Septiembre 2022

WP6-2022-CM4

N° de serie

DOCUMENTO DE TRABAJO

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COLOMBIA CIENTÍFICA
Conocimiento Global para el Desarrollo

ALIANZA EFI
Economía Formal e Inclusiva

Costly Norm Enforcement through Sanctions and Rewards: An Experiment with Colombian Future Police Officers*

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September 1, 2022

Abstract

The increasing lack of trust in the police around the globe reduces their indirect benefits, related to citizens' feelings of safety and beliefs that the police are "doing something" to fight crime. We explore whether this generalized lack of trust among citizens correlates with their beliefs' accuracy regarding fairness norm enforcement in a lab-in-the-field experiment conducted with future police officers. Two hundred nine police students played a dictator-like game with costly third-party reallocation. Participants acting as a third party could use one-fourth of their endowment to either decrease (i.e., sanction) or increase (i.e., reward) the highest payoff among the two other players, the initial allocator and the transfer's recipient. We randomized whether a police student or a civilian was the recipient. Police students transfer roughly 40% of their endowment, regardless of the recipient's identity. They are likely to incur costly reallocations between 55 and 75 percent of the time, especially when initial allocations are more inegalitarian and the recipient is also a police student. Moreover, when police students interact only with in-group members, they are more likely to reward, whereas they are more likely to sanction if the transfer's recipient is a civilian. The subsequent prediction survey, conducted with over 200 civilians, reveals that respondents expected some in-group favoritism in the transfer and in the likelihood to reward. Although the probability of sanctioning was high, respondents overestimated the likelihood that police students engage in costly sanctions. Incentives and reporting a higher trust in the police are correlated with higher predictive accuracy.

Keywords: third-party reallocation; prediction survey; dictator games; public servants

JEL Classification Codes: C90, D63, D73

*We thank the research assistance from Silvia Ortiz and Ferley Rincón, as well as comments from Mariana Blanco and Enrique Fatás. This research was funded by the National Planning Department and the program *"Inclusión productiva y social: programas y políticas para la promoción de una economía formal, código 60185, que conforma la Alianza EFI, bajo el Contrato de Recuperación Contingente No. FP44842-220-2018."* is also gratefully acknowledged. Declarations of interest: None.

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1 Introduction

A principal-agent problem explains the gap between what society desires from its public servants and what these public servants feel obliged to do. The ultimate principals are the citizens, who lack enforcement capacity because their mechanisms to discipline agents, such as elections, are coarser and limited (Finan et al., 2017). This principal-agent problem is partly mitigated by the intrinsic preferences driving workers with a mission toward public service to the public sector, who may exert more effort (Banuri & Keefer, 2016). Nonetheless, in developing countries, selection may also be affected by the (factual or perceived) quality of public institutions. When quality is low, potential workers may expect to benefit from their discretionary power by action (e.g., bribing) or omission (e.g., shirking), or their overseer's omission (e.g., low accountability).

This study measures how willing police officers are to enforce fairness norms. Police officers constitute a public servant type for whom the principal-agent problem has two additional complexities. First, financial incentives need to be more carefully designed because they could lead to inaccurate enforcement with undesired and even tragic consequences (Miller & Selva, 1994; Acemoglu et al., 2020), or they can lead to a reallocation of efforts from non-incentivized to incentivized tasks (Baicker & Jacobson, 2007). Second, police officers and departments are tasked with crime reduction and promoting feelings of safety. At the same time, they need to maintain their legitimacy by minimizing the acts with indiscriminate or biased use of force (Owens & Ba, 2021). Recent evidence reveals that interventions aiming at replacing the fast-thinking and gut-based police behaviors with "slow-down" techniques of procedural justice reduce the use of police force and citizens' complaints (Owens et al., 2018; Wood et al., 2020). Moreover, interventions fostering positive (i.e., non-enforcement) contact with the community improves legitimacy and willingness to cooperate with the security authorities (Peyton et al., 2019; Blair et al., 2022).

The duality in the police objective function is accompanied by a weighting consideration between their direct and indirect benefits. Whereas the former benefits are related to crime prevention, the latter are related to making civilians feel safe and promoting the belief that the police are "doing something" about crime (Owens & Ba, 2021). Paradoxically, most benefits are indirect because only a minor percentage of the population is a direct victim of crime. Hence, legitimacy and trust in the police results are as crucial as crime-fighting activities. In particular, because police-citizen trust is the basis for constructing policies in favor of citizen security that is relevant, sustainable, and transparent (Easterly et al., 2006).

We aim to understand citizens' beliefs about police behavior regarding fairness norm enforcement and the accuracy of such beliefs. To do so, we devise a lab-in-the-field experiment for measuring this costly enforcement and whether police students opt for sanctions or rewards. In a further stage, we exploit the game's simplicity to create an incentivized prediction survey. We conducted this survey among civilians to elicit their beliefs about police students' costly allocations and reallocations, and whether this behavior was different for in-group and out-group interactions, reflecting some in-group favoritism.

First, in our lab-in-the-field experiment, we employ incentivized allocation experiments to measure the willingness to enforce fairness norms. Our dictator-like game involves three players: an initial allocator, a recipient of the allocator's transfer, and a third-party who can reallocate payoffs between the other two participants at a cost (25% of her endowment) to herself. When incurring this costly reallocation, the third-party may opt to decrease the payoff of the player receiving the largest amount (i.e., sanctioning) or increase the payoff of the player receiving the lowest amount (i.e., rewarding). Previous studies show that future police members are more willing to incur these norm enforcement costs, with respect to non-police comparison groups, for France and Germany (Dickinson et al., 2015; Friebel et al., 2019). However, the effect for developing countries is *ex ante* not clear due to negative selection concerns (Finan et al., 2017) and the evidence of poor sanctioning norms in countries with weak norms of civic cooperation and weakness of the rule of law (Herrmann et al., 2008). We provide evidence for Colombia, where police students are more willing to incur third-party reallocation costs than a comparison sample of university students and slightly less willing to incur these costs compared to a (small) group of experienced police members.

Allowing third-parties to either increase or decrease others' payoffs interacts with our treatment variation: the identity of the passive recipient of the allocators' transfer, who could be another police student (i.e., an in-group member) or a civilian (i.e., an out-group member). This variation is helpful in our design to detect in-group favoritism among police students. We do not observe in our experiment the primary form of favoritism, transferring more to their in-group. However, we find some evidence of selective norm enforcement at the extensive (i.e., more norm enforcement in the in-group) and intensive (i.e., conditional on enforcement, more sanctions than rewards in the in-group) margins.

Second, our incentivized prediction survey aims to understand citizens' beliefs about (future) police officers' attitudes toward enforcing fairness norms. The relevance of these beliefs dwells on the indirect benefits of police legitimacy and citizens' trust in their work. Given the low trust levels in the Colombian National Police (CNP hereafter), we aimed to employ the accuracy of behavior predictions in our simple game to infer how citizens expect police members to react in front of opportunities for fairness norm enforcement. We asked survey respondents about their predictions regarding *(i)* transfers from the initial allocator and *(ii)* the likelihood of modifying initial allocations through sanctions or rewards. These predictions are elicited by treatment (i.e., whether the recipient was another police student or a civilian) and by the allocator's gender. We aim to detect beliefs about favoritism with separate answers by treatment and whether gender was a mediator of expected punishment behavior. We randomized the prediction incentives between payment for performance, where respondents earned a higher payment if they had a prediction score above the median, and payment for participation, where incentives were not correlated with the quality of guesses.

Survey respondents overestimated the likelihood that police students invest in costly payoff reallocations. In particular, this is driven by an expectation of a very high likelihood of punishment, which was already large but still overestimated. Respondents also expected in-group favoritism in transfers and the choice of rewards over sanctions. Nevertheless, the evidence matching these predictions was scarce. Regarding the accuracy of predictions, perhaps not surprisingly, incentives work: payments for accuracy increased this dimension by 0.28 standard deviations. What we find surprising, on the other hand, is that having studied in a public university has roughly the opposite result: it reduces accuracy by 0.25 standard deviations. We conjecture that narratives of antagonism between the police and public university students may have persistent consequences on how the latter perceived the behavior of the former.

We also contribute to the literature using experimental economics paradigms in studies involving public servants as participants. Dickinson et al. (2015) and Friebel et al. (2019) show that police officers (and applicants) in France and Germany are more likely to invest in norm enforcement. Harris et al. (2022) show that a police training program in Ghana, aiming to create a new collective identity in the force, lowered the officers' propensity to behave unethically. This behavior was measured with an incentivized cheating game (in addition to improved attitudes toward the citizens). Joseph & Miquel-Florensa (2022) employ threshold public games with employees from the water sanitation authority in Addis Ababa. The game remarked on the importance of collaboration within the crews and explored the ability to reach a coordination equilibria. In Colombia, Cardenas & Sethi (2010) use a "distributive dictator game" where public servants from different offices must assign differential probabilities of receiving a sum of money to actual program beneficiaries. Their findings reveal that some beneficiaries' attributes, such as sex and being a victim of violence, increase prioritization in the rankings.

This paper proceeds as follows. Section 2 provides details on the context of the CNP and the current low trust levels from the citizens. Section 3 describes the experimental design of the main reallocation game and the associated hypotheses and implementation procedure. Section 4 displays the game results. Section 5 introduces the experimental design, and the results of the prediction survey carried out with civilians, using the results of the reallocation game. In Section 6, we summarize our findings and discuss the interplay between observed police students' behavior and the citizens predicted behavior.

