, the literature shows that signal processing (from the recipients' end) displays negativity bias. Ito et al. (1998) study the neurological basis for this phenomenon, while Soroka (2006) finds that negative economic news shift attitudes more than positive news.

In sum, while we cannot determine whether negativity biases arise because of biased information sending and/or processing, we proceed with the assumption that indirectly exposed individuals heard reports of their friends' or family members' negative experiences, for example, that somebody took their belongings, destroyed their houses, or that people were raped, injured, or killed. But, importantly, they did not hear (or heard much less) about the positive experience of

group solidarity that directly involved people may have had.[7] In media reports, negativity bias around conflict is well-documented (Slone 2000; Lee and Maslog 2005; Spyer 2002, p. 24-25, 27), including in Maluku, where our experiment was conducted. Indirectly exposed people may also have been too far from the conflict to assess the risks, or to distinguish friend from foe, as all fighting groups may have been victims and perpetrators at the same time (Qurtuby 2015). In sum, we expect people who only learn about the violence through second-hand reports to be prone to distrust others, without a focus on certain groups:

Hypothesis 1: Indirect exposure is related to low levels of trust toward all groups.

In contrast, *directly* exposed people may have had two kinds of experiences. On the one hand, they felt their life was in danger when they were injured, saw others being killed, or experienced the frightening situation of being physically threatened (Bertrand 2002, p. 75; Spyer 2002, p. 25). On the other hand, directly exposed people may have experienced people risking their lives to defend others (Tucker and Ferson 2008, p. 114). They may have experienced solidarity and emotional support of people comforting them when they lamented the death of a family member (Gilligan et al. 2014, pp. 615-616). They may have been given shelter when their house was destroyed or received care when they were injured. They experienced the strength of a group in reconstructing houses and other buildings that were destroyed in the conflict (Barron et al. 2010).

Due to these two different kinds of experiences, direct exposure to violent conflicts may induce a gap between behavior toward the in-group and out-group (Tajfel and Turner 1979; Inglehart et al. 2006). This gap can stem from preferential treatment of in-group members or disadvantageous treatment of out-group members compared to neutral subjects (Singh et al. 1998).

Hypothesis 2: Direct conflict exposure increases parochialism.

As people in a crisis usually approach those they are familiar with, the aforementioned positive experiences are more likely to have taken place within families, neighborhoods or communities, i.e. with in-group members (Lyons et al. 1998; Colletta and Cullen 2000, pp. 74-75). When dealing with somebody they do not know personally, people tend to assess the person's

[7] Note that there are certainly competing and complementary explanations for why indirectly affected people may be less trusting. They may also have been socialized by their parents to be excessively careful and not to easily trust others, or may have observed behavior of witnesses of the conflict that reflect fear, much like the children of former combatants in the Dutch East Indies deemed by therapists to lack basic trust in others (Jakob 2020; Section 1). We view such explanations as reasonable complements to negativity bias, and with similar testable implications, although we cannot empirically distinguish between the two sets of explanations.

trustworthiness based on ascriptive characteristics like gender, ethnicity, or religion, and on perceived similarities or differences based on these characteristics (Fearon and Laitin 1996). Directly exposed people may thus project their positive experiences with some in-group members onto the entire group and reveal higher trust towards all in-group members.

Hypothesis 2a: Direct conflict exposure enhances in-group trust.

On the other hand, the negative experiences are likely to have destroyed trust in out-group members because directly exposed subjects may blame the entire out-group. This pattern has been found in several conflict-affected regions in Europe, Africa, Asia, and Central America, including in Cambodia, Rwanda, Guatemala, Somalia (Colletta and Cullen 2000), Croatia, Serbia, and Bosnia and Herzegovina (Mironova and Whitt 2016a). Directly exposed people are thus expected to show lower levels of trust toward out-group members than toward neutral subjects or in-group members.

Hypothesis 2b: Direct conflict exposure erodes out-group trust.

Hypotheses 2a and 2b are in line with the in-group/out-group hypothesis (Allport 1954; Tajfel and Turner 1979), which states that conflicts may increase preferential treatment of in-group members and discrimination against out-group members. Zussman (2014) and Sambanis and Shayo (2013) show that conflict can stiffen group identities. This view also receives support by evolutionary theories that consider inter-group conflict as an important origin of parochialism in the form of human in-group cooperation and out-group hostility (Darwin 1873; Choi and Bowles 2007; Bowles 2008).

4 Experimental Study: The Maluku Conflict

4.1 Background

The Maluku islands, located in the eastern part of Indonesia, were struck by a violent conflict that was mainly fought between Muslim and Christian residents from 1999 to 2002. Spreading from one island to many others, the communal violence resulted in the displacement of more than 700,000 people and caused between 5,000 and 10,000 fatalities (Spyer 2002; Lowry and Littlejohn 2006). Despite the Malino Peace Agreement in 2002, which officially terminated the conflict,

tensions and deep resentments are perceptible until today and the conflict surged again several times (Lowry and Littlejohn 2006; Adam 2010).[8]

The resentments partly go back to colonial times when Christians enjoyed a privileged status in Maluku, receiving better access to education and high-ranking positions, and their ensuing fear of losing these rights under President Suharto (Spyer 2002; Sukma 2005). But, although fighting took place between Christians and Muslims, ethnic tensions between indigenous Moluccans and non-indigenous ethnic groups preceded the conflict and were even deemed to be one of the triggers of the conflict (Mearns 1999). In one of history's largest resettlement efforts, Suharto had transmigrated thousands of people from other Indonesian islands to Maluku between the 1970s and the 1990s, which had significantly increased the number of non-indigenous groups in the Maluku region (Bazzi et al. 2019). After Suharto's fall in 1998, the non-indigenous were increasingly perceived as intruders. In the tumultuous times after the fall of Suharto, religious identities were increasingly politicized (Adam 2010). Christians feared becoming a small and politically weak minority in a Muslim-dominated country and Muslims feared that Christians would re-establish their local dominance in Maluku (Bertrand 2002).

Importantly, despite the existence of prior ethnic tensions, the Maluku conflict was unexpected. Thus, any empirical patterns found in our experiment are unlikely to merely reflect pre-conflict phenomena. People lived peacefully together until "sudden and surprising violence broke out" (Bertrand 2002, p. 57). The violence "took the Indonesian Government and many other actors by surprise" (Brown et al. 2005, p. xiii) and people on both sides said they were "caught completely by surprise" (van Klinken 2001, p. 10).

4.2 Experimental Design

We measured trust in a standard trust game (Berg, Dickhaut and McCabe 1995). We matched subjects into pairs and assigned them either the role of the trustor or the trustee. All subjects received equal information on the game: Both players would receive an initial endowment of 30,000 Indonesian Rupiah (IDR, which was approximately equal to $3 at the time of the

[8] The existing literature on the Maluku conflict mainly focuses on the political causes of the conflict. These include Dutch colonial rule (Mearns 1999; Bertrand 2002; Spyer 2002), Suharto's authoritarian rule and his transmigration program (Mearns 1999; Rabasa and Chalk 2001; Spyer 2002; Brown et al. 2005; Sukma 2005; Lowry and Littlejohn 2006; Adam 2010), an increasing insecurity after Suharto's departure and institutional failures (Rabasa and Chalk 2001; Spyer 2002; Sukma 2005; Adam 2010) and the intervention of external provocateurs (Rabasa and Chalk 2001; Bertrand 2002; Spyer 2002; Adam 2010) as important triggers of conflict escalation.

experiment). From this point forward, we refer to 1,000 IDR as 1 RPK (1 kilo-Rupiah). The trustor, called Person A in the instructions, had the possibility to send an amount between 0 and 30 RPK, in increments of 3, to Person B (the trustee). Any RPK sent to Person B would be tripled by the experimenter before being assigned to Person B. Person B then had the possibility to return any amount from RPK 0 up to a maximum of the amount possessed (the sum of the initial endowment of RPK 30 plus three times the amount sent by Person A) to Person A.

The unique subgame-perfect equilibrium implies no transfers because a payoff-maximizing trustee should not return anything and a rational trustor would anticipate this and thus transfer nothing. Yet, the efficient outcome that maximizes joint payoffs requires the trustor to transfer her total endowment, because the transfer is tripled. Positive transfers by trustors are commonly interpreted as a measure of trust because the trustor, in order not to make a loss, has to believe that the trustee will pay her at least her transfer back. The transfer thus reflects the expectation that the other person will reciprocate. This enables an interaction that is beneficial to both players, but involves the risk that the counterpart defects (see Johnson and Mislin 2011 for a meta-analysis and Houser et al. 2010 for evidence that trust games measure trust rather than mere altruism or risk attitudes).

To determine the degree of conflict exposure of each participant, we relied on the questionnaire provided in Figure B.9 in the Appendix. We categorize subjects as indirectly exposed if they responded that a friend's belongings were destroyed in the conflict. Direct exposure comprises the most direct and physical experiences a survivor of the conflict can have made: having been injured and having been physically threatened during the conflict (Engel and Ibáñez 2007, p. 346). 29% of the subjects report neither direct nor indirect exposure and 30% report indirect, but no direct exposure. 26% had direct exposure: 23% reported physical threat, 15% injury, and 6% both. In the Appendix, we provide robustness checks with different broader measures of indirect and direct exposure.

4.3 Treatments

Subjects were uniformly assigned to one of three treatments, namely interaction with an *in-group member*, *out-group member* or *neutral subject*. In each treatment, subjects received subtle cues about their counterpart. When interacting with an in-group or an out-group member, participants received a cue about their counterpart's religious affiliation (in the religious condition), or ethnic group (in the ethnic condition).

In the *religious condition*, we disclosed the name of the university the counterpart was attending. As we ran the experiments at a Christian university (with 96% Christian students) and a Muslim university (99% Muslims), the counterpart's university affiliation provided a reliable source of information on the counterpart's religion.[9] Interviews and pre-tests prior to the experiments showed that subjects immediately thought about the counterpart's religion when learning her university affiliation, rather than simply regarding her as either a fellow student or a student of a different university. To insulate from experimenter demand effects, the university affiliation form we asked participants to complete (see Appendix B) also included an option for a mixed-faith university in Maluku and a nondescript "other university" option.

In the *ethnic condition*, we determined whether a participant was of indigenous Maluku background based on their knowledge of local languages. Participants were asked to select their parents' mother tongue from a list of languages including native Maluku languages, like Bahasa Ambon, for example, or languages spoken elsewhere in Indonesia, such as Javanese.

When interacting with *neutral subjects*, subjects received only information that was purposefully irrelevant to the conflict. Specifically, we disclosed to the participant whether her counterpart preferred to drink coffee or tea, or to eat tofu or tempe. There are no ethnic or religious cleavages in these two food and drink choices, so they were uninformative about the counterpart's personal background. Table 1 illustrates all possible resulting subject pairs. Table A.2 in the Appendix shows that there is a small overlap between religion and ethnicity, but that it is too small to infer the religion from the ethnicity or vice versa.

Table 1: Matching scheme

	Religious condition	**Ethnic condition**
Treatment	**Trustor => Trustee**	**Trustor => Trustee**
In-group member	Christian => Christian Muslim => Muslim	Indigenous => Indigenous Non-indigenous => Non-indigenous
Out-group member	Christian => Muslim Muslim => Christian	Indigenous => Non-indigenous Non-indigenous => Indigenous
Neutral subject	Trustee's religion and ethnicity unknown	

Each subject was able to see two answers her counterpart had given: Subjects interacting with a *neutral subject* saw their counterpart's two irrelevant answers on culinary questions. In the religious condition, subjects saw their counterpart's university affiliation plus one food and drink answer. Likewise, subjects in the ethnic condition saw their counterpart's local language skills

[9] There is no sports rivalry or other form of rivalry between the two universities and, in post-experimental questionnaires, subjects stated their religion was an important part of their personal identity (significantly more important than their major (two-sided t-test, p=0.0000), their nationality (p=0.0002) or their ethnicity (p=0.0000)).

plus one irrelevant answer. By giving each participant two answers, we disguised the purpose of the study from subjects. In addition, showing two pieces of information to each participant allows us to hold the amount of information constant across treatments and reduce the probability of minimal group effects (caused, for example, by a shared preference for coffee over tea). Additional robustness checks reconfirmed that a shared food preference did not increase transfers. Subjects knew that their counterpart would see their own responses to the exact same two questions. For example, a subject seeing her counterpart's university affiliation and drink preference knew that the counterpart would also see her university affiliation and drink preference.

4.4 Experimental Procedures and Sample

We ran the experiments in pen-and-paper format. Our sample consists of 364 students from two universities, one 99% Muslim and the other 96% Christian. These universities were, respectively, Institut Agama Islam Negeri (IAIN, which translates approximately to State Islamic Institute), and Universitas Kristen Indonesia Maluku (UKIM, or Indonesian Christian University of Maluku). The data collection took place in September 2013, over the course of four weeks[10]. This was 11 years after the peak time of the conflict, but violence had briefly surged again every year, the last time in May 2013. Thus, this conflict is salient for our experimental participants. A major advantage of the student sample is that subjects were, on average, 6 to 8 years old during the peak time of the conflict and thus unlikely to have self-selected into conflict exposure by actively participating in fighting (Bauer et al. 2016; see section 5.6 for a discussion of self-selection).

We put a lot of care into the instructions, translations and pre-tests to make sure our subjects would have a maximum level of understanding. Role assignment and assignment to the treatments in-group and out-group were completely random. Within these two treatments, however, whether a subject received information on the religious or ethnic group identity was partly based on scarcity, such that we achieved a similar share of Muslims and Christians in the religious treatments. Half of the subjects in each religious group became trustors and the other half became trustees. More details on the data collection is provided in Appendix B and in Werner and Lambsdorff (2019).

[10] Subjects filled in the questionnaire that was used for treatment assignment in the first week and made their decision as trustors, if applicable, in the second week. In the third week, trustees made their decisions and in the fourth week, subjects filled in the post-experimental questionnaire, received feedback on their counterpart's decisions and their payoffs.

Table 2: Sample characteristics of trustors

Religion	N	%	Age	N	%
Christian	136	69.7%	16-18	31	15.9%
Muslim	59	30.3%	19-22	111	56.9%
			23-26	20	10.3%
			27-35	3	1.5%
Gender	**N**	**%**	**Conflict Exposure**	**N**	**%**
Female	115	59%	No exposure	56	28.7%
Male	47	24.1%	Indirect only	58	29.7%
Missing	33	16.9%	Direct exposure	50	25.6%
			Missing	31	15.9%

As this paper focuses on trust, we present only the trustors' behavior here. The behavior of the trustees is analyzed in Werner and Lambsdorff (2019). We obtained a total of N=195 observations for trustors. Table 2 provides an overview of the sample.[11] The subjects' mean age in 2013 was 20.5 years. One might conjecture that some of the subjects may have been too young during the conflict to remember anything and for conflict exposure to have any impact on their behavior. However, the vast majority of the subjects were 5 to 15 years old during the peak time of the conflict and even the youngest subject was between 2 and 4 years old during this peak time from 1999-2002 (and older during later incidents of violence). As pro-social motivations and social preferences develop during childhood (Sutter and Kocher 2007; Eckel et al. 2011), children or adolescents may be particularly sensitive to conflict exposure (Bauer et al. 2014)[12]. Conflict exposure also does not significantly differ between older and younger subjects: The correlations between age and direct / indirect conflict exposure are very low and we do not find any impact of age on the probability of having been directly or indirectly exposed to the conflict (Appendix Table A.8).

4.5 Results

On average, trustors transferred 42% of their endowment, similar to transfers measured by Gilligan et al. (2014) in post-conflict Nepal, and slightly less than in some other studies where mean trustor

[11] Our plan was to conduct our experiments at scheduled class times. But, as the semester started with delays at both universities, we were not able to ensure that our subjects would attend all sessions, because our assistants were not able to find them in class in case they did not show up. This caused attrition, which explains missing data in Table 2. Attrition was independent of treatment assignment: dropouts were highest in the second week (see Appendix for details), when subjects were still unaware of their treatment status; in the remaining weeks, attrition was similar across all treatments. Each trustor whose trustee had dropped out of the study received a copy of another trustee's decision (which was randomly drawn from the decisions of other trustees with the same characteristics as the missing subject) for feedback.

[12] Gangadharan et al. (2022) show that exposure to war can also have long-lasting effects on young children, even if the exposure occurred below the age of 6.

transfers amounted to 50% of the endowment (e.g. Camerer 2003). Appendix Figure A.1 illustrates the pitfalls of looking at trust without considering conflict exposure. There are no average differences between transfers to the in-group, out-group or neutral group.[13]

While trustors on aggregate do not discriminate between in-group and out-group, trustors with certain conflict experiences do. Figure 1 compares trustor transfers of subjects with neither direct nor indirect exposure (left side) to those with indirect exposure but no direct exposure (middle) and those with direct exposure (right side). As personal experiences are likely to loom larger than friends' experiences and thus overshadow them, the bars on the right side include subjects who had only direct exposure (34% of the directly exposed subjects) and subjects with both direct and indirect exposure (66%).[14]

Neither subjects with no conflict exposure nor subjects with mere indirect exposure trust in-group members more than out-group members. Both on the left side and in the middle section of the graph, differences between treatments turn out to be insignificant (Wilcoxon-Mann-Whitney test for the difference between in-group and out-group; left side: $z=-0.40$, $p=0.69$; middle: $z=0.55$, $p=0.58$).

Yet, the bars in the middle of Figure 1 are clearly lower than the ones on the left. Averaging all treatments, the mean transfer of the 58 indirectly exposed subjects of RPK 9.1 (middle) is significantly lower than the transfer of RPK 15.8 of the 56 subjects with no exposure (left side; $z=2.58$, $p=0.01$). This confirms H1, which states that indirect conflict exposure has a negative impact on average trust.

Result 1: Indirect exposure is related to low levels of trust toward all groups.

[13] There were neither differences in average trust levels of Christian and Muslim participants, nor did just one of the religious groups discriminate. The absence of discrimination among trustors is mirrored by a similar absence of discrimination among trustees (see Werner and Lambsdorff 2019).

[14] The regressions in Section 4.6 below and Figure A.2 in the Appendix confirm that the behavior of subjects with only direct exposure does not significantly differ from the behavior of subjects with direct and indirect exposure.

