



Information and data manipulation in political economy and household contexts

Oscar David Barrera Rodriguez

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**Information and data manipulation in
political economy and household
context**

Thèse dirigée par: Flore Gubert

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Oscar David Barrera Rodriguez

**Information and data manipulation in political economy and
household contexts**

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Mama, te fuiste antes de tiempo pero dejaste todo el camino listo. En homenaje al tiempo que estuviste aquí viviendo, sufriendo y riendo. A sumerced.

Abstract

Access to information is essential for several reasons. First, it is a way to create knowledge. Economists consider that information has economic value because it allows individuals to make choice that yield higher expected payoffs or expected utility than they would obtain from choices made in the absence of information. Access to information is also essential to the health of democracy. It first ensures that citizens make responsible, informed choices rather than acting out of ignorance. It also serves a “checking function”: having access to information allows citizens to check and to pass judgement on the conduct of their elected representatives. While information is easy to create and spread, it is in the meantime easy to manipulate. It is thus crucial that individuals have the skills to evaluate information and its sources critically before incorporating selected information into their knowledge base and value system.

This dissertation deals precisely with these issues, by exploring the role that information has along three axes. It first assesses the effect of an intervention aimed at informing parents about parenting practices on early child development outcomes (Chapter 1). It then investigates the impact of providing fact-checks of statements by candidates on voters’ electoral preferences and perceptions (Chapter 2). It finally explores the link between political ideology and the reliability of homicide data in countries with local conflicts (Chapter 3).

Chapter 1 analyzes the impact of information about parenting practices on early childhood development through an experiment consisting in randomly assigning households in rural Nicaragua to receive text messages about child nutrition, health, stimulation or the home environment. The intervention led to significant changes in self-reported parenting practices. However, it did not translate into improvements in children’s cognitive development. When local opinion leaders are randomly exposed to the same text message intervention, parental investments decline and children’s outcomes deteriorate. Since interactions with leaders about parenting practices also decline, the negative effects may have resulted from a boycott or a crowding-out of local leaders.

Chapter 2 investigates the effectiveness of fact-checking in countervailing *alternative*

facts, i.e., misleading statements by politicians, on electoral preferences and perceptions. It uses a randomized online experiment during the 2017 French presidential election campaign during which 2,480 French voters were subjected to alternative facts by the extreme-right candidate, Marine Le Pen, and/or corresponding facts about the European refugee crisis from official sources. The results show that: (i) alternative facts are highly persuasive; (ii) fact-checking improves factual knowledge of voters (iii) but it does not affect policy conclusions or support for the candidate; (iv) exposure to facts alone does not decrease support for the candidate, even though voters update their knowledge. Evidence is also consistent with the view that at least part of the effect can be explained by raising salience of immigration issues.

Chapter 3 explores the link between political ideology and the reliability of homicide data in countries with local conflicts. It examines this question empirically using a regression discontinuity design (RDD) approach to close-run elections in the context of the Colombian conflict. It first shows that the number of homicides perpetrated by rebel troops increased substantially following close-run elections in municipalities where mayors adopted the incumbent ideology, whereas no impact is observed on the number of homicides perpetrated by any other group. It then provides empirical evidence suggesting that the reported increase in violence is more likely to be due to the alteration of records by official bodies for political purposes than to acts of retaliation by the rebels.

Résumé

L'accès à l'information est essentiel pour plusieurs raisons. Premièrement, il s'agit d'un moyen de créer de la connaissance. Les économistes considèrent que l'information a une valeur économique car elle permet aux individus de faire des choix qui leur procurent des gains et/ou une utilité espérés plus élevés que ceux auxquels ils pourraient prétendre en l'absence d'information. L'accès à l'information est également essentiel pour la démocratie. Il permet d'abord aux citoyens de faire des choix responsables et éclairés plutôt que d'agir dans l'ignorance. Il remplit également une fonction de contrôle : avoir accès aux informations permet aux citoyens de contrôler et d'évaluer les actions de leurs élus. Bien qu'il soit facile de produire et de diffuser des informations, il est également relativement aisé de les manipuler. Il est donc crucial que les individus soient dotés des compétences nécessaires pour évaluer de manière critique les informations qui leur parviennent et la fiabilité de leurs sources avant qu'elles ne soient intégrées dans leur socle de connaissances et leur système de valeurs.

Cette thèse traite précisément de ces questions, en analysant le rôle de l'information dans trois contextes différents. Le premier chapitre évalue les effets d'une campagne de sensibilisation faite auprès de parents pour améliorer le développement cognitif et non cognitif de leurs très jeunes enfants. Le deuxième chapitre étudie dans quelle mesure la vérification des faits pour contrer les fausses informations qui circulent dans les discours politiques, notamment chez les candidats à une élection, modifie les préférences partisans et les perceptions des électeurs. Enfin, le troisième chapitre examine dans quelle mesure les chiffres sur les homicides peuvent faire l'objet de manipulation à des fins politiques, dans le contexte d'un pays en conflit.

Le chapitre 1 porte sur une campagne de sensibilisation menée auprès de parents résidant en milieu rural au Nicaragua. Son objectif est de mesurer l'impact de cette campagne sur le développement cognitif et non cognitif des très jeunes enfants. Pour ce faire, certains parents ont été exposés de façon aléatoire à différents messages reçus par SMS portant selon les cas sur la nutrition, la santé, l'éducation ou encore l'environnement familial. Les résultats de l'évaluation montrent que l'intervention a

entraîné des changements importants dans les pratiques des parents, sans que cela ne conduise à une amélioration du développement cognitif de leurs enfants. De plus, lorsque des personnalités influentes au sein des communautés ont elles-mêmes été exposées aux messages, l'intervention entraîne un moindre investissement des parents dans les soins apportés à leurs enfants et une dégradation du développement cognitif de ces derniers. Ce résultat semble être lié au fait que les parents interagissent moins avec ces personnes, soit par boycott, soit par effet d'éviction.

Le chapitre 2 étudie l'impact que peut avoir la vérification des faits pour contrer les fausses informations qui circulent dans les discours des responsables politiques, sur les préférences partisans et les perceptions des électeurs. Pour ce faire, il s'appuie sur les résultats d'une expérimentation réalisée en ligne lors de la campagne électorale française de 2017. Dans le cadre de cette expérimentation, 2 480 électeurs français ont été exposés de façon aléatoire à des faits alternatifs (*alternative facts*) tels qu'ils ont été exprimés par la candidate d'extrême droite, Marine Le Pen, et/ou à des faits relatifs à la crise des réfugiés européens provenant de sources officielles et donc avérés. Les résultats montrent que: (i) les électeurs sont très convaincus par les faits alternatifs; (ii) la vérification des faits améliore la connaissance factuelle des électeurs (iii) Elle n'affecte toutefois pas les avis qu'ils peuvent avoir sur la candidate ou le soutien qu'ils lui apportent; (iv) l'exposition à des faits avérés ne diminue pas le soutien à la candidate, même si les électeurs mettent à jour leurs connaissances. Il est possible que ce résultat soit en partie lié à l'emphasis mise sur les questions d'immigration.

Le chapitre 3 explore le lien entre idéologie politique et fiabilité des chiffres sur les homicides perpétrés par les groupes rebelles dans les pays en proie à des conflits locaux. Il examine cette question de manière empirique à partir du cas de la Colombie, en utilisant une régression sur discontinuité - *regression discontinuity design* –, i.e. en se focalisant sur les municipalités dans lesquelles un candidat favorable à l'idéologie du président Uribe a été élu ou éliminé de justesse. Les résultats montrent tout d'abord que les chiffres relatifs aux homicides perpétrés par les troupes rebelles ont considérablement augmenté au cours de la période post-électorale dans les municipalités où les maires se sont déclarés favorables à la politique présidentielle, alors qu'aucun changement n'est observé dans les

chiffres relatifs aux homicides perpétrés par les autres acteurs du conflit. Ils montrent ensuite que l'augmentation de la violence dans ces municipalités, telle que suggérée par les chiffres, est plus vraisemblablement due à la manipulation des données par les organes officiels à des fins politiques qu'à des actes de représailles de la part des rebelles.

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Introduction

Access to information is essential for several reasons. First, it is a way to create knowledge. Economists consider that information has economic value because it allows individuals to make choice that yield higher expected payoffs or expected utility than they would obtain from choices made in the absence of information. Access to information is also essential to the health of democracy. It first ensures that citizens make responsible, informed choices rather than acting out of ignorance. It also serves a “checking function”: having access to information allows citizens to check and to pass judgement on the conduct of their elected representatives. While information is easy to create and spread, it is in the meantime easy to manipulate. It is thus crucial that individuals have the skills to evaluate information and its sources critically before incorporating selected information into their knowledge base and value system.

This dissertation deals precisely with these issues, by exploring the role that information has along three axes. It first assesses the effect of an intervention aimed at informing parents about parenting practices on early child development outcomes (Chapter 1). It then investigates the impact of providing fact-checks of statements by candidates on voters’ electoral preferences and perceptions (Chapter 2). It finally explores the link between political ideology and the reliability of homicide data in countries with local conflicts (Chapter 3). This dissertation is thus at the cross-roads of two fields in Economics: Development Economics on the one hand, and Political Economics on the other hand. It has a strong empirical content, with the use of experimental methods in order to identify the causal impact of information on various outcomes in Chapters 1 and 2, and the use of a Regression Discontinuity Design to elicit the causal effects of political ideology on homicide reporting biases in Chapter 3.

Chapter 1 (joint with Karen Macours, Renos Vakis and Patrick Pemand) is motivated by the global situation of children. It is estimated that 250 million children under 5 years old are at risk of not reaching their developmental potential in the developing world (Black, 2017). Early childhood development (ECD) is an important predictor of success later in life (Almond, 2018). Early childhood interventions can have high

returns given the malleability of the brain (Knudsen, 2006) and the hypothesized dynamic complementarities in human capital investments (Heckman, 2006).

Interventions that aim to improve parenting knowledge or skills have been effective at changing parenting practices, for instance through home visiting programs (Jeong, 2018). However, whether such interventions can be scaled-up to improve child development in a cost-effective way remains an open question. Technology-assisted interventions offer a promising avenue for such a scaling-up (Hall, 2015). Text-messaging interventions, for e.g., are an appealing channel to deliver information on ECD due to their low cost, the increasingly widespread use of mobile phones, and their potential to circumvent quality issues with decentralized service delivery (Carta, 2013; Ajzenman, 2019). They seem particularly relevant in contexts when personal interactions are difficult, either because of social distancing requirements or because of more general constraints to accessibility.

In order to test this, I participated to the impact assessment of an experiment which took place in rural Nicaragua. This intervention was aimed at improving early child development outcomes through text messages on parenting practices sent to parents. Related to this, the impact assessment had a twofold objective: first, identifying the causal effect of providing such information on child development outcomes; and second, testing whether the effect of the intervention varied depending on local opinion leaders' own exposure to the text messages. The baseline and endline questionnaires were designed so as to collect information on both intermediate and final outcomes such as parental practices, parental beliefs and parents' investments in nutrition, health, etc. and young children's cognitive and socio-emotional development. Baseline data were collected in Fall 2014, using a short household survey questionnaire capturing information on household composition, socio-economic status, cellphone usage, social interactions and economic activities, as well as child-specific information on health practices, preschool participation, nutritional intakes, early childhood practices and attitudes. In addition, the baseline was used to identify, for each child, a "mother" and a "father" figure, i.e. the child's biological mother and father, or in their absence, the individual(s) with a similar caregiver role, as defined by the household. The child-level follow-up survey included a number of tests to assess early childhood development. First, the four sub-scales of the

Denver Developmental Screening Test (Frankenburg, 1967) were used to assess social-personal, language, fine motor, and gross motor skills for all children aged between 12 and 84 months.

More precisely, we used a modified version of the Denver Test, similar to the one used in Nicaragua's national early childhood stimulation program. We also used three additional tests for children aged between 36 and 84 months: (1) the TVIP, the Spanish-speaking version of the Peabody Picture Vocabulary Test (PPVT) (Dunn 2006); (2) the digit span, an associative numeric memory test; and (3) a marshmallow test to measure self-control, that we adapted to the local (humid) survey context by using a hard red candy instead of a marshmallow. My role in the project was to supervise the whole data collection process and to clean and analyze the data. Thanks to the time I spent in the field, I gained insights and understanding of some of the results of this research. I also realized how difficult it was to create an effective communication with very poor households. Overall, we found that thanks to the intervention, parents were able to update information about good practices. But we found no impact on final children test scores. This evidence suggests that extra efforts are needed to change parents' habits and make them effectively adopt good parental practices. Later interventions in Colombia and Peru did actually complement the SMS reminders with the visits of some professional trainers in the targeted households and produced more significant results on children outcomes.

Chapter 2 (joint with Sergei Guriev, Emeric Henry and Ekaterina Zhuravskaya) is motivated by the extensive use of "alternative facts", i.e. statements by politicians which directly or indirectly contradict real facts. Examples of such alternative facts abound. As an illustration, pro-Brexit campaign falsely claimed that EU membership cost the UK over 350 million British pounds per week (about 500 million US dollars at the pre-Brexit exchange rate) and this money could be saved by the national budget in case of a UK exit from the European Union. In order to counter this, mainstream media in established democracies have increasingly invested in checking politicians' claims and provided rebuttals. In this perspective, it is interesting to explore the effectiveness of these fact-checking efforts on citizens' knowledge and decisions.

To this end, I designed and implemented an online experiment during the 2017 French presidential election campaign, where some voters were randomly exposed to alternative facts (with attribution to their source), while other voters were exposed to alternative facts and the respective fact-checking (also attributed to the source). The advantage of such a design is that it allowed identifying the causal effect of alternative facts and fact-checking in a real-world setting, using real statements pronounced by the French extreme right candidate Marine Le Pen. The online survey involved 2,480 French voting-age individuals in total, using the Qualtrics online platform, an analogue of the Amazon Mechanical Turk. This platform is mostly used by companies to conduct market research. The survey respondents were drawn at random from a pool of Qualtrics subscribers, which is composed of individuals who are used to participating in online surveys in exchange for remuneration. Potential survey participants were first contacted by the Qualtrics team via email. This email provided information on the compensation fee upon completion of the survey and the link to it, which the participants could choose to click on. At the start of the survey, the participants were given a brief introduction to the survey indicating its focus on political preferences, voting intentions, and attitudes toward immigrants. It was also stated that only aggregate results would be published. There was no mention of any political party or political candidate. After this introductory page, participants were allowed to drop out. While I initiated and designed the intervention and evaluation protocol, both benefited much from the many discussions I had with my co-authors.

Overall, we find that alternative facts are highly persuasive: voters exposed to Marine Le Pen's rhetoric move their policy conclusions and voting intentions towards her. We further find that fact checking is successful in correcting factual knowledge, but does not translate into an impact on voting intentions. Alternative facts are equally effective with and without fact checking in convincing voters to vote for the politician who uses narratives based on alternative facts. We then explore the potential mechanisms behind these results, and cannot exclude that at least part of the effect can be explained by raising salience of the immigration issue. This paper has been published in the *Journal of Public Economics*.

Chapter 3 is motivated by a desire to contribute to the literature on conflict persistence. Despite the high social, economic and human costs that civil conflicts incur (Weinstein, 2006; Fearon, 2003; Collier, 2004), conflicts may be hard to solve. One of the reasons behind conflicts' persistence may relate to the direct benefits in the form of economic or political rents they bring to powerful elites. Specific groups or actors may indeed gain from war or disorder and hence have low incentives to exert political and economic pressure to prevent or stop ongoing conflict. With this in mind, my objective in this chapter is to show that in order to get credibility and voters' support, politicians who choose the use of coercion to solve a conflict need to convince voters that their approach to conflict resolution is the most feasible one, or, in other terms, that their enemy is too dangerous for another approach, such as a political peace agreement, to be considered. Using Colombia as a case study and following this line of reasoning, I argue, based on some empirical evidence, that some local politicians in this conflict-stricken country may have been tempted to provide upwardly-biased reports on the number of homicides committed by guerrilla groups for political purposes.

To this end, I combine different sources of administrative data, and use a Regression Discontinuity Design to elicit the causal effects of political ideology on homicide reporting biases. The first source of data provides conflict-related data from the count of incidents recorded by El Centro Nacional de Memoria Historica. These highly precise data include day-to-day information on attacks, bombings, murders and so on perpetrated by all the players involved in the Colombian civil conflict. This dataset combines different sources, including CINEP's *Noche y Niebla* magazine with its detailed accounts of all incidents by perpetrator. The second source of data provides detailed information on attacks extracted from the websites of human rights NGOs known for monitoring political violence in Colombia. The third source provides detailed information on attacks covered by the leading national and local newspapers. The paper also uses geographical and demographic controls measured at the municipality level and provided by the Center for the Study of Economic Development (CEDE) at Universidad de Los Andes. This dataset forms a supplementary source of data on attacks, murders, kidnappings, and so on, taken from official and institutional sources such as the National Police Agency and the Ministry

of Defense.

With regards electoral outcomes, the data are taken from the National Registry in charge of running elections (1958-2015) which provide information on each candidate, total votes, share of votes, parties and electoral outcomes for the various recent elections that took place in Colombia including the 2016 national referendum to ratify the peace agreement.

Overall, the results show that the number of homicides perpetrated by rebel troops increased substantially following close-run elections in municipalities where mayors adopted the incumbent ideology, whereas no such impact is observed on the number of homicides perpetrated by any other group. Further empirical evidence suggests that this reported increase in violence is more likely to be due to the alteration of records by official bodies for political purposes than to acts of retaliation by the rebels.

Chapter 1

Texting Parents about Early Child Development: Behavioral Changes and Unintended Social Effects

1.1 Introduction

An estimated 250 million children under 5 years old are at risk of not reaching their developmental potential in the developing world (Black et al., 2017). Early childhood development (ECD) is an important predictor of success later in life (Almond et al., 2018). Early childhood interventions can have high returns given the malleability of the brain (Knudsen et al., 2006) and the hypothesized dynamic complementarities in human capital investments (Heckman, 2006). A growing literature provides experimental evidence on the effects of ECD interventions in the short-term (on cognitive or socio-emotional outcomes) or the long-term (on school performance, wages, or criminality). The issue is high on the policy agenda in both the developed (Currie, 2001; Schweinhart, 2005; Nores et al., 2005) and the developing world (Berlinski and Schady, 2015).

Interventions that aim to improve parenting knowledge or skills have been effective at changing parenting practices, for instance through home visiting programs (Jeong et al., 2018). Whether such interventions can be scaled up to improve child development in

cost-effective ways remains an open question. Technology-assisted interventions offer a promising avenue for scale-up (Hall and Bierman, 2015). Text-messaging interventions are an appealing channel to deliver information on ECD due to their low cost, the increasingly widespread use of mobile phones, and their potential to circumvent quality issues with decentralized service delivery (Carta et al., 2013; Ajzenman and López Bóo, 2019). Text messaging interventions are particularly relevant in contexts when personal interactions are not possible, either because of social distancing requirements or more general constraints to accessibility.

A growing literature considers the effectiveness of scaling up early childhood interventions by relying on local service providers, such as community health workers (Hamadani et al., 2019) or social workers (Attanasio et al., 2020; Premand and Barry, 2020). The broader economics literature provides evidence of the potential role of local influencers (Banerjee et al., 2019; Alatas et al., 2019) and the existence of social multiplier effects in health or education interventions (Oster and Thornton, 2012; Bobonis and Finan, 2009). In a rural area of Nicaragua close to the setting of this paper, social interactions with local leaders were found to increase the impacts of a conditional cash transfer program on households' investments in education and nutrition, both in the short and medium term (Macours and Vakis, 2014, 2017). Yet a recent literature also highlights negative effects of outside interventions that crowd-out local service providers Deserranno et al. (2020) or interfere with local institutions Baldwin et al. (2020).

This paper provides novel experimental evidence on a text message intervention aiming to improve parenting practices in rural Nicaragua. To the best of our knowledge, this is the first paper analyzing the causal effect of providing parenting information by text message on early childhood development in a developing country context. Our first contribution is to test the direct effect of sending text messages to parents on parenting practices and children's cognitive development. Our second contribution is to test whether impacts vary depending on local opinion leaders' exposure to the text messages. We measure impacts on young children's cognitive and socio-emotional development using a battery of age-appropriate tests. We also measure impacts on the parental practices, beliefs and investments that were promoted by the intervention. This

captures intermediary mechanisms through which the text messages could have effected ECD outcomes.

The intervention sent daily text messages with advice about parenting practices conducive to early childhood development. The messages covered key risk factors for ECD (Walker et al., 2007b) related to nutrition, health, stimulation and the home environment. They were delivered to parents of children between 0 and 6 years of age in 97 villages over a period of approximately 10 months. The text messages were personalized and gave age-appropriate advice. In a random subset of villages, local opinion leaders (such as community health volunteers, preschool teachers, etc...) received similar text messages. Additional experimental variations were introduced to analyze to what extent impacts differ depending on the type of recipient (mother, father, or both) or the specific risk factors on which the messages focused (nutrition and health, stimulation and the home environment, or both).

Results show that treated households improve parenting practices. Parents receiving messages about nutrition and health report better nutritional and hygiene practices. Likewise, parents receiving messages about stimulation report higher stimulation and progressive beliefs about ECD consistent with those messages. However, no effects on children's cognitive or socio-emotional outcomes are found. Changes in parenting practices are not sufficient to induce gains in cognitive or socio-emotional development.

To help understand the limited impact of the text message intervention, the analysis considers how impacts vary depending on local opinion leaders' exposure to the text messages. Results show a significant negative impact of leaders' exposure to the intervention on cognitive development of children in their village, a finding that goes against our prior when designing the experiment and earlier results in the literature. Leaders' exposure to text messages also negatively affects a number of intermediary outcomes, including stimulation and nutritional practices. While the experimental design does not allow to isolate the exact reasons for these negative effects, we discuss three main potential mechanisms. While the negative effects are unlikely to be driven by confusion about the messages, we find evidence consistent with boycotts or crowding-out of local opinion leaders. In particular, interactions about ECD practices between local leaders and

parents decrease in villages where leaders receive the text messages. We speculate that the text messages may have been perceived by local opinion leaders as interfering with their traditional domain of influence, either demotivating them or possibly leading them to actively work against them. While we cannot fully disentangle these potential pathways, the negative effects from leaders' exposure to the text messages and the overall lack of impacts on children's cognitive outcomes are robust. This suggests that the roll-out of technology-based solutions requires careful attention to the social spill-overs they may generate, as well as to the response by local leaders they may generate.

The paper contributes to several strands of the literature. Few studies have assessed the effect of providing parenting advice via text messages on children's cognitive outcomes in early childhood. A set of recent studies on (pre-)kindergarten children in the US shows that sending text messages to parents can help increase early reading skills (York et al., 2018). Effects vary based on the content, personalization and frequency of the text messages (Cortes et al., 2018; Doss et al., 2019). In an earlier study, Hurwitz et al. (2015) find a positive effect of delivering parental advice via text message on parental engagement in learning activities in early childhood. For older school-age children, Bettinger et al. (2020) find a relatively large impact of text nudges on learning in Brazil.

This paper complements a broader literature showing the potential of text messages as nudges for health-related behaviors such as prevention and disease management (Cole-Lewis and Kershaw, 2010), treatment adherence (Pop-Eleches et al., 2011), improved child eating behavior (Chai et al., 2019), or vaccination (Kazi et al., 2019). It links to evidence of text messages influencing pro-social behaviors (Konrath et al., 2015). The analysis of the effects of sending text messages to local opinion leaders may also be relevant for the literature on the use of technology to support teachers in Africa and Asia (Gaskell and Mills, 2009; Valk et al., 2010; Walsh et al., 2013), with positive effects found on children's literacy and classroom practices in Kenya (Jukes et al., 2017).

The paper further adds to the literature analyzing impacts of ECD interventions on children's outcomes in developing countries. While no study currently analyzes the impact of parenting advice sent by text message on children's cognitive development, many interventions expose parents to parenting advice through other - often more

intensive and direct - means. A seminal study in Jamaica shows that children randomly assigned to receive early childhood stimulation through home visits have remarkable improvements in cognitive outcomes in the short term, and some effects remain twenty years after the intervention (Grantham-McGregor et al., 1991; Walker et al., 2007a, 2000; Powell et al., 2004). Experimental evidence from similar interventions, now available for at least 11 low and middle income countries, generally confirm positive impacts on early childhood development, though effect sizes vary substantially, and delivering effective interventions at scale has proved challenging (Jeong et al., 2018; Attanasio et al., 2020; J-PAL, 2020). In a similar context than the current study, Macours et al. (2015) find positive but small effects of a home visit parenting program.

A range of other interventions have been considered to improve ECD. Cash transfer programs aim to change human capital investments in poor households. They have led to shifts in parental investment and gains in cognitive development in Nicaragua (Macours et al., 2012), as well as other developing countries (Paxson and Schady, 2010; Levere et al., 2016) and when combined with parenting information (Arriagada et al., 2018). Evidence on long run impacts of exposure in early childhood is also emerging (see Millán et al. (2019) for a review). Experimental studies from interventions primarily focused on behavioral change in nutrition or preventive health practices also show evidence of positive changes in parental investment and nutrition behaviors¹, but rarely measure impacts on cognition (Fitzsimons et al., 2016; Ahmed et al., 2019; Field and Maffioli, 2020). Recent exceptions include Levere et al. (2016) and Premand and Barry (2020), who find positive effects of behavioral change promotion on parenting practices, but not on cognition.²

The rest of the paper proceeds as follows. In the next section, we describe the text message intervention and the data, including ECD measurement. Section 3 presents

¹There is a related literature on interventions directly improving nutrition through supplementation. A well-known study in Guatemala shows positive impacts on reading comprehension and test scores years later, and ultimately also to higher wages (Maluccio et al., 2009; Hoddinott et al., 2008).

²Other ECD interventions do not directly target parental investments. For instance, there is relatively limited and mixed evidence from experimental studies analyzing the impact of preschools in developing countries (Martinez et al., 2017; Bouguen et al., 2018; Dean and Jayachandran, 2019; Blimpo et al., 2019), even if earlier non-experimental studies suggest that preschool attendance is linked with better school performance in Argentina (Berlinski et al., 2009) and Uruguay (Berlinski et al., 2008).

the empirical strategy. Section 4 presents the main experimental results. Section 5 analyzes potential mechanisms behind the negative impacts of leaders' exposure to text messages. The last section concludes. Tables and figures are presented in annex (and in a supplementary online appendix).

1.2 Intervention, Study Design and Data

1.2.1 Experimental Design

The text message intervention was implemented from late 2014 to late 2015 in four municipalities in rural Nicaragua³. Participants received a daily text message with a short practical recommendation about positive parenting practices. The messages covered early stimulation, the home environment, nutrition, and preventive health care. The messages were sent to parents and focused on the youngest child below 6 years old in the household. The text messages were personalized (making references to the name of the child) and age appropriate (with different messages sent to parents of children 0-6 months old, 7-12 months old, 13-24 months old, and older than two years old). Messages were designed to provide actionable advice based on a government early childhood development curriculum, and built on prior experiences with early childhood development programs in the country. The text message intervention was designed by a Nicaraguan early childhood specialist, in consultation with the ministry of the family and with support from the World Bank. The intervention cost approximately \$ USD 50 per household.⁴

Prior to program implementation, a baseline survey was conducted in 97 rural villages in the four municipalities, including all households with children 0 to 6 years of age or pregnant women. Earlier work in nearby municipalities (Macours and Vakis, 2014) had demonstrated the role of local leaders in influencing behavioral changes about investment in nutrition and health of young children. The baseline survey therefore also identified,

³Totogalpa, Telpaneca, Yalaguina and Palacaguina in the North West of Nicaragua.

⁴Total costs included cell phones (\$ USD 50,000), text messaging distribution platform and incentives (\$ USD 65,000) and field implementation (including registration assemblies) and monitoring (\$ USD 35,000)

among households with children 0-6 (or pregnant women), all households with local opinion leaders regarding early childhood practices. This included community health workers, preschool teachers, primary school teachers, and a village leader.⁵ In the 97 villages, there were a total 2990 households with at least one child younger than 6 years old, including 400 opinion leaders. 5 villages had no opinion leaders among households with young children.

Among households without leaders, a household-level randomization determined which households would receive text messages: 75% were randomly selected to receive text messages, with the remaining 25% serving as control.

In addition to treatment assignment, orthogonal treatment variations were introduced among households selected to participate in the program. First, we randomly selected whether text messages were sent to (i) the father figure (25 percent of the sample), (ii) mother figure (25 percent of the sample) or (iii) both (25 percent of the sample). Second, the thematic content of the text messages varied. Treated households were randomly assigned to either receive text messages focused on (i) early childhood stimulation and the home environment; (ii) nutrition and preventive health care; or (iii) a combination of topics. The household level randomization was stratified on the level of education of the main caregiver (in most cases the mother of the child) distinguishing those with less than 4 grades, 4-6 grades and more than primary; and on whether there is a male caregiver (typically the father) in the household.

Among the 92 villages with at least one opinion leader, a village-level randomization determined whether household with opinion leaders received no message (27 villages), whether both the men and women in the leader households received messages (27 villages), or whether only the women (27 villages) or only the men (11 villages) in the leader household received text messages. The village-level randomization of the “leader” treatment was stratified on the average level of education of the main caregiver in households with leaders (specifically a variable capturing the tercile in the education distribution).⁶

⁵Not included were religious leaders, and people responsible for programs unrelated to health, education or ECD.

⁶Randomization of the content of the messages among leader households was also stratified on access

A registration assembly was organized in each village in December 2014. All households from the treatment and control groups were invited. They all received a basic cell phone labelled as being assigned to the mother or the father of the target child (as per the randomization). This was done because a relatively large share of parents did not have cell phones. Households randomly assigned to have both the mother and father figure receive text messages were given two cell phones. Cell phones were also distributed to control households in order to rule out any effect resulting from an increase in cell phone access. Households assigned to treatment were then assisted to activate the text message intervention. This was done by sending a text message to a central server, with only treated cell phones able to register. Once activated, cell phones started receiving daily messages. Treatment households that did not participate to the assemblies received the cell phones, information about the program and help with activation at their home after the assembly. The registration assemblies took a month to complete for all villages, resulting in a take-up (activation) rate of 90%.

1.2.2 Incentives and Compliance

In addition to receiving daily text messages, registered households had the opportunity to participate in a weekly quizz. They were sent a multiple choice question about one of the parental practices highlighted in earlier messages, and given 24 hours to respond. All individuals who responded correctly entered a lottery to earn free airtime. Every week about 20% of correct answers received 1 US\$ airtime, and once a month, winners received 5 US\$ airtime. The quizzes provided a small incentive for participants to continue reading the text messages. This was considered particularly important as cell phone coverage and electricity access are not universal in the villages, and receiving text messages at times requires walking to a location with signal or charging phones outside of the home. We use the weekly information about households' response to the quizz as a proxy for active participation in the text message intervention.

Figure 1.1 shows the trivia response rate over time. The response rate peaked at 70

to electricity at home. This is only done for leaders because access to electricity is highly correlated with education among non-leaders.

percent about two months after the start of the intervention (i.e. when all registrations were finalized), and then slowly decreased over the year to reach about 25 percent after 10 months.⁷ Qualitative field work indicated that this decline reflected three factors. First, some households reported having lost or broken the cellphone or its charger.⁸ Second, some households voluntarily stopped using the mobile phone all together (some of the phones were seen being used as a toy instead). Third, others kept using the mobile phone but simply stopped responding to quizzes out of lack of interest or motivation to search for phone signal.

1.2.3 Data

Baseline data was collected in the Fall of 2014 using a short household survey instrument. It captured information on household composition, socio-economic status, cellphone usage, social interactions and economic activities, as well as child-specific information regarding health practices, preschool participation, nutritional intake, early childhood practices and attitudes. The baseline was used to identify for each child a "mother" and a "father" figure, i.e. the child's biological mother and father, or in their absence, the individuals with similar caregiver roles, as defined by the household.

A follow-up survey was implemented in July and August 2015, approximately nine months after the households had started receiving the text messages.⁹ The follow-up survey targeted children in sample households, as well as the main caregiver. We limit the analysis to children under 7 years of age at follow-up (2803 children), in order to use the full battery of tests. Questions on early childhood practices and attitudes were asked separately to mother and father figures.

⁷This percentage is computed over all the cellphone numbers distributed to the treatment households, including leaders and non-leaders. Cellphone numbers distributed to control households did not receive the quizzes. The dip in responses in April likely corresponds to a temporary drop in cell-phone coverage, which can depend on weather conditions and the positioning of the cellphone tower signals.

⁸Cell phones for which issues were reported in the first months of the intervention were replaced.

⁹The follow survey was fielded a bit before the end of the text message intervention so that data collection could take place before the rainy season, when access can become challenging. The duration of exposure is similar to other early childhood interventions where impacts on ECD outcomes were found in nearby municipalities in Nicaragua (Macours et al., 2012). That said, about 2 percent of observations was collected later, during a short tracking phase in May 2016 when missing households were re-visited.

The child-level follow-up instrument includes a number of tests to assess early childhood development. First, the four sub-scales of the Denver Developmental Screening Test (Frankenburg and Dodds (1967)) were used to assess social-personal, language, fine motor, and gross motor skills for all children between 12 and 84 months of age. We use a modified version of the Denver previously used as part of a national early childhood stimulation program in Nicaragua.

Three additional tests were conducted for children between 36 and 84 months old: 1) The TVIP, the Spanish-speaking version of the Peabody Picture Vocabulary Test (PPVT) (Dunn et al., 2006); 2) The digit span, an associative numeric memory test; and 3) A version of the marshmallow test to measure self-control.¹⁰ The test was adapted to the local (humid) survey context by using hard red candy instead of a marshmallow.

The tests have been applied to similar populations in Latin America, including in previous studies in Nicaragua (Macours et al. (2012) and Barham et al. (2013)), Ecuador (Paxson and Schady (2007) and Paxson and Schady (2010)) and Mexico (Fernald et al. (2008)). All tests were extensively piloted in the field and adjusted when necessary. Test administrators were selected based on their background (trained as psychologists, social workers, or similar fields) and for their ability to quickly establish a strong rapport with young children. They were intensively trained on the standardized application of the tests, as well as on putting children at ease before starting the tests. Tests were administered at home. The privacy of the test-taker and the confidentiality of the results were ensured throughout the process. Test administrators were randomly assigned to households. The quality and standardized application of the tests was closely monitored in the field. A key advantage of the tests is that they provide observed and objective measures of child development, rather than parent-reported measures that may suffer from reporting biases.¹¹

The test scores are aggregated using principal component analysis to determine factor weights in the control group. We retain the first principal component as a summary

¹⁰The marshmallow test is a well-known test of delayed gratification, evaluating preschool-age children's ability to wait before eating a marshmallow in exchange for being rewarded an additional one. Mischel et al. (1989) show that the test predicts outcomes later in life.

¹¹Only a few items of the Denver are reported by caregivers.

indicator of early childhood cognitive development. The first component accounts for 32% of the overall variation in the 7 tests, and for 35% of the overall variation in the 4 sub-components of the Denver. Table 1.13 reports the factor weights, showing that cognitive development tests all have relatively high weights (Denver language, TVIP, memory, fine motor skills).

The aggregate cognitive score constitutes our main ECD outcome. In addition, socio-emotional skills are measured using the Strength and Difficulties questionnaire (SDQ) and a behavioral screening test consisting of questions to caregivers about a set of positive and negative behaviors for children 36 to 83 months old. These scales were complemented with questions on inhibitory control and positive demeanor. Another standardized set of age-appropriate behaviors was asked for children aged 18 to 35 months old. We consider socio-emotional measures as a secondary outcome. We analyze it separately because information self-reported by caregivers is sensitive to potential reporting biases, which may be exacerbated by the text message intervention. As for the cognitive score, we construct an aggregate index across the various socio-emotional development subscales. We use principal component analysis to determine factor weights in the control group (see Table 1.14).¹²

The text message intervention aimed to improve early childhood cognitive and socio-emotional development by changing parental investment behavior and attitudes related to ECD risk factors. To measure intermediate outcomes related to parenting practices, we construct seven indices on nutrition, protein intake, micro-nutrient intake, stimulation, health, caregiver attitudes regarding ECD, and hygiene. Each index is constructed using a set of questions about parental behaviors, ECD risk factors, and attitudes.¹³ The nutrition index is based on a set of questions measuring the number of days the child receives nutritious food during the week before the survey. The stimulation index aggregates questions on whether the caregiver gives toys to the child, reads or tells stories to the child, and whether there is pen and paper in the house for the child to draw. The health

¹²While some of the subscales have low or even negative weights, we restrict the analysis to the first principal component as for the cognitive outcome. For comparability, we also restrict the analysis to the sample of children with cognitive tests.

¹³We provide additional details on the questions used in each index when discussing related results in the next section.

index is based on twenty questions related to preventive health behaviors, including tooth brushing, hand washing, use of mosquito nets, vaccination, boiling water etc. The micro-nutrient index is based on three variables measuring whether the child has received vitamin A, ferrous sulfate, or de-worming medicine during the last six months. The protein index measures consumption of proteins (milk, eggs, cheese, meat) during the last week. The attitudes index is based on variables measuring the caregiver's opinion about ECD and stimulation practices. Finally, the hygiene index captures the condition of the child during the interview. In contrast with other intermediate outcomes, the hygiene index is directly observed by the enumerator. Each index score is standardized using the mean and standard deviation in the control group.

1.2.4 Balance and Attrition

The endline sample is balanced on baseline characteristics between treatment and control households (Table 1.1). It is also balanced between villages with and without opinion leaders assigned to treatment (Table 1.2). Table 1.1 provides relevant contextual information about the study setting. Both fathers and mothers have low levels of education, with mothers having completed 3.3 years of education on average, and fathers 3 years. 26 percent of household heads are illiterate. 76% of target children lived with their father at baseline, and 96% with their mother. Children on average consume meat and vegetables one to two days per week, and fruit and eggs about 3 days per week. By contrast, coffee consumption is high (four days per week). Only 22% of children had an adult read to them in the month before the baseline survey.

Attrition at follow-up is 13.8 percent for tests administered to the child, 10.4 percent for questions asked to the main caregiver, 12 percent for the mother-specific questions and 21 percent for father-specific questions.¹⁴ Attrition is balanced between treatment and control groups for the household and mother-reported questions, but there is a slight imbalance (significant at the 10%) for child-level outcomes (Table 1.15). Baseline observables are, however, similar for attrited observations in the treatment and control

¹⁴We do not use the father-specific questions given the relatively higher attrition among fathers.

groups for all outcomes (Table 1.16). This suggest that the profile of children and parents lost due to attrition are similar in the treatment and control groups. This is confirmed by the balance tests shown in Table 1.1. Moreover, attrition is not significantly different between households receiving different thematic contents (Table 1.15, panel B), and is also balanced between villages with and without opinion leaders' assigned to receive the text messages. Indeed, the coefficients for child and household level attrition in panel C of Table 1.15 are close to zero, suggesting attrition is not likely to bias the estimated effects of leaders' assignment to the text message intervention.

1.3 Empirical Specification

We estimate the following child-level intent-to-treat regression:

$$Y_{iv} = \alpha T_i + \beta X_i + \delta_v + v_{iv} \quad (1.1)$$

where Y_{iv} is the outcome for child i in village v ; T is an indicator denoting treatment assignment, which takes the value of one for children in households randomly assigned to receive the text message intervention; X is a set of control variables, including the stratification variables¹⁵ as well monthly age dummies, the gender of the child, and test administrator fixed effects. We also include a binary variable indicating whether data was collected during the tracking phase in 2016.¹⁶ δ_v is a village fixed effect, which controls for the village-level assignment of the leader treatment.

We also estimate intent-to-treat regressions to estimate the effects of randomized assignment to different treatment variations:

$$Y_{iv} = \alpha_1 T_{1i} + \alpha_2 T_{2i} + \alpha_3 T_{3i} + \beta X_i + \delta_v + v_{iv} \quad (1.2)$$

¹⁵The stratification variables include: the level of education of the main caregiver, whether there is a male caregiver in the household, whether it is a leader household, and whether it is a leader household with access to electricity.

¹⁶As indicated above, in May 2016 the data collection team tracked 63 households that were not found during the initial visit. The control variable helps account for the fact that the intervention had ended by then, and for the tests depending on the age of the child.

where T_1 , T_2 , and T_3 are indicators denoting children whose parents were randomized to receive text messages focused on nutrition, stimulation, respectively both nutrition and stimulation. We use a similar specification to estimate treatment effects on children's outcomes depending on whether the mother, the father or both parents were randomly selected to receive the text messages.

Spill-over effects from opinion leaders' exposure to the text message intervention are estimated through the following model:

$$Y_{iv} = \beta_1 L_v + \delta T_i + \beta_2 X_i + \epsilon_{iv} \quad (1.3)$$

Y_{iv} is the outcome for child i in village v . L_v takes the value of 1 in villages where opinion leaders were randomly assigned to receive text messages. Controls include the stratification variables used for the leader randomization (the level of education of the leader) and other household-level controls as in specification 1. To isolate spillover effects, equation (3) is estimated by excluding the households of the opinion leaders themselves. The coefficient of interest (β_1) captures the spill-over effects of being in a village where leaders receive text messages. Randomization took place at the village level and we cluster standard errors at the village level.

1.4 Results

1.4.1 Direct effects of text message intervention

The main results on the effect of the text message intervention on early childhood development are reported in Table 1.3. Panel A reports ITT estimates for the aggregate cognitive development index for all children 1-7 year old (column 1), as well as separately for children 1-3 year old (column 2), and 3-7 years old (column 3). The estimated impact on early childhood development is very small and not significantly different from zero.¹⁷ This result holds for the different age groups. Table 1.17 in appendix shows that this

¹⁷Given the small and insignificant effect of direct exposure to text messages on cognitive outcomes, using bounds to correct for the small attrition differential mentioned in section 2.4 would not change the findings.

also holds for the individual tests when they are analyzed separately. Table 1.18 shows consistent results across the treatment variations focusing on nutrition, stimulation or both, as well as depending on whether the intervention targets the mother, the father, or both. Tables 1.19 and 1.20 further show that there is no effect on the overall behavioral (socio-emotional) index in column 1, which holds for most of its individual components as well (in additional columns).

Panel B of Table 1.3 reports ITT estimates by level of education of the main caregiver. This shows a small negative coefficient of -0.12 standard deviations among children of the least educated caregivers (those with 3 or less years of education), significant at the 5% level (column 1). In contrast, interaction terms are positive for children whose caregiver completed primary school, though the treatment effect is only significant at the 10% level for the youngest children (with an effect size of 0.16 standard deviations). Finding more variations among children aged between 1 and 3 years old is in line with the literature suggesting higher malleability at a young age. It is also consistent with the hypothesis that interventions in early childhood can be particularly important for cognitive development, a key rationale to target this age group. Differences by education levels suggest that caregivers' ability to read and understand the messages could (intuitively) be important for the messages to be effective. There is no such heterogeneity, however, for older children for whom there is little impact overall.

Table 1.4 shows results for intermediate outcomes for all children between 12 and 83 months old. Positive ITT estimates are found for all intermediate outcomes, with magnitudes between 0.07 to 0.16 standard deviations. Results for individual questions used to construct the indices are reported in the Appendix Tables 1.21, 1.22, 1.23. Panel B of Table 1.4 reports estimated effects on the 7 intermediate outcome, separately for each (randomized) variation in the content of messages. The impacts on the indices for nutrition, micro-nutrients intake, proteins and hygiene are larger for households assigned to the nutrition and preventive health messages (though not significantly so). Impacts on stimulation and caregivers' attitudes toward ECD are significantly larger for those assigned to the stimulation and home environment messages. Overall these results show that changes in parental investments and practices broadly reflect the content of the text

messages that caregivers received. That said, none of the thematic variations lead to significant changes in early childhood development outcomes (as mentioned above in relation to Table 1.18).