2 Context

Seven out of ten people worldwide trust in their local police, though these results vary significantly by region. For instance, people in Latin America and the Caribbean have been the least likely (49%) to trust their local police (Gallup, 2020b). Some reasons explaining this lack of trust include police violence in protest scenarios and the viralization on social networks and digital media of police abuse cases ((Sun et al., 2016); (Verduzco, 2017); (Wu & Sun, 2009). It has also been found that the lack of trust represents a lack of generalized trust in the society as a whole (Kääriäinen & Sirén, 2012), as well as the absence of institutional transparency (Rothstein & Teorell, 2005), among others.

Colombia is an example of this lack of trust in Latin America: sixty percent of the citizens do not have a favorable view of its police (Gallup, 2020a). A short-term explanation is a legitimacy crisis experienced in the last years (Arias et al., 2019), partly nourished by the escalation of police violence during several days of protests in 2021[1] and the increase in social media exposure (Prieto et al., 2020). A long-term explanation is that, unlike in many other countries, the CNP has a militarized organizational and service dynamics rather than a civilian nature (Rodríguez, 2018).

[1]See international coverage here: `https://www.bbc.com/mundo/noticias-america-latina-57016978` and `https://www.nytimes.com/es/2021/05/12/espanol/protestas-colombia-policia.html`

Thus, the same officer can be assigned to rural or urban areas, but also to tasks of domestic violence reports or countering armed groups. The duality in the military and civic duties contrasts with the predominant military training, which may create tolerance toward human rights violations and reinforces the lack of public trust that hinders collective action between officers and citizens (León, 2019; Jímenez & Turizo, 2011).

This systematic loss of confidence negatively impacts crime prevention and reactive policies in Colombia (Blattman et al., 2022; Godoy et al., 2018). Citizens are a fundamental actor in constructing preventive strategies (Braga et al., 2014; Collazos et al., 2020; Ham et al., 2022). However, if the police-citizen relationship gets weakened, the strategic role of citizens as an allied source of information is threatened and relegated (Blair et al., 2022). As a response, CNP has made efforts to improve their image as trustworthy among the citizens (Gelvez-Ferreira, 2018). For instance, the Colombian State has encouraged citizen participation in the police's recent transformation,[2] made public bodies more locally accountable and responsive, and has also implemented laws for human rights protection (Congreso de Colombia, 2016).

Regarding selection, to become a police patrol person (*patrullero* or *patrullera*, the initial rank in the institution), a candidate requires specific demographic, physical, and academic characteristics. They include taking physical and academic exams and not having been criminally convicted or being linked to human rights violations investigations.[3] Even though scholars have found that active police members prioritize work values associated with supervision, workplace, safety, achievements, and their co-workers (Basinska & Dåderman, 2019), little is known about the motivations and behaviors of future police officers in Colombia. For their initial training, police patrol persons spend their first 12 months in dedicated boarding schools, where they are trained in four areas: police affairs, Constitution and law, human science, and research (Serrano Daza, 2021).

3 Experimental paradigm for the lab-in-the-field experiment

We measure the intentions to redistribute resources in a dictator game when a third-party's costly reallocation decision follows the first mover's decision. This third-party can either *decrease* the payoff of the participant with the highest allocation, *increase* the payoff of the participant with the lowest allocation, or *do nothing*. The two reallocation decisions are costly: the third-party spends one fraction of her endowment to increase (resp. decrease) another player's payoff by twice the spent amount. We are interested in understanding *(i)* allocation decisions and *(ii)* investments in reallocation from police students. Previous studies in France and Germany remarked a higher willingness to enforce social norms in economic experiments among police-people in formation and applicants to the police force, compared to non-police related participants (Dickinson et al., 2015; Friebel et al., 2019).

Reallocation or dictator-like games help understand fairness norms when they conflict with self-interested motives (Henrich et al., 2001; Krupka & Weber, 2013). For instance, Engel's (2011) meta-analysis on dictator games reveals that a 50-50 split of the endowment is the modal choice

[2] See `https://transformacion.policia.gov.co/`

[3] See `https://www.policia.gov.co/incorporacion/nivel-ejecutivo/bachiller`

for non-students. Regarding norm enforcement, costly sanctioning of unfair behavior is common across different animal species when they are directly affected. However, third-party sanctions are a particular trait of humans in response to norm violations (Fehr & Fischbacher, 2003, 2004). Moreover, sanctioning could involve direct punishment or the withholding of rewards to norm-violators (Balafoutas et al., 2014), and both effectively enforce norms of cooperation and fairness (Balliet et al., 2011).

Whereas the focus in Dickinson et al. (2015) and Friebel et al. (2019) dwells on cooperation, trust and trustworthiness, we employ a more straightforward allocation game because, in the second part of our study, we elicit beliefs from non-police participants on the police students' behavior. As in Dickinson et al. (2015), we introduce rewards in our setting since they may amplify the differences in responses from police students when facing an in-group or an out-group (i.e., a civilian) participant. Unlike their study, sanctions and rewards are not treatment variations but rather constitute the actions in the choice set of the third-party in our game, together with the option of doing nothing. We opted for this game variation to study if sanctions or rewards were more likely to be chosen depending on the group composition, given the potential concerns of exposing group members in front of an out-group (Mantilla et al., 2021; Eriksson et al., 2017). Below, we explain the game in detail.

3.1 Experimental design

The game

There are three players per group, labeled A, B, and C. Player A is the first mover and must choose how to allocate an endowment of 75 kCOP[4] between Player B and herself. There are four predefined available allocations, in which Player A could keep $x_i \in \{25, 35, 45, 55\}$, and the rest of the endowment will be transferred to Player B. In these allocations, Player A keeps 33, 47, 60, and 73 percent of the initial endowment. We eliminate the 50-50 split to make the deviation from this egalitarian norm less prominent and avoid more sanctioning behavior for the remaining choices.

Player C observes Player A's allocation, and makes a reallocation decision, labeled as $y_i \in \{\text{(N)othing}, \text{(S)anction}, \text{(R)eward}\}$. When Player C chooses "(N)othing," Player A's original allocation is preserved, and Player C obtains a final payoff equal to her endowment of 40 kCOP. When Player C chooses "(S)anction," she will reduce the payoff of the participant with the largest earnings between Players A and B in 20 kCOP, at a cost to herself of 10 kCOP (yielding 30 kCOP as her payoff). Finally, when Player C chooses "(R)eward," she will increase the participant's payoff with the lowest earnings between Players A and B in 20 kCOP. Again, this will inflict a cost to herself of 10 kCOP in her final payoff.

Table 1 reports, in parenthesis, the resulting outcomes from Player A and C's decisions in the form of vectors indicating the earnings of Players A, B, and C, in that order. The underlined numbers correspond to the earnings modified after Player C decides to sanction or reward. Below each payoff vector, we report its Gini coefficient in squared brackets. This measure of inequality is helpful in understanding when inequality-aversion motives might drive Player C's investment

[4]By the time of the experiment, this amount corresponded to 19 USD (1 USD = 3,900 COP) and it was 2.5 times the Colombian daily minimum wage.

Table 1: Payoff vectors dictating earnings of Players A, B, and C.

Player A	Player C decision, y_i		
keeps… (x_i)	Nothing	Sanction	Reward
25	(25, 50, 40)	(25, $\underline{30}$, 30)	($\underline{45}$, 50, 30)
	[0.145]	[0.039]	[0.107]
35	(35, 40, 40)	(35, $\underline{20}$, 30)	($\underline{55}$, 40, 30)
	[0.029]	[0.118]	[0.133]
45	(45, 30, 40)	($\underline{25}$, 30, 30)	(45, $\underline{50}$, 30)
	[0.087]	[0.039]	[0.107]
55	(55, 20, 40)	($\underline{35}$, 20, 30)	(55, $\underline{40}$, 30)
	[0.203]	[0.118]	[0.133]

Underlined payoffs correspond to Player A or B's modified earnings after Player C's sanction or reward. Gini coefficient from each allocation reported below, in squared brackets.

in reallocations. Note that the costly actions from Player C will reduce the Gini coefficient only when Player A chooses an extreme allocation (either keeping 25 or 55 kCOP).

Although Player B does not take any action (technically, she is not even a "player" for not being a strategic decision-maker), she might be sanctioned when Player A chooses $x_i \in \{25, 35\}$. This punishing behavior from Player C might be explained by inequality aversion motives when $x_i = 25$, but not when $x_i = 35$. On the other hand, Player C's rewarding behavior when Player A chooses $x_i \in \{25, 35\}$ is also puzzling. It suggests that the expectation of a future reward can explain Player A's altruism. Both situations may signal a willingness to incur costly redistribution where outcomes appear to dominate intentions: punishing a third-party taking no strategic decision (Player B) or rewarding a player deliberately opting for a disadvantageous allocation (Player A).

We finally remark on one feature of the attainable payoffs through reallocation: if Player C sanctions or rewards, $x_i = 25$ and $x_i = 45$ offer identical payoff vectors despite the initial transfer from Player A being different. A similar situation also occurs for payoff vectors obtained from $x_i = 35$ and $x_i = 55$. This payoff configuration would be helpful better to understand the reasons behind incurring costly reallocation by disentangling motives from outcomes.

Introducing exogenous variation in the identity of Player B

The third-party choice between sanctions and rewards allows us to test whether parochial altruism, the joint tendency of in-group favoritism and out-group harm (Bernhard et al., 2006; Choi & Bowles, 2007; De Dreu et al., 2010), is common among police students. We introduced exogenous variation in providing information about the identity of Player B, which could be either another police student or a civilian. We did not vary the identities of Players A and C, and it was common knowledge that they were police students. We fixed these roles to increase the credibility of our payment procedure: all the decisions required to compute the payoffs were taken during our visits to the police students' campuses, and payoffs to civilians could be implemented *ex post*.

Moreover, these fixed roles also minimize our concern for higher-order strategic interactions (i.e., different combinations of player types in the roles of players A, B, and C).