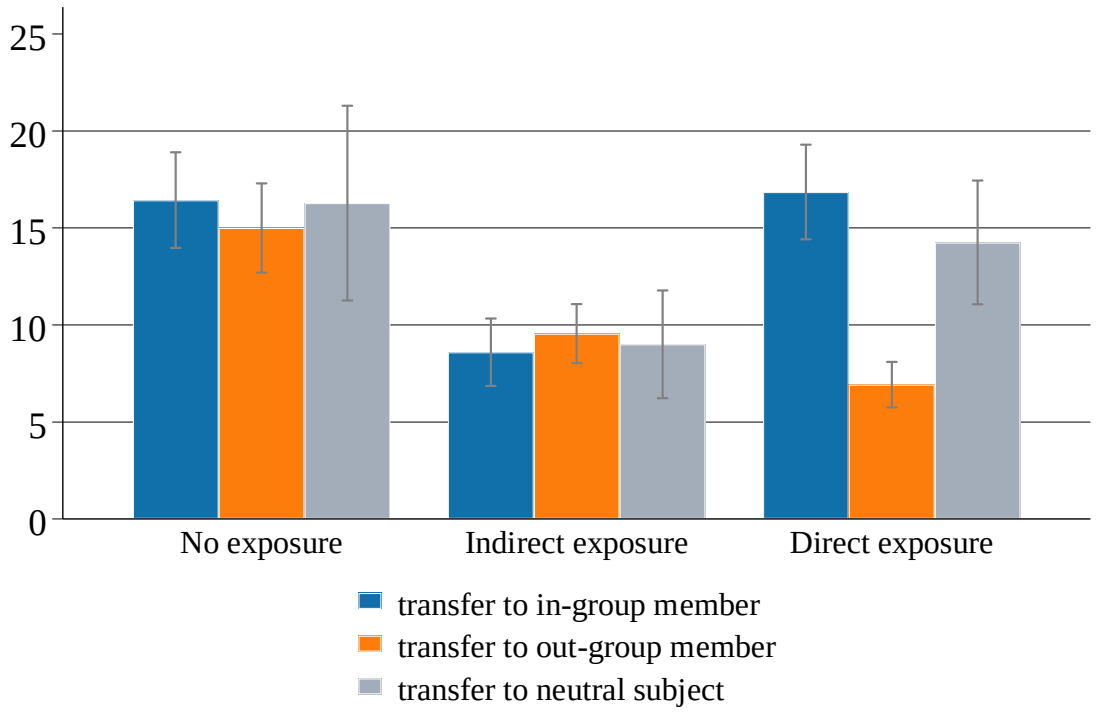

Figure 1. Trustor transfers by trustee type and conflict exposure type.

Notes. The bars depict mean treatment transfer by trustees in thousands of Indonesian Rupiah. Error bars represent one standard error.

Parochial trust emerges among directly exposed subjects. On the right-hand side of Figure 1, for trustors with direct conflict exposure, there is a huge difference between in-group transfers of RPK 16.9 (blue bar) and out-group transfers of 6.9 (orange bar; z=-2.5, p=0.01). Directly exposed subjects thus have significantly higher trust in in-group than in out-group members. The difference between the blue and gray bar on the right side suggests that directly exposed subjects also have higher trust in in-group members than in neutral subjects. This effect is only weak, as the difference between transfers of RPK 16.9 to in-group members and 14.3 to neutral subjects misses conventional levels of significance (z=-1.0, p=0.3). Second, we can compare the subsample of directly exposed subjects to the subsamples with no exposure (left) and with indirect exposure (middle). The blue bar on the right side with transfers of 16.9 is only slightly higher than the blue bar on the left side (16.4), but much higher than the blue bar in the middle which corresponds to mean transfers of 8.6. Hence, directly exposed subjects only have slightly higher in-group trust than non-exposed subjects have, but significantly higher in-group trust than subjects with mere indirect exposure (z=2.6; p=0.01).

Result 2a: Direct exposure is related to higher in-group trust than indirect exposure.

The same two comparisons can be made for out-group transfers. The orange and gray bars on the right side suggest that directly exposed subjects' transfers to out-group members of RPK 6.9 are lower than transfers of RPK 14.3 to neutral subjects. This difference is not significant (z=-0.89, p=0.37). When comparing only transfers to the religious out-group (i.e. transfers to a more clear-cut out-group, on which exposure to the mainly sectarian conflict may have a stronger impact than on transfers to the ethnic out-group) to transfers to neutral subjects, the difference increases, but still misses conventional levels of significance (z=-1.41; p=0.15; see Figure A.3 in the Appendix). Second, there is no significant difference between out-group transfers of RPK 9.6 by indirectly exposed subjects (orange bar in the middle) and RPK 6.9 by directly exposed subjects (orange bar on the right side, z=0.73, p=0.46). The difference between out-group transfers of RPK 15 by subjects with no exposure (orange bar on the left) and RPK 6.9 by directly exposed subjects (orange bar on the right) proves weakly significant (z=1.69, p=0.09), implying that directly exposed subjects have lower out-group trust than subjects with no exposure. Again, the result is much stronger and turns significant when only transfers to the religious out-group are compared between the subsamples with no exposure and with direct exposure (z= 2.17; p=0.03; see Figure A.3). This provides evidence in favor of H2b.[15]

Result 2b: Direct exposure is negatively related to out-group trust.

Admittedly, the sample sizes in some individual bars are too small to completely rule out that there might be significant effects if we had more statistical power. In section 4.7, we apply randomization inference to establish the robustness of our findings using close-to-exact statistics which would be obtained under all possible treatment assignments. Appendix Figures A.4, A.5 and Table A.3 use broader measures of direct and indirect exposure to show that the results hold when more different kinds of experiences are considered and when the number of observations for the subsamples with indirect and direct exposure is higher. In summary, our experimental results show that: (i) indirectly exposed individuals are less trusting of everyone *regardless* of in-group vs. out-group status; (ii) directly exposed individuals display sizable in-group bias: they trust in-groups significantly more and out-groups significantly less. Interestingly, these results are consistent with Shayo and Zussman (2011), who show that, in Israel, both Jewish and Arab judges

[15] The results are not reflected in the trustee returns, which suggests that the trustors' transfers indeed reflect trust and not mere altruism.

favour their own in-groups, but only in areas which experienced civilian fatalities from terrorism in the year preceding the judgment.

4.6 Regression Analysis

Table 3 presents OLS regressions confirming the non-parametric results.[16] Heteroskedasticity-robust standard errors are reported in parentheses. In the first three regressions, the treatment *neutral subject* serves as the reference category and is captured in the constant. The treatment dummies *in-group* and *out-group* thus indicate whether allocations in these two treatments significantly differ from transfers to neutral subjects.

[16] Tobit regressions were also run to account for the fact that trustor transfers are have a lower bound of 0 and an upper bound of 30. As the results are qualitatively equivalent, more readily interpretable OLS estimates are reported here.

Table 3: Regression results

Dependent variable: Trustor allocation									
	Religious and ethnic in-/ out-group						Only religious in-/out-group		
	(1) All	(2) All	(3) All	(4) No direct exp.	(5) Direct exp.	(6) Wald: Δ coefficient (5) - (4) (χ^2)	(7) No direct exp.	(8) Direct exp.	(9) Wald: Δ coefficient (8) - (7) (χ^2)
in-group	1.5	0.3	0.05	15.9	17.0	1.1	16.1	19.2	3.1
s.e.	(2.2)	(2.5)	(2.7)	(2.0)***	(3.1)***	(0.11)	(2.5)***	(4.3)***	(0.4)
RI *p*-val.	0.33	0.88	0.98	0.00***	0.000***	0.89	0.00***	0.00***	0.43
out-group	-1.1	-2.2	-2.5	15.6	7.2	-8.4	17.3	5.7	-11.6
s.e.	(2.1)	(2.3)	(2.6)	(1.9)***	(2.5)***	(7.83)***	(2.5)***	(2.8)*	(10.3)***
RI *p*-val.	0.47	0.19	0.12	0.00***	0.04**	0.01***	0.00***	0.19	0.00***
neutral				15.9	14.5	-1.4	16.4	15.0	-1.4
s.e.				(3.0)***	(4.0)***	(0.09)	(3.2)***	(4.4)***	(0.07)
RI *p*-val.				0.00***	0.00***	0.57	0.00***	0.00***	0.59
indirect exp.		-5.0	-4.7	-6.6	-0.3	6.3	-7.5	-1.0	6.5
s.e.		(1.7)***	(1.7)***	(1.9)***	(3.2)	(3.02)*	(2.5)***	(4.0)	(2.08)
RI *p*-val.		0.01***	0.01**	0.00***	0.92	0.08*	0.00***	0.83	0.07*
direct exp.		1.4	-0.3						
s.e.		(1.8)	(1.9)						
RI *p*-val.		0.52	0.92						
constant	12.3	15.8	19.3						
	(1.7)***	(2.5)***	(9.3)**						
controls	No	No	Yes	No	No		No	No	
N	195	164	159	114	50		70	40	
R^2	0.012	0.067	0.138	0.618	0.649		0.646	0.636	

Heteroskedasticity-robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Specification (1) confirms that, on aggregate, there are no differences between treatments:

Neither the coefficient for *in-group* nor for *out-group* significantly differs from transfers to neutral subjects. Wald tests for specifications (1)-(3) with p-values around 0.15 reveal that the coefficients for *in-group* and *out-group* are not significantly different from each other, either.

Specification (2) includes the conflict exposure variables to examine whether indirect and direct conflict exposure have an impact on average trust.[17] In line with the findings from the middle section of Figure 1, the negative and significant coefficient of -5.0 on indirect exposure confirms result 1 that indirect conflict exposure reduces average trust, while direct exposure has no impact on average trust.[18] Specification (3) shows that the results are robust to the inclusion of demographic controls (age, gender, religion, family size and income).[19]

Two separate regressions for subjects without direct exposure (specification (4)) and with direct exposure (specification (5)) confirm the opposing effects of direct exposure on in-group and out-group trust from Figure 1. From specification (4) onward, we suppress the constant. In specification (4), the coefficients for the treatments *in-group*, *out-group* and *neutral subject* have very similar values. A Wald test on the equality of the coefficients for *in-group* and *out-group* (corresponding to the difference between the blue and the orange bar on the left side of Figure 1) is insignificant (F=0.01, p=0.91). Similar results hold for the differences between transfers to a neutral subject and transfers to the in-group and out-group. Indirect exposure has a significantly negative impact for subjects who had no direct exposure.

In specification (5), the Wald test on the equality of the coefficients for *in-group* and *out-group* (comparison of the orange and blue bar on the right side of Figure 1) confirms that directly exposed subjects transfer significantly more to in-group than to out-group members (F=12.3, p=0.001). A Wald test on the equality of coefficients for *out-group* and *neutral subject* in specification (5) also shows that directly exposed subjects have significantly lower trust in out-group members than in neutral subjects (F=4.42; p=0.04). Indirect exposure does not have a significant impact on trust of subjects with direct exposure .

Column (6) presents the results of Wald tests between subsample coefficients (subjects with direct exposure – no direct exposure, χ^2-values in parentheses), which correspond to a

[17] In the middle section of Figure 1, subjects with direct exposure were excluded to isolate the effect of indirect exposure. This means subjects with no exposure were compared to subjects with only indirect (but no direct) exposure and to subjects with direct (and possibly indirect) exposure. In contrast to Figure 1, specification (2) uses the entire sample because the two exposure dummies allow to investigate the impact of one type of exposure while holding the other type constant. Specifications (4) and (5) control for indirect exposure to isolate the effect of direct exposure.

[18] Adding interaction terms between indirect exposure and the treatment dummies shows that indirect exposure does not affect in-group and out-group trust differently. Those results are available from the authors upon request.

[19] The coefficients of the control variables (Table A.5 in the appendix) reveal that male subjects are more trusting than female ones and a higher income is weakly related to lower levels of trust. The other coefficients turn out to be insignificant. In particular, age might have been an important confounder, but empirically turned out to be unrelated to levels of trust. Further regressions in the Appendix (Table A.8) show that age is also not related to differences in in-group and out-group trust.

comparison of bars with the same color between the left and the right side in Figure 1. While there is no significant difference between in-group transfers of not directly exposed and directly exposed subjects, the difference in out-group transfers between the two groups is significant at the 1%-level. These Wald tests confirm that direct exposure had no significant impact on in-group trust (although, qualitatively, the effect goes in the expected direction), but a significantly negative impact on out-group trust. There is also a weakly significant difference between the coefficients on indirect exposure, reflecting the result that indirect exposure only matters when there was no direct exposure.

Specifications (7) and (8) confirm the findings from Section 4.6 that the effect of direct exposure is stronger when only transfers to the religious out-group are compared to transfers to neutral subjects and the religious in-group (instead of pooling the transfers to both the religious and ethnic out-group). The difference between the coefficients for in-group and out-group in the subsample with direct exposure (specification (8)) is larger than in specification (5) and the significance is higher (Wald test between coefficients: F=14.16; p=0.0006). The same holds for the difference between the coefficients for out-group and neutral subject (F=7.1; p=0.01). Column (9) also shows that the difference between out-group transfers by subjects with and without direct exposure is larger and significant at the 0.1%-level when only transfers to the religious out-group are compared.

4.7 Randomization Inference Estimates

A potential concern in our regression results is that our sample size may not be large enough to support robust inference, owing to delayed semester starts at both universities. In Table 3, we thus also report randomization inference (RI) p-values, relaxing any distributional assumptions that may hold only asymptotically (Fisher 1935, Young 2019), which allows us to estimate the actual distribution of test statistics in our experimental data.[20] In our randomization, we have up to 195 observations, and we perform i.i.d. random assignment to placebo treatments, such that approximately half the observations are placebo-treated. There are thus $C_{195}^{97} \approx 2.86 * 10^{57}$ possible treatment allocations. Obtaining all corresponding test statistics is prohibitively expensive

[20] Young's (2019) recommendations have generated some discussion as to whether randomization inference approaches outperform previously available small-sample corrections, in particular Long and Ervin's (2000) HC3 correction (for a discussion, see Simonsohn 2021). Our results (available upon request) do not change if we implement the Long and Ervin (2000) approach.

from a computational standpoint; we therefore perform 2,000 randomizations to obtain close-to-exact p-values. Our randomizations are seeded and thus fully replicable.

The randomization inference p-values confirm all our previous results. In Columns (1)-(5) and (7)-(8) of Table 3, we verify, using close-to-exact statistics, that the significance (or insignificance) of each coefficient remains unchanged. The coefficient of *out-group* in specification (8) is a minor exception: this coefficient was marginally significant in Table 3 ($p<0.10$) and is now insignificant ($p=0.19$). In Columns (6) and (9), we report randomization-based Wald tests on the equality of coefficients across specifications; our results again remain unchanged. Thus, we confirm that our results hold over and above placebos, thus alleviating the potential concern that the independent variables in Table 3 were picking up the effects of some other variables.

4.8. Further Robustness Checks

All regressions and Wald Tests displayed in Table 3 are robust to the inclusion of the battery of demographic controls employed in specification (3). As our subjects also played a dictator game, which is part of another study, we are able to use this as a measure of altruism, reflecting otherwise unobservable characteristics. When including it in the regressions, none of the results changed and the coefficients on this measure turned out statistically insignificant and small in size.

Conflict exposure cannot be randomized by experimental design. Hence, a concern may be that subjects self-selected into exposure, based on characteristics that correlate with trust. Could older subjects be more likely to have witnessed the conflict and at the same time be less trusting? Do lower-income subjects have a higher probability to be caught in the conflict, because they may live in more turbulent areas, while income also affects trust? While we do not think that these concerns are strong enough to cast doubt on our findings, these questions can ultimately only be resolved empirically. We ran logistic regressions with each of the two measures of conflict exposure as the dependent variable on the battery of demographic variables from specification (3) (Appendix Table A.4). None of these observable characteristics significantly increased the likelihood of having direct conflict exposure (only being male turned out to have a weak impact) and only family size had an impact on indirect exposure. Furthermore, Kolmogorov-Smirnov tests find hardly any significant differences between subsamples (Appendix Table A.7). There is only a weakly significant difference in the distribution of males between indirectly and directly exposed subjects. Altogether, we find no systematic differences between subsamples. This is in line with

the finding that controlling for the battery of observable demographic characteristics in specification (3) does not substantially change the effects of direct or indirect exposure on trust.

Subsamples might also differ with respect to subjects' individual characteristics and preferences. However, subjects were children during the conflict and are thus very unlikely to have systematically self-selected into conflict based on individual characteristics (Bauer et al. 2016, p.14), but can only have self-selected based on family characteristics which are largely covered by the battery of demographic variables. The inclusion of the dictator game transfer as a proxy for altruistic preferences does not change the findings. All in all, the robustness checks suggest that self-selection into conflict is unlikely to explain the results.

5 Comparative evidence across four datasets

5.1. Background

So far, we have found support for our hypotheses in the lab-in-the-field experiment conducted in Maluku, Indonesia. However, a crucial concern arises when conducting surveys or experiments within a single context: the extent to which the observed results hold true beyond that particular context remains uncertain. To address this challenge, we employ three distinct survey datasets: the Afrobarometer, the Social Well-Being Survey in Asia, and the World Values Survey, leveraging various forms of conflict exposure (spatial and temporal). Our analysis encompasses diverse samples from different regions (Asia, Africa, and globally) and maintains the fundamental essence of the original definition of conflict exposure (first-hand vs. second-hand knowledge). Table 4 presents an overview of each dataset and their relevant characteristics.

Table 4. Definitions of conflict exposure and in-group / out-group status in the Afrobarometer, Social Well-Being Survey in Asia, and World Values Survey.

	Conflict			Trust			
	Where?	**Directly exposed respondents…**	**Indirectly exposed respondents…**	**In Group**	**Weight**	**Out Group**	**Weight**
Afrobarometer	Sub-national region (ADM1)	Live in region where conflict took place	Live in conflict-free region in the same country	Own ethnic group	1	Other ethnic groups	1
Social Well-Being Survey in Asia (SWBS)	Residential area	Experienced damages during conflict	Heard about others' previous experiences on conflict in residential area; did not experience damages	Neighbours Family Friends	0.35 0.30 0.35	Strangers	1
World Values Survey (WVS)	Country	Were contemporaries of conflict	Did not experience a conflict during their lifetime, but older cohorts in the country did	Family Neighbours	0.59 0.41	Other nationality Other religion Know personally Meet for first time	0.26 0.26 0.22 0.25

Before delving into the details of each individual dataset, we would like to make three general remarks. First, in the case of the Afrobarometer and WVS, we report intention-to-treat (ITT) estimates rather than average treatment effects. This is due to the fact that the survey datasets only offer cohort-level variation in conflict exposure (spatial and temporal cohorts, respectively), unlike the experiment and SWBS where we have access to individual-level information regarding conflict exposure. Consequently, the ITTs are expected to be smaller, as we will show in the specification curve analysis (Figure 4). Second, because of the somewhat coarse nature of the treatment variable (conflict exposure), we cannot consistently define an unexposed category across all the survey datasets and experiment. Therefore, we exclude the unexposed category and solely compare the direct and indirect categories across the three survey datasets and the experiment. Third, owing to the varying definitions of the indirect exposure category (spatial vs. temporal), we can zero in on how the transmission of conflict-related experiences operates. We find evidence consistent with the notion that stories are shared both vertically and horizontally. Indirectly affected individuals are less trusting both when they learn about conflict from previous generations (vertical; World Values Survey and Social Well-Being Survey in Asia) and when they learn about conflict from affected contemporaries (horizontal; experiment and Afrobarometer).

5.2. Datasets

Social Well-Being Survey in Asia. The SWBS (International Consortium for Social Well-Being Studies 2020) was fielded between 2015 and 2017. Our sample consists of individuals from those countries where both treatment cells (direct and indirect exposure) are non-empty. This yields 1,111 respondents from seven countries (Indonesia, Japan, South Korea, the Philippines, Thailand, Taiwan, and Vietnam; see Appendix Table A.11 for a full breakdown).

*Conflict exposure***.** SWBS respondents indicated whether they have, at any point in their lives, personally experienced damages from war. This variable thus matches our experimental measure of direct conflict exposure. Respondents were also asked whether experiences on war are handed down in their residential area. This variable captures second-hand knowledge of conflict, which is closely related to our experimental measure of indirect exposure. We retain only countries where both treatment cells (direct and indirect exposure) are non-empty.

*In-group and out-group trust***.** There are no explicit measures of in-groups and out-groups in the SWBS. Therefore, we follow a transparent and relatively assumption-free approach in the sense that we do not impose any priors on who is regarded as an in-group and who is not. We proceed in three steps (see Appendix Tables A.9 and A.10 for exact details):

(i) We perform principal component analysis with the available trust variables (trust in family members, friends, neighbors, and strangers) and retain the first two principal components.

