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Thèse de doctorat



Essays in Development and Experimental Economics

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préparée à l'École polytechnique

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Summary

Experimental economics offers the possibility to study the behavior of economic agents in ideal conditions, as it allows for the complete control of the environment of participants. This thesis presents results from several field experiments, as well as their contributions to the literature. This manuscript is composed of five chapters. The first three chapters contribute to the literature on the contact theory, a methodology used to improve intergroup relations. Chapters 1 and 2 present results from field experiments, in France and in Senegal, while chapter 3 presents a meta-analysis of the experimental literature on the topic. The last two chapters present results of various experiments. Chapter 4 investigates the influence of providing rankings on performance, depending on the nature of the task to be performed. Chapter 5 talks about genetic adaptation to fishing risk in Senegal.

Chapter 1 discusses a field experiment investigating the effects of a brief and controlled discussion on trust between disadvantaged students in Paris' suburbs, and police officers. The contact protocol is adapted from the social psychology literature and aims at quickly creating friendships. Results indicate that the contact intervention increases trust of students toward the specific police officers met, but it does not increase trust in the police in general.

Chapter 2 analyzes an experimental protocol closely related to that of chapter 1, but applied in the context of inter-ethnic relations in Senegal. The short contact is effective at increasing inter-ethnic trust with the specific individuals met, but does not increase altruism toward the outgroup in general. Moreover, the effect at the individual level evaporates within one month of the intervention. Machine learning techniques enable to discover that contact is particularly effective for older and less educated participants.

Chapter 3 proposes a meta-analysis of the rapidly growing experimental literature on the contact hypothesis. Based on 62 outcomes from 37 research papers, the analysis revealed three main results. First, there exists a large heterogeneity in the definition of a "contact", making difficult the comparison of papers. Second, on average, contact interventions are effective, by significantly improving the perception of the outgroup. Third, machine learning algorithms enable the study of determinants of the most effective contact interventions.

Chapter 4 investigates the effects of providing rankings on performance, depending

on the nature of the task. The analysis is performed by analyzing the existing empirical literature, proposing a novel theoretical framework and the empirical analysis of two experiments. Results indicate that the main moderating factor of the effects of relative performance feedback is the existence of room for technological improvement.

Chapter 5 investigates the interplay between the effects of the dopamine receptor gene D4 and background risk on risk-aversion for fishermen in Northern Senegal. The analysis consists in the genetic and behavioral comparison of two populations differing in their main professional occupation. Results indicate that the 7R allele is associated with an increased risk-tolerance, through an additive, rather than dominance, effect. Moreover, the effects of the 7R allele are not driven by the background risk, as we do not observe heterogeneous effects depending on the level of background risk.

Résumé

L'économie expérimentale offre la possibilité d'étudier le comportement des agents économiques dans des conditions idéales, puisqu'elle permet le contrôle presque total sur l'environnement des participants. Cette thèse présente les résultats de diverses expériences menées sur le terrain, ainsi que des synthèses de la littérature. Ce manuscrit est composé de cinq chapitres. Les trois premiers chapitres traitent de la théorie du contact, qui vise à améliorer les relations entre différents groupes. Les chapitres 1 et 2 présentent les résultats de deux expériences de terrain, en France et au Sénégal, tandis que le chapitre 3 présente une méta-analyse de la littérature empirique. Les deux derniers chapitres présentent les résultats d'expériences d'économie sur des sujets divers. Le chapitre 4 étudie l'effet de donner le classement sur la performance des agents, en fonction de la nature de tâche à effectuer. Le chapitre 5 traite d'adaptation génétique au risque lié à la pêche au Sénégal.

Le chapitre 1 traite d'une expérience de terrain étudiant les effets d'une discussion courte et structurée sur la confiance entre des jeunes de lycées défavorisées de banlieue parisienne et des policiers. La méthodologie est adaptée de la littérature en psychologie sociale et a pour but de créer des liens d'amitié rapidement. Les résultats indiquent que le contact engendre une augmentation du niveau de confiance des lycéens envers les policiers rencontrés, mais ne permet pas d'augmenter la confiance envers les policiers et la police en général.

Le chapitre 2 analyse un protocole expérimental proche du chapitre 1, mais l'applique dans le contexte des relations inter-ethniques au Sénégal. Les discussions cadrées et rapides ont un effet sur la confiance inter-ethnique avec les individus rencontrés, mais ne permettent pas l'amélioration de la générosité à l'égard des autres groupes ethniques en général. De plus, les effets semblent dissipés seulement un mois après le traitement. Des méthodes d'apprentissage statistique permettent de trouver que le traitement est particulièrement efficace pour les individus âgés et peu éduqués.

Le chapitre 3 offre une méta-analyse de la littérature empirique sur le contact, qui s'est grandement développée au cours des dernières années. Se basant sur 62 mesures, issues de 37 articles, l'analyse révèle trois résultats principaux. Tout d'abord, il existe une très grande hétérogénéité dans la définition de « contact », ce qui rend difficile la comparaison des différents articles. Ensuite, en moyenne, les interventions de contact sont efficaces en permettant d'améliorer significativement la perception de l'autre groupe.

Enfin, l'utilisation d'apprentissage statistique permet de de déterminer les déterminants de l'efficacité des interventions.

Le chapitre 4 analyse l'effet de l'apport de classements sur la performance, en fonction de la nature de la tâche à effectuer. L'analyse est effectuée au regard d'une analyse de la littérature, d'un modèle théorique novateur et de l'étude empirique de deux expériences. Les résultats indiquent que le facteur qui module grandement l'effet des classements est l'existence, ou non, de possibilité d'amélioration de la technologie de production de la performance.

Le chapitre 5 étudie le lien entre les effets du gène codant le récepteur à la dopamine D4 et du risque environnemental sur l'aversion au risque chez des pêcheurs du nord du Sénégal. L'analyse consiste en la comparaison comportementale et génétique de deux populations qui diffèrent selon leur activité professionnelle principale. Les résultats indiquent que l'allèle 7R est associé à une plus grande tolérance au risque, par un effet additif et non de dominance. De plus, aucune hétérogénéité n'a été détectée en fonction du risque environnemental.

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General Introduction

This thesis is at the intersection of two vast areas of research: experimental economics and development economics. In this introduction, I first give general insights about the two fields, and present the themes to which the thesis contributes, in particular the field of research devoted to the contact hypothesis. Lastly, I detail the contributions of the five chapters of this thesis.

1. Development and experimental economics

In 1980, it was estimated that nearly 40% of the world population lived with less than \$ 1.90 per day. This figure was unevenly distributed, with more than 60% of the population in Sub-Saharan Africa or South Asia living below the international extreme poverty line, and less than 5% in Western Europe or North America ([World Bank, 2022](#)).

The field of development economics, historically, aimed at understanding the causes of differences in wealth between countries ([Lewis, 1954](#)). The ideas of structural change, poverty traps and the role of human and physical capital have had a profound effect on policies implemented to reduce global poverty. Most of the empirical research focused on comparing countries with different policies, and trying to identify links between policies and levels of wealth ([Easterly, 2001](#)).

The introduction of randomized controlled trials, pioneered by Esther Duflo and Abhijit Banerjee, came more recently to development economics. They have enabled the analysis of interventions at the individual level. The concept of RCTs comes from the medicine literature and investigates the effect of an intervention by *randomly* allocating units (individuals, villages) to either a treatment or control groups. The underlying principle, called the Rubin Causal Model ([Rubin, 1974](#)) is that because of randomization, the control group would have behaved identically to the treatment group, had they received the treatment. The introduction of RCTs enabled a great increase in the credibility of results of development policies, and shifted the focus of development economics research, from evaluating the sources of poverty, to the careful evaluation of policies. Evaluations have enabled the wide implementation of policies, in fields such as education ([Duflo et al., 2011](#)), health ([Dupas, 2014](#)) or labor markets ([Imbert and Papp, 2015](#)).

Contrary to development economics, the use of experiments in a controlled environment has been at the core of the field of experimental economics since its inception (Kahneman et al., 1990; Thaler et al., 1997). In experiments, experimenters typically do not have to worry about attrition, compliance or other factors which are cause of worry for field experiments. The use of carefully designed experiments can therefore be useful to test theories and to identify mechanisms of interventions.

Another strand of literature in experimental economics has been the attempts at measuring economics concepts, such as risk-aversion (Binswanger, 1980; Gneezy and Potters, 1997), time preferences (Frederick et al., 2002) or concepts which are difficult to measure with open survey questions, such as intimate partner violence (Aguero and Frisanchio, 2022) or lying (Fischbacher and Föllmi-Heusi, 2013).

The use of laboratory, or lab-in-the-field, experiments can help design better policies, which can be then evaluated by field experiments. I am glad that this thesis is at the intersection of these two fields.

2. Economics of prejudice and discrimination

The theme of the three first chapters of this dissertation is the use of *contact interventions* as a tool to reduce prejudice and increase trust. Before detailing what is the contact hypothesis, I introduce the topic of research on prejudice.

Prejudice has been a focus of research in economics, psychology and sociology for many decades. It can be defined as a feeling directed against a particular individual or a group based on a distinct characteristic.

To summarize very quickly the history of the literature on prejudice in economics, three main phases can be identified.¹ Of course, the three phases are not at all exclusive, but the distinction is interesting for illustrative purposes. The first phase, starting as far back as the 1950s (Becker, 1957) and until the early 2000s, aimed at *identifying* prejudice and discrimination. The debate of the time was to establish whether the source of observed differences between groups were due to animus against specific groups (Becker, 1957) or due to statistical discrimination, under which groups are signals about the true value of individuals (Arrow, 1973). This literature has used different tools, from audit studies (Ayres and Siegelman, 1995; Cain, 1996), correspondence studies (Bertrand and Mullainathan, 2004), implicit association tests (Greenwald et al., 1998), to list randomization (Kuklinski et al., 1997) and willingness to pay for working with outgroup members (Mobius and Rosenblat, 2006; Hedegaard and Tyran, 2018). The identification of the sources of discrimination has been in particular studied in the case of the labor market (List, 2004).

¹For a detailed analysis of the use of experiments to study discrimination, see Bertrand and Duflo (2017).

The second phase of the literature on prejudice lasted approximately from the early 2000s to the mid-2010s. The main focus of the literature then was to identify the *consequences* of discrimination. These consequences include stereotype threat (Steele and Aronson, 1995; Aronson et al., 2002), self-identification Benjamin et al. (2010) or Pygmalion effects (Jussim and Harber, 2005) as self-fulfilling prophecies (i.e. a individual afraid of being stereotyped will reduce his or her effort and therefore perpetuate group differences). The lack of representation of minority groups has also been shown to have negative effects on policies implemented (Beaman et al., 2009).

The third phase of the literature started very early (some papers date back as far as the 1950s) but only really took off around 2010, and its aim is to find solutions to *mitigate* prejudice and discrimination. Many solutions have been implemented, such as increasing the diversity in leadership positions (Beaman et al., 2009; Bagues et al., 2017), presenting role models (Cheryan et al., 2011) or implementing of debiasing strategies (Banerjee et al., 2013; Madva, 2017). However, the literature which has, by far, received the most attention by academics is the implementation of intergroup contact interventions. This topic is at the center of this thesis, and is explained below.

3. The contact hypothesis

The contact hypothesis is probably the solution to reduce prejudice which has been the most extensively studied by scholars (Bertrand and Duflo, 2017). The idea that intergroup contact can reduce prejudice dates back at least since the 1950s, with Allport (1954) stating that prejudice “may be reduced by equal status contact between majority and minority groups in the pursuit of common goals. The effect is greatly enhanced if this contact is sanctioned by institutional supports (i.e., by law, custom, or local atmosphere), and provided it is of a sort that leads to the perception of common interests and common humanity between members of the two groups.” (p.281).

A lot of research has been devoted to the investigation of the effects of contact. In a seminal meta-analysis, Pettigrew and Tropp (2006) famously review 515 studies testing the effects of contact on prejudice. Their overwhelming conclusion was that contact *is* effective at reducing prejudice, as the more people are in contact with outgroup members, the less prejudiced they are.

However, with only a few exceptions at the time, the literature they surveyed was correlational. Typically, papers compared people who are in contact, or are friends with, outgroup members, and people who are not. They then evaluated prejudice using different methodologies, and found that the more outgroup friends, the less prejudiced. However, it is impossible to positively conclude on the effect of contact from such studies, as they could suffer from selection bias and thus, in fact, reverse causality: it is not the fact of having more encounters with outgroup members that causes a reduction in prejudice, but

the fact of being less prejudiced which will make people interact more with other groups, choose a neighborhoods with more diversity, etc.

With the turn of the 21st century, more and more experimental, or quasi-experimental, protocols have been developed. Contact interventions have been tested in the military with new recruits (Carrell et al., 2019; Finseraas et al., 2016; Cáceres-Delpiano et al., 2021), in school programs (Scacco and Warren, 2018; Clunies-Ross and O'meara, 1989; Freddi et al., 2022; Rao, 2019), through door-to-door canvassing and other forms of scripted discussions (Broockman and Kalla, 2016; Kalla and Broockman, 2020; Page-Gould et al., 2008), for outgroup roommates for university students (Boisjoly et al., 2006; Corno et al., 2019; Van Laar et al., 2005) or in sports leagues (Mousa, 2020; Lowe, 2021). In a recent review of the growing experimental literature, Paluck et al. (2019) find the same results as Pettigrew and Tropp (2006) that contact *does* reduce prejudice, but the results are less strong and more heterogeneous.

The first two chapters of this thesis contribute to the literature by improving the *replicability* and *comparability* of contact protocols, by implementing the same type of protocol, based on the “fast-friend” procedure (Aron et al., 1997), and evaluating the effects of contact on the same outcomes, specifically the trust game (Berg et al., 1995). The two chapters also contribute to the *scalability* of contact interventions, with a much shorter and cheaper protocols.

The third chapter of this thesis is a meta-analysis exercise of the literature of contact, updating previous meta-analytic work (Paluck et al., 2019) by adding new, more recent, papers, and testing for predictors of the efficacy of contact interventions.

The field of research of contact interventions is really vibrant, and it is a great pleasure to modestly contribute to it with these three chapters.

4. Other themes in this thesis

During the course of my doctoral studies, I also had the opportunity to work on two unrelated subjects, which are presented in the last two chapters of this thesis.

The first additional theme is at the center of the fourth chapter, and looks at the effects of providing relative performance feedback (RPF), such as rankings, on performance. Providing such information is ubiquitous in the society (TripAdvisor provides a ranking of the best ranked hotels in a region, academics are able to compare themselves using rankings based on citation indices, employers provide information on workers' relative productivity, etc). While countless papers have investigated the different forms of RPF in a lot of different contexts, there seems to be a large part of heterogeneity in results (Villevall, 2020). I found working on this topic for the chapter very interesting, as it enabled me to discover how to really run an experiment based on theoretical predictions, rather than developing a theoretical framework to match empirical findings.

The second additional theme of this thesis is the link between genetics and human behavior. The relative importance of nature and nurture, or what share of behavior can be explained by genetic factors, and what is due to the environment or idiosyncratic characteristics, has been a cause of a huge debate in social, as well as “hard”, sciences for a very long time (Haldane, 1946). There is also a large debate in the experimental economics literature about the influence of the environment on attitudes. This debate is particularly strong in the case of risk attitudes. Competing theories have been developed to investigate whether, in a risky environment, people should be more risk-averse in accordance to the risk vulnerability hypothesis (Gollier and Pratt, 1996). The fifth and final chapter of this thesis investigates the interplay between nature and nurture in the case of a specific gene, the dopamine receptor D4 gene, in a field experiment in Senegal. The chapter required a collaboration with more researchers than standard in economics. In addition, it involved working with an inter-disciplinary team and preparing a chapter which is in a different format, standard in hard sciences, but not in economics.

5. Presentation of chapters

Chapter 1

Presentation Chapter 1 presents the results of a lab-in-the-field experiment to test the effects of a brief contact on trust toward police officers. The chapter starts from three observations: trust in the police is low for parts of the French population; trust in the police can improve well-being in the community; and trust has been found to be malleable due to outside events. I design an experiment involving a brief and very controlled contact intervention in two high-schools in poor suburbs of Paris, with discussions of approximately ten minutes between students and police officers. A placebo group involving young university students from the area is used to distinguish the effects of a simple discussion from the effects of a contact with a police officer, specifically. I also investigate who is more influenced by the treatment, in particular evaluating the role of previous encounters with the police.

Results I find that the contact intervention is effective at increasing trust toward the specific police officers that the students met. The magnitude of the effect is relatively important with approximately 0.4 standard deviation, especially when considering that the discussions were extremely short. However, trust in the police in general is not significantly improved by the intervention. To make sense of this fact, I developed a theoretical framework which indicates that previous interactions should moderate the effects of contact. The model is supported by the finding that all the effect at the individual level is driven by students who have never been subject to identity controls by the police, used

as a proxy for negative interactions with the police. This chapter has implications for the most widely used policy to improve the perception of the police, namely community policing.

Note: This chapter circulates as a working paper (Clochard, 2021).

Chapter 2

Presentation Chapter 2 analyzes a similar protocol as the one used in Chapter 1, but the context is completely different. In the case of Chapter 2, the source of prejudice is ethnic, and is investigated in the context of Senegal. In addition to the replication of the protocol of the previous chapter, which enhances comprehension of the “fast-friend procedure”, this paper further contributes to the literature by analyzing the duration of effects, through a follow-up survey one month after the intervention, and by analyzing the heterogeneity of findings using machine learning algorithms (Chernozhukov et al., 2018).

Results We find that the contact protocol is only effective at increasing trust toward the specific individuals met during the intervention. Contact is found to have no effect on generosity toward the outgroup in general. Moreover, the effects completely vanish merely one month after the intervention. Using generic machine learning techniques enable us to identify that the effect is stronger for older and less educated participants. We discuss the implications of our findings and of the protocol for future contact interventions.

Note: This chapter circulates as a working paper (Clochard et al., 2022).

Chapter 3

Presentation Chapter 3 is a meta-analysis of the experimental literature on the contact hypothesis. Following G.W. Allport’s book (Allport, 1954), a large stream of research aimed at testing the contact hypothesis. In a seminal work, Pettigrew and Tropp (2006) identified 515 studies and found that contact is, on average, effective at reducing prejudice. However, only a small fraction of the studies identified involved an experimental variation of contact, thus inducing potential selection biases - i.e. less prejudiced individuals tend to have more outgroup friends. In this chapter, I further the work of previous meta-analyses of the experimental contact literature (Paluck et al., 2019) by adding more recent papers, and I investigate the moderating effects of paper contexts and characteristics of the contact. I conclude by discussing potential paths for the future of the contact literature.

Results The main results from the meta-analysis are threefold. First, I confirm results from previous meta-analyses (Pettigrew and Tropp, 2006; Lemmer and Wagner, 2015; Paluck et al., 2019) by showing that, on average, contact interventions *are* effective at reducing prejudice. The typical intervention will have an effect of approximately 0.3

standard deviation, which is a small to moderate effect, according to Cohen (1969)'s rule of thumb. Second, there exists a large heterogeneity in what can be called a contact intervention, rendering the comparison of studies difficult. Third, conditions identified by Allport (1954), Pettigrew and Tropp (2006) or Lemmer and Wagner (2015) as moderators of the efficacy of contact do not seem to play a role, as none are selected as predictors of the treatment effect in the estimations.

Note: This chapter circulates as a working paper (Clochard, 2022).

Chapter 4

Presentation Chapter 4 investigates the effects of providing relative performance feedback (RPF) on performance. The chapter starts from one puzzle: while many experiments have implemented and tested the effects of providing RPF to participants (Azmat and Iriberry, 2010; Barankay, 2011), there is no consensus about whether RPF has positive, null or negative effects on performance (Villeval, 2020). Chapter 4 aims at analyzing this puzzle, in particular by introducing the notion of room for technological improvement. The analysis is performed in three ways. First, we review the empirical literature on RPF. Second, we propose two novel theoretical frameworks to explain the effects of providing RPF on performance, depending on the nature of the task. Third, we design and implement two experiments in which the content of RPF is similar, but the task different.

Results Our results illustrate that the effect of RPF is greatly moderated by the presence, or lack thereof, of room for technological improvement. First, the treatment effects for papers from the literature where room for improvement was present were significantly more positive. Second, our theoretical framework finds that all the distribution improves under the room for improvement, while only part of the distribution (especially at the top) improves for without room for improvement. Third, our experimental results confirm our findings, with effects all along the distribution of performances in the presence of room for improvement, and no improvement being detected without it.

Note: This chapter circulates as a working paper (Clochard et al., 2021).

Chapter 5

Presentation Chapter 5 investigates the interplay between genetics and background risk. Risk tolerance has been found to be partly explained by genetics (Cesarini et al., 2009). The dopamine receptor gene D4 (*DRD4*) has been in particular identified as influencing risk-tolerance, the 7R allele increasing risk-tolerance (Dreber et al., 2009; Kuhnen and Chiao, 2009). Moreover, background, or environmental risk, has also been shown to influence risk attitudes (Lee, 2008). Much less is known, however, about the interplay between the genetic and background risk factors in risk attitudes. We conduct

a field experiments in Northern Senegal to test the differential effect of the 7R allele on risk attitudes, depending on the exposure to the serious risk of fishing.

Results We find that, in accordance with the risk vulnerability hypothesis, individuals living in the risky area are more risk-averse than the individuals living in the non-risky area. Moreover, we corroborate the literature finding that, in both areas, the 7R allele on the DRD4 gene reduces risk-aversion, and contribute by showing that this effect comes from an additive effect, not a dominance one. Importantly, we do not find evidence of heterogeneity of the effect of the 7R allele on risk attitudes depending on the level of background risk, indicating that the effects of the DRD4 gene are independent of environmental risk. This work contributes to the emerging literature on the interplay between environmental and genetic forces in shaping human behavior.

Note: This chapter circulates as a working paper ([Clochard et al., 2022](#)).

Note

The five chapters of this dissertation are independent research articles. This is why some information may be redundant and why the term *article* is sometimes used instead of *chapter*. Chapter 1 is solo-authored. Chapter 2 is co-authored with Guillaume Hollard and Omar Sene. Chapter 3 is solo-authored. Chapter 4 is co-authored with Guillaume Hollard and Julia Wirtz. Chapter 5 is co-authored with Aby Mbengue, Clément Mettling, Birane Diouf, Charlotte Faurie, Omar Sene, Emilie Chancerel, Zoe Delporte, Guillaume Hollard, Michel Raymond and Marc Willinger.

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Chapter 1

Improving the Perception of the Police by the Youth

Abstract

While previous research has highlighted the positive consequences of a high trust in the police, parts of the French population exhibit a lack of trust toward the police. In this paper, I use a lab-in-the-field experiment in two high-schools in France to investigate the effect of a brief and controlled discussion - *contact* - between police officers and students on trust. Results indicate a positive effect of contact on trust at the individual level, i.e. toward the specific police officer met. The magnitude corresponds to an increase of approximately 0.4 standard deviation. However, the effect fails to translate to an increase in trust in the police in general. A theoretical model of belief formation can shed light on why a single contact cannot be sufficient in case of prior - negative - interactions. This paper has implications for the most widely used policy to improve the perception of the police, namely community policing.

JEL Codes: C93, C92

Keywords: Contact hypothesis, Trust, Police, Lab-in-the-field¹

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1. Introduction

The relationship between the police and parts of the population is tense in many countries, with demonstrations explicitly against the behavior of the police regularly making news headlines, sometimes even escalating to violence. Previous research has shown that trust in the police is particularly low for some segments of the population, especially the less well-off and minority citizens (Eurostat, 2015). The situation is particularly tense in France, ranking among the countries with the lowest trust in the police in Europe (Eurostat, 2015), and especially in the suburbs around Paris - *banlieues* (Roux, 2017). Yet, trust in the police is an essential part of well-functioning societies, as higher trust in the police has been associated with higher legitimacy and effectiveness of police actions (Lyons, 2002; Sunshine and Tyler, 2003; Goldsmith, 2005; Carr et al., 2007) and better capacity of the state to provide basic citizen security (Goldsmith, 2002; Desmond et al., 2016). Moreover, the literature has highlighted the possibility for the perception of the police to change, due to exogenous events. This change can be either positive (Jobard, 2016) or negative (Katz, 2014; Adam-Troian et al., 2020).

The three facts combined - low trust in the police for parts of the population, trust in the police is a public good and trust in the police can be modified - highlight the possibility to look for policy tools to improve the perception of the police. In the literature, one of the main policy tools identified to increase trust is to create personal contact (Allport, 1954; Bertrand and Duflo, 2017). The idea behind the *contact hypothesis* is that direct interactions (or contacts) improve the perception of individuals met from an out-group (here, the police) and, in turn, can increase trust toward the out-group in general. Contact is also a central component of community policing, the most common policy applied by central and local governments to improve the perception of the police.¹

In the present paper, I present the results from a pre-registered experiment² in which I use the methodology from the social psychology literature (Aron et al., 1997) to investigate whether face-to-face discussions between police officers and high-school students in relatively poor towns near Paris can increase trust.

In the experiment, subjects are randomly paired with either a police officer or a Bachelor student from the area (representing the in-group) and are randomly assigned to one of three treatments: a control group in which subjects are not told any information about the person they are paired with; a photo treatment, in which subjects are presented the photo of their pair; and a contact treatment, in which students talk for 10 minutes with their pair. The treatment is an adaptation of the “fast-friend” procedure (Aron et al., 1997) for quickly generating closeness: we ask pairs to alternately answer questions which

¹See for instance the New York Police Department’s “Neighborhood Policing Initiative”, the London Police’s “Community Policing”, the “Police de Proximité” in France.

²<https://www.socialsciregistry.org/trials/7116>

become more and more intimate.

I find that the contact protocol has a positive effect on the amount sent in a trust game with the specific police officer met. The effect is statistically significant and the magnitude is relatively large - corresponding to an increase of approximately 0.4 standard deviations. However, I find no effect at the collective level: subjects in the control group do not send more tokens in a trust game played with a randomly-selected police officer, nor do they show less bias against the police in a novel Implicit Association Test.

This paper contributes to two strands of literature. First, it contributes to the literature on methods to improve police-population relations. For instance, [Peyton et al. \(2019\)](#) find that a brief visit of a police officer to citizens' doors to discuss methods of improvement of policing in their neighborhoods improves the perception of the police. The fact that no effect is found at the collective level could be an indication that discussions about policing are necessary to translate the effect towards the out-group in general, although this result would need to be confirmed by future research. Regarding the recurring political debate about proximity or community policing, results from the present paper imply that the contact can improve relations at the individual level, and might be an argument in favor of having officers patrolling the same neighborhoods regularly.

Second, I contribute to the literature on the contact hypothesis. I show that even a brief, cheap and easy to replicate contact can have a positive effect on trust at the individual level, which is an advantage relative to previous protocols which were much longer in time, and therefore potentially difficult to scale-up ([Scacco and Warren, 2018](#); [Mousa, 2020](#); [Lowe, 2021](#)). This paper is also one of the first attempts to illustrate theoretically why the effect at the individual level does not translate to the out-group, in particular if participants have had several (potentially negative) interactions with out-group members ([Page-Gould et al., 2008](#); [Clochard et al., 2022](#)).

The remainder of the paper is organized as follows. In Section 2., I review the relevant literature in police-population relations and the contact hypothesis. In Section 3., I present the experimental design of the experiment I conducted and the data. I present empirical results, as well as a theoretical framework which can explain some findings in Section 4.. Section 5. concludes.

2. Literature review

This paper is linked to two main strands of literature: the first is the literature on trust in the police by the population, the second is the literature on the contact hypothesis.

2.1. Police-population relations

While the economic literature on police-population relations is relatively scarce,³ a relatively large number of papers tackle this issue in the sociology and social psychology literature - see for instance [Brown and Benedict \(2002\)](#); [Hagan et al. \(2005\)](#) or [Bolger et al. \(2021\)](#) for a detailed meta-analysis. In general, this literature focuses on finding factors, such as socioeconomic status, age or race which correlate with trust or satisfaction in the police. In particular, the literature has found that trust in the police tends to be lower for individuals who are younger, economically disadvantaged and from minority groups ([Roux, 2017](#); [Roché et al., 2020](#)). The (experimental) literature on how to *improve* trust in the police, however, is scarce.

Moreover, it has been shown using exogenous events that trust in the police is not constant over time. For instance, in the French context, [Jobard \(2016\)](#) highlighted that following the Paris terrorist attacks of 2015, trust in the police increased significantly, while [Adam-Troian et al. \(2020\)](#) found that after incidents involving the police during demonstrations of the *Yellow Vests* movement, trust in the police had decreased for demonstrators. Similar results have been found for negative events involving police officers in other parts of the world.⁴ Moreover, [Simpson \(2021\)](#) shows that simply displaying pictures of smiling police officers improves the perceptions of these officers, relative to neutral faces. The fact that trust in the police is malleable represents an opportunity for policy, as it implies that it might be possible to find policy tools to increase trust in the police. In this paper, I contribute by showing that trust in police officers can be purposefully changed.

In this regard, using contact is relevant for two reasons. First, contact has been widely viewed in the discrimination and prejudice literature as the main policy tool to reduce prejudice and increase trust ([Bertrand and Duflo, 2017](#); [Paluck et al., 2019](#)). Second, meetings with the population outside the “standard” interactions with the police - e.g. investigations and arrests - are a central piece of a policy which has been implemented in many parts of the world, namely community policing. However, little experimental evidence exists about the effect of community policing policies on citizens’ trust in the police, with two main exceptions. The first exception is [Peyton et al. \(2019\)](#) who investigate the effect of an intervention by the New Haven, CT police department, in which patrol officers went door-to-door to gather information from the public about how they felt the image of the police could be improved. They found that this intervention significantly improved the views of the population. The second exception is [Blair et al. \(2020\)](#) who investigate several community policing initiatives in the Global South, and find very limited effects on public perceptions of the police, measured through surveys. The present paper contributes

³The economic literature on the police force in general is not scarce, see for instance [Ba et al. \(2021\)](#); [Ang \(2021\)](#) or [Fryer Jr \(2019\)](#).

⁴For instance, [Katz \(2014\)](#) highlights a deteriorated trust in the police for African-Americans following the deaths of Michael Brown and Eric Garner.

to this literature by directly investigating the effects of a discussion between high-school students and police officers on the perceptions of the police officers in question, and of the police force in general. In particular, this paper shows that it is possible to *voluntarily change* the perception of police officers.

2.2. Contact hypothesis

The second strand of literature the present paper contributes to is the literature on the contact hypothesis. The hypothesis was first coined by Gregory Allport in 1964, stating that “Under specific conditions, personal contact can reduce prejudice and increase trust” (Allport, 1954). The following decades saw a lot of descriptive papers trying to assess the validity of the hypothesis, but until the late 2010s, this literature lacked experimental evidence and therefore suffers from potential significant biases (Pettigrew and Tropp, 2006; Paluck et al., 2019).

Since then, a growing number of experiments or quasi-experiments have been analyzed and have highlighted the potential of contact interventions to improve cross-group relations in different contexts. In the context of education, Rao (2019) showed that an intervention to increase the share of poor pupils in primary schools in Delhi improved their perception by better-off children; Scacco and Warren (2018) found that having students perform tasks with members of another religion in Nigeria reduced discrimination and increased generosity towards the out-group; Boisjoly et al. (2006) and Corno et al. (2019) found that having a Black roommate reduces White students’ prejudice in an American and South African University, respectively. Another context in which the contact hypothesis has been studied is through army recruits: Carrell et al. (2015) found that White recruits of the US Air Force Academy are more likely to choose a Black roommate for the second year if they had a Black recruit in their squadron; Finseraas et al. (2019) found an increase in trust for a generic minority after having a minority roommate during training; Cáceres-Delpiano et al. (2021) find that Spanish men born in regions with a weak Spanish identity who served their military service in another region have increased identification as Spanish. The last main context in which contact interventions have been applied is sports, with Mousa (2020) finding that after playing in mixed-religious teams, Iraqi Christians are more tolerant towards the Muslim players of their teams, although the effect, as in the present paper, does not translate to the out-group in general. Lowe (2021) found that playing in mixed-caste teams increases cross-caste friendships and trade efficiency, but adversarial contact (playing *against* other-caste teams) reduces these effects. Meta-analytic work (Paluck et al., 2019) has shown that on average, contact seems to be effective at reducing prejudice and discrimination, at least towards members of the out-group participants specifically met. In the broader discrimination and prejudice literature, contact has therefore started to be seen as one of the best (if not only) tools to

increase inter-group cooperation and trust (Bertrand and Duflo, 2017).

However, as highlighted in a recent review (Paluck et al., 2021), the literature on contact suffers from four main limitations. The first limitation of the literature is the small sample sizes in most interventions. My sample consists of more than 360 students, thus putting the present paper in the top fifth of sample sizes as counted by the review. A second limitation of the literature is to focus on survey measures, with no repercussion for dishonest answers, and therefore potentially suffering from experimenter-demand effect (Zizzo, 2010). In this paper, I use an incentivized outcome - specifically the trust game (Berg et al., 1995) - and an Implicit Association Test (Greenwald et al., 1998), in which participants are primed to answer as quickly as possible, not on the outcome of the test (which would be subject to experimenter demand effect). The third and perhaps most significant limitation of the literature is the difficulty to replicate the setups in which the studies were conducted. For instance, the sports league conducted by Mousa (2020) and Lowe (2021) lasted for several weeks, it might be complicated to have entire populations joining the military as in Carrell et al. (2015) or Finseraas et al. (2019). The protocol presented in the present paper, which is an adaptation of Aron et al. (1997), is more replicable as it is much shorter, and does not require elaborate settings. The fourth and final limitation of the literature on the contact hypothesis is the lack of a general theoretical framework of why contact may have an effect. This paper contributes to this in proposing a model of belief formation which can explain why contact can have an effect at the individual level, which, however, fails to translate to the out-group in general, a result that has been found in the literature (Mousa, 2020; Clochard et al., 2022).

3. Experimental Design and Data

Context The experiment took place in March 2021 in two high-schools in the Paris region, in the towns of Saint-Denis and Corbeil-Essonnes (see a map in Appendix A). The high-schools were selected because they are located in towns which are relatively impoverished (37% and 26%, respectively, of the population live below the national poverty rate, relative to 15% nationwide), with a large share of immigrants⁵ (39% and 27%, respectively, relative to 9.6% nationwide) and have a population which is relatively younger than the rest of the country (about 45% of residents are below 29 years old in both towns, relative to 30% for the whole country). According to the literature, the population of these towns is therefore likely to distrust police more than the country average (Roux, 2017; Roché et al., 2020). Indeed, clashes between parts of the population and police

⁵Under French law, it is illegal to ask individuals about their ethnicity or race. The only distinction allowed in France regards the nationality and place of birth. The figures presented here represent the share of immigrants, which are defined as individuals born outside of France, whose nationality of birth is not French and who currently resides in France.

officers have occurred in the past in both towns.⁶

Setup With approval from high-school administrations, participation was mandatory for students (provided the teacher had given their approval), and sessions were conducted during school time. The sample consisted in 366 high-school students, which were on average 17 years old and were selected from all curricula (general, technological and professional). Participants, being minors, were not financially compensated, but they were incentivized using grades. At the end of the experiment, one game was selected at random and determined the number of tokens earned by each participant. The higher the number of tokens, the higher the grade. Participants were guaranteed a show-up grade of 10 out of 20. For each additional token, half a point was awarded.

The data was collected on tablets using the o-Tree software (Chen et al., 2016).

Treatments Upon arrival, students were randomly allocated to one of three treatment arms. The first treatment arm ($N=92$) is a control, the second ($N=145$) is the *Photo* treatment, and the third ($N = 129$) is the *Contact* treatment. In the *Photo* and *Contact* treatment arms, subjects were paired either with a police officer or with a first-year university student who grew up in Paris' suburbs. Treatments are summarized in Figure 1.1. The treatments resemble the protocol set up in a previous paper (Clochard et al., 2022).

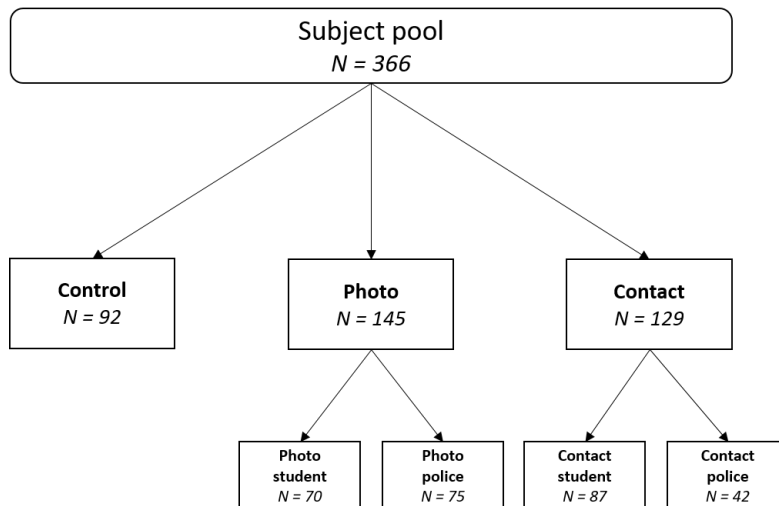


Figure 1.1 – Treatment arms

In the *Control* group, participants are not told who they are going to play with - they are told that they are not playing with someone from the class. The *Control* condition is used to have a measure of average trust in the specific group.