We thus call our treatments *B-Police* and *B-Civilian*, referring to the revealed identity of Player B. We are interested in three potential patterns identifiable by comparing Player A's and C's behavior across treatments. First, whether Player A's transfer is on average larger in the *B-Police* treatment compared to the *B-Civilian* treatment. This pattern would indicate more in-group favoritism in the initial allocation. Second, whether norm enforcement is more likely to occur in the *B-Police* treatment compared to the *B-Civilian* treatment. A higher enforcement of fairness norms when Player B is an in-group member would reveal a preference for correcting payoff allocations for the in-group, a signal of favoritism. Nonetheless, we acknowledge that the expected behavior can be more complex because Player C can opt for sanctions or rewards. For instance, we may observe similar reallocation levels, but more rewards in in-group allocations. We translate this list of expected behaviors into the following hypotheses:

- **H1.** *Player A's transfers are larger in the B-Police than in the B-Civilian treatment.*
- **H2.** *Player C is more likely to invest in payoff reallocations in the B-Police than in the B-Civilian treatment.*

Note that we merged the expected sanctioning and rewarding behaviors into a single hypothesis, **H2**. The reason is that both behaviors align with Player C's incurring costly decisions. Econometrically, it is much simpler to test (given our limited sample) any investment in reallocation versus a null investment. Nonetheless, we will add evidence to the discussion on whether sanctioning or rewarding behavior drives the results.

Design aspects on data collection

To maximize the number of observations, every participant made decisions in two roles: Player A and Player C. Moreover, we employed the strategy method for Player C: *conditional on* each allocation that Player A could make, Player C chose to sanction, reward, or do nothing. The strategy method was also employed in previous experiments with police applicants and police students involving trust games (Dickinson et al., 2015; Friebel et al., 2019).

In the written and oral instructions, we informed participants that they would make choices as Player A and Player C and that, to compute payments, they would be assigned to any of the three roles, Player A, B, or C. Participants randomly assigned to the *B-Police* treatment were grouped in triads, and each group member was then assigned to one of the roles. Participants in the *B-Civilian* treatment were grouped in pairs, and each group member was then assigned to the role of either Player A or C. The payments for Player B in this treatment were further assigned to students at Universidad del Rosario in a subsequent experiment.

3.2 Implementation

Before implementing Study 1, we ran a pilot with 28 police officers on July 27th, 2021. These police officers had at least 20 years of experience within the Police force by the time of the pilot study. They were together within an officers' police school for several months as part of their promotion course. Our objective was to test the experimental design's timeline, logistics, and instructions. Given the reduced sample size and our ability to implement payoffs at that time, we only implemented the *B-Police* treatment.

At the end of 2021, we conducted two large experimental sessions with police students that, by that time, had spent between six and twelve months in a National Police school. The first session took place on December 1st with 110 subjects identified as male, and, two days later, another session with 100 subjects identified as female. Each session was conducted at a different National Police school in Cundinamarca, Colombia. The male and female Police Schools are 32 and 45 miles away from Bogotá. However, they gather students born and raised in several Colombian regions. Although both police schools are in the same department, they are approximately 60 miles from each other. Given this distance, and since the CNP's internal rules limit their access to a cellphone, our concerns regarding information transmission between sessions are minimal.

The following procedure describes the timing of each session. First, we explain the game, using audiovisual material, to all the participants in a single room in each Police School. Second, we provide access to the online instrument, completed individually, using a laptop or a mobile phone. In the male Police School, most participants responded to our instrument from a laptop; in the female Police School, the majority employed their mobile phones. The reason was an unreliable internet connection for the laptops at the time of the experiment. An additional difference between the two sessions is that, after receiving the oral instructions, women were divided into rooms of 60 and 40 participants. The reason was that the main room was smaller than in the male school. Third, we computed the payments for each participant and paid them individually (i.e., each one was called outside the room and received an opaque envelope with the respective earnings).

Each session lasted approximately 40 minutes. This time comprises around 15 minutes of delivering the instructions and another 25 minutes while they respond to the instrument. They waited about 60-70 additional minutes in the room, while we computed their earnings and called them individually to deliver their payment, which was, on average, 38 kCOP (about 9.75 USD at the time of the experiment).

Three months after the main sessions conducted with police students, on March 2022, we conducted two additional sessions with a total of 32 undergraduate students from Economics and Finance majors from Universidad del Rosario. We conducted these sessions in two Microeconomics courses without previous notice for the students. The purpose was to minimize self-selection and replicate the environment participants may know each other, even though they would not know the matching within the experiment.

4 Results

4.1 Player A's transfer

We find that police students in the role of Player A transferred on average 29.29 kCOP to Player B. Panel A, in Table 2, reveals the frequency for each allocation within a row. A transfer of 30 kCOP is modal (44%), and very close to the average. It is followed by the transfers of 20 and 40

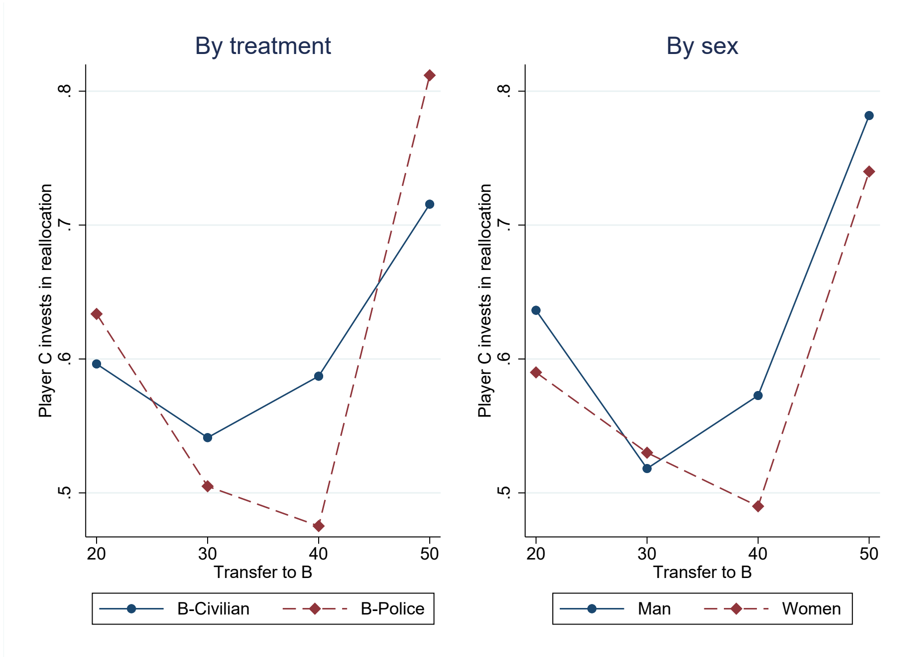

Figure 1: Probability that Player C invests in payoff reallocation

kCOP (34 and 17%, respectively). In this panel, we also show the lack of treatment differences in the frequency of choices. A $t-$test comparing the average transfer yields similar results: 29.4 in *B-Police* and 29.2 in *B-Civilian* ($p-$value 0.842). Although this is not an experimental variation, we also show in this table the differences in transfers between female (29.6) and male (29.0) police students. We do not find differences in frequencies nor in average transfers ($t-$test with $p-$value 0.605).

Result 1: *Player A's transfers do not differ between the B-Police and B-Civilian treatments.*

We thus reject H1, as we do not find evidence of the in-group favoritism expected among police students in their initial allocation.

4.2 Player C's decision to reallocate

We find that, for every potential allocation made by Player A, most participants in the role of Player C invested in payoff reallocation. Figure 1 reveals that the likelihood of reducing their payoff by 10 kCOP to punish or reward another player by 20 kCOP has a U-shape, which is preserved if we split the responses by treatment or sex. As one would expect, the more unequal the allocation between Player A and Player B, the higher the likelihood of the costly reallocation. However, it is surprising that Player C is much more likely to invest in payoff reallocation when Player A transfers the largest possible amount to Player B.

Table 2: Share of observed choices for Player A and Player C (by treatment and sex)

	Player A's Transfer to Player B [kCOP]			
	20	30	40	50
	(kept 55)	(kept 45)	(kept 35)	(kept 25)
Panel A. Player A's transfer to Player B				
Overall	0.338	0.443	0.171	0.048
By treatment (χ^2 test $p-$value 0.982)				
B-Police	0.327	0.455	0.168	0.05
B-Civilian	0.349	0.431	0.174	0.046
By sex of police student (χ^2 test $p-$value 0.756)				
Player A Woman	0.31	0.46	0.19	0.04
Player A Man	0.364	0.427	0.155	0.054
Panel B. Player C's reallocation decision				
Overall				
Reward	0.395	0.278	0.335	0.481
Sanction	0.219	0.244	0.196	0.281
Nothing	0.386	0.478	0.469	0.238
By treatment				
B-Police: Reward	0.386	0.320	0.327	0.515
: Sanction	0.248	0.180	0.149	0.297
: Nothing	0.366	0.500	0.524	0.188
B-Civilian: Reward	0.404	0.239	0.343	0.450
: Sanction	0.193	0.303	0.241	0.266
: Nothing	0.403	0.458	0.416	0.284
χ^2 test ($p-$value)	(0.622)	(0.098)	(0.165)	(0.261)
By sex of police student				
Player C Woman: Reward	0.380	0.280	0.380	0.450
: Sanction	0.210	0.250	0.110	0.290
: Nothing	0.410	0.470	0.510	0.260
Player C Man: Reward	0.409	0.275	0.294	0.501
: Sanction	0.227	0.239	0.228	0.273
: Nothing	0.364	0.486	0.479	0.226
χ^2 test ($p-$value)	(0.788)	(0.970)	(0.011)	(0.663)

Returning to the comparison between treatments, the left panel in Figure 1 reveals that the likelihood that Player C invests is higher under *B-Police* than under *B-Civilian* for the most extreme allocations. By contrast, in the slightly disadvantageous allocation for Player B (i.e.,45-30), the likelihood that Player C invests is minimal under *B-Police*. Panel B in Table 2 reports, for each allocation, whether the differences between treatments are statistically significant when the three choices of Player C are treated separately. The differences are marginally significant when Player A transfers 30 kCOP.