1.4.2 Impact of opinion leaders' exposure to text message intervention

Table 1.5 reports the estimated β_1 coefficients from equation 1.3 on the main outcome, as in Table 1.3. Panel A shows the estimated effect of living in a village where opinion leaders were sent the text messages. It shows that opinion leaders receiving messages has a negative spillover effects on cognitive outcomes of children from other (nonleader) households in the same village. Children living in villages where the leader was assigned to treatment have a score on average 0.11 standard deviations lower than children from households in villages where leaders were not treated (column 1). While the spillover coefficient is very small and insignificant for the younger children, it is negative and highly significant for children between 3 and 7 years old (-0.14 standard deviations). When considering impacts on individual tests, Table 1.24 shows that the negative results are strongest for the two language scores (Denver language and the receptive vocabulary test), which are often considered the best proxies for cognition. On the other hand, Tables 1.19 and 1.20 show no significant impact of the leader treatment on socio-emotional outcomes.

Panel B reports the leader spillover effects on cognitive outcomes distinguishing by caregivers' level of education. Results show a negative spillover effect from leaders' exposure to text messages for all age groups among children whose caregivers have less than 4 years of schooling. Children with the least educated parents hence appear the most affected by the negative leader effect. That said, for the older children, and for all children together, the effect is also negative for those whose parents completed primary education (p-value for the joint significance test for the high educated group is 0.05). Similarly consistent with this last result, when distinguishing by education levels of the leaders, Table 1.25 shows that the negative effects are found for leaders with the lowest and the highest education levels.

Exposure of opinion leaders to parenting information by text messages leads to a deterioration of early childhood outcomes among children in their village. As such, the effects of opinion leaders' exposure to text messages go in the opposite direction than anticipated, and suggest a possible negative influence of these opinion leaders. Since this is arguably a surprising finding, we use an alternative specification to test its robustness. We define a variable measuring the number of dwellings between each household and the closest opinion leader. The variable captures physical distance to opinion leaders, which can be used to test whether the negative leader effects are driven by households that are closer to the leaders. Of course, the variable could also capture remoteness more generally (if opinion leaders live in more central locations), or social distance (if, for instance, members of the same extended families live closer to each other), among other factors. Even so, the interaction effect between the distance variable and the leader treatment provides a useful check about the plausibility that the negative experimental leader effects comes from exposure to those opinion leaders. The interaction effect in Table 1.6 (panel A) shows that the negative leader effect is indeed stronger for households living close to the leaders, and weakens as distance increases (column 1). This effect is particularly strong for younger children (column 2). For them, the significant interaction term indicates that the negative leader spillover effect disappears for households living 6 or more dwellings away from the leader's house.¹⁸

Panel B further shows that the negative leader effect is particularly strong for children from households that do not receive text messages themselves (-.25 standard deviations for the older children). Possibly this is because it was not offset by any positive direct treatment effect. For older children, the interaction between household level treatment and leader spillover is positive and significant at 10%, suggesting that negative effects of leaders' exposure may be weaker for those receiving text messages. Nevertheless, the

¹⁸Appendix Tables 1.26 and 1.27 further test whether the negative leader effects are stronger for households who had more social interactions about parenting practices at baseline. At baseline, households were asked whether they had talked to different types of members in the community in the last 7 days about ECD practices. We measured in particular whether anybody in the household had any interaction about ECD practices with the health promoter, pre-school teacher, primary school teacher, other teachers, family members, neighbours, or elected leaders in the village. About half of the households had talked to at least one other community member about ECD practices. Results in Tables 1.26 and 1.27 show that there is no clear heterogeneity by this indicator of baseline social interactions.

joint significance test reported at the bottom of the table shows that the negative leader effect is also significant for households who received the text messages. For younger children, results suggest no significant difference in the leader effects between treatment and control households.

Table 1.7 (panel A) reports estimated coefficients from equation 1.3 for the seven intermediate outcome indices. Negative leader effects are found for several of the indices and related ECD risk factors, specifically in the domains of nutrition, micronutrients and proteins (see columns 1, 4, 5). This highlights a likely channel through which leaders' exposure can negatively influence parenting practices and contribute to negative impacts on ECD outcomes. Table 1.7 (panel B) reports results on the same outcomes by the caregivers' education level but shows no clear pattern of heterogeneity.

1.5 Mechanisms

The negative effect of opinion leaders' exposure to the text message intervention on young children's development outcomes and (some) parental practices goes against the initial hypothesis motivating this experimental variation. The experimental design does not allow to causally identify the underlying mechanism. Still, given its potential relevance for the design of similar information interventions, we attempt to further unpack this result, acknowledging that the analysis is more speculative in nature.

This section considers a number of possible explanations. First, confusion: because of the design, leaders received a different text message than many of the other parents close to them. Potentially, this could have led to confusion when they exchanged information. Second, boycott: leaders' prior beliefs about optimal parenting practices may not have coincided with the text messages, and leaders receiving the messages may have attempted to offset them. And third, crowding-out: local leaders may have felt delegitimized or demotivated by the text message intervention, which directly interfered with their sphere of influence. Or parents might have decreased their interactions with leaders now that they obtained information directly through text messages.¹⁹

¹⁹Another mechanism may be competition, whereby leaders' receiving text messages favored advancing

1.5.1 Confusion

Within each village, households received various types of text messages. This is because of the randomized variation in content, but also because different age-appropriate messages were sent to households with children in different age groups. Very often, opinion leaders therefore received a different text message than other households living nearby. For instance, on a given day, an opinion leader may have received a text message about nutrition of her 12 month old child, while a household in her proximity received a message on stimulation of her 4 year old child. To the extent that opinion leaders engage with households in their villages based on the messages they receive, this could have caused confusion, possibly especially among the least educated parents.

We investigate this hypothesis by analyzing whether ECD outcomes are better in cases when parents and the closest leader received the same message (compared to those who received a different message). The point estimate of the difference between those two groups is very small, -0.00 (s.e. 0.09), hence providing no evidence in support of the confusion hypothesis. Note, however, that given the large number of potential combinations of text messages between leader and non-leader households, there are relatively few pairs of leader and non-leader households who received the same text message (80 out of a total 1004 pairs, of which 71 are children between 36 and 84 months), so that the comparison may be underpowered. Even so, the precisely estimated zero suggests that confusion is unlikely to be a major driver of the negative leader results.

their own children's outcomes at the expense of other children. For instance, leaders may have reallocated resources (such as time) to their own children at the cost of their responsibilities in the community. To analyze whether such competitive motives contribute to the observed negative effects, we analyze treatment effects on leaders' children. In absence of the intervention, leaders' children have better ECD outcomes (Table 1.31). This is in line with the higher education levels and general status of opinion leaders in the communities. However, Table 1.31 shows that there is no significant treatment effect on leaders' children. Table 1.32 further shows that changes in intermediate outcomes are not significantly different between leader and non-leader households. (Proteins are an exception, with a negative and significant interaction between the treatment and leader). Overall, changes in behavior appear more limited for leaders, possibly because leaders engage more in some of the practices irrespective of the intervention. While these results are an imperfect test of a competition hypothesis, the lack of impact on leaders' practices and their children's outcomes suggest that leaders exposed to text messages did not increase investments in their own children. This makes it unlikely that the negative effect on non-leaders' children resulted from a deliberate effort by leaders to favor their own children.

1.5.2 Boycott

Another potential explanation for the negative leader effect could be that opinion leaders have strong prior beliefs about parenting practices, and may tell people not to believe messages if they go against their own beliefs. The baseline survey includes a set of five questions about caregivers' beliefs regarding early childhood development, in particular on stimulation and the home environment. The baseline questions about beliefs are similar to those used at follow-up to capture attitudes toward early childhood development. The baseline scores at those questions show that half the leaders disagree with 2 or more of the 5 messages about attitudes toward early childhood development. Table 1.8 and 1.9 test for heterogeneity in the leader effect based on the baseline attitudes toward ECD of the closest leader. The interaction term indicates whether the leader effect is different for households living close to leaders whose prior opinions were largely in line with the stimulation and home environment text messages (i.e. they agreed with 4 out of 5 promoted messages, and we hence label them as being "agreeing" leaders). With the exception of the intermediary outcome on micro-nutrients, interaction effects are not significant. For the early childhood development outcomes, they go in the direction of the boycott hypothesis for the younger children, but not for the older ones.

Unfortunately, there are no baseline measures of leaders' beliefs about nutrition and health practices, which are the risk factors where the negative leader effects appear to be the strongest (in Table 1.7). Qualitative evidence, however, does suggest that some of the strongest opinions against the text messages were expressed about early childhood nutritional practices going against traditional beliefs. Taken together, the evidence does not allow to rule out the boycott hypothesis.

To complement this analysis, we also explore whether the negative opinion leader effect varies by leaders' predicted commitment or engagement with respect to the text message intervention. We use the frequency of leaders' participation in the quizzes as a proxy for leaders' interest in the text message intervention.²⁰ Of course we do not have

²⁰By this measure, opinion leaders do not appear to be more committed to the intervention than other households: there is no significant difference in the number of quizzes that leader households responded to (22 quizzes) compared to households without leaders (21 quizzes).

such a proxy for leaders in the control group. We therefore predict the frequency of quizz participation for leaders randomly assigned to treatment, using a Lasso estimation on baseline observables.²¹ We obtain a relatively precise prediction (with a R^2 of 0.65) and define "committed" leaders as those who are predicted to be in the top 20% of quizz participation. This threshold corresponds to leaders participating approximately half of the time. We can then analyze whether the leader effect differs depending on the predicted commitment of the closest leader to each non-leader household.²²

Table 1.10 shows that negative leader effects in column 1 and 3 are attenuated for children from households in proximity of committed leaders. Although the interaction term is not significant, the overall leader effect on ECD outcomes for those children is not significantly different from zero (bottom row of the table). That pattern is confirmed in Table 1.11, which shows the same specification for the intermediate outcomes. There are no significant leader effects for households in proximity of committed leaders for 6 out of the 7 outcomes (the micro-nutrient index is an exception). Moreover the point estimates indicate that the leader effects for households whose closest leader is committed to the program are in fact positive for stimulation, health, and attitudes, though none of these effects are significant.²³ While a high predicted program participation may be capturing many different characteristics of the leaders (and those living in their proximity), this heterogeneity analysis at least provides suggestive evidence that the negative effects of opinion leaders are driven by leaders that were less likely to actively engage with the program. This result could be seen as consistent with the boycott hypothesis. It is also consistent with a de-legitimization or demotivation of leaders inducing a crowding-out of interaction with leaders, a mechanism we now consider in more details.

1.5.3 Crowding out

Another potential mechanism is that the text message intervention crowded out local leaders. On the one hand, the text message intervention may have been perceived by local

²¹See appendix for details, including a list of covariates used in the prediction model.

²²We consider heterogeneity based on the closest leader as the prediction is done for each leader separately (and hence varies within a community). The closest leader is defined as in section 4.2.

²³Appendix Table 1.33 and 1.34 show results for each quintile of predicted participation.

opinion leaders as demotivating or de-legitimizing them because it directly interfered with their sphere of influence. If such crowding out led to less interactions between parents and local opinion leaders, parents may have missed out on advice tailored to the specific needs of their children. This may have contributed to the negative effects observed in Table 1.5. By demotivating the service providers, it may also have lowered the quality of education or basic health services in the community. On the other hand, the direct provision of information to parents may have decreased the need for parents to seek guidance from local leaders through direct interactions with them.

Table 1.12 shows that leaders' randomized exposure to text messages led to significantly lower interactions about ECD practices between opinions leaders and parents. Importantly, there is no direct effect of direct exposure to the text messages on interactions between caregivers and leaders. This suggests that caregivers themselves did not decrease their consultations with local leaders after receiving parenting information through text message. Instead, the effects of leaders' exposure to the text messages on interactions about ECD interactions is negative for all types of leaders. It is significant for primary school teachers, other educators and community leaders. This is broadly consistent with the results indicating that the negative leader effects were also observed for the most educated leaders. Moreover, a comparison with Table 1.35 shows that these negative effects on social interactions are limited to opinion leaders that were directly targeted with the text messages. There are no significant reductions in social interactions about ECD practices with family or neighbours, and possibly even a slight increase in interaction with religious leaders. While the data does not allow analyzing local service delivery or the nature of interactions between leaders and parents in more details, these results suggest that opinion leaders' exposure to the text messages led to a crowding out of their direct interactions with parents of young children in the community. The evidence is also consistent with the decrease of interaction originating from leaders rather than caregivers.

1.6 Concluding remarks

A large share of children in developing countries suffer from signs of cognitive delays, which start at a very young age and affect their lifelong prospects. Experimental evidence has shown that interventions aiming to improve parental investments and practices can positively impact cognitive and socio-emotional development in early childhood. Growing evidence has also demonstrated that early investments can lead to longer-term gains in terms of education, earnings and social outcomes. In light of this evidence, there is growing interest in interventions that seek to improve parenting practices, with major question on whether they can be delivered cost-effectively at scale. Text message interventions offer an appealing alternative to more intensive approaches such as home visiting programs, especially at times when personal interactions are not possible due to limited accessibility or social distancing requirements.

This paper presents results from the RCT of an intervention sending daily text messages on parenting practices to caregivers in poor households in rural Nicaragua. The program enhanced knowledge and improved some parental practices associated to nutrition and stimulation. However, no improvements were found on children's cognitive or socio-emotional outcomes. These results contrast with those from more intensive interventions improving parental investment and early child development in Nicaragua and elsewhere in the developing world. Importantly, the results are obtained from a context where cell phone coverage and access to electricity is far from universal, where low levels of education may have limited parents' ability to fully internalize the text messages and where few households were covered by a government-led ECD program. These contextual factors can help explain the lack of more positive results, and are worth considering when contemplating the external validity of our findings. At the same time, lack of electricity, limited cell phone ownership, low coverage of ECD programs, and low literacy are common in many parts of the developing world.

The second main result of this paper is that opinion leaders' randomized exposure to the text message intervention led to a significant decline in cognitive outcomes among children from non-leader households. These negative spill-over effects are stronger for

children from the least educated parents, and for those living closest to the local opinion leaders. We explore potential mechanisms that may explain this negative effects of opinion leaders' exposure to text messages. While the experimental design does not allow us to fully disentangle the underlying mechanisms, empirical results point to either a boycott of the intervention by opinion leaders or a crowding-out of their interactions with other households. A decrease in interactions about ECD practices between leaders and parents is observed. Interestingly, it stems from leaders and may be driven by their demotivation or de-legitimization, possibly leading to reduced efforts in the provision of ECD information or services.

Overall, the lack of impacts of text messages to parents on ECD outcomes and the negative effects of leaders' exposure to the text messages call for caution before advocating for the large-scale roll-out of text message parenting interventions in high-poverty settings. Such interventions may need to be complemented with other types of ECD programs. And they may need to explicitly incorporate strategies to crowd-in local opinion leaders, for instance by giving them an active role in implementation. These hypotheses would deserve to be tested in future research.

1.7 Appendix

FIGURE 1.1: Number of treated individuals participating in weekly quizzes over time

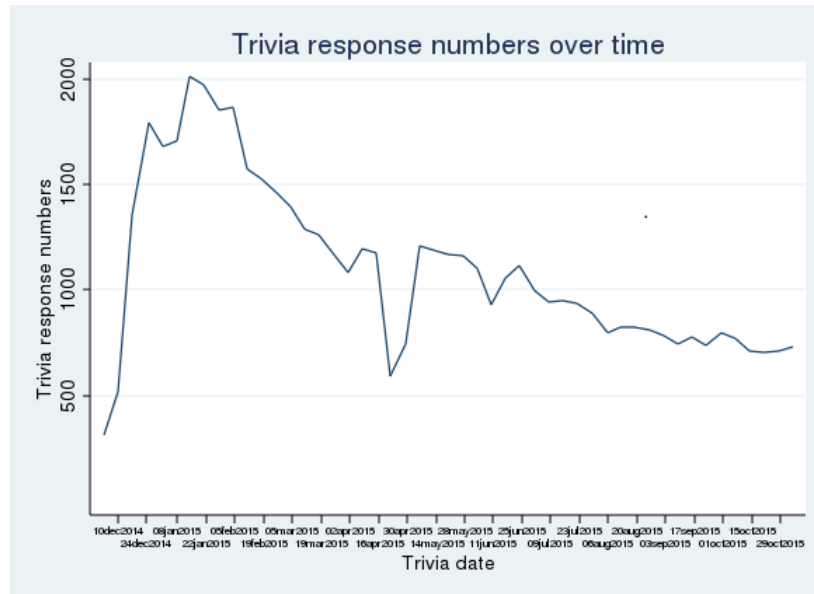


TABLE 1.1: Balance in baseline characteristics between treatment and control groups

	N	Control	Treatment	P-values with stratification (T - C)	P-Values Unconditional (T - C)
Child-specific characteristics					
Age at baseline	2,416	2.27	2.32	0.533	0.457
Father lived in household at baseline	2,416	0.76	0.75	0.770	0.535
Mother lived in household at baseline	2,416	0.96	0.96	0.916	0.969
Years education mother	2,414	3.26	3.27	0.468	0.889
Years education father	2,291	3.03	3.00	0.634	0.689
Health and Nutrition					
# Days in bed sick in last month	2,416	0.19	0.22	0.638	0.631
Received deworming in last 6 months	2,416	0.39	0.38	0.738	0.769
Received vitamins in last 6 months	2,416	0.39	0.38	0.786	0.801
# Days in the week the child had vegetables	2,415	1.71	1.88	0.068	0.074
# Days in the week the child had fruit	2,415	2.88	3.02	0.240	0.241
# Days in the week the child had meat	2,415	1.20	1.17	0.702	0.713
# Days in the week the child had eggs	2,414	2.69	2.64	0.689	0.653
# Days in the week the child had breast milk	2,414	2.46	2.45	0.936	0.972
# Days in the week the child had coffee	2,414	4.35	4.06	0.042	0.052
# Days in the week the child had soup	2,415	0.50	0.61	0.073	0.069
# Days read to the child in the last week	2,415	0.76	0.67	0.256	0.220
Someone read to the child in the last month	2,415	0.22	0.22	0.924	0.844
Household-level characteristics					
Male household head	2,512	0.86	0.86	0.443	0.827
Age household head	2,512	41.39	41.25	0.759	0.825
Literate household head	2,512	0.74	0.74	0.756	0.898
Number of men in the household	2,512	1.58	1.56	0.914	0.720
Number of women in the household	2,512	1.66	1.61	0.311	0.205
Number of boys (age 7-14) in the household	2,512	0.57	0.52	0.106	0.101
Number of girls (age 7-14) in the household	2,512	0.49	0.47	0.415	0.444
Number of young boys (age 0-6) in the hh	2,512	0.66	0.64	0.070	0.466
Number of young girls (age 0-6) in the hh	2,512	0.60	0.57	0.284	0.286
Number of rooms in house	2,511	1.91	1.84	0.139	0.111
Network					
Distance (in min) to closest cellphone signal	2,505	32.48	29.65	0.193	0.233
Distance (meters) to closest cellphone signal	2,343	2,116.53	1,979.93	0.832	0.478

Baseline survey characteristics of children and households who were visited in the following up survey. The sample includes all baseline children with cognitive tests at follow-up and all households interviewed at follow-up. Children born after the baseline are not included. P-values are of the test of difference between control and treatment, after controlling for stratification variables (column before last), or without any controls (i.e. unconditional, last column). P-values based on standard errors clustered by community.

TABLE 1.2: Baseline characteristics of villages with leaders assigned to text message intervention (treatment) or not (control)

	N	Control	Treatment	P-Values (T - C)
Village level characteristics				
Average number of leaders	92	4.59	4.23	0.557
Average number of households	92	34.81	30.22	0.277
Average age of the targeted children	92	2.36	2.41	0.509
Average number of households with fathers living in the household	92	24.59	22.11	0.401
Average number of households with mothers living in the household	92	32.30	27.94	0.263
Average number of households in the government ECD program	92	4.78	4.69	0.952
Average number of households with access to electricity	92	19.41	15.69	0.457
Avg years of education fathers	92	2.78	2.64	0.331
Avg years of education mothers	92	3.14	3.01	0.352

The treatment group is composed of villages where opinion leader households were selected to receive text messages, the control group is composed of villages where opinion leader households were not selected to receive text messages. Five villages without opinion leaders in the sample are not included.

TABLE 1.3: Impact of text messages on Early Childhood Development outcomes

	(1) ECD full sample	(2) ECD young	(3) ECD Old
<i>Panel A: Intention to treat</i>			
ITT	0.00 (0.03)	-0.01 (0.06)	0.00 (0.04)
<i>Panel B: Heterogeneity by caregiver education</i>			
ITT	-0.12** (0.06)	-0.24* (0.13)	-0.07 (0.07)
ITT X Medium educ (4-6 years of education)	0.17** (0.08)	0.18 (0.16)	0.13 (0.09)
ITT X High educ (more than primary education)	0.18** (0.08)	0.41*** (0.15)	0.04 (0.09)
Observations	2485	774	1711
<i>P-values joint significance test</i>			
ITT + ITT X Medium educ = 0	0.35	0.48	0.22
ITT + ITT X High educ = 0	0.27	0.07	0.74

Note: All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old, and the first principal component of all 7 tests for children 36-84 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Standard errors reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.4: Impact of text messages on intermediate outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Nutrition	Stimulation	Health	Micronutrients	Proteins	Attitudes	Hygiene
<i>Panel A. ITT</i>							
ITT	0,06 (0,04)	0,16*** (0,04)	0,07* (0,04)	0,08* (0,04)	0,06 (0,04)	0,11** (0,04)	0,09** (0,04)
<i>Panel B. By treatment variation (thematic focus)</i>							
Nutrition & Stimulation	0,01 (0,05)	0,20*** (0,05)	0,11** (0,05)	0,05 (0,05)	0,04 (0,05)	0,16*** (0,06)	0,08 (0,05)
Nutrition	0,10** (0,05)	0,05 (0,05)	0,00 (0,05)	0,13** (0,05)	0,07 (0,05)	-0,02 (0,06)	0,09* (0,05)
Stimulation	0,08 (0,05)	0,23*** (0,05)	0,08 (0,05)	0,03 (0,06)	0,06 (0,05)	0,19*** (0,06)	0,10* (0,05)
Observations	2500	2501	2512	2495	2501	2396	2506
P-values Statistical Significance Test							
Nutrition & Stimulation = Nutrition	0.14	0.00	0.02	0.15	0.52	0.00	0.94
Nutrition & Stimulation = Stimulation	0.25	0.58	0.10	0.81	0.70	0.56	0.66
Stimulation = Nutrition	0.76	0.00	0.57	0.10	0.80	0.00	0.71

Note: All outcome variables are standardized using the mean and standard deviation of the control households. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. Impacts on each of the individual items used to construct the indices are reported in tables 1.21, 1.22, 1.23. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.5: Impact of leaders' exposure on Early Childhood Development outcomes

	(1) ECD full sample	(2) ECD young	(3) ECD Old
<i>Panel A ITT of leaders' exposure</i>			
ITT Leader	-0.11*** (0.04)	-0.02 (0.05)	-0.14*** (0.05)
<i>Panel B Heterogeneity by caregiver education</i>			
ITT Leader	-0.20*** (0.06)	-0.22* (0.12)	-0.18*** (0.07)
ITT Leader X Medium educ (4-6 years of education)	0.17** (0.07)	0.35** (0.16)	0.09 (0.08)
ITT Leader X High educ (more than primary education)	0.08 (0.07)	0.17 (0.14)	0.01 (0.10)
Observations	2051	624	1427
<i>P-values joint significance test</i>			
ITT Leader + ITT Leader X Medium educ = 0	0,58	0,20	0,13
ITT Leader + ITT Leader X High educ = 0	0,03	0,49	0,05

Note: Sample includes only non-leader households in 92 villages with opinion leaders. Five villages without leaders were excluded from the estimation. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, child age and gender, and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.6: Impact of leaders' exposure by distance to the closest leader's dwelling

	(1) ECD full sample	(2) ECD young	(3) ECD Old
<i>Panel A: Leader treatment interacted with distance from closest leader</i>			
ITT Leader	-0.168*** (0.05)	-0.126* (0.07)	-0.195*** (0.07)
Distance from closest leader	-0.009*** (0.00)	-0.017*** (0.00)	-0.006 (0.00)
ITT leader X distance from closest leader	0.012*** (0.00)	0.022*** (0.01)	0.010* (0.01)
<i>Panel B: Leader treatment interacted with household treatment status</i>			
ITT	-0.046 (0.06)	0.036 (0.13)	-0.112* (0.06)
ITT Leader	-0.151** (0.06)	0.029 (0.13)	-0.251*** (0.07)
ITT X ITT Leader	0.060 (0.07)	-0.071 (0.15)	0.150* (0.08)
Observations	2051	624	1427
<i>P-values joint significance test</i>			
ITT Leader + ITT Leader X ITT	0,04	0,51	0,07

Note: Distance from leader is measured in terms of the number of houses between the household and the closest household with an opinion leader. Sample includes only non-leader households in 92 villages with opinion leaders. Five villages without leaders were excluded from the estimation. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender, and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.7: Impact of leaders' exposure on intermediate outcomes

	(1) Nutrition	(2) Stimulation	(3) Health	(4) Micronutrients	(5) Proteins	(6) Attitudes	(7) Hygiene
<i>Panel A: ITT of leaders' exposure to text messages</i>							
ITT Leader	-0,14* (0,08)	0,02 (0,06)	-0,00 (0,04)	-0,16** (0,06)	-0,23** (0,09)	-0,00 (0,06)	-0,11* (0,06)
Observations	2062	2062	2073	2059	2062	1970	2068
<i>Panel B: Heterogeneity by caregiver education</i>							
ITT Leader	-0,01 (0,12)	0,10 (0,11)	-0,01 (0,07)	-0,23*** (0,09)	-0,18* (0,09)	0,00 (0,10)	-0,08 (0,09)
ITT Leader X Medium educ (4-6 years of education)	-0,04 (0,10)	-0,04 (0,12)	-0,01 (0,08)	0,08 (0,09)	0,03 (0,09)	0,04 (0,11)	-0,05 (0,10)
ITT Leader X High educ (more than primary education)	-0,34*** (0,13)	-0,21 (0,13)	0,04 (0,10)	0,10 (0,10)	-0,20 (0,12)	-0,06 (0,13)	-0,04 (0,10)
Observations	2062	2062	2073	2059	2062	1970	2068
<i>Joint significance test</i>							
ITT Leader + ITT Leader X Medium Educ	0,58	0,46	0,66	0,08	0,12	0,61	0,16
ITT Leader + ITT Leader X High Educ	0,00	0,12	0,67	0,16	0,01	0,60	0,13

Note: Sample includes only non-leader households in 92 villages with opinion leaders. Five villages without leaders were excluded from the estimation. All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. Impacts on each of the individual items used to construct the indices are reported in Tables 1.28, 1.29, 1.30. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.8: Impact of leaders' exposure on ECD outcomes, by leaders' prior beliefs

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT Leader	-0.08* (0.05)	-0.09 (0.06)	-0.08 (0.05)
Agreeing leader	0.07 (0.07)	-0.10 (0.09)	0.15* (0.08)
ITT leader X Agreeing leader	-0.06 (0.08)	0.16 (0.11)	-0.16 (0.10)
Observations	2051	624	1427
ITT Leader + ITT leader X Agreeing leader = 0	0.04	0.44	0.01

Note: Agreeing leader is a binary variable indicating that the closest leader agreed with at least 4 out of 5 program messages on parenting practices at baseline. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old, and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.9: Impact of leaders' exposure on intermediary outcome,s by leaders' prior beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Nutrition	Stimulation	Health	Micronutrients	Proteins	Attitudes	Hygiene
ITT Leader	-0.12 (0.09)	0.06 (0.06)	-0.01 (0.04)	-0.25*** (0.07)	-0.25*** (0.09)	0.01 (0.07)	-0.10 (0.08)
Agreeing leader	0.09 (0.09)	0.11 (0.09)	0.07 (0.06)	-0.11 (0.10)	0.06 (0.10)	0.01 (0.08)	0.06 (0.10)
ITT leader X Agreeing leader	-0.04 (0.11)	-0.12 (0.11)	0.01 (0.07)	0.22* (0.12)	0.05 (0.12)	-0.03 (0.10)	-0.02 (0.12)
Observations	2062	2062	2073	2059	2062	1970	2068
ITT Leader + ITT leader X Agreeing leader = 0	0.12	0.58	0.96	0.77	0.11	0.82	0.19

Note: Agreeing leader is a binary variable indicating that the closest leader agreed with at least 4 out of 5 program messages on parenting practices at baseline. All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.10: Impact of leaders' exposure on ECD outcomes, by closest leader's commitment

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT Leader	-0,12** (0,05)	-0,01 (0,07)	-0,17*** (0,06)
Committed leader	-0,01 (0,05)	0,05 (0,13)	-0,04 (0,05)
ITT Leader X Committed leader	0,08 (0,09)	-0,03 (0,17)	0,12 (0,09)
Observations	2051	624	1427
ITT Leader + ITT Leader X Committed leader	0,56	0,74	0,59

Note: Committed leader is a leader predicted to be in the top quintile of leaders with highest participation in the quizzes. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 and 35 month. In column 3, the dependent variables is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level.* p<0.1, ** p<0.05, *** p<0.01

TABLE 1.11: Impact of leaders' exposure on intermediary outcomes, by closest leader's commitment

	(1) Nutrition	(2) Stimulation	(3) Health	(4) Micronutrients	(5) Proteins	(6) Attitudes	(7) Hygiene
ITT Leader	-0.16* (0.08)	-0.01 (0.06)	-0.02 (0.04)	-0.13* (0.07)	-0.25** (0.10)	-0.05 (0.06)	-0.13* (0.07)
Committed leader	-0.15 (0.12)	-0.16 (0.11)	-0.06 (0.07)	0.19** (0.08)	-0.10 (0.10)	-0.25*** (0.08)	-0.15 (0.13)
ITT leader X Committed leader	0.09 (0.17)	0.08 (0.14)	0.10 (0.09)	-0.11 (0.11)	0.11 (0.14)	0.23* (0.12)	0.06 (0.15)
Observations	2062	2062	2073	2059	2062	1970	2068
ITT Leader + ITT Leader X Committed leader	0.67	0.61	0.38	0.02	0.26	0.14	0.65

Note: Committed leader is a leader predicted to be in the top quintile of leaders with highest participation in the quizzes. All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.12: Impacts on social interactions about ECD with targeted leaders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	# leaders	Health promotor	Pre-sch Teacher	Primary Teacher	Other Educator	Local leader	Other com. leader
ITT	-0.01 (0.05)	0.02 (0.02)	-0.04 (0.02)	-0.01 (0.02)	-0.01 (0.01)	0.01 (0.01)	0.00 (0.01)
ITT Leader	-0.18** (0.07)	-0.03 (0.03)	-0.03 (0.02)	-0.07*** (0.02)	-0.02** (0.01)	-0.02 (0.02)	-0.02** (0.01)
Observations	2073	2073	2073	2073	2073	2073	2073
Mean Control	0.89	0.13	0.33	0.24	0.05	0.10	0.03

Note: The dependent variables are the social interactions between the household and targeted opinion leaders during the previous week. Column 1 is an index accounting for the social interaction between the household and health promoters, preschool-teachers, primary school teachers, other teachers, the local leader or other community leader. The dependent variables in the remaining columns are binary and indicate whether the household had at least one interactions with each type of leader during the previous week. All regressions include controls for the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects, as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

1.7.1 Supplementary tables

TABLE 1.13: Principal component analysis: ECD (cognitive) outcomes

	(1) Variance	(2) Proportion	(3) Factor loadings
Denver subcomponents (used for children 1-3 years old)	1.390	0.3476	
Personal - Social			0.385
Language			0.651
Fine Motor			0.675
Gross Motor			0.601
All tests together (used for children 3-7 years old)	2.268	0.324	
Denver Personal - Social			0.100
Denver Language			0.744
Denver Fine Motor			0.595
Denver Gross Motor			0.486
Vocabulary (TVIP)			0.754
Memory			0.735
Self-control			0.100

Note: Column 1 reports the eigenvalue of the first principal component. Column 2 reports the proportion of variance accounted for by the first principal component. Column 3 reports the principal component factor loadings, which show the correlation between the variables and the first principal component.

TABLE 1.14: Principal component analysis: socio-emotional outcomes

	(1) Variance	(2) Proportion	(3) Factor loadings
Behavioral (used for children 1-3 years old)	1.320	0.440	
Control			0.504
Affect			0.761
Ability			0.697
Strengths and Difficulties (used for children 3-7 years old)	1.718	0.245	
Emotional			0.687
Conduct			0.783
Hyperactivity			0.124
Peer			0.733
Pro-social			-0.075
Control			0.262
Laugh			-0.059

Note: Column 1 reports the eigenvalue of the first principal component. Column 2 reports the proportion of variance accounted for by the first principal component. Column 3 reports the principal component factor loadings, which show the correlation between the variables and the first principal component.

TABLE 1.15: ITT effects on attrition

	(1) Child	(2) Household	(3) Mother	(4) Father
Panel A				
ITT	0.022* (0.013)	0.015 (0.012)	0.010 (0.014)	0.016 (0.015)
Observations	2803	2803	2803	2803
Mean control	0.089	0.071	0.115	0.199
Panel B				
Nutrition & Stimulation	0.018 (0.017)	0.012 (0.015)	0.000 (0.018)	0.006 (0.019)
Nutrition	0.039** (0.017)	0.025* (0.015)	0.029 (0.018)	0.035* (0.019)
Stimulation	0.017 (0.017)	0.009 (0.015)	0.015 (0.018)	0.025 (0.019)
Observations	2803	2803	2803	2803
Mean control	0.089	0.071	0.115	0.199
P-values Statistical Significance Test				
Nutrition & Stimulation = Nutrition	0.261	0.389	0.136	0.144
Nutrition & Stimulation = Stimulation	0.952	0.890	0.460	0.362
Stimulation = Nutrition	0.245	0.325	0.474	0.610
Panel C				
ITT Leader	-0.004 (0.017)	-0.001 (0.015)	-0.010 (0.021)	-0.013 (0.016)
Observations	2332	2332	2332	2332
Mean Control	0.105	0.082	0.125	0.219

Note: The dependent variable measures the probability of attrition for children's test outcomes (column 1), and information obtained from the main caregiver in the household (2), mothers (3) and fathers (4). Coefficients in panel A and B are estimated on the full sample of children less than 7 years old at baseline using equation 3.1 and equation 1.2 (including controls for the stratification variables, child age and gender, enumerator fixed effects, as well as village fixed effects). Coefficients in panel C are estimated on children less than 7 years old from non-leader households only using equation 1.3 (including controls for the household-level treatment, the average level of education of the leaders, as well as the level of education of the main caregiver, whether there is a male caregiver in the household, child age and gender, and test-administrator fixed effects). The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.16: Balancing test on attrited observations

	N	Control	Treatment	P-values with stratification (T - C) with controls	P-Values Unconditional (T - C)
Children without test-results at follow-up					
Age at the base line	274	2.23	2.09	0.228	0.464
# Days in bed over last month	274	0.17	0.13	0.430	0.678
Received deworming drugs over last 6 months	274	0.45	0.38	0.545	0.303
Received vitamins over last 6 months	274	0.52	0.38	0.033	0.056
# Days in the week the child had vegetables	274	1.81	1.60	0.317	0.483
# Days in the week the child had fruit	274	2.77	2.69	0.962	0.842
# Days in the week the child had meat	274	1.36	1.31	0.738	0.832
# Days in the week the child had eggs	274	2.42	2.66	0.632	0.507
# Days in the week the child was breast fed	274	2.53	2.43	0.475	0.838
# Days in the week the child had coffee	274	3.52	4.00	0.329	0.313
# Days in the week the child had soup	274	0.77	0.47	0.025	0.057
# Days read to the child in the last week	274	0.88	0.59	0.248	0.190
Someone read to the child in the last month	274	0.22	0.21	0.877	0.875
Attrited households					
Male household head	223	0.93	0.88	0.949	0.363
Age household head	223	41.33	41.18	0.980	0.953
Literacy household head	223	0.78	0.76	0.714	0.828
Number of men in the household	223	1.56	1.59	0.657	0.826
Number of women in the household	223	1.63	1.64	0.894	0.947
Number of boys (age 7-14) in the household	223	0.31	0.46	0.283	0.159
Number of girls (age 7-14) in the household	223	0.43	0.43	0.693	0.999
Number of young boys (age 0-6) in the hh	223	0.59	0.62	0.662	0.797
Number of young girls (age 0-6) in the hh	223	0.50	0.59	0.886	0.360
Number of rooms in the house	223	2.07	1.69	0.086	0.010
Attrited mothers					
Mother lived in household at baseline	340	0.02	0.03	0.866	0.668
Years education mother	287	1.21	1.09	0.860	0.554
Male household head	340	0.89	0.84	0.602	0.268
Age household head	340	43.73	42.31	0.355	0.477
Literacy household head	340	0.75	0.76	0.282	0.882
Number of males in the household	340	1.64	1.57	0.493	0.628
Number of women in the household	340	1.76	1.75	0.445	0.906
Number of boys (7-14) in the household	340	0.44	0.43	0.733	0.901
Number of girls (7-14) in the household	340	0.44	0.39	0.239	0.519
Number of young boys (0-6) in the hh	340	0.73	0.66	0.697	0.386
Number of young girls (0-6) in the hh	340	0.49	0.56	0.884	0.334
Number of rooms in house	340	2.11	1.79	0.048	0.012
Attrited fathers					
Father lived in household at baseline	619	0.01	0.01	0.849	0.977
Years education father	481	0.97	1.09	0.791	0.489
Male household head	620	0.59	0.63	0.184	0.455
Age household head	620	43.13	43.93	0.682	0.594
Literacy household head	620	0.74	0.74	0.246	0.910
Number of men in the household	620	1.39	1.40	0.701	0.884
Number of women in the household	620	1.82	1.88	0.766	0.559
Number of boys (age 7-14) in the household	620	0.52	0.49	0.399	0.625
Number of girls (age 7-14) in the household	620	0.37	0.46	0.448	0.141
Number of young boys (age 0-6) in the hh	620	0.73	0.68	0.120	0.398
Number of young girls (age 0-6) in the hh	620	0.52	0.56	0.815	0.473
Number of rooms in house	620	2.13	1.81	0.017	0.001

All data from 2014 baseline survey. P-values based on standard errors clustered by community. The number of observations (N) indicating child-level attrition is the difference between the number of baseline households and the number of children for whom the Denver test was completed; N for household-level attrition is the number of baseline households for whom no follow up household survey was collected; and N for mother(father)-level is the number of baseline households for whom the mother(father) could not be interviewed at follow-up.