In the *Photo* treatment, participants are shown the photo of their pair, and told whether their pair is a police officer or a student. The *Photo* treatment is assumed not to

⁶See for instance [this article](#) in Corbeil-Essonnes or [this article](#) in Saint-Denis.

have any effect on the outcomes of interest (i.e. one is not to trust more or less the police simply by being shown a photo of a police officer), but to test for pre-existing differences of trust between police officers and students, thus testing whether participants exhibit a form of in-group (or out-group) bias.

In the *Contact* treatment, participants met their pair face to face, and both alternately answer progressively more personal questions. The questions are drawn from the methodology used by Aron et al. (1997), which has been proven to create friendships very quickly. The original protocol is adapted so that discussions last 10 minutes. In details, each pair has to answer one question from each of the three sets of questions from Aron et al. (1997). In the first set (“light closeness”), an example of question drawn is “Would you like to be famous? In what way?”, while in the last set (“intense closeness”), the questions are much more intimate, e.g. “Of all the people in your family, whose death would you find most disturbing? Why?”. All questions are presented in Appendix B, and the questions they had to answer were drawn at random within each set.

Outcomes As stipulated in the pre-analysis plan, the analysis focuses on three primary outcomes. The first outcome is a standard Trust Game (Berg et al., 1995). In the trust game, participants - playing the role of the *truster* are endowed with 10 tokens. They choose a number of tokens to be sent to the other player. Each token is then multiplied by 3, and the other player - the *trustee* - chooses how many tokens to send back to the *truster*. The measure of trust used is the share of tokens sent by the *truster*, with an increase associated with a higher degree of trust. The first outcome - *Trust Pair* - is the result of the game played with the pair. This outcome captures the effect of contact on trust towards the individual met.

The second outcome - *Trust Police* - is again measured via a Trust Game, with a random policeman. Specifically, they were told that a group of police officers from the Paris region - *Ile-de-France* - have played the trust game with high-school students from Paris’ suburbs and have declared how many tokens they are willing to send back for each possible amount of tokens sent. One of their answers has been randomly selected and will be used to determine the participant’s gains. This outcome is used to test the effect of contact on the police as a whole, not specifically on the individual met.

The third outcome is the result of a novel version of the Implicit Association Test (Greenwald et al., 1998) - a measure commonly used in social psychology to measure implicit stereotypes, regarding ethnicity, race, gender, sexual identity or disability - in which the two categories compared were the police and health services. Participants were instructed to associate as fast as possible (but without mistakes) images of the police and health services with either negative or positive words. First - after some training rounds - participants were instructed to associate the police with negative words, and health services with positive rounds. Second - after some more training rounds - the places for

the police and health services were reversed. The outcome used - *IAT* - is the difference between the two response times, divided by the standard deviation of times from a pilot study with a different class in the first high-school. The variable is coded so that a higher *IAT* variable is associated to a stronger association between police images and positive words.⁷ The variable is used to test the effect of contact on subconscious association of the police to bad or good.

To summarize, the first outcome - *Trust Pair* - maps the effect of contact at the individual level, while the two other outcomes - *Trust Police* and *IAT* map the effect of contact at the collective level.

Estimation strategy I estimate a cross-treatment OLS regressions for each of the three outcomes (Equation 1.1) . The dependent variables are the two treatments (*Contact* and *Photo*) and an interaction of each treatment and a dummy equal to 1 if the participant is paired with a police officer. Because in the control treatment, participants are not told anything about the participants, the variable *Police* is by default set to 0. This means that the β_2 coefficient in Equation 1.1 identifies the interaction between the *Photo* treatment and the *Police* variable. Standard errors are clustered at the class level.

Controls include age, education and whether the participant was victim of a set of crimes and misdemeanors. I also included a question known as an instructional manipulation check, typically used in online experiments (Hauser and Schwarz, 2016), used to measure attention.⁸

$$Y = \alpha + \beta_1 Contact + \beta_2 Police + \beta_3 Contact \times Police + \beta_4 Photo + \gamma X + \epsilon \quad (1.1)$$

The main coefficient of interest is β_3 . A positive β_3 would indicate that participants who met a police officer tend to exhibit more trust in their partner than average. A negative β_2 would indicate that police officers tend to be trusted less than average for participants the *Photo* treatment (i.e. a negative out-group bias). β_1 represents the treatment effect of contact for individuals meeting a student, while β_4 evaluates whether there is a difference between the average level of trust (in the control group) and the level of trust in students (or in-group bias).

⁷i.e. a shorter response time for the participant to associate the police with positive words than negative words.

⁸The question was: “In high-school, it is very common for students to have a preferred subject. We would like to know what is your favorite subject, but also check that you read questions carefully. To show that you have read this question well, please disregard the following question and select *Civics education*. What is your favorite subject of study?”

Discussion of protocol There are several points which might need to be clarified regarding the implemented protocol. First, participating police officers are clearly a selected sample and not representative of the police force. All participating officers are members of an association which aims at improving the dialogue between the police and citizens, meaning that it is relatively safe to assume that they have a more proactive attitude to discussions than the average police officer. This fact can be a threat for the interpretation of the results, in particular with respect to the external validity of the protocol. However, it can also be a strength of the protocol, especially in regards to the theoretical framework presented below, as we can assume that contacts will be positive.

The second point worth highlighting regards the race of police officers and students. The relationship between the race/ethnicity of the population and/or police officers and the perceptions of the police has been a focus of a large share of the police-population relations literature - see for instance [Antonopoulos \(2003\)](#); [Hasisi and Weitzer \(2007\)](#); [Brunson and Weitzer \(2009\)](#). The context of France is very specific compared to many other countries, particularly the US, because ethnic/racial statistics are forbidden: I therefore do not have any individual information about ethnicity or race.⁹ As mentioned above, it is possible to say though, that both high-schools are located in towns with a relatively large share of immigrants.

The third and most significant issue with the present protocol is the fact that all outcomes are measured right after the end of the intervention, and I do not have no measure of outcomes months - or even weeks - after the intervention. The lack of evidence of lasting effects of contact has been identified as a weakness of the contact interventions ([Paluck et al., 2021](#)). I originally intended to collect information one month after the intervention for one high-school. However, due to sanitary restrictions to tackle the spread of COVID-19, high-schools in France were closed for the entire month of April 2021,¹⁰ and data collection had to be canceled.

The fourth point worth mentioning about the paper is the effect of contact on trust of *police officers* towards students. As with students, it is likely that trust by the police is also affected by the protocol. However, I am not able to evaluate this effect for multiple reasons. First, only a limited number of police officers (seven, to be precise) participates in the experiment. I therefore would not have enough power to detect an effect. Second, each police officer meets several students, therefore identifying the effect of each contact would be tricky. Third, even if the effect of contact on trust of police officers was measurable, participating police officers, as mentioned above, are selected, and the result of the experiment could not easily be generalized to the police as a whole.

⁹The only question legally allowed to be asked relates to the nationality of parents. However, administrations of the high-schools did not allow the collection of these sensitive data, as most students were minor and they feared the questions could make some students nervous.

¹⁰<https://www.education.gouv.fr/covid-19-les-mesures-en-vigueur-dans-les-ecoles-colleges-et-lycees-partir-du-5-avril-2021-322868>

4. Results

Descriptive statistics are displayed in Appendix C. On average, participants were 17 years old, with a relative majority of girls (60%). About one third of participants (35%) declared at least one negative past encounter with the police, and 21 percent declare that they have been discriminated against.

The treatments are well balanced (Appendix D) across nearly all characteristics. The only exception relates to the attention variable, with participants in the *Contact* treatment paying relatively less attention than others.

4.1. Primary results

In Table 1.1, I display the results of the estimations for the three outcomes. In accordance with the pre-analysis plan, I corrected p-values for three one-sided tests, corresponding to a modification of 2/3 of standard p-value thresholds. Normalized treatment effects for all three outcomes are also displayed in Figure 1.2. Raw averages for the three outcomes for all treatments are displayed in Appendix E.

Table 1.1 – Treatment effect on primary outcomes

	(1) Trust Pair	(2) Trust Police	(3) IAT
Contact	0.031 (0.040)	-0.001 (0.032)	-0.274* (0.143)
Police	-0.050 (0.033)	-0.008 (0.042)	0.061 (0.167)
Contact × Police	0.086** (0.038)	0.013 (0.062)	0.022 (0.400)
Photo	0.023 (0.034)	-0.010 (0.043)	-0.134 (0.166)
Constant	0.427 (0.309)	0.088 (0.289)	0.294 (0.983)
Controls	Yes	Yes	Yes
R^2	0.091	0.071	0.077
No. obs	359	359	359
Mean Control	0.338	0.385	-0.665
Std dev. Control	0.223	0.236	0.627

Corrected p-values for three one-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column 1, the outcome variable is the amount sent in the trust game with the individual met, in column 2, the outcome is the amount sent in a trust game with a random police officer. In column 3, the outcome is the result of the Implicit Association Test. Controls include gender, level of education, age, indicators of whether the participant was victim of certain crimes and misdemeanors and the level of attention. Standard errors are clustered at the class level.

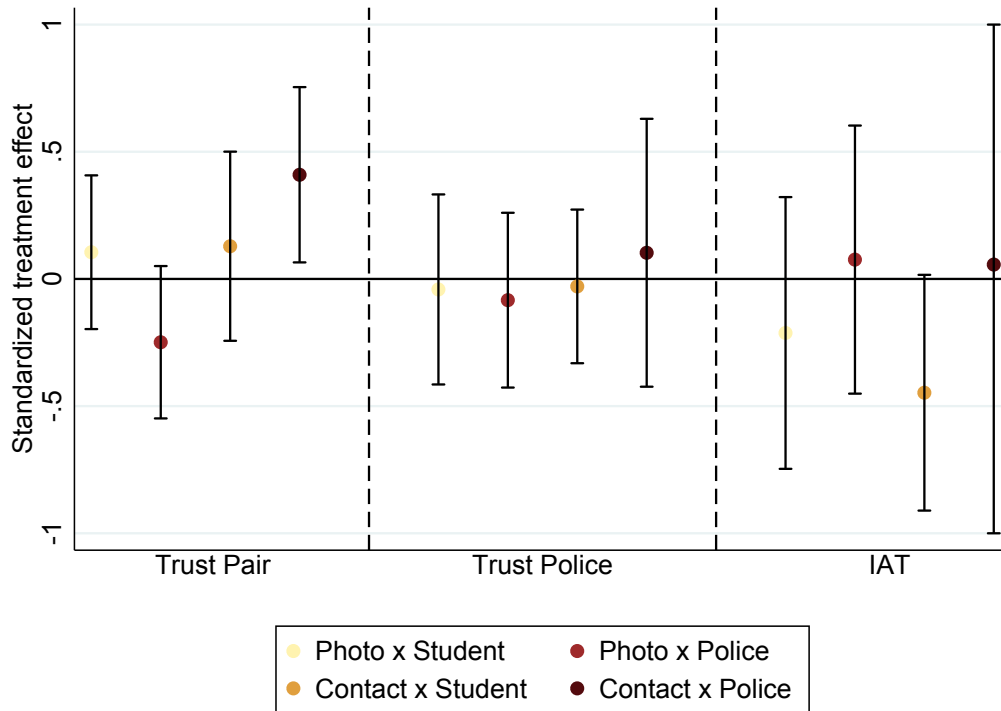


Figure 1.2 – Normalized treatment effects for the three outcomes

From column 1 and the left-hand panel of Figure 1.2, it appears that the contact has an effect on the amount sent in the trust game played with the person met only if participants met a police officer. The effect is significant at the 5 percent level (Corrected- $p = 0.044$), and the magnitude is large - corresponding to an increase of 0.38 standard deviations. Being presented a photo of a police officer has a relatively negative effect on trust, although the difference is not significant ($p = 0.14$). The lower trust in the *Photo × Police* treatment indicates a slight negative prior feeling toward police officers, relative to the average level of trust in the control group.

The results therefore indicate that contact with a police officer has a positive effect on trust at the individual level - i.e. towards the specific police officer met. The difference between the *Photo × Police* and *Contact × Police* coefficients is highly significant ($p < 0.01$). Having a contact with a student appears not to have an effect on trust, indicating that there is a differentiated effect of contact depending on the person met.

However, the positive individual-level results are not carried over to a change in trust toward the police in general, as captured by the results presented in columns 2 and 3 of Table 1.1. Column 2 and the middle panel of Figure 1.2 present treatment effects for the trust game played with a “random” police officer, while column 3 and the right-hand panel of Figure 1.2 present the treatment effects on the Implicit Association Test. The point estimates of the effect of a contact with a police officer are in both cases positive, but the effect is clearly insignificant.

The primary results therefore indicate that while contact with a police officer has an

effect on trust toward the specific police officer met, the effect fails to translate to an increase in trust toward the police in general.

In the following Section, I present an exploratory theoretical framework to understand how contact can have a positive effect at the individual level but this effect is not translated at the collective level.

4.2. Theoretical framework and empirical test

In this Section, I develop a model of belief formation which could explain why contact can have an effect at the individual level - an increase of trust toward the specific police officers met - but the effect is not observed at the collective level - no increase of trust toward the police in general. The main assumption of the model is that individuals have received, prior to the contact, a limited number of signals from the other group.

Setup

I assume that an agent - in the experimental setup, a student - has to evaluate the value - trustworthiness - of a police officer. The trustworthiness of the police officer is a random variable denoted by $x \in \{0, 1\}$, which I assume to be a Bernoulli variable taking the value 1 with a probability θ . I also assume that the parameter θ is unknown and that it is drawn from a uniform distribution over $[0, 1]$. The agent updates her beliefs using Bayes' rule.¹¹

Prior to the interaction we are focusing on, I assume that the agent has received $n \in \mathbb{N}$ i.i.d. signals (previous interactions with police officers), denoted (x_1, \dots, x_n) .

The likelihood is

$$p(x|\theta) = \theta^{\sum_{i=1}^n x_i} \times (1 - \theta)^{n - \sum_{i=1}^n x_i} \quad (1.2)$$

The prior for the value of θ is $p(\theta) = 1$ (uniform distribution).

Using Bayes' rule, the posterior is therefore

$$p(\theta|x) \propto \theta^{(\sum_{i=1}^n x_i + 1) - 1} \times (1 - \theta)^{(n - \sum_{i=1}^n x_i + 1) - 1} \quad (1.3)$$

The estimated value of θ thus follows a Beta distribution with parameters $(\sum_{i=1}^n x_i + 1, n + 1 - \sum_{i=1}^n x_i)$ (Figure 1.3).

The expected value of θ is

$$\theta_n = \frac{1 + \sum_{i=1}^n x_i}{n + 2} \quad (1.4)$$

¹¹I therefore consider that all signals have the same weight, and no other factors - such as similarity (Bordalo et al., 2021) - enter into the beliefs.

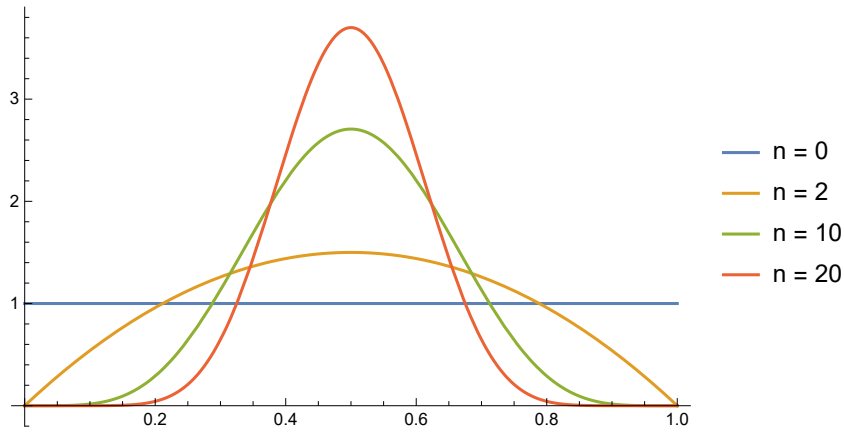


Figure 1.3 – Densities of probability for θ , depending on the value of n ($\theta = 0.5$ and $\frac{\sum x_i}{n} = \frac{1}{2}$)

Effect of contact

I assume that the contact works as a new, independent signal x_{n+1} (perfectly observed) received.

Without contact, the expected trustworthiness of the police officer is the expected value of the parameter θ , θ_n . At the individual level, after contact, the trustworthiness is perfectly observed, therefore the treatment effect at the individual level should be

$$x_{n+1} - \theta_n \quad (1.5)$$

The new signal changes the estimated value of θ to a Beta distribution with parameters $\sum_{i=1}^{n+1} x_i + 1$ and $n + 2 - \sum_{i=1}^{n+1} x_i$. The expected value of θ after receiving the $n + 1$ -th signal becomes

$$\theta_{n+1} = \frac{1 + \sum_{i=1}^{n+1} x_i}{n + 3} \quad (1.6)$$

The treatment effect at the collective level - i.e. the difference between estimations of the expected trustworthiness of the group before and after the signal - is

$$\begin{aligned} \theta_{n+1} - \theta_n &= \frac{1 + \sum_{i=1}^{n+1} x_i}{n + 3} - \frac{1 + \sum_{i=1}^n x_i}{n + 2} \\ &= \frac{x_{n+1} - \theta_n}{n + 3} \end{aligned} \quad (1.7)$$

The treatment effect at the collective level is thus equal to the treatment effect at the individual level, deflated by a factor $n + 3$. The number of prior interactions is therefore predicted to have a major influence on the treatment effect at the collective level (Figure 1.4).

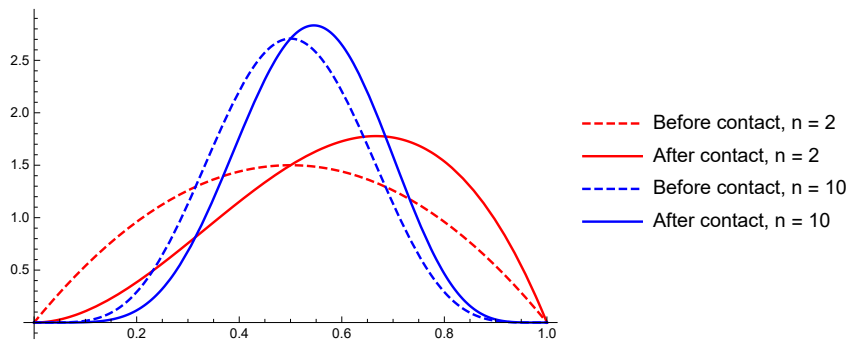


Figure 1.4 – Difference of the effect of a positive contact ($x_{n+1} = 1$), depending on the value of n

Discussion of the model

There are three points worth highlighting regarding the present theoretical framework. First, I assume that the trustworthiness of the police officer is a binary variable. This assumption is a simplification, as the main result - the collective effect being of the same size, but of a smaller magnitude than the individual effect - would hold for any distribution. Second, I assume that the beliefs about the out-group is correct, given all received signals. This assumption therefore excludes potential bias connected to incorrect beliefs (Bursztyn and Yang, 2021) - although it is likely that the expected value of the trustworthiness θ_n differs from the true value θ due to sampling issues. Third, I assume that all signals, including the contact, are equally weighed by the agent, which is a standard consequence of the Bayesian updating process. This means that I do not consider non-standard channels which would be path-dependent, such as representativeness (Bordalo et al., 2016, 2021), attention (Kohlhas and Walther, 2021) or overconfidence (Rabin and Schrag, 1999).

Empirical relevance of the model

The presented theoretical framework predicts that the effect of contact at the collective level - trust towards the police - should be of the same sign as the treatment effect at the individual level - trust towards the person met, and the magnitude should be lower.

Although it is difficult to observe directly the number of past interactions with police officers (and especially whether they were positive or not), but in the questionnaire, participants were asked to give the number of identity controls they were subjected to in the past three years.¹² In what follows, I use the number of identity controls as a lower bound for the number of prior interactions with police officers (n in the model).

The distribution of answers (Figure 1.5) is heavily skewed, with approximately 60 percent of respondents declaring they have not been subjected to an identity check, and

¹²identity controls are widely used by the French police force to deter criminality. They have been the source of a large political and societal debate, both about their effectiveness (Tiratelli et al., 2018) and about the treatment of minority citizens (Beauchemin et al., 2016; Roché, 2016).

several participants declaring they had been subject to more than 20 checks. The average value of the number of identity controls is 2.

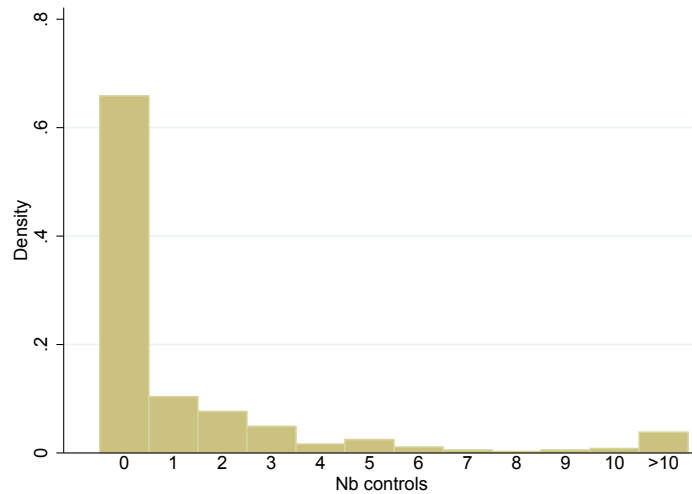


Figure 1.5 – Distribution of the number of identity controls in the three years prior to the experiment

The prediction of the model is that the effect of contact at the individual level should be $n + 3$ times as large as the effect at the collective level. Using $n = 2$ as a lower bound, we should therefore observe an individual effect which is a little more than 5 times as large as the collective effect. When comparing the two point estimates, we find that the effect is approximately 6.6, which is not far from the theoretical prediction (although the point estimates are very noisy).

An additional remark is that if the point estimate is correct, the statistical power of the experiment is simply too small to detect it. Results from a quick sample calculation indicate that in order to be able to detect an effect size of approximately 0.07 standard deviation, the sample required to reach a power of 0.80 is approximately 5,000 observations, or more than 13 times the sample size of this experiment. This experiment in this case is therefore clearly not powered enough to detect an effect on contact on trust at the collective level.

4.3. Exploratory results

In this Section, I present results from estimations which were not included in the pre-analysis plan, but which could be an avenue for future research.

Other measures of trust in the police In Table 1.2, I analyze the effect of contact with a police officer on *stated* measures of trust in the police. In the first three columns, participants were asked to state whether they agree with several statements. In column 1, participants were asked whether, should they be victim of a crime, they would be certain

of reporting it to the police. In column 2, they were asked whether they believe the career of police officer to be honorable. In column 3, they were asked whether they are considering becoming a police officer in the future. In column 4, they were asked whether they believe the police to be violent - the outcome in Table 1.2 is reversed so as to move in the same direction as the other outcomes.

As can be seen in Table 1.2, results indicate that the treatment has no effect on these measures of beliefs about police quality. This result is another indication that contact does not appear to have an effect on the police in general.

Table 1.2 – Treatment effect on views of the police

	(1) Likelihood to report	(2) Police honorable	(3) Police career	(4) Police non violent
Contact	0.003 (0.142)	0.179 (0.114)	0.101 (0.092)	-0.060 (0.096)
Police	0.121 (0.169)	0.102 (0.171)	0.032 (0.086)	0.087 (0.139)
Contact × Police	-0.279 (0.171)	0.084 (0.197)	0.197 (0.212)	0.147 (0.153)
Photo	-0.058 (0.146)	-0.033 (0.128)	0.017 (0.071)	-0.140 (0.103)
Constant	2.521** (1.096)	4.470*** (0.838)	1.009 (0.711)	3.461*** (1.009)
R^2	0.109	0.080	0.108	0.101
No. obs	359	359	359	359
Mean Control	3.022	2.750	1.141	2.522
Std dev. Control	0.877	0.721	0.434	0.718

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For columns 1 through 3, the outcome is a variable from 1 to 4 on whether participants agree with the statement. In column 1, the statement is: Imagine in the future you are victim of theft. You are certain to report it to the police. In column 2, the statement is: I believe that police officer is an honorable career. In column 3, the statement is: I am considering a career as a police officer for my future. In the last column, the statement was: I believe police officers are violent. The outcome presented here is the opposite of the answer of participants (i.e. their disagreement with the statement). Standard errors are clustered at the class level.

Heterogeneity analysis Results from an heterogeneity analysis, with respect to prior police interactions and gender, are presented in Table 1.3. In the Table, the variables of interest are the triple interactions of *Contact*, *Police* and the heterogeneous variable.

In column 1 the heterogeneity variable is gender. Results on the main treatment effect become insignificant, and the treatment effect seem not to vary much on gender.

Results with police controls, presented in column 2, are more interesting. The variable of heterogeneity is a dummy variable on whether participants have had any identity controls over the past three years. Although insignificant, they indicate that the treatment effect is reduced for participants subject to identity controls, with the coefficients $Contact \times Police$ and $Contact \times Police \times Controls$ almost canceling each other entirely.

Table 1.3 – Heterogeneous treatment effect

	(1) Gender	(2) Police interactions
Contact	0.036 (0.062)	0.006 (0.047)
Police	-0.071 (0.063)	-0.058 (0.042)
Controls		-0.041 (0.042)
Police \times Controls		0.014 (0.060)
Contact \times Police \times Controls		-0.102 (0.111)
Contact \times Police	0.101 (0.088)	0.124* (0.063)
Female	-0.082 (0.051)	
Police \times Female	0.027 (0.072)	
Contact \times Police \times Female	-0.018 (0.127)	
Photo	0.024 (0.036)	0.025 (0.033)
Constant	0.496 (0.316)	0.441 (0.305)
R^2	0.083	0.088
No. obs	359	359
Mean Control	0.338	0.338
Std dev. Control	0.223	0.223

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable is the trust in the pair. *Female* is coded as 1 for girls, 0 for boys. Standard errors are clustered at the class level.

In Figure 1.6 are plotted the coefficients of the quantile regressions for the $Contact \times Police$ variable. The dependent variable is the share of tokens sent in the trust game played with the pair. The estimations indicate that there is no significant difference between deciles.

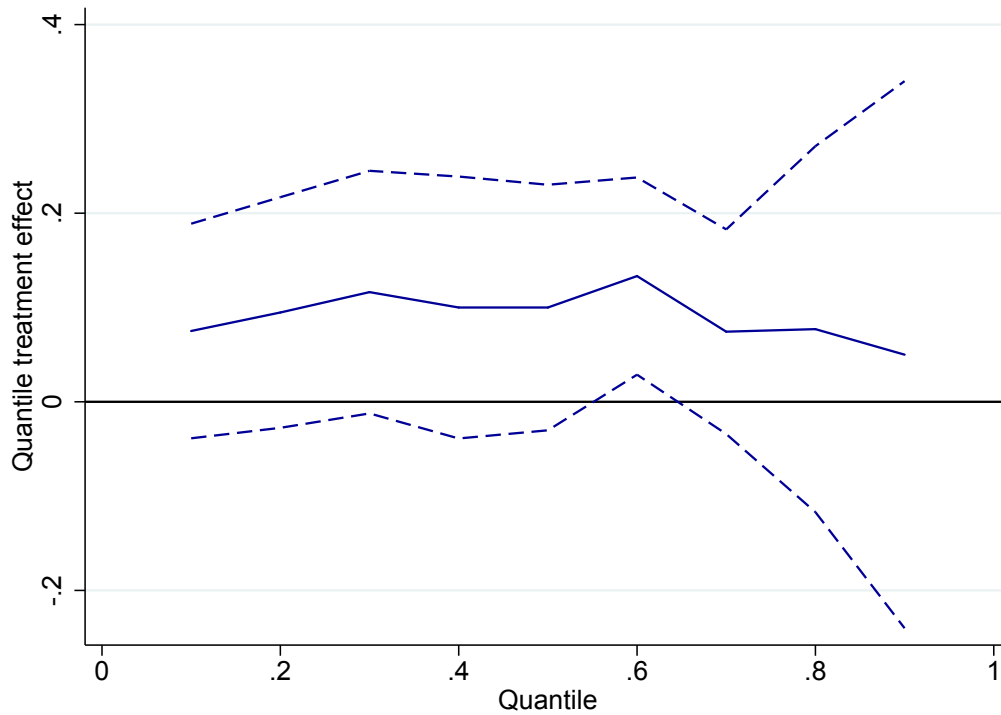


Figure 1.6 – Coefficients of $Contact \times Police$ in quantile regressions. The dotted lines represent 95% confidence intervals.

5. Conclusion

In this paper, I test the effect of a brief contact, borrowing from the highly replicable “fast-friend” procedure (Aron et al., 1997), between police officers and high-school students in two French high-schools. I show that the level of trust toward the specific police officer met (what I call in the paper the effect of contact at the *individual level*) is significantly positive, and the magnitude of the effect is important (corresponding to a 0.39 deviation increase).

However, the positive effect of contact at the individual level fails to translate at the collective level, either using a measure of trust towards a *random* police officer, or with a novel measure of an Implicit Association Test. The theoretical framework presented in Section 4. can help understand this fact, with a decrease of contact effects due to prior interactions with police officers.

The results presented above - contact having a positive effect at the individual level but no effect at the collective level - point an avenue for future research on the contact hypothesis. In particular, there is a lack of consistency of results regarding the effect of contact at the collective level: in several contexts, contact has been found to have a positive effect towards the out-group in general (Carrell et al., 2015; Corno et al., 2019; Lowe, 2021) while in other contexts the positive effects of contact have been found only for the out-group members specifically met (Mousa (2020); Clochard et al. (2022)); the present pa-

per). Further efforts should be made to understand what factors (context, nature and/or duration of contact, representativeness of met out-group members, etc) can translate the positive effects of contact to out-group members in general. The theoretical framework presented here, showing that a crucial factor seems to be pre-existing interactions with the out-group, could be a first step in this direction. The question of whether meeting an individual can change the perception of the entire out-group has been the focus of several papers in social psychology, with for instance the work on person-positivity bias (Miller and Felicio, 1990), but the literature does not offer a robust answer yet.

Another important avenue for future research is the literature on the contact hypothesis is to further investigate *how* contact can change perceptions. As presented in Section 4.3., it does not appear that the change in behavior can be attributed to a change in the beliefs about the quality of the police. In Appendix F, I present preliminary results from estimations of a change in beliefs and altruism due to the treatment. Although the results presented here are insignificant, it could be a first step to understand channels through which contact is effective.

Moreover, taken at face value, the results presented in this paper also highlight a potential benefit of community policing policies. Community policy typically entails having specific police officers routinely patrolling the same neighborhoods and interacting with citizens on issues outside the scope of standard law enforcement. If the results from the present paper replicate to these situations, community policing policies have the potential to increase trust toward the specific police officers patrolling given neighborhoods, which in turn could be a potential benefit to local communities, as trust in police officers has been shown to increase the probability to contact police when a crime has been committed (Carr et al., 2007).

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Appendices

A Experiment location

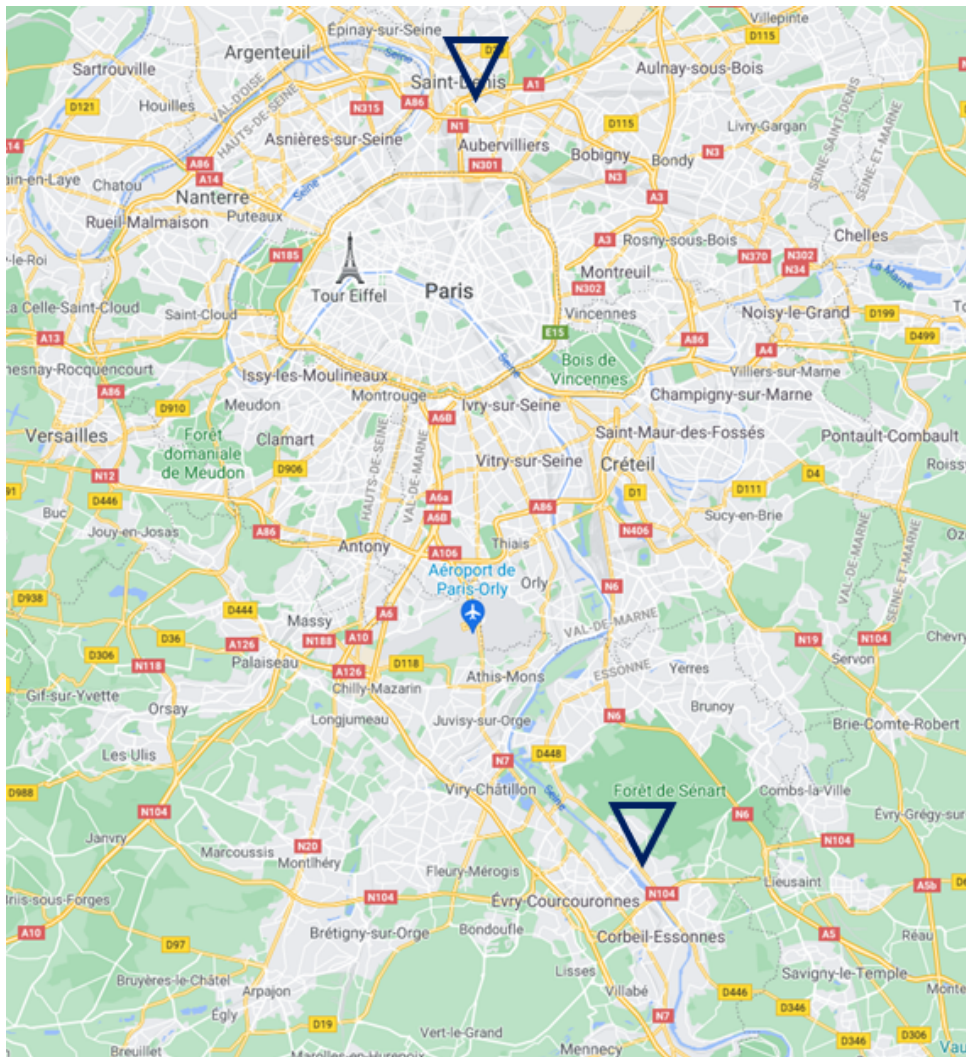


Figure A.1 – Locations of the two high-schools

B Questions for the *Contact* treatment

Set I (light closeness)

1. Given the choice of anyone in the world, whom would you want as a dinner guest?
2. Would you like to be famous? In what way?
3. Before making a telephone call, do you ever rehearse what you are going to say? Why?
4. What would constitute a “perfect” day for you?
5. When did you last sing to yourself? To someone else?
6. If you were able to live to the age of 90 and retain either the mind or body of a 30-year-old for the last 60 years of your life, which would you want?
7. Do you have a secret hunch about how you will die?
8. Name three things you and your partner appear to have in common.
9. For what in your life do you feel the most grateful?
10. If you could change anything about the way you were raised, what would it be?
11. Take 4 minutes and tell your partner your life story in as much detail as possible.
12. If you could wake up tomorrow having gained any one quality or ability, what would it be?

Set II (intermediate closeness)

13. If a crystal ball could tell you the truth about yourself, your life, the future, or anything else, what would you want to know?
14. Is there something that you’ve dreamed of doing for a long time? Why haven’t you done it?
15. What is the greatest accomplishment of your life?
16. What do you value most in a friendship?
17. What is your most treasured memory?
18. What is your most terrible memory?
19. If you knew that in one year you would die suddenly, would you change anything about the way you are now living? Why?
20. What does friendship mean to you?
21. What roles do love and affection play in your life?
22. Alternate sharing something you consider a positive characteristic of your partner. Share a total of 5 items.
23. How close and warm is your family? Do you feel your childhood was happier than most other people’s?
24. How do you feel about your relationship with your mother?

Set III (intensive closeness)

25. Make three true “we” statements each. For instance, “We are both in this room feeling...”
26. Complete this sentence: “I wish I had someone with whom I could share...”
27. If you were going to become a close friend with your partner, please share what would be important for him or her to know.
28. Tell your partner what you like about them; be very honest this time saying things that you might not say to someone you’ve just met.
29. Share with your partner an embarrassing moment in your life.
30. When did you last cry in front of another person? By yourself?
31. Tell your partner something that you like about them already.
32. What, if anything, is too serious to be joked about?
33. If you were to die this evening with no opportunity to communicate with anyone, what would you most regret not having told someone? Why haven’t you told them yet?
34. Your house, containing everything you own, catches fire. After saving your loved ones and pets, you have time to safely make a final dash to save any one item. What would it be? Why?
35. Of all the people in your family, whose death would you find most disturbing? Why?
36. Share a personal problem and ask your partner’s advice on how he or she might handle it. Also, ask your partner to reflect back to you how you seem to be feeling about the problem you have chosen.