On the other hand, the right panel in Figure 1 reveals that males are generally more likely to invest in payoff reallocation in their role of Player C. Panel B in Table 2 reports whether female and male police students in the role of Player C differ in their choices. We find a statistically significant difference only when Player A transfers 40 kCOP. However, this difference is explained by the higher (resp. lower) likelihood of women to reward (resp. sanction) compared to men.

Following the insights from Figure 1, we run a linear probability model to test whether Player C's likelihood to reallocate payoffs varies between treatments when the allocations between Player A and Player B are more unequal. Table 3 reveals that Player C is one percentage point (pp hereafter) more likely to invest in reallocation for each 1 kCOP of payoffs difference between Player A and Player B (see column 1). The interaction between this absolute payoff distance and the treatment variable reveals that this higher likelihood to reallocate increases to 1.5 pp per 1 kCOP of difference, but only in the (omitted) *B-Police* treatment (see column 2). In the B-Civilian treatment, the slope of 0.3 pp per kCOP of difference is not statistically different from zero. This result favors H2, but it does not disentangle rewarding from sanctioning behavior.

In column 3, we added some individual controls to better understand the relationship between reallocations as Player C and social norms. We find that participants are 8pp less likely to reallocate payoffs for the outcome they chose in the role of Player A. This behavior is a signal of consistency in their choices across different roles. We asked participants about their preferred allocation, but this variable does not seem to predict reallocations. Our explanation is that the modal response was the most selfish allocation (i.e., keeping 75 kCOP), chosen 39% of the time. By contrast, their beliefs about other participants' preferred allocation are negatively correlated with incurring costly reallocation. For the allocation that they consider to be the others' preferred, they are 17 pp less likely to reallocate. Our interpretation is that if one believes that others prefer a given allocation, there is a lower need to invest in a reallocation because there is no transgression of a fairness norm.

An attentive reader may wonder whether collinearity problems yield a null coefficient for the self-reported preferred allocation. We argue that this is not the case, as the Spearman's correlation between the preferred and the others' preferred allocation is relatively low (0.13, $p-$ value 0.0003). Moreover, we added the interaction between these two terms to prevent this prediction may be driven by a bias where the participant's preferred allocation drives beliefs about others' preferred allocations. This interaction term's lack of statistical significance indicates that the effect of beliefs about others' preferred allocation does not capture this bias.

Finally, as an external validity check, we include as an additional control a measure of negative reciprocity borrowed from Falk et al. (2018).[5] We find that one unit increase (on a 1-10 Likert scale)

[5]The question reads "How willing are you to punish someone who treats you unfairly, even if there may be costs

Table 3: Likelihood that Player C reallocates payoffs as a function of the payoff distance to an egalitarian split between Player A and Player B

	Dependent variable: Player C reallocates payoffs		
	(1)	(2)	(3)
$\lvert$Payoff distance from 50-50 split$\rvert$	0.010***	0.015***	0.016***
	(0.003)	(0.005)	(0.005)
Treatment: B-Civilian	-0.003	0.114	0.120
	(0.037)	(0.075)	(0.077)
$\lvert$Payoff distance$\rvert$ $\times$ Treatment		-0.012*	-0.012*
		(0.006)	(0.006)
Male (police student)	0.040	0.031	0.021
	(0.037)	(0.075)	(0.077)
$\lvert$Payoff distance$\rvert$ $\times$ Male		0.001	0.002
		(0.006)	(0.006)
Allocation as Player A			-0.078**
			(0.039)
Preferred allocation			0.016
			(0.046)
Others' preferred allocation			-0.168***
			(0.048)
Preferred $\times$ Others' preferred allocation			0.045
			(0.092)
Negative reciprocity: Willing to punish			-0.012**
			(0.006)
Constant	0.493***	0.436***	0.532***
	(0.049)	(0.063)	(0.070)
Observations	840	840	828
R-squared	0.014	0.018	0.054

Clustered standard errors at the participant level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

reduces the likelihood of punishment by 1.2 pp (see column 3). In contrast, the other coefficients of interest remain almost identical to the previous specification. At first sight, this result sounds counter-intuitive because participants reporting higher negative reciprocity are less likely to incur a costly reallocation of payoffs in the role of Player C. However, bear in mind that these reallocations may also reward Player A or B, the one with the lowest payoff. This result suggests that efficiency-driven motives mainly guide investments of Player C through a rewarding decision.

We thus partially validate **H2**, but only under considerable payoff inequality between Players A and B:

Result 2: *Player C is more likely to reallocate payoffs in the B-Police treatment as the payoff distance increases.*

We further explore whether police students that invested in reallocation were more likely to reward or sanction and its interplay with the treatment condition. We thus run another linear probability model with a binary dependent variable equal to one for participants that rewarded the participant with the lowest payoff between Player A and B, and zero if they punished the participant with the highest payoff between Player A and B.

Table 4 displays the regression results. We find that police students in the role of Player C are less likely to reward (i.e., more likely to punish) in the *B-Civilian* than in the *B-Police* treatment. However, as the distance to the 50-50 split increases, the treatment difference becomes smaller. Moreover, we find that male police students are less likely to reward than female police students for allocations closer to a 50-50 split.

In columns 2 and 3, we divide the sample into the scenarios where Player A selects an advantageous allocation (i.e., transferring 20 or 30 kCOP) and a disadvantageous allocation (i.e., transferring 40 or 50 kCOP), respectively. We find that the higher likelihood to choose sanctions over rewards in the *B-Civilian* treatment is driven by the choices where Player C punishes Player A for keeping more than half of the 75 kCOP. In particular, by the most egalitarian (though advantageous) choice of transferring 30 kCOP to Player B. In this case, Player C is more likely to reward Player B when she is another police student; but more likely to punish Player A if Player B is a civilian. Hence, group homogeneity evokes efficiency-enhancing behavior through rewards, whereas the presence of an out-group evokes sanctions. Conversely, treatment differences are no longer significant when Player A chooses disadvantageous allocations.

The following result summarizes some insights regarding the choice of sanctions and rewards, depending on Player B's identity:

Result 3: *Player C is more likely to sanction when Player B is a civilian and to reward when Player B is a police student, but only for Player A's advantageous allocations.*

---

for you?"

Table 4: Likelihood that Player C rewards Player A or B with the lowest payoff (instead of punishing Player A or B with the highest payoff). Participants who did not invest in reallocation are excluded from the regression.

	Likelihood that Player C rewards (excluded category is C's likelihood to punish)		
		Player A's selected inequality	
		Advantageous	Disadvantageous
	(1)	(2)	(3)
|Payoff distance from 50-50 split|	-0.012*	-0.001	-0.021**
	(0.006)	(0.010)	(0.010)
B-Civilian	-0.179**	-0.409**	-0.102
	(0.088)	(0.171)	(0.113)
|Payoff distance| × B-Civilian	0.013*	0.027**	0.007
	(0.007)	(0.012)	(0.012)
Male (police student)	-0.188**	0.068	-0.328***
	(0.089)	(0.171)	(0.112)
|Payoff distance| × Male	0.015**	-0.004	0.030**
	(0.007)	(0.012)	(0.012)
Constant	0.784***	0.633***	0.871***
	(0.076)	(0.144)	(0.093)
Observations	509	238	271
R-squared	0.017	0.035	0.033

Clustered standard errors at the participant level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.3 Intentions and outcomes

Our explanation of the payoff vectors in Table 1 remarked that, for the initial allocations of Player A $x_i \in \{25, 45\}$, the final payoffs would be the same if Player C sanctions (25,30,30) or rewards (45,50,30), and an analogous situation occurs for $x_i \in \{35, 55\}$. Since we use the strategy method, we can test if our participants cared about other players' intentions or whether they only cared about the outcome. If the latter is true, one would expect that the likelihood of sanctioning and rewarding would be relatively the same conditional on Player A choosing $x_i = 25$ or 45 (and similarly for Player A choosing $x_i = 35$ or 55). If, by contrast, intentions matter, one would expect differences in the proportion of sanctioning and rewarding.

Table A.3 reveals that the distribution of Player C's choices differ between the scenarios with $x_i = 25$ and 45 (see Panel A), and those with $x_i = 35$ and 55 (see Panel B). This difference is statistically significant regardless of whether we include the costless (do "Nothing") action or not. Hence, as one would expect, intentions matter: the frequencies of costly reallocations differ between scenarios with identical payoff vectors obtained from different Player A's allocations. A less evident result is that the differences are driven by rewarding behavior: this choice was made 48 and 28 percent in the scenarios with $x_i = 25$ and 45, respectively; and 47 and 40 percent in the scenarios with $x_i = 35$ and 55. By contrast, the probability of sanctioning is similar between comparable scenarios: 28 and 24 percent for $x_i = 25$ and 45, and 20 and 22 percent for $x_i = 25$ and 45, respectively.[6]

4.4 Comparison with other samples

We mentioned in Section 3.2 that we applied the same instrument to 28 experienced members of the CNP and to a group of 32 undergraduate students. Although both samples are relatively small, they are helpful for a comparative analysis. Whether police students behave more similarly to experienced police officers or undergraduate students (in Economics or Finance) of their same age will shed light on the importance of self-selection and the reinforcement of behaviors related to norm enforcement. Moreover, we argue that having police officers in our pilot who were part of a promotion course is not a weakness but rather a strength for our comparison. Any social desirability bias triggered by being in an environment where an instructor could judge group or individual behavior is similar across the three samples.