TABLE 1.17: Impact of text messages on individual ECD tests

	ITT (1)	S.e (2)	P-Value (3)	Obs (4)
<i>Younger children (12 - 36 months)</i>				
ECD index (first principal component)	-0.01	(0.06)	0.87	774
Denver Personal-social	0.02	(0.08)	0.82	790
Denver Language	0.04	(0.05)	0.39	784
Denver Fine Motor	0.01	(0.07)	0.87	781
Denver Gross Motor	-0.13	(0.10)	0.17	779
<i>Older children (37 - 84 months)</i>				
ECD index (first principal component)	0.00	(0.04)	0.95	1,711
Denver Personal-social	0.01	(0.04)	0.81	1,757
Denver Language	0.06	(0.04)	0.81	1,753
Denver Fine Motor	0.04	(0.05)	0.37	1,746
Denver Gross Motor	-0.01	(0.05)	0.80	1,742
Memory	-0.05	(0.04)	0.21	1,927
TVIP (Vocabulary)	-0.03	(0.04)	0.41	1,757
Self control	0.00	(0.05)	0.93	1,713

Each row corresponds to a separate estimation (equation 1). All outcome variables are standardized using the mean and standard deviation of control households. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variables for a set of households surveyed in 2016, as well as community fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.18: Impacts of text messages on ECD Outcomes, by treatment variations

	(1) ECD full sample	(2) ECD young	(3) ECD Old
Panel A: By treatment variation (thematic focus)			
Nutrition & Stimulation	-0,00 (0,04)	-0,05 (0,07)	0,02 (0,05)
Nutrition	-0,02 (0,04)	0,02 (0,08)	-0,05 (0,05)
Stimulation	0,02 (0,04)	0,00 (0,08)	0,02 (0,05)
Observations	2485	774	1711
Panel B: By treatment variation (targeted caregiver)			
Mother	0,03 (0,04)	0,05 (0,08)	0,02 (0,05)
Father	-0,01 (0,04)	-0,01 (0,08)	-0,01 (0,05)
Mother and Father	-0,02 (0,04)	-0,06 (0,07)	-0,02 (0,05)
Observations	2485	774	1711

Note: All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months and the first principal component of all 7 tests for children 36-84 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effect. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.19: Impacts of text messages on socio-emotional outcomes for children aged 3-7 years

	(1) Behavioral Index	(2) Emotional	(3) Conduct	(4) Hyperactive	(5) Peer	(6) Pro Social	(7) Control	(8) Laugh and Smile
Panel A: Equation 1								
ITT	- 0.02 (0.05)	0.08 (0.06)	-0.10* (0.06)	0.10* (0.06)	0.00 (0.05)	-0.01 (0.05)	-0.03 (0.06)	0.02 (0.05)
Observations	1723	1723	1723	1723	1723	1723	1723	1723
Panel B: Equation 2								
Nutrition & Stimulation	0.04 (0.07)	0.16** (0.07)	-0.03 (0.07)	0.13* (0.08)	-0.02 (0.07)	0.01 (0.06)	0.00 (0.07)	0.06 (0.07)
Nutrition	-0.03 (0.07)	0.04 (0.07)	-0.11 (0.07)	0.05 (0.08)	0.04 (0.07)	-0.03 (0.07)	-0.09 (0.07)	0.00 (0.07)
Stimulation	-0.03 (0.07)	0.07 (0.07)	-0.16** (0.07)	0.09 (0.08)	0.02 (0.07)	0.00 (0.07)	-0.00 (0.07)	-0.00 (0.07)
Observations	1723	1723	1723	1723	1723	1723	1723	1723
Panel C: Equation 3								
ITT Leader	-0.05 (0.06)	-0.00 (0.05)	-0.05 (0.07)	0.02 (0.08)	-0.06 (0.06)	-0.00 (0.06)	-0.03 (0.06)	-0.13** (0.06)
Observations	1438	1438	1438	1438	1438	1438	1438	1438

Note: The dependent variable in the first column is the first principal component of the 5 subscales of the "Strengths and difficulties" questionnaire, and modules measuring inhibitory control scale and positive demeanor (Laugh and Smile). Controls in panel A and B as in Table 3, controls in panel C as in Table 4. The standard errors (in parentheses). * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.20: Impacts of text messages on socio-emotional outcomes for children aged 1-3 years

	(1) Behavioral index	(2) Control	(3) Affect	(4) Ability
Panel A: Equation 1				
ITT	0.02 (0.08)	-0.14 (0.09)	0.04 (0.09)	0.09 (0.09)
Observations	717	717	717	717
Panel B: Equation 2				
Nutrition & Stimulation	- 0.01 (0.11)	-0.16 (0.11)	0.01 (0.11)	0.10 (0.11)
Nutrition	0.07 (0.11)	-0.27** (0.11)	0.11 (0.11)	0.22** (0.11)
Stimulation	0.02 (0.12)	0.05 (0.11)	-0.00 (0.11)	0.01 (0.11)
Observations	717	717	717	717
Panel C: Equation 3				
ITT Leader	0.02 (0.10)	-0.01 (0.09)	0.04 (0.09)	0.00 (0.11)
Observations	577	577	577	577

Note: The dependent variable in the first column is the first principal component of 3 sub-scales of the early childhood behavior questionnaire: effortful control (column 2), negative affectivity (column 3) and socialability (or extraversion, column 4). Controls in panel A and B as in Table 3, controls in panel C as in Table 4. Standard errors (in parentheses). * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.21: Impact of text messages on individual intermediary outcomes: nutrition and stimulation

	ITT	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
<i>Nutrition</i>					
Nutrition index	0,06	(0,04)	0,12	0,00	2.500
<i>Beverages and soups</i>					
Soy Milk	0,06	(0,06)	0,33	0,37	2.501
Juice	0,16	(0,11)	0,15	3,72	2.501
Soup	0,00	(0,06)	0,96	0,71	2.501
<i>Vegetables</i>					
Salad	-0,13	(0,09)	0,17	0,34	2.501
Potatoes	0,10	(0,07)	0,18	1,08	2.501
Tomatoes	0,06	(0,12)	0,58	2,97	2.501
Onion	0,22	(0,13)	0,10	1,98	2.501
Other vegetables	-0,02	(0,09)	0,85	1,69	2.501
<i>Staples</i>					
Rice	0,03	(0,08)	0,73	5,85	2.500
Beans	-0,06	(0,09)	0,49	6,09	2.501
Bread	-0,02	(0,12)	0,85	4,12	2.501
Tortilla	0,11	(0,10)	0,28	5,82	2.499
Cookies	0,13	(0,09)	0,16	1,28	2.501
<i>Proteins</i>					
Protein index	0,06	(0,04)	0,11	0,00	2.501
Milk	-0,04	(0,11)	0,74	1,76	2.501
Eggs	0,30	(0,09)	0,00	1,72	2.501
Cheese	-0,00	(0,10)	0,98	2,17	2.500
Meat	-0,00	(0,06)	0,98	1,22	2.501
Breast milk	0,22	(0,25)	0,38	3,15	802
<i>Stimulation</i>					
Stimulation index	0,16	(0,04)	0,00	0,00	2.501
Buy toys for the child	0,03	(0,01)	0,01	0,86	2.501
Tell tales to the child	0,04	(0,02)	0,02	0,79	2.501
Read books to child	0,04	(0,02)	0,05	0,27	2.501
Have pen and paper	0,02	(0,02)	0,12	0,84	2.501

Each row corresponds to a separate estimation (equation 1) and shows the ITT estimates on aggregate indices or individual questions measuring investments in nutrition or stimulation as reported by the caregiver. Questions on food items measure how many days in the last 7 days the child was given the specific item. The nutrition index was calculated by summing the days over all food items. The protein index was calculated by summing the days over all protein items. The individual items about stimulation are answers to yes/no questions, and the index is obtained by summing over yes answers. The nutrition, protein, and stimulation indices are standardized using the mean and standard deviation for control households. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

TABLE 1.22: Impact of text messages on individual intermediary outcomes: preventive health

	ITT	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
<i>Health</i>					
Health index	0,07	(0,04)	0,07	0,00	2.512
Insist on brushing teeth	0,01	(0,01)	0,41	0,02	2.512
Ensure a healthy diet	0,03	(0,01)	0,02	0,06	2.512
Keep child away from stove	-0,00	(0,00)	0,93	0,00	2.512
Ensure child's good hygiene	-0,01	(0,02)	0,46	0,21	2.512
Wash child's clothes properly	0,01	(0,02)	0,60	0,12	2.512
Avoid giving coffee	-0,00	(0,00)	0,32	0,00	2.512
Make child wash hands	0,00	(0,02)	0,86	0,27	2.512
Smoke at a distance from child	-0,00	(0,00)	0,74	0,00	2.512
Do not give child unhealthy snacks	0,00	(0,01)	0,49	0,01	2.512
Cover fresh food	0,00	(0,02)	0,78	0,13	2.512
Teach child to chew slowly	-0,00	(0,00)	0,61	0,00	2.512
Prevent child from catching the flu	0,00	(0,01)	0,80	0,06	2.512
Boil or chlorinate the water	0,01	(0,01)	0,13	0,02	2.512
Ensure that child vaccines are up-to-date	0,02	(0,01)	0,23	0,07	2.512
Use a mosquito net	-0,00	(0,01)	0,71	0,01	2.512
Insist child wears shoes	0,01	(0,01)	0,56	0,03	2.512
Make sure child eats regularly	0,00	(0,00)	0,25	0,00	2.512
Keep child away from dangerous products	0,00	(0,00)	0,75	0,00	2.512
Show child affection	-0,01	(0,00)	0,09	0,01	2.512
Other	-0,00	(0,02)	0,97	0,17	2.512

Every row corresponds to a separate estimation (equation 1) and shows the ITT estimates on the aggregate health index or on individual questions measuring whether the caregiver reported a given practice when asked how to avoid their young child getting sick. The health index is calculated based on the number of different preventive health practices. The index is standardized using the mean and standard deviation of the control households. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

TABLE 1.23: ITT on individual intermediary outcomes: micronutrients, ECD attitudes and observed hygiene

	ITT	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
<i>Micronutrients</i>					
Micronutrients index	0.08	(0.04)	0.06	-0.00	2,495
Received Vitamin A	0.02	(0.02)	0.21	0.73	2,495
Received deworming medicine	0.02	(0.02)	0.37	0.76	2,495
Received iron supplementation	0.04	(0.02)	0.07	0.49	2,495
<i>ECD Attitudes of main caregiver</i>					
Attitudes index	0.11	(0.04)	0.01	0.00	2,396
Should always answer child's questions	0.02	(0.02)	0.28	0.87	2,396
Boys can play with dolls	0.07	(0.02)	0.00	0.36	2,396
Girls can play with cars	0.04	(0.02)	0.04	0.51	2,396
Early language develops from stimulation	-0.00	(0.02)	0.87	0.67	2,396
Brain develops from very early on	0.01	(0.02)	0.51	0.86	2,396
<i>Hygiene (observed by test administrator)</i>					
Hygiene index	0.09	(0.04)	0.03	0.00	2,506
Child has clean face	0.03	(0.02)	0.09	0.67	2,506
Child has clean hair	-0.00	(0.02)	0.83	0.80	2,506
Child has clean hands	0.06	(0.02)	0.00	0.56	2,506
Child has clean clothes	0.07	(0.02)	0.00	0.56	2,506
Child is wearing shoes	-0.04	(0.02)	0.05	0.33	2,506
Child does not cough	-0.01	(0.02)	0.40	0.85	2,506
Child has clean nose	0.00	(0.02)	0.99	0.84	2,506
Child does not have skin problems	0.00	(0.01)	0.83	0.93	2,505

Every row corresponds to a separate estimation (equation 1). The vitamin index is based on three variables: the child has received vitamin A, ferrous sulfate and deworming medicine during the last six months (yes/no questions). The hygiene index captures the hygienic condition of the child observed by the test administrator. The Attitudes index is calculated based on answers by the caregiver to the following questions: 1. Do you answer your child's questions? 2. Do you think boys can play with dolls? 3. Do you think girls can play with cars? 4. Do you think children start talking by nature? 5. Does the child's brain develop from gestation or when the child starts going to school? Each outcome variable is standardized using the mean and standard deviation for control households. The rows shows ITT estimates. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and dummy variable for a set of households surveyed in 2016, as well as village fixed effect. Standard errors reported in parentheses. $p < 0.1$, $p < 0.05$, $p < 0.01$

TABLE 1.24: Impact of leader's exposure on individual ECD tests

	Leader ITT (1)	S.e (2)	P-Value (3)	Obs (4)
<i>Younger children (12 - 36 months)</i>				
ECD index (first principal component)	-0.02	(0.05)	0.68	624
Denver Personal-social	-0.03	(0.07)	0.70	639
Denver Language	0.03	(0.06)	0.56	633
Denver Fine Motor	-0.05	(0.08)	0.49	630
Denver Gross Motor	-0.06	(0.07)	0.42	629
<i>Older children (37 - 84 months)</i>				
ECD index (first principal component)	-0.14	(0.05)	0.01	1,427
Denver Personal-social	-0.02	(0.05)	0.72	1,449
Denver Language	-0.14	(0.05)	0.01	1,449
Denver Fine Motor	-0.01	(0.05)	0.85	1,449
Denver Gross Motor	-0.09	(0.06)	0.15	1,449
Memory	-0.07	(0.05)	0.19	1,449
TVIP (Vocabulary)	-0.13	(0.05)	0.02	1,449
Self control	-0.08	(0.06)	0.17	1,427

Every row corresponds to a separate estimation (equation 1). All outcome variables are standardized using the mean and standard deviation of control households. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variables for a set of households surveyed in 2016, as well as community fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.25: Impact of leaders' exposure on ECD outcomes, by leaders' education levels

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT Leader	-0,14** (0,05)	-0,01 (0,09)	-0,18*** (0,06)
ITT Leader X Leaders medium educ	0,12 (0,08)	-0,03 (0,14)	0,18* (0,10)
ITT Leader X Leaders high educ	-0,03 (0,09)	-0,00 (0,13)	-0,06 (0,11)
Observations	2051	624	1427
Adjusted R^2	0,457	0,155	0,539
ITT Leader + ITT Leader X Leaders medium educ	0,79	0,69	0,98
ITT Leader + ITT Leader X Leaders high educ	0,02	0,89	0,01

Note: All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.26: Impact of leaders' exposure on ECD outcomes, by baseline social interactions about ECD

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT Leader	-0,10** (0,04)	-0,08 (0,08)	-0,11** (0,06)
Social (Had interactions about ECD at baseline)	0,02 (0,07)	-0,08 (0,09)	0,05 (0,09)
ITT Leader X Social	-0,02 (0,08)	0,14 (0,12)	-0,06 (0,10)
Observations	2051	624	1427
ITT Leader + ITT Leader X Social	0,07	0,48	0,04

Note: Social is a binary variable indicating that the household had talked to at least one other community member about ECD in the week before the baseline survey. At baseline, households were asked whether they had talked to other community members about ECD practices in the last 7 days. This includes interaction about ECD practices with the health promoter, pre-school teacher, primary school teacher, other teachers, family members, neighbours, or elected leaders in the village. About half of the households had talked to at least one other community member about ECD practices. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old, and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.27: Impact of leaders' exposure on intermediary outcomes, by baseline social interactions about ECD

	(1) Nutrition	(2) Stimulation	(3) Health	(4) Micronutrients	(5) Proteins	(6) Attitudes	(7) Hygiene
ITT Leader	-0.14 (0.10)	-0.04 (0.06)	-0.06 (0.06)	-0.19** (0.08)	-0.23*** (0.09)	-0.08 (0.07)	-0.14* (0.08)
Social (Had interactions about ECD at baseline)	0.05 (0.08)	-0.06 (0.09)	-0.01 (0.08)	0.03 (0.07)	-0.04 (0.10)	0.06 (0.09)	0.02 (0.08)
ITT Leader X Social	0.01 (0.10)	0.13 (0.11)	0.13 (0.08)	0.06 (0.09)	0.01 (0.11)	0.18* (0.10)	0.06 (0.09)
Observations	2062	2062	2073	2059	2062	1970	2068
ITT leader + ITT leader X Social	0.13	0.35	0.21	0.10	0.07	0.29	0.34

Note: Social is a binary variable indicating that the household had talked to at least one other community member about ECD in the week before the baseline survey. At baseline, households were asked whether they had talked to other community members about ECD practices in the last 7 days. This includes interaction about ECD practices with the health promoter, pre-school teacher, primary school teacher, other teachers, family members, neighbours, or elected leaders in the village. About half of the households had talked to at least one other community member about ECD practices. All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.28: Impact of leaders' exposure on individual intermediary outcomes: nutrition and stimulation

	ITT Leaders	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
<i>Nutrition</i>					
Nutrition index	-0.14	(0.08)	0.08	0.16	2,062
<i>Beverages and soup</i>					
Soy Milk	-0.05	(0.09)	0.60	0.44	2,062
Juice	-0.31	(0.19)	0.11	4.05	2,062
Soup	-0.06	(0.08)	0.44	0.76	2,062
<i>Vegetables</i>					
Salad	0.06	(0.04)	0.17	0.33	2,062
Potatos	-0.19	(0.11)	0.07	1.29	2,062
Tomate	0.02	(0.23)	0.95	3.17	2,062
Onion	-0.10	(0.18)	0.57	2.28	2,062
Other Vegetables	-0.07	(0.13)	0.59	1.75	2,062
<i>Staples</i>					
Rice	0.06	(0.17)	0.74	5.90	2,061
Beans	-0.10	(0.10)	0.29	6.16	2,062
Bread	-0.05	(0.25)	0.83	4.26	2,062
Tortilla	0.11	(0.15)	0.45	5.82	2,061
Cookies	-0.24	(0.12)	0.05	1.53	2,062
<i>Proteins</i>					
Protein index	-0.23	(0.09)	0.14	0.00	2,062
Milk	-0.32	(0.24)	0.19	1.97	2,062
Eggs	-0.23	(0.14)	0.11	2.03	2,062
Cheese	-0.55	(0.24)	0.02	2.48	2,061
Meat	-0.22	(0.12)	0.08	1.37	2,062
Breast milk	-0.18	(0.34)	0.60	3.33	647
<i>Stimulation</i>					
Stimulation index	0.02	(0.06)	0.80	0.13	2,062
Buy a toys for child	-0.01	(0.02)	0.71	0.89	2,062
Tell tales to child	0.02	(0.02)	0.40	0.82	2,062
Read books to child	-0.01	(0.03)	0.84	0.31	2,062
Have paper and pen	0.01	(0.02)	0.73	0.84	2,062

Every line corresponds to a separate estimation (equation 3) and shows the ITT estimates on aggregate indices or individual questions measuring investments in nutrition or stimulation as reported by the caregiver. Questions on food items measure how many days in the last 7 days the child was given the specific item. The nutrition index was calculated by summing the days over all food items. The protein index was calculated by summing the days over all protein items. The individual items about stimulation are answers to yes/no questions, and the index is obtained by summing over yes answers. The nutrition, protein, and stimulation indices are standardized using the mean and standard deviation for control households. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

TABLE 1.29: Impact of leaders' exposure on individual intermediary outcomes: preventive health

	ITT Leaders	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
Health					
Health index	-0.00	(0.04)	0.37	0.00	2,073
Insist on brushing teeth	-0.00	(0.01)	0.78	0.02	2,073
Ensure a healthy diet	0.01	(0.01)	0.05	0.06	2,073
Keep child away from the stove	-0.00	(0.00)	0.88	0.00	2,073
Ensure good hygiene	-0.01	(0.02)	0.27	0.21	2,073
Wash clothes properly	-0.00	(0.01)	0.71	0.12	2,073
Avoid giving coffee	0.00	(0.00)	0.29	0.00	2,073
Make child wash hands	0.00	(0.02)	0.90	0.27	2,073
Smoke at a distance from child	-0.00	(0.00)	0.74	0.00	2,073
Do not give child unhealthy snacks	0.00	(0.01)	0.72	0.01	2,073
Cover fresh food	0.02	(0.02)	0.86	0.13	2,073
Teach child to chew slowly	0.00	(0.00)	0.80	0.00	2,073
Prevent child from catching the flu	-0.01	(0.01)	0.64	0.06	2,073
Boil or chlorinate the water	0.00	(0.01)	0.42	0.02	2,073
Ensure that child vaccines are up-to-date	-0.00	(0.01)	0.34	0.07	2,073
Use a mosquito net	-0.00	(0.01)	0.39	0.01	2,073
Insist child wears shoes	-0.01	(0.01)	0.60	0.03	2,073
Make sure child eats regularly	-0.00	(0.00)	0.46	0.00	2,073
Keep child away from dangerous products	-0.00	(0.00)	0.85	0.00	2,073
Show child affection	0.00	(0.00)	0.26	0.01	2,073
Other	0.01	(0.02)	0.87	0.17	2,073

Each row corresponds to a separate estimation (equation 3) and shows the ITT estimate on the aggregate health index or on individual questions measuring whether the caregiver reported a given practice when asked how to avoid their young child getting sick. The health index is calculated based on the number of different preventive health practices. The index is standardized using the mean and standard deviation of the control household. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. Five villages without leaders were excluded from the estimation. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.30: Impact of leaders' exposure on individual intermediary outcomes: micronutrients, ECD attitudes and observed hygiene

	ITT Leaders	S.e	P-Value	Mean Control	Obs
	(1)	(2)	(3)	(4)	(5)
<i>Micronutrients</i>					
Micronutrients index	-0.16	(0.06)	0.02	0.13	2,059
Received Vitamin A	-0.06	(0.03)	0.05	0.76	2,059
Received deworming medicine	-0.05	(0.03)	0.05	0.80	2,059
Received iron supplementation	-0.04	(0.03)	0.15	0.55	2,059
<i>ECD Attitudes of main caregiver</i>					
Attitudes index	-0.00	(0.06)	0.97	0.08	1,970
Should always answer child's questions	-0.01	(0.02)	0.63	0.89	1,970
Boys can play with dolls	0.02	(0.03)	0.62	0.39	1,970
Girls can play with cars (toy)	0.01	(0.03)	0.82	0.53	1,970
Early language develops from stimulation	0.01	(0.02)	0.67	0.68	1,970
Brain develops from very early on	-0.03	(0.01)	0.04	0.88	1,970
<i>Hygiene (observed by test administrator)</i>					
Hygiene index	-0.11	(0.06)	0.09	0.06	2,068
Child has clean face	-0.03	(0.03)	0.25	0.70	2,068
Child has clean hair	-0.05	(0.02)	0.02	0.82	2,068
Child has clean hands	-0.07	(0.03)	0.01	0.61	2,068
Child has clean clothes	-0.06	(0.03)	0.03	0.61	2,068
Child is wearing shoes	0.03	(0.02)	0.20	0.30	2,068
Child does not cough	0.01	(0.02)	0.64	0.82	2,068
Child has clean nose	0.00	(0.02)	0.94	0.83	2,068
Child does not have skin problems	0.00	(0.02)	0.81	0.92	2,067

Every row corresponds to a separate estimation (equation 3). The vitamin index is based on three variables: the child has received vitamin A, ferrous sulfate and deworming medicine during the last six months (yes/no questions). The hygiene index captures the hygienic condition of the child observed by the test administrator. The Attitudes index is calculated based on answers by the caregiver to the following questions: 1. Do you answer your child's questions? 2. Do you think boys can play with dolls? 3. Do you think girls can play with cars? 4. Do you think children start talking by nature? 5. Does the child's brain develop from gestation or when the child starts going to school? Each outcome variable is standardized using the mean and standard deviation for control households. The rows shows ITT estimates. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age, gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. Five villages without leaders were excluded from the estimation. The standard errors (in parentheses) are clustered at village level. $p < 0.1$, $p < 0.05$, $p < 0.01$

TABLE 1.31: Impact of text messages on ECD outcomes for children of leaders versus non-leaders

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT	-0.01 (0.03)	-0.02 (0.07)	-0.01 (0.04)
Child of Leader	0.10 (0.07)	0.24* (0.12)	0.03 (0.08)
ITT X Child of Leader	-0.06 (0.08)	-0.02 (0.13)	-0.08 (0.10)
Observations	2393	747	1646
ITT + ITT X Child of Leader =0	0.39	0.73	0.32

Note: All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months old, and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 to 35 months. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, Memory and self-control) of children aged 36 to 83 months. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Five villages without leaders were excluded from the sample. Standard errors reported in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.32: Impact of text messages on intermediary outcomes for children of leaders versus non-leaders

	(1) Nutrition	(2) Stimulation	(3) Health	(4) Micronutrients	(5) Proteins	(6) Attitudes	(7) Hygiene
ITT	0.08 (0.05)	0.19*** (0.05)	0.04 (0.04)	0.07 (0.05)	0.06 (0.05)	0.12** (0.05)	0.09* (0.05)
Child of Leader	-0.14 (0.11)	0.31*** (0.11)	-0.04 (0.10)	0.21* (0.11)	-0.03 (0.10)	0.21* (0.11)	0.05 (0.11)
ITT X Child of Leader	-0.09 (0.12)	-0.16 (0.12)	0.15 (0.11)	-0.11 (0.12)	-0.24** (0.11)	0.06 (0.12)	-0.07 (0.12)
Observations	2407	2408	2419	2403	2408	2303	2413
ITT + ITT X Child of Leader = 0	0.88	0.82	0.06	0.71	0.09	0.12	0.91

Note: All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. All the regressions include controls for the stratification variables, child age and gender, enumerator fixed effects, and a dummy variable for a set of households surveyed in 2016, as well as village fixed effects. Five villages without leaders were excluded from the sample. Standard errors reported in parentheses.* p<0.1, ** p<0.05, *** p<0.01

TABLE 1.33: Impact of leaders' exposure on ECD outcomes, by closest leader's commitment

	(1) ECD full sample	(2) ECD young	(3) ECD Old
ITT Leader	-0.05 (0.09)	-0.04 (0.18)	-0.05 (0.09)
ITT leader X low commitment	-0.13 (0.11)	-0.06 (0.18)	-0.19 (0.12)
ITT leader X medium commitment	-0.15 (0.12)	-0.12 (0.24)	-0.19 (0.12)
ITT leader X high commitment	0.08 (0.11)	0.30 (0.20)	-0.01 (0.13)
ITT leader X very high commitment	-0.05 (0.12)	-0.02 (0.28)	-0.06 (0.12)
Observations	2051	624	1427
ITT leader + ITT leader X low commitment = 0	0.01	0.33	0.01
0.00 0.00 0.00 ITT leader + ITT leader X medium commitment = 0	0.01	0.20	0.01
ITT leader + ITT leader X high commitment = 0	0.74	0.02	0.54
ITT leader + ITT leader X very high commitment = 0	0.24	0.74	0.19

Note: Low, medium, high and very high commitment indicates whether leader is predicted to be in the 2, 3, 4, or top quintile of leaders with highest participation in the quizzes. All outcome variables are standardized using the mean and standard deviation of control households. In column 1, the dependent variable is the first principal component of the 4 Denver sub-components for children below 36 months and the first principal component of all 7 tests for children 36-83 months old. In column 2, the dependent variable is the principal component for the four Denver sub-components for children aged 12 and 35 month. In column 3, the dependent variable is the first principal component of all 7 tests (4 Denver sub-components plus TVIP, memory and self-control) of children aged 36 to 83 months. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

TABLE 1.34: Impact of leaders' exposure on intermediary outcomes, by closest leader's commitment

	(1) Nutrition	(2) Stimulation	(3) Health	(4) Micronutrients	(5) Proteins	(6) Attitudes	(7) Hygiene
ITT Leader	-0.08 (0.15)	0.06 (0.09)	0.03 (0.10)	-0.07 (0.12)	-0.01 (0.14)	0.04 (0.10)	-0.04 (0.11)
ITT leader X low commitment	-0.13 (0.16)	-0.07 (0.15)	-0.06 (0.15)	-0.20 (0.16)	-0.31* (0.18)	-0.02 (0.15)	-0.21 (0.15)
ITT leader X medium commitment	-0.18 (0.20)	-0.16 (0.15)	-0.21 (0.13)	-0.17 (0.16)	-0.36* (0.21)	-0.14 (0.15)	-0.14 (0.15)
ITT leader X high commitment	0.03 (0.20)	0.01 (0.14)	0.08 (0.14)	0.12 (0.17)	-0.27 (0.19)	-0.21 (0.16)	0.04 (0.15)
ITT leader X very high commitment	-0.01 (0.22)	0.00 (0.15)	0.03 (0.13)	-0.18 (0.16)	-0.16 (0.19)	0.15 (0.16)	-0.03 (0.18)
Observations	2062	2062	2073	2059	2062	1970	2068
ITT leader + ITT leader X low committed = 0	0.01	0.89	0.72	0.01	0.045	0.84	0.04
ITT leader + ITT leader X medium committed = 0	0.08	0.34	0.05	0.02	0.03	0.41	0.13
ITT leader + ITT leader X high committed = 0	0.64	0.55	0.20	0.71	0.04	0.23	0.95
ITT leader + ITT leader X very high committed = 0	0.63	0.64	0.42	0.02	0.19	0.13	0.65

Note: Low, medium, high and very high commitment indicates whether leader is predicted to be in the 2, 3, 4, or top quintile of leaders with highest participation in the quizzes. All outcome variables are standardized using the mean and standard deviation of the control household. The first 6 columns show ITT estimates on indices aggregating information regarding early childhood investments, as reported by the caregiver. Column 7 aggregates information regarding hygiene of the child, as observed by the enumerator. All regressions include controls for the household-level treatment, the average level of education of the leaders (the stratification variable for village level leader randomization), as well as the level of education of the main caregiver, whether there is a male caregiver in the household, whether the leader household had access to electricity, child age and gender and test-administrator fixed effects as well as a binary indicator indicating the data was collected in 2016. The standard errors (in parentheses) are clustered at village level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 1.35: Impacts on social interactions about ECD with other members of the community

	(1) # other members	(2) Relatives	(3) Neighbors	(4) Religious leader
ITT	0.05 (0.04)	0.03 (0.02)	0.03 (0.02)	-0.01 (0.02)
ITT Leader	0.04 (0.05)	-0.01 (0.02)	0.02 (0.03)	0.03* (0.02)
Observations	2073	2073	2073	2073
Mean Control	0.95	0.40	0.36	0.19

Note: The estimation corresponds to the estimator β_2 in equation 1.3. The dependent variables are the social interactions between the head of the household and social leaders during the previous week. Column 1 is an index accounting for household head interaction with relatives, neighbors and religious leader. The remaining columns are binary: 1 whether the household head had at least one interactions with any respective leader during the referent week. All the regressions include controls for the stratification variables: education of the household, titular of the household is male, whether the leader has access to electricity, targeted child age and sex, enumerator and a dummy variables for a set of households surveyed in 2016 (one year after the intervention). The standard errors (in parentheses) are clustered at village level. * p<0.1, ** p<0.05, *** p<0.01

1.7.2 Lasso prediction of quiz participation

Information on quiz participation can be seen as a measure of compliance or engagement with the text message intervention. As the control group did not received text messages, they did not participate in the quiz. We therefore use baseline observables to predict the frequency of participation in the quiz for each leader household, using Lasso (Tibshirani (1996)).

We minimize the sum of the square residuals (SSR) correcting with a Lasso regression penalty given by the following expression

$$SSR + \lambda * (|\beta_1 + \beta_2 + \beta_3 + \dots + \beta_k|)$$

where lambda is the penalized Lasso parameter, and β_k are the OLS parameters for the variables k , excluding the intercept.

When $\lambda = 0$ the Lasso prediction will be the same as the OLS prediction. But as λ increases, Lasso will shrink the slope of a subset of parameters to 0, reducing the number of covariates used for the prediction.

The Lasso regression has more bias than OLS, but it has lower variance of the predicted values and hence improve the overall prediction accuracy.

To obtain the predictive model, we start from 145 baseline variables, including questions on parental practices, nutrition and stimulation of the child, household economic activity, migrant status, social interactions with the rest of the community, labor supply of all household members, as well as a demographic characteristics of the household head and targeted child. We also included questions on education and health. We force the model to keep all stratification variables.

By setting lambda = 10, we can explain 65% of the variation keeping the following 22 variables²⁴: caregiver's assessment of child's fine motor skills, household is engaged in agriculture activity, has livestock activities, and has private wage job activities, the head of the household has a small manufacturing business activity, someone in household has migrated to work as a nanny, someone in household has received training on ECD, the

²⁴By comparison the OLS model (lambda = 0), leads to a model with $r^2 = 0,662$.

household has had social interactions with the health promoter, with the primary school teacher and a local political leader, the caregiver considers that hitting a child is a good parenting practice, the caregiver considers that playing games and giving affection is good parenting practice, someone in household sometimes threatens the the child by saying that a monkey may take him away, someone in the household sometimes uses a phone in other households, someone in household has a cell phone with Movistar coverage, the household has Movistar network coverage at home, the child drank milk during the previous week, number of day that the child consumed fruit in previous week, at least one child has a caregiver other than his/her mother, household size, number of adults in the household who have migrated temporarily over last 12 months, household has access to electricity, and presence of a male caregiver.

Chapter 2

Facts, Alternative Facts, and Fact Checking in Times of Post-Truth Politics

2.1 Introduction

The recent rise of nativist populism in the West has been accompanied by politicians' extensive use of "alternative facts," statements on key policy issues that directly or indirectly contradict real facts. Many anti-establishment politicians have used easily refutable statements to promote their political agenda. For example, pro-Brexit campaign falsely claimed that EU membership cost the UK over 350 million British pounds per week (about 500 million US dollars at the pre-Brexit exchange rate) and this money could be saved by the national budget in the case of exit from the European Union.¹ Donald Trump and his 2016 campaign staff repeatedly circulated wrong unemployment numbers for the US and made false claims about US homicide rate being at its highest in several decades.² Alternative facts are noticed by voters: Allcott and Gentzkow (2017) show that fake news in favor of Trump were shared 30 million times on Facebook. The use of alternative facts is not confined to populists: some mainstream politicians also resort to

¹See, for instance: <http://www.telegraph.co.uk/news/0/eu-referendum-claims-won-brexit-fact-checked/> (accessed on May 26, 2017).

²See, for instance: <http://edition.cnn.com/2017/02/07/politics/donald-trump-murder-rate-fact-check/> and <http://www.npr.org/2017/01/29/511493685/ahead-of-trumps-first-jobs-report-a-look-at-his-remarks-on-the-numbers> (both accessed on May 26, 2017).

them.

As alternative facts become part of modern politics in established democracies, so does fact checking: mainstream media have increasingly invested in checking politicians' claims and provided rebuttals. For example, *Le Monde*, one of the leading French newspapers, identified and corrected 19 misleading statements made by Marine Le Pen, the extreme-right candidate who reached the runoff of the 2017 French presidential election, during her televised debate against Emmanuel Macron.³ Similar efforts are taken by most leading media in the US and Europe — as well as by many independent organisations.⁴

Given the substantial fact-checking efforts, it is puzzling why populist politicians double down on their use of alternative facts. If such behavior is rational, this means that, even in the presence of fact checking, alternative facts bring political benefits. In this paper, we show that fact checking may indeed be ineffective in correcting the impact of the politicians' propaganda.

What are the potential explanations for the ineffectiveness of fact checking? One possibility is that voters lack trust in mainstream media and the experts on whom the media relies for fact checking. If voters are more confident in numbers provided by politicians than by the media, they would rationally update their prior beliefs in the direction of the alternative facts away from the truth provided by the fact checkers. This explanation is empirically testable, by conducting a randomized control trial where some voters are exposed to alternative facts (with attribution to their source), while other voters are exposed to alternative facts and the respective fact checking (also attributed to the source). In such an experiment, if the voters do not have much trust in the source of fact checking, the posterior of voters exposed to alternative facts and fact checking should be closer to the posterior of those exposed to alternative facts alone than to the posterior of the control group.

³http://www.lemonde.fr/les-decodeurs/article/2017/05/03/des-intox-du-debat-entre-emmanuel-macron-et-marine-le-pen-verifiees_5121846_4355770.html (accessed on May 26, 2017).

⁴See for example <https://www.nytimes.com/spotlight/fact-checks>, <https://www.bbc.com/news/topics/cp7r8vgl2rgt/reality-check>, <https://www.channel4.com/news/factcheck>, http://www.repubblica.it/argomenti/Fact_Checking (all accessed on July 13, 2018) and the report on the rise of fact checking in Europe by the Reuters Institute at Oxford (Graves and Cherubini (2016)).

Another explanation is that being exposed to the numbers (true or false) raises the salience of the issue central to the politician's narrative (for instance, immigration), understood as the story or the argument linking the facts and the conclusions. The voters may then choose to support the politician who focuses on this issue irrespective of their posterior beliefs on facts, and this is all that matters for the politician.⁵ To test this explanation one could expose a group of voters to true facts alone (also with attribution to their source). If salience explains the ineffectiveness of fact checking, one should expect to see a shift in voting intentions in favor of the politician who puts this contentious issue at the center of her program, after exposing voters to true facts on a contentious issue (e.g., immigration). This could happen even when voters find official sources credible and do not doubt the numbers provided by fact checkers.

In this paper, we shed light on these alternative hypotheses on the impact of fact checking. We test how exposure of voters to alternative facts, fact checking, or true facts affect voting intentions, policy positions, knowledge of facts, and trust in official institutions.

In March 2017, during the French presidential campaign, we administered an online-survey-based experiment to 2480 voting-age French inhabitants of five French regions with traditionally strong support for the extreme right. The sample was stratified on gender, age and education to make it similar to a nationally representative sample.

The participants were randomly allocated to four equally sized groups: (i) control group, (ii) alternative facts group, (iii) fact checking group, and (iv) real facts group. The participants in different groups were asked to read different messages. The control group was presented with no information. Participants in the group "Alt-Facts" (for alternative facts) were asked to read several statements by Marine Le Pen (MLP) on immigration, each containing factually incorrect or simply misleading information, used as part of a logical argument. Participants in group "Facts" were asked to read a short text containing facts from official sources on the same issues. Participants of the group

⁵The effect of salience is similar to the "availability heuristic"; both are well documented in experimental economics and psychology. Salience is one of the four pillars of political scientist John Zaller's seminal integrated theory of public opinion (Zaller, 1992). Note that salience is different from priming as the former is about bringing the audience's attention to a specific issue (immigration) rather than influencing the point of view on the issue directly.

“Fact-Check” were provided first with the same quotes from MLP and then the same text with facts from official sources. All texts presented to participants had a clear indication of the source. Before being subjected to the treatments, participants of all groups filled in a short questionnaire about their socio-economic background and were asked one question that aimed at measuring their prior knowledge of the statistics on immigration. After the treatments, following general questions on political opinions, participants were asked about their voting intentions (using three different methods), their opinions on immigration policy, and their posterior beliefs about the facts, related to numbers cited in the treatments, as well as their past voting behaviour.

The results of our experiment confirm that on average the use of alternative facts increases the political support of the politicians irrespective of fact checking, which explains why politicians use alternative facts despite facing the risk of being fact checked. We find that political statements based on alternative facts are highly persuasive and fact checking is ineffective in undoing their effect on voting: being exposed to MLP’s rhetoric significantly increases voting intentions in favor of MLP by 5 percentage points, *irrespective* of whether they are or are not accompanied by fact checking. The effects of all treatments are stronger for those respondents whose prior belief about the unemployment rate among migrants is an overestimation compared to the official statistics. Among those with overestimated priors about unemployment of migrants, Alt-Facts treatment increased MLP voting intentions by 8 percentage points, Fact-check treatment by 7 percentage points, and Facts treatment by 5 percentage points.

We explore the reasons for the absence of voters’ reaction to fact checking. We start by rejecting the explanation that voters trust the politician providing the alternative facts more than they trust the official sources providing the fact checking. In general, voters behave as Bayesians, updating factual knowledge in the direction of the signal they receive, having much higher confidence in the statistical facts from the official sources than in the alternative facts from MLP. The majority of voters presented with official statistics learn them (irrespective of whether they were exposed to alternative facts). Both the facts and the fact-checking treatment (i.e., the combination of alternative facts with facts) shifts voter posteriors on facts significantly towards the truth (relative to the control

group). In other words, fact-checking works well in terms of communicating the facts. Voters also learn facts presented in isolation: posteriors are much closer to the truth in the Facts group compared to the Control group. Voters presented with alternative facts alone move their posterior beliefs away from the truth, but the absolute magnitude of the effect of alternative facts treatment on posterior knowledge is much smaller than that of the facts treatment. Furthermore, the Alt-Facts treatment does not significantly affect the rate of giving correct responses to factual questions but increases the average distance to the truth, which means that those voters who knew correct answers to start with were not misled by the alternative facts and only those who had incorrect priors were moved even further away from the truth by the alternative facts.

To understand better what makes voters turn to MLP as a result of the treatments, we consider the effect of the treatments on the subjective opinion of voters about the policy issues. In particular, the answers to the questions: (i) whether refugees come for security or for economic reasons (MLP argues the latter) and (ii) whether the respondents agree with MLP specifically on immigration policy. Participants in the Alt-Facts and Fact-Check treatments think that refugees come for economic reasons in significantly higher proportions than participants in the control group. The difference with control group is 13 percentage points for Alt-Facts and 7 percentage points for Fact-Check. These effects are statistically different in size. Facts treatment, in contrast, does not significantly affect the assessment of reasons for refugees to come. This suggests that the narrative used in the alternative facts plays a role in persuasion: those voters who are exposed to the MLP's conclusion that refugees come for economic reasons tend to believe it more. In contrast, the agreement with MLP on immigration policy is significantly affected only by Alt-Facts treatment: voters in Alt-Facts treatment are 5 percentage points more likely to agree with MLP, while the agreement with MLP on immigration policy among Fact-Checking and Facts control group is not significantly different from that in the control group (albeit also negative).

We consider several potential explanations of these results. First, we show that neither experimenter demand effects nor the non-linearities in the relationship between facts and voting intentions are consistent with the evidence. Second, we discuss two potential

mechanisms. Alt-Facts narrative could send a signal about the candidate in addition to a signal about the state of the world. If this signal is positive, voters can react positively to the communication by the candidate even if she is proven to cite false facts. This could happen, for instance, if voters originally thought that the candidate is more extreme than she appears in the Alt-Facts narrative. Furthermore, all communication (by that Alt-Facts, Fact-Checking or Facts alone) could increase the salience of the immigration issue. We argue that both of these potential channels can be at play, but salience is necessary to explain all pieces of evidence.

In particular, voters exposed to true facts without MLP's statements are not less likely to vote for Marine Le Pen compared to the control group on average and are significantly more likely to vote for MLP if their priors are such that they overestimate the unemployment rate among migrants. This, however, does not mean that the facts are irrelevant — we observe a strong and significant association between facts and voting intentions in control group: those voters who believe that the situation with refugees is worse than it actually is are more likely to vote for MLP. Thus, the exposure to facts alone may have two effects that go in the opposite directions: on the one hand, facts increase the salience of the immigration issue, which boosts support for MLP, and on the other hand, it corrects the beliefs about facts in the direction that lowers the support for MLP. Indeed, we find that the effect of both Facts and Fact-Checking treatments is positive and significant in the full sample when we control for the posterior knowledge of facts. This result is consistent with the hypothesis that the exposure to information about migrants raises the salience of this issue in voters' minds and, therefore, leads to a higher support of a candidate with anti-immigrant agenda. The fact that we find larger effects for voters with incorrect priors compared to voters with correct priors is also consistent with the salience mechanism, as the previous research (e.g., Bordalo et al., 2012, 2013) has shown that the role of salience increases with the distance between the prior and the truth. In addition, the finding that Alt-Facts and Fact-Check have the same-size effect on voting, whereas Fact-Check has a smaller effect compared to Alt-Facts on policy conclusions of voters (significant for the beliefs about the reasons for the refugees to come and insignificant for the overall agreement with MLP on immigration policy) is also consistent with the

salience mechanism: voters change their voting intentions more than their policy views, on average, because the treatments make them see this particular aspect of policy (i.e., immigration) as more important.

The magnitude of the average treatment effects is fairly large: the persuasion rates to declare the intention to vote for MLP of our treatments, calculated using the formula from DellaVigna and Gentzkow (2010), are 7.8% for the alternative facts treatment and 7.7% for the fact-checking treatment. It is likely that the magnitude of these effects decreases over time, as suggested by existing studies both in experimental and in the real-world settings (e.g., Gerber et al., 2011; Swire et al., 2017). Furthermore, one cannot directly translate a change in reported voting intention to a change in how people vote in an election. The literature generally finds stronger effects for voting intentions than for actual voting (Gerber et al., 2011, 2009; Chiang and Knight, 2011). Importantly, our results and conclusions rely on the direction and on the relative magnitudes of the effect across treatments rather than on the absolute magnitude of the effect in each of the treatments. The important message of our analysis is that the effects of the Alt-Facts and Fact-Check treatments on voting intentions are similar, whereas on posteriors on facts they go in the opposite directions. There is no reason to believe that these relative effects evolve differentially over time. Another striking comparison is between the persuasion rates for voting intentions and for the factual knowledge. The latter are much larger in magnitude, e.g., for the percentage of men among migrants, they are 37% of the fact-checking treatment and 52% of the Facts treatment; voters do get convinced about the information from the official sources that they receive in these treatments.

We use the self-reported voting intentions as the main political outcome. To show that voting intentions are not just cheap talk, we use two different methods: dictator games and list experiments. The survey participants were asked to play two dictator games with real payoffs: one with a random anonymous counterpart among survey participants and the other with an anonymous counterpart randomly chosen among survey participants who said that he or she intended to vote for MLP. First, we show that larger donations to MLP supporters are associated with the intention to vote for MLP. Second, we show that alternative facts treatment significantly reduces the share of respondents who chose

to donate to a random participant, but does not share any money with a MLP supporter. The effects of other treatments on the dictator game outcomes are imprecisely estimated, but the signs of the coefficients are consistent with the effects of treatments on voting intentions.

One could potentially worry about a Bradley effect, i.e., respondents hiding their support for MLP in their responses, for instance due to shame. Even though it is unlikely, as we argue below, we take this concern seriously and carry out a list experiment. This experiment is specifically designed to infer the average support for MLP within a group of participants without having the participants admit that they support MLP. We present each respondent with a list of presidential candidates and ask *how many* of them they would support, without asking *whom* they would support. One half of these lists includes the names of four presidential candidates and does not include MLP; the other half lists the same four names plus MLP. We randomize both the exposure to the lists with and without MLP's name and the order of candidates within each list. The average difference in the responses about the number of candidates between lists with and without MLP is a measure of inferred average support for MLP. The results of the list experiment corroborate our findings for voting intentions. First, we find a statistically significant correlation between the responses to the question about voting intentions and the support for MLP inferred from the list experiment. Second, the level of inferred support for MLP across treatments lines up in a way consistent with the effect of treatments on voting intentions; however, the differences between treatments are not statistically significant due to a small sample size.

Our main contribution to the literature, which we briefly review in the next section, is in identifying the causal effect of alternative facts and of fact checking in a real-world setting.

The rest of the paper is structured as follows. Section 2.2 discusses the related literature. Section 2.3 describes the design of the study. Section 2.4 presents the main results and discusses potential alternative mechanisms. Section 2.5 establishes the validity of our measure of voting intentions and examines heterogeneity of the results. Section 2.6 concludes.

2.2 Related literature

The impact of slanted political information on political outcomes has been extensively studied in the context of traditional media (e.g., Gerber et al., 2009; DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Adena et al., 2015). Recently, researchers turned to studying the circulation of biased or outright false news on new online media platforms and social media, where fact checking standards are lax or missing? Mocanu et al. (2015), for example, document the rapid spread of fake news over social media during the 2012 elections in Italy. Allcott and Gentzkow (2017) show that fake stories were intensely shared on Facebook during the 2016 U.S. presidential election campaign. Zhuravskaya et al. (2019) review this literature.

With the important exception of the two studies in political science, Swire et al. (2017) and Nyhan et al. (2017), to the best of our knowledge, there is little systematic evidence about the impact of fact checking on subjective beliefs and voting intentions. Both of these studies focus on Trump's presidential campaign of 2016. Swire et al. (2017) conducted a randomized controlled trial treating participants with Trump's misinformation with and without attribution to Trump, subsequently correcting the misinformation either immediately or one week later. They found that the impact on the beliefs depend on both attribution to the source and partisanship (i.e., whether the participants were Trump supporters to start with). Using within-subject variation (rather than comparison across treatments), they also found that Trump supporters did not change their voting behavior after seeing the corrective information. Nyhan et al. (2017) conducted a randomization experiment to show that when Trump's misinformation is corrected, Trump voters update their factual beliefs but do not change their level of support of Trump. In both of these studies, the main effect of fact checking is to show that the candidate was lying and both studies conclude that it does not affect voting intentions of Trump's supporters. We reach a similar conclusion about the ineffectiveness of fact checking. The robustness of this finding across different contexts (Trump vs. MLP) and methods (experimental and non-experimental) strongly suggests external validity, which usually is hard to claim for any individual randomized control trial (RCT) study.

Our paper contributes to the literature in a number of additional ways. As the alternative facts are included in a narrative in our study, we explore the effect of fact checking separately on each of the three elements of the narrative: beliefs about facts, policy impressions and voting intentions; this has not been done in the previous literature.⁶ Further, we find the effect on both supporters and non-supporters of MLP, showing that policy conclusions can be swayed, even for non-supporters. Finally, because our experiment includes the Facts treatment, absent in the other studies, we provide new evidence for the salience explanation for the ineffectiveness of fact checking.