C Descriptive statistics

Table C.1 – Descriptive statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
<i>Panel A. Primary outcomes</i>					
Trust Pair	0.351	0.226	0	1	366
Trust Police	0.374	0.239	0	1	366
Trust Youth	0.363	0.236	0	1	366
Difference Trust Game	-0.012	0.239	-1	0.700	366
IAT	-0.813	0.987	-7.48	2.79	366
<i>Panel B. Secondary outcomes</i>					
Expected amount sent back Pair	4.724	4.092	0	30	366
Expected amount sent back Police	4.88	4.419	0	30	366
Expected amount sent back Youth	4.197	3.801	0	25	366
Difference Expected	-0.683	4.389	-27	18	366
Altruism Dictator	3.292	2.404	0	10	366
Altruism police	0.661	0.474	0	1	366
<i>Panel C. Controls</i>					
Gender	0.596	0.491	0	1	359
Age	17.112	0.781	15	20	366
Vocational Training	0.123	0.329	0	1	366
Technological Training	0.243	0.43	0	1	366
General Training	0.634	0.482	0	1	366
Education	5.522	2.077	1	8	366
Victim theft with violence	0.107	0.309	0	1	366
Victim theft without violence	0.131	0.338	0	1	366
Victim violence	0.082	0.275	0	1	366
Victim sexual violence	0.063	0.243	0	1	366
Victim threats	0.161	0.368	0	1	366
Victim insults	0.41	0.492	0	1	366
Victim scam	0.167	0.373	0	1	366
Victim discrimination	0.208	0.406	0	1	366
Attention	0.536	0.499	0	1	366
Nb controls	1.918	6.034	0	60	366

D Balance across treatments

Table D.1 – Balance across treatments

	Mean		Difference		SE		Difference		SE	
	control	photo	control	photo	control	photo	control	photo	control	photo
Gender	0.578	0.601	0.603	0.024	0.066	0.025	0.068	0.002	0.060	0.060
Age	17.163	17.076	17.116	-0.087	0.110	-0.047	0.102	0.040	0.094	0.094
Education	5.587	5.407	5.605	-0.180	0.292	0.018	0.282	0.198	0.241	0.241
Negative past experience with police	0.348	0.331	0.364	-0.017	0.063	0.017	0.066	0.033	0.058	0.058
Victim theft with violence	0.098	0.097	0.124	-0.001	0.040	0.026	0.043	0.027	0.038	0.038
Victim theft without violence	0.109	0.110	0.171	0.002	0.042	0.062	0.048	0.060	0.042	0.042
Victim violence	0.054	0.103	0.078	0.049	0.037	0.023	0.034	-0.026	0.035	0.035
Victim sexual violence	0.076	0.048	0.070	-0.028	0.032	-0.006	0.036	0.021	0.028	0.028
Victim threats	0.130	0.186	0.155	0.056	0.049	0.025	0.048	-0.031	0.046	0.046
Victim insults	0.391	0.441	0.388	0.050	0.066	-0.004	0.067	-0.054	0.060	0.060
Victim scam	0.163	0.193	0.140	0.030	0.052	-0.024	0.049	-0.054	0.045	0.045
Victim discrimination	0.217	0.193	0.217	-0.024	0.054	-0.000	0.057	0.024	0.049	0.049
Attention	0.554	0.586	0.465	0.032	0.066	-0.089	0.068	-0.121**	0.060	0.060
Police controls	1.098	1.793	2.643	0.695	0.683	1.546*	0.800	0.850	0.825	0.825
N	92	145	129							

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Histograms of treatment effects

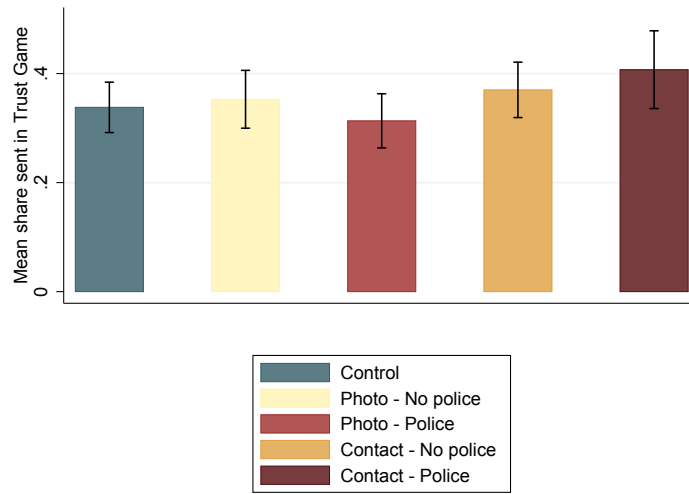


Figure E.1 – Average amounts sent in the trust game played with the pair

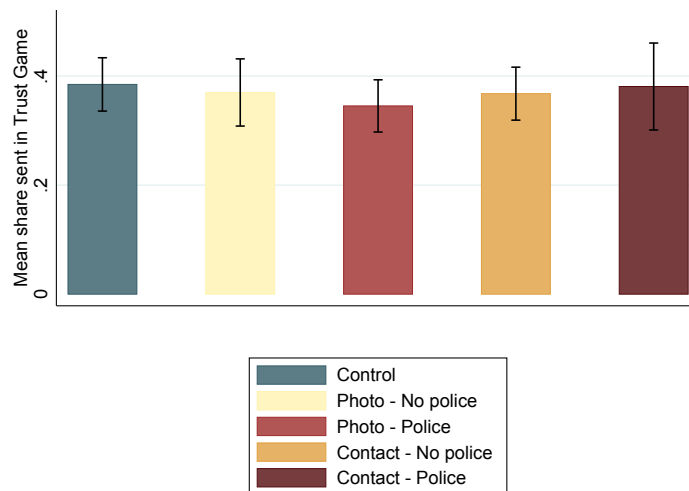


Figure E.2 – Average amounts sent in the trust game played with a random policeman

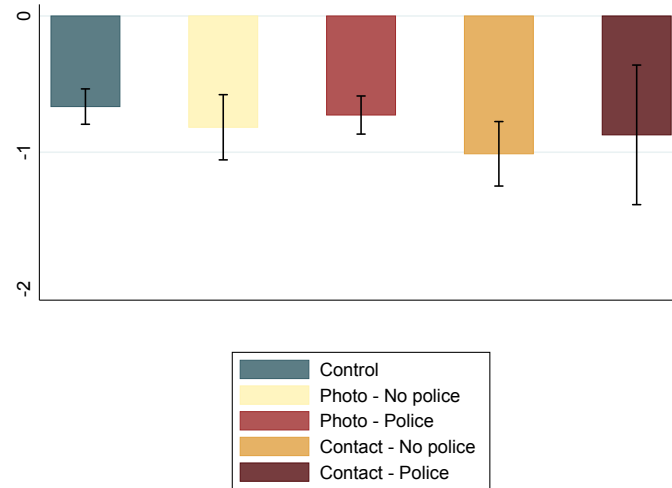


Figure E.3 – Average difference in response time of the Implicit Association Test. A negative result means that participants took more time associating pictures of the police with positive words than pictures of health services with the same positive words.

F Channels

In accordance with the PAP, I investigate the empirical channels through which contact might affect trust. As stated in List (2020), standard economic theory would expect that any change in actions must be attributed to a change in at least one of three parameters: incentives - how strategies are translated into payoffs-, beliefs - how other players are likely to play and therefore influence the payoff of the agent's own strategy - and/or utility function - how material payoffs are translated into well-being. In this experiment, there is no difference between treatment arms in terms of incentives, as they all play the same games. The channels through which contact could influence behaviors are therefore only either a change in beliefs, or a change in utility function.

Change in beliefs

To measure whether contact has an effect on beliefs, I use an incentivized elicitation of the participants' beliefs about how many tokens the other player will send in the trust game. Specifically, I had participants answer a question about how many tokens they believed the other would send back, and earned a bonus of 5 tokens if their answer falls within two units of the actual answer of the other player.

I elicited participants' beliefs for each Trust Game played, i.e. for the game played with their individual partner - *Expected pair* -, as well as for that played with a random police officer - *Expected police*.

Change in utility

As a measure of a change in the utility function, I use one parameter which is likely to be affected through contact, namely altruism. To measure altruism at the individual level, I use the standard Dictator Game (Kagel and Roth, 1995), in which each participant has to decide on a split of an endowment of 10 tokens between herself and the other player. The variable *Altruism pair* is then re-scaled to [0,1] to represent the share of endowment sent to the other player.

To measure altruism at the collective level, I asked participants to choose one of two charities to which to give 2€. The first charity is a charity called “L’Oeuvre des Orphelins de la Préfecture de Police”, which works at providing help to children of police officers who died on the job.

The choice of the second charity was done during a pilot in February 2021 and involved 16 subjects (which are not part of the final sample). For a number of candidate charities, participants were asked if they prefer the given charity or the police charity. The results of the survey is displayed in Table F.1. I decided to use the “Apprentis d’Auteuil” charity in the final questionnaire because the share of respondents favoring this association was the closest to 0.5. The charity helps struggling adolescents through training, mentoring and help for their career path.

The variable *Altruism police* is thus a dummy variable with value 1 if the participant chose the police charity, and 0 otherwise.

Table F.1 – Choice of charity

<i>Charity</i>	<i>Domain</i>	<i>Percentage of respondents</i>
<i>Restos du Cœur</i>	Poverty and hunger	12.5
<i>Association pour la protection des animaux sauvages</i>	Wildlife protection	68.8
<i>Fondation Abbé Pierre</i>	Poverty and housing	0.0
<i>Apprentis d’Auteuil</i>	Social rehabilitation of youth	43.8
<i>Ordre de Malte</i>	Poverty and disability	12.5
<i>Association Prévention Routière</i>	Road safety	75.0

The third column represents the percentage of respondents to the pilot who said they preferred the police charity (*Oeuvre des Orphelins de la Préfecture de Police*) to the charity in question. *Source: Author, based on a pilot study involving 16 participants.*

Results

In Table F.2, I present the results from estimations of the treatment effect of contact on each of the secondary outcomes presented above. As can be clearly seen in the Table, contact does not have an effect on any presented outcome, neither at the individual nor collective level. This lack of result could come from two reasons: either the true effect is 0, in which case the channel through which contact affects behavior, or the sample is too small to pick up an effect.

Table F.2 – Treatment effect on secondary outcomes to investigate channels

	(1) Expected pair	(2) Altruism pair	(3) Expected police	(4) Altruism police
Contact	-0.149 (0.181)	0.601 (0.663)	0.698 (0.435)	0.095 (0.494)
Police	0.164 (0.173)	0.223 (0.478)	0.663 (0.476)	0.512 (0.685)
Contact × Police	-0.012 (0.314)	0.755 (0.938)	0.564 (0.631)	-0.636 (1.172)
Photo	-0.210 (0.193)	0.169 (0.565)	0.086 (0.521)	-0.522 (0.652)
Constant	0.471 (1.789)	7.279 (6.188)	0.011 (3.189)	11.319** (4.277)
R^2	0.057	0.092	0.084	0.063 [†]
No. obs	359	359	359	359
Mean Control	0.685	4.315	2.750	5.022
Std dev. Control	0.467	3.706	2.375	4.019

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column 1, the outcome variable is the expected share of tokens sent back by the pair in a trust game. In column 2, the outcome is the amount sent in a dictator game to the pair. In column 3, the outcome is the difference between the expected share sent back by a random police officer or a random high-school student in a trust game. In column 4, the outcome is the probability to select the police association. Controls include gender, level of education, age, indicators of whether the participant was victim of certain crimes and misdemeanors and the level of attention. [†]: Pseudo- R^2

G Comparison of photo and contact treatments only

Table G.1 – Comparison of Photo and Contact treatments

	Trust Pair	Trust Police	IAT
Contact	0.008 (0.035)	-0.007 (0.045)	-0.121 (0.176)
Police	-0.078 (0.052)	-0.114* (0.057)	0.042 (0.262)
Contact × Police	0.070* (0.040)	0.018 (0.061)	0.009 (0.411)
Constant	0.248 (0.422)	0.199 (0.440)	-0.422 (1.251)
R^2	0.125	0.086	0.083
No. obs	269	269	269
Mean Control	0.338	0.385	-0.665
Std dev. Control	0.223	0.236	0.627

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column 1, the outcome variable is the amount sent in the trust game with the individual met, in column 2, the outcome is the amount sent in a trust game with a random police officer. In column 3, the outcome is the result of the Implicit Association Test. Controls include gender, level of education, age, indicators of whether the participant was victim of certain crimes and misdemeanors and the level of attention. Partner-fixed effects are included. Standard errors are clustered at the class level.

H Difference with the pre-analysis plan

The experiment was pre-registered on the registry for randomized controlled trials in economics held by the American Economic Association (AEA RCT Registry) on February 3, 2021, before the data collection began. The url for the archive is <https://www.socialscienceregistry.org/trials/7116>.

The paper differs from the pre-analysis plan (PAP, click [here](#)) in a few dimensions.

Sample size The PAP was drafted before the administration of the second high-school agreed to participate to the experiment. This enabled me to increase the sample size from the initially-expected 200 to 366.

Removal of the socio-professional category of the parents In the PAP, I mentioned that I would use the socio-professional category of the parents as a control. In the presented analysis, I decided to remove it, in accordance with the missing values Section. Indeed, 134 (resp. 112) participants declared either that they did not know the socio-professional category of their father (resp. mother), amounting to 37 (resp. 31) percent of respondents.

I Questionnaire

If you wish to consult the questionnaire given to participants (with the English translation), please click on the following [LINK](#).

Chapter 2

Low-Cost Contact Interventions Can Increase Inter-Ethnic Trust: Evidence from Senegal

Note: This chapter is co-authored with Guillaume Hollard and Omar Sene.

Abstract

Existing experimental evidence on the contact hypothesis has mainly used long and unstructured interventions, with implications for the replicability and scalability of existing contact protocols. We here test the effect of a brief contact, using a structured protocol that can be implemented in a wide range of situations at a reasonable cost. We also evaluate the lasting effects one month after the intervention. Contact is only found to be effective at increasing trust toward the specific individuals met, and only in the short-run. Generic Machine Learning techniques enable us to identify characteristics of the most and least affected groups.

JEL Classification: C92, C93

Keywords: contact hypothesis, trust, experiment¹

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1. Introduction

Under the “contact hypothesis”, interpersonal contact across group lines can reduce prejudice and discrimination (Allport, 1954). Recent causal evidence has corroborated the contact hypothesis, finding that contact reduces prejudice and discrimination across group lines (Scacco and Warren, 2018; Carrell et al., 2019; Corno et al., 2019). However, contact interventions often use long and unstructured interventions - e.g. by having people from different groups participating in long sports leagues (Mousa, 2020; Lowe, 2021). A recent meta-analysis found no clear evidence that the number of encounters or the duration of the intervention are primary factors to determine the effect of contact (Clochard, 2022). In this paper, we implement a “light-touch” contact protocol, based on the “fast-friend” procedure by Aron et al. (1997), which lasts only a few minutes. The protocol is easily implementable in a wide variety of contexts, at a reasonable cost. We propose to measure the effect of contact using two outcomes that are economically relevant and comparable across contexts. We use the trust game, which is a standard measure of inter-personal trust. We also use a simple question in which subjects are asked to divide a sum of money between two individuals. These measures are easy to implement and can help shed light on the nature of the discrimination: statistical or taste-based discrimination. Lastly, using statistical tools proposed by Chernozhukov et al. (2018), we can ex-post identify the characteristics of subjects who are the most affected by the treatment. Last, we run a follow-up survey a month after the intervention to evaluate the duration of the effects.

We run our pre-registered experiment¹ in the Saint-Louis region in Northern Senegal. Saint-Louis is known for episodes of inter-ethnic tension.² Our sample consists of 895 participants from a local fishing village, a farming village and the local university.

The proposed protocol consists in face-to-face encounters, with two individuals answering a series of questions drawn from a pre-existing list, where the questions gradually become more personal. The first questions are relatively trivial (e.g. “Would you like to be famous? In what way?”) while questions in the end are very intense (e.g. “Of all the people in your family, whose death would you find most disturbing? Why?”).³ In our experiment, the subjects are paired with an assistant and discussions last approximately 10 minutes.

We also use a *Photo* treatment, in which participants are only presented with a photo of the person they are paired with. This treatment enables us to cleanly distinguish the effect of meeting, in person, a person (a *contact*) from simply having basic information about the other person.

We show that brief, but intense, contact increases investment in the trust game by 0.4

¹<https://www.socialscisceregistry.org/trials/8964>

²See for instance <https://observers.france24.com/fr/20200207-senegal-saint-louis-guet-ndar-affronteme>

³The list of questions is presented in Appendix A

standard deviations at the individual level (with the specific individuals met during the intervention). The effect is statistically significant and only appears for participants who met an assistant from another ethnic group. However, the treatment is found to have no effect on generosity toward the outgroup in general, as there is no treatment effect on our money-splitting measure. Moreover, the treatment effects completely vanish - even at the individual level - one month after the intervention.

Two non-mutually excluding reasons could drive our results. To a part, our results could be due to the “light-tough” aspect of our contact protocol - as compared to longer interventions which span over weeks or months. It is therefore possible that our protocol is simply not sufficient to change perceptions of the entire outgroup. Another explanation of our results is the studied population. Saint-Louis in Senegal, where our experiment took place, is known for episodes of inter-ethnic violence but in most instances, ethnic groups interact in their daily life, inter-ethnic marriages are frequent, etc. Effects could be limited in magnitude because there is only little ethnic discrimination in Saint Louis.

This paper makes several contributions to the contact literature. First, we contribute to the *replicability* of contact interventions. The “fast-friend” procedure used is replicable to a wide-variety of contexts⁴. Second, we contribute to the *representativeness* of contact interventions. Our experiment is run in the field, with the general population and in a developing country, for which there is a lack of evidence of the effect of contact - Clochard (2022) found that most experiments, especially light-touch ones, tend to be run on university students in the US. Third, we contribute to the *comparability* of contact interventions, by using outcome measures which can be easily replicated in widely different settings. Fourth, we contribute to the open question on the *durability* of the effects of contact. We show that the effects of an intervention as light-touch as ours are not lasting over the long-run, something which was hinted but not conclusively found in the recent literature review by Paluck et al. (2021). Fifth, we contribute to the question of *heterogeneity* of contact interventions, by using machine learning algorithms to identify characteristics of the most and least affected groups. Our results illustrate that the most affected individuals appear to be the most distant from their partners. This has potential implications for the implementations of future contact interventions, and in particular for the choice of people who might be put in contact.

The remainder of the paper is organized as follows. We describe the experimental design in Section 2.. Results from the primary estimations are presented in Section 3., while results from heterogeneity tests are presented in Section 4.. Last, Section 5. concludes.

⁴See for instance Page-Gould et al. (2008) and Clochard (2021).

2. Experimental Design

2.1. Protocol

Upon arrival, subjects were randomly assigned to one of three treatments: *Contact*, *Photo* or *Control*. The treatment arms are summarized in Figure 2.1.

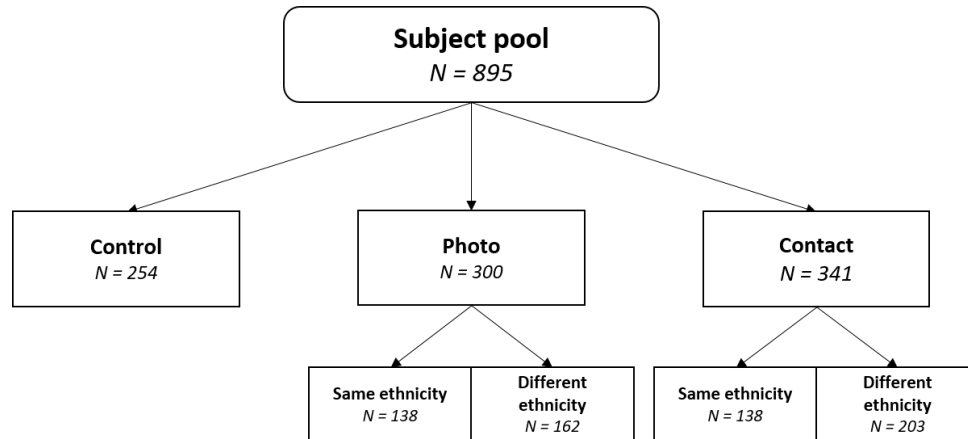


Figure 2.1 – Treatment arms

The *Contact* treatment is an adaptation from the protocol developed by [Aron et al. \(1997\)](#): subjects are paired with an assistant, and take turns in answering a series of questions. The questions, directly drawn from the “Closeness-Generating Procedure” in [Aron et al. \(1997\)](#), gradually become more self-disclosing and relationship-building. The first questions are fairly neutral, to get the procedure started (e.g. “Before making a telephone call, do you ever rehearse what you are going to say? Why?”). The second set of questions then aims to reveal more personal details (e.g. “What do you value most in a friendship?”, “How do you feel about your relationship with your mother?”). Last, the third set covers very personal topics, to create a strong link between the two individuals (e.g. “If you were going to become a close friend with your partner, please share what would be important for him or her to know”). The subject and the assistant were both asked to answer one question from each of these three sets (without any supervision from experimenters).

In the *Photo* treatment, subjects are again paired with one of the assistants from the *Contact* treatment. However, they do not meet them, but are only shown their photo with their names - which in Senegal include significant information about ethnicity ([Madubuiké, 1976](#)). The *Photo* treatment is not expected to have any effect on trust itself. The comparison between the *Contact* and *Photo* treatments enables us to distinguish the effect of a contact from the mere provision of information about the other person.

In both the *Contact* and the *Photo* treatments, subjects were explicitly told that the games played during the experiment will be with the person presented (either in person or in the photo).

In the *Control* treatment, subjects are not presented the person with whom they will play, but are rather told that they are going to play with “someone selected at random among participants”. The *Control* treatment is used to measure a form of *average level of trust* in our sample.

Participants were matched with assistants, rather than with other participants. This matching procedure allowed us to introduce individual assistant fixed effects in the *Contact* and *Photo* treatments. Using assistants was also more convenient, as it ensured that at least one person in the pair was literate - and therefore did not require a third person to supervise the discussions (which could have influenced the results) - and because we used a paper format for the photos in the *Photo* treatment and so required photos in advance. Last, having assistants as pairs allowed us to randomize meetings with respect to ethnicity, which would have been much more difficult otherwise, especially in the fishing village which is almost entirely ethnically-homogeneous. Assistants were explicitly asked to behave similarly with all the participants they met.

In total, five assistants were selected from the local university, with three of them from the main ethnic group in Senegal (*Wolof*) and two of them from the second largest group (*Pulaar*). After being randomly matched with an assistant, participants were asked whether they know the assistant. If the answer was yes, then we randomly re-matched the participant with another assistant.

2.2. Outcomes

After the treatment phase of the experiment, subjects played a standard trust game (Berg et al., 1995) with the person they had just met (or, in the case of the *Control* treatment, with someone “drawn at random”). Our *Trust* variable is the share of the endowment sent. The subjects earnings are the real outcomes of this one-shot trust game. The assistant could play the trust game however he/she wished, and in the *Control* treatment the recipient’s behavior was drawn at random. The *Trust* measure captures the effect of contact at the individual level.

To see whether the contact interaction affects attitudes towards entire ethnic groups, we developed a measure of prejudice as follows. We designed two “standard” drawings of a Pulaar man and a Wolof man (the two main ethnic groups in Senegal), shown in Figure 2.2, inspired by Blouin and Mukand (2019). Subjects were asked the following hypothetical question: “Here are two fictional individuals, Ibrahima Ba (on the left) and Mamadou Ndiaye (on the right). Ibrahima Ba owns two sheep and Mamadou Ndiaye has three brothers and one sister. Imagine you have 10 000 Francs to split between these two individuals, how much would you like to send to Ibrahima Ba?”. The names Ba and Ndiaye are typical of the two ethnic groups (*Pulaar* and *Wolof*, respectively). The information about sheep and siblings is necessary so as to provide an excuse for giving more to one

individual or the other, without explicitly involving ethnicity (Blouin and Mukand, 2019). In order to be able to measure the *absolute* level of prejudice, the information between the two would have been randomly varied, but for practical reasons (in particular due to the pen and paper data collection), this was not done, and we can only compare *relative* levels of prejudice between groups.

Figure 2.2 – Drawings of “standard” individuals from the two main ethnic groups in Senegal



For the longer-term survey, we modified the names and characteristics of the individuals of question: “Here are two individuals. Oumar Sow has three children, one son and two daughters, and Abdoulaye Dieng owns a red motorcycle. Imagine you have 10 000 Francs to split between Oumar Sow and Abdoulaye Dieng, how much would you like to give to Oumar Sow?”. Again, Sow is a typical *Pulaar* name and Dieng is a typical *Wolof* name.

The variable *Prejudice* is then defined as the share of the endowment sent to the member of the participant’s own ethnic group. The variable is defined as missing for participants not belonging to the two main ethnic groups (which represent 83% of our sample).

While several factors might influence the *absolute* levels of the *Trust* and *Prejudice* variables - the characteristics of the imaginary individuals, for instance, or the absolute level of trust of the individuals - the identifying assumption is the fact that these determinants are independent of treatment.

There was no feedback until the end of the experiment. In particular, subjects were not aware of the outcome of the trust game when answering subsequent questions.

2.3. Controls and dimensions of heterogeneity

We control for subject age, education, gender and ethnicity. When comparing the *Photo* and *Contact* treatments, we also include metrics of proximity with partners (same age, ethnicity, gender, etc.).

As indicated in the pre-analysis plan, the main dimension of heterogeneity of the effect of contact we focused on is the prior exposure to other ethnic groups. Previous papers have used different methods to identify the interactions with outgroup members: Clochard (2021) uses the number of police identity controls as a proxy for previous interactions, Freddi et al. (2022) use whether the students have minority peers in their class. Because such measures are irrelevant to our setting, in this paper we defined outgroup exposure using the following method. We asked participants to give the last names of the five persons with whom they interact the most on a daily basis, outside of their families. After the data collection phase, we computed the list of all names given by participants. This gave us a list of 161 different names. We then asked a group of 20 students from the local university to state which ethnicity they most associate with each name. We assigned the mode of the answers as the ethnicity of each name.⁵

After we assigned an ethnicity to each name in our data base, we constructed a variable of exposure to out-group people, *Exposure*, as a dummy variable of whether the participant declared more than the median number of friends belonging to ethnic groups other than her own.

2.4. Setup

The experiment took place between February and March 2022 in the Saint-Louis region of Northern Senegal. A total of $N = 895$ people participated in the experiment. Three sites were selected for the experiment, in order to have a diverse set of populations: a fishing village called Guet Ndar ($N = 327$), a farming village called Mouit ($N = 212$) and the local university (*Université Gaston Berger*, $N = 356$). On average, participants earned approximately XOF 2 400 (€3.65) for a session which lasted approximately one hour.⁶

To measure the effect of contact in a longer time scope, we ran a phone survey one month after the intervention with participants from the *Contact* and *Photo* treatments. $N = 208$ participants (67% of participants who initially provided their phone number) were successfully identified and agreed to answer the enumerators. No difference in attrition was found between the *Contact* and *Photo* treatments (31 and 34%, respectively, t-test $p = 0.53$).

⁵This measure of ethnicity is relatively precise, as we can correctly guess 75% of the ethnicities of our participants, using their names.

⁶8 subjects decided to drop out of the experiment before completion, and were excluded from the analysis.

3. Results

3.1. Descriptive statistics and balance across treatments

The descriptive statistics are displayed in Table B.1. Our sample is comprised of adults who are on average 32 years old, with approximately 59% of women. The fact that the sample is not only comprised of students or children is a contribution to the contact literature, as there currently exists a gap in evidence of the effects of contact among adults (Paluck et al., 2021). Most of our sample has received several years of education, but with a lot of discrepancy (at the university, most participants have graduated high school, whereas 60% of the sample from the fishing village never went to school).

Compared to the national average, we have a slight over-representation of Wolofs (the main ethnic group in Senegal), with 65%, compared to approximately 50% nationwide. This difference is mainly due to the fact that the experiment took place in the Saint-Louis region with a higher share of this group, and because almost all fishers belong to the ethnic group.

Treatments are relatively well balanced (Table C.1).

3.2. Short-term effect of contact on trust and prejudice

To estimate the effect of contact on trust and prejudice, we estimate Equation 2.1. Results are displayed in Table 2.1.

$$Y_i = \beta_0 + \beta_1 \text{Contact}_i + \beta_2 \text{Contact}_i \times \text{Same Ethnicity}_i + \beta_3 \text{Photo}_i + \beta_4 \text{Photo}_i \times \text{Same Ethnicity}_i + \gamma X_i + \epsilon_i \quad (2.1)$$

We find that contact has a positive and significant effect on trust at the individual level, i.e. when looking at the trust game played with the person participants are paired with (Column 1). The effect is statistically significant and is relatively large, as it corresponds to 0.38 standard deviations. There is no significant difference between contact with someone from the participants' own ethnic group or another.

However, the positive effect of contact at the individual level fails to replicate to a change in the perception of the out-group in general, as we find no significant effect on our prejudice measure (Column 2).⁷ The contact then had no effect on the out-group as a whole, but only on the specific individual who subjects met.⁸

⁷Note that the number of observations is lower relative to the previous column for two reasons: first, results from two waves are included in the Trust measure but not the prejudice one, and second, subjects which do not belong to one of the two main ethnic groups had to be removed, as the prejudice variable is not defined in their case (see Section 2.).

⁸Another possibility is that the measure of prejudice is not perfect, especially as the decision here is

These results in the short-run are strikingly similar to those of a previous paper (Clochard, 2021), in which the effect at the individual level was found to be 0.4 standard deviations, and no effect was found at the collective level.

The results of the comparison of the *Photo* and *Contact* treatments, shown in Appendix E, are similar, although the significance levels drop.

Table 2.1 – Treatment effect on trust and prejudice in the short-run

	(1)	(2)
	Trust Game	Prejudice
Contact	0.086** (0.035)	0.100 (0.070)
Same ethnicity	-0.004 (0.031)	-0.042 (0.056)
Contact × Same ethnicity	-0.021 (0.041)	-0.030 (0.076)
Photo	0.046 (0.035)	0.039 (0.073)
R^2	0.040	0.302
No. obs	845	462

Note: In column 1, the dependent variable is the amount sent in the trust game, while in column 2, the dependent variable is the measure of prejudice (see the description in Section 2.). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Controls include age, level of education, gender, ethnicity and partners.

3.3. Longer-term outcomes

One significant contribution from this paper is to be able to test the duration of the effects of contact (Paluck et al., 2021). We present in Table 2.2 results from the phone survey which was collected one month after the experiment took place. The effects are underwhelming, as we find no significant effect of contact neither on trust nor on prejudice. The results are disappointing, but might be linked to the smaller sample size. It therefore appears that the effect of contact, or at least when methodology based on Aron et al. (1997), completely fades away even within one month.

Table 2.2 – Treatment effect on trust and prejudice in the long-run

	(1) Stated trust	(2) Prejudice
Contact	-0.367 (0.396)	0.049 (0.070)
Same ethnicity	0.070 (0.458)	-0.028 (0.070)
Contact × Same ethnicity	0.199 (0.606)	-0.011 (0.095)
R^2	0.101	0.562
No. obs	208	163

Note: In column 1, the dependent variable is the declared trust in the person participants met, while in column 2, the dependent variable is the measure of prejudice (see the description in Section 2.). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Controls include age, level of education, gender, ethnicity and partners.

4. Heterogeneity analysis

4.1. Heterogeneity based on prior interactions

The main dimension identified in the literature ([Page-Gould et al., 2008](#); [Freddi et al., 2022](#); [Clochard, 2021](#)) as a potential source of heterogeneity in the effect of contact on trust is the level of interactions outside the intervention with outgroup members. In a nutshell, the more previous interactions with the outgroup in the everyday life, the less effective the contact. The variables of interest are defined in Section 2..

Results are displayed in Table F.1, and highlight that across the three measures of exposure to outgroup members, there appears to be no heterogeneity in the effect of contact based on prior interactions. This result could be the consequence of the fact that most participants declare to have at least 1 person outside of their family belonging to another ethnic group (only 108 participants stated no friend from another ethnic group).

4.2. Generic Machine Learning for heterogeneity analysis

After analyzing the heterogeneity of the treatment effect based on previous interactions (an identified dimension of heterogeneity), we used the Generic Machine Learning (Generic ML) approach from [Chernozhukov et al. \(2018\)](#). The principle of the Generic ML is to estimate and infer parameters based on repeated data splitting, and taking medians of p-values and confidence intervals. The method estimates the most and least affected groups, and then computes averages for a selected set of variables.

The characteristics of the least and most affected groups are displayed in Table 2.3. They indicate that the treatment is most effective for individuals who were paired with

someone from another ethnic group (as in the primary results, $p = 0.04$), are older ($p < 0.01$) and less educated ($p < 0.01$). The treatment is also less effective on students ($p < 0.01$). Interestingly, there is no gender difference between the most and least affected groups.

In Figure F.1, we display the treatment effects for the different groups. While the differences between the treatment effects are not significant, the effect is important and significantly positive for the most affected group. The effect for the least affected group appears to be a relatively precise zero.

Table 2.3 – Differences in characteristics

	Most Affected	Least Affected	Difference
Same Ethnicity	0.225 (0.084,0.371)	0.475 (0.327,0.620)	-0.225 (-0.426,-0.029) [0.041]**
Age	- 35.58 (31.96,39.09)	- 23.38 (19.90,26.87)	12.150 (7.239,17.13) [0.000]***
Gender	- 0.425 (0.271,0.581)	- 0.525 (0.372,0.678)	-0.063 (-0.286,0.160) [1.000]
Education	- 5.407 (4.348,6.570)	- 8.188 (7.049,9.357)	-2.545 (-4.198,-0.988) [0.004]***
Student	- 0.350 (0.199,0.498)	- 0.725 (0.586,0.874)	-0.375 (-0.578,-0.172) [0.001]***

Using the methodology from [Chernozhukov et al. \(2018\)](#). This table displays the average characteristics in the most and least affected quintiles. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors display the median standard errors in each quintile.

5. Discussion and conclusion

The existing literature on the Contact Hypothesis has focused on long interventions, which are thus potentially difficult to scale-up. We here present the results of a short, but intense, contact intervention, borrowing from the method in [Aron et al. \(1997\)](#). The proposed protocol is designed to create closeness between two individuals, irrespective of their characteristics (e.g. gender, age, social position and ethnicity) by appealing to their emotions. This protocol is therefore more likely to be replicable in different contexts than existing interventions in the literature. Furthermore, since the questions in the protocol are freely-available and relevant for virtually any individual, the protocol is much more

structured than those currently used and allows for replication. We find a positive effect of contact on inter-ethnic trust, at the individual level and for individuals who are paired with partners very different from them. The effects of our protocol, however, completely fade away one month after the experiment.

The use of a light-touch contact intervention can be viewed as a solution to reveal the nature of the prejudice in a given society. By nature, we here mean characteristics such as (1) the dimensions along which prejudice occurs, (2) the origin of the prejudice (e.g. statistical or taste-based) and (3) the extent to which the prejudice is important. As we explain below, our intervention allows to gain insights on these three questions at a rather low cost.

Dimensions of prejudice Prejudice can occur along several dimensions (ethnicity, gender, age, education, disability, etc.). In general, researchers come up with an assumption about a particular dimension along which some discrimination is expected. In the case of this paper, our premise was that the ethnic dimension was the main driver of prejudice. A direct consequence of our focus on ethnicity is the choice of the pool of assistants. Remember that assistants are the individuals that participants will meet during contacts or see on photo. The main dimension of variation between assistants was their ethnicity, and on other dimensions, assistants were relatively similar (they were young and educated, for instance). In the measure of prejudice, with the “money-splitting” task, we also highlighted primarily ethnic cues. If the dimension which matters for prejudice was different than ethnicity, the broad design could have remained the same (i.e. contact vs photo vs control), but some characteristics would have been different.

However, the use of the methodology of [Chernozhukov et al. \(2018\)](#) can provide insights on which characteristics make participants more affected by the treatment, and thus can provide information on which dimensions are likely to matter for prejudice. For future contact interventions, and in particular for RCTs with pre-post comparisons, our cheap and quick intervention can help identify which characteristics are important, and can help in the design of the interventions themselves, e.g. if the dimension of prejudice appears to be age, then there should be some variation in the age of partners.

Source of prejudice A large fraction of the prejudice and discrimination literature has been devoted to understanding the source of prejudice, broadly categorized as taste-based ([Becker, 1957](#)) or statistical ([Arrow, 1971](#)). Our design is able to identify which is at play in our sample, and which is affected by the treatment. In the design, the *Photo* treatment allows for the measure of differences in the amount sent to assistants in the trust game. The difference in average amount sent between, say, subjects shown a photo of an assistant from another ethnicity and those shown a photo of an assistant from their ethnic group can give a sense of the level of statistical discrimination at play. In our case, we observe

no difference across ethnicities of assistants, indicating that statistical discrimination is limited in our sample. However, the fact that the treatment has an effect on the amount sent in the trust game for those who have met someone from another ethnicity indicates that our treatment can affect the belief about others.

Subjects in our experiment are also asked to split an amount of money between two individuals who differ on two dimensions, one being ethnicity.⁹ Because chosen splits do not differ according to participants' ethnicity, we can conclude that there is little taste-based discrimination based on ethnicity in the considered group.

To conclude, it is possible to speculate a little about a potential theory that could encompass all of our results. We here appeal to a series of papers looking for the root of prejudice in biases in the human memory system (Bordalo et al., 2021). The key finding is that beliefs are distorted in favor of the first instance of the group that comes to mind. So, what comes to mind first has a profound impact on beliefs.¹⁰ When previous contacts were scarce, the individual met during the intervention is likely to be the first to come to mind when thinking about the other group, in which case our light-touch intervention might be sufficient to reduce prejudice. If, on the other hand, encounters with out-group members are plentiful, a more-significant intervention may be necessary, which will help saturate the image of the out-group with memories from the experiment, as in Mousa (2020).

⁹As explained in Section 2., the other dimension is important as it allows subjects to “hide” their possible taste-based motivation behind another dimension than ethnicity.

¹⁰The classic example of this theory of recall is given in Bordalo et al. (2020): when asking about white items in a kitchen, people state significantly different words when cued with the word “milk” or “plate”.