(ii) We assign each trust variable to the axis (component) on which it loads the highest.

(iii) For each component, we normalize the sum of the weights from the assigned variables to 1, thus yielding for each variable, the weights shown in Table 4.

We can then examine the validity of our approach by looking at whether the output is sensible: trust in out-groups is trust in strangers, while trust in in-groups is a weighted average of trust in family, friends, and neighbors. The validity of family and friends as in-groups is straightforward; the view that neighbors are in-groups is consistent with the seminal work of Thomas Schelling (1969), who inspired a large literature showing that individuals self-select into neighborhoods in order to live in proximity to people they consider their in-groups. At any rate, our results do not depend on idiosyncratic definitions of in-group and out-group, as we show employing World Values Survey and Afrobarometer data.

Afrobarometer. We combine survey responses from the Afrobarometer with geo-referenced conflict data from the Uppsala Conflict Data Project Georeferenced Event Dataset. Our sample consists of individuals from those countries where both treatment cells (direct and indirect exposure) are non-empty. This yields 15,723 from 11 countries (see Appendix Table A.11 for a full breakdown).

Conflict exposure. The relevant variation in conflict exposure is at the sub-national level: *directly* exposed individuals are those who reside in a sub-national region which has experienced conflict; *indirectly* exposed individuals are those who reside in a country which has experienced conflict, but in a sub-national region which has been conflict-free.

***In-group and out-group trust*.** In Africa, ethnicity is highly salient, particularly in conflict settings, partly because of colonial policies of divide-and-rule including straight-line border designs (Michalopoulos and Papaioannou 2016). Notions of in-group and out-group are well-defined in the African context; this is reflected in the Afrobarometer survey, which asks respondents how much they trust members of their own ethnic group, and how much they trust individuals from other ethnic groups. We take these two questions as our measures of in-group and out-group trust, respectively.

World Values Survey. We combine survey responses from the World Values Survey to country-level conflict data from the Uppsala Conflict Data Project / Peace Research Institute of Oslo (UCDP/PRIO, Gleditsch et al 2002).[21] Our sample consists of individuals from those countries where both treatment cells (direct and indirect exposure) are non-empty. This yields 28,073 from 12 countries (see Appendix Table A.11 for a full breakdown).

Conflict exposure. The relevant variation in conflict exposure is at the country-cohort level: specifically, we define a survey respondent as *directly* exposed if they are a contemporary eyewitness of conflict in their country or *indirectly* exposed if they were born after the last conflict in their country and thus could have only had second-hand notions of conflict.[22]

[21] UCDP/PRIO records the number of distinct conflicts across four types of conflict: internal, interstate, internationalized, and extra-systemic. The global distribution of these data, which are available from 1946 onward, is shown in Appendix Figure A.6. Because we are interested only in conflicts that occur on home soil, we focus on internal, internationalized and interstate conflicts. We thus effectively disregard extra-systemic conflicts from UCDP/PRIO, as these happen outside a country's territory.

[22] In a similar way, Gangadharan et al. (2022) argue that those born after the Khmer Rouge regime in Cambodia can only have indirect exposure and Booth et al. (2022) use birth cohorts to identify potential direct and indirect victims of the Chinese Cultural Revolution in their household survey data.

*In-group and out-group trust***.** One drawback of the WVS is that, like in the SWBS, there are no consistent definitions of in-groups and out-groups across countries. We therefore rely on the same procedure as we do with the SWBS (principal component analysis and weighting, see Appendix Table A.9 for details). Our PCA-derived measure of in-group trust correlates most closely with trust in family members and trust in neighbors. Our PCA-derived measure of out-group trust comprises trust in people of another religion and in people of another nationality. The out-group and in-group measures in the WVS, as defined by PCA, are conceptually very similar to the PCA-derived measures for the Social Well-Being Survey (Appendix Table A.10), which is reassuring as far as construct validity is concerned.

5.3. Non-Parametric Results

Mean trust (H1). Figure 2 compares mean trust levels, averaged across trust in in-groups and trust in-out-groups, for directly exposed individuals (yellow bars) and indirectly exposed individuals (green bars) in the experiment and the three survey datasets described in Section *5.2.* Each panel also reports a p-value for the difference between bars. For comparability, trust is re-scaled between 0 and 1 for all four datasets. With the exception of the SWBS, where we do not detect a significant result, directly exposed individuals are more trusting than the indirectly exposed. The differences are highly significant for the experiment, World Values Survey, and Afrobarometer datasets, thus offering support for hypothesis H1.

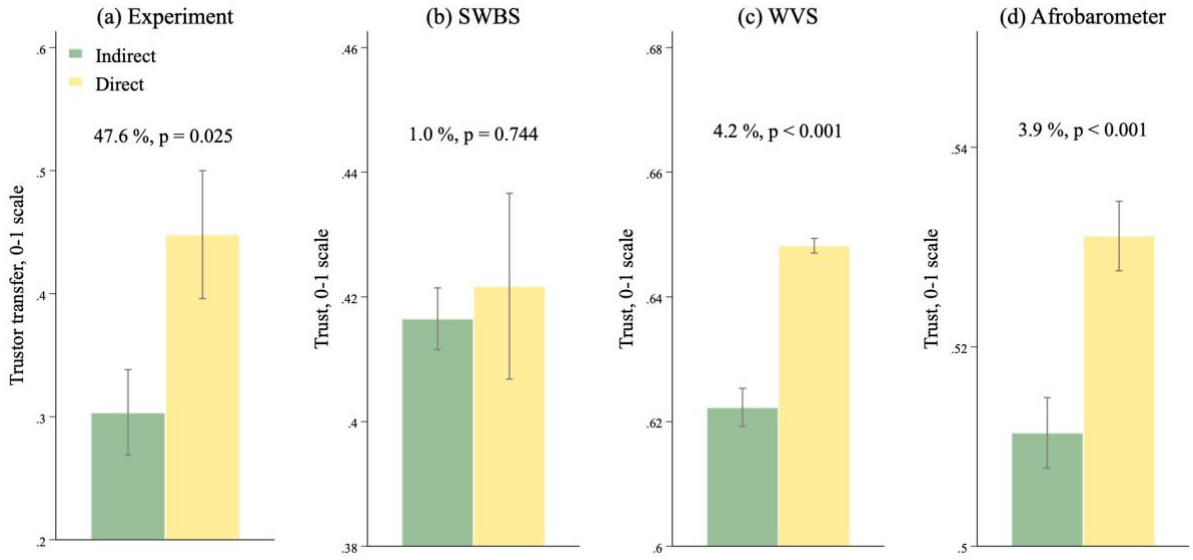

Figure 2. Mean trust (0-1 scale) by conflict exposure status (H1) across the experiment and survey datasets.

Note: error bars represent one standard error.

Parochial trust (H2). Figure 3, which compares *parochial* trust levels by conflict exposure status, provides strong support for hypothesis H2, which states that directly exposed individuals are more parochial than the indirectly exposed. Parochial trust is the difference between trust in in-groups and trust in out-groups (trustor transfer in the experiment; survey responses in the secondary datasets). The differences in the four datasets are all strongly significant, with effect sizes ranging from 8% in the Afrobarometer to 16% in the SWBS.[23]

[23] Error bars cannot be drawn in Panel (a), since parochialism measures can only be defined at the group level (direct vs. indirectly exposed), because our experimental participants only make one trust decision (and thus there is no within-subject variation). In contrast, survey participants answer questions about their trust attitudes both towards in-group and out-group members.

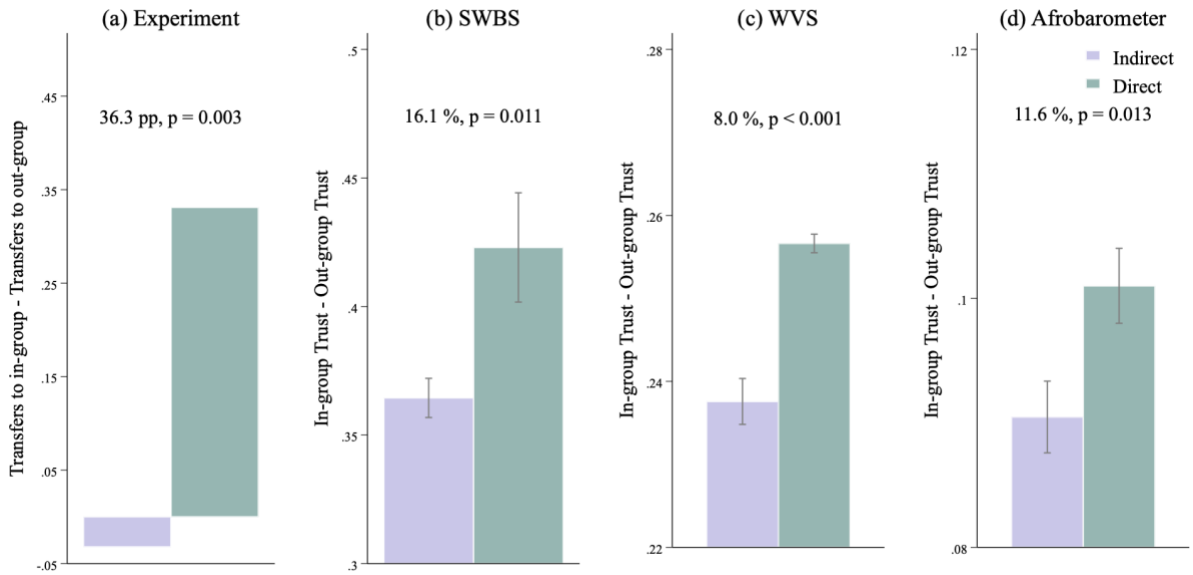

Figure 3. Parochial trust (trust in in-groups minus trust in out-groups) by conflict exposure status (H2) across the experiment and survey datasets.

Note: error bars represent one standard error.

5.4 Parametric Evidence: Specification Curves

For each of the experimental data, World Values Survey, Afrobarometer, and Social Well-Being Survey in Asia, we estimate variants of Equations (1) and (2):

$$Trust_{igct} = \beta_0 + \gamma_c + \beta_1\, Ingroup_{igct} + \beta_2\, Direct_{ict} + \mathbf{X}_{ict}\, \rho + \varepsilon_{igct} \quad (1)$$

$$Trust_{igct} = \delta_0 + \gamma_c + \delta_1\, Ingroup_{igct} + \delta_2\, Direct_{ict} + \delta_3 Direct_{ict} * Ingroup_{ict} + \mathbf{X}_{ict}\, \gamma + \mu_{igct} \quad (2)$$

Equation (1) estimates how much person *i*, surveyed in country *c* at time *t*, trusts group *g* = {InGroup; OutGroup}. γ_c is a set of country fixed effects, thus allowing us to compare individuals within countries. *Ingroup* is a dummy variable equal to 1 if the dependent variable measures trust in in-groups and 0 if *Trust* relates to trust in out-groups, and thus captures differences in trust that are driven by preference for in-groups over out-groups, i.e. parochialism. *Direct* is a dummy variable equal to 1 if the respondent experienced direct conflict exposure and 0 if the respondent experienced indirect conflict exposure. The coefficient of *Direct* in Equation (1) is the analog of the difference between bars in Figure 2, which informs us about differences in trust between directly and indirectly affected individuals. The coefficient of interest in Equation (2) is the

coefficient of *Direct* * *Ingroup*, which tells us whether there is additional parochialism among those directly affected, above and beyond differences in trust that are explained by *Direct* and *Ingroup*. δ_3 in Equation (2) is thus the analog of the difference between bars from Figure 3 and allows us to check whether directly affected individuals are more parochial than the indirectly affected. To facilitate comparability across the four datasets, we only include directly and indirectly exposed (but not unexposed) respondents from the experiment and the SWBS dataset, since both the WVS and Afrobarometer lack an 'unexposed' category. **X** is a vector of controls including age, education, income, religiosity, and gender. The dependent variable, *Trust*, is standardized.

For each dataset, we estimate Equations (1) and (2) with every possible permutation of the control variables, for a total of 96 estimates per equation, which we report in the specification curves shown in Figure 4. For the WVS estimates, we do not control for age, since age is highly collinear with the treatment (exposure to conflict is defined at the country-cohort level). For the experiment, we do not control for education, since it does not vary meaningfully between the university students who participated in our experiment in Indonesia.

Figure 4 documents strong evidence in favor of our hypotheses H1 and H2. In Panel (a), the coefficient of *Direct* in Equation (1) has a mean of 0.13, indicating that directly affected individuals are on average 0.13 SD more trusting than indirectly affected individuals. The 96 estimates of *Direct* are all positive, with a mean t-statistic equal to 3.93, thus offering strong support for H1. In Panel (b), *Direct* * *Ingroup* from Equation (2) has a mean t-statistic of 2.33 and a mean coefficient equal to 0.30 SD, thus providing unambiguous confirmation for the idea that directly exposed individuals are more parochial (H2).

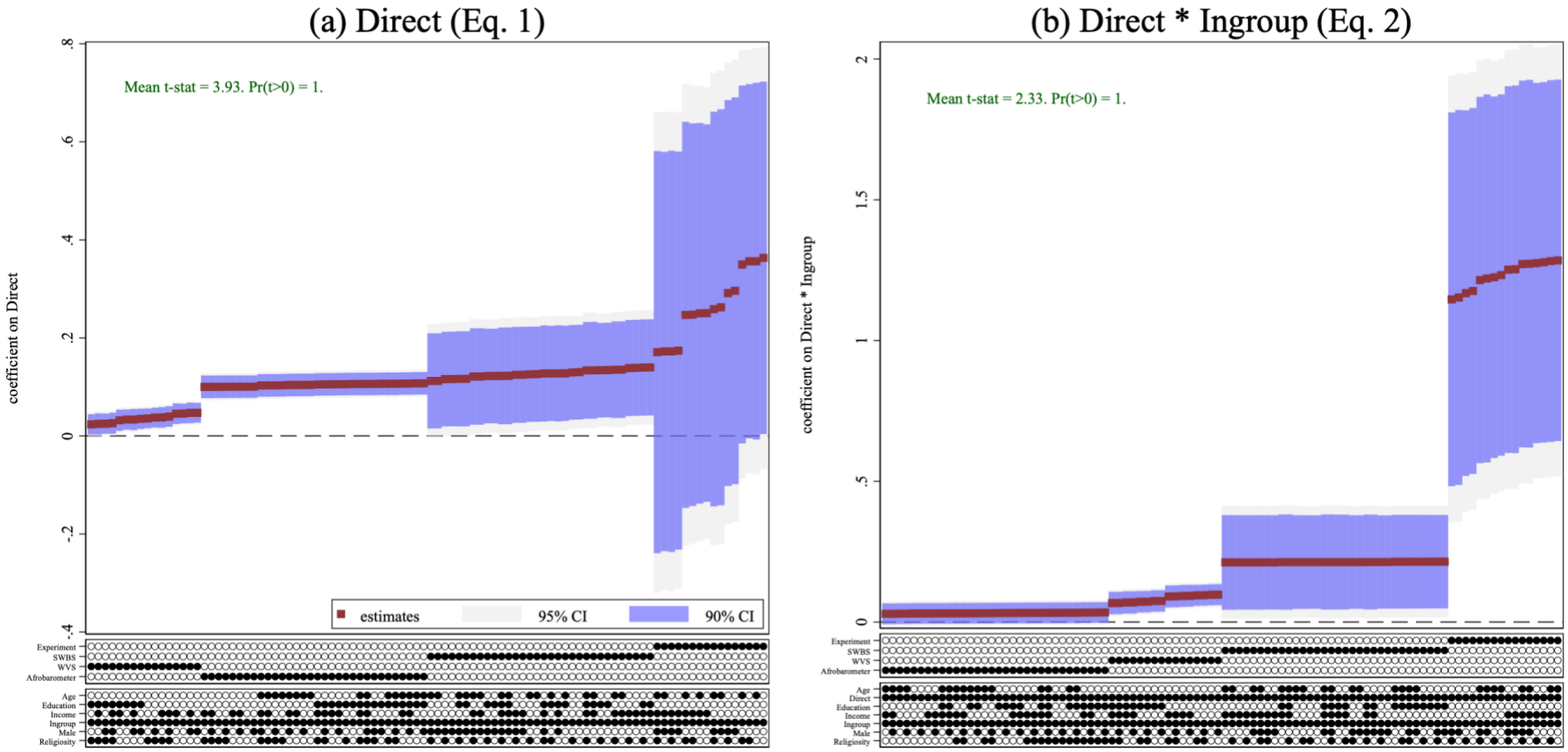

Figure 4. Specification curves for *Direct* (Eq. 1) and *Direct * Ingroup* (Eq. 2).

Notes. Dependent variable: trust (z-score). Estimates are sorted by size. The curve is obtained by enumerating all possible permutations of controls. The black dots in the top and bottom panels indicate, respectively, which dataset and which control variables a given estimate pertains to.

5.5. Conflict Exposure at the Intensive Margin Increases Parochialism

So far, we have documented that individuals directly exposed to conflict display more parochial trust than indirectly exposed individuals. We conjecture that more tangible hardship creates both *(i)* opportunities to improve in-group trust via within-group solidarity, and *(ii)* 'opportunities' for deteriorating out-group trust as individuals may witness more numerous instances of conflict with out-groups. A natural way to test this conjecture is to focus on directly exposed individuals and check whether witnessing more conflict leads to more parochial trust.

We report results from this analysis in Figure 5, where we employ different proxies of conflict severity, allowing conflict severity to affect trust heterogeneously towards in-group and out-group members. For the sample of directly exposed respondents, the regression equation is:

$$Trust_{igct} = \sigma_0 + \gamma_c + \rho_1 \, (Severity \mid Ingroup = 1)_{igct} + \rho_2 \, (Severity \mid Ingroup = 0)_{igct} + \upsilon_{igct} \qquad (3)$$

Equation (3) estimates the within-country responsiveness of trust in out-groups and in-groups to conflict severity. The variable *Severity* denotes standardized values of various conflict proxies as detailed below[24].

In Panel (a) of Figure 5, we study the relationship between conflict severity and trust in the WVS. Our proxies for the intensity of conflict exposure are the number of conflicts, the number of conflict years, and the number of major conflicts an individual has been the contemporary of, at the time she was interviewed in the WVS. We use inverse hyperbolic sine transformations of these three count variables, which allow for elasticity interpretations just as natural logarithms do, while avoiding the pitfalls of taking the log of 0 or of adding a constant before taking logs. The first two proxies follow the conventions in the literature (see Bazzi and Blattman 2014), but may suffer from measurement error in the sense that all conflicts are treated as equally severe. We therefore complement these two proxies with the number of major conflicts. Major conflicts are those defined in UCDP/PRIO as those for which the number of battle deaths exceeds 1,000 in a year. We recognize that this proxy is coarse, and thus may also suffer from measurement error, although it does reflect differences in the level of violence and is the most widely agreed upon proxy for

[24] We perform this analysis only for the WVS, the Afrobarometer and the Experiment, not for the SWBS, because we cannot isolate variation in conflict exposure at the intensive margin in the latter dataset. SWBS respondents indicate whether they experience conflict or not, but now how much conflict they experience.

conflict intensity across multiple countries and years. Across the board, the results are stark: more conflicts, conflict-years, and major conflicts all reduce trust in outgroups while increasing trust in in-groups, thus introducing a wedge of parochialism between the two.