A growing literature in economics, political science and psychology studies the impact of information on political beliefs and knowledge. Kuziemko et al. (2015) carried out a randomized online experiment exposing participants to information on US income inequality and found a strong effect of this information on the support for the estate tax. Grigorieff et al. (2016) carried out a series of randomized experiments measuring the impact of information on the attitude toward immigrants. Alesina et al. (2018) studied the impact of information about immigrants on preference for redistribution in a large sample of respondents in six Western countries. Bursztyn et al. (2017) estimate the causal impact of Donald Trump's rise on the willingness to express xenophobic opinions publicly. Yet another important paper by Robbett and Matthews (2018) shows that when information is readily available to the participants, it does correct partisan bias; however, when the access to this information costs them even as little as fifty cents, the voters may choose to remain rationally ignorant and maintain their partisan stereotypes.

A number of studies examined the effect of information on knowledge. For example, Nyhan and Reifler (2010, 2015) document the shift in posterior beliefs about facts in the direction opposite of what the content of the information would imply for extremely salient issues, such as WMD in Iraq in 2005 and vaccine safety. However, the literature finds no such "backfiring" of information on facts for less salient issues (Wood and

⁶Note that our definition of the narrative (the story or arguments linking the facts with the conclusions) is closer to the one in Shiller (2017) ("a simple story or easily expressed explanation of events that many people want to bring up in conversation or on news or social media because it can be used to stimulate the concerns or emotions of others, and/or because it appears to advance self-interest") than to the one in Bénabou et al. (2018) ("stories people tell themselves, and each other, to make sense of human experience that is, to organize, explain, justify, predict and sometimes influence its course"); the latter is not necessarily argumentative.

Porter, 2016) or even more salient issues, such as gun control, minimum wage, and capital punishment (Guess and Coppock, 2018). Hatton (2017) analyzes survey data on Europeans’ attitudes to immigration and showing that public opinion on immigration in Europe depends on both preferences and salience of the immigration issue. Swire et al. (2017) synthesize the literature on this issue saying that “backfire effects only occur when an issue is strongly and currently connected with an individual’s political identity.”⁷ In addition, Berinsky (2015) shows that rumors may gain power due to “fluency”: attempts to fact check them using credible sources leads to repeating the rumor, which increases its diffusion.

2.3 Experimental design

2.3.1 Context

We use the context of the French presidential election and focus on the misleading statements of the extreme-right candidate Marine Le Pen (MLP). The 2017 French presidential election was held on April 23 (first round) and May 7 (runoff). It attracted global attention for a number of related reasons. First, this election witnessed the downfall of traditional parties: the candidates from both mainstream parties, the one on the right (LR) and the other one on the left (PS), did not qualify for the runoff. Second, this election led to the victory of a relative newcomer in politics, who created his party a few months before the election and ran on a pro-European platform. Finally, candidates from populist parties, both of the extreme left (Jean-Luc Melenchon) and the extreme right

⁷Backfiring can be explained by motivated cognition (or the “self-confirming bias”) where information is evaluated in a biased way to reinforce pre-existing views (Lord et al., 1979; Edwards and Smith, 1996; Taber and Lodge, 2006). Bénabou and Tirole (2016) provide a recent review of this literature and discuss many examples of motivated beliefs and self-deception. They suggest three mechanisms avoiding costly cognitive dissonance: strategic ignorance, reality denial and self-signaling. Strategic ignorance involves choosing to avoid information sources that contradict the preferred beliefs. Reality denial is the failure to update the beliefs even in the presence of the bad news. Finally, self-signaling is the manufacturing of signals that can be interpreted as the objective proof of desired conclusions. While our experiment does not allow for a direct test of self-signaling, we can distinguish between strategic ignorance and reality denial. The respondents in our experiment do learn the facts but fail to update conclusions based on these facts. Thus, our results are consistent with the importance of reality denial rather than strategic ignorance.

(Marine Le Pen) performed very well.⁸

Marine Le Pen's strong results in 2017 elections followed a series of electoral successes of her party National Front (FN, for *Front National* in French) in the preceding years. In the elections for the European Parliament in May 2014 the FN came first with nearly 25% of the votes. In the regional elections of December 2015 it nearly won several regions in spite of an alliance between the other main parties against FN. Throughout the 2017 campaign, Marine Le Pen was expected to get into the runoff polling first or close second. The final result was considered disappointing for MLP. She did qualify for the runoff but by a relatively small margin (21% of votes against Emmanuel Macron's 24% and François Fillon's 20%) and lost by a large margin in the second round with 34% of the total vote.

2.3.2 Facts and alternative facts

Following an influx of refugees into Europe, the issue of immigration policy played an important role in the 2017 presidential campaign. The anti-immigration stance was one of the MLP's key messages during the campaign, even though she did not make it the central one during the first stages of the campaign, preferring to focus on economic and social issues and on attacking the European union, in an effort to change the image of her party in the public opinion. She returned to immigration as a central theme only in late April 2017 after the 1st round of the election (i.e., after our experiment was completed).

Her immigration policy proposals included closing the French borders to refugees and substantially limiting legal immigration. MLP tried to convince voters that immigrants, including refugees, come to France for economic rather than security reasons, in particular, with the intention to benefit from the generous French welfare system. She often provided factually incorrect or misleading numbers, albeit with substantial prudence in the way they were expressed, and provided arguments that used these misleading numbers to make her point.

In the experiment, we use three quotes from MLP, which were characteristic of the arguments she made during the campaign. The *alternative facts* on which MLP based

⁸We follow the conventional French classification of parties into extreme left (Melenchon), center-left (PS), center (Macron), center-right (LR), and extreme right (Le Pen).

her arguments can be and were checked using official sources, such as the UN High Commissioner for Refugees (UNHCR) and INSEE, the French statistical institute. Each of the statements of MLP that we use for the experiment were made in the media and were subsequently fact checked by the newspaper *Liberation* and/or the online edition of the radio station *Europe 1*.⁹ Below, we present the precise quotes of MLP and the corresponding text with facts from official sources as they were presented to the participants of our experiment. The full text can be found in the Appendix.

Argument 1: If refugees had really been fleeing their countries for security reasons, they would not have left their families behind.

- **Alternative fact:** MLP: *"A very small minority of them are really political refugees (...). I have seen the pictures of illegal immigrants coming down, who were brought to Germany, to Hungary, etc... Well, on these pictures there are 99% of men (...). Men who leave their country leaving their families behind, it is not to flee persecution but of course for financial reasons. Let's stop telling stories. We are facing an economic migration, these migrants will settle."*¹⁰
- **Official fact:** *The UNHCR estimates that among the migrants crossing the Mediterranean in 2015, 17% are women, 25% are children and 58% are men.*

Argument 2: Migrants come to benefit from France's generous welfare system.

- **Alternative fact:** MLP: *"5% of the foreigners who come to France have a work contract. This means there are 95% of those coming to France who are taken care of by our nation (...). There are 95% of people who settle in France who don't work, either because of their age, or because they can't as there is no work in France."*¹¹

⁹In the Facts and Fact-Check treatments we did not expose participants to the whole text of the published fact-checking articles; instead, we showed short factual statements containing the statistical figures and their sources.

¹⁰Source: <http://lelab.europe1.fr/marine-le-pen-affirme-a-tort-que-les-refugies-sont-tres-majoritairement-des-migrants-economiques-debarquant-sans-leur-famille-2511737> (accessed on July 15, 2017).

¹¹Source: http://www.liberation.fr/france/2013/12/09/le-pen-met-les-immigres-au-chomage-force_965300 (accessed on July 15, 2017).

- **Official fact:** *According to the National Statistics Institute (INSEE) in 2015, 54.8% of the immigrant population were in the labor force (working or looking for a job) versus 56.3% for the rest of the French population. The rate of unemployment for the immigrant population is 18.1% against 9.1% for the rest of the population. There is therefore **44.9%** of the immigrant population that works (51.1% for the rest of the population).*

Argument 3: Refugees should really not flee but fight.

- **Alternative fact:** *MLP: “Everyone of us has good reasons to flee war, but there are also some who fight. Imagine during the Second World War, there were surely many French, believe me, who had good reasons to flee the Germans and yet, they went to fight against the Germans.”¹²*
- **Official fact:** *During the First and Second World Wars, the French fled war zones in much larger numbers than the current refugees. After the defeat of the French army in the North of France in the Spring 1940, 8 million civilians, that is one quarter (25%) of the population of the time, took the road to go to the South of the country that was not occupied (according to Jean-Pierre Azema, a renowned French historian).*

Some of the “alternative facts” statements are somewhat ambiguous because one is not sure whether the statement is a lie or not. For example’s MLP’s “99% refugees crossing the Mediterranean are men” claim could be considered to be a figure of style that just means “mostly men.” Moreover, MLP mentions that her evidence comes from pictures and is therefore not falsifiable. It is an open question whether our results would extend to the case of outright lies. We, however, believe that the type of statements that we study is highly relevant, as it is predominantly used by politicians in practice.

2.3.3 Setup of the experiment

In March 2017, one month before the first round of the presidential election, we conducted an online survey of 2480 French voting-age individuals using the Qualtrics online

¹²Source: <http://lelab.europe1.fr/refugies-comme-nadine-morano-marine-le-pen-prend-lexemple-des-francais-qui-sont-alles-se-battre-contre-les-allemands-pendant-la-seconde-guerre-mondiale-2515045> (accessed on July 15, 2017).

platform, an analogue of the Amazon Mechanical Turk. This platform is mostly used by companies to conduct market research. The survey respondents were drawn at random from a pool of Qualtrics subscribers, individuals who participate in online surveys for pay. The pool of potential participants of our survey was contacted by Qualtrics team via email. This email indicated the compensation fee upon completion of the survey and the link to it, which the participants could chose to click on. At the start of the survey, the participants were presented with a brief introduction to the survey indicating its focus on political preferences, voting intentions, and attitudes toward immigrants. It was also stated that only aggregate results would be published. There was no mention of any political party or political candidate. The introductory page allowed participants to drop out at that stage. The academic institutions to which we belong were not specified, since the participants might have inferred possible ideological biases of survey designers from that information. We describe the sample in detail in the next section.

The survey consisted of four parts. In the first part, we asked all participants a series of questions regarding their socio-economic characteristics, such as age, gender, education, income, religion. In addition, the first part of the survey included one question measuring the respondents' prior knowledge of facts related to immigration. In particular, we asked: "What do you think the unemployment rate among immigrants was in France in 2015?" The respondents were asked to pick their response from 10 intervals: (1): 0-10%, (2): 11-20%, ..., (10): 91-100%.

The second part of the survey varied across treatments. The participants were randomly allocated to four equally-sized groups. Each participant in three out of four groups was asked to read a short text before going to the third part of the survey. The texts were different across groups. In the Appendix, we present the full text of each treatment.

- *Control group (Control)* received no text to read, and the respondents were immediately directed to the third part of the survey;
- *Alternative facts group (Alt-Facts)* was presented with a one-sentence introduction ("You will read several statements by Marine Le Pen about migrants: their reasons for coming, the impact of migrants on French working and retired population; read them

carefully”), and then with quotes from MLP containing alternative facts, including those that we presented in the previous section, stating the exact date these statements were made;

- *Facts group (Facts)* was presented with a different one-sentence introduction (“*You will read below several numbers about migrants related to their reasons to come and their impact on French working and retired population; read them carefully*”) followed by the real facts corresponding to alternative facts from the MLP’s quotes, stating their official sources;
- *Fact-checking group (Fact-Check)* was first presented with the same text as the Alt-Facts group followed by exactly the same text as in the Facts group.

The third part of the survey was designed to measure voting intentions and attitudes toward MLP’s program. In addition to asking a set of questions regarding voting intentions, we carried out a list experiment. We also used two dictator games: the first one played with a random participant and the second played with a participant who reported that he/she was likely or very likely to vote for MLP.¹³

The fourth part of the survey examined opinions on the reasons for migration, asking the participants whether they thought migrants were coming for security or economic reasons and then tested the participants knowledge on the three main facts used in the study.¹⁴

2.3.4 Sample, balance across treatments and descriptive statistics

The sample was drawn from five French regions, presented in Figure 2.6 in the Appendix. These five regions were those with the highest score for the FN in the regional elections of 2015 (as presented on the left of Figure 2.7 in the Appendix) and were chosen to guarantee a sufficient proportion of MLP supporters among respondents. The regions are Hauts de

¹³The participants got no new information or payoffs in between the two games.

¹⁴The questionnaire translated into English is presented in the Appendix. The original survey in French is available online at: https://survey.eu.qualtrics.com/jfe/form/SV_cZ80nbVMLPTfvYFj (accessed on June 12, 2017).

France, Provence-Alpes-Côte d’Azur, Occitanie, Grand Est et Centre Val de Loire.¹⁵ Most of our sample comes from the region Hauts-de-France (35,8%), followed by Provence-Alpes-Côte d’Azur (26,1%) and Grand Est (19%).¹⁶ MLP indeed did relatively well in these regions in the 2017 election: they ranked 1st, 2nd, 3rd, 6th, and 7th out of 13 regions of mainland France in terms of MLP’s vote share in the first round of the presidential election (see the map on the right of Figure 2.7 in the Appendix).

We stratified our sample on education, age and gender by treatment. The sampling quotas were designed to make the sample as representative of the French adult population eligible to vote as possible.¹⁷

For a broad range of variables, Table 2.1 presents the means by treatment group (Columns 1 to 4 show the means in Alt-Facts, Fact-Check, Facts, and Control groups, respectively) and the p-values for the test of the equality of these means across different treatment groups (columns 5 to 10). In column 11, we correct for multiple hypotheses testing. The table suggests that the four randomized groups are largely balanced in observable characteristics. The largest imbalance that we observe is in the proportion of wage earners vs. pensioners: wage earners are 7 and 5 percentage points more frequent in the Fact-Check group and in the Facts group, respectively, compared to Control and the Alt-Facts groups; and there are no significant differences between Control and Alt-Facts groups and between Facts and Fact-Check groups. In all regressions that we present below, we control for a dummy indicating whether respondent is a wage earner as well as other socio-economic characteristics.

In line with the results of the European elections of 2014, regional elections of 2015, and the presidential elections of 2017 in the regions from which the sample was drawn, 22% of the sample voted for Marine Le Pen in the previous presidential election. Television is

¹⁵The region Bourgogne Franche Comté had a slightly higher score for the FN in the 1st round of the regional election than Centre Val de Loire, but this was an unexpected result due to the particularities of the race in the region. We thus chose Centre Val de Loire instead.

¹⁶The respective population of these regions in 2016 was Hauts-de-France 6 million, Occitanie 5.7M, Grand Est 5.5M, Provence-Alpes-Côte d’Azur 5M and Centre Val de Loire 2.6M. The unemployment rates in these regions was as follows in 2016: 12.2 for Hauts de France, 11.7 for Provence-Alpes-Côte d’Azur, 11.7 for Occitanie, 9.9 for Grand Est and 9.6 for Centre Val de Loire.

¹⁷Qualtrics allowed for three levels of quotas. We imposed quotas on gender (50% male, 50% female), on birth year (25% 1981 - 1989, 45% 1956 - 1980, 30% \leq 1955), on education (high school and below 72%, undergraduate degree 12%, graduate degree 16%).

the main source of information for the majority of respondents, that is 61% of the sample, whereas about 22% of the sample prefer to get information from the Internet and only 10% of the respondents use radio as their main source of information. In addition, we observe that our sample has a strong representation of Catholics (57%) and of those who reported no religion (37%). Table 2.6 in the Appendix provides summary statistics for the main variables of interest in the full sample.

2.3.5 Variables

2.3.5.1 Voting intentions

Participants were asked how likely they were to vote for MLP in the upcoming presidential election using a four-point scale (“very unlikely”, “unlikely”, “likely”, “very likely”). We created a binary measure of voting intentions that indicates whether the respondent self-reports that she is “likely” or “very likely” to vote for Marine Le Pen.¹⁸ To check whether self-reported measure is a valid measure of support for MLP, we use two additional methods to assess political preferences. A potential concern is the Bradley effect mentioned in the introduction. While underreporting of the intended vote for FN was a big issue for pollsters during the 2002 presidential campaign leading to a surprise qualification of MLP’s father for the second round of elections, underreporting is no longer quantitatively important: in the 2017 campaign pollsters applied the same intentions-to-vote correction factor to FN as to other parties and they were proven right to do so *ex post*.¹⁹ Nevertheless, we take this issue seriously and address it in two ways.

First, we use the list method (as described in Blair and Imai, 2012). Each respondent is randomly allocated to one of the two groups: participants in the first group are presented with a list of four key MLP’s competitors in the 2017 presidential elections:

¹⁸The mean of this voting intention outcome, namely, 37% is close to the vote share of MLP in the second round of the election where she obtained 34% of the vote.

¹⁹See, for instance, the articles published on June 2, 2016 in the French addition of *the Slate* magazine entitled “A taboo has fallen: the vote FN is no longer under-declared in the polls,” <http://www.slate.fr/story/118917/tabou-vote-fn-sondages> (accessed on September 29, 2017) and on April 24, 2017 in *the Guardian* entitled “Pollsters breathe sigh of relief after calling French election right,” <https://www.theguardian.com/world/2017/apr/24/french-pollsters-relief-after-calling-election-right> (accessed on September 29, 2017).

Francois Fillon, Benoit Hamon, Emmanuel Macron, Jean-Luc Melenchon (in random order). Participants in the second group are presented with a list of five candidates, which includes the four who appear in the list of the first group plus Marine Le Pen, also in random order. Then, all respondents, irrespective of which list they see, are asked how many politicians they support overall (see the exact formulation of the question in the Appendix). There are no questions *which* politicians the respondents support — the respondents only are asked to give the *number* of supported politicians. Due to the law of large numbers, the average difference in the number of supported politicians between the two groups reveals the average support of Marine Le Pen in the population.

The second approach is based on the dictator game with real payoffs. All participants played two dictator games in a row. In the first game they were asked how much out of 10 euros they would send to another randomly selected participant of the study. In the second game participants were asked how much out of 10 euros they would send to another randomly selected participant of the study among those who reported he/she was likely or very likely to vote for MLP. The difference in amounts transmitted between the first and the second game can be seen as a measure of support for MLP. The literature shows a strong in-group bias for supporters of the same party in such dictator games.²⁰

2.3.5.2 Past election outcomes

As it is often harder to influence voting intentions of those voters who once already voted for the candidate (Mullainathan and Washington, 2009), we asked respondents whom they voted for in the 2012 presidential elections. In order not to contaminate the experiment by framing effect or other aspects of cognitive dissonance, we asked this question after the experiment (in the third part of the survey). This, however, means that the answers to could potentially be affected by the treatment. We check this and find that the past vote for each candidate, including MLP, is balanced across treatment and control groups as reported in Table 2.1. 21.6% of respondents reported having voted for

²⁰For instance, Fowler and Kam (2007) found that Democrats and Republicans in the US both give more to the anonymous experiment participants from their own party than to those from the opposing party. In addition, they observed that independents give more to independents than to partisans, while partisans behave in the opposite way (see also Rand et al., 2009).

MLP in 2012, which is consistent with the aggregate election results for the regions in our sample.²¹

2.3.5.3 Prior knowledge

In order to test how the effects of alternative facts and fact checking depends on the knowledge of voters about the subject matter, we need a measure of prior beliefs. In the first part of the survey, before the experiment, all participants were asked about their beliefs on the rate of unemployment among the immigrant population in 2015. In particular, they were asked to chose their response from ten 10-percentage-point intervals. Unemployment rate among working-age foreign-born residents of France in 2015 was 18%, thus falling into the second category. Overall, 27.1% have a correct prior, 9.6% of respondents (238 people) underestimate the unemployment rate among immigrants, and 63.3% of respondents overestimate the unemployment rate among immigrants to a varying degree. 39% of respondents overestimate the unemployment among immigrants grossly, i.e., by at least two categories (believing that unemployment among immigrants is 31% or above).²² In the analysis below, we differentiate between respondents with “correct priors,” “overestimated priors” and “underestimated priors.” The priors are balanced across the four treatments as can be seen from the last four rows of Table 2.1.

Figure 2.8 in the Appendix present the histograms of the answers to the question on prior knowledge splitting the sample by the level of education, vote for MLP in

²¹We also asked whether respondents ever voted for the National Front in the past. In this variable, we find a small, but statistically significant imbalance: in each of the treatment groups, Alt-Facts, Fact-Check, and Facts, the share of those who voted for FN in the past is 33%, whereas as in control group, it is 38%. These differences are statistically significant but only if we do not correct standard errors for multiple hypothesis testing (see the last row of “prior voting behavior” section of Table 2.1). In order not to contaminate our analysis by controlling for a variable that potentially can be affected by the treatments, we do not control for whether respondents voted for FN in the past in our regressions. Note, however, that this imbalance (if it is a result of random realization) potentially could bias our results against finding positive effect of the treatments on the intention to vote for MLP compared to the control group. Consequently, our results are qualitatively similar, but stronger when this variable is included in the list of covariates (results are available upon request).

²²This is consistent with the results of polls that show that Europeans countries overestimate the presence of immigrants and their importance of the economy. See, for instance, the results of a study by Ipsos MORI, which shows that native populations of France, Italy, Belgium, Poland and Germany vastly overestimate the number of Muslims living in their countries, and that the largest misconception was in France: <https://www.theguardian.com/society/datablog/2016/dec/13/europeans-massively-overestimate-muslim-population-poll-shows> (accessed on October 12, 2017).

2012, rural/urban status, and level of regional unemployment. The figure shows that MLP supporters in 2012 elections, rural residents, residents in regions with higher unemployment rate, and less educated respondents are more likely to overstate the level of unemployment among migrants.

2.4 Results

The experimental design allows us measuring the impact of alternative facts and fact checking on voting intentions and understand whether it is driven by differences in knowledge of facts or by impressions about policy conclusions. We address the following questions: How do different treatments affect voting intentions? Do the participants learn factual information differently depending on who provides it? Does knowledge of facts translate into policy impressions, such as opinions on the reasons for migration? Do policy impressions translate into voting intentions?

2.4.1 The average treatment effect

Figures 2.1-2.5 provide an illustration of the main results by plotting the distributions of raw outcome variables across treatments. Due to randomization and balance across treatments, our empirical methodology is based on a simple comparison of means conditional on several covariates. In particular, to make the estimates more precise, as the baseline, we control for the conventional determinants of political preferences. We regress the outcomes on dummies indicating each of the three treatments, namely, Alt-Facts, Fact-Check, and Facts (our main variables of interest) controlling for gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, and dummies for having voted for each of the main candidates in the 2012 presidential elections. In all the reported results, we adjust standard errors for heteroscedasticity.

In Table 2.2, we present the baseline results for the main outcomes. Panel A of the table

presents the regression results. Column 1 shows that the exposure to MLP's rhetoric, *with or without* fact checking from official sources, results in additional 5 percentage points in terms of intention to vote for MLP relative to the control group. Thus even in the presence of fact checking, alternative facts do deliver political benefits for the populist politician. Moreover, exposure to facts from official sources positively affects voting intentions for MLP, with a 3 percentage point difference between Facts and the Control groups, even though this difference is not significant.

The last four rows of panel A of Table 2.2 report the p-values of the tests for the equality of the effects between different treatments (Alt-Facts vs. Fact-Check; Facts vs. Fact-Check; and Alt-Facts vs Facts) and of the test for whether the coefficient on the Fact-Check treatment is equal to the sum of the coefficients on the Alt-Facts and Facts treatments. The point estimates of the effects of the Alt-Facts and Fact-Check treatments are virtually identical. The point estimate of the effect of Facts treatment is substantially smaller in magnitude than that of the other two treatments; however, we cannot reject the equality of the effects across all three treatments.²³ The magnitude of the effect of Alt-Facts and Fact-Check treatments is large compared to the average intention to vote for MLP in the Control group, which is equal to 37.3% (as reported at the bottom of the table), but it is consistent with the immediate effects of political campaign ads on voting intentions found in the literature.²⁴

In Table 2.7 in the Appendix, we show the effect of including controls on the point estimates and their standard errors. Columns 1 to 4 focus on voting intention outcome. In column 1, there have no controls apart from the variables that determined our sampling strategy: gender, age, education, and region dummies. In column 2, we add only the individual-level controls. Column 3 presents our baseline specification, i.e., including controls for voting in 2012 presidential elections. In column 4, in addition to baseline

²³As we discuss below, the fact that the effect of Alt-Facts is not significantly different from the effect of true Facts on voting intentions suggests the importance of salience as a mechanism explaining the impact of Alt-Facts.

²⁴The magnitudes are also comparable to those reported by Bartels (1996) who analyzes survey data on the actual voting in the U.S. presidential elections and shows that the incumbent candidate's vote share would have been five percentage points lower if all voters were "fully informed." He shows that the informed voters are more likely to vote right (Republican) rather than left (Democrat): the Republican candidate would have had two percentage points higher score if all voters were "fully informed."

controls, we include the full set of interactions between the demeaned measures of past voting behaviour and treatment dummies into the list of covariates.

We find that the results for voting intentions are not statistically significant without controls for past voting behaviour. The inclusion of these controls with or without interacting them with treatment dummies reduces standard errors by about 15%, which makes a difference for the statistical significance of the average treatment effects on voting intentions for the Alt-Facts and Fact-Check treatments. The magnitude of the coefficients without controls is such that the effects would have been significant if the level of standard errors was as in the specification with the past-voting controls. The question is whether adding controls beyond strata dummies to an RCT, like ours, is a valid empirical strategy. In a general case, even when the treatment is uncorrelated with controls—which the balancing tests show to be the case in our data—adding controls to RCT could lead to an underestimation of standard errors (Freedman, 2008). However, Lin (2013) shows that this does not occur when samples are sufficiently large and covariates are balanced across treatments. Furthermore, he shows that OLS estimates generate asymptotically valid confidence intervals and consistent point estimates when a full set of treatment-covariate interactions is included. The comparison between columns 3 and 4 of Table 2.7 shows that the estimates are virtually the same in specifications with baseline set of controls and with interactions of controls with treatments in addition to the baseline. This suggests that the problem described in (Freedman, 2008) does not apply to our setting.

Note that previous voting behaviour is an important determinant of voting intentions. In particular, having voted for MLP in the past is a single most important determinant of voting intentions. In the control group, among those who reported having voted for MLP in the past, 81% report intention to vote for her in 2017, whereas among those who did not vote for MLP in 2012, only 24% intend to vote for her in 2017. Left panel of Figure 2.1 illustrates the average voting intention across treatments in the absence of controls.²⁵

The comparison of the effects of Alt-Facts and Fact-Check treatments suggests that

²⁵Figure 2.9 in the Appendix presents differences in voting intentions across treatments separately for those who reported having voted and not having voted for MLP in the past. The figures show that, qualitatively, the effects of the treatments are similar, but the level of intention to vote for MLP is drastically different.

fact checking is completely ineffective in undoing the persuasion effect of populist arguments based on alternative facts: both of these treatments, on average, increase the voting intention by 5 percentage points. Does this mean that fact checking fails in communicating the facts or that voters distrust official sources more than MLP? In columns 2-5 of Table 2.2, we address this question. In column 2, the dependent variable is the absolute value of the distance between individual (posterior) responses and the true value for the proportion of men among refugees crossing the Mediterranean. In column 3, it is the absolute value of the distance between the responses and the true value for the share of working among migrants. We find that participants do learn the statistical facts when the facts are provided to them. Both alternative facts and facts are effective but participants attach a much higher weight to the official sources compared to MLP. The absolute value of the distance to true value for both questions decreases substantially after the Facts treatment and slightly increases after the Alt-Facts treatment; both effects are statistically significant. The absolute value of the point estimate is much smaller for Alt-Facts treatment than for the Facts treatment. Furthermore, the Fact-Check treatment significantly reduces the absolute value of the distance to truth compared to the control group, suggesting that information from official sources dominates the effect of alternative facts. The effect of the Fact-Check treatment on the distance to truth is similar in magnitude to the sum of the positive effect of the Facts treatment and the negative effect of the Alt-Facts treatment.

We compare the shares of participants who report the correct answers across treatments in columns 4 and 5. Alt-Facts treatment does not significantly affect the probability of being correct on either of these factual questions in sharp contrast to both Facts and Fact-Check treatments. The comparison between the results presented in columns 2 and 3 vs. columns 4 and 5 implies that MLP manages to change the opinion about the facts mostly among those who did not know these facts to begin with. We explicitly test this hypothesis below.

Facts and Fact-Check treatments increase the probability of a correct response about the share of men among refugees by 44 and 31 percentage points from the 16% mean (i.e., the share of correct responses in the control group) and increase the probability of

a correct response about the share of working among migrants by 38 and 26 percentage points from the mean of 8%.

The results about the effect of treatments on posterior knowledge are not sensitive to the choice of covariates as shown on Table 2.8 in the Appendix. We illustrate how respondents update their posteriors on facts as a results of the treatments without any controls in Figures 2.2 and 2.3. The figures present the distributions of answers to the questions on the proportion of men among refugees and on the share of working among migrants across treatments. We do observe that the mass of respondents moves slightly toward the alternative facts in the Alt-Facts treatment and moves substantially towards the true facts in Facts and Fact-Check treatments, as compared to the control group.²⁶

The evidence presented so far shows that fact checking moves voting intentions and posteriors on facts in the opposite directions. In column in column 6 of Table 2.2, we examine how the treatments affect voters' impressions about the reasons for refugees' migration. Respondents in both the Alt-Facts and the Fact-Check group are more likely to believe that migrants come for economic reasons. Fact checking corrects the factual knowledge, but does not correct the policy conclusions advocated by MLP. The fact-checking treatment increases the belief that refugees come for economic reasons by 7 percentage points and alt-facts treatment by 13 percentage points (compared to the 32% mean in the control group.) The Facts treatment does not affect the policy-relevant impressions at all. We illustrate these findings in Figure 2.4 and show that they are also unaffected by controls in columns 4 to 6 of Table 2.7 in the Appendix.

Finally, column 7 of Table 2.2 shows that the discourse of MLP (Alt-Facts) makes people more likely to agree with her on immigration policy. Participants in the Alt-Facts group are 5 percentage points more likely to agree with MLP than those in the

²⁶Table 2.9 and Figure 2.10 in the Appendix present the results for the effect of the treatments on the respondents' knowledge about the percentage of French population that fled to the South during the Second World War. We find no significant effect of any of the treatments for the absolute value of the distance to truth, but for the probability of the correct response, treatments have similar effect as for getting correct responses on other factual questions: Alt-Facts had no effect, while Facts and Fact-Checking groups have significantly higher rate of correct responses (by 11 and 14 percentage points, respectively) compared to the Control group, in which 5% of respondents gave the right answer. Note, however, that on this particular question, MLP did not provide an actual alternative figure but just suggested that the French had not fled but had fought during the war. We relegate these results to appendix because there are no explicit alternative facts.

control group. The rate of agreement with MLP in Fact-Check and Facts treatment is not statistically different from that in the control group. Yet, both coefficients have positive signs. Panel B of Table 2.2 presents persuasion rates of treatments for each of the binary outcomes. In particular, the persuasion rate of MLP's narrative with or without fact-checking on voting intentions for her candidacy is about 8%. As for the beliefs about the reasons for migration, alternative facts are about twice as persuasive as alternative facts accompanied by fact checking (12 vs. 7%).²⁷

As with voting intentions, for the agreement with MLP on immigration policy, standard errors are substantially smaller when we control for past voting (see results in column 9 of Table 2.7 in the Appendix as compared to column 7). The reason for this is that the agreement with MLP is also strongly affected by the past voting behaviour. Figure 2.5 illustrates the unconditional treatment effects for this outcome.

Overall, we find that alternative facts treatment does convince voters to vote for MLP, fact checking corrects the beliefs about facts but does nothing for voting intentions and only partially corrects policy conclusions of voters, the fact treatment has no significant effect on average on voting intentions or policy conclusions, but corrects posterior knowledge.

2.4.2 Heterogeneity with respect to the prior knowledge

Priors should matter for Bayesian updating. The variation in prior beliefs about the unemployment rate among migrants allows us to study the heterogeneity of the effects of the treatments with respect to prior knowledge. As described above, we measure the correctness of the prior with three dummy variables: correct, overestimated, and underestimated unemployment rate among migrants. The numbers of respondents with these types of priors are 672, 1,570, and 238, respectively.

Experimental design limited solicitation of the prior to one question only in order to avoid framing. Moreover, for that same reason, the prior and the posterior beliefs

²⁷The magnitudes of these persuasion rates are similar to those found in comparable papers, see Figure 2.11 in the Appendix. In their survey of the empirical literature on persuasion, DellaVigna and Gentzkow (2010) list thirteen estimates of persuasion rates for studies of persuading voters in different contexts. These estimates range from 1% to 20% with the mean of 10% and the standard deviation of 6%.

are about related, but not exactly the same questions. We start with documenting that the prior about the unemployment rate among migrants is a good proxy for the priors about the share of men among refugees and about the percentage of working migrants. We have information about the priors on all three dimensions of knowledge in the control group, as posteriors were solicited in the absence of any treatment. Table 2.10 presents the correlation in the control group between the answers to all three questions about facts. It shows that respondents with overestimated prior about unemployment among migrants are also more likely to believe that there is a larger share of men among refugees and smaller share of migrants working. This is true both at the extensive margin (Panel A compares the average beliefs for correct and overestimated priors on migrant unemployment rate) and intensive margin (Panel B shows the significant correlation of among the 10-category measures of knowledge). In all regressions, we control for the dummy for underestimated prior, for which we do not find significant differences from correct priors. This evidence suggests that we can use the correctness of the prior to test for heterogeneity in treatment effects.

We take the specification presented in Table 2.2 and add to it the dummies for correct and underestimated priors and their interaction terms with treatment dummies (leaving the respondents with overestimated priors as the comparison group). The results for the main outcome of interest, voting intentions, and for the posteriors on facts, for which priors should matter most, are presented in Table 2.3. The coefficients on the treatment dummies estimate the treatment effects for the respondents with overestimated priors. Column 1 focuses on voting intentions as outcome variable. It shows that there is a large and significant effect of all three treatments, including the Facts treatment, on the voting intention for MLP. In this group of voters the mean voting intention in control group is 41%, Alt-Facts, Fact-Check, and facts treatments increase the self-reported voting intention by 8.2, 6.8, and 4.8 percentage points, respectively. The coefficients on the interaction of treatment dummies with the dummy for correct prior are negative and large in magnitude, implying that the point estimates of the treatment effects for the correct-prior group are negative 4 percentage points for Alt-Facts and Facts and about zero for Fact-Check treatment. The effects of treatments on respondents with correct priors are

not statistically significant. The difference between the effects for informed respondents and uninformed respondents, who overestimate the unemployment among migrants, is significant for the Alt-Facts treatment and imprecise for the other two treatments. Nonetheless, the estimates of the treatment effects for the respondents with overestimated priors are large and precise. Right panel of Figure 2.1 illustrates the unconditional means of voting intentions by treatment in this group of voters. The fact that the results are larger and more precise for those who hold overestimated priors is consistent with the salience explanation (considered in the following section), which implies that the topic becomes particularly salient when the truth is far from the prior (e.g., Bordalo et al., 2012, 2013). The estimates for the 238 respondents who underestimated the prior are not precise, so that we cannot conclusively differentiate them from respondents with correct or overestimated priors.

In columns 2 to 5 of Table 2.3 we examine how the priors affect updating beliefs about facts following the treatments. The most striking result is the difference between the effects of the Fact-Checking treatment on respondents with correct and with overestimated priors: the informed respondents update in the direction of true facts a lot more than the uninformed respondents. Note also that Alt-Facts treatment has a precisely-estimated zero effect on the the probability to get a correct posterior on the share of men among refugees and on the share of migrants working among those respondents whose prior is correct. This evidence suggests that respondents behave as Bayesian updaters, who have higher confidence in the official sources than in MLP, when they update their knowledge of facts. Figures 2.12 and 2.13 in the Appendix provide further evidence in that regard. They show how the non-parametric relationship between the prior and the posterior is affected by the treatments. For every prior, the Facts and the Fact-Check treatments lower the posterior on the share of men among refugees (with a stronger effect of the Facts treatment), whereas the Alt-Facts treatment increases respondents' posteriors about the share of men among refugees and about immigrants' employment rates. Overall, we find overwhelming evidence that participants learn the facts whenever exposed to them.

2.4.3 Interpretation

To sum up, our main findings are as follows: fact checking corrects posterior knowledge of facts, but does not undo the strong persuasion effect of alternative facts and the effects are stronger for uninformed voters, such that all treatments lead to a significant increase in intention to vote for MLP among respondents with overestimated priors. While we cannot establish all the exact mechanisms driving these results because our experimental design is not suited for testing between alternative mechanisms, below we discuss whether the non-experimental evidence is consistent with four potential explanations behind these results. We first examine whether our results could be explained by two purely technical explanations: the non-linear relationship between facts and voting intentions and Experimenter Demand Effects and conclude that they cannot be. Second, we consider two potential mechanisms: (1) treatments are viewed by voters as two-dimensional signal: providing information about the quality of the candidate and about facts and (2) treatments increasing the salience of the immigration issue. We argue that both of these potential channels can be at play, but the salience mechanism is necessary to explain all pieces of evidence.

2.4.3.1 Nonlinearities in mapping facts to votes

The conflicting effects of fact-checking treatment on posteriors about facts and on voting intentions could emerge if the relationship between facts and voting intentions were highly non-linear. To illustrate this, suppose that the support for MLP depends only on the beliefs about the unemployment rate among immigrants. Suppose further that voters have a simple decision rule in which they vote for MLP if they think that the unemployment among immigrants is above 10%. If the prior is uniformly distributed, the average belief about the unemployment among migrants in the control group would have been 50% and the share of MLP supporters would have been 90%. Further, suppose that voters have full confidence in the official figures, which means that in the Facts and Fact-Check treatments they learn that the unemployment rate among immigrants is 18%. In that case, the average posterior beliefs would have converged to the true value, i.e., would

have fallen from 50 to 18%, but the voting intentions would have increased from 90 to a 100%. This theoretical possibility is, however, not supported by our data. In Figure 2.14 in the Appendix we plot the unconditional non-parametric relationships between factual knowledge in the control group and the likelihood of voting for MLP; this exercise does not reveal any striking nonlinearities, suggesting that this mechanism is not at play.

2.4.3.2 Experimenter demand effects

Large magnitudes of effects in experimental studies may be driven by the Experimenter Demand Effects (EDE) (Zizzo, 2010), such as the Hawthorne effect.²⁸ Even though it is difficult to rule out such effects formally, they seem unlikely for the outcome of voting intentions in our study for three reasons. First, for the demand effect to be the main driver of the magnitude, the participants would have to infer from the way we present the evidence on MLP (which was rather neutral) that we actually want them to express support for MLP. Second, to generate the comparison between Alt-Facts and Fact-Check treatment, they would in addition need to infer that the facts can be ignored when they report voting intentions. Note that it was very difficult to make inferences about our own preferences based on the experiment's introduction.²⁹

2.4.3.3 Signal about the candidate

It could be that case that the MLP's statements provide information about the candidate herself in addition to numbers and the narrative's conclusion. If this information is positive, the treatments could lead to a boost in MLP's electoral support irrespective of the treatments' impact on the posteriors on facts. One possibility is that the narrative based on numbers makes MLP look more competent. If the prior of a median voter is that she is not familiar with statistical facts, the quotes in the treatment may impress the

²⁸See, however, Mummolo and Peterson (2018) who show that in studies like ours EDEs are actually uncommon.

²⁹One cannot completely rule out experimenter demand effects for the posteriors on facts if the respondents believed that the survey designers shared the official rather than MLP's version of facts, despite the fact that there was no indication of experimenter preferences or affiliations presented to the participants. Yet, if the pro-establishment EDE were present for the facts treatment, they should have worked in the opposite direction to our findings for the voting intentions, making participants less likely to report voting intentions for MLP in all treatments containing the official facts.

respondents with MLP's command of statistics. Alternatively, the MLP's rhetoric could signal that she has different policy positions from her father, who was the leader of the National Front before her and held extreme nationalistic views. As the MLP's quotes justify the tough immigration policy by economic needs rather than outright xenophobia (which was the case for her father), they could be seen as a positive signal and as a result increase her political support.

Updating on facts and quality of the candidate separately could explain why MLP's rhetoric in Alt-Facts treatment is effective in changing voting intentions in her favor compared to the control group. Is this mechanism consistent with the results for the other treatments? The respondents appear to accept the Fact-Check correction of the numbers (as documented in Table 2.2), and so believe that MLP is proven wrong. If competence is the quality of the candidate, on which the voters update, Fact-Check treatment should decrease their propensity to vote for her, as they learn that her numbers are not correct after all.

If the quality that the voters update on is MLP's distance from her father's views, Fact-Check should be completely ineffective because what is important is that MLP views immigration as an economic problem rather than a threat to French national identity. The actual numbers used in MLP's narrative are irrelevant for the conclusion how extreme or moderate her policy positions are. Thus, updating on MLP's distance from her father in addition to numbers is consistent with the results for both Alt-Facts and Fact-Check treatments.

However, updating on the quality of MLP as a candidate cannot explain why Facts alone have a positive impact on MLP's vote share among the uninformed voters, since MLP is never mentioned in this treatment.

2.4.3.4 Salience

Finally, we consider a possibility that the effects of the Fact-Check and Facts treatments could be driven by raising salience of the immigration issue in voters' minds. Thinking about immigration may bring about fears associated with it and, therefore, could shift voters closer to MLP's agenda, who has always identified immigration as the top

issue of her agenda. The salience mechanism can explain that the Facts treatment significantly increases the propensity to vote for MLP among voters who overestimate the unemployment among migrants since these voters presumably have higher fears associated with immigration.

Below, we discuss whether the salience mechanism is consistent with the results in the full sample. We start by showing that beliefs about facts are related to political outcomes independently of the treatments. In order to do so, we regress the three political outcomes (voting intentions, beliefs of respondents about the reason for refugees to come to France, and the general agreement with MLP on immigration policy) on the individual beliefs about the share of men among refugees and the share of working among migrants, focusing only on the Control subsample. Columns 1, 3, and 5 of Table 2.4 present the results. In the absence of any treatment, all three outcomes are significantly associated with stronger beliefs that refugees come for economic reasons and that immigrants do not work.³⁰

Given this relationship, the salience mechanism implies the following testable predictions. After controlling for posteriors on facts, all the treatments—including the Facts treatment—should have a positive effect on the support for Marine Le Pen in the full sample. This is because all three treatments, including the Facts treatment, attract voters' attention to the issue of immigration. Furthermore, controlling for posteriors on facts, the effects of both the Facts treatment and the Fact-Check treatment should be larger in magnitude than without such a control because these treatments make people update away from the belief that immigrants pose a threat to them. In contrast, the effect of the Alt-Facts treatment should decrease in magnitude with the inclusion of the controls for the posteriors on facts because this treatment moves factual beliefs in the anti-immigrant

³⁰In Figures 2.14, 2.15, and 2.16 we present the relationships (again, for the Control group) between, on the one hand, the factual beliefs (on unemployment among migrants, their employment rates and share of men among refugees) and, on the other hand, the voting intentions, agreement with MLP on immigration issues, and the belief that immigrants come for economic reasons. The graphs are generally consistent with the results in the Columns 1, 3, and 5 of Table 2.4. An important takeaway from Figure 2.16 is that the French voters interpret the “economic reason” as the risk that immigrants come to abuse France’s generous welfare system rather than to “steal jobs.” If the latter were the case, the voters would be more likely to believe that the reason to migrate is economic whenever they believed that most migrants actually work and are never unemployed. This is not what the second and the third charts in Figure 2.16 show.

direction.