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Appendices

A Questions for the *Contact* treatment

Set I (light closeness)

1. Given the choice of anyone in the world, whom would you want as a dinner guest?
2. Would you like to be famous? In what way?
3. Before making a telephone call, do you ever rehearse what you are going to say? Why?
4. What would constitute a “perfect” day for you?
5. When did you last sing to yourself? To someone else?
6. If you were able to live to the age of 90 and retain either the mind or body of a 30-year-old for the last 60 years of your life, which would you want?
7. Do you have a secret hunch about how you will die?
8. Name three things you and your partner appear to have in common.
9. For what in your life do you feel the most grateful?
10. If you could change anything about the way you were raised, what would it be?
11. Take 4 minutes and tell your partner your life story in as much detail as possible.
12. If you could wake up tomorrow having gained any one quality or ability, what would it be?

Set II (intermediate closeness)

13. If a crystal ball could tell you the truth about yourself, your life, the future, or anything else, what would you want to know?
14. Is there something that you’ve dreamed of doing for a long time? Why haven’t you done it?
15. What is the greatest accomplishment of your life?
16. What do you value most in a friendship?
17. What is your most treasured memory?
18. What is your most terrible memory?
19. If you knew that in one year you would die suddenly, would you change anything about the way you are now living? Why?
20. What does friendship mean to you?
21. What roles do love and affection play in your life?
22. Alternate sharing something you consider a positive characteristic of your partner. Share a total of 5 items.
23. How close and warm is your family? Do you feel your childhood was happier than most other people’s?
24. How do you feel about your relationship with your mother?

Set III (intensive closeness)

25. Make three true “we” statements each. For instance, “We are both in this room feeling...”
26. Complete this sentence: “I wish I had someone with whom I could share...”
27. If you were going to become a close friend with your partner, please share what would be important for him or her to know.
28. Tell your partner what you like about them; be very honest this time saying things that you might not say to someone you’ve just met.
29. Share with your partner an embarrassing moment in your life.
30. When did you last cry in front of another person? By yourself?
31. Tell your partner something that you like about them already.
32. What, if anything, is too serious to be joked about?
33. If you were to die this evening with no opportunity to communicate with anyone, what would you most regret not having told someone? Why haven’t you told them yet?
34. Your house, containing everything you own, catches fire. After saving your loved ones and pets, you have time to safely make a final dash to save any one item. What would it be? Why?
35. Of all the people in your family, whose death would you find most disturbing? Why?
36. Share a personal problem and ask your partner’s advice on how he or she might handle it. Also, ask your partner to reflect back to you how you seem to be feeling about the problem you have chosen.

B Descriptive statistics

Table B.1 – Descriptive statistics

Variable	Mean	(Std. Dev.)	N
<i>Panel A. Short-term outcomes</i>			
Trust game	0.452	(0.248)	885
In-group bias	0.211	(0.407)	482
<i>Panel B. Long-term outcomes</i>			
Long-term stated trust	0.628	(0.291)	211
Long-term in-group bias	0.336	(0.21)	166
<i>Panel C. Treatments</i>			
Contact	0.381	(0.486)	895
Photo	0.335	(0.472)	895
Control	0.284	(0.451)	895
Same ethnicity	0.308	(0.462)	895
<i>Panel D. Controls</i>			
Age	31.798	(13.719)	872
Student	0.415	(0.493)	895
Education	5.79	(4.017)	879
Gender	0.593	(0.491)	888
<i>Ethnicities</i>			
Wolof	0.649	(0.477)	895
Pulaar/Toucouleur	0.136	(0.343)	895
Serer	0.064	(0.244)	895
Diola	0.019	(0.137)	895
Mandinka	0.027	(0.162)	895
Soninke	0.006	(0.075)	895
Manjack	0.003	(0.058)	895
Bainouk	0	(0)	895
Lebou	0.047	(0.212)	895
Only Senegalese	0.001	(0.033)	895
Other ethnicities	0.036	(0.186)	895
<i>Panel E. Exposure to other ethnic groups</i>			
Number of friends from a different ethnicity	2.308	(1.759)	552
More out-group friends than median	0.489	(0.5)	603
Any out-group friend	0.821	(0.384)	603
Outgroup exposure	3.256	(1.062)	885

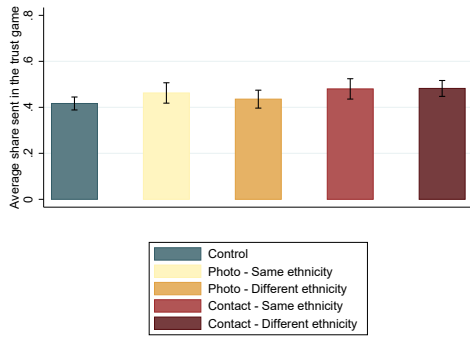
C Balance across treatments

Table C.1 – Balance across treatments

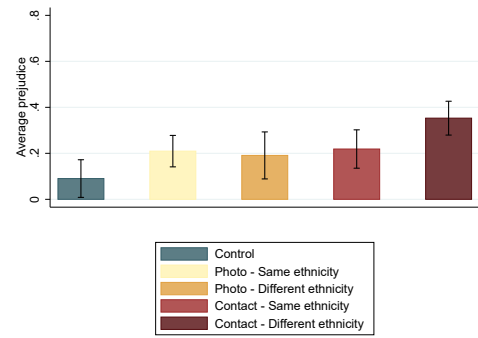
	Mean control	Mean photo	Mean contact	Diff control photo	SE	p	Diff control contact	SE	p	Diff photo contact	SE	p
Age	30.850	32.969	31.473	2.120	1.211	0.081*	0.623	1.086	0.566	-1.496	1.135	0.188
Student	0.472	0.417	0.370	-0.056	0.042	0.188	-0.103	0.041	0.012**	-0.047	0.039	0.223
Education	6.226	5.736	5.515	-0.490	0.351	0.164	-0.711	0.332	0.033**	-0.221	0.319	0.489
Outgroup exposure	3.320	3.226	3.237	-0.094	0.091	0.301	-0.083	0.087	0.337	0.011	0.086	0.898
Number of outgroup friends	2.602	2.189	2.235	-0.412	0.203	0.043**	-0.366	0.190	0.055*	0.046	0.171	0.788
More outgroup friends than median	0.568	0.481	0.450	-0.088	0.054	0.104	-0.118	0.052	0.023**	-0.030	0.047	0.518
Any outgroup friend	0.849	0.801	0.821	-0.048	0.042	0.245	-0.029	0.039	0.464	0.020	0.037	0.592
Wolof	0.614	0.640	0.683	0.026	0.041	0.532	0.069	0.039	0.080*	0.043	0.037	0.248
Pulaar / Toucouleur	0.154	0.150	0.111	-0.004	0.031	0.908	-0.042	0.028	0.131	-0.039	0.027	0.147
Serer	0.059	0.070	0.062	0.011	0.021	0.603	0.003	0.020	0.898	-0.008	0.020	0.668
Diola	0.024	0.017	0.018	-0.007	0.012	0.560	-0.006	0.012	0.606	0.001	0.010	0.928
Mandinka	0.035	0.027	0.021	-0.009	0.015	0.552	-0.015	0.013	0.267	-0.006	0.012	0.609
Soninke	0.012	0.003	0.003	-0.008	0.007	0.241	-0.009	0.007	0.191	-0.000	0.004	0.928
Mandjack	0.008	0.000	0.003	-0.008	0.005	0.124	-0.005	0.006	0.401	0.003	0.003	0.349
Bainouk	0.000	0.000	0.000	0.000	0.000	.	0.000	0.000	.	0.000	0.000	.
Lebou	0.043	0.040	0.056	-0.003	0.017	0.846	0.012	0.018	0.495	0.016	0.017	0.355
Only Senegalese	0.000	0.000	0.003	0.000	0.000	.	0.003	0.003	0.389	0.003	0.003	0.349
Other ethnicity	0.039	0.037	0.032	-0.003	0.016	0.868	-0.007	0.015	0.643	-0.004	0.014	0.760
N	254	300	341									

* p<0.10, ** p<0.05, *** p<0.01.

D Treatment effect in the short and long run

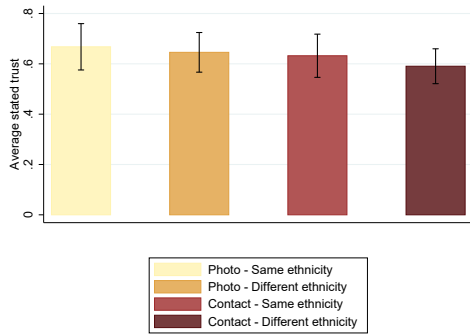


(a) Average share of endowment sent in the trust game

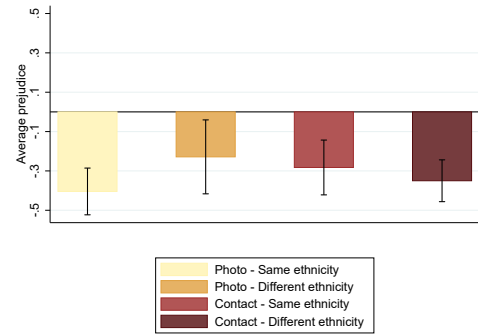


(b) Average level of prejudice

Figure D.1 – Treatment effect in the short term



(a) Average stated trust



(b) Average level of prejudice

Figure D.2 – Treatment effect in the long term

E Comparison between the Contact and Photo treatments

Table E.1 – Treatment effect on trust and prejudice in the short-run, comparing only the *Contact* and *Photo* treatments

	(1) Trust Game	(2) Prejudice
Contact	0.039 (0.028)	0.058 (0.055)
Same ethnicity	0.003 (0.033)	-0.043 (0.055)
Contact × Same ethnicity	-0.021 (0.042)	-0.026 (0.074)
R^2	0.037	0.275
No. obs	605	355

Note: In column 1, the dependent variable is the amount sent in the trust game, while in column 2, the dependent variable is the measure of prejudice (see the description in Section 2.). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Controls include age, level of education, gender, ethnicity and partners.

F Heterogeneity analysis

Prior interactions

Table F.1 – Heterogeneous treatment effect on trust based on interactions with other ethnic groups.

	(1) More outgroup friends than median	(2) Any outgroup friend	(3) Exposure to outgroup
Contact	0.050 (0.039)	0.024 (0.060)	0.052 (0.038)
Same ethnic	0.004 (0.039)	0.025 (0.065)	0.018 (0.041)
Contact × Same ethnic	-0.035 (0.058)	0.084 (0.105)	-0.072 (0.060)
Friends Median	-0.035 (0.036)		
Contact × Friends Median	-0.015 (0.050)		
Same ethnic × Friends Median	0.043 (0.061)		
Contact × Same ethnic × Friends Median	0.029 (0.087)		
Any friend		-0.040 (0.045)	
Contact × Any friend		0.023 (0.065)	
Same ethnic × Any friend		-0.008 (0.073)	
Contact × Same ethnic × Any friend		-0.116 (0.114)	
Exposure			-0.015 (0.026)
Contact × Exposure			-0.001 (0.045)
Same ethnic × Exposure			-0.013 (0.051)
Contact × Same ethnic × Exposure			0.080 (0.076)
R^2	0.046	0.051	0.041
No. obs	574	574	838

Note: In all three columns, the dependent variable is the share of the endowment sent in the trust game. In column 1, the the *Contact* and *Same ethnicity* variables are interacted with a dummy variable equal to 1 if the participant declared more outgroup friends than the median. In column 2, the variable interacted is a dummy variable equal to 1 if the participant declared any outgroup friend. In column 3, the variable interacted is a dummy variable equal to 1 if the participant completely agreed to the question “I often spend time with people from other ethnic groups”. * p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses. Controls include age, level of education, gender, ethnicity and partners.

Generic Machine Learning

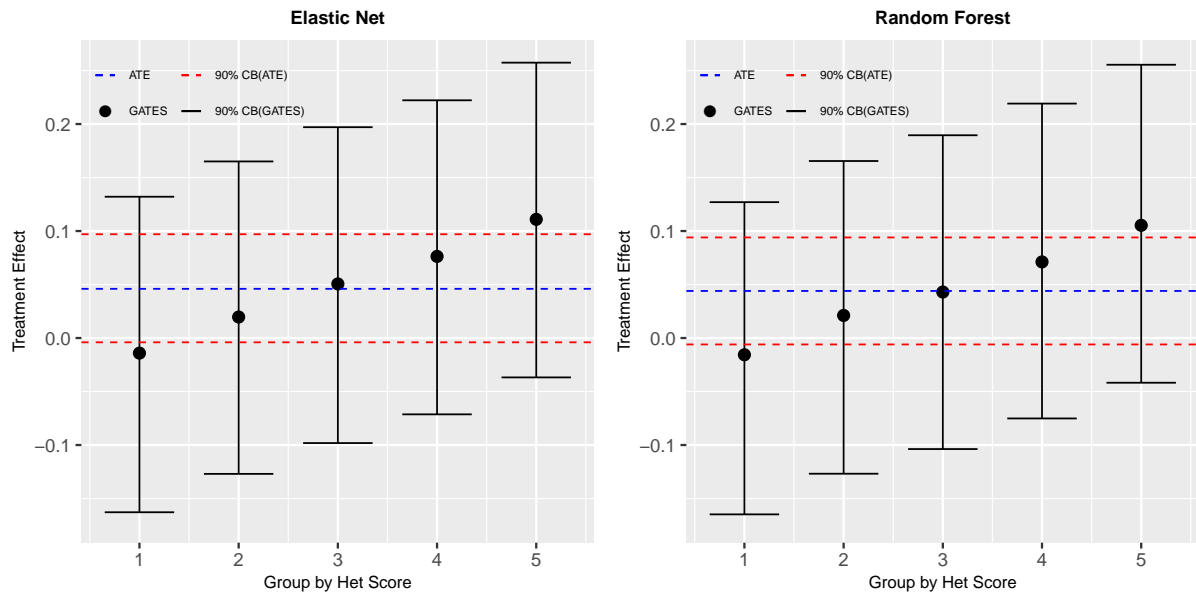


Figure F.1 – Heterogeneity in treatment effects

Chapter 3

Contact Interventions: A Meta-Analysis

Abstract

For decades, intergroup contact has been viewed as one of the main tools to reduce prejudice and improve intergroup relations. This paper reviews the experimental literature on the contact hypothesis. Based on an analysis of 62 measures from 37 papers, the conclusions are threefold. First, contact interventions are, on average, effective at reducing prejudice. Second, there exists a very large heterogeneity in the type of interventions labelled as contact. Third, characteristics of the experimental context, rather than the intervention itself, seem to matter for the efficacy of contact. Implications for the future of the contact literature are discussed.

JEL Codes: C93, C12, C83

Keywords: contact hypothesis, meta-analysis, prejudice reduction, field experiments

1. Introduction

Because of its consequences on social inequality (Durante and Fiske, 2017), xenophobia (Kumar et al., 2011) or reduced economic output (Hjort, 2014), solutions to reduce prejudice have been studied in psychology, sociology and economics for decades. Among candidate solutions, *contact* interventions have received the most attention (Bertrand and Duflo, 2017).

The so-called contact hypothesis was first coined by Allport (1954), who posited that prejudice “may be reduced by equal status contact between majority and minority groups in the pursuit of common goals. The effect is greatly enhanced if this contact is sanctioned by institutional supports (i.e., by law, custom, or local atmosphere), and provided it is of a sort that leads to the perception of common interests and common humanity between members of the two groups.” (p.281). Since Allport, interventions promoting contact between groups have been seen as one of the main tools to reduce prejudice (Bertrand and Duflo, 2017; Paluck et al., 2021).

The literature devoted to the investigation of the contact hypothesis developed dramatically after Allport’s book, with the seminal meta-analysis by Pettigrew and Tropp (2006) identifying no less than 515 studies, covering more than 250,000 people from 38 countries, over a period ranging from the 1940s to the year 2000. The overwhelming conclusion was that contact is effective at reducing prejudice, noting that “Results from the meta-analysis conclusively show that intergroup contact can promote reductions in intergroup prejudice” (p.751).

However, only a small fraction analyzed in the meta-analysis employed an experimental design, with a clear definition of a treated and control group. After the removal of non-experimental protocols - and the addition of more recent papers investigating contact, Paluck et al. (2019) are left with 27 studies investigating contact. Of these, the largest share (33%) investigated the effect of contact on racial and ethnic prejudice for university students or young adults (18-25 years old), and only two were conducted in developing countries - Scacco and Warren (2018) in Nigeria and Corno et al. (2019) in South Africa.

In this paper, I update the analysis by Paluck et al. (2019) by adding new papers on the expanding field of contact interventions. I identified 62 measures from 37 papers. I also deepen the analysis by investigating which characteristics of interventions appear to be associated with a larger impact. Allport (1954) identified four necessary conditions of effectiveness of contact interventions - equal status among groups, common goals, positive contact and the support of authorities. Subsequent work (Pettigrew and Tropp, 2006; Lemmer and Wagner, 2015) also identified friendship potential and scriptedness of encounter as potential mediators of the effect of contact. Importantly, no formal test of the relevant hypotheses has, to the best of my knowledge, ever been performed. I therefore estimate the relevance of characteristics of the interventions, as well as variables indicative

of the context of the paper (e.g. prejudice on race/ethnicity or caste), for the efficacy of contact interventions.

My main results are threefold. The first result is confirming that of [Paluck et al. \(2019\)](#) by showing that contact interventions *are* effective at reducing prejudice and improving measures of intergroup cohesion. The typical intervention will have an effect of approximately 0.33 standard deviations. According to [Cohen \(1969\)](#)'s rule of thumb, contact interventions therefore typically have a small to moderate effect on prejudice.¹ There exist, however, a large heterogeneity between studies and measures.

The second result is that there exists a lack of consistency in use of the term “contact”. Just like there exists some debate about what falls under the umbrella of intergroup conflict ([Lee and Salvatore, 2022](#)), the term contact has been used to describe very different protocols, ranging from short face-to-face discussions with very scripted protocols ([Page-Gould et al., 2008](#); [Clochard, 2021](#); [Clochard et al., 2022](#)), sports leagues lasting several months ([Mousa, 2020](#); [Lowe, 2021](#)), education programs ([Sorensen, 2010](#); [Scacco and Warren, 2018](#)) to interactions between army recruits over boot camps ([Carrell et al., 2015](#); [Finseraas et al., 2016](#)). The variety of protocols renders difficult the exercise of understanding the determinants of efficacy of contact interventions.

The third result is on the characteristics of the most effective contact interventions. The analysis consisted of a Lasso and post-lasso OLS estimation, with the effect size as the dependent variable, and all characteristics of the papers as regressors. Results indicate that the largest effects are found for studies investigating prejudice against older people and people with disabilities. The effect is lower when no physical encounter with a person was run, and when the outcome is measured 1 to 30 days after the end of the intervention. Interestingly, no condition identified by [Allport \(1954\)](#), [Pettigrew and Tropp \(2006\)](#) or [Lemmer and Wagner \(2015\)](#) are found to be strong predictors of the effect. Although these conditions are almost never explicitly randomized,² these results indicate that these conditions are neither necessary nor sufficient for an effective contact.

After the presentation of the results, I discuss the implications of this meta-analysis for the future on the research on the contact hypothesis.

The remainder of the paper is organized as follows. Section 2. presents the method of selection of the papers in this analysis, describes the variables of interest and presents the methodology of analysis. Section 3. presents the results. Section 4. discusses the implications of the findings and concludes on future for research on the contact hypothesis.

¹[Cohen \(1969\)](#) identifies effects with $d = 0.2$ as small, $d = 0.5$ as moderate, $d = 0.8$ as large.

²One significant exception is [Lowe \(2021\)](#) who randomizes the common goal condition.

2. Method

2.1. Paper selection

The aim of this paper is to investigate the effect of contact interventions on intergroup prejudice. In order to be included in the present analysis, studies had to involve 1) an *experimental* induction of 2) in-person contact, with clearly defined, 3) existing groups.

The experimental criterion was one of the main reason for excluding papers from the analysis. It implies that studies involving quasi-experimental variations (Vertier and Viskanic, 2018; Rao, 2019; Steinmayr, 2021) were not included in the analysis. Studies with no random assignment at all (Alesina et al., 2003; Danckert et al., 2017) were also excluded from the analysis.

The second criterion for inclusion was in-person contact. A second group of excluded studies, which is rapidly growing in quantity (in particular since the COVID-19 pandemic), regroups studies involving online encounters, such as Lenz and Mittlaender (2022).³

The third criterion was the application of an intervention on real groups, therefore excluding studies involving artificial groups. These typically include many laboratory experiments, as for instance Whitt et al. (2021), which induce conflict between groups formed during the experiment.

The papers were selected from recent meta-analyses (Lemmer and Wagner, 2015; Paluck et al., 2019), and from Google Scholar searches. For Scholar searches, all papers citing the meta-analyses or Allport's book (after 2015) were searched, as well as a word search for *contact* and *prejudice*. After the application of the three criteria of inclusion, I was left with 37 papers, with publications between 1972 and 2022, spanning nearly all continents and covering, in total, more than 17,000 individuals. The full list of papers is presented in Table A.1.

Importantly, for all selected papers, I included the main outcome variables included from the articles. For instance, in Mousa (2020), I used whether participants attended an event with Muslim players, whether they voted for a Muslim to receive an award and whether they trained with a Muslim six months after the experiment. This distinction was made because some papers have outcomes measuring different things: in Clochard (2021), I investigate the effect of contact separately on trust the specific police officers met, but also toward the police in general. Other papers reported measures separate in time, and were also included. In total, this left 62 measures from the 37 papers.

³For a meta-analysis of contact in online contexts, see Imperato et al. (2021), who find a positive effect of online contact with outgroup members.

2.2. Description of variables

There are two broad sets of variables used in this analysis: variables related to the contexts of the paper, and variables related to the contact intervention itself. Descriptive statistics are presented in the following Section.

Variables on contexts For the variables on papers and their contexts, I define six variables of interest, which I categorize as follows.

Publication year: I split the sample in three categories: before 2000, between 2001 and 2010 and after 2011.

Sample size: I used four categories: [0,50], [51,100], [101,500] and 501+.

Average age: The variable uses the average age of participants provided by the paper. Three main categories were identified, 0-18 years old, 18-25 and 25+. These categories can be broadly thought of as corresponding to “Children to high-school students”, “University students or young adults” and “General adult population”.

Zone: I divided papers according to geographical areas. This category includes Asia, Middle-East and Northern Africa, North America, Oceania, Sub-Saharan Africa and Western Europe.

Type of prejudice: This variable captures the main dimension of prejudice targeted by the contact intervention in the paper.⁴ The variable is divided in ten categories: age, caste, disabilities, gender, immigrants, LGBTQ+, police, prisoners, race / ethnicity and religion.

Type of outcome: This variable defines the type of outcome used in the paper. This variable is divided into three categories. The first category is behavior (or actions), corresponding to observed actions by participants toward the outgroup. This category can range from experimental games (Finseraas et al., 2019; Clochard, 2021) to the number of friends from another group (DeVries et al., 1977) and the number of emails exchanged with outgroup members (Marmaros and Sacerdote, 2006). The second type of outcome is explicit beliefs or attitudes about the other group. This typically involves participants to declare whether they agree with a pre-defined set of statements explicitly about the other group, e.g. “Affirmative action in college admissions should be abolished” (Boisjoly et al., 2006), “Disabled people are often grumpy and moan about everything” (Krahé and Altwasser, 2006). The last outcome category is implicit behavior. The main outcome used in this case is some version of the Implicit Association Test (Greenwald et al., 1998), for which participants have to click as fast as possible to associate positive or negative words between the different groups. I also defined a dummy variable called *Measure for the entire group* to distinguish measures involving the specific individuals met by participants and the entire outgroup.

⁴For instance, in Clochard et al. (2022) the contact appears to be more effective for older individuals and people with lower education, but the main prejudice dimension studied is inter-ethnic trust.

Variables on contact intervention The contact interventions are widely heterogeneous. I identified several characteristics, which were coded as the following.

Type of contact intervention: The variable consists in broad categories of contact interventions. Seven categories are defined: army recruits, classmates (or participation in a similar course), participation in a collaborative task, discussions (from scripted discussions as in Page-Gould et al. (2008) to door-to-door canvassing in Kalla and Broockman (2020)), lectures, roommates/neighbors and sports teams.

The second broad set of intervention variables are used to characterize the interventions with respect to Allport (1954)'s conditions. The first is *equal status* among groups, with typically members of sports teams or classmates considered equal, but canvassing operations considered as non-equal. The second condition is the clear objective of a *common goal* between participants. For instance, playing with someone from another caste in one's own team in Lowe (2021) is considered as having a common goal, but when the other caste member is on the other team, the common goal condition is not satisfied. The third condition is a *positive contact*. This condition is satisfied if the individual met during the intervention counters the initial stereotype. For instance, in Carrell et al. (2019), African American peers with excellent academic records are considered as inducing a positive contact, but peers with low high-school grades are not. The fourth category is the *support of authorities*. The support of authorities was typically coded as 0 if the focus of the exchange was explicitly not framed as involving the prejudice, e.g. the focus of the course in Scacco and Warren (2018) is to improve computer skills, not inter-religious relations.

The third set of variables relate to other characteristics of the interventions which have been found in the literature to potentially moderate the influence of contact. The first variable from this set is *Personal interaction*, which is equal to zero, for instance, if participants are presented with individuals from the outgroup but do not personally interact with them, as in Grutzeck and Gidycz (1997). The second characteristic is the *friendship potential*, typically defined for members of the same sports teams (Mousa, 2020) but not for teachers (Dessel, 2010). The third variable is the scriptedness of the interactions between members. The intervention was considered as scripted when there is a clear detail of what the participants needed to discuss (Broockman and Kalla, 2016; Freddi et al., 2022), and not scripted if the interaction was more free-form (Barnhardt, 2009; Finseraas et al., 2016). Variables for Allport's conditions, personal interaction, friendship potential and scriptedness were all coded as dummy variables.

Several variables related to the repetition of contacts were also defined as follows. The *number of encounters* with people from the other group was divided into four categories, 0, 1, 2-10 and 10+. The *duration of the contact* (in days) intervention was also categorized as 1 (typically one shot intervention), 2-30 and 30+. The length of *time between intervention and measure* was also coded as 0 (immediately after the intervention, including the end of the year for year-long interventions), 1-30 and 30+ days.

2.3. Analysis methodology

For all papers in the analysis, the effect size was normalized using Cohen (1969)'s d statistic ($d = \frac{\text{Effect Size}}{\text{Standard Deviation}}$). The variable was coded so that the effect is positive if contact improves intergroup perceptions (increased trust, more outgroup friends, etc).

The average effects, as well as the heterogeneity measures were performed using the methodology by Deeks et al. (2001): the average effect θ_{IV} is calculated as the weighted average of all treatment effects $\theta_{IV} = \frac{\sum w_i \theta_i}{\sum w_i}$, with reciprocals of the standard errors as weights. The heterogeneity metric is Cochran (1950)'s $Q = \sum w_i (\theta_i - \theta_{IV})^2$.

To investigate which characteristics of the contact matter most for efficacy, a Lasso estimation was performed with the standardized effect as the dependent variable, and all the characteristics presented above as regressors. A post-lasso OLS estimation was then performed.

3. Results

3.1. Descriptive statistics

In Tables 3.1 and 3.2 are presented the descriptive statistics of the papers used in the paper. While a significant number of papers were published before 2000, the bulk of the experimental contact literature has been done since 2010, with almost half of papers considered published between 2011 and 2022.

As was highlighted in previous analyses (Lemmer and Wagner, 2015; Paluck et al., 2019), a very large fraction of contact interventions were ran in North America and Western Europe, and no paper was run in Latin America, for instance. A very large fraction of the interventions were also conducted on young samples, from children to university students, although more recent papers focused more on general adult populations. The three modal prejudices studied are race or ethnicity, LGBTQ+ and religion.

One interesting fact is that almost all measures focus on the effects of contact on the entire outgroup, and not the outgroup members specifically concerned. We discuss potential implications in the last section.

One clear result is that there exists a lot of heterogeneity in the type of contact interventions used. The most common form of intervention typically involves scripted discussions (Broockman and Kalla, 2016; Clochard, 2021). The army, sports teams and roommates also provide special contexts which have been studied a lot. Contacts can last for a long time - e.g. roommates sharing a room for the entire first year of university (Boisjoly et al., 2006; Corno et al., 2019) - or be very short (Page-Gould et al., 2008; Boag and Wilson, 2014).

Contrary to what was found in the broader prejudice-reduction literature (Paluck et al.,

Table 3.1 – Descriptive statistics of contexts of papers

Variable	N
<i>Publication year</i>	
≤ 2000	11
2001-2010	17
2011-2022	34
<i>Sample size</i>	
[0,50]	8
[51,100]	6
[101,500]	26
501+	22
<i>Average age</i>	
0-18	16
18-25	28
25+	18
<i>Zone</i>	
Asia	3
Middle-East and Northern Africa	4
North America	35
Oceania	4
Sub-Saharan Africa	5
Western Europe	11
<i>Type of prejudice</i>	
Age	1
Caste	2
Disabilities	6
Gender	2
Immigrants	7
LGBTQ+	10
Police	2
Prisoners	1
Race or ethnicity	25
Religion	6
<i>Type of outcome</i>	
Behavior	21
Explicit beliefs or attitudes	37
Implicit behavior	4
Measure for the entire outgroup	54

Table 3.2 – Descriptive statistics of contact interventions

Variable	N
Army recruits	6
Classmates	9
Collaborative task	2
Discussions	28
Lecture	1
Roommates / neighbors	9
Sports team	7
<i>Allport's conditions</i>	
Equal status	41
Common goal	42
Positive contact	56
Support of authorities	49
Personal interaction	58
<i>Other conditions</i>	
Friendship potential	42
Scriptedness	31
<i>Number of encounters</i>	
0	2
1	16
2-10	16
10+	28
<i>Duration of the contact (in days)</i>	
1	20
2-30	12
30+	30
<i>Length between contact and measure (in days)</i>	
0	32
1-30	19
30+	11

2021), the samples for the papers are relatively large, with the median sample consisting of approximately 350 subjects, and while a large fraction of the literature investigated the immediate effects of contact, 11 studies provide tests of enduring effects of contact after one month, with [Camargo et al. \(2010\)](#) measuring the effects of being randomly assigned a Black roommate two years after the end of the first year of university.

3.2. Is contact effective?

The forest plot of effects and standard errors are plotted in Figure 3.1. We can see that contact is not found to significantly increase prejudice for any paper, and that for the majority of papers, contact induces a significant reduction of prejudice (positive effect).

Meta-analytic results indicate that the average estimated effect of contact is 0.329 (SE = 0.007). This measure is highly significant ($p < 0.01$). This result means, that, on average, contact *is effective at reducing prejudice*. The magnitude is very similar to that found [Paluck et al. \(2019\)](#), and can place, on average, contact as having small to moderate effects, as categorized by [Cohen \(1969\)](#)'s rule of thumb.

There also exists a large heterogeneity between effects ($Q = 4243$, $p < 0.01$). Figure 3.2 displays the funnel plot of the sample. While we cannot reject a systematic bias, for instance due to publication bias, I do not replicate [Paluck et al. \(2019\)](#)'s result of correlation between effect size and standard error ($p = 0.825$). However, there exists a correlation between the standardized effect and the sample size (Appendix B), which could be a sign of bias. On the other hand, the funnel plot is relatively symmetric, with 30 outcomes lower than the weighted average, and 32 higher. It is therefore arguable that the heterogeneity of observed effects is due to heterogeneity in contexts and methodologies.

3.3. What characteristics matter for contact?

Differences by characteristic In Appendix C are presented effect sizes as a function of the different variables presented in Section 2..

Age: It appears that studies involving the general population (age 25+) tend to have a lower average effect (Table C.1), and also tend to be have more precise estimates (Figure C.1). This result could be the mere consequence of the fact that studies involving the general adult population tend to be better powered, and therefore provide more accurate estimates, but could also indicate that contact intervention are less effective among adults (although this result would contradict the heterogeneous treatment effects found in [Clochard et al. \(2022\)](#)).

Type of prejudice: No clear pattern emerges as a function of the type of prejudice studied. Most prejudices have a wide range of variation, and although the average effect is higher for papers about prisoners or for race/ethnicity, the number of papers by category is too small to be definitive about significant differences (Table C.2, Figure C.2).

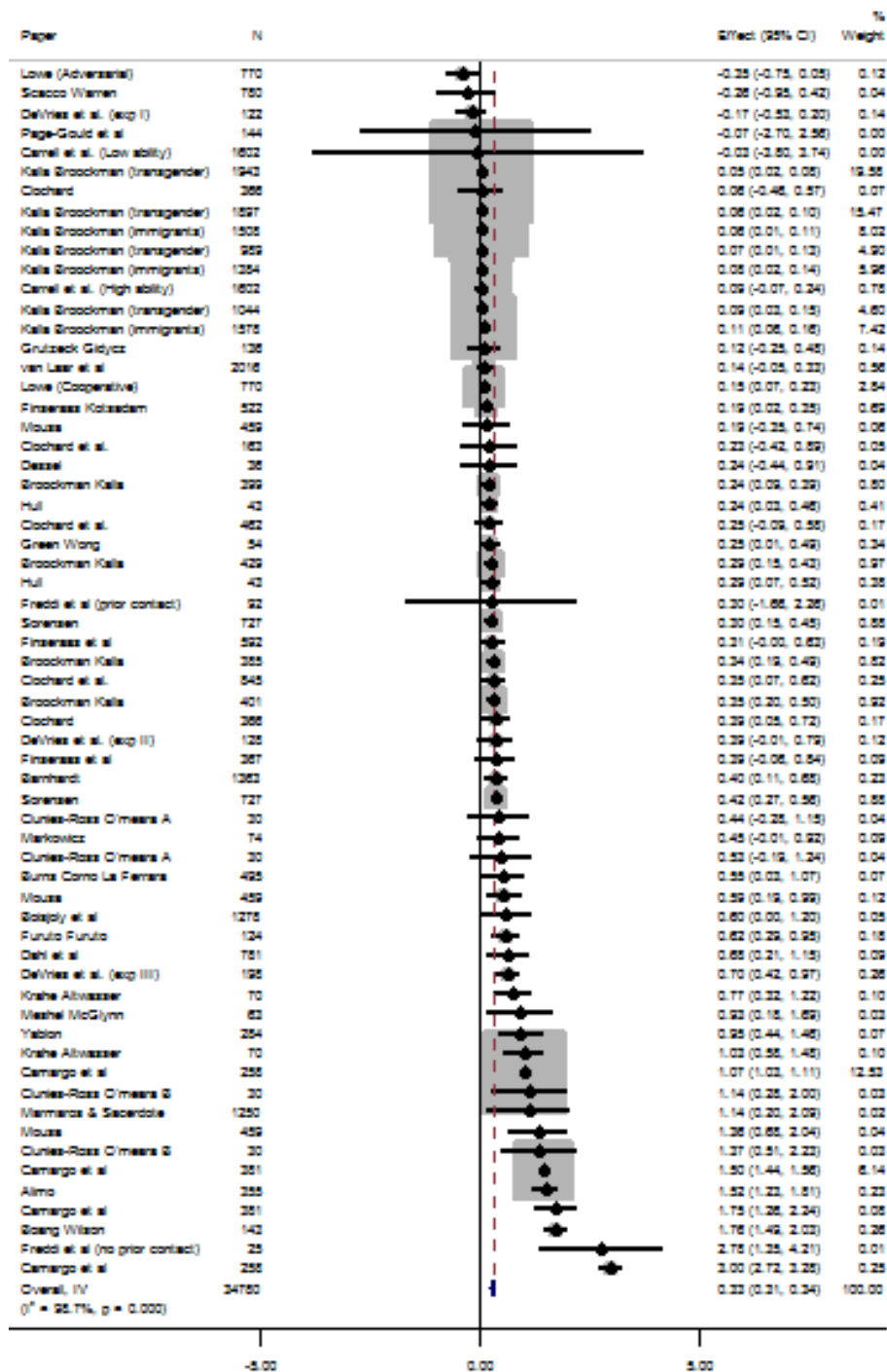


Figure 3.1 – Forest plot of estimated treatment effects. Papers are ordered by their point estimates, with the lowest at the top, and the largest treatment effects at the bottom. The overall estimated effect of contact is displayed at the bottom of the graph.

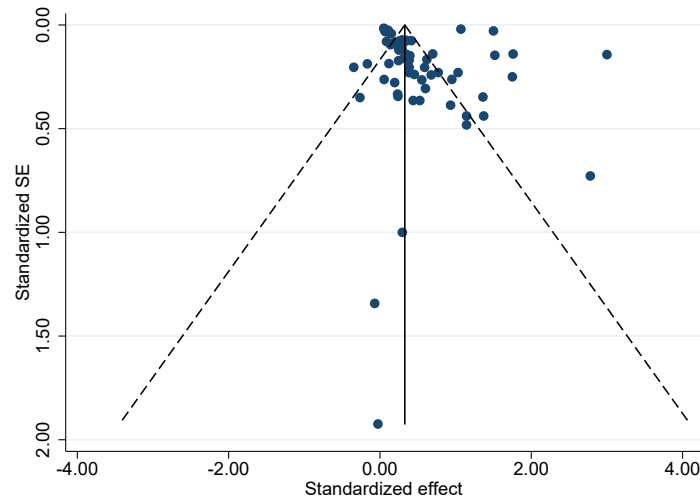


Figure 3.2 – Funnel plot

Type of intervention: As for the type of prejudice, no clear pattern emerges from the comparison of the type of contact intervention, in large part due to the low number of observations for the majority of categories (Table C.4, Figure C.4).