In Panel (b), we repeat the analysis in the Afrobarometer data, with the number of events and battle deaths in a respondent's sub-national region, both calculated from the UCDP Georeferenced Event Dataset, as our severity proxies. Increases in both proxies are associated with significant increases in trust in in-groups and significant declines in trust in out-groups. The wedge of parochialism is also found in the Afrobarometer data.

In Panel (c), in the experimental data, we define conflict severity as the number of conflict-related situations a participant has experienced, as measured by our questionnaire (see Appendix Table B.9). The conflict situations are damages or destruction to one's personal belongings; whether a participant was injured during the conflict; whether a participant knows someone who was killed during the conflict; whether a participant witnessed the setting of houses on fire during the conflict; and whether a participant was physically threatened during the conflict.

Figure 5 shows that, across datasets, increases in conflict exposure among the directly affected are associated with increases in parochialism (calculated as the difference between ρ_2 and ρ_1 from Equation (3)) in the order of 0.36 – 0.39 SD in the WVS, 0.06 – 0.12 SD in the Afrobarometer, and 0.37 SD in the experiment. These differences (Wald tests) are all statistically significant, such that more direct exposure to conflict appears to trigger a larger parochialism wedge.

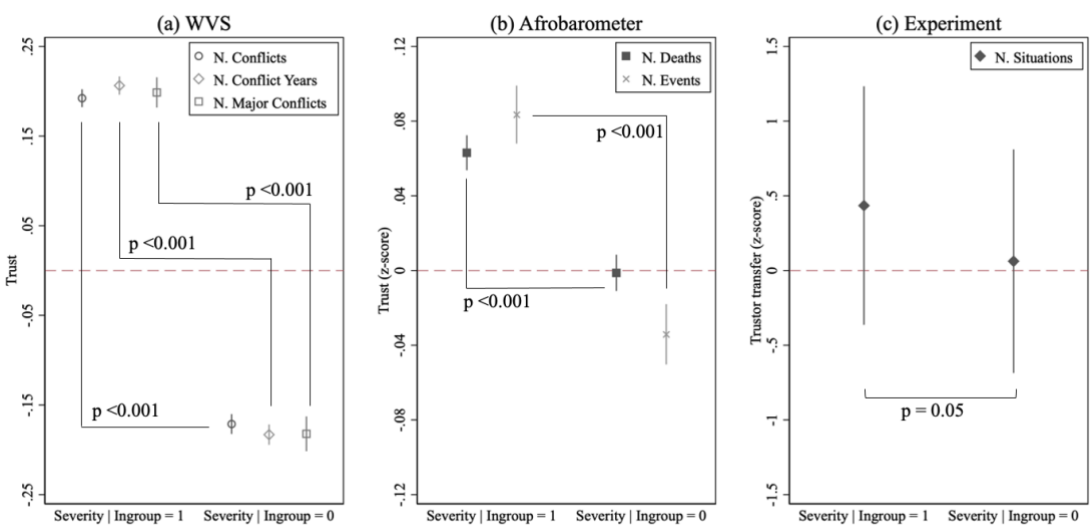

Figure 5. Heterogeneous responses of trust in in-groups and out-groups by conflict severity.

6 Discussion and Conclusion

Using a lab-in-the-field experiment in Maluku, Indonesia, and survey evidence from the Social Well-Being Survey in Asia, the World Values Survey, and the Afrobarometer (for a total of 44,907 respondents from 29 countries), we have shown that (i) indirect conflict exposure tends to reduce interpersonal trust, and (ii) direct conflict exposure significantly increases the gap between trust in in-group members and trust in out-group members. This supports propositions by Lavi et al. (2012) and Schmid and Muldoon (2013) that conflict exposure increases threat perceptions and prejudice related to the out-group and erodes out-group trust. The studies presented in this paper show that direct exposure to conflict induces discriminatory trust. Discrimination is likely to compound into exclusion (Qian and Tabellini 2022), fracture social cohesion beyond the conflict, and thus make public good provision more difficult (Martinez-Bravo et al 2023; Boustan 2016), inducing large societal losses as a result.

We hope that our results bring researchers one step closer to understanding the ever-important phenomenon of parochial altruism (Bauer et al. 2016). Indirect exposure was found to be related to lower average trust, which lends support to findings by Booth et al. (2022). We find this in our experimental and survey data, in different regions of the world and using different ways of operationalizing exposure to conflict. A possible explanation is that people with indirect exposure have felt the negative consequences of conflict sufficiently to reduce their faith in others. At the same time, being further away from conflict, indirectly exposed subjects are less likely to have experienced the solidarity that directly exposed subjects may have experienced. The indirect exposure left them distrusting of everyone, irrespective of group affiliation.

Our findings may explain seemingly contradictory results in the literature. A group of studies investigated the impact of conflicts on discrimination, but did not distinguish different kinds of conflict exposure (group 1). For example, Gneezy and Fessler (2011) did not find increased in-group trust during or after the conflict. Other studies, however, did find discrimination. While context matters, it is possible that the type of conflict exposure may have been an unobserved confounder in this class of studies. Of the studies that investigated the effect of conflict exposure on average trust (group 2), some found a positive effect. The findings from the present study suggest that this positive effect can be explained by direct exposure and a sample that implicitly or explicitly signaled to subjects that they were interacting with in-group members. This is also pointed out by Mironova and Whitt (2016a). For example, Voors et al. (2012) investigated behavior towards neighbors and Gilligan et al. (2014) investigated trust towards fellow community

members. This is also supported by the fact that Bellows and Miguel (2006, 2009) found higher levels of civic and political engagement in their local communities among directly exposed people.

In Maluku, and in other conflict-torn areas, different conflict resolution programs for directly or indirectly exposed people could be useful, using the channels through which people were affected. For directly exposed people with negative out-group experiences, mere communication might not help. They may need to make positive experiences with out-group members and should thus be encouraged to participate in mixed-group community meetings and joint efforts to rebuild houses, villages, and infrastructure (described by Barron et al. (2010)). Harmonious interaction of previously fighting groups should be further encouraged and targeted towards directly exposed people.

Our research also suggests that policies should not be limited to directly affected regions and people. Indirectly exposed people make up a larger share of the population (Korovkin and Makarin 2023) and may be easy to influence via communication. The media could help to reduce their threat perceptions and to re-establish trust. In line with suggestions by Lee and Maslog (2005), the media should be encouraged to focus on how to prevent violence, on the harmonious interaction before the conflict started, rather than emphasizing injuries and fatalities. They could also report on joint reconstruction efforts to enable indirectly exposed people to partake in cooperation between groups. Zussman (2023) shows how media coverage can bring about positive change in attitudes and behavior. On a cautiously optimistic final note, Whitt et al. (2021) show that intergroup contact, post-conflict, can help restore out-group altruism. Altruism, of course, is not trust: for any intergroup contact to occur in the first place, trust is paramount, hence the urgency of rebuilding it.

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Appendix to "Violent Conflict and Parochial Trust: Lab-in-the-Field and Survey Evidence"

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Appendix A: Background Information and Additional Results

Table A.1: Experiments in (post-) conflict environments

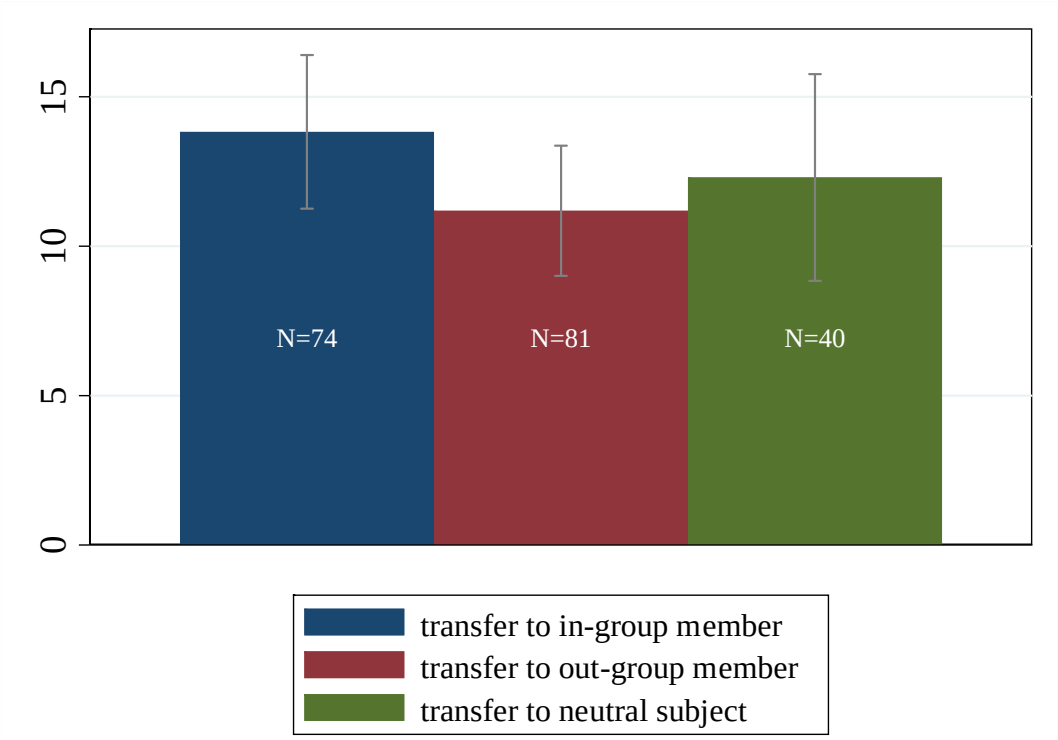
Authors	Degree of exposure	Discrimi-nation	Country	Game(s)	Main findings
Group 1: Discrimination in conflict environments					
Whitt, Wilson 2007	×	✓	Bosnia	dictator	Higher altruism toward in-group than out-group after conflict.
Whitt 2012	×	✓	Bosnia	dictator (third-party)	High number of fair distributions; some discrimination after conflict.
Schubert, Lambsdorff 2014	×	✓	West Bank	ultimatum	Palestinian preference for discrimination against Israelis, in particular among those who favor a political role for Islam.
Gneezy, Fessler 2011	×	✓ (in-group only)	Israel	ultimatum, trust	Higher costly pro-social punishment and rewards within a group during wartime than before/after war. No difference in trust.
Group 2: Degree of subjects' exposure					
Becchetti et al. 2011	✓	×	Kenya	trust, public goods	No effect of conflict exposure on trust, negative effect on cooperation and trustworthiness learning.
Cassar et al. 2013	✓	×	Tajikistan	dictator, ultimatum, trust	Injury or death in the family reduce trust in people from same village; increased pro-sociality toward a person from a distant town by highly exposed subjects.
Gilligan et al. 2014	✓ (community measure)	×	Nepal	dictator, trust, public goods	Exposure is related to increased altruism, trust and cooperation within communities.
Voors et al. 2012	✓ (household measure)	×	Burundi	social value orientation	Exposure is related to increased altruism and social capital within neighborhoods.
Gangadharan et al. 2017	✓ (birth cohorts & regional measure)	×	Cambodia	Money-burning, dictator, trust, risk	For directly exposed, conflict intensity reduces anti-social behavior, altruism and trust, whereas for indirectly exposed, honesty increases with intensity.
Group 3: Both dimensions					
Mironova, Whitt 2014	✓	✓	Kosovo	dictator, expectation, public goods, trust	Higher pro-sociality toward in-group than out-group members; gap increases with exposure. Highly exposed are less trusting of out-groups.
Bauer et al. 2014	✓	✓	Georgia, Sierra Leone	sharing & envy games	Children share more equally with in-group than out-group members; difference increases with exposure.

Table A.2: Lab-in-the-field: Relationship between religion and ethnicity in the full sample

	Christian	Muslim	Sum
Indigenous	386	210	596
Non-indigenous	13	71	84
Undefined	1	43	44
Sum	400	324	724

We could not find any official data on the relationship between ethnicity and religion in Ambon, but the subject pool implies that there is only a weak overlap: There is a correlation of 33 per cent between ethnicity and religion. Of the indigenous participants in the full sample (participants in all games and treatments), 65 per cent are Christian and 35 per cent are Muslim, while of the non-indigenous participants, 15 per cent are Christians and 85 per cent are Muslim.

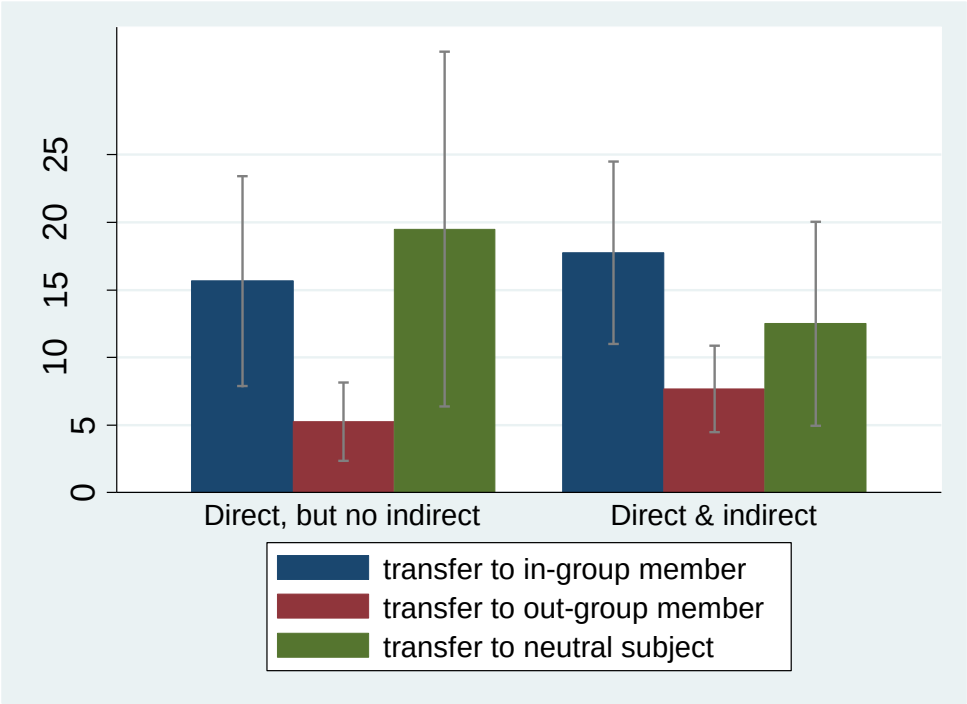
Figure A.1: Transfers to in-group members, out-group members, and neutral subjects



Notes. The bars depict mean treatment transfer by trustees in thousands of Indonesian Rupiah. Capped ranges indicate 95% confidence intervals.

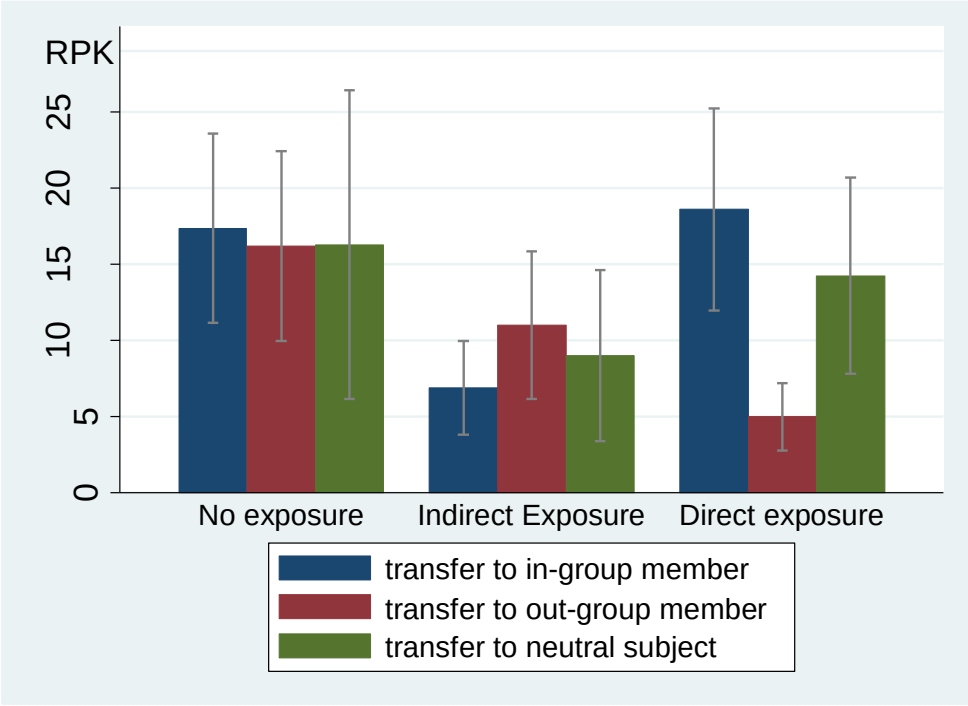
Figure A.1 illustrates mean trustor transfers to the religious in-group and out-group and to subjects of unknown religion. The high blue bar in Figure A.1 suggests a tendency to favor in-group over out-group members and neutral subjects, but neither the difference between in-group and out-group transfer (Wilcoxon-Mann-Whitney Test, z=-1.46; p=0.14) nor between in-group and neutral subjects (z=-0.75; p=0.46) meet conventional significance levels of 5%.

Figure A.2: Lab-in-the-field: Comparison of subjects with direct, but no indirect exposure to subjects with direct and indirect exposure



The bars depict mean treatment transfers in RPK. Capped ranges indicate 95% confidence intervals.

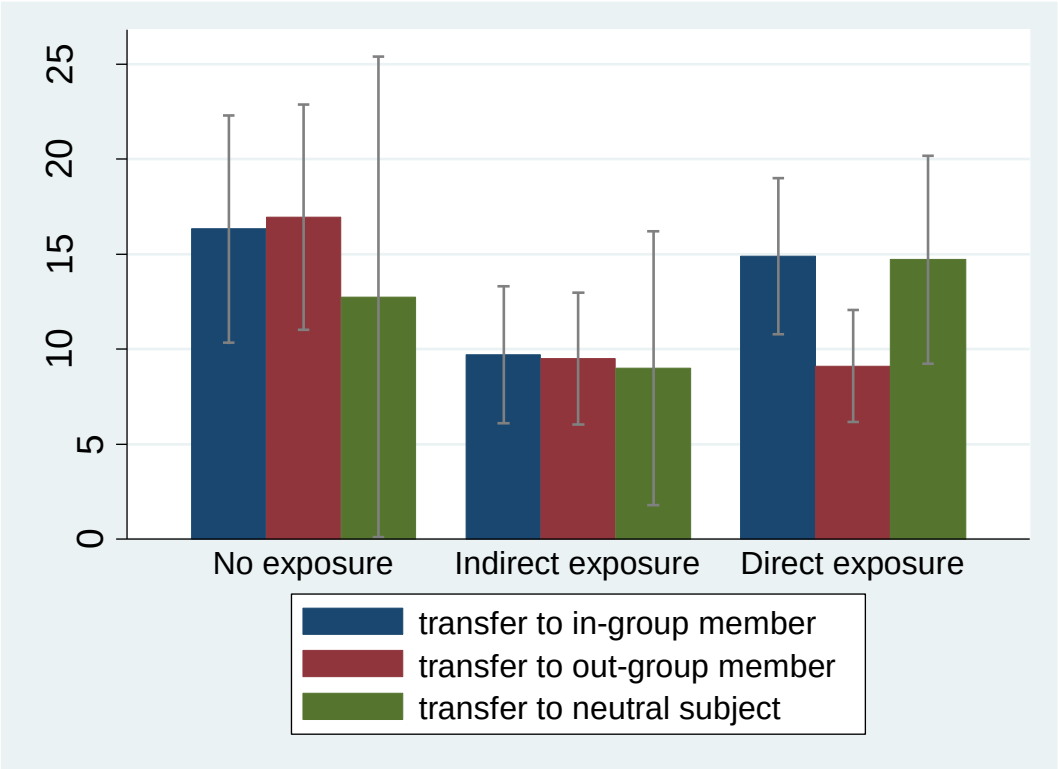
Figure A.3: Lab-in-the-field: Results when only religious in-group and out-group are included



Lab-in-the-field: Application of broader measures of direct and indirect exposure

The results are robust to the use of broader measures of exposure. The measure for direct exposure includes, in addition to having been injured or physically threatened, having lost one's belongings and having experienced the injury or death of a family member. In addition to showing that the results are robust to the inclusion of broader experiences, this broader measure increases the number of observations for subjects with direct exposure to 77 and thus allows for a more meaningful analysis. The broader measure for indirect exposure includes, in addition to having a friend who lost his/her belongings, to have witnessed the setting of houses on fire.