Columns 2, 4, and 6 of Table 2.4 test and confirm these predictions. Controlling for the (posterior) beliefs about the share of men among refugees and the share of working among immigrants, Facts treatment, despite having no negative content about refugees, makes people significantly more likely to report intention to vote for MLP, more likely to agree with her on immigration policy and more likely to believe that refugees come for economic rather than security reasons. The magnitudes of both Facts and Fact-Check treatments conditional on posteriors on facts are larger than without this control (which can be seen from comparing the coefficients on treatment dummies in Tables 2.2 and 2.4) and the opposite is true for the effect of the Alt-Facts treatment, which, nonetheless, remains positive for all outcomes and statistically significant and rather large for the belief about the economic reason for refugees to come.

The salience mechanism explains why fact checking is ineffective: the effect of the shift in factual knowledge, which makes voters move away from the anti-immigrant policy position, is compensated by the increased salience of the issue of immigration.

Overall, the salience mechanism can explain all our results, possibly in combination with the mechanism related to updating on MLP's degree of extremism.

2.5 Additional results

2.5.1 Credibility of self-reported voting intentions

In the analysis above we proxied the support for Marine Le Pen by the self-reported voting intentions. In this section we check the validity of this measure.

2.5.1.1 Evidence from the dictator games

In order to check whether the self-reported voting intentions are not a cheap talk, we administered two dictator games involving real payoffs to survey participants (see section 2.3.5.1). In the first game, every respondent was given a 10 percent chance to win 10 euros. He/she was *ex ante* requested to decide which part of this prize he/she would

share with another, randomly selected respondent. The second game was exactly the same except that respondents were told that they are sharing the money with another participant randomly selected among those who reported that they were likely or very likely to vote for MLP in the upcoming election. 42% of respondents did not share any money with a random counterpart; 50% of respondents did not share money with a MLP supporter; 18.5% of respondents decided to share a higher amount with a potential MLP voter than with a random participant; 13.2% of respondents chose to give some money to a random participant but gave nothing to a MLP supporter.

In Panel A of Table 2.5, we examine how donations in these dictator games are related to self-reported voting intentions and whether outcomes of dictator games were affected by the treatments. In column 1 we show that the amount given to a MLP supporter is highly correlated with self-reported willingness to vote for MLP. Column 2 shows that the individuals reporting intention to vote for MLP are less likely to make a donation to a random participant and are more likely to give to another MLP supporter. As we express donations in euros (with the potential range from 0 to 10), a one euro increase in a donation to a MLP supporter, conditional on the amount donated to a random counterpart, is associated with additional 3.9 percentage points in the probability to vote for MLP. In column 3, we show that those who shared monetary payoffs with a random participant, but gave no money to an MLP supporter are 18.4 percentage points less likely to be supporters of MLP themselves. These results suggest that the self-reported voting intentions do reflect the real preferences of respondents.

The last two columns Panel A of Table 2.5 examines differences in the outcome of dictator games across treatments. In column 5, we show that there is no significant effect of treatments on the amounts donated to the MLP supporters in the second dictator game. Column 6, however, shows that people who donated a non-zero amount to a random counterpart and gave strictly zero a MLP supporter are significantly less frequent in Alt-Facts group. Among those who gave non-zero amounts in the first dictator game, those who received Alt-Facts treatment are 3.5 percentage points more likely to give to MLP supporters as well. The effects of other treatments on this outcome are imprecisely estimated, but have the same sign as the effects of treatments on voting intentions.

Given that the overall rate of donations is rather small, and therefore, one would need very large samples to detect significant differences across treatments, we take this evidence as supportive of the conclusion that we can rely on voting intentions as an informative measure of political preferences. Another reason to use the survey question rather than the approach using the dictator game is that donations are on average low, even in the first dictator game where 41.7% of the participants transferred 0, compared to the standard results in the literature (Fowler and Kam, 2007; Rand et al., 2009). It is worth noting that there are two differences between our setup and the conventional dictator games. First, we stated that there was one chance out of ten that participants would actually receive the amount and have the transfer implemented. Second, the amounts were expressed in Qualtrics points rather than euros, yielding higher nominal amounts.³¹ Both differences might account for the nonstandard behavior of our subjects in the dictator game. Future research could use the behavior of the dictator game as an outcome variable with larger samples and a more standard version of the dictator game.

2.5.1.2 Evidence from the list experiments

We use the results of the list experiment (see section 2.3.5.1) as yet another check of the validity of self-reported voting intentions. Panel B of Table 2.5 reports the results. In the first column, we regress the response about the total number of supported politicians from the list on a dummy indicating whether the list contained the name of Marine Le Pen. The estimated coefficient on this dummy equals 0.44. This implies that in our sample about 44% of the respondents support MLP. This is slightly higher than 39% share of those who self-reported their intention to vote for MLP. This difference may mean that about 5% of voters do support MLP but are not willing to openly declare intentions to vote for her. However, this difference may also be due to the difference in the formulations of the list experiment's question ("overall support of the politician's program") and the voting intention question ("intention to vote"). On that point, we note that the percentage of participants reporting 0 candidates in the list without MLP is 35% while it is 18% in

³¹10 euros is equivalent to 2500 Qualtrics points. These points are used also to reward the participation in the survey and can be used as currency with the Qualtrics partners.

the list with MLP. The difference between these two figure corresponds closely to the percentage of individuals reporting to be very likely to vote for MLP, suggesting that many participants considered a rather conservative interpretation of “overall support of the politician’s program.”

In columns 2 and 3 of Table 2.5 we check whether support for Marine Le Pen inferred from the list experiment is higher among those who declared an intention to vote for her. In particular, we repeat the exercise presented in column 1 separately for the subsample of those who did and who did not declare intention to vote for MLP (columns 2 and 3, respectively). As expected, the inferred level of support for MLP is much higher among those who self-report their support of her: 91.5% vs. 12%. To show that this difference is statistically significant we use the whole sample and add the voting intention dummy and its interaction with the dummy for the list with MLP to the set of covariates (in column 4). The coefficient on the interaction term is highly statistically significant. The confidence interval for the inferred support for MLP among those who self-declare the intention to vote for her is $[0.79; 1.04]$ and therefore includes 1. Thus, we cannot reject the hypothesis that everyone who reported intention to vote for MLP supported her in the list experiment.

Finally, in the last column of Table 2.5, we report the estimates of the inferred support for MLP in each of the treatment groups and in the control group. The sample size is not sufficiently large for the differences in the inferred support for MLP to be significantly different across treatments, but the differences in magnitudes of point estimates are consistent with the effects of the treatments on voting intentions. The inferred support for MLP is the lowest in the control group, and is equal to 38%. It is 46% in both Alt-Facts and Fact-Checking groups, and it is 45% in the Facts group. (Formal tests cannot reject equality of any of these numbers.) Overall, the list experiment’s results also suggest that the self-reported voting intentions are rather reliable.

2.5.2 Heterogeneity with respect to other observables

Tables 2.11 and 2.12 in the Appendix explore potentially relevant dimensions of heterogeneity of treatment effects on the main political outcomes (voting intentions, the dummy for a belief that refugees come for economic reason, and a dummy for agreement with MLP on immigration policy) and on posteriors on facts (absolute value of the distance to truth on the posterior beliefs about the share of men among refugees and absolute value of the distance to truth on the posterior about the share of working among migrants). Each panel of these Tables presents the coefficients on the interaction terms between each treatment and a particular characteristic from five different regressions. We also present the coefficients estimating direct effects of these characteristics in the control group, when they matter for interpretation of the results about the treatment heterogeneity.

In Panel A of Table 2.11, we show that having voted for MLP in the past does not interact with treatments despite being an important determinant of voting intentions. In Panel B, we show that those individuals who get their news mainly from TV (about 60% of the sample) are more responsive to MLP's arguments when it comes to voting intentions and posteriors on the reasons for refugees to come. In contrast, Panel D shows that Alt-Facts treatment is less effective on those who get their news from internet (20% of the sample). Panel D shows that those who get most of their income from social security and pensions (35% of the sample) are, on average, more inclined to vote for MLP, but their voting intentions are less sensitive to any of the treatments than for the rest of the population. In Panel E, we show that having completed secondary education (62% of the sample) makes people adjust their posteriors more toward the truth after being exposed to official information in facts and fact-checking treatments, but does not affect sensitivity of respondents' voting intentions to treatments.

Panel A of Table 2.12 shows that individuals with higher income tend to be more sensitive to official information in the Fact-checking and Facts treatment, which makes them less likely to believe that refugees come for economic reason. The rest of the Table 2.12 shows no heterogeneity of treatments' effects with respect to age, gender, being a second-generation immigrant (we have no first-generation immigrants in the sample),

self-reported score on the left-right political axis, or regional-level election results.

2.6 Concluding remarks

We report the results of an online randomization experiment to measure the persuasion power of alternative facts and the effectiveness of fact checking to counter their impact. We find that fact checking can correct biases in factual knowledge introduced by politically-charged alternative facts. Voters update their priors as rational Bayesian updaters with greater confidence in official sources than in politicians providing alternative facts. On the other hand, the fact checking's success in correcting factual knowledge does not translate into an impact on voting intentions. Alternative facts are equally effective with and without fact checking in convincing voters to vote for the politician who uses narratives based on alternative facts.

We cannot definitively establish the mechanism behind these results, but the evidence is consistent with a hypothesis that mentioning the immigration issue in alternative facts or real facts statements raises salience of this issue in voters minds, which in turn moves some voters toward anti-immigration policy agenda. In addition, it is possible that alternative-facts narrative in our experiment was interpreted by voters as a signal not only about the state of the world, but also about the candidate herself.

Taken together, our results suggest that providing the correct statistical evidence is not sufficient to counter the effect that populist politicians have on voters. When a statistical fact is used in a narrative presenting a logical link to reach a conclusion, fact checking would presumably need to question the policy conclusion, using the correct facts, logical links and narratives. In our experiment, fact checking is the exposure of voters to raw facts from official sources. In practice, fact checkers do sometimes produce longer analyses and discussions of facts. For instance, the article in newspaper *Le Monde* which fact checked the statement by Marine Le Pen on the proportion of men among refugees, embedded true facts in a short narrative containing several paragraphs. The exposure to this sort of fact checking may have a different effect from the one demonstrated by our experiment.

The effects of counter-narratives are understudied and should become subject of future research. However, if salience is, indeed, one of the mechanisms behind our results, the main conclusion of our paper should hold even in the case of a counter-narrative: by insisting on the same issue as the original political communication based on alternative facts, fact checking may contribute to an increase in the salience of this issue, which may indirectly serve the goal of the original communication. This assertion, however, needs to be verified by future research as the salience mechanism should be studied directly in experimental setting.

Figures

FIGURE 2.1: Voting intentions, by treatment

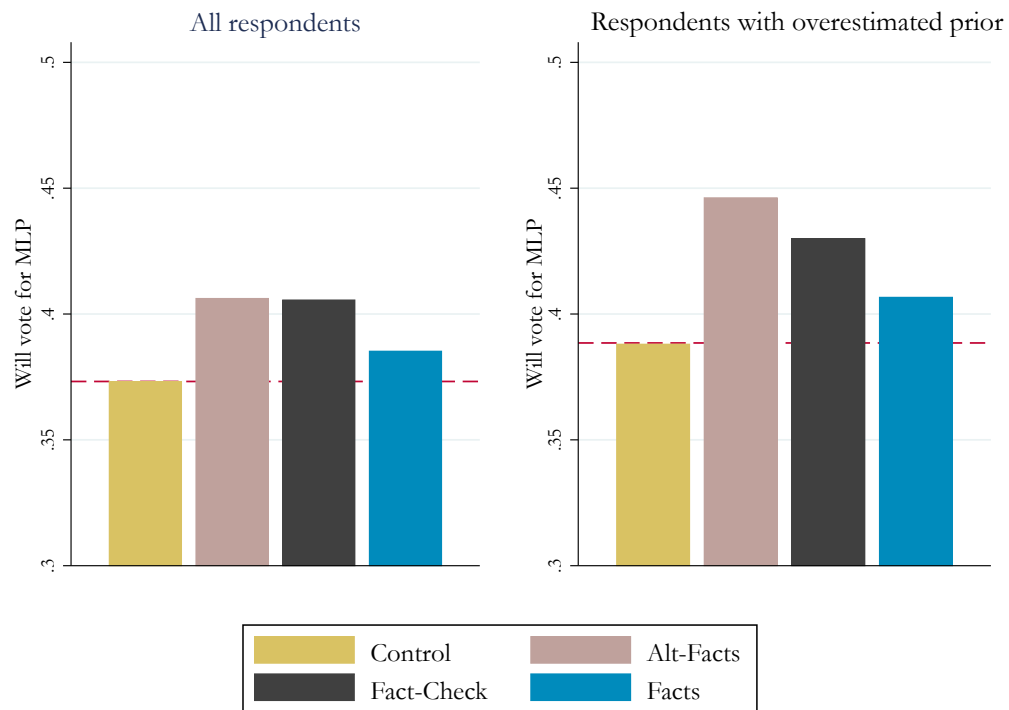
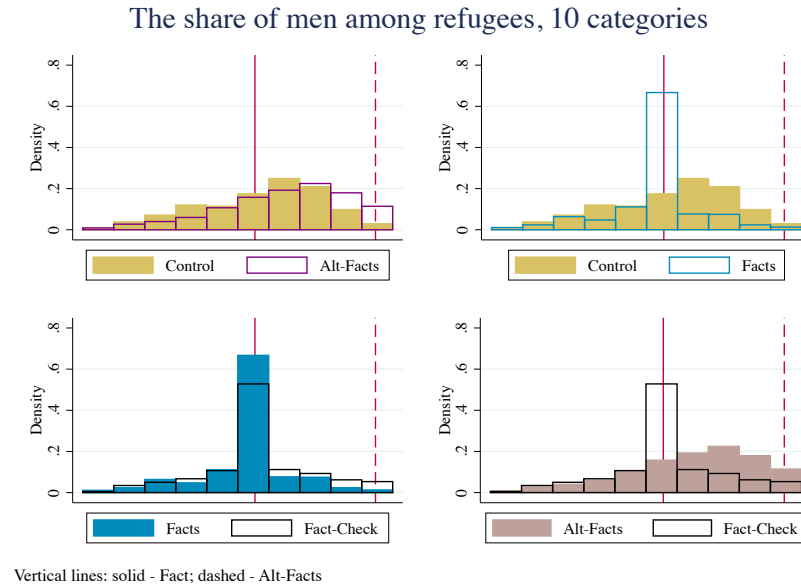
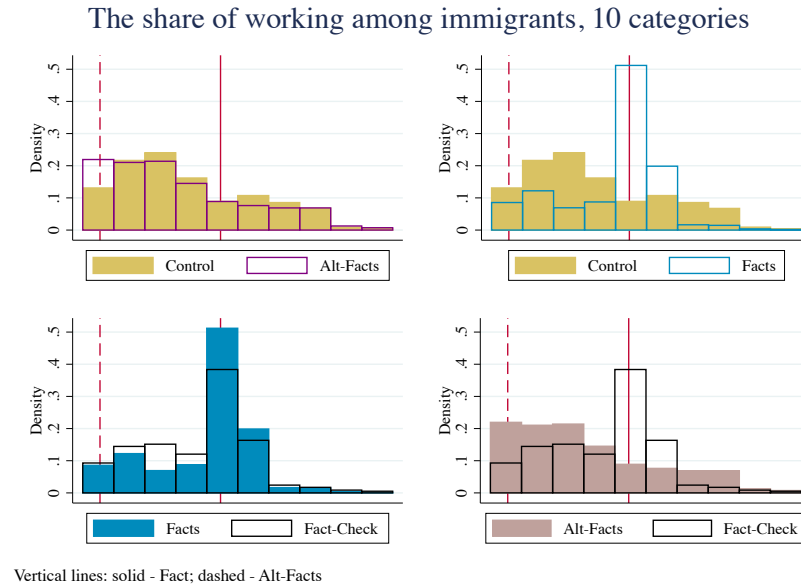


FIGURE 2.2: Posterior beliefs on proportion of men among refugees



Note: Horizontal axis represents the 10 percentage point intervals for the proportion of men among refugees.

FIGURE 2.3: Posterior beliefs on the share of working among migrants



Note: Horizontal axis represents the 10 percentage point intervals for the share of working among migrants.

FIGURE 2.4: Reported reasons for migrants to come

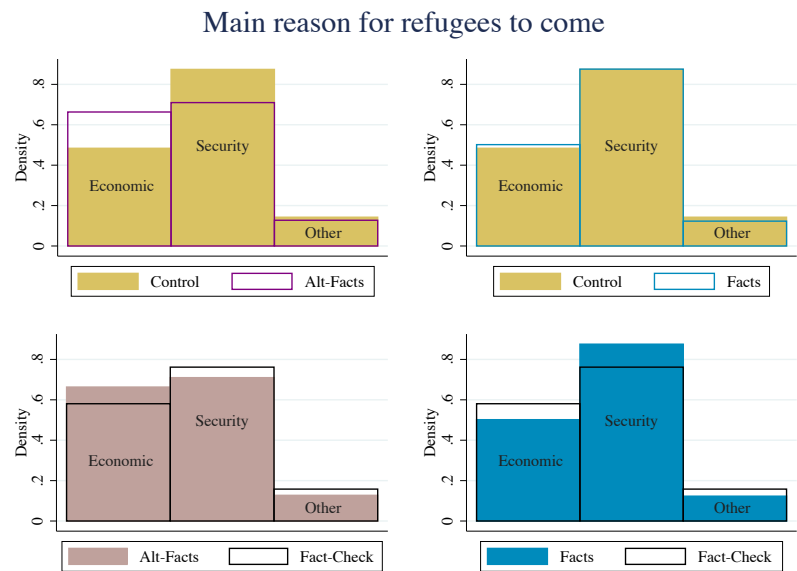
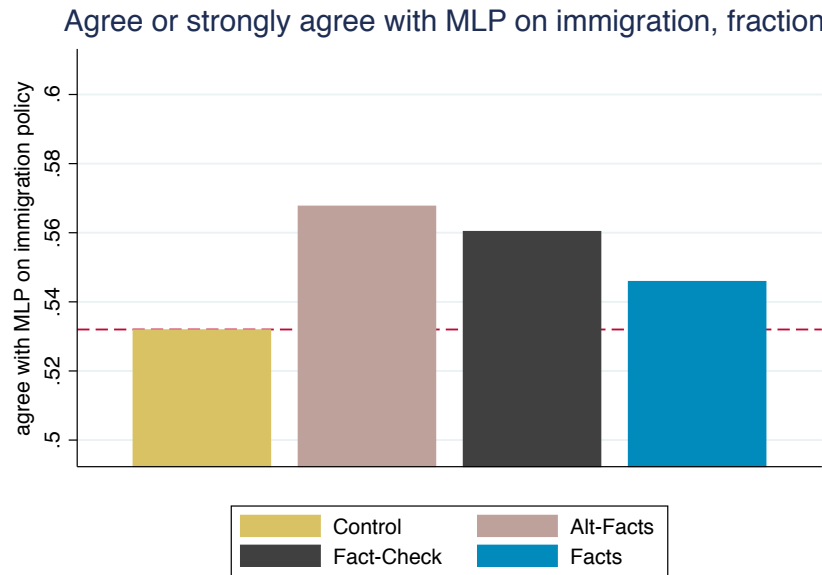


FIGURE 2.5: Overall policy impressions: agreement with MLP on immigration policy



Tables

TABLE 2.1: Balancing test across randomized groups

	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)		(11) Signif. under multiple hypotheses testing:
	Mean of variable by treatment				P-value for the test of equality of means				Alt-Fact vs Control				Facts vs Control				Alt-Fact vs Facts				
	Alt-Fact	Fact-Check	Facts	Control	Alt-Fact	Fact-Check	Facts	Control	Alt-Fact	Fact-Check	Facts	Control	Alt-Fact	Fact-Check	Facts	Control	Alt-Fact	Fact-Check	Facts	Control	
Socio-economic characteristics																					
Have children	0.68	0.71	0.70	0.70	0.48	0.69	0.97	0.97	0.27	0.73	0.46	No									
Number of children	2.08	2.19	2.10	2.10	0.73	0.18	0.97	0.97	0.09*	0.18	0.77	No									
Married	0.49	0.48	0.43	0.41	0.01**	0.02**	0.67	0.67	0.87	0.06*	0.04**	No									
Single	0.18	0.20	0.23	0.21	0.30	0.72	0.36	0.36	0.49	0.20	0.05*	No									
Income level	4.95	5.03	4.91	4.76	0.17	0.05***	0.27	0.27	0.55	0.38	0.78	No									
Land owner	0.50	0.53	0.51	0.48	0.53	0.10*	0.27	0.27	0.31	0.60	0.63	No									
Student	0.05	0.04	0.03	0.04	0.41	0.62	0.54	0.54	0.74	0.27	0.15	No									
Unemployed	0.08	0.08	0.09	0.08	0.90	0.99	0.68	0.68	0.89	0.68	0.59	No									
Full or part time worker	0.52	0.48	0.51	0.53	0.86	0.08*	0.55	0.55	0.11	0.24	0.67	No									
Retired	0.24	0.31	0.26	0.26	0.49	0.05**	0.99	0.99	0.01***	0.05**	0.48	No									
Source of income – wage	0.63	0.53	0.57	0.62	0.83	0.00***	0.05**	0.05**	0.00***	0.15	0.03**	Yes (at 1%)									
Source of income – social benefits	0.06	0.08	0.08	0.06	0.73	0.16	0.21	0.21	0.29	0.90	0.36	No									
Source of income – pension	0.25	0.32	0.29	0.26	0.72	0.01**	0.14	0.14	0.01***	0.36	0.06**	Yes (at 10%)									
Source of news – TV	0.58	0.65	0.61	0.60	0.35	0.10	0.74	0.74	0.01***	0.19	0.21	No									
Source of news – radio	0.11	0.09	0.10	0.09	0.20	0.76	0.62	0.62	0.11	0.42	0.42	No									
Source of news – internet	0.23	0.20	0.21	0.24	0.82	0.09*	0.22	0.22	0.15	0.66	0.32	No									
Religion – Catholic	0.54	0.60	0.56	0.57	0.25	0.32	0.59	0.59	0.03**	0.12	0.54	No									
Religion – Muslim	0.03	0.02	0.02	0.02	0.26	0.73	0.43	0.43	0.14	0.25	0.73	No									
Religion – none	0.38	0.34	0.38	0.37	0.67	0.30	0.62	0.62	0.14	0.13	0.95	No									
Prior voting behavior																					
Voted in 2012 – Hollande	0.22	0.21	0.22	0.19	0.21	0.29	0.13	0.13	0.83	0.64	0.80	No									
Voted in 2012 – Sarkozy	0.20	0.21	0.23	0.20	0.97	0.73	0.16	0.16	0.76	0.28	0.17	No									
Voted in 2012 – Melenchon	0.07	0.07	0.05	0.07	0.81	0.88	0.19	0.19	0.93	0.24	0.29	No									
Voted in 2012 – Le Pen	0.22	0.22	0.20	0.23	0.61	0.70	0.27	0.27	0.90	0.47	0.56	No									
Voted in 2012 – Other candidate	0.12	0.10	0.12	0.12	0.61	0.28	0.99	0.99	0.11	0.28	0.62	No									
Did not vote in 2012	0.18	0.20	0.18	0.20	0.30	0.94	0.28	0.28	0.32	0.31	0.97	No									
Voted for FN in the past	0.33	0.33	0.33	0.38	0.05*	0.05**	0.05*	0.05*	0.99	0.99	0.99	No									
Prior knowledge																					
Unemployment rate	3.56	3.54	3.63	3.60	0.74	0.60	0.78	0.78	0.54	0.85	0.42	No									
among immigrants, scale 1-10	0.29	0.26	0.26	0.28	0.73	0.36	0.57	0.57	0.37	0.21	0.73	No									
Correct prior, dummy	0.62	0.64	0.64	0.63	0.80	0.65	0.65	0.65	0.48	0.48	1.00	No									
Overestimated prior, dummy	0.09	0.10	0.10	0.09	0.91	0.54	0.92	0.92	0.82	0.46	0.61	No									
Underestimated prior, dummy																					

Note: First four columns present mean values by randomized groups and the rest of the table presents p-values for the test of difference in means across groups. Standard errors are corrected for heteroscedasticity. Last column reports the results of these balancing tests if one, in addition to heteroscedasticity, corrects for the multiple hypotheses testing (Romano and

Wolf, 2005). * $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$.

TABLE 2.2: Effect of the treatments on the main outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Differences in outcomes across treatments							
<i>Dep. Var.</i>	Will vote for MLP	Distance to truth on %: men-refugees	migrants working	men-refugees	Correct posterior on %: migrants working	Reason for refugees: Economic	Agree with MLP on immigrants
Alt-Facts	0.049** (0.023)	0.298*** (0.070)	0.253*** (0.069)	-0.023 (0.021)	-0.006 (0.016)	0.127*** (0.027)	0.050*** (0.024)
Fact-Check	0.048** (0.024)	-0.505*** (0.070)	-0.685*** (0.070)	0.312*** (0.025)	0.255*** (0.022)	0.067*** (0.027)	0.036 (0.024)
Facts	0.030 (0.023)	-0.845*** (0.068)	-0.984*** (0.071)	0.444*** (0.025)	0.376*** (0.023)	0.017 (0.027)	0.022 (0.025)
Observations	2480	2480	2480	2480	2480	2480	2480
Adjusted R ²	0.305	0.137	0.175	0.188	0.172	0.068	0.280
Mean of DV in control group	0.373	1.651	2.115	0.157	0.080	0.322	0.532
p-val: Alt-Facts=Fact-Check	0.959	0.000	0.000	0.000	0.000	0.026	0.570
p-val: Facts=Fact-Check	0.432	0.000	0.000	0.000	0.000	0.062	0.576
p-val: Alt-Facts=Facts	0.403	0.000	0.000	0.000	0.000	0.000	0.272
p-val: Alt-Facts+Facts=Fact-Check	0.351	0.680	0.649	0.002	0.000	0.046	0.300
Panel B: Persuasion rates of treatments for binary outcomes							
<i>Dep. Var.</i>	Will vote for MLP	Correct posterior on %: men-refugees		migrants working		Reason for refugees: Economic	Agree with MLP on immigrants
Alt-Facts	7.8%	-		-		12.2%	3.7%
Fact Check	7.7%	37.0%		23.6%		6.5%	2.6%
Facts	4.8%	52.7%		34.8%		1.6%	1.6%

Note: The set of unreported covariates is as follows: gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections. Robust standard errors are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

TABLE 2.3: Heterogeneity with respect to accuracy of prior knowledge

	(1)	(2)	(3)	(4)	(5)
<i>Dep. Var.</i>	Will vote for MLP	Distance to truth on % of: men-refugees men-refugees migrants working migrants working	Distance to truth on % of: men-refugees men-refugees migrants working migrants working	Correct posterior on % of: men-refugees men-refugees migrants working migrants working	Correct posterior on % of: men-refugees men-refugees migrants working migrants working
Alt-facts	0.082** (0.029)	0.345*** (0.084)	0.205** (0.086)	-0.031 (0.027)	-0.023 (0.021)
Fact-Check	0.068** (0.029)	-0.361*** (0.087)	-0.509*** (0.087)	0.283*** (0.031)	0.196*** (0.027)
Facts	0.048* (0.029)	-0.819*** (0.080)	-0.855*** (0.088)	0.441*** (0.031)	0.317*** (0.029)
Alt-Facts \times Correct prior	-0.122** (0.052)	-0.174 (0.156)	0.034 (0.154)	0.040 (0.048)	0.062* (0.035)
Fact-Check \times Correct prior	-0.060 (0.053)	-0.444*** (0.152)	-0.557*** (0.155)	0.099* (0.057)	0.187*** (0.050)
Facts \times Correct prior	-0.085 (0.053)	0.026 (0.155)	-0.379** (0.160)	-0.024 (0.057)	0.177*** (0.052)
Alt-Facts \times Underestimated prior	0.013 (0.088)	0.027 (0.272)	0.406* (0.238)	-0.038 (0.059)	0.001 (0.042)
Fact-Check \times Underestimated prior	-0.025 (0.084)	-0.283 (0.268)	-0.270 (0.250)	0.036 (0.077)	0.096 (0.067)
Facts \times Underestimated prior	0.047 (0.087)	-0.302 (0.269)	-0.246 (0.255)	0.097 (0.081)	0.110 (0.075)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.307	0.142	0.185	0.189	0.177

Note: The set of unreported covariates is as follows: dummies for correct prior and for underestimated priors, gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections. Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.4: The effect of the treatments on voting intention and policy preferences
controlling for posterior knowledge

<i>Dep. Var:</i>	(1)		(2)		(3)		(4)		(5)		(6)	
	Will vote for MLP		Reason for refugees: Economic		Agree with MLP on immigration policy							
<i>Sample:</i>	Control	Full	Control	Full	Control	Full	Control	Full	Control	Full	Control	Full
(Posterior) knowledge about % men-refugees	0.021** (0.009)	0.020*** (0.005)	0.050*** (0.009)	0.042*** (0.005)	0.015* (0.009)	0.025*** (0.005)						
(Posterior) knowledge about % working migrants	-0.027*** (0.009)	-0.021*** (0.004)	-0.019** (0.010)	-0.022*** (0.005)	-0.026*** (0.008)	-0.035*** (0.004)						
Alt-Facts		0.028 (0.023)		0.089*** (0.027)		0.020 (0.024)						
Fact-Check		0.058** (0.023)		0.080*** (0.027)		0.050** (0.024)						
Facts		0.051** (0.023)		0.050* (0.027)		0.053** (0.025)						
Observations	611	2480	611	2480	611	2480	611	2480	611	2480	611	2480
Adjusted R^2	0.300	0.316	0.086	0.098	0.086	0.303	0.086	0.098	0.309	0.303	0.309	0.303

Note: (Posterior) knowledge about % of men among refugees and (Posterior) knowledge about % of working among migrants range from 1 to 10 and measure 10-percentage-point intervals: from 0-10% (category 1) to 91-100% (category 10). The set of unreported covariates is as follows: gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 2.5: Voting intentions are not cheap talk

Panel A: The results of the dictator game					
<i>Dep. Var.:</i>	(1)	(2)	(3)	(4)	(5)
	Will vote for MLP			Donation to MLP	Give others not MLP
Donation to MLP	0.010*** (0.004)	0.039*** (0.005)			
Donation to anybody		-0.037*** (0.004)		0.679*** (0.024)	
Give others, not MLP			-0.184*** (0.018)		
Alt-Facts				0.004 (0.091)	-0.035* (0.019)
Fact-Check				-0.073 (0.092)	-0.017 (0.019)
Facts				0.029 (0.104)	-0.007 (0.020)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.306	0.324	0.319	0.529	0.051

Panel B: The results of the list experiment					
<i>Dep. Var.:</i>	(1)	(2)	(3)	(4)	(5)
<i>Sample:</i>	Full	Will vote for MLP:		Full	Full
		Yes	No		
List with MLP	0.438*** (0.042)	0.915*** (0.061)	0.122** (0.055)		
Will vote MLP				-0.698*** (0.048)	
Will vote MLP \times List with MLP				0.915*** (0.061)	
List with MLP \times Control					0.380*** (0.070)
List with MLP \times Alt-facts					0.457*** (0.069)
List with MLP \times Fact Check					0.464*** (0.064)
List with MLP \times Facts					0.447*** (0.070)
Observations	2480	974	1506	2480	2480
Adjusted R^2	0.041	0.187	0.003	0.083	0.040

Note: The set of unreported covariates in Panel A is as follows: gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections. There are no additional covariates in Panel B. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Additional Tables

TABLE 2.6: **Summary statistics for main outcome and treatment variables**

	Obs.	Mean	SD	Min	Max
Outcomes:					
Will vote for MLP	2,480	0.39	0.49	0	1
Reason for migration: economic	2,480	0.37	0.48	0	1
Agree with MLP on immigration policy	2,480	0.55	0.50	0	1
Distance to truth for:					
The share of men among refugees	2,480	1.37	1.31	0	5
The share of migrants working	2,480	1.73	1.37	0	5
The share of french refugees during WWII	2,480	1.57	1.59	0	7
Correct about:					
The share of med among refugees	2,480	0.35	0.48	0	1
The share of migrants working	2,480	0.24	0.43	0	1
The share of french refugees during WWII	2,480	0.32	0.47	0	1
Treatment groups:					
Alt-Facts	2,480	0.25	0.43	0	1
Fact-Check	2,480	0.26	0.44	0	1
Facts	2,480	0.25	0.43	0	1
Control	2,480	0.25	0.43	0	1

TABLE 2.7: Adding controls to regressions with voting and impressions as outcomes

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Will vote MLP			Reason for migration: Economic				Agree with MLP's immigration policy			
Alt-Facts	0.038 (0.027)	0.039 (0.027)	0.049** (0.023)	0.049** (0.024)	0.123*** (0.028)	0.121*** (0.028)	0.127*** (0.027)	0.129*** (0.027)	0.039 (0.028)	0.042 (0.027)	0.050** (0.024)	0.049** (0.025)
Fact-Check	0.043 (0.027)	0.043 (0.027)	0.048** (0.024)	0.046* (0.023)	0.068** (0.027)	0.066** (0.028)	0.067** (0.027)	0.065** (0.027)	0.036 (0.027)	0.031 (0.027)	0.036 (0.024)	0.033 (0.024)
Facts	0.009 (0.027)	0.016 (0.027)	0.030 (0.023)	0.028 (0.023)	0.012 (0.027)	0.012 (0.027)	0.017 (0.027)	0.017 (0.027)	0.012 (0.028)	0.014 (0.028)	0.022 (0.025)	0.019 (0.025)
Observations	2480	2480	2480	2480	2480	2480	2480	2480	2480	2480	2480	2480
Adjusted R ²	0.040	0.073	0.305	0.306	0.014	0.016	0.068	0.073	0.049	0.092	0.280	0.281
Strata controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls		✓	✓	✓		✓	✓	✓		✓	✓	✓
Prior voting controls			✓	✓			✓				✓	✓
Interactions of prior voting w / treatments				✓				✓				✓

Note: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Strata controls: gender, 3 age groups, 4 education groups, and region dummies. Individual controls: 10 income categories, linear age, 9 education levels, religion dummies, dummy indicating that the respondent is a wage-earner, marital status. Voting controls: 6 dummies for voting for each candidate in the 2012 presidential elections. Interactions of voting controls with treatments: interactions between demeaned votes for each of the candidate in the 2012 presidential elections and each treatment dummy.

TABLE 2.8: Adding controls to regressions with posterior knowledge as outcome variables

Panel A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Distance to truth about		share of men among refugees		share of men among refugees		Right about	
Alt-Facts	0.291*** (0.069)	0.292*** (0.070)	0.298*** (0.070)	0.302*** (0.070)	-0.023 (0.021)	-0.022 (0.021)	-0.023 (0.021)	-0.023 (0.021)
Fact-Check	-0.504*** (0.070)	-0.506*** (0.070)	-0.505*** (0.070)	-0.505*** (0.070)	0.311*** (0.025)	0.312*** (0.025)	0.312*** (0.025)	0.313*** (0.025)
Facts	-0.854*** (0.067)	-0.851*** (0.068)	-0.845*** (0.068)	-0.846*** (0.068)	0.446*** (0.025)	0.445*** (0.025)	0.444*** (0.025)	0.444*** (0.025)
Observations	2480	2480	2480	2480	2480	2480	2480	2480
Adjusted R ²	0.126	0.127	0.137	0.136	0.188	0.187	0.188	0.187
Strata controls	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls		✓	✓	✓		✓	✓	✓
Prior voting controls			✓	✓			✓	✓
Prior voting controls			✓	✓			✓	✓
Interactions of prior voting w/ treatments			✓	✓			✓	✓
Panel B:	Distance to truth about		share of migr. working		share of migr. working		Right about	
Alt-Facts	0.229*** (0.069)	0.243*** (0.069)	0.253*** (0.069)	0.248*** (0.069)	-0.002 (0.016)	-0.004 (0.016)	-0.006 (0.016)	-0.004 (0.016)
Fact-Check	-0.705*** (0.070)	-0.686*** (0.071)	-0.685*** (0.070)	-0.690*** (0.070)	0.262*** (0.022)	0.255*** (0.022)	0.255*** (0.022)	0.258*** (0.022)
Facts	-0.999*** (0.072)	-0.996*** (0.072)	-0.984*** (0.071)	-0.987*** (0.072)	0.380*** (0.023)	0.378*** (0.023)	0.376*** (0.023)	0.379*** (0.023)
Observations	2480	2480	2480	2480	2480	2480	2480	2480
Adjusted R ²	0.156	0.163	0.175	0.177	0.164	0.167	0.172	0.178
Strata controls	✓	✓	✓	✓	✓	✓	✓	✓
Individual controls		✓	✓	✓		✓	✓	✓
Prior voting controls			✓	✓			✓	✓
Prior voting controls			✓	✓			✓	✓
Interactions of prior voting w/ treatments			✓	✓			✓	✓
				✓				✓

Note: Robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Quota controls: gender, 3 age groups, 4 education groups, and region dummies. Individual controls: 10 income categories, linear age, 9 education levels, religion dummies, dummy indicating that the respondent is a wage-earner, marital status. Voting controls: 6 dummies for voting for each candidate in the 2012 presidential elections. Interactions of voting controls with treatments: interactions between demeaned votes for each of the candidate in the 2012 presidential elections and each treatment dummy.

TABLE 2.9: Effect of the treatments on knowledge about French refugees in WWII

	(1)	(2)
	The share of refugees among French population in WWII:	
	distance to truth	correct answer
Alt-Facts	0.058 (0.088)	-0.022 (0.025)
Fact-Check	-0.106 (0.089)	0.136*** (0.026)
Facts	0.033 (0.093)	0.105*** (0.027)
Observations	2480	2480
Adjusted R^2	0.021	0.052
mean of Dep. Var. in control	1.589	0.264

Note: Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The set of unreported covariates is as follows: gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections.

TABLE 2.10: Correlation between the priors about unemployment and priors (posteriors in control group) about % of men among refugees or % of working among migrants

Dep. Var.:	(1)	(2)	(3)	(4)
	Prior knowledge (i.e., posterior in control group): % men-refugees (10 categories) % working migrants (10 categories)			
Panel A: Dummies for correct/incorrect priors about unemployment among migrants				
Dummy: Overestimated migrant unemployment	0.610*** (0.185)	0.634*** (0.199)	-0.537** (0.209)	-0.323* (0.192)
Dummy: Underestimated migrant unemployment	-0.245 (0.354)	-0.261 (0.350)	-0.534 (0.348)	-0.410 (0.348)
Adjusted R^2	0.025	0.059	0.010	0.129
Panel B: Continuous priors about unemployment among migrants				
Prior about migrant unemployment (10 categories)	0.199*** (0.041)	0.226*** (0.044)	-0.175*** (0.048)	-0.101** (0.049)
Dummy: Underestimated migrant unemployment	-0.098 (0.350)	-0.077 (0.346)	-0.665* (0.343)	-0.465 (0.341)
Adjusted R^2	0.042	0.082	0.021	0.132
Controls		✓		✓
Observations	611	611	611	611

Note: Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Controls are: gender, age (linearly and as a dummy for each age quota), family status, income (with dummies for each of the 10 income categories), education (with dummies for each of the 9 education levels), regional dummies, religion dummies, a dummy indicating that the respondent is a wage-earner, dummies for voting for each candidate in the 2012 presidential elections.

TABLE 2.11: Heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Will vote for MLP	Distance to truth on %: men-refugees	migrants working	Reason for refugees: economic	Agree with MLP on immigrants
Panel A: Voted for MLP during 2012 presidential elections					
Voted for MLP, 2012 \times Alt-facts	0.033 (0.053)	0.072 (0.170)	-0.279* (0.167)	0.026 (0.068)	-0.014 (0.042)
Voted for MLP, 2012 \times Fact-Check	-0.000 (0.054)	0.069 (0.179)	0.007 (0.173)	0.008 (0.067)	0.012 (0.039)
Voted for MLP, 2012 \times Facts	0.070 (0.053)	-0.107 (0.178)	0.092 (0.180)	-0.015 (0.069)	0.019 (0.042)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.305	0.137	0.176	0.067	0.279
Panel B: News from TV					
News from TV \times Alt-facts	0.093** (0.047)	0.016 (0.143)	-0.014 (0.142)	0.118** (0.056)	0.064 (0.050)
News from TV \times Fact-Check	0.007 (0.048)	-0.196 (0.147)	-0.017 (0.146)	0.140** (0.055)	0.016 (0.050)
News from TV \times Facts	0.048 (0.049)	-0.118 (0.140)	0.038 (0.148)	0.031 (0.056)	-0.019 (0.052)
News from TV	-0.010 (0.034)	0.061 (0.095)	-0.076 (0.097)	-0.041 (0.040)	0.051 (0.036)
Observations	2415	2415	2415	2415	2415
Adjusted R^2	0.307	0.137	0.173	0.071	0.285
Panel C: News from internet					
News from internet \times Alt-facts	-0.118** (0.054)	0.030 (0.162)	-0.218 (0.160)	-0.076 (0.065)	-0.066 (0.056)
News from internet \times Fact-Check	-0.047 (0.058)	0.424** (0.173)	-0.059 (0.166)	-0.077 (0.065)	-0.048 (0.058)
News from internet \times Facts	-0.040 (0.058)	0.108 (0.162)	-0.126 (0.174)	0.039 (0.066)	-0.033 (0.060)
News from internet	0.051 (0.040)	-0.146 (0.105)	0.162 (0.108)	0.014 (0.046)	0.020 (0.040)
Observations	2415	2415	2415	2415	2415
Adjusted R^2	0.306	0.139	0.173	0.069	0.280
Panel D: Recipient of social security benefits					
Income from soc.security \times Alt-facts	-0.076 (0.051)	-0.252* (0.151)	-0.113 (0.145)	-0.037 (0.059)	0.023 (0.054)
Income from soc.security \times Fact-Check	-0.076 (0.049)	-0.232 (0.146)	-0.149 (0.143)	-0.048 (0.056)	0.069 (0.051)
Income from soc.security \times Facts	-0.125** (0.049)	-0.286** (0.143)	-0.185 (0.147)	-0.020 (0.058)	-0.039 (0.053)
Income from soc.security	0.116** (0.049)	0.275* (0.145)	0.211 (0.145)	0.066 (0.056)	-0.025 (0.051)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.306	0.137	0.175	0.067	0.280
Panel E: Secondary education					
Secondary education \times Alt-facts	0.022 (0.050)	-0.092 (0.143)	-0.004 (0.141)	-0.028 (0.056)	0.021 (0.051)
Secondary education \times Fact-Check	0.085* (0.049)	-0.324** (0.143)	-0.110 (0.143)	-0.047 (0.055)	0.032 (0.049)
Secondary education \times Facts	0.019 (0.050)	-0.391*** (0.142)	-0.314** (0.151)	-0.067 (0.056)	0.030 (0.051)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.305	0.140	0.176	0.067	0.279

Note: Robust standard errors in parentheses.