Regarding the transfer as Player A, police officers transfer more (34.2 kCOP) than police and university students (29.3 and 27.5 kCOP, respectively). The OLS regression results, reported in column 1 of Table 5, reveal that police officers transfer 5 kCOP more (after controlling for other factors). In this table, the variable "Player B is in-group" corresponds to the between-treatment difference in the transfer for police students, which we already knew was not different from zero. When B is an out-group, university students transfer less compared to police students. However, there are no differences between these two populations in their transfer when B is an in-group.

We also compare the reallocation decisions across samples when participants were in the role of Player C. Figure 2 reports the likelihood that Player C invests in reallocation, for each treatment condition and each sample. The left panel reveals that experienced police officers are more likely to invest in reallocations than police students. For police officers, we observe a more pronounced U-shape, revealing that reallocations are very likely to occur (about 90% of the time) when Player A's initial allocations are more extreme. The comparison between panels confirms the results from studies involving police students and applicants in France and Germany: they are more likely to invest in costly norm enforcement compared to reference populations (Dickinson et al., 2015; Friebel et al., 2019). This result is accurate except for the most generous transfer from Player A, which led to a 72% chance of reallocation among university students, a value comparable to the one observed for the police students. For the less generous allocations, university students are less likely to reallocate payoffs: it goes from 47 percent when $x_i = 20$ kCOP, to 25 and 28 percent when x_i increases to 30 and 40 kCOP, respectively.

Columns 2 and 3 from Table 5 complement these findings. Controlling for the absolute distance to the egalitarian payoff split between Players A and B, police officers are 13 pp more likely to reallocate than police students; and university students are 17 pp less likely to reallocate than police students. In column 3, adding the interaction between university students and the distance to the egalitarian payoff decreases their likelihood to reallocate to 29 pp (compared to police stu-

[6]Table A.3 also reveals that the proportion of players rewarding across the two comparable scenarios (with respect to those who reward in only one) is much larger than the equivalent proportion of players sanctioning in both scenarios.

Table 5: Comparison across samples for Player A's transfer and Player C's reallocation decision

	(1)	(2)	(3)
	Dependent variable:		
	Player A's transfer	Player C reallocates payoffs	
Player B is in-group	0.167	0.009	-0.100
	(1.173)	(0.036)	(0.068)
Police Officers (Pilot)	4.967***	0.134**	0.108
	(1.888)	(0.056)	(0.107)
University Students	-3.638*	-0.174***	-0.289***
	(2.136)	(0.058)	(0.093)
Player B is in-group x University Students	4.175		
	(3.210)		
Male	-0.382	0.046	0.046
	(1.064)	(0.034)	(0.034)
$\lvert$Payoff distance from 50-50 split$\rvert$		0.012***	0.004
		(0.003)	(0.004)
In-group treatment $\times\lvert$ Payoff distance$\rvert$			0.011*
			(0.006)
Pilot Officers $\times\lvert$ Payoff distance$\rvert$			0.003
			(0.008)
University Students $\times\lvert$ Payoff distance$\rvert$			0.011
			(0.008)
Constant	29.406***	0.462***	0.536***
	(1.030)	(0.041)	(0.053)
Observations	268	1,080	1,080
R-squared	0.046	0.046	0.051

Clustered standard errors at the participant level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

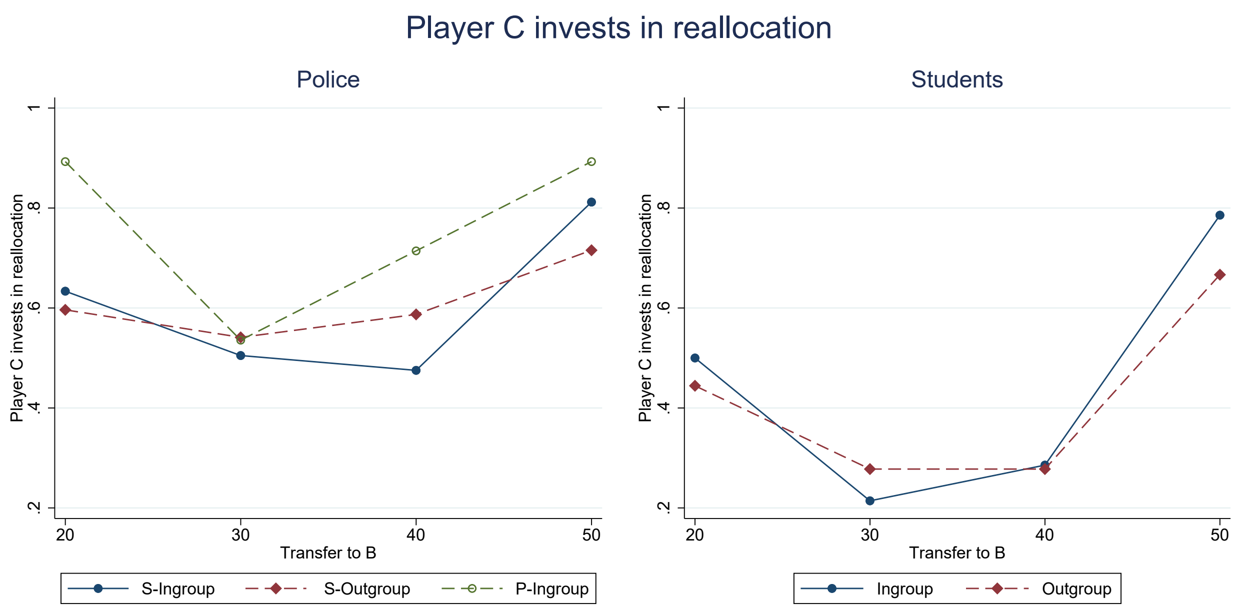

Figure 2: Comparison across samples for the probability that Player C invests in payoff reallocation. *S-Ingroup* and *S-Outgroup* refer to police students in the *B-Police* and *B-Civilian* treatment, respectively. *P-Ingroup* refers to police officers, which were all assigned to the *B-Police* treatment.

dents). This result suggests that university students mainly invest in norm enforcement when *ex ante* inequality levels are significant.

Unfortunately, our comparison between samples is less informative regarding the choice of rewards over sanctions. Given the small number of students and the low likelihood of reallocating for $x_i \in \{30, 40\}$, the number of observations is tiny to draw conclusions.[7] We do not find considerable differences in the comparison between police students and police officers (see Figure A.1 in the Appendix).

5 Prediction survey

Simple economic games, such as our dictator game with third-party reallocation, help measure the participants' expected behavior from another set of respondents not involved in the experiment. These behavioral predictions are particularly informative when the identity of the participants, police students in our case, can drive the respondents' predictions. We are interested in the accuracy of predictions made by a set of 209 civilians who responded to an incentivized survey. We will call them "respondents" to avoid confusion with the term participants (i.e., police students) intervening in the main experiment.

[7] For $x_i = 30$, we only have 5 university students in the *B-Police* treatment, and 3 in the *B-Civilian* treatment. Similarly, $x_i = 40$, we only have 5 university students in the *B-Police* treatment, and 4 in the *B-Civilian* treatment.

We elicit in this survey the predicted generosity (i.e., transfers from police students acting as Player A) towards their in-group and out-group, and the expected willingness to engage in costly norm enforcement (i.e., decisions from police students acting as Player C). We argue that respondents' predictions of costly behaviors from future police members help understand citizens' approval and trust in the police's behavior when performing their duties.

5.1 Experimental design

We had a total of 20 predictions of interest. Four predictions were related to Player A's transfer: two by treatment (*B-Police* and *B-Civilian*) and two by sex of the police students (women and men). The remaining sixteen predictions correspond to Player C's decision in four scenarios, one for each possible transfer of Player A. Within each scenario of Player C's behavior, we also included differential predictions by treatment and sex of the police students. Differential predictions by treatment (i.e., behavior in in-group and out-group interactions) are our main focus of study. However, differences by sex help understand whether citizens' beliefs about sanctioning behavior are driven by aggressive traits more likely to be seen in men than women.

At the end of the survey, we included questions regarding the respondents' approval of the CNP, its anti-riot group, and whether a recent change in the Police uniforms was an opportunity to improve their image. To maximize the chance that respondents completed the survey, we limited the prediction questions to 12. All the respondents predicted Player A's transfer, whereas half of them predicted Player C's behavior when Player A's transfer was $x_i \in \{20, 40\}$ and the other half when Player A's transfer was $x_i \in \{30, 50\}$. In this way, each respondent was asked about Player A's transfer (4 questions) and Player C's norm enforcement when Player A chose an advantageous (4 questions) and a disadvantageous (4 questions) allocation. Hence, our first source of randomization between respondents does not obey variations in the desired elicited responses but is instead related to procedural aspects that maximize survey completion.

We also randomized the incentives for responding to the survey. Assuming that the identity of the participants in the experiment affects the respondents' predictions, and given the low levels of citizens' trust in the CNP, non-incentivized responses may trigger the use of this survey as an instrument to complain about the police. In the treatment *Payment for Accuracy (PfA)*, respondents are informed before making their predictions that their payment depends on whether they are below or above the median predictive performance. Those above the median will receive 30 kCOP, whereas those below the median will receive 10 kCOP. Median predictive performance is a score of at most 1200 points (100 points per prediction), with a linear scoring rule penalizing incorrect predictions. The respondent j's score in question i regarding Player A's transfer, when the respondent submits its predicted transfer τ_{ij} is given by:

$$S_{ij}^{A} = \left(1 - \frac{|\tau_{ij} - T_i^*|}{30}\right) \times 100,$$

where T_i^* is the correct transfer in question i. The denominator takes into account that the difference between the maximum and minimum Player A's transfer is 30 kCOP.