Figure A.4: Lab-in-the-field: Trustor transfers by treatment and exposure (broader measures of direct and indirect exposure)

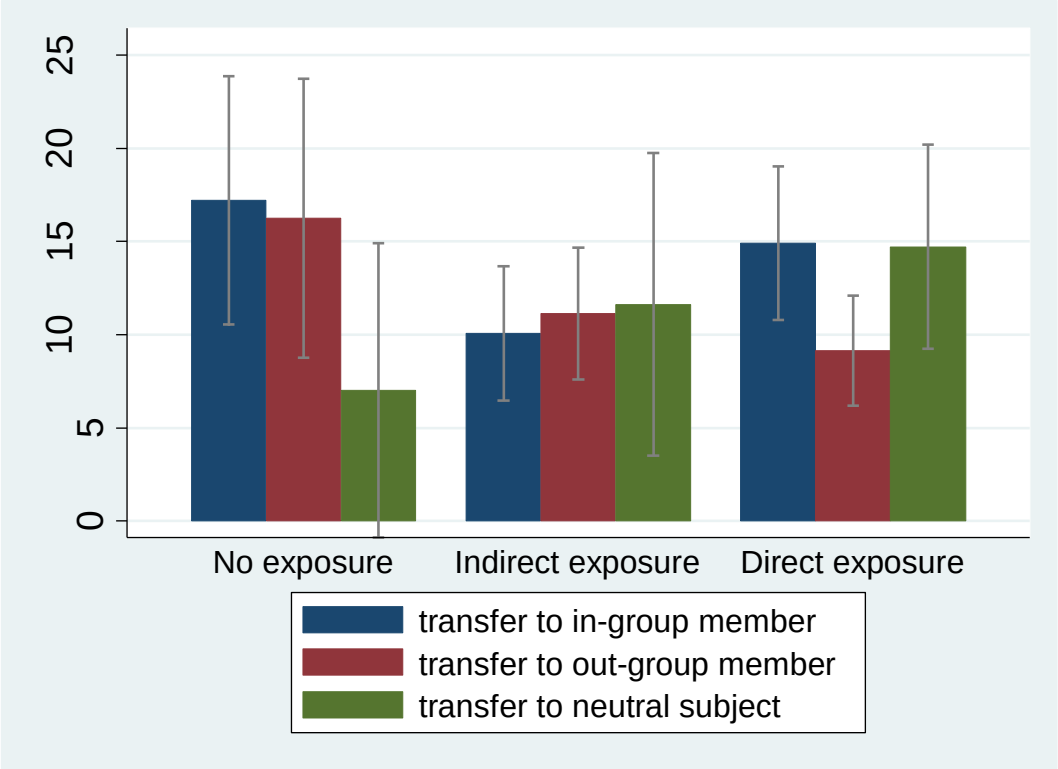


The bars depict mean treatment transfers in RPK. Capped ranges indicate 95% confidence intervals.

Again, there are no significant differences between the bars in the left section of the graph and no significant differences between the bars in the middle section of the graph. Both subjects with neither indirect nor direct conflict exposure and subjects with indirect, but no direct exposure do not trust in-group members more than out-group members. However, subjects with indirect

exposure are, on average, less trusting than subjects with no exposure: Mean transfers of RPK 9.5 by the 47 subjects with indirect, but no direct exposure (middle section of the graph) are significantly lower than mean transfers of 16.2 by the 39 subjects with no exposure in the left section of the graph (Mann-Whitney Test, z=2.18; p=0.029). On the right side of the graph, the difference between transfers of RPK 14.9 to the in-group (blue bar on the right side) by the and 9.1 to the out-group (red bar on the right side) reconfirm that directly exposed people have higher trust in in-group than in out-group members (Mann-Whitney Test, z=1.74; p=0.08). As in the previous analysis, the difference becomes larger and more significant, when only the religious in-group and out-group instead of ethnic and religious in-group and out-group are taken into account (mean in-group transfer of 16.2 and mean out-group transfer of 7.9; Mann-Whitney-Test: z=1.82; p=0.06). The comparison of the red bar on the left side and the red bar on the right side also reveals that directly exposed subjects transfer less to out-group members than subjects without conflict exposure (z=1.82; p=0.07).

Figure A.5: Lab-in-the-field: Trustor transfers by treatment and exposure (broader measures of direct and indirect exposure)



In Figure A.5, the measure of indirect exposure additionally includes subjects who reported that a friend of them was injured during the conflict.

Table A.3: Lab-in-the-field: Regression results including broader measures of direct and indirect exposure

	(1) All	(2) All	(3) All	(4) No direct exposure	(5) Direct exposure
in-group	1.5	0.2	-0.2	1.7	0.2
	(2.2)	(2.5)	(2.8)	(3.5)	(3.5)
out-group	-1.1	-2.3	-2.8	1.9	-5.6+
	(2.1)	(2.4)	(2.7)	(3.4)	(3.2)
indirect exposure		-4.6*	-3.9+	-6.6**	-0.4
		(2.0)	(2.1)	(2.3)	(3.3)
direct exposure		1.6	0.7		
		(1.6)	(1.7)		
constant	12.3***	16.1***	19.8*	14.6***	15.1***
	(1.7)	(2.8)	(9.6)	(3.5)	(4.2)
Demographic controls	No	No	Yes	No	No
N	195	163	158	86	77
R^2	0.012	0.046	0.114	0.102	0.060

Robust standard errors in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A.4: Lab-in-the-field: Logit regressions on conflict exposure

Dependent variable: Probability to have:	(1) Direct exposure	(2) Direct exposure: marginal effects	(3) Indirect exposure	(4) Indirect exposure: marginal effects
muslim (d)	0.06	0.01	0.4	0.07
	(0.5)	(0.1)	(0.6)	(0.09)
male (d)	0.7+	0.2+	-0.4	-0.08
	(0.4)	(0.09)	(0.5)	(0.09)
age	0.1	0.03	0.2	0.04+
	(0.09)	(0.02)	(0.1)	(0.02)
siblings	-0.1	-0.03	-0.2*	-0.04*
	(0.09)	(0.02)	(0.1)	(0.02)
income	-0.3	-0.09	0.1	0.02
	(0.3)	(0.08)	(0.4)	(0.07)
dictator transfer	0.002	0.0005	-0.005	-0.0009
(% of endowment)	(0.006)	(0.002)	(0.007)	(0.001)
piety	0.3	0.08	0.2	0.04
	(0.3)	(0.07)	(0.3)	(0.06)
direct exposure (d)			1.7***	0.3***
			(0.4)	(0.07)
constant	-1.9		-3.5	
	(1.9)		(3.0)	
N	158	158	158	158
pseudo R^2	0.059	0.059	0.170	0.170

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

For direct exposure, only being male is a (weakly) significant predictor. Coming from a smaller family is related to a lower probability of having indirect exposure.

Table A.5: Lab-in-the-field: OLS Regression with demographic controls

Dependent variable: Trustor allocation				
	(1) All	(2) All	(3) No direct exposure	(4) Direct exposure
in-group	0.05	0.09	0.6	0.7
	(2.7)	(2.8)	(3.2)	(4.7)
out-group	-2.5	-2.5	0.7	-11.0*
	(2.6)	(2.7)	(3.1)	(4.7)
indirect exposure	-4.7**	-4.6**	-6.9***	2.2
	(1.7)	(1.7)	(2.0)	(3.5)
direct exposure	-0.3	-0.4		
	(1.9)	(1.9)		
male	4.2*	4.2*	5.5*	3.0
	(1.9)	(2.0)	(2.5)	(3.4)
age	-0.05	-0.03	-0.3	-0.07
	(0.4)	(0.4)	(0.6)	(0.6)
family size	0.5	0.5	0.3	1.1
	(0.4)	(0.4)	(0.5)	(0.9)
income	-2.8+	-2.6	-4.2*	-3.7
	(1.7)	(1.8)	(2.0)	(3.4)
muslim	-0.7	0.02	-2.3	2.8
	(2.3)	(2.4)	(2.7)	(3.9)
dictator transfer (% of endowment)		-0.006		
		(0.0)		
piety		1.4		
		(1.4)		
constant	19.3*	18.6+	27.5+	15.6
	(9.3)	(9.6)	(15.0)	(13.6)
N	159	159	109	50
R^2	0.138	0.144	0.209	0.236

Standard errors in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A.6: Lab-in-the-field: Balance between samples (Kolmogorov-Smirnov tests)

	In-group Member	Out-group Member	Neutral Subject	Combined K-S test (corrected p-values)		
				In-group vs. out-group	Out-group vs. neutral	Neutral vs. in-group
Male	24%	29%	39%	1.0	0.94	0.61
Age	19.9	19.9	22.8	0.5	0.00***	0.00***
Income	1.6	1.6	1.7	1.0	1.0	1.0
Family size	3.8	4.1	4.1	0.72	1.0	0.37
Muslim	35%	39%	20%	1.0	0.0***	0.0***
Autochthonous	84%	80%	97%	1.0	0.25	0.64
Dictator game (share left to receiver)	43%	43%	39%	1.0	0.84	0.91

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Income is measured as 1=below average, 2=about average, 3=above average

The treatments *in-group member* and *out-group member* are completely balanced in terms of all characteristics. The treatment *neutral subject* is slightly older than the other two. Furthermore, there are fewer Muslims in this treatment group. This is because Muslims were scarcer than Christians in the entire sample and therefore a high share of Muslims had to be assigned to the treatments *in-group member* and *out-group member*.

Table A.7: Lab-in-the-field: Balance between samples (Kolmogorov-Smirnov tests)

	No exposure	Indirect exposure	Direct exposure	Combined K-S test (exact p-values)		
				No vs. indirect	Indirect vs. direct	No vs. direct
Male	25%	21%	44%	1.0	0.09+	0.24
Age	19.9	20.6	21	0.17	0.64	0.12
Income	1.6	1.7	1.5	0.63	0.16	0.99
Family size	4.5	3.6	3.8	0.6	0.77	0.33
Muslim	38%	19%	24%	0.24	1.0	0.66
Autochthonous	77%	89%	77%	0.67	1.0	0.51
Dictator game (share left to receiver)	47%	36%	45%	0.39	0.71	0.95

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A.8: Lab-in-the-field: Effect of age on in-group vs out-group allocation

Dep. Variable: Trustor allocation to	(1) In-group trustee	(2) Out-group trustee
Direct exposure	4.8	-4.8*
	(3.0)	(1.95)
Indirect exposure	-4.2	-3.7
	(2.8)	(2.4)
age	-0.9	-0.4
	(1.0)	(0.7)
constant	32.7+	21.2
	(17.5)	(13.7)
N	66	66
R^2	0.1	0.1

Standard errors in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table A.9: World Values Survey: Principal Component Analysis

	Eigenvectors	
Variable: Trust in:	Out-group trust	In-group trust
People you meet for the first time	**0.4558**	-0.159
People you know personally	**0.4096**	0.3034
People of another religion	**0.4769**	-0.3465
People of another nationality	**0.4804**	-0.353
Your family	0.1721	**0.6515**
Neighbours	0.3697	**0.4621**

The World Values Survey has several trust questions, which we exploit to build measures of in-group and out-group trust. These seven variables can be summarized by two principal components with eigenvalues greater than one. Trust in people you meet for the first time, people of another religion, people of another nationality, and people you know personally, scored the highest on the same component, which we label out-group trust. On the other hand, trust in family and neighbors scored the highest on the other component, which we label in-group trust. While neighbors may not necessarily be part of the in-group in every single social situation, a large literature studies sorting decisions into neighborhoods by people who wish to live in close proximity to people they consider their in-groups, starting with the seminal work of Thomas Schelling (1969, 1971).

Principal components analysis decomposes the variation across the seven variables above into two orthogonal components. The two components, out-group and in-group trust, are thus uncorrelated by design, which is an overly restrictive assumption. Instead, we build our measure of in-group trust as (0.65 * Family + 0.46 * Neighbours) / (0.65 + 0.46), where 0.65 and 0.46 are the relevant weights for each variable as indicated by the PCA. Our measure of out-group trust follows the same logic, i.e. (0.46 * First time + 0.41 * Personally + 0.48 * Other religion + 0.48 * Other nationality) / (0.46 + 0.41 + 0.48 + 0.48).

Figure A.6: Global distribution of conflicts on home soil since 1946 from UCDP/PRIO.

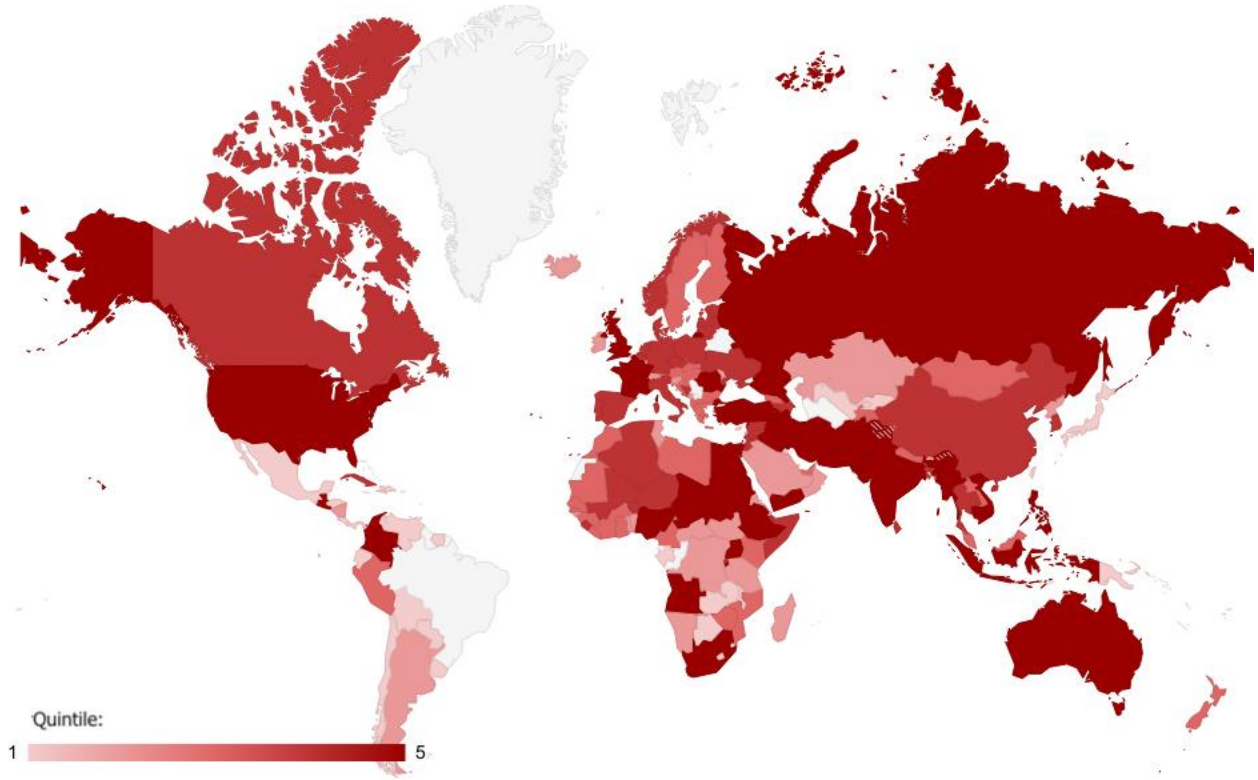


Figure A.7: African countries included with level 1 sub-national boundaries

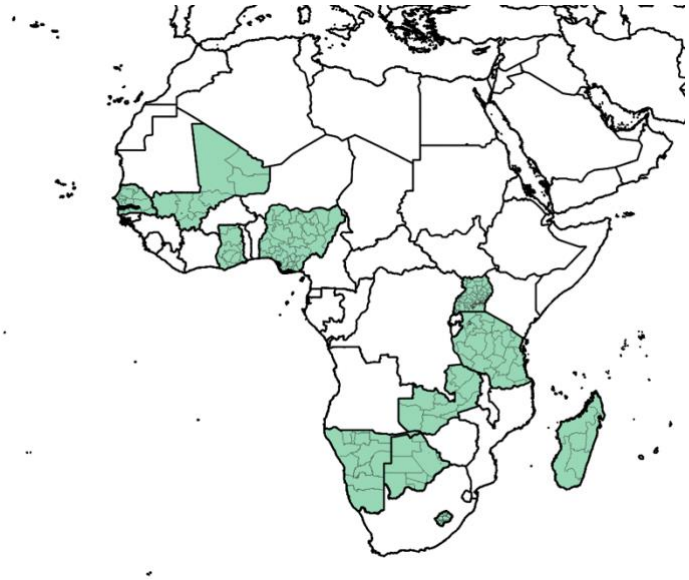


Table A.10: Social Well-Being Survey: Principal Component Analysis

	Eigenvectors	
Variable: Trust in:	Out-group trust	In-group trust
Strangers	**0.8035**	0.3718
Friends	-0.1028	**0.5605**
Family	-0.5829	**0.4755**
Neighbours	0.0637	**0.5670**

We performed principal component analysis with the set of trust variables from the SWBS. Retaining components with eigenvalues greater than 1, the four trust variables can be summarized with two variables. Trust in friends, family, and neighbours loaded highly on the same component, which we label trust in in-groups, while trust in strangers loaded highly on a different component, which we label trust in out-groups. Out-group and in-group trust are constructed with the same weighting procedure as the one described in the notes to Table A.9 above.