Number of encounters: Figure C.5 shows that the average effect is significantly higher for studies with more than 10 encounters. It would thus appear that the higher the number of signals received, the higher the effect.

Duration of contact: The pattern which seems to appear from Figure C.6 and Table C.6 is that the longer the contact, the stronger its effect. This would, as for the number of encounters, point to the result that the more signals received, the more effective the contact.

Time between end of intervention and measurement: No clear pattern emerges from Figure C.7 and Table C.7. One thing clear is that there exist some protocols for which the effect of contact lasts a long time, up to two years following the intervention.

Measure for the entire group: As we can see from Table C.8, and although the sample of measures of the effect of contact toward specific outgroup members is relatively small ($N = 8$), it seems that the effect of contact appears to be larger for the specific individuals met than for the entire outgroup. While the average effect of contact on prejudice is still significantly positive for measures on the effect toward the entire outgroup, it would appear to be only 10% of that of individuals. This can be relatively easily understood as coming from a weaker signal for the entire outgroup than for the specific individuals met.

Lasso In order to understand which characteristic matter more to the magnitude of the effect of contact on prejudice, I performed a Lasso estimation, using the standardized effect size as the outcome, with all the variables described above as regressors. After the estimation, for the selected variables, I performed an OLS estimation to observe the

unbiased effect of these variables on the effect. For the estimations, sample sizes were used as analytical weights.

Results are presented in Table 3.3. They indicate that the effect of contact on prejudice is stronger if the prejudice considered is Age or Disabilities, and if there is a strictly positive number of encounters.

Interestingly, the algorithm selected neither the characteristics proposed by Allport (1954) nor those proposed by later reviews (Pettigrew and Tropp, 2006; Lemmer and Wagner, 2015; Paluck et al., 2019). It therefore appears that these conditions are neither necessary nor sufficient for effective contact interventions. Of course, this analysis is descriptive, as the conditions are rarely randomly allocated among participating individuals (Lowe (2021) being the exception).

The implications of this meta-analytic work, and hypothetical paths for the future of the contact literature are discussed in the following section.

Table 3.3 – Lasso coefficients and Post-Lasso OLS estimation

	Lasso	Post-Lasso OLS
Prejudice = Age	0.408	0.577*** (0.179)
Prejudice = Disabilities	0.072	0.293 (0.179)
Number of encounters = 0	-0.070	-0.301* (0.179)
Days after end of contact 1-30	-0.016	-0.232 (0.210)
Constant	0.341	0.588*** (0.110)
R^2		0.077
No. obs		62
Average dependent variable		0.545
Standard deviation dependent variable		0.644

In column 1 are displayed the Lasso coefficients for the selected variables. Results of the OLS estimation with only the selected coefficients are presented in column 2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

4. Discussion

In this paper, I conduct a meta-analysis of the literature on the contact hypothesis. While the sample of the initial meta-analysis by Pettigrew and Tropp (2006) consisted almost entirely on descriptive, non-experimental evidence on the effect of contact, the number of experiments using contact is rapidly growing, with the added bonus of widening

the geographic origins of samples. While the bulk of the research still takes place in the US, there is now a growing number of studies from other parts of the world, and in particular developing countries.

Moreover, the experimental literature on contact is also becoming more credible, with the use of relatively large sample sizes. Nearly all papers published after 2010 in this analysis would be considered a “large study”, using the taxonomy by [Paluck et al. \(2021\)](#), with an average contact group of more than 75 individuals.⁵ All papers after 2015 have also made use of a pre-analysis plan, reinforcing the credibility of findings.

The present meta-analytic exercise, however, has highlighted three main limitations of the current literature on contact. The first main limitation of the contact literature is the lack of discussion about pre-experimental prejudice. In the typical literature, the absolute level of prejudice is computed from a survey at baseline (before treatment), but no discussion is had about the meaning of observed levels. Moreover, there typically lacks the counterfactual exercise of a comparison group. For instance, White participants are asked whether they agree with the statement “Do you think Black people can be trusted?”, but never “Do you think White people can be trusted?”. This lack of comparison group forbids much of the literature to identify prejudiced behavior at baseline.

Furthermore, whenever papers do identify pre-experimental levels of discrimination, such as [Finseraas et al. \(2016\)](#), they rarely identify the underlying source of prejudice (i.e. taste-based vs statistical). There also often is a lack of explicit discussion about the absolute levels of prejudice.

The second main limitation of the literature is the lack of consistency in use of the term “contact”, as was described in Section 2.. The wide variety of protocols falling under the umbrella of contact – from sports leagues to canvassing to interactions between classmates or army recruits – makes difficult the comparison of different interventions. In my opinion, future experiments should put more emphasis on the exact content of the contact interventions, not merely the context in which they occur. Now that several meta-analytical works have been carried out, all highlighting the benefits of contact interventions, the focus of the literature should be shifting from the question of *whether* contact interventions are effective, but on the *how* they work. On the note of understanding the mechanisms through which contact can impact prejudice, more effort should be devoted to randomly allocating contact conditions within an experimental sample - *à la* [Lowe \(2021\)](#). This would enable the field to understand which characteristics of protocols are effective at improving intergroup relations. This exercise could help confirm, or disprove, the descriptive results from the lasso analysis above.

The third limitation of the literature is the lack of a theoretical framework which could explain the effects of contact. Of all the papers analyzed in the present paper, only

⁵The only exception would be the sub-group without minority peers in [Freddi et al. \(2022\)](#).

two (Lowe, 2021; Clochard, 2021) make attempts at designing a theoretical framework to explain the effects of contact on prejudice. The literature should work hand in hand with the literature on belief updating. One potential solution could be to integrate a form of updating in the literature on stereotyping (Bordalo et al., 2020). In this regard, analyzing a clear distinction between the effects of contact on the specific individuals met, and the effects of contact on the entire outgroup, could prove useful.

To conclude, the results of the present analysis point to a potential hope for the future of contact interventions. One of the main consistency in the existing literature is the fact that the more interactions participants have with members of the outgroup, the more effective the contact (Page-Gould et al., 2008; Clochard, 2021). This result could mean that a contact functions as a signal about the outgroup, and the higher the number of signals, the larger the shift of the distribution. Because more contacts induce stronger responses, this means that contact can be more suited to reducing statistical discrimination than taste-based discrimination. This could mean that contact is a particularly interesting tool to reduce prejudice in the labor market, as it has been found that the main source of prejudice in this domain is statistical (List, 2004). Now that we know contact interventions can reduce prejudice, efforts should be made to investigate whether contact interventions can also reduce the pernicious effects of prejudice on society and the economy.

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Appendices

A List of all papers

The description of all the papers with all the variables used in the present paper can be found [here](#).

The list of papers is detailed in Table A.1.

Table A.1 – List of all papers used in the analysis

Paper
Alimo (2012)
Barnhardt (2009)
Boag and Wilson (2014)
Boisjoly et al. (2006)
Broockman and Kalla (2016)
Camargo et al. (2010)
Carrell et al. (2019)
Clochard (2021)
Clochard et al. (2022)
Clunies-Ross and O'meara (1989)
Corno et al. (2019)
Dahl et al. (2021)
Dessel (2010)
DeVries et al. (1977)
Finseraas et al. (2016)
Finseraas et al. (2019)
Finseraas and Kotsadam (2017)
Freddi et al. (2022)
Furuto and Furuto (1983)
Deeks et al. (2009)
Grutzeck and Gidycz (1997)
Hull IV (1972)
Kalla and Broockman (2020)
Krahé and Altwasser (2006)
Lowe (2021)
Markowicz (2009)
Marmaros and Sacerdote (2006)
Meshel and MCGlynn (2004)
Mousa (2020)
Page-Gould et al. (2008)
Scacco and Warren (2018)
Sorensen (2010)
Van Laar et al. (2005)
Yablon (2012)

B Effect size as a function of the sample size

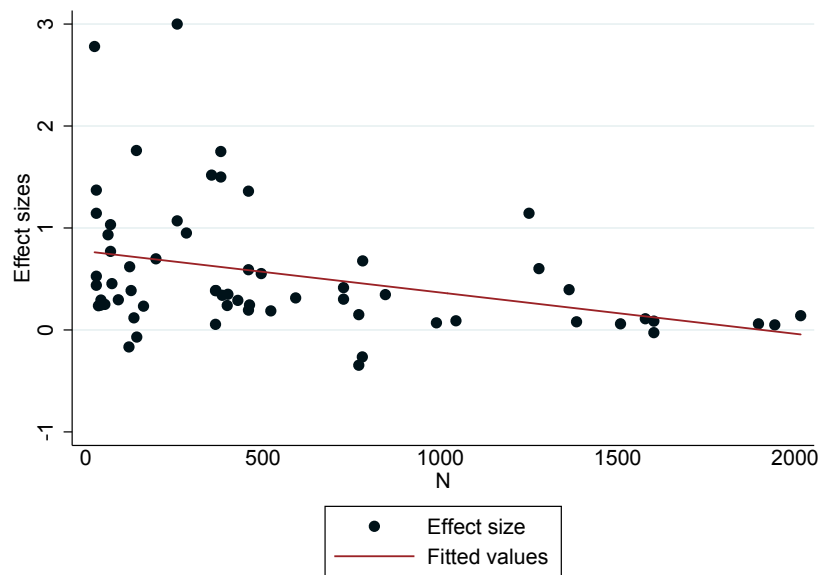


Figure B.1 – Effect size as a function of the sample size

Table B.1 – Effect size as a function of the sample size

Effect by sample size	
N	-0.0004*** (0.000)
Constant	0.7718*** (0.111)
R^2	0.120
No. obs	62

The dependent variable is the standardized effect size. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

C Effect size as a function of characteristics

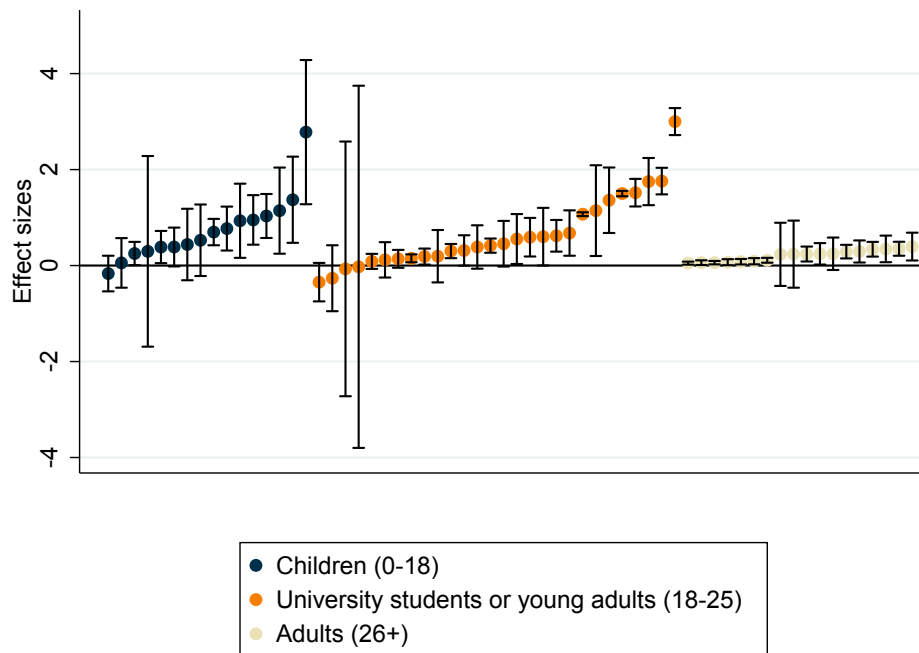


Figure C.1 – Effect size as a function of age category of the sample

Table C.1 – Average effect size per age category

Age category	Average effect	Standard error	Number of measures
0-18	0.492	0.057	16
18-25	0.953	0.014	28
25+	0.084	0.008	18

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same age category.

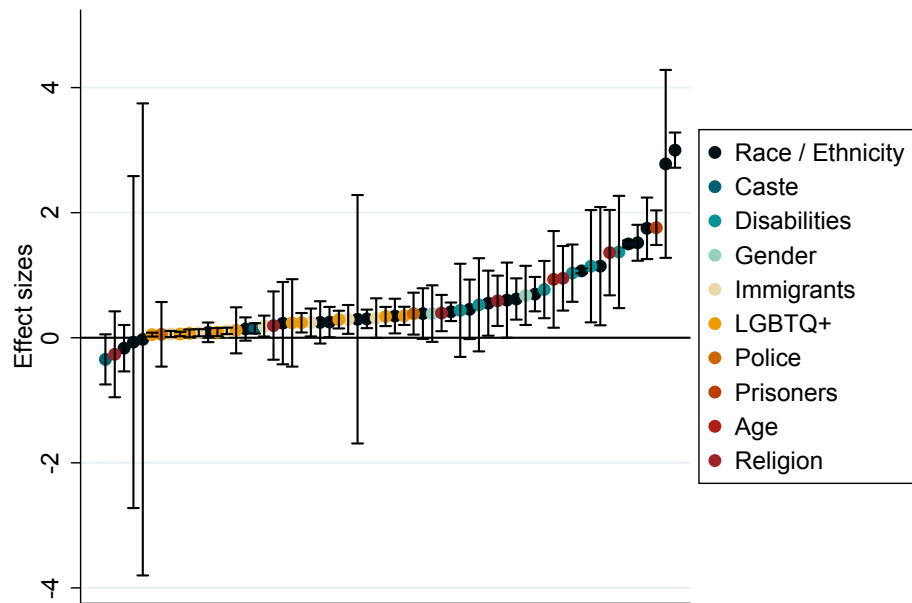


Figure C.2 – Effect size as a function of the type of prejudice

Table C.2 – Average effect size per prejudice

Type of prejudice	Average effect	Standard error	Number of measures
Age	0.933	0.387	1
Caste	0.130	0.041	2
Disabilities	0.860	0.126	6
Gender	0.525	0.166	2
Immigrants	0.094	0.015	7
LGBTQ+	0.078	0.010	10
Police	0.288	0.143	2
Prisoners	1.760	0.140	1
Race / ethnicity	1.051	0.014	25
Religion	0.508	0.094	6

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same category of prejudice.

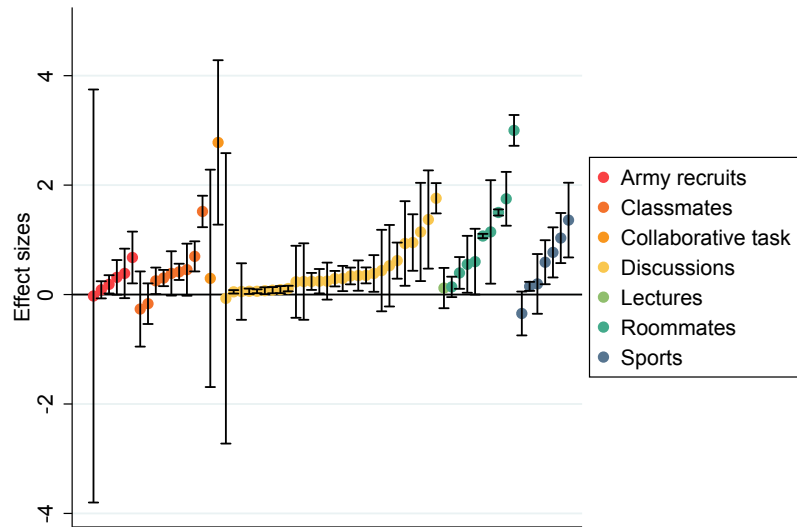


Figure C.3 – Effect size as a function of the type of intervention

Table C.3 – Average effect size per type of contact intervention

Type of contact intervention	Average effect	Standard error	Number of measures
Army recruits	0.191	0.052	6
Classmates	0.437	0.041	9
Collaborative task	1.920	0.589	2
Discussions	0.094	0.008	28
Lecture	0.119	0.186	1
Roommates	1.192	0.016	9
Sports	0.206	0.039	7

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same type of contact intervention category.

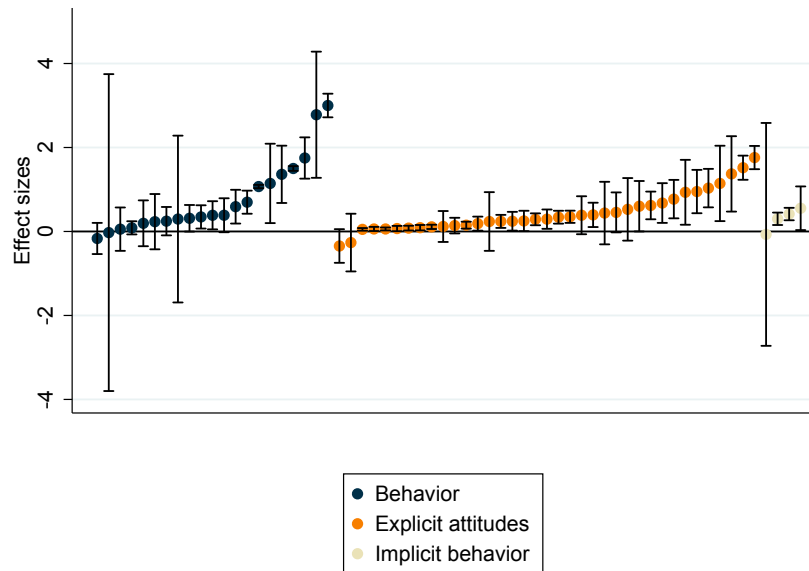


Figure C.4 – Effect size as a function of the type of outcome

Table C.4 – Average effect size per type of outcome

Type of outcome	Average effect	Standard error	Number of measures
Behavior	1.129	0.015	21
Explicit attitudes or beliefs	0.104	0.008	37
Implicit behavior	0.365	0.052	4

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same type of outcome category.

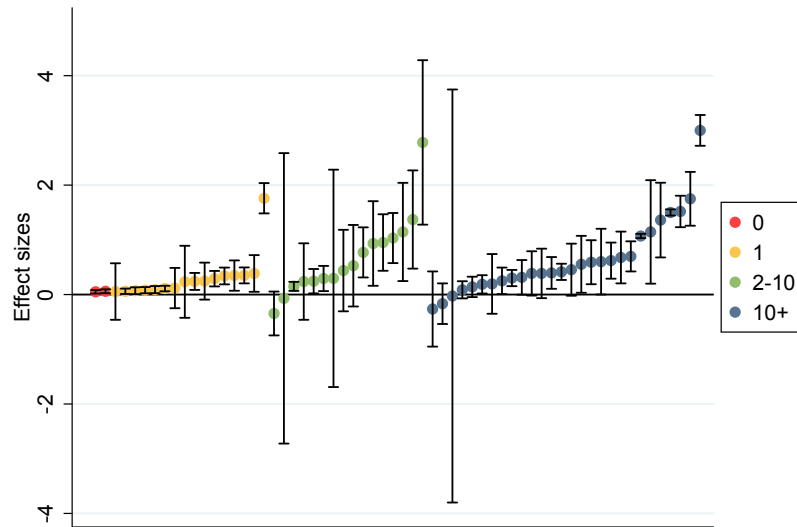


Figure C.5 – Effect size as a function of the number of encounters

Table C.5 – Average effect size per number of encounters

Number of encounters	Average effect	Standard error	Number of measures
0	0.054	0.012	2
1	0.121	0.012	16
2-10	0.238	0.034	16
10+	1.020	0.014	28

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same category of number of encounters.

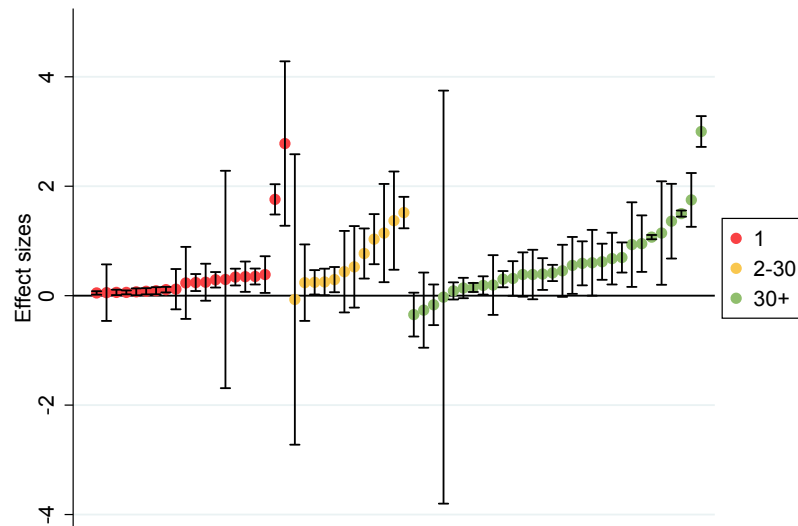


Figure C.6 – Effect size as a function of the duration of the contact

Table C.6 – Average effect size per duration of the contact intervention

Duration of the contact (days)	Average effect	Standard error	Number of measures
1	0.088	0.008	20
2-30	0.541	0.054	12
30+	0.930	0.013	30

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same category intervention duration.

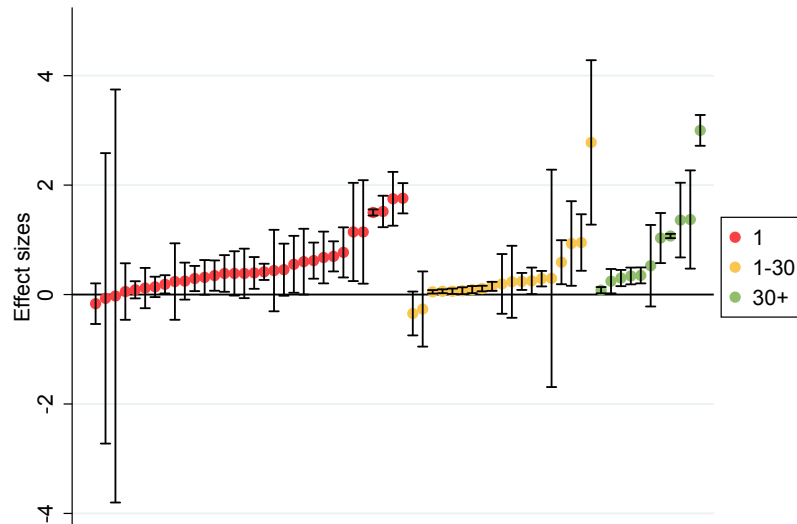


Figure C.7 – Effect size as a function of the time between the end of the intervention and the measure

Table C.7 – Average effect size per length between the intervention and measure

Length between end of intervention and measure (in days)	Average effect	Standard error	Number of measures
1	0.952	0.020	32
2-30	0.078	0.009	19
30+	0.719	0.015	11

This table represents average effect sizes and standard errors, from a meta-analysis of all papers in the same category length of outcome.

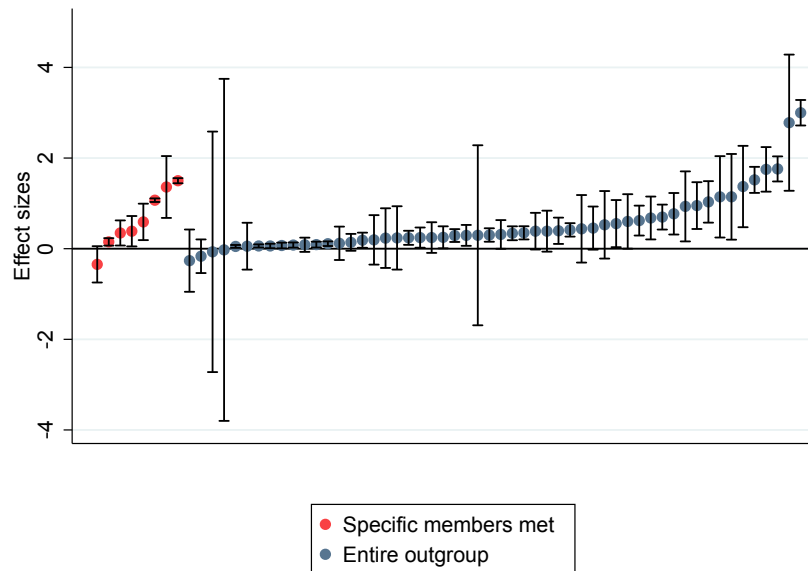


Figure C.8 – Effect size as a function of whether the outcome measures prejudice against specific individuals met or the entire outgroup

Table C.8 – Average effect size on whether the outcome measures prejudice against specific individuals met or the entire group

Measure	Average effect	Standard error	Number of measures
Specific members met	1.048	0.015	8
Entire outgroup	0.123	0.008	54

This table represents average effect sizes and standard errors, from a meta-analysis of papers by whether the measure was for the entire outgroup.

Chapter 4

More Effort or Better Technologies? On the Effect of Relative Performance Feedback

Note: This chapter is co-authored with Guillaume Hollard and Julia Wirtz.

Abstract

Relative performance feedback (RPF) allows agents to compare their performance to that of others. Current theory assumes that RPF affects performance by changing the optimal level of effort. We introduce a *technology* channel in which agents use RPF to improve their technologies. We compare the effort and technology channels by combining three elements: an extensive review, an original model and two field experiments. Under the technology channel, we highlight that RPF increases performance even at the *bottom* of the distribution and has a *cumulative* effect across periods. We draw implications for education and social norms.

JEL Codes: D83, D84, D91

Keywords: Relative performance feedback, rankings, technology improvement, Education, Social Norms¹

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1. Introduction

Relative performance feedback (RPF henceforth) consists of information, such as rankings, that enables individuals to compare their own performance to that of others. RPF is ubiquitous in the economy: employers provide information on workers' relative productivity, academics are able to compare themselves using publicly-available citation indices, the TripAdvisor website publishes rankings of hotels, students are sometimes told their rank as well as their grades, and so on. The effect of RPF on performance is, however, still open to debate.

We are specifically interested in what agents can learn from RPF in order to take optimal decisions. To date, the effect of RPF on performance has always been examined with the assumption that the only choice variable affecting performance is *effort*. We refer to this as the effort channel. The novel approach of this paper is to introduce the possibility that RPF may also trigger a change in *technology* by helping to identify better technologies. We refer to this as the technology channel.

Consider the example of a student competing in a selective University-entrance exam which takes place over several rounds. On the one hand, RPF gives her information about the payoff of her effort. If she learns that she is close to making the cut, she may work harder; if she instead learns that she ranks poorly, she may scale back her effort. On the other hand, RPF provides information about the quality of her (learning) technology. If she ranks badly, she may realize that there exist better technologies and change the way in which she prepares for the exams (e.g. studying in a group rather than alone), which may produce substantially better performance. The introduction of a technology channel in the analysis of the effect of RPF brings about a significant departure from the predictions of the effort channel, which should be particularly notable in situations in which technology likely has a large effect on performance. In what follows, we refer to these situations, in which agents can change to a better technology (and have the time necessary to do so), as having large *room for improvement*.

To assess the economic relevance of the technology channel, we combine three elements. First, we systematically *re-evaluate existing evidence* on the effect of RPF to establish whether the specific setting in each study has any scope for technological change, considering the task and the timing of feedback. We find that controlling for room for improvement allows us to make sense of otherwise conflicting evidence on the effect of RPF on performance. For instance, all of the analyses involving a task with room for improvement find a positive performance effect for low performers (while this effect is mixed when there is no room for improvement). Second, we *propose an original model* of technology improvement and contrast its predictions regarding the effect of RPF to those from a standard model of effort choice under tournament incentives. Last, since few experiments have been designed to compare the effect of RPF with and without room for

improvement, we consider the results from *two experiments*: one in which the technology channel is shut down, so as to leave effort as the only active channel for performance improvement, and the other in which there is clear room for improvement. The first experiment uses a task (counting numbers) in which little technological improvement is expected. In contrast, the second experiment involves pupils who have to perform several math tests. The experiment took place over several weeks so as to allow improvement over time. Both experiments consist in comparing performance with and without RPF under tournament incentives.

Our main finding is that the three elements (review, model and experiments) complement each other and underline two important differences between the effort and technology channels. (1) The effect of RPF on low performers is strikingly different when there is room for technological improvement (while there are only small differences for top or average performers): in general, low performers benefit from exposure to RPF when there is room for improvement. On the contrary, the effect of RPF is expected to be zero or negative when technological change is not an option. The effect of RPF thus depends crucially on the availability of alternative technologies to carry out the task. (2) Repeated exposure to RPF produces a *cumulative* effect over time when there is room for improvement; In contrast, when effort is the only source of performance improvement the RPF effect is expected to be constant over time. The effect of repeated exposure has rarely been analyzed to date and, to the best of our knowledge, the present work is the first to explain why the effect of repeated exposure may depend on the room for improvement. While a complete analysis of the effect of repeated exposure to RPF is beyond the scope of the present paper, we nonetheless emphasize the dynamic aspect of feedback as a blind spot in the literature.

When should RPF be provided? Considering a technology channel, in addition to the traditional effort channel, suggests that the answer to this question should depend on the nature of the task to be performed (i.e. the possibility of technological improvement), an element that is so far absent from analysis. A clear argument can be made for the use of RPF in tasks with room for improvement. Our approach can explain the large performance improvement of low-ability students that is often documented in the empirical RPF literature. Rank feedback may indeed help students to improve their learning technology, and in particular those who initially rank poorly.¹ It is noteworthy that providing (private) RPF, for instance to students, entails a negligible cost.

The remainder of this paper is organized as follows. In Section 2. we review the empirical evidence on the effect of RPF on performance and show that we are able to organize this literature neatly by controlling for the room for improvement. Section 3. then reviews the existing theoretical models of the effect of RPF on performance, and presents

¹The results presented do not require that feedback be public. In particular, the feedback can be sent privately so as to avoid the effect of public shaming.

our own model of technological change. In Section 4. we present the two experiments we carried out, which differ in their scope for technological innovation - i.e. the room for improvement. Section 5. concludes.

2. A review of the effect of RPF through the lens of the technology channel

In this section we review the empirical evidence on the performance effect of RPF. The novelty of this review is to distinguish between tasks with little room for improvement and those with greater scope for technological innovation. We show that the effort channel to be dominant when there is little room for improvement, while the technology channel becomes relevant when there is more room for improvement. We will present forest plots separating high and low performers, and show that RPF has strikingly different effects towards the bottom of the distribution.

2.1. Paper selection

We consider all references in the extensive and very-recent review of [Villeval \(2020\)](#) on performance feedback. We select all of the contributions in this review that satisfy the following three conditions:

(1) The experiment includes a control group that does not receive any RPF. For instance, [Genakos and Pagliero \(2012\)](#) is not included as all subjects received an interim ranking, so that there is no control group without ranking.²

(2) There is a clear measure of performance. For example, [Wozniak et al. \(2014\)](#); [Banerjee et al. \(2018\)](#); [Danz \(2020\)](#) focus on the effect of RPF on competitiveness, rather than performance per se. We also exclude [Ertac \(2011\)](#); [Mobius et al. \(2011\)](#); [Ertac et al. \(2016\)](#); [Zimmermann \(2018\)](#), which focus on beliefs about performance. [Jalava et al. \(2015\)](#) is also excluded, as they look at the effect on performance of making rankings public and not the effect of receiving ranking information itself.

(3) The same incentive scheme is used in the control and treatment groups. We only include papers comparing the effect of RPF while holding the incentive scheme constant. Incentive schemes include tournaments, piece-rate, flat-rate, and so on. We exclude for example [Casas-Arce and Martínez-Jerez \(2009\)](#), who provide flat-rate incentives to the control group while the treatment group with RPF takes part in a tournament.

We are left with 42 papers after excluding those that do not satisfy all three criteria. As some of these include multiple treatments, our final analysis is of 66 different treatments, which are summarized in Tables A.1 to A.5.

²Other papers excluded for the same reason are [Gill and Prowse \(2012\)](#); [Buser et al. \(2018\)](#); [Haenni \(2019\)](#).

2.2. Classification

(1) **Incentive scheme.** We sort the treatments according to the four incentive schemes subjects may face: piece rate, flat-rate, tournament, and grades.³

(2) **Room for improvement** We separate the treatments into two categories: little and large room for improvement. This depends both on the timing of feedback and the nature of the task that subjects carry out. While there is some degree of subjectivity in this classification, it is straightforward in most cases: it is unlikely that subjects can quickly change their technology to improve their performance when feedback is given with little or no time for adjustment. This is the case for subjects who see their competitors while running (Fershtman and Gneezy, 2011) or the scores of other participants while adding or multiplying numbers (Eriksson et al., 2009; Kuhnen and Tymula, 2012). On the other hand, there is greater room for improvement in tasks that are new to subjects, where there exist a number of potential technologies to complete the task, and when subjects have sufficient time to revise their technology (Azmat and Iriberry, 2010; Blanes i Vidal and Nossol, 2011; Tran and Zeckhauser, 2012).

We consider three outcome variables: the average effect, and the effect at the top and the bottom of the performance distribution. To enable comparisons across studies, results are normalized using Cohen's d -statistic. "Top" and "Bottom" are roughly defined, as it is not always possible to obtain information for the exact same sub-group (like, say, the top and bottom quartiles). Despite our best efforts, the definition of "top" and "bottom" does then vary across papers.

2.3. Results

We present three graphs that summarize the existing empirical evidence on the effect of RPF. A first result is that for top performers the effect is positive in most treatments. As can be seen in Figure 4.1, only two treatments find a significant negative effect with little room for improvement and one with large room for improvement. The incentive scheme does not appear to play a clear role. In contrast, room for improvement is associated with a larger and more-frequently positive effect of RPF.

An even sharper difference can be seen when we consider the bottom of the distribution in Figure 4.2. Intuition suggests that poor performers may be harmed by RPF, becoming discouraged and dropping out when they realize that they are performing poorly. This intuition may well hold when there is little room for improvement, and we see a significant share of experiments in which the effect is negative. In sharp contrast, when

³We consider grades separately, as the associated incentives are ambiguous. Grades may be equivalent to piece-rate incentives (when the individual's payoff increases proportionally to the grade), but may have a tournament aspect (when rank matters for future opportunities) or could work like a flat rate (when the student attaches little importance to them).

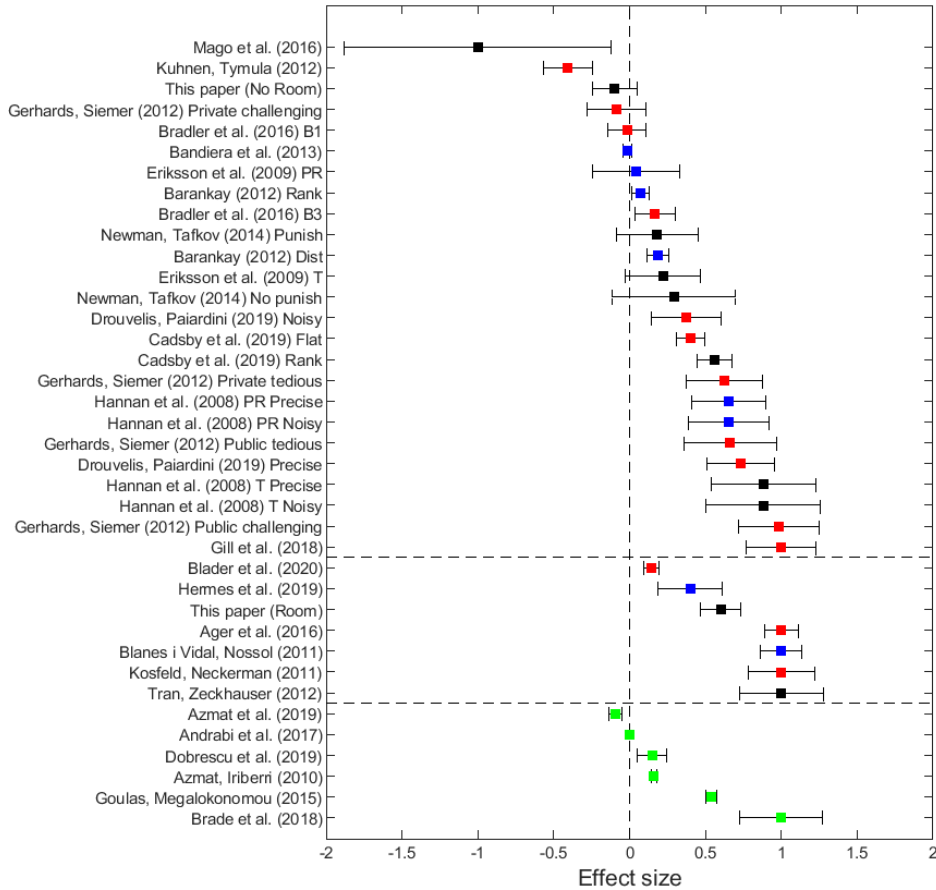


Figure 4.1 – Forest plot of the effect of RPF at the top of the distribution

Note: Each square represents an effect size at the top of the performance distribution for each of the treatments, and the bars the confidence intervals. The colors represent the incentive schemes: flat (red), grades (green), piece-rate (blue) and tournament (black). There is little room for improvement in the treatments above the first dashed line, and large room for improvement below. The treatments are ranked by effect size in each category (room vs. no room for improvement). The grade treatments appear separately below the second dashed line. For visibility, the effect sizes are truncated to lie within the [-1,1] range.

there is more room for improvement, the effect is always positive. We suggest that subjects benefit from RPF as they realize that there exist better technologies, which they can use to improve their performance. Again, while room for improvement appears to play a role, the incentive scheme does not have a clear and predictable effect.