In addition, the score for respondent j in question i regarding Player C's decision, when she predicts σ_i sanctions (out of 100) and ρ_i rewards (out of 100) is:

$$S_{ij}^{C} = \max\{100 - |\sigma_{ij} - \sigma_i^*| - |\rho_{ij} - \rho_i^*|, 0\},$$

where σ_i^* and ρ_i^* are the correct number of sanctioning and rewarding decisions in question i, respectively. The maximum function limits the scores of highly inaccurate predictions to zero, avoiding negative scores.

As we explained above, participants in the *PfA* treatment were told that their payment depends on predictive performance. The additional details on the total score and the linear scoring rule for penalties were provided after they submitted their predictions. The purpose was to keep the conditions as similar as possible between our treated and our controlled respondents. In our control condition, defined as *Payment for Completion (PfC)*, respondents are informed before making their predictions that they will receive their payment after completing the survey. To match the incentives from the *PfA* treatment, half of the participants in the *PfC* received 10 kCOP and the other half 30 kCOP. We call these variations *PfC10* and *PfC30*, respectively.

5.2 Data collection procedure

We conducted the prediction survey in December 2021, some days after collecting the experimental data from the police students. We sent the invitation to take the survey to four hundred previous participants registered in the Rosario Experimental and Behavioral Economics Lab-REBEL subject's pool of non-students (and students from other universities). These participants had prior experience, two months before this survey, in an unrelated study involving a proctored online experiment with electronic payments. We opted for an online format given the access restrictions to the university campus for non-students, the short survey duration, and its associated payments. Moreover, taking the survey from their device outside the laboratory may help reduce social-desirability biases in the predicted behaviors.

The invitation announced an incentivized survey about behavioral predictions in a game involving costly money allocations. We then informed respondents that the participants in the original game were police students, the expected survey duration (15 minutes), and the available slots (200). We had a total of 209 participants who earned, on average, 20.3 kCOP. Given the asynchronous nature of the survey, we announced that payments would be delivered within two business days. This asynchronous nature of the survey raised a concern. If participants learned about the different incentives between treatments, they might abandon and re-enter the survey to be assigned to another treatment with a higher payoff. To avoid this situation, we asked for the participants' name and e-mail after obtaining their consent. We reveal the treatment they were assigned to, *PfA* or *PfC*, and their potential earnings, only after receiving the name and e-mail.

5.3 Results

Descriptive statistics

One-hundred and twenty-nine participants self-identified as female (61.7%) and 79 (37.8%) as male. They were, on average, 27.5 years old (std. dev. 6.1). Most of our participants completed their undergraduate degree (57.4%), followed by participants currently enrolled in undergraduate studies (27.8%). Ten percent completed or are pursuing graduate degrees, and 4.8% completed high school or less. Among those who attended university, 48.2% went to a private university, 38.7% to a public university, and 13.1% to both. This division might be important in the face of attitudes and beliefs regarding the CNP, given the violent encounters between students from public universities and the police, mostly during larger protests in the first half of 2021.[8] We also asked participants about their closer relationship with a member of the police. Forty percent reported no relationship at all, followed by acquaintances (23%), close relatives (16.8%), friends (10.5%), and distant relatives (9.6%).

We employed a (1 to 5) Likert scale in the survey questions measuring attitudes toward the CNP. We find low levels of trust (2.13, std. dev. 0.87), combined with an extended belief that the police require a reform (4.62, std. dev. 0.74). We asked if the anti-riot group is the one requiring this reform, not the entire police, and opinions are more varied and intermediate (3.21, std. dev. 1.45). By the time of the experiment, there was a recent significant change in the National Police colors and uniform. Participants mostly disagreed that this was an opportunity to improve the National Police's image (1.70, std. dev. 1.13).

Table A.1 reveals that all the described variables are balanced across treatments. In Table A.2, we show that there were 88 dropout registries, half of which occurred during the first page (i.e., before giving consent and providing any personal information). From the other half, we tracked that 13/44 completed the survey in a second (or third try). Of these 13, only six respondents dropped out after being assigned to the treatment. These are the only six observations (2.9% of the sample in the analysis) where multiple treatment assignment may have occurred.

Predictions on how much kept Player A

We find that, on average, participants expected Player A to keep 40.1 kCOP in the *B-Police* treatment and 42.7 kCOP in the *B-Civilian treatment*. Recall that Player A kept 45.6 and 45.8 kCOP, respectively.[9] We also compute, for each survey respondent, her predicted difference between treatments (by subtracting the prediction in *B-Civilian* from the prediction in *B-Police*). We find that the average predicted treatment effect is -2.66, which is statistically significant ($p-$value 0.0056). Respondents expect police students in the role of Player A to be more generous when Player B is another police student. We do not find treatment differences between *PfA* and *PfC* for any of these predictions. These results are displayed in Table A.4 in the Appendix.

[8] See international coverage here: `https://www.nytimes.com/2021/05/18/world/americas/colombia-protests-what-to-know.html`

[9] In Section 4, we displayed this result in terms of transfers. We switched to the amount kept since we elicit the transfer in this form.

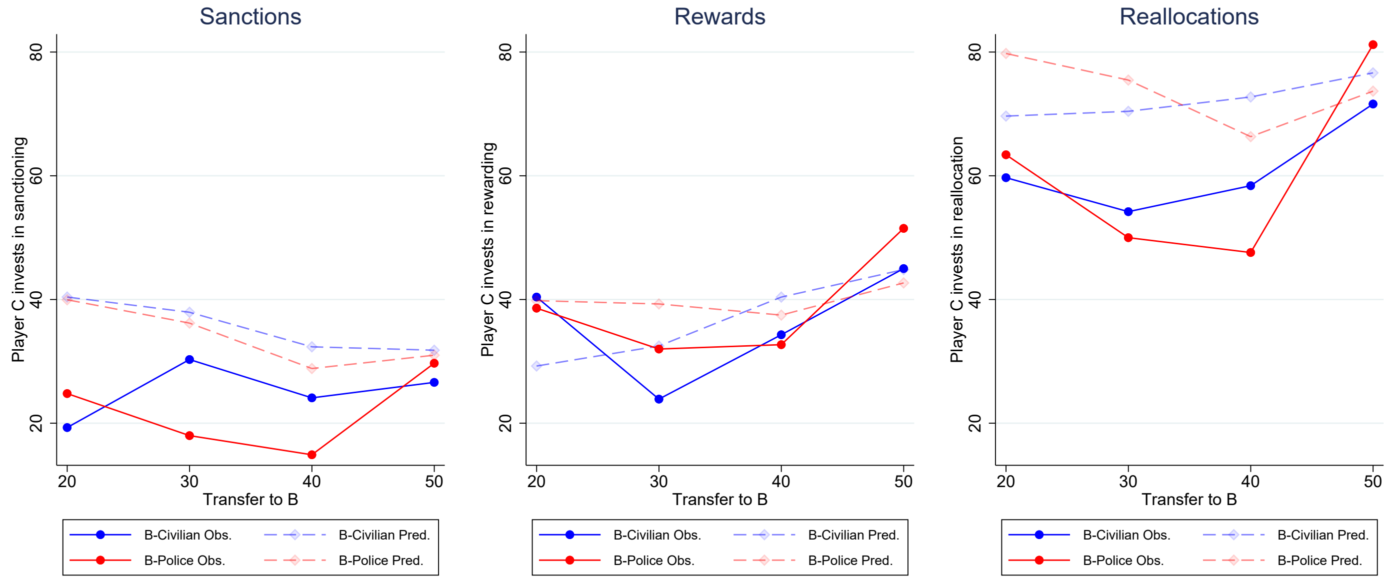

Figure 3: Survey predictions on Player C's reallocation decisions between treatments. Average predictions for each transfer level are shown in semi-transparent dashed lines. Blue lines correspond to the *B-Civilian* treatment, and red lines correspond to the *B-Police* treatment.

We thus conclude that survey respondents expected more generosity from police students than we observed in our main study. The respondents' average predictions were also aligned with (our non-supported) H1, as they also expected more generosity when Player B was also a police student. This result is confirmed by the predicted treatment effect, with expected transfers being 2.66 kCOP higher when Player B was an in-group.

Predictions on Player C's reallocation decisions

Figure 3 summarizes the predictions made by survey respondents regarding the expected share of police students enforing norms as Player C. This figure includes three panels, where the rightmost one corresponds to adding the other two. Dark lines correspond to the observed behavior in each treatment from the main study (*B-Civilian* and *B-Police*), and the semi-transparent dashed lines correspond to the predicted behavior in these treatments. We do not divide the sample by the survey treatments, *PfA* and *PfC*, because the differences between treatments are non-significant in all but one (15/16) of the elicited behaviors regarding Player C (see Table A.5 in the Appendix).

The most important conclusion from this figure is drawn from the rightmost panel, where we observe that respondents overestimate, by up to 20pp, the likelihood that Player C incurs a costly payoff reallocation. This overestimation comprises all, except the most generous transfer of Player A (i.e., for $x_i = 50$). As we discussed before, roughly three-quarters of the participants in the role of Player C chose this costly option, mainly to reward Player A for its large transfer.

The leftmost panel reveals that this overestimation is mostly driven by an expectation of frequent sanctioning behavior, especially when Player A's transfers are low. There are two additional patterns in the predictions for sanctioning behavior. First, respondents do not expect major differences between the *B-Civilian* and *B-Police* treatments. In the main study, we found that police students are likelier to choose sanctions against Player A when Player B is not a police student. Second, the predicted probability of sanctioning decreases with Player A's transfer. In the main study, this only occurred in the *B-Police* treatment, for $x_i < 50$.