Baseline set of controls and the direct effects of treatments and of the variable with respect to which we study heterogeneity are included.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 2.12: Heterogeneity, continued

	(1) Will vote for MLP	(2) Distance to truth on %: men-refugees	(3) Distance to truth on %: migrants working	(4) Reason for refugees: economic	(5) Agree with MLP on immigrants
Panel A: Income					
Income \times Alt-facts	-0.002 (0.009)	0.002 (0.029)	-0.013 (0.030)	-0.014 (0.011)	0.000 (0.010)
Income \times Fact-Check	0.009 (0.010)	-0.032 (0.028)	-0.076*** (0.028)	-0.017 (0.011)	0.001 (0.010)
Income \times Facts	-0.004 (0.010)	-0.040 (0.028)	-0.031 (0.030)	-0.029*** (0.011)	-0.006 (0.010)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.305	0.137	0.177	0.070	0.279
Panel B: Age					
Age \times Alt-facts	-0.000 (0.002)	-0.007 (0.005)	-0.004 (0.004)	-0.004** (0.002)	0.002 (0.002)
Age \times Fact-Check	-0.003* (0.002)	-0.009* (0.005)	-0.005 (0.005)	-0.005*** (0.002)	-0.000 (0.002)
Age \times Facts	-0.003* (0.002)	-0.002 (0.004)	-0.001 (0.005)	-0.003* (0.002)	-0.001 (0.002)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.306	0.138	0.175	0.070	0.280
Panel C: Gender					
Male \times Alt-facts	-0.009 (0.047)	-0.291** (0.138)	-0.009 (0.138)	-0.039 (0.054)	-0.060 (0.049)
Male \times Fact-Check	-0.031 (0.047)	-0.022 (0.138)	-0.086 (0.139)	-0.064 (0.053)	-0.079* (0.048)
Male \times Facts	-0.033 (0.047)	0.043 (0.133)	0.286** (0.140)	0.017 (0.054)	-0.055 (0.049)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.304	0.138	0.177	0.068	0.280
Panel D: Parents born outside France					
Immigrant parents \times Alt-facts	-0.100 (0.065)	0.385* (0.207)	-0.027 (0.208)	-0.004 (0.078)	-0.046 (0.070)
Immigrant parents \times Fact-Check	-0.097 (0.069)	0.100 (0.206)	-0.174 (0.196)	-0.051 (0.077)	-0.130* (0.071)
Immigrant parents \times Facts	-0.008 (0.075)	0.298 (0.203)	-0.042 (0.222)	0.085 (0.084)	0.034 (0.079)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.305	0.137	0.175	0.069	0.281
Panel E: Political orientation					
Score on left-right axis \times Alt-facts	0.010 (0.008)	0.015 (0.025)	-0.000 (0.024)	0.027*** (0.009)	0.005 (0.008)
Score on left-right axis \times Fact-Check	0.003 (0.007)	-0.012 (0.024)	0.022 (0.024)	0.021** (0.009)	0.007 (0.007)
Score on left-right axis \times Facts	0.010 (0.007)	-0.027 (0.023)	0.002 (0.025)	0.005 (0.009)	0.007 (0.007)
Score on left-right axis	0.036*** (0.006)	0.016 (0.018)	-0.001 (0.018)	0.017** (0.007)	0.039*** (0.006)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.338	0.137	0.174	0.090	0.315
Panel F: Regional-level election results					
Reg. vote for MLP, 2nd round \times Alt-facts	0.010 (0.007)	0.011 (0.019)	-0.026 (0.019)	0.007 (0.007)	0.007 (0.007)
Reg. vote for MLP, 2nd round \times Fact-Check	0.001 (0.006)	0.031* (0.018)	-0.002 (0.018)	0.007 (0.007)	-0.005 (0.007)
Reg. vote for MLP, 2nd round \times Facts	0.009 (0.007)	0.018 (0.021)	-0.002 (0.023)	0.003 (0.008)	-0.000 (0.008)
Reg. vote for MLP, 2nd round	-0.002 (0.007)	0.007 (0.021)	0.018 (0.022)	-0.001 (0.008)	0.006 (0.008)
Observations	2480	2480	2480	2480	2480
Adjusted R^2	0.305	0.137	0.175	0.067	0.280

Note: Robust standard errors in parentheses

Baseline set of controls and the direct effects of treatments and of the variable with respect to which we study heterogeneity are included.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Figures

FIGURE 2.6: 5 regions from which the sample was drawn

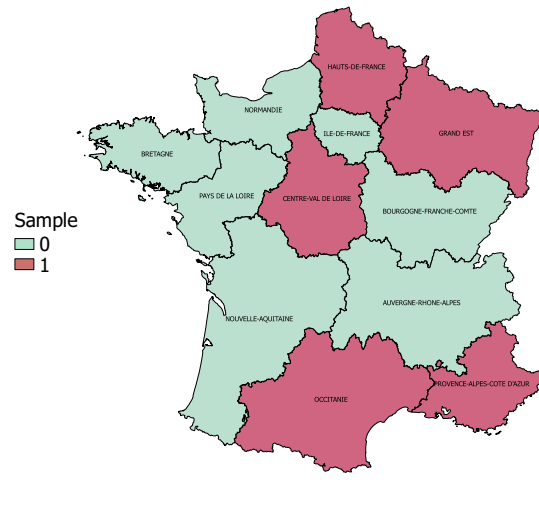


FIGURE 2.7: Vote for FN in the 2015 regional elections (left) and for MLP in the first round of the 2017 presidential elections (right)

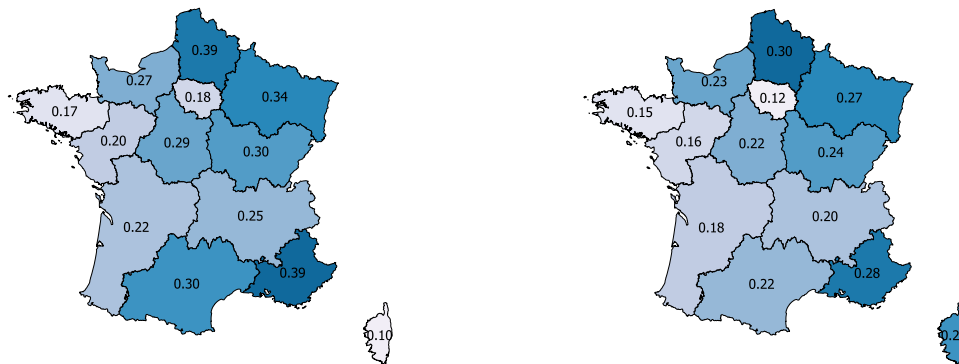
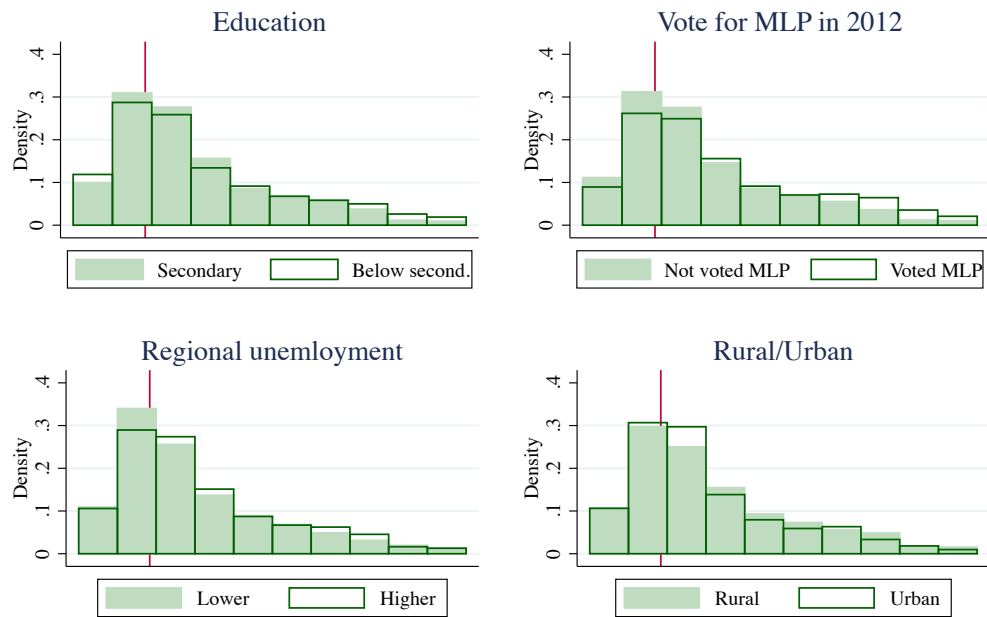


FIGURE 2.8: Prior beliefs about unemployment among immigrant population

Prior on unemployment rate among immigrants, 10 categories



Full sample; vertical lines indicate 18%

Note: Horizontal axis represents the 10 percentage point intervals for the unemployment among immigrant population.

FIGURE 2.9: Voting intentions separately for non-supporters of MLP (left) and supporters of MLP (right)

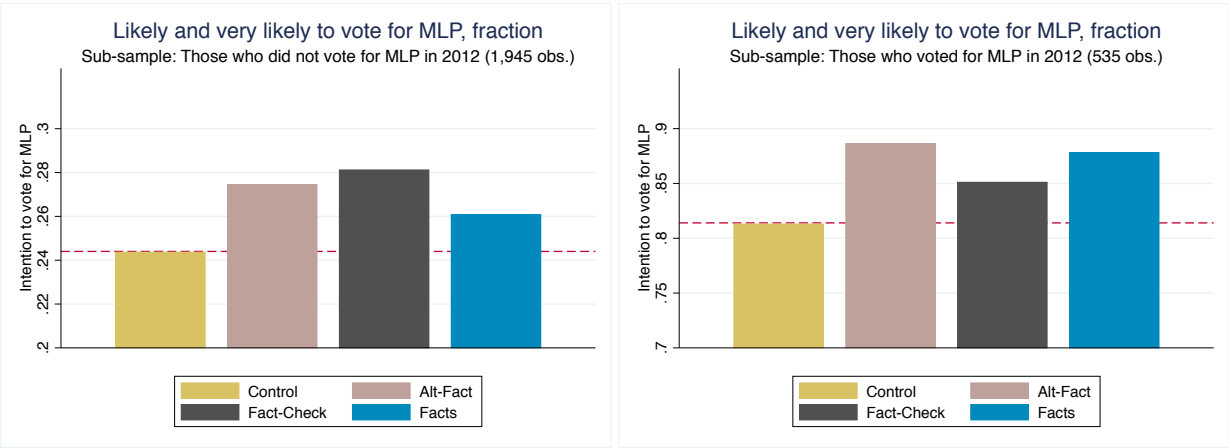
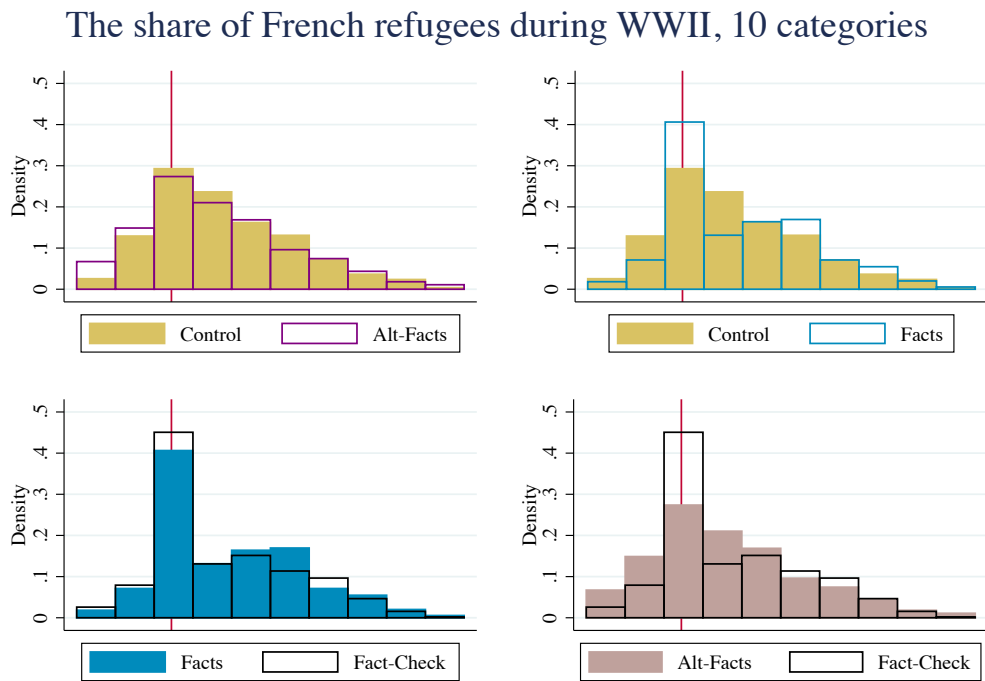


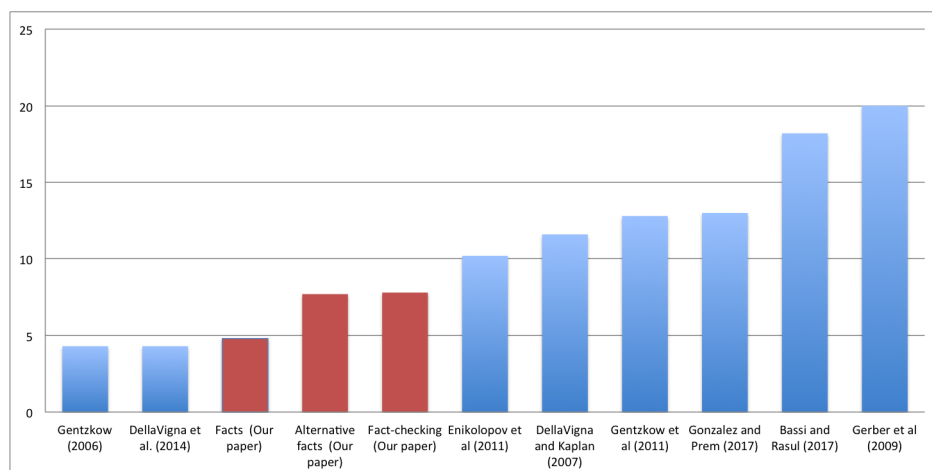
FIGURE 2.10: Posterior beliefs on the share of French refugees during WWII



Vertical line indicates the Fact

Note: Horizontal axis represents the 10 percentage point intervals for the share of French refugees during WWII.

FIGURE 2.11: Persuasion rates in comparable papers vs. estimates in Table 2.2.



References in order of appearance on Figure 2.11:

Gentzkow, M. (2006). Television and voter turnout. *Quarterly Journal of Economics*, 121(3): 931–972.

DellaVigna, S., Enikolopov, R., Mironova, V., Petrova, M., and Zhuravskaya, E. (2014). Cross-border media and nationalism: Evidence from Serbian radio in Croatia. *American Economic Journal: Applied Economics*, 6(3):103–32.

Enikolopov, R., Petrova, M., and Zhuravskaya, E. (2011). Media and political persuasion: Evidence from Russia. *American Economic Review*, 101(7): 3253–85.

DellaVigna, S. and Kaplan, E. (2007a). The Fox news effect: Media bias and voting. *Quarterly Journal of Economics*, 122(3):1187–1234.

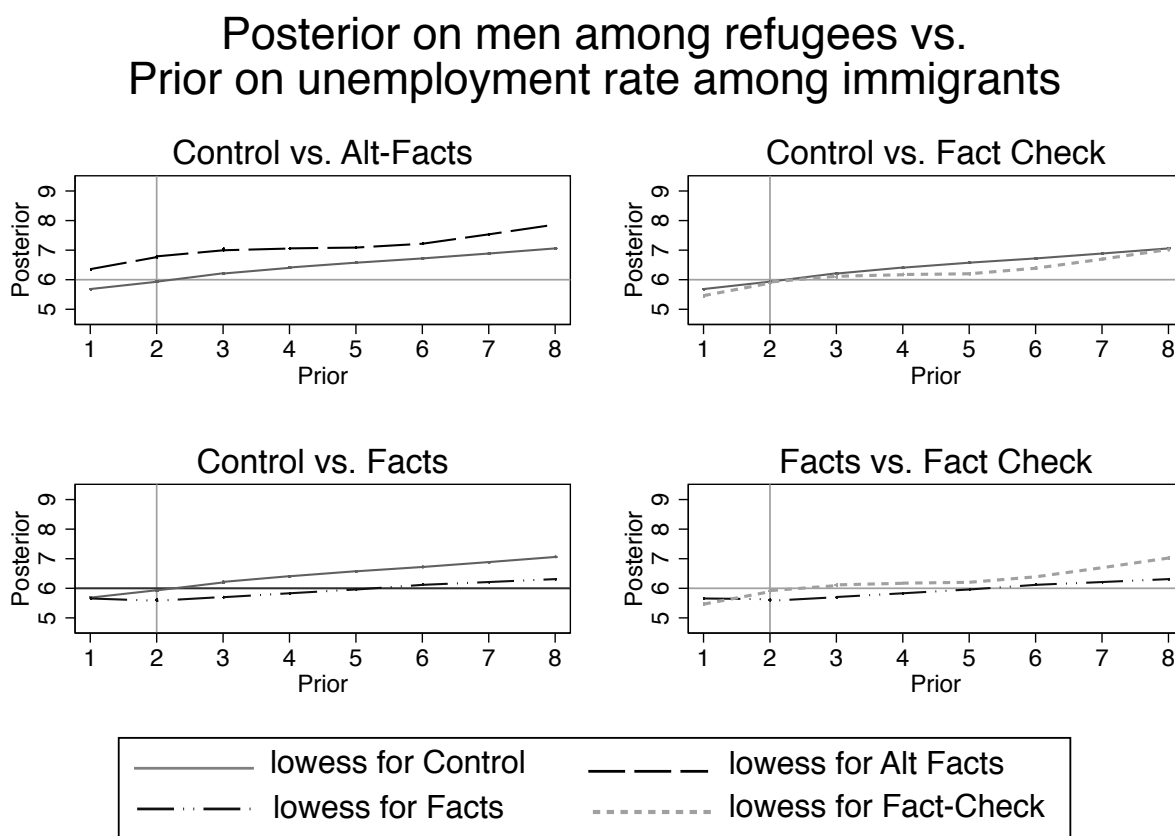
Gentzkow, M., Shapiro, J. M., and Sinkinson, M. (2011). The effect of newspaper entry and exit on electoral politics. *American Economic Review*, 101(7): 2980–3018.

González, F. and Prem, M. (2018). Can television bring down a dictator? Evidence from Chile’s “No” campaign. *Journal of Comparative Economics*, 46(1):349–361.

Bassi, V. and Rasul, I. (2017). Persuasion: A case study of papal influences on fertility-related beliefs and behavior. *American Economic Journal: Applied Economics*, 9(4):250–302.

Gerber, A. S., Karlan, D., and Bergan, D. (2009). Does the media matter? A field experiment measuring the effect of newspapers on voting behavior and political opinions. *American Economic Journal: Applied Economics*, 1(2):35–52.

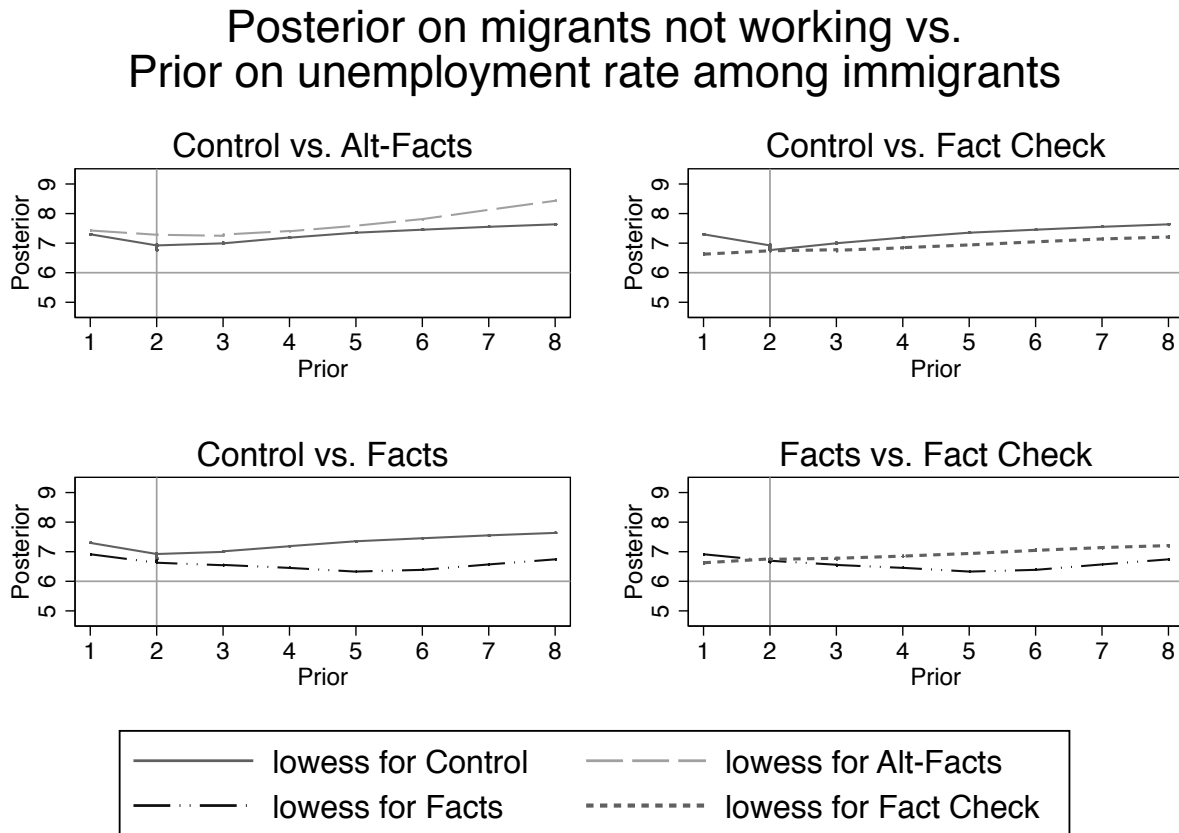
FIGURE 2.12: The relationship between posteriors and priors by treatment



Vertical and horizontal lines indicate the truth for prior and posterior, respectively.

Horizontal axis: categories of priors on unemployment rate among immigrants: 1 for 0-10%, 2 for 11-20% etc. We do not report the 9th and 10th categories where the number of observations is very small. Vertical axis: average for the posterior on the share of men among refugees crossing the Mediterranean Sea (1 for 0-10%, 2 for 11-20% etc.) averaged out for the respondents with the respective priors.

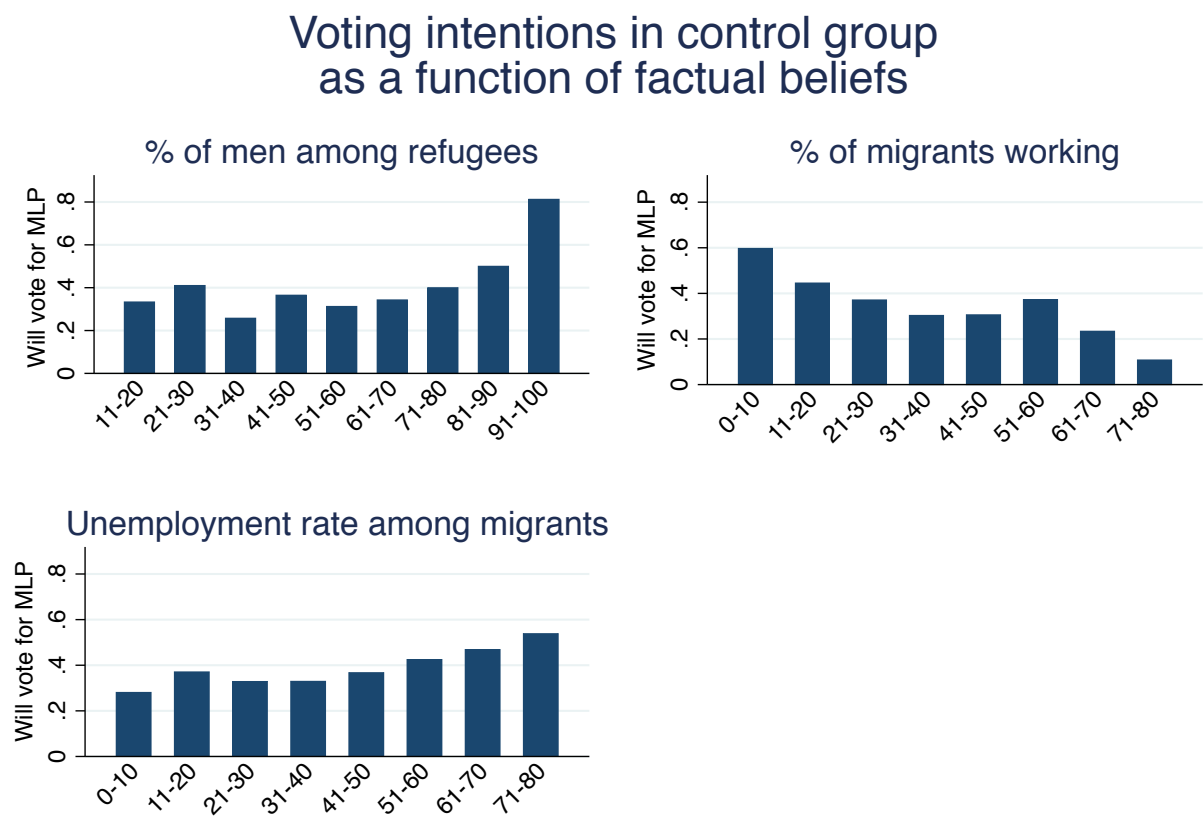
FIGURE 2.13: The relationship between posteriors and priors by treatment



Vertical and horizontal lines indicate the truth for prior and posterior, respectively.

Horizontal axis: categories of priors on unemployment rate among immigrants: 1 for 0-10%, 2 for 11-20% etc. We do not report the 9th and 10th categories where the number of observations is very small. Vertical axis: average for the posterior on share of immigrant population working (1 for 0-10%, 2 for 11-20% etc.) averaged out for the respondents with the respective priors.

FIGURE 2.14: The relationship between voting intentions and factual beliefs in the control group



Voting intentions calculated for categories of x-variable with at least 20 observations in each graph

FIGURE 2.15: The relationship between agreement with MLP on immigration and factual beliefs in the control group

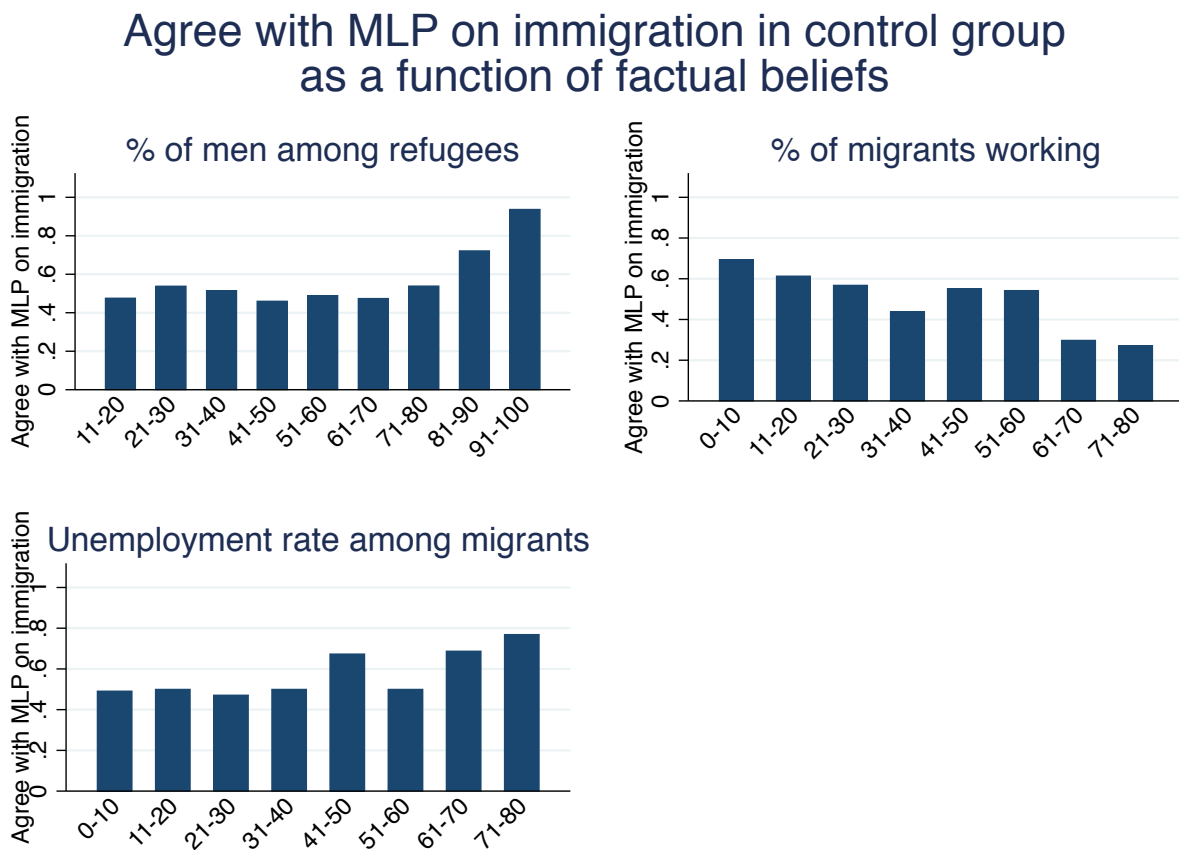
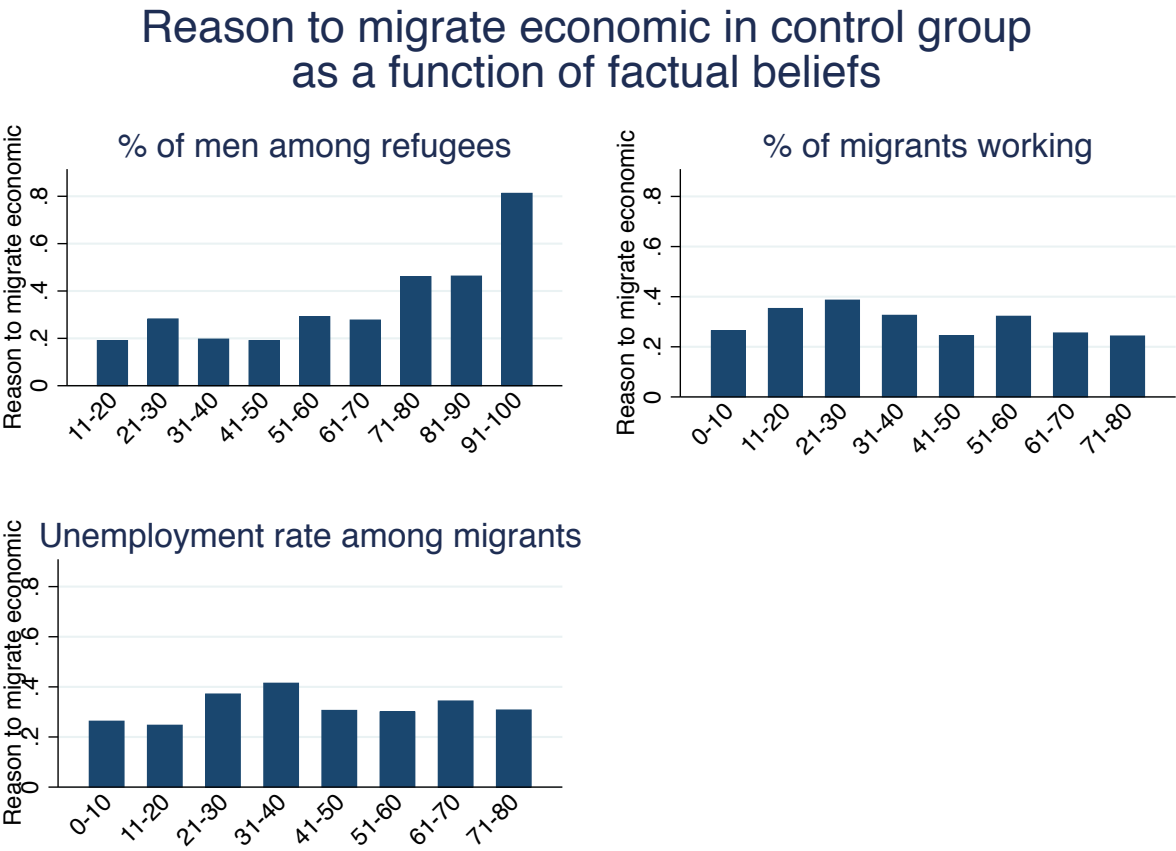


FIGURE 2.16: The relationship between belief that immigrants come for economic reasons and factual beliefs in the control group



Chapter 3

Political influence on homicide reports under civil conflict

3.1 Introduction

Despite evidence of their human, social and economic costs (Weinstein, 2006; Fearon and Laitin, 2003; Collier and Hoeffler, 2004; Hegre and Sambanis, 2006), conflicts may be hard to solve. One of the reasons behind conflicts' persistence relates to the direct benefits of economic or political rents they may bring to powerful elites. Specific groups or actors may indeed derive benefits from war or disorder and hence have low incentives to exert political and economic pressure to prevent or stop ongoing conflict: this is obviously the case for the arms industry, and also for mining companies (Guidolin and La Ferrara, 2007) or other private firms.

As explained by Fergusson (2019), powerful politicians might also have weak incentives to stop violence if some political rents are associated to it. The author uses the long-lasting Colombian conflict as a case study to show that, despite substantial human costs for the last 37 years, with many deaths, forced displacements, and kidnappings, politically powerful groups in this country have been able to cope quite well with the conflict. This has resulted in the political elite doing little to stop violence and opposing ending conflict for fear of losing an electoral advantage and political power. In a less recent contribution, Fergusson et al. (2016) developed a political economy model of the "need for enemies", showing how a politician who is good at undertaking a particular task has an incentive not to complete it fully since he needs to keep the task alive in order to maintain his strategic advantage in an election. While their model may apply to many different situations, they focus on the particular case of an ongoing civil war where incumbent politicians have to decide whether or not to fight the insurgents. The model predicts that politicians may well decide not to fight insurgents to sustain their political power.

This paper follows this line of reasoning but moves a step forward. It argues that, in order to get credibility and voters' support, politicians who choose the use of coercion or threat of force to solve a conflict, need to take care of two things. First, they need to show that their active military strategy is a way to reach a sustainable conflict resolution (or they should at least create the illusion that it is bringing a potential solution). Second, they need to convince voters that their approach to conflict resolution is the most feasible one, or, in other terms, that their *enemy* is too dangerous for another approach, such as a political peace agreement, to be considered. In the Colombian context, the first condition triggered perverse side effects, leading to the false positives scandal¹. My contribution in this paper is to show that the second condition also had perverse side effects and resulted in particular in manipulated figures on homicides. Few papers have investigated the role played by propaganda in the literature on the political economy of conflict. One exception is Huang (2015), who reviews the role of pro-regimes propaganda on citizens' preferences and opinions about China's regime.

In the case of Colombia, painting a gloomy picture of the rebels, especially in large cities, would increase popular support, legitimacy, and political acceptance that the elite needs to remain in office while reducing the risk of massive popular support for opposition ideologies. Thus, in a post-conflict scenario, any potential rebels' political participation would be unable to be massively supported by society. The new party would be, therefore, ineffectual as a political opposition party. The paper analyzes the Colombian case, where a political elite exploits political benefits from the conflict. The incumbent is, therefore, in favor of an active military confrontation to solve the conflict. At the local level, there are two types of politicians, one who is exploiting political rents from the conflict in favor of the national incumbent, called a coalition and second, the national opposition fraction, who is devoted to set-up a political dialogue with the insurgents.

The starting point of the paper is the result of a statistical exercise that I conduct using a regression discontinuity design (RDD) that I apply to close elections. I compare municipalities in which the incumbent associates narrowly won versus municipalities in which they narrowly lost the mayoral race. I find that in those municipalities where allies of the national incumbent won the election by a short margin, the homicide rates perpetrated by the insurgents were significantly larger. By contrast, I find no discontinuity in the number of homicides perpetrated by regular forces (army) or right-wing groups.

Can this result be interpreted as a consequence of the strategy followed by the political elite to create a bad reputation of the enemy, gain electoral advantage and popularity, and justify the massive national spending on war to sustain its political power? Not necessarily. Two alternative testable interpretations can be pushed forward to explain such results. A first interpretation is that the guerrilla groups did

¹The False Positives scandal (Escandalo de los Falsos Positivos in Spanish) was a series of murders in Colombia for 2002 and 2008, as part of the armed conflict between the government and guerrilla forces of the FARC and the ELN. Members of the military had poor or mentally impaired civilians lured to remote parts of the country with offers of work, killed them, and presented them to authorities as guerrillas killed in battle, to inflate body counts and receive promotions or other benefits in municipalities of Colombia.

commit a higher number of homicides in municipalities where the locally elected politicians adopted the incumbent strategy as a military response to the government's active military confrontation. This seems a plausible scenario in that the rebels could have retaliated in response to harassment resulting from the adoption of the incumbent active confrontation strategy by local authorities. The rebels might have taken an offensive stance against the establishment by murdering politicians, local leaders, or merely the ruling party's supporters. A second interpretation is that the higher rate of murders by guerrilla groups might have been due to figures being altered by the army or police in collusion with local political authorities, which will confirm the government's need to show that the enemy is dangerous. In contrast, where an opposition party won local office by a close margin, the cooperative local political authority might have monitored and overseen the civilian homicide recording procedures used by the police and army to stem the likelihood of any doctoring of the figures.

In order to favor one or the other interpretation, I run various tests. First, in addition to using data on homicides, I use data on other outcomes associated with rebel operations at the local level, such as the number of clashes, military operations, kidnappings, rapes, and massacres (collective murders) committed by the guerrillas. The results suggest no discontinuity in any of the tested variables. I conclude that the lack of discontinuity in the listed outcomes is because these variables are less exposed to manipulation than homicides. The rationale is that it is easier to establish responsibility for any of these forms of violence since, unlike homicides, the victims themselves can identify the perpetrator. There is, therefore, less chance of attributing the action to another actor.

As a second test, I use a benchmark data source on homicides provided by El Centro de Investigacion y Educacion Popular (CINEP) to see whether homicide rates are also found to differ between perpetrators with this alternative data source. The CINEP database recorded homicides and eye witness accounts of homicides during the armed conflict in Colombia. It is an appropriate benchmark dataset as it compiles direct accounts reported by local communities, leaders, priests and victims' relatives, which means that the figures are less likely to have been manipulated. I also disentangle the number of homicides reported by CINEP from the records provided by the National Police Agency (NPA) and other official intelligence agencies. This exercise confirms a potential bias in the way the different institutions recorded the murders. The doctored figures hypothesis becomes more plausible if the discrepancies found between the two data sources lead to different conclusions, in particular if the main results of the paper can be replicated with the NPA data, but not with the CINEP records. In this case, it could be assumed that there is a greater probability that the police or army recorded a civilian murder perpetrated by the guerrilla group when Uribist local mayoral authority and won office by a narrow margin.

As a third test, I focus on the national "false positives" scandal to explore the link between our baseline results and a proven case of army-manipulated homicide records. To do this, I divide my sample into municipalities that reported false-positive cases and municipalities without any reported case and find that my baseline results are closely associated with the "false positives" scandal. This evidence suggests that the

confronting strategy led local elected officials to allow the military and members of the police force, possibly involved in the "false positives" scandal, to interfere with the reported number of murders perpetrated by guerrillas. In contrast, municipalities governed by opposition parties might have exercised some control and oversight of military operations conducted by the police and the army. These institutions may therefore have had less room for maneuver to tamper with homicide rates.

Lastly, as a supplementary analysis, I provide anecdotal evidence about a national controversy concerning the reliability of the records and the responsibilities of each player during the conflict. Following the 2018 presidential elections, there was a national outcry over the head of the Centro Nacional de Memoria Historica (CNMH), the institute in charge of collecting data on the Colombian conflict. Community and political leaders argued that the new government's candidate had a biased opinion of the military's responsibilities in the conflict. Indeed, the candidate had publicly stated on several occasions that the records and reports presented by the CNMH tended to present a positive image of the guerrilla groups, while seeking to discredit the paramilitary groups. The issue received a great deal of media coverage nationwide and raised widespread concern over data collection quality on the conflict. This article hence makes a significant contribution to this debate.

The active incumbent strategy could have affected several outcomes. This paper, however, focuses on reported homicides by perpetrator as the primary outcome variable. The study claims that homicide rate is a variable more sensitive to being altered since, as mentioned before, the responsibility of the perpetrator(s) cannot necessarily be unequivocally checked, especially in the absence of witnesses. The resulting uncertainty leaves the door open to the arbitrary blaming of certain groups with ideological or political purposes. By contrast, there is less room for manipulation when it comes to the perpetrator(s) of any other violent action (e.g., kidnapping, extortion, rape, and threat), as the victim(s) can testify and identify the perpetrator(s). We also focus on the cumulative number of homicides committed over the three years after the election. However, our results hold when the focus is on the two years following the elections².

The next section presents an overview of the related literature. Section 3 provides a brief outline of the Colombian conflict between 2002 and 2010. In Section 4, I test whether the outcomes of close-run elections affect violence in the municipalities where these elections are held. I then investigate the underlying mechanisms that could drive the results in Section 5. In Section 6, I test for the presence of a backfiring effect in the municipalities which chose the confronting strategy. The last section presents the conclusions of the paper.

²The political term in Colombia lasts 4 years. However, in our main estimates, we compute our outcome variables using the three years following the elections, as the fourth year is usually affected by the political campaigns which may affect the military strategies

3.2 Elections and conflict persistence in the literature

This paper relates to two strands of literature. The first is a growing body of close elections research on civil conflict or violence in Latin America. For instance, Dell (2015) uses outcomes of close elections to provide evidence on the dramatic increase in drug trade-related homicide rates measured at the municipality level. To this end, he compares municipalities in which the conservative National Action Party (PAN) took office with municipalities in which a non-PAN mayor took office. My analysis differs from Dell's paper because I differentiate homicides perpetrated by different actors in conflict (i.e. rebel forces, paramilitary, unknown actors and the National Army) and because my goal is not to document the causes of the conflict but to understand some of the mechanisms underlying the prolongation or persistence of the conflict.

Among the other papers investigating the effect of close elections on homicides, the most similar to ours is the paper by Fergusson et al. (2020) in which the authors assess the impact on violence of the narrow election of previously excluded left-wing parties to the local executive office in Colombia. They find an increase by one standard deviation in violent events by right-wing paramilitaries, which they interpret as a reaction of traditional elites to offset the increase in outsiders' access to formal political power. By contrast, they find that violence by left-wing guerrillas and other actors was unaffected by the victory of right-wing or other new parties in close elections. The main difference between Fergusson et al.'s study and ours lies in the political environment: in our paper, the focus is on the effects of close right-wing victories at a time when the national incumbent adopted strong measures to fight the left-wing rebels.

The second strand of literature focuses on the incentives to perpetuate a local conflict. One of the first papers to investigate this issue is the paper by Collier et al. (2000). They find that economic opportunities (rather than greed as a motivational factor) are a major factor in the escalation of violence. Their findings are supported by analyses emphasizing the economic benefits derived from conflicts, which can also be specified as long-term incentives (Garfinkel and Skaperdas, 2000). In another contribution, Collier and Hoeffler (2005) stress that natural resource abundance increases the likelihood of conflict onset and conflict duration. The present paper has nothing to say about whether economic rents or natural resources have perpetuated the Colombian conflict. Its focus, instead, is on the political incentives to perpetuate a conflict. However, economic factors come into play because, as I argue, conflict and political instability in the presence of local economic rents play a different role than it is generally assumed, due to the particular governance structure that developing economies may develop.

In an interesting case study of Colombia, Acemoglu et al. (2020) document the perverse side effects produced by the provision of high-powered incentives for the military and security services. I claim that this is an important element to consider as homicide reporting in conflict countries is highly political and as the potential for manipulating homicide figures is considerable and could perpetuate hostilities, guaranteeing political and economic rents to the elite. I try to provide novel empirical evidence in support of this claim. By exploring the underlying mechanisms that could explain the main results of the paper, I

suggest that the authorities might have resorted to manipulation for political ends. To my knowledge, this is the first paper investigating the issue of data manipulation for political purposes in a conflict country. However, the idea that governments of all types may have some incentives to lie about numbers or facts has received particular attention. Politicians may have indeed strong incentives to create fake figures in order to reach their objectives, and new online media platforms and social media may facilitate their dissemination. (Mocanu et al., 2015), for example, document the rapid spread of fake news over social media during the 2012 elections in Italy. Allcott and Gentzkow (2017) show that fake stories were intensely shared on Facebook during the 2016 U.S. presidential election campaign. Elections in hybrid regimes are not usually useful tools for political accountability, as they are easily manipulated through state-controlled media or outright-fraud (Enikolopov et al., 2013) and (Enikolopov et al., 2011). Zhuravskaya et al. (2020) review this literature.