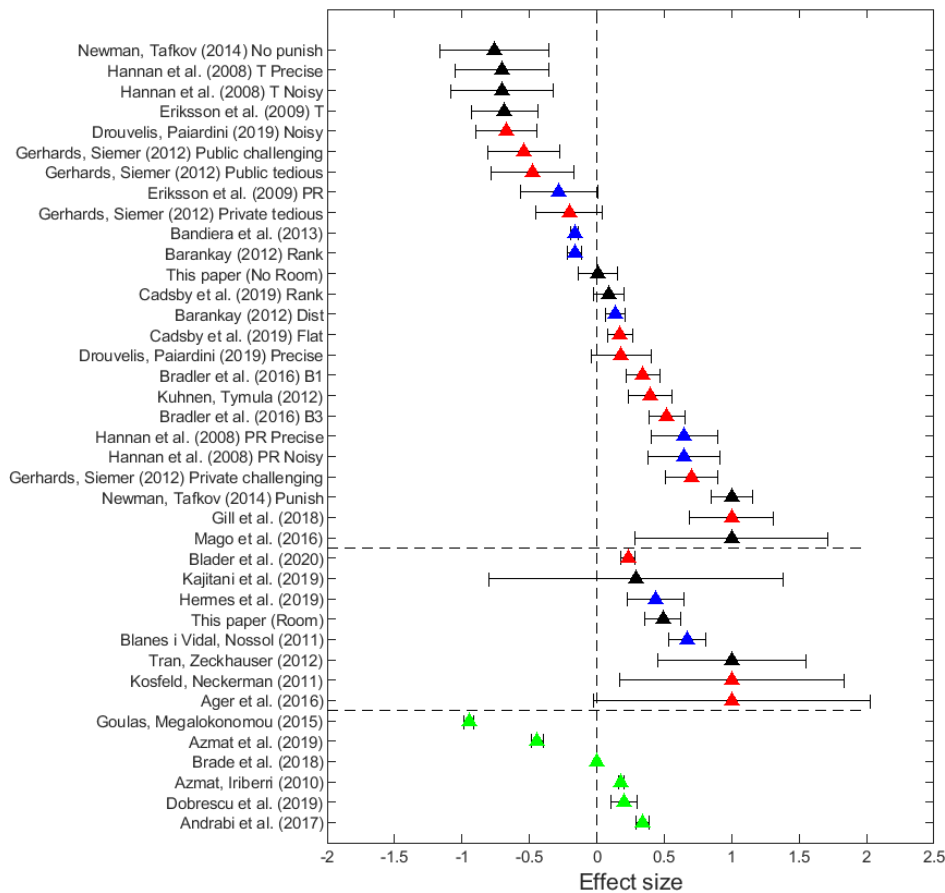


Figure 4.2 – Forest plot of the effect of RPF at the bottom of the performance distribution

Note: Each triangle represents the effect sizes at the bottom of the performance distribution for each of the treatments, and the bars the confidence intervals. The colors represent the incentive schemes: flat (red), grades (green), piece-rate (blue) and tournament (black). There is little room for improvement in the treatments above the first dashed line, and large room for improvement below. The treatments are ranked by effect size in each category (room vs. no room for improvement). The grade treatments appear separately below the second dashed line. For visibility, the effect sizes are truncated to lie within the $[-1, 1]$ range.

We now consider the average effect of RPF on performance.⁴ We observe, as in the previous figures, that the incentive scheme does not have a univocal performance effect. We also confirm that the RPF performance effect can be negative. The effect is more

⁴The overall distribution of performance is not simply the sum of the top and bottom effects in the previous figures, as we now include performance close to the median.

often positive when there is more as opposed to less room for improvement, although this gap is less marked for the average than for the top or bottom of the distribution.

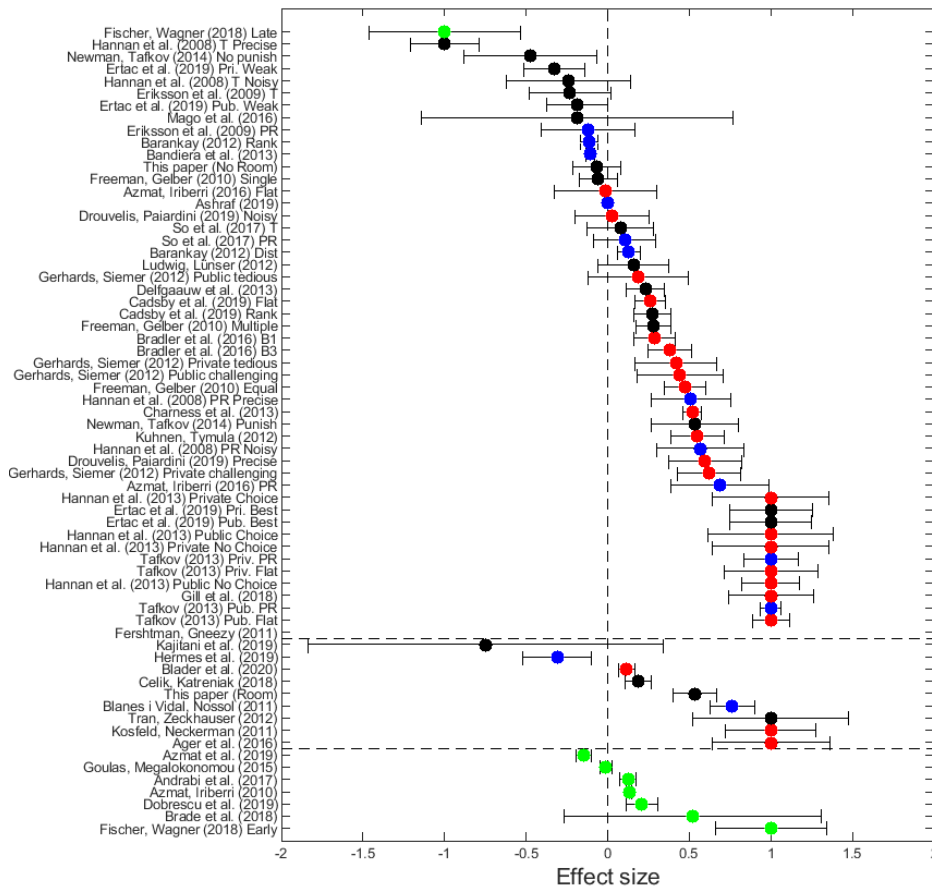


Figure 4.3 – Forest plot of the average RPF effect

Note: Each circle represents the average RPF effect size, and the bars the confidence intervals. The colors represent the incentive schemes: flat (red), grades (green), piece-rate (blue) and tournament (black). There is little room for improvement in the treatments above the first dashed line, and large room for improvement below. The treatments are ranked by effect size in each category (room vs. no room for improvement). The grade treatments appear separately below the second dashed line. For visibility, the effect sizes are truncated to lie within the $[-1, 1]$ range.

Unfortunately, the existing evidence on the effect of RPF over a number of periods is too scarce to provide a clean picture. We contribute to the discussion about the cumulative effect of RPF, with or without room for improvement, in the Sections below.

3. Theory: The effect of RPF on performance.

This section provides a theoretical analysis of the effect of RPF on performance. After briefly reviewing the literature, we present a theoretical model of the effect of RPF on performance. In our setting, RPF improves the agent's information about their performance in the presence of aggregate uncertainty.

In subsection 3.2. we consider the effect of RPF when the only choice variable is effort. In subsection 3.3. we contrast this with the case when there is room for improvement through technological innovation. This is a novel contribution, and introduces the possibility of RPF allowing agents to identify better technologies. The two models can be understood as polar cases. The effort model, at one extreme, assumes that all agents have the same technology and only decide on the effort to exert; the technology model, at the other extreme, assumes that all agents exert the same effort but choose which technologies to use.

There are two major differences between the effort and technology approaches. The first regards the influence of performance from one period to the next. Under the effort channel, performance in one period does depend on the level of effort in previous periods.⁵ By way of contrast, technological change is persistent. The second difference relates to the cost: we assume that effort is costly but that technological change is costless.

3.1. Related theoretical literature

There is a small theoretical literature on the effect of RPF on performance in two-player tournament settings: [Ederer \(2010\)](#); [Aoyagi \(2010\)](#); [Goltsman and Mukherjee \(2011\)](#); [Gershkov and Perry \(2009\)](#). These contributions consider how RPF information affects effort incentives, and do not predict that this is unambiguously positive: if subjects increase their effort for some feedback values, they necessarily must reduce it for other values, in line with the law of total expectation. Feedback will discourage effort when the agent learns that this effort is unlikely to improve their chance of winning. For individual incentives, [Fuchs \(2007\)](#) analyzes a principal-agent setting where the principal privately observes output and may have an incentive to give dishonest feedback to reduce the agent's pay. When agents have the option to choose and discard technologies, the riskiness of their output changes. This connects our work to the literature on risk-taking in contests ([Hvide, 2002](#); [Anderson and Cabral, 2007](#); [Seel and Strack, 2013](#)), who point out that agents who lag behind in a contest tend to benefit from increased risk-taking.

The idea that agents learn over time about the quality of the technology they use, and may learn from the performance of others appears in the strategic-experimentation literature. The closest paper to ours is [Halac et al. \(2017\)](#), who consider the optimal

⁵However, the outcome of previous periods may affect the nature of the incentives in subsequent periods, and thus moderating the optimal effort (see below).

feedback and prize structure in an innovation contest. As in our model, feedback allows agents to learn about the quality of the technology they use, and their chance of winning the contest. However, there is only one technology which all agents use and about which they become increasingly pessimistic. The agent only has the choice of how much and for how long to invest in the technology, but cannot switch to a different one. The possibility of RPF allowing agents to improve the technology they use has rarely been analyzed. One exception is [Wirtz \(2016\)](#), who focuses on the strategic interaction of two agents. The technology model in section 3.3. applies this idea to a linear incentives and a stylized tournament with many participants.

For simplicity, we consider a stylized model with a continuum of agents. This has two effects: First, rank feedback allows the agents to infer their individual performance precisely. Second, tournament incentives are reduced to a non-strategic setting, where an individual agent knows precisely the performance necessary to obtain a reward. While it significantly simplifies the analysis, the results would not change qualitatively if this assumption was relaxed. Moreover, our analysis is more applicable to real world settings in education where there are typically many competitors than a two-person setting which is often studied in the theoretical literature.

3.2. Effort choice

In this section we consider a model where agents choose effort to maximize their payoff. There is a continuum of identical agents.⁶ Each agent is assigned to one of two groups: the control or treatment (RPF) group. In each period $t = 1, \dots, T$, an agent produces output

$$x_t = e_t + b_t + \varepsilon_t.$$

where e_t denotes effort, which is provided at cost $c(e_t) = \frac{e_t^2}{2}$. b_t denotes a common shock, which affects all agents in a group in the same way, and is independently and identically distributed (*iid*) across periods, according to the cumulative density function (CDF) $F_b(\cdot)$ and the density $f_b(\cdot)$. ε_t denotes an individual error term, which is *iid* across agents and periods, with CDF $F_\varepsilon(\cdot)$ and density $f_\varepsilon(\cdot)$. Both densities are unimodal at 0 and twice continuously-differentiable. We denote the sequence of common shocks and individual errors up to period t as $b^t \equiv b_1, \dots, b_t$ and $\varepsilon^t \equiv \varepsilon_1, \dots, \varepsilon_t$.

The agent's payment $g(x)$ depends on her aggregate output $x \equiv \sum_{t=1}^T x_t$. We assume

⁶In our model, we abstract away from differences in ability between agents. [Ederer \(2010\)](#) finds that, when ability is unknown and affects the productivity of effort, relative performance feedback increases effort for the leader and decreases effort for the follower. This aligns with empirical (non experimental) evidence for a positive effect of rank information at the top of the distribution and a negative or no effect at the bottom ([Goulas and Megalokonomou, 2021](#); [Elsner et al., 2021](#); [Murphy and Weinhardt, 2020](#)). In contrast, if ability is known or does not affect the efficacy of effort there should be no effect of relative performance feedback on effort provision.

$g'(\cdot) \geq 0$ and $\mathbb{E}[g''(\cdot)] < 1$.⁷ In each period, the agent chooses effort to maximize her expected payoff, which is given by the expected payment minus the cost of effort:

$$U = \mathbb{E} \left[g(x) - \sum_{t=1}^T c(e_t) \right]$$

After each period, the agent receives feedback about her performance. Both the RPF and the control group learn their individual performance x_t after each period. At the beginning of period t the agent thus knows her performance in all previous periods, summarized as $\mathbf{x}^{t-1} \equiv x_1, \dots, x_{t-1}$. If the agent is in the RPF group, she additionally learns her rank r_t for each period, where the highest rank is 1 and the lowest rank 0. We assume that there is no information spillover between groups. The information the agent derives from all past rankings at the beginning of period t is denoted by $\mathbf{r}^{t-1} \equiv r_1, \dots, r_{t-1}$.

Average effects

Lemma 4.1. *The agent can infer the values of b_t and ε_t from her rank r_t .*

Proof of Lemma 4.1. In period 1, all agents exert the same effort, since they have no prior information. In addition, the common shock b_1 affects all agents in the same way. Differences in performance are then entirely due to the respective draws of the individual error term ε_1 . The agent's rank is given by: $r_1 = F_\varepsilon(\varepsilon_1)$. The agent can thus derive $\varepsilon_1 = F_\varepsilon^{-1}(r_1)$ and $b_1 = x_1 - e_1 - \varepsilon_1$. For subsequent periods, let output net of the aggregate shock in period t be given by: $y_t \equiv x_t - b_t = e_t + \varepsilon_t$. All agents exert optimum effort, given the information they have received. The agent can therefore predict the equilibrium distribution of y_t for all agents across the continuum: F_{y_t} . Rank is given by: $r_t = F_{y_t}(e_t + \varepsilon_t)$. We have $\varepsilon_t = F_{y_t}^{-1}(r_t) - e_t$ and $b_t = x_t - e_t - \varepsilon_t$. \square

Proposition 4.1. *When the choice variable is effort, there is no effect of RPF on expected average performance.*

Proof of Proposition 4.1. We define total output net of effort in period t as $x_{-t} \equiv x - e_t$. The optimal effort in period t then satisfies:

$$e_t^* = \mathbb{E} [g'(x_{-t} + e_t^*)].$$

A control-group agent knows her previous performances \mathbf{x}^{t-1} . Her optimal effort in period t is thus:

$$e_t^C = \mathbb{E} \left[g'(x_{-t} + e_t^C) \mid \mathbf{x}^{t-1} \right].$$

⁷The first assumption excludes that payment falls in output; the second ensures that the first-order condition identifies the maximum.

A RPF-group agent additionally knows her previous rankings \mathbf{r}^{t-1} . Her optimal effort in period t is then:

$$e_t^R = \mathbb{E} \left[g'(x_{-t} + e_t^R) \middle| \mathbf{x}^{t-1}, \mathbf{r}^{t-1} \right].$$

According to the law of total expectation, we have

$$\begin{aligned} \mathbb{E} [e_t^C] &= \mathbb{E} \left[\mathbb{E} \left[g'(x_{-t} + e_t) \middle| \mathbf{x}^{t-1} \right] \right] = \mathbb{E} [g'(x_{-t} + e_t)] \\ &= \mathbb{E} \left[\mathbb{E} \left[g'(x_{-t} + e_t) \middle| \mathbf{x}^{t-1}, \mathbf{r}^{t-1} \right] \right] = \mathbb{E} [e_t^R]. \end{aligned}$$

Average effort in the RPF and treatment groups is then identical in expectation.⁸ \square

Preferences for rank. While theory predicts no effect of RPF on expected average performance if agents care only about their payment, a number of contributions find evidence of this effect. Some papers have addressed this by introducing an *ad hoc* term into the payoff function, to reflect a *taste for rank*. The idea is that, when agents receive RPF, they start caring about rank per se in addition to the payment.⁹ The expected payoff function takes the following form:

$$\mathbb{E} \left[g(x) + \mathbb{1}_{RPF} \sum_1^T h(r_t) - \sum_1^T c(e_t), \right] \quad (4.1)$$

where the taste for rank in period t is denoted by $h(r_t)$ and only enters the payoff function once RPF is received. We assume that the payoff weakly increases in rank ($h'(\cdot) \geq 0$),¹⁰ and that rank weakly increases in effort ($r'_t(e_t) \geq 0$).

Lemma 4.2. *With a taste for rank there is a positive effect of RPF on average performance in expectation.*

Proof of Lemma 4.2. With a taste for rank, the optimal effort \hat{e}_t^R of an RPF-group agent in period t is:

$$\hat{e}_t^R = \mathbb{E} \left[g'(x_{-t} + \hat{e}_t^R) + h'(r_t) r'_t(\hat{e}_t^R) \middle| \mathbf{x}^{t-1}, \mathbf{r}^{t-1} \right].$$

As the additional term is positive, the optimal effort of an RPF-group agent rises with the taste for rank:

$$\begin{aligned} \hat{e}_t^R &= \mathbb{E} \left[g'(x_{-t} + \hat{e}_t^R) + h'(r_t) r'_t(\hat{e}_t^R) \middle| \mathbf{x}^{t-1}, \mathbf{r}^{t-1} \right] \\ &\geq \mathbb{E} \left[g'(x_{-t} + e_t^R) \middle| \mathbf{x}^{t-1}, \mathbf{r}^{t-1} \right] = e_t^R. \end{aligned}$$

⁸This holds as long as $c'''(\cdot) = 0$. For $c'''(\cdot) > 0$ (< 0), the average effort in the RPF group is lower (higher) in expectation. This is analogous to Proposition 1 in Ederer (2010).

⁹In an alternative specification, the agent also cares about rank when she does not receive RPF, but the payoff increases in the precision of the information. The results are qualitatively identical.

¹⁰Recall that $r_t \in [0, 1]$, where 1 is the highest rank.

Meanwhile, the optimal effort of a control-group agent \hat{e}_t^C is unchanged by a taste for rank, as she does not receive information about her rank: $\hat{e}_t^C = e_t^C$. We thus have

$$\mathbb{E} [\hat{e}_t^C | \mathbf{x}^{t-1}] = \mathbb{E} [e_t^C | \mathbf{x}^{t-1}] = e_t^R \leq \hat{e}_t^R,$$

which proves the result. Taste for rank increases the marginal return of effort for the RPF group relative to the control group. \square

Distribution effects

While RPF is predicted to have no effect on average performance without the taste for rank, we are interested in the effect of RPF along the performance distribution. The prediction here will depend on the shape of the payment function $g(\cdot)$. We consider examples of individual and tournament incentives, as these are the most common incentive schemes in the empirical literature. We first consider individual incentives in the form of a linear payment scheme, where $g(x) \equiv ax$, with $a > 0$.

Proposition 4.2. *With a linear payment scheme, RPF has no effect on expected future performance anywhere in the distribution.*

Proof. In this case, the optimal effort is $e_t = a$, independent of feedback and everywhere on the performance distribution. \square

Second, we consider a stylized model of tournament incentives with $T = 2$ periods. We denote the aggregate output net of the aggregate shock by: $y \equiv \sum_{t=1}^T y_t \equiv \sum_{t=1}^T x_t - b_t$. The agent receives a prize of 1 if this value exceeds a threshold of s , with the payoff of losing being normalized to zero. With a continuum of agents, this is equivalent to some share \hat{s} of agents with the highest total output winning the prize.¹¹ We assume that the threshold s is restrictive, i.e. it is high enough that the share \hat{s} of agents that attain it is under $\frac{1}{2}$. The expected payoff function then takes the following form:

$$U = \mathbb{E} [\mathcal{P}(y > s) - c(e_1) - c(e_2)] = \mathbb{E} \left[1 - F_\varepsilon(s - e_1 - e_2 - \varepsilon_1) - \frac{1}{2} (e_1^2 + e_2^2) \right],$$

where $\mathcal{P}(y > s)$ denotes the probability that the agent's output reach the threshold. Optimal effort in period t is

$$e_t^* = \mathbb{E} [f_\varepsilon(s - \varepsilon_1 - e_{-t} - e_t^*)],$$

¹¹As the aggregate shock b_t affects all agents, it is irrelevant for the outcome of the tournament. All agents exert optimum effort, given the information they have received. The agent can therefore predict the equilibrium distribution of output net of the aggregate shock y_t for all agents across the continuum: F_{y_t} . If the share \hat{s} of agents with the highest total output wins the prize, the agent can derive the threshold s she has to attain to win from: $1 - F_y(s) = \hat{s}$.

where e_{-t} denotes the effort level in the other period. As shown in Lemma 4.1, a RPF group agent can derive ε_1 after receiving feedback on r_1 . Her optimal effort in period 2 is thus:

$$e_2^R(\varepsilon_1) = f_\varepsilon(s - e_1 - e_2^R(\varepsilon_1) - \varepsilon_1).$$

Meanwhile, her optimal effort in period 1 is:

$$e_1^R = \mathbb{E} [f_\varepsilon (s - e_1^R - e_2^R(\varepsilon_1) - \varepsilon_1)].$$

We therefore have $e_1^R = \mathbb{E} [e_2^R]$: A RPF-group agent's first-period effort is equal to her expected second-period effort. First-period effort is the same for all RPF-group agents, as everyone has the same information at the start of period one.

A control-group agent only learns her output x_1 after period 1. Her optimal effort in period 2 is thus:

$$e_2^C = \mathbb{E} [f_\varepsilon (s - e_1 - e_2^C - \varepsilon_1) | x_1].$$

Meanwhile, her optimal effort in period 1 is:

$$e_1^C = \mathbb{E} [f_\varepsilon (s - e_1^C - e_2^C(\varepsilon_1) - \varepsilon_1)].$$

We thus have $e_1^C = \mathbb{E} [e_2^C]$: A control-group agent's first-period effort is equal to her expected second-period effort, and first-period effort is identical for all control-group agents. In addition, first-period effort is equal for the control and RPF groups and second-period effort is equal in expectation, as we have:

$$\begin{aligned} e_1^C &= \mathbb{E} \left[\mathbb{E} [f_\varepsilon (s - e_1 - e_2 - \varepsilon_1) | x_1] \right] = \mathbb{E} [f_\varepsilon (s - e_1 - e_2 - \varepsilon_1)] \\ &= \mathbb{E} \left[\mathbb{E} [f_\varepsilon (s - e_1 - e_2 - \varepsilon_1) | x_1, r_1] \right] = e_1^R. \end{aligned}$$

This confirms that expected average effort is equal for both groups with tournament incentives, as stated generally in Proposition 4.1.

Proposition 4.3. *For tournament incentives, RPF has a positive effect on expected future performance at the top of the distribution and a negative effect at the bottom.*

Proof. As all agents exert the same level of effort in period 1, the first-period error term ε_1 drives the differences in first-period performance and, consequently, in second-period effort. We first consider second-period effort in the RPF group. As the mode of f_ε is at 0, maximal second-period effort is exerted when ε_1 satisfies $s - e_1 - e_2 - \varepsilon_1 = 0$. As ε_2 is distributed symmetrically around 0, this agent has a winning probability of $\frac{1}{2}$. This would be the case for the agent whose first-period error term is $1 - F_\varepsilon(\varepsilon_1) = \hat{s}$. Given that we assume that the tournament is restrictive, i.e. $\hat{s} < \frac{1}{2}$, the highest effort is exerted by an agent on the top half of the distribution. Second-period effort is monotonically decreasing

both up and down the distribution starting from \hat{s} . In the control group, maximal effort is necessarily lower, as $E \left[f_\varepsilon (s - e_1 - e_2 - \varepsilon_1) \middle| x_1 \right] < f_\varepsilon (0)$ for all x_1 . Since expected effort is equal for both groups, it is necessarily the case that, for ε_1 low enough, expected effort is higher for the control group. Thus, the expected treatment effect is positive at the top and negative at the bottom of the distribution. \square

3.3. Technology choice

This section introduces a model in which RPF affects performance through the improved identification of good *technologies*. Instead of choosing effort, agents have to make decisions about the technologies they use. Technologies are ways of working that could either improve or worsen performance. In the context of education, examples could be taking notes on a computer, studying with classmates, drinking coffee, or studying late the night before the exam. The impact of these technologies could be positive or negative, and is ex ante uncertain. As in Section 3.2., there is a continuum of identical agents who carry out the same task over T periods. Agents draw technologies θ from a normal distribution with mean zero and variance 1, with CDF $F_\theta(\cdot)$ and density $f_\theta(\cdot)$. The technology drawn in period t is denoted by θ_t . Technologies are *iid* across agents and periods. Output in period t is affected by a common shock B_t , which affects all agents in the same way. This takes the form of a random walk with increments b_t , which are *iid* according to a normal distribution with mean zero and variance σ^2 , with CDF $F_b(\cdot)$ and density $f_b(\cdot)$.¹² The agent makes two decisions in each period: $d_t \in \{0, 1\}$ and $k_t \in \{0, 1\}$. At the start of period $t \in [1, T]$, she decides whether to draw a new technology θ_t ($d_t = 1$) or not ($d_t = 0$). At the end of the period $t \in [1, (T - 1)]$, she decides whether to keep the technology she just tried out for future periods ($k_t = 1$) or to drop it ($k_t = 0$).¹³ We assume that this decision is final, and that the agent keeps the technology when she is indifferent. Output in period t is determined by the technologies currently in use:

$$x_t = \sum_{r=1}^{t-1} (\theta_r \cdot \mathbb{1}_{(k_r=1)}) + \theta_t \cdot \mathbb{1}_{(d_t=1)} + B_t.$$

Aggregate output is given by the sum of per-period outputs:

$$x = \sum_{t=1}^T x_t = \sum_{t=1}^T \theta_t \left[\mathbb{1}_{(d_t=1)} + \mathbb{1}_{(k_t=1)}(T - t) + B_t \right]$$

¹²Modelling the common shock as a random walk is convenient, as the agent only updates her beliefs about the value of the technology once, after the realization of x_t . In the subsequent periods this belief will remain constant as long as the agent keeps the technology.

¹³For coherence, it must be the case that if $(d_t = 0)$ then $(k_t = 0)$. If the agent did not draw a new technology in period t , it cannot be used in future periods.

In each period, the agent chooses k_t and d_t to maximize expected payoff, given by $U = \mathbb{E}[g(x)]$. As in Section 3.2., agents in the control group only learn their own output x_t at the end of period t , while RPF-group agents additionally learn their rank r_t .

Lemma 4.3. *The agent can infer the values of B_t and θ_t from her rank r_t .*

Proof of Lemma 4.3. In period 1, the common shock B_1 affects all agents in the same way. Differences in performance are therefore entirely due to the individual technology draws θ_1 . The agent's rank is then given by: $r_1 = F_{x_1}(x_1) = F_\theta(\theta_1)$. The agent can thus derive $\theta_1 = F_\theta^{-1}(r_1)$ and $B_1 = x_1 - \theta_1$. In subsequent periods, let output net of the aggregate shock in period t be given by:

$$y_t \equiv x_t - B_t = \sum_1^{t-1} (\theta_r \cdot \mathbb{1}_{(k_r=1)}) + \theta_t \cdot \mathbb{1}_{(d_t=1)}.$$

All agents drop technologies below a certain threshold $\bar{\theta}_t$. The agent can therefore predict the equilibrium distribution of y_t for all agents across the continuum: F_{y_t} . Rank is given by:

$$r_t = F_{y_t} \left(\sum_1^{t-1} (\theta_r \cdot \mathbb{1}_{(k_r=1)}) + \theta_t \cdot \mathbb{1}_{(d_t=1)} \right).$$

The agent can thus derive

$$\theta_t = F_{x_t}^{-1}(r_t) - \sum_1^{t-1} (\theta_r \cdot \mathbb{1}_{(k_r=1)}) \quad \text{and} \quad B_t = x_t - \sum_1^{t-1} (\theta_r \cdot \mathbb{1}_{(k_r=1)}) - \theta_t \cdot \mathbb{1}_{(d_t=1)}.$$

□

Lemma 4.4. *For agents in the control group, the updated belief of the value of the technology after observing output $\theta_t|x_t$ is distributed normally with mean $\frac{x_t - x_{(t-1)}}{1 + \sigma^2}$ and variance $\frac{\sigma^2}{1 + \sigma^2}$, where we define $x_0 = 0$.*

Proof. We denote $\Delta x_t \equiv x_t - x_{(t-1)}$. According to Bayes Rule, we have:

$$\begin{aligned} f_{\theta|\Delta x_t}(\theta_t) &= \frac{f_{\Delta x|\theta_t}(\Delta x_t) \cdot f_\theta(\theta_t)}{f_{\Delta x}(\Delta x_t)} = \frac{f_b(\Delta x_t - \theta_t) \cdot f_\theta(\theta_t)}{f_{\Delta x}(\Delta x_t)} \\ &= \frac{\frac{e^{-\frac{(\Delta x - \theta_t)^2}{2\sigma^2}}}{\sqrt{2\pi\sigma^2}} \cdot \frac{e^{-\frac{\theta_t^2}{2}}}{\sqrt{2\pi}}}{\frac{e^{-\frac{(\Delta x)^2}{2(1+\sigma^2)}}}{\sqrt{2\pi(1+\sigma^2)}}} = \frac{e^{-\frac{(\theta - \frac{\Delta x_t}{1+\sigma^2})^2}{\frac{\sigma^2}{1+\sigma^2}}}}{\sqrt{2\pi \frac{\sigma^2}{1+\sigma^2}}}. \end{aligned}$$

□

The updated distribution of the technology θ_t , given output x_t , has the CDF $F_{\theta_t|x_t}(\cdot)$ and density $f_{\theta_t|x_t}(\cdot)$. We next analyze the agent's optimal technology decisions for the cases of individual and tournament incentives.

Individual incentives

We first consider individual incentives in the form of a linear payment scheme, where $g(x) \equiv ax$, with $a > 0$.

Lemma 4.5. *For agents in the RPF group, we have $k_t = 1$ iff $\theta_t \geq 0$ and $d_t = 1$ always.*

Proof. As shown in Lemma 4.3, agents who receive RPF can deduce the value of θ_t . Keeping a technology increases the expected payoff by $a\theta_t(T - t)$. The agent therefore keeps any technology with a positive value ($\theta_t \geq 0$) and discards any technology with a negative value. Taking a new draw in period t has no effect on the expected payoff in this period, as $\mathbb{E}(\theta_t) = 0$. However, since the agent only keeps technologies with a positive value, the effect on the expected aggregate payoff is positive:

$$a(T - t)\mathbb{E}(\theta_t | \theta_t > 0) = a(T - t)\sqrt{\frac{2}{\pi}} > 0.$$

The agent therefore always takes a new draw. □

Lemma 4.6. *For agents in the control group, we have $k_t = 1$ iff $x_t \geq x_{(t-1)}$ and $d_t = 1$ always.*

Proof. Agents in the control group use the updated belief about technology θ_t given output x_t to make decisions. Keeping a technology increases increase expected payoff by

$$a(T - t)\mathbb{E}(\theta_t | x_t) = a(T - t)\frac{x_t - x_{(t-1)}}{1 + \sigma^2}.$$

The agent therefore keeps any technology with a positive expected value, which is the case when the output increases over the previous period: $x_t \geq x_{(t-1)}$. Similarly to RPF-group agents, control-group agents always draw new technologies. Taking a new draw in period t has no effect on expected output in this period, as $\mathbb{E}(\theta_t) = 0$. However, since the agent only keeps technologies with a positive expected value, the effect on aggregate output, and thus the payoff, is positive:

$$a(T - t)\mathbb{E}(\theta_t | x_t > x_{(t-1)}) = \sqrt{\frac{2\sigma^2}{\pi(1 + \sigma^2)}} > 0.$$

The agent therefore always takes a new draw. □

Proposition 4.4. *When the agent can explore new technologies and faces linear incentives, there is a positive effect of RPF on expected performance. The treatment effect is positive everywhere along the distribution of prior performance, and is symmetric around the mean.*

Proof. Since both the RPF and the Control group draw a new technology in every period, this decision should not affect the treatment effect. Due to the imprecise information about the value of θ_t , some control-group agents will, however, take sub-optimal decisions about keeping or dropping their technology when $b_t \neq 0$. Consider first the case $\theta_t > 0$. For values of the common shock such that $(\theta_t + b_t < 0)$, the agent's output declines ($x_t < x_{(t-1)}$) even though the technology has a positive value. Therefore, control-group agents will *not keep* the technology ($k_t = 0$) when the the common shock is low enough. The expected output is reduced by θ_t relative to RPF-group agents in all $(T - t)$ future rounds. The probability of treatment is $\mathcal{P}(\theta_t + b_t < 0)$. The expected average treatment effect over all future periods, given a technology $\theta_t > 0$, is therefore therefore positive and given by:

$$\mathbb{E}(TE|\theta_t > 0) = (T - t)\theta_t \cdot \mathcal{P}(\theta_t + b_t < 0) = (T - t)\theta_t \cdot F_b(-\theta_t) > 0.$$

Second, consider the case $\theta_t < 0$. For values of the common shock such that $(\theta_t + b_t > 0)$, the agent's output increases ($x_t > x_{(t-1)}$) even though the technology has a negative value. Therefore, control-group agents will *keep* the technology ($k_t = 1$) when the the common shock is high enough. The expected output is reduced by $-\theta_t$ relative to RPF-group agents in all $(T - t)$ future rounds. The probability of treatment is $\mathcal{P}(\theta_t + b_t > 0)$. The expected average treatment effect over all future periods, given a technology $\theta_t > 0$, is therefore therefore positive and given by:

$$\begin{aligned} \mathbb{E}(TE|\theta_t < 0) &= (T - t)(-\theta_t) \cdot \mathcal{P}(\theta_t + b_t > 0) = (T - t)(-\theta_t) [1 - F_b(-\theta_t)] \\ &= (T - t)|\theta_t|F_b(\theta_t) > 0. \end{aligned}$$

In both cases, the average performance of control-group agents falls relative to the treatment group. The expected treatment effect is equal for an agent with technology above the median ($\theta_t > 0$) and the corresponding agent with a technology which is equidistant below the median $\theta'_t = -\theta_t < 0$. The expected treatment effect must therefore be equal at the top and the bottom of the distribution of prior performance. In summary, the treated segment is $\theta_t \in (\min\{-b_t, 0\}, \max\{-b_t, 0\})$ and, if treated, the treatment effect for a given technology θ_t is its absolute value. The only case when all agents agent make the optimal decision k is when $b_t = 0$. The expected treatment effect over the whole distribution of θ_t is positive and given by:

$$\mathbb{E}(TE) = \sum_{t=1}^T (T - t) \int_{-\infty}^{\infty} |\theta_t| F_b(\theta_t) d\theta = \frac{\sigma^2}{8} T(T - 1) > 0.$$

Over time, the possibility of trying new technologies leads to an increase in expected performance for both groups. However, the increase is greater for the RPF group since

they are better able to distinguish productive from unproductive technologies than the control group. Consequently, they only keep productive technologies, whose positive effect persists for all future periods. \square

Tournament incentives

Second, we consider the stylized tournament incentives introduced in Section 3.2.. For simplicity, we analyze the case of $T = 2$ periods. Aggregate output net of the aggregate shock is given by:

$$y = \sum_{t=1}^2 x_t - B_t = \theta_1(1 + \mathbb{1}_{(k=1)}) + \mathbb{1}_{(d=1)}\theta_2.$$

The agent receives a prize of 1 if this value exceeds a threshold of $s > 0$, with the payoff of losing being normalized to zero. The expected payoff function takes the following form:

$$U = \mathcal{P}[y \geq s] = 1 - \Phi\left(\frac{s - \mathbb{E}[y]}{\sqrt{\text{Var}[y]}}\right).$$

Lemma 4.7. *For agents in the RPF group, we have $k = 1$ iff $\theta_1 \geq 0$ and $d = 1$ iff $\theta_1 < \frac{s}{2}$.*

Proof. As shown in Lemma 4.3, agents who receive RPF after period 1 can infer the value of θ_1 . If the technology has a positive value ($\theta_1 \geq 0$), keeping it clearly increases the probability that total net output y will exceed the threshold s . Therefore, the agent keeps any technology with a positive value and discards technologies with negative value.

Next, we consider the decision whether to take a new draw in period 2. When $2\theta_1 \geq s$, the probability of reaching the threshold is 1 if the agent does not draw a new technology. However, the probability drops to $1 - F_\theta(s - 2\theta_1)$ if the agent takes another draw. Therefore, the agent never takes a new draw when $\theta_1 \geq \frac{s}{2}$. When $2\theta_1 < s$, the probability of reaching the threshold is 0 if the agent does not draw a new technology. However, the probability rises to $F_\theta(s - \theta_1(1 + \mathbb{1}_{(k=1)}))$ if the agent takes another draw. Therefore, the agent takes a new draw in period t when $\theta_1 < \frac{s}{2}$. \square

We define $\underline{x} \equiv -\frac{s}{3} \left(\sigma^2 + \sqrt{(1 + 2\sigma^2)(1 + 5\sigma^2)} - 1 \right)$ and $\bar{x} \equiv \frac{s}{2}(1 + \sigma^2)$.