Table A.11: List of countries and number of respondents

(A) World Values Survey (28,073 respondents from 12 countries)

Country	*N*	*Country*	*N*	*Country*	*N*
Argentina	1,666	Ghana	2,948	Romania	2,408
Burkina Faso	1,334	Lebanon	1,128	South Africa	5,939
China	3,551	Malaysia	2,371	Spain	1,745
Ecuador	1,123	Morocco	2,248	Trinidad & Tobago	1,612

(B) Afrobarometer (15,723 respondents from 11 countries)

Country	*N*	*Country*	*N*	*Country*	*N*
Botswana	1,194	Mali	1,242	Tanzania	1,299
Ghana	1,187	Namibia	1,182	Uganda	2,395
Lesotho	1,161	Nigeria	2,356	Zambia	1,200
Madagascar	1,340	Senegal	1,167		

(C) Social Well-Being Survey in Asia (1,111 respondents from 7 countries)

Country	*N*	*Country*	*N*	*Country*	*N*
Indonesia	178	The Philippines	177	Vietnam	79
Japan	485	Thailand	171		
South Korea	15	Taiwan	6		

Figure A.8: Countries included in the SWBS, WVS, and Afrobarometer

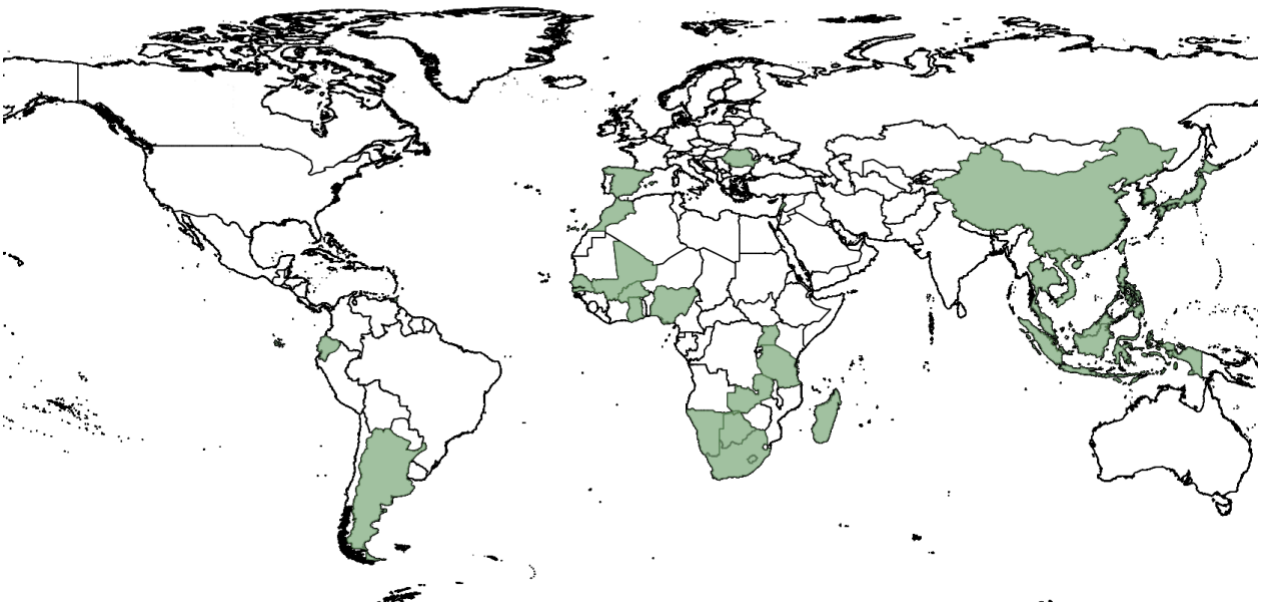


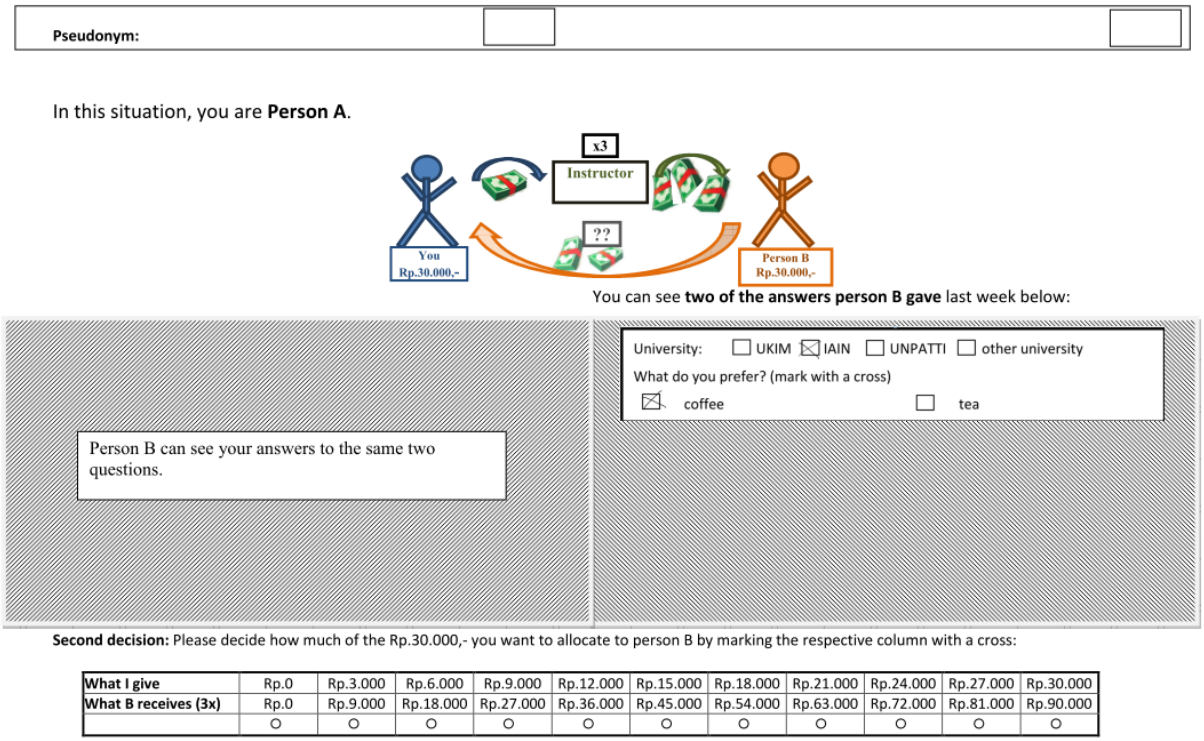
Table A.12: Summary statistics for Section 5

	WVS	**Afrobarometer**	**SWBS**
N respondents	28,073	15,723	1,111
N countries	12	11	7
Direct exposure	Ref. cat.: Indirect Mean = 0.88 SD = 0.33	Ref. cat.: Indirect Mean = 0.45 SD = 0.50	Ref. cat.: Indirect + None Mean = 0.07 SD = 0.26
Trust in in-groups	Mean = 0.73 SD = 0.12	Mean = 0.57 SD = 0.33	Mean = 0.59 SD = 0.17
Trust in out-groups	Mean = 0.52 SD = 0.18	Mean = 0.47 SD = 0.33	Mean = 0.26 SD = 0.21

Appendix B: Procedural Details and Instructions

The treatment information was copied from the counterpart's questionnaire on each subject's game sheet with the help of a stencil with cut-out holes where the respective treatment information was located. Hence, each subject received a personalized game sheet with the hand-written crosses of the counterpart (see Figure 1) to ensure that subjects would not doubt the existence of the other player (Frohlich et al. 2001).

Figure 1: Sample game sheet after use of the stencil (English translation)



What I give	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
What B receives (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
	○	○	○	○	○	○	○	○	○	○	○

With the help of university staff members, participants were invited to one of ten sessions at their campus in the first week (with 50 participants per session) and were asked to attend further sessions at the same time each week for the next three weeks.[25] Subjects were seated in lecture halls, at

[25] This work is part of a larger study in which additional dictator and ultimatum games were played (see Werner, Lambsdorff 2016), so not all of the 1000 subjects took part in the trust game.

sufficient distance to prevent them from observing each other's choices. Across all tasks in the four weeks, a unique pseudonym on the game sheet only visible to the experimenter, but not to the counterpart, ensured the highest possible degree of double blindness (Camerer, Fehr 2004, pp. 72-73).

In the first week, subjects answered the short questionnaire used for the treatment assignment. Kolmogorov-Smirnov tests (appendix, Table A.10) show that the treatments are balanced in terms of gender, age, income, family size, religion, ethnicity, self-stated religiosity and altruism as measured in the dictator game. In the second week, all subjects used in this analysis first played a dictator game in the role of dictator.[26] Right after the dictator game, half of them played a trust game in the role of the trustor. Incentivized beliefs regarding the trustor's sending amount were elicited from the other half of the subjects. In the third week, the latter played the trust game in the role of the trustee. Previous week's trustors answered questions on ethnic stereotypes as a filler task. Perfect stranger matching was used between the dictator and the trust game. Subjects were assured they would face a different counterpart in each game. To underline this, subjects who had faced an out-group member in the dictator game faced an in-group counterpart in the trust game and vice versa. Subjects in the neutral treatment continued to interact with a neutral member in the trust game.

In the fourth week, all subjects answered a questionnaire on demographics, religiosity and conflict exposure (appendix Figure B.9 displays the full questionnaire). It contained an event list, as it is standard practice for measuring conflict exposure (Netland 2005; Schmid, Muldoon 2013), to indicate what kind of events they had experienced in the conflict (injury, injury of family member, injury of friend, death of family member, physical threat, loss of property, loss of family's

[26] The dictator transfer served as a measure of subjects' other-regarding preferences to distinguish trust from mere altruism. The same treatments (in-group, out-group and neutral subjects) were applied.

property, loss of property by a friend, witnessing the setting of houses on fire). After having returned the completed questionnaire, each subject received a sealed envelope containing the payoff and a result sheet which listed all of the subject's and its counterpart's decisions and resulting payoffs.

In each experimental session, subjects received written instructions (Appendix B) and learned that they would have to answer incentivized comprehension questions. The instructor subsequently explained the experiments again (Appendix C). To avoid problems of computation, subjects then stated in chorus the amount both players would receive for every possible trustor decision, thus making all possible transfers equally salient and avoiding anchoring effects (Tversky, Kahneman 1974; Mehta et al. 1994; Cardenas, Carpenter 2008, pp. 330–331 explain the necessity of oral instructions and examples in the field). When the instructor was sure that everybody had understood the instructions, subjects solved three comprehension tasks before playing the game.

All instructions were provided in Indonesian language and had been translated using standard back translation (Brislin 1970). A pretest run at a mixed-faith university in Ambon in April 2013 ensured comprehensibility. As two experimenters were needed to simultaneously run the experiments at both universities, differences in experimenter effects (Roth et al. 1991) were kept as minimal as possible by using similar-looking experimenters. They followed a strict protocol (e.g. Camerer 2003, p. 69) and had practiced and video-taped the similar reading of the instructions and the use of similar gestures before. Sessions lasted on average 20 minutes. Mean payoffs amounted to RPK 110 (approximately $10).

The trust game is part of a larger study which was run over the course of four weeks. Initially, 1000 subjects participated in the first week. Since the semester had not officially started in September 2013, only 710 participated in the second week.

50% played a standard dictator game while the other 50% played a reversely framed dictator game where the dictator was able to take money from the counterpart instead of giving money to her. More details on the dictator games can be found in Werner, Lambsdorff (mimeo). Those who had played the reversely framed dictator game played the trust game afterwards. First movers of the ultimatum and trust game played in the second week, while second movers listened to the same instructions and were asked for their expectations of the first mover's decision as a filling task. When second movers played in the third week, first movers received a short questionnaire on ethnic stereotypes as a filling task.

Across all treatments, subjects were informed that their own pieces of information were identically transferred to the respective counterpart. An example from the religious out-group treatment illustrates this. A Muslim trustor deciding on the offer to a Christian in the religious out-group treatment was informed about the religious affiliation of the trustee while the trustee knew that transfers would come from a Muslim.

To guarantee maximum understanding, instructions had to be kept very simple and we needed to explain and repeat things that would be more obvious to Western students. In each session, subjects received written instructions first. After reading these, the game was explained once more using oral instructions and a Powerpoint presentation. Both written and oral instructions (including graphs from the presentation) can be found below. A pre-test with 30 subjects was run at a mixed-faith university in April 2013.

Figure B.1: Game Sheet first week (basic information)

Thank you for participating in this study.

For privacy reasons, please do never let other people know what you decided or which answers you gave in this study.

First task:

Personal identification code:

Please write down on the line below:

1. Your mother's first name
2. Your birthday
3. Your father's first name

(example: If you were born on June 15th and your mother's first name is Annisa and your father's first name Edi, your personal identification code is Annisa1506Edi).

_____

Second task:

Please fill in the statistical information below:

University: ☐ UKIM ☐ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☐ coffee ☐ tea

What do you prefer? (mark with a cross)

☐ tofu ☐ tempe

Your parents' mother tongue (mark with a cross if you parents' mother tongue is among the languages below)

☐ Bahasa Ambon, Bahasa Melayu Ternate, Tidore, Kei, Makian,Tobelo, Banda, Tanimbar, Dobo, Kelompok Maluku Barat Daya.

☐ Bahasa Jawa, Bahasa Buton/ Wolio, Bahasa Cia-Cia, Bahasa Bugis, Bahasa Makasar/Bahasa Melayu Makassar, Bahasa Cina.

Figure B.2: Written Instructions Dictator Game (second week)

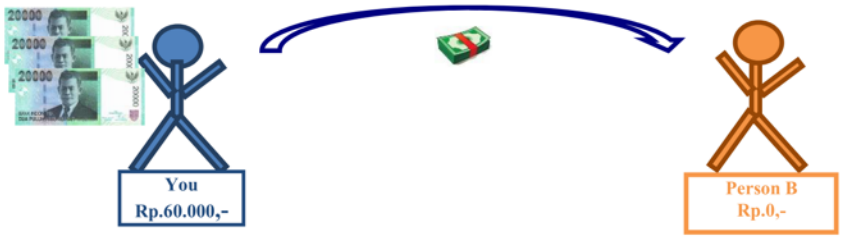
«Pseudonym»

Today you will be asked to make two decisions in two situations. Each situation describes an interaction. You will be paid out all the money you receive in these two situations in cash in the final session.

First situation:

In this situation, two persons interact (A and B). You have been assigned the role of person A. You will interact with another person, person B. However, you will not know who person B is and person B will not know who you are.

In this situation, you receive Rp.60,000 for the start.



If you want (there is no obligation) you can share this money with person B (who does not receive anything in this situation) by giving person B an amount between Rp.0 and Rp.60,000.

In this situation, person B has to accept your decision and has no decision to make. Person B will only be informed on the amount received in the final session. In the following situations you will interact with a different person.

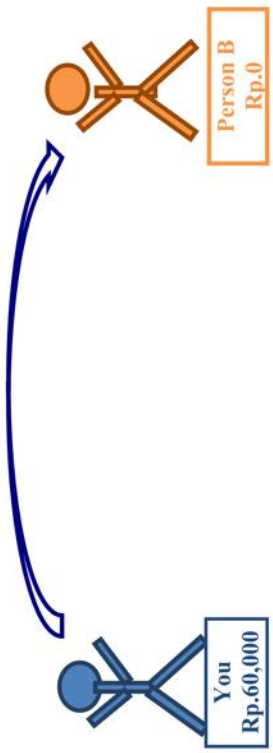
Please make sure that you have well understood the instructions. Below you can find three statements on the situation. Please indicate for each statement whether it is right or wrong. You will receive an additional Rp.2.000,- for each correct answer.

As person A I have to give money to person B.			
☐	Right	☐	Wrong
Person B has no decision to make and has to accept my decision.			
☐	Right	☐	Wrong
Person B will know who I am.			
☐	Right	☐	Wrong

1

Figure B.3: Game Sheet Dictator Game (second week)

Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.



In this situation, **you are person A.**

You can see **2 of the answers person B gave** last week below:

What do you prefer? (mark with a cross)

☐ coffee ☒ tea

What do you prefer? (mark with a cross)

☐ tofu ☒ tempe

First decision: Please decide how much of the Rp.60,000 you want for yourself and how much you want to give to person B by marking the respective column with a cross:

For me	Rp.60.000	Rp.55.000	Rp.50.000	Rp.45.000	Rp.40.000	Rp.35.000	Rp.30.000	Rp.25.000	Rp.20.000	Rp.15.000	Rp.10.000	Rp.5.000	Rp.0
For person B	Rp.0	Rp.5.000	Rp.10.000	Rp.15.000	Rp.20.000	Rp.25.000	Rp.30.000	Rp.35.000	Rp.40.000	Rp.45.000	Rp.50.000	Rp.55.000	Rp.60.000
	○	○	○	○	○	○	○	○	○	○	○	○	○

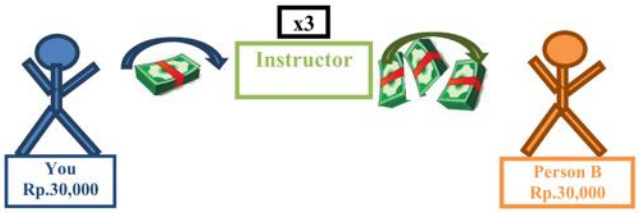
2

Figure B.4: Written Instructions Trustor (second week)

Second situation: **«Pseudonym»**

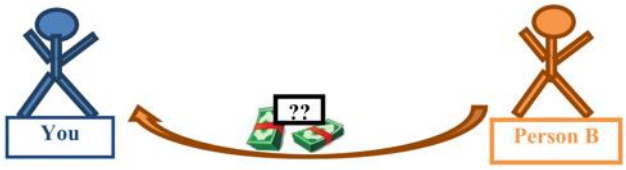
In this situation, there are again two persons (A and B). **You have been assigned the role of person A**. You will interact with another person, person B. However, you will not know who person B is and person B will not know who you are. The amount of money you receive in this situation depends on your own decision and the decision of person B.

In this situation both you and person B receive Rp.30,000 for the start.



You will be asked to decide on how much you want to transfer to person B (between Rp.0 and Rp. 30,000). The amount you transfer will be tripled.

Next week, person B will be asked to decide whether to transfer money back to you (there is no obligation). The amount person B transfers back to you will not be tripled.



Please make sure that you have well understood the instructions. Below you can find two statements on the situation. Please indicate for each statement whether it is right or wrong. You will receive an additional Rp.2,000 for each correct answer.

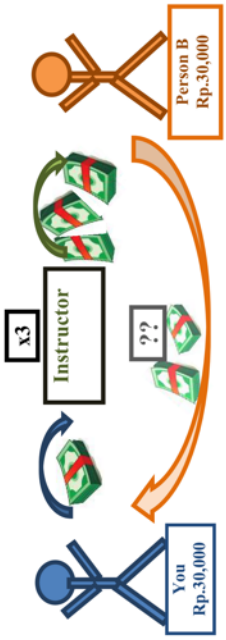
If I transfer money to person B, person B has to transfer money back to me.			
☐	Right	☐	Wrong
The amount person B transfers back to me will be tripled.			
☐	Right	☐	Wrong

3

Figure B.5: Game Sheet Trustor (second week)

Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.

In this situation, you are **Person A**.



You can see **two of the answers person B gave** last week below:

University: ☐ UKIM ☒ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☒ coffee ☐ tea

Person B can see your answers to the same two questions.

Second decision: Please decide how much of the Rp.30,000 you want to transfer to person B by marking the respective column with a cross:

What I transfer	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
What B receives (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
	○	○	○	○	○	○	○	○	○	○	○

4

Figure B.6: Written Instructions Trustee (third week)

Second situation: **«Pseudonym»**

In this situation, there are again two persons (A and B). **You have been assigned the role of person B**. You interact with another person, person A. However, you will not know who person A is and person A will not know who you are. The amount of money you receive in this situation depends on your own decision and the decision of person A.

In this situation, both you and person A have received Rp.30,000 for the start last week.

Last week, person A was asked to decide on how much to transfer to you (between Rp.0 and Rp. 30,000). The amount transferred was tripled by the instructor. This means, for each Rp.1,000 person A transferred, you will receive Rp.3,000.

Today you will be asked to decide on how much you want to transfer back to person A. The amount you transfer to person A will not be tripled. At the moment, you possess the Rp.30,000 you received in the beginning of the situation plus three times the amount person A transferred to you.