Finally, there is a large literature in social psychology on the notion of causes and strategies for conflict resolution. Deutsch (1949a,b) proposes a theoretical frame on different components in the conflict: cooperation - competition, constructive - destructive, equity, equality and psychological orientation. Bercovitch et al. (2008) reviews fully this literature.

3.3 Context: The Colombian conflict and elections

After the independence from Spain and Libertador Simon Bolivar's death, Colombia set a two-party system (Conservative-Liberal) that lasted for more than 150 years. Inter-party violence was widespread during the nineteenth and twentieth centuries, and reached a peak between 1948 and 1953, a period known as *La Violencia*. To pacify the situation, the Conservative and Liberal parties agreed to the Frente Nacional (National Front, FN) deal³, which excluded radically civil sectors, peasants, workers, and all groups ideologically aligned with the left.

Given the absence of political opportunities for outsiders, combined with the lack of state presence in the Colombian periphery and the survival of Liberal rural guerrillas from *La Violencia*, in the early 1960s, some rural minorities formed left-leaning guerrilla movements. The most powerful of them was the Armed Revolutionary Forces of Colombia (Fuerzas Armadas Revolucionarias de Colombia FARC). In the late 1970s, to finance their activities, the FARC and other guerrilla movements began kidnapping and extorting wealthy individuals, particularly landowners. This precipitated the creation of paramilitary self-defense militias by local elites, which in many cases operated with at least the implicit complacency of the national army and local politicians (Acemoglu et al., 2013).

In the 1980s, the government was forced to negotiate with insurgents due to increasing violence in rural areas and repression of left-leaning supporters. As part of the peace talks, and to signal a credible

³As part of this deal, the presidency alternated every four years between 1958 and 1974, and parity in party representation across all government bodies was ensured.

opening of the country's democratic system, the electoral systems reformed to allow the direct election of local mayors by simple plurality rule starting in 1988⁴. The 1991 constitution further consolidated the opening of political system, allowing historically excluded groups (i.e., left-wing, peasants, indigenous, union workers, religious, etc.) to participate in elections. The policy positions of left-wing parties were particularly threatening to the interests of local landowning elites.

In 1998, the candidate of the Conservative Party, Andres Pastrana, was elected as Colombia's president who conducted peace talks with the FARC. However, after three years of talks, President Pastrana officially announced the peace process's breakdown as both parties had failed to reach any agreement. Public opinion consequently took a sharp turn in favor of a military crackdown to end the Colombian armed conflict. The conflict with the FARC thus dominated the 2002 presidential electoral campaign. The conservative politician Alvaro Uribe Velez, using a radical speech of cracking down hard on the rebels, gained high popularity and won the presidential elections easily in the first round in 2002. During his term in office, Uribe led an aggressive political, ideological and military campaign against all guerrilla groups and imposed a drastic position against considering any political conflict resolution with the FARC. His radical position against the FARC divided civil society and political parties into two: those in favor of a military incursion as the strategy for the conflict resolution and those in favor of a cooperative strategy to negotiate the end of the conflict.

Before 2006 in Colombia, presidential re-election was not considered in the national Constitution. However, by the end of his presidential period, Uribe managed to take advantage of his popularity to change the Constitution and removed the one-term limit. He was re-elected on a landslide and led the country for an additional term, from 2006 to 2010. The president attempted to change the Constitution again in order to run for a third period in 2010, but the Constitutional Court ruled it unconstitutional. Nonetheless, Uribe remained very popular, therefore his successor and former Defence Minister Juan Manuel Santos were elected mainly on a platform that emphasized continuity with a commitment to beat the rebels.

3.4 Effect of close electoral outcomes on murders

This section presents our empirical RDD approach to test whether the outcomes of close-run local allies with the national incumbent leader affected violence in those municipalities with a narrow win-loss margin between a Uribist and non-Uribist party. It first presents the econometric framework. It then describes the data sources and coding procedure for classifying the different parties into two categories (Uribist and non-Uribist). It then provides some tests of the main assumptions underlying the Regression Discontinuity

⁴The first local elections were held in Colombia in 1992; the second were held two years later in 1994; the third took place in 1997; the fourth were held in 2000; the fifth in 2003; the sixth in 2007; and the seventh in October 2011.

Design (RDD) method. It finally examines the relationship between the local elected mayor's ideology and violence, and presents a placebo analysis.

3.4.1 Econometric framework

This paper uses a RDD to identify the impact of the Urihist ideology on homicide rates in Colombian municipalities. It focuses on close-run mayoral elections in Colombia in two different electoral periods 2003 and 2007. The RDD exploits the fact that the propensity to be Urihist changes discontinuously at the threshold between Urihist and anti-Urihist parties win-loss threshold. I make the assumption that the outcome of close races is as good as random. Municipalities where Urihist lost by a large margin are more likely to differ from municipalities where Urihist-aligned parties won by a large margin. However, narrowing our focus to the set of municipalities with close-run election results makes it more plausible that election outcomes are determined by idiosyncratic factors and not by systematic differences in municipal characteristics that might also affect homicide rates.

This section examines the plausibility of the RDD's identifying assumptions in details. As our baseline analysis, we estimate the following regression model for Urihist parties that won or lost local elections by a narrow margin:

$$\begin{aligned} homicides_{m,p} = & \alpha_0 + \alpha_1 * Uwin_{m,p} + \alpha_2 * Uwin_{m,p} * f(spread)_{m,p} + \\ & \alpha_3 * (1 - Uwin_{m,p}) * f(spread)_{m,p} + \lambda_p + \epsilon_{m,p} \end{aligned} \quad (3.1)$$

with α_1 our RD estimate. The outcome variable $homicides_{m,p}$ is homicide rate in municipality m and electoral period p . In other specifications, homicide rate will be disaggregated by perpetrator, depending on whether the murderer is the official army, the guerrilla or a paramilitary group. $Uwin$ is a binary variable equal to 1 if the winning candidate is Urihist in municipality m and electoral period p . $spread$, the forcing variable, is the candidate's winning margin in municipality m and electoral period p . We test for the robustness of our results to different functional forms, $f(spread)$, for the RD polynomial, which is estimated separately on either side of the Urihist win-loss threshold. More formally, I compute the forcing variable $spread$ as follows:

Let

$$spread_{mp} = \frac{Vu_{it} - Vnu_{mp}}{T_{mp}}$$

Vu is the total number of votes obtained by the Urihist candidate who arrived first in municipality m and electoral period p , while Vnu is the total number of votes obtained by the anti-Urihist candidate who arrived first in municipality m and electoral period p . T is the turnout in municipality m and period p ⁵. As

⁵I also replicate the analysis by using the sum between the two main candidates in the denominator and get the exact same results.

a result, the treatment $Uwin$ takes the value 1 when an Uribe candidate won the elections in municipality m , and 0 otherwise. Formally:

$$Uwin_{mp} = 1 \text{ if } spread_{mp} > 0$$

and

$$Uwin_{mp} = 0 \text{ if } spread_{mp} < 0$$

In what follows, we focus on the sub-sample of municipalities m where $spread_{mp}$ is smaller than bandwidth $|spread|_m$ in the sense that the outcome of the elections can be considered as good as random. In the baseline specification, the value of h is set up at $h = 0,05$ as in Dell (2015). As robustness checks, I will relax $|spread|$ to optimal bandwidths as in Calonico et al. (2017) and Imbens and Kalyanaraman (2012). An un-conditional sense of the RD estimates can be expressed as follows:

$$\alpha = E[homicides_{mp}(Uwin_{mp} = 1) - homicides_{mp}(Uwin_{mp} = 0) \mid |spread_{mp}| \leq h]$$

The baseline specifications use linear or quadratic RD polynomials ($f(spread)$) as in Dell (2015) and technically developed in Imbens and Kalyanaraman (2012). Further results using higher-order polynomials are reported in Appendix. Some robustness specifications also include additional baseline controls such as region fixed effect, rural GDP, urban GDP, total GDP, population, distance to Bogota, altitude, coca farming, access to drinking water and turnout.

Colombia holds mayoral elections one year after the presidential elections, giving local candidates the leeway to take a stand for or against the central government. This study considers two close-run electoral periods 2003 and 2007 at different times throughout the sample period. The core of the study focuses on the baseline sample of municipalities with a 2003-2007 electoral vote spread of 5 percentage points or less. This is the bandwidth suggested by the literature (Dell, 2015). In order to provide more exhaustive evidence on how pre-characteristics vary around the threshold, I repeat this analysis limiting the sample to vote spread bandwidths of 4%, 3% and 2%, as well as the optimal bandwidth of 13.3% proposed by Imbens and Kalymanran (2012) and the optimal bandwidths proposed by Calonico et al. (2014) and used in Fergusson et al. (2020). All in all, these tables present qualitatively similar patterns.

3.4.2 Data

The analysis draws on three main sources of data. The first source is conflict-related data from the count of incidents recorded by Centro Nacional de Memoria Historica. These highly precise data include day-to-day information on attacks, bombings, murders, and so on perpetrated by all the players involved in the Colombian civil conflict. This dataset combines different sources, including CINEP's Noche y Niebla magazine with its detailed accounts of all incidents by player. The second source is detailed information

on attacks provided by the websites of human rights NGOs known for monitoring political violence in Colombia. The third source is detailed information on attacks covered by the leading national and local newspapers.

The paper also uses geographical and demographic controls measured at the municipality level and provided by the Center for the Study of Economic Development (CEDE) at Universidad de los Andes. This dataset forms a supplementary source of data on attacks, murders, kidnappings, and so on, since it presents information from official and institutional sources such as the National Police Agency and the Ministry of Defense.

The National Registry in charge of running elections (1958-2015) provided the electoral data with information on each candidate, total votes, share of votes, parties and electoral outcomes of the 2016 national referendum to ratify the peace agreement.

In this paper, I refer to the local elections held in Colombia in 2003 and 2007 following the election and re-election of President Alvaro Uribe Velez (in 2002 and 2006). Alvaro Uribe campaigned in the 2002 elections to combat the guerrillas without considering any political agreement with the enemy. His radical strategy against the FARC brought him massive support in the polls. This shift in the polls led a number of members of the Conservative Party (the second largest party in Colombia) to drop their party's official candidate and join forces with Uribe. He also received the support of a number of other parties and movements that used his radical position as their flagship campaign for the mayoral elections in 2003 and 2007 respectively.

The empirical strategy used in this paper entails identifying and coding the parties in support of or opposed to the military confrontation. The coding process is a three-step procedure. First, I identify whether the party was openly allied with the national incumbent (officially a member of the coalition). The second criterion is whether the party or its candidate had unofficial sympathies with the president⁶. The third criterion is whether the party or candidate was involved in the "para-politics" scandal⁷. These three criteria classify some 1,300 local mayors as coalition members and some 850 local mayors as members of opposition parties⁸ distinguishing by year 2003 and 2007.

Figure A1 in Appendix shows the distribution of the winning parties at the local elections of 2003 and 2007. The two most traditional parties, Liberal and Conservative, largely dominated both elections, with the Liberal Party being the leading opposition party in both cases. Figure 3.1 presents the distribution

⁶For this criterion, I searched extensively online for mentions, meetings, speeches, news and articles regarding each candidate to find any association with the national incumbent

⁷The Colombian "para-politics" or *parapolitica* scandal in Spanish (a combination of the Spanish words paramilitar and politica) refers to the 2006-2010 Colombian congressional scandal where a number of congressmen and other politicians were indicted for collusion with the United Self-Defense Forces of Colombia (AUC), a paramilitary group responsible for killing thousands of Colombian civilians.

⁸**Coalition parties:** Partido Social de la U, P Conservado C. , M Alas-Equipo Col, P Cambio Radical, P Convergencia Ciudadana, P Colombia Siempre, M Apertura Liberal, some other minors. **Non coalition parties:** Liberal Colombiano, Movimiento Alianza Social Indigena, Partido Verde OC, Movimiento Alianza Social Afrocolombiana, Movimiento Mira, Polo Democratico A.

of winning parties for the entire sample (panel A) and for the 5% win-lose margin sample (panel B). As already mentioned, Urihist parties dominated election outcomes at the national level. Yet, as expected in our sample, Panel B shows a balanced propensity for an election win between coalition and non-coalition parties in close-run elections, assuming that the outcomes of close-run races are as good as random.

3.4.3 Testing Assumptions

Identification calls for an absence of selective sorting around the Urihist win-loss windows threshold. This assumption would be violated, for example, if close-run elections were systematically won by parties associated with the incumbent party. To formally test for sorting, I implement the McCrary test (McCrary, 2008) by collapsing the election data to 1-percentage-point vote spread bins and using the observation count in each bin as the dependent variable in the equation 3.1. Figure 3.2 shows that the discontinuity estimate point is $-0,034$ ($SE = 0,121$) revealing non-significant discontinuous change in close-run Urihist vote at the win-loss threshold.

In addition, identification requires all relevant factors besides treatment to vary smoothly at the threshold between a Urihist victory and loss. In other words, outcomes from $(y \mid DS = 1)$ and $(y \mid DS = 0)$ should be continuous at the win-loss threshold, i.e. $spread = 0$. This condition is necessary for municipalities where Urihist candidates barely lost to be an appropriate counterfactual for those where they barely won. This assumption would be violated if the outcomes of close-run elections were determined not by idiosyncratic factors, but by a systematic advantage to the winners. More formally, let:

$$X_{it} = \frac{Vc_{it}}{T_{it}} - \frac{Vnc_{it}}{T_{it}}$$

Then, the treatment is defined as:

$$T_{it} = 1 \text{ if } X_{it} > 0$$

and

$$T_{it} = 0 \text{ if } X_{it} < 0$$

The focus is on the sample where X_{it} is smaller than bandwidth h , in the sense that the outcome of such races can be considered as good as random.

$$\alpha = E[Y_{it}(T_{it} = 1) - Y_{it}(T_{it} = 0) \mid X_{it} \leq h]$$

Table 3.1 displays the test for the plausibility of the identifying assumptions. It examines whether demographic, violence, economic and geographical pre-characteristics are balanced across the Urihist win-loss threshold. It also tests the same assumptions for variables of violence measured in the first year

following the elections.

Column 1 reports the mean value of different characteristics measured in municipalities where a pro-Uribe candidate won by a narrow margin; column 2 does the same for municipalities where anti-uribist candidates won by a narrow margin; column 3 reports the p-values for the means difference tests. Column 4 reports the RD coefficients, while their associated p-values are reported in column 5. The number of observations is reported in column 6. Despite an apparently imbalanced population across the two groups, it can be observed that most variables are continuous at the threshold⁹. While some coefficients are noisy, nearly all are not statistically significant.

3.4.4 Graphical analysis

I first analyze graphically the relationship between close-run election outcomes and violence attributable to different actors in the Colombian conflict. This paper focuses on close-run election outcomes at municipality level over the 2002-2010 period, which was the period during which Alvaro Uribe occupied the presidency.

The following graphs illustrate the findings using homicide rates per 100,000 inhabitants on the one hand (left-hand side panels) and the probability of recording at least one homicide in the municipality, with both variables measured in the three years following the election on the other hand (right-hand side panels). While Panels A and B consider all homicides (i.e. whatever their perpetrator), all other panels focus on homicides perpetrated by a given perpetrator. In the X-axis is the mayors margin of victory at the five percent threshold, with a negative margin indicating a Uribist loss. Each point represents the average probability in 0.5-percentage-point vote spread bins. All panels plot predicted values from a quadratic polynomial in the vote margin estimated separately on either side of the Uribist win-loss threshold, and the gray lines show the 95% confidence intervals.

Panels A and B of Figure 3.3 suggest that close Uribist victories significantly increased neither the total number of homicides nor the probability of reporting a homicide. However, dis-aggregating the figures by perpetrator brings a different picture. In particular, Panel C illustrates that the rate of homicides perpetrated by guerrillas increased by 12 per 100,000 inhabitants following a close-run Uribist victory. This result constitutes the main result of this paper. However, as seen in Panel D, the probability of reporting one homicide perpetrated by guerillas in a given municipality does not appear to be discontinuous at the threshold. It suggests that the difference only lies in the intensive margin, which could suggest an over-counting of homicides in municipalities where guerrillas have been found to have committed at least one homicide.

Panels E and F of Figure 3.3 shows that both the rate of homicides perpetrated by paramilitary groups and the probability of a homicide perpetrated by paramilitary groups in the three years following the

⁹The imbalance is mainly due to two large cities in which candidates were opposed to the Democratic Security Policy.

elections are similar regardless of whether the Uribeism barely won or lost the mayoral elections. We do observe a slight positive discontinuity at the intensive margin, but given the wide spread in homicide rates at the cut-off, the difference turns out to be non-significantly different from zero. The magnitude of these estimates is a concern, but is addressed in the next section.

Panels A and B of Figure 3.4 display the rate of homicides perpetrated by any official force (army or police), as well as the probability of having at least one homicide perpetrated by the army or the police in a given municipality in the three years following the elections. It shows that the difference between pro and anti-Uribeist municipalities converges towards zero for both outcomes. Lastly, Panel C and Panel D display the results when homicides cannot be attributed to any particular perpetrator and are registered as homicides from unknown actors. Again, the two panels respectively display the homicide rate and the probability of having a homicide reported at the Uribeist win-lose threshold in the three years following the elections. Note that both estimates are negative, but statistically non-significantly different from zero. While Panel C suggests that the rate of homicides committed by an unknown actor is lower at the threshold by 10 units per 100,000 inhabitants, Panel D shows that the probability of reporting a homicide perpetrated by an unknown actor drops by about 22 percentage points in municipalities where an Uribeist party closely won the mayoral election. These two results could be related to those found for homicides perpetrated by guerrillas. It could be indeed that when homicides cannot be attributed to a precise perpetrator, municipalities in which the newly elected mayor is Uribeist tend to attribute them to guerrilla groups. This could explain both the apparent increase in homicides committed by guerrillas and the symmetric apparent decrease in homicides committed by unknown actors. This will be further investigated in the section on mechanisms.

3.4.5 Baseline Results

Table 3.2 reports the RD estimates of equation 3.1 for both the 2003-2007 and 2007-2011 five percent close-run margin mayoral election samples. The table considers as the outcome variable the number of homicides per 100,000 inhabitants in the municipality in the three years following the election, excluding the lame duck period¹⁰. Estimates suggest that Uribeist mayoral victories increase homicides perpetrated by guerrilla groups by 12.09 (SE=5.47) in the three years following the elections. By contrast, estimates using all homicides or homicides perpetrated by paramilitary groups only reveal no significant difference between municipalities where Uribeist candidates barely won or lost the election. All these results are robust to changes in the length of the period analyzed, using different polynomials and optimal bandwidths and controls, as can be observed in the section on robustness checks (Tables, 3.3).

In Panel A, point estimates for the total number of homicides (Column 1) and homicides committed by

¹⁰In politics, the lame duck period corresponds to the period when the successor of an outgoing elected official has already been elected or will be soon. The official is hence often considered as having less influence due to the limited time left in office.

the paramilitary in panel A (Column 4) appear to be quite relevant, with their non-significance being due to the fact that the standard errors are large. Our results suggest that the total number of murders increases by almost 20 per 100,000 inhabitants due to the increase in the number of homicides perpetrated by guerrillas and the paramilitary in the municipalities where mayors won by a narrow margin. An important concern is that there does not appear to be any difference between the point estimates in Columns 3 and 4. The reason for this is that the standard deviation of the distribution of the two variables is different. The large standard deviation in Column 4 can be explained by the very high number of homicides in two municipalities. To account for this, Panel B displays the results of the main regression after excluding outliers, where outliers are those municipalities in which the homicide rate exceeds the mean plus three standard deviations of each distribution. Overall, results remain significant for guerrillas but the RD estimate using homicides perpetrated by paramilitary bands falls to -2.853 , which confirms that outliers drive the large coefficient in Panel A¹¹.

Another aspect to be highlighted from Panels A and C of Table 3.2 concerns Column 5. The point estimate is negative and significant, with the magnitude of the coefficient suggesting that municipalities in which local elected officials adopted the Uribe ideology are significantly less likely to report a homicide committed by an unknown actor. We will come back to this interpretation of this effect in the next section.

3.4.6 Placebo tests

As a placebo exercise, I estimate Equation 3.1 separately for the election year and following the inauguration of the new authorities. Figure 3.5 plots the coefficient of the "Uribe win" dummy variable using quarterly data on the number of homicides perpetrated by guerrillas per 100,000 inhabitants before and after the election year. Each point is the RD point estimate of a regression where the dependent variable is the number of homicides perpetrated by guerrillas per 100,000 inhabitants measured from 24 quarters (8 years) before the elections up to 12 quarters (3 years) after the elections. The homicide rate is cumulative starting at each election cut-off (say quarters -24 and -12), which explains why the confidence intervals increase from -24 and -12 periods. The dash vertical line represents the election date. As suggested by the figure, the number of homicides perpetrated by guerrilla groups (per 100,000 inhabitants) was balanced between Uribe and anti-Uribe municipalities up to the elections (even going far back) but started to diverge between the two groups of municipalities in the first post-election year, being significantly higher in municipalities where the winner supported the Democratic Security Policy.

¹¹Note that the difference in the number of observations from panel A to panel B corresponds to the number of municipalities where homicides are considered as outliers

3.4.7 Robustness checks

All these results are robust to a change in the length of the period analyzed, using different polynomials and optimal bandwidths and controls, as shown in Table 3.3. Column 1 reports the baseline estimates of Table 3.2. In Column 2, observations are weighted assuming a uniform distribution at the 5 percent bandwidth. Columns 3 and 4 show estimates using different polynomials, Column 5 provides different data-driven bandwidth selectors based on the recent work of Calonico et al. (2017). Column 6 includes the *Plug-In* procedure to estimate the optimal bandwidth of balancing the degree of bias and precision adapted to RD by Imbens and Kalyanaraman (2012). Finally, Column 7 includes a large set of controls. Overall, Table 3.3 confirms that the estimates in Table 3.2 are robust to those different changes.

3.5 Mechanisms

This paper does not claim to causally identify the mechanisms driving the results presented in the previous section. Instead, the study considers a number of alternative explanations. This is not meant to be an exhaustive list of possible explanations, but rather a review of those that seem the most plausible. First, we investigate whether the increase in the number of homicides perpetrated by guerrillas could be due to a reaction from the rebels. As an alternative, we explore whether it could rather be due to an over-count of the number of murders attributed to guerrillas in municipalities where local leaders were aligned with the national incumbent's policy.

3.5.1 Reaction

Given the active policy against the guerrillas, rebels might have retaliated against local politicians in anticipation of an offensive from local Uribist ideology. If this retaliation hypothesis were true, we would expect to see guerrilla groups expressing their discontent not only through homicides but also through other acts of violence in Uribist municipalities up to three years after the elections. To see whether this happened, Graphs 3.6 and 3.7 display the RD estimates of the "Uribist win" dummy variable on rapes, terrorist attacks, kidnappings and military presence by guerrillas in the sample municipalities. We find no discontinuity in any of these variables. The lack of discontinuity in the number of violent acts generally strongly associated with murders may suggest that the increase in the (reported) number of homicides committed by guerrillas is due less to military retaliation, than to an over-count in the records.

In order to further test the retaliation assumption, I run equation 3.1 on a binary variable accounting for the presence of different military groups in the municipality: FARC or Paramilitary groups. The results are presented in Table 3.4. The dependent variable in each column is a dummy variable taking the value of 1 if the group had any presence in a given municipality one year before the elections (Columns 1 and 2), one year after the elections (Columns 3 and 4), two years after the elections (Columns 5 and 6) and three years

after the elections (Columns 6 and 7). This exercise shows that there is no discontinuity at the threshold, whatever the period. This suggests either that homicides were committed by guerrilla groups on incursion from outside of where they were based, or that the homicide records were manipulated.

If guerrilla groups retaliated in anticipation of local authorities launching a military offensive against them, violence from these groups would express itself in different ways such as through increased terrorist attacks and kidnappings. Table 3.5 displays RD estimates obtained using the occurrence of kidnappings perpetrated by different actors in the first year following the elections (Results using data on the occurrence of kidnappings in other years are shown in Appendix). The dependent variable in Column 1 is a dummy variable taking the value of 1 if a kidnapping was committed (whatever the perpetrator) in a given municipality in the year following the elections. The dependent variables in Columns 2,3,4 and 5 respectively capture the likelihood of having a kidnapping committed by paramilitary groups, guerrillas groups, the FARC and an unknown actor. The occurrence of Kidnappings at the municipality level is an interesting variable since guerrilla groups widely used this method to fund their actions. In fact, most of the cases of kidnappings were associated with guerrilla groups. However, Table 3.5 finds no effect of close-run electoral outcomes on kidnappings. Given that any anticipation strategy by the guerrillas would be associated with other forms of warfare, it could be concluded that the absence of an effect on kidnappings is in line with the hypothesis of manipulation of the main figures to give the guerrilla groups a bad reputation in the eyes of the public opinion.

3.5.2 Reporting biases

In this section, I test whether the number of homicides perpetrated by guerrillas might have been misreported in order to give the guerrillas a bad reputation in municipalities where the incumbent party and their allies won the elections. This hypothesis is not new in the literature. Loftin et al. (2015) pointed out the lack of accuracy in the Uniform Crime Reporting Program's Supplementary Homicide Report (SHR) data in the USA in order to give a good image of the police.

In a companion paper, Loftin et al. (2017) found that coverage and nonresponse errors were the primary reasons for underreporting. They concluded that if police involvement was not mentioned on a death certificate, the death was misclassified as a civilian homicide. In both papers, Loftin and his colleagues found inconsistent reporting by local police departments as a systematic source of error. They pointed out that erroneous facts might be used by politicians who following certain ideology could confuse the public intentionally or unintentionally.

3.5.2.1 From unknown perpetrator to guerrillas

A first evidence of such misreporting is presented in Panel A of Table 3.2. Two aspects are worth emphasizing. First, Column 3 shows that, at the threshold, the number of homicides by guerrillas per

100,000 inhabitants is larger and significant when the coalition member won by a narrow margin. In the meantime, Column 5 shows that the number of homicides by unknown actors per 100,000 inhabitants is lower in municipalities where the coalition party won by a short lead, with estimates being of similar magnitude. As mentioned above, this could reflect the fact that when homicides could not be attributed to a precise perpetrator, municipalities in which the newly elected mayor is Uribist attributed them to guerilla groups. In addition, Panel C of Table 3.2 shows that the probability of reporting a homicide by an unknown actor in municipalities where a coalition member won by a slight margin is lower (Column 5). I further find that in at least ten pro-coalition municipalities, there was no homicide at all by unknown actors, but some victims of guerrillas. This pattern is not observed in municipalities where coalition members lost by a small margin.

3.5.2.2 A benchmark data source

Testing the reliability of the official records ideally calls for accessing data that are proven to be free of bias. This paper draws on a new *benchmark* data source. The dataset codes violent events covered by *Noche y Niebla*¹² reports published by non-governmental organization (NGO) Centro de Investigacion y Educacion Popular (CINEP), set up by the Company of Jesus in Colombia. Each report provides a detailed description of the violent event, its date, the municipality in which it occurred, the perpetrator's identity, and the body count. The reliability of this source resides in the fact that the data are collected jointly with the Catholic Church, which is present even in the most remote areas of the country, allowing for extensive coverage of violent events across the entire country.

Panel A of Table 3.6 reports the results of estimating equation 3.1 using data on homicides recorded solely by Noche y Niebla (NyN) magazine at the municipality level. The dependent variable in Column 1 is the number of homicides perpetrated by official forces per 100,000 inhabitants in the three years following the elections. The dependent variables in Columns 2, 3 and 4 are the number of homicides perpetrated by paramilitary forces, guerrilla groups and unknown actors respectively, over the same period. Given the reliability of the data, the plausibility of a body overcount mechanism would be confirmed if no discontinuity were observed in Column 3.

As an additional test, I estimate equation 3.1 using solely the records put together by the Police, Ministry of Defense and the National Army, as provided by La Universidad de los Andes. Panel B of Table 3.6 reports RD estimates using the total number of homicides per 100,000 inhabitants (column 1), the number of homicides perpetrated by the paramilitary only (per 100,000 inhabitants) (column 2), the number of homicides perpetrated by guerrillas (per 100,000 inhabitants) (column 3), and the number of homicides perpetrated by unknown actors (per 100,000 inhabitants) (column 4). The patterns are found to be similar to those reported in the previous section. Note that the discontinuity in disfavor of guerrilla groups is more

¹²Noche y Niebla

pronounced in municipalities where coalition members won by a narrow margin and where the homicides were reported by the police. None of the other point estimates is significant. It is also interesting to see that the RD estimate for homicides by unknown actors is negative with a point estimate of similar magnitude. Comparing the results of Table 3.6 panel B with those obtained using the benchmark data 3.6 panel A, it could be concluded that the difference is due to a potential overcount of bodies attributed to guerrilla groups in municipalities where the Uribeism won the mayoral election.

3.5.2.3 Subgroup analysis of municipalities with false positive cases

In order to push further the analysis, I test whether a link can be established between the previous results and the false positives scandal. To this end, I conduct an RDD subgroup analysis to compare municipalities which reported cases of false positives with municipalities that did not. Table 8 shows that both groups are statistically different considering their observed characteristics. I then use the propensity score weighting methodology proposed by Carril et al. (2017) and Carril et al. (2018) to balance both groups¹³. The overall P-value for the covariate joint significance test changes from 4.7213 to 0.3695. This suggests that differences in the estimates obtained after running the RDD separately in each subgroup after weighting is unlikely to be driven by differences in the characteristics shown in Table 3.7.

The results are displayed in Table 3.8 and Figure 3.8. I find a significant effect only in those municipalities involved in the false positives scandal and the difference between both groups is statistically significant. Note that if I did not apply the propensity score weighting, I would be underestimating the differential effect. This result suggests that the impact of electing a coalition party on the number of murders is only significant in municipalities in which political authorities were involved in the false positives scandal and were able to alter the reported number of guerrilla victims.

3.5.2.4 Subgroup analysis of official figures for false positive municipalities

In this section, I run naive estimates combining the official records provided by the Universidad de Los Andes with the data on the false positive zones. Taking the figures reported by the National Police, National Army and Ministry of Defense, as in Table 3.6, I run equation 3.1 solely on the sub-sample of municipalities that experienced at least one case of false positives.

Panel A of Table 3.9 shows the results of estimating equation 3.1 solely for the sub-sample of municipalities that experienced false positive cases. The dependent variable in Column 1 is total homicides in the respective municipality per 100,000 inhabitants. The dependent variables in columns 2, 3 and 4 are

¹³I conduct a binary subgroup analysis using settings based on the inverse propensity score weights (IPSW) proposed by Carril et al. (2017) and Carril et al. (2018). The observations for each subgroup are weighted by the inverse of their conditional probabilities of belonging to that subgroup, given a set of covariates. Analysis of the differential treatment effect in the reweighted sample isolates the difference due to the subgroup characteristic of interest from other observable dimensions.

homicides perpetrated by paramilitary groups, guerrillas and unknown actors per 100,000 inhabitants respectively. Again, we find that at the threshold, the number of homicides committed by guerrillas in municipalities led by Uribe parties is about 20 units higher than in municipalities where the non-coalition parties lost by a narrow margin. Note that the estimates in Column 4 (homicides perpetrated by unknown actors) is of similar magnitude in absolute terms, which brings support to the hypothesis that the number of bodies were especially overcounted in municipalities where false positive cases occurred. This result is robust to different polynomials, uniform kernel distribution, and different optimal bandwidths (see Table A1).

Table 3.9 Panel B, presents the main results of equation 3.1 estimated on the sub-sample of municipalities where no cases of false positives occurred. No discontinuity is observed in any of the variables in these municipalities. This suggests that our main results are driven by municipalities involved in the false positives scandal, which comes in support of the existence of a link between cases of false positives and the overcount of the number of homicides assigned to the guerrillas.

3.5.2.5 Overcount of coalition vs undercount of opposition

The main results presented in table 3.2 could also be due to close-run leftwing party election winners undercounting the number of homicides perpetrated by guerrillas and recording them as homicides committed by unknown actors. In order to test this channel, we exclude from our sample extreme leftwing parties that have expressed sympathies with the guerrilla groups either in the past or the present.

To classify the leftwing parties, this paper uses the method presented in Fergusson et al. (2020), who checked party names, mottos, and slogans for words that clearly identified a party as leftwing (e.g. communist or socialist). For instance, these criteria classified the Communist Party of Colombia as leftwing. Second, given that only a few parties met these criteria, they took up the method used by Budge, Bara, Volkens, and Klingemann (2001) and also searched in party statutes (when available) for clearly leftwing policy stances. They coded a party as leftwing if the party statutes included at least three of the following five leftwing policy positions: (1) pro-peasant, (2) greater market regulation, (3) defense of workers against exploitation, (4) state-owned or collective property rights, and (5) anti-imperialism. Note that similar policy positions were the mainstay of most of the guerrilla groups in Colombia.

A total of ten parties are classified as leftwing on the basis of these criteria. Table 3.10 presents the results of equation 3.1 excluding all leftwing parties. We note that results are robust to extracting leftwing parties from the sample, parties that would have stood more to gain from undercounting the number of homicides by guerrillas. The coefficient of interest in Column 3 is of even higher magnitude when those parties are excluded. This result should come as no surprise. The institution in charge of recording homicides at local level is not the administrative unit, but the local police or army. I put it that non-coalition parties winning office by a narrow margin might have overseen the army's homicide recording and reporting to prevent it from arbitrarily assigning responsibility to the rebels.

3.5.3 Anecdotal evidence

This section finally provides some anecdotal evidence mainly drawn from newspapers to document the national controversy over the potential biases in official records of the conflict. Between 2010 and 2018, the Colombian government, led by Juan Manuel Santos, held peace talks with the main rebel group, the FARC. In 2016, the parties reached an agreement to end the conflict in Colombia. In 2011, the Colombian government set up the Centro Nacional de Memoria Histórica (CNMH), to officially contribute to the country's duty of remembrance with respect to the rapes and other violent acts committed during the Colombian armed conflict. Since then, this institute has been in charge of (officially) documenting all incidents associated with the Colombian conflict.

In 2018, the party led by former president Alvaro Uribe Velez won the presidential elections. This victory implied a change in the heads of the different public institutes, including the CNMH. The leading candidate was journalist Mario Javier Pacheco, who had previously publicly stated that the CNMH was a "structure infiltrated by the guerrillas"¹⁴. He also said:

"(...) Note that this institute (the CNMH) is another of those infiltrated structures funded by the government to legitimize the State itself... They present reports loaded with tendentious omissions that seek to minimize the horrors committed by the guerrillas and blame the State and paramilitary groups."

Pacheco's controversial opinions were such that the President of Colombia had to find someone else for the position. The president chose to appoint Dario Acevedo. Mr. Acevedo had also been criticized for openly denying the existence of a conflict in Colombia, arguing that the country's problem was a terrorist threat. Civil associations and victims published a letter sent to the Colombian president in which they rejected Mr. Acevedo's appointment to the institute.

"We consider that Mr. Acevedo does not meet the criterion of impartiality since, throughout his personal and professional life, he has failed to show the objectivity required to write the history of the war, which implies an extremely dangerous bias for the construction of the memory of Colombian society."

In addition, 89 victim and human rights defense organizations signed a letter sent to the country's President asking him to reconsider his decision, arguing that Mr. Acevedo did not meet the required criterion of objectivity to govern the institute. Pablo Cala, a human rights defender and member of the Hasta Enconlos Foundation, put it that Acevedo's appointment to the CNMH raised concerns about the manipulation of historical memory:

"Justifications are extracted from his writings, not only about the non-existence of armed conflict, but elements that may end up justifying certain rationales such as paramilitarism. That will end up being a misrepresentation of the CNMH."

¹⁴<https://www.kienyke.com/kien-escribe/el-pais-de-la-guerrilla-por-mario-javier-pacheco> et <https://www.elparchedelcapuchino.com/carta-abierta-a-marquez-y-al-paisa-de-las-farc/>

Wilfredo Canizarez, Director of Progresar, a human rights defense NGO, announced that his organization and others would not contribute by sharing more than 30 years of records. He even raised concerns about the previous director of the CNMH, saying that they had reservations about how inquiries were conducted in the regions and that there were too many potential measurement errors. He stated:

"We were cautious about them (CNMH) because we did not like their methodology when recording events, collecting information and writing books. They just went into the field to interview people two or three days, that's all."

This is very serious accusation, since he is insinuating that there are systematic errors in the data collection methodology, which is in line with the findings of this paper. For that reason, many NGOs have started withdrawing all the files they supplied to the CNMH. Collecting data without any rigor can lead to the construction of a biased narrative about the war, as is documented in this paper.

3.6 Concluding remarks

This study examines the direct effect of electoral outcomes on violence in Colombia over the 2002-2010 period and reaches the following conclusions. First, regression discontinuity estimates show that the rate of homicides by guerrillas at the municipality level increased substantially when a candidate in favor of the Uribism won the local election by a close margin (12.03 per 100,000 inhabitants). These results are robust to changing the kernel type, different optimal bandwidths, the inclusion of covariates and different order polynomials. By contrast, the probability of a homicide committed by an unknown perpetrator in Uribist municipalities is significantly lower by 20%.

This paper explores two potential mechanisms that could explain these results. First, the higher level of homicides by guerrilla groups in Uribist municipalities could be due to an overcount of bodies by local authorities intended to give the guerrillas a bad reputation. Second, guerrillas might have anticipated an offensive in municipalities where Uribist parties won by a narrow margin. They might then have retaliated against the local authorities by killing politicians and other civilians.

The paper uses two alternative methods to provide evidence of the first mechanism. It first focuses on the 'false positives' scandal. A higher level of homicides by guerrillas is found in municipalities implicated in the false positives scandal where elected representatives were Uribist (58 more victims per 100,000 inhabitants). These findings point to a clear link between the number of homicides allegedly perpetrated by guerrillas and cases of false positives.

Second, the sources of the records on civilians murdered in the conflict – national police agency (NPA) and non-governmental organizations (NGOs) – are disentangled, revealing that the difference in homicides by guerrillas holds only in the NPA records and more especially in those municipalities implicated in false positive cases. This result brings support to the existence of a link between murders reported by the NPA and false positives. Furthermore, the study gets a significantly negative unknown perpetrator coefficient

comparable in magnitude to the homicides by guerrilla groups. This suggests that uribist municipalities may well have counted bodies as murders committed by guerrillas when they would normally have been reported as being perpetrated by persons unknown. All this points to a link between local preferences for Uribism and an over-count of homicides by guerrillas, probably in order to build the guerrillas' bad reputation.

For the second mechanism to hold, a discontinuity at the threshold would be expected in the number of clashes with and military operations against guerrillas and other forms of violence perpetrated by guerrillas such as kidnappings. In order to test for this, we combine different data sources on the conflict, and find no significant differences in the number of clashes, kidnappings and other forms of violence between official forces and guerrillas in municipalities where Uribist candidates won by a narrow margin. We do not find any difference between the probabilities of guerrilla presence in the two groups of municipalities either. This study recommends that society, the international community and governments set up post-conflict commissions tasked with telling the truth about the conflict in order to counter hatred and polarization, and thereby consolidate peace.