Lemma 4.8. *For agents in the control group, the optimal technology choices are*

$$\begin{cases} k = 1, d = 0 & \text{for } x_t \geq \bar{x} \\ k = 1, d = 1 & \text{for } \bar{x} > x_t \geq \underline{x} \\ k = 0, d = 1 & \text{for } x_t < \underline{x}. \end{cases}$$

Proof. The probabilities that the agent attain the threshold given her first-period output

x_1 for all possible combinations of her decisions k and d are given by:

$$\begin{aligned}\mathcal{P}(y > s | x_1, k = 1, d = 0) &= 1 - \Phi\left(\frac{s - \frac{2x_1}{1+\sigma^2}}{\sqrt{4\frac{\sigma^2}{1+\sigma^2}}}\right) \\ \mathcal{P}(y > s | x_1, k = 1, d = 1) &= 1 - \Phi\left(\frac{s - \frac{2x_1}{1+\sigma^2}}{\sqrt{4\frac{\sigma^2}{1+\sigma^2} + 1}}\right) \\ \mathcal{P}(y > s | x_1, k = 0, d = 1) &= 1 - \Phi\left(\frac{s - \frac{x_1}{1+\sigma^2}}{\sqrt{\frac{\sigma^2}{1+\sigma^2} + 1}}\right) \\ \mathcal{P}(y > s | x_1, k = 0, d = 0) &= 1 - \Phi\left(\frac{s - \frac{x_1}{1+\sigma^2}}{\sqrt{\frac{\sigma^2}{1+\sigma^2}}}\right)\end{aligned}$$

The maximum probability of reaching the threshold depending on first-period output x_1 is:

$$\begin{cases} \mathcal{P}(y > s | x_1, k = 0, d = 1) & \text{for } x_1 < \underline{x} \\ \mathcal{P}(y > s | x_1, k = 1, d = 1) & \text{for } \underline{x} \leq x_1 < \bar{x} \\ \mathcal{P}(y > s | x_1, k = 1, d = 0) & \text{for } \bar{x} \leq x_1. \end{cases}$$

□

Note that $\underline{x} < 0$: The threshold for keeping a technology is below zero. This means that the agent will keep technologies with an expected value slightly below zero. The reason is that the agent has a bias for more-variable output in the tournament setting. She does not care about her expected output, but only about the probability of crossing a high threshold. This probability increases when the output has a higher variance, which is achieved by keeping the technology. The agent is willing to trade off a slight decrease in expected output for a higher variance.

Proposition 4.5. *When the agent can explore new technologies and faces tournament incentives, there is a positive effect of RPF on expected performance. The treatment effect is positive everywhere along the distribution of prior performance and larger at the bottom.*

Proof. Due to imprecise information about the value of θ_1 , some control-group agents will take sub-optimal decisions about keeping or dropping their first-period technology when $b_t \neq x_1$. First, consider the case $\theta_1 > 0$. These agents performed above the median in period 1. For values of the common shock such that $(\theta_1 + b_1 < \underline{x})$, the agent's output is below the threshold ($x_1 < \underline{x}$), even though, unbeknownst to the agent, the technology θ_1 has a positive value. Therefore, control-group agents will *not keep* the technology ($k = 0$) when the common shock is low enough. The expected output in all future rounds is reduced by the value of θ_1 relative to RPF-group agents. The probability of treatment is

$\mathcal{P}(\theta_1 + b_1 < \underline{x})$. Thus, the expected treatment effect for a technology $\theta_1 > 0$ is:

$$\mathbb{E}(TE^+) = \theta_1 \cdot \mathcal{P}(\theta_1 + b_1 < \underline{x}) = \theta_1 \cdot F_b(\underline{x} - \theta_1) > 0.$$

Second, consider the case $\theta_1 < 0$. These agents performed below the median in period 1. For values of the common shock such that $(\theta_1 + b_1 \geq \underline{x})$, the agent's output is above the threshold ($x_1 \geq \underline{x}$), even though the technology θ_1 has a negative value. Therefore, control-group agents will *keep* the technology ($k = 1$) when the common shock is high enough. The expected output in all future rounds is reduced by the value of $-\theta_1$ relative to RPF-group agents. The probability of treatment is $\mathcal{P}(\theta_1 + b_1 \geq \underline{x})$. The expected treatment effect, for a technology $\theta_1 < 0$ is thus:

$$\mathbb{E}(TE^-) = (-\theta_1) \cdot \mathcal{P}(\theta_1 + b_1 \geq \underline{x}) = (-\theta_1) [1 - F_b(\underline{x} - \theta_1)] > 0.$$

The expected performance of control-group agents is reduced relative to the treatment group, both for agents who performed above the median in period 1 ($\theta_1 > 0$) and those who performed below ($\theta_1 < 0$). Moreover, provided an agent is treated, the size of the treatment effect is equal for agents with technology above the median ($\theta_1 > 0$) and agents with a technology that is equidistant below the median $\theta'_1 = -\theta_1$. However, the probability that the respective agents are treated is different. Since f_b is symmetric around zero and $\underline{x} \leq 0$, it is less likely that the agent above the median is treated.

$$\begin{aligned} \mathcal{P}(\theta'_1 + b_1 < \underline{x}) &= F_b(\underline{x} - \theta_1) = 1 - F_b(-\underline{x} + \theta_1) \\ &\leq 1 - F_b(\underline{x} - \theta'_1) = \mathcal{P}(\theta'_1 + b_1 \geq \underline{x}) \end{aligned}$$

It is more likely that a control-group agent keeps a technology with a negative value ($\theta_1 < 0$) than that she discards a technology with a positive value ($\theta_1 > 0$). Therefore, the expected treatment effect is larger at the bottom of the distribution.

In summary, the treated segment is $\theta_1 \in (\min\{\underline{x} - b_1, 0\}, \max\{\underline{x} - b_1, 0\})$ and, if treated, the treatment effect for a given technology θ_1 is its absolute value. The only case when all agents agent make the optimal decision k is when $b_1 = \underline{x}$. The expected treatment effect is positive at the top and the bottom of the distribution of first-period performance ($\theta_1 > 0$ and $\theta_1 < 0$). Thus, the expected treatment effect over the whole distribution is also positive, and is given by:

$$\mathbb{E}(TE) = \int_{-\infty}^0 (-\theta_1) [1 - F_b(\underline{x} - \theta_1)] d\theta + \int_0^{\infty} \theta_1 \cdot F_b(\underline{x} - \theta_1) d\theta > 0.$$

The decision whether to draw a new technology in period 2 does not affect expected total output in the two-period scenario, as the expected value of the new technology is zero. \square

3.4. Comparison

In summary, our model predicts that when agents choose effort there is no effect of relative performance feedback on expected average performance (Proposition 4.1), unless an *ad hoc* preference for rank is introduced (Lemma 4.2). If agents face linear individual incentives, no effect is expected anywhere on the distribution (Proposition 4.2). If agents face tournament-style incentives, a positive effect of RPF is expected at the top of the distribution, while that at the bottom of the distribution is expected to be negative (Proposition 4.3).

Meanwhile, when agents can explore new technologies, RPF helps to distinguish good and bad technologies and there is a positive treatment effect everywhere on the distribution (Propositions 4.4 and 4.5). When agents face tournament-style incentives, the effect is expected to be larger at the bottom of the distribution (Proposition 4.5). We can also conclude that the specific incentive scheme makes little difference for the RPF effect on average performance, in particular when there is little room for improvement.

Finally, the effort and technology channel differ in the influence of RPF on performance over time. Under the effort channel, exerting effort in one period does not carry over to the subsequent period. By contrast, technological change is persistent. For linear incentives, we show that there is a cumulative treatment effect. RPF group agents are better able to correctly identify a productive or unproductive technology, and the effect carries over to all future periods.

4. Empirics

We now describe two field experiments in which the tasks to be performed differ according to the possibility of improving performance by changing technology, i.e. in terms of room for improvement. In the first experiment the task consists in counting 1's in a matrix over a short time period. In line with the theoretical predictions for the effort channel, we find no particular effect of RPF, except perhaps at the top of the distribution. In the second experiment the task consists in maths tests at one-week intervals. In sharp contrast to the first experiment, this task leaves ample room for improving technology. We find that RPF speeds up learning, and there is performance improvement all along the distribution. As will be explained in more details, the two experiments provide similar incentives and mainly differ in the nature of the task. However, we note that subject pools differ (Students vs Turkers) between the two experiments. There is however no reason to believe that differences between treatments would be qualitatively affected.

4.1. Experiment 1: No room for improvement

Experimental design

The experiment was run on Amazon Mechanical Turk (AMT). The task consisted in counting the number of 1's in grids containing only 0's and 1's. Each grid had 6 rows and 6 columns as in Figure 4.4. The experiment consisted of four rounds. In each round, subjects had 180 seconds to count as many grids as they can. Remaining time is displayed on the screen.

All participants received a flat payment of \$2 and could receive an additional bonus payment of \$8, depending on their performance. Each correct answer is rewarded with 1 point, each incorrect answer costs 1/2 a point. The first round is an unincentivized trial. If the total points from Rounds 2 to 4 is above a certain threshold, the participant received the bonus payment. The threshold was set according to the score at the 5th percentile of a previous trial group.¹⁴

0	0	0	0	1	1
0	1	0	1	0	0
1	1	0	1	0	0
1	1	0	1	1	0
1	0	1	0	0	0
1	1	0	0	0	1

Figure 4.4 – Screenshot of a typical grid used

All subjects received feedback about their performance between rounds. In the control group, subjects were only told their individual score; in the treatment group, subjects were told both their individual score and their rank in comparison to the trial group. A total of 204 subjects participated in the experiment on AMT in March 2020, with 96 subjects in the treatment and 108 in the control group. On average subjects received \$2.3 for an experiment that lasted no more than 20 minutes (which is relatively high compared to standard earnings on AMT, [Hara et al. \(2018\)](#)).

Results

The distribution of scores by round is shown in Figure 4.5. In line with our expectations, RPF appears to have no significant effect on performance and the treatment effect does not vary over time.

The evolution of average performance in the treatment and control groups over rounds is shown in Figure 4.6. Note that Round 1 is a trial round that does not count towards

¹⁴The trial group consisted of 50 participants on AMT. Their performance was recorded prior to the experiment, with subjects receiving a piece rate. This procedure was followed as it is not feasible to stipulate simultaneous participation on AMT.

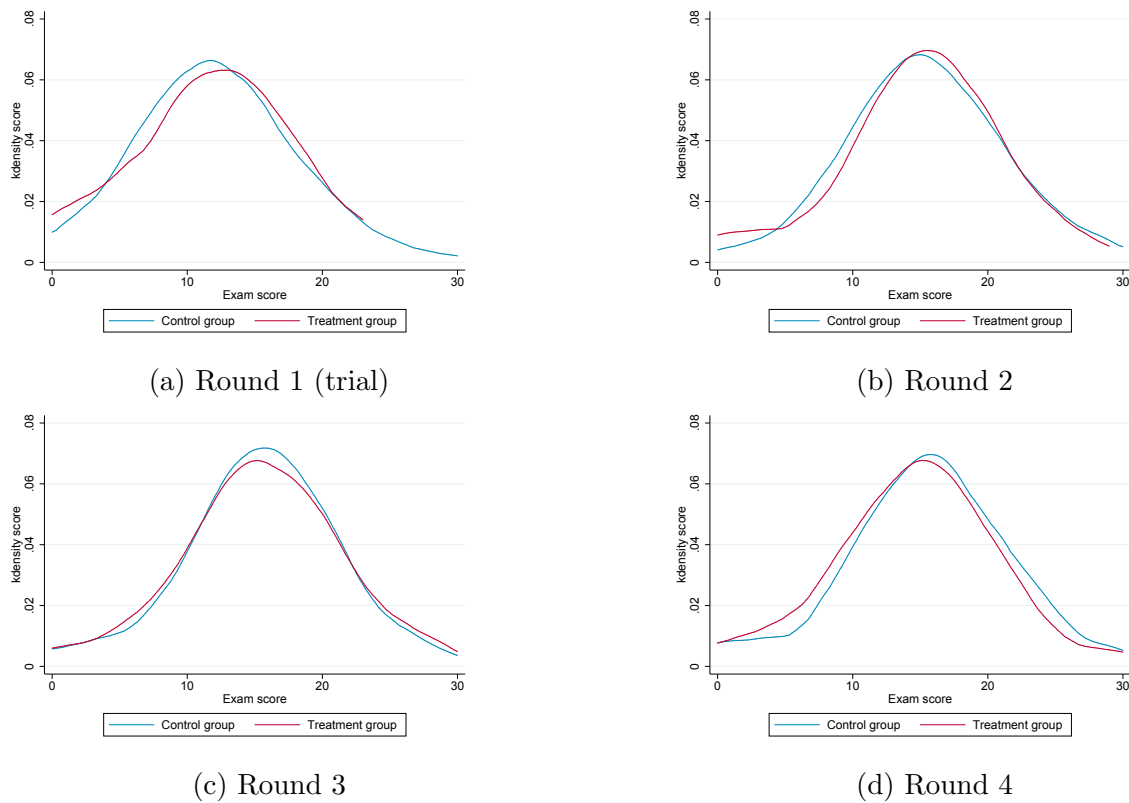


Figure 4.5 – The change in the densities of performance over time for the control (blue) and treatment (red) groups

the aggregate score. This figure illustrates that (1) RPF has no effect on performance, and (2) the effect does not vary over time. The absence of a significant difference between the RPF and control group is even more striking in column 1 of Table 4.1, in which we show the results from a difference-in-difference estimation (Equation 4.2).

$$Performance_{it} = \alpha_0 + \beta_1 Treatment_i + \sum_{t=2}^4 \gamma_t Period_t * Treatment_i + \delta_t + \eta X_i + \varepsilon_{it} \quad (4.2)$$

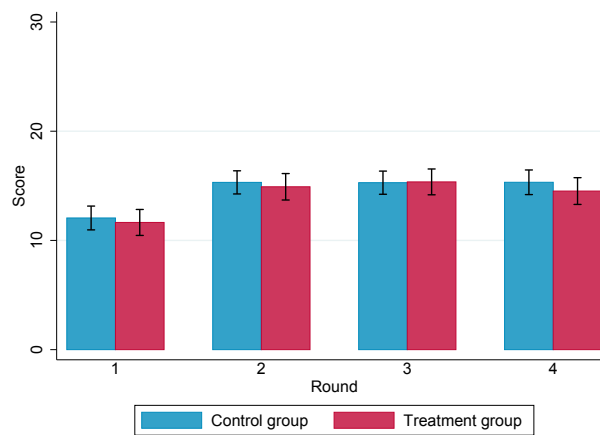


Figure 4.6 – Evolution of average performance across rounds

In line with the theoretical predictions, there might be, if anything, a slight effect of RPF at the very top of the distribution, with slightly more subjects reaching the threshold in the treatment group relative to the control group (4 out of 96 in the treatment group, 4.2%, vs. 3 out of 108 in the control group, 2.8%). The average performance of the top 5% in Round 4 is also slightly higher in the treatment group (27.9 points vs. 26.8 points), although this difference is not statistically significant. The results from quantile regressions in columns 2 and 3 of Table 4.1 show that RPF has no effect both at the first and last quartile.

Overall, the results from this first experiment confirm our theoretical predictions: when there is little room for improvement, RPF has a very small effect on performance, except perhaps at the very top of the distribution, and the effect does not vary by round.

4.2. Experiment 2: Large room for improvement

Experimental design

For the second experiment, subjects were eighth-grade students at two girls' schools in Iran, with an average age of 14.¹⁵ There were three classes per school and around 30 students per class. The students took a series of four weekly maths exams, which were framed as part of the curriculum. The experiment was run with tournament incentives: in each class, the two best performers received a significant prize of 1.000.000 Iranian Rials.¹⁶ The students were ranked according to the sum of their scores over the last three exams. The first exam was conceived as a trial-run and did not count for the final score.

In the control group, students only received feedback about their individual grade after each exam. In the treatment group, students additionally received feedback about their rank within the class. The total number of students in the control group was 91, with 83 in the treatment group. In order to prevent spillovers of information, all students from one school were allocated to the control group while all students from the second school were allocated to the treatment group. As there was a significant distance between the schools, there was little chance of the students realizing that a similar (but different) tournament was being organized in another school. To ensure comparability, we ran a pilot study to identify schools that were sufficiently similar.

Each exam consisted of 40 multiple-choice questions. The difficulty of the questions was evaluated using a pilot study, to ensure that the grades across the tests were comparable. Any improvement across rounds should therefore be attributed to better performance rather than changes in difficulty. For each question there were four answer options, with only one correct choice. Four points were awarded for a correct answer, while one point was subtracted for an incorrect answer. Unanswered questions were not penalized. There was a one-week gap between each exam, and students received feedback the day after each exam, to ensure that they had time to react.

Results

Figure 4.7 shows the histograms of student scores in the treatment and control groups. These show that (1) there is a strong positive effect of RPF on average exam scores and (2) students who receive RPF continually improve, as the treatment effect rises over time.

Moreover, as indicated in Figure 4.8, RPF appears to shift the entire distribution upwards.

We confirm these observations via a standard difference-in-differences regression (Equation 4.2: the coefficients of interest are the γ_t 's). The results in column 4 of Table

¹⁵The experiment was run as part of Mahmoud Farrokhi-Kashani's PhD thesis (Farrokhi-Kashani, 2012).

¹⁶At the time roughly equivalent to 50 euro, about one week's wages for a low-skilled worker.

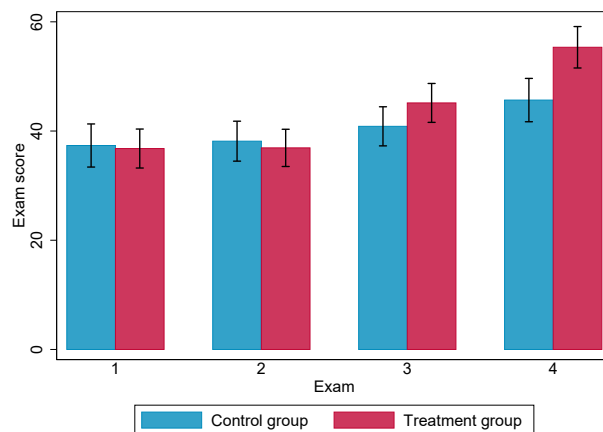
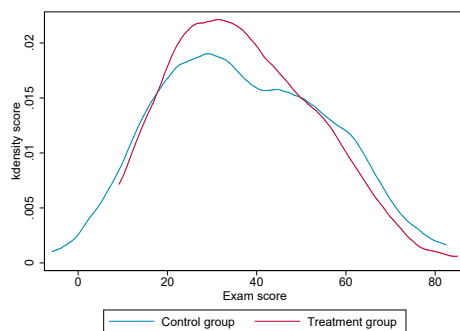
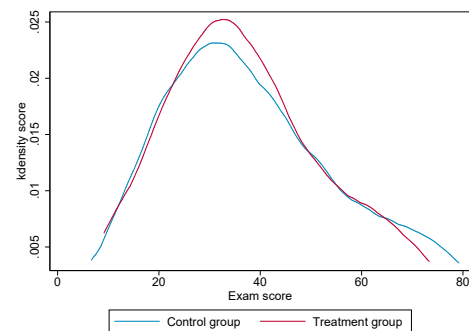


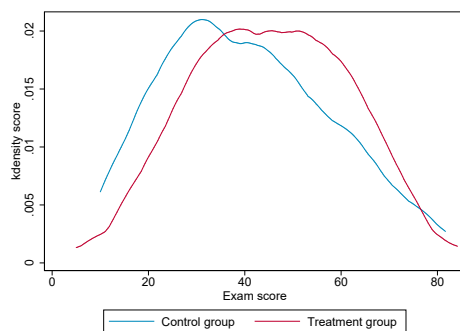
Figure 4.7 – The evolution of average exam scores over time for the control and treatment groups.



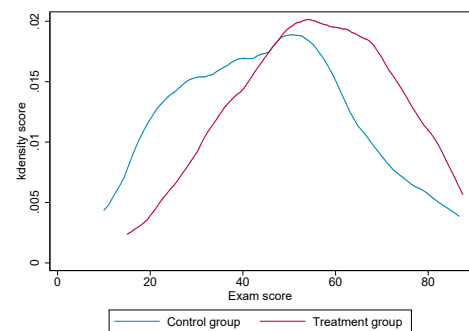
(a) Exam 1



(b) Exam 2



(c) Exam 3



(d) Exam 4

Figure 4.8 – The change in the densities of exam scores over time for the control (blue) and treatment (red) groups

4.1 confirm that the size of the RPF effect is large (in exam 4 approximately 10 points out of a maximum score of 160 points, corresponding to an increase of 0.56 standard deviations) and robust to the introduction of the available controls (parents' education and number of siblings).

The performance of low-performing students deserves particular attention. It is often assumed that RPF will harm weaker students. Across rounds, some students are less and less likely to have a chance to win any prize. However, we observe that the score at the bottom of the distribution does improve (see Figure 4.9). Figure 4.9 also suggests that RPF has a homogeneous effect across the performance distribution.

To test the homogeneity of the RPF performance effect, we run quantile regressions at the 25th, 50th and 75th percentile. The results in columns 5 and 6 of Table 4.1 indicate that the effect is statistically significant over the entire distribution. The effect is not statistically different between the first and the last quartile, although the coefficient is slightly larger in the former.

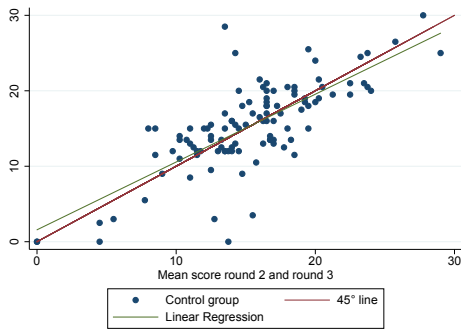
Overall, our empirical findings fit the predictions of the technology channel well: the treatment effect increases across periods. This feature is hard to explain through the effort channel alone. The fact that the treatment effect is found over the whole performance distribution matches the predictions of the technology model. Since there obviously is room for improvement in this experiment, we conjecture that students improved their performance by gradually improving their learning technology.

4.3. Comparison between the two experiments

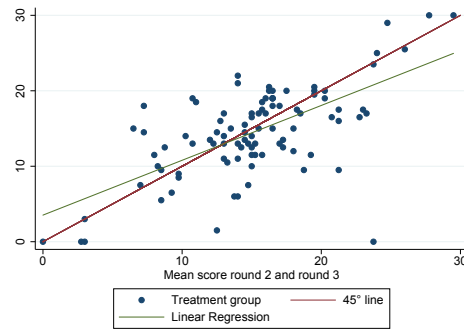
Table 4.1 shows the results from both experiments, which allows us to draw three conclusions.

Average effect Without room for improvement, there does not seem to be an average performance effect. When there is considerable room for improvement, however, there is a substantial and significant positive average effect of RPF on performance.

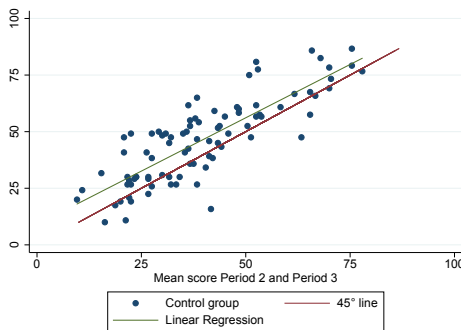
Distribution effect Comparing the effects at the bottom of the distribution (the first quartile in columns 2 and 5 in Table 4.1), we can see that there is, after a few rounds, a substantial difference between the results of the two experiments. There is no effect in the experiment without room for improvement, but a positive significant effect with room for improvement. At the top of the distribution (the top quartile in columns 3 and 6 of Table 4.1), we see a similar pattern of a positive effect with room for improvement and no effect without room for improvement.



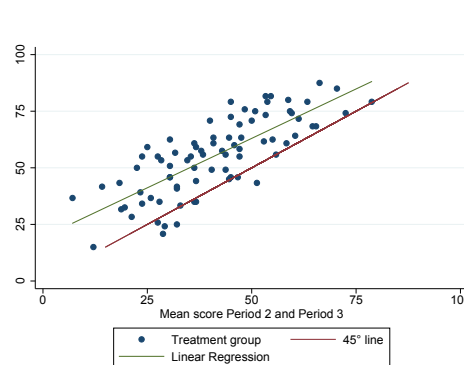
(a) Control group without room for improvement



(b) Treatment group without room for improvement



(c) Control group with room for improvement



(d) Treatment group with room for improvement

Figure 4.9 – Comparison of the score in Round 4 (Y-axis) to that in Rounds 2 and 3 (X-axis). Each dot represents a subject. Figures 4.9a and 4.9b show performance in the experiment without room for improvement, and Figures 4.9c and 4.9d performance in the experiment with room for improvement.

Cumulative effect The third and final empirical difference between the two experiments concerns the difference in the provision of RPF over several rounds. There does not seem to be a performance effect after providing the information just once. However, after a number of RPF a significant difference between the two experiments appears, with a clear positive effect all along the performance distribution in the experiment with large room for improvement, but none in the experiment without room for improvement.

Table 4.1 – Effect of RPF on performance without (columns 1 to 3) or with (columns 4 to 6) room for improvement

	No Room for Improvement			Large Room for Improvement		
	ATE (1)	Bottom (2)	Top (3)	ATE (4)	Bottom (5)	Top (6)
Round 2	3.259*** (.448)	2.827*** (.668)	2.839*** (.762)	1.385 (1.508)	5.256** (2.470)	-3.833 (2.479)
Round 3	3.231*** (.621)	3.837*** (.781)	2.455*** (.696)	4.014** (1.580)	6.282** (2.782)	2.167 (2.520)
Round 4	3.269*** (.510)	3.431*** (.689)	3.514*** (.749)	8.681*** (1.806)	9.936*** (2.660)	6.000** (2.525)
Treatment	-.459 (.813)	-.634 (1.467)	-.398 (.866)	-4.301 (2.800)	1.474 (3.303)	-7.833** (3.344)
Round 2 x Treatment	.006 (.665)	.931 (1.330)	.054 (1.070)	-1.256 (2.127)	-4.231 (3.350)	-.500 (3.153)
Round 3 x Treatment	.482 (.872)	.188 (1.823)	.729 (.965)	5.223** (2.397)	3.269 (3.438)	7.500** (3.495)
Round 4 x Treatment	-.394 (.835)	.050 (1.554)	-.568 (1.053)	10.013*** (2.517)	9.231** (3.657)	11.333*** (3.790)
Constant	14.515*** (1.775)	11.139*** (2.091)	18.018*** (1.591)	29.579*** (4.779)	12.051*** (3.923)	42.333*** (6.093)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	.064	.060	.059	.174	.158	.161
No. obs	816	816	816	641	641	641

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$. The dependent variable for columns 1 to 3 is performance in counting matrices, for columns 4 to 6 the dependent variable is exam scores. Standard errors (in parentheses) are clustered at the individual level. The “bottom” and “top” columns represent quantile regressions at the bottom and top quartiles. In the first three columns, the controls are education and age; in the last three columns, controls are parents’ education and number of siblings.

5. Conclusion

The main claim of this paper is that RPF provides information about peers' performance which helps to better - and faster - identify good technologies. In particular, low performers can benefit from RPF, an effect that has been consistently observed but poorly explained. A common explanation is that agents have an intrinsic taste for ranking (e.g. self-image concerns). In this paper we propose a less *ad hoc* and more satisfactory explanation based on technology improvement.

At a more general level, considering a technology channel suggests that for the assessment of the RPF effect it is crucial to know whether the task at hand offers a possibility of technological improvement, which we call room for improvement. Surprisingly enough, the nature of the incentives (tournament, individual, etc) appear to be of lesser importance. Indeed, even when rank has no influence on payoffs, as with individual incentives, agents may nonetheless use the feedback to improve their technology. Section 2. confirms that the specific incentive scheme does not make a great difference to the RPF effect, in particular when there is little room for improvement. RPF is provided in countless situations, and the focus on the technological aspect of RPF makes it possible to shed new light on two branches of the literature: education and social norms.

In education, teachers are often reluctant to provide rankings to young pupils, as RPF is thought to affect poor performers negatively, for instance by lowering their self-image. A better understanding of the role of RPF suggests, on the contrary, that providing *private* rankings is likely to trigger technological improvement, while avoiding public shaming. This is indeed what some work has concluded: [Hannan et al. \(2013\)](#) and [Gerhards and Siemer \(2016\)](#) find a positive effect of (private) ranking on performance, even for low performers. Differently from previous work, this paper does not rely on an *ad hoc* "taste for ranking" to account for this improvement.

Social norms are usually thought to influence behavior by specifying what is acceptable and what is not in a society or a group. Informing agents that a large fraction of their peers respect a particular norm is often taken as a way of increasing compliance. It is usually assumed that agents have a preference for compliance, or that they try to avoid the possible costs of deviating from the norm. Our work here suggests an alternative interpretation. Informing agents about compliance to a given social norm provides information about the behavior of others. This information is a form of feedback which may, in fact, trigger learning about new technologies. One good example is electricity consumption. Electricity consumers have been found to reduce their consumption when provided with information about their consumption relative to that of similar households in their neighborhoods ([Goldstein et al., 2008](#); [Schultz et al., 2007](#)). Agents who consume more than their neighbors may realize, thanks to RPF, that it is possible to use electricity in a more efficient way. Agents may thus comply with the norm (i.e. reduce their con-

sumption), not (only) because they wish to comply *per se* or to avoid costs imposed on deviators, but because they learn about better technologies.

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Appendices

A Description of the papers included in the empirical literature review

Table A.1 – Papers included in this review with large room for improvement (1)

Paper	Task	Nature of Feedback	Incentives	N	Average effect	Effect at bottom	Effect at top
Ager et al. (2016)	Pilot fighters	Praise in Armed Force Bulletin	Flat	5081	+	+	+
Andrabi et al. (2017)	Tests	Quintile rank + mean score	IG	12110	+	+	0
Azmat and Iriberrri (2010)	Tests	Class average	IG	3414	+	+	+
Azmat et al. (2019)	Tests	Decile rank every 6 months	IG	977	-	-	0
Blader et al. (2015)	Transport	Ranking	Flat	5000	+	0	+
Blanes i Vidal, Nossol (2011)	Wholesale retail	Ranking	PR	63	+	+	+
Brade et al. (2018)	Tests	Distance median 80th percentile	IG	1609	+	0	+
Celik Katremiak (2018)	Tests	Performance few classmates + ranking	T	7150	+		

This table lists papers in which the task is considered by the authors as having large room for improvement. Papers are ranked alphabetically. PR: Piece-rate; IG: Individual grades; T: Tournament

Table A.2 – Papers included in this review with large room for improvement (2)

Paper	Task	Nature of Feedback	Incentives	N	Average effect	Effect at bottom	Effect at top
Dobrescu et al. (2019)	Tests	Ranking	IG	1101	+	+	+
Fischer and Wagner (2018)	Tests	Early	IG	123	+		
Goulas and Megalokonomou (2015)	Tests	Ranking	IG	45746	-	0	+
Hermes et al. (2019)	Tests	Class ranking improvement	PR	378	+	+	0
Kajitani et al. (2020)	Tests	Ranking	T	255	0	+	-
Kosfeld and Neckermann (2011)	Data entry	Announce of best	Flat	184	+	0	+
This paper (Room)	Tests	Ranking	T	172	+	+	+
Tran and Zeckhauser (2012)	Tests	Ranking	T	124	+	-	+

This table lists papers in which the task is considered by the authors as having large room for improvement. Papers are ranked alphabetically. PR: Piece-rate; IG: Individual grades; T: Tournament

Table A.3 – Papers included in this review with no room for improvement (1)

Paper	Task	Nature of Feedback	Incentives	N	Average effect	Effect at bottom	Effect at top
Ashraf (2019)	Sweater production	Private ranking	PR	366	0+		
Azmat and Iriberry (2016)	Adding numbers	Average	Flat	156	0		
Bandiera et al. (2015)	Fruit	Team ranking	PR	656	-	-	0
Barankay (2011)	Image class. Amazon MT	Ranking	PR	147	-	-	0
Barankay (2012)	Furniture salesmen	Ranking + Distrib.	PR	1754	+	+	+
Bradler et al. (2016)	Data entry	Ranking	PR	1754	-	-	0
		Best 1	Flat	82	+	+	0
		Best 3		95	+	+	0
Cadsby et al. (2019)	Adding numbers	Ranking	Flat	560	+	+	+
			T	560	+	0	+
Charness et al. (2014)	Encryption	Distrib.	Flat	585	+		
Delfgaauw et al. (2013)	Sales	Team ranking	T	128	+		
Drouvelis and Paiardini (2019)	Encryption	Noisy	Flat	248	0	-	0
		Precise		248	+	0	+
Eriksson et al. (2009)	Adding numbers	Ranking	PR	88	0	-	0
			T	120	0	-	0

This table lists papers in which the task is considered by the authors as having little room for improvement. Papers are ranked alphabetically. PR: Piece-rate; IG: Individual grades; T: Tournament

Table A.4 – Papers included in this review with no room for improvement (2)

Paper	Task	Nature of Feedback	Incentives	N	Average effect	Effect at bottom	Effect at top
Ertac et al. (2019)	Effort task	Private	Best link	165	+		
		Public	Weak link	165	-		
Fershtman and Gneezy (2011)	60m races	Continuous feedback	Best link	165	+		
			Weak link	165	0		
Fershtman and Gneezy (2011)	60m races	Continuous feedback	T	471		-	+
Fischer and Wagner (2018)	Tests	Late	IG	117	-		
Freeman and Gelber (2010)	Mazes		Flat	156	+		
			T	156	+		
			T	156	0		
Gerhards and Siemer (2016)	IQ		Flat	50	+	+	0
			Public	45	0	-	+
			Private	50	+	0	+
			Public	45	0	0	+
Gill et al. (2018)	Verbal / maths		Flat	306	+	+	+
			Flat	306	+	+	+
Hannan et al. (2008)	Production game	Noisy	PR	23	+	+	+
			T	22	0	-	+
	Precise	PR	21	+	+	+	
		T	22	-	-	+	

This table lists papers in which the task is considered by the authors as having little room for improvement. Papers are ranked alphabetically. PR: Piece-rate; IG: Individual grades; T: Tournament

Table A.5 – Papers included in this review with no room for improvement (3)

Paper	Task	Nature of Feedback	Incentives	N	Average effect	Effect at bottom	Effect at top
Hannan et al. (2013)	Verbal and maths (choice)	Private ranking	Flat	15	+		
				15	+		
	(no choice)	Public ranking		15	+		
				15	+		
Kuhnen and Tymula (2012)	Multiplications	Ranking	Flat	54	+	+	-
				72	0		
Ludwig and Lünser (2012)	Effort task	Distribution	T	240	0	+	-
				40	-	-	+
Mago et al. (2016)	Effort task	Distribution	T	40	+	+	+
				40	0		
Newman and Tafkov (2014)	Production game	Ranking	T	84	0		
				76	0		
So et al. (2017)	Multiple cue learning task	Ranking	PR	20	+	+	-
				20	+	+	0
Tafkov (2013)	Multiplications	Private ranking	Flat	20	+	+	-
				20	+	+	0
This paper (No Room)	Counting task	Public ranking	Flat	20	+	+	-
				20	+	+	0
		Ranking	T	204	0	0	0

This table lists papers in which the task is considered by the authors as having little room for improvement. Papers are ranked alphabetically. PR: Piece-rate; IG: Individual grades; T: Tournament

B Instructions given to participants

No Room for Improvement (Experiment 1)

Thank you for taking part in this experiment.

To thank you for your participation, we give you \$2, regardless of your performance in this experiment. In addition to this amount, we give you the opportunity to earn more money.

Your task consists in counting the number of 1s in grids containing only 0s and 1s. Each grid has 6 rows and 6 columns as in the example below.

0	0	0	0	1	1
0	1	0	1	0	0
1	1	0	1	0	0
1	1	0	1	1	0
1	0	1	0	0	0
1	1	0	0	0	1

This experiment will consist in 4 rounds. For each round, you will have 60 seconds to count as much grids as you can.

The remaining time is displayed at the top left of the screen.

When you have counted the number of 1s in the grid, you will need to enter your answer in the box next to the grid.

After entering your answer, you will have to click outside the input area to enable the "Next grid" button.

In each round, you will be rewarded with 1 point for each correct answer. However, wrong answers will be penalized.

For each incorrect answer, you will lose 1/2 point. It is therefore very important that you give your best for every grid.

The first round will be a trial.

Your point totals for rounds 2, 3 and 4 will give you the grand total of points you have earned. This grand total will be compared to that of 50 other persons, randomly chosen among those who have already participated in the same experiment.

If your grand total (the sum of your points in round 2, 3 and 4) is higher than that of 95% of others (i.e. you are in the top 3), you will earn an extra \$8.

At the end of each round, we will display the number of points you have earned during the round and all previous rounds.

At the end of each round, we will display the number of points you have earned during the round and all previous rounds. We will also display your rank among the 50 others for this round.

Room for Improvement (Experiment 2)

Answer sheet of Math Exam n°4

Seat Number:

First Name – Last Name:

The duration of the test is 40'.

Please indicate the correct answer using an X.

For each correct answer, 1 point will be awarded. For each incorrect answer, 1/3 point will be removed.

1	1	2	3	4
2				
3				
4				
5				
6				
7				
8				
9				
10				
11				
12				
13				
14				
15				
16				
17				
18				
19				
20				

21	1	2	3	4
22				
23				
24				
25				
26				
27				
28				
29				
30				
31				
32				
33				
34				
35				
36				
37				
38				
39				
40				

Final grade:

Number of unanswered questions:

Number of incorrect answers:

Number of correct answers:

پاسخنامه آزمون ریاضی ۴

نام و نام خانوادگی :

🕒 وقت آزمون ۴۰ دقیقه می باشد.