Please make sure that you have well understood the instructions. Below you can find two questions on the situation. You will receive an additional Rp.2,000 for each correct answer.

In the box on page 2 you can see the decision person A made last week marked by a cross.

1. How much money does person A have left (after having transferred money to you)?

   Answer: Rp. _____

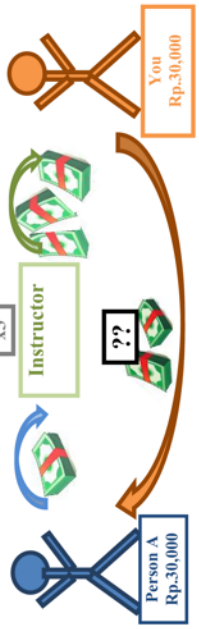
2. How much money do you possess now (after having received the tripled amount person A transferred and including your initial Rp. 30,000)?

   Answer: Rp. _____

Figure B.7: Game Sheet Trustee (third week)

Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.

In this situation, **you are person B**.



You can see **two of the answers person A gave** in the first week below:

University: ☐ UKIM ☒ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☒ coffee ☐ tea

Person A was able to see your answers to the same two questions.

Below you can see the **decision person A has made last week**, i.e. what person A transferred, how much you receive and how much you possess now (maximum amount to be divided):

What person A transferred	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
How much you receive (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
Maximum amount to be divided	Rp.30.000	Rp.39.000	Rp.48.000	Rp.57.000	Rp.66.000	Rp.75.000	Rp.84.000	Rp.93.000	Rp.102.000	Rp.111.000	Rp.120.000
	○	○	○	☒	○	○	○	○	○	○	○

Do you want to give something back to person A (**between Rp.0 up to a maximum of the amount you possess now**)?

Decision: I want to give Rp. _____ to person A.

2

Figure B.8: Sample Result Sheet for a Trustor (fourth week)

«Pseudonym»

Results of your decisions:

Second week

In the first situation, of the Rp.60000 which Person B received, you decided to take **Rp. «Amount taken dictator game»** for yourself.

In the second situation, you were assigned the role of person A. You decided to give **Rp. «Amount sent trust game»** of the Rp. 30.000 which you were given for the start to Person B. Person B decided to return **Rp. «amount received back»** , which means that you receive **Rp. «Payoff trust game»** in this situation.

On page 1, you answered **«no. correct answers dictator game»** questions correctly and on page 3 you answered **«no. correct answers trust game»** questions correctly. That means you receive an additional Rp. **«payoff comprehension week 2»** for your answers.

Third week

You received **Rp. 3,000** for filling in the questionnaire.

Fourth week

You have not been informed yet that you additionally participated in a third situation. In this situation, you had no decision to make. Another person decided to give an additional Rp. **«amount received dictator game»** to you.

Your total income:

	Amount
First situation	Rp. «Amount taken dictator game»
Second situation	Rp. «payoff trust game»
Comprehension questions	Rp. «Payoff comprehension week 2»
Questionnaire	Rp. 3,000
Third situation	Rp. «amount received dictator game»
Total	**Rp. «total»**

Please sign:

Name	Date	Amount received	Signature
		Rp. «total»	

Figure B.9: Translation of Questionnaire (fourth week)

Question	Answers	
How old are you?		
How many siblings do you have?		
Gender	male	female
Religion	Muslim, Protestant, Catholic, Hindu, Buddha, other religion, atheist	
In which major are you enrolled?		
In my opinion, my parents' income is	Above average; average; below average	
Are you a member or a volunteer in an organization / group as listed below (more than one answer possible)	Theater, music or dance club; political organization; religious club or organization; other (please name)	
Do you base your voting decision on your religion?	Yes	No
How much do you agree with the following statements (1= not agree at all; 7= completely agree)		
We have to be careful in dealing with other people because most people cannot be trusted.		
I consider myself very religious		
I live according to [Sharia / Christian laws]		
Religion has to play an important role in shaping the political opinion of [Muslims/Christians]		
Armed attacks against [Christians/Muslims] can be justified		
I want [Indonesia to become an Islamic state / Maluku to become an autonomic region with Christian laws]		
Being Indonesian is an important part of my identity		
Being [Muslim/Christian] is an important part of my identity		
My ethnicity is an important part of my identity		
My major is an important part of my identity		
Being a member of one of the organizations in question 8 is an important part of my identity		
If somebody criticizes my religion, I take it as a personal insult		
When I talk about [Muslims/Christians], I say "we" rather than "they"		
I feel similar to other [Muslims/Christians]		
I feel similar to other people of my ethnicity		
I feel discriminated in [Maluku because I am Muslim / Indonesia because I am Christian]		
Sometimes I feel discriminated because of my ethnicity		
I fear to be pushed aside by other ethnicities		
We are allowed to beat others if they insult our religion		
How often do you do/have you done the following things (1= never; 7= very often)?		
Go to [the mosque/church]		
I tell everybody about God's strength and convince them to become [Muslim / Christian]		
I fight for the rights of [Muslims / Christians]		
I spend time with [Christians / Muslims]		
I spend time with people of other ethnicities		
I participate in activities together with people of other religions than my own one		
I have had a fight with a person of another religion		
I have argued with a person of another religion		

I have participated in pela
I have participated in gotong-royong
In the following, there is a list of events / situations. Please mark with a cross which of them you have experienced during the conflict in Ambon:
My personal belongings got damaged or destroyed in the conflict
My family's belongings got damaged or destroyed in the conflict
My friends' belongings got damaged or destroyed in the conflict
I got injured in the conflict
Family members of mine got injured in the conflict
Friends of mine got injured in the conflict
A family member or close friend of mine died because of violence in the conflict
I witnessed the setting of houses on fire
I was physically threatened during the conflict

B.10: Additional explanation on randomization inference

The intuition dates back to Fisher's (1935) tea tasting experiment, and has been compellingly advocated for by Young (2019). Consider the following thought experiment, with a continuous outcome and a binary treatment. Each observational unit is either treated or not treated: the entire universe of all potential treatment allocations is therefore known. Whereas a t-test compares the observed test statistic to Student's t-distribution, Fisherian randomization inference compares the observed test statistic to the distribution of test statistics that could have been obtained under all possible treatment allocations. Comparing the observed test statistic to the distribution of all possible test statistics therefore yields an exact p-value, even in smaller samples.

In Fisher's experiment, British scientist Muriel Bristol claimed to be able to discern whether milk or tea was poured first into a cup. Fisher tested her claim with 8 cups, 4 of which had milk poured first, and 4 with tea poured first. The number of possible test statistics is $C_8^4 = \frac{8!}{4!(8-4)!} = 70$. In our randomization, we have up to 195 observations, and we perform i.i.d. random assignment to placebo treatments, such that approximately half the observations are placebo-treated. There are thus $C_{195}^{97} \approx 2.86 * 10^{57}$ possible treatment allocations. Obtaining all corresponding test statistics is prohibitively expensive from a computational standpoint; we therefore perform 2,000 randomizations to obtain close-to-exact p-values. Our randomizations are seeded and therefore fully replicable.

Appendix C: Oral Instructions (with excerpts from the Powerpoint presentation included)

C.1 First week: General instructions

Good morning. I am a PhD student from Germany. Together with a colleague (who is at another university at the moment where we also run our study), I would like to run a scientific study in Ambon. First of all, I would like to thank you for your willingness to participate in this study. You are probably interested in the goal of this study. We are conducting this study to learn about Indonesian students' economic behavior and their way of making decisions dealing with money. This is interesting to us as many similar studies on economic decision-making have been run, but only few of them in Asian countries. Our study is quite large: In total, 1000 students from different universities will participate.

Introduction

- Thanks for your participation
- Research topic:
  - Economic decision-making
  - Way to make money-related decisions
- 1000 participants

Starting from today, we will meet once a week for the next three weeks. We meet every week at the same time here in this room. Every time we meet, the study will take about 15 minutes.

Schedule

- 4 sessions (15 minutes)
- **First session**: now
- **Second session**: [date], [time]
  - Two interaction situations
- **Third session**: [date], [time]
  - One interaction situation
- **Fourth session**: [date], [time]
  - Questionnaire and payoffs

Next week and the week after, you will receive a short text which describes an interaction situation between you and another person of whom you do not know the name or identity. After you have read the text, I will orally explain the interaction situation again. Afterwards, you will be asked to answer three comprehension questions on the instructions and to make a decision. Please note that this study is not a test. There is no right or wrong answer. All decisions are at your own discretion.

Agenda

- Interaction situations with a person you do not know
  - Written instructions first
  - Additional oral explanations
  - Comprehension questions
  - decision
- there is no wrong decision

In each interaction, you will be asked to make an economic decision that has to do with money. The decisions are not difficult and you only need a pen. Important: All the money you receive in this interaction will be paid out to you privately on [date]: You will receive a sealed envelope containing the money and information on the results of your decisions. Others will not see how much money you have earned. The amount of money you receive depends on your own decisions and on the decisions of other persons. In similar studies, students have earned on average Rp. 110.000,- per person. Important: You will only receive money if you attend all the sessions (that means today, [date], [date] and [date]), four sessions in total.

Money

- You will receive money on [date]

- Sealed envelope
- Amount depends on decisions
- Rp. 110,000 on average

For each decision you will be paired with another person whose name you do not know. You will never get to know who the other person is and the other person will never get to know who you are. For each decision, you will be paired with a different person, i.e. you will never interact with the same person twice.

Identification code

Today there is no decision to make yet. Today you are only asked to create your personal identification code and answer some statistical questions. Your personal identification code is a combination of letters and numbers which will be used to guarantee your anonymity. It is important for you to know that we are not interested in collecting information on you as an individual, but only on average decisions. Due to the code, I will be able to see which decisions have been made, but I will not know who of you made which of the decisions. Please never put your name anywhere in this study, but always use your personal identification code. By that we will not know your name. Furthermore, we will not know who gave which answers and made which decisions. The other participants in the study will not know who made which decision either. We will neither be able to announce the name of the participants in the study nor who was paired with whom.

Anonymity

- Interact with a different person in each situation
- Name and identity unknown
- Personal identification code protects privacy

Identification code

- Protects your anonymity
- Nobody will know who made which decision
- Names remain unknown
- **Never use your name, but always your personal identification code in this study!**

To create your personal identification code, please write down on the line on the left side of your sheet:

1 Your mother's first name
2 Your birthday
3 Your father's first name

(example: If you were born on June 15th and your mother's first name is Annisa and your father's first name Edi, your personal identification code is Annisa1506Edi).

First task:

Personal identification code:

Please write down on the line below:

1. Your mother's first name
2. Your birthday
3. Your father's first name

(example: If you were born on June 15th and your mother's first name is Annisa and your father's first name Edi, your personal identification code is Annisa1506Edi).

_____

Please create your identification code now.

After creating your personal identification code, please answer the 4 statistical questions you find on the right side of your sheet by marking the correct answers with a cross.

University: ☐ UKIM ☐ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☐ coffee ☐ tea

What do you prefer? (mark with a cross)

☐ tofu ☐ tempe

Your parents' mother tongue (mark with a cross if you parents' mother tongue is among the languages below)

☐ Bahasa Ambon, Bahasa Melayu Ternate, Tidore, Kei, Makian,Tobelo, Banda, Tanimbar, Dobo, Kelompok Maluku Barat Daya.

☐ Bahasa Jawa, Bahasa Buton/ Wolio, Bahasa Cia-Cia, Bahasa Bugis, Bahasa Makasar/Bahasa Melayu Makassar, Bahasa Cina.

Please answer the statistical questions now. When you are finished, please fold your sheet and put it in this box. We will meet again next week [date] at [time] here in this room. Please do not forget to come here and keep in mind that you will receive money for your participation on [date], but only if you have attended all the four sessions.

C.2 Second week: Dictator

Thanks again for showing up today to participate in our study. The session today is the longest of the four sessions. The remaining sessions will only take about 10 minutes.
Today you are asked to make two decisions in two different situations. Keep in mind that you will be paid out all the money you receive in the situations on [date].
Please open your folders.

First decision

Please read the instructions on page 1 first. After you have read the instructions, I will explain the situation again and answer your questions if something is not clear. After that you will be asked to answer the questions on page 1. After having answered the questions you will be asked to make the first decision. Please read the instructions on page 1 now. Read them carefully as you will receive additional money if you answer the questions on page 1 correctly (Rp.2,000 for each correct answer). This means it is important that you read the instructions carefully and understand them well.

Instructions

- 2 persons interacting (A and B)
- You are person A
- Interact with another person (B)
- B is not in this room
- Name and identity unknown
- 2 answers

After reading: If you have finished reading the instructions, I will now explain the situation once more. In this first situation, there are two persons (A and B). This time, all of you are assigned the role of person A. Each of you will interact with another person, Person B (one of the 1000 participants) who is most likely not in this room. Each of you interacts with a different person B. You will neither know the name nor identity of Person B, but on page 2 you will be able to see two answers the person gave to the statistical questions last week.

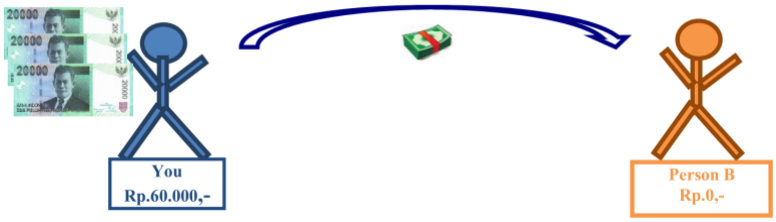
In this first situation, you receive Rp.60,000. You can share this amount with person B if you want (there is no obligation). Person B has not received anything in this situation. This means, if you want, you can give Person B an amount between Rp. 0 and Rp.60,000. In this situation, B has to accept your decision. In other words, person B has no decision to make. Important: Person B will not be informed on the amount received until the final session on [date of last session] when all decisions have been made. Person B will neither know your name nor your identity. In the following situations you will interact with another person. Your personal identification code

First decision situation

1. Read instructions on page 1
2. Listen to oral instructions
3. Answer comprehension questions (page 1)
4. Make decision (page 2)

Instructions

- you receive Rp.60,000

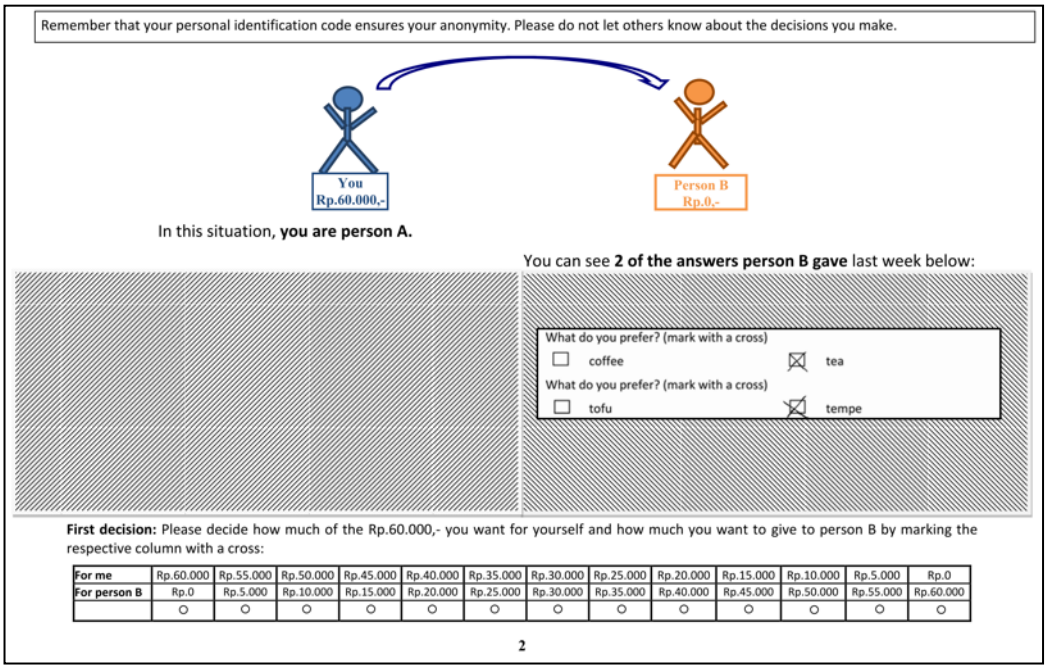


- can be shared with person B (not mandatory)
- person B has no decision
- person B will not be informed on the amount until the final session
- name and identity unknown

protects your anonymity; this means also I will not know your name and identity.

Do you have any questions? If you have understood the instructions, please answer the three questions on page 1. Keep in mind that you will receive an additional Rp.2,000 for each question you answer correctly.

Please turn to page 2 now.



Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.

You
Rp.60.000,-

Person B
Rp.0,-

In this situation, **you are person A.**

You can see **2 of the answers person B gave** last week below:

What do you prefer? (mark with a cross)
☐ coffee ☒ tea
What do you prefer? (mark with a cross)
☐ tofu ☒ tempe

First decision: Please decide how much of the Rp.60.000,- you want for yourself and how much you want to give to person B by marking the respective column with a cross:

For me	Rp.60.000	Rp.55.000	Rp.50.000	Rp.45.000	Rp.40.000	Rp.35.000	Rp.30.000	Rp.25.000	Rp.20.000	Rp.15.000	Rp.10.000	Rp.5.000	Rp.0
For person B	Rp.0	Rp.5.000	Rp.10.000	Rp.15.000	Rp.20.000	Rp.25.000	Rp.30.000	Rp.35.000	Rp.40.000	Rp.45.000	Rp.50.000	Rp.55.000	Rp.60.000
	○	○	○	○	○	○	○	○	○	○	○	○	○

2

On top of the page, you can see your role (person A) and an illustration of the game. Below, you can see two answers person B gave last week in the grey box. For example, in this case you can see that a sample person B likes to drink tea and to eat tempe.

You can see **2 of the answers person B gave** last week below:

What do you prefer? (mark with a cross)

☐ coffee ☒ tea

What do you prefer? (mark with a cross)

☐ tofu ☒ tempe

In the bottom, there is a box for making your decision. Each column represents one option; for each option you can see how much you keep for yourself in the first line and how much you give to person B in the second line. Please decide how much you want to keep for yourself and how much you want to give to person B by marking the respective column with a cross.

For me	Rp.60.000	Rp.55.000	Rp.50.000	Rp.45.000	Rp.40.000	Rp.35.000	Rp.30.000	Rp.25.000	Rp.20.000	Rp.15.000	Rp.10.000	Rp.5.000	Rp.0
For person B	Rp.0	Rp.5.000	Rp.10.000	Rp.15.000	Rp.20.000	Rp.25.000	Rp.30.000	Rp.35.000	Rp.40.000	Rp.45.000	Rp.50.000	Rp.55.000	Rp.60.000
	○	○	○	○	○	○	○	○	○	○	○	○	○

C.3: Second week: Trustor and filling task Trustee

Second decision situation

1. Read instructions on page 3
2. Listen to oral instructions
3. Answer comprehension questions (page 3)
4. Make decision (page 4)

Read carefully => Rp.2,000 for each correct answer.