3.7 Appendix

FIGURE 3.1: Distribution of DS vs non-DS winning parties in all municipalities and in sample municipalities

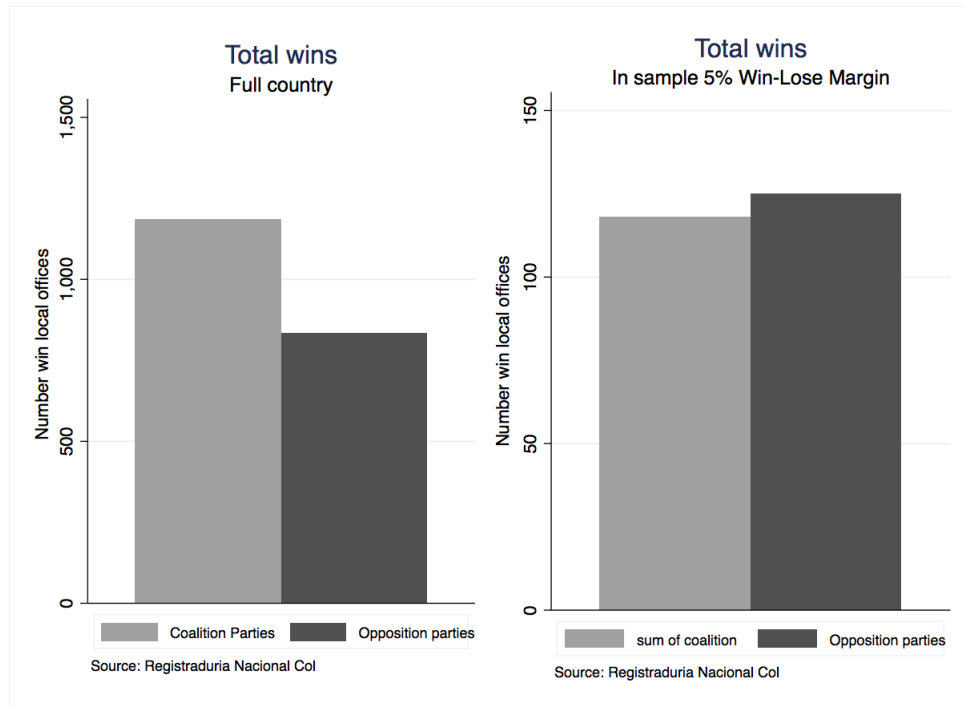


FIGURE 3.2: Sorting Around the Uribist Winning Threshold Vote

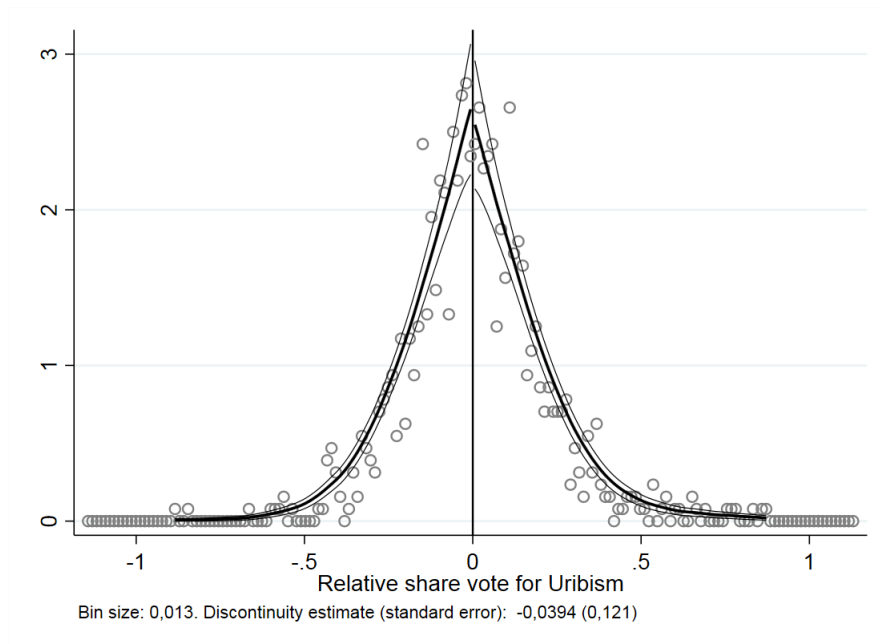
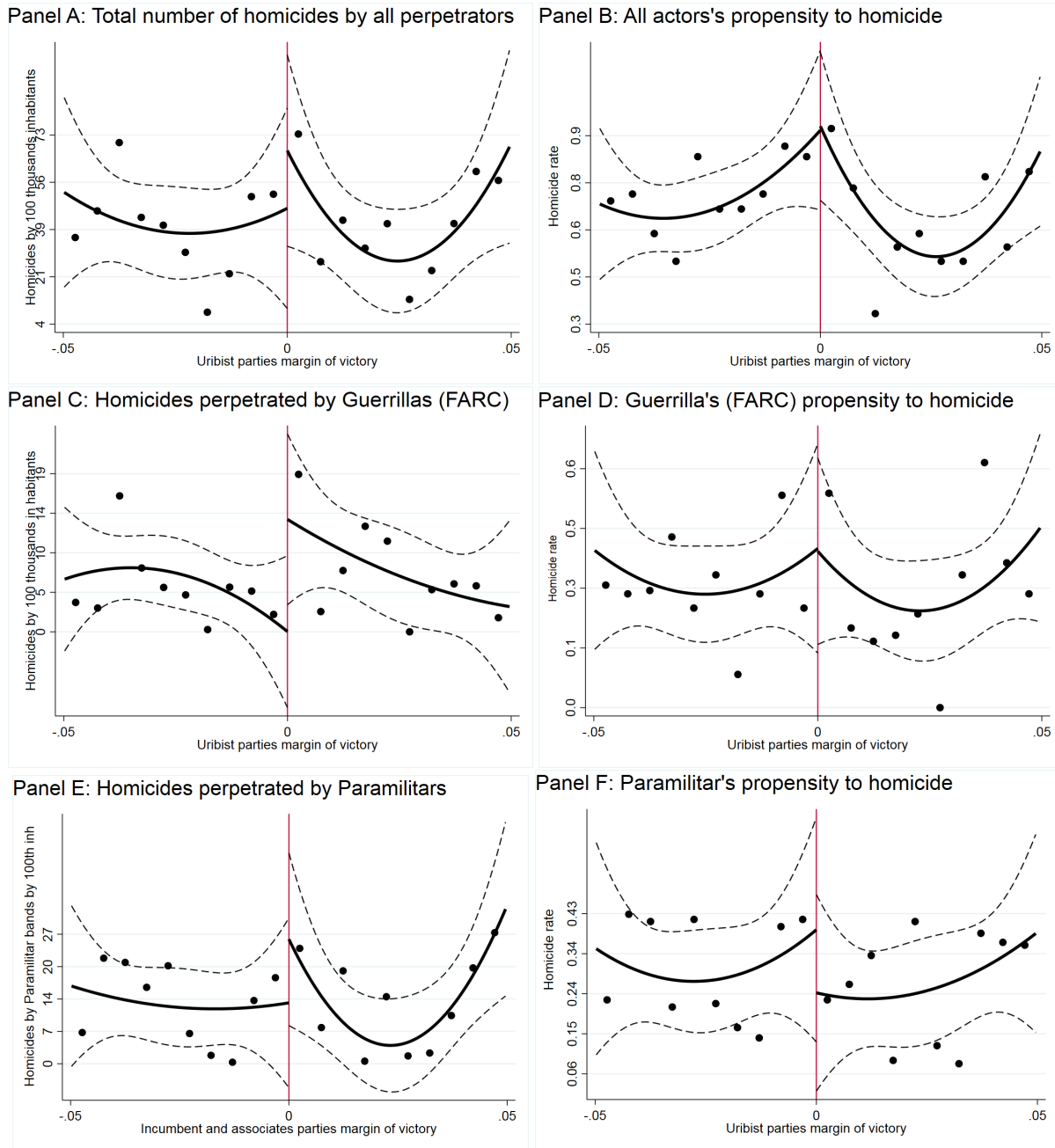


TABLE 3.1: RD estimates on baseline characteristics

	5 percent vote spread		P value	RDD	P-value
	Coalition Mean	Opposition Mean	Diff	Estimate	RDD
<i>Demographics at the base line</i>					
Access to DW	12,54	15,43	0,31	9,02	0,04
Rural pop	11370,98	11348,77	0,99	3525,03	0,22
Urban Pop	20815,17	26015,07	0,51	2352,46	0,91
Distant from cap	333,1	341,11	0,75	67,29	0,12
Altitude	1073,13	1000,07	0,51	-56,54	0,80
Agricultural GDP	1,65	1,69	0,88	-0,86	0,07
Industrial GDP	2,41	3,01	0,45	-3,31	0,17
Share pob secondary	0,13	0,13	0,77	0,00	0,43
Literacy index	84,25	82,75	0,2	-2,89	0,27
<i>Violence base line</i>					
Manual Eradication	90,63	43,76	0,51	-77,58	0,11
Labs dismantling	1,17	0,70	0,39	0,46	0,40
Kidnapping Param	0,08	0,14	0,56	0,25	0,13
Kidnapping Guerrilla	0,76	0,38	0,25	1,29	0,06
Displacement Out	480,45	373,18	0,29	312,83	0,28
Displacement In	516,94	393,71	0,41	418,78	0,12
<i>Violence post elections</i>					
Coca cultivation year 1	0,23	0,16	0,17	0,08	0,45
Coca cultivation year 2	0,20	0,17	0,57	0,09	0,43
Manual Eradication	107,71	83,91	0,75	-11,31	0,84
Labs dismantling	0,28	0,90	0,30	0,58	0,24
Kidnapping	0,75	0,98	0,47	0,04	0,96
AUC's Kidnapping	0,08	0,09	0,88	-0,04	0,45
Farc's Kidnappings	0,33	0,19	0,50	0,56	0,50
Displacement out	1531,41	1721,89	0,59	430,92	0,47
Displacement in	1129,52	983,09	0,52	371,52	0,35
Observations	120	128		248	

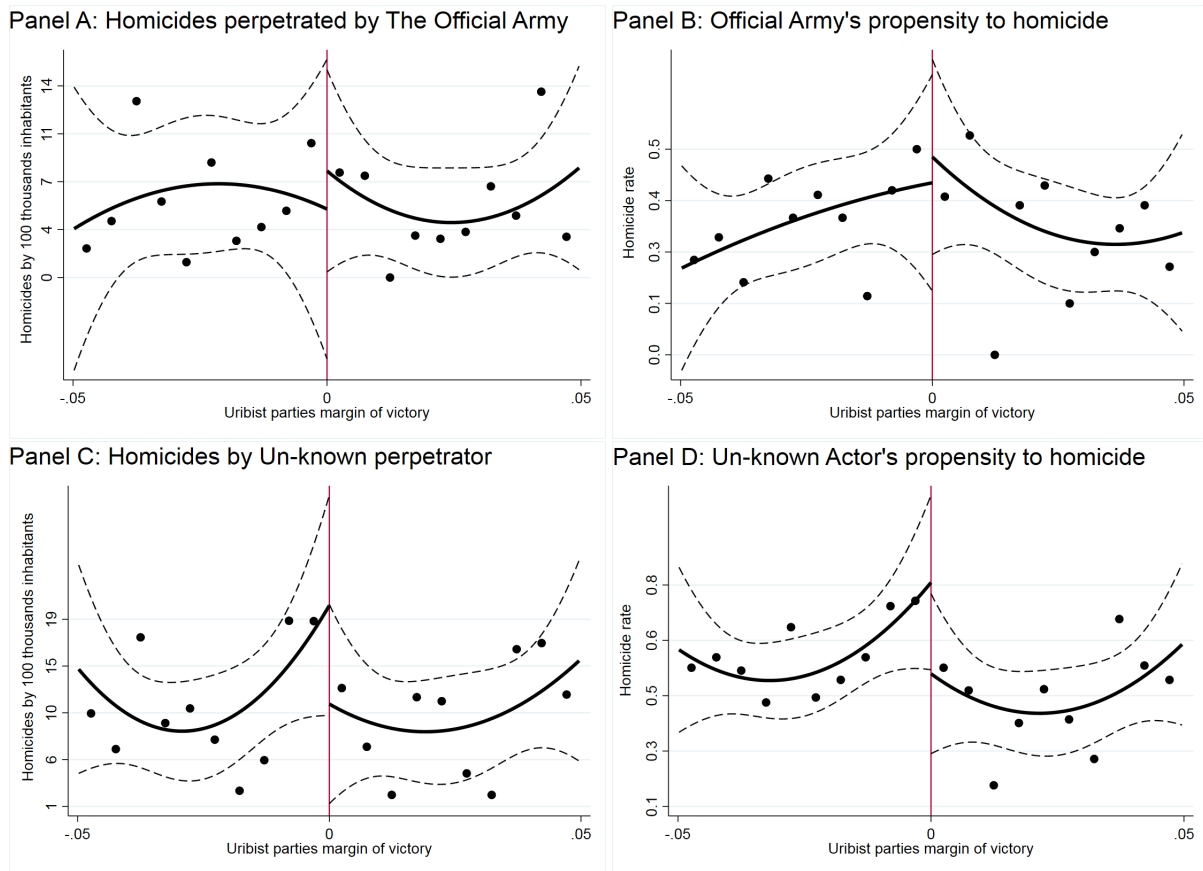
Notes: Columns 1 through 5 only concern municipalities with close-run elections. Column 3 reports the p-value for the difference in means between municipalities where coalition candidates barely won and where they barely lost. Column 4 reports the coefficient for a coalition win from a standard RD specification where the respective characteristic is used as the dependent variable, and column 5 reports the respective p-value.

FIGURE 3.3: Post-election homicides at the Uribist Win-Loss Threshold



On the left-hand side panels, the Y-axis is the number of homicides per 100,000 inhabitants in the three years following the mayoral elections. The X-axis is the Uribist margin of victory. On the right-hand side panels, the Y-axis is the probability of having at least one homicide reported in the municipality in the three years following the mayoral elections. Likewise, on the X-axis is the Uribist margin of victory. A negative margin indicates an Uribist loss, while a positive margin indicates an Uribist win. Each point represents the average value of the outcome in vote spread bins of width one-half of a percentage point. The solid line plots predicted values, with separate quadratic vote spread trends estimated on either side of the Uribist win-loss threshold. The dashed lines show the 95 percent confidence intervals.

FIGURE 3.4: Post-election Homicides at the Uribist Win-Loss Threshold (Continue)



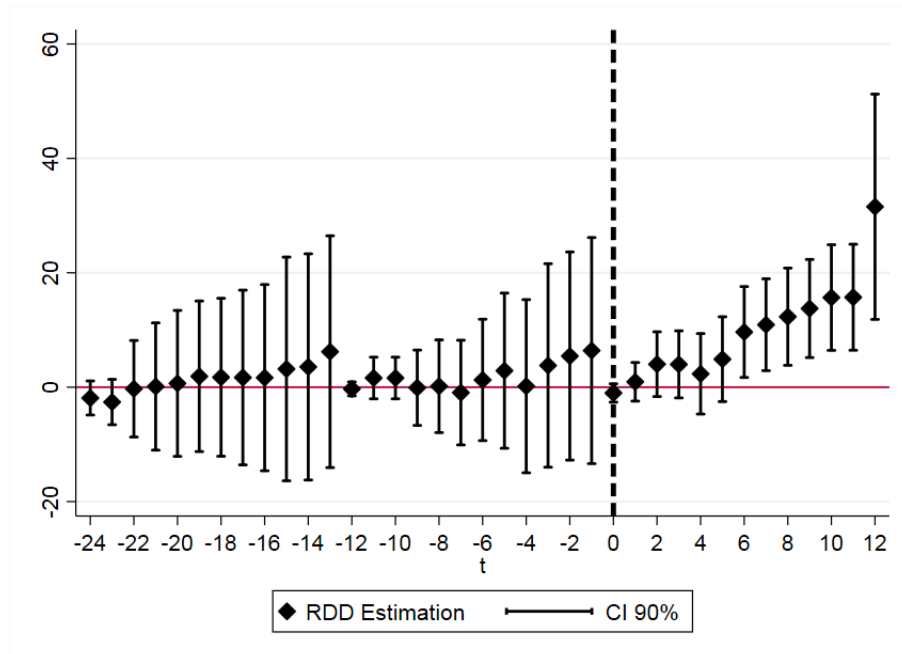
On the left-hand side panels, the Y-axis is the number of homicides per 100,000 inhabitants in the three years following the mayoral elections. The X-axis is the Uribist margin of victory. On the right-hand side panels, the Y-axis is the probability of having at least one homicide reported in the municipality in the three years following the mayoral elections. Likewise, on the X-axis is the Uribist margin of victory. A negative margin indicates an Uribist loss, while a positive one indicates an Uribist win. Each point represents the average value of the outcome in vote spread bins of width one-half of a percentage point. The solid line plots predicted values, with separate quadratic vote spread trends estimated on either side of the Uribist win-loss threshold. The dashed lines show the 95 percent confidence intervals.

TABLE 3.2: Impact of close Uribist elections on homicide rates by perpetrator (2003 and 2007 elections)

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitar	(5) Unknown
<i>Panel A: on Homicide rates</i>					
Uribist win	19.66 (20.54)	0.754 (4.147)	12.03** (5.478)	10.90 (12.66)	-10.319 (7.536)
Observations	248	248	248	248	248
Mean Control	41.10	5.10	5.90	12.61	11.20
<i>Panel B: Excluding outliers</i>					
Uribist win	1.188 (17.89)	-0.407 (3.965)	9.265* (5.345)	-2.853 (7.851)	-5.522 (7.542)
Observations	245	241	243	245	247
Mean Control	39.60	4.23	5.38	11.35	10.93
<i>Panel C: on probability</i>					
Uribist win	-0.0255 (0.0971)	0.0350 (0.151)	0.00243 (0.117)	-0.000196 (0.0746)	-0.224* (0.133)
Observations	248	248	248	248	248

Panel A and B examine the effect of Uribist close win mayoral elections on the number of homicides per 100,000 inhabitants at the municipality level. In column 1, the dependent variable is homicide rate committed by all actors in conflict. In Columns 2 to 5, the dependent variable is homicide rate perpetrated by the Official Army, Guerrillas (FARC), Paramilitary and Unknown actors respectively. The variable "Uribist win" is an indicator equals to one if an Uribist candidate won the election in a given municipality. The sample includes all municipalities where the Uribist candidate was first or second by a 5 percentage point or less vote spread margin at the 2003 and 2007 elections. Control mean is the average of the dependent variable computed on those municipalities where anti-Uribist candidates won the election. In Panel B, outliers are excluded from the sample. In Panel C, the dependent variable is the probability of having at least one homicide in a given municipality perpetrated by a specific actor. All columns include a linear RD polynomial estimated separately on either side of the threshold and triangular kernel type. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE 3.5: Number of homicides perpetrated by guerrilla groups (per 100,000 inhabitants)



Each point coefficients is a separate RD estimate of the impact of coalition victory on the number of homicides perpetrated by any guerrilla band, the thin lines plot 95 percent confidence intervals.

TABLE 3.3: Effect of Urihist elections on homicide rates - Robustness checks

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Dependent variable:							
<i>Homicides by the Army</i>							
Urihist win	0.754 (4.147)	-1.014 (3.308)	3.776 (5.985)	8.955 (8.102)	-0.171 (2.296)	-0.878 (2.593)	0.367 (3.622)
Observations	248	248	248	248	649	487	248
Bandwidth	0,05%	0,05%	0,05%	0,05%	0,16 %	0,113 %	0,05 %
CI length change							-2040.03
Panel B: Dependent variable:							
<i>Homicides by Guerrillas</i>							
Urihist win	12.03** (5.478)	10.19** (4.560)	14.50 (8.885)	29.34* (17.32)	5.087 (3.141)	7.583** (3.820)	9.834** (4.053)
Observations	248	248	248	248	616	455	248
Bandwidth	0,050	0,050	0,050	0,050	0,148	0,105	0,50
CI length change							-2462.93
Panel C: Dependent variable :							
<i>Homicides by Paramilitary groups</i>							
Urihist win	6.910 (5.113)	2.716 (9.561)	21.80 (18.24)	25.89 (44.00)	2.620 (5.378)	3.582 (6.910)	10.57 (11.38)
Observations	248	248	248	248	645	479	248
Bandwidth	0.050	0.050	0.050	0.050	0.158	0.112	0.050
CI length change							-7840.56
Panel D: Dependent variable:							
<i>Homicides by unknown actors</i>							
Urihist win	-5.319 (7.536)	-4.786 (5.827)	-6.300 (10.59)	-5.180 (23.07)	-2.415 (3.668)	-2.582 (4.713)	-5.244 (7.069)
Observations	248	248	248	248	578	425	248
Bandwidth	0.050	0.050	0.050	0.050	0.138	0.097	0.050
BW Type	Manual	Manual	Manual	Manual	Calonico	Imbens	Manual
Covariates	No	No	No	No	No	No	Yes
Order Loc. Poly. (p)	1	1	2	4	1	1	1

The panels refer to the different outcome variables used in Table 3.2 and display the effect of Urihist close win mayoral elections on the number of homicides per 100,000 inhabitants at the municipality level depending on the identity of the perpetrator. More precisely, Panels A, B, C and D refer to the Official Army, Guerrillas, Paramilitary groups and unknown groups respectively. Column 1 shows the baseline estimates; Column 2 changes the kernel type to uniform; Column 3 change the polynomial to quadratic; Column 4 reports the outcomes using the polynomial at level 4; Column 5 provides different data-driven bandwidth selectors based on the recent work of Calonico et al. (2017); Column 6 includes the *Plug-In* procedure to estimate the optimal bandwidth of balancing the degree of bias and precision adapted to RD by Imbens and Kalyanaraman (2012); and Column 7 includes a set of control variables. The sample includes all municipalities where the Urihist candidate was first or second by a 5 percentage point or less vote spread margin at the 2003 and 2007 elections. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

FIGURE 3.6: Effect of Urihist elections on alternative acts of violence by guerrillas

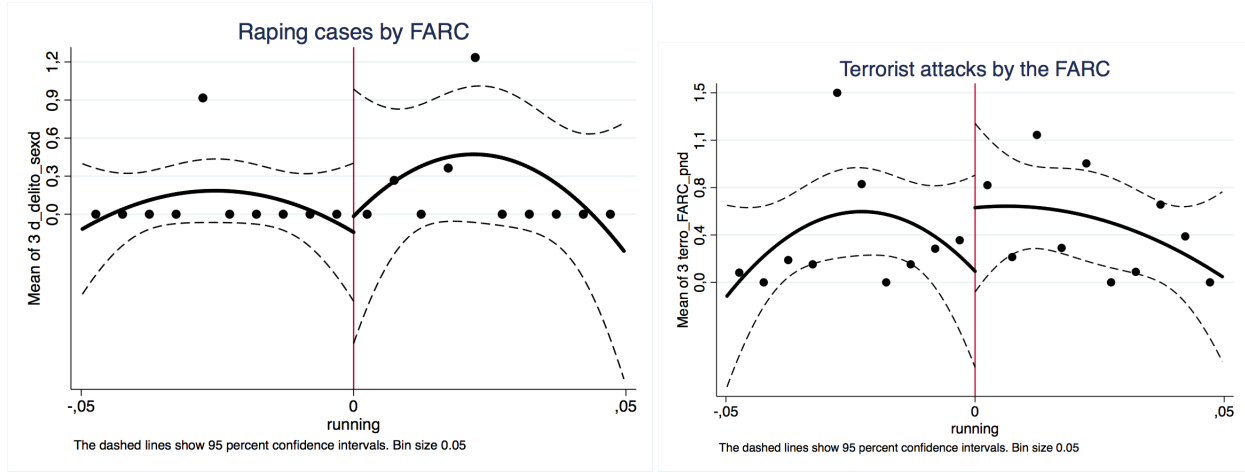


FIGURE 3.7: Effect of Urihist elections on alternative acts of violence by guerrillas II

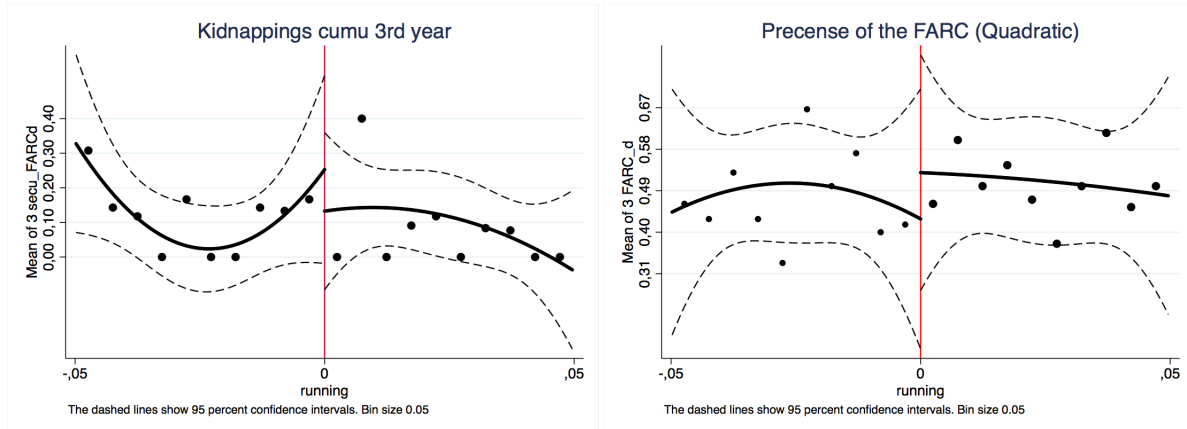


TABLE 3.4: Effect of Urihist elections on FARC and paramilitary presence in municipalities

	One year before the elections		One year After the elections		Two years after the elections		Three years after the elections	
	(1) FARC	(2) Paramilitar	(3) FARC	(4) Paramilitar	(5) FARC	(6) Paramilitar	(7) FARC	(8) Paramilitar
Urihist win	0,12 (0,13)	-0,05 (0,11)	0,11 (0,13)	-0,06 (0,09)	0,20 (0,13)	0,07 (0,08)	0,05 (0,13)	-0,04 (0,07)
Observations	248	248	248	248	248	248	248	248

The table reports the effect of the Urihist party winning by a close margin on paramilitary and guerrilla's presence in a given municipality. The dependent variable in each column is a dummy variable taking the value of 1 if the group had any presence in a given municipality one year before the elections (columns 1 and 2), one year after the elections (column 3 and 4), two years after the elections (Columns 5 and 6) and three years after the elections (Columns 7 and 8), with a bandwidth of 5 percent and a linear polynomial. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 3.5: Number of kidnappings by actor over the year after the election

	(1) All	(2) Paramilitar	(3) Other Guerillas	(4) FARC	(5) Un-known
Uribe win	-0,06 (0,65)	-0,10 (0,14)	-0,06 (0,14)	0,54 (0,41)	-0,12 (0,24)
Observations	248	248	248	248	248

The table displays the effect of the Uribist party winning by a close margin on the occurrence of kidnappings by different groups in a given municipality. The dependent variable in each column is a dummy variable taking the value of 1 if a group committed any kidnapping in a given municipality in the year following the elections, with a bandwidth of 5 percent and a linear polynomial. Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01

TABLE 3.6: Close Uribist election on homicides by sources

VARIABLES	(1) Army	(2) Paramilitar	(3) Guerrilla	(4) Unknown
Panel A: Specification source: Noche y Niebla				
Uribist win	1.125 (2.962)	2.429 (4.644)	4.647 (3.381)	0.644 (1.746)
Panel B: Specification with Official source				
Uribist win	5.168 (8.301)	5.734 (4.902)	4.916* (2.940)	-5.482 (4.208)
Observations	248	248	248	248
Bandwidth	0.050	0.050	0.050	0.050

The table in Panel A and B examines the effect of Uribist close win mayoral elections on the number of homicides per 100,000 inhabitants at the municipality level, using two different sources. In Column 1, the dependent variable is total homicide rate. In Columns 2 to 4, the dependent variables are homicide rate perpetrated by paramilitary, Guerrillas (FARC), and unknown actors respectively. The variable "Uribist win" is a dummy variable equals to one if an Uribist candidate won the election in a given municipality. The sample includes all municipalities where an Uribist candidate arrived first or second by a 5 percentage point or less vote spread margin during the 2003 and 2007 elections. All columns include a linear RD polynomial estimated separately on either side of the threshold and triangular kernel type. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 3.7: Balance Improvement: Sample in 5% Range around the Cutoff

	Original balance				Balance after propensity score-weighted			
	Non False [+] (n=214)	False [+] (n=34)	St.mean diff	P-value	Non False [+] (n=182)	False [+] (n=33)	St.mean diff	P-value
	Mean	Mean			Mean	Mean		
<i>Differences</i>								
Log GDP Agro	9,53	9,83	.	0,217	9,65	9,63	.	0,935
Log GDP Industrial	9,84	11	.	0,0001	10,2	10,2	.	0,976
Log of GDP tot	11,4	12,3	-0,79	0,0001	11,7	11,7	-0,0386	0,85
Log of population	9,6	10,6	-0,941	0,0000	9,91	10,2	-0,245	0,224
Distance to the K	340	321	0,0992	0,592	338	342	-0,0175	0,93
Height	1053	925	0,146	0,431	999	1006	-,00744	0,97
Hectares of Coca	84,2	126	-0,0808	0,663	104	158	-0,0986	0,636
Share Secondary	0,13	0,131	-0,088	0,634	0,131	0,132	-0,123	0,546
Conflict Activity	0,028	,176	-0,69	0,0001	,0532	0,0759	-0,0985	0,625
Share turnout	0,415	0,317	0,866	0,0000	0,379	0,355	0,246	0,208
F-statistic	4,7213				0,3695			
P-value	0,0000				0,9664			

TABLE 3.8: Subgroup analysis by false positives municipalities

	(1) Victims Guerrilla	(2) Victims Guerrilla	(3) Victims Guerrilla	(4) Victims Guerrilla
<i>Panel A: Nonweighted</i>				
Uribist Win*NO-FP	1,20 (4,429)	1,20 (2,54)	5,07 (6,72)	5,08 (3,45)
Uribist win*FP	75,24*** (10,60)	75,23*** (26,53)	66,69*** (14,51)	66,69 (43,00)
Difference Estimate	74,03*** (11,49)	74,03*** (26,82)	61,61*** (16,00)	61,61 (42,71)
Observations	248	248	248	248
<i>Panel B: Propensity score-weighted</i>				
Uribist win*NO-FP	2,29 (2,67)	2,29 (2,42)	6,77 (4,36)	6,77 (4,83)
Uribist win*FP	58,40 *** (15,76)	58,40** (28,01)	49,46*** (16,62)	49,46 (37,37)
Difference Estimate	56,11*** (15,98)	56,11** (27,65)	42,69** (17,18)	42,70 (37,64)
Observations	215	215	215	215
Bandwidth	± 0.05	± 0.05	± 0.05	± 0.05
Spline	Linear	Linear	Quadr.	Quadr.
Controls	Yes	Yes	Yes	Yes
Standard Error	Cluster	Bootstrap	Cluster	Bootstrap

The outcome variables are the number of homicides perpetrated by guerrillas per 100,000 inhabitants in the three years following the elections at the municipality level. Panel A reports the Uribist win effect in the municipalities with and without false positive cases. Panel B was obtained using propensity score weighting. The control variables include the log of agricultural and industrial GDP, rural and urban population, distance to the capital in km, city altitude, hectares of coca farming at the baseline, share of the population in secondary school, dummy if there was any conflict activity, and share of the population voting in the elections by region and period.

***p<0.01, **p<0.05, *p<0.1

FIGURE 3.8: RD Post-Election Municipalities with and without false positive cases

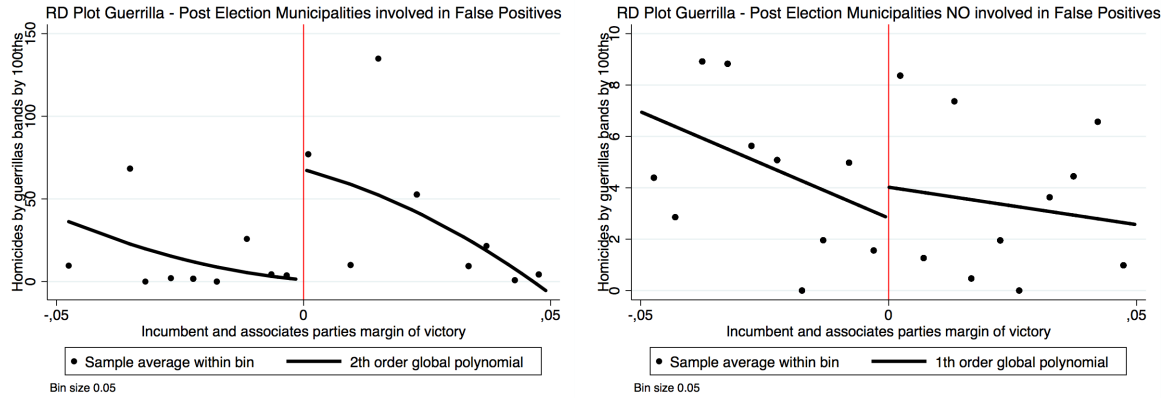


TABLE 3.9: Victims by perpetrator in post-election period according to official sources in False Positives zones

VARIABLES	(1) Total	(2) Paramilitar	(3) Guerrillas	(4) Unknown
Panel A: Sub sample				
Municipalities with False Positives cases				
Uribeist win	1.023 (18.35)	-0.428 (2.442)	19.70** (9.045)	-18.25 (14.38)
Observations	34	34	34	34
Panel B: Sub sample				
Municipalities withno False Positives cases				
Uribeist win	7.081 (9.363)	6.850 (5.366)	3.102 (3.213)	-2.871 (4.158)
Observations	214	214	214	214
Bandwidth	0.050	0.050	0.050	0.050

Source: Official records by the police and official forces. Panels A and B report the effect of Uribeist close win mayoral elections on the number of homicides per 100,000 inhabitants in the sub-sample of municipalities with False Positives and Non False Positives respectively. In Column 1, the dependent variable is the homicide rate committed by all actors in conflict. Columns 2 to 5 is the homicide rate perpetrated by paramilitary, guerrillas and unknown actors, respectively. The variable "Uribeist win" is an indicator equals to one if an Uribeist candidate won the election in a given municipality. The sample includes all municipalities where where a Uribeist candidate arrived first or second by a 5 percentage point or less vote spread margin in the 2003 and 2007 elections. All columns include a linear RD polynomial estimated separately on either side of the threshold and triangular kernel type. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 3.10: Mains specification excluding extreme left wing parties

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitar	(5) Unknown
Uribist win	16.10 (21.20)	-0.419 (4.360)	12.66** (5.672)	9.014 (13.00)	-11.378 (7.868)
Observations	237	237	237	237	237
Mean Control	41.655	6.128	5.547	13.198	11.514

The table displays the effect of Uribist close win mayoral elections on homicides per 100,000 inhabitants in the sub-sample of municipalities after excluding the left wing parties. In Column 1, the dependent variable is the number of homicides perpetrated by all actors in conflict per 100,000 inhabitants. In Columns 2 to 5, the dependent variables are the number of homicides perpetrated by paramilitary groups, guerrillas and unknown actors respectively, per 100,000 inhabitants. The variable "Uribist win" is a dummy variable equals to one if an Uribist candidate won the election in a given municipality. The sample includes all the municipalities where the Uribist candidate arrived first or second at the 2003 and 2007 elections by a 5 percentage point or less vote spread margin. All columns include a linear RD polynomial estimated separately on either side of the threshold and triangular kernel type. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

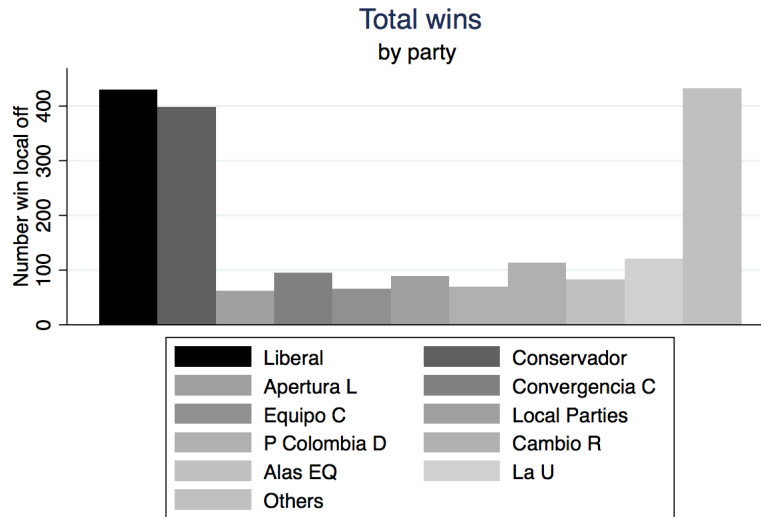
TABLE A1: Victim by author post election by official source in False Positives zones

VARIABLES	(1) Guerrillas Off	(2) Guerrillas Off	(3) Guerrillas Off	(4) Guerrillas Off	(5) Guerrillas Off
RD_Estimate	19.70** (9.045)	27.75*** (9.517)	26.98** (12.56)	20.40** (8.557)	22.18** (9.123)
Observations	34	34	34	60	43
Kernel Type	Triangular	Uniform	Triangular	Triangular	Triangular
BW Type	Manual	Manual	Manual	mserd	cerd
Conventional p-value	0.029	0.004	0.032	0.017	0.015
Robust p-value	0.287	0.373	0.001	0.019	0.016
Order Loc. Poly. (p)	1.000	1.000	3.000	1.000	1.000
Bandwidth	0.050	0.050	0.050	0.094	0.074

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

FIGURE A1: Main parties distribution



Source: Registraduría Nacional Col

A1 Further placebos

Given that municipalities are observed in two different periods of time, I classify the municipalities in three groups: (1) municipalities which meet the conditions for being in the 5% margin sample in period 1 only, (2) municipalities with a close margin in the second period only, and (3) municipalities in both groups. This gives rise to 89 municipalities in group 1, 129 municipalities in group 2, and 15 municipalities in group 3. Tables A2 and A3 present the RD estimation for both groups separately. As expected, larger effects are observed for the first analyzed period, since the DS policy was particularly intense in the first period in municipalities where coalition members won office.

TABLE A2: P1

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitar	(5) Unknown
RD_Estimate	57.66 (52.85)	4.795 (7.371)	13.92** (6.377)	34.38 (37.91)	-1.426 (17.72)
Observations	104	104	104	104	104
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	Manual	Manual	Manual	Manual	Manual
Conventional p-value	0.275	0.515	0.029	0.364	0.936
Robust p-value	0.254	0.165	0.143	0.343	0.872
Order Loc. Poly. (p)	1.000	1.000	1.000	1.000	1.000
Bandwidth	0.050	0.050	0.050	0.050	0.050

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

TABLE A3: P2

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitary	(5) Unknown
Uribist win	-2.055 (16.16)	-3.299 (5.579)	10.12 (7.235)	0 (0)	-7.670 (7.792)
Observations	144	144	144	144	144
Bandwidth	0.050	0.050	0.050	0.05	0.050

Tables A4 and A5 report the RD estimations for the main specification when the groups are changed. The dependent variables in table A4 are civilian victims of the different perpetrators in the second period for the sample of 89 municipalities corresponding to period 1 (2003 - 2006). Similarly, table 25 portrays the RD estimation for the number of civilian victims in the first period by perpetrator in municipalities with a close-run election in period 2 (2007 - 2010). Table A5 shows the RD estimation to be slightly significant. Table A6, however, shows that this significance is robust to neither more flexible polynomial orders nor optimal bandwidth approaches.

TABLE A4: placebo p1

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitar	(5) Unknown
RD_Estimate	14.74 (10.51)	-0.686 (0.897)	5.573 (3.664)	0 (0)	4.564* (2.690)
Observations	89	89	89	89	89
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	Manual	Manual	Manual	Manual	Manual
Conventional p-value	0.161	0.444	0.128	e(pv_cl)	0.090
Robust p-value	0.059	0.216	0.082	e(pv_rb)	0.085
Order Loc. Poly. (p)	1.000	1.000	1.000	1.00	1.000
Bandwidth	0.050	0.050	0.050	0.05	0.050

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE A5: placebo p2

VARIABLES	(1) All actors	(2) Official Forces	(3) Guerrillas	(4) Paramilitar	(5) Unknown
RD_Estimate	-15.90 (31.56)	5.375 (4.189)	7.333* (4.044)	-13.31 (20.04)	-12.03 (15.51)
Observations	129	129	129	129	129
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	Manual	Manual	Manual	Manual	Manual
Conventional p-value	0.614	0.199	0.070	0.506	0.438
Robust p-value	0.417	0.870	0.905	0.589	0.353
Order Loc. Poly. (p)	1.000	1.000	1.000	1.000	1.000
Bandwidth	0.050	0.050	0.050	0.050	0.050

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE A6: placebo p2

VARIABLES	(1) Guerrillas	(2) Guerrillas	(3) Guerrillas	(4) Guerrillas	(5) Guerrillas	(6) Guerrillas
RD_Estimate	7.333* (4.044)	10.25* (5.307)	-0.556 (4.642)	-7.149 (6.657)	-0.788 (3.923)	1.386 (2.970)
Observations	129	129	129	129	43	34
Kernel Type	Triangular	Uniform	Triangular	Triangular	Triangular	Triangular
BW Type	Manual	Manual	Manual	Manual	mserd	cerrd
Conventional p-value	0.070	0.054	0.905	0.283	0.841	0.641
Robust p-value	0.905	0.485	0.283	0.728	0.814	0.708
Order Loc. Poly. (p)	1.000	1.000	2.000	3.000	1.000	1.000
Bandwidth	0.050	0.050	0.050	0.050	0.016	0.012

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Here is an example of the records by Noche y Niebla

- Febrero 09/2008 - DEPARTAMENTO: HUILA MUNICIPIO: ALGECIRAS
Guerrilleros de las FARC-EP dieron muerte de varios impactos de bala, hacia las 10:00 p.m., en el barrio Abraham Palacios, al periodista y concejal de este municipio por el partido político Union Indigena Afrocolombiana.
- Enero 21/2008 DEPARTAMENTO: ARAUCA MUNICIPIO: ARAUQUITA Guerrilleros de las FARC-EP y el ELN causaron la muerte a Wilson de 35 años y a Lucio de 48 años de edad, en momentos en que combatían en el sitio Mate Coco, lugar en el que las víctimas se encontraban pescando.
- Febrero 03/2008 DEPARTAMENTO: TOLIMA MUNICIPIO: ANZOATEGUI Sanchez Fernandez, propietario de la emisora comunitaria Accion FM Stereo de Anzoategui, Tolima, recibió una llamada amenazante por teléfono celular de una persona que dijo pertenecer a las FARC-EP. El mensaje fue una frase: *Si no apaga en una hora, se muere, periodista*. La emisora transmitía desde el 16 de enero de 2008 mensajes publicitarios sobre la desmovilización guerrillera, en los cuales el Ejército invitaba a los guerrilleros a dejar las armas. Había recibido amenazas previas.

Conclusion

The goal of this dissertation has been to shed light on the role of information in different contexts. First, it improves our understanding of the way information spreads at the household and municipality levels in the context of a developing country. Second, it shows that alternative facts expressed by extreme right political leaders might persuade voter to modify their electoral preferences. Finally, it analyses whether and to what extent homicides data can be reported with bias for political purposes in a conflict context.

The first chapter uses a randomized controlled trial design to evaluate the effect of an intervention consisting in sending daily text messages on parenting practices to caregivers in poor households in rural Nicaragua. The program has been found to enhance knowledge and improve some parental practices associated to nutrition and stimulation. However, no improvements were found on children's cognitive or socio-emotional outcomes. These results contrast with those from more intensive interventions aimed at improving parental investment and early-child development (ECD) in Nicaragua and elsewhere in the developing world. Importantly, these results are obtained in a context where cellphone coverage and access to electricity are far from universal, where low levels of education may have limited parents' ability to fully internalize the text messages and where few households were covered by a government-led ECD program. These contextual factors could explain the lack of more positive results, and are worth considering when contemplating the external validity of our findings. At the same time, poor electricity access, limited cellphone coverage and ownership, low coverage of ECD programs, and low literacy are common in many parts of the developing world.

The second main result of the first chapter is that opinion leaders' randomized exposure to the text message intervention led to a significant decline in cognitive outcomes among children from non-leader households. These negative spill-over effects are stronger for children from the least educated parents, and for those living closest to the local opinion leaders. We explore potential mechanisms that could explain these negative effects of opinion leaders' exposure to text messages. While the experimental design does not allow us to fully disentangle the underlying mechanisms, empirical results point to either a boycott of the intervention by opinion leaders or a crowding-out of their interactions with other households. A decrease in the number of interactions about ECD practices between leaders and parents is observed. Interestingly, it stems from leaders and may be driven by their demotivation or de-legitimization, possibly leading to reduced efforts in the provision of ECD information or services.

Overall, the lack of impact of text messages to parents on ECD outcomes and the negative effects of leaders' exposure to the text messages call for caution before advocating for the large-scale roll-out of text message parenting interventions in high-poverty settings. Such interventions may need to be complemented with other types of ECD programs. And they may need to explicitly incorporate strategies to crowd-in local opinion leaders, for instance by giving them an active role in implementation. These hypotheses would deserve to be tested in future research.

In chapter 2, we report the results of an online randomized experiment to measure the persuasion power of alternative facts and the effectiveness of fact-checking to counter their impact. We find that fact-checking can correct biases in factual knowledge introduced by politically-charged alternative facts. Voters update their priors as rational Bayesian updaters with greater confidence in official sources than in politicians providing alternative facts. On the other hand, the fact-checking's effectiveness in correcting factual knowledge does not translate into an impact on voting intentions. Alternative facts are equally effective with and without fact-checking in convincing voters to vote for the politician who uses narratives based on alternative facts.

We cannot definitively establish the mechanism behind these results, but the evidence is consistent with a hypothesis that mentioning immigration issues in alternative facts or real facts statements raises salience of these issues in voters' minds, which in turn move some voters toward anti-immigration policy agenda. In addition, it is possible that alternative-facts narrative in our experiment was interpreted by voters as a signal not only about the state of the world, but also about the candidate herself.

Taken together, our results suggest that providing correct statistical or stylized facts is not sufficient to counter the effect that populist politicians have on voters. When a statistical fact is used in a narrative presenting a logical link to reach a conclusion, fact-checking would presumably need to question the policy conclusion, using the correct facts, logical links and narratives. In our experiment, fact-checking is the exposure of voters to raw facts from official sources. In practice, fact-checkers do sometimes produce longer analyses and discussions of facts. For instance, the article in newspaper *Le Monde* which fact-checked the statement by Marine Le Pen on the proportion of men among refugees embedded true facts in a short narrative containing several paragraphs. The exposure to this sort of fact-checking may have a different effect from the one demonstrated by our experiment. The effects of counter-narratives are understudied and should become subject of future research. However, if salience is, indeed, one of the mechanisms behind our results, the main conclusion of our paper should hold even in the case of a counter-narrative: by insisting on the same issue as the original political communication based on alternative facts, fact-checking may contribute to an increase in the salience of this issue, which may indirectly serve the goal of the original communication. This assertion, however, needs to be verified by future research as the salience mechanism should be studied directly in experimental setting.

Finally, in an internal conflict context, chapter 3 evaluates the consequences of ideology on homicide rates in the presence of political and economic incentives to perpetuate the confrontation. It compares reported homicide rates in municipalities where extreme-right ideology parties won the mayoral elections with those municipalities where they lost by a narrow margin. It finds that the extreme right ideology leads to a significant increase in reported rebels' homicide rate in conflicting areas. In contrast, this study does not see any difference in homicide rates committed by other actors in conflict (i.e., Official Army, Paramilitaries or unknown perpetrators). Besides, it does not find any other difference in other forms of violence such as rapes, kidnappings, terrorist attacks or military presence in the region. The study presents

strong suggestive evidence that data on homicides committed by rebels in areas where the right party is in office, are presumably manipulated to exploit political rents from it.

Local extreme right political leaders might have several incentives to promote and perpetuate violent confrontations in developing economies (Fergusson, 2016), in part due to their lack of strong institutions. There are several ways how extreme right parties can extract political or economic rents from war. For instance, in order to gain more favorable opinions in the polls, politicians might have perverse incentives to create an "enemy" and lead a negative campaign against them by overestimating the number of homicides committed by rebels. Politicians may also receive the political support of some economic groups that derive some direct benefits from the war, via weapons' production (Guidolin, 2007), military spending, etc. This study recommends that society, the international community, and governments set up post-conflict commissions tasked with telling the truth about the conflict to counter hatred and polarization, thereby consolidating peace.

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