In the central panel, the four lines are more intertwined, suggesting that predictions were closer to the actual behavior of the police students. Nonetheless, survey respondents failed to predict the U-shape behavior we observed for the likelihood of rewarding Player A or B, the one with the lowest payoff. Note that the predictions for the *B-Civilian* treatment increase with Player A's transfer. This result suggests that respondents expected that police students substituted sanctions for rewards as Player A became more generous while keeping the likelihood to reallocate constant. Two patterns emerge from this expected substitution. First, when Player A's allocations are advantageous, survey respondents expected that Player C rewards Player A more often in the *B-Police* treatment, compared to the *B-Civilian* treatment. This difference, revealing an expectation of in-group favoritism, is statistically significant (see Table A.5). Second, the predicted reallocations (i.e., the sum of sanctioning and rewarding predictions) look less responsive to inequality between Players A and B than they were, especially in the *B-Civilian* treatment.

Did incentives for accuracy matter?

Table A.5 reveals that the average individual predictions (i.e., the predicted transfers, sanction, and reward decisions) did not differ between payment schemes. Nevertheless, this table also reveals a higher standard deviation for most of the elicited predictions in the *PfC* treatment. In this subsection, we explore whether the incentives for accuracy employed in the *PfA* treatment induced responses closer to the observed behavior among police students. The aggregate score across the twelve assigned questions was, on average, 750.9 (out of 1200 points). The standard deviation was 133, with a minimum score of 370 and a maximum of 1033. To ease the interpretation, we take as a dependent variable the standardized prediction score.

In addition to the *PfA* treatment, we include as covariates whether the respondent self-identified as female, her age, whether she attended university, and a set of variables that may be related to pre-conceptions regarding police members' behavior. In this set, we include closeness to police members,[10] whether the respondent attended a public university (as opposed to a private university, or both), and their beliefs regarding citizens' trust in the police.

Table 6 reports the coefficients for an OLS regression with robust standard errors. The *PfA* treatment increases the score in 0.24-0.29 standard deviations. By contrast, our categorical variable for attending a public university is associated with a decrease in the score of about 0.26 standard deviations. We interpret this result with caution, as we do not have any variable controlling for income or political orientation. We also find that older respondents obtain higher scores (recall

[10]We coded the variable measuring the closer relationship to a police member as 4 for close relatives, 3 for distant relatives, 2 for friends, 1 for acquaintances, and 0 for no relationship at all with a police member.

Table 6: OLS coefficients for (standardized) prediction score

Dependent variable: (standardized) Prediction score	Full sample (1)	(2)	(3)	University students (4)
Payment for Accuracy	0.285**	0.277**	0.250*	0.240*
	(0.138)	(0.138)	(0.135)	(0.137)
Female		0.0212	-0.0002	-0.0252
		(0.141)	(0.141)	(0.146)
Age		-0.0245**	-0.0259**	-0.0262***
		(0.0108)	(0.0101)	(0.0101)
Attended University		-0.166	-0.0333	
		(0.324)	(0.342)	
Closeness to Police Member			-0.0644	-0.0590
			(0.0527)	(0.0539)
Public University			-0.259*	-0.261*
			(0.142)	(0.141)
Trust in the Police			0.127	0.158*
			(0.0819)	(0.0830)
Constant	-0.147	0.676*	0.528	0.453
	(0.106)	(0.390)	(0.408)	(0.340)
Observations	209	209	209	199
R-squared	0.020	0.045	0.076	0.081

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

that the mean age was 27 years old, with a standard deviation of 6.1). By contrast, note that neither gender, attending a University, nor the closest relationship with a police member predicts more accurate scores.

The variable capturing trust in the police has a $p-$value of 0.127 for the entire sample, but it becomes statistically significant for the sample of university students (which only excludes ten respondents). We obtain similar results with another measure of attitudes toward the police: whether the respondent considers that the recent change in the police colors and uniforms constitutes an opportunity to improve the police image. The coefficients are also positive, though non-significant, for the beliefs that the CNP (and its anti-riot group) requires a major reform.

6 Discussion and concluding remarks

We conducted a study to understand citizens' beliefs about police behavior regarding fairness norm enforcement. First, we conducted a lab-in-the-field experiment with 109 male and 100 female police students in Colombia. In our setting, participants took allocation decisions in a modified dictator game, including a third-party reallocator. We employed the strategy method to ask participants about their costly reallocation decisions for each potential dictator's allocation, between sanctioning the player with the highest payoff, rewarding the player with the lower payoff, or leaving the initial allocation as it was at no cost. Since we were interested in detecting and measuring expectations of in-group favoritism, we randomly varied whether the transfer's recipient was another police student or a civilian. After collecting the police students' choices, we deployed an incentivized prediction survey to measure citizens' beliefs and their accuracy regarding police students' generosity and norm enforcement behavior.

We find that police students, on average, transfer roughly 40% of their endowment, which is also the modal choice (44%). The differences when the recipient is a police student (i.e., their in-group) or a civilian (i.e., their out-group) are negligible. Using the strategy method, we collected the norm enforcement behavior for each of the four potential transfers from the first mover. We find that police students are more likely to reallocate payoffs when the initial allocations are more unequal (by 1pp for each 1 kCOP in the payoff distance between the two other players). However, this effect is more prominent when the recipient (Player B) is also a police student. Conditional on a costly reallocation, police students are more likely to reward in homogeneous (i.e., the *B-Police* treatment) than in heterogeneous groups (i.e., *B-Civilian*). In the latter, costly reallocations are more likely to involve a sanction.

Survey respondents expected larger initial transfers in the *B-Police* treatment, as we also suggested with our first hypotheses on in-group favoritism, for which we did not find empirical support. Regarding third-party behavior, participants overestimate the likelihood of a costly reallocation. This effect was driven mainly by expecting police students to sanction more often than they did (in both treatments). We find a positive effect of providing incentives for accurate responses (the score increased by 0.25-0.29 standard deviations). Also, that respondents who studied at a public university had a lower accuracy of roughly the same magnitude, and that lower trust in the police also decreased the predictions' quality (by 0.16 standard deviations).

We now put together the results from the experiment and the prediction survey, aiming to enrich the discussion on actual and perceived behavior from police members. First, investment in reallocation from police students is high. It is much closer to the behavior observed among experienced police officers than among university students of about the same age. Still, survey respondents expected an even larger investment in reallocation from police students. In particular, they perceive police members as more engaged in costly punishment than they are. Second, respondents predict that more generous transfers from Player A decrease sanctions and increase rewards. These predictions do not capture that reallocations from police students are more likely for more extreme initiall allocations (i.e., Player A being too selfish or too generous). Summing up, respondents acknowledge (and even overestimate) that police students are more likely to engage in costly behaviors to themselves to balance the payoffs of others, but fail to detect that this costly behavior is more easily triggered in more unequal situations.

Third, respondents predicted that, for Player A's advantageous allocations, police students in the role of Player C are more likely to reward in the *B-Police* than in the *B-Civilian* treatment. This prediction is correct for $x_i = 45$, suggesting that in-group leniency is correctly anticipated as a form of favoritism. Another way to read the same behavior is that sanctions are more common in the presence of an out-group. This result goes against previous experimental evidence of not punishing in-group members in the presence of an out-group in cooperation dilemmas (Mantilla et al., 2021), and on the willingness to incur in a payment to not expose an in-group member (Eriksson et al., 2017). We offer two non-mutually exclusive conjectures to reconcile this apparent contradiction. On the one hand, our game offers the opportunity to sanction the participant with the largest payoff, or reward the participant with the lowest payoff. Hence, police students might avoid increasing the payoff of their out-group. On the other hand, the two studies mentioned above were conducted in China, and the cultural differences in violation of norms regarding prosocial behavior may differ.

Our survey instrument validated that trust in the police is low and the support for a police reform is high. Nonetheless, these beliefs are weakly correlated with the predicted police students' behavior in our lab-in-the-field experiment. First, survey respondents correctly predicted "average" generous police students who, in addition, are willing to incur a cost to themselves to reallocate other participants' payoffs toward more egalitarian outcomes. We return to the discussion on the selection quality of police members in developing countries. Our results suggest relatively high levels of altruism and willingness to enforce fairness norms at a cost to themselves. This behavior is similar to the one observed in the experiments conducted in France and Germany (Dickinson et al., 2015; Friebel et al., 2019), and it goes against the idea of negative selection of police officers due to poor sanctioning social norms in developing countries.

Another alternative is that the low trust in the police was correlated with in-group favoritism in the initial allocations and in the choice of rewarding over sanctioning behavior. We measured beliefs regarding differential behavior when the transfer recipient was a police or a civilian. However, by comparing the predicted behavior between treatments, we do not find systematic evidence of beliefs about in-group favoritism beyond mild differences in the expected transfers.

The overestimation of sanctioning behavior is the best clue for how the negative perception of police members maps into our prediction survey. Moreover, survey respondents fail to predict that police students are likelier to enforce norms in more inegalitarian situations. One interpretation for this lack of sensitivity to the context in which norm enforcement appears is that respondents do not expect police students to alter their behavior depending on the context's legitimacy. Unfortunately, our study cannot go beyond speculation on this particular point. Future experiments aiming to correlate public servants' behavior in incentivized games with citizens' perceptions may focus on the institution's collective identity rather than on their members' individual behavior. For instance, one may design games with endogenous institutional choice through voting and then elicit citizens' beliefs about such voting outcomes.