📌 گزینه صحیح را با علامت × مشخص نمایید.

📌 هر پاسخ صحیح ۱ نمره مثبت و هر پاسخ نادرست $\frac{1}{3}$ نمره منفی دارد .

شماره صندلی :

۱	۱	۲	۳	۴
۲				
۳				
۴				
۵				
۶				
۷				
۸				
۹				
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۲۱	۱	۲	۳	۴
۲۲				
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نمره :

نزده :

نادرست :

صحیح :

Chapter 5

Effect of the 7R allele on the *DRD4* locus on risk-tolerance does not depend on background risk: Evidence from Senegal

Note: This chapter is co-authored with Aby Mbengue, Clément Mettling, Birane Diouf, Charlotte Faurie, Omar Sene, Emilie Chancerel, Zoe Delporte, Guillaume Hollard, Michel Raymond and Marc Willinger.

Abstract

Previous research has highlighted the correlation between genotypes and risk attitudes, in particular for the 7-repeat allele of the dopamine receptor locus D4 (*DRD4*). It has also been shown that living in risky environments, as well as having a risky occupation, can moderate risk-tolerance. Much less is known, however, about the interplay between the 7R allele and risky environments. We demonstrate that the increase of risk-tolerance due to the 7R allele occurs through an additive, and not dominance effect, and is independent of the environmental risk in two populations in Northern Senegal, one of which is exposed to a very high risk of fishing.¹

¹The authors gratefully acknowledge financial support from GENES, the Key initiatives MUSE Sea & Coast and Investissements d’Avenir (ANR-11-IDEX-0003/Labex Ecodec/ANR-11-LABX-0047).

1. Introduction

Humans need to adapt their behavior as a result of risk. Previous research has shown that risk behavior is partly heritable (Cesarini et al., 2009). Genes involved in the regulation of the dopaminergic system are good candidates to explain the heritability of risk behavior. Dopamine is a neurotransmitter that provides reinforcement for behaviors associated with the expectation of reward in the brain. The dopamine receptor gene D4 is a highly polymorphic gene (Van Tol et al., 1991; Gong et al., 2003). Expressed in the prefrontal cortex, it shows an unusually large variable repeat region in the third cytoplasmic loop, coding for 16 amino acids. While the 4 repeat (4R) variant is the ancestral, and most common allele in all human populations (Chang et al., 1996), there exist variations between 2 and 11 repeats (2R to 11R). In particular, the 7R allele has been shown to be linked to more risk-tolerant attitudes (Dreber et al., 2009; Kuhnen and Chiao, 2009; Carpenter et al., 2011) and novelty-seeking behavior (Ebstein et al. (1996); Benjamin et al. (1996), although this is more controversial Kluger et al. (2002)).

Humans also adapt their risk attitudes as a response to the level of risk in their environment (Lee, 2008). In particular, people have been found to be more risk-averse in the presence of unfair background risk (Harrison et al., 2007; Malmendier and Nagel, 2011; Beaud and Willinger, 2015; Cameron and Shah, 2015), in accordance with the “risk-vulnerability” hypothesis (Gollier and Pratt, 1996; Eeckhoudt et al., 1996).

Much less is known, however, about the interplay between genetic and environmental risk factors in shaping risk attitudes. The aim of the present paper is to test the interaction between the influence of the 7R allele on risk-tolerance and the level of risk to which people are exposed.

2. Results

2.1. Risk-tolerance by zone

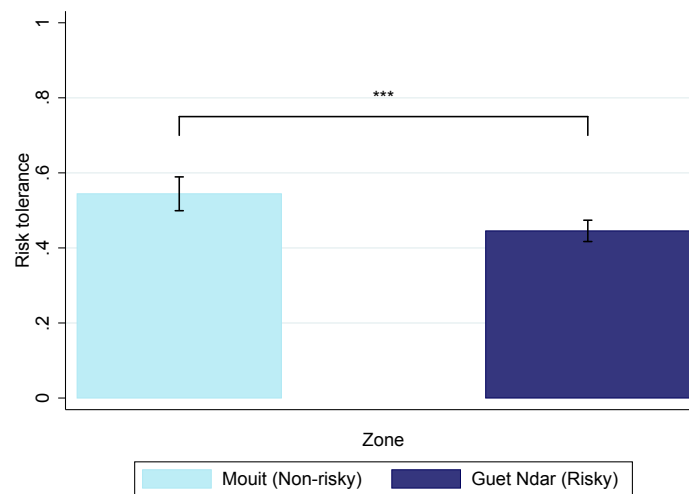
The village of Guet Ndar (Saint-Louis region in Northern Senegal) is famous for its fisheries. Fishing in the area is very dangerous, with authorities reporting 25 deaths due to fishing on average per year over the past 20 years. Given the demography of the village, with 20 000 inhabitants, among which fishing represents the main occupation of approximately 80% of the adult male workforce, this corresponds to approximately 5% of the male population who died due to fishing in the last 20 years. The prevalence of deaths is strongly linked to the intersection of strong currents coming from the Senegal river and an upwelling current from the ocean (Laloë and Samba, 1989). However, these currents attract a lot of fish, making fishing more profitable than other activities in the region (fishermen in our sample declare income significantly higher than non-fishermen,

$p < 0.01$, Table S.1).

In this paper, we compared populations from the fishing village of Guet Ndar ($N = 609$), which is labelled as the *risky area*, and that of a farming village called Mouit, 23 kilometers away ($N = 263$), labelled the *non-risky area*. Importantly, the two populations are mostly composed of the same ethnic group (the Wolofs, representing approximately 80% of the sample in both areas). Because fishing is an activity predominately performed by men, our sample only consists of men.

Our experimental measure of risk-tolerance was based on a lottery task (Binswanger, 1980). A description of the task is provided in the Supplementary Materials. Results indicate that risk-tolerance varied between the risky and non-risky areas. Participants from the risky area tended to exhibit less risk-tolerance than participants from the non-risky area (Figure 5.1, Student's t -test $p < 0.01$). The difference remains significant after controlling for age and education (Table S.2). Our data is consistent with field data and laboratory experiments showing that people exposed to high background risk tend to exhibit less risk-tolerance, in accordance with the “risk-vulnerability hypothesis” (Gollier and Pratt, 1996; Eeckhoudt et al., 1996).

Figure 5.1 – Distribution of risk-tolerance by zone



Note: The higher the risk-tolerance variable, the riskier the choice of participants. Segments represent 95% confidence intervals. Student's t -test * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.2. Genotypes by zone

Genotypes at the DRD4 locus displayed two common alleles (4R and 7R, with 4 and 7 repeats, respectively), which was expected for populations in Africa (Chang et al., 1996), and 5 minor alleles with negligible frequencies (2R, 3R, 5R, 6R and 8R) leading to 21 different genotypes (Table 5.1). Within each area, populations were not at Hardy-

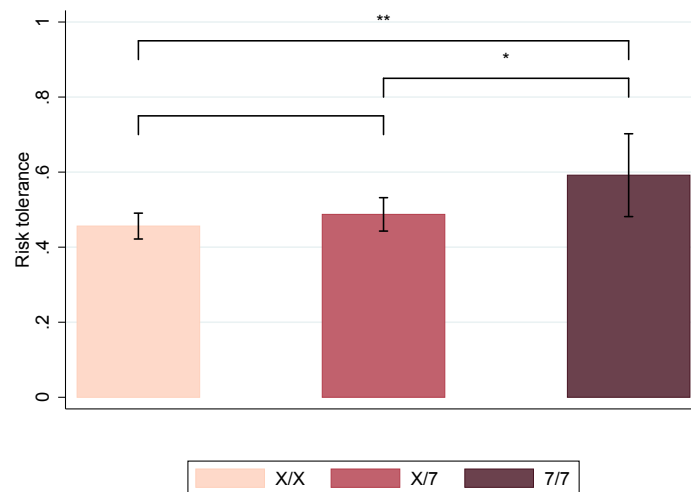
Weinberg equilibrium ($p < 0.01$ in the non-risky area, $p = 0.02$ in the risky area).

Because we are primarily interested in the effect of the 7R allele on risk-taking, we combined all other alleles as an allele “X”. This combination yields three genotypes: XR/XR, XR/7R and 7R/7R. Hardy-Weinberg equilibrium was rejected ($p = 0.01$) for the non-risky area, but not for the risky area ($p = 0.40$), see Table 5.1).

2.3. Risk-tolerance by genotype

Risk-tolerance was not independent of genotype at the DRD4 locus (Figure 5.2 and Table 5.2, Column 1). The 7R allele demonstrated a significant additive effect ($p = 0.01$), and no dominance effect was found ($p = 0.31$). The 7R allele increases risk-tolerance. Importantly, the result holds after controlling for age, education and the living area (Table 5.2, Column 2). Our results indicate that the 7R allele is associated with more risk-tolerant attitudes than other genotypes, in line with previous literature (Dreber et al., 2009; Kuhnen and Chiao, 2009).

Figure 5.2 – Distribution of risk-tolerance by genotype



Note: The higher the risk-tolerance variable, the riskier the choice of participants. X/X, X/7 and 7/7 represent genotypes, with all alleles not 7R combined into the X allele. Segments represent 95% confidence intervals. Student’s t -test * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Moreover, environmental risk did not appear to significantly moderate the effect of the 7R allele. First, its additive effect holds when analyzing both areas separately (Table 5.2, Columns 3 and 4, Figure S.1), although the significance levels drop slightly due to sample limitations ($p = 0.05$ and $p = 0.08$ in the non-risky and risky area, respectively). Second, the interaction between the additive effect and the area (Table 5.2, Column 5) was not significant $p = 0.25$.

Table 5.1 – Genotypic composition at the *DRD4* locus of populations from the Saint-Louis region in the non-risky and risky areas.

Genotype	Non-risky area		Risky area	
	<i>N</i>	%	<i>N</i>	%
<i>Panel A. Without combination of genotypes</i>				
22	3	1.4	3	0.6
24	7	3.3	14	2.8
25	1	0.5	-	-
27	-	-	2	0.4
34	3	1.4	-	-
36	1	0.5	-	-
37	1	0.5	-	-
44	84	39.1	202	40
45	19	8.8	34	6.7
46	14	6.5	18	3.6
47	48	22.3	149	29.5
48	5	2.3	12	2.4
55	4	1.9	3	0.6
56	-	-	1	0.2
57	4	1.9	13	2.6
58	-	-	2	0.4
66	2	0.9	1	0.2
67	1	0.5	12	2.4
77	15	7	34	6.7
78	3	1.4	4	0.8
88	-	-	1	0.2
<i>HW equilibrium</i>				
<i>p</i>	<0.01		0.023	
<i>Panel B. Allele 7R versus other alleles</i>				
XX	143	66.5	291	57.5
X7	57	26.5	181	35.8
77	15	7.0	34	6.7
<i>HW equilibrium</i>				
<i>p</i>	0.011		0.40	

The measure of departure from Hardy-Weinberg (HW) equilibrium ($LL - \chi^2$) refers to the estimated log-likelihood-ratio χ^2 coefficient. The p-value (p) corresponds to an exact test of departure from HW equilibrium with heterozygote deficiency as the alternative hypothesis. Genotype ij refers to the *DRD4* genotype iR/jR . For *Panel B*, all alleles not 7R are combined in the X allele.

Table 5.2 – Differences between genotypes in risk-tolerance

	(1) Without controls	(2) With controls	(3) Non-risky area only	(4) Risky area only	(5) Interaction
7R: additive effect	0.068** (0.027)	0.064** (0.028)	0.097* (0.049)	0.056* (0.032)	0.107** (0.042)
7R: dominance effect	-0.037 (0.036)	-0.026 (0.037)	-0.003 (0.042)	-0.036 (0.042)	-0.027 (0.036)
Age		-0.002* (0.001)			
Education		-0.004 (0.005)			
Risky area		-0.120*** (0.032)			-0.079** (0.036)
Risky area × 7R: additive effect					-0.054 (0.047)
Constant	0.456*** (0.017)	0.613*** (0.053)	0.507*** (0.030)	0.431*** (0.021)	0.509*** (0.029)
R^2	0.009	0.030	0.026	0.006	0.028
No. obs	721	699	215	506	721

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable is risk-tolerance. Standard errors in parentheses.

3. Discussion

In this paper, we find that the 7R allele of DRD4 affects risk-attitudes by an additivity effect, not a dominance effect. This is in contrast with [Faurie et al. \(2016\)](#) who found that heterozygotes 2R/4R had lower risk tolerance. Moreover, we do not find evidence that the 7R allele is associated with novelty seeking, as found in [Kluger et al. \(2002\)](#). Interestingly, we find no evidence of differential genetic selection at the DRD4 locus between areas. While the sample would satisfy [Kirkpatrick \(1996\)](#)'s conditions for a genetic adaptation to habitat (limited migration with 74% of grandparents of participants of the risky area born in the same village, Table S.3, and a strong economic benefit to living in the area), we find no specific genetic differentiation at DRD4 locus relative to 29 unlinked microsatellites loci (Table S.4 and Figure S.2). Moreover, if there was genetic differentiation, it would move in the opposite direction as the risk-vulnerability hypothesis found in previous work for DRD4 ([Faurie et al., 2016](#)), as the 7R allele, favoring more risk-tolerant attitudes, is more prevalent in the risky area. Altogether, our results indicate that there is no selection at the DRD4 locus in our sample. Another point worth mentioning is that the observed differences between zones could also be the reflect of the effects of occupation on risk attitudes, because of a strong correlation between the living area and the probability of being a fisherman (85% of the sample in the risky area declared their main activity as fishing, vs. 4% in the non-risky area).

Further work should focus on genetic adaptation at other loci, for instance using the work of [Karlsson Linnér et al. \(2019\)](#). Moreover, identifying other solutions for people to cope with risk in risky environments could also be further investigated.

4. Methods

A field study was conducted in the Saint-Louis region in Northern Senegal between March 2018 and March 2020. All experiments were conducted in accordance with relevant guidelines and regulations. The protocol (including genotyping) was approved by the Senegalese National Ethics Committee (*Comité National d'Ethique en Recherche en Santé*), and informed consent was obtained from all participants. Behavioral measures were made at the same time as samples were collected for genotyping, so genotypes were not established at the time of measure. Investigators were blind to the behavioral measures during the genotyping.

Measure of risk-tolerance We relied on a standard measure of risk-elicitation task from the experimental economics literature ([Binswanger, 1980](#)). Instructions were displayed in French (the official written language of the country) and enumerators were present to explain the instructions in Wolof, the vernacular language of Senegal. Par-

ticipants were invited to choose a card among five. On each card, two amounts were displayed, with an associated color (red or black) and the corresponding amount in coins of XOF 100, in order to have a more visual representation. At the end of the experiment, one ball was randomly drawn by a local child and gains were calculated. The cards ranged from completely risk-free (400 XOF for both balls) to extremely unequal (0 XOF if Red, 1200 XOF if Black). At each new card, the risk is increased, but so is the average amount won. Cards used are displayed in Figure S.3.

Genotyping DRD4 genotyping was done as described in (Faurie et al., 2016). Briefly, DNA was collected on FTA paper, and amplified with the appropriate primers. Relevant allele was estimated by the size of the PCR product on a 2% agarose gel.

Microsatellite genotyping was based on high-throughput sequencing technology (SSRseq). 30 microsatellite tests were designed according to a streamlined SSRseq development workflow described in (Lepais et al., 2020), of which 29 gave differentiation information (one had only one allele for all individuals). The genomic localisation of the 29 microsatellites and their corresponding F_{st} between the 2 populations is described in Table S.4

Population genetics DRD4 locus was tested for conformity with Hardy-Weinberg (HW) equilibrium using the exact probability test (Rousset and Raymond, 1995). Deviations from HW equilibrium were measured using the F_{is} estimator (Weir and Cockerham, 1984). DRD4 and microsatellite loci genotypic differentiation between populations was tested for by calculating an unbiased estimate of the P-value of a log-likelihood (G) based exact test (Goudet et al., 1996), a global test over loci was calculated using Fisher's method. Population differentiation was measured using the F_{st} estimator (Weir and Cockerham, 1984). Calculations were performed using Genepop R package (V. 1.1.7), based on Raymond and François (1995).

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Supplementary Materials

Table S.1 – Differences in income for fishermen

	(1) Income level
Fisherman	0.958*** (0.138)
Constant	2.211*** (0.109)
R^2	0.073
No. obs	616

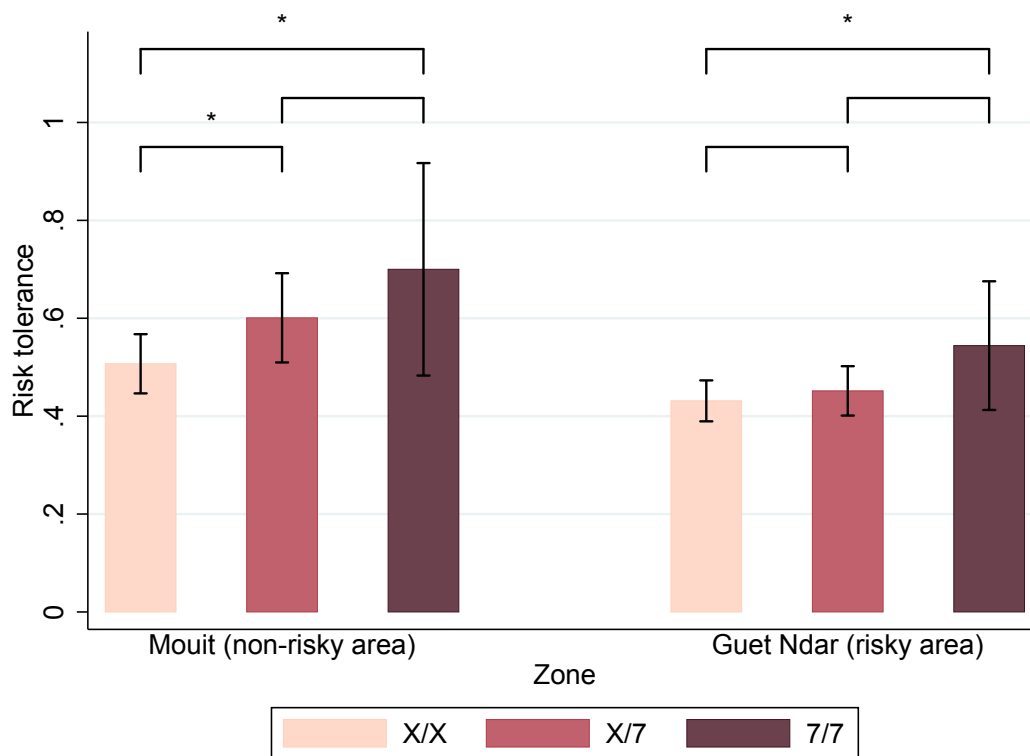
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable is the declared income level. Standard errors between parentheses. Both risky and non-risky areas were combined.

Table S.2 – Differences between zones in risk-tolerance

	(1) Without controls	(2) With controls
Risky area	-0.099*** (0.027)	-0.105*** (0.029)
Age		-0.002 (0.001)
Education		0.000 (0.005)
Constant	0.544*** (0.022)	0.591*** (0.048)
R^2	0.016	0.019
No. obs	860	833

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable is risk-tolerance. Standard errors between parentheses.

Figure S.1 – Distribution of risk-tolerance by genotype by zone

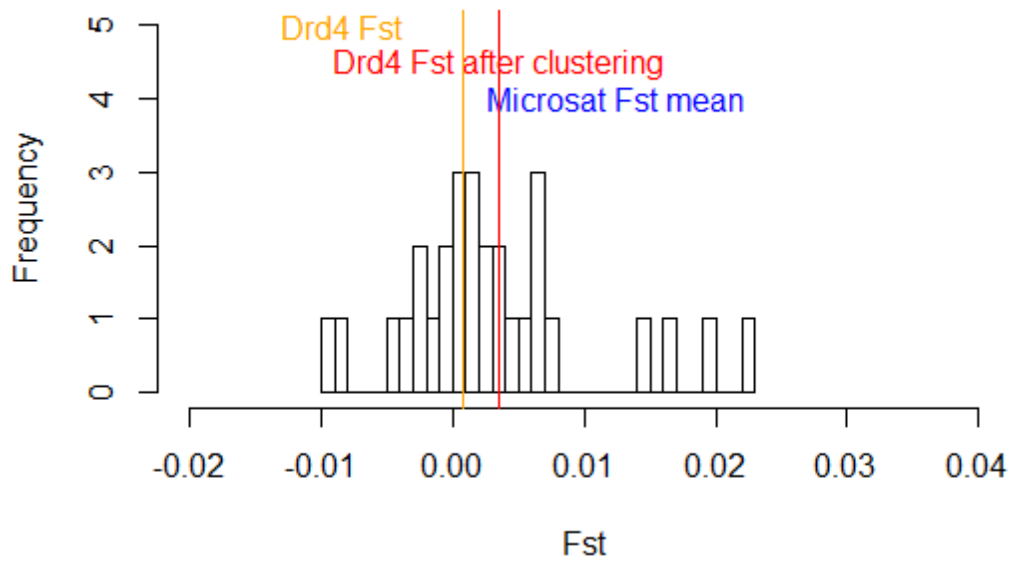


Note: The higher the risk-tolerance variable, the riskier the choice of participants. X/X, X/7 and 7/7 represent genotypes, with all alleles not 7R combined into the X allele. Segments represent 95% confidence intervals. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table S.3 – Percentage of ancestors born in the same village as participants

	(1) Risky area	(2) Non-risky area	(3) Total
Participants	81	67	77
<i>Total parents</i>	<i>72</i>	<i>58</i>	<i>68</i>
Mother	71	67	66
Father	73	49	70
<i>Total grandparents</i>	<i>68</i>	<i>50</i>	<i>62</i>
Maternal grandfather	67	42	59
Maternal grandmother	69	51	63
Paternal grandfather	67	58	64
Paternal grandmother	68	49	62

Figure S.2 – Distribution of genetic differentiation between the risky and non-risky areas



The figure displays F-statistics for genetic differentiation between the two areas, for DRD4 without clustering, for DRD4 after clustering all alleles different from 7R into an “X” allele, and for the 29 micro-satellites selected. The yellow line represents the Fst for the DRD4 locus without clustering, the red line for DRD4 after clustering, and the blue line represents the average Fst for the micro-satellites.

Table S.4 – Genetic differentiation between the non-risky and risky areas on the DRD4 locus and micro-satellites.

Loci	F_{ST}	p -value	(S.E.)
<i>Panel A. DRD4</i>			
DRD4	0.0007	0.0096	(0.0013)
DRD4 after clustering	0.0036	0.0941	(0.0038)
<i>Panel B. Micro-satellite</i>			
<i>No. Chromosome: position</i>			
chr1 : 25140881-25140899	0.0053	0.1993	(0.0053)
chr2 : 9753457-9753484	-0.0081	0.7238	(0.0029)
chr2 : 16239536-16239584	0.0010	0.0285	(0.0017)
chr2 : 44975447-44975477	0.0067	0.1032	(0.0030)
chr21 : 15100302-15100333	0.0001	0.2965	(0.0033)
chr3 : 13815554-13815575	0.0069	0.1144	(0.0034)
chr3 : 28213984-28214005	-0.0015	0.6527	(0.0027)
chr4 : 1467533-1467565	0.0067	0.1639	(0.0029)
chr5 : 12484633-12484657	-0.0001	0.1842	(0.0047)
chr6 : 12334400-12334425	-0.0024	0.7504	(0.0038)
chr6 : 21033972-21033999	0.0199	0.0054	(0.0008)
chr6 : 34463481-34463513	-0.0048	0.3117	(0.0035)
chr8 : 37410868-37410898	-0.0023	0.6902	(0.0043)
chr9 : 4357561-4357589	0.0033	0.2067	(0.0060)
chr9 : 10942052-10942074	-0.0010	0.4405	(0.0031)
chr9 : 23340635-23340654	0.0017	0.1651	(0.0020)
chr10 : 7553026-7553045	0.0012	0.1980	(0.0044)
chr11 : 2009564-2009584	0.0007	0.4635	(0.0007)
chr12 : 607351-607380	0.0041	0.1359	(0.0013)
chr12 : 41680046-41680064	-0.0033	0.8597	(0.0016)
chr13 : 24648458-24648485	0.0027	0.14873	(0.0042)
chr13 : 67656823-67656870	0.0079	0.2031	(0.0050)
chr16 : 5398567-5398589	0.0226	0.2637	(0.0035)
chr16 : 12462593-12462611	0.0150	0.0664	(0.0022)
chr17 : 49684195-49684223	0.0008	0.3120	(0.0036)
chr19 : 6517969-6517988	0.0165	0.1566	(0.0016)
chr19 : 28667178-28667198	0.0022	0.5640	(0.0022)
chr20 : 56130369-56130397	-0.0094	1	(0)
chr21 : 5520201-5520225	0.0040	0.1951	(0.0048)
Combined	0.0035	0.007798	

F_{ST} The measure of differentiation F_{ST} refers to the estimate from [Weir and Cockerham \(1984\)](#). p refers to the p-value of a log-likelihood based exact test. The global test over micro-satellite loci was calculated using Fisher's method.

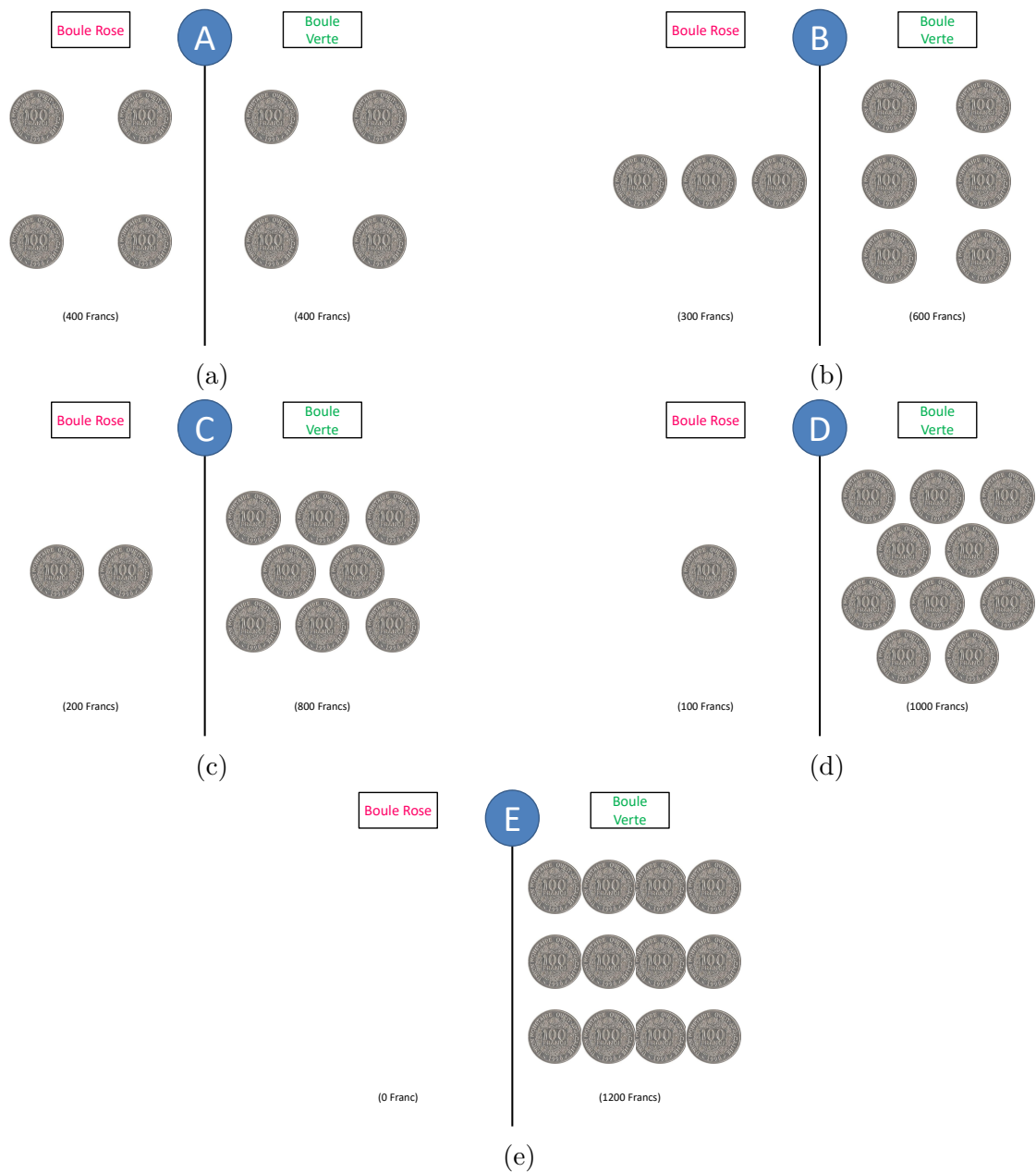


Figure S.3 – Cards displayed in the risk-tolerance elicitation task.

Note: Participants were asked to choose a card among these five. The result is a variable *Choice* from 1 to 5. The variable *Risk-tolerance* was then computed using the following formula: $Risk - tolerance = \frac{Choice - 1}{4}$. The *Risk-tolerance* variable is therefore a variable ranging from 0 (the participant chose the safest option, i.e. the card A) to 1 (the participant chose the riskiest option, i.e. the card E).

General Conclusion

This thesis contributes to the fields of experimental and development economics. In particular, the three first chapters contribute to the literature on the contact hypothesis. The first and second chapters present results from the implementation of similar protocols in widely different contexts. The third chapter provides a meta analysis of the literature and highlights potential avenues for future research. The other two chapters talk about other topics. The fourth chapter sheds light on the use of rankings on performance in different tasks, while the fifth chapter provides insights about the interplay of genetics and environment in shaping risk attitudes.

I would like to end this dissertation by providing three open questions, which I consider to be of prime interest. These questions focus on the contact literature, as it is the main theme of this thesis.

The first open question relates to the consequences of prejudice, and the role contact interventions can play on the topic. Now that the positive effect of contact has been established, the focus should now turn to whether contact interventions are sufficient to mitigate the negative consequences of prejudice. To give an example, it has been proven that working under biased managers tend to decrease workers' productivity (Glover et al., 2017). Contact interventions can help reduce the level of prejudice of these managers. The question then becomes: does this reduction in managers' prejudice induce an increase in productivity for the employees?

The second open question is linked to the comparison of studies involving contact interventions. As was mentioned numerous times in this thesis, there exist a very large heterogeneity of contact interventions and outcome measures used for contact interventions. An interesting direction the literature could take would be to implement joint interventions, with similar protocols, in different contexts (perhaps adapting protocols to better fit some contexts). This effort, which has been undertaken in other fields such as information and accountability of leaders (Dunning et al., 2019) or community policing (Blair et al., 2021). Whether similar findings can be found in different contexts could greatly help analyzing the determinants of effects of contact.

The third and final open questions, which I believe are a prime importance for the future of the field of contact interventions is to understand which policies are the best in terms of cost effectiveness. If contact interventions are effective, and if the answer to the

first open question is yes - i.e. implementing contact can help reduce the consequences of prejudice - then an effort should be made to understand the returns to investments in these policies. If the costs of implementing contact protocols are high relative to their benefits, perhaps interventions should be modified.

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Titre : Essais en Economie Expérimentale et du Développement

Mots clés : Expériences, Confiance, Théorie du Contact, Développement

Résumé : L'économie expérimentale offre la possibilité d'étudier le comportement des agents économiques dans des conditions idéales, puisqu'elle permet le contrôle presque total sur l'environnement des participants. Cette thèse présente les résultats de diverses expériences menées sur le terrain, ainsi que des synthèses de la littérature. Ce manuscrit est composé de cinq chapitres. Les trois premiers chapitres traitent de la théorie du contact, qui vise à améliorer les relations entre différents groupes. Les chapitres 1 et 2 présentent les résultats de deux expériences de terrain, en France et au Sénégal, tandis que le chapitre 3 présente une méta-analyse de la littérature empirique. Les deux derniers chapitres présentent les résultats d'expériences d'économie sur des sujets divers. Le chapitre 4 étudie l'effet de donner le classement sur la performance des agents, en fonction de la nature de la tâche à effectuer. Le chapitre 5 traite du lien entre génétique et environnement dans l'adaptation au risque lié à la pêche au Sénégal. Le chapitre 1 traite d'une expérience de terrain étudiant les effets d'une discussion courte et structurée sur la confiance entre des jeunes de lycées défavorisées de banlieue parisienne et des policiers. La méthodologie est adaptée de la littérature en psychologie sociale et a pour but de créer des liens d'amitié rapidement. Les résultats indiquent que le contact engendre une augmentation du niveau de confiance des lycéens envers les policiers rencontrés, mais ne permet pas d'augmenter la confiance envers les policiers et la police en général. Le chapitre 2 analyse un protocole expérimental proche du chapitre 1, mais l'applique dans le contexte des relations inter-ethniques au Sénégal. Les discussions cadrées et rapides ont un effet sur la confiance inter-ethnique avec les individus rencontrés, mais ne permettent pas l'amélioration de la générosité à l'égard des autres groupes ethniques en général. De

plus, les effets semblent dissipés seulement un mois après le traitement. Des méthodes d'apprentissage statistique permettent de trouver que le traitement est particulièrement efficace pour les individus âgés et peu éduqués. Le chapitre 3 offre une méta-analyse de la littérature empirique sur le contact, qui s'est grandement développée au cours des dernières années. Se basant sur 62 mesures, issues de 37 articles, l'analyse révèle trois résultats principaux. Tout d'abord, il existe une très grande hétérogénéité dans la définition de « contact », ce qui rend difficile la comparaison des différents articles. Ensuite, en moyenne, les interventions de contact sont efficaces en permettant d'améliorer significativement la perception de l'autre groupe. Enfin, l'utilisation d'apprentissage statistique permet de déterminer les déterminants de l'efficacité des interventions. Le chapitre 4 analyse l'effet de l'apport de classements sur la performance, en fonction de la nature de la tâche à effectuer. L'analyse est effectuée au regard d'une analyse de la littérature, d'un modèle théorique novateur et de l'étude empirique de deux expériences. Les résultats indiquent que le facteur qui module grandement l'effet des classements est l'existence, ou non, de possibilité d'amélioration de la technologie de production de la performance. Le chapitre 5 étudie le lien entre les effets du gène codant le récepteur à la dopamine D4 et du risque environnemental sur l'aversion au risque chez des pêcheurs du nord du Sénégal. L'analyse consiste en la comparaison comportementale et génétique de deux populations qui diffèrent selon leur activité professionnelle principale. Les résultats indiquent que l'allèle 7R est associé à une plus grande tolérance au risque, par un effet additif et non de dominance. De plus, aucune hétérogénéité n'a été détectée en fonction du risque environnemental.

Title : Essays in Development and Experimental Economics

Keywords : Experiments, Trust, Contact hypothesis, Development

Abstract : Experimental economics offers the possibility to study the behavior of economic agents in ideal conditions, as it allows for the complete control of the environment of participants. This thesis presents results from several field experiments, as well as their contributions to the literature. This manuscript is composed of five chapters. The first three chapters contribute to the literature on the contact theory, a methodology used to improve intergroup relations. Chapters 1 and 2 present results from field experiments, in France and in Senegal, while chapter 3 presents a meta-analysis of the experimental literature on the topic. The last two chapters present results of various experiments. Chapter 4 investigates the influence of providing rankings on performance, depending on the nature of the task to be performed. Chapter 5 talks about the interplay between genetic and environmental factors in shaping risk attitudes for fishermen in Senegal. Chapter 1 discusses a field experiment investigating the effects of a brief and controlled discussion on trust between disadvantaged students in Paris' suburbs, and police officers. The contact protocol is adapted from the social psychology literature and aims at quickly creating friendships. Results indicate that the contact intervention increases trust of students toward the specific police officers met, but it does not increase trust in the police in general. Chapter 2 analyzes an experimental protocol closely related to that of chapter 1, but applied in the context of inter-ethnic relations in Senegal. The short contact is effective at increasing inter-ethnic trust with the specific individuals met, but does not increase altruism toward the outgroup in general. Moreover, the effect at the individual level evaporates within one month

of the intervention. Machine learning techniques enable to discover that contact is particularly effective for older and less educated participants. Chapter 3 proposes a meta-analysis of the rapidly growing experimental literature on the contact hypothesis. Based on 62 outcomes from 37 research papers, the analysis revealed three main results. First, there exists a large heterogeneity in the definition of a "contact", making difficult the comparison of papers. Second, on average, contact interventions are effective, by significantly improving the perception of the outgroup. Third, machine learning algorithms enable the study of determinants of the most effective contact interventions. Chapter 4 investigates the effects of providing rankings on performance, depending on the nature of the task. The analysis is performed by analyzing the existing empirical literature, proposing a novel theoretical framework and the empirical analysis of two experiments. Results indicate that the main moderating factor of the effects of relative performance feedback is the existence of room for technological improvement. Chapter 5 investigates the interplay between the effects of the dopamine receptor gene D4 and background risk on risk-aversion for fishermen in Northern Senegal. The analysis consists in the genetic and behavioral comparison of two populations differing in their main professional occupation. Results indicate that the 7R allele is associated with an increased risk-tolerance, through an additive, rather than dominance, effect. Moreover, the effects of the 7R allele are not driven by the background risk, as we do not observe heterogeneous effects depending on the level of background risk.