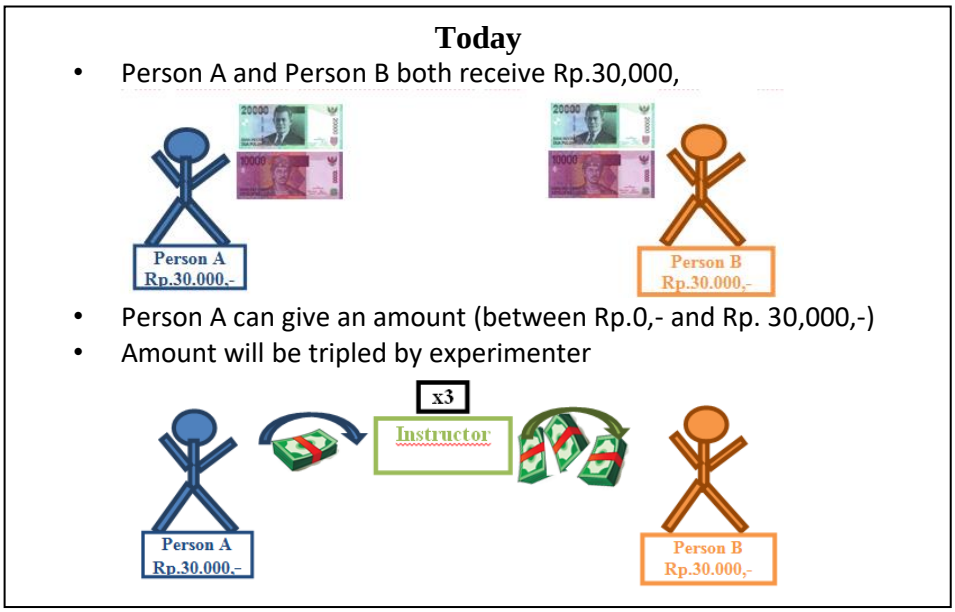
Let us continue with the second situation. Please first read the instructions on page 3 carefully. As in the previous situation, after you have finished, I will explain the situation again before you will be asked to answer the questions on page 3. As in the previous situation you will receive Rp.2,000 for each correct answer.

Second decision situation

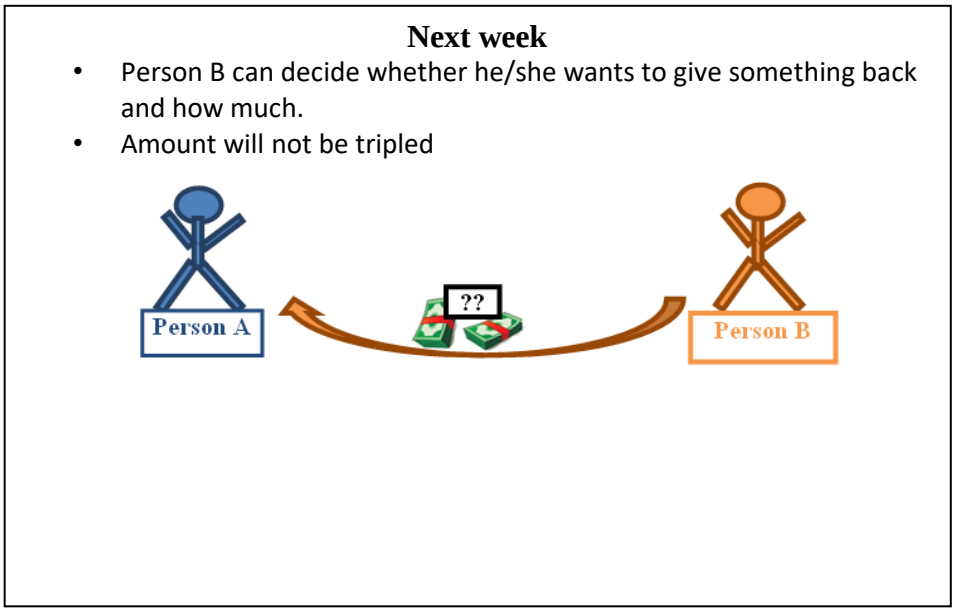
- 2 people interact (A and B)
- Some of you will be person A, some person B
- You can see your role on page 3
- The person you interact with is most likely not in this room
- Different from the person in situation 1
- Money depends on your decision and the other person's decision

In this situation, there are also 2 persons (A and B) who interact. This time, some of the people in this room will be given the role of person A and some will be person B. You can see your role in the first row on page 3. You will interact with another person (one of the 1000 participants) who is most likely not in this room. This time, you interact with a different person, so you definitely do not interact with the person from the first situation. The money you receive does not only depend on your own decision, but also on the decision of the other person.

In this situation, both person A and person B receive Rp.30,000. All of you who have been assigned the role of Person A will be asked to decide whether you want to give an amount between Rp 0,- and Rp. 30,000 to Person B and how much to give. The amount given will be tripled by the experimenter. That means, for every Rp. 1000 Person A gives, Person B will receive Rp. 3000.

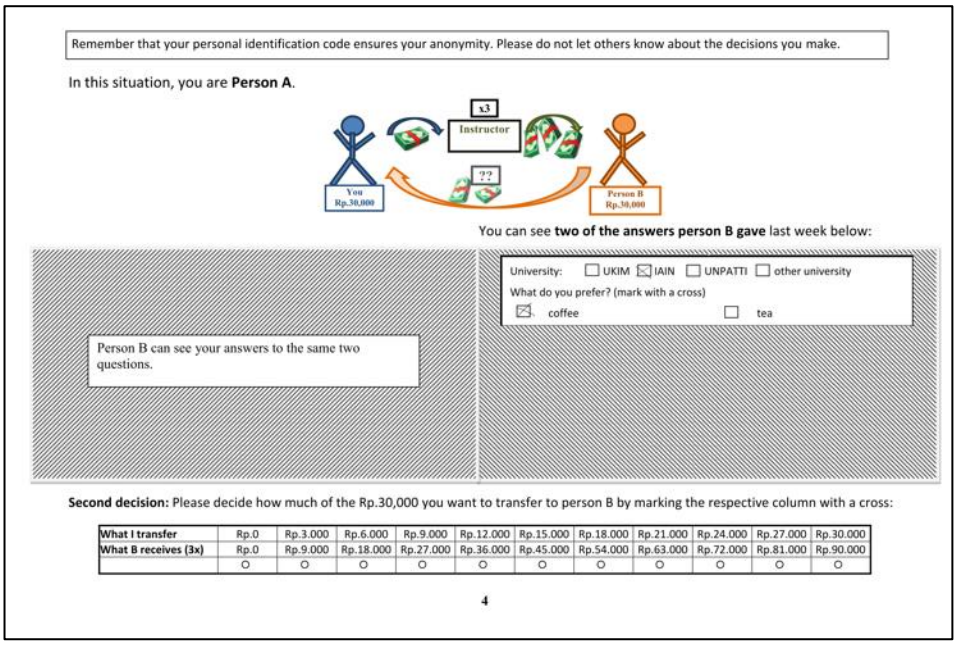


Next week, all of you who were assigned the role of Person B will be asked to decide whether you want to send any amount of the money received back to Person A and, if so, how much. The amount Person B sends back will <u>not</u> be tripled. Do you have any questions? If you have



understood the instructions, please answer the three questions on page 3. Keep in mind that you will receive an additional Rp.2,000 for each question you answer correctly.

If you are ready, please turn to page 4 now.



Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.

In this situation, you are **Person A**.

You can see **two of the answers person B gave** last week below:

University: ☐ UKIM ☒ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☒ coffee ☐ tea

Person B can see your answers to the same two questions.

Second decision: Please decide how much of the Rp.30,000 you want to transfer to person B by marking the respective column with a cross:

What I transfer	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
What B receives (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
	O	O	O	O	O	O	O	O	O	O	O

4

On top of the page, you can again see your role and an illustration of the game. As in the previous situation, you will not find out who the other person is and the other person will not find out who you are. But as in the previous situation, in the grey box below you can see two answers the other person gave last week. The other person will be able to see your answers to the same two questions. That means, if you can see the other person's answer to the coffee/tea questions, the other person will also know whether you prefer drinking coffee or tea.

At the bottom, there is a box to make your decision which differs for person A and B.

For Person A:

Second decision: Please decide how much of the Rp. 30,000,- you want to transfer to person B by marking the respective column with a cross

What I give	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
That means Person B receives (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
	O	O	O	O	O	O	O	O	O	O	O

For Person B:

While Person A is making a decision, we would like to know what you expect person A to do. If you can estimate the amount of money Person A will give to you correctly, you will receive a bonus of Rp. 4.000,-. So, how much will person A give to you?

What I give	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
That means Person B receives (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
	O	O	O	O	O	O	O	O	O	O	O

All of you who were assigned the role of person A are asked to decide how much to transfer to person B. In the first line, you can see the amount you can transfer and in the second line, you can see what Person B receives after the amount has been tripled. Person B will not know who you are and also I will not know which decision was made by whom. All of you who were assigned the role of person B are asked to guess which division of money person A will transfer to you. If your guess is correct, you will receive an additional Rp. 4000,-. Person A will not be informed on your guess.

Do you have any questions? Please make your decision by marking the respective column with a cross.

Thanks for your participation today. Please remember that this was the longest of the four sessions; the two remaining sessions will only take about 10-15 minutes. Next week there will be the last decision situation. Please do not forget to come to this room again on [date] at [time].

C4: Third Week: Trustee

- Situation that was started last week will be continued
- person B makes decision
- person A answers questionnaire

Thank you for attending today's session. Today there is the last decision to be made. Next week, you will be paid out in cash all the money you received in the interaction situations last week and today and the money for your correct answers. Today the interaction situation that was started last week will be continued. Remember that last week all participants who were assigned the role of person A already made a decision, but all the person Bs have not made a decision yet. Because of that, all the participants who were assigned the role of person B will be asked to make a decision while all the person As will be asked to answer a questionnaire.

Please open your folders. If you are person A, you will find a questionnaire in the folder and can start filling it in immediately. You will receive an additional Rp.3,000 if the questionnaire is

Agenda

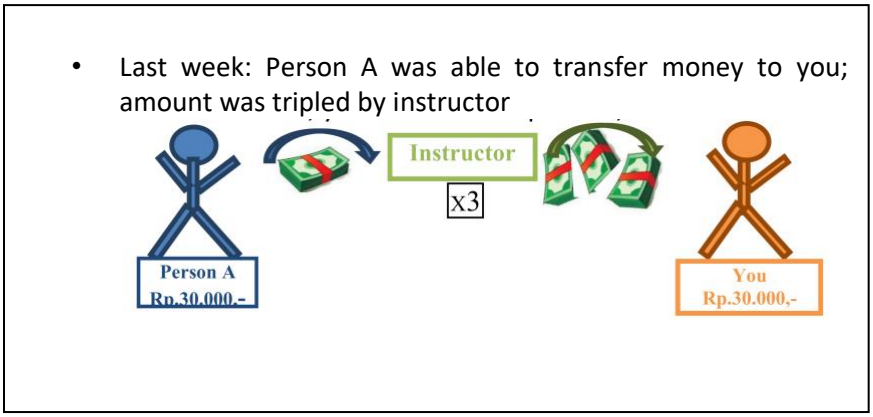
- Person A: questionnaire
- Person B:

1. Read instructions on page 3
2. Listen to oral instructions
3. Answer comprehension questions (page 3)
4. Make decision (page 4)

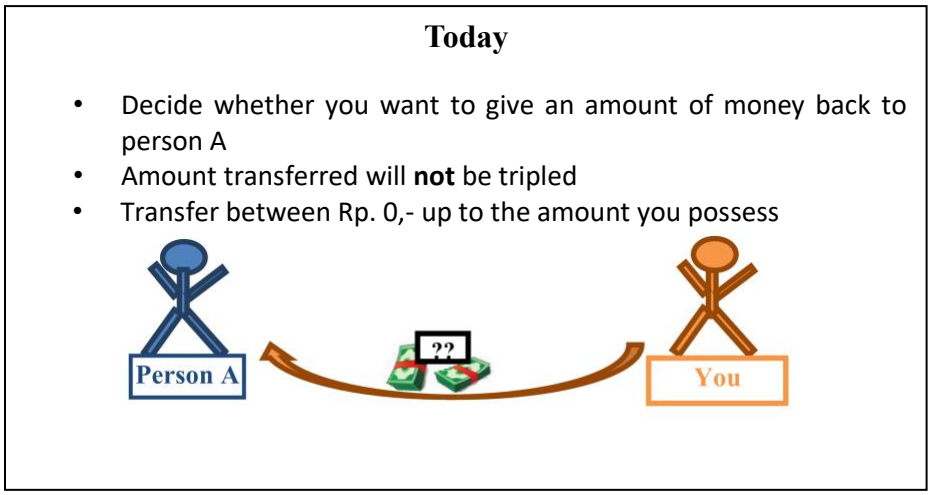
Read carefully => Rp.2,000 for each correct answer.

complete. If you are person B, please first read the instructions on page 1 carefully. After reading, I will explain the situation again before you will be asked to answer the questions on page 1. As last week, you will receive an additional Rp.2,000 for each correct answer.

After reading: For all of you who were assigned the role of Person B last week: We now continue with the interaction situation that was started last week. Remember: Last week, both person A and you were given Rp.30,000.
Person A was asked to decide how much of the sum (an amount between 0 and 30.000) to transfer to you. This sum was tripled by the instructor (me). In other words, for every Rp. 1,000 that player A gave to you, you will receive Rp.3,000. In a moment you will see how much person A decided to give to you.



Today you will be asked to decide whether and how much of the money you possess now you want to give back to person A. The amount you give to person A will not be tripled.



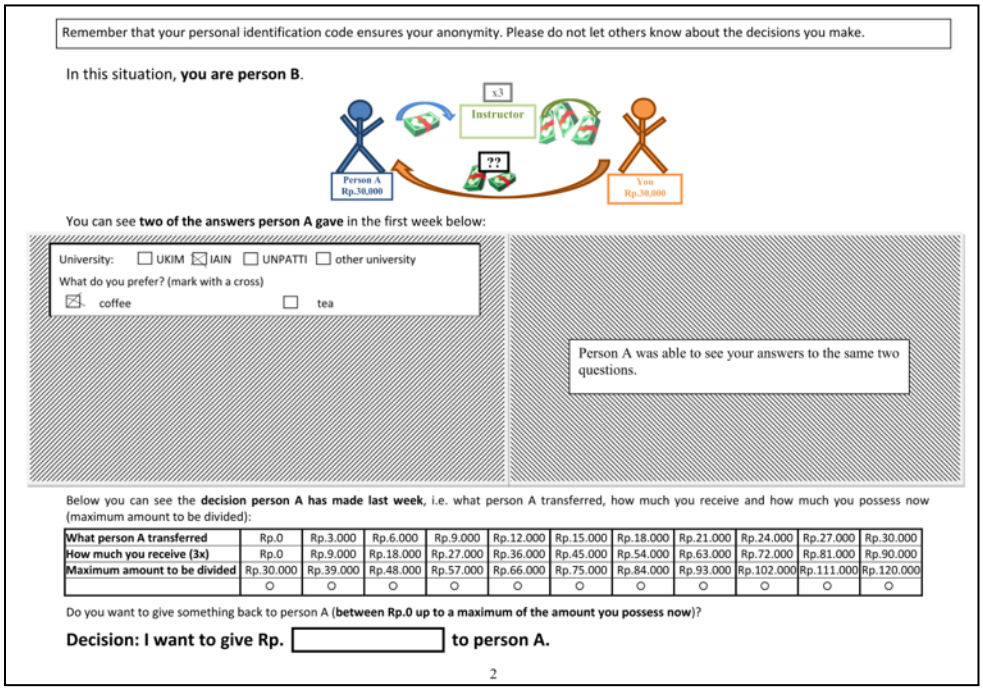
How much do you possess in this situation? Remember that you now have the Rp.30,000 that you received in the beginning of this situation (last week) plus three times the amount person A gave to you. Please open page 2. In the box in the bottom, you can see how much person A gave, how much you receive after the amount is tripled and how much you have in total to divide (including the initial Rp.30,000). [discussion of examples]

Below you can see the **decision person A has made last week**, i.e. what person A transferred, how much you receive and how much you possess now (maximum amount to be divided):

What person A transferred	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
How much you receive (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
Maximum amount to be divided	Rp.30.000	Rp.39.000	Rp.48.000	Rp.57.000	Rp.66.000	Rp.75.000	Rp.84.000	Rp.93.000	Rp.102.000	Rp.111.000	Rp.120.000
	○	○	○	○	○	○	○	○	○	○	○

Please make sure that you have understood the instructions well. If you have already understood everything, please answer the questions on page 1 first. For each correct answer you will receive an additional Rp.2,000.

After answering: Please open page 2 now. On top, you can again see an illustration of the situation. As last week, you will never know who person A is and person A will never know who you are. But as last week you can see two answers person A gave to the statistical questions in the first session. In the box at the bottom, you can see the decision person A made last week, how much money you receive and how much you possess now. Below you can make your decision on how much you want to give back to person A (between Rp.0,- up to the total amount you possess now, including the Rp.30,000,- you received last week). Remember that the amount you give to person A will not be tripled.



Remember that your personal identification code ensures your anonymity. Please do not let others know about the decisions you make.

In this situation, **you are person B**.

You can see **two of the answers person A gave** in the first week below:

University: ☐ UKIM ☒ IAIN ☐ UNPATTI ☐ other university

What do you prefer? (mark with a cross)

☒ coffee ☐ tea

Person A was able to see your answers to the same two questions.

Below you can see the **decision person A has made last week**, i.e. what person A transferred, how much you receive and how much you possess now (maximum amount to be divided):

What person A transferred	Rp.0	Rp.3.000	Rp.6.000	Rp.9.000	Rp.12.000	Rp.15.000	Rp.18.000	Rp.21.000	Rp.24.000	Rp.27.000	Rp.30.000
How much you receive (3x)	Rp.0	Rp.9.000	Rp.18.000	Rp.27.000	Rp.36.000	Rp.45.000	Rp.54.000	Rp.63.000	Rp.72.000	Rp.81.000	Rp.90.000
Maximum amount to be divided	Rp.30.000	Rp.39.000	Rp.48.000	Rp.57.000	Rp.66.000	Rp.75.000	Rp.84.000	Rp.93.000	Rp.102.000	Rp.111.000	Rp.120.000
	○	○	○	○	○	○	○	○	○	○	○

Do you want to give something back to person A (**between Rp.0 up to a maximum of the amount you possess now**)?

Decision: I want to give Rp. ☐ **to person A.**

2

Do you have any questions? Please decide how much you want to give to person A and write the amount in the box below.

That's it for today. We will meet again next week on [date] at [time] for the final session. Next week, you will receive the money you have earned during the entire study in cash. Thank you for your participation and see you next week!

Next week

- Final session [date]

- You will receive the money

C.5: Fourth week: Payoffs

Good morning and welcome to the final session of our study.

Today you will be asked to fill in a questionnaire. After having filled in the questionnaire you will be paid for your participation according to the decisions you have made. When you have finished the questionnaire, please put it in the box and get the envelope with your identification code on it. In the envelope you will find the money and a list of results.

In the result list you can see all decisions you have made and the decisions the persons you interacted with have made, how much money you have received for each situation and how many questions you answered correctly. After reading, please sign at the bottom of the result list and put the part with the signature in this box before leaving the room. The signatures are important for me to prove to a German foundation that I paid out money. I will not look at the signatures and give them to the foundation which will use them for matters of accounting only. We will not be able to match your name with your identification code and thus will not know how much money you received.

Today

- Fill in questionnaire
- Take your envelope with the money and explanations of results
- IMPORTANT: Sign at the bottom of the result sheet and put the part in the box.
- THANK YOU!

Thank you very much for your participation. We hope you enjoyed participating in this study.