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Online Appendix

Costly Norm Enforcement through Sanctions and Rewards: An Experiment with Colombian Future Police Officers

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A Additional Figures and Tables

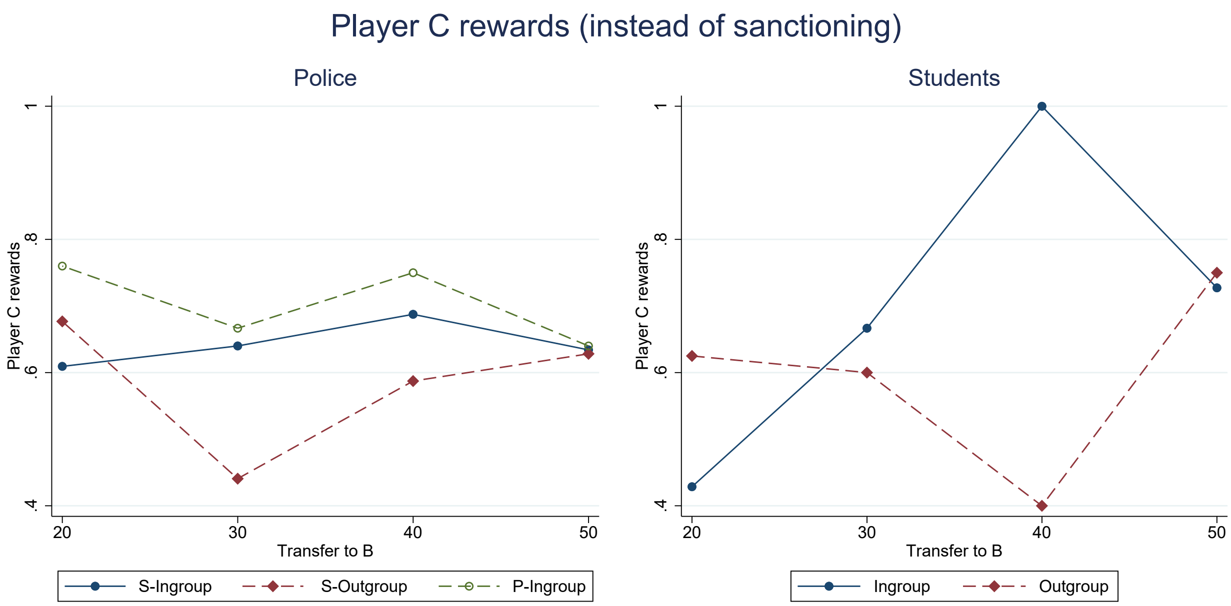

Figure A.1: Probability that Player C chooses reward over a sanction. *S-Ingroup* and *S-Outgroup* refer to police students in the *B-Police* and *B-Civilian* treatment, respectively. *P-Ingroup* refers to police officers, which were all assigned to the *B-Police* treatment.

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Table A.1: Balance across participant's characteristics in the prediction survey

	PfC	PfA	$p-$value
Female	0.634	0.602	0.636
Age	27.8	27.1	0.437
Educational attainment			0.527
Completed undergraduate	0.555	0.592	
Currently undergraduate	0.267	0.287	
Graduate studies	0.109	0.093	
High-school or less	0.069	0.028	
University type			0.74
Private	0.457	0.505	
Public	0.415	0.362	
Private and Public	0.128	0.133	
Closest relationship with police member			0.968
Close relative	0.168	0.167	
Distant relative	0.089	0.102	
Friend	0.119	0.092	
Acquaintaince	0.218	0.241	
None	0.406	0.398	
Trust in the Police	2.09	2.18	0.473
Police needs a reform	4.57	4.68	0.319
Only anti-riot police needs a reform	3.25	3.15	0.621
Police uniforms as opportunity to improve image	1.78	1.62	0.331

$p-$values of $t-$tests reported for continuous variables. $p-$values of χ^2 tests reported for categorical variables (in italics).

Table A.2: Dropouts registered in the survey dataset

Dropout stage	N	Identifiable	Incomplete	(completed) Assigned to One treatment	(completed) Assigned to Multiple treatments
Initial page	44	No	.	.	.
Before treatment assignment	11	Yes	6	5	.
After treatment assignment	33	Yes	25	2	6
Total	88		31	7	6

Registries of dropping out in the initial page cannot be tracked. For those providing name and e-mail, we checked *(i)* whether they dropped out before or after entering the page where treatment assignment occurred, and *(ii)* whether they completed the survey or not. The only potentially problematic dropouts in our analysis are those being assigned to multiple treatments.

Table A.3: Likelihood that Player C selects each choice for outcomes where costly reallocations would yield the same final payoffs. Marginal probabilities for each strategy reported outside each box.

Panel A. Comparison when $x_i = 25$ and $x_i = 45$

		A keeps 45			
		Sanction	Reward	Nothing	
A keeps 25	Sanction	24	13	21	27.8%
	Reward	13	39	49	48.3%
	Nothing	14	6	30	23.9%
		24.4%	27.8%	47.8%	

χ^2 $p-$value comparing all C's strategies when A keeps 25 and 45: <0.001
χ^2 $p-$value comparing whether C invested when A keeps 25 and 45: 0.045
χ^2 $p-$value comparing costly C's strategies when A keeps 25 and 45: <0.001

Panel B. Comparison when $x_i = 35$ and $x_i = 55$

		A keeps 55			
		Sanction	Reward	Nothing	
A keeps 35	Sanction	13	13	15	19.6%
	Reward	6	33	31	46.9%
	Nothing	27	37	34	33.5%
		22.0%	39.7%	38.3%	

χ^2 $p-$value comparing all C's strategies when A keeps 35 and 55: 0.02
χ^2 $p-$value comparing whether C invested when A keeps 35 and 55: 0.28
χ^2 $p-$value comparing costly C's strategies when A keeps 25 and 45: 0.003

Table A.4: Survey predictions regarding Player A's amount kept

	Aggregate	By treatment		
		PfA	PfC	$p-$value
Kept in *B-Police* *(True value: 45.6)*	40.08[a]	40.56	39.57	0.426
	(8.88)	(8.82)	(8.97)	
Kept in *B-Civilian* *(True value: 45.8)*	42.73[b]	42.29	43.22	0.550
	(11.18)	(11.07)	(11.28)	
Within-subject predicted difference	-2.66[c]	-1.74	-3.64	0.316
	(13.72)	(13.93)	(13.49)	

[a]This value is statistically different from 45.6 with a $p-$value < 0.0001.
[b]This value is statistically different from 45.8 with a $p-$value 0.0001.
[c]This value is statistically different from zero with a $p-$value 0.0056.

Table A.5: Survey predictions regarding Player C's decisions

Predicted behavior	Player A's Allocation							
	55-20		45-30		35-40		25-50	
Sanctions								
B-Police: Sanction	40.0	(2.25)	36.2	(2.15)	28.8	(2.39)	31.0	(2.36)
PFA	37.5	(2.82)	38.0	(3.01)	26.1	(2.93)	29.1	(3.29)
PFC	42.9	(3.59)	34.3	(3.08)	32.2	(3.92)	32.8	(3.37)
	(0.233)		(0.389)		(0.204)		(0.429)	
B-Civilian: Sanction	40.4	(2.70)	37.9	(2.61)	32.4	(2.41)	31.8	(2.34)
PFA	40.5	(3.30)	33.6	(3.22)	30.5	(2.88)	32.4	(3.37)
PFC	40.2	(4.47)	42.1	(4.03)	34.6	(4.06)	31.3	(3.28)
	(0.955)		(0.105)		(0.401)		(0.814)	
Within-subject predicted difference in sanction (B-Police - B-Civilian)	-0.3	(2.54)	-1.5	(2.44)	-3.5	(2.55)	-0.8	(2.26)
	(0.912)		(0.548)		(0.172)		(0.724)	
PFA	-2.8	(3.29)	-4.1	(3.61)	-4.4	(2.94)	-3.3	(3.03)
PFC	2.7	(3.94)	1.2	(3.27)	-2.4	(4.42)	1.6	(3.34)
	(0.284)		(0.284)		(0.693)		(0.285)	
Rewards								
B-Police: Reward	39.8	(2.17)	39.3	(2.15)	37.5	(2.57)	42.7	(2.43)
PFA	40.3	(2.66)	41.4	(3.25)	36.1	(3.08)	44.3	(3.50)
PFC	39.2	(3.58)	37.3	(2.85)	39.2	(4.33)	41.2	(3.40)
	(0.802)		(0.351)		(0.544)		(0.526)	
B-Civilian: Reward	29.3	(2.01)	32.5	(2.33)	40.4	(2.72)	44.8	(2.61)
PFA	30.0	(3.61)	38.4	(3.57)	39.6	(3.39)	44.5	(3.69)
PFC	28.7	(2.20)	26.6	(2.84)	41.3	(4.44)	45.2	(3.71)
	(0.742)		**(0.011)**		(0.756)		(0.892)	
Within-subject predicted difference in reward (B-Police - B-Civilian)	**10.4**	(2.59)	**7.0**	(2.31)	-2.9	(2.62)	-2.2	(2.34)
	(0.0001)		**(0.003)**		(0.269)		(0.357)	
PFA	11.6	(3.00)	**2.9**	(3.37)	-3.6	(2.84)	-0.2	(3.32)
PFC	9.0	(4.46)	10.9	(3.11)	-2.1	(4.71)	-4.0	(3.30)
	(0.612)		**(0.083)**		(0.786)		(0.417)	

The within-subject predicted difference in sanction/reward corresponds to the survey respondents' predicted treatment effect for each one of these choices. It is computed as the predicted sanction/reward rate for the *B-Police* treatment minus this rate for the *B-Civilian* treatment. Differences in bold are statistically significant.